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Verifying Exchange Rate Regimes [§]

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Abstract

Credibility and transparency are at the core of the current debate on exchange rate regimes. Among the reasons why intermediate regimes have fallen out of favor, a possibly important one is that they are not transparent: it is difficult to verify them. This paper investigates how difficult it is for investors to verify from observable data if the authorities are in fact following the exchange rate regime that they claim to be following. Of the various intermediate regimes, we focus on the case of basket pegs with bands. Statistically, it can take a surprisingly long span of data for an econometrician or an investor to verify whether such a regime is actually in operation. We find that verification becomes more difficult as the regime's bands widen and/or more currencies enter in the basket peg. At the other extreme, we also analyze regimes announced as free-floating, and find that in some cases the observed exchange rate data are consistent with the announced regime.

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I. Introduction and The Corners Hypothesis

The choice of exchange rate regime – floating, fixed, or somewhere in between – is an old question in international monetary economics. But the steady increase in magnitude and variability of international capital flows has complicated the question. This is particularly the case for the developing countries that in the 1990s became full-fledged participants in international financial markets.

A major new element in the debate is the proposition that emerging market countries are, or should be, abandoning basket pegs, crawling pegs, bands, adjustable pegs, and various combinations of these. The currently-fashionable view is that countries are being pushed to the "corners," the extremes of either free floating or firm fixing. The intermediate regimes are said to be no longer viable. This proposition is variously called the hypothesis of the vanishing intermediate regime, the missing middle, or the corners solution. Its life history has gone from birth to conventional wisdom in a remarkably short period of time.

The motivation of this paper is the observation that, as fashionable as this proposition has become, few of its proponents, if any, have offered an analytical rationale for it, let alone a fully worked out theoretical model. Our aim is to offer a possible theoretical rationale. We seek to introduce the notion of *verifiability,* and to suggest that a simple peg or a simple float may be more readily verifiable by market participants than a more complicated intermediate regime. Verifiability is a concrete instance of the more general principle of "transparency" that is so often invoked in recent discussions of the new international financial architecture but so seldom made precise.

I.a Motivation

Consider the exchange rate regime that a number of emerging markets had in the 1990s: a band around a central parity that itself is a basket with a rate of crawl. So far as existing theory is concerned, the complexity of this arrangement has no implications for its credibility. But, in truth, when a central bank announces a regime of this type, the public has no way of verifying quickly, by observing the exchange rate, whether the central bank is doing what it claims to be doing.

A central bank does not earn credibility merely by announcing a monetary regime with a nominal anchor such as the exchange rate, even if its intentions are sincere. The public will judge credibility from data available to it. Easily verifiable regimes can reduce uncertainty, since economic agents are able to observe the government's actions. So they have an important piece of information regarding the behavior of the exchange rate. Reduced uncertainty does not necessarily imply that the exchange rate is more sustainable, however it can influence future investment and consumption decisions.

If the announced exchange rate regime is a simple dollar peg, a market participant need only check that the exchange rate today is the same as the exchange rate yesterday, in order to verify that the central bank is indeed following its announced policy. If the announced regime is a pure float, a participant can essentially check every month whether the central bank has intervened in the market by seeing whether its reserve holdings have changed. Under the basket band, by contrast, the market participant needs more months of data in order to be able to verify that the central bank is indeed implementing the announced policy. When comparing the corners, simple pegs tend to be more immediately verifiable than floating regimes. Typically, a market participant needs some extra piece of information, like reserves, or more data to check that an exchange rate is truly floating. Sometimes, the quality and frequency of this information makes inference a difficult exercise. How many months of data he or she needs is the central analytical exercise of this paper.

We are not claiming that verifiability is necessarily the complete story behind the purported non-viability of intermediate regimes. And we are certainly not claiming that it is the only criterion, or even the most important criterion, in the larger debate about fixed and floating exchange rate regimes. Many other factors, whether from the traditional optimum currency area literature or the newer criteria associated with credibility and financial markets, need to be taken into account.[1] Our goal is rather to offer an attempt at what, so far as we are aware, may be the first explicit analytical rationale for the proposition that intermediate regimes are less viable than the corner regimes.

In this paper, we demonstrate the difficulties of verifiability for the case of a band around a basket peg. We believe that the same difficulties apply to other intermediate exchange rate regimes, such as a managed float or adjustable peg. One could model a managed float, as a central target and a central bank policy of intervening partially to offset market forces when they push the exchange rate away from that target. But one would have to estimate the central target, and measure somehow the pressure of current market forces in order to figure out to what extent the authorities were intervening to resist them, a difficult econometric exercise. One could model an adjustable peg as a fixed exchange rate with an escape clause: the central bank has an explicit or implicit rule of abandoning the peg when an exogenous shock of a particular size occurs, and when a particular percentage of its foreign exchange reserves have been exhausted. Verifying that sort of rule would be even harder than the others because usually few relevant observations will occur in the sample period, and even when the adjustment takes place, there is little way in practice of verifying whether on the one hand the putative exogenous shock in fact occurred, or on the other hand the government's commitment to monetary discipline was not sincere in the first place. We choose to explore verifiability for the case of the basket band rather than the other examples because it is a cleaner econometric exercise. We also look at countries that are believed to be floating, to offer a contrast to those that are believed to follow basket bands.

This paper explores the amount of information that it takes for market participants to verify announced exchange rate regimes from observed data. The goal of the paper is to show the difficulty to verify exchange rate regimes and how this varies with regimes. To our knowledge, this is the first paper that performs this type of exercise. Because published information on foreign reserve holdings is in many countries of limited reliability and often becomes available only with significant delays, the paper focuses on regime verification based on exchange rate data. We use both observed exchange rate data and simulated data to provide empirical estimates. The fact that countries vary their exchange rate regime over time allows us to run this experiment for regimes of different complexity.

Regarding bands, the paper confirms the intuitive notion that wide bands are harder to verify than narrow bands. It is often difficult or impossible to estimate the weights of the central parity with only one or two years of data. Regarding regimes announced as free-floating, the paper shows that in some cases the exchange rates

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[1] Two recent reviews are Larrain and Velasco (1999) and Frankel (1999).

observed under such regimes are correlated with those of major currencies. In this sense, they behave similarly to the basket countries.

To complement the tests performed with real data, we run Monte Carlo experiments to obtain more general conclusions and to provide results regarding the amount of information necessary to estimate regimes of interest. Monte Carlo experiments, displayed in the Appendix, confirm that more complex regimes take a larger amount of data to be verified. The Monte Carlo exercise shows the role of a number of factors in determining verifiability: the band size, number of currencies in the basket, the rate of crawl, sample period, periodic adjustments of the central parity. The results confirm the intuition that the amount of information necessary to verify the exchange rate regimes increases with the complexity of the regime.

The rest of the paper is organized as follows. The rest of the introduction introduces the verifiability problem. Section II describes the framework and empirical strategy used to verify exchange rate regimes. Section III presents estimations for the case of exchange rate bands. Section IV shows the results from free-floating regimes. The main conclusions are summarized in Section V. Appendix 1 displays a small Monte Carlo exercise extending the study of regime verification to simulated models, and Appendix 2 gives more details on the construction of the numeraire and the estimated models.

I.b Intellectual Origins of the Corners Hypothesis

What is known about the origins of the hypothesis of the vanishing intermediate regime? The original reference is believed to be Eichengreen (1994). The context was not emerging markets, but rather the European Exchange Rate Mechanism. The ERM crisis of 1992 and band-widening of 1993 suggested to some that a gradual transition to European Economic and Monetary Union, where the width of the target zone was narrowed in steps, might not be the best way to proceed after all. (Crockett, 1994, made the same point.) Obstfeld and Rogoff (1995) concluded, "A careful examination of the genesis of speculative attacks suggests that even broad-band systems in the current EMS style pose difficulties, and that there is little, if any, comfortable middle ground between floating rates and the adoption by countries of a common currency." The lesson that "the best way to cross a chasm is in a single jump" was seemingly borne out subsequently, by the successful leap from wide bands to EMU in 1998-99.

After the East Asia crises of 1997-98, the hypothesis of the vanishing intermediate regime was applied to emerging markets. In the effort to "reform the international financial architecture" so as to minimize the frequency and severity of crisis in the future, the proposition was rapidly adopted by the international financial establishment as the new conventional wisdom.

For example, Summers (1999a)[2]:

> "There is no single answer, but in light of recent experience what is perhaps becoming increasingly clear – and will probably be increasingly reflected in the advice that the international community offers – is that in a world of freely flowing capital there is shrinking scope for countries to occupy the middle ground of fixed but adjustable pegs. As we go forward from the events of the past eighteen months, I expect that countries will be increasingly wary about committing themselves to fixed exchange rates, whatever

[2] Other high-profile examples include Eichengreen (1999, p.104-105), Minton-Beddoes (1999) and Council on Foreign Relations (1999, p.87).

the temptations these may offer in the short run, unless they are also prepared to dedicate policy wholeheartedly to their support and establish extra-ordinary domestic safeguards to keep them in place."

The International Monetary Fund has now agreed that countries that get into trouble by following an intermediate regime will in the future not be bailed out, though it qualified the scope of the generalization a bit, for example, by allowing a possible exception for "systemically" important countries.

It may be that the *Economist* (1999, p.15-16) is right that "Most academics now believe that only radical solutions will work: either currencies must float freely, or they must be tightly tied (through a currency board or, even better, currency unions)." But the proposition remains to be modeled, let alone proven. It seems intuitively right that these countries, facing finicky international investors and rapidly disappearing foreign exchange reserves, had little alternative but to abandon their pegs and baskets and bands and crawls and move to a float, unless they were prepared to go to the opposite corner. But what is the rationale for this proposition?

I.c Lack of Theoretical Foundations

What is the analytical rationale for the hypothesis of the disappearing intermediate regime (or the "missing middle")? Surprisingly, none currently exists, to our knowledge.

At first glance, it appears to be a corollary to the principle of the Impossible Trinity.[3] That principle says that a country must give up one of three goals: exchange

[3] Summers (1999b, p. 326) is explicit: "…the core principle of monetary economics is a trilemma: that capital mobility, an independent monetary policy, and the maintenance of a fixed exchange rate objective

rate stability, monetary independence, and financial market integration. It cannot have all three simultaneously. If one adds the observation that financial markets are steadily becoming more and more integrated internationally, that forces the choice down to giving up on exchange rate stability or giving up on monetary independence. But this is not the same thing as saying one cannot give up on *both*, that one cannot have half-stability and half-independence. There is nothing in existing theory, for example, that prevents a country from pursuing a target zone of moderate width. The elegant line of target-zone theory begun by Krugman (1991), in which speculation helped stabilize the currency, always assumed perfect capital mobility. Similarly, there is nothing that prevents the government from pursuing a managed float in which half of every fluctuation in demand for its currency is accommodated by intervention and half is allowed to be reflected in the exchange rate. (To model this, one need only introduce a "leaning against the wind" central bank reaction function into a standard monetary model of exchange rate determination.) And there is nothing that prevents a country from pursuing a peg that is abandoned whenever there is a shock large enough to use up half its reserves.

Another justification that has been offered is that when a government establishes any sort of exchange rate target, as did the East Asian countries, its banks and firms foolishly underestimate the possibility of a future break in the currency value.[4] As a result, they incur large unhedged dollar liabilities abroad. When a devaluation occurs, their domestic-currency revenues are inadequate for servicing their debts, and so they go bankrupt, with devastating consequences for the economy. "It follows that in a world of

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are mutually incompatible. I suspect this means that as capital market integration increases, countries will be forced increasingly to more pure floating or more purely fixed exchange rate regimes."

high capital mobility there are only two feasible approaches to exchange rate policy. One is not just to peg the exchange rate, but to lock it in – the Argentine strategy….The vast majority of countries will … have to follow the other alternative of allowing their currencies to fluctuate. If the exchange rate moves regularly, banks and firms will have an incentive to hedge their foreign exposures…" (Eichengreen, 1999, p.105).

There is little doubt that the focus on unhedged foreign-currency debt describes accurately why the 1997-98 devaluations were economically devastating to East Asia. But the argument, as stated, has some weaknesses. First, it appears to depend on irrationality on the part of banks and firms. Second, it appears to imply that a country would be better off by gratuitously introducing extra noise into the exchange rate, to deter borrowers from incurring unhedged dollar liabilities. This seems unlikely to be right. Third is the point emphasized by Ricardo Hausmann: because foreigners are unwilling to take open positions in the currencies of emerging-market countries, the admonition to avoid borrowing in dollars is to some extent an admonition to avoid borrowing at all. (An admonition to hedge the dollar exposure is not helpful; someone has to take the other side of the futures contract, and this will be difficult in the aggregate if foreigners are unwilling to take the open position.) It may well be that this is the right road to go down, that exchange rate volatility is a way to put some sand in the wheels of the excessive capital movements, and that a lower volume of total debt is a good outcome. But if this is the argument, the proponents should be explicit about it. In any case, it seems doubtful that this argument could be captured by conventional models.

[4] The version of this argument in Eichengreen (1999, p.104) overstates the extent to which the East Asians had "a stated commitment to the peg," as most commentators have done as well. In fact few of the East Asian countries had explicit dollar pegs.

A third possible justification is that governments that adopt an exchange rate target, and sometime later experience a major reversal of capital inflows, tend to wait too late before abandoning the target. As of 1998, we thought we had learned that the one thing an emerging-market government can do to minimize the eventual pain from a currency crisis is to try to devalue early enough (or else raise interest rates early enough, as would happen automatically under a currency board – anything to adjust, rather than try to finance an ongoing deficit). Mexico, Thailand and Korea made the mistake of waiting too long, until reserves ran very low, so that by the time of the devaluation there was no good way out, no combination of interest rates and exchange rate that would simultaneously satisfy the financing constraint externally and prevent recession domestically. But exiting from an exchange rate target can be difficult politically. The lesson is drawn that, to avoid this difficulty, governments should either adopt a rigid institutional fixed-rate commitment (such as the currency boards of Hong Kong and Argentina), or, if not prepared to do that, abandon the peg early.[5]

On this basis, when Brazil in the autumn of 1998 delayed the seemingly inevitable jettisoning of the *real* target, many thought this would be a repeat of the earlier mistakes. Instead, when the devaluation finally came in January 1999, Brazil's trade balance improved sharply, the lack of confidence subsided, and output and employment subsequently performed far better than in neighboring Argentina. Thus it is more difficult to generalize from recent experience than widely believed. Furthermore, if we are to use government reluctance to exit a target arrangement as the basis of a model of

[5] Even then we had a counter-example: Indonesia had widened the band right away in 1997, and yet that didn't save it. But one could argue that political instability would have done Indonesia in no matter what. Taiwan devalued promptly, and suffered less than the others.

the unviability of intermediate regimes, it seems that we would again require some sort of irrationality (or political constraints[6]) on the part of policy-makers.

