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A Regime Switching Approach to Uncovered Interest Parity

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A REGIME SWITCHING APPROACH TO UNCOVERED INTEREST PARITY

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Abstract

This paper reviews the empirical evidence on violations of uncovered interest parity and explores whether the evidence is consistent with the behaviour of speculative bubbles. The problems of testing for bubbles in exchange rates, without an accepted model of fundamentals, are then examined and a variety of tests are suggested. The switching regression test is the most advanced of these and it offers a robust test of the null hypothesis of time-varying risk premia. It may also explain why exchange rate changes seem to fit mixed normal distributions and why they appear to be conditionally heteroscedastic. Extensive tests are run using weekly forward rate and survey data for seven major exchange rates. The results give a very strong and robust rejection of various risk-premium models and show that exchange rates fit a switching process, as the bubble model predicts. The switching regression also suggests a simple measure of the degree of exchange rate misalignment.

Résumé

La présente étude fait la revue des résultats empiriques relatifs aux violations de la parité des taux d'intérêt sans couverture et cherche à établir si ces résultats sont compatibles avec la présence de bulles spéculatives sur les marchés des changes. On discute les problèmes de détection des bulles lorsqu'il y a incertitude quant aux déterminants fondamentaux du taux de change. Divers tests sont proposés. Le plus sophistiqué est celui de la régression avec changement de régime qui constitue un test robuste de l'hypothèse des primes de risque variables dans le temps. Ce test pourrait aussi expliquer pourquoi les variations du taux de change semblent correspondre à des distributions mixtes normales et pourquoi elles semblent douées d'hétéroscédasticité conditionnelle. De nombreux tests sont effectués avec des séries hebdomadaires de taux à terme et avec des données d'enquête pour sept des principaux taux de change. Les résultats traduisent un rejet très ferme et très robuste des différents modèles de prime de risque et montrent que ces taux de change semblent suivre un processus sélectif, tel que prédit par le modèle de bulles spéculatives. La régression avec changement de régime propose de plus une mesure simple du degré de déviation des taux de change par rapport à leur valeur fondamentale.

I) Introduction

Extensive work on the behaviour of spot and forward exchange rates in the 1970s and 1980s has shown that it is hard to reconcile their behaviour with the common assumptions of rational expectations and uncovered interest parity (UIP). The main purpose of this paper is to construct a model which could effect such a reconciliation. The first part of this paper develops a simple model which generalizes speculative bubbles to allow market agents' perceptions of the probability of a bubble collapse to deviate from the frequency of collapses actually observed. If collapses are rare, small sample problems may cause a false statistical rejection of UIP. Alternately, agents may simply misperceive the true probabilities of collapse.

The remainder of this paper addresses the problem of testing for these bubbles in the absence of a generally agreed upon model of fundamentals. Unlike earlier tests[1] where misspecification could lead to a false rejection of the null hypothesis of no bubbles, the aim is to use tests in which misspecification only reduces the new test's power to reject the null. After presenting results from a variety of established parametric and nonparametric tests, an entirely new approach is developed based on estimating an endogenous regime switching model. The new approach, which can simultaneously test several competing models of fundamentals and is robust to specification errors, is a natural one to apply to foreign exchange markets.

The new test works by detecting the presence of regime switches (bubble collapses.) Bubbles are not the only possible cause of such switches, however, so the test results should be interpreted with care. In particular, a positive finding of bubbles could be caused by the presence of a peso problem. The test's main value lies in comparing the bubble or peso problem model to non-switching models of exchange rate behaviour, particularly the time-varying risk-premium model.

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1 For example, see West(1987a), West(1987b), Meese(1986) or Borensztein(1987).

Bubble tests are run on weekly data for the G-7 currencies and the Swiss franc, covering most of the modern floating-exchange-rate period. They strongly reject the risk-premium model in favour of bubbles. Similar evidence of bubbles is produced when survey data on expected exchange rates are used instead of forward rate data. The switching regression also gives interesting measures of the degree of exchange rate misalignment and of the probability of exchange rate collapses.

The paper surveys the economic literature, noting the apparent violations of uncovered interest parity. Thereafter, the stochastic bubble model is presented, and its ability to explain exchange rate behaviour is explored. Various ways of testing the model are considered, along with their empirical results. Further interpretation of the switching model's results is given, followed by a brief conclusion. There are several appendixes, which provide more detail on the data, the bubble-testing literature, and the econometric techniques used.

II) Uncovered Interest Parity - Criticism and Rebuttal

In a simple, two-nation world populated with many identical, rational, competitive and risk-neutral investors, speculation will ensure that forward exchange rates are efficient and unbiased predictors of future spot exchange rates. Put another way, this means that

1) $F_t \equiv E(S_{t+1} | \Omega_t)$

and therefore

$$E(e_{t+1} | \Omega_t) = 0$$

where[2]

S_{t+1} ≡ the spot exchange rate at time t+1.

F_t ≡ forward exchange rate at time t for foreign exchange delivered at t+1

$e_{t+1} \equiv S_{t+1} - F_t$ This is also called the forecast error, or excess returns.

E ≡ the expectations operator

Ω_t ≡ the investors' information set at time t.

The forward rate is in turn set to prevent arbitrage profits, which requires that the forward discount is equal to the international interest differential, or

$$fd_t \equiv \ln F_t - \ln S_t = i_t - i_t^*$$

where

fd_t ≡ the log of the forward discount

i_t ≡ the log of (1 + the rate of return on domestic deposits)

i_t^* ≡ the log of (1 + the rate of return on foreign deposits)

Combining these two conditions gives the uncovered interest parity (UIP) condition

$$2) \quad E(s_{t+1}|\Omega_t) - s_t = i_t - i_t^*$$

where

s_t ≡ the log of the spot exchange rate at time t.

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[2] Notice that these variables are all specified in levels. However, the literature investigating this relationship often uses variables specified in logs. Strictly speaking, Jensen's inequality implies that this relationship need not hold in logs. However, due to the very small size (≅ 1%) of the exchange rate changes observed, the log transformation is locally very close to linear, so this equality should be very nearly satisfied. For the remainder of this paper, it is assumed that the log of the expected exchange rate is equivalent to the expectation of the log of the exchange rate, and all variables will be specified in logs.

There is a large empirical literature that examines how well the data conform to equation (2). Most papers seem to find some kind of significant violation of this relationship. Examples of this include the following stylized "facts":

I) UIP requires that forward rates are unbiased and efficient predictors of spot rates. Yet the U.S. dollar kept appreciating in 1984-85 while forward rates (and most other measures of fundamentals) consistently predicted depreciation.[3]

II) Evans(1986) shows that for U.S.-U.K. exchange rates from 1981-84, excess returns have a significantly non-zero *median*, even after allowing for data mining or one model of risk premium. This non-parametric approach avoids the assumptions of normality that underlie the inference in other studies.

III) Several researchers[4] have found that the forecast errors of the forward rate are conditionally heteroscedastic, and/or dependent on lagged information. While conditional heteroscedasticity is not necessarily a problem, the dependence on lagged information means that the forward rate is not an efficient predictor, which therefore violates UIP.

IV) Many researchers[5] have directly tested (2) by running regressions of the form $\Delta s = \alpha + \beta\cdot(i_t - i_t^*)$. Most reject the joint hypothesis that α=0, β=1. Some can even reject $\beta \geq 0$.

V) Froot and Frankel[6] used survey data on agents' expectations, running regressions of the form $\Delta s = \alpha + \beta\cdot E(\Delta s)$ to test whether these expectations

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3 See Froot and Frankel(1989) for a discussion of this, including a rejection of the "safe haven" theory.

4 Cumby & Obstfeld(1984), Giovannini & Jorion (1987), Hodrick & Srivastava(1984) are all cited by Obstfeld(1987). Also see Diebold(1988).

5 See Frankel and Froot(1987).

6 See Frankel and Froot(1987) and Froot and Frankel(1989).

appear to be rational (i.e. whether $\alpha=0$ and $\beta=1$.) They were able to strongly reject rationality.

VI) Froot and Frankel(1989) decompose $\Delta s = \alpha + \beta\cdot(i_t - i_t^*)$ using survey data on expected spot exchange rates. They find that most of the deviation of β from 1 is due to expectational errors.

These six findings all contradict the simple rational expectations model of UIP. In response to this, much research has gone into developing more sophisticated models to explain these results. The most popular of these is the risk-premium model.[7] By relaxing the assumption of risk neutrality, it introduces a risk premium ρ into (1) to give

1') $\rho + f_t \equiv E(s_{t+1}|\Omega_t)$

which then gives the modified UIP condition

2') $E(s_{t+1}|\Omega_t) - s_t = \rho + i_t - i_t^*$

This could explain the significance of α in the above regressions as well as the non-zero median of excess returns. With a time-varying premium, it could potentially make headway on the estimates of β and the excessively strong dollar, as well as the heteroscedasticity and serial correlation of forecast errors.

This line of research runs up against some seemingly insurmountable obstacles, however. First, Froot and Frankel(1989) use survey data to show that the risk premium has the wrong sign to explain α, and its effect is too small (and possibly the wrong sign) to explain β. Second, Frankel(1986) shows that for reasonable parameter values, the risk premium

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[7] Lewis(1988) presents an intriguing model based on learning, but empirical results have not been favourable to this approach. See Appendix B for a brief discussion.

will be too small to explain the bias in the forward discount.[8] Finally, survey papers by both Obstfeld(1987) and Frankel & Meese(1987) note the failure of the implicit risk premium in the forward discount to conform to any known model of asset pricing, although work in this area continues.

A different approach to interpreting some of the puzzling findings on UIP is found in the peso-problem literature. The peso problem retains all the standard assumption of the UIP model but adds an important assumption about the conditional distribution of the forecast error e_{t+1}. While agents continue to believe $E(e_{t+1}) = 0$, they believe the distribution of e_{t+1} to be highly skewed. In the simplest case, such as the possible collapse of a fixed-exchange-rate regime, they assign a low probability π to a large rise Δ in the spot exchange rate, and a high probability $(1 - \pi)$ to having the exchange rate remain fixed. This implies that, if $\Delta > 0$, $f_t > s_{t+1}$, and

$$e_{t+1} = s_{t+1} - f_t = s_{t+1} - \{(1-\pi)\cdot s_t + \pi\cdot(s_t + \Delta)\}$$

$$= s_{t+1} - (s_t + \pi\cdot\Delta)$$

$$\therefore \quad e_{t+1} = -\pi\cdot\Delta \quad \text{with probability } 1 - \pi$$
$$= (1-\pi)\cdot\Delta \quad \text{with probability } \pi.$$

This highly non-normal distribution of forecast errors makes correct inference in small samples difficult and casts doubt on the distributional assumptions found in most of the previously cited empirical work. The exception to this is Evans(1986), which uses non-parametric methods to avoid such distributional assumptions. His conclusion that forecast errors have a non-zero median is completely consistent with a peso problem.

The peso-problem approach has its difficulties. The possible large change in s_{t+1} is explained as a possible exogenous change in government policy. This means that without a model of what might cause government

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[8] There is a correction to this article by Pagan(1988) which notes that Frankel's argument requires that the time series in question be normally distributed or follow linear processes. Frankel(1988) offers a rebuttal.

policy to change, or a model of how agents form their perceptions of possible shifts in government policy, the theory is very difficult to test. In fact, a common approach is to conclude that peso problems exist simply because systematic forecast errors are observed.[9] Another problem is that this model may not be consistent with rational expectations. Frankel(1985) presents some rough calculations which suggest that in order to observe such persistent forecast errors as were associated with the appreciation of the U.S. dollar in the mid 1980s, agents must have been overestimating the probability of a large depreciation.

The bubble model developed below has much in common with the peso-problem model. Agents in the bubble model are again concerned about a small probability of a large change in the exchange rate. The difference is that the change is not due to an exogenous change in government policy, but results from the endogenous switching that is possible in a multiple equilibrium system. Aside from this, the two models are isomorphic. The bubble model shares the peso problem's abilities to explain most of the apparent violations of UIP mentioned above. After formally introducing the new model in the next section, the remainder of this paper will be devoted to developing and implementing tests of the bubble model.

III) A Regime Switching Model with Stochastic Bubbles

It is well known that the arbitrage condition $fd_t = i_t - i^*_t$ used above can give rise to multiple short-run equilibria, or bubbles.[10] The economy

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[9] Borensztein(1987) is a notable exception. See Appendix B.

[10] For example, see Blanchard and Watson(1982). It should be noted that there is a theoretical literature ruling out strictly rational divergent speculative bubbles, which is based on practical limitations on their maximum size. (For example, see Obstfeld and Rogoff(1986) and the references therein.) There are two possible counterarguments to this. One is that agents may not have *perfectly* rational expectations. While the maximum possible bubble size is limited in reality, on a week-to-week basis agents may fail to take full account of the remote possibility of running into this upper bound. The other argument is that foreign exchange markets lack the long-term speculation necessary to enforce a fundamental solution. Dornbusch(1989) and Goodhart(1988) suggest that this might be due to a variety of institutional factors. Boughton(1983) explores whether rational

may move stochastically between these solutions. For ease of exposition, it will be assumed that there are only two possible equilibria or "regimes" at any moment, and that at each time t, there is probability π_{t+1} of being in regime 2 in the next period. Regime 1 is associated with the bubble continuing on its expected course, and regime 2 corresponds to a "collapse" of the bubble. Conditional on being in regime 1, the spot rate next period will be $\tilde{s}_{t+1}$. If regime 2 occurs, however, then the expected value of next period's spot rate is $\tilde{s}_{t+1} - \mu_{t+1}$.[11] This means that agents expect next period's exchange rate to be

$$\hat{s}_{t+1} = \pi_{t+1}\cdot(\tilde{s}_{t+1} - \mu_{t+1}) + (1 - \pi_{t+1})\cdot\tilde{s}_{t+1} = f_t$$

3) $$\therefore \tilde{s}_{t+1} = f_t + \pi_{t+1}\cdot\mu_{t+1}$$

Now, assume that π_{t+1} and μ_{t+1} are both functions of the size of the bubble, $\tilde{s}_{t+1} - \bar{s}_{t+1}$, where $\bar{s}_{t+1}$ is the "fundamental" exchange rate.[12] In particular, assume that the larger the bubble, the greater the probability of bursting, and that upon bursting the exchange rate tends to move towards its fundamental value. This means

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agents unsure of the true fundamental value would have enough information to engage in stabilizing speculation, and Delong et al.(1988) note the considerable risk involved in such a strategy.

Assuming that there is an upper limit to the size of a bubble also avoids an econometric problem. Standard asymptotic distribution theory precludes regressors which grow exponentially without limit.

[11] To expand this analysis to more numerous equilibria, one can think of $\tilde{s}_{t+1}$ and μ_{t+1} as being stochastic. Also, notice that a collapse need not imply that s returns to its fundamental value. This avoids a critique of earlier bubble models which noted that in a rational market, bubbles cannot restart once extinguished. In models where it is assumed that a collapse reduces the size of the bubble to exactly zero, this implies that eventually the market becomes bubble free. In the above model this does not occur, however, since the event reducing the bubble's size to zero always has zero measure in probability space.

[12] In this context, the fundamental value can be thought of as the unique rational expectations solution that satisfies all transversality conditions and any other minimum or maximum constraints on asset prices.

$$4)\quad \frac{\partial \pi_{t+1}}{\partial(\tilde{s}_{t+1} - \bar{s}_{t+1})} \equiv \pi' > 0 \Leftrightarrow \tilde{s}_{t+1} > \bar{s}_{t+1},\ \pi' < 0 \Leftrightarrow \tilde{s}_{t+1} < \bar{s}_{t+1}$$

$$5)\quad \frac{\partial \mu_{t+1}}{\partial(\tilde{s}_{t+1} - \bar{s}_{t+1})} \equiv \mu' > 0, \text{ and } |\tilde{s}_{t+1} - \bar{s}_{t+1}| \geq |\mu_{t+1}| \geq 0$$

$$\therefore\quad \frac{\partial(\pi_{t+1} \cdot \mu_{t+1})}{\partial(\tilde{s}_{t+1} - \bar{s}_{t+1})} = \pi \cdot \mu' + \pi' \cdot \mu > 0$$

A peso-like problem occurs when the probability that agents assign to a collapse of the bubble differs from its sample probability. In the extreme case, the assumption is that the collapse is never observed in sample. More generally, it will be assumed that the sample probability of collapse p_{t+1} is $p(\tilde{s}_{t+1} - \bar{s}_{t+1})$, where $p'>0 \Leftrightarrow \tilde{s}_{t+1}>\bar{s}_{t+1}$, and $p'<0 \Leftrightarrow \tilde{s}_{t+1}<\bar{s}_{t+1}$. In the peso-problem literature, the discrepancy between p and π is usually attributed to non-random or small samples. Notice, however, that the case where agents have inaccurate beliefs about the chances of switching in large random samples is observationally equivalent.

