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De Facto Exchange Rate Regime Classifications Are Better Than You Think

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Abstract

Several *de facto* exchange rate regime classifications have been widely used in empirical research, but they are known to disagree with one another to a disturbing extent. We dissect the algorithms employed and argue that they can be significantly improved. We implement the improvements, and show that there is a far higher agreement rate between the modified classifications. We conclude that the current pessimism about *de facto* exchange rate regime classification schemes is unwarranted.

Keywords: exchange rate regimes, trade, volatility

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1. INTRODUCTION

Accurate identification of the exchange rate regime is important for many reasons. It is required to implement proper empirical tests of theoretical hypotheses, such as the effect of the exchange rate regime on macroeconomic and trade outcomes, or whether the "impossible trinity" holds. It is therefore unsatisfactory that the economics profession is not even getting close to an agreed classification system.

Dissatisfaction with the truth of countries' declared exchange rate regimes has led to the development of a variety of alternative classification systems, termed *de facto* because they rely to a greater or lesser degree on statistical data (see Tavlas *et al*., 2008, for a comprehensive survey). Since 1999 even the IMF has used its own judgement in classifying countries' regimes rather than merely recording what countries claim it to be.

Comparison of the outcomes of these alternative classification systems has been a sobering exercise, because they fail to agree with each other to a disturbing extent (Bleaney and Francisco, 2007; Eichengreen and Razo-Garcia, 2013; Frankel and Wei, 2008; Tavlas *et al*., 2008). For example Eichengreen and Razo-Garcia (2013), using three regime categories (hard pegs, soft pegs and floats), find pairwise agreement rates between three schemes over the period 1980-2004 of 60%, 69% and 75%. When one considers the number of cases which are uncontroversial, these statistics indicate an alarming rate of disagreement in potentially difficult cases.

By themselves such statistical analyses offer little insight into the source of these disagreements, and therefore they also fail to offer a path towards their resolution. In this paper we dissect three schemes in order to analyse the cause of the disagreements. The paper most closely related to ours is that of Eichengreen and Razo-Garcia (2013), who find that disagreements are most common for emerging markets, and who investigate what variables are associated with disagreements, but they do not evaluate the algorithms used by the different classification schemes as we do here.

To keep things simple, we confine the study to three schemes for which annual classifications are available from 1971 to 2011, and that are based exclusively on exchange rate behaviour. These three schemes differ quite dramatically in the proportion of pegs identified, and they also show different trends over time. The disagreements have broadly three components: (1) the data series (the official exchange rate against a single reference currency, the parallel-market rate or the residual from a regression against several currencies); (2) the key statistic derived from these data; and (3) the threshold level of this statistic that defines the boundary between a float and a peg. The three schemes differ from each other in all three aspects. We show that, when an identical data series is used, the differences in the key statistic are of relatively minor importance. With consistent thresholds the extent of agreement between alternative schemes increases dramatically. We conclude that the current pessimism about *de facto* exchange rate classifications is unwarranted.

2. EXCHANGE RATE CLASSIFICATIONS

Tavlas *et al*. (2008) refer to more than ten *de facto* classification schemes that have been suggested, but quite a few of them have rather limited time or country coverage. We focus on three schemes that are based exclusively on exchange rate behaviour, and do not take into account other forms of intervention such as movements in foreign exchange reserves (as do Levy-Yeyati and Sturzenegger, 2005). Because of this one would hope that the schemes might agree quite closely with one another, even though they use different statistics. Using 40 years of annual classifications generated by these schemes, we find considerable disagreement in the proportion of floats identified and even in the trend in that proportion over recent years.

The three schemes are:

Shambaugh (2004) [hereafter termed JS]. If the maximum and minimum of the log of the exchange rate against the identified reference currency (the US dollar being the default) do not differ by more than 0.04 over the calendar year, that observation is a peg. Alternatively, if there is a realignment so that the 0.04 threshold is exceeded, the observation is still a peg if the log of the exchange rate is unchanged in eleven months out of twelve. Thus effectively the level of the exchange rate is allowed to vary by ±2%, or by a realignment in one month and 0% in the remaining eleven months, for a peg to be coded. Note that basket pegs and crawling pegs may well not meet these criteria.

