EXCHANGE RATE STABILITY IN THE NINETEEN-THIRTIES

BRITAIN, NORTH AMERICA AND THE GOLD BLOC

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CONTENTS

Tables and Diagrams ii
Abbreviations iv
Declaration v
Summary vi

SECTION I: INTRODUCTION
Chapter 1: Introduction 1
Chapter 2: The Purchasing Power Parity Hypothesis and Other Theories of Exchange Rate Determination 10
Chapter 3: Empirical Studies of Exchange Rate Determination 59

SECTION II: BILATERAL EXCHANGE RATES
Chapter 4: A Model of Exchange Rate Determination 83
Appendix: Official Exchange Rate Intervention 123
Chapter 5: The Pound-Dollar-Franc Triangle 136
Chapter 7: The Canadian Dollar 272
Chapter 8: Preliminary Conclusions 305

SECTION III: MULTILATERAL EXCHANGE RATES
Chapter 9: The Methodology of Effective Exchange Rates 323
Appendix: The IMF's Multilateral Exchange Rate Model (MERM) 340
Chapter 10: Effective Exchange Rates for the 1930's and Some Applications 347
Chapter 11: Effective Exchange Rates and Stability 370

SECTION IV: CONCLUSIONS
Chapter 12: Summary and Conclusions 406
APPENDIX I: Details of the Constructed Variables 416
APPENDIX II: Data Sources 421
APPENDIX III: Effective Exchange Rates: Methods of Construction and Data 422

Bibliography 458
TABLES AND DIAGRAMS

Figure 5.1 : Pound-Dollar-Franc Exchange Rates (July, 1931-December, 1938) 137
Table 5.1 : British Speculative Dummies 144
Table 5.2 : American Speculative Dummies 150
Table 5.3 : French Speculative Dummies 157
Table 5.4 : The Pound-Dollar Final Estimates 161
Figure 5.2 : Estimated Seasonal Variation in the Pound-Dollar 164
Table 5.5 : The Pound-Franc Final Estimates 168
Figure 5.3 : Estimated Seasonal Variation in the Pound-Franc 171
Table 5.6 : The Dollar-Franc Final Estimates 175
Figure 5.4 : Estimated Seasonal Variation in the Dollar-Franc 178
Table 5.7 : Summary of the Performance of the Speculative Dummies 186
Table 5.8 : A Summary of the Performance of "Economic Fundamentals" Appearing in the Estimates of More than One Exchange Rate 188
Table 5.9 : Summary of Large Residuals in the Estimates of the Pound-Dollar-Franc Triangle 192
Table 6.1 : Belgian Speculative Dummies 220
Table 6.2 : Dutch Speculative Dummies 226
Table 6.3 : Swiss Speculative Dummies 231
Table 6.4A : Belga-Pound Final Estimates 234
Table 6.4B : Belga-Dollar Final Estimates 235
Table 6.4C : Belga-Franc Final Estimates 236
Table 6.5A : Guilder-Pound Final Estimates 246
Table 6.5B : Guilder-Dollar Final Estimates 247
Table 6.5C : Guilder-Franc Final Estimates 248
Table 6.6A : Swiss Franc-Pound Final Estimates 256
Table 6.6B : Swiss Franc-Dollar Final Estimates 257
Table 6.6C : Swiss Franc-French Franc Final Estimates 258
Table 7.1 : Percentage Shares of Total Canadian Trade 272
Table 7.2 : Canadian Trade Policy Dummies 281
Table 7.3 : Canadian Speculative Dummies 286
Table 7.4 : Canadian Dollar-Pound: Final Estimates 288
Table 7.5 : Canadian Dollar-U.S. Dollar: Final Estimates 294
Table 7.6 : Canadian Dollar Residuals 297
Table 8.1 : Summary of the Performance of the Major Speculative Dummies Across All Exchange Rates 314
Table 9.1 : Percentage Change in Exchange Rates (January, 1931 to January, 1936) 325
<table>
<thead>
<tr>
<th>Abbreviation</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>COL</td>
<td>Cost of Living (indices)</td>
</tr>
<tr>
<td>CPI</td>
<td>Consumer Price Index</td>
</tr>
<tr>
<td>CORC</td>
<td>Cochrane-Orcutt (Estimation Technique)</td>
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<tr>
<td>EEA</td>
<td>Exchange Equalisation Account</td>
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<td>EER</td>
<td>Effective Exchange Rate</td>
</tr>
<tr>
<td>ID</td>
<td>(Official) Intervention Dummy</td>
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<tr>
<td>MERM</td>
<td>Multilateral Exchange Rate Model</td>
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<tr>
<td>OLSQ</td>
<td>Ordinary Least Squares</td>
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<td>PPP</td>
<td>Purchasing Power Parity</td>
</tr>
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<td>SV</td>
<td>Speculative Variable</td>
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<tr>
<td>ROW</td>
<td>Rest of the World</td>
</tr>
<tr>
<td>WG</td>
<td>Wage (indices)</td>
</tr>
<tr>
<td>WP</td>
<td>Wholesale Prices</td>
</tr>
<tr>
<td>WPI</td>
<td>Wholesale Price Index</td>
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<tr>
<td>WW1</td>
<td>World War I</td>
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DECLARATION

The material contained in Chapters 9 and 10 has been used in the following:

(1) An Effective Exchange Rate for the Pound in the 1930's (Warwick Economic Research Papers, No. 123, Department of Economics, University of Warwick, November, 1977).

(2) An Indicator of the Effective Exchange Rate of the Pound in the Nineteen-Thirties (Economic History Review, February, 1980).

SUMMARY

The starting point of the thesis is the contention of Nurkse (1944) that exchange rates in the 1930's were unstable and subject to destabilising speculation. This view has been widely accepted and the few studies that have examined the issue since tend to be obscure and limited in scope. The principal objective of the present study is to provide a thorough (mainly econometric) test of the counter-hypothesis that exchange rates were, in fact, determined by "economic fundamentals" (particularly relative prices, incomes and interest rate differentials) in this period. Consequently, a model of exchange rate determination is developed and applied to the currencies of Britain, America, Canada, France, Belgium, Holland and Switzerland, both bilaterally and multilaterally.

The calculation of multilateral (or effective) exchange rates for the 1930's was considered a useful exercise in itself since they did not appear to be available. It seemed, therefore, desirable to digress and examine the methodology of their construction and to consider some general implications of referring to multilateral (instead of bilateral) exchange rates in the 1930's, before using them to test the central hypothesis multilaterally.

The main conclusion of the thesis is that, to a large extent, exchange rates were indeed determined by "economic fundamentals" and were not distorted by persistent destabilising speculation (the Nurkse view), although the latter was important occasionally. The evidence was stronger for bilateral than multilateral rates; however, this probably reflects the limited nature of the tests involving the latter. Finally, where there was sufficient information for an adequate test, official intervention was also found to be important and, more generally, it was argued that any instability was probably due more to government intervention than to destabilising speculation.
SECTION I: INTRODUCTION
The collapse of the Bretton Woods system and the consequent widespread adoption of floating exchange rates in the 1970's has created a revival of interest in earlier periods during which exchange rates were not fixed. Of particular interest in this context is the 1930's; not only is this the most recent historical episode of generalised floating but it has been put forward as a classic example of all the dangers inherent in floating exchange rates\(^{(1)}\) and this interpretation played an important role in the debate over international monetary arrangements to be established after the second world war, which ultimately produced the Bretton Woods adjustable peg system. It is therefore somewhat surprising that no systematic, quantitative study of the fluctuations of the major currencies in this period exists, although studies of individual exchange rates are now beginning to emerge. \(^{(2)}\)

The condemnation of floating exchange rates which contributed most to the accepted wisdom that has prevailed throughout much of the post-World War II period originated in the work of Nurkse (1944) and this is the starting point of the present study. Basing his conclusions on little more than casual empiricism Nurkse gave three reasons why floating rates were to be avoided:

"In the first place, they create an element of risk which tends to discourage international trade....Secondly, as a means of adjusting the balance of payments, exchange fluctuations involve constant shifts of labour and other resources between production for the home market and production for export."
Such shifts may be costly and disturbing; they tend to create frictional unemployment, and are obviously wasteful if the exchange-market conditions that call for them are temporary. Thirdly, considerable or continuous movement of the exchange rate is liable to generate anticipations of a further movement in the same direction, thus giving rise to speculative capital transfers of a disequilibrating kind. (3)

Particular stress is placed on the third argument which is reiterated at various stages in Nurkse's study, most explicitly:

"If currencies are left free to fluctuate, 'speculation'... is likely to play havoc with exchange rates." (4)

In support of this contention Nurkse draws on the experience of floating in the inter-war period. However, it is very noticeable that his most convincing examples of destabilising speculation relate largely to the 1920's - particularly the French franc in 1922-26 - and that his examples for the 1930's are very limited (confined to one page) and are rather questionable. In fact they are really assertions rather than evidence. He simply states that:

"After the breakdown of exchange stability in the 'thirties, whenever exchanges were left to their own fate, such disequilibrating capital movements...played an important part." (5)

and gives three examples:

"...the American dollar in 1933, the French franc at certain times during 1937 and the pound sterling in the first few months after September 1931". (6)

Whether these examples can justifiably be used to provide evidence of destabilising speculation under floating exchange rates is debatable. In all three cases the currencies concerned had just left the gold standard, and it is not unreasonable to argue, that such instability of exchange rates as did occur, can more correctly be attributed to the
misguided attempts to fix the exchange rate in the immediately preceding period and are therefore more properly interpreted as examples of the disadvantages of fixed exchange rates. Nurkse's view, that the presence of destabilising speculation in the 1930's illustrates a major drawback of floating exchange rates, would thus appear to rest on rather flimsy evidence and would seem to be more an assertion than an empirically supported conclusion.

This point has been taken up by Aliber (1976) who argues that during times of (price) instability, such as the 1930's, fixed exchange rates cannot function. Consequently, instability in the 1930's precluded the use of fixed exchange rates and made floating inevitable. The implicit conclusion of Nurkse - that fixed rates would have worked better - is therefore incorrect and the 1930's cannot be held up as an example of a period in which this is true (although this point of view does not exactly conflict with Nurkse's argument about destabilising speculation but rather modifies it to state that floating exchange rates were made more unstable than they could have been by such speculation).

The present study attempts to take the argument one step further by examining the hypothesis that not only does the experience of the 1930's provide little support for fixed exchange rates (Aliber's point) but it does not even provide particularly good evidence that floating rates tend to be unstable. This hypothesis is to be tested by specifying a model of exchange rate determination which contains two sets of variables: "economic fundamentals" (such as relative prices), which might plausibly be expected to cause exchange rates to change; however, such changes do not imply disequilibria but simply movements from old to new equilibria; and "non-economic" or perhaps better, "speculative" variables which seek to allow for destabilising speculation of the Nurkse variety. To
the extent that the former group of variables are significant and
the latter are not, then this provides support for the hypothesis
that exchange rates were not subject to violent fluctuations caused
by destabilising speculation in the 1930's but such changes that did
occur were simply movements to new equilibria (justified by changes
in the "fundamental" economic variables which determined exchange
rates).

(II)

At this stage something needs to be said about exactly what is
meant by exchange rate "equilibrium" and the associated concept of
exchange rate "stability". In common with many others, Nurkse often
seems to implicitly define stability as "lack of movement"; indeed, he
addresses himself to "the question of fluctuating versus stable exchanges".\(^{(8)}\)
However, whether stability and lack of variation amount to the same
thing is debatable. The dilemma is succinctly summed up by Viner:

"...one economist's 'stability' may be another one's
'rigidity' and....one economist's 'instability' may be
another one's 'flexibility'...." \(^{(9)}\)

In fact a floating exchange rate which changes gradually according to
the underlying trend may be construed as more "stable" than a fixed
rate with periodic, discontinuous (and comparatively large) changes
to new fixed levels.

This is because an exchange rate can only really be stable if it
clears the market and equates supply and demand for foreign currency
because only then will there be no pressure on the exchange rate to
change; the exchange rate that does this is the equilibrium rate and, in fact, any attempt to peg the rate (excepting a fortuitous accident) must lead to disequilibrium. Indeed this is the explicit (as opposed to the seemingly implicit) definition adopted by Nurkse:

"...the equilibrium rate of exchange...is the rate which, over a certain period, maintains the balance of payments in equilibrium without any net change in the international currency reserve". (10)

This is also the position adopted here. Such a definition would make the exchange rate a function of the variables that influence demand and supply for foreign currency and insofar as these variables change then the equilibrium exchange rate must change also. These variables are the "economic fundamentals" referred to earlier and are specified in detail in chapter four. Consequently, the equilibrium exchange rate is seen as constantly changing as these "economic fundamentals" change and, to the extent that exchange rates were determined by these variables in the 1930's (which is the hypothesis tested in the present study), then exchange rates were "stable". The concept of exchange rate equilibrium receives further attention in the next chapter.

However, even if Friedman's dictum - that "stability is not rigidity" (11) - were accepted, there would still be some (presumably including Nurkse) who would argue that floating exchange rates still change more than they ought to (for example, in the 1930's) and are consequently "unstable" (by whatever definition) because of destabilising speculation. The present study also addresses itself to this question. A theoretical debate has raged over this matter for some years beginning with Friedman's well-known contention that speculation can only be destabilising if speculators, on the average, lost money. (12) The subsequent contributions mainly consisted of attempts to devise examples
in which speculation could be both destabilising and profitable\(^{(13)}\) and the debate was summarized by Sohmen (1969) who concluded that destabilising speculation was unlikely to be profitable.\(^{(14)}\) However, the lack of convincing theoretical examples of destabilising and profitable speculation is not in itself conclusive and, perhaps Baumol's position is the most reasonable:

"I believe...that the effects of profitable speculation on stability is in part an empirical question and that attempts to settle it by a priori arguments must somewhere resort to fallacy". \(^{(15)}\)

This is very much the view adopted here and attempts to explicitly model destabilising (or otherwise) speculation are discussed in chapter four.

Moreover, this position is reinforced by recognition of an important implicit assumption underlying the above debate (including Friedman's initial contribution) which is that speculators, or at least the majority of them, are successful in that they make a profit. However, it may well be that speculators sometimes make mistakes, and whilst this cannot go on indefinitely (unless there is a continuous supply of new, inept speculators), it may still go on long enough to exert a destabilising influence on exchange rates; this is especially true in periods of uncertainty, during which speculators may be as uncertain as everyone else. Indeed Sohmen uses this explanation as a justification of the interpretation of the 1930's as a period of destabilising speculation:

"It should not be too surprising that speculators' anticipations should often have turned out to be incorrect under the chaotic conditions of the depression years when even policymakers frequently had no idea what to do next". \(^{(16)}\)
The empirical tests to be undertaken in the present study should shed some light on this contention.

(III)

The main objective of the thesis is therefore, having defined stability, to test the hypothesis that exchange rates were stable in the 1930's whilst, at the same time, testing for the presence of destabilising speculation (in an explicit rather than implicit way). Since stability has been defined in terms of dependency of the exchange rate on "economic fundamentals", an attempt is made to identify the latter and develop a simple model of exchange rate determination. What eventually emerges is basically an extended purchasing power parity hypothesis. An additional point of interest relates to the fact that the 1930's saw the establishment of exchange intervention funds to "manage" exchange rates, first of all in Britain, but later in many other countries; since it is obviously necessary to include official intervention in the model, then an examination of this development is a lesser objective of the study.

The rest of this section is devoted to two surveys, the first of which (Ch. 2) examines various theories of exchange rate determination, concentrating on the purchasing power parity hypothesis, whilst the second (Ch. 3) looks at the relevant empirical evidence; this involves examining comparable studies - that is, those which use similar methodology - and also some rather dissimilar approaches, giving particular attention to those relating to earlier periods of floating (notably the inter-war period and the Canadian float in the 1950's). On the
basis of these surveys, a model of exchange rate determination is developed in chapter four, the opening chapter in Section II, which concentrates on bi-lateral exchange rates. The model is then tested for a variety of bi-lateral rates involving the three major world currencies at that time - the pound, dollar and (French) franc - and four "minor" currencies - those of Canada, Belgium, Switzerland and Holland and some preliminary conclusions are drawn (Chs. 5-8).

Section III approaches exchange rate stability in the 1930's from a hitherto unexplored angle by applying the concept of multi-lateral or effective exchange rates (developed in the 1970's). This involves examining the methodology of effective rates (Ch. 9); effective rates are then calculated for the seven currencies included in the study and some general observations are made about their behaviour compared to the more conventional bi-lateral rates (Ch. 10); finally, a crude test of purchasing power parity is then carried out which requires the calculation of comparable multilateral prices indices (Ch. 11). Section IV draws together the two main threads of the thesis and attempts to come to some conclusions about the stability of both bi-lateral and multilateral exchange rates in the 1930's. Some tentative conclusions are also suggested relating to official intervention in the 1930's, the "pure" purchasing power parity hypothesis and the role of speculative influences on exchange rates (and consequent destabilising speculation) in this period.
NOTES TO CHAPTER 1

1. The standard (and one of the earliest) references which made this assertion is Nurksee (1944), which is discussed in detail below.

2. See Hudgins (1973), Ozmun (1976) and Ridpath (1975). This deficiency of empirical studies is in sharp contrast to the early 1920's which seem to have attracted much more attention. See, for example Aliber (1962), Frenkel (1960a), Grissia (1967), Hodgson (1971) (1972), Hodgson and Phelps (1975), Stolper (1948), Thomas (1973a) (1973b), and Tsaiang (1959).


5. Ibid, p. 118.

6. Ibid.


CHAPTER 2

THE PURCHASING POWER PARITY HYPOTHESIS AND
OTHER THEORIES OF EXCHANGE RATE DETERMINATION

The purchasing power parity (PPP) hypothesis represents one of the oldest theories of exchange rate determination; its development, at least in modern times is normally attributed to Cassel (1918, 1922). The model developed and tested in the present study is very much in the spirit of this hypothesis (if it does not quite follow the letter) and, as already indicated, it can (in many ways) be considered as an extended version of the PPP hypothesis. This choice of the PPP hypothesis as the starting point would seem to be justified on the grounds that few theorists (of whatever persuasion) would completely discount the potential role of relative prices. It is probably quite true that "under the skin of any international economist lies a deep-seated belief in some variant of the PPP theory of the exchange rate". Nevertheless some attention must be paid to the development of new theories of exchange rate determination as well as the readoption of old ones that has accompanied the revival of interest in the subject associated with the recent widespread adoption of floating exchange rates.

As a consequence of this, it soon becomes very clear that an eclectic approach to exchange rate determination is likely to be most productive and so an attempt is made below to incorporate elements of newer (and older) theories into the model to be tested here. Therefore, a brief survey of the theory of PPP is followed by a survey of alternative approaches, highlighting those aspects that might usefully be
added to extend the PPP hypothesis. The problems relating to testing PPP are then discussed in detail on the grounds that, just as a simple PPP test would have to deal with these problems, so will an extended version.

It should be pointed out that the objectives of much of the more recent work and those of the present study are rather different. The former are usually concerned with either predicting the future course of exchange rates or explaining recent exchange rate movements which have, to some extent, been rather more erratic than might have been expected in the light of the debate over fixed and floating exchange rates in the 1950's and 1960's. In connection with this second aim it should be remembered that the 1970's environment is somewhat different to that of the 1930's. The degree of complexity of the international financial and commodity markets has much increased and various elements – the eurocurrency market being one of the most striking examples – simply did not exist in the 1930's. Nor were there so many important agents operating in the 1930's – clearly there was no OPEC and the role of Japan (and the yen) was minimal.

A similar comparison has been made by Frenkel (1980b) who compares the 1970's to the 1920's and much of what he says is equally applicable to a comparison between the 1970's and 1930's. In particular, he highlights a number of important developments in the post-World War 2 period: the greater integration of world capital markets, the greater role of "real" shocks and their effects on expectations, changes in tariffs and non-tariff barriers to trade, the development of exchange rate management and, of course the creation of the International Monetary Fund. These differences may justify a simpler approach for the 1930's. Furthermore, returning to the objective of the present study, this too
is rather simpler and in its starkest terms mainly relates to an examination and test of the hypothesis that movements in exchange rates were determined rather more by changes in "fundamental economic variables" (and therefore did not represent disequilibrium but movements from old equilibria to new ones) than is commonly supposed.

(1)

The theory of PPP has a long and distinguished history dating at least as far back as Ricardo and Wheatley. (2) Interest in more modern times has been generated largely by the work of Cassel (1918, 1922) (3) in the inter-war period, who produced a more systematic version of the theory and introduced some numerical content, and its more recent exponents have included Yeager (1958, 1976) and Thygesen (1978). Nor has the theory been short of critics both in the times of Cassel (4) and in more recent years. (5) In fact the debate over the validity of the PPP has been one of the most long-standing controversies in economics. The fact that there are a variety of different versions of the theory (and various extended versions like that developed here) has fuelled the controversy and led to some confusion over what exactly a particular author is rejecting or accepting. Moreover, there are those who would agree that prices and exchange rates move together over time but who, instead of accepting the PPP interpretation - that changes in prices lead to changes in exchange rates - would either argue in favour of what might be called "reverse causality" (exchange rate changes lead to price changes) (6) or simply that prices and exchange rates are both endogenous and are affected in the same way by a variety of (exogenous) other variables. (7)
The basic premise of the PPP hypothesis is perhaps best stated by Cassel himself:

"...the rate of exchange between two countries is primarily determined by the quotient between the internal purchasing power against goods of the money of each country... At every moment the real parity between two countries is represented by this quotient... I propose to call this parity the "purchasing power parity". " (8)

This is the theory at its simplest and some elaboration is required:

"The propositions of PPP theory are (1) that the short run equilibrium exchange rate is a function of the long run exchange rate in the sense that the former variable tends to approach the latter and (2) that the PPP is either the long-run equilibrium exchange rate or the principal determinant of it". (9)

Thus, exchange rates (both long and short run) are conceived as being ultimately determined by relative prices in some sense.

There are essentially two versions of PPP - or perhaps better "two approaches to implementing its exchange rate-calculation aspect". (10)

The "absolute" version (equation 1.1) suggests that the PPP between two countries is simply the ratio of their price levels at any given time, with the price level measured as an index with the same basket of goods and services (and consequently the same weights) for each country. The "relative" (or "comparative") version (equation 1.2) asserts that the current PPP can be calculated by comparing current price levels with those prevailing in some base period when the exchange rate was at an equilibrium level: this involves simply multiplying the base period exchange rate by the ratio of the two countries' price indices which obviously have to have the same base year as the exchange rate, and their method of construction should not have changed, but they do not have to have the same weights. Thus:
\[ \text{PPP(Ab.)}_t = \frac{p^A_t}{p^B_t} \quad (1.1) \]
\[ \text{PPP(Rel.)}_t = \left( \frac{p^A_t}{p^B_t} \right) \cdot R o \quad (1.2) \]

where \( \text{PPP(Ab.)}_t \) = absolute PPP in period \( t \) (in units of A currency per B currency).

\( \text{PPP(Rel.)}_t \) = relative PPP in period \( t \) (in units of A currency per B currency)

\( R_o \) = actual base period of exchange rate (A currency per B currency)

\( p^A(B)_t \) = an index of country A's (B's) prices in period \( t \)

In principle, there is no strong case for preferring the absolute to the relative version or vice-versa; the main difference occurs in practice where relative PPP has one big advantage, namely that direct comparison of purchasing powers requires the use of the same index in both countries containing the same standard assortment of goods and services (which must be representative in both countries) with the same weights. Clearly the construction of such indices is quite a task in itself. Relative PPP, on the other hand, simply involves looking at the degree of change in the separate (and not necessarily similarly constructed) national price indices. This is not to say that the relative version of PPP is without problems - indeed, in sidestepping one problem it creates others such as the choice of base period and so on. However such problems are more easily overcome (or at least their effects minimized) and will be discussed in a later section. Moreover, the fact remains that relative PPP has been favoured empirically (and the present study is no exception) and therefore insofar as we are talking about a particular version, it is relative PPP.
that is implicitly being discussed. However, in actual fact, this is not especially important since although some of the criticisms to be discussed can be more forcefully applied to one or other of the versions, in general, any argument which undermines use of absolute price levels as the major determinant of exchange rates will also undermine the use of changes in absolute price levels (that is relative PPP).

The main mechanism by which changes in relative prices are transmitted to exchange rates is commodity arbitrage. This is described by Yeager:

"Suppose....that prevailing exchange rates unmistakably undervalue the British pound in relation to the purchasing powers of the pound and of foreign currencies. Foreigners - say Americans - will offer dollars for pounds to buy British goods at bargain prices. Britons will offer relatively few pounds for dollars to buy American goods at their apparently high prices. Unmatched attempts to sell dollars and buy pounds will bid the exchange rate toward the equilibrium level. The converse analysis applied if the pound is overvalued." (11)

The fact that this arbitrage is not instantaneous due to various lags and market imperfections is why PPP tends to be a longer run phenomenon although expectations and consequent stabilising speculation may help push rates towards their PPP level even in the short run.

This is what might be called the "naive" PPP theory. There is nothing logically wrong with it although acceptance of it rather depends on whether or not the existence of long run commodity arbitrage is also accepted. There is conflicting evidence on this: convincing evidence that this "law of one price" does not hold is provided by Kravis and Lipsey (1978) for a variety of countries and by Richardson (1978) in the Canada-U.S. case; on the other hand, a critical review of such
studies and some conflicting evidence is provided by Genberg (1978) thereby
suggesting that commodity arbitrage may be effective after all. Moreover,
there is an inherent logic in Yeager's argument that commodity arbitrage
must begin to work within certain limits; to show this he uses Heckscher's
concept of "commodity points" which are similar to gold points under
the gold standard (although they may be wider):

"Just as gold-standard exchanges fluctuate within the gold points, so paper exchanges fluctuate within 'commodity points'. Just as the spread of upper and lower gold points from mint par depends on the costs of shipping gold, so the spread of upper and lower commodity points from 'price parity' (as Heckscher called it) depends on the costs of and other obstacles to shipping commodities". (12)

Now, whilst the work of Kravis and Lipsey (1978) and the other critics may be interpreted to suggest that the "other obstacles" are quite substantial so that these so-called commodity points are quite wide, they must surely exist and so PPP will hold, albeit within fairly wide limits, and this is really all that the naive version of PPP is suggesting. Moreover many economists, in criticising "naive" PPP, have really been attacking what Holmes (1967) has called the "dogma" of PPP - that the relative prices-exchange rate relationship is a rigid and exact one. This is really an exercise in setting up a straw man. Cassel's theory was certainly not the "dogma" nor even the "naive" version; even he realised the "naive" version was naive, and consequently allowed for a variety of other factors which could distort exchange rates so that they did not approach their PPP levels even in the long run. (13)

The first of these and, in the context of the 1930's, perhaps an important one were tariffs, quotas and other trade restrictions
about which Cassel is quite explicit:

"...Although restriction of trade will not cause the rate of exchange to move from this purchasing power parity as long as they strike the trade in both directions equally.... if the trade between the two countries is hampered more severely in one direction than in the other the rate of exchange will deviate from its purchasing power parity". (14)

Furthermore Cassel recognised that not all trade restrictions were artificial:

"The restrictions of which we have to take account in this connection may be of various kinds... may... take the form of artificial hindrances or natural difficulties hampering transport for the country A to the country B more seriously than the transport in the opposite direction. The result will be some undervaluation of the money of A in that of B". (15)

These two factors - artificial trade restrictions and transport costs - could cause an exchange rate to deviate from its PPP even in the long run if they were a permanent feature of trade between two countries.

There are also a number of other factors which can create short run deviations from PPP, most of which were recognised by Cassel, although his contention that their effect is a short run one has been criticised. (16) Two of these factors relate to people's views about the further course of exchange rates and presumably, if such views were correct, would tend to push exchange rates towards their long run PPP whilst creating a divergence from short run PPP.

The first of these concerns inflationary expectations:

"A depreciation of a currency is often merely an expression for discounting an expected fall in the currency's internal purchasing power. The world sees that the process of inflation is continually going on.... The international valuation of the currency will, then, generally show a tendency to anticipate events....." (17)
The second, closely linked, is speculation:

"The value of a currency may also be depressed below the purchasing power parity by speculations in exchange". (18)

Although potentially stabilising, inflationary expectations and, especially, speculation can lead to large deviations from PPP if they get out of hand and Cassel believes that this indeed did happen in the case of the German mark in the early 1920's. (19)

Officer (1976a) also lists three other factors which Cassel recognised as potential causes of short run deviations from PPP.

One of these was long term capital movements:

"The third group of factors creating divergence from PPP contains those disturbances that may be caused by international movements of capital. When various conditions are fulfilled... and when no capital movements in either direction take place, the rate of exchange must stand at the equilibrium level represented by the Purchasing Power Parity and cannot show more than small and quite temporary deviations from this level". (20)

"A closer study of the dollar in pounds sterling during the period 1919-23 shows that the origin of the deviations of the actual rates of exchange from the purchasing power parity is to be found essentially in international movements of capital". (21)

Thus Cassel explicitly recognised that a long term capital outflow could depress a country's currency below its PPP level and a capital inflow push it above its PPP level.

Cassel also recognised that government intervention could cause a divergence from PPP although he did not construe the intervention as a means of deliberately influencing the course of the exchange rate but rather as a means of obtaining foreign exchange:
"By far the most important of the depreciating factors
now under discussion, however, is the practice of selling
at any price a country's exchange in other countries in
order to procure funds in their money.....The case of
Germany offers the best means of studying the whole
problem.....When the central Government...[was]...in
dire need of foreign means of payment, and it did not
seem possible to procure it in any other way, the country
was beguiled into thus getting money on its currency". (22)

A final factor creating a divergence from PPP listed by Officer (1976a)
concerns real changes in the economy (from the base period) as in­
dicated by changes in relative prices within a country leading to a
divergence between the exchange rate and relative PPP as measured by
a general price index. In particular Cassel seemed to be concerned
about changes in the relative prices of traded and non-traded goods. (23)

His answer to the problem (in the context of equilibrium exchange rates
for the post-World War 1 period) appeared to be either a hope that
these real changes would reverse themselves and trade would return
to its pre-World War 1 pattern or, failing this, a continuous study
of these real changes and then allowance for them (assuming they could
be adequately explained by known factors).

Thus there are a number of factors which can cause deviations
from PPP (especially in the short run). The first two of those dis­
cussed above - trade restrictions and transport costs - can be ignored
in principle, as far as relative PPP calculations are concerned, if they
remain at the same level in both the current period and the base period.
Unfortunately, in practice, whilst this may be true for the latter in
the 1930's, it is certainly not true of the former and more will be
said about trade restrictions in Chapter four below. The opposite
is true of real changes in the economy of the type discussed above
and international capital movements which, though valid in principle,
can probably be largely dismissed in practice for the 1930's: the
timespan between the base period and the period for which the model is being tested is possibly sufficiently small to make significant real changes unlikely whilst international long term capital movements were muted because of increased uncertainty due to political tension in Europe, war in the Far East and the increase in defaulting on foreign bonds which occurred in the 1930's and, in any case, the growth of exchange controls made the transfer of both capital and interest payments very difficult in some parts of the world.

Three other factors have been mentioned. The first of these, inflationary expectations, would act to push the short run exchange rate towards its long run PPP equilibrium and would tend to suggest a central role for current prices in the determination of short run exchange rates; the role of inflationary expectations is hence not only accepted but plays an important part in the model tested here. Similarly, the roles of government intervention and (private) speculation in causing the exchange rate to deviate from its PPP path are recognised as being potentially very important and are explicitly incorporated into the model developed in Chapter four. As far as the PPP hypothesis is concerned, this addition of variables raises the question of when the theory of PPP ceases to be and transforms itself into something else. Some theories start from the opposite extreme and do not include or emphasise prices in particular and then work back towards the PPP hypothesis. It is to these alternative approaches which we now turn.

(II)

There are many non-price factors which influence exchange rates,
some of which are by no means rational - for (an extreme) example, the role of the Invergordon "mutiny" in Britain's leaving the gold standard in 1931 - and it is quite clear that at certain times the course of a particular exchange rate has been largely determined by such factors in the short run. A good historical example of this is provided by the French franc in the 1924–26 period (24) and at the present time the fact that eight European currencies are committed to remaining within the European Monetary System must have some independent influence on their exchange rates. A whole list of non-price variables that may cause deviations from PPP has already been presented in the previous section and this was not exhaustive. The objective here is to re-examine some of these variables and to briefly consider any further potential non-price, influences along with any cohesive alternative (to PPP) theories of exchange rate determination.

An examination of such non-price influences is provided by Schadler (1977) who posed the question as to why exchange rates had been so erratic in the first four years following the widespread adoption of floating exchange rates in the 1970's. Four possible (and not mutually exclusive) explanations for this were suggested: destabilising speculation, different speeds of adjustment (suggested by the asset market approach to exchange rate determination), uncertainty and risk, and government intervention. The first of these (speculation) plays an important role in the model developed here: stabilising speculation would push the exchange rate towards its long run PPP level and consequently its existence would support the use of only a short lag on prices (or, put another way, a role for current or very recent price levels in determining the current exchange rate) whilst destabilising speculation is explicitly incorporated in
the form of dummy variables. The role of government intervention will also be modelled and indeed testing for its significance is an important subsidiary issue.

Uncertainty and risk (caused by instability of the world economy in general) may influence exchange rates through two potential avenues: "via the variability of fundamental determinants of the exchange rate...and also via the uncertainty about exchange rate expectations that such variability generates". (25) Clearly fundamental variables are directly included here and the second avenue - changes in exchange rate expectations - will, to a large extent, be picked up by the dummy variables to be included (discussed in later chapters). Schadler's final possible explanation relates to the asset market approach which will be discussed shortly. The study by Schadler (1977) typifies much recent work which approaches movements of exchange rates in terms of trying to explain "overshooting" - that is deviations from some equilibrium exchange rate path which is often, implicitly or explicitly, derived from PPP.

Two popular, though incomplete, approaches to the short run behaviour of exchange rates are the interest rate parity theory (or forward exchange theory) and what might be termed the "speculative run view". The latter is based on "the view that exchange rates move in speculative runs, perhaps touched off by a change in (or a revision of expectations about) fundamental economic conditions, but thereafter reflecting a self-sustaining speculative mentality". (26) In a sense, this is merely an updated version of destabilising speculation and its validity has been questioned; (27) nevertheless, it does stress the role of expectations in exchange rate determination, particularly in the 1970's, and, to that extent, is very much in tune with the asset
market approach (discussed below). (28)

The interest rate parity theorem has some historical pedigree, dating back to the period in which Cassel was developing the PPP hypothesis; (29) it suggests that the spot exchange rate is "explained" by the forward exchange rate and interest rates. In principle, since it is basically a pure arbitrage condition, interest rate parity should hold at all times but empirical evidence has indicated the existence of deviations in practice. However, a number of explanations have been put forward, (30) most plausibly the effect of transaction costs and the fact that different securities in different countries (and hence the rates of interest) are not comparable because they are associated with different degrees of risk; the possibility of exchange controls has been suggested in the latter context. (31)

This would seem a particularly relevant consideration in the 1930's - for example, it was widely expected that extensive exchange controls would be adopted in France after the election of a Socialist government in 1936 - and, consequently, would indicate that the interest rate parity theorem is an inappropriate framework in which to examine exchange rate determination in the 1930's. Furthermore, its validity as a general theory of exchange rate determination has been questioned on the grounds that all three variables involved are really endogenous and, in fact, spot exchange rates are actually determined by a set of exogenous variables which also determine forward exchange rates and interest rates. (32)

Another, potentially more complete approach to exchange rate determination is the traditional balance of payments view which suggests
that exchange rates vary so as to ensure equilibrium in the currency market. While most economists would accept that this is generally true, it would not be widely supported as a useful approach in its naive sense, in which it implies that exchange rates depend on the current (or trade) account position. Even though some relationship between the exchange rate and the current balance no doubt exists, the capital account cannot be so blatantly neglected. This view of exchange rate determination is behind the underlying payments disequilibria approach to calculating the equilibrium exchange rate which it defines as that which is compatible with "a desirable and sustainable balance-of-payments position". This is more sophisticated, in that, for calculating the gap between the future equilibrium balance of payments position and exchange rate and the actual, it advocates a complete world trade model to project their future paths; consequently it seeks to correlate the exchange rate not simply with the current account but with the variables that determine the current account. The balance of payments approach is intuitively very appealing and while the model to be presented here has been referred to as an extended PPP hypothesis it is sufficiently eclectic to be approached by this alternative route; indeed it can be presented in such a way as to appear to use the balance of payments view as its starting point with the PPP hypothesis incorporated within this.

The final (and most important) alternative approach to exchange rate determination to be considered, is the asset demand or asset market theory. Facilitated by developments in the analysis of portfolio balance, this approach emerged as a result of the Canadian experience of floating exchange rates in the 1950's when it became clear that prices were not the major determinant of exchange rates in
that particular case:

"The Cassellian view of the primacy of price changes rests on the assumption that demands for currencies are largely derived from current trade, and that holdings of foreign financial assets are motivated mainly by the purchasing power they yield over goods produced in the foreign country. In the Canadian balance of payments, however, capital transactions - especially with the United States - dominate and these financial and direct investment flows are transparently sensitive to changes in profit opportunities arising out of international differences in interest rates and trends in unit costs". (35)

More explicitly:

"The asset-market theory.....focusses on the equilibrating role of the exchange rate in balancing the foreign demand for domestically issued financial assets and the domestic demand for foreign financial assets.....the relative demands for domestic and foreign assets by private market participants depend on the expected relative yield on these assets. The expected relative yield in turn depends on the interest rate differential.....and on the expected change in the value of the spot rate...." (36)

There are two important differences between the asset market approach and a more traditional (relative price-based) view of exchange rate determination: firstly, the former sets exchange rate determination in the markets for stocks of assets, whilst the latter assumes that exchange rates are determined in the market for flows of funds (to buy goods and services); and secondly, the asset approach views exchange rates as the price of national monies and exchange rates as being determined by stock adjustments of people's holdings of different monies, whilst the traditional theory considers the exchange rate as the price of national output and consequently it is the relative prices of goods and services which determine the exchange rate. Despite these differences, however, it may well be correct to assert that the two theories are "tailored to different and extreme market conditions": (37) the traditional theory is most suitable where
currencies are held mainly for transactionary motives such as those of small open economies like Belgium and the Netherlands while an asset theory clearly applies where currencies are held for purely precautionary reasons which would seem to apply to many holders of Swiss francs in recent times, for example. Of course, in many (if not most) cases currencies will be held for both transactionary and precautionary motives and so some synthesis is required.