Now consider the observed behaviour of the exchange rate in sample.

$$
\begin{aligned}
& E(s_{t+1}|\Omega_t) = (1 - p_{t+1}) \cdot \tilde{s}_{t+1} + p_{t+1} \cdot (\tilde{s}_{t+1} - \mu_{t+1}) \\
\therefore\quad & s_{t+1} = \tilde{s}_{t+1} - p_{t+1} \cdot \mu_{t+1} + e_{t+1} \\
\therefore \text{ from (2)}\quad & \quad = f_t + \pi_{t+1} \cdot \mu_{t+1} - p_{t+1} \cdot \mu_{t+1} + e_{t+1} \\
& \quad = f_t + \{\pi_{t+1} - p_{t+1}\} \cdot \mu_{t+1} + e_{t+1} \\
6)\quad & s_{t+1} - s_t = fd_t + \{\pi_{t+1} - p_{t+1}\} \cdot \mu_{t+1} + e_{t+1}
\end{aligned}
$$

where

e_t is a zero mean white noise expectational error.

Notice that only if $\pi_{t+1} - p_{t+1} \neq 0$ does a peso-like problem exist, and only then will the forward rate be a biased predictor of spot rate changes.

To understand how this model can explain some of the regression "facts" mentioned above, consider the behaviour of $\partial(s_{t+1} - s_t)/\partial fd_t$. From (6),

$$7)\quad \frac{\partial(s_{t+1} - s_t)}{\partial fd_t} = 1 + \frac{\partial(\tilde{s}_{t+1} - \bar{s}_{t+1})}{\partial fd_t}\cdot\frac{\partial[\{\pi_{t+1} - p_{t+1}\}\cdot\mu_{t+1}]}{\partial(\tilde{s}_{t+1} - \bar{s}_{t+1})}$$

This determines the value of β in the equation $\Delta s = \alpha + \beta\cdot f_t$. From (3), it can be shown that

$$\frac{\partial(\tilde{s}_{t+1} - s_t)}{\partial fd_t} = 1 + (\pi'\cdot\mu + \pi\cdot\mu')\cdot\frac{\partial(\tilde{s}_{t+1} - \bar{s}_{t+1})}{\partial fd_t}$$

$$= \left(1 - (\pi'\cdot\mu + \pi\cdot\mu')\right)^{-1}\cdot\left(1 + \frac{\partial s_t}{\partial fd_t} - (\pi'\cdot\mu + \pi\cdot\mu')\cdot\frac{\partial\bar{s}_{t+1}}{\partial fd_t}\right)$$

$$\therefore\quad \frac{\partial(\tilde{s}_{t+1} - \bar{s}_{t+1})}{\partial fd_t} = \left(1 - (\pi'\cdot\mu + \pi\cdot\mu')\right)^{-1}\cdot\left(1 + \frac{\partial(s_t - \bar{s}_{t+1})}{\partial fd_t}\right)$$

Under fairly weak assumptions, this last expression can be shown to be >0.[13] No similar results apply to the last term in (7), which measures how the size of the peso-like problem reacts to an increase in the bubble. Since the sign of this is indeterminate, the model is compatible with $\beta <> 1$. The former case would imply that peso-like problems shrink with bubble size, or in other words

$$\beta < 1 \Rightarrow \frac{\partial[\{\pi_{t+1} - p_{t+1}\}\cdot\mu_{t+1}]}{\partial(\tilde{s}_{t+1} - \bar{s}_{t+1})} < 0 \Rightarrow \text{either } \pi' < p',\text{ or } \pi < p,\text{ or both.}$$

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[13] The sign of $\partial(s_t - \bar{s}_{t+1})/\partial fd_t$ depends on how $\bar{s}_{t+1}$ is defined. If it is the fundamental solution to a Dornbusch style overshooting model, then $\partial(s_t - \bar{s}_{t+1})/\partial fd_t > 0$. If it is the exchange rate needed to give a trade surplus just equal to interest payments on the net foreign debt, then again $\partial(s_t - \bar{s}_{t+1})/\partial fd_t > 0$. If it is a purchasing power parity exchange rate, then $\partial(s_t - \bar{s}_{t+1})/\partial fd_t \approx 0$. For any of these definitions however, it is reasonable to assume $\partial(s_t - \bar{s}_{t+1})/\partial fd_t > -1$. This, together with the assumption that the expected size of collapse does not increase faster than bubble size ($\pi'\cdot\mu+\pi\cdot\mu' < 1$), gives the desired result.

Now consider how well the bubble model can explain the six stylized facts of exchange rate behaviour. Clearly, it explains the fact that exchange rates seem to deviate from fundamentals (I). Allowing for peso-like problems can also explain biased forecast errors (I). It is consistent with the finding in (II) that excess returns have a non-zero median, since the probability of collapse can skew the distribution of excess returns. Because the size of collapse changes with time and is correlated with lagged information, one would expect forecast errors to be both heteroscedastic and dependent on lagged information (III). Finally, equation (7) shows that it can explain the regression results in IV, V and VI. In summary, the bubble model could offer an explanation for all six facts.

It could be argued that the methods used below are testing for peso problems and not necessarily for bubbles. This is because the model above could be reinterpreted as a peso-problem model if the reason for exchange rate collapses were exogenous changes in policy rather than endogenous changes in expectations. In addition, some of the series used below to proxy for the size of the bubble are similar to those used by Borensztein(1987). He interprets them as proxies for the size of a shift in policy, however, and therefore as indicators of the size of a peso problem.[14] The tests suggested below will not distinguish between these two alternative hypotheses. As the aim here is to explain the six puzzles presented above and since peso problems can account for them, such an alternative interpretation is not a problem. Whether one sees the results below as evidence of a bubble or a peso problem depends on how one chooses to interpret the data series used.

IV) Univariate Tests for Switching[15]

In recent years, several methods have been proposed for testing the existence of bubbles. Many were subsequently found to be faulty, and few

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[14] See Appendix B for a fuller discussion of Borensztein(1987).

[15] The next two sections may be skipped without loss of continuity. While they present evidence consistent with the presence of bubbles, the

have been applied to exchange rate markets. Those that have are handicapped by the lack of a good model of exchange rate fundamentals.[16] Their results are dismissed by some critics as meaningless unless the model of fundamentals is correctly specified. The tests proposed below differ in approach from the rest of the bubble-testing literature. Instead of attempting to test directly whether exchange rates depart from their fundamental values, the intention is to test first whether exchange rates' characteristics are consistent with the regime switching model presented above.

The testing approach will center around trying to reject a "simple model" of UIP as the null hypothesis, with allowances for a risk premium. A constant premium is assumed in this section, based on the evidence from survey data that the risk premium is of significant size, but its variation over time is quite small[17]. For the simple model, therefore, it is assumed that UIP holds up to a constant risk premium and a mean zero *i.i.d. normal* expectational error. In other words, the null hypothesis is

$$2'') \quad s_{t+1} - s_t = \rho + f_t + e_t$$

where

$\rho \equiv$ constant risk premium

$e_t \sim N(0,\sigma_e^2)$

Another approach is to drop the risk premium and replace the forward discount f_t with survey data on expected depreciation. While this greatly reduces the number of observations available, it will be used to provide a check on the forward data results.

For the purpose of this paper, mere rejection of the null hypothesis is not enough, since the "facts" noted above cite the various ways in which

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most conclusive evidence is given in Section VI.

16 For a discussion of some of these papers, see Appendix B.

17 See Frankel & Froot(1986b). This assumption will be relaxed below.

(2") is violated. Instead, the intention is to see whether the violations *predicted by the bubble model* are present. The tests used fall into two main phases. The first phase examines whether the assumption of i.i.d. normal errors in the simple model is violated in ways consistent with the switching predicted by the bubble model. The second phase constructs various measures of the size of bubbles and tests whether these have any power in explaining deviations from the simple model.

There are a number of bubble tests based on departures from normality. They assume that bubble collapses are expected to be infrequent, but at least some are actually observed. This means that the distribution of the error term in (2") will be both skewed[18] (most observations are not collapses) and fat-tailed (if they are infrequent, collapses must be large relative to the non-collapse change.) This will be tested using the standard tests for skewness and kurtosis, together with a non-parametric sign test for skewness based on the proportion of observations above the sample mean.[19] Errors should also be persistent based on the fact that a bubble will cause unusually long strings of consecutive positive (or negative) returns.[20] This can be tested using the standard Durbin-Watson or Ljung-Box Q test.[21] The Wald-Wolfowitz non-parametric runs test, which tests for persistence based on the number of sign changes in the data, will also be used.

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[18] This is not the case if positive and negative bubbles are equally present in the sample. Therefore tests based on skewness will have low power against the null when this is true.

[19] Evans(1986) also uses the sign test to test for a non-zero median in U.S.-U.K. excess returns, but uses Monte Carlo methods to adjust significance levels to allow for data mining. Here the mining problem is avoided by using the longest available sample period rather than focussing on an interesting subsample as Evans does.

[20] This is only the case if one regime is much more likely than the other. Therefore, tests based on persistence will have low power against the null when the probability of collapse is close to 0.5.

[21] Parametric tests for persistence will have power against the null only when there is a peso-like problem in addition to the above conditions.

These tests are run on weekly data for excess returns for the G-7 and Swiss-U.S.$ exchange rates. Excess returns are measured as the forecast error of both the one week forward rate and the expected spot rate using survey data. Surveys from both the New York and London financial markets are used. The longest available sample period is used in each case, although this varied by currency and source. Starting periods for the forward rate data vary from mid-1973 to 1978, while the New York survey started in November 1982 and the London survey in 1984. All series end at the end of 1987. More information on the data used can be found in Appendix A.

Table 1 shows the results of this first phase of tests, which are very supportive of the bubble model. Using parametric tests on forward rate data, excess returns for all seven currencies are significantly skewed and fat-tailed as the bubble model would predict. The non-parametric sign test is unable to detect much skewness, however, which presumably reflects a lack of power. The reverse is true for persistence where the non-parametric test finds significant runs in all seven cases, while the Q and Durbin-Watson tests find little serial correlation.

Using survey data gives a more confusing picture. Survey data from New York markets give results that partly mirror those of the forward rate data; all four currencies have significant skewness and high kurtosis, and two of them also have significant persistence. In two cases, however, the direction of the skew is opposite to that found in the forward rate data for the same period. The London survey data give sharply different results, however. Only two of the four currencies show significant skewness or excessive kurtosis. None of the tests finds significant persistence; in fact, they all lean towards a negative rather than positive serial correlation. To see whether the survey data test results are truly representative or whether they are due to the particular sample period used, all tests are run on the forward rate data for the same dates to allow for a comparison. This shows that the weak persistence in survey data is partly explained by the sample period, as the corresponding forward rate data show little persistence despite showing strong persistence in the longer sample.

Another approach to testing for non-normality would be to compare the data's fit to the normal and other distributions. Similar work has been done by Boothe and Glassman(1987), who compare the fit of changes in exchange rates to four different distributions: the normal, the student t, the stable paretian, and the normal mixture (with identical means).[22] They find that the student and the normal mixture fit best, while the normal fits worst. Engel and Hamilton(1988) also get good results fitting exchange rate changes to a normal mixture, this time with unequal means. A constant-sized bubble might cause excess returns to be well modelled by the normal mixture with non-identical returns: one mean in the case of collapses, and one in the non-collapse case. Removing the restriction of identical means could only improve the fit of the normal mixture in Boothe and Glassman(1987).

One problem with most of the papers of this type is that they use exchange rate changes instead of forecast errors. Another is that they do not explicitly test the null hypothesis of normality against the alternative of a mixture of normals, which may be because the usual distribution of the likelihood-ratio test statistic does not apply in this case.[23] To remedy these problems, mixed normal distributions were estimated for each currency's forecast errors, and a Monte Carlo study was done to determine the correct critical value for the likelihood-ratio test statistic. The test results are displayed in Table 2. For the forward rate data, the null hypothesis of a normal distribution was always rejected in favour of a mixed normal distribution at the 1% significance level. The New York survey data was able to reject the null in every case as well; the London survey was able to do so in 2 of 4 cases.

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[22] Also see Gallant, Hsieh and Tauchen(1988) for an interesting extension of the normal mixture model, Akgiray and Booth(1988) and Tucker and Pond(1988) for work comparing the normal, the mixed normal, and the mixed diffusion jump process, and Jorion(1988) for a comparison of the ARCH and mixed diffusion jump processes Mandelbrot(1964) observes that processes with stable paretian increments may resemble mixed diffusion jump processes.

[23] See Appendix C for a discussion of this problem.

While all of the test results so far are broadly consistent with the bubble model, these rejections of normality may be consistent with many other things as well. The next phase of tests relies on having some measures of the size of the bubble, and testing their power to explain the behaviour of excess returns. This gives results that are a clearer indication that the process creating the data is a bubble, and not some other phenomenon.

V) Simple Multivariate Tests for Switching

The simplest approach to testing for bubbles is to rewrite equation (6) as

$$6') \quad s_{t+1} - f_t = \rho + (\pi_{t+1} - p_{t+1})\cdot\mu_{t+1} + e_t$$

where a constant risk premium ρ has been added to be consistent with (2"). Since μ_{t+1} increases with the size of the bubble and p_{t+1} and π_{t+1} increase with its absolute value, this suggests regressing the forecast errors on some measure of $(\tilde{s}_{t+1} - \bar{s}_{t+1})$ and hoping that $\partial[(\pi_{t+1} - p_{t+1})\cdot\mu_{t+1}] / \partial(\tilde{s}_{t+1} - \bar{s}_{t+1})$ is not too close to 0. A significant coefficient would be interpreted as a sign of both switching and a peso-like problem. A simple non-parametric test like Spearman's rank correlation coefficient, which is robust to non-normality and heteroscedasticity in small samples, can also be used to test for this relationship.

One possible drawback to these tests is that if the risk premium varies over time and is correlated with bubble measures, this would give a false indication of bubbles. In this case, further evidence of bubbles could be given by a test for conditional heteroscedasticity. The bubble model predicts that the size of a collapse is an increasing function of the size of the bubble. Therefore, the dispersion of excess returns will increase with bubble size under the alternative hypothesis, but not under the null. This is tested using a standard Breusch-Pagan test.

Six proxies for the size of the bubble are used in these tests.[24] The first two (called 1a and 1b respectively) are based on a simple overshooting model and use data on m_t, w_t and also y_t in the case of 1b. The next two are based on purchasing power parity (PPP), using either data on relative export prices (called 2a) or relative manufacturing wholesale prices (2b). The final two are the relative current-account imbalance as a fraction of total trade (3a), and the terms of trade (3b). Notice that the raw data used to calculate 1a and 1b use only bilateral data, while those for 2a, 2b, 3a and 3b use multilateral data.

Under the null hypothesis of a constant risk premium (2"), excess returns should be an arbitrary constant plus an error term which is unpredictable using any contemporaneously available information. To ensure that the bubble proxies do not include any information that was not available to market participants, all are lagged by 15 weeks to allow for announcement lags. This means that even if some (or all) of the proxies fail to adequately measure the size of the bubble, none of the tests should find any significant evidence of bubbles. In that sense, the tests below are robust to misspecification of the model of exchange rate fundamentals. Of course, measuring fundamentals correctly should increase the power of the test.