Thus, each of the three arguments offered – the impossible trinity, the dangers of unhedged dollar liabilities, and the political difficulty of exiting – contains some important truth. But none seems able to stand as a theoretical rationale for the superiority of the corner solutions over the intermediate regimes. Is the corners hypothesis, then, just a misplaced manifestation of the temptation to believe that the grass is always greener somewhere else?

II. Assessing Verifiability

The idea behind verifiability is that the government's announcement of an exchange rate regime is more likely to be credible if market participants can check for themselves from observable data that the announced regime is in fact in operation. Specifically, the goal of our paper is to study how long it takes for financial markets to identify from the data the rules guiding the intervention behavior of the authorities in the foreign exchange markets

The process of verification can be modeled along the lines of statistical inference familiar to econometricians. We are not suggesting that market participants will literally run OLS or other sorts of regressions, but rather that they must do something similar implicitly to process the available information.

The paper's framework encompasses a broad variety of regimes – simple and basket pegs with bands and crawl as well as floating regimes. However, if a country

[6] Governments may have an incentive to postpone devaluations until after elections. See Ernesto Stein and Jorge Streb (1998, 1999).

follows an exact basket peg (i.e., with no band), the problem of statistical inference is of limited interest.[7] In practice, however, there is almost always some range of variation in the observed exchange rate data, even if it is only within a narrow bid-ask spread quoted by the banking system, or within the +/- 1% range that constituted a fixed exchange rate under the rules of the Bretton Woods system. Then the problem of statistical inference is not trivial. For bands of substantial width, the statistical inference can in fact be difficult, as we shall see. This is all the more true if one allows for the ever-present possibility of shifts in the parameters—basket weights, band width, rate of crawl, or level of parity—or changes in the regime altogether, especially if some of these shifts are not announced.

In our empirical analysis, we work with a set of emerging countries, for which we know the announced exchange rate regimes. We will begin with an analysis of the basket bands followed by Chile and Israel during the late 1980s and 1990s.[8] The experience of these two countries—with multiple regimes, including different crawls and bands—are the most natural candidates for the empirical exercise. Chile and Israel changed their band parameters over time, so we are able to examine them under different regime configurations. Then, we move on to the regimes officially declared as floating followed by Brazil, Mexico, Peru, South Korea, and Thailand.

If the currency in question is in fact following a basket band, the question of interest is how many data points are necessary, i.e., how much time must elapse, in order to verify that the regime is in fact in operation. In general, we will consider an anchored exchange rate regime to have been verified if it passes two tests. (1) We fail to reject the

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[7] In that case, the announcement of a basket of N major currencies can be verified with N+1 observations, which is the number needed to estimate exactly the basket weights. As noted, however, this does not constitute verification of an adjustable peg since we don't observe the terms of the "escape clause."
[8] A detailed description of these regimes can be found in Appendix Tables A.1 and A.2.

hypothesis that the exchange rate is following the announced basket peg. (2) We can find statistically significant basket parameters, i.e., can reject the hypothesis that the currency is behaving like any "random" currency. These two tests are informative only if they have adequate power. To judge the power of the tests, we perform the same tests with a randomly generated variable and with a freely floating exchange rate as the dependent variable. When using these latter variables we should reject the null hypothesis in (1) and fail to reject that in (2). In the case of floating regimes, since there are no announced pegs we only use the second test. Below we specify more explicitly the null hypotheses under consideration.

If an announced regime of basket bands does not pass these tests, one can argue that it is not verifiable, which suggests that the country cannot reap the credibility gains that an anchored exchange rate regime theoretically offers—credibility in the eyes of workers and producers who set wages and prices, and in the eyes of speculators who have the ability to attack the central bank's reserves and bring about a crisis. If viability requires verifiability, such a regime may not be viable.

In the case of bands, we are especially interested also in seeing how the ability to confirm the announced nominal regime is statistically affected by features such as the width of the band and the number of foreign currencies in the basket.

Our approach focuses on the empirical estimation of the parameters describing the exchange rate rule at different sample sizes – e.g., 50, 100, and 200 observations. The point estimates, their precision, and the tests of the above hypotheses constructed using them tell us how well can market participants identify the parameters of the regime when the latter is 10, 20, and 40 weeks old. For this empirical analysis, we need a basic framework and a testing procedure. The rest of this section is devoted to these questions.

II.a Basic Framework

We adopt a general formulation to "nest" a number of alternative regimes. We assume that the exchange rate for a given small country is given by a weighted combination of N foreign currencies, with a possible rate of crawl d and an error term. The exchange rate is: [9]

$$s_t = c + d \times t + \Phi\left(w_i, s_{i,t}\right) + e_t \,. \tag{1}$$

where s_t is the spot exchange rate of the domestic currency with its value measured in terms of a numeraire that we will explain momentarily; $s_{i,t}$ are the spot exchange rates of the major "strong currencies" measured vis-à-vis the same numeraire; d is the rate of crawl, which for now is assumed to be fixed during a given sample period;[10] and w_i are the weights given to the currencies included in the basket. Depending on the specification of the basket, Φ may take different forms, with the simplest one being the familiar $\Phi\left(w_i, s_{i,t}\right) = \sum_{i=1}^{N} w_i s_{i,t}$.

Simple Pegs, Basket Pegs, Crawling Pegs, Crawling Baskets

This general case captures many possible regimes, including simple pegs, basket pegs, crawling pegs, crawling baskets, target zones, certain forms of managed floating,

[9] The precise models that we estimated are described in Appendix 2, which also provides a description of the procedure followed by Chile and Israel to construct the basket used as central parity in their band systems.

and free floating. In the case of simple pegs, the value of the currency follows the exchange rates of the foreign currency to which it is pegged, plus the crawling rule, and a stochastic error. The latter is the error allowed or incurred by the government when setting the exchange rate. In the case of simple pegs, N (the number of currencies in the basket) is equal to one. Under basket pegs, N is bigger than one. Crawling pegs imply that $d>0$. Under crawling baskets, $N>1$ and $d>0$.

In the case of an exact peg, the error term would vanish, and an OLS regression of the domestic currency on the foreign currencies to which it is pegged would yield an R^2 equal to 1. Verification is a trivial exercise, whether the peg is simple or to a basket. This can be easily illustrated by examining the behavior of the central parity in band regimes. Central parities behave like simple or basket pegs (with or without crawl), depending on the regime. Frankel, Schmukler and Servén (2000) report estimations of a version of equation (1) above using as dependent variable the Chilean peso central parity. In all the cases examined there, the weights of the central parity converge to their announced values almost immediately. In our present context, the pegged regime is verified instantaneously. Thus, in the remainder of this paper we concentrate on the cases of exchange rate bands (target zones) and floating regimes.

Target Zones

In a regime of target zones, a central parity is defined as a function of a single or multiple foreign currencies and the exchange rate is allowed to float within a pre-specified band around this central parity. Whenever it hits the boundary, the government

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[10] One alternative would be to use past domestic or future inflation rates relative to international inflation rates, where the authorities are believed to be following an indexation policy.

intervenes to keep the exchange rate inside the band. In many cases, governments make intra-band interventions as well.

In a target zone, the log difference between the observed spot exchange rate and the central parity, s_t^*, is determined by the following equation:

$$s_t^* = \begin{cases} -\mathrm{b} & \text{if } s_t < -\mathrm{b} \\ \mathrm{b} & \text{if } s_t > \mathrm{b} \\ v_t & \text{otherwise} \end{cases}$$

where s_t is defined by equation (1) above and b is the band width.

According to theory, the distribution of v can be quite complicated. Even under two simplifying assumptions made by Krugman (1991) in a famous article that generated a sub-field of research on target zones—that the band is 100% credible and that the authorities intervene only at the boundaries—the distribution is not normal, but rather follows a particular S-shape.[11] But extensive empirical investigation of the European Exchange Rate Mechanism in the 1980s and early 1990s established that the spot rate does not in fact obey the predicted distribution. There are a number of likely reasons for this, among them the lack of full credibility of the zones[12] and the prevalence of intra-marginal intervention.

For these reasons we shall assume in our work that v follows instead an autoregressive process, of the form $v_t = r v_{t-1} + u_t$, where u is iid. In fact, we will focus on

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[11] When the spot rate draws close to one edge, speculators are aware that there is a limit on how far it can continue to move in that direction. The expected value will show a regression back toward the central parity. As speculators respond to that expectation, they will push the spot rate away from the margin, even without any intervention.

[12] Imperfect credibility was in the event justified by realignments in the early 1980s, and especially by the ERM crises of 1992-93. It is also relevant for the present exercise, which is entirely based on a starting point that assumes imperfect credibility.

the random walk case of $r = 1$, in accordance with most time series analyses of exchange rates, which cannot reject the unit root hypothesis.[13]

Managed Floating and Free Floating

While there are many possible patterns of exchange rate intervention, our basic framework [equation (1)] only allows us to test whether d or w_i are different from 0. In other words, the government is using some form of nominal anchor or crawling peg rule to guide its operations. Other forms of intervention are not nested in our specification[14]. Hence, we will consider that failure to reject that d=0 and w_i=0 is a characterization of a pure floating regime.[15] In such case, the disturbance term accounts for the entire variance of the exchange rate.

The Choice of Numeraire

The question of what to use as the numeraire to measure the values of the domestic and foreign currencies is a surprisingly subtle one. In the case of exact pegs it makes no difference – so long of course as the same one is used for both dependent and independent variables alike. The correct weights should emerge, with a perfect statistical fit, regardless of the numeraire. But in the general case, the choice of numeraire does make a difference. Past studies have used a variety of numeraires, including the

[13] In unreported results, we found that estimates of ρ were practically 1 in most regressions using equation (1). One extension for further research would be to use statistical distributions implied by more sophisticated versions of the target zone theory. Another would be to take the observed statistical distribution from historical episodes such as the ERM currencies in the 1980s or 1990s.

[14] It is possible to assume that the government follows a variance-reducing form of intervention but, without imposing some a priori value for the variance of the underlying process, it is not possible to identify the intervention parameter.

consumer basket of domestic goods (Frankel, 1993, which emphasized Asian currencies), the SDR (Frankel and Wei, 1995, which emphasized policies of European currencies), the Swiss franc (Frankel and Wei, 1994 and Ohno, 1999) and the dollar (Benassy-Quere, 1999).

Upon further reflection, these measures are not quite right. We wish to consider regimes where the central bank monitors a central parity, but routinely allows appreciations or depreciations relative to that parity in response to such factors as inflation, unemployment, trade deficits or surpluses, various market pressures and so on. These factors are only partially accommodated under an intermediate regime such as a band or managed float, but they have a role nonetheless. We have not chosen to model explicitly these factors; they are comprised by the error term. The authorities are presumed to be trading off the long-term credibility benefits of sticking relatively close to their central nominal parity against the monetary-independence benefits of responding to short-term developments. But in framing this tradeoff, there is no reason for them to think of the departure above or below the central parity in terms of dollars or a basket of goods, and still less reason to think in terms of Swiss francs. The most useful way to phrase these appreciations and depreciations is, rather, in terms of an effective exchange rate, that is, a weighted average of trading partners' currencies.

In this paper we measure values of currencies in terms of a weighted basket of the G7 currencies. One possible set of weights is the bilateral trade shares of the smaller country in question. This has a drawback: it leaves out the role of all the other bilateral trade partners, as well as third-country markets and competitors. But most of those are

[15] We use the term free-floating to refer to a case where there is no correlation between the studied currency and any of the strong currencies. It is possible to argue that under pure free-floating, market forces might

linked to some combination of the major currencies. Here we adopt the simple approach of using the G7 countries' weights in gross world production. In this way it is hoped that, for example, the large weight of the US will roughly reflect the importance of dollar-linked countries in the trade of Chile or Indonesia beyond the share of the US in bilateral trade of those two countries.[16] Thus, the exchange rates, both of the major currencies and the currencies under study, are calculated as the number of units of the currency necessary to purchase a weighted basket of strong currencies.[17]

II.b Empirical Strategy

We use daily data in our empirical experiments.[18] To assess how verifiable different exchange rate regimes are, we use explicit statistical tests that attempt to replicate those implicitly carried out by financial market participants to learn about the actions of the monetary authorities. For countries that have announced a basket band – such as Chile and Israel during the sample periods we use (examined in section III below), we seek to establish the amount of information (days) needed to reach a judgment on whether the data support the hypothesis that the exchange rate is following the announced regime. In the case of currencies that have declared their regime as a pure float – like post-crisis Mexico and Thailand (section IV below) – the purpose of the exercise is to offer a standard of comparison for the first set of currencies.

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induce some correlation with the currencies of major trade partners.

[16] A second advantage of using GDP weights is that one does not need to obtain the full set of bilateral trade data and re-compute a new set of weights for each country. But using bilateral trade weights is a possible extension for future research.

[17] See Appendix 2 for a detailed description of the construction of the numeraire.

[18] Data on major currencies and some of the emerging countries was extracted from Bloomberg and Datastream. Data for the case studies of Chile and Israel was downloaded from the respective central bank web sites.

A test that fails to reject the announced regime for the currencies following basket bands, has low power if the same test also fails to reject an analogous hypothesis applied to floating currencies. We wish to see whether the public can distinguish the two sorts of policies statistically, rather than having to rely on the assumption that it can instantaneously intuit the true policy of their central bank.

To make this approach operational, we summarize the exchange rate regime in terms of the basket weights in equation (1). Tests of hypotheses about the exchange rate regime then are just tests of hypotheses regarding the basket weights. The tests we perform are the following.

<u>Test 1 (T1)</u>: Market participants test whether the weights obtained from empirical estimation of equation (1) are equal to the announced weights. Conditional on the announcement being true, we expect that this null will not be rejected. The null and alternative hypotheses are:

H0: w_i= announced weights; HA: $w_i \neq$ announced weights.

<u>Test 2 (T2)</u>: The second test inquires more generally, whether we can reject that the currency is freely floating. We assume that market participants do not know what the government is doing, for example because the government has not explicitly announced a regime, or else they do not necessarily believe the announced exchange rate regime. The null hypothesis is that the value of the currency follows a random walk with or without drift. Therefore, we think of market participants as testing if all the weights on the strong currencies are jointly equal to zero. Formally,

H0: $w_1 = 0$ ... and ... $w_N = 0$; HA: $w_1 \neq 0$ … or ... $w_N \neq 0$.