In fact, proponents of the asset market approach generally assume, for the sake of simplicity, that there is only one asset (money) and refer to "the monetary view (or more generally an asset view) of exchange rate determination".\(^{(38)}\) The monetary approach has been summarized as follows: "First...exchange rates are best thought of as relative prices of different national monies...and are determined primarily by the conditions for equilibrium between the demands for the stocks of various national monies and the stocks of these monies available to be held. Second...exchange rates are strongly influenced by asset holders' expectations of the future behaviour of asset prices".\(^{(39)}\) Thus the monetary approach would see the major determinants of exchange rates as national money supplies and expectations about the future level of the exchange rate (which are closely related to expectations about the future course of money supplies); the importance of expectations, in particular, is very strongly stressed by the "monetarists" and has been used to explain the erratic movements of exchange rates in the 1970's. A role would also be given in a monetary model to changes in real income.\(^{(40)}\)

However, the monetary approach has been criticised for taking too narrow a view of what constitutes "financial assets":

"...there is hardly any theoretical presumption that shifts in the market for money are 'more important' (for the exchange rate and/or the balance of payments) than shifts in the markets for interest-bearing financial assets. Empirical evidence in recent years suggests, in fact, that portfolio shifts have often taken place between earning assets.....rather than just money. Thus there is hardly any reason to turn an 'asset theory' into a 'monetary theory'..." (41)

An important implication of such criticism is that more general asset market theories should be preferred which would indicate a substantial role for interest rates. This criticism, combined with the fact that the model developed here contains some variables included in asset market models - real income, interest rates (42) and, most important, speculative or expectations variables - perhaps constitutes a good enough reason for not adopting an explicit monetary approach in the present study.

In any case the monetary approach incorporates the PPP concept in a number of ways. Most fundamentally, it actually accepts the relationship embodied in the PPP hypothesis, but interprets it as an equilibrium relationship between two endogenous variables with no causality implied; it is a "shortcut" with both variables principally determined by variations in money supplies. Secondly, it should be observed that acceptance of the primacy of the asset market theory in the determination of exchange rates in the short run is not necessarily incompatible with acceptance of PPP in the long run:

"As to the evolution of the exchange rate in the longer run, however, the asset-market view is fully consistent with the traditional view that it is essentially determined by the purchasing power of the currency in the goods markets. The long-run adjustment comes through arbitrage in the goods markets and, as already noted, through the influence of long-run expectations. Deviations of the exchange rate from its PPP value will be self-correcting in the long run..." (43)
However, in the present study, it is the short run that is important but even here, although "...explicit price effects.... are sometimes dispensed with altogether", this is apparently not true at other times since it is sometimes argued that "...one aspect of the return to foreign currency holding is the purchasing power it currently affords over goods produced in the issuing country..." which would seem to imply some role for relative prices.

Furthermore, in the context of the model used here (developed in chapter four), there is a direct point of contact with the monetary/asset approach (and possibly the PPP hypothesis) in that expectations are specifically included both here and in monetary models. This would seem important:

"The central insight obtained from this...approach is the recognition that expectations concerning future exchange rates are among the prime determinants of current exchange rates". (46)

The relationship with PPP comes from recognising the fact that expectations of exchange rate changes may be related to expectations about future levels of inflation (although a "monetarist" would presumably deny this by arguing that, since money supplies determine prices, it is expectations about future money supplies that determine exchange rate expectations) and/or to a belief that PPP holds in the long run. Moreover, a common proxy for the expected future exchange rate in monetary models - the forward market premium/discount - is also tried in the present study (in an adjusted form) for much the same purpose (although ultimately dummy variables are preferred).

In a sense, all this leads back to the question of when exactly
the PPP hypothesis ceases to be and becomes something else. The three major "complete" approaches to exchange rate determination discussed above - the traditional balance of payments view, the PPP hypothesis and the asset market approach - would appear to have some similarities. All would seem to accept some relationship between relative prices and exchange rates (although the monetary approach would not see it as a causal one): the traditional view would stress commodity arbitrage, a Casselian PPP hypothesis would accept this but propose links between prices of traded and non-traded goods and hence between exchange rates and all prices or else propose that shifts in the relationship between prices of traded and non-traded goods were temporary, and a monetary approach, whilst also accepting commodity arbitrage, would view prices not as the price of output but as the price of money.

The final equation estimated here could be seen as being based on either of the first two models: a balance of payments view (which is how it is presented in chapter four) in which determinants of trade (and capital) flows determine a floating exchange rate or an extended PPP hypothesis, beginning with simple PPP, and then adding variables to take account of various other factors, both those identified by Cassel and by others. An explicit monetary approach will be rejected on the grounds that it is too narrow and also because it is based on a monetarist view of the world which may not have been valid in the 1930's, whilst a more general asset market approach is not pursued because a number of variables which would be included in such a model are incorporated in the extended PPP/balance of payments framework developed here anyway (most importantly expectations). Thus, in view of the central importance of the PPP hypothesis in this
study, the rest of the chapter reviews the problems associated with it and suggests how they might be overcome.

(III)

The major theoretical objection to the PPP hypothesis is that it may be subject to some kind of systematic bias. A potential source of such bias arises from differences in countries' internal price ratios, that is the ratio of the prices of traded goods to those of non-traded goods. Commodity arbitrage is based on traded goods but PPP is calculated from a more general index which includes non-traded goods. If internal price ratios differ between countries then a PPP calculation (based on a general index) will make the currency of a country with relatively lower priced non-traded goods appear undervalued. An attempt to provide a rationale for such differences was undertaken by Hagen who argued that the internal price ratio is a function of per capita income. This was based on the observation that in low income countries cheap labour is readily available whilst non-traded goods are labour intensive; consequently, the lower per capita income, the lower the relative price of non-traded goods and therefore the higher the internal price ratio. Unfortunately, this plausible line of reasoning tends to break down when it is recognised that exports of many low income countries are, in fact, labour intensive - for example, textiles.

However, even if Hagen's argument is not accepted a plausible and widely accepted alternative rationale for differences in internal price ratios has been provided by Balassa (1964) based on differences in productivity within and between countries. The argument runs as
follows: productivity differences between countries tend to be higher in the traded goods sector than in the non-traded goods sector mainly because there is more scope amongst the type of commodities that are traded - mainly manufacturing and agricultural goods - for productivity increases than amongst non-traded goods which are often labour-intensive services. Since wages are related to productivity then inter-country wage differentials in the traded goods sector will reflect productivity differentials with wages correspondingly high in high productivity countries.\(^{(51)}\)

According to commodity arbitrage the exchange rate will equate the prices of traded goods - the "law of one price" - but this does not apply to non-traded goods. If inter-country productivity differences between traded and non-traded goods were the same or, failing this, if prices of non-traded goods mainly reflected productivity differences alone this would not matter, but the former is not the case and neither is the latter (for a number of reasons including the use of some traded goods as inputs for non-traded goods, competitive bidding for factors of production and, mainly, because of the tendency for wages to be equalised within countries). Consequently the prices of non-traded goods tend to be higher in high-productivity countries and there is a systematic bias in internal price ratios and so a bias in PPP:

"The greater are productivity differentials in the production of traded goods between two countries, the larger will be differences in wages and in the prices of services and, correspondingly, the greater will be the gap between purchasing-power parity and the equilibrium exchange rate".\(^{(52)}\)

If per capita income levels are taken as representative of productivity levels which does not seem unreasonable since a country is likely to
have a higher per capita income precisely because of higher productivity - then the bias can be expressed as follows: the ratio of PPP to the exchange rate is an increasing function of income levels. Balassa then went on to provide an empirical verification of his hypothesis.

Although Balassa's reasoning has become fairly widely accepted, his results have been strongly criticised on both theoretical and empirical grounds, principally by Officer (1974,1976a,1976b). The first theoretical criticism relates to the implicit assumption, made by Balassa, that the impact of a given increase in productivity on the internal price ratio is the same for all countries. Specifically, Officer suggests that if this impact is inversely related to levels of productivity amongst countries then converging internal price levels and diverging productivity levels are quite compatible (although no good reason why such an inverse relationship should actually exist is given). A second, more convincing, theoretical criticism relates to the fact that Balassa ignores quality differences in consumer services amongst countries.

Officer begins his empirical attack by arguing that tests of the Balassa hypothesis have produced rather mixed results and an examination of the evidence would seem to bear this out: Delahaut and Kirschen (55) and Balassa, himself, in a later article (1973), support his conclusion whilst Clague and Tanzi (1972) found the Balassa hypothesis holds for OECD countries but not Latin American countries, and Grunwald and Salazar-Carrillo (56) supported this latter conclusion; whilst failure of the Balassa hypothesis to hold for developing countries is not surprising, De Vries (1968) using a sample of sixty-two countries (including both developed and developing),
found no support for the hypothesis either. In the light of this, Officer (1974, 1976b) sets out to rigorously test the Balassa hypothesis. He begins by criticising the methodology of earlier studies (especially that of Balassa himself), in particular Balassa's use of only one variable - GNP per capita - to proxy productivity and also his use of the current exchange rate as an approximation for the equilibrium (PPP) rate; in addition, there are numerous other, less important criticisms. Officer then tests the Balassa hypothesis (including re-running Balassa's original tests) using his improved methodology and finds no support for productivity bias in absolute PPP and virtually none for bias in relative PPP.

In the present context, the presence of productivity bias (if it does exist) would not seem likely to cause any problems conceptually as a suitable variable to allow for it can easily be incorporated into the eclectic approach adopted here. However, at the empirical level there are problems in that even the simplest proxy for productivity, and one much criticised by Officer, GNP per capita, is not available for the period under examination. In fact, national income is included (for other reasons) in the model and a proxy (based on employment data) has had to be found. Consequently, whilst productivity bias is allowed for in that a GNP proxy is one of the independent variables in the model it must be accepted that it is a poor proxy for productivity and completely lacks sophistication in this context.

A completely different, but probably more fundamental theoretical criticism of PPP (which cannot be sidestepped by simply adding another variable) should also be discussed before moving on to examine the practical problems of applying the hypothesis; this is, what might be called, the "reverse-causation" argument which states that exchange
rates determine prices rather than the other way round. If this is true then as Yeager notes:

"...the statistical evidence in apparent support of purchasing power parity loses force if exchange rates determine rather than reflect the price levels used in the calculations". (58)

In fact it is Yeager who provides the most extensive arguments in defence of prices to exchange rates causation. His major argument is very simple: if exchange rate changes lead to changes in the prices of some goods then unless domestic money supply changes, other prices must move in the opposite direction because in the absence of a permissive monetary policy (or short run changes in velocity) the overall price level cannot change. To support his argument Yeager invokes an adaptation of the quantity theory of money. (59)

In practice a permissive money supply is quite possible but then the fact that causation runs from exchange rates to prices is "due not to inexorable linkages but to policy". (60) Yeager then goes on to attack the general presumption that devaluation and depreciation must be inflationary: if devaluation (depreciation) simply replaces trade controls which forced the balance of payments into equilibrium (by artificially raising prices) then the net effect on the price level is not necessarily inflationary. (61) Some evidence to support reverse causation is provided by Frenkel (1978) who argues that, insofar as there is any causation between the two variables, it runs from exchange rates. However, closer examination of his choice of price indices throws some doubt on this conclusion since two out of the three - wholesale prices and material prices - are heavily biased towards traded goods and thus composed chiefly of prices governed by exchange rates with the prices which undergo compensatory opposite
changes tending to be left out.

In fact, the relationship between devaluation (or depreciation) and inflation has been widely discussed in another context, namely the debate over the relative merits of fixed and floating exchange rates. The assertion that floating exchange rates (or more correctly depreciating exchange rates) are inflationary has been expounded at great length by proponents of fixed exchange rate systems. What is interesting, and perhaps very relevant with regard to the question as to whether prices determine exchange rates or vice versa, is the uni-directional nature of the argument. The logically converse argument, that just as depreciation causes prices to rise so appreciation causes prices to fall, whose acceptance is not critical for the fixed versus floating exchange rate debate, but is important as far as causation is concerned, attracts much less attention and support.

Nevertheless, there seems little doubt that causation between prices and exchange rates runs both ways to some extent, but the question as to which dominates may have different answers at different times. Whilst in present day Britain, trade unions can be relied on to make sure that any depreciation of the pound does influence prices, it is far from clear that this was true in Britain, North America and the gold bloc in the 1930's; the sanctions of mass unemployment were probably much stronger then than they are now. Indeed there is some contemporary evidence which rejects the hypothesis that devaluation led to inflation. In addition, to the extent that trade was subject to high levels of protection, the Yeager/Sohmen argument (discussed above) may come into play; certainly in one case (Belgium), the level of protection was reduced following devaluation in an attempt to discourage retaliation. In the light of all this, the view
adopted in the present study, that causation ran from prices to exchange rates does not appear entirely unreasonable.

There is, however, a third view adopted by Frenkel (1978) (and already referred to in the discussion of the asset market approach) who argues that there is no causation between prices and exchange rates, but, instead, interprets PPP as an equilibrium relationship between two endogenous variables. Frenkel then goes on to provide a theoretical argument to explain his findings that exchange rates appear to determine prices rather than vice versa (criticised above) in terms of differential speeds of adjustment: adjustment is faster in asset markets than in commodity markets and consequently exchange rates are influenced more quickly than prices by the third factors which determine them both, and therefore causality appears to run from exchange rates to prices.

If this is accepted then "reverse causality" is no longer a problem since money (supply), not prices, is the truly exogenous variable and this can be introduced to replace prices as the major independent variable in the exchange rate equation. Even if this were theoretically acceptable, and much of the argument presented here suggests that it is not, there are technical problems in terms of finding appropriate data for the 1930's. Data on currency in circulation and, in some cases, demand deposits, are available (in the League of Nations Monthly Bulletin) but their reliability and completeness as a measure of the "money supply" is dubious for many countries and both Hudgins (1973) and Ozmun (1976) obtained poor results when they made use of this data. Moreover, given the central role in the present study of the PPP hypothesis (which has been acceptable in some form to many

(page 36A follows)
economists, for reasons already indicated), to exclude relative prices as a dependent variable rather defeats the object; indeed it actually prejudges the issue in favour of PPP since a starting point of the models of Frenkel and others is that PPP holds as an equilibrium relationship between two endogenous variables.

Ultimately, of course, the question of price-exchange rate causality may well be an empirical one since there are logical and theoretical bases for causation running both ways and what is important is which variable is dominant (assuming that the endogeneity argument is rejected along with the monetary approach). It would have been possible, at this stage, to actually test for causality using the methodology developed by Sims (1972) and Pierce (1977). Causality testing is a relatively recent development in econometrics and much of the empirical work has examined the relationship between money and income. However, there have also been several historical applications of the methodology, by Brillembourg and Khan (1979), Eichengreen (1980), Hatton, Lyons and Satchell (1981) and Frenkel (1978), although only the latter is of direct relevance, since it specifically examines the exchange rate-price relationship in 1921-25. Unfortunately, Frenkel presents only his conclusions and not a detailed account of his tests (because the sub-section concerned only constitutes a small part of a much broader paper). Nevertheless, it should be acknowledged that his results indicated that causation seemed to run from exchange rates to prices (although his choice of price indices can be and has been criticised above). This conclusion is discouraging (from the point of view of the present study) and more pessimism is generated by the empirical work of Major (1979) who examined the 1971-78 period and found no evidence of a causal relationship between prices and exchange rates.

(page 36B follows)
However, whether or not conclusions based on these results can command any general acceptance is debateable since the extent to which any degree of confidence can be placed in the "science" of causality testing is rather dubious. There is an initial problem of providing a suitable definition of causality in either a philosophical or operationally useful sense. The usual approach adopted is that of Granger who defined causality in terms of predictability: X "causes" Y if present Y can be better predicted by using past values of X than by not doing so (with all other available information, such as past values of X, also taken into account in either case). Moreover, even if this is accepted, and it may not be, then the statistical tests for "Granger causality" have, in their turn, also been controversial. The two types of test developed, by Sims (1972) and Pierce (1977) using regression and cross-correlation methods respectively, have been heavily criticised in terms of both their validity as tests of causality and the difficulties involved in their implementation. Consequently, scepticism about causality testing is widespread and it has not been adopted as a general practice.

Of course, it remains desirable in principle to test initially for causality, particularly for the exchange rate-price relationship examined here, but also more generally because of the force of the argument that "correlation does not imply causation." However, in eschewing the use of causality tests in the present study, it might be argued, at the one level, that such an approach is following (extremely) well trodden ground in empirical work and, at another (more fundamental) level, that the controversy surrounding such tests, along with the difficulties in definition, interpretation and implementation, mean that the results generated may well be so conditional (in definitional
and technical senses) as to be worthless or may quite likely be simply inconclusive. As one study observes:

"It has, in fact, become a common feature of causality testing that no causality is found". (64F)

Consequently, since evidence of causation would be generated by a dubious methodology and lack of such evidence may indicate more about this methodology itself rather than causality, it seems probable that causality testing can be rejected without undue concern.

In any case, the specific question of the direction of causation between exchange rates and prices loses some importance in the present study to the extent that the hypothesis being tested is whether exchange rates were determined by "economic fundamentals" in the 1930's, only one of which was relative prices. Furthermore, even if there is really an equilibrium rather than a causal relationship between relative prices and exchange rates (as the monetary approach argues), provided that there is some correlation between the two then this would indicate that this equilibrium "held" in the 1930's and hence indirectly suggest that exchange rates (and relative prices) were not subject to erratic movements but were determined by the

continued over/........
exogenous economic variables. Such a conclusion would be sufficient to question the hypothesis that exchange rates were determined by erratic speculative forces in the 1930's (which is principally what is being examined).

Finally, whilst it is true that in most cases depreciation and rising prices occurred more or less simultaneously in the 1930's, the former can arguably be better viewed as permitting the latter rather than causing them. Specifically, if a policy of deflation was required to maintain the gold standard parities in the gold bloc countries, then it follows that if prices had not been held back the gold bloc exchange rates could not have been maintained and would have been depreciated; hence price levels determined exchange rates. Once the decision to abandon the gold standard was taken in these countries prices were allowed to rise. However, it was not the depreciations that cause prices to rise but rather the upward pressure on prices (in the sense of pressure against further deflation) that forced the exchange rates to be devalued and depreciate.

(IV)

Having considered the theoretical problems associated with PPP it is now necessary to examine the major empirical problems involved in testing the hypothesis. In fact these are not exactly the same for the two versions of the hypothesis: use of relative PPP avoids some of the problems of the absolute version but creates some new difficulties of its own. As the present study (like most others) tests the relative version, the problems peculiar to absolute PPP can be dealt with fairly briefly. The first of these involves various
exogenous economic variables. Such a conclusion would be sufficient to question the hypothesis that exchange rates were determined by erratic speculative forces in the 1930's (which is principally what is being examined).

Finally, whilst it is true that in most cases depreciation and rising prices occurred more or less simultaneously in the 1930's, the former can arguably be better viewed as permitting the latter rather than causing them. Specifically, if a policy of deflation was required to maintain the gold standard parities in the gold bloc countries, then it follows that if prices had not been held back the gold bloc exchange rates could not have been maintained and would have been depreciated; hence price levels determined exchange rates. Once the decision to abandon the gold standard was taken in these countries prices were allowed to rise. However, it was not the depreciations that cause prices to rise but rather the upward pressure on prices (in the sense of pressure against further deflation) that forced the exchange rates to be devalued and depreciate.

(IV)

Having considered the theoretical problems associated with PPP it is now necessary to examine the major empirical problems involved in testing the hypothesis. In fact these are not exactly the same for the two versions of the hypothesis: use of relative PPP avoids some of the problems of the absolute version but creates some new difficulties of its own. As the present study (like most others) tests the relative version, the problems peculiar to absolute PPP can be dealt with fairly briefly. The first of these involves various
index number problems. In practice these are usually minimized by using a Fisher ideal index but, in fact, as far as relative PPP is concerned, such problems are not relevant since it is changes in prices that are being considered and therefore there is no reason why using indices with different goods and different weights for different countries should lead to problems provided the method of constructing the index does not change in the period under consideration.

The two other problems which are normally associated with absolute PPP, although when expressed in terms of changes are equally applicable to relative PPP, involve distortions created by transport costs and trade controls. These have already been discussed in theoretical terms and are well known: if transport costs are higher in one direction than another or a country has higher tariffs than others then this would create deviations from PPP. In an empirical situation, such deviations would have to be allowed for. However, to the extent that these two variables remain at the same level as they were in the base period, they can be ignored in tests of relative PPP in much the same way as index number problems but, on the other hand, if they do change then they will cause distortions. In a test of relative PPP over a period of only eight years, the assumption that deviations caused by changes in transport costs can be ignored, does not seem too objectionable, especially as their importance even in tests of absolute PPP is likely to be minimal (except in extreme cases). Unfortunately, this is not true of trade controls which certainly did change, in fact, dramatically so. The empirical implications of this (and attempts to deal with it) are taken up in chapter four below.
Thus, with one notable exception, the problems associated with absolute PPP can be ignored in the present study. However, this still leaves one major problem associated with both versions of PPP - the choice of price index - and another, associated solely with relative PPP, namely the choice of base period, which obviously do have to be considered in some detail.

Taking the latter problem first, the base period should be one in which the exchange rate is at its long run equilibrium - that is when absolute PPP holds:

"Using a base period for which this was not the case can cause serious problems of interpretation in empirical studies. A finding that the change in the exchange rate between two periods was only a fraction of the relative change in the price levels of two countries could constitute positive evidence of a dynamic movement towards purchasing power parity if the base period exchange rate was not equal to its PPP equivalent. On the other hand, if the results showed the exchange rate offsetting perfectly movements in the price levels we might wrongly infer that PPP was being maintained when in fact slow adjustments were keeping the exchange rate away from its PPP path". (66)

It is also preferable for the base period to be fairly close to the current period; how close is "fairly" is, of course, a matter of judgement but the greater the time span between base and current period the more likely are trade restrictions, transport costs, productivity levels and any other potential sources of bias to have changed. (Yeager talks of the "attenuated meaningfulness of price level comparisons over long spans of time". (67))

This may provide the source of the first major problem in that these two criteria - an equilibrium and recent base period - may conflict; the most recent period in which the exchange rate was at an equilibrium level may be some time past. This means that in taking a rather distant
base period the PPP calculation becomes prey to all the various biases mentioned above. This is Yeager’s "ancient history" argument:

"What may have happened in the years since the base period is hardly more than ancient history with respect to forces currently determining the exchange rate". (68)

Nevertheless, it remains relatively easy to define conceptually what a base period should be. However, actually choosing one empirically is a rather different matter. In a sense, the ability to choose correctly a base period actually precludes the need for relative PPP in that a correct choice implies that we know how to recognise whether actual and equilibrium exchange rates are diverging so why bother to calculate PPP to work out something we already know? Of course, in reality, the closest we are likely to be able to get to identifying an equilibrium base period is an approximation where the actual and equilibrium exchange rates did not diverge too much. Given this, three possible approaches could be suggested as practical methods of selecting a base period for empirical work: first, several different base periods could be tried and a kind of sensitivity analysis could be applied to see if this made much difference to the results but this is rather messy and in any case we still need criteria for deciding which periods qualify as potential bases; also, if we find that the different bases all generate similar results does this mean that they are all suitable base periods or that they are all unsuitable?

A second method is suggested by Officer who is highly critical of the arbitrary way in which most researchers choose their base period:

"...the rationale of selecting a period in which the exchange rate is in long run equilibrium is generally ignored. In particular, no researcher has used balance of payments data (or apparently any quantitative data) to determine an optimal base period". (69)
The possibility is taken up and discussed by Artus:

"Such an identification of an appropriate base period can possibly be made by looking at the current balance. If the current balance was at a 'satisfactory' level given the relative cyclical position of the country, the existence of other temporary factors, and account being taken of possible lagged effects of past relative price changes which are still in the pipeline, then, perhaps, the actual rate was close to the equilibrium rate". (70)

This is conceptually a very appealing approach and is further discussed below.

A third method of choosing the base period has been suggested by Genberg:

"By making the assumption, admittedly not unobjectionable, that PPP holds on the average during the particular sample period, I could let a constant term determine the appropriate base period". (71)

This is superficially appealing in that the base period is indeed a period rather than a single year or (at best) a few years; by, in a sense, relying on the law of averages, this method also has the advantage of avoiding the possibility of being completely wrong, inherent when a base year is chosen arbitrarily. Unfortunately, as Genberg himself points out, this method is "not unobjectionable" especially if the period under consideration is a fairly turbulent one or contains a large sub-period clearly characterised by disequilibrium exchange rates. However, this remains an empirically easy approach, although the Officer balance of payments method is perhaps the most appealing.

Unfortunately, the latter may be difficult to apply in practice. Even adopting Artus' (1978) suggestion and concentrating solely on the current account, there is an immediate practical problem relating to
whether or not the base year, for the pound-dollar exchange rate, for example, should be chosen with reference to current account dealings between Britain and the U.S.A. only. If it is, then powerful third country influences which may have pulled the pound-dollar rate away from its equilibrium (PPP) value are ignored; if it is not, and the complete current account of each country is examined, then it may be that the years in which the British current account was "satisfactory" (and which therefore qualify as suitable base periods) do not coincide with those suggested by examining the U.S. current account.

There is also, in the context of the present study which adopts an extended PPP (or total balance of payments) approach, a more fundamental criticism which also applies to Artus' suggestion, referred to above (that the equilibrium period can be defined with reference to a "satisfactory" current account). This relates to the contention that the equilibrium exchange rate should not be calculated solely with reference to relative prices alone (or the current account only) but to all the "fundamental" economic variables that determine exchange rates (or the balance of payments as a whole). Use of the current account only may be acceptable if a pure PPP hypothesis is to be tested but, in an extended version, equilibrium not only depends on relative prices but also relative incomes and, if the capital account is incorporated, interest rate differentials and various other variables.

Thus, the contention that a particular period is unacceptable as a base period because the exchange rate was not at its equilibrium level in terms of relative prices, is inappropriate in an extended PPP approach if such deviation was justified by the levels of the variables used to "extend" the PPP hypothesis. Consequently, the widely
held belief that the pound was overvalued in the late 1920's may not necessarily preclude the use of this period as a base in the present study, as in total balance of payments terms (allowing for the influences of non-price, "fundamental" economic variables) the pound may, in fact, have been in equilibrium.

An examination of the British balance of payments in the 1927-30 period, during which the pound was allegedly overvalued bears this out. The large trade deficit (over £350m. each year) may seem to reflect overvaluation in simple PPP terms but the current account, as a whole, was in surplus (over £100m. except for 1930) presumably thereby reflecting undervaluation. However, if a total balance of payments view were taken then this would involve looking at the capital account also. The net position on current plus capital account is reflected in gold movements, which were never more than £15.2m., indicating approximate equilibrium in all four years. A basic balance approach would involve treating short term capital as accommodating and concentrating on the current account plus long term capital only which would indicate a balance of payments position of -$35.9m. in 1928, +£12.0m. in 1929 and -£11.6m. in 1930. (Data on short term capital was not available for 1927). This would suggest that either 1929 or 1930 are most suitable as base years for extended PPP calculations against the pound, although in the light of the arguments in the previous paragraph, this is only really true for the pound's effective exchange rate and not its bi-lateral rates.

Furthermore, the contention that the British balance of payments was in equilibrium has, in fact, been strongly made elsewhere:
"If...balance-of-payments equilibrium were the sole
criterion of the equilibrium rate of exchange, there
might be little justification for regarding the pound
sterling in the years 1925-30 as overvalued. For there
was little sign of disequilibrium in the British balance
of payments. The Bank of England's gold reserve remained,
on balance, practically constant during those six years..." (74)

Moreover, the same author goes on:

"If we apply our definition of the equilibrium rate literally,
the pound cannot be said to have been overvalued. The British
balance of payments was kept in equilibrium, however, only at
the cost of depressed conditions at home compared with conditions
in the outside world". (75)

Thus although the pound may have been overvalued in a "pure" PPP
sense, if allowance is made for other economic fundamentals, particu­
larly relative incomes, it is possible to argue that the pound was at
an equilibrium level in a total balance of payments sense. (76)

Additional and perhaps more useful evidence (since it applies
to five of the seven currencies in the present study and not just the
pound) is provided by Gaillot (1970). He attempts to calculate whether
PPP held over a very long period (1900-04 to 1963-67) for the exchange
rates of seven currencies against the dollar and examines a number of
intermediate periods, including 1927-31, in which he finds that the
actual exchange rates of the U.K., Canada, France and Switzerland
were all within six per cent of their PPP rates. (77)

Thus, there is reason to believe that the pound, and possibly
some of the other currencies in the study, were in equilibrium in a
total balance of payments sense in the late 1920's. Furthermore,
identifying a base period in this way does not preclude the simul­
taneous adoption of Genberg's approach of including a constant. The
The implied assumption of PPP holding on average in the 1930's may be
a heroic one but is not totally without foundation as, for example,
contemporary calculations of simple PPP of the major currencies identified periods of both over and undervaluation of the pound and the dollar.\(^{(78)}\) In addition using two methods of determining the base year – and equilibrium balance of payments approach and inclusion of a constant term – make it more acceptable to adopt a common approach to all the exchange rates examined (which is what is actually done), thereby avoiding the conceptually correct, but empirically messy, possibility of having to use different base periods for different bi-lateral exchange rates.

A more general problem, which applies equally to both absolute and relative PPP, relates to the choice of type of price index and many studies would seem to adopt a haphazard approach to this, often not discussing it at all. A variety of indices have been used – wholesale prices, cost of living indices, export prices, unit factor costs, wages, the GNP deflator and several others. There are two stages in the choice of index decision: firstly, the question as to whether the index should be biased towards traded goods, non-traded goods or be simply a general index and secondly, having chosen the type of index, what are the advantages and disadvantages of the available indices of that type?

If PPP is construed as being based solely on commodity arbitrage then it has been argued\(^{(79)}\) that there is no reason why the "law of one price" should apply to non-traded goods – indeed there is a good reason why it should not in the form of the productivity bias argument – and consequently an index based on the prices of traded goods is what is required. This line of argument could be used to justify the widespread use of wholesale price indices in empirical work (although lack of available alternatives may be a much more important reason for this).
However, reliance on indices based only on the prices of traded goods may have its pitfalls; in particular, the short run price of exports may be set more with current competitiveness in mind than overall domestic cost and price levels, and so their reliability as indicators or PPP and therefore equilibrium exchange rates may be very limited.

More fundamentally, even if the "law of one price" does hold for tradeables, PPP based on a traded good prices index is simply a truism. As Haberler notes:

"...equality of international prices...is a necessary but clearly not a sufficient condition for international equilibrium. Even if there exists a large and prolonged deficit in a country's balance of payments and hence its currency is seriously overvalued, prices of internationally traded goods will not, or at least need not, show any deviation from purchasing power par". (80)

Verification of PPP based solely on price indices of traded goods is therefore spurious and this was very quickly realised by Keynes who used this argument to attack the British return to gold in 1925 at the old parity which, insofar as it was based on any rational economic argument, stemmed from a PPP calculation based on wholesale price indices (which are heavily weighted with traded goods).

If one rejects commodity arbitrage as the principal mechanism linking prices to exchange rates or, perhaps better, accept its role as far as traded goods are concerned but dismisses the fact that the law of one price holds as self-evident and therefore trivial, and instead emphasizes the role of equilibrium in asset markets then a more general price index is preferred. If this view is taken to its logical conclusion and it is argued that the law of one price is automatic, then what is really required is an index of the prices of non-
traded goods. The favourite choice is usually some index of wages. (81)

An alternative rationale, more in keeping with the traditions of commodity-arbitrage PPP, is that in the absence of any biases in the internal price ratio (non-traded to traded goods) then a general price index is preferable since it avoids the criticism of spurious verification, allows emphasis to remain on commodity arbitrage and the asset market approach to be rejected and is much more in keeping with Cassel’s own views since when he wrote about "the internal purchasing power against goods" (82) he quite clearly meant all goods:

"Some people believe that Purchasing Power Parities should be calculated exclusively on price indices for such commodities as form the subject of trade between the two countries. This is a misrepresentation of the theory.... the whole theory of purchasing power parity essentially refers to the internal value of the currencies concerned, and variations in this value can be measured only by general index figures representing as far as possible the whole mass of commodities marketed in the country". (83)

Emphasis on commodity arbitrage still remains at the centre, however, since an essential ingredient of Cassel’s theory is that the prices of traded and non-traded goods are closely related by various links:

"Actually, the line between domestic and internationally traded goods is a fuzzy and shifting one. Various domestic and international goods and services are related in price, though not rigidly, by the fact that some are ingredients of others, by the use of common factors of production, and by direct or indirect substitutability in consumption". (84)

Thus, if we are to provide a genuine test of PPP – certainly of the version expounded by Cassel – then a general price index is to be preferred. In addition, such an index would also be preferred by exponents of the asset market theory of exchange rate determination and consequently, its acceptability to both PPP and asset market
theorists make it particularly appropriate for an eclectic approach of the kind adopted here.

Turning to the specific choice of index, it remains true that, in spite of all the above rhetoric, one of the most popular indices, albeit often due simply to availability, has been a wholesale price index which would normally be regarded as an index of traded goods prices; consequently, initially some examination of such indices must be made in passing if much of the empirical evidence is not to be ignored. Not surprisingly wholesale price indices (WPI's) often perform better than any others, although whether any weight can be given to the argument that if we are interested in simply predicting exchange rates, rather than testing the PPP hypothesis then WPI's are better practical indicators due to their "higher explanatory power", and that therefore their use is justified, rather depends on one's views about the direction of causality between prices and exchange rates.

Of the indices which contain mostly traded goods WPI's are perhaps the least objectionable in that they do contain some non-traded goods. However, they are still heavily biased towards tradeables largely because they concentrate mainly on commodities; the tendency to spuriously support PPP is accentuated by the fact that they often give relatively large weights to primary products (which tend to obey the "law of one price") and omit altogether highly differentiated products (which do not). There are also statistical difficulties of comparability due to differences in coverage and weighting of national WPI's (although this is more a problem for absolute than relative PPP). Whether or not the OPTICA conclusion - that it is "legitimate, though not ideal, to consider WPI a useful candidate series for measuring PPP"(86)
is acceptable is perhaps questionable. Finally, at least one study\(^{(87)}\) has tried indices based completely on traded goods in the shape of indices of export prices which (not surprisingly) performed very well. However, this type of index provides such a biased test of PPP that these results can be taken as little more than the result of efficient commodity arbitrage.

The most popular of the more general indices have been consumer price indices (CPI's) which, of course, are not subject to the conceptual objections that plague WPI's although they can be criticised for lack of comprehensiveness; in 1970 private consumption represented less than two-thirds of GDP in most major developed countries. Nevertheless, as far as testing the PPP hypothesis is concerned CPI's are without equal in practical terms since they are widely available and not especially biased towards traded goods. Moreover, criticism of their lack of comprehensiveness is, to some extent, a red herring since all the available indices are subject to statistical shortcomings of one sort or another and it is rather too easy for a supporter of a particular index to begin justifying his choice by pointing out the statistical weaknesses of the alternatives.

However, the index that is most widely recognised as being the "best" of the general price indices for PPP purposes is the GNP or preferably GDP price deflator:

"Of the three \(\Delta \text{WPI}, \Delta \text{CPI} \text{ and GDP deflator}\), the GDP implicit deflator has the strongest claim to represent a general measure of a country's price level. It is based on a conceptual framework that assigns an appropriate weight to each good, whatever the classification chosen - as for example, between tradeables and non-tradeables". (88)
"...the GDP deflator... is, in the view of some observers, the most suitable in measuring PPP, because it is the most broadly based". (89)

"Cassel does not directly identify the kind of general price level that would be optimal in computing PPP, but the most logical interpretation would be a price measure of a country's GDP". (90)

In addition the scope of implicit deflators is likely to be very similar since they are usually based on a common accounting framework (the UN System of National Accounts). The major difficulty associated with the GDP deflator is a practical one in the form of its lack of availability. Even in more recent times it is often only available on an annual basis in some countries which may preclude the use of this rather promising indicator of general price trends. It is for this reason - especially for pre-World War 2 periods - that CPI's are usually the least objectionable of the available general price indices.

An alternative method of testing PPP is not to use price indices per se but to use indices of costs because they give a much better indication of the true underlying level of prices for two main reasons:

"Arguments in favour of cost over price parity theories.... are outlined as follows. (1) Costs of production are less subject to adjustment to exchange rate changes than are prices of traded goods. (2) Costs exclude the volatile component of profits and so are more likely than product prices to represent long-run prices (for absolute parity) and to reflect permanent rather than temporary changes in prices upon inflation or deflation (for relative parity)". (91)

However, as Officer goes on to point out, it should be noted that such arguments "do not justify a cost parity as such, only its superiority in certain respects over a price parity". (92) and so all the theoretical
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However, as Officer goes on to point out, it should be noted that such arguments "do not justify a cost parity as such, only its superiority in certain respects over a price parity" (92) and so all the theoretical
criticisms applying to price parities apply with equal force to cost parities. Indeed Houthakker has presented a cost parity theory which reduces to a price parity theory although this has been strongly criticised on theoretical and other grounds. However, there is one factor in favour of the cost parity approach in that the reverse-causation argument - that exchange rates determine prices - loses much of its force when applied to cost levels instead of price levels.

Nevertheless, while it has advantages, the cost parity approach could be viewed rather negatively as having all the disadvantages of the price parity approach plus one or two others. In particular the "cost level" is a vague and ambiguous concept and to make it operational even at a theoretical level several problems have to be dealt with: for example, a firm's costs vary with its level of output which means that an appropriate output level has to be chosen at which to measure costs and furthermore costs may not even reflect long run prices if there is an element of oligopoly or monopoly in an industry; at a more practical level the specific firms and industries to be included in the cost index have to be chosen and in addition allowance must be made for differences in long run productivity growth across countries.