The results of the heteroscedasticity, regression and rank correlation tests are given in Table 3. The results using forward rate data are supportive of the bubble model. The straightforward regression of excess returns on the proxies gives significant coefficients for all currencies, a result confirmed by non-parametric methods for six of the seven cases. More importantly, five of the seven currencies show significant conditional heteroscedasticity related to the bubble proxies. This allows for a clear rejection of the simple rational model and strongly supports the bubble model. In the latter model it also implies that a peso-like problem exists.

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[24] See Appendix A for more details on the data series used and the construction of the indices of bubble size.

The survey data provide limited evidence for the bubble model. There was no sign of a direct correlation between excess returns and the bubble proxies in five of eight cases, regardless of whether parametric or non-parametric methods were used. This seems to be a result of the sample period, however, as the corresponding forward rate data give similar results, despite showing stronger correlations in the full sample. On the other hand, six of the eight series tested had significant conditional heteroscedasticity, and for the London market it was more pronounced in the survey data than in the respective forward rate data.

There is an interesting testable restriction on the above results. Recall that the results in IV, V and VI imply that $\pi_{t+1} <> p_{t+1}$, and that $\partial[(\pi_{t+1} - p_{t+1})\cdot\mu_{t+1}] / \partial(\tilde{s}_{t+1} - \bar{s}_{t+1}) < 0$. This implies that the rank correlations and regression coefficients in the above results should always be negative if the bubble model is to be consistent with the data. On the surface, this condition seems to be violated as often than it holds. Only 9 out of 16 regression coefficients significant at the 10% level are negative, and only 1 out of 5 significant at the 1% level. The corresponding figures for the rank correlations are 17/26 and 4/10. However, this hides very different results for the forward and survey data sets. Using a 10% significance level, the forward data gave negative regression coefficients only 4 out of 11 times, and negative rank correlations 5 out of 13 times. In contrast, the survey data gave negative signs in 5/6 and 13/13 cases respectively. This suggests that the many violations of the sign restriction in the forward data could be due to the influence of a time-varying risk premium (which was not considered in deriving the above restriction) which is correlated with some of the bubble measures. Once the risk premium is purged, however, the results are generally consistent with the implications of the model.

VI) Switching Regression Tests

There are two problems with the results of the tests above. The first is that the significant correlations in the forward rate data seem to be due in part to a time-varying risk premium rather than switching. The second is that they have no power at all against certain kinds of bubbles: those where $\pi_{t+1} = p_{t+1}$ and $\pi'_{t+1} = p'_{t+1}$. In other words, they can only detect bubbles if the frequency of collapse that agents perceive diverges from that in sample. A more desirable test would have power against bubbles regardless of whether or not this condition has been met and would distinguish between time-varying risk premia and bubbles. One approach would be to try to split the sample into those periods in which the bubble is believed to have burst and those in which it is not. This would allow estimation of the two separate equations

$$7'")\quad s_{t+1} - f_t = \rho_{t+1} + \pi_{t+1}\cdot\mu_{t+1} \qquad \text{Doesn't burst}$$

$$7")\quad s_{t+1} - f_t = \rho_{t+1} + (\pi_{t+1} - 1)\cdot\mu_{t+1} \qquad \text{Bursts}$$

The coefficients are now independent of p_{t+1}, and the risk premium can now be time-varying.

The problem in estimating the two separate equations is the difficulty in separating the sample correctly. This problem has been addressed in the econometrics literature as the "switching regression" problem.[25] The canonical form of the problem is to estimate two equations of the form

$$8)\quad Y_{1t} = X_{1t}'\beta_1 + e_{1t} \qquad e_{1t} \sim N(0,\sigma_1)$$

$$9)\quad Y_{2t} = X_{2t}'\beta_2 + e_{2t} \qquad e_{2t} \sim N(0,\sigma_2)$$

with only observations on X_{1t}, X_{2t} and Y_t, where Y_t is some unknown mixture of observations on Y_{1t} and Y_{2t}. Sometimes an additional set of variables X_{3t} is used where X_{3t} gives some information on whether Y_t belongs to Y_{1t} or Y_{2t}, according to the classifying equation

$$10)\quad Y_{3t} = X_{3t}'\beta_3 + e_{3t} \qquad e_{3t} \sim N(0,\sigma_3)$$

where

$$Y_{3t} \le 0 \Rightarrow Y_t = Y_{1t}$$
$$Y_{3t} > 0 \Rightarrow Y_t = Y_{2t}$$

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[25] See Appendix C for details.

Hartley(1978) derives the corresponding likelihood function $\mathcal{L}(Y, X_1, X_2, X_3)$ and the first-order conditions for its maximum. Kiefer(1978) shows that a local maximum of the likelihood function gives consistent, efficient and asymptotically normal parameter estimates. The results are asymptotically equivalent to estimating each equation separately by weighted least squares, where the weights are the logit probabilities of being in that regime.[26]

A number of other papers have applied switching models in both financial and non-financial contexts. Non-financial applications include Dickens and Lang(1985)'s use of a Hartley switching model to describe a dual labour market, and Lee and Porter(1984)'s examination of cartel stability with a Markov switching model. On the financial side, Akgiray and Booth(1988) and Tucker and Pond(1988) both looked at foreign exchange data and fit them to mixed diffusion jump and compound normal distributions. Engle and Hamilton(1988) fit a Markov model of segmented trends to similar data. Turner, Startz and Nelson(1989), Cecchetti, Mark and Lam(1988), and Schwert(1988) all fit Markov switching models to stock price data, and Hamilton(1988) fit them to data on the term structure of interest rates.

The above financial papers differ from this one in two important ways. First, all of them use Markov switching models rather than Hartley switching. Second, none of them attempts to relate the observed switching behaviour to any other economic fundamentals; they simply use switching to get a univariate model of their data series. This means that they give no insight into *why* the data should follow a switching process. The application of switching in this paper seems to be unique in the financial literature; it provides an economic rationale for switching and estimates the model in a multivariate setting.

To fit the bubble model into this econometric framework, excess returns are used as Y_t and all of the bubble measures are used for both X_{1t} and X_{2t}. The parameter estimates are initialized so that regime 1

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26 See Hartley(1978) and Kiefer(1980) for proofs.

corresponds to the case of positive excess returns (i.e. holding dollar assets was more profitable than expected) and regime 2 corresponds to negative excess returns. Aside from the excess returns series, the only information available for regime classification is the absolute size of the bubble, since it is assumed that the probability of collapse does not decrease with bubble size. This means using some measure of the magnitude of the bubble (such as the square of its size) as X_{3t}. Alternately, under the assumption that the bubble never changes sign, the bubble size could be used directly. The latter assumption was used in the estimates presented below, as this avoided the need to define a base period when exchange rates are assumed to be at their fundamental values.

The switching regression model can be used to test the hypothesis of switching against three simpler null hypotheses that can be nested within it. Under the null that excess returns are determined by a time-varying risk premium, then while any of the coefficients in (7') and (7") may be significant, their values should be identical in both equations (as should be the variances of their error terms). This implies that the switching regression model should collapse into a standard linear regression model. This can be tested using a likelihood-ratio test.[27] If the bubble model is correct, then the coefficients in (7') should be significantly different from those in (7"). Note that this no longer depends on whether or not a peso-like problem exists.

One reason that this test may not be convincing, however, is that it assumes that forecast errors will be conditionally homoscedastic under the null, despite evidence (see Section V) that they are not. The switching regression model may be able to reject the null simply because it can accommodate one form of conditional heteroscedasticity, where the error term switches between a high variance and a low variance state.[28] While this behaviour is consistent with bubbles, it could also be due to other

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27 This will not explain the significance of any coefficients, however, if the dependent variable is the forecast error from the survey data, since this by definition does not contain a risk premium.

28 This is sometimes referred to as the "error contamination" model. See Judge et al.(1985) for references.

factors, and therefore may not be sufficient grounds for concluding that bubbles are present. Fortunately, this possibility is easily tested. If it were true, then a likelihood-ratio test should be unable to reject the null that the coefficients in (7') and (7") are identical although the variances of their errors may not be. Notice that a rejection of this null hypothesis implies a rejection of the simpler, time-varying risk-premium hypothesis discussed above.

Even this may not convince some observers that the results are due to bubbles. If the distribution generating forecast errors were a mixture of two normal distributions, this would nest within the switching regression model and should cause rejection of both of the above null hypotheses. (Evidence that forecast errors seem to be well approximated by mixtures of normal distributions was discussed in Section IV.) It would imply that only the constants (and/or the variances) in (7') and (7") were significantly different and that the bubble proxies would have no significant impact on the behaviour of forecast errors. Because the normal mixture model can be nested within the switching regression model, this hypothesis can also be tested using standard likelihood-ratio tests.

There is an important problem in trying to test the first null hypothesis of a simple, time-varying risk premium. Under this null, β_3 is not identified. This means that the derivative of the constrained likelihood function with respect to β_3 is identically 0, so the usual derivation of the asymptotic distribution of the likelihood-ratio test statistic breaks down. Appendix C reviews various suggestions on how to deal with this problem. To ensure that the correct critical values are used, a Monte Carlo study was done to determine the behaviour of the test statistic under this null. The resulting critical values were much higher than in the standard case. These new higher values are used to test the null hypothesis of no switching in the results that follow. However, this problem does not apply to testing the switching regression model against the null of conditional heteroscedasticity, or a mixed normal distribution, where the usual critical values are used instead.

Notice that the switching regression gives a test for bubbles that is robust to misspecification in the same way that the earlier conditional heteroscedasticity and correlation tests were. If some of the models of exchange rate fundamentals used in constructing the bubble measures are misspecified, this should not lead to a false rejection of the null hypothesis of no switching. It should simply lead to a zero coefficient on that measure and reduce the power of the test. Claiming that exchange rate fundamentals have been incorrectly measured might explain the failure to find bubbles where they exist, but it cannot explain the spurious detection of bubbles. At a minimum, the latter requires some explanation of why information freely available to market participants can help predict market returns, and why it can do so in a switching manner. Time-varying risk premia do not meet this criterion, although peso problems, as noted above, do.

The results of likelihood-ratio tests of the three null hypotheses discussed above are presented in Table 4.[29] They overwhelmingly favour the switching model, as all three nulls are typically rejected at significance levels of less than 1%. For the forward rate data, time-varying risk-premium models are always rejected at the 1% level in favour of a switching one. In the case of the French franc, this was apparently due only to the presence of conditional heteroscedasticity; for all other currencies, the other two nulls are always rejected at the 1% significance level. The New York survey data also always prefer the switching model to all others, except in the case of West Germany, where the switching regression cannot reject the simple regression model.[30] However, given that the survey data by definition precludes a risk premium, this is still evidence against a time-varying premium. The London data give similar results. For half of the four currencies, regression model is weakly rejected in favour of a

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[29] The values of the likelihood functions used to compute these test statistics are given in Appendix C.

[30] Notice, however, that the switching regression can reject the heteroscedastic null, which should imply rejection of the linear regression model as well. The reason for the failure to reject the latter when tested directly is probably due to a loss of power, linked to the much higher critical values needed to test non-switching hypotheses against switching alternatives.

switching regression. However, the conditionally heteroscedastic model is rejected for all currencies[31], as is the mixed normal model.

These results give extremely strong support to the contention that deviations from UIP are caused by some form of switching (be it bubbles or peso problems) and not by time-varying risk premia. The very large test statistics above are striking: they reflect the much better fit offered by the switching model. They also reflect the size of the obstacle to be overcome by proponents of risk premia. To reconcile these results with their models, they need to explain: 1) why bubble measures have significant explanatory power, 2) why the data prefer a switching model to a non-switching one even after allowing for conditional heteroscedasticity, 3) why forward rate and survey data can give similarly strong results, and 4) why these results are consistent across all but one of the currencies examined.

VII) Interpretation of Switching Regressions

The likelihood-ratio tests of the previous section show that the switching model is capable of fitting the data relatively well. This section explores some of the switching model's estimated parameters to consider how reasonable the model's implications are, and to illustrate how much information the model provides. For the sake of brevity, only the results for Japan using the forward rate and New York survey data will be discussed. Of course, the analysis presented below could easily be applied to the other currencies in the data set.

The first things to consider are the parameter estimates shown in Table 5. The model requires that, in the absence of risk premia, the coefficients on the bubble measures will have opposite signs in the two different regimes. This is satisfied in the two cases where the bubble measure enters significantly in both regimes. However, the model would also predict that all coefficients (except the constant) should be positive

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[31] See the previous footnote.

in regime 1 and negative in regime 2. This sign restriction was violated more times than it was satisfied. Similarly, although probability of collapse is expected to increase with the size of the bubble, most of the coefficients in the classifying equations were significantly negative.

The estimated standard deviations of the disturbance terms in each regime also seem odd. In the forward rate data, regime 1 is associated with only half as much uncertainty as regime 2. In the survey data, however, these roles are reversed with regime 1 having half again as much noise as regime 2.

In light of these potentially odd parameter estimates, it seems important to consider whether the model's implications are reasonable in other respects. The fitted values for each regime are shown in the top graph in Figures 1 and 2. In the absence of a risk premium or measurement error, the fitted values should lie on opposite sides of zero (or else the expected excess returns must be non-zero.) While this is not always true, the exceptions seem to be small enough in magnitude that they could plausibly be due to either the error in the parameter estimates or, in the case of the forward data, to a time-varying risk premium with a fairly small variance. The latter would be consistent with the evidence on risk premia presented by Froot and Frankel(1989).

The center graph in Figures 1 and 2 shows the difference between these fitted values. This gives an estimate of the expected size of the potential collapse, μ_t. Included in the graphs are the approximate 5% confidence intervals for the estimated μ_t; any estimate outside these bounds should be statistically significant. Both data sets give a μ_t which fluctuates very significantly over time, and both give a peak value for μ_t of roughly 5% of the exchange rate. This is plausible value for the size of the potential collapse, remembering that it should measure the potential for collapse in one week, not the total misalignment from the fundamental exchange rate.

The movements in this indicator of exchange rate misalignment are striking. In the forward rate data, it shows the yen being briefly overvalued (negative) at the time of the second oil price shock in 1979, followed by a lengthy period of slight undervaluation from the end of 1979 to the start of 1982. From roughly zero at the start of 1983, the size of collapse then steadily increases to a peak of 5% in the first months of 1985, only to collapse to roughly zero again by the end of the sample period. During the oil shock and the flucutations of the U.S. dollar in the 1980s, this indicator mirrors the often-expressed view that exchange rates had departed significantly from fundamentals. In particular, the timing of the peak in 1985 corresponds almost exactly to that of the dollar's effective exchange rate.

The survey data tell a similar, but not identical, story. It implies that the yen is overvalued rather than correctly valued at the start of 1983, and that the return to fundamentals is not quite complete by the end of 1987. The rise and fall are less clearly delineated, and the peak occurs roughly eight months later than in the forward data. Despite this, the pattern is still broadly consistent with the views mentioned above. Notice, however, that the composition of the size of the collapse differs significantly between the forward rate and the survey data. The former gives a small positive return in regime 1, balanced against a large negative return in regime 2. The latter has the sizes reversed, with regime 1 having a large positive return and regime 2 a small negative. This plays a role in interpreting some of the evidence presented below.

In addition to providing a measure of the degree of exchange rate misalignment, the switching regression also provides a variety of measures of the probabilities of collapse over time. First, if one is willing to assume that the risk premium is zero, then the agents' ex ante perceived probabilities of collapse can be estimated. To see this, compare equations (7') and (7") with (8) and (9), and notice that if $\rho_t=0$, then the fitted values $X_{1t}'\beta_1$ give a consistent estimate of $\pi_{t+1}\cdot\mu_{t+1}$, and $X_{2t}'\beta_2$ gives a consistent estimate of $(\pi_{t+1} - 1)\cdot\mu_{t+1}$. Algebra then shows $X_{1t}'\beta_1/(X_{1t}'\beta_1 - X_{2t}'\beta_2)$ gives a consistent estimate of π_{t+1}, called $\hat{\pi}_{t+1}$.