One problem with this approach is that Test 1 might fail to reject the null due to lack of power—e.g., if we work with too short a time sample. Market participants know instinctively that a failure to reject the regime is an informative finding only when that test would be capable of rejecting the regime in the case where it was false. Otherwise, the test would not be distinguishing between true and false models. There are different ways to assess whether this exercise is informative. To see if Test 1 has power, we complement it with two experiments, in which we replace the dependent variable with fictitious data and with a floating exchange rate. Then, we test the null hypothesis that the weights are equal to the ones announced by Chile and Israel. We perform this experiment for the cases in which Test 1 fails to reject the null hypothesis. In this experiment, we expect to reject the null, given that we are using a false model, false weights. Analogously, to check that Test 2 is not rejecting the null hypothesis when it is true (i.e., it is not making a Type I error), we perform a similar experiment for Test 2. In this case, we should fail to reject the null hypothesis of Test 2 because we are using fictitious data and supposedly floating regimes.[19]

To estimate equation (1) and carry out inference on its parameters, a variety of procedures are potentially applicable. In this paper we report results using a "naïve" estimation procedure, which we implicitly assume to be the one that market participants apply to process the observed data. Specifically, we compute OLS estimates of equation (1) in first differences.[20] We do this for all the exchange rate regimes explored in the

[19] Alternatively to T2, which checks the null that all weights are zero, we used another test of the null that all the strong currencies have the same weight, obtaining very similar rejection frequencies.
[20] The estimated models are described in Appendix 2. To save space, we do not report unit root tests or alternative specifications like the error-correction model, whose estimation results for the case of Chile are reported in detail in an earlier working paper version of this paper (Frankel, Schmukler, and Servén, 2000).

paper. While more complex models, such as those derived from the recent target zone literature, might offer some advantage in terms of consistency, their estimation would also require a vastly larger amount of data. Therefore, we work with these relatively simple specifications to carry out our tests and illustrate the point of the paper.

As independent variables for the basket band regimes, we use those currencies that were included in the announced basket. In some cases we found that some of these currencies were strongly correlated over some periods (particularly the Deutsche mark and French franc, both included in the Israeli basket), so that the estimations were plagued by severe multicollinearity and identification of the specific weights was almost impossible. To solve this problem, we opted for computing also estimates of a "restricted" model combining the most highly correlated currencies, using the ratio of their announced weights. We return to this below.

III. Verifying Exchange Rate Bands

Using the framework and empirical approach just described, this section focuses on the verifiability of the exchange rate bands followed by Chile and Israel over recent years.

III.a Chile

A number of successive exchange rate regimes have been in place in Chile since the early 1980s.[21] In 1982, Chile had a crawling peg vis-à-vis the US dollar, with daily devaluations following the difference between domestic and external inflation. The peg

[21] A detailed chronology of the exchange rate system in Chile is presented in Table A.1 in the appendix.

to the dollar continued until 1992, with bands of varying width around the central parity and with realignments of the central parity. In 1992, the government decided to adopt a target zone around a basket peg. The weights on the currencies defining the central parity changed over time and there were realignments, but the central parity was always tied to the US dollar (US$), the Deutsche mark (DM), and the Japanese yen (JY). Finally, in September 1999 the central bank decided to float the peso.

The entire period of exchange rate bands can be broken down into a number of sub-periods distinguished by different levels of the central parity, basket composition and band width. To analyze the verifiability of Chile's exchange rate band system, we focus on seven of those sub-periods, selected on the basis of a minimum duration (specifically, those comprising at least 249 daily observations, amounting to approximately one year). The relevant parameters characterizing these sub-periods are summarized in Table 1.a. The first three sub-periods involve a peg to the US dollar with a band, while the last four involve a basket peg with a band.

Figure 1 displays the observed exchange rate in terms of the weighted basket numeraire, along with the announced bands. The figure shows that the trend of the peso has been to depreciate over time, with significant appreciations and depreciations on several occasions, and highlights the fluctuations of the exchange rate within the band, as well as the gradual widening of the latter. In some periods, like 1991-92, the exchange rate is close to the lower band. In other periods, like 1994-95, the exchange rate fluctuates farther inside the band. After suffering pressure on the peso, the authorities decided to narrow the band from 12% to 3.5% in September 1998 to show their commitment to the value of the peso. The band was widened back to 8% in December 1998.

Table 1.b reports the results of the verifiability tests using the Chilean exchange rate data, based on first-difference OLS estimates of the basic equation. For each of the seven sub-periods under consideration, the table presents the cumulative rejection frequencies of the null hypotheses of Test 1 and Test 2 at increasing sample sizes – 50, 100 and 200 observations. For example, a rejection frequency of 100 for 50 observations in Test 2 means that in 100 percent of the estimations with sample sizes smaller than 50 we can reject the null hypothesis that the weights are equal to 0.[22] In addition, the table also reports point estimates and standard errors of the weight of the US$ in the basket defining the central parity (to save space, we omit the estimated weights of the other currencies). Finally, the last two columns of the table give a measure of the precision of the estimates, in terms of their mean absolute error – that is, the sum of absolute deviations of the estimated weights from their announced values.[23]

For periods 4-7, when the central parity is defined by a basket of several currencies, the table presents two sets of results. The first set is based on an unrestricted version of the model, in which we attempt to estimate the individual weights of all currencies in the basket. As already mentioned, however, this procedure may run into difficulties due to the high correlation among some of these currencies in some sub-periods, and therefore we also present results from a restricted model version combining the most highly-correlated currencies in the proportions dictated by the announced

[22] The first estimation starts with the minimum number of observations required to estimate the models.
[23] Since the announced weights sum up to 1, no re-scaling is required.

weights. In the Chilean case, this involved combining in such fashion the US dollar and the yen.[24]

The results in Table 1.b show a clear difference between periods 1-3 and 4-7, regardless of whether we use the restricted or unrestricted model in the latter sub-periods. In the former sub-periods, the point estimates of the US dollar weight approach fairly quickly the announced weight (equal to one), especially in periods 1-2. In these two periods the estimated weights are not statistically different from the announced value (Test 1), but are statistically different from zero (Test 2) for any sample size. In turn, in sub-period 3, with an increased bandwidth (equal to 5%) relative to periods 1-2, the point estimate of the US dollar weight still approaches its announced value, although at a somewhat slower pace than in periods 1-2. However, we also find a higher rejection rate in Test 1 and a somewhat reduced rejection rate in Test 2. On the whole, these results tend to suggest that the widening band makes verification more difficult.

In contrast, for periods 4-7, characterized by a currency basket and much wider bands, none of the estimates in Table 1b – whether restricted or unrestricted – appears close to the announced values even after a reasonably large number of observations. Precision is much poorer than in the earlier periods, although the restricted estimates are in general substantially more precise than the unrestricted ones (see the last two columns in the table). In any case, both sets of estimates appear clearly biased; indeed, some point estimates are even negative. As a result, while Test 2 generally rejects the null of zero weights, Test 1 also rejects the announced weights in most samples, and this applies both to the restricted and unrestricted estimates.

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[24] The correlation between the first differences of these two currencies exceeds .85 in some of the sub-periods of analysis.

These results can be more easily understood with the help of Figure 3, which presents scatter plots of the observed exchange rate of the Chilean peso against the central parity. In the first three sub-periods, the points cluster along the 45-degree line, reflecting a relatively close match between the peso and the central parity. As the band widens in the last four periods, the peso can fluctuate further away from the central parity. This is particularly apparent in periods 5-7, whose scatter plots display little or no clustering along the 45-degree line. Thus, it is not surprising that in the first three periods the basket weights defining the central parity can be estimated fairly precisely from the observed exchange rate data, while this is not the case in later periods.[25]

On the whole, the results for Chile strongly suggest that the widening of the band, together with the adoption of multiple instead of simple pegs, make verification of the announced regime more difficult using simple econometric estimates. [26]

III.b Israel

Israel presents another interesting experience of basket band with weight changes and progressive widening of the band. During the periods on which we will focus, the band included the same five currencies [US dollar (US$), Deutsche mark (DM), British

[25] The scatter diagram, along with Figure 1, also provides some clues for the relatively poorer verifiability of the third period vis-à-vis the first two. In the early part of the third period (approximately 50 observations), the exchange rate was practically pegged to the upper part of the band, but starting in early 1990 the peso started appreciating until it finally reached the lower band. This marked break in the trajectory of the peso, clearly visible from the scatter plot in Figure 3, is behind the poorer performance of Tests 1 and 2 in the third period that is apparent from Table 1b.

[26] One could object that the contrast between the results we obtain for the earlier and later periods of Chile's band regime might be due instead to some underlying change in the behavior of the strong currencies or in the way the peso moved within the band. However, the intuition that verifiability is more difficult with wider bands and baskets with more currencies is confirmed by the Monte Carlo experiments in Appendix 1, which are not subject to those objections.

pound (BP), French franc (FF), and Japanese yen (JY)] and the bandwidth rose from 3% to 15%.

The Bank of Israel had already introduced an exchange rate band around a basket in 1976.[27] It lasted for a year before being replaced with a floating exchange rate, followed in turn by a dollar peg in 1985 and a basket peg in 1986, with basket weights determined by trade shares and subject to relatively frequent revisions.[28]

At the beginning of 1989, the Bank of Israel reintroduced a band system by allowing the exchange rate to fluctuate within a region of ±3% around the currency basket defined by the five currencies already mentioned. The band was later widened to 5% in March 1990, 7% in May 1995, and then gradually since June 1997, to reach 15% by the end of that year.[29] Most importantly, since December 1991 a pre-announced, constant rate of crawl was added to both the midpoint and the band – a system known as a crawling band.

Figure 2 shows the evolution of the Israeli exchange rate and the exchange rate band. One feature that stands out is the frequency of realignments of the central parity, particularly in the early years of the band. For the analysis of verifiability, we divided the sample into different sub-periods characterized by different bandwidth, basket weights and/or rate of crawl of the exchange rate band. Table 2.a lists the periods under consideration, their beginning and ending dates, and the relevant parameters of the band.

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[27] For a detailed account of the exchange rate policy in Israel, see http://www.bankisrael.gov.il and Appendix Table A.2.

[28] The number of units of each currency in the new basket was originally determined according to its share in trade during the previous calendar year and to international cross rates. Since then, the trade shares were revised annually and when significant changes produced, the weights and units in the basket were recalculated. The number of units of each currency in the basket is kept constant, but its weight – understood as the share in the total *cost* of the basket - can change daily according to changes in cross rates (see Appendix 2 for more details).

In the case of Israel, collinearity among basket currencies is more of an issue than in Chile due to the larger number of currencies and, especially, to the simultaneous inclusion of the French franc and DM in the basket.[30] Thus, for the restricted model estimation we combined the DM with the franc and the US dollar with the yen, using in each period the ratio of announced weights.

The empirical results for Israel are reported in Table 2.b, which is analogous to Table 1.b for Chile. It is apparent from the table that the exchange rate system can be unambiguously verified by our procedure only in the third sub-period (labeled 2.2 in the table), when the announced weights cannot be rejected by Test 1 and zero weights are clearly rejected by Test 2 – particularly when using the restricted model estimates. In the other sub-periods, the unrestricted estimates wander off very far from the announced values, and lead to rejection of both null hypotheses in the majority of cases, even though their precision is extremely poor. In turn, the restricted estimates are much more precise, and generally closer to the announced weights. In general, they lead to rejection of the null of zero weights for sufficiently large samples in all sub-periods, but tend also to lead eventually to rejection of the announced weights except in period 2.2.

Like in the case of Chile, the scatter plots presented in Figure 4 help understand these empirical results. Period 2.2 is the only one in which the observed exchange rate behaved in a fashion similar to the central parity. During this period, which coincides with the introduction of a crawl in the path of the central parity, the exchange rate hovered around the midpoint of the band, and the boundaries were never reached.

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[29] Appendix Table A.2 provides more details on the developments of exchange rate policy in Israel since 1986.

[30] The correlation between these two currencies exceeds .98 in some of the sub-periods under consideration.

In contrast, during periods 1 and 2.1 the frequent level adjustments to the band already mentioned are reflected in the disconnected scatter plots of Figure 4. From the perspective of verifiability, these jumps make identification of the basket weights more difficult. Finally, in the wider-band periods 3-6, the scatter plots are more reminiscent of those corresponding to the multiple-currency periods of the Chilean band: they show little correspondence between the central parity and the observed exchange rate.

In summary, one interpretation for the poor verifiability results in the Israeli case probably lies in the additional complexity induced by the presence of five mutually correlated currencies in the central basket. Even after reducing to three the number of regressors, identification is still poor. The sharp discontinuous changes in the central parity in the earlier periods, and the augmented band width in the later ones, are also likely obstacles to the verification of the regime.

III.c Is the Test Informative?

We conclude this section with a reassessment of the robustness of our findings for the cases in which we achieved verification (periods 1-2 in Chile and 2.2 in Israel). We do this by constructing a randomly generated variable and using it to replace the observed exchange rate as dependent variable in the empirical estimation and testing procedures performed earlier. The results are reported in Table 3, from which it is apparent that Test 1 rejects the announced weights in most cases, and Test 2 fails to reject the zero weights in all the cases. This suggests that problems with test power are not behind the success in verifying the exchange regime in these episodes.

As a final exercise to reassess the robustness of our findings, we replace the Chilean peso and the Israeli shekel with the Swiss franc. We choose the Swiss franc because Switzerland had a floating regime during the periods of interest. Table 3 shows that we reject the null hypothesis that the weights are equal to the announced weights. As reported before, for the same periods, the estimations with the Chilean peso and the Israeli shekel fail to reject the announced weights. Therefore, one can conclude that periods 1-2 in Chile and period 2.2 in Israel are verifiable.

Table 3 also shows that the Swiss franc rejects the null hypothesis that the weights are equal to zero. This rejection does not mean that the Swiss franc is not freely floating during the periods under consideration. Exchange rates are correlated for other reasons than government intervention. This tends to yield rejections of zero correlation. The next section of the paper explores whether it is possible to fail to reject free floating using the methodology applied for band regimes.