Reinhart and Rogoff (2004) [hereafter termed RR]. Movements of the log of the exchange rate against various reference currencies are analysed. Where available, the exchange rate in the parallel market rather than the official rate is used. If, over a five-year period from years *T–4* to *T*, more than 80% of monthly changes in the log of the exchange rate against any of the reference currencies fall within the range ±0.02, the exchange rate regime in all of the years *T–4* to *T* is classified as some form of peg or band. Alternatively, even if this criterion is not met, if the change in the exchange rate is zero for four months or more, it is classified as a peg for those months. Otherwise it is a float. If the exchange rate moves by more than 40% in a year, that observation is placed in a separate "freely falling" category (these observations are omitted from the comparison with other schemes). Thus the scheme focuses on the upper and lower tails of the distribution of monthly exchange rate movements, and specifically the proportion that exceed 2% in absolute value. Note the use of the parallel exchange rate; crawling pegs should meet the criteria for a peg if the crawl is slow enough, but basket pegs may well not do so.

Bleaney and Tian (2014) [hereafter termed BT]. The scheme is based on the root mean square residual (RMSE) from a regression similar to that of Frankel and Wei (1995) for identifying basket pegs. For each calendar year, the change in the log of the official exchange rate against the Swiss franc (the chosen *numéraire* currency) is regressed on the change in the log of the US dollar and of the euro against the Swiss franc. Occasionally, other reference currencies are added.[1] If the RMSE from this regression is less than 0.01, that country-year observation is coded a peg. If the RMSE is greater than 0.01, twelve new regressions are estimated each including a dummy variable for a particular month as a test for a realignment. If the F-statistic for the most significant of these dummy variables (April, say) is less than 30, the regime is coded a float. If the F-statistic for April is greater than 30, and the RMSE is less than 0.01, the observation is coded a peg with a realignment; otherwise it is a float. The regression approach should cater for basket pegs (through the regression coefficients) or crawls (through the intercept), but errors may arise from the small number of degrees of freedom in each regression.

Realignments of pegs are always a tricky issue for *de facto* classifications. In the JS system, any shift of the exchange rate of greater than 4% is a potential realignment, and a peg is then coded only if all the other monthly movements are zero. In the BT system, one realignment is explicitly catered for by monthly dummies, but the coding boundary of RMSE=0.01 is identical with or without a realignment. The RR system relies on the proportion of large monthly changes, of which a realignment is just one example, so that the algorithm does not have to cater explicitly for realignments.

[1] See Bleaney and Tian (2014) for details. A similar regression approach to regime classification has been suggested by Benassy-Quéré *et al*. (2006) and Frankel and Wei (2008), but they focus on the estimated coefficients rather than the goodness of fit.

3. COMPARISON OF CLASSIFICATIONS

We analyse a simple binary classification of pegs and floats.[2] Figure 1 shows the proportion of pegs for each year from 1971 onwards for the three classifications. The JS scheme always identifies many fewer pegs than the other two, and since 1990 the RR scheme has registered only about half the number of floats as the BT scheme. Over the whole period, the RR scheme records that 86.8% of the country-year observations are pegs, compared with 77.2% for BT and only 48.2% for JS. In the BT case, a realignment is identified in 795 out of 5,339 peg observations (14.9%).

Figure 2 shows the agreement rate for each pair of schemes in each year (the percentage of observations on which the schemes agree). The JS/RR pair always has a relatively low agreement rate, around the 60% mark. The BT/JS agreement rate was around 70% from the mid-1970s to the mid-1990s, since when it has been on an increasing trend, reaching over 80% in the last few years. The BT/RR agreement rate has been fairly constant at around 80%.

Why do these classifications disagree so much? Is it because they use different data: the official exchange rate against a single reference currency (JS), the parallel-market rate where available (RR) or regression residuals (BT)? Alternatively, is it because of differences in the key statistic: the root mean square residual (BT), the proportion of large monthly movements (RR), or the variation in the level (JS)? Finally, are the differences driven by idiosyncratic choices of thresholds that others might question? To address this issue, we develop some amended classification schemes that are based on identical data series but retain the key statistics of the original schemes; we then consider the effect of shifting the

[2] It seems unnecessary to split pegs into "hard" and "soft" categories, because there is little disagreement about this division. RR floats are independent or managed floats in their coarse classification. The observations categorised by RR as "freely falling" are omitted throughout.

thresholds. We show that appropriately amended schemes agree much more with each other than do the original schemes.