This can be compared with an estimate corresponding to p_{t+1}. The fitted values from the classifying equation give an objective ex ante probability that a given observation will come from regime 1 (no collapse) or regime 2 (collapse.) This is calculated as $\hat{p}_{t+1} = \Phi(-X_{3t}'\cdot\beta_3)$ where $\Phi()$ is the standard normal cumulative distribution function.

Finally, the switching model also provides an ex post estimate of the probability that a given observation came from regime 1 or 2. This gives an indication of the certainty with which the switching regression is capable of separating the sample. The probability it came regime 1, denoted W_{1t}, is calculated by correcting $\hat{p}_t$ for the relative likelihood that a residual of the correct size could have come from regime 1 or regime 2. (The exact formula is given in Appendix C.) Notice that by construction W_1 and $\hat{p}$ are between 0 and 1, while measurement error or the presence of risk premia may cause $\hat{\pi}$ to fall outside this range.

Figure 3 compares these three different probability measures for the forward rate data. Several important features are clearly visible. First, the estimates of π are not very good, frequently falling outside of the [0,1] interval. Second, the W series is quite jagged, which indicates that regime changes take place frequently. Third, all three measures move closely together. All identify late 1980-early 1981, spring 1982, and early 1983 as being predominantly periods with a small probability of a large loss on the U.S.$, whereas such losses are almost certain from autumn 1983 to mid-1985. Thereafter, however, the objective probabilities of a loss rise rapidly, while the market's subjective probabilities seem to rise much more slowly, and with a lag. While this suggests the existence of a peso-like problem, it develops later than expected, only after the U.S.$'s effective value has started to decline.

The survey data tell a slightly different story. The subjective probability measures track the objective ones less accurately. While all show a virtually zero probability of a large return on the U.S.$ from mid-1983 to September 1985, the objective and subjective measures seem to diverge thereafter with the objective ones giving more frequent weight to small negative returns on the U.S.$. As above, the subjective measure seems to respond to this change gradually, and with a lag. Again this suggests a peso-like problem, albeit later than expected.

The weighting series W_{1t} is also useful for computing a number of summary statistics to judge the performance of the switching model. First, weighting the residuals of each equation by the probability that that observation came from that regime, the accuracy of the equations' fit can be measured by R^2.[32] As shown above in Table 5, R^2's for the individual equations are quite high (for asset market data), ranging between 24% and 45%. The same weights can be used to combine the residuals from each regime into a single series to assess the overall explanatory power of the model. Using these weighted residuals gives an overall R^2 of 24% for the forward data and 34.4% for the survey data. This compares to an R^2 of 6.2% and 8.3% for a simple OLS regression of forecast errors on the bubble measures. Table 5 also shows the results of tests for serial correlation on the weighted residuals. Durbin-Watson and Ljung-Box Q tests failed to detect any significant serial correlation there, despite finding it in the raw forecast errors. This is consistent with the hypothesis (discussed above) that a peso-like problem could create evidence of serial correlation, but that this would be captured by a switching regression model.

In summary, the switching model seems to have several interesting strengths and weaknesses. The weaknesses include parameter estimates with seemingly incorrect signs that differ significantly when survey data are used instead of forward data. Fitted values for each regime seem to be contaminated with measurement errors and perhaps also time-varying risk premia, with the result that estimates of agents' perceived probabilities of collapse are not completely reliable. The strengths of the model include quite high R^2's, indications of peso-like problems that are consistent with the results of earlier tests, and a complete accounting for the serial correlation observed in the original data. More importantly, the estimated values of μ_t seem to be a natural indicator of exchange rate misalignments, and the classifying equation seems to give good ex ante estimates of the probability of collapses. The latter is potentially useful from a policy standpoint.

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32 The R^2 for each equation is identical to that for a weighted-least-squares regression of excess returns on the bubble measures, using as weights W_{1t} for equation 1 and $(1 - W_{1t})$ for equation 2. See Appendix C for more information.

VIII) Conclusions

This paper has developed a bubble model which can reconcile the idea of uncovered interest parity with much of the evidence offered against it. In particular, the model offers fresh evidence against the time-varying risk-premium approach while explaining some of the results found by Frankel and Froot using survey data. It is also consistent with the univariate statistical properties of excess returns that other researchers have found.

Simple univariate and multivariate tests generally supported the model's predictions. Using a long sample period and forward rate data, all seven currencies tested showed significant skewness, excess kurtosis and positive serial correlation, and most also showed significant conditional heteroscedasticity. Tests based on survey data were less supportive, but to some extent this was due to the shorter sample period. Most importantly, the data showed that excess returns were generally correlated with lagged information, which allows a strong rejection of the simple, constant risk-premium model. In the context of the bubble model, this implies that agents do not accurately perceive the sample probabilities of switching regimes.

Estimation of the switching regression model produced the most convincing results. The model gave a relatively good fit and could strongly reject risk premia, conditional heteroscedasticity, and normal mixture models of uncovered interest parity. The same results were found for a large number of currencies, using both forward rate and survey data. The implied size of collapses also seemed to capture major exchange rate developments. This, together with an estimate of the ex ante probability of collapse, could be useful in the formulation of economic policy.

The results presented here offer a challenge to those who favour time-varying risk-premium explanations of forecast errors. In that paradigm, if the bubble measures used here do not capture the behaviour of the risk premium, then they should not have been significant in the OLS regressions, the switching regressions, or in Spearman's non-parametric test. If they do capture the risk premium, however, then the switching model should not have been able to reject the null hypotheses of no switching, conditional heteroscedasticity or a normal mixture.

While the findings presented above generally support the bubble model, there are enough discrepancies to suggest further work on the model is needed. In particular, coefficients estimates with the wrong signs suggest some misspecification. While the switching regressions strongly support the bubble model, in earlier tests the survey data provided much weaker support for the model. Finally, the differences between the London and the New York survey data results could be explored more fully.

Appendix A - Data Specifications

Unless otherwise specified, all data are for the G-7 nations plus Switzerland, and all exchange rates are in units per U.S.$.

The first series to consider is the forecast error of the forward rate, which is also known as the excess returns on holdings of foreign exchange. The main source of data is the data set constructed by Giovannini and Jorion(1987). They used noon Thursday New York spot exchange rates from the DRI database, and constructed a one-week forward discount series assuming covered interest parity and using the Thursday close interest rates on 7-day notice Eurocurrency deposits in London. On Thursdays for which the market was closed, data for the last day on which markets were open are used. The Giovannini-Jorion data set covers 1 June 1973 to 20 December 1984 for Canada, West Germany and Switzerland, but does not start until 5 July 1974 for the United Kingdom, and 9 June 1978 for France, Japan and Italy. All of these series were extended to 31 December 1987 using Thursday London market-close exchange rates and the same interest rate series as above, both taken from the Financial Times of London.

The survey data used to construct forecast errors are those used by Frankel and Froot(1987). They used Money Market Services data for the median expected spot rate in 7 days' time, and DRI spot exchange rates. These data are available only for Japan, West Germany, Switzerland and the United Kingdom. For the New York market, they span the period from 22 November 1984 to 20 April 1987, and for the London market they cover from 2 July 1984 to 8 April 1987. Both series contain several missing observations.

The major concern for testing the bubble model is obtaining a measure of the "deviation from fundamentals," $(\tilde{s}_{t+1} - \bar{s}_{t+1})$. A simple proxy for $\tilde{s}_{t+1}$ would be to use the forward rate f_t. If there is a risk premium, however, this will be a biased measure. Using expected spot rates from the survey data would avoid this problem, but the availability of the survey data is too limited for this to be a general solution. Using s_{t+1} as a proxy will be unbiased if there is no peso-like problem, but since it is not known at t, it may be related to the dependent variable under the null hypothesis. The preferred solution to this is to use s_t as a proxy for $\tilde{s}_{t+1}$. In weekly data, $s_t - \bar{s}_{t+1}$ should be very close to $\tilde{s}_{t+1} - \bar{s}_{t+1}$. Furthermore, since s_t is part of agents' information set, it should be independent of the forecast error under H_o.

Creating a measure of the fundamental exchange rate $\bar{s}_{t+1}$ is more difficult, in part because there is disagreement over its determinants. For this reason, several proxies will be used. The first is based on Dornbusch's overshooting exchange rate model as calibrated by Buiter and Miller(1982). Using their "reasonable" parameter values for the United Kingdom, the saddle path behaviour of exchange rates is given by

$$s_t = \alpha_{1a} - \frac{3}{8}\cdot w_t + \frac{11}{4}\cdot m_t$$

where

$\alpha_{1a} \equiv$ an arbitrary constant

$w_t \equiv$ log relative price deflator for domestic value added.

$m_t \equiv$ log relative money supply.

Alternatively, if the assumption that output is endogenous is relaxed to allow for the use of output data as well, then

$$s_t = \alpha_{1b} - 3\cdot w_t + 4\cdot m_t - 7\cdot y_t$$

where

$\alpha_{1b} \equiv$ an arbitrary constant

$y_t \equiv$ log relative domestic output.

Either of these equations can be used to generate the values for $\bar{s}_{t+1}$ implied by the overshooting model.[33]

Another proxy for $\bar{s}_{t+1}$ could be competitiveness. The law of one price in export markets requires that

$$s_{t+1} + P^x_t - P^{x*}_t = 0$$

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[33] Due to the problem of choosing units, $\bar{s}_{t+1}$ is identified only up to an arbitrary constant for most of the proxies discussed.

where

P^x_t = log of export price of home nation

P^{x*}_t = log of export price of foreign nation

This suggests using $\bar{s}_{t+1} = P^{x*}_t - P^x_t$ as another proxy for the fundamental exchange rate. The drawback to using export prices, especially unit value data, is that uncompetitive products tend to vanish from the sample, and so the degree of deviation from purchasing power parity (PPP) may be understated. An alternative is to do the calculation using wholesale prices in manufacturing instead of export prices. Both approaches are used to construct measures of bubble size.

A third approach is to use the external balance as a guide to $\bar{s}_{t+1}$. One measure simply uses the relative external imbalance (as a fraction of total external trade) as a proxy for the <u>deviation</u> from $\bar{s}_{t+1}$. This could be influenced by temporary changes in relative aggregate demand, however, which has little effect on the value of s_{t+1} consistent with long-run external balance. Abstracting from such changes, the other major determinant of external balance is the real exchange rate $s + P^m - P^x$ (where P^m and P^x are relative import and export prices respectively). To remain at a long run balance then requires

$$\bar{s}_t = (P^m_t - P^x_t)/2$$

which gives a final proxy for the fundamental exchange rate.

The money supply data used for the overshooting model are monthly (or weekly, where available) figures for M1 as published by each nation's central bank. Quarterly GNP deflator data are used for prices, and the index of industrial production published in the U.S. Dept. of Commerce *Business Conditions Digest* is used to measure output. For the PPP bubble measures, export prices are measured by quarterly (or monthly where available) data on export unit values and export price indices. The measures based on wholesale manufacturing prices are taken from Morgan Guaranty's index of real effective exchange rates for 40 currencies. External balance is measured by the ratio of quarterly observations on the current account balance to the sum of merchandise imports and exports, all measured in domestic currency. Finally, the terms of trade uses the export price data mentioned above together with the corresponding import price data.

While exchange and interest rates are instantly observable, much of the information in the bubble indices, such as money stocks or current account balances, is available only after a lag. In the simple model with rational expectations, excess returns at time t should be uncorrelated with all information known at that time. However, since the information in the bubble indices is not released until later, they may be correlated with excess returns even under the null hypothesis. To avoid this problem, all indices of fundamental exchange rates $\bar{s}_t$ are lagged by 15 weeks (more than a full quarter) to ensure that they contain only information known to agents.

Appendix B - Other Tests for Bubbles or Peso Problems

This appendix selectively reviews some of the literature on violations of uncovered interest parity. The focus is on other econometric approaches to testing for bubbles and peso problems and how they differ from the approach used in this paper. Previous work on testing for the existence of bubbles has been received with healthy skepticism. This may be because some of these "tests" were later found to be incapable of detecting bubbles.[34] It may also be because the tests usually rely on the assumption that the fundamentals of the process being tested are correctly specified. This is particularly troubling in foreign exchange markets, where there is little consensus on what these fundamentals are.

Consider the very interesting bubble test proposed by West(1987a) and performed by Meese(1986). West's test is based on the Hausman specification test which uses two different estimators, both of which are consistent under the null hypothesis of no bubble, but one of which is inconsistent under the alternative hypothesis of a bubble. Significantly different estimates from these two different methods imply the presence of a bubble. West suggests using the arbitrage equation directly for one estimate, and then estimating the fundamental exchange rate using the Hansen and Sargent(1980) methods of guessing the ARIMA process for the driving variables, solving the rational expectations system, and estimating the reduced-form equations imposing the rational-expectations cross-equation restrictions. This will give consistent estimates only if there is no bubble present. Meese(1986) uses this test to reject the null hypothesis of no bubbles for monthly U.S.-U.K. and U.S.-West German exchange rates from 1973 to 1982. West(1987b) uses the same model of fundamentals as Meese with a test for bubbles based on a forecast variance bounds argument, however, and is unable to detect bubbles in the U.S.-West German exchange rate from 1973 to 1984.

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[34] For example, see Flood and Hodrick(1986).

The tests proposed in this paper differ from previous work in several important respects. The first is that the accuracy of the latter depends crucially on choosing the right ARIMA process for the fundamentals, while the former do not. Similarly, misspecifying the exchange rate fundamentals in the earlier tests could give a false rejection of the null. West(1987a) applies the test to stock market data where there is widespread agreement that stock dividends are the fundamentals. There is no similar widespread agreement about the correct fundamentals for exchange rates.[35] In the tests proposed in this paper, misspecifying fundamentals does not give power against the null. Finally, the main objective of this paper is not to test for the presence of bubbles, but to explain excess returns. For the specification tests, rejecting the null proves misspecification but does not explain the behaviour of excess returns.

An important criticism of bubble models and tests is made in Flood & Hodrick(1987). They note that it is impossible to distinguish empirically between a bubble solution and a fundamental solution where the econometrician has misspecified agents' future beliefs. In simple terms, the reason agents appear to be off the saddle path could be explained by either a bubble or by an anticipated shift in the saddle path that the researcher failed to account for. Therefore, whenever the bubble model is mentioned, the implication is that there is a fundamental solution story that can give the same results.

In the context of exchange rate behaviour in the 1980s, however, a bubble seems more intuitively appealing.[36] In the bubble story, agents think that the dollar is "overvalued," which seems to be in line with the sentiments of the time. In the expectations story, they are expecting a future change in policy that will justify the higher dollar.[37] The bubble

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[35] Meese(1986) uses a monetary model of fundamentals, based on uncovered interest parity and money demand functions, and rejects the null hypothesis of no bubbles.

[36] For example, see Meese(1986).

[37] They must be expecting higher future $i-i^*$ even as current $i-i^*$ falls, or a more appreciated long-run value of e, or both.

interpretation gives a ready explanation for forecast error bias and the presence of switching; agents fear a sudden but uncertain depreciation due to a collapse of the bubble. In the fundamental interpretation, this arises only if agents are not certain when the future changes they expect will actually occur.

Another interesting paper is that of Borensztein(1987). Borensztein's objective is very similar to this paper's: to explain the seemingly excessive returns on U.S.-dollar assets in the early 1980s. He considers both peso problems and stochastic bubbles as possible models. As he defines them, the difference between the two models is that in a peso problem the process switches because of a change in policy, whereas in a bubble the switch is due to a change in agents' expectations. However, in light of the data he uses and the Flood and Hodrick(1986) note, it is doubtful that he has managed to separate these two phenomena. In fact, he finds support for both. He also assumes that there is no process switch observed in his sample (which ends in February 1985), an assumption relaxed in the model developed above.