IV. The Case of Floating Regimes

To compare with the band cases, we now turn to floating regimes. These regimes are of a different nature than the ones analyzed before, because governments do not assume a commitment to follow a certain exchange rate rule when opting for floating regimes. Under these regimes, central banks can either let the exchange rate move freely or can intervene with no specific rule (what is called managed floating). Therefore, the concept of verifiability has a different meaning here. There are no exchange rate rules to be verified, except perhaps that the exchange rate is floating or that the government is not intervening in the market. Applying the methodology used for exchange rate bands, market participants can check if the exchange rate is uncorrelated with major exchange rates.

A rejection of no correlation is not necessarily a rejection of a free-floating regime. Using observed exchange rate data, it is generally difficult to fail to reject that weights are equal to zero, either because governments intervene or because exchange rates co-move in response to common shocks. However, failing to reject zero weights, with a reasonable sample size, is a good sign of no intervention (or no pegging to other currencies), assuming the tests have enough power. We rejected zero weights in the case of the Swiss franc above. Now, we move to the case of emerging markets.

Before proceeding with the estimations, note that there are other alternative ways of verifying free-floating regimes. Market participants can essentially check every month whether the central bank has intervened by seeing whether its reserve holdings have changed. Also, banks usually know who is participating in the market, so they can tell the difference between a system where the central bank never intervenes and where it intervenes occasionally. These methods require some type of additional information beyond observed exchange rates (such as reliable data on foreign reserves) which might be difficult to gather on a prompt and frequent basis. In this paper, for ease of comparison with the previous section, we stick to verifiability just using exchange rate data.

For the verification of floating regimes, we focus on Brazil, Mexico, Peru, South Korea, and Thailand during specific periods in the 1990s. These countries provide a good opportunity to compare periods of intervention with periods of free-floating. Brazil, Mexico, South Korea, and Thailand suffered exchange rate crises in the 1990s, which forced them to abandon previous exchange rate arrangements and adopt systems officially described as free-floating by their respective authorities. Therefore, for these countries we perform the same statistical tests for periods labeled as free floating and for periods during which other regimes were in operation – namely periods of managed floating, bands, crawling pegs, and basket pegs. Peru, on the other hand, is an interesting case because despite declaring a free-floating regime for the entire decade, several papers have noted that the Peruvian exchange rate has remained surprisingly steady. (See Calvo and Reinhart, 2000, Hausmann, Panizza, and Stein, 2000, and Edwards and Savastano, 1999, for characterizations of floating regimes.) As in other cases, the observed pattern calls into question whether the government is in truth following the regime that it says.

Following the methodology used in the previous section, we estimate our basic equation (1) for each of these countries over the periods noted in Table 4 and dictated by data availability over the 1990s. We allow for a constant rate of crawl, include as regressors the five major currencies (US dollar, yen, DM, British pound and French franc), and as before estimate the model by OLS in first differences.[31]

In free-floating regimes, we expect to fail to find any peg of the exchange rate vis-à-vis foreign exchange rates, so we would expect Test 2 not to reject the null of zero weights. Table 4 reports the percentage of observations for which the test does reject the null hypothesis that weights are equal to zero. If the exchange rate is in fact free-floating, one would expect to find low values in the table, meaning that we only find a relationship between the local currency and strong currencies in very rare occasions. On the other hand, when the central bank follows a specific rule relative to one or several strong

[31] As we are only testing the hypothesis of all the weights being equal to zero (a standard goodness of fit test), we are not concerned by the potential multicollinearity problem.

currencies, we expect to find large values in the tables (mostly rejections of the hypothesis that weights are equal to zero).

For ease of comparison, the shaded areas in Table 4 correspond to periods labeled as free-floating. The results displayed in the table show that in the case of pegs, bands, and managed floating regimes we reject in almost every sample the null hypothesis that the weights are equal to zero. The only exception is the case of Brazil, during part of her period of managed floating. On the other hand, in the episodes declared as free-floating we generally fail to reject the same hypothesis. There are two exceptions, however: Peru and most of the post-Tequila period in Mexico.

The samples used for the tests reported in Table 4 start at the beginning of the year except when a specific date is known for the transition to floating. As an alternative approach, Figure 5 shows the rejection percentage (right-hand scale) of the zero-weights hypothesis in rolling samples of 100 observations during the 1990s, together with the exchange rate vis-à-vis the US$ (in the left-hand scale).

The pattern is similar to that shown in Table 4. It is possible to see how rejection rates fall dramatically right after a major devaluation in the three countries affected by the late 1990s crisis: Brazil, Korea, and Thailand. In the last two cases, we observe a similar pattern in the sense that after approximately one year has elapsed since the large devaluation, rejection rates appear to rise again. In the Mexican case, rejection rates fall only during short periods after the late 1994 crisis. Much interest has been devoted to the Mexican free-floating regime that followed the Tequila episode of 1994, particularly during the stable period starting in late 1995. Edwards and Savastano (1998) found that the volatility of the exchange during 1996 was not smaller than that of other currencies widely considered as free-floating, but there seemed to be some form of feedback rule from the exchange rate to monetary policy.

Finally, it is also clear from the figure that periods of marked stability of the exchange rate are matched by rejections of the zero-weights hypothesis. Examples of this are the Brazilian band period (1995-1998), the Korean, Mexican, and Thai pre-crisis periods, and the Peruvian free-floating regime of the 1990s.

V. Summary and Concluding Remarks

The new conventional wisdom is that intermediate exchange rate regimes, such as baskets, crawls, and bands, are no longer viable. According to this proposition, countries are being pushed to the "corners," the extremes of either free floating or firm fixing. We have argued that a theoretical rationale for this proposition is currently lacking; none of the candidates offered – the impossible trinity, the dangers of unhedged foreign liabilities, or government reluctance to abandon ship in time – is quite up to the job. We offered such a rationale, by introducing the notion of *verifiability*. By verifiability we mean the ability of a market participant to infer statistically from observed data that the exchange rate regime announced by the authority is in fact in operation. Verifiability is an instance of transparency, a means to credibility. Our point is that a simple regime such as a clear dollar peg, or even a free float, may be more verifiable by market participants than a complicated intermediate regime. In this way, simpler regimes could also contribute to reduce uncertainty, which in turn could affect consumption and investment decisions. In this paper we have made a first attempt at assessing empirically the verifiability of various exchange rate regimes. We first focused on the verification of exchange rate bands, drawing from the experiences of Chile and Israel. In the case of Chile, when the band was relatively narrow and the peg involved only the dollar, verification is relatively easy to achieve. But from 1992 to 1999, when the band became wider and the peg involved additional currencies, our simple statistical procedures fail to achieve verification. In the case of Israel, whose basket involved five currencies, two of which were very strongly correlated, we only achieve verification in a period of relatively narrow band in which the central parity does not experience sharp realignments, and only when using a restricted specification involving a reduced number of currencies. In wider-band periods, and in narrow-band periods with frequent realignments, our procedures again fail to achieve verification of the regime. This is precisely the result we expected.

On the whole, the results suggest that higher band width, as well as the adoption of multiple instead of simple basket pegs, and frequent parity realignments, all make more difficult the econometric verification of the announced regime.

The finding that Chile and Israel fail to reject the announced weights for some particular periods seems to be an informative test. For the same time periods, we reject those weights when we replaced the peso and shekel by a randomly generated variable and by the Swiss franc. This means that for narrow bands we are able to verify the announced exchange rate regime.

We also examined the verifiability of regimes self-declared as free floating in several Latin American and East Asian countries in the 1990s – Brazil, Mexico, Peru, South Korea, and Thailand. We followed the same methodology used for bands, testing whether supposedly floating exchange rates are correlated with major exchange rates. Our tests do not show significant evidence against the hypothesis that the exchange rates of these countries are indeed floating, with the exception of Peru and part of the post-Tequila period in Mexico. In these cases, we find some evidence that the exchange rate is in fact moving along with some weighted combination of strong currencies. This appears to agree with the conclusions reached by other researchers and, partly, with the evidence obtained for exchange rate bands. In sum, in various cases we reject free-floating regimes, even when governments might not be intervening, perhaps due to cross-currency correlations reflecting common shocks. Whether these findings can be extended for long periods of free floating, after the high volatility in the aftermath of crises has vanished, is a question for future research.

The analysis in the main text was complemented by means of Monte Carlo tests reported in Appendix 1 assessing the effects of bandwidth and number of currencies in the basket on the time needed to verify exchange rate bands. On the whole, the results agree with the above findings. As expected, when the range of variability of the exchange rate is relatively large, the number of observations needed to verify the regime increases considerably with the width of the band. The number of observations needed to differentiate the crawling basket from a random variable in at least half of the samples is under 100 days when the band width is 2%, as it was for Chile from 1985 to 1987, but is over 500 days when the band width is 10%, as it was for Chile from 1992 to 1998. Regarding the role of the number of currencies in the basket, we find that moving from a single-currency parity to a 3-currency basket increases the amount of data needed to distinguish the basket from a random currency by an extra year's worth of observations (assuming a 10% band, and again using the criterion of finding statistically significant weights at least half the time).

If we are right that it is hard for a central bank to establish credibility for its proclaimed monetary regime without verifiability, then our results confirm that complicated combinations of baskets, crawls, and bands, are less likely to satisfy skeptical investors than are simpler regimes. We thus offer a possible and much-needed rationale for the hypothesis of the vanishing intermediate exchange rate regime.

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Appendix 1: Monte Carlo Simulations

We turn now to the Monte Carlo simulations, which offer a more general testing ground for verifiability of intermediate regimes. For our experiments, we generate 1,000 samples according to the simple model described by equation (1), using for the baskets actual data on the exchange rates of the major currencies (valued in terms of the GDP-weighted numeraire). We use daily data between February 1986 and September 1999. The parameters of the data-generating process are c (level of exchange rate), d (yearly rate of crawl), $w_1 ... w_3$ (weights on US$, DM, and JY), σ (standard deviation of the error term), and t_0 (initial observation). We use a log linear version of equation (1). The log error term is generated as *i.i.d.* normal with mean zero. Based on this basic framework, we study the effect of different model specifications on the amount of time to reject our proposed null hypotheses. For each sample, we calculate the number of observations necessary to obtain 10 rejections of the null hypothesis that both the weights and the rate of crawl are zero (Test A) and the null hypothesis that the weights are zero (Test B).

Role of Band Size

Clearly, it should be harder to verify a basket regime with a wide band than one with a narrow band, and harder to verify a basket regime with a loosely managed float (i.e., a small tendency to intervene when the exchange rate drifts from the parity) than another with a tightly managed float (a strong tendency to intervene). To verify the role of band size in determining the amount of information needed to reject the proposed null hypotheses, we generate sets of 1,000 samples. Each set has a different standard deviation of the underlying disturbance (σ), representing different band sizes.

For this exercise, we generate the samples using a level parameter equal to 1, a rate of crawl of 1% per year, and equal weights for all major currencies, and starting from observation 1 (2/24/1986). We let the standard deviation σ vary from 1% to 10%. In this regard, recall that 2% was the width of Chile's band from mid-1985 to 1987, and 10% was the width of the band during the period 1992-97. For purposes of comparison, 2¼% was the width of the ERM target zone followed by many European countries up until 1992 (and still followed today by Denmark), 6% is the width of the ERM target zone followed by Italy and the United Kingdom up to 1992, and 15% is the width of the ERM zone for France and others from 1992 until the beginning of EMU in January 1999.

The results appear in Appendix Figure 1. The graphs plot the quantiles of Test A and Test 2 against the standard error (σ) used to generate the samples. Each line corresponds to one quantile, and depicts the number of observations needed to achieve rejection of the null hypothesis (at the 5% level) in x% of the 1,000 samples—where x is the quantile in question.

As expected, the graphs show that, for both tests, the number of observations needed to reject the null of zero weights and rate of crawl in any given percentage of the samples rises steadily with σ. This is reflected by the fact that the lines corresponding to the various quantiles have positive slopes. In other words, wider bands make it more difficult for investors to reject specific hypotheses concerning the weights of the central parity—they need more time to get an accurate assessment of the parameter values. And the additional time needed is not negligible. For Test B, for example, the number of observations needed to reject the null in 50% of the samples ranges from under 100 days for an (old-) EMU-sized band (2% width) to over 500 for a Chilean-sized one (10% width).

Role of Number of Currencies in Basket

Intuitively, the larger the number of unknown parameters that need to be estimated, the harder it should be to verify that the data match the announced policy regime. This applies not only to the number of currencies in the basket, but also to the presence of a non-zero rate of crawl.

To verify this assertion, we next examine the impact of different basket sizes on the amount of information needed to reject the nulls underlying Tests A and B. For this purpose, different numbers of currencies were included in the Data Generating Process (DGP). We construct a simple peg (the US dollar), a two-currency basket (the US dollar and the Deutsche mark), and a three-currency basket (the dollar, the Deutsche mark, and the Japanese yen). In each basket the currencies are equally weighted. The other assumptions are like in the previous exercise.

The results are portrayed in Appendix Figure 2. To avoid cluttering the pictures, only the medians of Test A and Test B (defined as before) are presented. They are plotted against alternative values of the standard deviation of the random disturbance assumed in the simulation.

As expected, increasing the number of currencies in the basket shifts the quantile lines upward, reflecting the fact that for any given value of the standard deviation more observations become necessary to reject the null hypotheses. As before, the increase in information requirements is sometimes substantial. For example, with a bandwidth of

10% (as observed in Chile in recent times), moving from a single to a 3-currency basket raises the 50% quantile of Test B by over 200 observations—implying that an extra year of data becomes necessary to reject the null hypothesis.

Role of Rate of Crawl

What about the rate of crawl? Intuitively, its value should have little consequence for Test B, which is concerned only with the basket weights. However, for Test A it can make a big difference—rates of crawl further away from zero must help reject the null hypothesis more quickly, since the latter involves a zero rate of crawl.

This is verified in Appendix Figure 3, which shows the effects of different rates of crawl on the verification time, as reflected by the 50% quantile of Test A and Test B. For a given value of σ, we generate different samples assuming increasing rates of crawl. As expected, the time to reject Test A (measured by the left scale) declines steadily as the rate of crawl rises away from zero, while the time to reject Test B (measured by the right scale) shows only modest variation.

Role of Period

The power of these tests depends on the precision of the parameter estimates, itself given by the noise-to-signal ratio—or the relative size of the variances of the dependent and independent variables. When the variance of the dependent variable is large relative to the variance of the independent variable, the estimates are imprecise and it is difficult to reject a given null hypothesis. Since these relative variances are not constant over time, the verifiability of a given model may depend on the specific time period over which it is observed.