Figure 1. Percentages of pegs identified by year

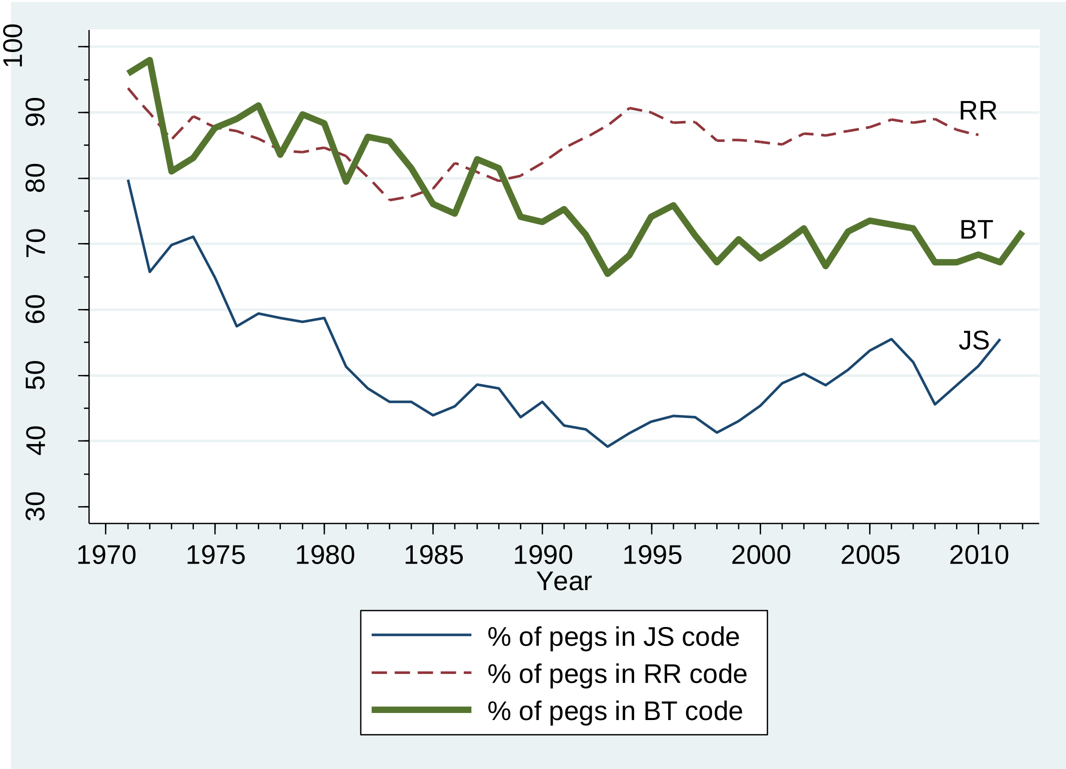
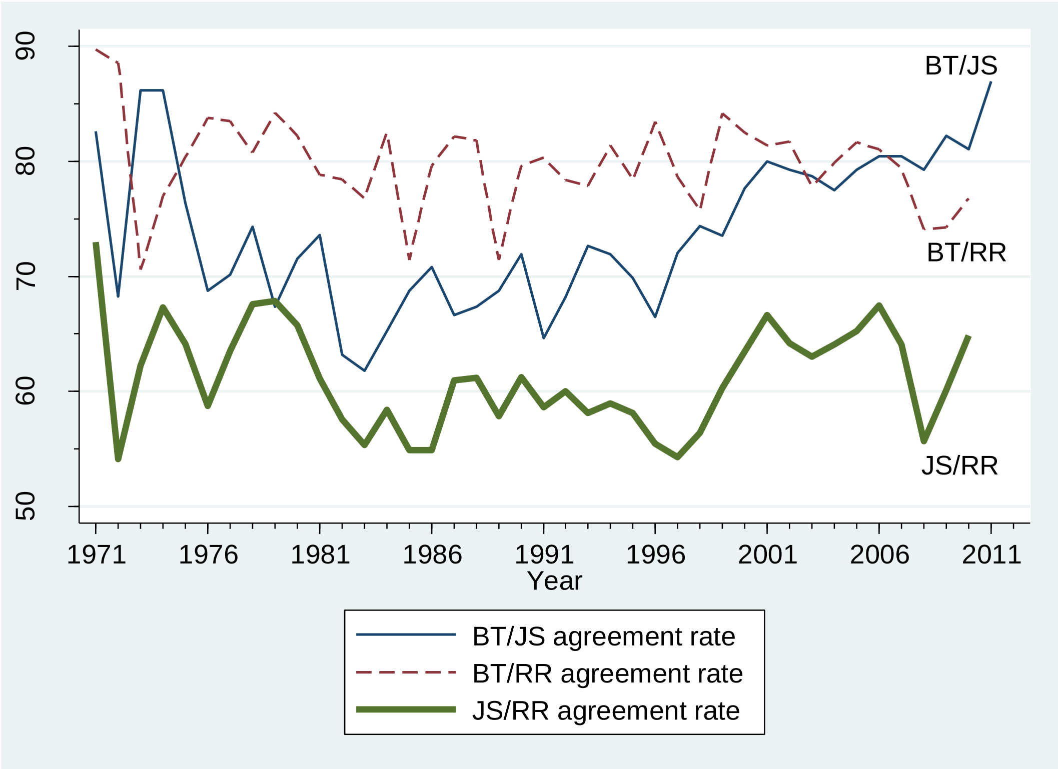