His approach to testing for bubbles is very different from that developed in this paper. He begins by ignoring risk premia and assuming that no burst bubbles are observed in the sample, but that if they had, the exchange rate would return to its fundamental value. He then iteratively solves the asset arbitrage equation forward to obtain an expression for the bubble in terms of expected future fundamentals, interest differentials, and an explosive bubble term. His estimation strategy is also that of Hansen and Sargent(1980): assume ARIMA processes for the fundamentals (he assumes the fundamental exchange rate is constant), solve the rational expectations system, estimate the system using a minimum distance method, and impose the cross-equation restrictions implied by rationality.

He presents estimates for only U.S.-West German data. Most of the parameters are significant, with the correct sign and size, and he is unable to reject the cross-equation restrictions implied by rationality. He claims that the bubble term $A \cdot \pi^{-t}$ is significant, even though A alone is not. Aside from the large number of assumptions made (no risk premium, constant fundamental exchange rate, no collapses in the sample, etc.) the most serious criticism of these results would be the very small sample sizes used. Depending on the subperiod used, his sample has 24 to 51 observations which are used to estimate 7 parameters in a non-linear system.

His approach to testing for peso problems is similar in spirit to that used in this paper. Excess dollar returns (Y) are regressed on X (some variables thought to be related to the risk premium), and on Z (some variables thought to be instruments for the likelihood of a change in policy). The regressions are run with monthly data for G-5 currencies and the Swiss franc from 1974 to 1985, and the coefficients on Z are generally significant, which he interprets as evidence in favour of peso problems. Furthermore, while excess returns have often been found to be conditionally heteroscedastic and serially correlated, the residuals from his regressions (except in the case of the U.K.) are not.

There are three problems with his results. The first is that the Z variables are multiplied by an indicator function that sets them to zero outside the 1980-1984 period. The reason for this is not explained, but seems to be that this is the period when Y is observed to be behaving abnormally. Formally, this means that exogenous variables are chosen conditioned on the endogenous variable, which should invalidate formal tests of statistical significance. Less formally, by using an indicator function for a period where Y is known to be unusually high, the Z variables may simply be acting as a dummy variable (i.e. a constant) and have little to do with peso problems. Put another way, he should explain why, under the alternative hypothesis, variables thought to indicate possible changes in policy create peso problems during the 1980-1984 period, but not before. The second problem is that his results imply that the probability that the policy would change in a given month was between 15% and 80%. This means that the odds of observing a four-year period with no policy change (which he assumes) are between 0.04% and $2.8\text{x}10^{-32}\%$. Finally, as discussed above, this approach is likely to have low power if the sample contains regime changes.

Another approach to testing for peso problems is that of Krasker(1980). His aim is to test whether forward rates are unbiased predictors of spot exchange rates (speculative efficiency) while allowing for the presence of peso problems. His method is to construct a proxy for the expected size of the collapse and to assume that the probability of collapse at time t is given by $e^{-(\alpha+\beta\cdot t)}$. Further assuming that no collapses are seen in sample, he adjusts the excess return series for the expected value of the collapse, and claims that speculative efficiency can be rejected only if the adjusted series rejects it for all reasonable values of α and β. In his case, a grid search across these parameters (using data from the mark-pound markets during the German hyperinflation) does not always reject the null.

The difficulties with this early paper fall into two categories. The many assumptions required (no collapses in sample, deterministic size of potential collapses, no risk premium, arbitrary specification of collapse probabilities) make the robustness of his conclusion suspect. Furthermore, his inability to narrow the range of reasonable parameters for α and β must greatly lower the test's power.

As an alternative to other models explaining violations of UIP, the learning model developed in Lewis(1988a) shows that if agents are uncertain about whether an increase in the rate of growth of money <u>demand</u> has occurred, then rational updating of their beliefs will produce some desirable properties. First, ex post forecasts of the exchange rate will appear to be biased for a time after a process switch, even though they are made rationally. Second, during this time, exchange rates will appear to be deviating from fundamentals. Third, the changing subjective probabilities of a switch add a new source of volatility to exchange rates, making them more volatile than fundamentals and making forecast errors conditionally heteroscedastic. Finally, Lewis presents some small sample Monte Carlo results which show that her model could generate significant estimates of serial correlation in forecast errors and negative values of α and β in regressions of $\Delta e = \alpha + \beta\cdot E\Delta e$. This line of research could therefore potentially answer all six of the puzzles listed above. However, empirical support for this model remains weak. Typically, the simulated forecast errors generated by her model do not correlate well with the actual errors, and they consistently predict that forecast errors should have died away much more quickly than they did.[38]

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[38] See Lewis(1989).

Appendix C - The Econometrics of Switching Regressions

This appendix gives an overview of the basic econometrics of switching models and in particular of the switching regressions used in this paper. It surveys some of the many methods available for estimating such models and notes some of their drawbacks. It also notes different approaches to the problem of testing the null hypothesis of no switching against a switching alternative, and produces the results of a Monte Carlo experiment designed to determine the appropriate critical values of the likelihood-ratio statistic for this case. Finally, a table comparing the likelihood functions for various models is presented.

Many different approaches to estimating switching models are available. In a seminal paper, Goldfeld and Quandt(1973) consider the case without X_{3t} where the probability of switching from one regime to the other at any given time is just a constant, λ. They derive a maximum likelihood (ML) procedure for obtaining consistent estimates of β_1, β_2, σ_1, σ_2 and λ.[39] This approach has some drawbacks, however, as well as many proposed improvements. The latter include a Bayesian method due to Swamy and Mehta(1975), a moment-generating-function method suggested by Ramsey and Quandt(1978) and refined by Schmidt(1982), a refinement of the ML method due to Cosslett and Lee(1985), and a variant of the EM algorithm developed by Hamilton(1989).

The problem with the above papers is that the transition probability λ is constant over time, an assumption relaxed in the bubble model. Therefore the second strand of this literature, which considers the case with X_{3t}, is more appropriate for the bubble model. Hartley(1978) derives the likelihood function $\mathcal{L}(Y, X_1, X_2, X_3)$ and the first-order conditions for

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[39] It is not possible to identify both λ and σ_3 (or in the full model, β_3 and σ_3), so σ_3 is set to an arbitrary constant.

its maximum.[40] He shows that the ML estimators of β_1 and β_2 can be calculated by a weighted-least-squares (WLS) regression of X_{1t} and X_{2t} respectively on Y_t, using weights W_{1t} and W_{2t}. ML estimates of σ_1 and σ_2 are derived from the weighted residuals of these regressions. ML estimates of β_3 are obtained from an OLS regression of X_{3t} on a third set of weights W_{3t}.

The weights W_{1t}, W_{2t}, W_{3t} used in these regressions need some explanation. The first two are

$$W_{1t} \equiv \lambda_t \cdot f_1(Y_{1t}|Y_t)$$
$$W_{1t} \equiv (1 - \lambda_t) \cdot f_2(Y_{2t}|Y_t)$$

where

$$\lambda_t \equiv \Phi(-X_{3t}'\beta_3)$$

= the probability of being in regime 1 given the information from the classifying equation.

$$f_1(Y_{1t}|Y_t) \equiv \frac{\phi\left(\dfrac{Y_t - X_{1y}'\beta_1}{\sigma_1}\right)}{\lambda_t \cdot \phi\left(\dfrac{Y_t - X_{1y}'\beta_1}{\sigma_1}\right) + (1-\lambda_t)\cdot\phi\left(\dfrac{Y_t - X_{2y}'\beta_2}{\sigma_2}\right)}$$

= the probability of being in regime i conditioned on Y_t.

{Notice that $f_1(Y_{1t}|Y_t) \equiv 1 - f_2(Y_{2t}|Y_t)$}

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[40] As noted by Swamy and Mehta(1975), the likelihood function for this sample is unbounded, and its matrix of second derivatives is potentially singular. To see the former, notice that for appropriate β_3 we appear to have only one observation on Y_{1t}, and for arbitrarily small σ_{e3} we think we know this separation with certainty. If we now choose β_1 to fit this one point exactly, then we can make $\mathcal{L}$ arbitrarily large by using $\sigma_{e1} \rightarrow 0$ and $\sigma_{e2} \rightarrow \infty$.

However, Kiefer(1978) shows that a local maximum of $\mathcal{L}$ gives consistent, efficient, asymptotically normal parameter estimates. Hartley(1978) shows that this local maximum corresponds to a convergence of the iterative WLS algorithm, which is equivalent in this case to the EM algorithm. Hartley(1978) and Masson(1985) both document the favourable performance of this algorithm in Monte Carlo exercises.

$\Phi(\cdot) \equiv$ the standard normal cdf.

$\phi(\cdot) \equiv$ the standard normal pdf.

Kiefer(1980) illuminates this further by noting that these weights can be interpreted as the estimated logit probabilities of being in regime 1 or 2. The third weighting series is given by

$$W_{3t} \equiv X_{3t}'\beta_3 - \frac{W_{1t}\cdot\phi(0)}{\lambda_t} + \frac{W_{2t}\cdot\phi(0)}{1 - \lambda_t}$$

which is just the conditional expectation of Y_{3t}.

Notice that in Hartley's method, the coefficient estimates are functions of the weights used, and the weights are functions of the coefficients used. Analytically solving this system of equations is cumbersome. Hartley instead shows that iterating over successive approximations of these parameters can give a solution. Hartley(1978) and Masson(1985) document the favourable performance of this algorithm in Monte Carlo studies.

Hartley's algorithm shares one very important drawback with the standard MLE approach, however. As noted by Swamy and Mehta(1975), the likelihood function for any given sample is unbounded, and its matrix of second derivatives is potentially singular. (To see the former, notice that in the simple case where X_{1t} and X_{2t} are scalars, for appropriate β_3, we would believe with near certainty that we have only a single observation on Y_{1t}. If we now choose β_1 to fit this one point exactly, then we can make the value of the likelihood function arbitrarily large by using $\sigma_{e1} \to 0$ and $\sigma_{e2} \to \infty$.) Kiefer(1978) shows that there is still a *local* maximum of the likelihood function that gives consistent, efficient and asymptotically normal parameter estimates. Hartley(1977) shows that his algorithm converges either to a boundary solution, which corresponds to the unbounded area of the likelihood function, or to an interior solution, which corresponds to a local maximum of the likelihood function. If more than one interior solution is found, it is presumed that the correct one is the one that maximizes the likelihood function. In practice, convergence depends on good initial estimates and is helped by having very different sets of variables in X_1 and X_2, which are unfortunately identical by definition in the bubble model.

The alternative to the WLS and the MLE estimation procedures is the moment-generating-function approach mentioned above. While the likelihood function may sometimes be poorly behaved, there is no such problem with the moment-generating function, so achieving convergence should not be problem. This method has problems of its own, however. It is not an efficient approach, and estimators based on higher order moments tend to be less stable.[41] Ramsey and Quandt also note that it is not clear which moments should be used for optimal results, and that conflicting results may be produced by using different combinations of moments. As mentioned above, the moment-generating model has only been derived for a more restrictive switching model than the one used here. For these reasons, the moment-generating-function approach was not used in this paper.

The actual estimation procedure used for this paper was the MLE method. Initial values for each regime were created from separate OLS regressions of positive and negative forecast errors on bubble measures. Initial values for the classifying equation were taken from an OLS regression which used all the forecast errors and scaled the coefficients by the inverse of the standard error. In cases where these starting values were troublesome, the parameters of a mixed normal distribution were estimated first and used as initial values for the switching regression. The estimation itself was done on an IBM PC using the GAUSS Maxlik procedure with analytical derivatives.

In many applications of switching models, it is useful to be able to test the null hypothesis of no switching against a switching alternative. Jorion(1988), Akgiray and Booth(1988), and Tucker and Pond(1988) all test this using a likelihood-ratio statistic, which they assume has the usual χ^2 distribution. Unfortunately, the standard proof of the asymptotic distribution of the likelihood-ratio statistic does not apply in this case. To nest the null of no switching within the switching model, either all the

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[41] See the comments by various authors immediately following Ramsey and Quandt(1978).

observations are assumed to come from one regime, which means the parameters of the other regime are not identified, or the two regimes are assumed to be identical, which means the parameters of the classifying equation are not identified. This also implies that the score test statistic is identically zero, and that the information matrix is singular.

Lee and Chesher(1986) consider the problem of testing in this situation. They suggest various reparameterizations for other models, which unfortunately do not apply to the switching case. They show that in some cases, although the standard proof breaks down, the likelihood-ratio test will still have the usual χ^2 distribution. Again, this does not apply to the switching model. Finally, they suggest trying to construct an extremum test based on higher-order derivatives of the unrestricted likelihood function.

Other applied researchers have used other ways around this problem. Dickens and Lang(1985) cite Goldfeld and Quandt(1976)'s Monte Carlo evidence that an approximately correct likelihood-ratio test can be constructed by adjusting the degrees of freedom under the null from merely the number of constrained parameters to that plus the number of undefined parameters. However, Turner, Startz and Nelson(1989) note experimental work by Wolfe(1971) and Everitt(1981) that suggests using different distributions under the null. Wolfe recommends correcting the likelihood-ratio test statistic by a factor of $(n - 1 - d - c_1/2)/n$, and testing it against a $\chi^2(2d(c_1 - c_2))$, where n = the number of observations, d = the number of parameters constrained under the null, c_1 = the number of regimes under H_a, and c_2 = the number of regimes under H_0. Everitt(1981) does a larger Monte Carlo study and finds that this is justified only when $n > 10d$.

The above-cited Monte Carlo work used a Markov model of switching. To determine its applicability for the model in this paper, a new Monte Carlo study was done to examine the behaviour of the likelihood-ratio test statistic. Results are shown in Table D1. Four basic cases were examined; testing the null of a normal distribution against the alternative of a normal mixture, and testing the null of a standard regression with normally distributed errors against the alternative of a switching regression. Both of these cases were simulated for 100 and 500 observations and then estimated. Figure 5 compares the distribution of the resulting likelihood-ratio statistics with the best fitting χ^2 distribution.

For the normal mixture case, the test statistics for both the 100- and 500-observation cases look to be distributed $\chi^2(4)$. However, Kolmogorov-Smirnov tests are able to reject the null that they follow any χ^2 distribution at significance levels of better than .01%. This may not be a serious problem, given that most of the power for rejecting the null is coming from the difference in the lower tails of the distributions, whereas the test statistic will rely on the similarity of the upper tails. Table D1 compares the critical values of the empirical distribution with those of the $\chi^2(4)$. The critical values look very similar in the 10%, 5% and 1% cases. Wolfe's recommended correction also seems to fit well. The Kolmogorov-Smirnov test was unable to reject the null that the empirical distributions for the 100- and 500-observation cases came from the same underlying population.

In the case of the switching regression, the Kolmogorov-Smirnov tests reject the null that the test statistics come from *any* χ^2 distribution. They also reject the null that statistics from the 100- and 500-observation cases come from the same distribution, and while one is best approximated by 25 degrees of freedom, the other requires 27. Figure 5 clearly shows that the test statistics' distributions do not look χ^2. The graph compares them both to a $\chi^2(29)$, which offers the best fit in the upper tail of the distribution. Table D1 shows that the tails for the empirical distributions appear to be larger than those of the best fitting χ^2, or those of Wolfe's corrected χ^2 distributions.

These results suggest that as the number of regressors increase, the χ^2 becomes a less accurate approximation of the likelihood-ratio test statistic's distribution. While a $\chi^2(4)$ seems to be acceptable for testing the alternative of mixed normal distribution, the switching regression's test statistic does not seem to be χ^2. For applied work, the empirical cutoff values are more likely to accept the null of no switching.

For the tests comparing the linear regression model to the switching regression model, the empirical critical values were used. Other hypotheses were tested using standard χ^2 critical values. A list comparing the values of the likelihood functions for the various models is presented in Table D2.