Finally, there is the problem of availability of data. The only factor price that is readily available is that of labour and consequently prices of intermediate inputs, capital services and raw materials have to be excluded. Frenkel (1978) does try an index of material prices with some success but this can be criticised on exactly the same lines - it excludes everything else; moreover, since raw materials often consist of primary products which are not differentiated then they are very likely to obey "the law of one price" and
hence provide a biased test of PPP. In any case, reliable indices of labour costs are not always widely available:

"The use of unit labour costs (ULC) would appear to... be an appropriate choice... But again there are statistical... causes for unease; in particular the quality of the available statistics is suspect. Indirect labour costs such as pay-roll taxes and unemployment insurance are largely excluded; for two EC European Community countries ULC data do not exist at all, while for four other EC-countries they are not published nationally". (95)

Thus use of cost parity either to represent or provide an alternative for price parity must ultimately be rejected in most cases simply due to lack of data.

With regard to the present study data deficiencies mean that use of cost indices is simply not possible (with one exception) and consequently use must be made of whatever price indices can be found. In practice GDP deflators are obviously not available either for the 1930's, but fortunately WPI's and CPI's usually are, which at least allows the study to try both a traded good biased index, based on WPI's, thereby providing comparability with many other studies, and a more general index, which provides a much better test of PPP (although, of course, the study is concerned with rather more than this).

This completes the survey of PPP and the problems associated with its application. Its widespread acceptance and application make it a useful starting point for any theory of exchange rate determination. However, its implicit concentration on the current account make
it necessary to examine other theories and various elements of these can be usefully drafted on to produce an extended PPP hypothesis which adopts an eclectic (and more acceptable) view of exchange rate determination. The theoretical and practical problems associated with PPP are substantial but not completely insurmountable. Having surveyed the theoretical aspects of exchange rate determination, the next chapter examines some of the empirical work concentrating especially on studies which adopt some form of PPP approach and (or) deal with past periods of floating exchange rates (particularly the inter-war period).
FOOTNOTES TO CHAPTER 2


3. See Holmes (1967) and Officer (1976a) for a more complete bibliography of Cassel's contribution.

4. See, for example, Einzig (1935), F.W. Taussig, "International Trade" (1927) Ch. 6, J. Viner, "Studies in the Theory of International Trade" (1937) and the work of X. Zolotas (discussed below).

5. See Balassa (1964) and Kravis and Lipsey (1978), for example.

6. Many authors suggest this - for example, Samuelson (1964) - and the point will be taken up further on.

7. This line of argument is taken up by Frenkel (1978) p. 188: "The relationship embodied in the traditional formulations of PPP should not be viewed as a theory of the determination of exchange rates".

8. Cassel (1918) p. 413.


16. See Kalamatousakis (1978) for a discussion of the work of Zolotas in the inter war period: "As a result of these other factors the price of foreign exchange can exhibit wide variations regardless of the purchasing power that it represents. This view is also accepted by Cassel, but he argues that the influence of the factors mentioned above is temporary and cannot last more than a year" p. 165.

18. Ibid, p. 150.
22. Cassel (1922) p. 150.
23. Ibid, pp. 154-6. This possibility is discussed at length in the form of the productivity bias argument.
27. Ibid, pp. 16-18.
28. It should, however, be pointed out that the asset approach would take the view that expectations were rational (and correct) whilst the speculative run view would seem to suggest that they were irrational.
30. See Frenkel and Levitch (1975) for a summary.
31. Risk, in general, and fear of exchange controls, in particular, are stressed by Aliber (1973).
32. See Isard (1978) p. 16.
34. See, for example, J. Tobin, "The Theory of Portfolio Selection" in F. Hahn and F.P.R. Brechling (eds.), "The Theory of Interest Rates" (1965).
40. See ibid p. 237 for a discussion of this. It should be noted, however, that the postulated effect of real incomes on exchange rates is not through a change in demand for imports (and hence foreign currency) as in the model developed here, but via the effect of income changes on the demand for money; this difference actually leads to different expected signs on the income variables to those suggested in chapter four.


42. It should be noted that these two variables have different expected signs to those suggested in the model developed here.


44. Batchelor (1977) p. 54.

45. Ibid, p. 45.


47. In high periods of unemployment when prices were stable or falling it is reasonable to argue that a change in the money supply would have a greater (or at least some) influence on output and employment than on prices. This certainly seems to have been the case in 1932-37 in Britain when there was a sharp recovery in UK GNP at (fairly) constant prices during a period of cheap money and a distinctly rising monetary base and money stock. Moreover, there is sufficient doubt about the validity of monetarism in general to preclude taking a wholly monetary view of exchange rate determination.

48. Cassel, himself was not unaware of this problem and the solution ultimately adopted here is, in fact, that suggested by Cassel (and already discussed earlier in the chapter).


51. This correspondence would be exact under the assumption that prices equal marginal costs.


53. Officer (1976b) p. 569.


56. J. Grunwald and J. Salazar-Carrillo, "Economic Integration, Rates of Exchange, and Value Comparisons in Latin America" in D.J. Daly (ed.) "International Comparisons of Prices and Output" (1972).

58. Yeager (1976) p. 223. See Officer (1976a) p. 17 for a list of exponents of this argument.


60. Ibid., p. 225.

61. This argument is also put forward by Sohmen (1969) pp. 172-3.


63. However, it is only fair to say that such an argument is not completely untenable by any means. For example, in the 1979-81 period, the strong pound no doubt held import prices and hence the general rate of inflation down to some extent.


64A. For example, Sims (1972) for the U.S.A., Williams, Goodhart and Gowlan (1976) for the U.K. and Brillemour and Khan (1979) for a historical treatment of the U.S.A.

64B. See Zellner (1979) pp. 12-21, for a discussion of this.

64C. See ibid., pp. 30-35.


65. See the Statist 15/6/35 p. 997: "The recent devaluation of the Belgian currency is regarded as an effective increase in the tariff, the rates of which are therefore undergoing an offsetting reduction".


72. For example, see the Economist 26/9/31 p. 551 and 18/11/33 pp. 948-9.

73. The data discussed is that provided by Chang (1951) p. 144ff.
75. Nurkse (1945), p. 7. See also Nurkse (1944), p. 126.
76. Whether or not this was bought at the expense of an unacceptable degree of internal imbalance is another question.
77. See Gaillot (1970) Table 2, p. 352.
78. For example, see the Economist 30/5/36, p. 492.
79. This is not how Cassel saw things; his view will be outlined shortly.
81. An alternative rationale for using an index of wages is the theory of cost parity as opposed to, or perhaps as a proxy for, price parity; this is discussed below.
82. Cassel (1918), p. 413.
84. Yeager (1958), p. 522. A number of additional channels through which the prices of traded and non-traded goods are related are given in Batchelor (1977), p. 46.
85. This is the reason why the OPTICA reports (discussed in Genberg, 1978) favoured wholesale prices.
87. Ibid., pp. 303-7.
92. Ibid.
93. Ibid., p. 57 for references and ibid, pp. 11-13 and p. 23 and Samuelson (1964), pp. 149-51 for criticisms.
94. This is the Balassa productivity bias argument again, discussed in this context by Officer (1974), p. 26.
The widespread adoption of floating exchange rates in the 1970's has led to a growth of interest in the performance of floating rates and has consequently generated a large number of empirical studies. Clearly it is not possible to survey all the literature nor is it necessarily desirable to do so since in many cases the objectives differ to those of the present study - for example, an author may be seeking to provide evidence to support or discredit a particular theoretical approach - or the period being examined may not be particularly relevant in the context of the 1930's. In connection with the latter point, an observation made by Yeager (1969) is perhaps instructive:

"...we must beware of excessive generalization about the lessons of historical experience with fluctuating exchanges. Superficially similar experiences may be fundamentally different, even though in ways usually unappreciated and requiring detailed, episodic study. When tempted to draw lessons from experience we should make sure we understand just what the experience was of". (1)

The present study does not seek to test any particular theory in the normal sense - it is not concerned with providing support (or lack of it) for a pure PPP hypothesis or an asset market (or monetary) approach, but instead it attempts to be eclectic because the focus of interest is really on two rather basic (or even crude) and connected questions. Firstly, to what extent were exchange rates "stable" in
the 1930's in the sense of being determined by "economic fundamentals" and secondly, to what extent were they "unstable" in that they deviated from the equilibrium path indicated by these "economic fundamentals"; this second question involves the question of the importance of destabilising speculation. Consequently the studies to be examined will be those that address themselves to these problems.

Returning to the question of the relevance of the performance of exchange rates during different historical periods, there are basically four periods in which exchange rates have floated and hence which have provided data for examining the determinants and stability of floating exchange rates: firstly, there are a few studies of the pre-World War 1 years although these are obviously hampered by data deficiencies; secondly, the inter-war years (particularly the early 1920's) have attracted some interest; a third period is the 1950's during which the Canadian dollar was floating (arguably providing the only example of floating during a "normal" period); finally, the adoption of floating rates in the 1970's has obviously led to a large number of studies which examine the current period.

It is conventional to talk in terms of drawing from historical experience to shed light on contemporary events. In the present context it is rather the other way round in that part of what follows involves examining contemporary experience with a view to deciding whether this can shed any light on events in the 1930's. However, mindful of Yeager's comments (above), it is proposed to only briefly survey the experiences of the 1970's, the 1950's and the pre-1914 period and concentrate mainly on the empirical evidence relating to the inter-war period.
In this section the empirical evidence relating to the pre-World War 1 period, the 1950's and the 1970's will be briefly discussed. Empirical studies of the nineteenth century experience of floating exchange rates are scant although such periods did exist, despite the widespread portrayal of this century as the heyday of the gold standard (and fixed exchange rates). The two main episodes which have been examined are the American "greenback" period (1861-79) during which the U.S.A. was effectively on an inconvertible paper standard with a floating exchange rate - the "gold dollar" remained at par but the greenback price of gold fluctuated - and the floats of the currencies of Austria-Hungary and Russia (1879-91).

The latter were examined by Yeager (1969). A PPP calculation suggested that the (Austrian) guilden was strongly related to relative prices and although it was occasionally subject to non-economic factors (including "a weak psychological connection...with the price of silver"\[2\] and fears of war), it was, on balance, remarkably stable (arguably more so than the Canadian dollar in the 1950's). An important reason for this was arbitrage in international securities.\[3\] There is no suitable price index for Russia and so a PPP calculation is ruled out but it is clear that the ruble fluctuated much more than the guilden. However, according to Yeager, this was due to "the ruble's greater sensitivity....to the psychological magnification of real influences"\[4\] including mistrust of official Russian policy (fear of inflation) and the greater likelihood of Russian involvement in wars and he concludes that "...the foregoing points suggest...\[that\]...the instability of the ruble does not necessarily show that destabilising
speculation ordinarily prevailed in the foreign exchange market". (5)

There are (at least) two relevant studies of the American "greenback" period, one relating to the whole period and the other to a specific incident (the gold crisis of 1869), which is often cited as an example of destabilising speculation. The former study forms chapter two of Friedman and Schwartz (1963) and deals with wider issues, but in the present context, the examination of the PPP relationship in the 1861-79 period and the discussion of deviations from PPP is instructive (pp.61-78). Basically their data suggest that the greenback dollar broadly followed its PPP path throughout the period but was (approximately) 20 per cent undervalued in the 1861-64 period and 10 per cent overvalued in 1865-71 although it was very close to its PPP level in 1873-79.

Nevertheless, these discrepancies do not necessarily furnish evidence of destabilising speculation. In the 1861-64 period speculation (a capital inflow) limited the depreciation and, as after the Civil War the exchange rate did appreciate, "the speculators were ultimately correct and their speculation in retrospect was 'stabilising'". (6) Moreover, although Friedman and Schwartz do not go this far, the continued capital inflow in 1865-71, which caused the greenback to be worth more than its PPP value, could also be interpreted as "stabilising" speculation to the extent that, because the greenback was appreciating (in PPP terms), the capital inflow could be construed as speculators speeding up the adjustment process.

Wimmer (1975) examined the gold crisis of 1869; this involved an attempt to artificially lower the value of the greenback which ultimately backfired and led to the original "Black Friday", (24th Sept.,
1869), a day in which the value of the greenback fluctuated widely. Wimmer criticises the traditional view that this provides a good example of the "evil consequences" of floating exchange rates, and argues that such interpretations are due to "inadequate economic analysis and lack of empirical examination". Indeed, he goes much further and claims that the opposite is true. Forcing the price of the greenback away from its equilibrium level was only made possible, in fact, by very large purchases of gold dollars and "required the strong suspicion among private speculators of government complicity". Furthermore, once the "mistake" was realised, "the market returned to its previous price and stability with amazing speed". Thus, this is more an example of stabilising rather than destabilising speculation.

Turning to the 1950's the float of the Canadian dollar is widely documented and is well-known as a good example of how a floating exchange rate can be stable and quite free of destabilising speculation. It is particularly instructive in that it is the only example of floating which cannot be said to have taken place against a backdrop of uncertainty and international disorder. The conclusion that the Canadian exchange rate was stable is all the stronger because it is reached by different authors via different routes. Ehrnberg (1960) (1964) uses a full scale model and ultimately concludes:

"The model presented in this paper confirms the view that in an otherwise stable economic environment unrestricted capital movements need not be feared as a source of instability". Others estimate a single equation to show that capital flowed in when the dollar depreciated and out when it appreciated thereby stabilising it. Two other points are of interest: firstly, the importance of interest rates in determining the Canadian exchange
rate (13) - these will be included in the model here - and secondly, the fact that it was private short term capital that stabilised the exchange rate not official intervention which was of secondary importance, (14) although it did tend to stabilise the rate (until 1961).

Thus the Canadian dollar in the 1950's provides a good example of a floating but stable exchange rate which was not subject to destabilising speculation. Before leaving the Canadian dollar an obvious question must be dealt with: if the Canadian float was so successful then why was it abandoned in 1961? The answer would seem to lie in the monetary policy of the Canadian government. In late 1960 the government engineered a depreciation in an attempt to reduce unemployment and the main reason why the floating rate was abandoned was because this depreciation got out of control. Basically it was not floating the exchange rate that caused the problems but the Canadian government's monetary policy and its attempt to manipulate the exchange rate:

"The speculation and crises and alternation of heavy gains and losses of external reserves...in the 11 months before adoption of the new fixed parity occurred under a regime of officially manipulated flexible rates, not of free rates". (15)

It is interesting to note that Tsiang in his study of exchange rates in the early 1920's comes to essentially the same conclusion about the franc:

"Thus the instability of the French franc from 1923 to 1926 was the result of an extremely elastic money supply, which would have caused great instability in the economy whether the exchange rate was freely fluctuating or controlled". (16)
Consequently, the ultimate return of the Canadian dollar to a fixed value did not reflect failure of the float but rather the government's inept monetary policy.

Before briefly examining the 1970's, reference should be made to two other relevant episodes which have been documented to a lesser extent than those discussed so far. Ford (1958), in a study mainly concerned with balance of payments adjustment, does not appear to have considered that the Argentinian peso was subject to destabilising speculation in the 1885-1900 period and in a detailed study of the Peruvian float (1950-54) Tsiang concludes:

"From...[1950]...to the end of 1952,...the exchange rate...was remarkably stable...In 1952, however the...rate depreciated by 28 per cent...[but]...exchange rate stability was restored in 1954...." (17)

Thus the Peruvian exchange rate appears to have been stable in four of its five year float and, in fact, its depreciation in 1953 does not appear to have been associated with speculative factors either:

"Such evidence as is available...indicates that, on the whole, speculative capital movements during 1953 were on balance not destabilising". (18)

The general conclusion with regard to the 1970's would seem to be that exchange rate movements have been rather more erratic than might have been expected, particularly in the light of the large volume of academic support for their adoption. This was the conclusion reached by a study of the first two years of the 1970's float(19) and subsequent events have tended to reinforce this view. Whether this constitutes evidence of an inherent tendency to "instability" is perhaps debatable. That the float began from a position of disequilibrium is axiomatic; it constitutes one of the main reasons why the fixed exchange
rate system that preceded it collapsed. Proponents of fixed exchange rates would nevertheless interpret this as history repeating itself and floating rates being proved "unstable" once again but an alternative view of how history has repeated itself is put forward by Yeager who argues that the criticisms of the performance of floating rates "are further examples of something drearily familiar in monetary history: floating exchange rates, left on the scene after fixed rates have broken down, are routinely blamed for the economic disorders that had caused the breakdown and to which the fixed rates had themselves contributed." (20)

Furthermore, there have been a number of important shocks to the economic system, particularly the oil price increases and, in the British context, the impact of North Sea oil. Much can be made of these factors:

"floating exchange rates functioned in an environment in which fixed rates had broken down and could not have been soon restored. The world oil situation brought what has been called 'the biggest international financial crisis since the 1930's' and the largest structural shift in international payments since the German reparation problem after World War 1". (21)

The 1970's is also clearly not a period of freely floating rates. Widespread official intervention (and temporary pegging), the remnants of Bretton Woods and the existence of one-way speculative options in the European "snake" have added to the difficulties. To some extent, therefore one is justified in concluding that floating rates could not have been expected to be "stable" in the 1970's.

However, one can go rather further than this, and argue that, in spite of wide fluctuations, exchange rates have shown some tendency to be influenced by "economic fundamentals". In particular high inflation
countries have tended to have weak currencies (Britain, Italy) while low inflation economies have tended to have strong ones (Germany, Switzerland); in addition, the role of interest rates as an important influence has been apparent, most recently in the context of the strong U.S. dollar in 1981. More formally, a simple monthly model of the pound-dollar exchange rate estimated for the September, 1975 to June, 1980 period found that British and American (consumer) prices and the interest rate differential were significant determinants of the exchange rate and, in fact, predicted a depreciation of the pound in 1981 (although not to the extent that actually occurred).\(^{22}\)

Other studies, which have examined PPP in the post-war period and have extended as far as 1975, have also produced some support for the hypothesis that exchange rates and prices are related (although the purposes of these studies has not been to examine stability of exchange rates).\(^{23}\)

Finally, a certain amount of empirical work on the 1970's has gone into exploring the so-called monetary approach to the exchange rate (which can be seen as an asset market approach in which the main asset is money). To the extent that these studies are principally concerned with providing evidence to support the monetary approach, they are of less interest in the present context. However, given that they explicitly use the PPP hypothesis as one of their "building blocks",\(^{24}\) and that the exchange rate is expressed as a function of relative money supplies, interest rate differentials and levels of real income,\(^{25}\) the apparent success of these models in empirical tests can be construed to some extent, as providing further evidence of the importance of "economic fundamentals" in exchange rate determination. Monetary models also give an important role to expectations which are
incorporated in the model developed here (in the form of dummy variables).

(III)

The bulk of the evidence relating to periods other than the inter-war years can be interpreted as broadly supporting the hypothesis that floating exchange rates are, to a large extent, determined by "economic fundamentals". However, one must be wary of excessive generalisation and obviously the empirical studies relating specifically to the inter-war years are of most interest. There are a substantial number of studies of exchange rates in the 1920's and rather less of the experience of the 1930's. The classic reference for the period is Nurkse (1944). However, whether or not this can really be called an "empirical" study is debateable. Its conclusions about the performance of floating rates would seem to be based on casual observations and are perhaps better described as assertions. In fact, it is precisely the lack of a sound empirical basis in Nurkse's book which provided the starting point of the present study.

Fortunately, a number of authors have examined the period more thoroughly. The studies can be discussed in terms of the two main strands which they tend to highlight (and are of primary interest in the present context): firstly, the importance of "economic fundamentals" in exchange rate determination and, in particular, the validity of the PPP hypothesis; and secondly, the evidence of destabilising speculation (or lack of it). With regard to the former, the debate over the merits of the PPP hypothesis has carried on at both theoretical and empirical levels for years and has already been discussed in some detail (with
particular reference to the theoretical aspects) in chapter two. Stolper (1948) was one of the first to examine an episode in the inter-war period and, using three different price indices (27) to approximate the PPP of the pound against the dollar (1919-25), he concludes that the exchange rate was principally determined by relative prices and any deviation from the PPP rate was usually due to differences in income levels. Tsiang (1959) also looks at the pound-dollar exchange in this period and in addition considers the (Norwegian) kroner-dollar and (French) franc-dollar exchanges. Only in the case of the French franc does he find large and continual deviations from the PPP rate.

More recently, several versions of a simple econometric model, originally developed by Hodgson (1971) (1972), have been used by various authors (28) to examine exchange rate fluctuations in the inter-war years. Its performance is especially relevant because the model used in the present study is basically an extended version of Hodgson's model. It hypothesises that exchange rate fluctuations are determined chiefly by "economic fundamentals", the most important of these being relative price levels (thereby incorporating the PPP doctrine). Other "fundamentals" of importance are relative income levels, relative money supplies (a cash balance effect), interest rate differentials and seasonal factors.

The simplest version of the basic model is given by Thomas (1973a):

\[ ER = f (P_A, P_B, i_A, i_B, Y_A, Y_B) \] (3.1)

where \( ER \) = price of one unit of B currency in terms of A currency
\[ P_A = \text{price level index in A} \]
\[ P_B = \text{price level index in B} \]
\[ i_A = \text{"the" interest rate in A} \]
\[ i_B = \text{"the" interest rate in B} \]
\[ Y_A = \text{the level of real income in A} \]
\[ Y_B = \text{the level of real income in B} \]

He then estimates this in ratio form to reduce multicollinearity and to increase degrees of freedom:

\[ ER = b_0 + b_1 P_r + b_2 i_r + b_3 Y_r + e \quad (3.2) \]

where \( P_r = P_B/P_A \), etc.

Finally, in the presence of serial correlation, he introduces seasonal dummies and a time trend into his final estimating equation which he then estimates for the exchange rates of six countries (Canada, France, Britain, Spain, Sweden and the Netherlands) against the U.S. dollar. In all six cases the relative price variable carried the correct sign (negative) and was highly significant; moreover one or more of the other variables were also significant thereby suggesting that exchange rates were indeed determined by "economic fundamentals" (especially relative prices) in this period.

Hodgson, himself (1971) (1972), came to much the same conclusion in his comprehensive analysis of the sterling-dollar exchange in the early 1920's. Only three detailed case studies of the 1930's experience have come to light: those by Ozmun (1976) on the pound-franc exchange, Hudgins (1973) on the pound-dollar exchange and Ridpath (1975) on the
Canadian dollar - U.S. dollar and Canadian dollar - pound exchanges. A priori one would expect some difficulty in applying this model to the 1930's partly because there was a greater tendency to "manage" the exchanges in this period but mainly because the existence of tariffs, quotas and other trade controls would tend to render the PPP hypothesis inapplicable. However, despite the fact that official intervention (especially British) was found to have played an important role, Hudgins, Ridpath and Ozmun all concluded that "economic fundamentals" were still the major underlying determinants of the exchange rate. In particular, relative prices were very important which lends more support to the PPP hypothesis, and relative income levels were significant although relative money supplies and interest rate differentials did not show up very well.

A much shorter study of the 1930's by Whitaker and Hudgins (1976) also exists. This is basically a modified and extended version of the part of the earlier work by Hudgins dealing with the role of the British EEA which was found to have had a small but significant influence on the pound-dollar exchange. Nevertheless it also concludes that "much... of the variation in the £-£ rate can be accounted for by broad changes in economic conditions and policies bearing on foreign exchange markets"(30) However, it is worth noting that although relative incomes and (marginally) interest rates influenced the exchange rate, relative prices were found to be insignificant.

Finally, some exponents of the monetary approach to the exchange rate have also used the early 1920's as a testing ground, although the number of studies is small and they are mainly concerned with providing support for their theory rather than examining the question of exchange rate stability. Clement and Frenkel (1980) looked at the pound-dollar
exchange rate in the 1921-25 period and Frenkel (1976) examined the German exchange rate during the 1923 hyperinflation. Both studies found some support for the monetary approach and so, to some extent, can be interpreted as suggesting that exchange rates were determined by "economic fundamentals".

Thus there is quite a lot of evidence to support the use of an extended PPP hypothesis as an appropriate framework for examining exchange rates in the inter-war period. Moreover, in a paper testing distributed lag variants of the hypothesis for fourteen currencies against the American dollar in the early 1920's, Hodgson and Phelps (1975) found that, in eleven cases, over ninety per cent of the monthly variation in exchange rates was explained by lagged price level movements. Although they made no attempt to disentangle the effects of speculation from those of ordinary trade-induced adjustments of exchange rates to price level movements they are implicitly suggesting that speculation was probably stabilising (and speeded up the adjustment process). However, many people would dispute this contention that foreign exchange speculation was mainly stabilising in the inter-war period; indeed, the argument that the adoption of flexible exchange rates would generally lead to great instability due to destabilising speculation is one of the main pillars of the case against flexible rates.

There are a variety of definitional and measurement problems involved in dealing with speculation in the foreign exchanges and these are discussed at length in chapter four. In the present section, the main concerns are the results and conclusions of the studies of speculation. Perhaps the most extensive of these is provided by Aliber (1962) who looks at five exchange rates in the early 1920's:
the pound, the French franc, the Belgian franc, the Dutch guilder and the Swiss franc (against the U.S. dollar, the U.S. dollar, the French franc, the pound and the U.S. dollar, respectively). His conclusions are rather gloomy, the principal one being that speculation was incapable of keeping fluctuations small in the early 1920's; speculators as a group (especially in the forward market) showed little flexibility and hence did little to dampen exchange rate movements caused by political crises and temporary economic dislocations. He bases his observations on speculation on an examination of changes in the differential between spot and forward exchange rates (which is a proxy for changes in speculative expectations and hence speculation itself) and he backs these up by evidence derived from comparing actual exchange rates to PPP rates.

He quotes the French cases as a very good example of destabilising speculation eventually providing its own justification, and the Belgian experience as "an interesting example of the difficulty of maintaining economic independence in a system of flexible exchange rates", the implication being that it was a bad experience since the Belgian franc was dragged along in the wake of the French franc because of the close relationship between the two economies and the strong expectations that the two currencies should, and eventually would, exchange on a one-to-one basis. Moreover, although both the Netherlands and Switzerland avoided the circle of (unstable) depreciating currencies and rising prices and eventually returned to their pre-war parities, their economies were very similar to those of France and Belgium in some ways and, had it not been for extensive intervention in the exchange market by the authorities and (more important) strong expectations of a return to the pre-war parity with sterling on the
part of the speculators, they would probably have succumbed to cumulative destabilising speculation in much the same way as the French and Belgian francs. Nor was the pound free from speculation; though most speculative pressure was upward and could not be described as destabilising, it could not be said to be stabilising either, and in fact ultimately led to the pound returning to the gold standard at too high a level.

Aliber's conclusions were the most strongly critical of flexible exchange rate systems of all the empirical work surveyed. While there is fairly general agreement that the French franc suffered from destabilising speculation in the early 1920's (see below), nowhere (other than in Nurkse (1944), of course) is general condemnation of flexible rates and the implication that destabilising speculation either occurred or nearly occurred in most cases expressed except in Aliber's work. Not surprisingly he has been strongly criticised, notably by Pippenger (1975) on two counts: firstly, Pippenger argues that his choice of price indices and method of calculating changes in price levels and exchange rates is such as to exaggerate fluctuations and hence the degree of instability; secondly, Aliber implicitly assumes that the only reason for a discount or a premium in the forward market (adjusted for interest rate differentials) is speculation; Pippenger disagrees and argues that various other factors may cause this. Several other criticisms also come to mind: Aliber does not use rigorous econometric methods like Hodgson and others; also he uses deviations from the PPP rate as back up evidence ignoring the fact that such deviations may be caused by non-speculative variables such as changes in relative income levels. Finally, Aliber only really provides evidence of destabilising speculation in the case of France
and Belgium (which is arguably a special case because of the close relationship between the French and Belgian francs and the expectation that they should exchange on a one-to-one basis); consequently, his ensuing generalisations are based more on hypothesis than fact.

Thomas (1973a) (1973b) also considers the possibility of destabilising speculation in the early 1920's. He conducts two tests for the currencies of twelve countries against the U.S. dollar from January 1920 to June 1924. He defines destabilising speculation as occurring if speculators cause the magnitude of exchange rate fluctuations to exceed that which would exist in the absence of speculation. His first test assumes that deviations of the actual exchange rate from the PPP rate were caused by speculation. Thus if speculation causes the exchange rate to change by more than is indicated by changes in relative prices then it is destabilising in the sense defined above. It would therefore be possible to test equation (3.3) to see if $\beta_1$ is greater than unity.

$$\log ER = \alpha_1 + \beta_1 \log P + \epsilon_1$$  \hspace{1cm} (3.3)

(where the variables are as defined above)

However, this test is open to criticism as it ignores the effect of other non-speculative variables and may therefore bias $\beta_1$ downwards and so equation (3.4) is estimated to see if $\beta_2$ is above unity.

$$\log ER = \alpha_2 + \beta_2 \log P + \beta_3 \log i + \beta_4 \log Y + \epsilon_2$$  \hspace{1cm} (3.4)

Only the French franc-dollar exchange rate provided a $\beta_1$ or $\beta_2$ above unity.

The second test involves the use of an adaptive expectations model.
Given the importance of relative price levels, a plausible equation showing exchange rate determination is given below:

\[ ER_t = a_1 P_t^p + a_2 P_t^E \]  \hspace{1cm} (3.5)

where \( P_t^p \) = the current ratio of price indices of the two countries
\( P_t^E \) = speculators' current notion of the "expected" or "normal" price ratio

Suppose that \( P_t^E \) is revised during each time period on the basis of the deviation between the currently observed price ratio and that which had been expected in the previous period:

\[ P_t^E - P_{t-1}^E = \beta (P_t - P_{t-1}^E) \]  \hspace{1cm} (3.6)

The greater the value of \( \beta \), the more sensitive is the expectations adjustment process; thus if \( \beta \) is substantially greater than zero then an exogeneous increase in the price ratio induces a large upward revision of the "expected" price ratio (and hence increases speculation). He therefore derives a method of estimating the value of \( \beta \) and finds only two countries (France and Italy) where \( \beta \) is substantially greater than zero and as both of these experienced high, sustained inflation in the period in question this is arguably not surprising.

Tsiang (1959) simply looks at deviations of the actual exchange rate from the PPP rate for the pound (1919-25), the Norwegian kroner (1919-26) and the French franc (1919-26), all against the dollar. In the case of the pound he finds the two rates move very closely together from 1922 onwards; in the period 1919-21 he finds three sharp twists in the actual rate only one of which attributes to speculation. These three sharp movements also occur in the Norwegian and (more severely)
in the French rates and he attributes them to similar factors. From 1922 onwards, however, in both the latter cases, the actual rate tends to deviate progressively more from the PPP thus indicating (on the assumption that all such deviations are due to speculation) the presence of destabilising speculation. In Norway this progressive deviation was arrested by a vigorous tight money policy but in France an extremely elastic money supply with rigid interest rates allowed it to continue. Thus Tsiang puts the blame for the French situation from 1924 to 1926 on government policy (and also political instability) and argues that there would have been difficulties even under fixed exchange rates.

Stolper (1948), as reported above, has argued that deviations of the actual exchange rate of the pound in the early 1920's from its PPP rate were due not to speculation but to differences in relative income levels. In fact, he is quite explicit about the (lack of a) role for speculation:

"As far as speculative movements of funds are concerned it must be stated categorically that except for one period in 1924 they cannot be considered to have played a major part in the determination of the actual rate". (35)

Grissa (1967), in an extensive study of speculation in the early 1920's, comes to a similar conclusion:

"The experience of the franc and pound, therefore, rather support Friedman's hypothesis that given the stability of the underlying economic conditions, flexible exchange rates would tend to be stable". (36)

His basic conclusions are that the pound was "stable" (and not subject to destabilising speculation) from 1921 to 1925 and the franc was "stable" from 1921 to 1924 but not from 1924 to 1926; however, this
instability was due to government policy rather than speculation.

It would seem that there is now substantial evidence to suggest that, at least in the 1924-26 period, the French franc was subject to destabilising speculation. However, the argument that destabilising speculation affected any other countries in the inter-war years receives virtually no support at all, and furthermore, whether or not the case of the French franc is in any way indicative of the general pattern that might be expected when exchange rates are floating is highly contentious. It seems that it was largely the expansive monetary policy of the French government which caused the problems rather than any inherent tendency for destabilising speculation to occur during periods of floating exchange rates (the apparent lack of destabilising speculation in other currencies also supports this hypothesis) and at least one author, (Tsiang, 1959), is of the opinion that similar difficulties would have occurred even if the French exchange rate had been fixed.

An interesting final comment on the French franc in the early 1920's is provided by Eichengreen (1980), who uses a modified monetary approach to explicitly examine the assertion that the franc was subject to destabilising speculation. He generates a counterfactual exchange rate, based on non-speculative determinants of the franc-pound exchange rate, and compares it to the actual rate. He found that speculation could be considered both stabilising and destabilising: stabilising, in the sense that over the entire period speculation decreased the variance of the exchange rate, but destabilising, in that speculation may have increased the variability of the exchange rate around its trend.
Finally, in the same (monetary) vein, it is worth quoting the views of an exponent of the monetarist approach to exchange rate determination, which are contained in an article which examines past periods of floating exchange rates (in much the same way as this chapter has attempted to do):

"Furthermore, our own data and the opinion of most other economists writing about these periods, with the exception of Nurkse (1944), seem to agree about the conclusion that there is very little evidence of inherent instability and destabilising speculation, except for Germany under hyperinflation and possibly Russia in the nineteenth century and France in the 1920's. In the latter two cases available information indicates that a lack of control of the money supply was probably the main reason behind this". (37)

The brief survey of the importance of destabilising speculation in periods of floating exchange rates contained in the present chapter would suggest a similar conclusion.

(IV)

There is a great problem of interpretation in looking at past periods of floating exchange rates with a view to using this evidence as a basis for theorising about particular sub-periods. In particular, different conditions may render any particular episode inappropriate as a basis for examining another and it may well be that every experience should be treated as being unique. Nevertheless, it is clear that, in spite of the differences between particular "floats", much of the evidence, and indeed most of the econometric evidence, suggests that floating rates were normally not subject to destabilising speculation but were generally "stable" in the sense of being determined by "economic fundamentals". In fact, in the inter-war years, the only alleged occurrence of destabilising speculation whose existence
would receive widespread support, is the experience of the French franc from 1924 to 1926, and even then most authors would be inclined to blame the instability of the franc on government monetary policy, rather than any inherent tendency towards destabilising speculation.

This conclusion and the theoretical survey (presented in chapter two above) would suggest that it is not unreasonable to hypothesise that exchange rates are determined by "economic fundamentals", most especially relative prices but also relative incomes and interest rate differentials. The most under-researched period (and the one therefore most subject to assertion rather than conclusions based on empirical evidence) is the 1930's. Consequently, the following section (Chs. 4-8) attempts to develop and test a model of exchange rate determination based primarily on the assumption that exchange rates were largely determined by "economic fundamentals", even in the 1930's, arguably the most turbulent period of floating exchange rates with the exception of the 1970's.
FOOTNOTES TO CHAPTER 3.


2. Ibid, p. 67.

3. A depreciation of the guilden led to cheaper Austrian securities which attracted a demand for them and hence for guilden with which to buy them, thereby offsetting the original depreciation.


8. Ibid, p. 120.

9. Ibid.

10. See Yeager (1976) p. 551 n.19 for a list of references.


24. Frenkel (1976) p. 208. The other main building block is the standard monetarist assumption that money supplies determine prices.
25. This is the relationship tested for the 1972-75 period in Ch. 6 of Frenkel and Johnson (1978).

26. Nurkse's study is criticised in detail in Ch. 1.

27. The wholesale price index of all goods, the wholesale price index of raw materials only and a cost of living index. Most of the studies discussed in this section used wholesale price indices (of all goods). The appropriate choice of price index has been discussed in detail in Ch. 2.


29. This problem is discussed in chapters 2 and 4.


32. Pippenger (1973) pp. 613-4. Aliber's use of a retail price index in the French cases is especially criticised on the grounds that this particular index is a poor one.

33. Ibid, pp. 616-8. Furthermore, it may be argued that Aliber employs a very advantageous double standard: on the one hand, he dismisses the use of deviations from the PPP parity as his main measure of speculation because they may understate speculation (Aliber 1962, p. 179) but on the other hand, he omits to tell us that the method he uses to measure speculation may overstate it. (This is Pippenger's point).

34. France, England, Belgium, Denmark, Netherlands, Norway, Sweden, Spain, Italy, Switzerland, Canada, Japan.

35. Stolper (1948) p. 244.


SECTION II: BILATERAL EXCHANGE RATES
CHAPTER 4

A MODEL OF EXCHANGE RATE DETERMINATION

The starting point of the model to be developed below is the PPP hypothesis which, in its least dogmatic form, states that relative prices are the major determinant of exchange rates. This is normally postulated as a long run theory and, as such, it is fairly widely accepted in some general sense. However, the role of prices in short run exchange rate determination is less widely acknowledged and requires some discussion. In the first place, to the extent that the short run exchange rate is a function of the long run exchange rate \(^{(1)}\) an indirect role for prices in the determination of the former clearly exists. A related point is that if the relationship between prices and exchange rates is primarily a long run one then the latter can presumably be taken to be a function of lagged values of the former, one of which (in a monthly model) is that of the previous month and so even according to a long run version of the PPP hypothesis relative prices in the immediate past would have some role to play in the determination of the current exchange rate.

In any case, an eclectic view of exchange rate determination allows recourse to other theoretical frameworks which may suggest a short lag on prices is most appropriate. A balance of payments approach (which is further developed below) would indicate a very short lag on prices as, in buying and selling, people are reacting to current or very recent price levels. \(^{(2)}\) A wide version of the
asset market approach may also suggest a short lag on prices as people adjust their portfolios in accordance with a number of variables including (presumably) all current (or very recent) prices. A final, possible, theoretical reason for a short lag on prices relates to the role of stabilising speculation. If the traditional long run PPP relationship between prices and exchange rates is recognised then any change in current prices (probably after a small lag) will have an immediate impact on exchange rates as (stabilising) speculators behave in such a way as to push the exchange rate towards its (new) long run PPP equilibrium.