Figure 2. Pairwise agreement rates by year



The amended schemes are all based on the regression approach since this seems to be the most effective way of identifying the reference currency basket.[3] In detail, they are as follows.

Bleaney-Tian. Used on twelve monthly observations, the regression has a maximum of nine degrees of freedom (and only eight in the event of a realignment, when a one-month dummy is also included). This is an unsatisfactorily low number which is only too likely to induce errors. By contrast RR use five years of data to identify the exchange rate regime. We therefore extend the regression period to 24 months, using data from January in year $T - 1$ to December in year T to identify the regime in year T, but otherwise retaining exactly the same procedure as before, allowing for one realignment. A longer period entails a greater risk of a change of regime during the period, but this risk is offset by the increase in the degrees of freedom. The amended scheme is denoted BT24.

Shambaugh. The level criterion for a peg is liable to lead to basket pegs and crawling pegs and bands being misidentified as floats. In the case of basket pegs this is because only a single reference currency is considered; in the case of crawls it is because they necessarily involve a steady trend in the level of the exchange rate. To address this point we use the cumulated residuals from the BT 24-month regression (with a realignment if warranted) in place of the rate against a single reference currency.[4] The residuals have to be cumulated to simulate the level criterion. The new criterion is that, for a peg, the difference between the maximum and the minimum cumulated residual must not exceed 0.04. This is equivalent to allowing a 4% variation in the exchange rate level (possibly against a basket of currencies), after allowing for any crawl arrangement and one possible realignment in the 24-month period. An idiosyncratic feature of the Shambaugh scheme is the marked disparity between

[3] Reinhart and Rogoff (2004, pp. 42-3) admit that their scheme may miss some basket pegs.
[4] In the case of a single-currency peg there should be very little difference between the two.

the deviation about the central rate permitted for a peg classification when there is no realignment (±2%) and when there is a realignment (zero). It is not clear what the justification is for this difference. It must surely mean that many currencies that are pegs most of the time are coded as floats in years where there is a realignment. This is confirmed by the fact that 64.2% (489 out of 672) of the observations that are pegs with realignment according to BT are floats according to JS, compared with 27.3% (1,143 out of 4,194) of those that BT classify as pegs with no realignment. By applying the same criterion for the cumulated residuals whether or not a realignment is identified, we are eliminating this idiosyncratic feature of the Shambaugh scheme. The amended JS scheme is denoted JS24. We also consider what happens if we use the BT24 residuals but make the scheme more similar to JS by requiring a peg to have a very small RMSE (< 0.001) if a realignment is identified (denoted JS24A).