TABLE 1 - Univariate Tests

Forward Rate Data

Kurtosis	Skewness	Sign Test	Durbin Watson	Q-Stat.	DoF	Runs Test
CANADA: *760 Obs. from 1/6/73 to 18/12/87*						
1.46‡	.21†	.18	1.77	135.24‡	81	-2.18†
FRANCE: *498 Obs from 9/6/78 to 18/12/87*						
3.16‡	-.23†	.40	1.89	79.64	66	-1.55†
FEDERAL REPUBLIC OF GERMANY: *760 Obs. from 1/6/73 to 18/12/87*						
3.36‡	-.36‡	.18	1.84	116.19‡	81	-3.41‡
ITALY: *498 Obs. from 9/6/78 to 18/12/87*						
2.49‡	-.32‡	.31	1.85	94.96†	66	-2.16†
JAPAN: *498 Obs. from 9/6/78 to 18/12/87*						
3.44‡	-.83‡	2.46†	1.61‡	101.44‡	66	-2.16†
SWITZERLAND: *760 Obs. from 1/6/73 to 18/12/87*						
3.09‡	-.38‡	.76	1.99	83.28	81	-3.07‡
UNITED KINGDOM: *703 Obs. from 5/7/74 to 18/12/87*						
4.88‡	.21†	-1.43	1.94	96.60‡	78	-2.41‡

† ≡ Significance < 10%   ‡ ≡ Significance < 1%

See end of table for additional notes.

TABLE 1 - Univariate Tests - Continued

Survey vs. Forward Rate Data - New York Markets

	Kurtosis	Skewness	Sign Test	Durbin Watson	Q-Stat	DoF	Runs Test
FEDERAL REPUBLIC OF GERMANY: *204 Obs. from 12/11/82 to 11/12/87*							
▹ Survey data	1.76‡	.47‡	-.49	1.80	38.21	42	-1.46†
▹ Forward rate data	1.21‡	-.19	.49	1.82	39.88	42	-.87
JAPAN: *204 Obs. from 12/11/82 to 11/12/87*							
▹ Survey data	3.72‡	.95‡	-.63	1.51‡	51.13	42	-2.12†
▹ Forward rate data	1.34‡	-.77‡	2.03†	1.62‡	48.21	42	-2.01†
SWITZERLAND: *192 Obs. from 14/1/83 to 11/12/87*							
▹ Survey data	6.33‡	-.39†	-.51	1.85	26.94	39	-1.84†
▹ Forward rate data	2.41‡	-.45†	.36	2.16	32.33	39	-.76
UNITED KINGDOM: *204 Obs. from 12/11/82 to 11/12/87*							
▹ Survey data	3.42‡	-.83‡	1.05	2.02	39.57	42	.07
▹ Forward rate data	4.40‡	-.67‡	.21	1.78	28.83	42	-1.46

† ≡ Significance < 10% ‡ ≡ Significance < 1%

See end of table for additional notes.

TABLE 1 - Univariate Tests - Continued

Survey vs. Forward Rate Data - London Markets

Kurtosis	Skewness	Sign Test	Durbin Watson	Q-Stat	DoF	Runs Test
FEDERAL REPUBLIC OF GERMANY: *138 Obs. from 29/6/84 to 3/4/87*						
▷ Survey data						
.40	-.36†	-.09	2.12	28.18	33	2.15
▷ Forward rate data						
2.43‡	-.46†	-.60	1.80	29.16	33	1.68†
JAPAN: *138 Obs. from 29/6/84 to 3/4/87*						
▷ Survey data						
1.00‡	-.32	-1.11	2.16	43.50	33	.84
▷ Forward rate data						
9.05‡	-1.90‡	1.79†	1.58‡	36.27	33	-1.45
SWITZERLAND: *137 Obs. from 29/6/84 to 3/4/87*						
▷ Survey data						
-.20	.23	0.0	2.11	18.29	33	.94
▷ Forward rate data						
2.20‡	-.53†	-.17	2.09	23.86	33	-.17
UNITED KINGDOM: *137 Obs. from 29/6/84 to 3/4/87*						
▷ Survey data						
1.76‡	.52†	-.17	2.11	26.81	33	1.15
▷ Forward rate data						
4.36‡	-.75‡	-.17	1.79	28.95	33	-1.90†

† ≡ Significance < 10% ‡ ≡ Significance < 1%

See end of table for additional notes.

TABLE 1 - Univariate Tests - Notes

Under the null hypothesis of normally distributed excess returns, the kurtosis statistic Ku and the skewness statistic Sk both have an expected value of 0. To convert them to N(0,1) test statistics, use the formulas:

$$Z_{Ku} \equiv Ku \cdot \sqrt{\frac{(N-1)\cdot(N-2)\cdot(N-3)}{24\cdot N\cdot(N+1)}} \qquad Z_{Sk} \equiv Sk \cdot \sqrt{\frac{(N-1)\cdot(N-2)}{6\cdot N}}$$

The significance levels reported above are based on a one-tailed test for Ku and a two-tailed test Sk.

The sign and runs test statistics are distributed N(0,1) under the null. The sign test statistic is calculated as

$$Z_{sign} = \frac{p - .5\cdot N - .5}{.5\cdot\sqrt{N}}$$

where

$p \equiv$ the number of positive or negative signs
$N \equiv$ the number of observations

and the runs test statistic is calculated as

$$Z_{runs} = \frac{R - m(R) + .5}{\sigma(R)}$$

where

$R \equiv$ the number of sign changes + 1
$m(R) \equiv 2\cdot N_1\cdot N_2/(N_1 + N_2)$
$\sigma(R) \equiv 2\cdot N_1\cdot N_2\cdot(2\cdot N_1\cdot N_2 - N_1 - N_2) \ / \ [(N_1 + N_2)^2\cdot(N_1 + N_2 - 1)]$
$N_1 \equiv$ the number of negative observations
$N_2 \equiv$ the number of positive observations

The significance levels reported are for a two-tailed and a one-tailed test, respectively.

The Durbin-Watson statistic is constructed using only a constant as the dependent variable. In lieu of precise published tables of significance levels for this case, extrapolated values are used based on the expanded tables by Savin and White(1977). Note that for this statistic only, † ≡ Significance < 5%.

The Ljung-Box Q statistic is distributed χ^2 with the indicated degrees of freedom under the null hypothesis of no serial correlation. Significance levels are based on a one-tailed test.

TABLE 2 - Lkelihood Ratio Tests of Normal vs. Normal Mixture

Currency:	LR Stat.
Forward Data	
Canada	44.422‡
Federal Republic of Germany	79.926‡
France	43.098‡
Italy	34.580‡
Japan	43.090‡
Switzerland	92.986‡
United Kingdom	79.124‡
New York Survey Data	
Federal Republic of Germany	11.420†
Japan	28.096‡
Switzerland	35.024‡
United Kingdom	38.968‡
London Survey Data	
Federal Republic of Germany	2.798
Japan	10.358†
Switzerland	2.034
United Kingdom	10.026†

† ≡ Significance < 10%   ‡ ≡ Significance < 1%

See Table D1 for critical values.

TABLE 3 - Heteroscedasticity Tests and Simple Regressions

Forward Rate Data

	Heteroscedasticity	F-test	Spearman's Z
CANADA: 745 Obs. from 14/9/73 to 18/12/87	24.96‡	2.61†	2.25†
FRANCE: *430 Obs. from 22/9/78 to 12/12/86*	6.20	1.48†	-2.82†
FEDERAL REPUBLIC OF GERMANY: *745 Obs. from 14/9/73 to 18/12/87*	21.88‡	2.90‡	-3.04‡
ITALY: *483 Obs. from 22/9/78 to 18/12/87*	10.53†	3.02‡	-3.72‡
JAPAN: *483 Obs. from 22/9/78 to 18/12/87*	3.97	5.20‡	-4.17‡
SWITZERLAND: *745 Obs. from 14/9/73 to 18/12/87*	15.96‡	2.68†	-2.38†
UNITED KINGDOM: *688 Obs. from 18/10/74 to 18/12/87*	35.62‡	2.92‡	1.48

† ≡ Significance < 10% ‡ ≡ Significance < 1%

See end of table for additional notes.

TABLE 3 - Heteroscedasticity Tests and Simple Regressions

Survey vs. Forward Rate Data - New York Markets

	Heteroscedasticity	F-test	Spearman's Z
FEDERAL REPUBLIC OF GERMANY: *204 Obs. from 12/11/82 to 11/12/87*			
▷ Survey data	12.00†	2.08†	2.48†
▷ Forward rate data	14.61†	.58	2.48†
JAPAN: *204 Obs. from 12/11/82 to 11/12/87*			
▷ Survey data	10.15	2.98‡	3.05‡
▷ Forward rate data	8.00	1.92†	3.22‡
SWITZERLAND: *194 Obs. from 14/1/83 to 11/12/87*			
▷ Survey data	5.46	1.71	2.45†
▷ Forward rate data	19.70‡	1.29	2.64†
UNITED KINGDOM: *204 Obs. from 12/11/82 to 11/12/87*			
▷ Survey data	29.05‡	2.45†	1.67
▷ Forward rate data	29.05‡	2.45†	1.67

† ≡ Significance < 10%   ‡ ≡ Significance < 1%

See end of table for additional notes.

TABLE 3 - Heteroscedasticity Tests and Simple Regressions
Survey vs. Forward Rate Data - London Markets

	Heteroscedasticity	F-test	Spearman's Z
FEDERAL REPUBLIC OF GERMANY: *138 Obs. from 29/6/84 to 3/4/87*			
▷ Survey data	14.68†	1.36	.81
▷ Forward rate data	7.62	1.54	.54
JAPAN: *138 Obs. from 29/6/84 to 3/4/87*			
▷ Survey data	12.77†	.60	.86
▷ Forward rate data	7.49	1.17	5.70‡
SWITZERLAND: *137 Obs. from 29/6/84 to 3/4/87*			
▷ Survey data	12.79†	.96	.80
▷ Forward rate data	12.27†	1.01	.80
UNITED KINGDOM: *137 Obs. from 29/6/84 to 3/4/87*			
▷ Survey data	15.88†	1.23	-.89
▷ Forward rate data	16.95‡	2.07†	7.92‡

† ≡ Significance < 10%   ‡ ≡ Significance < 1%

See end of table for additional notes.

TABLE 3 - Heteroscedasticity Tests and Simple Regressions - Notes

All tests use all six bubble proxies. Bubble proxies are constructed using data lagged 15 weeks to allow for announcement lags.

Heteroscedasticity is tested using a variant of the Breusch-Pagan test described in Judge et al. (1985) which is more robust to skewness of the error terms. The test statistic is formed by regressing squared excess returns on X_t, the bubble measure. Under the null hypothesis of no conditional heteroscedasticity, the $T \cdot R^2$ from this regression is asymptotically distributed χ^2_{s-1}, where s is the number of explanatory variables in X_t including the constant.

The F statistic is from the regression of excess returns on a constant and six bubble proxies and is distributed F(6,N-7) under the null.

Spearman's Z is a standard normal test statistic based on Spearman's rank correlation coefficient. The figure reported is the highest test statistic for the six bubble proxies individually tested. The critical values of the normal distribution were adjusted accordingly in calculating the significance level. Reported significance levels are for a two-tailed test.

TABLE 4 - Tests of the Switching Hypothesis

Currency	Switching versus Varying Premium	Switching versus Cond. Heterosced.	Switching versus Normal Mixture
Forward Data:			
Canada	78.72‡	31.89‡	49.99‡
F.R.G.	158.55‡	64.61‡	96.00‡
France	57.90‡	15.97	23.75
Italy	76.85‡	36.80‡	60.34‡
Japan	90.94‡	55.36‡	77.32‡
Switzerland	136.23‡	41.07‡	56.64‡
U.K.	139.02‡	59.21‡	77.36‡
New York Survey Data:			
F.R.G.	39.38	26.10†	40.46‡
Japan	65.13‡	42.36‡	54.77‡
Swiss	61.20‡	23.85†	34.80‡
U.K.	75.01‡	47.60‡	50.74‡
London Survey Data:			
F.R.G.	45.33†	45.33‡	50.88‡
Japan	40.27†	32.60†	33.67†
Switzerland	19.68	19.68†	22.58†
U.K.	37.79	26.81†	35.33‡

Critical Values: *(500 Observations.)*

10%	41.6	19.81	25.99
5%	45.50	22.36	28.87
1%	55.4	27.69	34.81

Note: Swiss results have fewer degress of freedom, and therefore lower critical values.

TABLE 5 - Switching Regression Estimates
Japan Forward Rate Data

483 Obs. from 22/09/78 to 18/12/87

	Regime 1	Regime 2	Classifying
Parameter Estimates:			
Constant	0.0895	0.1636	64.4013‡
	(0.0969)	(0.1517)	(22.5432)
Bubble 1A	0.0034	0.0279†	5.0037†
	(0.0100)	(0.0141)	(2.2044)
Bubble 1B	-0.0087	-0.0106	-4.8508‡
	(0.0061)	(0.0069)	(1.4156)
Bubble 2A	-0.0340	0.0874‡	-8.5471†
	(0.0302)	(0.0270)	(4.9330)
Bubble 2B	-0.0509‡	0.0549‡	-5.3692
	(0.0197)	(0.0209)	(3.9618)
Bubble 3A	0.0038	0.0483‡	2.1343†
	(0.0058)	(0.0073)	(1.0995)
Bubble 3B	0.0979†	-0.2236‡	6.4919
	(0.0453)	(0.0472)	(8.2360)
Sqrt. of Var.	0.0080‡	0.0158‡	1.0000
	(0.0007)	(0.0007)	
R-Squared:	0.2640	0.2802	
Overall R^2:			0.2401
Durbin-Watson Statistic:			1.8772
Ljung-Box Q Statistic:			25.2315
DoF:			18

† ≡ Significance < 10% (< 5% for Durbin-Watson) ‡ ≡ Significance < 1%
Figures in parentheses are standard errors.

TABLE 5 - Switching Regression Estimates - Cont.
Japan New York Survey Data

204 Obs. from 17/11/82 to 18/12/87

Parameter Estimates:	Regime 1	Regime 2	Classifying
Constant	0.2158	-0.0916	-292.2837
	(0.5511)	(0.1729)	(180.0223)
Bubble 1A	-0.0402	-0.0020	-53.6840†
	(0.0405)	(0.0201)	(26.5925)
Bubble 1B	0.0243	0.0124	33.5259†
	(0.0341)	(0.0106)	(16.8394)
Bubble 2A	-0.2005	-0.0243	-7.4917
	(0.2157)	(0.0693)	(23.9906)
Bubble 2B	0.0174	0.0291	-76.2942†
	(0.1095)	(0.0493)	(45.3328)
Bubble 3A	-0.0714‡	0.0073	-12.5617
	(0.0186)	(0.0114)	(9.2067)
Bubble 3B	0.1734	-0.0358	178.0589†
	(0.2571)	(0.1105)	(97.9096)
Sqrt. of Var.	0.0182‡	0.0120‡	1.000
	(0.0016)	(0.0008)	
R-Squared:	0.44559	0.24489	
Overall R^2:			0.3443
Durbin-Watson Statistic:			1.8716
Ljung-Box Q Statistic:			14.8002
DoF:			13

† ≡ Significance < 10% (< 5% for Durbin-Watson) ‡ ≡ Significance < 1%
Figures in parentheses are standard errors.