Turning to the empirical evidence, the majority of the studies of the inter-war period (discussed in the previous chapters) found a strong relationship between exchange rates and either current prices or prices lagged one month. This would strongly suggest the use of a similar lag in the present study. However, such evidence is rather negative in that it does not actually test for any lag structure. Fortunately more positive evidence is available: in the first place, some of the above studies do, in fact, try a variety of lags - for example, Hodgson (1971) tries lags of up to four months and eventually selects a one month lag on the grounds that it gives the best fit. Secondly there is one fairly extensive study (of fourteen exchange rates) of the early 1920's which explicitly tested the hypothesis that exchange rates were determined by lagged prices and came to the following conclusions:

"The lag weights...suggest that the peak effect of price levels on exchange rates occurred within the initial month... for six countries, and in the first month following the initial disturbance for five countries, and in the second month following the initial disturbance in the case of one country". (4)
"In all fourteen cases the major impact of price level movement occurred within the first three months". (5)

This clearly supports a very short lag on prices. That study used a Koyck lag; in the course of the present study an Almon-type lag was applied to the pound-dollar exchange rate in the 1930's (not presented in the thesis but further discussed below) and, although the results were not particularly good they also suggested that the major impact of prices was in the first few months. Thus there is sufficient theoretical and empirical foundation to merit the use of a very short lag on the relative price variable.

The question now arises as to what variables should be used to "extend" this PPP hypothesis. The preceding two chapters would suggest a number of other "economic fundamentals" which might plausibly be hypothesised to have had a role in exchange rate determination, on theoretical or empirical grounds (or more usually both). These would include relative income levels, interest rate differentials and probably some variable to reflect seasonal factors. In addition official intervention in the foreign exchange market was found to be important and must clearly be incorporated. Finally, the inclusion of a trend variable would seem to be appropriate. However, this is all very unsystematic and ad hoc, although in principle, would appear to test the relatively simple hypothesis - that exchange rates were largely determined by "economic fundamentals" - quite adequately. Nevertheless, a much more satisfactory theoretical framework can be provided by an alternative route, namely the balance of payments view of exchange rate determination. A model explicitly along these lines was developed and tested by Hodgson (1971) (1972) and used by Hudgins (1973), Ozmun (1976), Ridpath (1975) and Thomas (1973a). It is this
model which underlies the one used here and it can be viewed as incorporating the PPP hypothesis as one of its main arguments. The main modification is that the present version explicitly allows for third country effects and is therefore more complete.

(II)

The Hodgson model is couched in terms of supply and demand for the currency in question (pounds). A two country model is used (U.K. and U.S.A.) and three sources of currency transactions are recognised corresponding to the current, capital and official transactions accounts of the balance of payments. Thus the first source of supply and demand for pounds is derived from trade in goods and services. The demand function is effectively a British export function - foreigners demand pounds to buy British goods and services; similarly the supply function is a British import function. The main explanatory variables are therefore relative price and (real) income levels; a trend variable is also included to reflect changes in tastes and seasonal variables are added. Thus current account currency flows are represented as follows:

\[
D_{\text{UK}}^T = f (P_{\text{UK}}, Y_{\text{US}}, T, Z) \tag{4.1}
\]

\[
S_{\text{UK}}^T = f (P_{\text{US}}, Y_{\text{UK}}, T, Z) \tag{4.2}
\]

where

- \(D_{\text{UK}}^T\) = demand for pounds
- \(S_{\text{UK}}^T\) = supply of pounds
- \(P\) = a price index
- \(Y\) = real income
- \(T\) = a trend variable
- \(Z\) = seasonal variables
Subscripts "UK" and "US" indicate British and American variables respectively.

Currency transactions will also be generated by "trade" in assets and their second source is therefore capital flows. Interest rate differentials may be important here and Hodgson (1971), Hudgins (1973), Ridpath (1975) and Ozmun (1976) also postulate a cash balance effect: for example, a rise in the supply of cash balances in Britain (not matched by an increase in demand) will tend to increase the demand of British residents for foreign currency as they increase their holdings of alternative assets, including foreign assets, in an attempt to re-establish their portfolio balance. Hodgson (1971) also included variables to represent "fears of capital loss" and "prospects for capital gain" while Ozmun included a similar variable to represent "psychological forces", and this procedure will also be followed here. The two structural equations generated are thus:

\[
D^K_L = f \left( i_{US} - i_{UK}, M_{US}, S \right) \tag{4.3}
\]

\[
S^K_L = f \left( i_{US} - i_{UK}, M_{UK}, S \right) \tag{4.4}
\]

where \( i \) = the interest rate

\( M \) = the money supply

\( S \) = a variable reflecting "speculative" factors

Finally there is the government sector consisting mainly of official intervention in the foreign exchange market (by central banks or by exchange intervention funds):

\[
D^G_L = f \left( I_{UK}, I_{US} \right) \tag{4.5}
\]

\[
S^G_L = f \left( I_{UK}, I_{US} \right) \tag{4.6}
\]

where \( I \) = official intervention
Equating supply and demand to give equilibrium, introducing the exchange rate \( R \) to convert variables from dollars to pounds where necessary and solving for the exchange rate gives:

\[
R = f \left( P_{UK}, P_{US}, Y_{UK}, Y_{US}, M_{UK}, M_{US}, i_{US} - i_{UK}, I_{UK}, I_{US}, T, z, s \right) \quad (4.7)
\]

Hodgson, Hudgins and Ozmun all go on to estimate linear approximations of this function.

Before extending the model to include third country effects three modifications to this two-country version would seem appropriate. Firstly, and most important, the inclusion of a money supply variable is rather questionable. It seems highly likely that the money supply is correlated with price levels, income levels and possibly interest rates also; consequently its inclusion will lead to multicollinearity. Moreover, not only does a money supply variable create technical difficulties, but the inclusion of both money supply and price variables would be theoretically incongruous to some, particularly those who espouse the monetary approach to exchange rate determination. It is also interesting to note that whilst Hodgson and Hudgins both include relative money supplies in their theses (1971) (1973), both exclude it in the published versions of their work. In view of all this, it seems prudent to drop the money supply variables in the present study.

A second modification relates to the role of invisibles in the trade sector; this is important conceptually and in many cases (such as Britain) empirically as well. The problem is that whilst it is clear that the demand for goods is determined largely by prices and levels of income it is not immediately obvious why this should be true of the demand for services; therefore some discussion of the
demand for currency for the purpose of "buying" and "selling" invisibles seems appropriate. In fact there turns out to be no need for additional variables.

The three major components of invisible trade for the countries in the study were shipping receipts and payments, interest and dividends from overseas investment and to foreign owners of domestic investments and tourist receipts and payments. Although it is possible to put forward specific variables which determine the supply of and demand for invisibles - relative costs of shipping freight and so on - it is impossible to collect any suitable data and in any case unnecessary since all three types of invisible flow are likely to be correlated with income levels which are already included in the model: earnings from overseas investments can be divided into two main components - dividends from equity investments and incomes from fixed interest bonds - both of which are likely to rise during prosperity (and fall during recessions), the former because of high (low) profits, the latter because of disappearance (appearance) of defaults; shipping receipts are related to traffic carried which depends on the volume of trade which in turn depends on the level of prosperity (real income); the relationship between tourist receipts and expenditures with income levels is obvious. In fact, in the case of Britain, Chang (1951) found such a relationship between invisibles and income for the 1924-38 period (annual data) when he regressed the volume of shipping services, interest and "other" receipts on income. (7)

Thus, in the trade (or current account) sector of the model, supply of and demand for currency due to flows of invisibles can be viewed as largely determined by the income variables. A third (and
final) modification to the two country model involves the introduction of a trend variable in the capital account (as well as the current account) section. This is to reflect the possibility of a gradual capital flight from Europe to America in particular, due to increasing fears of war which may be important for European currency-dollar exchange rates. In practice this requires no new variable but simply introduces an alternative interpretation of the trend variable (war scare). Another possible interpretation of the trend variable which could be mentioned at this point, is that it may proxy relative productivity growth which may have been greater in some countries than others.

A major criticism of the simple model described above (even incorporating the modifications) is that it excludes the effects of third countries. In the context of the market for pounds demand will emanate not only from America but also from other ("third") countries; thus the pound-dollar exchange rate could be influenced, for example, by the relative demand for pounds and dollars in France. Such possibilities were excluded by Hodgson and others mainly on the grounds of simplicity: in order to keep the number of explanatory variables down to a reasonable level, weighted averages of prices and incomes of third countries would have to be calculated which would "involve an extensive research project in itself... and so a second best ...procedure appears warranted". While some solace may be drawn from Stolper's contention that events on the continent had little effect on the pound-dollar exchange rate in the 1920's, there is no justification for simply assuming this was also true of the 1930's; indeed the presence of serial correlation in Hudgin's estimates of the pound-dollar exchange rate in the 1930's could be interpreted
as indicating the existence of omitted variables, some of which could be third country variables.

Moreover, an easy way of avoiding the dilemma of too many countries would seem to be to use just a single country as a proxy for the rest of the world, and in a world currency market dominated, as it was between the wars, by three major currencies - the pound, franc and dollar - the choice of an appropriate, representative third country is not too difficult. Once this problem of obtaining satisfactory data is overcome, the case for explicitly including third country effects on the grounds of its conceptual importance becomes overwhelming. There are also good econometric reasons for this: if the included and omitted (third country) explanatory variables are correlated then the estimators of the constant term and the coefficients on the included variables will be biased and inconsistent and, more importantly, if they are not (which should be the case if multicollinearity is to be avoided when they are included) then although the estimates will be unbiased, their variance will contain an upward bias possibly leading to rejection of significant variables.

With all this in mind an extended version of the model is presented below using the pound-dollar exchange rate as an example. The structural equations are very similar to those for the two-country version except that the money supply variables have been dropped and there are now two sources of demand for each currency and three markets: the pound, the dollar and the ROW's (rest of the world's) currency.
(I) Market for Pounds

\[ \£^\text{US}_D = f (R, P^\text{UK}, P^\text{US}, Y^\text{US}, i^\text{US}-i^\text{UK}, I^\text{UK}, I^\text{US}, S, Z, T) \]  
(4.8)

\[ \£^\text{US}_W = f (E^\text{ER}, P^\text{UK}, P^W, Y^\text{W}, i^\text{W}-i^\text{UK}, I^\text{UK}, I^\text{W}, S, Z, T) \]  
(4.9)

\[ \£^\text{US}_S = f (R, P^\text{UK}, P^\text{US}, Y^\text{US}, i^\text{US}-i^\text{UK}, I^\text{UK}, I^\text{US}, S, Z, T) \]  
(4.10)

\[ \£^\text{US}_W = f (E^\text{ER}, P^\text{UK}, P^W, Y^\text{W}, i^\text{W}-i^\text{UK}, I^\text{UK}, I^\text{W}, S, Z, T) \]  
(4.11)

\[ \£^\text{US}_D + \£^\text{US}_W = \£^\text{US}_S + \£^\text{US}_W \]  
(4.12)

(II) Market for Dollars

\[ \$^\text{UK}_D = f (R, P^\text{US}, P^\text{UK}, Y^\text{UK}, i^\text{US}-i^\text{UK}, I^\text{UK}, I^\text{US}, S, Z, T) \]  
(4.13)

\[ \$^\text{W}_D = f (E^\text{ER}, P^\text{US}, P^W, Y^\text{W}, i^\text{W}-i^\text{US}, I^\text{US}, I^\text{W}, S, Z, T) \]  
(4.14)

\[ \$^\text{US}_S = f (R, P^\text{UK}, P^\text{US}, Y^\text{US}, i^\text{US}-i^\text{UK}, I^\text{UK}, I^\text{US}, S, Z, T) \]  
(4.15)

\[ \$^\text{US}_W = f (E^\text{ER}, P^\text{UK}, P^W, Y^\text{W}, i^\text{W}-i^\text{US}, I^\text{US}, I^\text{W}, S, Z, T) \]  
(4.16)

\[ \$^\text{UK}_D + \$^\text{W}_D = \$^\text{US}_S + \$^\text{US}_W \]  
(4.17)

(III) Market for ROW's currency

\[ \$^\text{UK}_D = f (R, P^\text{US}, P^\text{W}, Y^\text{UK}, i^\text{W}-i^\text{UK}, I^\text{UK}, I^\text{W}, S, Z, T) \]  
(4.18)

\[ \$^\text{US}_D = f (E^\text{ER}, P^\text{US}, P^W, Y^\text{US}, i^\text{W}-i^\text{US}, I^\text{US}, I^\text{W}, S, Z, T) \]  
(4.19)

\[ \$^\text{US}_S = f (R, P^\text{UK}, P^\text{US}, Y^\text{US}, i^\text{US}-i^\text{UK}, I^\text{UK}, I^\text{US}, S, Z, T) \]  
(4.20)

\[ \$^\text{US}_W = f (E^\text{ER}, P^\text{UK}, P^W, Y^\text{W}, i^\text{W}-i^\text{US}, I^\text{US}, I^\text{W}, S, Z, T) \]  
(4.21)

\[ \$^\text{UK}_D + \$^\text{US}_D = \$^\text{US}_S + \$^\text{US}_W \]  
(4.22)
where:

**Endogenous variables**

\[ \begin{align*}
& \text{Endogenous variables} \\
& \$D_{US} = \text{demand for pounds by USA} \\
& \$D_{W} = \text{"" by ROW} \\
& \$D_{UK} = \text{"" dollars by UK} \\
& \$D_{W} = \text{"" by ROW} \\
& \$D_{W} = \text{"" ROW'S currency by UK} \\
& \$D_{US} = \text{"" by USA} \\
& \$S_{US} = \text{supply of pounds (by UK) to USA} \\
& \$S_{W} = \text{"" to ROW} \\
& \$S_{US} = \text{"" dollars (by USA) to UK} \\
& \$S_{W} = \text{"" to ROW} \\
& \$S_{W} = \text{"" ROW's currency (by ROW) to UK} \\
& \$S_{US} = \text{"" to US} \\
& R = \text{pound-dollar exchange rate} \\
& \text{ER = effective exchange rate of UK (against ROW)} \\
& \text{EER = \"" of US \""} \\
\end{align*} \]

**Exogenous variables**

\[ \begin{align*}
& \text{Exogenous variables} \\
& P^* = \text{an index of prices} \\
& Y^* = \text{an index of real income} \\
& \sqrt{I^*} = \text{an interest rate differential} \\
& I^* = \text{official exchange rate intervention} \\
& S = \text{a variable reflecting "speculative" factors} \\
& Z = \text{seasonal variables} \\
& T = \text{a trend variable} \\
& (P^* = P_{UK}^*, P_{US}^*, P_{W}^*, \text{etc.}) \\
\end{align*} \]

Subscripts UK, US and W refer to British, American and ROW variables respectively.
Solving for \( R \) would give the partially reduced form:

\[
R = f (P^*, Y^*, \sqrt{1 - P^*}, I^*, ER, EER, S, Z, T)
\]

The presence of endogenous, explanatory variables would make OLSQ (ordinary least squares) estimators of this equation inconsistent but fortunately, this is not true of the fully reduced form:

\[
R = f (P^*, Y^*, \sqrt{1 - P^*}, I^*, S, Z, T)
\]

There is still an identification problem but this is not crucial as it is not necessary to derive the structural coefficients (from the reduced form coefficients) to test the hypotheses that represent the focus of the study. Consequently the empirical chapters will concentrate on OLSQ estimates of equation (4.24).

A further question relates to whether or not equation (4.24) should be assumed to be linear or log-linear. A priori, there is no particular reason for preferring one to the other. In fact it may be preferable to try both and let the data decide the most appropriate form. Certainly, it would be interesting to use a log-linear form as this would provide information about elasticities. This would be especially useful in connection with relative prices, as the long run PPP hypothesis would predict a price elasticity of unity and presumably, therefore, the logarithm of relative prices in the model developed here, would be expected to have a coefficient no greater than unity. Indeed, the value of price elasticities of exchange rates has been used by one author as a test for destabilising speculation in that a coefficient greater than unity could be interpreted as implying that the exchange rate was "overshooting". Unfortunately the fact that some of the variables take negative values precludes
any log-linear form from being tested (since the logarithm of a negative quantity is not defined).

One final modification to equation (4.24) is required. This involves converting it to ratio form for two reasons: firstly, and least important, to increase the degrees of freedom, and secondly, to shift this BOP view of exchange rates closer towards a relative PPP hypothesis. Thus, the study will provide OLSQ estimates of a linear approximation of equation (4.24) in ratio form, that is, for the pound-dollar exchange rate:

\[
R = a_0 + \beta_1 \left( \frac{P_{US}}{P_{UK}} \right) + \beta_2 P_W + \beta_3 \left( \frac{Y_{US}}{Y_{UK}} \right) + 4 Y_W + \beta_5 (i_{US} - i_{UK}) + \beta_6 (i_W - i_{UK}) + \beta_7 (i_W - i_{US}) + \beta_8 I_{UK} + \beta_9 I_{US} + \beta_10 I_W + 11 S \beta_{12} Z + \beta_{13} T + \epsilon
\]  

(4.25)

At this point in an attempt to stress the eclectic nature of this balance of payments/extended PPP model, it is interesting to note its similarities (and differences) with the monetary approach, especially as adopting the latter was explicitly rejected in chapter two. A typical monetary model is to be found in Frenkel (1980):

\[
R = f (M, M^*, Y, Y^*, \pi)
\]  

(4.26)

where \( R \) = the exchange rate

\( M \) = domestic money supply

\( M^* \) = foreign money supply

\( Y \) = domestic real income

\( Y^* \) = foreign real income

\( \pi \) = a variable to proxy expectations
Substituting prices for money supplies (especially cost of living indices), \(^{(11)}\) which in an empirical sense would presumably not be entirely unobjectionable to monetarists, produces an equation not too dissimilar to (4.25), especially as a speculative/expectations variable is not only included in the latter but in some regressions is actually based on the forward exchange rate which is the indicator of expectations favoured by Frenkel. \(^{(12)}\) Furthermore, a more general asset approach would presumably also incorporate interest rates in some form creating even greater similarities.

However, there are important differences, most notably a monetary model would suggest the opposite sign for the real income variables \(^{(13)}\) and also, in certain circumstances, for the interest rate variables; \(^{(14)}\) in fact, this observation may be useful, to the extent that it may indicate that "wrongly signed" incomes and interest rates should be interpreted not as a failure of "economic fundamentals" to show up "correctly" in the regressions but rather as supporting a monetary view of exchange rate determination (which still precludes Nurkse's hypothesis of unstable exchange rates in the 1930's). In any case, the very fact that a number of the independent variables in equation (4.25) would also be included in a monetary model (albeit with different expected signs) would further support the use of the model developed here as a simple test of the Nurkse hypothesis (which is primarily what is intended).

A final area of interest in specifying the actual structure of the model is the length of lags. This has already been discussed in connection with relative prices above (Section I) and it is not proposed to engage in an extended discussion here. Clearly several variables should be unlagged: official intervention is obviously on
a day to day (or even hour to hour) basis and therefore the intervention dummies should not carry a lag.

In addition a short lag on most of the other variables seems appropriate both on theoretical grounds:

"It is generally accepted that in the short run disturbances in income levels, capital flows and seasonal factors have a significant effect on exchange rates". (15)

and empirical grounds: Hodgson (1971), Hudgins (1973) and Ozmun (1976) all experiment to some extent and all conclude that short (one month) lags give the best results "in terms of producing the lowest error-variance in the fit". (16) Hudgins also tried a Koyck lag but discontinued it because "the estimated coefficient of the lagged exchange rate implied either an unstable adjustment mechanism or one implying an implausibly long lag in adjusting the exchange rate to the other variables." (17)

Relating specifically to prices, further reference can be made to the experiments involving the fitting of Almon lags (18) to estimates of the pound-dollar exchange rate in the 1930's (undertaken in the present study). The drawbacks and limitations of this approach are well known (19) but, in the present context, a more important point is that the results of the Almon lag experiments suggested that a short lag on prices was more appropriate anyway. Furthermore, introducing additional lagged price terms did not significantly improve the performance of the model (in terms of increasing the value of the coefficient of determination). Finally, whether a detailed specification of the lag structure is necessary (especially in view of the evidence indicating the primary importance of short run effects) in order to test the relatively simple hypothesis presented here -
basically whether or not exchange rates were determined, to any significant extent, by "economic fundamentals" - is dubious. Consequently, it was decided to use a one period (month) lag for the price and income variables and current values of all the other variables.

Having described the model and decided on the form of estimation and lag structure, something needs to be said about the expected signs of the independent variables. In fact, this will vary according to whether or not the exchange rate is expressed in foreign currency per unit of domestic currency or vice versa. In other cases it will depend on how the variable is constructed: for example, whether the pound-dollar official intervention dummy is positive (negative) or negative (positive) when the pound (dollar) is being supported will determine its expected sign. Therefore, the expected signs of particular variables will be made clear where the empirical results are presented. The only general point that can be made at this stage is that the relative price and relative income variables will always be constructed so as to have expected negative signs.

Finally, it should be observed that as far as estimation of this model is concerned, there is one equation too many in the sense that if, in a three currency model, two exchange rates are estimated then the third, has also been implicitly estimated (via cross-rates); the third set of estimates is therefore redundant. There are still, however, a number of good reasons for estimating the third leg of an exchange rate triangle. In the first place it would seem better to directly test whether the model "works" for any given exchange rate rather than rely on an indirect test using cross-rates. In addition, such a test allows direct observation of the signs and size
of coefficients of the independent variables which determine the third exchange rate; this is extremely useful as it avoids having to attempt to work out these coefficients from the regressions of the other two. It may be argued that "correct" signs of variables in these other two regressions implies "correct" signs for the third exchange rate but, not only may ambiguities arise when only some of the variables were correctly signed but also, in some cases - particularly exchange fund dummies and speculative dummies - the actual size of the coefficient is important. It would seem, therefore, that if the hypotheses developed here are to be tested thoroughly then it is desirable to estimate all exchange rates directly.

Another factor that would reinforce this conclusion even more, is the possibility of disorderly cross-rates, which would imply that the coefficients of the independent variables and the predicted exchange rate derived indirectly through a cross-rate (based on regressions of the other two exchange rates) would be different to those derived from a direct regression of the third exchange rate (since the dependent variables - the implied cross-rate and the direct rate - would be different). This would create problems of interpretation because, although the predicted cross-rate can be adjusted by examining the difference between the actual direct and actual cross-rates, it is difficult to envisage how a similar adjustment could be applied to the size and signs of coefficients. This would not matter if the sole intention were to test the hypothesis that the model presented here largely explained exchange rate movements in the 1930's but it is not; hypotheses concerning individual variables are to be examined and reliance on estimates via cross-rates would create problems. It might, however, be useful to compare predicted cross-rates with predicted
direct rates, at least for the major (pound-dollar-franc) exchange triangle. These should, especially after adjustment for differences between actual direct and actual cross-rates, be fairly similar and, if they are not, then this would suggest some inconsistency that required explanation.

(III)

The provision of empirical counterparts for the variables in the model involves a variety of difficulties. Most obviously, there are problems involved in the choice of the price indices to be used as a measure of PPP which have already been discussed in Chapter 2. A further constraint may now be added in the context of the 1930's relating to the actual availability of data. The theoretical possibilities are very much narrowed down because of this: clearly GNP deflators are excluded because they do not even exist on an annual basis for the 1930's in many cases and this also applies to cost indices. The most readily available type of price indices are of wholesale prices although they are arguably the least suitable. However, availability has made wholesale price indices the most popular choice and for that reason alone, it seems prudent to estimate a version of the model using wholesale prices, if only for comparability with other studies. Fortunately, cost-of-living indices are available for all the countries to which the model is to be applied although these are possibly less reliable. It has also been possible to find wage indices for the U.S.A. and U.K. and so a relative wage version of the model can be estimated for the pound-dollar exchange rate. To summarize, two versions of the model will be estimated, one using wholesale price indices, one using cost-of-living indices and for
the pound-dollar equation (only) a third version using relative wages will be tried.

There are further problems involved in obtaining a variable for real income since there is no data as such for most countries or, if it does exist, it is not available on a monthly basis. Lack of actual data does, in fact, solve one problem, in that the decision as to which price index has to be used to deflate the income data (since the required variable is real income) is avoided. However, it creates another since a proxy has to be used and there are several available.

The three major potential proxies are employment, unemployment and production indices which are all directly observable, but an alternative (indirect) approach is to use monetary data, and this is explored first. It was done for New Zealand in the 1870-1918 period by Hawke (1975) and the method is quite simple: obtain money supply data, invoke Fisher's equation \(MV = PT\), adjust the money supply data for changes in \(V\) and what is left is an approximation for national income. However, in the present context, there are two problems: firstly, potential multicollinearity between the money supply and other variables, which has already been discussed above, and secondly, obtaining reliable (or, in some cases, any) estimates of \(V\). Consequently, in the light of these difficulties, the limited number of countries to which the method could be applied and the availability of alternative proxies, this approach was not pursued.

This reduces the number of available proxies but a choice still has to be made and there are at least four ways of approaching this: the most simple is to use the same proxies as earlier studies;
however, this is rather evasive and may simply involve repeating earlier mistakes. Three other avenues would seem rather more satisfactory: firstly, examine the annual national income data that is available and compare it to the annual values of the proxies to see which of them most closely approximates it; secondly, compare the available proxies to see if they differ very much because it they do not then the choice is not an important one; finally, all the proxies could be tried and the data could "decide" in the sense that the proxy that gave the "best" results could be used.

All three avenues were explored to some extent. An index of Feinstein's annual estimates of British GNP\(^{(25)}\) were compared to the annual values of the British income proxies. Although this is potentially misleading in that correlation on an annual basis does not necessarily imply correlation on a monthly basis, it was clear that the British employment based proxy most closely approximated Feinstein's GNP estimates, although all three indices had the same trend (upward) throughout the '90's, and so the unemployment based proxy cannot be entirely dismissed. Indeed, a comparison of all the available indices for each country in the study (except Switzerland where only unemployment data was available on a monthly basis) showed that, broadly speaking, in all cases the indices exhibited the same trend and the main difference was that the employment and (especially) production based proxies tended to exhibit wider movements.

The "empirical" approach of experimenting with different proxies was adopted in some preliminary estimates of the pound-dollar exchange rate (with France proxying the rest of the world). In fact, there was little to choose between the performance of the different proxies (although this is not perhaps entirely surprising given the similarity
of trends). However, in the American case the employment index gave slightly "better" results (in terms of higher t-statistics) whilst the production index was marginally the "best" proxy for French income. Ultimately, the problem can be avoided by trying all possible combinations but this is rather open-ended and would involve a large number of computations. To avoid this it was decided, on the basis of the above evidence, to choose the income proxy in the following order of precedence: employment indices, production indices and indices based on unemployment data. However, alternatives will be tried on occasion, especially when the chosen proxies are insignificant.

A third "economic fundamental" in the model - interest rate differentials - involves a similar problem of choice to the income proxies in that data for a number of interest rates are available. The question is which of these is most important in determining international capital flows. The choice would be easier for the 1920's since (according to Einzig) before 1931 most funds engaged in interest arbitrage were mainly invested in bills thereby making the discount rate differential an obvious choice. Unfortunately, after 1931, the discount rate differential declined in importance in this respect and a number of interest rates became influential in the 1930's. In fact, there were several "interest parities" relating to different types of interest rate and it would be desirable to take some sort of weighted average. The whole issue is further complicated by the existence (in some countries) of negotiable interest rates and by the fact that the interest rate differential has to reach a certain size - half a per cent according to Keynes before arbitrage becomes profitable; hence strictly speaking, any differential less than +\( \frac{1}{2} \)% should be treated as zero. The difficulty in finding a representative
interest rate may account for the poor results obtained by Hudgins (1973) and Ozmun (1976) in the only other comparable studies of the 1930's.

An alternative reason for this, which is arguably of some importance, is that interest rate differentials may have had little influence on international capital movements, given the low interest rates of the 1930's and the political turbulence and uncertainty of the period. It seems likely that the major concern of holders of capital may have been safety. This is stressed by Drummond:

"In such an environment, there was naturally more interest in protecting capital - in prospects for exchange rates and freedom to use and transfer funds - than in relative interest rates". (29)

For this reason, the speculative dummy variables (discussed below), which (mainly) reflect possible sources of socio-political influence on the confidence of holders of capital, are more likely to show up in the estimates, since they seem more likely to have determined international capital flows than interest rate differentials in the period. Returning to the choice of interest rates, which now begins to appear less important, the difficulties of producing a weighted average (and impossibility of providing any rationale for choosing the weights) suggested the continuation of the practice of earlier studies and monthly averages of the discount rates for three months' prime bills, which are readily available for all the countries in the study (except Canada), were used.

The modelling of official intervention in the foreign exchange market is difficult because such intervention had to be secret in order to be effective and so there is no data. However, various observers, who knew the market, were able to deduce when intervention
was taking place and this was reported in some contemporary sources, though only in a subjective manner. The practice of Hudgins (1973) and Ozmun (1976) was therefore followed and dummy variables were constructed. This is clearly a highly subjective exercise and the construction of these variables is discussed, at length, in an appendix to this chapter along with the highly controversial issue of competitive depreciation. Nevertheless, one possibility that needs to be discussed at this stage is that such variables may be endogenous.

Essentially intervention depends on the deviation between the actual exchange rate and what the exchange funds think it ought to be ($R^T$). The endogeneity (or otherwise) of the intervention dummies therefore depends on what determines $R^T$: if the funds really did follow their official aim of not preventing genuine trends and simply smoothed out "undue fluctuations" then $R^T$ depended on the variables that determined the exchange rate which are obviously included in the model and so the intervention dummies would be endogenous; however, if intervention (and hence $R^T$) were determined independently then it would be exogenous. It has been claimed in many sources that the British authorities deliberately held down the pound at various times in the 1930's, and the activities of other exchange funds also seem to suggest that they did rather more than simply smooth out "undue fluctuations" in this period. Consequently, it does not seem unreasonable to assume that official intervention can be treated as being exogenous.

The performance of seasonal variables in the three previous studies of the 1930's has shed some doubt on their importance: Hudgins (1973) Ridpath (1975) and Ozmun (1976) all found that they did not show up very well. Hudgins argues that the absence of seasonal patterns
would be expected "if arbitrage were working to exploit the profit-making opportunities that such patterns would offer",(32) although one might ask why this did not happen in the early 1920's since Hodgson (1971) (1972), like Hudgins dealing with the pound-dollar exchange rate, found that all his seasonal dummies were significant. Perhaps a more likely possibility is that the EEA (Exchange Equilisation Account) would smooth out seasonal (and therefore temporary) fluctuations in the exchange rate (since this was its stated aim) although such activities would be likely to show up in the official intervention dummies.

In fact, there are good reasons for expecting some kind of seasonal effect: in the pound-dollar exchange rate, the tendency for the pound to weaken in autumn due to a demand for dollars to finance purchases of American wheat, cotton, tobacco and other primary products, and then to strengthen in the spring once this is over, is well-known and these "usual" seasonal variations are referred to periodically in contemporary sources.(33) However, there is some evidence that such effects were weak in the mid-nineteen thirties (1934-36), due to the decline of America as a wheat exporter and a build up of stocks of primary products in Europe during the depression,(34) although there is conflicting evidence on this with, for example, the "Economist" suggesting why seasonal effects are less important and then using them as an explanation of movements of the pound in the same month.(35)

On balance, it seems likely that there is sufficient evidence of seasonal effects to make the inclusion of appropriate variables necessary. Furthermore, this does not just apply to the pound-dollar exchange rate since the demand for dollars to buy American primary products in autumn came from Europe generally, not just Britain, and
applied not only to American dollars but also Canadian dollars (given the importance of wheat in the Canadian economy). Finally, a totally different reason for expecting seasonal fluctuations in exchange rates relates to the importance of tourism for some countries; this would affect some currencies (such as the Swiss franc) more than others.

It remains to be decided exactly how seasonal effects are to be incorporated into the model. The conventional method is to use dummy variables and this should involve, in a monthly model, eleven such variables. For some reason (possibly to increase degrees of freedom) Hudgins (1973) Ridpath (1975) and Ozmun (1976) use quarterly dummies to test for seasonal effects, even though they are testing monthly models. This may account for the insignificance of their variables – if one month is significant but the other two are not, then the variable as a whole may not show up. Moreover how does one choose which three months constitute a quarter?

Nevertheless, the practice of using eleven dummy variables would waste degrees of freedom and, partly for this reason, a rather unconventional method of measuring potential seasonal influences is used here involving sine and cosine waves. This is based on the assumption that seasonal patterns were fairly regular and, in the first instance, values of six sine and six cosine waves were derived each with a different frequency. Thus, one sine wave peaks (and troughs) once in twelve months (A1), another twice (A2) and so on up to six times (A6) and the same for cosine waves (B1...6). On the basis of the expectation that the lower frequencies are most likely to be significant (since one would not expect seasonal patterns to peak more than two or three times per year) and a few preliminary
estimates which seemed to bear this out, it was decided, mindful of degrees of freedom, to only include A1, 2, 3 and B1, 2, 3. (36)

The rest of the world variables to be included will be based on a proxy country which involves a further choice. The need for including such variables seems fairly obvious; the importance of "foreign exchange triangles" (such as the pound-franc-dollar relationship) is stressed by Brown (1940). (37) The construction of weighted averages of all the "world" variables would involve an extensive exercise in itself and the reliability of the ensuing variables would be constrained by the availability (and reliability) of data especially for some of the more obscure countries. Such an exercise was therefore eschewed (although a limited version of the world "price" variable will be constructed for a related but different purpose in chapter eleven).

In fact the choice of proxy country is not difficult. The major "foreign exchange triangle" was obviously the pound-franc-dollar triangle and so the proxy for the ROW for the pound-dollar, pound-franc and franc-dollar exchange rates must obviously be France, America and Britain respectively. Another triangle is formed by the Canadian dollar, US dollar and pound; therefore for the Canadian dollar-pound and the Canadian-US dollar exchange rates, America and Britain will proxy the ROW. The choice for the minor gold bloc countries (Belgium, Holland and Switzerland) is less easy since there are two suitable proxies for each exchange rate against a major currency: for example, for the belga-pound exchange rate against a major currency, it is likely that the path of the belga (guilder, and Swiss franc) against each of the major currencies, was largely governed by the pound-dollar-franc triangle (and therefore the
movement of the underlying determinants of the exchange rates within this triangle), and so it may be appropriate to include both potential proxy countries.

It may seem that ROW variables (prices and incomes) should carry the same sign but in fact this is not the case because they influence the exchange rate in a number of different ways. For example, the effect of an increase in ROW prices on the pound-dollar exchange rate will be through three channels: US demand for pounds, UK demand for dollars and ROW demand for both pounds and dollars; or, put another way, Americans, Britons and consumers from the ROW will all switch from ROW goods to some combination of British and American goods. Changes in world income will have a less complex influence, presumably affecting only ROW demand patterns, although the relative effect on demand for British compared to American goods would be similarly unclear. This variety of avenues through which ROW prices and incomes affect a given exchange rate should, by itself, be sufficient to ensure that signs of the two variables may not be the same. However, once full allowance is made for differences in price and income elasticities, differences in tastes and the possible existence of money illusion, it becomes obvious that nothing whatsoever can be said about the expected signs of ROW variables and there is certainly no reason to believe that the signs of different ROW variables should be related in any particular way.

A final ("economic fundamental") variable included in the model is a linear trend. This is something of a residual variable in that it seeks to capture omitted influences. The usual rationale for its inclusion is that it reflects changes in tastes. However, it may indicate the presence of at least two other influences: firstly,
different rates of productivity growth and secondly, increasing
fears of war in Europe (and consequent capital flight). A problem
may arise with the trend variable in that it may be correlated with
other variables. Fortunately, this is minimized by the use of
variables in ratio form and by the existence of the 1937-38
recession (and the general uneveness of recovery) which means that
the price and income levels of the third countries proxying the ROW
did not exhibit a steady trend.

An important influence on trade flows in the 1930's (and hence
on demand for and supply of currencies and consequently on exchange
rates), which is excluded from the model, was the high degree of
protection practiced by many countries. This varied from country
to country with some (such as Belgium) less inclined to pursue it
than others. The likelihood that tariffs will undermine the PPP
relationship is well documented and even conceded by the proponents
of the PPP hypothesis (including Cassel); indeed, it has already been
discussed in chapter two. If the level of protection had remained
approximately the same throughout the 1930's as in the base year,
then this would be less of a problem but this is not the case.

At the empirical level it might be possible to proxy the level
of protection with the amount of customs revenue collected. Un-
fortunately, this method suffers from two major drawbacks: most
obviously, the lack of adequate data for some countries and, most
importantly, the conceptual criticism that this is only a partial
measure of protection, since it does not allow for quotas or exchange
controls. In the case of one country (Canada) there are fairly clear
turning points in its commercial policy and this enables the construc-
tion of (rather crude) dummy variables to approximate the effects of
protection on Canadian exchange rates. In all other cases, however, no variable to allow for protection is included.

Therefore one is forced to make two implicit assumptions which are, in the case of the pound-dollar exchange rate for example, as follows: firstly, net bi-lateral trade controls between the two countries in question (Britain and America) cancelled each other out; secondly, trade controls in the rest of the world, though having a large effect absolutely, had a negligible or equal effect on the markets for pounds and dollars (which implies that protection in third countries affected imports from Britain and America roughly equally). Whilst the former assumption is possibly not unreasonable, given that much protection was retaliatory and the removal of tariffs was largely based on bi-lateral agreements, the latter is questionable for the same reasons (especially since the ROW is actually represented by an individual country).

(IV)

The final problem that remains to be dealt with is how to model speculation or, put another way, how to quantify the variable \( S \) in the capital account section of the structural equations. Since stabilising speculation will tend to be picked up (to some extent) by the other variables, in that adjustment to changes in prices, incomes and other variables would be made more rapid by such speculation, this is mainly an exercise for testing for destabilising speculation. The quantification difficulties are discussed below but initially there is a problem of definition. Thomas has provided three definitions:
"Destabilising speculation... is a situation in which speculators... cause the exchange rate to diverge from some equilibrium rate over time... or influence the behaviour of the equilibrium exchange rate... or cause the amplitude of exchange rate movements to exceed that which would exist in the absence of speculation". (39)

An identical trio are given by Aliber:

"...one is that speculators cause the range of movements in the spot rate to be larger but without any impact on the equilibrium rate; the second is that speculators cause the actual rate to diverge from the equilibrium rate; and the third is that speculators influence the path of the equilibrium rate". (40)

Aliber's first and second (Thomas' third and first) definitions are usually the definitions used in empirical tests. In fact, in periods of floating exchange rates when the actual rate was free to change with the equilibrium rate these two definitions would seem to amount to the same thing: an excessive exchange rate movement is, presumably, one which causes the floating exchange rate to diverge from its equilibrium path. Aliber's third (Thomas' second) definition takes account of the possibility that speculation-induced exchange rate movements may influence internal price levels (for example) and therefore the behaviour of the exchange rate; this approach is not explored here as it would require a much more extensive model.