Reinhart-Rogoff. We use the residuals from the BT24 regression on official exchange rates, as with JS24, and for similar reasons. Since RR use parallel market rates, but fail to specify for which observations, it is unclear how much difference the use of official rates makes. For a float, RR require more than 20% of the monthly changes in the log of the exchange rate against every reference currency over a five-year period to exceed 0.02. The 20% figure appears to be derived from data for five currencies against the US dollar: the Australian dollar, the New Zealand dollar, the Japanese yen, the British pound and the Deutschmark/euro (Reinhart and Rogoff, 2004, p. 46). These are countries which are very distant from the US, and distance, which is associated with weaker trade links through the gravity effect, is positively correlated with exchange rate volatility (Bleaney and Tian, 2012; Bravo-Ortega and di Giovanni, 2006). In other words the 20% criterion may well be a mis-calibration, because it is based on a particularly high-volatility sample. This leads RR to code floating currencies that are not remote from their reference currencies, and therefore intrinsically less volatile because of their strong trade links, such as Canada/US and the British pound and the Swiss franc against the euro, as operating a ±2% band rather than a float for long periods.[5] This mis-calibration seems to be the source of the particularly low proportion of floats registered by the RR scheme. To retain the statistical approach of RR, whilst applying it to the BT24 residuals, we examine the distribution of residuals in the regression without monthly dummies. In version RR24 we code a float if the *second largest* residual is outside the range ±0.02; in a second version (RR24A) we code a float if the *third largest* residual is outside the range ±0.02; and in a third version (RR24B) we code a float if the *fourth largest* residual is outside the range ±0.02.[6]

In order to standardise the data series, these amended schemes are all based on the BT24 regression residuals, but they retain the spirit of the originals in that they are based on similar statistics to them: the RMSE (BT24); the variation in the level, allowing for not more than one realignment (JS24); and the proportion of large monthly changes (RR24). The differences between JS24 and JS24A, and between RR24, RR24A and RR24B, lie in the calibration of thresholds. Table 1 summarises the algorithms used.

[5] And even when they are recognised to be floating, these less remote currencies are characterised as managed floats, since the same sample of remote currencies is used to distinguish managed (lower-volatility) from independent (higher-volatility) floats.

[6] One would expect the regression approach to produce fewer residuals outside any given range than is observed in exchange rate movements against a single reference currency, because OLS tries hard to avoid large residuals. Therefore it is not clear what is the exact equivalent of RR's 20% criterion when regression residuals are used.

Table 1. Algorithms for amended schemes

Scheme	Description of algorithm for regime classification in year T
BT24	Regress monthly change in ln exchange rate against Swiss franc on change in ln US$ and euro rates against Swiss franc for January of year T-1 to December of year T. Code as peg if RMSE<0.01. If RMSE>0.01, repeat procedure 24 times, each with one monthly dummy added. If maximum F-statistic for addition of any monthly dummy > 30, use that regression in place of the original. If RMSE<0.01 at this second step, code as peg with realignment; otherwise code as float. Occasionally other reference currencies are added (for the same cases as BT).
JS24A	Estimate BT24 regressions. If maximum F-statistic for addition of any monthly dummy < 30, omit monthly dummies and cumulate residuals. Code as peg if maximum cumulated residual minus minimum cumulated residual < 0.04. If maximum F-statistic for addition of any monthly dummy > 30, use that regression in place of the original, and code as peg if RMSE<0.001; otherwise code as float.
JS24	Estimate BT24 regressions. If maximum F-statistic for addition of any monthly dummy < 30, omit monthly dummies and cumulate residuals. Code as peg if maximum cumulated residual minus minimum cumulated residual < 0.04. If maximum F-statistic for addition of any monthly dummy > 30, use that regression in place of the original, and code as peg if maximum cumulated residual minus minimum cumulated residual < 0.04; otherwise code as float.
RR24B	Estimate BT24 regression without any monthly dummies. If fourth largest residual in absolute value < 0.02, code as peg; otherwise code as float.
RR24A	Estimate BT24 regression without any monthly dummies. If third largest residual in absolute value < 0.02, code as peg; otherwise code as float.
RR24	Estimate BT24 regression without any monthly dummies. If second largest residual in absolute value < 0.02, code as peg; otherwise code as float.