This can be assessed using data from different time periods to carry out the Test A and B. Since our experiments use actual data on the hard currencies, any differences in time to reject Test A and B across replications, using hard-currency data from different time periods, should be attributed to changes over time in the variance-covariance matrix of the hard currencies.

The results of such an experiment are reported in Appendix Figure 4, which shows the median values of the time to reject Test A and B, obtained when the simulations use hard-currency data from different periods in 1986-96 and assuming a three-currency basket with equal weights.

To facilitate the interpretation of the results, we also show in the figure a measure of the variance of the hard currencies—specifically, the inverse of the average of their standard deviations. As the graph shows, variability of the hard-currency exchange rates was particularly high in the first and fourth periods considered. This results in a clear reduction in time to reject Test A and B in such periods, relative to the rest.

Appendix 2: Construction of Numeraire and Estimated Models

In this appendix, we describe how we constructed the weighted basket numeraire and the precise models we estimated in the case studies of Chile, Israel, and the floating regimes.

Construction of the Weighted Basket Numeraire

The numeraire was constructed using the bilateral exchange rates of seven strong currencies weighted by the GDP share in 1992. The specific units of each currency in the basket were chosen so that the basket is valued in 1 US dollar on January 2, 1990. The value, in US$, of the weighted basket (WB) at a given point in time is:

$$WB_t = a_1 + a_2\,DM_t + a_3\,BP_t + a_4\,FF_t + a_5\,JY_t + a_6\,CD_t + a_7\,IL_t \qquad (A1)$$

such that all the exchange rates are expressed in US$ over local currency. IL stands for the Italian lira and CD for the Canadian dollar.

Using 1991 GDP at market prices (constant 1995 US$) data from the World Development Indicators report, we defined the following weights:

Currency	US$	DM	BP	FF	JY	CD	IL
Weight	35.72%	13.01%	5.79%	8.34%	28.25%	2.97%	5.92%

These weights represent the share of the cost of each currency in the total value of the basket at the reference date (in this case 1/2/1990). Based on this definition, we can calculate the units of each currency (a_1 ... a_7):

$$w_1 = a_1 / WB_0 \rightarrow a_1 = w_1 * WB_0 \qquad (A2)$$

$$w_2 = a_2\,DM_0 / WB_0 \rightarrow a_2 = w_2 * WB_0 / DM_0$$

$w_3 = a_3\, BP_0 / WB_0 \rightarrow a_3 = w_3 * WB_0 / BP_0$

…

The resulting units are the following:

Currency	US$	DM	BP	FF	JY	CD	IL
Units (a_i)	0.3572	0.2192	0.03566	0.4803	40.707	0.0499	91.245

Using these units and equation (A1), we obtained the value of the weighted basket at any point in time. In order to obtain any exchange rate as a function of this numeraire, we just multiply the exchange rate of the local currency in terms of the US$ by WB_t.

Estimation of Basket Weights in Case Studies of Chile, Israel and free-floating regimes

In the two case studies undertaken in this paper, the baskets were, in fact, constructed in a similar way to our weighted basket numeraire. When the basket is defined for the first time, some strong currencies are selected. Initial weights are calculated according to trade weighs. The units of each currency that are used for the calculation of the basket from that moment on are defined according to the procedure described above. The units remain constant over time, but the actual weights of each currency depend on the bilateral exchange rates movements.

In order to complete the definition of the exchange rate regime, a path must be defined for the value of the basket (B_t). In some cases, this value is to remain constant, to increase at a constant rate or to vary with internal or external inflation rates. The local exchange rate, in the case of a basket peg, is determined by the equality of the predetermined path for the value of the basket (B_t) and the actual value of the basket, given the units chosen, the bilateral foreign exchange rates and the local rate:

$$B_t = b_1 S_t + b_2 S_t{*}DM_t + b_3 S_t{*}BP_t + b_4 S_t{*}FF_t + b_5 S_t{*}JY_t,$$

where S_t represents the local currency (in this example the Israeli Shekel vis-à-vis the US$). Using a formula analogous to (A2), we can express the previous equation in terms of the original weights:

$B_t/B_0= w_1 S_t/S_0+ w_2 (S_tDM_t)/(S_0DM_0)+ w_3 (S_t{*}BP_t)/(S_0BP_0)+ w_4 (S_t{*}FF_t)/(S_0FF_0)+ w_5 (S_t{*}JY_t)/(S_0JY_0)$.

Rewriting the previous expression with the local currency on the LHS, we have:

$S_0/S_t = w_1 B_0/B_t + w_2 B_0/B_t{*}DM_t/DM_0 + w_3 B_0/B_t{*}BP_t/BP_0 + w_4 B_0/B_t{*}FF_t/FF_0 + w_5 B_0/B_t{*}JY_t/JY_0$.

Finally, multiplying both sides of the previous equation by WB_0/WB_t we obtain an equation where all the exchange rates are expressed in terms of the numeraire. Redefining variables, we obtained the following equation:

$$Y_t = w_1 XUS_t + w_2 XDM_t + w_3 XBP_t + w_4 XFF_t + w_5 XJY_t. \qquad \text{(A3)}$$

In the case of basket bands, the actual value of the basket is allowed to fluctuate around the predetermined path, usually with a given percentage above or below (the band width). In those cases, we refer to the reference path as central parity. Equivalently, the band defined for the basket implies an analogous band for the local exchange rate vis-à-vis the numeraire.

In our analysis, we try to recover the original announced weights from the observed exchange rate, the bilateral exchange rates between the strong currencies and the predetermined path for the central parity. The movements of the observed exchange rate inside the band give rise to an error term. A stationarity assumption is certain to fail in a time series for the level of the exchange rate. A simple way to deal with this is to work with first differences. The basic equation we estimate in this paper, expressed in first differences, is the following:

$$\Delta Y_t = d_0 + w_1\, \Delta XUS_t + w_2\, \Delta XDM_t + w_3\, \Delta XBP_t + w_4\, \Delta XFF_t + w_5\, \Delta XJY_t + \varepsilon_t. \qquad \text{(A3)}$$

For the Chilean case, in the first three periods we included only the US$ and in the following four periods, the US$, the DM, and the JY.

As described in the next section, we finally used a restricted version of equation (A3) for the case studies of Chile and Israel but in the case of the free-floating countries, we estimated equation (A3) without worrying about the multicollinearity problem.

Dealing with Multicollinearity

As mentioned in the text, strong correlation between the included regressors (particularly between ΔXDM_t and ΔXFF_t and between ΔXUS_t and ΔXJY_t in some periods) gave rise to a significant multicollinearity problem. In order to deal with it, we combined pairs of regressors, using the ratio of announced weights:

$$\Delta Y_t = d_0 + w_1\, (\Delta XUS_t + w_{50} / w_{10}\, \Delta XJY_t) + w_2\, (\Delta XDM_t + w_{40} / w_{20}\, \Delta XFF_t) +$$

$$w_3\, \Delta XBP_t + \varepsilon_{t,} \qquad \text{(A5)}$$

where w_{10}, w_{20}, w_{40} and w_{50} represent the announced weights (which are known constants). With this specification, we were able to identify only the following parameters: d_0, w_1, w_2 and w_3.

Figure 1: Chilean Exchange Rate and Exchange Rate Band -- 1986-1999
Chilean Peso Relative to Weighted Basket

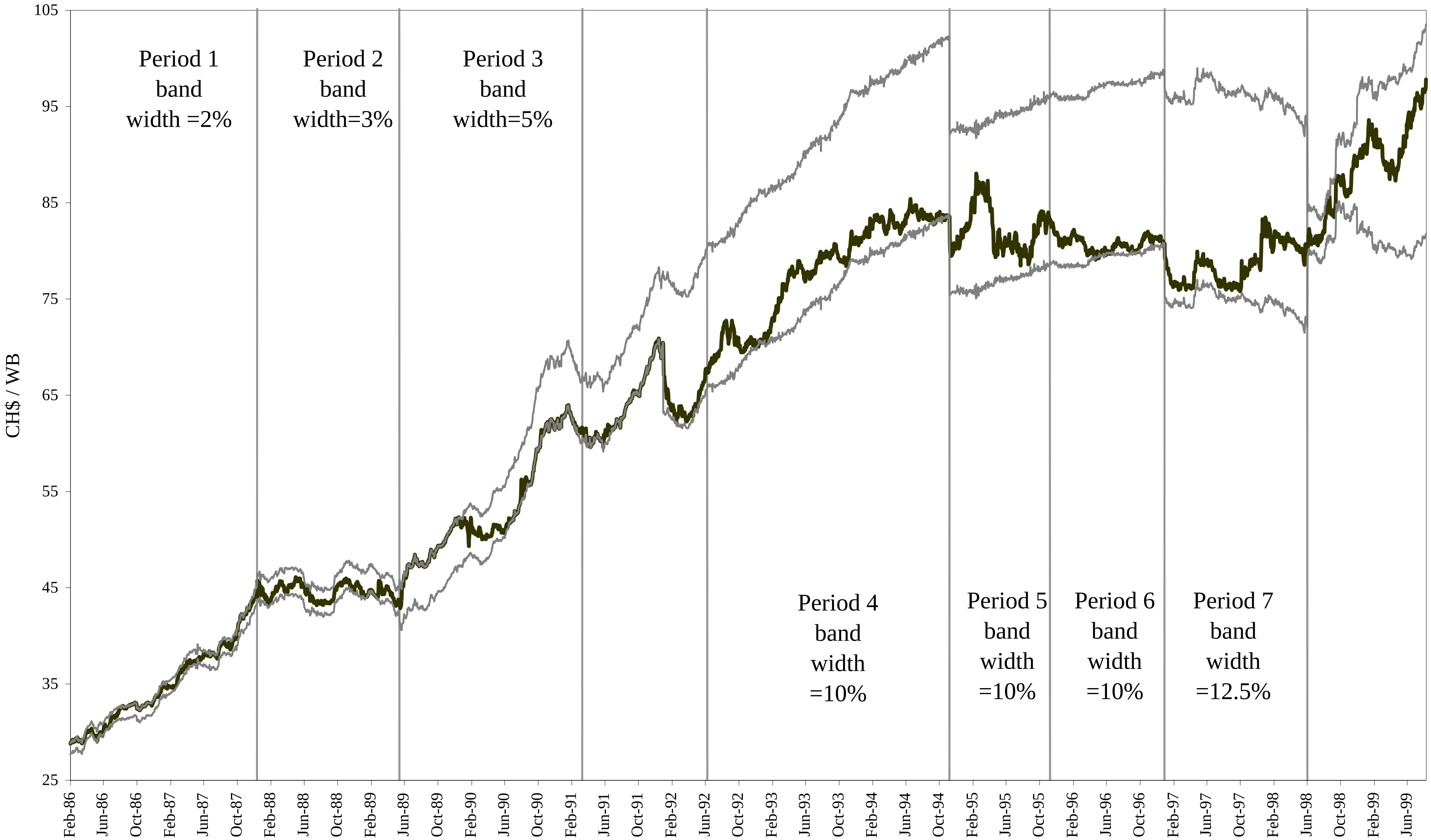


Figure 2: Israeli Exchange Rate and Exchange Rate Band -- 1989 - 1999
New Israeli Shequel Relative to Currency Basket

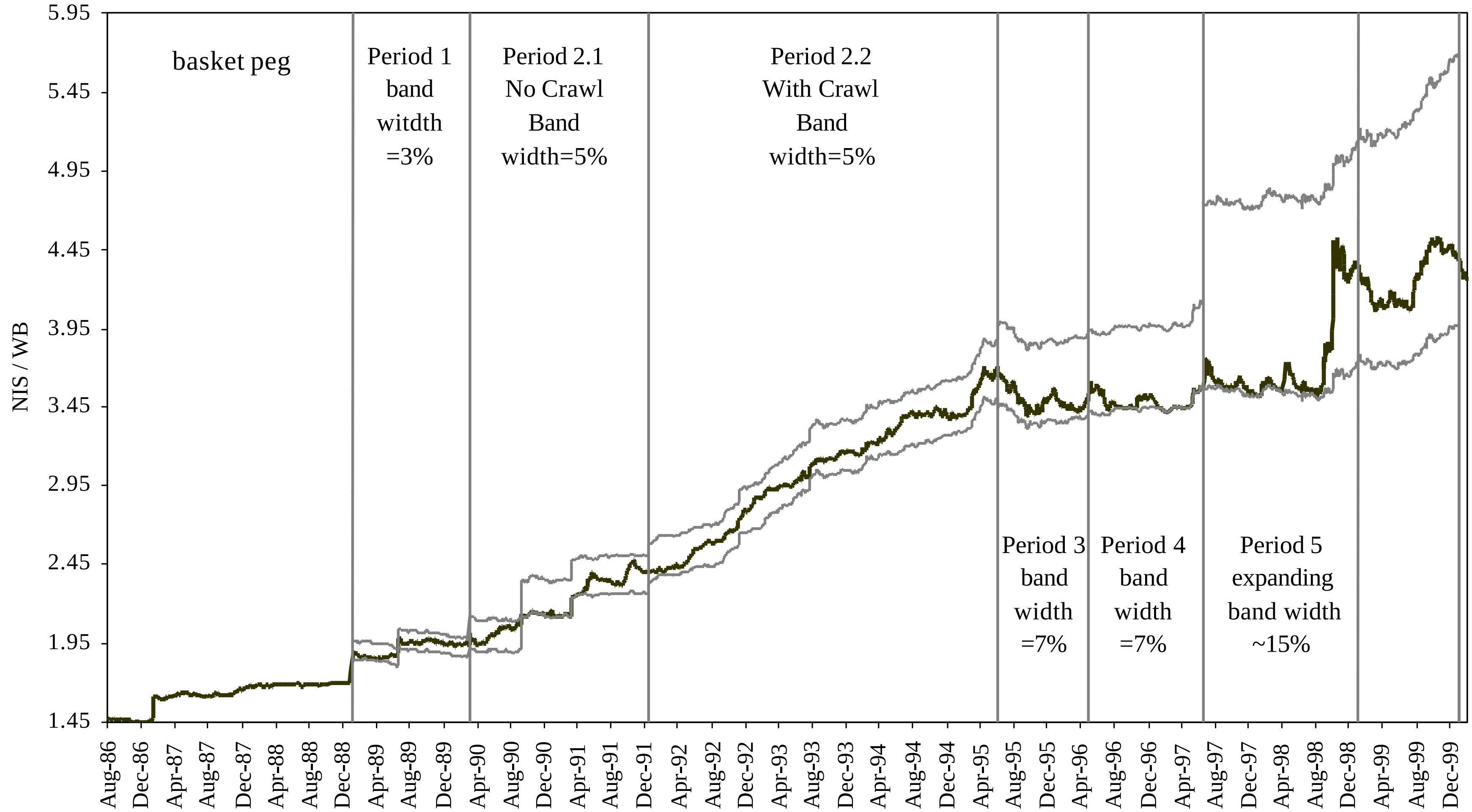


Figure 3: Chilean Peso and Central Parity - Scatter Plots
(Chilean Peso / Weighted Basket)