TABLE D1 - Critical Values for χ^2 and Empirical Distributions

Distribution	10%	5%	1%
$\chi^2(4)$	7.779	9.488	13.277
Wolfe's χ^2 (100 Obs.)	7.468	9.108	12.746
Mixed Normal - 100 Obs.	7.26	9.58	13.1
500 Obs.	7.54	9.64	14.5
$\chi^2(25)$	34.382	37.652	44.314
$\chi^2(27)$	36.741	40.113	46.963
Wolfe's χ^2 (100 Obs.)	19.168	21.553	26.518
Switch. Regress. - 100 Obs.	39.6	43.82	55.7
500 Obs.	41.6	45.50	55.4

TABLE D2 - Value of the Log Likelihood Function

Model:	Constant Risk Prem.	Time-Vary. Risk Prem.	Cond. Heterscd.	Switching Regress.	Normal Mixture
Currency:					
Forward Data					
Canada	2848.981	2856.830	2880.243	2896.188	2871.192
F.R.G.	2079.151	2087.839	2134.812	2167.114	2119.114
France	1145.378	1149.850	1170.818	1178.800	1166.927
Italy	1347.990	1357.026	1377.047	1395.449	1365.280
Japan	1343.735	1359.077	1379.868	1404.548	1365.888
Switzerland	1945.586	1952.283	1999.867	2020.401	1992.079
U.K.	1921.684	1930.416	1970.324	1999.928	1961.246
New York Survey Data					
F.R.G.	508.828	515.081	521.719	534.771	514.538
Japan	531.417	540.284	551.674	572.851	545.465
Switzerland	451.734	456.048	474.721	486.647	469.246
U.K.	504.111	511.461	525.165	548.964	523.595
London Survey Data					
F.R.G.	333.480	337.652	337.654	360.320	334.879
Japan	361.844	363.720	367.555	383.856	367.023
Switzerland	322.615	325.079	325.081	334.920	323.632
U.K.	326.566	330.348	335.839	349.244	331.579

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FIGURE 1

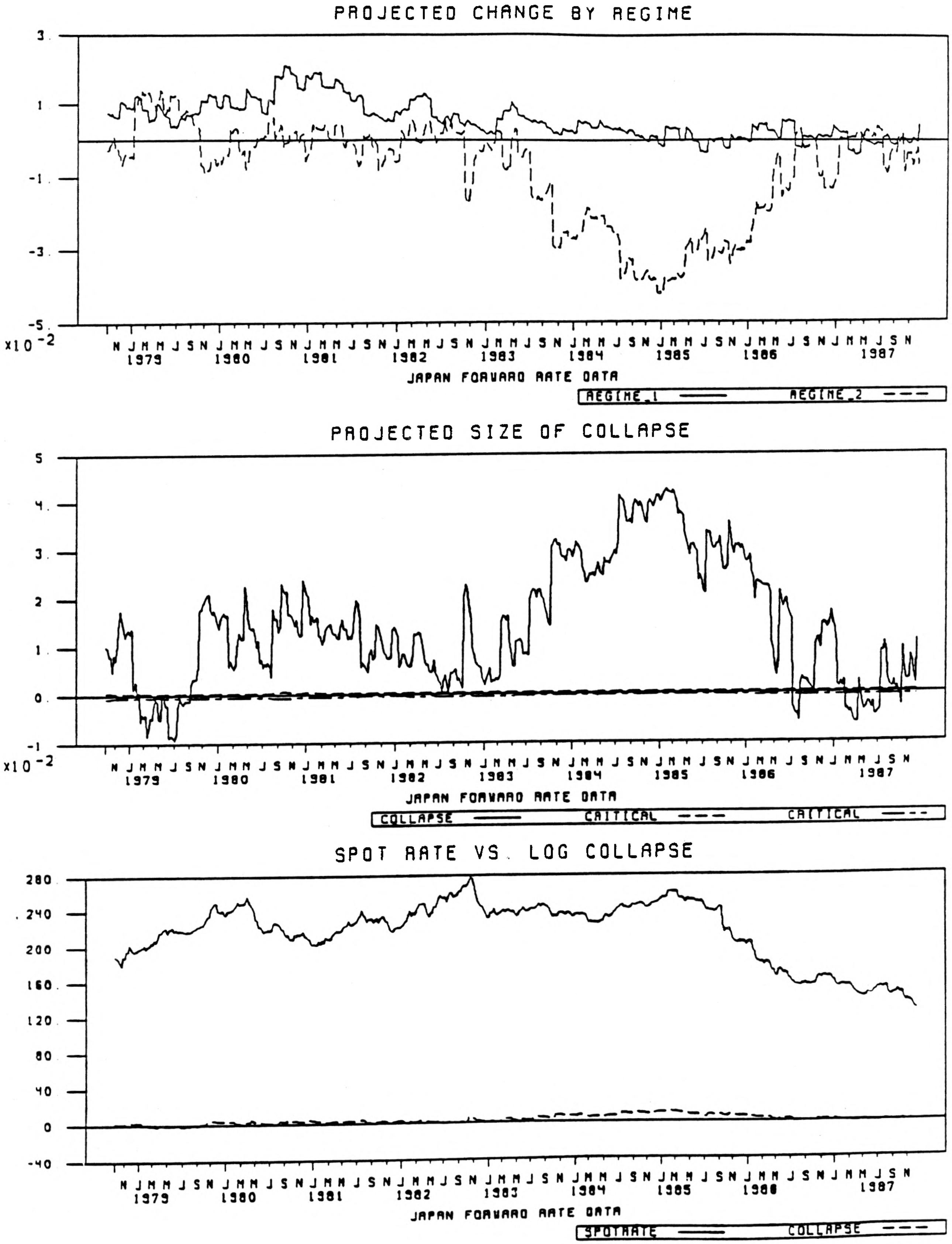
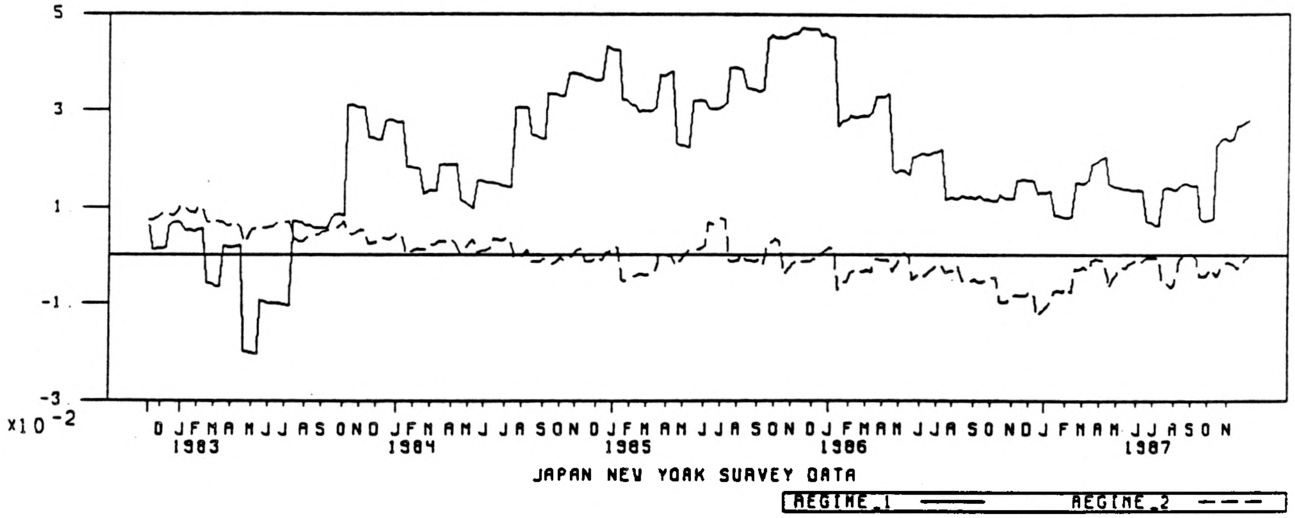
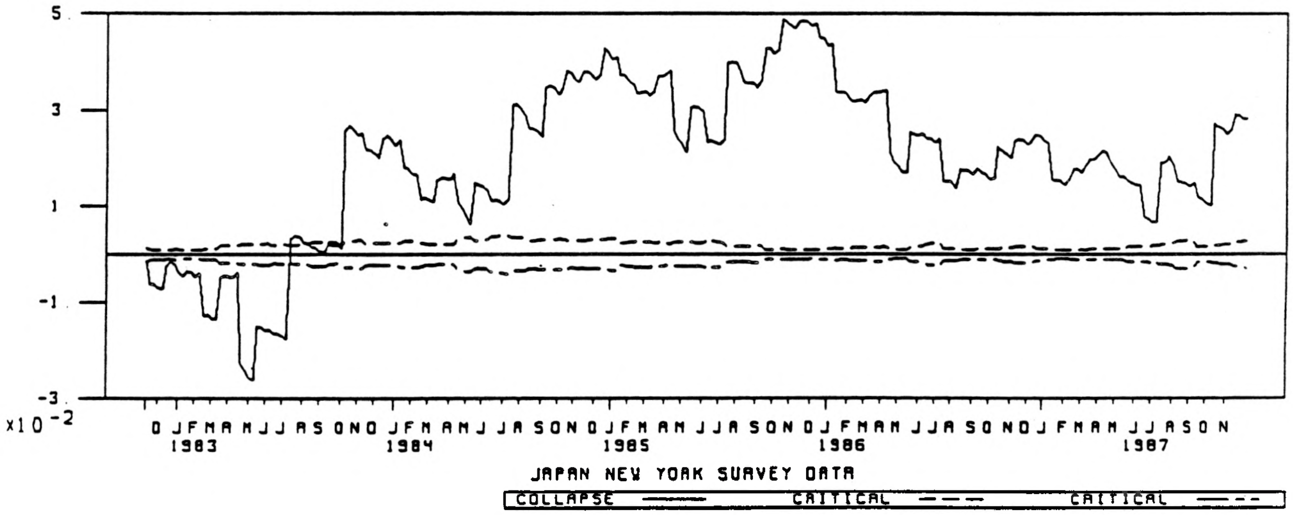

PROJECTED CHANGE BY REGIME
3
1
-1
-3
-5
x10 -2
N J M M J S N J M M J S N J M M J S N J M M J S N J M M J S N J M M J S N J M M J S N J M M J S N J M M J S N
1979 1980 1981 1982 1983 1984 1985 1986 1987
JAPAN FORWARD RATE DATA
REGIME_1
REGIME_2
PROJECTED SIZE OF COLLAPSE
5
4
3
2
1
0
-1
x10 -2
N J M M J S N J M M J S N J M M J S N J M M J S N J M M J S N J M M J S N J M M J S N J M M J S N J M M J S N
1979 1980 1981 1982 1983 1984 1985 1986 1987
JAPAN FORWARD RATE DATA
COLLAPSE
CRITICAL
CRITICAL
SPOT RATE VS. LOG COLLAPSE
280
240
200
160
120
80
40
0
-40
N J M M J S N J M M J S N J M M J S N J M M J S N J M M J S N J M M J S N J M M J S N J M M J S N J M M J S N
1979 1980 1981 1982 1983 1984 1985 1986 1987
JAPAN FORWARD RATE DATA
SPOTRATE
COLLAPSE

FIGURE 2

PROJECTED CHANGE BY REGIME



PROJECTED SIZE OF COLLAPSE



SPOT RATE VS LOG COLLAPSE

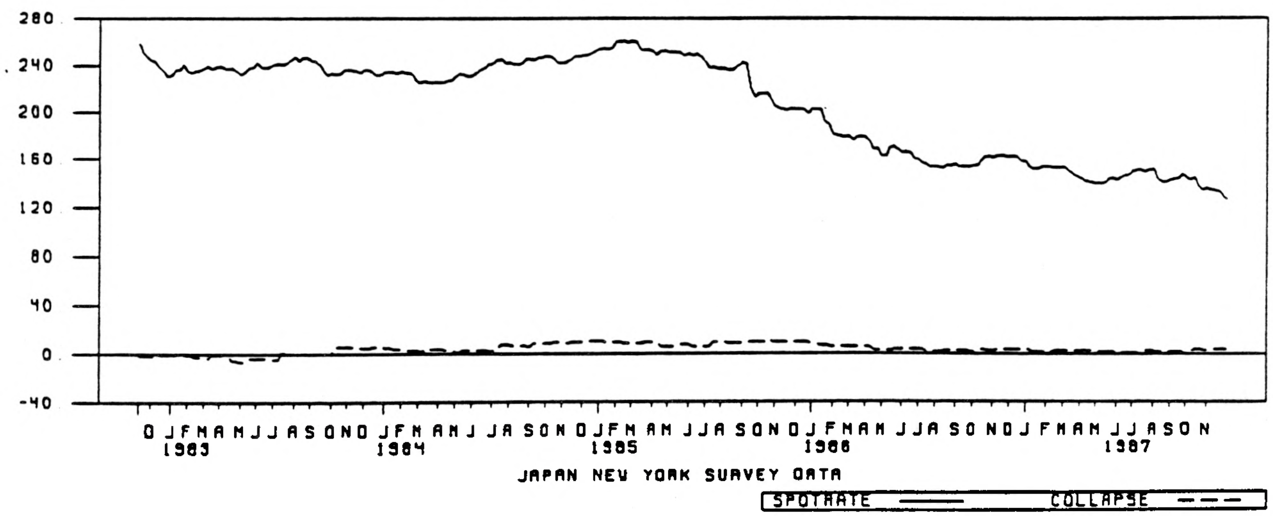
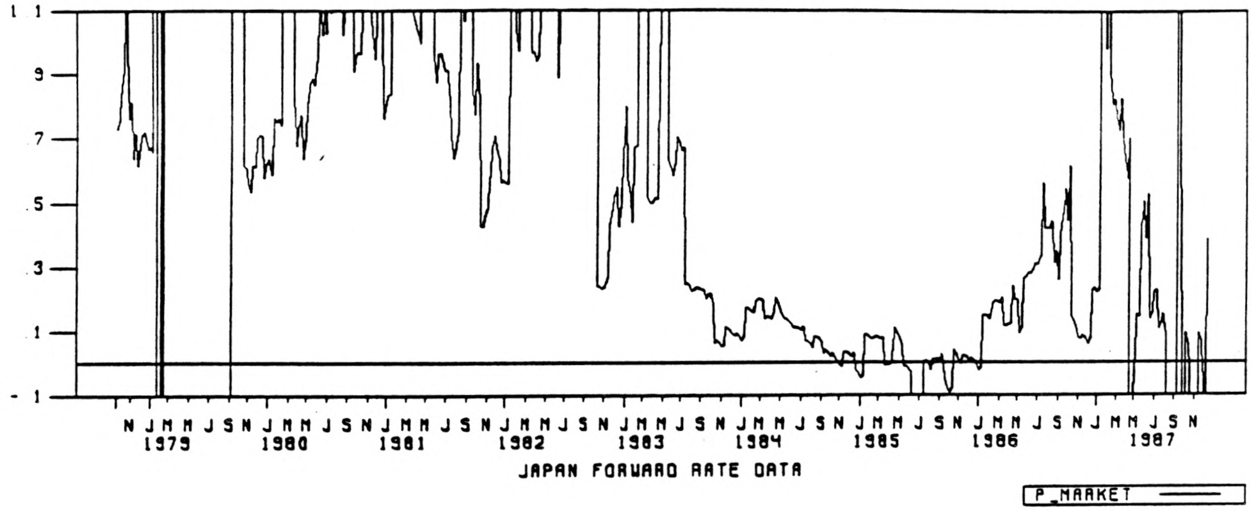
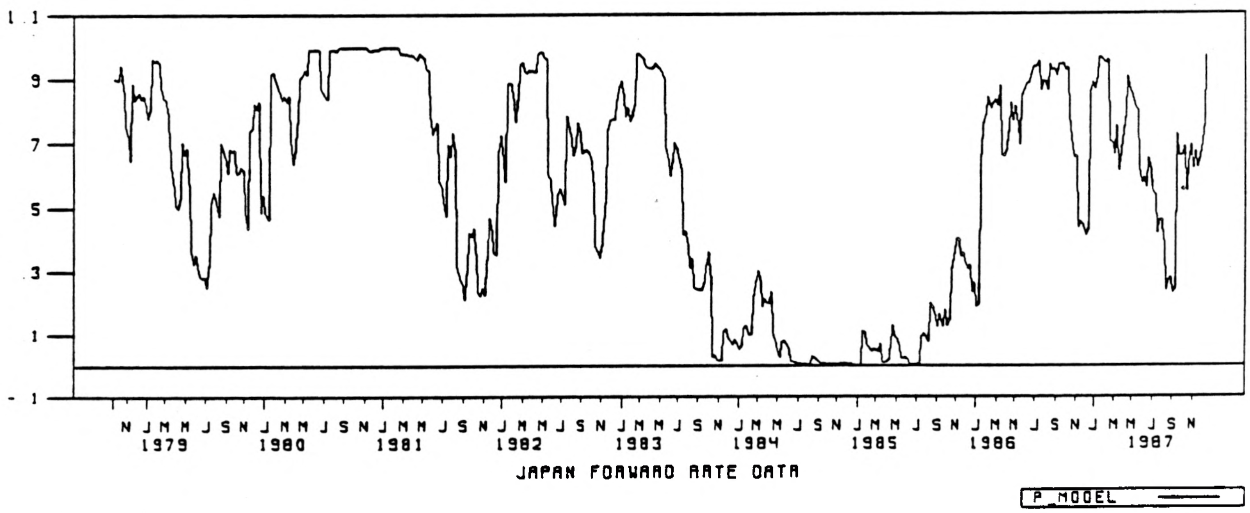