4. COMPARISON OF AMENDED SCHEMES

Table 2 compares the amended schemes with the originals. Using a 24-month instead of a 12-month regression, BT24 codes slightly more floats than BT (24.22% compared with 22.79%), with an agreement rate of 89.6%.

Using JS24, a similar proportion of floats is recorded (23.11%), which is fewer than half the number coded by JS (48.16%); JS24A, which retains the difference in the threshold in the case of realignments but is otherwise similar to JS24, records an intermediate proportion of floats (36.1%). The agreement rate with JS is 80.3% for JS24A and only 68.9% for JS24.

For RR, the three amended schemes differ only in the threshold applied. With the lowest threshold (RR24B), the fourth largest residual has to exceed 0.02 in absolute value for a float to be recorded. That scheme has a very similar proportion of floats to RR (13.59% compared with 13.16%), and there is a relatively high agreement rate between the two (85.1%), considering the difference in the algorithms. Shifting the threshold in favour of floats raises the proportion of floats to 17.63% for RR24A and 23.59% for RR24, the latter figure being very similar to those for BT24 and JS24. The agreement rate with RR drops slightly, to 83.3% (RR24A) and 80.5% (RR).

Table 2. Basic data on alternative classification schemes

Scheme	Sample size	% Floats	Agreement rate (%) with:		
			BT	JS	RR
BT	6915	22.79			
BT24	6915	24.22	89.6		
JS	6481	48.16			
JS24A	6481	36.57		80.3	
JS24	6481	23.11		68.9	
RR	5054	13.16			
RR24B	5054	13.59			85.1
RR24A	5054	17.63			83.3
RR24	5054	23.59			80.5

Notes. For details of schemes see Table 1 and text. The agreement rate is the percentage of observations for which both schemes code a peg or both code a float.

Table 3 examines the agreement rates of these amended schemes with each other. The first column shows the agreement rate; the second column shows what the agreement rate would be with a random distribution of classifications, given the proportions of floats recorded by each scheme; and the third column shows how many disagreements there are compared with a random distribution, which is probably a better measure than the simple agreement rate.

The three schemes with similar proportions of floats (BT24, RR24 and JS24) have the highest agreement rates (91.3% for BT24/JS24, 83.9% for BT24/RR24 and 86.1% for JS24/RR24). Moreover the disagreements are satisfyingly low relative to the random case for BT24/JS24 (23.8%), although they are somewhat higher when RR24 is involved (44.5% for BT24/RR24 and 39.4% for JS24/RR24). The rest of Table 3 shows that, as we move back towards the individual schemes, disagreements increase, both as a proportion of total observations and relative to what would occur under a random distribution. The pattern is different for different pairs of schemes, however. The agreement rate for BT24 with JS24A (77.9%), which is essentially the residual-based equivalent of JS, is not much higher than for BT24 with JS (74.2%), and disagreements are more than 50% of random in both cases. Thus in this case it is the shift from JS24A to JS24 that really matters. For RR, the picture is reversed: moving from the original RR scheme to the residual-based equivalent (RR24B) improves the agreement rate more than moving from RR24B to RR24. In addition, the figures for the JS24A/RR24A comparison are much closer to those for JS24/RR24 than for JS/RR.

Table 3. Pairwise agreement rates relative to a random distribution

Pair of schemes	Agreement rate (%) [A]	Agreement rate (%) with random distribution [R]	Disagreements as % random [(100 – A)/(100 – R)] * 100
BT24 - JS	74.19	50.92	52.59
BT24 – JS24A	77.87	56.72	51.11
BT24 – JS24	91.32	63.45	23.75
BT24 - RR	75.94	69.11	77.89
BT24 – RR24B	80.55	68.77	62.28
BT24 – RR24A	81.89	64.89	51.58
BT24 – RR24	83.85	63.70	44.49
JS - RR	61.45	47.31	73.16
JS24A – RR24A	80.16	57.79	47.11
JS24 – RR24	86.06	64.59	39.37

Notes. See Table 1 and text for scheme descriptions. The random agreement rate R = pq + (1 – p)(1 – q), where *p* and *q* are the proportions of floats in the two schemes.