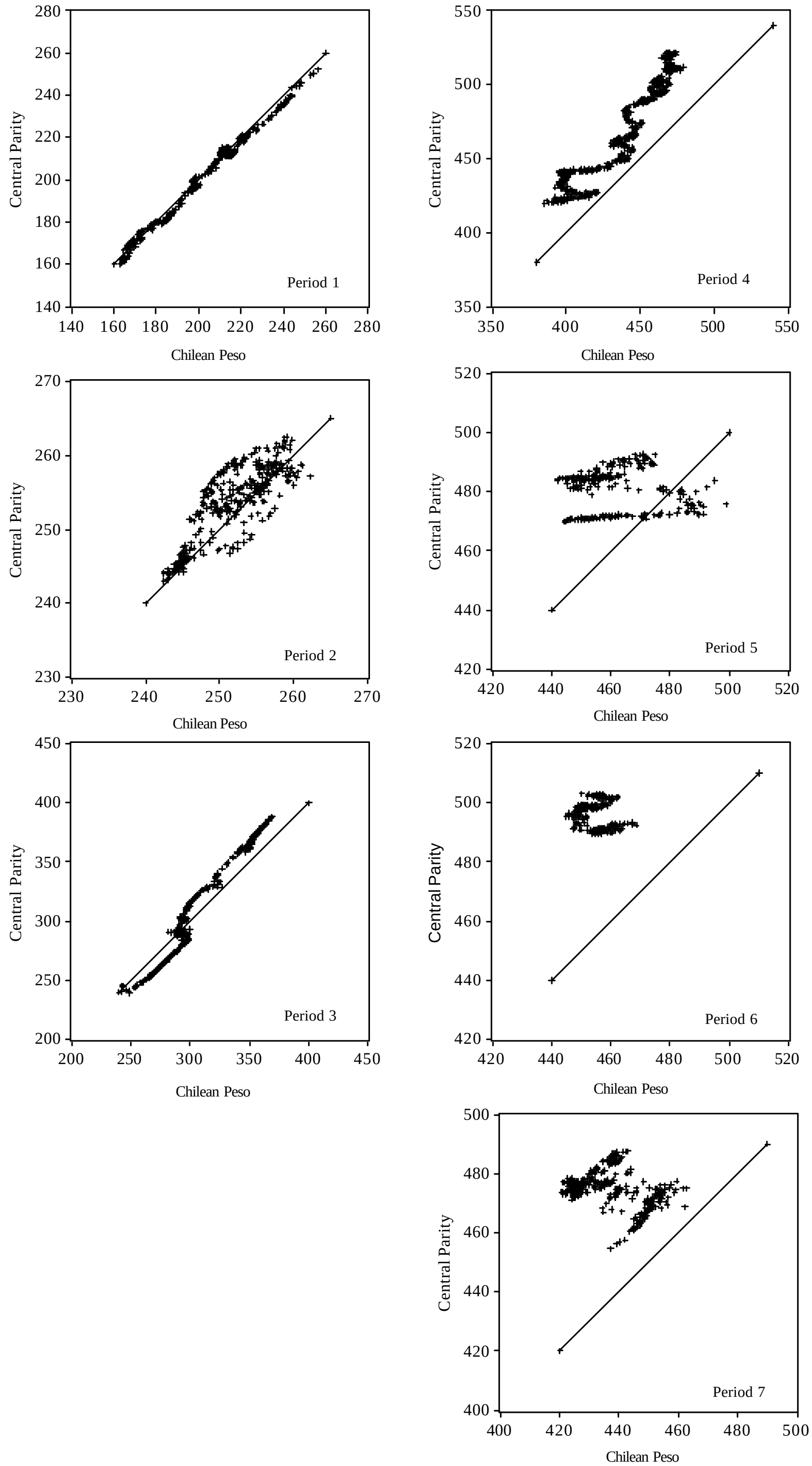


Figure 4: Israeli Shekel and Central Parity - Scatter Plots
(Israeli Shekel / Weighted Basket)

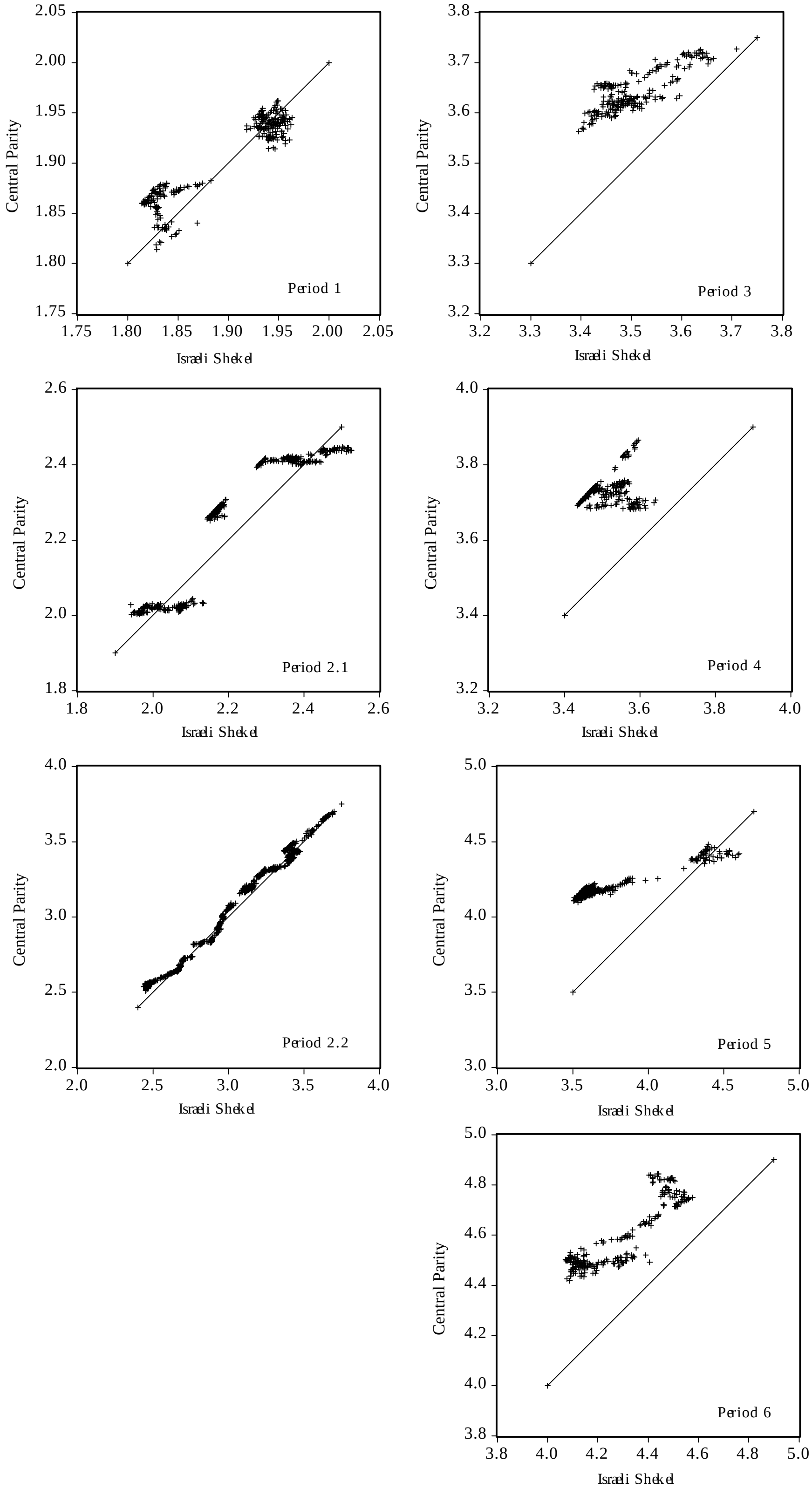
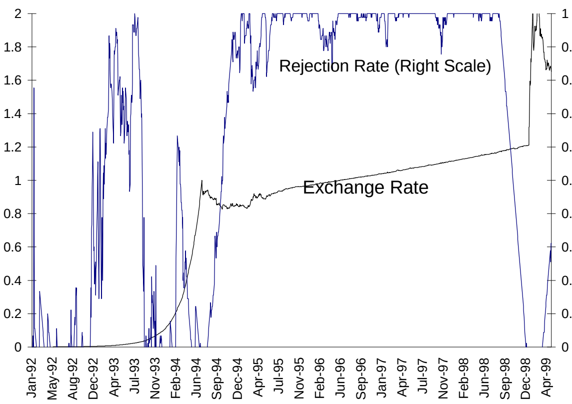
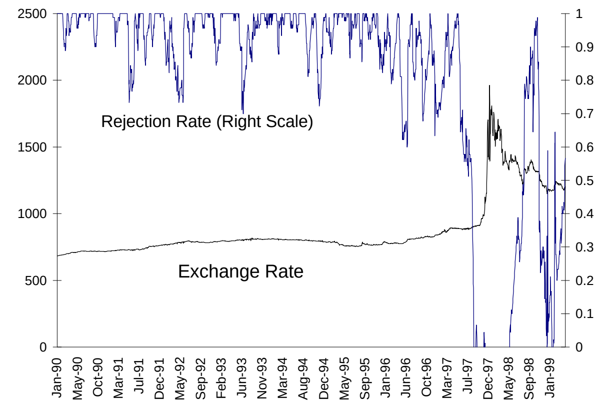


Figure 5: Exchange Rates Against US Dollar and Percentages of Rejections
Rejection Rate Corresponds to Testing H0: Weights=0
Model Tested Is First Differences of Domestic Currency on Major World Currencies
(Exchange Rates expressed as Domestic Currency / US$)

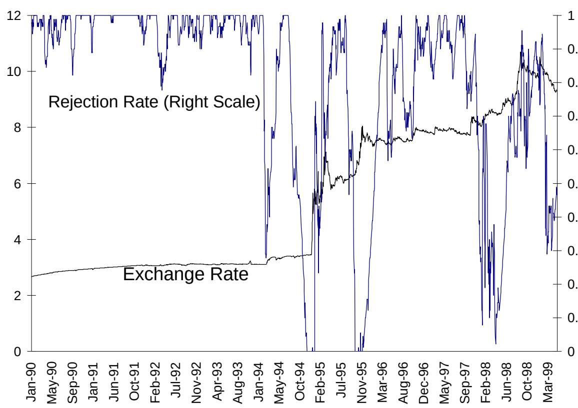
Brazilian Real



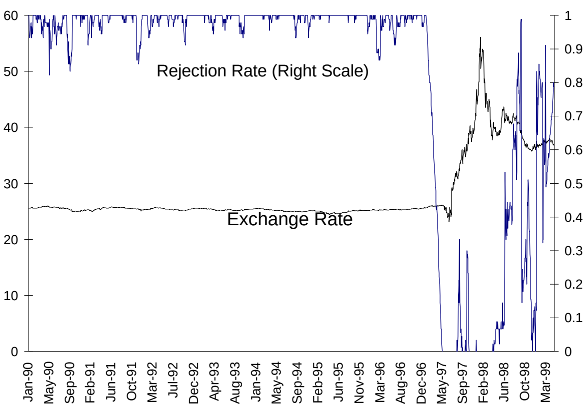
S. Korean Won



Mexican Peso



Thai Baht



Peruvian Sol

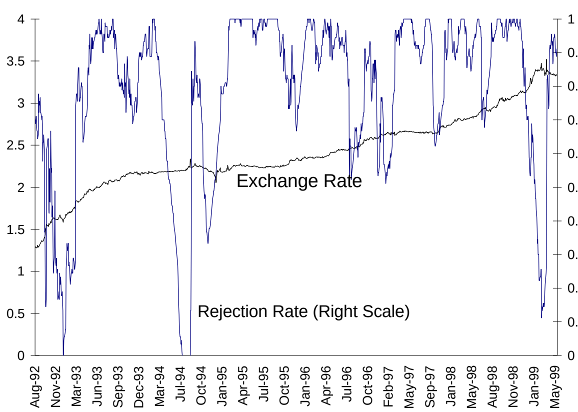


Table 1.a: Chilean Exchange Rate
Description of Exchange Rate Regimes

	Period				Weights of Central Parity		
	Begin	End	Number of Observations	Band Width (+/-)	U.S. Dollar	Deutsche Mark	Japanese Yen
1	February 24, 1986	January 4, 1988	434	2%	100%	0%	0%
2	January 5, 1988	June 5, 1989	340	3%	100%	0%	0%
3	June 6, 1989	April 2, 1991	449	5%	100%	0%	0%
4	July 1, 1992	October 31,1994	580	10%	50%	30%	20%
5	November 30, 1994	November 30, 1995	236	10%	45%	30%	25%
6	December 1, 1995	January 20, 1997	264	10%	45%	30%	25%
7	January 21, 1997	June 24, 1998	326	12.5%	80%	15%	5%

Only periods with at least 250 observations are listed. During these periods there were no changes in the exchange rate regime. The bands' width, the weights of the central parity, and the level of the central parity were held constant. The periods excluded include discrete devaluations / revaluations of the central parity. For more details about the exchange rate regimes in Chile, see Appendix table. The announced weights correspond to the relative importance of the respective currency in the first day when any new weight is defined. With relative movements between the foreign currencies, those weights vary with time. The estimation procedure, however, is designed to estimate the initial weight.