A wide variety of methods of testing for destabilising speculation have been employed, all of which have drawbacks of one kind or another. The basic problem stems from the fact that data on short term capital flows is simply not available for many earlier periods or if it is, it is not especially reliable as it is often based on an estimate derived from residuals. Given this lack of data there are two possibilities: the most obvious is to devise a proxy of some kind, the less obvious (if disarmingly simple), is to ignore it, look at
deviations from the predicted exchange rate (on the basis of the variables chosen to determine the equilibrium rate) and attribute these to speculation.

The latter method has been explored by Stolper (1948), Tsing (1959) and Hudgins (1973). It has the advantage of avoiding any criticism of the choice of variable to proxy destabilising speculation because no variable is used. Its major drawback is that it implicitly assumes that there are no omitted variables and that all the deviations are due to speculation. This may be a particularly dangerous inference to make in the present context as trade controls (with one exception) are not modelled and therefore must also be a prime candidate for the omitted variables which account for unexplained exchange rate movements. It also requires subjective judgement on how large deviations have to be in order to be considered. Studies which look at the difference between actual and PPP exchange rates are particularly prone to the accusation that the residuals are not necessarily due to destabilising speculation. The Tsing (1959) and Stolper (1948) studies come into this category (although the latter does consider other possible reasons for the residuals). Hudgins' (1973) study is more acceptable in that he includes more variables to determine the equilibrium exchange rate but the problem remains.

Another approach that does not involve including specific variables for speculative activities might also be discussed at this point. This involves using a log-linear form and looking at the elasticity of exchange rates with respect to prices and, on the assumption that PPP implies that this should be equal to unity, if it is greater than unity then this implies "overshooting" due to destabilising speculation. This is the first approach of Thomas (1973b)
but the possibility of using it here is excluded by the existence of variables with negative values which precludes the adoption of a log-linear form.

Attempts to proxy speculation are usually based on forward market data or some attempt to model speculators' expectations or resort to dummy variables. Aliber's (1962) (1970) approach is to use forward market data adjusted for changes in the money market interest rate differential; this assumes that the forward market is dominated by speculators. If the forward rate is at a discount then speculators are judged to be bearish, if at a premium then they are bullish. If the adjusted forward discount (premium) is increasing as the spot currency is depreciating (appreciating) then this provides evidence of destabilising speculation. A related method is that of Stein (1965) who regressed the forward premium on the uncovered interest rate differential and then used the residuals as his "speculative variable". This is basically a more sophisticated version of Aliber's variable and, in the present context, seems unnecessarily complex.

Another method of deriving a speculative variable is to base it on some hypothesis about the behaviour of speculators. Assuming that speculators base their behaviour on trends in the exchange rate, recent changes in the exchange rate could be used in this context: if speculators were operating in a stabilising manner they would view any change in the exchange rate as temporary, expect it to return to its previous level and react accordingly; if speculation were destabilising then presumably any change in the exchange rate would be amplified by their behaviour. This is the approach adopted by Mellish and Hawkins (1968) in their study of the 1950's Canadian float. A
similar, though more complex, approach is adopted by Arndt (1968), who assumes that speculative capital flows are a function of the difference between the actual exchange rate and the "expected normal rate" where the latter is determined by the deviation of the actual current rate from the previous "normal rate". A variation on this is the second method used by Thomas (1973b) who bases speculators' behaviour not on divergences between actual and expected rates but between actual and expected price levels (where prices determine exchange rates).

The problem with either ignoring speculation and looking at residuals or introducing a proxy for speculation of the kind discussed above is that such methods involve "blanket" measures which could potentially take in much more than just speculation. For this reason, the method of Hodgson (1971) (1972) and Ozmun (1976) is, to some extent, preferable. This involves attempting to identify events which are non-quantifiable (or "non-economic" in the sense of not due to "economic fundamentals") but may have influenced exchange rates, and then using dummy variables to test for the presence of speculation (in the form of short term capital flows) which may have occurred because of these events. To the extent that such capital flows are "non-economic fundamental" they are destabilising, and significance of these dummy variables can be taken as evidence of destabilising speculation. The main disadvantage of this approach is its subjectivity, both in deciding which events merit the inclusion of dummy variables and in assigning values to these dummies to reflect the intensity of influence of a particular event (which may change during the period it is effective). On the other hand, this method is not a "blanket" measure but, instead, provides a test for the effect of specific events on the exchange rate,
if a somewhat unsatisfactory one.

The various methods of incorporating speculative factors into the model, reviewed above, all have drawbacks of one kind or another and consequently, it seems desirable to try several approaches. Two broad types of approach have been identified: methods engaging in a "blanket" measurement and those (in fact only one) seeking to test for the effect of specific potential influences. It seems reasonable to adopt one approach of each type. Of the latter type, the dummy variable approach is the only candidate and will therefore be used. In fact, this is really the favoured approach as relating speculation to potential causes identified a priori is intuitively appealing and seems more intellectually honest. In addition, it encompasses the possibility that speculation may have been destabilising some but not all of the time (which a "blanket" measure can not). The ready availability of forward data (at least for the pound) would suggest that the "blanket" measure should make use of this and the simpler approach of Aliber (1962) (1970) is preferred to that of Stein (1965).

An additional reason for preferring the dummy variable to the "blanket" measure approach is that the "overshooting", implied by the significance of the latter, only indicates that speculation was destabilising insofar as speculators' expectations are incorrect (and, of course, to the extent that such variables measure only speculation.) This possibility is recognised by Aliber himself who describes this view as follows:

"...it is maintained that speculation should be considered stabilising in some cases in the inter-war years because speculators were only anticipating the weakness of the currencies resulting from non-speculative factors." (42)
This point is also taken up by Pippenger:

"When inflationary conditions are changing rapidly, any tendency for the foreign exchange market to respond more quickly than the capital market to inflationary pressure will...generate an adjusted discount". (43)

Such speculation may be seen in terms of speculators simply being correct in their expectations and causing the exchange rate to adjust more quickly and possibly more often.

This problem does not apply to dummy variables to the extent that they are constructed on the basis of potential influences which should not rationally have caused any change in the exchange rate. However, if, as was sometimes the case, speculators were quite right to believe that a bout of political instability, for example, could lead to a change in the exchange rate, then significance of a dummy variable which reflects the consequent capital outflow is not really indicative of destabilising speculation either. In fact, in the 1930's, it may have been very often the case that speculators were correct and so significance of any of the speculative variables included here cannot properly be interpreted as evidence that the foreign exchanges were bedevilled by destabilising speculation but only as evidence that speculation occurred. Nevertheless, to the extent that they can be constructed to reflect "irrational" influences on speculators' expectations, dummy variables will provide the best test for destabilising speculation.

However, in view of these ambiguities it may be instructive to be more precise about the exact nature of the dummy variables used here. They have been referred to as "non-economic" in the above discussion which is, to a large extent, a misnomer in that the influences being
picked up are in some senses "economic". Most commonly, a "non-
economic" dummy represents the economic actions stemming from expec-
tations of the economic consequences of a political (and in that
sense "non-economic") event, such as fears of inflation due to the
election of a new government, or occasionally even an economic event,
such as deliberate reflation by the government. Thus, more properly
speaking, these dummies are not really "non-economic", or even
"speculative", but they are rather "non-economic fundamental" and
represent the economic consequences of people's expectations about
the (economic) implications for exchange rates of various incidents,
events and attitudes, economic or otherwise; clearly, such expectations
need not necessarily be fulfilled for the actions of those who hold
them to influence the exchange rate (and consequently for the dummy
variables to be significant).

Nevertheless these dummies do reflect destabilising speculation
to the extent that such expectations are "incorrect" or, perhaps better,
imply a different level of the exchange rate to that believed to be
the "correct" level by the government. Given that in many cases one
or both of these situations (particularly the latter in the case of
the gold bloc) is applicable, then the dummy variables used here
will be picking up destabilising speculation and will, therefore,
be called "speculative" dummies. At the same time though, it should
be remembered that when "speculators" were "correct" or, more generally,
when neither of these two conditions applies, and this is probably the
case for at least some of the dummies, then the significance of a
speculative dummy is not necessarily indicative of the presence of
destabilising speculation.
Consequently, each case has to be examined individually and, in general, significance of the "speculative dummies" only implies, in the first instance, that exchange rates were being determined by influences other than "economic fundamentals". Therefore, to treat significance of these variables as indicative of destabilising speculation (and therefore supportive of the Nurkse hypothesis of unstable exchange rates in the 1930's) implies a rather severe test of the hypothesis (of stable exchange rates in the 1930's) being put forward in the present study.

Finally, in spite of the explicit inclusion of speculative variables, this does not preclude the implicit use of another method, discussed above, which is to examine the residuals: any deviations from the exchange rate predicted by the model could be due to speculation, although (as has been pointed out) it is dangerous to pursue this too far, as they could be caused by other omitted variables (such as the influence of trade controls). This completes the examination of the model and the derivation of empirical counterparts for the independent variables.
FOOTNOTES TO CHAPTER 4

1. See Officer (1976a) pp 2-3 (quoted in Ch. 2).


5. Ibid, p. 63.


9. For example, "the complete breakdown in international relations during the occupation of the Ruhr and the subsequent collapse of the mark left the pound rather unaffected". (Stolper, 1948, p. 251).

10. See Thomas (1973a) (1973b); this point is taken up again in the discussion of how to model speculation in Section IV below.

11. See Frenkel (1976b) pp. 202-3 for an argument that the PPP hypothesis should be tested using cost-of-living indices.


13. The position adopted here is a traditional one: an increase in real income leads to an increase in demand for imports and therefore a depreciation. The monetary approach would argue that an increase in real income would lead to an increase in the demand for money which, unless it is met by an expansion in the domestic credit component of the money supply, will cause an appreciation. This argument is put forward by Mussa (1976) p. 237.

14. This possibility is explained by Frenkel (1976) pp. 208-10. Whilst accepting the traditional view (adopted here) that, in the short run, high interest rates attract foreign capital and hence cause the exchange rate to appreciate - the "liquidity effect" - he argues that there is also an "expectations effect" which, in certain circumstances (such as hyperinflation), dominates the "liquidity effect", thereby causing a rise in interest rates to be associated with a depreciation. The "expectations effect" operates in the following way: an increase in the expected rate of inflation causes a rise in the nominal interest rate whilst, at the same time, it lowers the demand for real balances and therefore a higher price level is required to restore asset market equilibrium (assuming money stock is unchanged). Given that PPP holds and that, during hyperinflation, the foreign price level can be treated as being fixed, the higher (domestic) price level can only be achieved through a depreciation. Hence a rise in the interest rate is associated with a depreciation.


17. Ibid, p. 96.

18. The Almon technique involves the use of polynomials to approximate lag structures. There are two (essential) assumptions: firstly, the lag is of a finite length (n) and secondly, the lag structure follows a polynomial (of degree p - 1). See Almon (1965) for the original use of the technique.

19. Most obviously, since n and p are not known, some criteria has to be used to determine their values. See Thomas (1977) and Frost (1975) for a discussion of this and other problems.

20. In practice, small differences between cross-rates and direct rates inevitably occur as arbitrage would have to take into account transaction costs. In the present context it is larger differences which cannot be explained by transaction costs that are being discussed.


23. Another possibility is to use indices of railway freight receipts. However, this would seem to be very much a last resort and, as at least one of the other three is available for all the countries covered here, it is not pursued.

24. Hodgson (1971) (1972), Hudgins (1973), Ridpath (1975) and Ozmun (1976) all favoured employment indices whilst Thomas (1973a), dealing with a larger number of countries with, in some cases, limited data, varied his choice using indices of industrial production (Canada) or of railway freight receipts (Spain).

25. This was calculated from C.H. Feinstein: "National Income, Expenditure and Output of the UK, 1855-1965" (Cambridge University Press, 1972) Table 5, p. T16.


27. Ibid, p. 159.

28. Reported in Ibid, p. 151. A number of later authors would put this figure much lower - no more than 2% and probably even less. See Frenkel and Levitch (1975) p. 331.

29. Drummond (1979) p. 4. See also p. 3.

31. See Drummond (1979) for a discussion of the management of the franc in the 1930's. However, it should be remembered that definite official views and objectives about exchange rate levels do not necessarily involve competitive depreciation.

31A. The problem is more acute for intervention in exchange rates which were "fixed on both sides", that is where both countries were on the gold standard. In such cases there is a clearly defined exchange rate target with intervention aimed solely at offsetting any deviation in the exchange rate from par, presumably caused by the other variables in the model, and therefore it must be endogenous. Fortunately, only three of the fourteen exchange rates in the study were fixed in this way for an appreciable part of the 1931–8 period and, in all three cases, the intervention dummies were insignificant anyway.


33. See, for example, the Economist 24/9/32, p. 534, 14/10/33, p. 702, 21/8/34, p. 362, 22/10/38, p. 174 and so on.

34. See the Economist 31/8/35, p. 423 and the Statist 13/7/35, p. 42 for more details of this argument.

35. See the Economist 10/9/32, p. 452 and 24/9/32, p. 537 respectively.

36. Data for these variables are given in appendix I.


38. The Ottawa Agreements were based on this principle and from the mid-1930's onwards Cordell Hull, the US Secretary of State, negotiated a series of American bi-lateral agreements with other countries.


41. This is almost a monetarist approach since it is a common feature of monetarist models to use forward market data to proxy expectations. See, for example, Frenkel (1980).


43. Pippenger (1973) p. 616. See also Friedman (1953) p. 176.
APPENDIX TO CHAPTER 4

OFFICIAL EXCHANGE RATE INTERVENTION

An important secondary aspect of the study relates to the role of official intervention in the foreign exchange markets in the 1930's. An attempt to model this is both necessary, as it was (according to many sources) a potentially (and sometimes actually) important influence on exchange rate movements in this period, and desirable because the 1930's mark the establishment of the major official intervention funds and their activities in these early years represent a source of some controversy. The first intervention fund was the British Exchange Equalisation Account (EEA) which began operations in 1932 and was soon followed by the establishment of American (1934) and Continental (1936) equivalents although intervention was undertaken by the Central Banks of all these countries in the period preceding the setting up of intervention funds, if on a more limited scale, and the intervention dummies constructed below do reflect such intervention (to the extent that evidence of its existence could be found).

It is not necessary here to engage in a detailed discussion of the mechanics and activities of these exchange funds (although these are briefly described further on); a good account of the British EEA is to be found in Howson (1980) and the American and French funds are described and compared to their British counterpart in Hudgins (1973) and Ozmun (1976) respectively. There were important differences between the three particularly in relation to sterilizing the effects of their dealings in foreign exchange on domestic credit conditions (1) and to some extent in their objectives also: the EEA (ostensibly at
least) had the objective of "checking undue fluctuations in the exchange rate"\(^{(2)}\) whilst the US fund sought "to stabilise the dollar and to maintain its value as a currency"\(^{(3)}\) which seems to have meant maintaining the official dollar price of gold at $35 (per fine oz.) and the French fund initially aimed to hold the franc within the new legal limits following the devaluation in September, 1936 although this was modified at the end of June, 1937 so that French intervention became discretionary with a similar objective to that of the EEA.

The most controversial aspect of exchange intervention in the 1930's relates to the question of competitive depreciation or more exactly the contention that the EEA deliberately prevented the pound from appreciating in order to improve Britain's trading position. This was a widely held view at the time particularly in America where, for example, one contemporary observer referred to "the persistent and successful efforts....to depress sterling relative to other currencies which were characteristic of the operation of the Exchange Equilisation Account in its first year..."\(^{(4)}\) On the British side this was denied periodically, for example in the "Economist", which on one occasion provided some detail to "help to dispel the prevailing American belief that the British Exchange Equalisation Account was used to depress the pound against the dollar".\(^{(5)}\) On the basis of empirical testing of their models Hudgins (1973, pp. 220-1) and Ozmun (1976, pp. 29-31, 157, 161) come to a similar conclusion.

In fact the contention that exchange funds did not engage in competitive depreciation is probably too naive and the truth of the matter seems to have been rather more complex particularly in the case of the allegedly worst culprit, the EEA. Basically, internal
British recovery (and consequently the cheap money policy) was given precedence and there was to be no attempt to fix the exchange rate of the pound — in fact throughout 1935 the Treasury deliberately eschewed stabilisation largely for this reason though the desire to avoid giving speculators a potential one way option was also given some weight; indeed Britain was the least enthusiastic participant in the 1936 Tripartite Agreement. Nevertheless, this policy cannot really be construed as deliberate competitive depreciation especially as "to avoid antagonising Roosevelt" was one of its aims. Consequently, whilst the policy may have involved what were, on the face of it, deliberate attempts to hold the pound down — in the mid-1930's the EEA often intervened heavily whenever the pound threatened to rise and in fact total reserves (not surprisingly) therefore doubled between December 1933 and December 1936 – it is better interpreted not as deliberate competitive depreciation but as giving priority to internal recovery and managing the exchange rate in such a way as to facilitate this or, perhaps better, it is "incidental" competitive depreciation.

Fortunately, for the purposes of this study, whether the funds were working against each other or not does not really matter except insofar as it makes it difficult to decide the sign to be given to the dummy variable when competitive intervention was taking place. However, even this is not a major problem as evidence of such occurrences is virtually non-existent; indeed a much more frequent event is the existence of months in which a given fund operated in different directions on different occasions. This apparent lack of competitive intervention does not, nevertheless, really prove very much in the context of the debate on its existence and importance.
since during the period in which such activity was allegedly taking place (the early 1930's) only the British fund was active. However, as far as constructing a dummy variable is concerned there was no conflict between exchange funds in this period (as only the British were active) and so whether a fund was operating in a "competitive" sense or not is irrelevant; what is relevant is its effectiveness in influencing the course of the exchange rate.

The methods used for official intervention mainly consisted of buying and selling domestic currency, gold and foreign currency in the spot market. Recourse to the forward market was fairly rare, at least in the British case, due to the views of Governor Norman who "was almost obsessed by the fact that purely speculative transactions are facilitated by the existence of forward markets. Accordingly the Bank was still in the middle 1930's avoiding any regular intervention in forward markets..."(8) although on two occasions in the second half of the 1930's the EEA did engage heavily in forward transactions (in late 1936 and again late 1938) and kept a small amount of forward cover in francs in the 1934-36 period to offset any potential exchange loss that would accompany a devaluation of the franc. In addition other avenues of official influence on exchange rates - government borrowing, controls on capital movements, interest rate policy, budgetary policy and so on - were either ignored, used for other purposes or ineffective in the British case, although used to a greater degree in other countries. Thus it seems that concentrating solely on intervention in the spot market (which is what is done here) will lead to the construction of variables representing official intervention which are complete in principle, if potentially (as will be suggested below) incomplete in practice.
The actual construction of dummy variables to represent exchange fund activity is difficult and some discussion of the problems involved is necessary both to show how they were overcome and, to the extent that they were not, to illustrate the consequent limitations of the variables constructed. Contemporary sources of data do not exist due to the secrecy of official operations in the foreign exchange market and the data based on official sources that has become available recently, provided most extensively by Howson (1980), is incomplete for the last two years covered in this study (1937-38) and in any case only refers to the activities of the British fund. Therefore, the only remaining method of including official intervention would seem to be to follow the approaches of Hudgins (1973) and Ozmun (1976) and to construct dummy variables based on observations of exchange fund activity found in any available sources; such an approach becomes justified due to lack of alternatives.

Since the completion of these two studies two further sources of relevant information — Sayers (1976) and Howson (1980) — have been published and consequently the dummy variables constructed here are based on more information and are therefore potentially more reliable. The main sources were the "Economist", "Lloyds Bank Review", Howson (1980) and Sayers (1976); limited reference was also made to the "Statist" and indirectly (through reference to Hudgins' dummy variable which was partially based on this) the "Commercial and Financial Chronicle". The major intervention dummies relate to the pound-dollar, pound-franc and franc-dollar exchange rates though they were also constructed for the exchange rates involving "minor" currencies.

There are a number of problems involved in using this rather subjective method of incorporating exchange fund activity into the
model, most obviously that created by incomplete "data": it cannot be certain that all activity of the exchange funds is reported in the sources used. In the case of the EEA it is not unreasonable to assume that the information is fairly complete as Sayers and Howson have had access to official sources but the data on American, French and particularly Belgian, Dutch, Swiss and Canadian intervention is almost certainly deficient. The more reliance that has to be placed on contemporary sources such as the "Economist" and "Lloyds Bank Review" the more likely is something to be missed. Since evidence of "minor" currency intervention is almost totally dependent on such sources then the results produced provide only a limited test of the effect of official intervention on the exchange rates of these currencies (or, put another way, only provide a test of the importance of some of the intervention that actually took place) although to the extent that intervention by Canada and the smaller gold bloc countries may have been fairly limited, particularly before September 1936, this is perhaps not too important a problem.

Another problem relates to the qualitative nature of the information that actually is available. The problems can be broken down into two types: those relating to the period and those relating to the strength of intervention. In the first case how should intervention that occurs "for a while" be interpreted - is a "while" a week or a month or even longer? In the second case words such as "heavy", "extensively", "intermittently", "slight" and so on have to be given a quantitative meaning. A related problem occurs when intervention took place in both directions in the same month; a cancelling out cannot necessarily be assumed if intervention was "heavy" in one direction and "slight" in the other. A further aspect of this concerns
intervention in the same direction in two months when it was "heavy" in one but "slight" in the other - does the observation for the former month carry a higher value (which was the method employed by Hudgins and Ozmun)? To a certain extent such problems are more apparent than real since once the essentially subjective nature of these variables is accepted then a certain amount of discretion and good judgement can effectively minimize most of these difficulties. However, they inevitably remain and make the tests for the effect of exchange intervention only partial although in the wider context of the whole model this is less important since official intervention is only one variable among many (although an important subset of conclusions will relate to the effectiveness of this activity).

There is also the question of the "black and whiteness" of exchange rate intervention in that the rationale underlying the construction of these dummy variables implies that the authorities were simply either supporting or depressing an exchange rate at any particular time. Unfortunately, it was not always quite as simple as this and exchange intervention was sometimes rather "grey" when, on occasion, the EEA adopted a policy of holding the pound at a certain rate, then letting it go when pressure built up to an uncomfortable degree, pegging at the new lower or higher rate for a period, then letting it go again and so on. The EEA did this in July, August and October 1932, July-October 1933, August 1934 and again in June-September 1938(10) and also the French did something rather similar in the August 1937-May 1938 period when the franc engaged in what might be termed a "controlled" depreciation (which basically involved not letting the franc run away with itself but letting it run nevertheless). This is intervention in the "pure" sense of "smoothing out" a trend in the exchange rate
induced by market forces but in the present context might it not appear that official intervention was unsuccessful since attempts to hold the exchange rate were not working? The intervention dummy would presumably then be insignificant or even "wrongly" signed.

This problem is partly an unavoidable consequence of using time series analysis and arbitrarily cutting off each observation at the end of the month when official intervention might have been holding the exchange rate (and therefore "working") for a period of less than a month but then the exchange rate might have been let go in (for example) the last week. Of course, discretion and good judgement can help here and specific examples of this behaviour can be allowed for, although perhaps they ought not to be since, to the extent that such activities held up a trend in the exchange rate induced by market forces, official intervention may still appear to be significant; holding a peg until pressure builds up is more powerful than, and not quite the same as, smoothing out a trend.

In fact, there is a fundamental problem in constructing these dummy variables in that perhaps there ought to be a distinction between two types of intervention: firstly, day to day (or week to week) smoothing which is very often the type of intervention reported in the "Economist"; and secondly, more major operations which may have involved bucking a trend or at least slowing it down which is the type of intervention reported by Howson (1980) and Sayers (1976). The latter is more likely to show up in exchange rate movements whereas the former may very well not do so. Consequently, there may be a case for two variants of exchange intervention dummy, one including only major operations and the other all reported operations with the first variant more likely to be significant.
Finally, successful exchange intervention may make speculative dummies insignificant since the influences they represent may have been rendered ineffective by such intervention. Alternatively, in cases where no evidence of intervention has been found but it did occur or particularly strong intervention occurred but the intervention dummy does not differentiate between "weak" and "strong" intervention, then if such intervention was successful the speculative dummy may carry the "wrong" sign and in fact be picking up unreported (or very strong) official intervention. This is a multicollinearity problem. To a certain extent this may also apply to the seasonal dummies although it seems unlikely that intervention was strong enough to offset this altogether especially in the light of the large number of contemporary references to seasonal influences.

With the difference between smoothing out and major operations very much in mind, two parallel versions of the three exchange fund activity dummies for the pound-dollar-franc triangle were constructed initially: the first of these was based on information contained in Howson (1980), Sayers (1976) and Lloyds Bank Review and the second on the "Economist" (mainly, but not exclusively, on the foreign exchange column); not surprisingly the latter contained rather more months in which intervention was reported as occurring. A final stage involved examining the dummy variables constructed by Hudgins (1973) and Ozmun (1976) who engaged in a similar task although based on less information. The Ozmun variable was based solely on the "Economist" and so its examination was merely an exercise in double checking but the Hudgins variable added some new information in that it was based not only on the "Economist" but also on the "Commercial and Financial Chronicle" (to which the present author did not have access).
and so the second variant of the intervention dummy for the pound-dollar rate (based on the "Economist") was modified accordingly.

In fact reference to Hudgin's and Ozmun's variables was not very helpful as they do not seem to be entirely reliable and each of them contains apparent mistakes. This is less true of Hudgins' although specific errors can be spotted: for example, in March 1933 the only relevant comment that could be found referred to Control offsetting a big demand for sterling by selling pounds (11) which implies a value of -1 in Hudgins' dummy but in fact it carries a value of +1 whilst in November 1938 a reference to official support for the pound (in the "Economist") seems to have been missed. Furthermore, Hudgins' "joint" intervention dummy is not constructed as he himself describes. (12) A more fundamental criticism relates to the January 1934 to September 1936 period when Hudgins' dummy registers official British intervention in the pound-dollar exchange for 19 of the 32 months and yet the EEA was operating only in francs at this time. (13) The explanation of this would seem to be that Hudgins was assuming that, as the franc and dollar were both linked to gold, intervention in the pound-franc exchange would affect the pound-dollar exchange rate through arbitrage but then why does he not retain this assumption during other periods when the dollar and franc were both linked to gold (such as February 1937)? In any case, a preferable approach would seem to be to construct a separate pound-franc intervention dummy and to include that in the pound-dollar equation to test directly for significance of pound-franc intervention in the determination of the pound-dollar exchange rate (although this has the disadvantage of not separating out the periods in which such a relationship may have occurred - particularly 1934-36 - from other
periods when it may not have, that is, those in which the dollar and franc were not both linked to gold).

As indicated above the Ozmun dummy variable for pound-franc intervention is based on no unavailable or unused source and so it was only examined to double-check. Unfortunately this proved confusing as it contained many apparent errors. To give one example, in March 1933 the EEA bought francs (Ozmun, 1976, p. 33) and in March 1934 it sold francs (Ozmun, 1976, p. 34) so why, with no other activity reported in the "Economist" for these months, does Ozmun's EEA dummy carry the same sign (minus) in both these months? There are numerous other inconsistencies and ultimately it was decided to ignore Ozmun's dummy variable.

To summarize: two versions of the three main intervention dummies will be constructed, the first based on Sayers (1976), Howson (1980) and Lloyds Bank Review, the second based on the "Economist" with the pound-dollar variable modified slightly by reference to Hudgins (1973). Two variables proper will then be used in the regressions: a "narrow" intervention dummy based on the first of the two versions and a "wide" intervention dummy based on both and therefore on all information that could be found. A further possible variation - involving the use of different values to represent different reported strengths of intervention (as was done by Hudgins and Ozmun) - was rejected as being far too subjective. In the empirical tests the "wide" version will be preferred but in the cases where this performs badly the "narrow" version will be tried.

The exchange fund dummies for exchange rates involving the "minor" currencies contained few observations and were mainly derived
from information in the "Economist" although occasional reference
to minor currency intervention was found elsewhere - for example,
Lloyds Bank Review notes that the belga was being supported in May,
1938. Finally variables reflecting the activities of individual
exchange funds were not constructed but rather intervention in a
particular exchange rate by all the relevant authorities formed the
basis of the exchange intervention dummies; consequently, these are
"joint" variables in that they measure, in the case of the pound-
dollar rate for example, the combined activities of the EEA and the
American Stabilisation Fund and therefore provide a test for the
importance of official intervention in itself, rather than testing
for the significance of the operations of individual countries' intervention funds. All the exchange intervention dummies are
reproduced in Appendix I.
FOOTNOTES TO APPENDIX


4. Comstock (1933) p. 620. For other expressions of this view, both contemporary and more recent, see n. 30 of Ch. 4.


6. In fact, beating the speculators (and thereby discouraging destabilising speculation) was sometimes a major preoccupation of EEA policy. See Howson (1980) pp. 46-7 for examples of this.


9. Two references in Clarke (1977) on p. 2 (Jan., Feb., March, 1933) and on p. 10 (June, 1936) were also noted; these backed up evidence in Lloyds Bank Review. Reference was also made to a series of contemporary articles by F.W. Paish in "Economica" (1935, 1936, 1937) but these were not very useful and, according to the Economist (16/12/35, p. 35), inaccurate anyway.


11. Economist 4/3/33, p. 446. This is supported by evidence contained in Lloyds Bank Review (April, 1933, p. 152) and Clarke (1977) p. 2.

12. According to Hudgins (1973, p. 232) this should carry a positive sign when the pound was being supported and the dollar depressed, and presumably a negative sign when the dollar was being supported and the pound depressed. In fact close examination of this variable (Hudgins, 1973, Table A21, p. 255) suggests that the opposite is true.

13. This does not mean to say that the American authorities did not occasionally intervene to influence the pound-dollar exchange rate in this period.
The three most important currencies in the 1930's were the pound, the (US) dollar and the (French) franc. These were the currencies most frequently used for trading purposes and also to which other (minor) currencies tended to be pegged. It is therefore appropriate that the relationship between these three be examined first and, in fact, provide the main testing ground for the model.

Broadly speaking, the international monetary system in the first half of the 1930's can be described as comprising of five different groupings: the exchange control countries (to which the model developed here is obviously not applicable), the countries on a silver standard (such as Mexico and China which were peripheral and therefore of less interest) and the groups of countries attached to each of the three major currencies.

The most formally defined of these was the gold bloc which was officially formed in 1933 following the collapse of the London Conference when it became clear that an orderly return to fixed exchange rates was not to be forthcoming. Consequently, a number of countries (led by France) declared their intention to maintain their pre-1931 parities with gold and the gold bloc came into existence. Less formally defined, though considerably larger, was the sterling area, which included the Empire (except Canada) and some foreign countries (notably in Scandinavia) for whom Britain was the major trading partner. Finally, there was the dollar grouping which consisted of the dollar and a rather smaller number of satellite countries. Although this situation was modified
somewhat following the Tripartite Agreement in 1936, the pound-dollar-franc triangle remained at the centre of the system.

The degree to which any of these currencies were freely floating is actually very limited: this was only true of the pound from September, 1931 to spring, 1932 (when the EEA was established), of the dollar from April, 1933 to January, 1934 (or more strictly October, 1933 if the Roosevelt Administration's gold buying policy is taken into account), and of the franc from July, 1937 to early May, 1938 (though even then there was occasional official intervention by the French stabilisation fund). Moreover, if the EEA's activities are fully taken into account then managing the pound must inevitably have affected the franc and the dollar and so the latter two countries were never freely floating against the pound. Nevertheless, to the extent that official intervention and any obvious period of under or over-valuation can be captured by dummy variables, the model developed in the preceding chapter can usefully be applied to all three "sides" of the triangle.

The three exchange rates are presented graphically in Figure 5.1 for the period July, 1931 to December, 1938. As they are obviously interrelated an overview of their general movements is more easily presented in terms of what was happening in a given sub-period rather than by looking at the individual currencies. (A detailed account of the specific movements of each currency is provided in the next section). The first sub-period runs from September, 1931 to March, 1933 and began with Britain abandoning the gold standard. By the end of 1931 the pound had dropped by about a third of its gold standard value against both the franc and the dollar. There was a brief recovery in the spring of 1932 but the pound had dropped back to its December, 1931 level (or rather slightly below it) by the end of that year. The franc-dollar
FIGURE 9.1
POUND-DOLLAR-FRANC EXCHANGE RATES (July, 1931-Dec., 1938)

A = franc-dollar (cents per franc)
B = pound-dollar (dollars per pound)
C = pound-franc (francs per pound)
exchange rate was not especially affected by the pound's depreciation in this period as the two currencies both remained on the gold standard and so their rate of exchange fluctuated only slightly around its par value.

The next sub-period runs from April, 1933 to January, 1934; it begins with the US leaving the gold standard and ends with its return, the dollar having depreciated by some 40 per cent. Obviously this affected both the pound-dollar and franc-dollar exchange rates and furthermore, ushered in a period during which the dollar was clearly undervalued. The pound-franc rate was, of course, little affected by the dollar depreciation and the pound remained at more or less the same (depreciated) level against the franc in this period. During the next two and a half years (February, 1934 to September, 1936) the main focus of attention shifted to the franc which was still on the gold standard and subsequently had effectively appreciated against both the pound and the dollar. The depreciation of the pound in 1931 had caused some difficulty but the depreciation of the dollar in 1933-34 left the franc in a very vulnerable position and the French found themselves increasingly having to forego the possibility of domestic recovery and deflate the economy in order to maintain the value of the franc. The problems had obviously begun when Britain left gold in 1931 but this third sub-period witnessed the final deterioration and collapse of the French position as several other members of the gold bloc fell by the wayside and either devalued (Belgium) or imposed exchange controls (Italy). Meanwhile the pound-dollar rate fluctuated in the $4.80 to $5.10 range (approximately) and, in fact, continued to do so in the fourth (and final) sub-period - October, 1936 to December, 1938 - until the pound depreciated somewhat against the dollar in the latter half of 1938.
In September, 1936, the Tripartite Agreement was announced which marked a return to co-operation and the end of unilateral exchange rate changes. By mutual consent, the franc was devalued but this action proved to be insufficient to restore even relative stability and consequently, although the co-operation continued, the depreciation of the franc also continued against both the pound and the dollar until mid-1938. In conclusion, it is clear that there was considerable fluctuation in all of the three major exchange rates in the 1930's and hence they are well suited to econometric analysis. As indicated above, the next section looks at each currency (and country) individually and, on the basis of these examination, constructs dummy variables to take account of influences which would not be picked up by "economic fundamentals" but rather reflect speculative factors.

(II)

On September, 21st, 1931, Britain left the gold standard and this marked the beginning of the end as far as the inter-war gold standard was concerned. Whether Britain had adopted a wise course of action in returning to gold in the first place is highly dubious. This particular episode in British monetary history and the reasons why Britain ultimately abandoned the gold standard are well documented and need not be repeated here. Suffice it to say that the event was not entirely unexpected and did not cause the widespread panic that some had predicted. In the longer run it did contribute to the worldwide collapse of the gold standard but in the short run the effects were rather milder: a number of countries (mainly in the Empire and...
Scandinavia) also left the gold standard, there was a run on the dollar (which was weathered) and a widespread switch on the Continent from a gold exchange to a gold bullion standard (which contributed to the pressure on the dollar).

The pound itself depreciated rapidly at the end of September but then more or less stabilised in October before falling sharply in November and early December. Although seasonal sales of pounds were a factor, the depreciation of the pound in November–December, 1931 was arguably caused largely by temporary and speculative factors. These included speculative sales of pounds by people who had bought before the general election (thereby stabilising the pound in October) anticipating a government victory and who now feared inflation, the liquidation of sterling balances in Amsterdam (in December) and a rush of imports to beat the (imminent) tariff. Thus, it seems fair to conclude that:

"...on balance sterling fell during the closing months of 1931 under the pressure of non-recurrent forces, which by now Jan., 1932 should have largely spent themselves. In these circumstances it is not unduly optimistic to look for a rally in sterling to its 'natural' level." (4)

This is precisely what happened. The rising trend in the pound began, in fact, in late December and reached its peak with the "most sensational developments"(5) in early March. To some extent this reflected the lack of the normal demand for imports, many of which had been bought in the rush to beat the tariff. However, the major cause of this appreciation seems to have been a change in speculative sentiment:

"A more important role, however, in causing the recovery of sterling has been played by capital transactions, in whose case psychological factors are of primary importance... very few foreign observers believed that we should be able to avoid inflation; and the absence of any appreciable price rise here has created an unexpectedly favourable impression abroad". (6)
It therefore appears that, due largely to speculative factors, the pound was at an artificially low level in the December, 1931 to February, 1932 period and this marks the first British speculative dummy. From April (1932) to the end of the year the pound became much less erratic and depreciated gradually with no apparent major bout of speculation-induced variation.\(^{(7)}\)

In 1933 the foreign exchanges were dominated by events in America and any tendency of the pound to depreciate (or appreciate) due to "British" speculative influences is likely to be swamped (or amplified) by movements in the dollar. However, during part of the period immediately preceding the American devaluation (Jan.-Feb., 1933), the pound did show a tendency to appreciate due to a capital inflow and was "very strong against all countries":\(^{(8)}\)

"There is little doubt that the main factor in the recent tendency of sterling to appreciate has been an influx of capital...."\(^{(9)}\)

This period will constitute the second British speculative dummy. The only other incident of interest was a tendency of the pound to weaken against the Continental currencies in September but the Continent itself seems to have believed that this was to some extent seasonal\(^{(10)}\) and, since the pound staged a recovery in both the middle and at the end of the month,\(^{(11)}\) no dummy variable appears justified.

The year 1934 saw some disequilibrium with the pound overvalued against the dollar but undervalued against the franc but nevertheless:

"Taking the year as a whole.....the pound displayed marked stability".\(^{(12)}\)

However, in the middle of the year the pound appeared to deviate from
this stable trend. In May, a "fall in sterling against the dollar and to some extent against the French franc"\(^{(13)}\) was reported and this weakness continued through June and was "...to some extent psychological".\(^{(14)}\) This was caused by a combination of temporary factors (such as US profit taking on the London stock exchange) and speculative influences induced, for example, by a warning of the potential vulnerability of the pound in the form of a suggestion in a B.I.S. report that the level of sterling balances held by foreigners in London had become as high as it had been in 1930.\(^{(15)}\) The weakness recurred in August and, although it was partly seasonal, it was aggravated by speculative sales of pounds on the Continent associated with fears of a devaluation of the pound (to offset intensified American competition due to a further dollar devaluation thought to be likely). In September, however, the pound appears to have recovered and indeed strengthened slightly in the last quarter of 1934. Consequently, a third British dummy will be constructed to cover the pound's weakness in the May to August, 1934 period.