5. DISCUSSION

We have shown that *de facto* exchange rate regime classification schemes can be amended to produce much greater agreement than the originals. These amendments preserve the differences in statistical design, but use a common set of data. In this case the residuals from a Frankel/Wei-type regression are used because of their capacity to capture basket pegs and crawls. The amended BT scheme (BT24) agrees very well with the amended JS scheme (JS24), which differs from the original JS scheme in applying a similar threshold to the two cases of realignment and no realignment. The amended RR scheme with an appropriate threshold (RR24) registers a very similar proportion of floats to BT24 and JS24, but has more disagreements with them.

These three schemes differ in terms of the statistics used: the root mean square residual (BT24), the difference between the maximum and minimum cumulated residual (JS24), and the number of residuals outside a certain range (RR24). All of the original schemes are based on the idea that a peg or band should stay within ±2% of its central rate, which might be subject to a crawl and/or an occasional realignment. Only the JS24 scheme, however, is an exact statistical representation of such a target zone, since it specifies bounds on the deviation of the level of the exchange rate from the implied central rate. The other two schemes rely instead on the more general notion that pegs are less volatile than floats, and that this should be reflected in the distribution of residuals from a regression equation designed to identify the reference currency basket. It is less clear in these cases exactly where the appropriate threshold lies, and some sensitivity analysis on the part of scheme designers would seem desirable.

It also needs to be acknowledged that the literature on exchange rate volatility shows that some floating currencies, particularly those with high ratios of trade to GDP and subject to small terms-of-trade shocks, may naturally have low volatility. Such floats will always be hard to distinguish from a band of ±2% by purely statistical analysis of exchange rate movements. Table 4 illustrates the point for Canada and the United Kingdom between 1995 and 2011. Although each classification recognises that these currencies are floating most of the time, Canada meets the criteria for a peg in the BT24 scheme in 1997, and the UK does so in 2006. In the JS24 scheme, Canada is a peg in three years (1995, 1999 and 2000), and the UK in one (2001). The RR24 scheme identifies four years of pegs for Canada (1995, 1996, 2000 and 2002), but as many as seven years of pegs for the UK.

Table 4. Regime classifications for Canada and the United Kingdom, 1995-2011

	Canada			**United Kingdom**		
Scheme:	BT24	JS24	RR24	BT24	JS24	RR24
Year						
1995	Float	Peg	Peg	Float	Float	Float
1996	Float	Float	Peg	Float	Float	Float
1997	Peg	Float	Float	Float	Float	Float
1998	Float	Float	Float	Float	Float	Float
1999	Float	Peg	Float	Float	Float	Peg
2000	Float	Peg	Peg	Float	Float	Float
2001	Float	Float	Float	Float	Peg	Peg
2002	Float	Float	Peg	Float	Float	Peg
2003	Float	Float	Float	Float	Float	Float
2004	Float	Float	Float	Float	Float	Float
2005	Float	Float	Float	Float	Float	Peg
2006	Float	Float	Float	Peg	Float	Peg
2007	Float	Float	Float	Float	Float	Peg
2008	Float	Float	Float	Float	Float	Float
2009	Float	Float	Float	Float	Float	Float
2010	Float	Float	Float	Float	Float	Float
2011	Float	Float	Float	Float	Float	Peg

6. CONCLUSIONS

Previous research has registered the substantial disagreements between alternative *de facto* exchange rate regime classifications without providing much insight into the differences. We have compared three schemes that code very different proportions of floats. With suitable amendment, a much higher degree of concordance can be achieved. About half of the improved agreement rates come from the use of regression methods to capture basket pegs, crawling pegs and bands and tightly managed floats that imitate these regimes; the other half comes from specific amendments that address idiosyncratic choices of thresholds in the original schemes which seem to lack good justification. Our results suggest that the current widespread pessimism about *de facto* exchange rate regime classification schemes is unwarranted. Nevertheless a purely statistical analysis has its limitations, and needs to be supplemented with other information.

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