Table 1.b: Chilean Exchange Rate
Percentage of Observations for Which Null Hypothesis Is Rejected (1%)

Period	Obs	OLS First Differences Unrestricted Model			OLS First Differences Restricted Model			Precision $\sum\|w-w_0\|$	
		Test 1 H_0: Weights = announc	Test 2 H_0: Weights = 0	Point Estimate $W_{US\$}$ (s.e.)	Test 1 H_0: Weights = announc	Test 2 H_0: weights = 0	Point Estimate $W_{US\$}$ (s.e.)	OLS First Differences Unrestricted Model	OLS First Differences Restricted Model
1	50	0	100	0.94 (0.06)				0.06	
US$ band	100	0	100	0.90 (0.07)				0.10	
width=2%	200	0	100	0.92 (0.05)				0.08	
$W_{US\$}$=1									
2	50	0	100	1.27 (0.19)				0.27	
US$ band	100	0	100	1.09 (0.13)				0.09	
width=3%	200	0	100	1.00 (0.07)				0.00	
$W_{US\$}$=1									
3	50	21	92	0.80 (0.09)				0.20	
US$ band	100	22	97	0.85 (0.05)				0.15	
width=5%	200	34	98	0.90 (0.07)				0.10	
$W_{US\$}$=1									
4	50	100	100	1.10 (0.15)	100	100	1.08 (0.15)	0.90	0.87
basket	100	100	100	1.10 (0.09)	100	100	1.09 (0.08)	1.08	0.90
width=10%	200	100	100	1.04 (0.06)	100	100	1.04 (0.06)	1.06	0.87
$W_{US\$}$=0.5									
5	50	68	68	1.01 (0.55)	68	79	1.38 (0.26)	1.37	1.07
basket	100	86	86	1.67 (0.28)	86	91	1.02 (0.09)	1.69	0.95
width=10%	200	94	94	1.19 (0.22)	94	96	1.07 (0.08)	1.19	0.97
$W_{US\$}$=0.45									
6	50	58	50	0.37 (0.44)	45	45	0.95 (0.21)	1.44	0.86
basket	100	82	78	0.81 (0.25)	76	76	1.10 (0.12)	1.31	0.98
width=10%	200	91	90	1.02 (0.17)	89	89	1.12 (0.08)	1.27	1.00
$W_{US\$}$=0.45									
7	50	13	100	1.08 (0.14)	26	100	0.97 (0.07)	0.38	0.25
basket	100	63	100	1.14 (0.11)	68	100	1.03 (0.04)	0.44	0.33
width=12.5%	200	82	100	0.83 (0.16)	85	100	0.95 (0.07)	0.38	0.31
$W_{US\$}$=0.8									

In periods 1-3, only the US$ was considered in the estimation. Precision is calculated as the sum of absolutes deviations of the estimated weights at 50, 100 and 200, with respect to the announced weights. In the restricted model, for periods 4 to 7, the JY and the US$ were combined in one variable, using the relative announced weights. See Appendix 2 for details.

Table 2.a: Israeli Exchange Rate
Description of Exchange Rate Regimes

	Period				Weights of Central Parity				
	Begin	End	Number of Observations	Band Width (+/-)	U.S. Dollar	Deutsche Mark	Japanese Yen	French Franc	British Pound
1	January 3, 1989*	February 28, 1990	291	3%	60%	20%	5%	5%	10%
2.1	March 1, 1990	December 16, 1991	443	5% no crawl	60%	20%	5%	5%	10%
2.2	December 17, 1991	May 30, 1995	851	5% crawling	60%	20%	5%	5%	10%
3	May 31, 1995	April 29, 1996	222	7%	54.8%	24.2%	1%	5.6%	8.3%
4	April 30, 1996	June 17, 1997	273	7%	60.3%	21%	5.6%	5.1%	8%
5	June 18, 1997	December 31, 1998	374	15%**	60.3%	21%	5.6%	5.1%	8%

* The basket was introduced with the presented weights in August, 1986, but the exchange rate was allowed to vary around a 3% band in January 1989.

** Widening band designed to reach 15% by end of 1997.

Table 2.b: Israeli Exchange Rate
Percentage of Observations for Which Null Hypothesis Is Rejected (1%)

Period	Obs	OLS First Differences Unrestricted Model			OLS First Differences Restricted Model			Precision $\sum\lvert w-w_0\rvert$	
		Test 1 H_0: Weights = announc	Test 2 H_0: Weights = 0	Point Estimate $W_{US\$}$ (s.e.)	Test 1 H_0: Weights = announc	Test 2 H_0: weights = 0	Point Estimate $W_{US\$}$ (s.e.)	OLS First Differences Unrestricted Model	OLS First Differences Restricted Model
1	50	29	92	-1.37 (0.40)	0	89	0.64 (0.10)	3.94	0.06
basket	100	69	97	-2.52 (0.25)	0	95	0.45 (0.10)	6.19	0.29
width=3%	200	86	98	0.43 (0.02)	44	98	0.43 (0.02)	0.48	0.27
$W_{US\$}$=0.6									
2.1	50	100	100	-5.10 (0.24)	0	0	0.31 (0.34)	11.49	0.79
basket	100	100	100	-5.30 (0.15)	0	7	-0.12 (0.25)	11.98	0.91
width=5%	200	100	100	0.15 (0.07)	46	40	0.09 (0.06)	1.33	0.68
(w/o crawl)									
$W_{US\$}$=0.6									
2.2	50	0	89	-0.54 (0.36)	0	97	0.53 (0.07)	2.42	0.14
basket	100	0	95	0.01 (0.31)	0	99	0.57 (0.08)	1.55	0.11
width=5%	200	2	98	0.22 (0.13)	0	99	0.55 (0.05)	0.80	0.07
(with crawl)									
$W_{US\$}$=0.6									
3	50	63	100	-1.28 (0.44)	39	100	0.60 (0.15)	5.23	0.29
basket	100	84	100	-0.62 (0.30)	67	100	0.84 (0.08)	3.89	0.47
width=7%	200	93	100	-1.52 (0.21)	85	100	0.73 (0.08)	6.37	0.36
$W_{US\$}$=0.548									
4	50	100	100	-3.59 (0.45)	0	0	0.53 (0.25)	11.35	0.82
basket	100	100	100	-2.89 (0.32)	55	55	0.55 (0.17)	9.54	0.55
width=7%	200	100	100	-2.97 (0.25)	76	79	0.47 (0.12)	9.30	0.40
$W_{US\$}$=0.603									
5	50	100	100	-5.54 (0.46)	0	0	0.91 (0.30)	15.39	0.67
basket	100	100	100	-4.52 (0.36)	15	51	0.97 (0.17)	13.18	0.62
width~15%	200	100	100	-4.36 (0.28)	60	77	0.96 (0.10)	12.62	0.50
expanding									
band									
$W_{US\$}$=0.603									

Precision is calculated as the sum of absolutes deviations of the estimated weights at 50, 100 and 200, with respect to the announced weights. In the restricted model, the DM and the FF on the one hand, and the US$ and the JY on the other, were combined using the relative announced weights, to form new variables. See Appendix 2 for details.

Table 3: Swiss Franc and Randomly Generated Variable as Dependent Variable
Percentage of Observations for Which Null Hypothesis Is Rejected (1%)

		Swiss Franc		Random	
Period	Obs	Test 1 H_0: Weights = announced	Test 2 H_0: Weights = 0	Test 1 H_0: Weights = announced	Test 2 H_0: Weights = 0
Announcement: Chile-Period 1	50	100	100	97	0
	100	100	100	99	0
	200	100	100	99	0
Announcement: Chile-Period 2	50	100	100	89	0
	100	100	100	95	0
	200	100	100	98	0
Announcement: Israel-Period 2.2	50	100	100	76	0
	100	100	100	90	0
	200	100	100	95	0

The rejection percentages were recalculated for the referred country periods, replacing in each case the local currency with the Swiss franc and a randomly generated data. Fictitious data were generated following an AR(1) process with parameters obtained by fitting an AR(1) model to the original dependent variable.

Table 4: Floating Exchange Rate Regimes - First Differences Linear Model
Percentage of Observations for Which H0: Weights=0 Is Rejected (1%)

Brazil

obs.	1992	1993	1994	1995	1996	1997	1998	1999
	Managed Floating			Band				Free Floating
20	0	0	0	0	90.9	100	90.9	0
50	0	0	0	73.2	97.6	100	97.6	0
100	0	41.8	2.2	87.9	98.9	100	98.9	0
150	0	62.4	1.4	92.2	99.3	100	99.3	0
200	0	72.3	1	94.2	99.5	100	99.5	.

Mexico

obs.	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999
	Crawling Peg					Free Floating				
20	100	72.7	100	100	100	0	0	18.2	0	36.4
50	100	92.7	100	100	95.1	29.3	2.4	29.3	58.5	78
100	100	96.7	100	100	97.8	60.4	56	68.1	48.4	90.1
150	100	97.9	100	100	98.6	74.5	71.6	79.4	47.5	93.6
200	100	98.4	100	100	99	81.2	79.1	84.8	61.3	-

Peru

obs.	1993	1994	1995	1996	1997	1998	1999
	Free Floating						
20	0	9.1	0	0	9.1	90.9	0
50	51.2	70.7	14.6	63.4	56.1	97.6	70.7
100	78	86.8	61.5	83.5	79.1	98.9	68.1
150	85.8	91.5	75.2	89.4	86.5	99.3	79.4
200	89.5	93.7	81.7	92.1	.	99.5	.

S. Korea

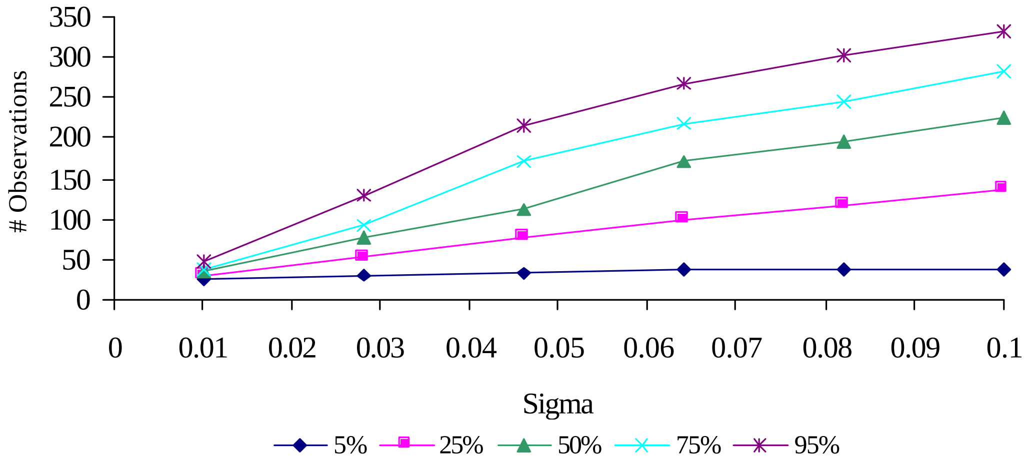
obs.	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999
	Managed Floating								Free floating	
20	100	90.9	100	63.6	45.5	54.5	18.2	18.2	0	0
50	100	97.6	100	90.2	85.4	87.8	61	26.8	0	0
100	100	98.9	100	95.6	93.4	94.5	82.4	67	0	0
150	100	99.3	100	97.2	95.7	96.5	88.7	78.7	14.2	32.6
200	100	99.5	100	97.9	96.9	97.4	91.6	84.3	.	.

Thailand

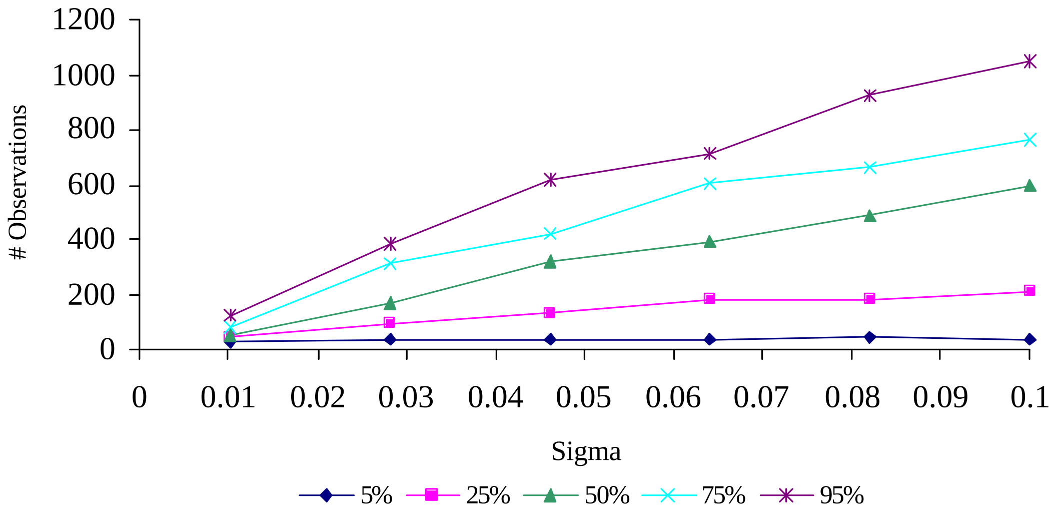
obs.	1990	1991	1992	1993	1994	1995	1996	1997 (1st half)	1997 (2nd half)	1998	1999
	Basket Peg								Free Floating		
20	100	90.9	81.8	100	100	100	63.6	100	0	0	0
50	100	97.6	95.1	100	100	100	90.2	100	0	0	0
100	100	98.9	97.8	100	100	100	95.6	100	0	0	9.9
150	100	99.3	98.6	100	100	100	97.2	.	.	0	41.8
200	100	99.5	99	100	100	100	97.9	.	.	0	.