Sterling weakened again in the first three months of 1935 but this was largely a reflection of dollar strength associated with the impending gold clause judgement (and is therefore picked up by an American dummy). However, the origin of the weakness was partly British and so a British dummy variable may be appropriate as the pound's relationship against Continental currencies may have also been affected. This was especially true of March when the "depreciation of the pound...proceeded so rapidly...that it...attained the status of 'front page news'".\(^{(16)}\) The reasons for the weakness were "of a psychological character...[with]...foreign confidence in sterling receiving a sudden shock through a combination of circumstances",\(^{(17)}\)
including events on the Stock Exchange and rumours of an early general election. In view of all this, it seems prudent to construct a fourth dummy variable for March, 1935. There was one further bout of (much less marked) sterling weakness in 1935 (Sept.-Oct.) but this was partly seasonal and partly due to nervousness in Europe generally caused by the Italy-Ethiopia dispute, and is therefore more appropriately viewed as dollar strength.

In 1936 attention in the foreign exchange market shifted to the gold bloc and disturbances emanated mainly from this source and will consequently be picked up by non-British dummies. This was also true of 1937 when the pound:

"preserved an even tenor, any possible consequences of the year's adverse trade balance being completely offset by international capital movements". (18)

More importantly, the reasons for these capital movements were external in origin and are therefore picked up elsewhere; they include a gold scare in the form of a rumour that the US buying price of gold was about to be lowered and a capital flight from France due to uncertainties there. The major speculative influence on the pound in 1938 occurred in the last quarter of the year and is (once again) better viewed as having an external origin:

"While it is natural to speak of the recent exchange movements as a depreciation of sterling it would be much more correct to describe the past fortnight as having witnessed an appreciation of the dollar. Such a description is justified, because the compelling force upon the exchanges has been the general flight of funds from Europe, rather than from England, to the United States". (19)

Thus there are no British speculative dummies for 1936, 1937 or 1938. The British dummies are summarized in Table 5.1 below.
TABLE 5.1: BRITISH SPECULATIVE DUMMIES

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>PERIOD ON</th>
<th>TENDENCY OF £</th>
<th>REASON</th>
</tr>
</thead>
<tbody>
<tr>
<td>K1</td>
<td>Dec. 1931-Feb. 1932</td>
<td>weak</td>
<td>Fears of inflation; rush of imports to beat the tariff.</td>
</tr>
<tr>
<td>K2</td>
<td>Jan.-Feb. 1933</td>
<td>strong</td>
<td>Capital inflow.</td>
</tr>
<tr>
<td>K3</td>
<td>May-Aug. 1934</td>
<td>weak</td>
<td>Temporary factors; fears of inflation; reports of the pound's vulnerability.</td>
</tr>
<tr>
<td>K4</td>
<td>Mar. 1935</td>
<td>weak</td>
<td>Loss of confidence in the pound (various reasons).</td>
</tr>
</tbody>
</table>

The Americans reacted quietly (though with some surprise) to Britain leaving gold; they took the view that Britain's position justified her action and consequently there was no great tendency to criticise. That is not to say that the British action did not have consequences for the dollar: on the fall of the pound, attention shifted to dollar and in October there was a large drain of gold from the U.S. (particularly to France), as numerous European currencies converted from a gold exchange to a gold bullion standard and consequently began to change devisen into gold at a great rate. There were also other reasons for the dollar to weaken including a loss of confidence in the American banks - "It is true that in certain respects the American banking position has been causing misgivings" - and a belief that Hoover's new economic proposals, particularly the formation of the National Credit Corporation, "represent the administration of a mild dose of inflation." The outflow of gold (to Europe) continued until the end of the year but October was the month when the dollar seemed under the greatest pressure.
and, to some extent, the outflow to Europe was being offset by an
inflow from Japan by November. Thus October, 1931 marks the first
American speculative dummy. (23)

The year 1932 was one of two halves for the dollar which tended
towards weakness until mid-June and then recovered in the second half
of the year. The beginning of the U.S. policy of credit expansion
(Hoover's version) in January and the passing of the Glass-Steagle
Act, which permitted a further expansion of Federal reserve credit,
in February led to some misgivings about the dollar, but the real
weakness seems to have developed in April-May after it had been
revealed that the American budget was unbalanced. This appears to
have led to a minor run on the dollar and therefore April-May, 1932
marks the second American speculative dummy. The dollar then recovered
in mid-June and maintained its position due to a combination of the
balancing of the budget, low British interest rates (the cheap money
policy) and seasonal demand for dollars - the latter two influences
being picked up by other variables in the model - and also the fact
that the process of running down her New York balances, which had
been undertaken by France in the first half of the year, came to an
end. This recovery was interrupted in October when Hoover revealed
that the U.S. had come close to abandoning the gold standard in the
spring but the consequent speculative movement of funds against the
dollar was short-lived and recovery was swift.

The next five months cover the interregnum between Roosevelt's
election and his taking office. This was a period of some uncertainty
aggravated by the lack of co-operation between the President and President-elect over the two major problems of the period - the war debt defaults
and the banking failures - with the latter apparently causing the dollar
to weaken in January–February 1933. Thus, there is a case for treating the whole period as one of expected dollar weakness although it may be argued that, given the extent of Roosevelt's electoral victory, a period of dollar strength in anticipation of his eventually taking power might have occurred. Either way, a dummy variable for the November, 1932 to March, 1933 period is appropriate with the probability (though not certainty) of it being associated with dollar weakness.

On April 19th, 1933 the gold standard was formally suspended in the U.S. In spite of the banking crises, the dollar was not forced off gold, as the pound had been before it, but rather it was a conscious act of policy. A nine month period of depreciation then took place until the dollar was restabilised in January, 1934; the depreciation was a rather uneven affair and was particularly rapid in the period immediately following the suspension of the gold standard and again in the October–November (1933) period, but with a pause in the early autumn and even a slight recovery in December. The rapid depreciation of the dollar in the April–June period was undoubtedly related, to some extent, to rises in U.S. prices (particularly wholesale prices although the cost-of-living index also rose) and to the rise in employment indices (which proxy income). However, the tendencies encouraged by the movements of these indices were aggravated by capital movements: there was a flight of U.S. capital out of the country, a withdrawal of foreign capital invested in American securities, a widespread failure to repatriate the proceeds of American export sales and also an element of purely "speculative" capital outflow. This continued until the early autumn when the depreciation came to a temporary halt due partly to bears of dollars covering their position and also the beginning of seasonal purchases of dollars but mainly because of rumours of impending stabilisation of the dollar.
In fact such rumours proved to be totally incorrect, since in late October Roosevelt announced a new policy of official gold purchases with the stated objective of depreciating the dollar and, for the next six weeks, deliberate government policy and a fresh flight of capital combined to cause a second phase of rapid dollar depreciation until it was stabilised at 59.06 per cent of its old value (in terms of gold) in January, 1934. There was no rise in the American price indices or income proxies during this second phase of rapid depreciation which suggests that it was due wholly to "non-economic fundamental" causes. Consequently, according to many sources, the dollar entered into a period during which it was considerably undervalued which lasted (approximately) until the end of 1934. This undervaluation of the dollar overshadows any tendency of the dollar to fluctuate (for speculative reasons) in 1934 although it should be pointed out that fears of inflation did cause the dollar to weaken slightly in three months - April, August and October. Thus it is clear that the whole of the April 1933 to December 1934 period was one of dollar undervaluation at least in part because of speculative factors, although this applies less to the first phase of rapid depreciation (April-June, 1933) than the second (November, 1933 to January, 1934). Therefore it seems plausible to construct two dummy variables - $S_4$ (April-October, 1933) and $S_5$ (November, 1933-December, 1934) - to allow for the implication of this which is that the degree of undervaluation may well have differed between the two sub-periods (being less for the first that the second).

In the next two years attention in the foreign exchange shifted to the gold bloc and therefore much of the speculative influence on the dollar is picked up elsewhere. Nevertheless, there are two periods
which would seem to merit an American speculative dummy. Firstly, there was the uncertainty caused at the beginning of 1935 by the impending Gold Clause Judgement. \(^{(28)}\) Had the decision gone against the government, there was a widespread feeling that the dollar would have been revalued (in terms of gold) and, consequently, speculative funds flowed into New York. On February 18th, the Supreme Court ruled narrowly (and controversially) in favour of the government and this removed any likelihood of revaluation. However, there was surprisingly no reaction against the dollar in the second half of the month:

"...contrary to previous expectations, there was no great rush to sell dollars..." \(^{(29)}\)

"...there was comparatively little selling of dollars.... This suggests that there has been a genuine movement of funds from Europe to the United States..." \(^{(30)}\)

Indeed, according to one source, \(^{(31)}\) the tendency of the dollar to appreciate continued until April (encouraged by the belga devaluation in 1935) and consequently, the sixth American dummy runs from January to April, 1935.

There were then a series of months during which the dollar may have been affected by speculative capital flows caused by various "non-economic fundamental" events: the American silver purchases in July, the nervousness in Europe in autumn caused by the Italy-Ethiopia conflict, fears of inflation in January-February, 1936 stemming from the passing of the Veteran's Bonus Bill with the consequent emergence of a budget deficit and the tendency of the dollar to weaken against the pound in sympathy with the franc in mid-1936. However, none of these events seem to have exerted a strong enough effect to merit
a dummy variable or, in one case (the Italy-Ethiopia war), the influence is likely to be picked up elsewhere. In fact, other than the Gold Clause Judgement, the only incident that would seem to require a speculative dummy in the 1935-36 period is the large inflow of "hot money" in October-November, 1936 which was of "a non-recurrent nature" and, to some extent, even this is more a gold bloc phenomenon than an American one.

In December, 1936 a programme of gold sterilization was introduced to prevent further gold inflows from creating excess reserves and in January, 1937 the Federal Reserve Board raised reserve requirements. However, the gold inflow continued in the first three months of 1937 and reached its height with the gold scares of (particularly) April and late May/early June. These "gold scares" involved rumours of a cessation of gold purchases and a drop in the dollar price of gold (and hence a dollar appreciation). This would seem to suggest a dummy variable for the whole of the January-June, 1937 period to take account of dollar strength.

In late 1937, however, the tendency of the dollar reversed itself:

"This second 'gold scare' reversed itself into a Dollar scare' late in the same year, after the stock market and business activity had slumped in the United States and some fear had arisen that the dollar might be devalued against gold as an antirecession measure. The speculative outflow of funds from the United States continued through the first half of 1938 and in the ten months up to July amounted to nearly $1 billion". (33)

This trend in the dollar was accelerated in April (1938) by Roosevelt's announcement of a new programme of "pump-priming" which was felt by many to be "inflationary in character" and by early June:
"The general feeling on the Continent is that sooner or later the dollar will once more be devalued. Early in the week there were strong rumours to this effect..." (35)

All this would suggest a further dummy variable for the October, 1937 to July, 1938 period to reflect dollar weakness.

The last five months of 1938 marked a political crisis in Europe and a consequent flow of funds to America. It began in August when "a world-wide rush into dollars and gold"(36) was reported which extended into September and reached its peak in early October when it was observed that:

"It is no longer a question of the flight of hot money. Instead balances which have been held in London for years are now migrating to New York". (37)

The nervousness in Europe (and capital outflow) was apparent throughout October even after the Munich Agreement and "continued at a slackened rate in November and December"(38). Therefore a final American dummy will be constructed for the August-December, 1938 period to reflect dollar strength. The American speculative dummies are summarized in Table 5.2 below.

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>PERIOD ON</th>
<th>TENDENCY OF DOLLAR</th>
<th>REASON</th>
</tr>
</thead>
<tbody>
<tr>
<td>S1</td>
<td>Oct. 1931</td>
<td>weak</td>
<td>Gold outflow (aftermath of pounds depreciation).</td>
</tr>
<tr>
<td>S2</td>
<td>Apr.-May 1932</td>
<td>weak</td>
<td>Fears of inflation; unbalanced budget.</td>
</tr>
<tr>
<td>S3</td>
<td>Nov. 1932-Mar. 1933</td>
<td>weak</td>
<td>Interregnum.</td>
</tr>
<tr>
<td>S4</td>
<td>Apr.-Oct. 1933</td>
<td>weak</td>
<td>Undervaluation.</td>
</tr>
<tr>
<td>S5</td>
<td>Nov. 1933-Dec. 1934</td>
<td>weak</td>
<td>Undervaluation.</td>
</tr>
<tr>
<td>S6</td>
<td>Jan.-Apr. 1935</td>
<td>strong</td>
<td>Gold clause judgement.</td>
</tr>
<tr>
<td>S7</td>
<td>Oct.-Nov. 1936</td>
<td>strong</td>
<td>Inflow of &quot;hot money&quot;.</td>
</tr>
<tr>
<td>S8</td>
<td>Jan.-June 1937</td>
<td>strong</td>
<td>&quot;Gold scares&quot;.</td>
</tr>
<tr>
<td>S9</td>
<td>Oct. 1937-July 1938</td>
<td>weak</td>
<td>&quot;Dollar scare&quot;.</td>
</tr>
<tr>
<td>S10</td>
<td>Aug.-Dec. 1938</td>
<td>strong</td>
<td>Political tension in Europe.</td>
</tr>
</tbody>
</table>
In many ways the experience of France in the 1930's contrasts sharply with that of Britain and the U.S.A. The depression did not reach France until fairly late but then lasted throughout the decade until a kind of recovery was brought about by rearmament in 1938. There was no cheap money policy or New Deal to facilitate recovery; indeed, there was "unbelievable ignorance of economic questions in business as well as political circles" and consequently no-one (not even exporters) wanted devaluation and, in the face of an overvalued currency for much of the period, the only alternative was deflation. This was pursued by a succession of governments and there were certainly plenty of these in France in the 1930's - nineteen different administrations in all in the September, 1931 to December, 1938 period. In fact political instability was a well-known feature of France at this time:

"A political wit once remarked that in France the Budget is always well on the way to being balanced, and that in the political sphere, France is generally in a state of crisis, with occasional lapses into stable government". (40)

One might therefore expect a large number of speculative dummies for France and this does rather turn out to be the case.

Examining events in more detail, the British devaluation in September, 1931, despite its longer run implications, does not seem to have had a dramatic effect on the franc; the U.S.A. was still on the gold standard, the French economy was still not feeling the full effects of the depression and, in any case, France still had ample gold reserves to deal with any run on the franc that might develop. Nevertheless, a shift towards deflationary policies was begun and, in the second half of 1932, Herriot's government continued the half-hearted attempt to institute budgetary equilibrium begun by the preceding...
Tardieu and Laval administrations. However, Herriot's slim and shifting majority proved insufficient and, in late 1932, his policies and his government were defeated. This marks the first of the French dummies (November-December, 1932) which is associated with franc weakness.

The difficulties continued into 1933 and, in the January 1933-February, 1934 period a succession of governments (five in all) tried and failed to impose fiscal cuts in an attempt to deflate the economy and maintain the position of the franc. The situation was clearly worsened by the depreciation of the dollar and matters came to a head in late 1933 when a contemporary article, entitled "The French Crisis", suggested:

"The present crisis in France has its origin in two distinct factors....the Budget disequilibrium....and....the....factor of group-politics". (41)

Confidence was further undermined by an element of social unrest which eventually led to riots. (42) Thus it appears likely that the franc may also have weakened because of "non-economic fundamental" influences in November-December, 1933 and the second French dummy covers this period.

In March, 1934, Gaston Doumergue, the ex-President, came out of political retirement to form a "Cabinet of National Union" and the presence of a "strong man" had the desired effect. (43) The budget situation was improved, confidence returned and the franc strengthened. (44) The Doumergue government lasted until late October when it fell in an unsuccessful attempt to get further fiscal cuts and the whole period (March-October, 1934) forms the third French dummy. In November, Flandin took office and, whilst he accepted deflation and a movement
towards budgetary equilibrium, he did engage in a mild attempt at reflation in some areas, lowering long term interest rates for example.

However, in 1935 outside events began to overtake France once again and at the end of March of that year Belgium devalued. This immediately put pressure on the rest of the gold bloc including a run on the franc which reached alarming proportions, encouraged by the Communist victories in the May municipal elections, and between May 20th and May 24th alone, the Bank of France lost 1½ billion francs. At the end of May Flandin fell, to be replaced by Laval on June 7th after a new government under Buisson which only lasted a few days. The Laval administration initiated a policy of savage deflation and this constituted the last sustained attempt to solve French problems by "orthodox" means. It was done by a series of decree laws - 549 in all - that affected nearly every part of the French economy and administration. Unfortunately, Laval was ultimately unsuccessful and there was no attempt to renew the powers of decree when they expired in October. The effect the Laval deflation had on confidence and therefore the franc is debateable. There is evidence that it distilled confidence in that the French Central Bank was able to lower Bank Rate in August, 1935, but on the other hand, there are contemporary reports of a weak franc. 

However, the franc certainly does appear to have weakened in the following month (November):

"The political situation in France is now causing some anxiety". (47)

"The franc....was seriously affected by new doubts concerning the general French financial outlook. Between October 25th and November 15th, the Banque de France lost Frs. 1,769 millions of gold...." (48)
To summarize, the franc would seem to require three speculative dummies for 1935: two of these to reflect weakness - April-May and November - due to the aftermath of the belga devaluation and political and financial uncertainties respectively and one, associated with the Laval deflation (June- October) of uncertain sign.

In early 1936 the franc appears to have weakened again. The January decrease in Bank Rate was soon proved to be premature and, at the end of the month, "the French Governmental crisis...accentuated nervousness over the franc". The weakness continued through February and March, aggravated by rumours of devaluation and the nervousness due to the German reoccupation of the Rhineland. A seventh dummy will therefore cover the January-March, 1936 period. However, the real difficulties for the franc in 1936 came later in the year after the election of the Popular Front in late May which was to govern for nearly two years with ultimately little success.

The first Blum administration (June, 1936-June, 1937) is associated with the "Blum experiment" and has been split into three sub-periods. In the first, June to September, 1936, business recovery and social reform were viewed as going hand in hand. Consequently, a number of reflationary measures were introduced in June including wage increases (the Matignon agreements) and the introduction of a forty hour week "in principle". This new reflationary policy was not a success: confidence was not revived and, in fact, production fell, unemployment increased and there were intermittent flights from the franc throughout the whole period (despite a brief rally in July).

Following the Tripartite Agreement of September 26th, the franc
was devalued on October 1st and the Poincaré franc with a fixed gold content (65½ mg.) was replaced by the Auriol "elastic franc" franc, so called because it was fixed only within limits (43 mg.-49 mg.); a stabilisation fund was also established. This marks the beginning of the next sub-period (October, 1936-February, 1937) which witnessed the general enforcement of the forty hour week and a brief, uneven (and temporary) revival of the economy. As far as the experience of the franc is concerned the period needs to be split in half. From October to December, 1936 there is conflicted evidence: on the one hand, there is some evidence that the capital outflow, if not quite reversing itself, at least temporarily came to halt; on the other (and in much greater volume) there are reports that the franc remained weak and the capital outflow continued.

This apparent continued weakness is not surprising. There were at least two good reasons for this: firstly, the government's attempt to penalise gold hoarders by forcing them to surrender it at the old parity or taxing them heavily discouraged any return of gold and, secondly, the inexact nature of the French devaluation - the "elastic" franc - created uncertainty about the precise level at which the franc would eventually settle, providing scope for further depreciation.

According to one author:

"It is by no means unlikely that the system of an elastic franc...both encouraged speculation against the franc and discouraged the return of capital from abroad". (55)

In the first two months of 1937 it was, however, quite clear that the franc was predominantly weak. On January 29th, the newly established exchange stabilisation fund was forced to admit that it had run out of gold in its attempts to support the franc and, throughout January and February, the French Finance Minister (Auriol) felt
it necessary to wage a campaign against rumours of further devaluation.

The third sub-period (March-June, 1937) follows the announcement by Blum on February 14th of a "pause" in social reform to allow present legislation to be "digested". This was another attempt to boost confidence and in fact some expenditure cuts were made. It effectively marked the end of the "Blum experiment". It may have had some beneficial effect but the dominant picture still seems to have been one of continued franc weakness due to the "confused political scene in France..." and "...the vacillating attitude of the Government". The "elastic" franc was abandoned and the currency was left free to find its own level. The whole period of the (first) Blum ministry seems to have been associated with rumour, uncertainty and consequently speculative influences generally and four dummy variables would seem appropriate: two of these (June-September, 1936 and January-February, 1937) are unambiguously associated with franc weakness and the other two (October-December, 1936 and March-June, 1937) are likely to be, although there were influences working in the opposite direction.

In July, 1937, the new Chautemps regime began by increasing taxes and restoring the 15% cut in tariffs imposed by Blum. However, these measures failed to restore confidence which still sagged, largely because the policies of the Blum government had not been successfully reversed - there was still a budget deficit and the forty hour week was still in force. There is ample contemporary evidence to indicate that the franc remained weak in the July-October, 1937 period and what seems to have happened is that the June crisis had not been resolved but merely converted to a situation of low confidence and, therefore, this
period — July-October, 1937 — marks the final French speculative dummy.

In fact, this weakness was such that the French President felt it necessary to call the Council of Ministers to his summer chateau at Rambouillet and to issue an official announcement — the Declaration of Rambouillet — to the effect that there was no technical or objective reason for the speculative attack on the franc but even so, the present government was still determined to solve the country's financial and economic problems without using exchange controls. This had an immediate beneficial effect on confidence and coupled with the stock market crash in New York a few weeks later led to a return of funds to France and a recovery of the franc in late October which also remained "steady in November and December".(58) Finally in 1938 there were a number of factors which may have affected the franc briefly, but nothing of exclusively French origin that seems likely to have produced a sustained effect on confidence. The French speculative dummies are summarized in Table 5.3 below.

**Table 5.3: French Speculative Dummies**

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>PERIOD ON</th>
<th>TENDENCY OF FRANC</th>
<th>REASON</th>
</tr>
</thead>
<tbody>
<tr>
<td>F2</td>
<td>Nov.-Dec. 1933</td>
<td>weak</td>
<td>Budget deficit, difficulties of forming a government, social unrest.</td>
</tr>
<tr>
<td>F4</td>
<td>Apr.-May 1935</td>
<td>weak</td>
<td>Aftermath of belga devaluation.</td>
</tr>
<tr>
<td>F6</td>
<td>Nov. 1935</td>
<td>weak</td>
<td>Political and financial uncertainties.</td>
</tr>
<tr>
<td>F7</td>
<td>Jan.-Mar. 1936</td>
<td>weak</td>
<td>Rumours of devaluation, nervousness due to German reoccupation of Rhineland</td>
</tr>
<tr>
<td>F8</td>
<td>June-Sept. 1936</td>
<td>weak</td>
<td>Beginnings of &quot;Blum experiment&quot;.</td>
</tr>
<tr>
<td>F10</td>
<td>Jan.-Feb. 1937</td>
<td>weak</td>
<td>Exhaustion of exchange fund reserves, rumours of devaluation.</td>
</tr>
<tr>
<td>F11</td>
<td>Mar.-June 1937</td>
<td>?</td>
<td>&quot;Passive&quot; phase of &quot;Blum experiment&quot;.</td>
</tr>
<tr>
<td>F12</td>
<td>July-Oct. 1937</td>
<td>weak</td>
<td>Aftermath of &quot;Blum experiment&quot;.</td>
</tr>
</tbody>
</table>
Estimation of the model proceeded as follows: two versions of each equation were normally estimated, one using cost-of-living indices, the other wholesale price indices; in the case of the pound-dollar exchange rate, the availability of appropriate data allowed the estimation of a third version using wage indices. The rest of the world was represented by France, the U.S.A. and Britain in the pound-dollar, pound-franc and franc-dollar exchange rates respectively. In the first instance employment indices are used to proxy income, the "wide" version of the official intervention dummies are used and the interest rates are monthly averages of market rates of discount for three month prime bills in the various centres.

Before any computations were undertaken the three sets of speculative dummies were compared to see if there was any overlap (which might explain insignificance in the initial estimates) or direct correspondence (which would imply perfect multicollinearity and therefore that certain variable(s) should be omitted). In fact only one incidence of direct correspondance occurred: this was for the franc-dollar estimates where S8 (January-June, 1937) corresponded exactly to a combination of F10 (January-February, 1937) and F11 (March-June, 1937) and consequently S8 was omitted from the franc-dollar estimates. Having done this, the procedure was then to estimate each version of the different equations with all variables included; the second stage involved omitting the clearly insignificant variables (or more exactly those with a t-statistic less than unity); the third (and final) stage involved experimentation by replacing some of the insignificant proxy variables by alternative proxies (for example, unemployment instead of employment indices for income), omitting more
insignificant variables\(^{(60)}\) and ultimately "final" versions of each equation would emerge (which are presented below).

Before considering the performance of individual variables, reference should be made to F-tests that were carried out for two sub-groups: the "economic fundamentals" (prices, national incomes, interest rates, official intervention and seasonal variables) and the speculative "non-economic fundamental" (dummy) variables. This seemed an appropriate preliminary exercise to the extent that the central hypothesis of the study relates to whether exchange rates were determined by one or the other (or both) of these groups and, more fundamentally, lack of significance of either or both groups would have led to extensive modification of the subsequent estimates. In fact, both groups of variables were clearly significant for all three exchange rates. However, it may be worth noting that the F-statistics were considerably higher for the "economic fundamentals\(^{(61)}\) although this may be unimportant, given that the speculative dummies probably only capture part of the total speculative effect.

The single most important exchange rate in the inter-war period was undoubtedly that between the pound and the dollar and this relationship is therefore discussed first. The first stage estimates produced results which were reasonably consistent with the general hypothesis with both "economic" and speculative variables generally significant (at various levels) and correctly signed, very high coefficients of correlation and Durbin-Watson statistics towards the top of the inconclusive range. There was little to choose between the three different versions of the equation in terms of performance although the WP (wholesale prices) equation did have a slightly lower standard error of the regression than the COL (cost of living) and WG (wage indices) versions.
The performances of individual variables were also satisfactory. The relative income variable and pound-dollar ID (official intervention dummy) were significant and correctly signed in all three versions and the relative price variable in two (WP and WG) although the U.K.-U.S. interest rate differential only in one (COL) and then only at the 10% level. In addition ROW (rest of the world) prices and the pound-franc ID were significant (and negative). ROW income and interest rate differentials were found to be insignificant. Furthermore there was evidence of a seasonal effect: as expected the seasonal variables with larger frequencies (A3 and B3) were not significant but A1, A2 and B2 were. The trend variable was significant and positive. All the speculative dummy variables were significant in one or more equations (although a few were incorrectly signed) with the exception of K2, K4 and S7. The insignificance of the latter is not altogether surprising as it represented an inflow of hot money into the U.S. in the October-November, 1936 period and was, to some extent, a gold bloc rather than American phenomenon. The second stage estimates (not surprisingly) tended to support these initial observations with only one modification: ROW income was proxied by another variable (French production instead of French income) and became significant. The final versions of the pound-dollar equation are presented in Table 5.4.

The importance of relative price and (especially) relative income effects of the kind predicted by the theory (a rise in U.K. prices or income leads to a depreciation of the pound) is clear. The insignificance of the COL ratio undermines this conclusion but, on the other hand, it also illustrates perfectly the necessity to test more than one type of price index. The use of WP's only by many other
TABLE 5.4(1): THE POUND-DOLLAR FINAL ESTIMATES

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>COL</th>
<th>WP</th>
<th>WAGE</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>7.14 (5.71)</td>
<td>7.65 (11.1)</td>
<td>6.51 (11.5)</td>
</tr>
<tr>
<td>U.K. prices/U.S. prices (2)</td>
<td>-1.17 (0.89)</td>
<td>-2.38 (3.58)</td>
<td>-0.79 (2.40)</td>
</tr>
<tr>
<td>French prices (3)</td>
<td>-1.29 (5.54)</td>
<td>-0.39 (1.53)</td>
<td>-1.46 (6.63)</td>
</tr>
<tr>
<td>U.K. income/U.S. income (4)</td>
<td>-1.23 (4.47)</td>
<td>-1.44 (7.23)</td>
<td>-0.60 (1.53)</td>
</tr>
<tr>
<td>French income (4)</td>
<td>0.76 (1.31)</td>
<td>0.91 (1.77)</td>
<td>0.53 (0.99)</td>
</tr>
<tr>
<td>U.S.-U.K. interest rates</td>
<td>-0.04 (0.84)</td>
<td>0.01 (0.34)</td>
<td>-0.03 (0.69)</td>
</tr>
<tr>
<td>Pound-dollar ID (5)</td>
<td>0.05 (2.66)</td>
<td>0.05 (3.07)</td>
<td>0.04 (2.42)</td>
</tr>
<tr>
<td>Pound-franc ID (5)</td>
<td>-0.04 (1.87)</td>
<td>-0.03 (1.74)</td>
<td>-0.03 (1.68)</td>
</tr>
<tr>
<td>Seasonal variables:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>A1</td>
<td>-0.06 (2.00)</td>
<td>-0.08 (3.69)</td>
<td>-0.05 (2.24)</td>
</tr>
<tr>
<td>A2</td>
<td>-0.04 (2.30)</td>
<td>-0.04 (2.58)</td>
<td>-0.03 (1.86)</td>
</tr>
<tr>
<td>B2</td>
<td>0.08 (4.12)</td>
<td>0.07 (4.22)</td>
<td>0.07 (3.99)</td>
</tr>
<tr>
<td>Trend</td>
<td>0.02 (12.2)</td>
<td>0.02 (10.2)</td>
<td>0.01 (6.89)</td>
</tr>
<tr>
<td>Speculative dummies:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>K1</td>
<td>-0.43 (4.70)</td>
<td>-0.33 (3.79)</td>
<td>-0.46 (5.38)</td>
</tr>
<tr>
<td>K3</td>
<td>-0.15 (1.93)</td>
<td>-0.14 (2.03)</td>
<td>-0.15 (1.99)</td>
</tr>
<tr>
<td>S2</td>
<td>0.12 (1.23)</td>
<td>0.16 (1.81)</td>
<td>0.07 (0.79)</td>
</tr>
<tr>
<td>S3</td>
<td>-0.22 (2.62)</td>
<td>-0.15 (2.08)</td>
<td>-0.28 (3.89)</td>
</tr>
<tr>
<td>S4</td>
<td>0.38 (4.33)</td>
<td>0.42 (5.46)</td>
<td>0.35 (4.34)</td>
</tr>
<tr>
<td>S5</td>
<td>0.74 (14.8)</td>
<td>0.64 (13.2)</td>
<td>0.74 (15.7)</td>
</tr>
<tr>
<td>S6</td>
<td>0.20 (3.06)</td>
<td>0.11 (1.82)</td>
<td>0.17 (2.68)</td>
</tr>
<tr>
<td>S8</td>
<td>-0.18 (2.79)</td>
<td>-0.15 (2.61)</td>
<td>-0.15 (2.40)</td>
</tr>
<tr>
<td>S9</td>
<td>0.30 (3.86)</td>
<td>0.21 (3.15)</td>
<td>0.32 (4.28)</td>
</tr>
<tr>
<td>S10</td>
<td>0.13 (1.18)</td>
<td>-0.06 (0.69)</td>
<td>0.10 (0.98)</td>
</tr>
</tbody>
</table>

R - SQUARED                      | 0.97         | 0.98         | 0.98         |
DURBIN-WATSON STATISTIC         | 1.72         | 1.99         | 1.67         |
STANDARD ERROR OF THE REGRESSION | 0.112        | 0.101        | 1.108        |
F - STATISTIC                   | 118.5        | 144.3        | 127.7        |

Notes:
1. T-statistics in brackets.
2. No French wage index was available and so in the WG equation French prices were represented by the French COL index.
3. These are both employment indices.
4. Production index (The employment index was insignificant).
5. These are the "wide" versions (see ch. 4, appendix.)
studies has already been criticised as being at best incomplete and at worst actually misleading; in fact, had this procedure been used in the present study, one might have (wrongly) concluded that there was strong support for a relative price effect and been (quite rightly) subject to the criticism, that the bias of WP prices towards traded goods makes the correlation spurious. The use of only WP and COL versions would have strengthened this criticism. However, the inclusion of a wage version pre-empts it since the wage index contains even less traded goods than a COL index (none, to be precise). Consequently, it can be argued that the results in Table 5.4 illustrate that whether a price index based on traded goods (WP) or non-traded goods (WG) is used there is still evidence of a significant relative price effect (despite the insignificance of the COL ratio).

The U.K.-U.S. interest rate differential was insignificant. This was not entirely surprising for at least three reasons: in the first place, a comparable study of the pound-dollar rate in the 1930's obtained similar disappointing results; secondly, only one interest rate (discount rates for three month prime bills) is used to proxy "the interest rate" and this may be inappropriate; thirdly, and more fundamentally, it is possible that fears of exchange rate changes far outweighed the importance of changes in interest rates as an influence on the movement of short term capital. However the pound-dollar ID was highly significant (at the 1% or 2% level) and correctly signed in all three equations, indicating that the presence of official intervention in any particular month added or subtracted (depending on direction), on average, approximately five cents to the exchange rate.

There is also evidence of a significant ROW effect particularly
with reference to prices and official intervention. However, the ROW income variable did not perform very well: when proxied by French employment it was insignificant and though the use of a French production index produced better results, even this was only significant at low levels in two equations (20% in the COL equation and 10% in the WP equation) and remained insignificant in the other (WG). Thus, whilst it can be argued with some certainty that an increase (decrease) in ROW (French) prices is associated with a depreciation (appreciation) of the pound against the dollar, one can only tentatively conclude that an increase (decrease) in ROW (French) income was possibly associated with an appreciation (depreciation). ROW interest rates appear to have had no effect; this is not surprising given the insignificance of the U.K.-U.S. interest rate differential. The pound-franc intervention dummy carries an unexpected sign in that it implies attempts to support the pound against the franc caused it to weaken against the dollar. This could suggest that the British authorities were successful in managing the pound-dollar but not the pound-franc exchange rate, which is not entirely implausible, but must wait until the performance of the same variable in the pound-franc estimates is considered before it can be examined more fully.

Three of the seasonal variables were significant and their combined effect is shown graphically in Fig. 5.2 (on the assumptions that \( A_1 = -0.06 \), \( A_2 = -0.04 \) and \( B_2 = +0.07 \), which would seem to be reasonable compromise values based on their coefficients in the three different versions of the equation). The main expectation of seasonal movement in the pound-dollar rate is the well-known (and well documented) tendency for the pound to weaken in the autumn and strengthen
in the spring. The former effect is clearly evident in Fig. 5.2, the latter less so. There is also evidence of a strengthening of the pound in the summer although this could be construed as sterling strength (or at least tendency not to weaken, if full account is taken of April-May) in the whole of the February-August period, in which case the sterling autumnal weakness and spring (and summer) strength hypothesis can be interpreted as being strongly supported. The trend variable is positive and significant. A gradually depreciating pound (against the dollar) reflecting growing fears of war in Europe would have implied a negative sign and so the trend presumably reflects higher productivity growth in Britain.

The speculative dummies perform very much as expected: they are mainly significant and, with two exceptions, carry the expected signs. Two of the British variables are significant, $K_1$ and $K_3$, picking up
the pound's artificial weakness at the turn of 1931 and the influence of the variety of factors which caused the pound to weaken in mid-1934 respectively. The variables that were insignificant in the first two stages were dropped from the final equations presented in Table 5.4 but should nevertheless be discussed also. Two of these were British variables but since both of these overlap with significant American dummies - K2 with S3 and K4 with S6 - and carry the same expected signs, then their effect is arguably being picked up elsewhere and hence their insignificance is easily explained.

Four of the nine U.S. dummies were significant and correctly signed in all three equations. S8 and S9 reflect the gold and dollar "scares" in the 1937-38 period and S4 and S5 the undervaluation of the dollar in 1933-34; this undervaluation was substantial (by approximately 40 cents in April-October, 1933 and 70 cents in November, 1933-December, 1934) and, as anticipated, rather larger after the initiation of the American gold buying policy in late 1933. S2 is also significant (at the 10% level) in one and correctly signed in all three equations, suggesting that the expected speculation against the dollar may have occurred in August-May, 1932. There were two insignificant American dummies: the insignificance of S7 is not entirely surprising, as this represented an inflow of "hot money" which was more a gold bloc than American phenomenon. (66) The other insignificant variable - S10 - was, in fact, significant and correctly signed in the second stage estimates and its performance in the final equations was puzzling.

The two final American speculative dummies - S3 and S6 - were both significant but incorrectly signed. S6 is perhaps easier to explain. It covers the January-April, 1935 period when there was an inflow of capital into the U.S. suggesting a negative sign. However,
it immediately follows the 1933-34 period in which the dollar was undervalued and a positive sign may therefore be construed as evidence that the undervaluation continued into 1935. In fact, one can go even further since the much smaller coefficient of S6 (compared to S5) indicates that the extent of the undervaluation had decreased from approximately 70 cents to less than 20 cents; the reason for this rapid correction was presumably, in part, the capital inflow and paradoxically, therefore, in spite of its "wrong" sign, the performance of S6 may still be interpreted as providing evidence of a capital inflow in early 1937.

In the case of S3, which represents the interregnum between Roosevelt being elected and taking office, some doubt was expressed about the expected sign in the first place. On balance, dollar weakness seemed more likely but the possibility that Roosevelt's victory engendered confidence was put forward. On reflection, this is perhaps improbable if not impossible, and a more plausible explanation of S3's sign may be that it is reflecting official intervention supporting the dollar during a period of weakness (not picked up by the pound-dollar ID). Indeed, this may turn out to be a reasonable general explanation of wrongly signed dummy variables, although the size of the coefficient in this case (0.15-0.28) is considerably larger than the average size of an intervention indicated by the pound-dollar ID (0.05). In fact, the coefficients of the speculative dummy variables are generally much larger than that of the ID; this may suggest that the pound-dollar exchange rate was occasionally subject to very large speculative movements which were too big for the authorities to resist.