FIGURE 3

MARKET PERCEIVED PROB. OF COLLAPSE



CALCULATED EX ANTE PROB. OF COLLAPSE



CALCULATED EX POST PROB. OF COLLAPSE

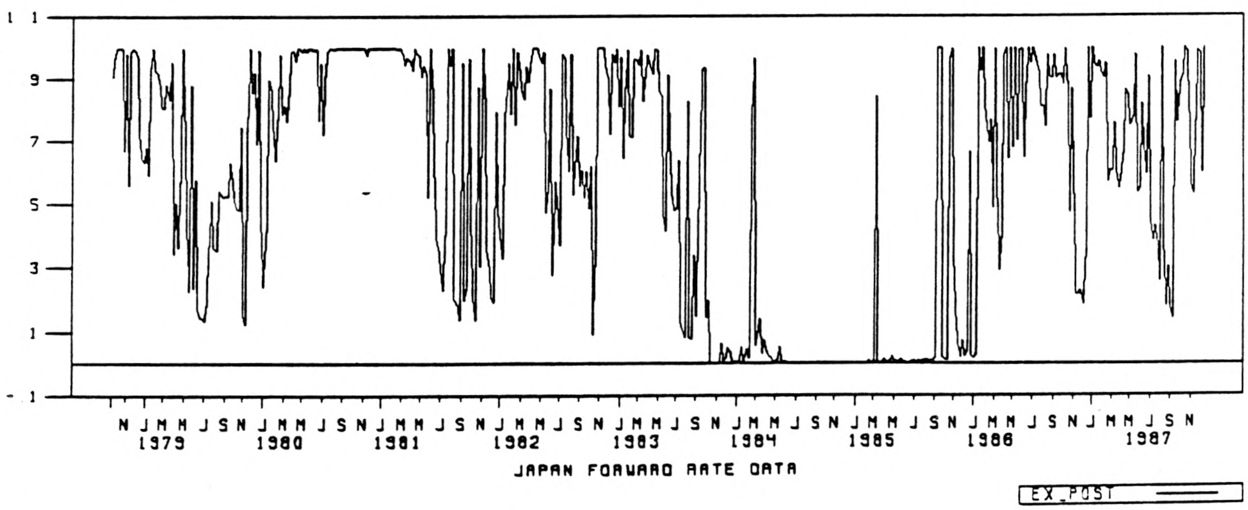
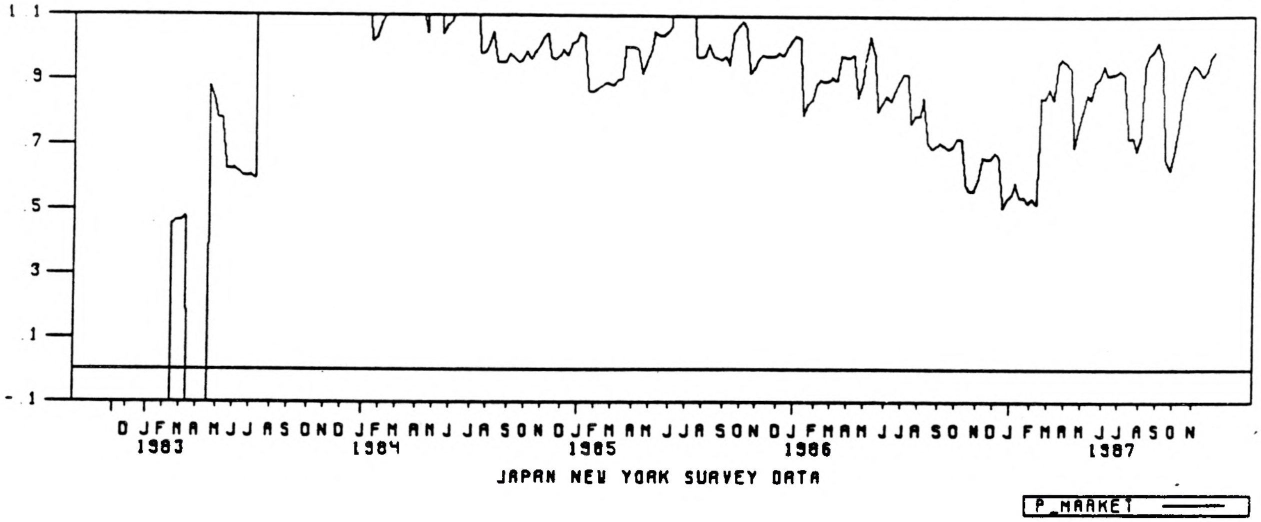
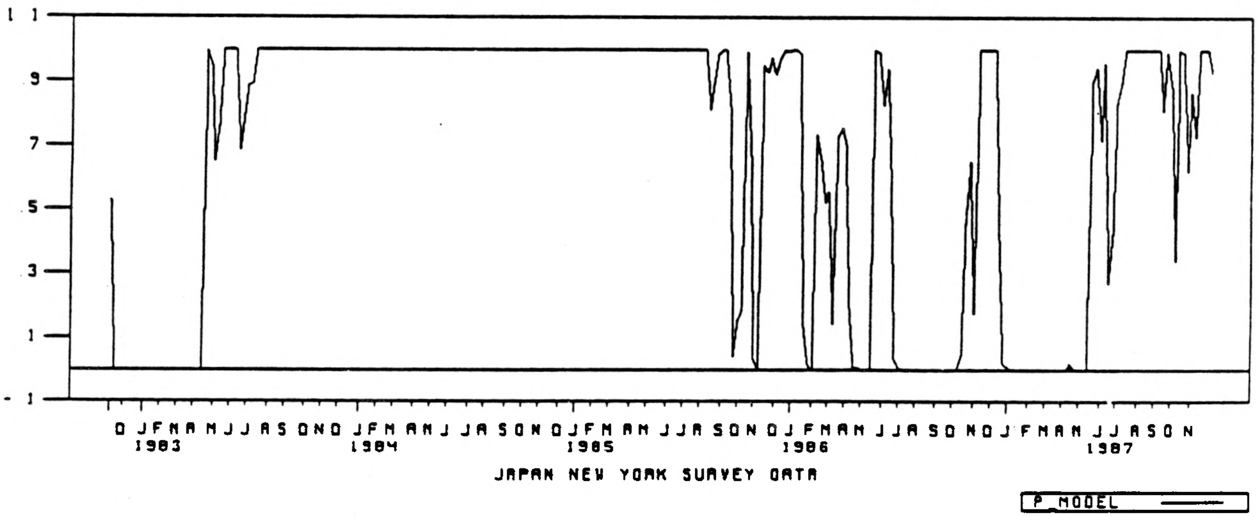


FIGURE 4

MARKET PERCEIVED PROB. OF COLLAPSE



CALCULATED EX ANTE PROB. OF COLLAPSE



CALCULATED EX POST PROB. OF COLLAPSE

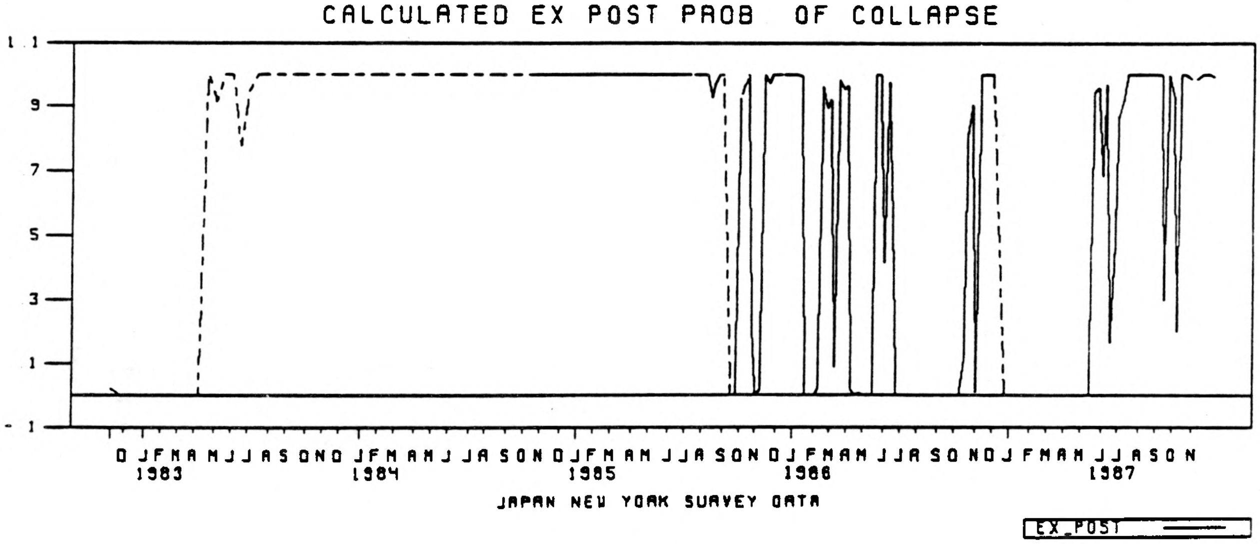
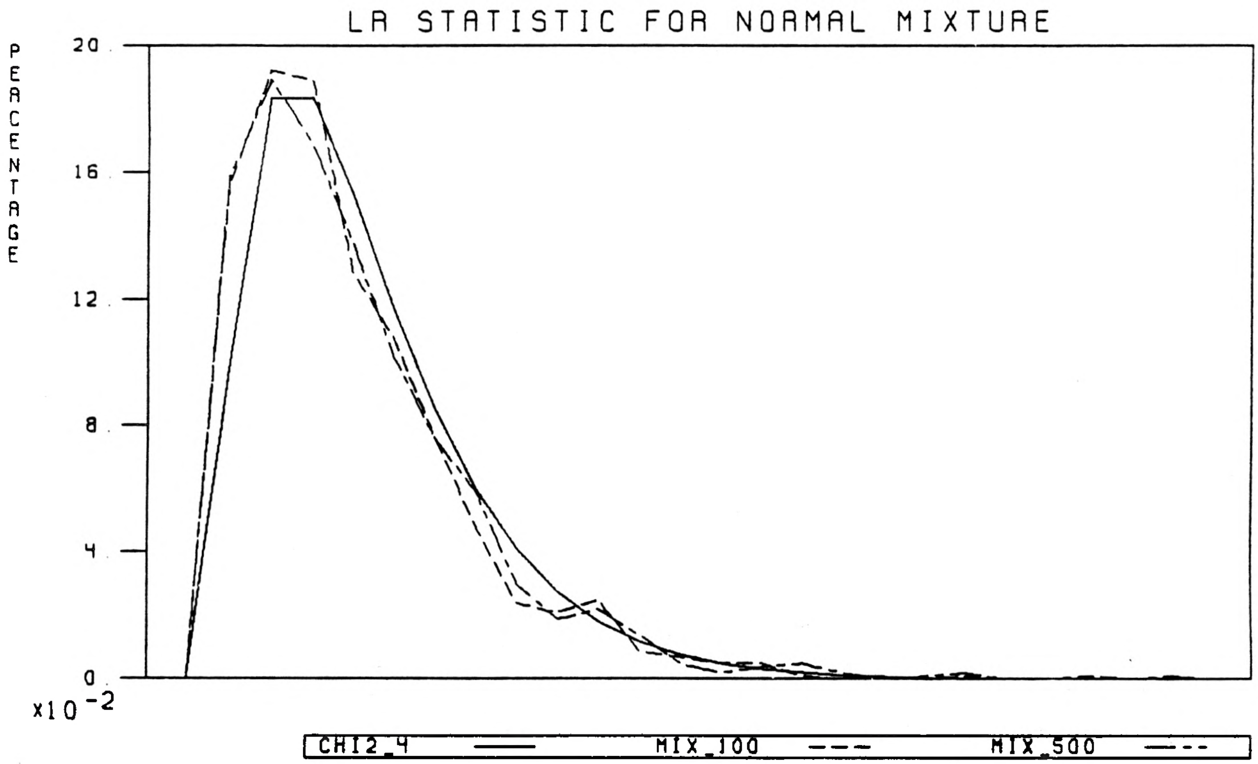
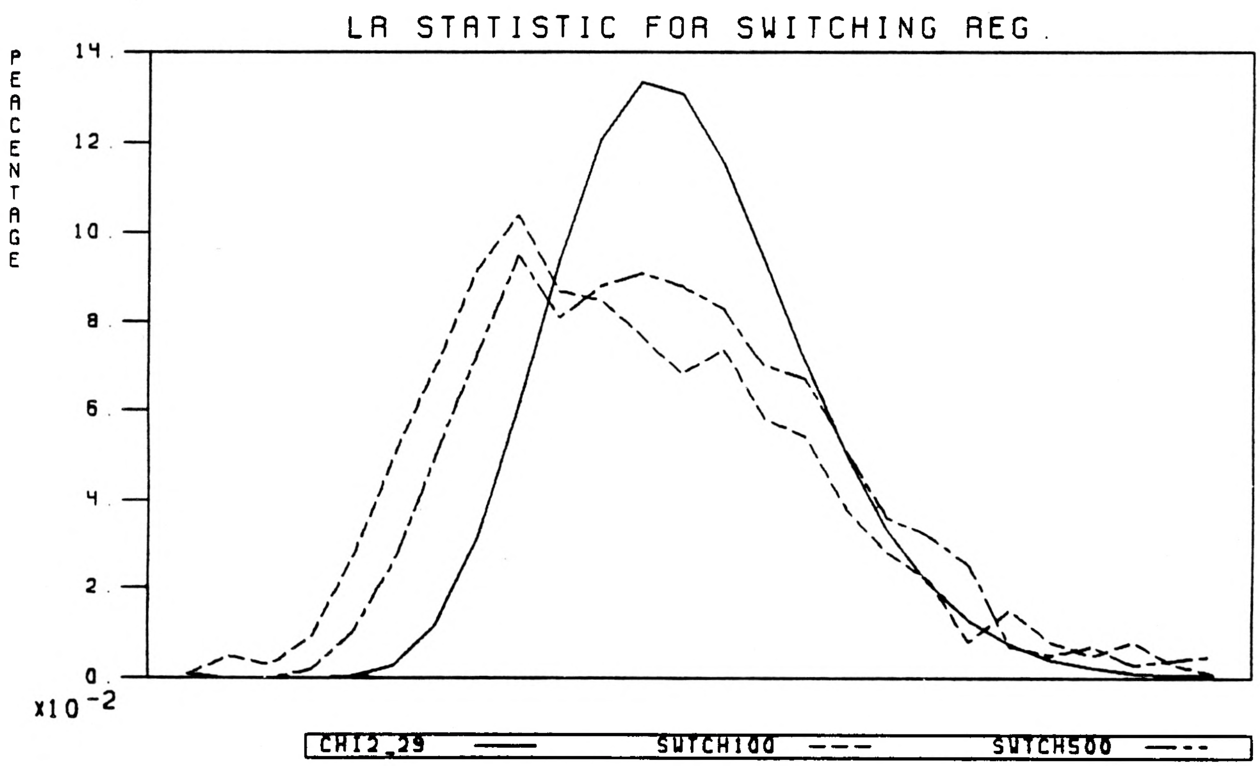


FIGURE 5


LR STATISTIC FOR NORMAL MIXTURE
PERCENTAGE
20
16
12
8
4
0
x10 -2
CHI2_4
MIX_100
MIX_500


LR STATISTIC FOR SWITCHING REG.
PERCENTAGE
14
12
10
8
6
4
2
0
x10 -2
CHI2_29
SWTCH100
SWTCH500

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