Appendix Figure 1: Monte Carlo Simulations—Role of Band Size

Quantiles of Test A (Weights=Rate of Crawl=0)

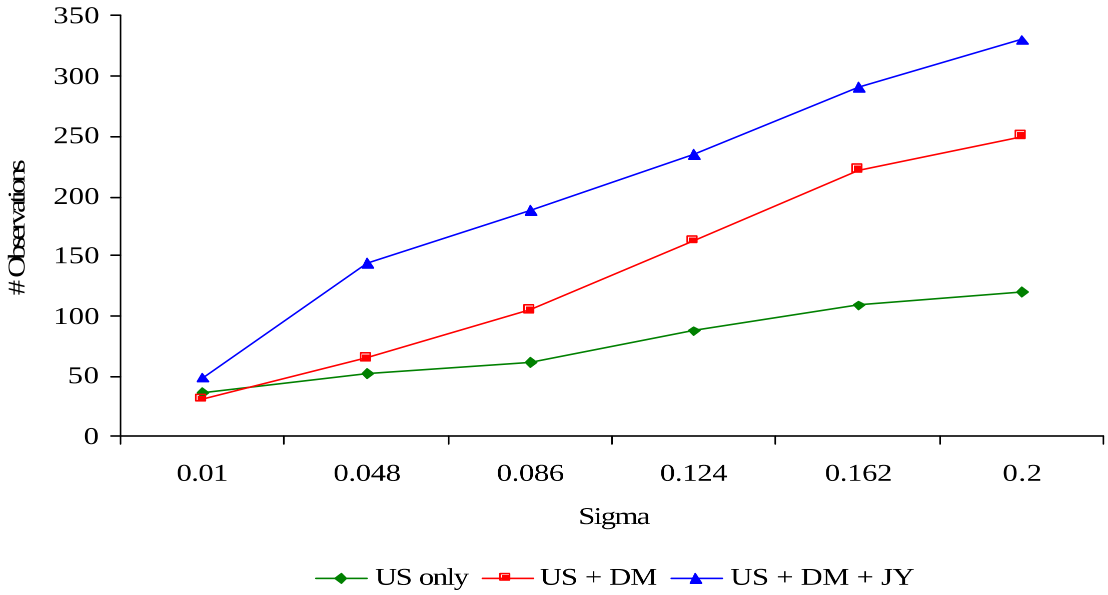


Quantiles of Test B (Weights=0)

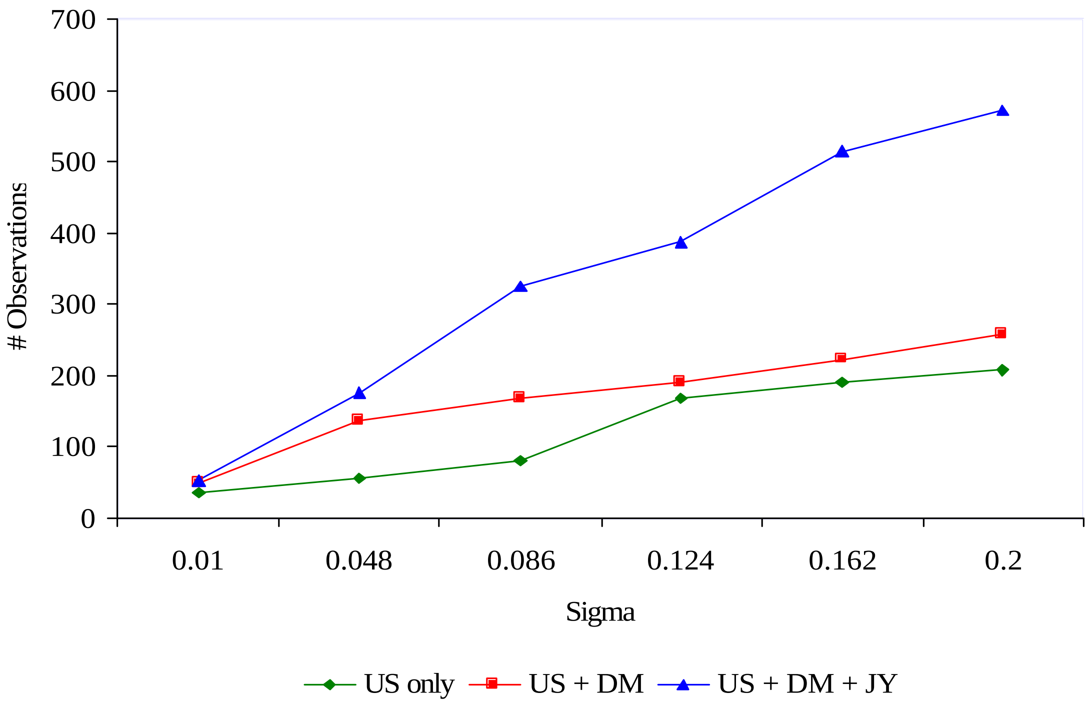


Parameters of estimations: 500 samples; weights on dependent variables 1/3 for US$, DM, and JY; initial observation February 24, 1986; constant=1; rate of crawl=0.10; sigma={0.01; 0.028; 0.046; 0.064; 0.082; 0.1}. Quantile values are calculated for the first 10 rejections.

Appendix Figure 2:
Monte Carlo Simulations—Role of Number of Currencies in Basket
Quantiles of Test A (Weights=Rate of Crawl=0)



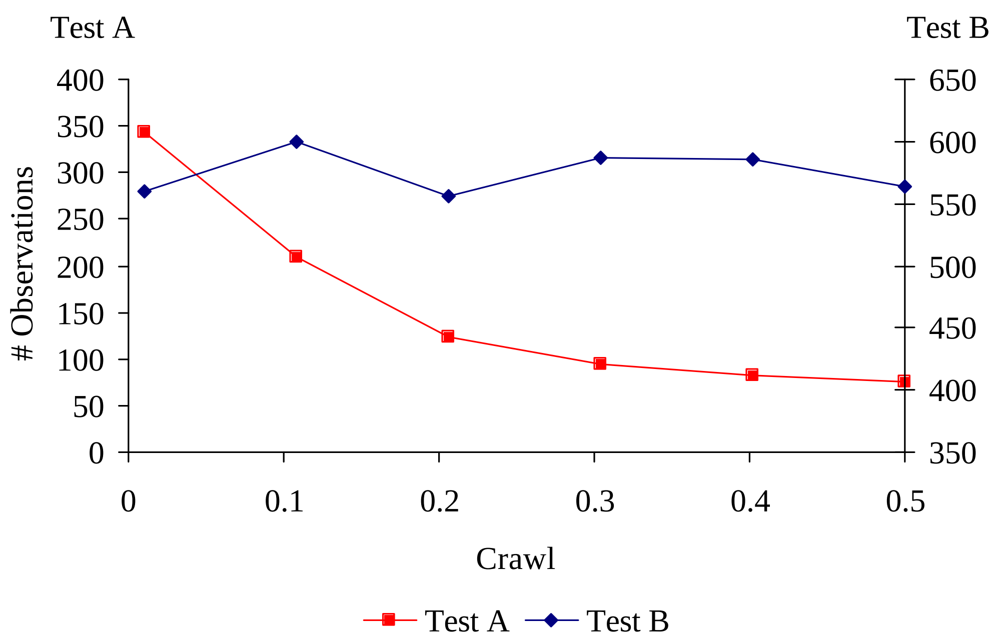
Quantiles of Test B (Weights=0)



Parameters of estimations: 500 samples; initial observation February 24, 1986; constant=1; rate of crawl=0.10; sigma={0.01; 0.048; 0.086; 0.124; 0.162; 0.2}; weights on dependent variables are 1, 1/2, and 1/3, for 1, 2, and 3 currencies in the basket respectively. Quantile values are calculated for the first 10 rejections.

Appendix Figure 3: Monte Carlo Simulations—Role of Rate of Crawl

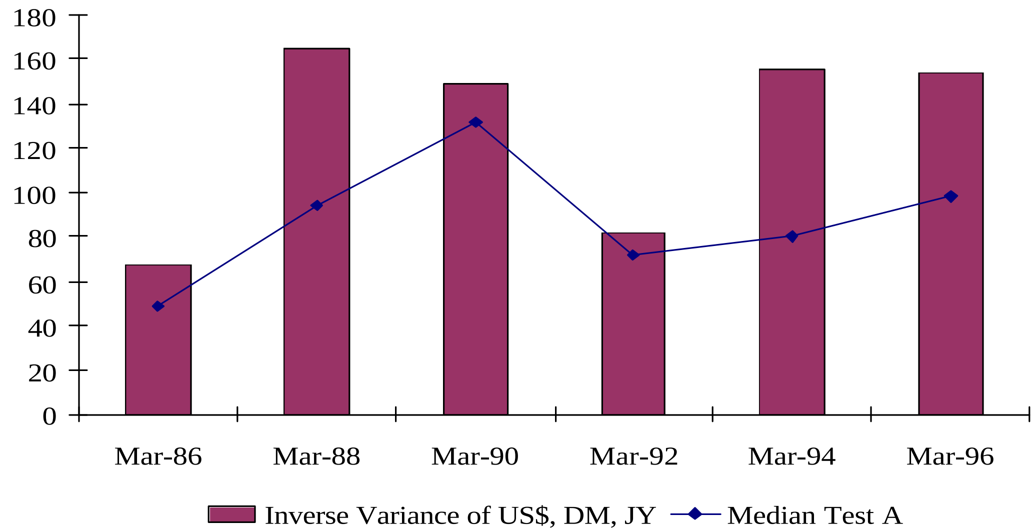
Median Valules for Tests A & B



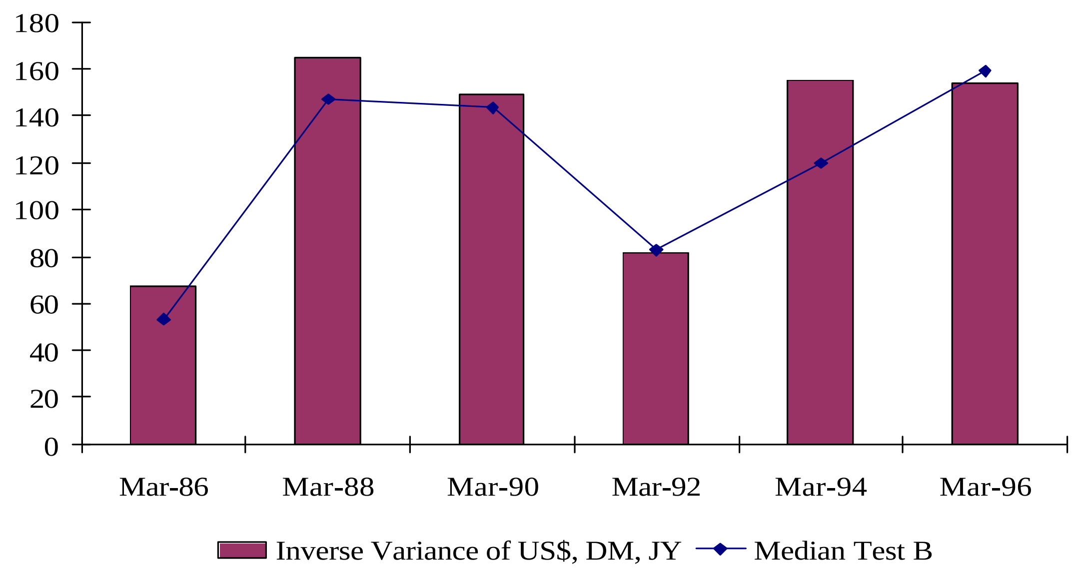
Test A: Weights=Rate of Crawl=0 Test B: Weights=0
Parameters of estimations: 500 samples; initial observation February 24, 1986; constant=1; rate of crawl= {0.01; 0.108; 0.206; 0.304; 0.402; 0.5}; sigma=0.1; weights on dependent variables are equal to 1/3 for each currency in the basket. Median values are calculated for the first 10 rejections.

Appendix Figure 4:
Monte Carlo Simulations—Role of Period and Variability of Regressors

Median Values for Test A



Median Values for Test B



Parameters of estimations: 500 samples; weights on dependent variables 1/3 for US$, DM, and JY; constant=1; rate of crawl=0.10; sigma=0.005. Median values are calculated for the first 10 rejections.
"Inverse Variance" is the inverse of the average standard error of the three currencies, for the first 50 observations of each respective period.

Appendix Table A.1: Exchange Rate Policy in Chile 19982-1999

Date	Policy
September, 1982	• Daily devaluations in line with domestic inflation in the preceding month minus an estimate of external inflation
August 1, 1984	• Band of +/- 0.5%
June, 1985	• Widening to 2%
January 5, 1988	• Widening to 3 %
June 6, 1989	• Widening to 5% • Accelerate the rate of real depreciation, which was achieved by reducing the estimate of international inflation • Adjustment of central parity: previous month inflation minus estimated international inflation
April 3, 1991	• 2% revaluation of central parity
January 23, 1992	• Band widened to 10% (from +/-5%) • Discrete 5% revaluation of central parity
March, 1992	• Managed floating is authorized
July, 1992	• Central parity: 50% U.S. dollar, 30% Deutsche mark, 20% Japanese yen
November, 1994	• Central parity: 45% U.S. dollar, 30% Deutsche mark, 25% Japanese yen
November 30, 1994	• 9.66% revaluation of central parity
December, 1995	• 2% revaluation; 2% annual revaluation
January 21, 1997	• 4% revaluation of central parity • New band: +/- 12.5% • New weight: 80% U.S. dollar, 15% Deutsche mark, 5% Japanese yen
June 25, 1998	• 2% annual revaluation • New asymmetric band: +2%, -3,5%
September 16, 1998	• New band: +/- 3.5% • The band is widened progressively until it accumulates and additional 1.5% in each extreme , such that by the end of the year the band would be +/- 5% • New estimates of annual international inflation from 2.4% to 0% for the rest of the year • The relevant internal inflation rate is the inflation target and not past inflation
December 23, 1998	• New band: +/-8% • No change in other parameters (central parity adjusts only with internal inflation and the band continue widening daily by 0,013575%)
January 1, 1999	• Deutsche mark is replaced by the euro, with the same weight
September 2, 1999	• Free floating with managed intervention only in exceptional cases • Release of new information regarding interventions in the foreign exchange markets

Source: Central Bank of Chile, Hussey and Morandé (1996), and Vergara (1994)

Appendix Table A.2: Exchange Rate Policy in Israel 1986-1999

Date	Policy
August 1, 1986	• Beginning of basket peg without crawl • Initial weights: 60% US$, 20% DM, 10% BP, 5% FF, 5% JY
January 3, 1989	• Central parity is devaluated 13% in a week • A ±3% band is introduced
June 23, 1989	• Midpoint raised by 6%
March 1, 1990	• Midpoint raised by 6% • Band widened to ±5%
September 10, 1990	• Midpoint raised by 10%
March 11, 1991	• Midpoint raised by 6%
December 17, 1991	• Introduction of crawling band • Midpoint raised by 3% • Slope of band 9%
November 9, 1992	• Midpoint raised by 3% • Slope reduced to 8%
July 26, 1993	• Midpoint raised by 2% • Slope reduced to 6%
May 31, 1995	• Midpoint raised by 0.8% • Band widened to ±7% • No change to slope • Weights: 54.8% US$, 24.2% DM, 8.3% BP, 5.6% FF, 7.1% JY
April 30, 1996	• Weights: 60.3% US$, 21% DM, 8% BP, 5.1% FF, 5.6% JY
June 18, 1997	• Band widened to reach ±15% by end of year • Slope of lower limit 4% • Slope of upper limit 6%
August 17, 1998	• Slope of lower limit 2% • Slope of upper limit 6%
January 4, 1999	• DM and FF are replaced by Euro • Weights: 61.4% US$, 8.9% BP, 5.2% JY, 24.5% Euro

Source: Bank of Israel, “Foreign Currency Exchange Rates In Israel 1999,” January 2000.