To summarize, the performance of the individual variables in the three versions of the pound-dollar equation is very much in keeping
with the expectations about each of them. In more general terms, the contents of Table 5.4 would seem to confirm the view that, whilst the pound-dollar rate was subject to speculative interference on many occasions, it was still largely determined by rational economic influences and in that sense was not violently unstable.

The second most important exchange rate in the 1930's was probably the pound-franc rate and, in general, the application of the model to this relationship was also reasonably successful. In the first and second stage estimates of both versions (COL and WP) there was strong evidence of relative price and interest rate differential effects. The relative income variable was insignificant and so (as in the pound-dollar equations) the French employment index was replaced by a production index to proxy income and this new variable (the ratio of U.K. employment to French production) was significant though wrongly signed. ROW (American) prices and income were significant in the COL version and some seasonal variables, the trend and approximately half the speculative dummies also appeared to be important. The coefficient of correlation was high and Durbin-Watson statistic was in the middle of the indeterminate range. The third and final stage estimates are presented in Table 5.5.

These final equations strongly support the hypothesis that bilateral economic influences were major determinants of the exchange rate: the relative price variable is highly significant and the income ratio and French-U.K. interest rate differential are also significant in both versions. Unfortunately the income ratio carries the "wrong" sign (and was insignificant in the earlier estimates when a different French income proxy was used in the earlier estimates), which would imply that a rise in British (French) income led to a decrease in
TABLE 5.5: THE POUND-FRANC FINAL ESTIMATES

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>COL</th>
<th>WP</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>158.0 (10.1)</td>
<td>177.8 (15.7)</td>
</tr>
<tr>
<td>U.K. prices/French prices</td>
<td>-175.0 (27.2)</td>
<td>-119.1 (20.7)</td>
</tr>
<tr>
<td>U.S. prices</td>
<td>132.5 (4.89)</td>
<td>11.3 (0.35)</td>
</tr>
<tr>
<td>U.K. income/French income (2)</td>
<td>13.4 (2.08)</td>
<td>18.7 (2.63)</td>
</tr>
<tr>
<td>U.S. income (3)</td>
<td>-45.5 (3.68)</td>
<td>-9.07 (0.48)</td>
</tr>
<tr>
<td>French-U.K. interest rates</td>
<td>-1.21 (2.12)</td>
<td>-2.64 (4.52)</td>
</tr>
<tr>
<td>Seasonal variables:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>A1</td>
<td>-0.38 (0.52)</td>
<td>-2.88 (3.62)</td>
</tr>
<tr>
<td>B1</td>
<td>3.19 (4.49)</td>
<td>1.93 (2.37)</td>
</tr>
<tr>
<td>B2</td>
<td>-1.23 (2.01)</td>
<td>-0.78 (1.08)</td>
</tr>
<tr>
<td>Trend</td>
<td>0.70 (12.4)</td>
<td>0.74 (9.79)</td>
</tr>
<tr>
<td>Speculative dummies:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>K2</td>
<td>4.71 (1.63)</td>
<td>0.83 (0.24)</td>
</tr>
<tr>
<td>F3</td>
<td>-9.42 (5.72)</td>
<td>-8.78 (4.97)</td>
</tr>
<tr>
<td>F5</td>
<td>8.19 (3.57)</td>
<td>8.05 (3.10)</td>
</tr>
<tr>
<td>F6</td>
<td>11.4 (2.86)</td>
<td>9.91 (2.15)</td>
</tr>
<tr>
<td>F7</td>
<td>5.10 (1.96)</td>
<td>0.04 (0.01)</td>
</tr>
<tr>
<td>F8</td>
<td>-11.0 (4.65)</td>
<td>-17.4 (6.09)</td>
</tr>
<tr>
<td>F9</td>
<td>1.34 (0.52)</td>
<td>-9.30 (2.99)</td>
</tr>
<tr>
<td>F10</td>
<td>-8.82 (2.97)</td>
<td>-16.9 (5.04)</td>
</tr>
<tr>
<td>F11</td>
<td>-15.7 (6.59)</td>
<td>-13.7 (4.99)</td>
</tr>
<tr>
<td>R - SQUARED</td>
<td>0.99</td>
<td>0.99</td>
</tr>
<tr>
<td>DURBIN-WATSON STATISTIC</td>
<td>1.38</td>
<td>1.34</td>
</tr>
<tr>
<td>STANDARD ERROR OF THE REGRESSION</td>
<td>3.64</td>
<td>4.19</td>
</tr>
<tr>
<td>F - STATISTIC</td>
<td>400.5</td>
<td>301.5</td>
</tr>
</tbody>
</table>

Notes: 1. T-statistics in brackets.
2. U.K. employment index and French production index.
demand for French (British) goods; such widespread existence of negative income effects would seem highly unlikely, although it is probably that only one of the two income variables is behaving like this and, had the variable been broken down with French and British income included separately in the regression, this might have been apparent.

Nevertheless, the implication still remains an odd one and the most likely explanation would seem to be that the income effects on the pound-franc exchange rate were being distorted by government intervention (trade controls and exchange fund activity), even though this does not seem to have occurred for the pound-dollar and franc-dollar rates where the income ratios were significant and correctly signed. Some solace may be drawn from Ozmun's study which, using the same data for the same period, strangely obtained the opposite results, finding significant and correctly signed income ratios whether French employment or production indices were used although, to some extent, this only adds to the confusion.

The French-U.K. interest rate differential, on the other hand, is significant and correctly signed, the only bi-lateral interest rate to be so in the pound-dollar-franc triangle. This is not surprising as there are numerous contemporary reports of the authorities raising the discount rate during periods of franc weakness and this result would indicate the success of such measures. However, this conflicts with Ozmun's study which found the same variable to be insignificant and incorrectly signed. He consequently suggested that:

"The discount rate was normally raised in response to depreciations of the franc and was therefore viewed by speculators as a signal that the franc was in trouble".
This argument is not unattractive but the results in Table 5.5 would appear to contradict it.

There is some evidence of a ROW effect in that ROW prices and income are significant in the COL equations suggesting that a rise in ROW prices and a fall in ROW income were associated with a franc depreciation against the pound. The less marked ROW effect compared with the pound-dollar exchange rate may reflect the choice of the ROW proxy: conscious efforts were avowedly made by France to expand links with the rest of the gold bloc and so some account of the gold bloc should possibly have been taken into considerations in formulating the ROW proxies.⁷¹ ROW interest rates appeared to play no role in determining the pound-franc rate (which was also the case for the pound-dollar rate). The trend variable is positive and significant indicating a secular decline of the franc against the pound; this may indicate higher British productivity (as in the pound-dollar case), increasing war scare on the continent - one would have anticipated a flow of funds to America but this might have occurred via Britain - or that the franc was overvalued in 1931 and this was gradually corrected throughout the sample period.

There is also evidence of a seasonal effect with a combination of three different variables significant. The seasonal effects indicated by the two versions of the equation are plotted in Fig. 5.3 (on the assumption that $A_1 = 0$ $B_1 = +3.19$ and $B_2 = -1.23$ in the COL version and $A_1 = -2.88$ $B_1 = +1.93$ and $B_2 = 0$ in the WP version). It can be seen that the pattern is basically the same particularly in the first part of the year, the main difference occurring in the final quarter with the WP version suggesting an earlier seasonal weakening of the pound against the franc (October) than the COL version.
FIGURE 5.3

ESTIMATED SEASONAL VARIATION IN THE POUND-FRANC

(December). The overall impression is of a weak pound (strong franc) in November-March and strong pound (weak franc) in April-October.

There is no particular expectation about seasonal variation in this exchange rate - the crop moving-induced variation of the pound against the dollar applied equally to Britain and Europe - but it could imply that France bought less of these North American goods than Britain. What is perhaps surprising is the larger seasonal variation (+3.9% of the average value of the exchange rate in 1931-8) of the pound-franc rate compared to the pound-dollar rate (+2.4%).

The speculative dummies did not perform as well as they had in the pound-dollar estimates. Nearly half (seven out of sixteen) were insignificant and these are examined first. The insignificance of
K4 was not entirely surprising as it had also failed to show up in the pound-dollar estimates but K1 and K3 had been significant and correctly signed. K3 is easy to explain: its period of operation falls exactly within that of a significant French dummy with the same expected (and actual) sign (F3). The reason for the significance of K1 is less obvious since there was a dip (strengthening of the franc) in the pound-franc rate in December 1931-February, 1932 similar to that in the pound-dollar rate. The most likely explanation would seem to be that this was caused by changes in other variables. An examination of the French price indices and interest rates did not reveal any potential cause but there was a discontinuous drop in the French "income" (production) index of twelve points from November, 1931 to March, 1932 (82.6 to 70.4) which may have caused a franc appreciation.

There are four insignificant French dummies. Three of these are also insignificant in the franc-dollar estimates: F1 and F2 represented potential speculation due to political uncertainties which clearly seems not to have occurred; the insignificance of F4, which represents the run on the franc after the Belgian devaluation in 1935, is more surprising but most probably reflects the existence of official support of the franc (not picked up by the intervention dummy). The fourth insignificant French dummy is F12; its performance would suggest that the collapse of the Blum regime and his replacement by Chautemps in July, 1937 did restore confidence to some extent.

Of the remaining eight (significant) French speculative dummies half were correctly signed, half incorrectly. In the former group is included F5 - the Laval deflation - which had no expected sign but was positive implying franc weakness; however, it was correctly signed in the sense that it was also significant with the same implication in the
franc-dollar estimates. The Doumergue ministry (F3) seems to have had the hoped for effect and caused the franc to strengthen (73) whilst the political and financial uncertainties of November, 1935 (F6) and early 1936 (F7) appear to have led to franc weakness. The four significant and wrongly signed variables - F8, F9, F10 and F11 - correspond with the Blum experiment and paradoxically, therefore, they suggest franc strength throughout the period (June, 1936 - June, 1937). In two cases there was some reason to be doubtful of the direction in which the franc would tend (F9 and F11), but it was felt that weakness was the most likely, and F8 and F10 should definitely be associated with weakness. The most plausible explanation (once again) would seem to be the presence of heavy official support for the franc.

It is perhaps instructive to examine the performance of the speculative dummies in Ozmun's study (1976) of the same exchange rate. The dummies here were constructed independently (and from more sources) and so there are important differences but, nevertheless, the major incidents led to the creation of similar dummies in both studies, although the period of operation may differ slightly. Broadly speaking, where similar dummies existed in both studies they performed in a similar manner with two exceptions: the Laval deflation dummy (F5) and the Blum "pause" dummy (F11) which, whilst being positive and negative (respectively) here, were curiously negative and positive in the Ozmun study.

However, Ozmun did find that the performance of a (single) "Blum experiment dummy" covering the June, 1936-February, 1937 period and corresponding to F8, F9 and F10, did unexpectedly indicate a stronger franc although he suggested rather different reasons for this:
"...there was in fact a strong "Keynesian" flavour to the Popular Front programmes. Measures which reduced unemployment and increased consumers' income served business and industry as well as the labour force. Higher wages could result in higher profits. To the extent that the experiments of the Popular Front government were viewed in this latter perspective, their effect would have resulted in an appreciation of the franc". (74)

In addition, two of Ozmun's variables that anticipated franc weakness and correspond (approximately) with F2 and F12 were insignificant as they were here. Finally, a variable representing the impact on the franc of the belga devaluation is significant and correctly signed, whereas F4 was not; however, Ozmun's variable also incorporates the November, 1935 capital flight which is picked up by another (significant) variable (F6) in the present study. In conclusion, the performance of the speculative dummies in the pound-franc estimates is clearly less satisfactory than in the pound-dollar estimates, but an important speculative effect nevertheless emerges, even if it is occasionally in an unanticipated direction.

The third leg of the pound-dollar-franc triangle was the franc-dollar exchange rate and the performance of the model in the earlier estimates were equally satisfactory. In the first stage estimates there was firm evidence of relative price and relative income effects and also of ROW effects. The coefficient of determination was high, as was the Durbin-Watson statistic (greater than two). In addition there was some indication that official intervention was effective although it was the pound-dollar ID rather than the franc-dollar ID that was significant; however, when the "wide" version of the latter variable was replaced by the "narrow" version it did become significant (and correctly signed). Finally, some seasonal variation seems to have occurred and numerous speculative dummies were significant though the trend variable was not. The final versions of dollar-franc equation are presented in Table 5.6.
### TABLE 5.6(1): THE DOLLAR-FRANC FINAL ESTIMATES

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>COL</th>
<th>WP</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>26.6 (7.89)</td>
<td>15.7 (5.71)</td>
</tr>
<tr>
<td>French prices/U.S. prices</td>
<td>-5.76 (14.8)</td>
<td>-4.98 (16.8)</td>
</tr>
<tr>
<td>U.K. prices</td>
<td>-10.8 (4.46)</td>
<td>-5.16 (4.02)</td>
</tr>
<tr>
<td>French income/U.S. income(2)</td>
<td>-2.54 (6.96)</td>
<td>-3.14 (7.77)</td>
</tr>
<tr>
<td>U.K. income(3)</td>
<td>-3.89 (1.40)</td>
<td>1.09 (0.40)</td>
</tr>
<tr>
<td>French-U.K. interest rates</td>
<td>0.07 (3.06)</td>
<td>0.06 (2.60)</td>
</tr>
<tr>
<td>Pound-dollar ID(4)</td>
<td>0.03 (1.04)</td>
<td>0.06 (2.18)</td>
</tr>
<tr>
<td>Dollar-franc ID(5)</td>
<td>0.22 (2.12)</td>
<td>0.16 (1.49)</td>
</tr>
<tr>
<td>Seasonal Variables: A1</td>
<td>0.05 (1.35)</td>
<td>-0.09 (2.75)</td>
</tr>
<tr>
<td>B2</td>
<td>0.11 (4.23)</td>
<td>0.11 (4.02)</td>
</tr>
<tr>
<td>Trend</td>
<td>0.003 (0.62)</td>
<td>-0.005 (1.13)</td>
</tr>
<tr>
<td>Speculative dummies: F4</td>
<td>-0.13 (0.92)</td>
<td>-0.11 (0.73)</td>
</tr>
<tr>
<td>F5</td>
<td>-0.19 (2.13)</td>
<td>-0.30 (3.34)</td>
</tr>
<tr>
<td>F9</td>
<td>-0.69 (4.09)</td>
<td>-0.39 (2.30)</td>
</tr>
<tr>
<td>F10</td>
<td>-0.71 (5.60)</td>
<td>-0.29 (1.99)</td>
</tr>
<tr>
<td>F11</td>
<td>-0.61 (5.26)</td>
<td>-0.23 (1.26)</td>
</tr>
<tr>
<td>F12</td>
<td>-1.00 (7.88)</td>
<td>-0.63 (3.67)</td>
</tr>
<tr>
<td>S3</td>
<td>-0.07 (0.63)</td>
<td>0.05 (0.44)</td>
</tr>
<tr>
<td>S4</td>
<td>0.60 (5.26)</td>
<td>0.55 (7.30)</td>
</tr>
<tr>
<td>S5</td>
<td>1.10 (12.7)</td>
<td>0.76 (11.4)</td>
</tr>
<tr>
<td>S6</td>
<td>0.15 (1.26)</td>
<td>-0.03 (0.30)</td>
</tr>
<tr>
<td>S7</td>
<td>-0.45 (2.31)</td>
<td>-0.66 (2.87)</td>
</tr>
<tr>
<td>S9</td>
<td>0.11 (1.22)</td>
<td>0.15 (1.50)</td>
</tr>
</tbody>
</table>

| R - SQUARED                            | 0.99        | 0.99        |
| DURBIN-WATSON STATISTIC                | 1.77        | 1.86        |
| STANDARD ERROR OF THE REGRESSION        | 0.156       | 0.155       |
| F - STATISTIC                          | 331.8       | 334.5       |

Notes:  
1. T-statistics in brackets.  
3. Based on British employment index.  
5. "Narrow" version.
The correct signs (negative) and high t-statistics of the relative income and (particularly) relative price variables are immediately apparent although the French-U.S. interest rate differential is excluded as it had been insignificant in the earlier estimates. An increase in ROW prices and possibly also ROW income was associated with an appreciation of the dollar against the franc. There were, however, problems with the latter variable which was significant in only the COL equation and then only at the 20% level. Furthermore, ROW (British) "income" in Table 5.6 is actually based on the British unemployment index as the employment index had been insignificant in the first stage estimates. The French-U.K. interest rate differential is significant (as it was in the pound-franc estimates) and carries a plausible sign implying that a widening of this differential caused the franc to strengthen not only against the pound but also the dollar. It does, however, make the insignificance of the French-U.S. differential puzzling although, since the U.K.-U.S. differential was insignificant in the pound-dollar estimates but the French-U.K. differential was significant in the pound-franc estimates, this may simply imply an inappropriate choice of American interest rate.

Official intervention seems to have influenced the franc-dollar exchange rate both directly and indirectly: the "narrow" franc-dollar ID is significant and correctly signed and the "wide" pound-dollar ID is negative and significant in the WP version implying that support for the dollar against the pound also caused it to strengthen against the franc. The insignificance of the "wide" franc-dollar ID (but significance of the "narrow" version) may imply that only major interventions had a significant effect on this exchange rate. This conclusion is supported by the size of the coefficient on the "narrow"
version which is comparatively large indicating that official intervention led to exchange rate changes of the order of 3.2 - 4.4%. This can be compared to the pound-dollar ID which was associated with changes of (approximately) only 1.1% in the pound-dollar rate (Table 5.4) and 1.2% in franc-dollar rate (Table 5.6).

The trend variable remains insignificant even though a secular decline in the franc against the dollar might have been expected due to increasing fears of war. It may be that the trend variable began too early to capture this effect (since Hitler did not come into power until 1933) or that the influence was an uneven one or that it was mostly felt in 1939 (which is not part of the sample period). There is, however, evidence of a seasonal effect although different combinations of variables are significant in each version of the equation. They are plotted in Fig. 5.4.

The seasonal effect in the COL version is not entirely clear: the same variables (as in the WP version) are significant but, whilst B2 has the same value and sign, A1 has a different value, the opposite sign and is only significant at the 20% level. Therefore two alternative seasonal patterns were plotted in Fig. 5.4 for the COL version, one based on both variables, the other with A1 = 0. In fact, all three potential seasonal patterns are strikingly similar, with identical peaks and troughs in terms of timing, the only difference being the value of the troughs. The main expectation about the seasonal pattern of the franc-dollar exchange rate is that it would probably be similar to that of the pound-dollar rate since European currencies as a whole tended to weaken against the dollar in the autumn and strengthen in the spring. This is precisely what Fig. 5.4 suggests. There was also an unexplained weakening of the franc (strengthening of the dollar) in
April-June and strengthening of the franc (weakening of the dollar) in July-September. This corresponds closely to the pound-dollar pattern with the dollar tending to strengthen and then weaken in the April-August period (Fig. 5.2). Furthermore the size of the seasonal variation in the franc-dollar rate (± 2.2%, ± 3.2%) is of a similar
order to that of the pound-dollar rate (± 2.4%) and to a lesser extent that of the pound-franc rate (± 3.9%).

Fewer speculative dummies were significant than in the pound-dollar and pound-franc estimates but all that were significant were correctly signed. A further difference relates to the tendency for smaller coefficients in the WP version than in the COL version (with the exception of F5 and S7). The performance of the American dummies was broadly consistent both with expectations and their performance in the pound-dollar estimates. S1, representing the flight of gold from the U.S. to the gold bloc in November, 1931, rather surprisingly fails to show up, and it must be assumed that official resistance (through intervention not picked up by the ID) prevented the implications of the gold movement from affecting the course of the exchange rate. S2, which was only significant at low levels in the pound-dollar equation, is insignificant here. This is less unexpected than the failure of S3 which was significant in the latter. However, the interregnum period (between Roosevelt's election and taking office) was one of conflicting influences, and it may be that, as far as the franc-dollar exchange rate is concerned, they cancelled each other out.

S4 and S5 represent the 1933-34 period of dollar undervaluation and perform as expected indicating that the undervaluation was greater in 1934 (following the U.S. gold buying policy) than in 1933. They further suggest, in conjunction with earlier results, that the undervaluation was greater vis-a-vis the franc than the pound, which is perfectly consistent with historical fact since any undervaluation of the dollar might have been expected to be compounded by the overvaluation of the franc in this period. It was anticipated that S6
would be associated with dollar strength due to the uncertainties caused by the impending gold clause judgement in early 1935, but it was, in fact, significant and wrongly signed in the pound-dollar estimates indicating dollar weakness; this was interpreted as suggesting a continued undervaluation of the dollar into 1935 and it may have been that in the determination of the franc-dollar exchange rate these two offsetting influences cancelled each other out thereby causing S6 to be insignificant. S7 and S9 are correctly signed and significant (albeit the latter at rather low levels) and, although S8 was excluded, because it corresponded exactly with two French dummies of the same expected sign (F10 and F11) their significance may be at least partly attributed to the influence represented by S8. S10 is insignificant as it was in the final estimates of the pound-dollar exchange rate.

The significant French speculative dummies are all correctly signed but there is some conflict with their performance in the pound-franc estimates where this was not the case. F1, F2 and F4 were insignificant in the estimates of both exchange rates. Four variables - F3, F6, F7 and F8 - which were significant in the pound-franc estimates were not significant here. This is a little surprising but it may reflect a tendency for events in France to have more impact (in terms of effects in confidence) in Europe than in America although this contention is somewhat undermined by the significance of F12 which was insignificant in the pound-franc estimates. However, this is not necessarily the case since the "Blum experiment" (and the dummies associated with it) do seem to be a plausible exception to the suggested rule that incidents in France may have influenced European opinion more strongly. Another exception is the Laval
deflation (F5) which seems to have created equal pessimism about the franc on both sides of the Atlantic.

The four "Blum dummies" (F8, F9, F10 and F11) were significant and wrongly signed in the pound-franc estimates but in the dollar-franc estimates three of them are significant and correctly signed. At first sight, therefore, the "Blum experiment" seems paradoxically to have caused the franc to strengthen against the pound but weaken against the dollar. This is not an impossible scenario but would seem an unlikely one; fortunately, a more plausible alternative explanation is available when the four variables are examined individually. F8 is insignificant and there is no conflict whilst F9, F10 and F11 correspond virtually exactly with American speculative dummies which represent periods of expected dollar strength. This is particularly true of F10 and F11 which correspond exactly with S8 which was significant and correctly signed in the pound-dollar estimates but was excluded here (to avoid multicollinearity). Consequently an alternative interpretation of F10 and F11 would be that they indicate the importance of American rather than French influence in the shape of a significant and correctly signed S8. This is strongly supported by the similarity of the actual coefficients of F10 and F11. Thus there is a case for arguing that the "Blum experiment" variables are picking up American and not French influences and therefore there is no conflict between the results in the pound-franc and pound-dollar estimates. (77)

In conclusion, it is clear that the performance of the speculative dummies and indeed, the general performance of the two versions of the franc-dollar exchange rate, are highly supportive of the main hypotheses incorporated in the model; in particular, there is strong evidence of
the expected relative price and relative income effects. However, before deriving some general conclusions about the pound-dollar-franc triangle three outstanding issues have to be dealt with: firstly, the possibility of replacing the speculative dummies by a "blanket" proxy for speculation based on forward exchange market data has yet to be empirically explored; secondly, since many of the variables (most obviously the speculative dummies) appear in more than one equation, an explicit examination of the consistency of their performance would seem appropriate - for example, if the same variable was associated with sterling appreciation against the dollar but depreciation against the franc then this would require explanation; finally, the possibility of positive serial correlation has to be addressed since the Durbin-Watson statistics are persistently in the indeterminate range. These issues are discussed in the next section.

(IV)

In the discussion of how to empirically approximate the existence of speculation (in chapter four) a variety of possibilities were discussed. Ultimately it was decided to try one variant of each of the "generic" types of method that were identified: a measure aimed at picking up the potential speculation induced by specific incidents (dummy variables) and a "blanket" measure (based on forward market data). The former was preferred for a number of reasons, most importantly the fact that "blanket" measures may pick up non-speculative influences and, at the practical level, the lack of suitable data from which to construct "blanket" measures; in fact, for this latter reason - lack of readily accessible forward market data - the "blanket" measure
used here could only be tried for exchange rates involving the pound.

Thus a series of regressions were run for the pound-dollar and pound-franc exchange rates in which the speculative dummies were dropped and replaced by an SV (speculative variable) which consisted of the average three month forward exchange rate adjusted by the three month interest rate differential. Speculation is thereby incorporated on the basis of the assumption that when there was an adjusted forward discount (premium) on a currency, speculators were expecting it to depreciate (appreciate) and capital was therefore flowing out of (into) the country in question in anticipation, causing the spot rate to weaken (strengthen). In both the pound-dollar and pound-franc equations the SV is constructed in such a way as to be expected to have a negative sign.

In the pound-franc equations it did indeed have a negative sign but was only significant at very low levels (20 per cent). In addition the two equations' performances were considerably worsened in terms of higher standard errors of the regression and (slightly) lower R-squareds and F-statistics. Furthermore, the Durbin-Watson statistic became very low indicating the presence of serial correlation. Insofar as this could be taken as evidence of omitted variables (which is a possible cause of serial correlation), one of which could be the presence of speculative capital flows inadequately proxied by the SV, it could be interpreted as suggesting that the SV picked up less of the speculative influence than the speculative dummies and so the latter are a better way of modelling speculation in practice (as well as in theory).
The SV in the pound-dollar estimates was highly significant but incorrectly signed. Offsetting official intervention, not picked up by the ID, might have caused insignificance but a positive sign is difficult to interpret. The explanation is most probably to be found in two sources: the variable may be based on inadequate data or it may not be a suitable proxy for speculation (or both). Concerning the former possibility, the choice of interest rates may have created some distortion; the U.K.-U.S. interest rate differential was insignificant and it has been suggested that the interest rates chosen may be inappropriate. With regard to the latter possibility, it is conceivable that the assumption that speculators dominated the forward market does not hold and therefore speculators were (for some reason) reacting against the expectations embodied in the forward rate or, more fundamentally, the SV was picking up something other than speculative capital flows. Perhaps some solace can be drawn from the observation that generally, in terms of R-squared, the Durbin-Watson and F-statistic and the standard error of the regression the pound-dollar equation performs better if speculative dummies are used rather than SV's. As this is even more clearly the case in the pound-franc estimates the use of speculative dummies in the final equations seems justified.

Use of speculative dummies introduces the question of consistency: do, for example, British speculative dummies perform in a consistent (or at least non-contradictory) way in the pound-dollar and pound-franc estimates? This question is also relevant for the French and American speculative dummies and also for some of the "economic fundamental" variables. The performance of the relevant variables is summarized in Tables 5.7 and 5.8. The expected signs
of the variables are also given in Table 5.7 although, in the present discussion, emphasis is placed mainly on consistency of performance rather than plausibility (since this has already been dealt with at some length above).

There are twenty six speculative dummies in Table 5.7. Four of these are significant and correctly signed in both exchange rates to which they are applicable whilst a further eleven are significant in one but not in the other, giving a total of fifteen dummy variables (58%) that are significant and correctly signed for at least one exchange rate. The former are clearly performing in a perfectly consistent manner whilst the latter are not actually behaving inconsistently. A reasonable explanation would be that significance in one exchange rate but not the other suggests, on the one hand, that events which had effects on foreign confidence did not influence opinion equally strongly in all foreign countries and, on the other, that domestically owned capital did not necessarily flow out to (or return from) all foreign countries in equal proportions when political factors were causing such capital to move; both of these possibilities are quite plausible. (81)

Of the other eleven dummy variables three are significant and "wrongly" signed in one or both exchange rates and, to the extent that adequate explanations were found for these "wrong" signs, there is no inconsistency here. Nor is there any in the performance of the five dummies that were always insignificant; clearly, the events that these variables represented did not have the effect on capital flows that might have plausibly been expected. This leaves three final variables - F9, F10 and F11 - which are significant and suggest a strong franc against the pound but a weak franc against the dollar,
### TABLE 5.7: SUMMARY OF THE PERFORMANCE OF
### THE SPECULATIVE DUMMIES

<table>
<thead>
<tr>
<th>POUND EXPECTED TO:</th>
<th>POUND-DOLLAR</th>
<th>POUND-FRANC</th>
<th>FRANC-DOLLAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>K1 Dec. 31-Feb. 32</td>
<td>D</td>
<td>D</td>
<td>X</td>
</tr>
<tr>
<td>K2 Jan.-Feb. 33</td>
<td>A</td>
<td>X</td>
<td>A</td>
</tr>
<tr>
<td>K3 May-Aug. 34</td>
<td>D</td>
<td>D</td>
<td>X</td>
</tr>
<tr>
<td>K4 Mar. 35</td>
<td>D</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>DOLLAR EXPECTED TO:</th>
<th>POUND-DOLLAR</th>
<th>POUND-FRANC</th>
<th>FRANC-DOLLAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>S1 Oct. 31</td>
<td>D</td>
<td>NI</td>
<td>X</td>
</tr>
<tr>
<td>S2 Apr.-May 32</td>
<td>D</td>
<td>D*</td>
<td>X</td>
</tr>
<tr>
<td>S3 Nov. 32-Feb. 33</td>
<td>D</td>
<td>A</td>
<td>X</td>
</tr>
<tr>
<td>S4 Apr.-Oct. 33</td>
<td>D</td>
<td>D</td>
<td>D</td>
</tr>
<tr>
<td>S5 Nov. 33-Dec. 34</td>
<td>D</td>
<td>D</td>
<td>D</td>
</tr>
<tr>
<td>S6 Jan.-Apr. 35</td>
<td>A</td>
<td>D</td>
<td>D*</td>
</tr>
<tr>
<td>S7 Oct.-Nov. 36</td>
<td>A</td>
<td>X</td>
<td>A</td>
</tr>
<tr>
<td>S8 Jan.-June 37</td>
<td>A</td>
<td>A</td>
<td>NI</td>
</tr>
<tr>
<td>S9 Oct. 37-July 38</td>
<td>D</td>
<td>D</td>
<td>D*</td>
</tr>
<tr>
<td>S10 Aug.-Dec. 38</td>
<td>A</td>
<td>A*</td>
<td>X</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>FRANC EXPECTED TO:</th>
<th>POUND-DOLLAR</th>
<th>POUND-FRANC</th>
<th>FRANC-DOLLAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>F1 Nov.-Dec. 32</td>
<td>D</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>F2 Nov.-Dec. 33</td>
<td>D</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>F3 Mar.-Oct. 34</td>
<td>A</td>
<td>A</td>
<td>X</td>
</tr>
<tr>
<td>F4 Apr.-May 35</td>
<td>D</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>F5 June-Oct. 35</td>
<td>?</td>
<td>D</td>
<td>D</td>
</tr>
<tr>
<td>F6 Nov. 35</td>
<td>D</td>
<td>D</td>
<td>X</td>
</tr>
<tr>
<td>F7 Jan.-Mar. 36</td>
<td>D</td>
<td>D*</td>
<td>X</td>
</tr>
<tr>
<td>F8 June-Sept. 36</td>
<td>D</td>
<td>A</td>
<td>X</td>
</tr>
<tr>
<td>F10 Jan.-Feb. 37</td>
<td>D</td>
<td>A</td>
<td>D</td>
</tr>
<tr>
<td>F11 Mar.-June 37</td>
<td>?</td>
<td>A</td>
<td>D</td>
</tr>
<tr>
<td>F12 July-Oct. 37</td>
<td>D</td>
<td>X</td>
<td>D</td>
</tr>
</tbody>
</table>

**Key:** A = Appreciate, D = Depreciate, X = Insignificant, NI = not included for this exchange rate, * = only significant at low levels or in one variant (WP or COL) of the equation.
thereby behaving in an apparently inconsistent manner. However, a close examination reveals (as already indicated in the discussion of the dollar-franc estimates) that this inconsistency is more apparent than real since all three variables correspond either exactly (F10 and F11 with S8) or almost exactly (F9 with S7) with American dummies associated with dollar strength. Hence F9, F10 and F11 are really picking up (expected) American effects in the dollar-franc estimates. Thus there is no inconsistency in the performance of these (or any other) speculative dummy variables.

The performance of the "economic fundamental" variables which might be expected to behave in a consistent manner across the three exchange rates is summarized in Table 5.8. Although one variable - the pound-franc ID - carries a curious sign in the pound-dollar estimates, there is no actual inconsistency at all in Table 5.8. Only one interest rate differential is significant; this is the U.K.-French differential which implies that a rise (fall) in French interest rates caused the franc to appreciate (depreciate) against both the pound and the dollar. There is no inconsistency but, at first sight, the significance of this variable and not the U.S.-French differential in the franc-dollar estimates seems rather strange. However, no differential which involved the U.S. interest rate (not even the U.K.-U.S. differential in the pound-dollar estimates) was ever significant and this has been put down elsewhere in the chapter to an inappropriate choice of U.S. interest rate and Table 5.8 would seem to support this conclusion. Only one intervention dummy was significant in two equations: this was the pound-dollar ID and its positive sign in both the pound-dollar and franc-dollar equations indicated that attempts to support the dollar against the pound were not only successful in doing just that but had
### TABLE 5.8: A SUMMARY OF THE PERFORMANCE OF "ECONOMIC FUNDAMENTALS"
APPEARING IN THE ESTIMATES OF MORE THAN ONE EXCHANGE RATE

<table>
<thead>
<tr>
<th>An increase in this variable is associated with:</th>
<th>POUND-DOLLAR</th>
<th>POUND-FRANC</th>
<th>DOLLAR-FRANC</th>
</tr>
</thead>
<tbody>
<tr>
<td>U.K.-U.S. interest rate differential</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>U.K.-French interest rate differential</td>
<td>X</td>
<td></td>
<td>D</td>
</tr>
<tr>
<td>U.S.-French &quot; &quot; &quot; &quot;</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Trend variable</td>
<td>A</td>
<td>D</td>
<td>A</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>A positive value of this variable is associated with:</th>
<th>POUND-DOLLAR</th>
<th>POUND-FRANC</th>
<th>DOLLAR-FRANC</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pound-dollar intervention dummy (wide)</td>
<td>A</td>
<td>D</td>
<td>X</td>
</tr>
<tr>
<td>&quot; &quot; &quot; (narrow)</td>
<td>NT</td>
<td>NT</td>
<td>NT</td>
</tr>
<tr>
<td>Pound -franc &quot; &quot; (wide)</td>
<td>D</td>
<td>A</td>
<td>X</td>
</tr>
<tr>
<td>&quot; &quot; (narrow)</td>
<td>NT</td>
<td>NT</td>
<td>X</td>
</tr>
<tr>
<td>Dollar-franc &quot; &quot; (wide)</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>&quot; &quot; (narrow)</td>
<td>NT</td>
<td>NT</td>
<td>NT</td>
</tr>
</tbody>
</table>

**KEY:** A = Appreciate, D = Depreciate, X = Insignificant, NT = not tried.
the side effect of causing the dollar to appreciate against the franc also (which seems quite plausible). The trend variable indicated a tendency for the pound to appreciate against both the dollar and the franc throughout the 1930's.

The third outstanding issue relates to the fact that in all the equations presented above, the Durbin-Watson statistic is in the inconclusive range, albeit towards the top of it in most cases. However, the suspicion of positive serial correlation nevertheless exists and must be faced. In many ways this is not surprising as an obvious a priori reason for it exists in the form of omitted variables, particularly trade controls, but also additional speculative or "non-economic" influences; in this latter connection low Durbin-Watson statistics might be interpreted as suggesting that speculative influences had a greater role in exchange rate determination than is indicated by the speculative dummies. There are various ways of investigating the possibility of serial correlation and two avenues are explored here: firstly, it seemed reasonable to re-estimate the final equations using the Cochrane-Orcutt (CORC) technique since this option was available as part of the computer package that was being used; secondly, and in some ways preferably, given that the Durbin-Watson statistic is really only a summary measure, is to examine the residuals of the OLS final equations (presented above). Doing this would also give some indication of exactly when excluded variables were important and, to the extent that their effects took the form of speculative capital flows, when dummy variables should have been included. The CORC estimates are described first.

Using the CORC technique did slightly "improve" the equations in terms of lower standard errors of the regression, higher R-squareds
and higher Durbin-Watson statistics (for two of the exchange rates) but, on balance, any other changes were inconsequential, and so the results are not presented in detail and only the changes are discussed. The most affected exchange rate was the pound-dollar: the relative price and income variables became significant in one less equation, leaving relative incomes significant in two out of three versions and relative prices in only one, although this was the WG version, arguably the most severe test of the PPP hypothesis, whilst ROW prices and incomes became insignificant in all three variants and the level of significance of the pound-dollar ID (insofar as it could still be called significant) dropped to 30 per cent; on the other hand, the awkwardly signed pound-franc ID is not even significant at this low level, half the previously significant speculative dummies are no longer significant and the U.K.-U.S. interest differential is now significant (and correctly signed) in all three versions.

Whether or not the CORC estimates actually provide any less support for the central hypothesis of "stable" exchange rates is therefore debatable. Furthermore some general doubts about the CORC estimates are created because of the change in sign (yet continued significance) of $S_4$ which now indicates that the dollar was over-valued in April-October, 1933 by some 20 cents. Not only does this run counter to the OLSQ and CORC estimates of the dollar-franc which indicate a substantial dollar undervaluation but it would seem to conflict with historical fact. Therefore, even if it were judged that there is a slight unfavourable balance in support for the model involved in switching from an OLSQ to CORC estimation technique, this should perhaps not be taken too seriously.

This is especially true given that the balance of support for
the model is higher in the CORC estimates of the pound-franc exchange rate. To the fact that half the speculative variables become insignificant, as for the pound-dollar, can be added the insignificance of the previously significant and wrongly signed relative income variable and the observation that the reinstated pound-franc ID is significant (at the 10 per cent level) and correctly signed. Finally as rho is insignificant in both WP and COL equations, the OLSQ and CORC estimates of the franc-dollar exchange rate are, not surprisingly, virtually identical. Thus the CORC estimates do not really shed any further light on the main hypothesis of the study although they would not appear to undermine the OLSQ results.

An examination of the residuals, as already indicated, is likely to be more useful to the extent that it suggests exactly when important variables were being excluded (assuming that this is a plausible reason for any serial correlation). Obviously there is an initial difficulty in that no rules exist for interpreting residuals: how large does a residual have to be to merit consideration and how many consecutive residuals of the same sign constitute a significant cluster?\(^{84}\) The procedure adopted here was to look for months in which the residuals were larger than one standard error of the regression in one of the variants and, unless the residual in the other variant(s) was very small or carried the opposite sign, to examine these plus any adjoining months in which the residuals of all variants carried the same sign. This rule, however, was not rigidly applied and sustained periods in which the residuals carried the same sign but remained smaller than one standard error were also considered. Such an approach should take in all periods of potential interest and all the periods identified by this "rule" are given in Table 5.9.
TABLE 5.9(1): SUMMARY OF LARGE RESIDUALS IN THE
ESTIMATES OF THE POUND-DOLLAR-FRANC TRIANGLE

<table>
<thead>
<tr>
<th>Year</th>
<th>POUND-DOLLAR</th>
<th>POUND-FRANC</th>
<th>DOLLAR-FRANC</th>
</tr>
</thead>
<tbody>
<tr>
<td>1931</td>
<td></td>
<td></td>
<td>Nov. +</td>
</tr>
<tr>
<td>1932</td>
<td></td>
<td>Jan. Feb. {+}</td>
<td>JAN. Feb. -</td>
</tr>
<tr>
<td></td>
<td></td>
<td>MAR. APR. May</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>June July AUG. -</td>
<td></td>
</tr>
<tr>
<td>1933</td>
<td>Sept. OCT. -</td>
<td></td>
<td>Sept. Oct. -</td>
</tr>
<tr>
<td></td>
<td>APR. -</td>
<td></td>
<td>APR. -</td>
</tr>
<tr>
<td></td>
<td>JULY +</td>
<td></td>
<td>JULY +</td>
</tr>
<tr>
<td></td>
<td>Aug. -</td>
<td></td>
<td>Aug. -</td>
</tr>
<tr>
<td></td>
<td>NOV. DEC. +</td>
<td></td>
<td>JAN. FEB. Mar</td>
</tr>
<tr>
<td></td>
<td>Jan. -</td>
<td></td>
<td>May JUNE July -</td>
</tr>
<tr>
<td>1935</td>
<td>SEPT. Oct. -</td>
<td></td>
<td>Nov. Dec. -</td>
</tr>
<tr>
<td></td>
<td>Nov. DEC. -</td>
<td></td>
<td>Nov. DEC. -</td>
</tr>
<tr>
<td></td>
<td>JAN. -</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1937</td>
<td></td>
<td>DEC. + Jan.</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>JUNE +</td>
<td></td>
</tr>
<tr>
<td></td>
<td>May June +</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>May June July</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Aug. +</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note (1) Months in block capitals are those in which the residual was larger than one standard error in all variants of the exchange rate final estimates.
It has been implied that large residuals probably represent omitted variables and therefore can be explained by examining what was happening in foreign exchange markets and specifically looking for potential reasons for speculation; this is the approach of Hudgins (1973) who does not use dummy variables. Unfortunately, in the present study it is likely to be difficult to identify reasons for large residuals because all the plausible reasons have been "used up" to derive dummies already incorporated into the model. However, there are several other possibilities: firstly, information on the effects of protection and all official intervention was not available and therefore either of these two influences may explain large residuals; secondly, dummy variables of constant value are "blanket" measures which ignore the possibility of differing intensity over time of the effects they are seeking to capture. Finally, an alternative interpretation is that serial correlation is not indicative of excluded variables but rather suggests a misspecification of the dynamics of the model; whilst this is a simple model with no dynamics as such, the possibility of such a misspecification still exists if it is interpreted in terms of whether or not the timing of the dummies in the model is correct. It would therefore be instructive to examine whether periods of large residuals are adjacent to periods covered by dummy variables to look for "mistiming".

It is not proposed to examine the residuals on an individual exchange rate basis since any excluded variable that affected, for example, the pound would presumably cause residuals in both the pound-dollar and pound-franc estimates. Therefore it is more appropriate to examine the residuals of all three exchange rates at the same time. In fact, the single large residual in 1931 only actually involved one
exchange rate and occurred in the dollar-franc estimates in November, indicating a weaker dollar than predicted by the model. The explanation for this would seem to be straightforward and lie in a mistiming of S1 which covers October, 1931 and was insignificant; hence the main impact of the outflow of gold from the U.S. to France on the exchange rate appears to have been felt in November, not October as originally postulated.^(85^

Mistiming of a dummy variable - S3 (November, 1932 - March, 1933) - would also seem likely to explain the September-October, 1932 residuals, indicating unexplained dollar strength, in both the pound-dollar and dollar-franc estimates. S3 was expected to be associated with dollar weakness, but in the pound-dollar estimates it was significant and "wrongly" signed. However, this sign was not entirely implausible and hence the mistiming argument used here is not inconsistent with the originally expected sign of S3. The January-May, 1932 positive residuals in the pound-franc estimates are probably explained by the pound's recovery in early 1932; in particular, the large residual in March is indicative of this, given that this is when the "most sensational developments"^(86) took place. The sudden drop in the pound in November, 1931 and its recovery in early 1932 had been modelled, in dummy variable terms, by treating the November, 1931 - February, 1932 period as one in which the pound was artificially weak rather than treating the March-April, 1933 period as one of exceptional sterling strength. This seems to have been appropriate for the pound-dollar exchange rate (since K1 is significant) but the experience of the pound in its fluctuations against the franc appear to have been slightly different.

The other two sets of large residuals in 1932 are less easy to
explain: the June-August residuals in the pound-franc equation suggest sterling weakness and these months did witness a gradual depreciation of the pound against the franc which may indicate some reaction to the sterling strength in the first half of the year and possibly that the pound had reached an artificially high level; alternatively, this may represent official British intervention, aimed at pushing the pound down, carried out by the EEA in the first few months of its existence. The January-February residuals in the dollar-franc equation suggest dollar strength which could also indicate undetected official intervention in the form of support for the dollar, still tending towards weakness in early 1932, although this is admittedly fairly tenuous.

In 1933 large residuals only occur in the two dollar exchange rates and are therefore clearly related to the U.S. abandonment of the gold standard in March. This period represented an artificial undervaluation of the dollar but the actual downward movement was an uneven one and the large residuals almost certainly reflect the inadequacy of using a dummy variable with a constant value to take account of this. The large April negative residuals indicate a strong dollar but the dummy variable (S4) gives the average level of undervaluation in the April-October, 1933 period and the dollar did not depreciate substantially until May. There was a sudden (and large) depreciation in July which the "averaging dummy" must inevitably understate, thereby explaining the large positive residuals in this month. Finally the downward trend in the dollar temporarily reversed itself in August which accounts for the large negative residual in the dollar-franc estimates. (There are also negative residuals in all three versions of the pound-dollar equation in August but they are all less than one standard error and therefore do not appear in Table 5.9).
The positive residuals in both dollar exchange rates at the end of 1933 and going into the beginning of 1934 can also probably be explained in terms of the inadequacy of a constant value dummy. In this case it is the fact that $S_5$ (November, 1933 - December, 1934) cannot fully allow for the rapid depreciation at the end of 1933 whilst simultaneously allowing for the levelling off of the dollar in 1934. Hence, in the November, 1933 - March, 1934 period $S_5$ only picks up part of the effect thereby leaving positive residuals, suggesting unexplained dollar weakness. The negative residuals at the end of 1934 are a symptom of the same problem: just as $S_5$, in "averaging out" the undervaluation of the dollar in November, 1933 - December, 1934, underestimates the degree of dollar undervaluation in the first part of the period, so it seems to overestimate it at the end, leading to negative residuals (suggesting dollar strength).

The two sets of residuals in the pound-franc estimates in 1934 obviously require a different explanation. The reason why there should be unexplained franc strength in January-February, 1934 (negative residuals) is not immediately clear. On the one hand, it does precede a dummy variable designed to pick up franc strength (March-October, 1934) so the mistiming of dummies may be a possibility but, on the other hand, this is rendered unlikely because these residuals immediately follow a period of alleged franc weakness in November-December, 1933 ($F_2$). A more likely explanation is that the franc was receiving official support in early, 1934. There are two good reasons to believe this: firstly, $F_2$ is not significant, possibly indicating official support in late 1933, and secondly, a contemporary source reports that the Bank of France was buying francs in January, 1934. (87) However, the suggestion that the November-December, 1934
residuals, also indicating franc strength, are due to a mistiming (or rather a continued applicability) of F3 is quite plausible.

In 1935, the large residuals are mainly in the pound-dollar estimates. The January residual (dollar weakness) could indicate another averaging problem since the sign and significance of S6 (January-April, 1935) suggests that the 1933–4 undervaluation of the dollar continued into 1935 and it may be that January is more properly considered as part of the period covered by the preceding dummy variable (S5). The May-December residuals, indicating further dollar weakness, are difficult to explain but could be interpreted as evidence that the dollar remained undervalued during the whole of 1935. The other residual in 1935 – December in the pound-franc – which carries over into January, 1936 could plausibly be interpreted as mistiming of (or rather a continuation of the effects of) F6 and F7, both of which are significant.

Just as the residuals in 1933 were all related to the dollar depreciation, so all the 1936 residuals appear to be related to the franc devaluation since they all occur in franc exchange rates. The June residual indicates unexplained franc weakness and could be due to a combination of the averaging implicit in the significance of F8 covering June-September (which indicated franc strength) and the fact that in June, the first month of the Blum administration, the franc was particularly weak. The September residual in the pound-franc equation could be interpreted as representing the very strong support for the franc required in its last month at its old value. Such support did occur and is represented in the pound-franc ID but this variable does not differentiate between strong and weak levels of intervention. Official support for the franc may also explain the positive July-August
residuals in the dollar-franc estimates especially since, although F8 was insignificant, it was positive (indicating a stronger franc).

The October and December residuals in the pound-franc equation are arguably due (once again) to the inability of dummy variables to pick up uneven effects. F9 (October-December) is insignificant in the COL version and significant (strong franc) in the WP version. The positive residual in October and negative residual in December would suggest that the forces causing the franc strength, being picked up by F9, gradually became stronger in this three month period and F9 seems to have gone through the middle of the period (November) averaging out the effect, thereby overstating the degree of franc strength in October and underestimating it in December. A similar problem seems to have occurred in the dollar-franc estimates in the same period, only in this case there are two significant dummies concerned - F9 (October-December) and S7 (October-November).\(^{(91)}\)

The negative residuals in November-December, 1937 in the dollar-franc show unexplained franc strength (dollar weakness). One possible interpretation would be mistiming (or rather continuation) of the final three French speculative dummies - F10, F11 and F12 - which were all significant, indicating sustained franc weakness throughout the first ten months of 1937. However, this is doubtful given that the franc apparently recovered at the end of 1937.\(^{(92)}\) A more plausible explanation may relate to the unevenness of the effect picked up by S9 (October, 1937 - July, 1938), a period of dollar weakness; this would suggest that the weakness of the dollar was more pronounced in the later part of this ten month period than in the earlier part. Such an explanation is consistent with the March-June, 1938 positive residuals in the pound-dollar estimates also being interpreted as
indicative of the unevenness of the effect picked up by S9, since the sign of the residuals would suggest greater dollar weakness in the latter part of the period covered by S9.

The other three sets of residuals involving the dollar in 1938 would all seem to be related to the reported flight of capital from Europe to America in autumn, 1938 and hence S10, which aims to pick up the associated dollar strength. S10 is insignificant in the dollar-franc estimates and the August-December residuals show why: it seems a fairly clear case of mistiming with S10, covering the whole of this period, insignificant because the dollar apparently strengthened in the first three months (especially September and October) and the franc recovered slightly at the end of the year. The August-October residuals in the pound-dollar also overlap with S10 which was insignificant in the final estimates. The positive sign of these residuals (sterling strength) may show why: their sign would indicate that there was some pressure on the pound to appreciate, possibly in the shape of official intervention to support the pound (which was certainly taking place at this time). Finally, the May-August residuals in the pound-franc estimates probably represent an undervaluation of the franc in this period in the sense that it was deliberately depreciated by Daladier in May, 1938. This action possibly required a dummy variable in the first place.

Thus it would seem that there are plausible explanations for the majority of the large residuals in the estimates of the pound-dollar-franc triangle. These explanations relate principally to mistiming of dummy variables, undetected official intervention and the inability of dummies with a constant value (unity) to adequately pick up speculative influences of varying intensity. To this list
might be added the distortions created by tariffs and import controls whose effects, by their very nature, are impossible to quantify and difficult to work out even in general terms (let alone in terms of explanations for specific residuals) and are therefore not used. It is also likely that a variety of minor speculative effects took place which it has not been possible to detect even from contemporary sources.

(V)

At this stage, it might be suggested that, having obtained these results, it would be appropriate to re-estimate modified versions of the final equations, perhaps using new speculative dummies derived from the analysis of the residuals. However, this is resisted for a number of reasons. Firstly, a certain amount of "experimentation" has already been carried out in various ways, most obviously the use of different income proxies and "narrow" and "wide" versions of the ID's in some equations. Secondly, such a procedure is, in some ways, rather dubious: economic models should be developed on the basis of economic theory not empirical results. Finally, and perhaps most important, a halt has to be called somewhere and, to the extent that the basic model "explains" exchange rates and that the large residuals have been satisfactorily accounted for, a modified version of the model is unlikely to shed any further light on the hypotheses being tested.

However, before drawing some preliminary conclusions a few words should be said about a theoretical matter, raised in chapter two, relating to the fact that in any exchange rate triangle to estimate
all three exchange rates involves, in a sense, one estimation too many, in that once two of the exchange rates have been estimated so has the third (implicitly) and its predicted value can be calculated through cross-rates. A number of arguments were advanced at that point, to suggest that the third exchange rate ought to be estimated anyway but it was also indicated that it might be interesting to examine the difference between the predicted direct rate and the predicted cross-rate for each of the three exchange rates in the major exchange triangle, and it was suggested that any large inconsistencies would merit some comment. Before doing this it seemed reasonable to make an adjustment for any differences between the actual direct and actual cross-rates; in fact, these turned out to be very slight (as would be expected) and so the adjustment was a minor one.

The results of this exercise were rather surprising: for all three exchange rates there were frequent and occasionally large differences. For the pound-dollar rate 55% of the observations have a difference between the predicted direct and cross-rates which is greater than one standard error of the regression of the final equation (and 30% are greater than two standard errors). The comparable figure for the other two exchange rates are less discouraging - 30% (10%) for the pound-franc and 40% (15%) for the dollar-franc (94) - but nevertheless quite large, sufficiently so to require some explanation.

This may be found by observing that there are, in fact, three conditions for the predicted direct and predicted cross-rates to be identical and that these may not have been fulfilled. Firstly, there should be no differences between the actual direct and cross-rates but this cannot be used as an explanation here because an adjustment
has been made to allow for it. Secondly, there must be a linear relationship between the dependent and independent variables in the equations for all three exchange rates; this has obviously been assumed to be the case in the estimations and therefore cannot be used as an explanation either. Thirdly, the independent variables should be the same in all three equations. This can be used as it is clearly not the case and no adjustment has been made to make allowance for it. Even in the initial estimates (with all relevant variables included) this was not the case: although the "economic fundamental" variables of all three countries are included in each exchange rate this is not true of the speculative dummies; thus, for example, the pound-dollar estimates include British and American but not French dummies and so on. In the final estimates the differences in the independent variables in the three equations are even greater because the insignificant variables omitted differ for the three exchange rates. Thus this may to a large extent explain the differences between the predicted direct and predicted cross-rates.

Nevertheless, these differences may be interpreted as providing some evidence to suggest that some important influences have been excluded from the model, probably relating to the effects of (unidentified) speculation and the high level of protection. On the other hand, however, it may be argued, with some justification given the lack of fulfilment of the third condition listed above, that to expect accurate predictions of cross-rates is a very severe test of the model and, an optimistic way of interpreting the above results would be to conclude that, not only does the model seem to largely determine movements in directly observed exchange rates, but it also "explains" what determines cross-rates (to within two standard
errors of their predicted direct values) for approximately four-fifths of the time.

To conclude then, the performance of the model in explaining the movements within the pound-dollar-franc triangle is, on the whole, good enough to generate a fair measure of support for most of the hypotheses embodied within it. There is substantial evidence to suggest that the major exchange rates were not exclusively determined by speculative factors but that the underlying "economic fundamental" influences were of great importance and, to that extent, these exchange rates were more "stable" than is commonly supposed.

Furthermore, where speculation was apparently important (in the shape of significant speculative dummies) it was not always entirely clear that it was "destabilising". In a situation where speculation (as represented by the dummy variables) is based on "correct" expectations about changes in "economic fundamentals" then it is stabilising rather than destabilising (although possibly still inconvenient from the government's point of view). Speculation can only really be described as destabilising either where there is some extrapolation of such expectations causing the exchange rate to fluctuate excessively or where it is based on reaction to non-economic fundamentals - for example, to political events - which should not influence the exchange rate. This difficulty of interpretation is present in the other tests of the model and is taken up again in the conclusions of this section.

In the next two chapters the model is tested for exchange rates involving four minor currencies and, on the basis of the results of these tests and of those in the present chapter, some conclusions will be drawn about bilateral exchange rates in the 1930's in general.
FOOTNOTES TO CHAPTER 5

1. This is not strictly true of the franc which was on the gold standard and had no currencies explicitly pegged to it. However, the ability of other countries to remain on the gold standard crucially depended on France staying on gold and, as soon as France left gold and devalued, the remaining gold bloc countries (Switzerland and Holland) were compelled to do the same (against their will). Hence the currencies of such countries were, in a sense, "pegged" to the franc.

2. The classic reference is Keynes' "The Economic Consequences of Mr. Churchill" (1925) and the majority of subsequent writers have concurred with Keynes' view that Britain did not adopt a wise course of action.

3. See D.E. Moggridge's "The Return to Gold, 1925" (1969) and also his "British Monetary Policy, 1924-31" (1972).


5. Economist, 9/7/32, p. 57.


7. The Economist did report some speculation in an article entitled "The Fall in Sterling" in October (29/10/32, p. 774) but argued that the fall was mainly seasonal and therefore a dummy variable is not justified.

8. Economist, 18/2/33, p. 334.


10. At the same time as the spot pound was weakening, Continental purchases of pounds in the forward market, in anticipation of a later seasonal appreciation, caused the forward pound to strengthen. (Economist, 23/9/33, p. 566.)


15. See the Economist, 26/5/34, p. 1145 and 14/7/34, pp. 68-9 for a more detailed list of potential causes of the weakness of the pound.


20. Economist, 10/10/31, p. 656.
21. Ibid, p. 646.
22. Ibid, p. 647.
23. However, it will not be included in the pound-dollar regressions as any tendency of the dollar to weaken was mainly in relation to the Continental currencies and, more importantly, the depreciation of the pound in 1931 would clearly swamp any dollar weakness as far as the pound-dollar exchange rate was concerned.
25. See, for example, Hodson (1938), p. 227, the Statist, 9/6/34, p. 944, the Economist, 10/2/34, p. 301 and R.I.A., Surveys of International Affairs, 1933, p. 92 and 1934, p. 4.
26. According to the Economist (15/12/34, pp. 144-5), the undervaluation had virtually disappeared by this time and most of the effects of it had worked their way through the American economy. The Economist reiterates this point at a later date (30/5/36, p. 492); indeed some of the calculations in the later article suggest that the dollar had become slightly overvalued against the pound by the end of 1934. In view of this, December, 1934 seems an appropriate cut-off point for any dummy variable which seeks to take account of the dollar's undervaluation.
28. The gold clause had been inserted into all American bonds since the "greenback" period and, whilst its exact wording varied, it basically amounted to a promise to redeem the bond in gold dollars. The Gold Clause Judgement related to four cases where the holders of bonds due for redemption were suing the government for the number of gold coins specified by the bond or, in default of that (since the possession of gold had become illegal), for an amount of (depreciated) paper dollars which would equal the value of gold dollars specified in the contract. This would have substantially increased the amount that the government would have to pay to redeem its bonds.
37. Economist, 15/10/38, p. 125.
40. Economist, 2/12/38, p. 1056.
41. Ibid.
43. Sauvy (1969), p. 231: "Doumergue...was hailed as a saviour...He had little difficulty in putting minds at rest".
44. Economist, 12/5/34, p. 1030: "The recovery of confidence in the franc is one of the most striking features of the past few weeks". See also Lloyds Bank Review, April, 1934, p. 166.
45. In fact Danzig actually succumbed on May 2nd, 1935 and devalued its currency by 42%.
49. Economist, 1/2/36, p. 242. See also Yeager (1976), p. 360: "Laval's resignation created another run on the franc".
51. This follows the procedure of Marjolin (1938) and Wolfe (1950).
56. Economist, 29/5/37, p. 194. See also Yeager (1976), p. 364, and Lloyd's Bank Review, April, 1937, pp. 190-1 and May, 1937, p. 248: "The market has lately been inclined to take a less hopeful view of the French financial outlook..."

57. For example, see Lloyd's Bank Review, August, 1937, pp. 463-4 and October, 1937, p. 575, Economist, 24/7/37, p. 187, 18/9/37, p. 563 and p. 568.


59. There were also two groups of partially overlapping dummies: firstly, where the period a (shorter) dummy was "on" fitted entirely within the (longer) period another dummy was "on" - K2 into S3, K3 into S5, K4 into S6, K3 into F3, F1 into S3, F2 and F3 into S5; and secondly, where two dummies overlapped for a month or more - F4 and S6, F9 and S7, F12 and S9.

60. This is an inexact exercise and some insignificant variables may still appear in the final equations. The main reason for this is that where a variable was clearly significant in one version of a particular equation (for example, pound-dollar cost-of-living indices), it was felt to be inconsistent not to include it in other versions of that equation (pound-dollar wholesale prices and pound-dollar wage indices), regardless of its level of significance in the first and second stage estimates of these other versions.

61. The F-statistics for the "economic fundamental" and "speculative" groups (followed in parenthesis by the F-statistic value to be exceeded to indicate significance at the .1% level) were respectively:
   - pound-dollar (COL) 65.5 (2.9) and 44.0 (3.4);
   - pound-dollar (WP) 57.0 (2.9) and 30.1 (3.4);
   - pound-dollar (WG) 52.6 (2.9) and 32.8 (3.4);
   - pound-franc (COL) 271.5 (3.0) and 14.0 (3.0);
   - pound-franc (WP) 186.3 (3.0) and 12.4 (3.0);
   - franc-dollar (COL) 58.2 (2.9) and 26.1 (2.5);
   - franc-dollar (WP) 51.7 (2.9) and 13.3 (2.5).

62. In addition S1 was excluded from the regressions. It represented weakness of the dollar in November, 1931 as attention shifted from the pound to the dollar. Such weakness of the dollar can hardly be expected to show up against the pound which had just depreciated sharply.

63. Hudgins (1973) tried a variety of interest rates and ultimately concluded: "The consistent failure of any interest rate - whether it be a call rate differential, a three month bond differential, or a treasury bill rate - to have a significant effect on the exchange rate indicates that funds did not flow from country to country because of a simple difference in money interest rates". (pp. 222-3).

64. These latter two possibilities are discussed in detail in Ch. 4, Section III.

65. This figure corresponds remarkably well with that of Whitaker and Hudgins (1976) who suggest that "a typical intervention by the EEA altered the exchange rates by between 2 and 8 cents" (p. 1483). It is true that the variable here refers to both British and American...
intervention, but if the conclusion of Hudgin's earlier study (1973, p. 220) is correct then the American fund was ineffective anyway and therefore the figures in Table 5.4 are comparable with those of Whitaker and Hudgins. However, Hudgin's earlier work also suggested a somewhat higher value - in the 4-11 cents range (1973, Tables 4.1 - 4.12) - although the figure in the present study still falls within this range.

66. In fact, this variable was significant and correctly signed in the franc-dollar estimates.

67. An alternative explanation would be that this result is supportive of a monetary approach to exchange rate determination which would predict a positive sign for the relative income variable. See footnote 12 of Ch. 4 for an explanation of why a positive sign should be expected.

68. See, for example. Ozmun (1976) Tables 4.5 and 4.6, p. 95 and p. 98 respectively.

69. See, for example, the Economist, 4/4/36, p. 17 and 30/1/37, p. 243.

70. Ozmun (1976), p. 86.

71. This was not done because it would have involved some clumsy and ill-defined averaging process.

72. It is true that the bi-lateral income ratio carried the wrong sign but it was suggested above that probably only one of the two income proxies was indicating a negative income effect and though they are not tested seperately here, the suspicion, based on Ozmun's study (1976), would be that it was the British not French income proxy that was behaving in this manner (See Ozmun, 1976, Tables 4.1, 4.2, 4.3 and 4.7 on pages 82, 88, 90 and 100 respectively). Therefore, the drop in French income at the turn of 1931 can be legitimately put forward as an alternative (to K1) reason for the strengthening of the franc against the pound in spite of the incorrect sign of the ratio form of the variable. A completely different explanation for the insignificance of K1 is that it is "mistimed". This possibility is taken up in the discussion of the residuals for this period in Section IV below.

73. As already suggested it may also have reflected the influence of K3 which was significant in the pound-dollar estimates but surprisingly not in the pound-franc estimates.

74. Ozmun (1976), pp. 84-5.

75. Another possibility is "mistiming" of S1 which is taken up in the discussion of the residuals below.

76. The coefficients of S4 and S5 would suggest that in April-October, 1933 the dollar was undervalued by 7.6-9.1% against the pound and 11.1-12.1% against the franc and in November, 1933 - December, 1934 by 13.9-16.1% against the pound and 15.3-22.1% against the franc.
77. Admittedly this is less true of F9 (October-December, 1936) which corresponds closely with S7 (October-November, 1936) since the latter was insignificant in the pound-dollar estimates and significant and correctly signed in the franc-dollar estimates (so that the "American effect" is arguably already being picked up). However, there was some ambiguity about the sign of F9 anyway (Table 53, Section II) so it is not entirely implausible for it to carry different signs in estimates for different exchange rates.

78. This is actually quite wide; in most of the equations the lower limit is approximately 1.1 and the upper limit 2.1.

79. \[ SV = \sum (i_F - i_{UK}) - (FR/SP \times 400) \]

where \( i_F \) = annual foreign interest rate on three month prime bills

\( i_{UK} \) = "British"

\( FR \) = three month forward premium (+) or discount (-) in units of foreign currency per pound

\( SP \) = spot exchange rate in units of foreign currency per pound.

80. Although this runs counter to the views of at least one very important contemporary observer - Governor Norman of the Bank of England - who certainly thought that it did. See Howson (1980), p. 39.

81. Thus, for example, political uncertainty in France may, on the one hand, have caused British owners of capital in France to repatriate it whilst having no effect on American owners of capital in France and, on the other, caused French owners of capital in France to shift their funds out of the country mainly to Britain. Therefore the franc might have weakened against the pound but not the dollar and hence, a speculative dummy seeking to pick up such capital movements would be significant in the pound-franc but not the dollar-franc estimates.

82. Some variables excluded from the final equations because of insignificance in the first stage estimates were restated in the CORC estimates. In particular, the pound-franc ID ("wide" version) was included in the pound-franc estimates.

83. This is an iterative process which involves obtaining estimates of the autoregressive parameter, \( \rho \), from the residuals of the original OLSQ estimates, re-estimating the transformed equation

\[ \sum Y_t - \beta Y_{t-1} = \alpha \sum Y_t - \beta Y_{t-1} + \beta \sum X_t - \beta X_{t-1} \]

and then repeating the procedure using the value of \( \beta \) calculated from the residuals of the transformed equation and so on until the value of \( \beta \) converges.

84. Allied to this, is the further problem that as more than one variant of each exchange rate is used, different variants may have different patterns of residuals. Fortunately, this problem does not materialise, particularly in the potentially most likely case (because there are three variants) - the pound-dollar - in which the three sets of residuals are virtually identical.
85. This gold outflow occurred throughout the October-December, 1931 period but, it was argued, was at its heaviest in October, which is why S1 covered that month. (S1 was not used in the pound-dollar estimates as it was not applicable. See footnote 25 above).

86. Economist, 9/7/32, p. 57.

87. Economist, 27/1/34, p. 179. In fact, this leads to the wide version of the pound-franc ID having a value of -1 in January, 1934.

88. It must be conceded that this is not a strong argument. S6 was, in fact "wrongly" signed since the dollar was expected to be strong in early 1935, partly because of the impending gold clause judgement which affected January in particular. The argument that S6 reflected continued undervaluation of the dollar was originally introduced as an explanation of its "wrong" sign.

89. In addition, the franc apparently rallied in July. See footnote 52 (above).

90. See the Economist, 19/9/36, p. 569 and 25/9/36, p. 618.

91. The values of S7 are -0.45 (COL) and -0.66 (WP) and of F9 are -0.69 (COL) and -0.39 (WP). These would suggest the following combined effect:

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<thead>
<tr>
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<tbody>
<tr>
<td>COL</td>
<td>-1.14</td>
<td>-1.14</td>
<td>-0.69</td>
</tr>
<tr>
<td>WP</td>
<td>-1.05</td>
<td>-1.05</td>
<td>-0.39</td>
</tr>
</tbody>
</table>

The residuals are identical (with opposite signs) in October and December in both COL (± 0.29) and WP (± 0.22) versions. This would adjust the above effect to indicate that the pressures causing franc weakness became progressively weaker:

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<tr>
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</tr>
</thead>
<tbody>
<tr>
<td>COL</td>
<td>-1.33</td>
<td>-0.85</td>
<td>-0.69</td>
</tr>
<tr>
<td>WP</td>
<td>-1.28</td>
<td>-0.82</td>
<td>-0.39</td>
</tr>
</tbody>
</table>

92. See the commentary on events in France and potential influences on the franc in Section II of this chapter above.

93. See the Economist, 7/5/38, pp. 302-3, for example.

94. Using R-squareds, Durbin-Watson statistics, t-statistics and the standard errors of the regression as criteria, the regressions involving predicted direct rates were clearly "better" than those using predicted cross-rates.
CHAPTER 6

THE REST OF THE GOLD BLOC: THE BELGA,

THE GUILDER AND THE SWISS FRANC

(I)

Following the abandonment of the gold standard by Britain in 1931, a number of European countries stayed on gold with France and formed what came to be known as the gold bloc. Although in a sense these currencies were "fixed" (to gold) for much of the period, some of them - the "free gold bloc currencies", that is those which did not engage in extensive exchange controls (like Italy) - might provide a further testing ground for the model developed here, to the extent that other currencies, like the pound floated against them and hence the pound-belga exchange rate, for example, could be viewed as "floating" (even though Belgium was on the gold standard). Furthermore, the guilder and the Swiss franc actually did float after the final collapse of the gold bloc in 1936.

Nevertheless, it might still be argued that the course of the belga, guilder and Swiss franc was largely determined by the relationships within the pound-dollar-franc triangle and that the exchange rates of these minor gold bloc currencies against these three major currencies were little more than cross-rates. However, whilst this may be true to a point, it might still be expected that the basic hypotheses of the model should hold in the sense that these currencies' exchange rates could not stray too far away from the path indicated by "economic fundamentals" and the model is therefore applicable in a permissive sense. Thus the relationship between the exchange rates
of the minor gold bloc currencies and the variables in the model is still hypothesised to hold but in a much looser sense although some indication that relative prices and the other "economic fundamentals" were important is still expected. In view of this, however, the depth of study of these currencies will be rather less than for the pound-dollar-franc triangle: only exchange rates between each of the three minor gold bloc currencies with each of the three major currencies will be examined (nine exchange rates in all) and the results will be presented and analysed more briefly; for example, there will be no detailed analysis of the residuals. This is in line with the lesser importance of these currencies and the lesser applicability of the model.

The estimation procedure will, nevertheless, be the same although there is an additional problem in that for each of the three minor gold bloc currency exchange rates considered there will be two potential "third countries" to represent the ROW. It may seem obvious that France, as the leading gold bloc currency, should represent the ROW wherever possible but, not only does this not solve the problem for exchange rates involving the French franc, it is also rather contentious. The importance of the U.K. and U.S. to these countries should not be overlooked both in terms of their positions as major countries in the world economy and also in terms of specific links; for example, Britain was one of Holland's two major trading partners (and the other was Germany not France) and Belgium was technically linked with the U.S. in 1936-8 due to the fact that they were both on the gold standard and indeed, the belga (and also the guilder and the Swiss franc) moved with the dollar in the 1937-8 period on occasion. The solution to the problem adopted here was to use both potential third
countries. For example, in the belga-pound case, two sets of preliminary estimates, one using the U.S. as the ROW and the other France, were estimated and the significant third country variables of both the U.S. and France were included simultaneously to produce only one set of estimates in later stages.

Before examining the economic history of these countries to derive speculative dummies, it might be useful to give a brief overview of the movements of the belga, guilder and Swiss franc in the 1930's. The history of the belga is very straightforward: it remained on the gold standard until the end of March, 1935 when it was devalued by 28% and the gold standard was maintained at this new value for the rest of the 1930's. With this one exception then, exchange rate movements of the belga were dominated by changes in the other currency: thus the belga appreciated unevenly against the pound in the September, 1931 to March, 1935 period, then depreciated sharply to a value approximately 20% higher than its 1929 value where it stayed until appreciating slightly in late 1938; it does much the same against the dollar only the appreciation is in the April, 1933 to March, 1935 period and there is no appreciation in late 1938; finally, the belga hovers around its gold parity with the franc until the sharp devaluation to a new parity in late March, 1935 where it remains until the franc begins its decline in late 1936 which continued until late 1938.

The fluctuations of the guilder against the major currencies were very similar to those of the belga, only the guilder depreciated not in March, 1935 but at the end of September, 1936. There were also two other differences: firstly, the depreciated level of the guilder was 30% (not 20%) above its 1929 value against the pound and the dollar in October, 1936 and, indeed, it actually appreciated by 10% against
the franc; secondly, and more importantly, the guilder did not readopt the gold standard at a lower parity but, according to the Dutch authorities, was floated - "we just want to see how the guilder will manage" — although it is probable that the guilder was temporarily and unofficially pegged to the dollar for part of 1937.

The course of the Swiss franc in 1931-8 was virtually identical to that of the belga with the obvious difference that the Swiss depreciation came in late September, 1936 (like the guilder) although the new level of the Swiss franc was similar to the belga's rather than the guilder's (in terms of its relationship with 1929 values). However, the type of exchange rate regime adopted by Switzerland was unlike that of Belgium or Holland in that the Swiss franc was neither fixed at a new gold parity nor freely floated; instead it was allowed to float but with the intention of trying to maintain its value approximately 10% above that of the pound and the dollar; furthermore, like the guilder (and belga), the Swiss franc moved with the dollar for part of the autumn, 1936 to 1938 period (though certainly not all of the time).

(II)

In spite of the importance of external pressures in determining the exchange rates of the minor gold bloc currencies, it seemed desirable to allow for possible speculative effects by deriving dummy variables in the same way as this had been done for the three major currencies and this exercise is carried out for each country in turn below. It would be useful to begin the discussion of Belgium and the belga with a few general points. In the first place, it should
be noted that there was a strong "psychological" link between the belga and the French franc, even as late as 1938:

"The position of the belga has undoubtedly been affected by the recent depreciation of the French franc....It is true that economic ties linking the two countries are no longer so close...nor are...their political relations. But the psychological link is still there and its power has been clearly demonstrated lately". (3)

This would imply that any weakness in the French franc would be transferred (via speculation) to the belga.

However, this relationship should not be overstressed: in autumn, 1938, the belga (for a time) strengthened with the dollar, even though other European currencies were weakening, because of the indirect link with the dollar due to both currencies being on the gold standard; furthermore, there were those who believed that Belgium's strongest attachment (via trade flows) was with the sterling bloc and that after the March, 1935 devaluation the belga should have been tied to sterling not gold, or even that the belga ought to have followed the pound in 1931. (4) A second major characteristic of Belgium in the 1930's that is particularly relevant here is that, mainly because of its position as an open economy, Belgian protectionism was generally half-hearted with a preference for "a policy of negotiation....to one of direct attack". (5) This would indicate that distortions due to tariffs and other import controls are likely to be less of a problem in the Belgian case. This is not necessarily true of speculation, however, and attention is now turned to deriving dummy variables to allow for this.

The British abandonment of the gold standard in 1931 caused very little alarm in Belgium. Indeed, in February, 1932, a contemporary
source was moved to comment:

"The general trust in the Belgian currency has caused the belga to stand at a premium in the exchange market, even in ratio to the French franc exchange except in September...." (6)

This confidence seems to have continued into the first half of 1932 despite a slight change in the composition of the government in May. However, in July, 1932, there was a miners' strike and even riots when some miners refused to accept wage cuts; the government, with less than a year left to run, was reluctant to deal with this and the further cuts in expenditure needed to balance the budget and eventually chose to resign (in October). Thus July-October, 1932 represents a period of probable belga weakness. On the election of a new government, in November, confidence returned and November-December, 1932 may well have represented a period of belga strength.

In 1933 the budget was brought back into balance (with the government ruling by decree for a period) and:

"The currency was stable throughout the year". (7)

This was undoubtedly due to the internal political stability of Belgium and also the country's general monetary position:

"The monetary situation of the country remains very strong....Belgium is the only country still on the gold standard which has seen its gold reserves increase in the last six months". (8)

No speculative dummies would therefore seem to be required for 1933.

However, the story is rather different in 1934, a year of recurrent speculation about the future of the belga, which became particularly excessive towards the end and carried on into 1935 until
the belga was devalued in late March. The deterioration began in mid-1934 with the fall in gold prices necessitating further deflation (if Belgium were to maintain the gold standard) at the same time as the effects of the U.S. undervaluation were beginning to be felt. At the end of September, the members of the gold bloc met and confirmed their intention to maintain the gold standard; the necessity to do this might have been interpreted as a sign of weakness. A week later the belga developed "sudden weakness as a result of the political crisis at the end of last week"(9) which involved rows over further deflation culminating in the resignation of two ministers. The arguments continued and ultimately the whole cabinet resigned (on November, 13th). To some extent this reflected a growing debate within the country in which those in favour of devaluation were rapidly gaining ground. The effect on the belga was a further decline in confidence:

"Rumours of devaluation and the consequent capital outflow became particularly intense in November, 1934". (10)

Loans were raised in America (in November) and Switzerland and Holland (in December) but the problems continued.

Matters were not helped by the fact that this loss of confidence in the belga coincided with a major banking crisis which, according to one source was ultimately the main cause of the devaluation:

"The need for fundamental reform in Belgian banking had become glaringly apparent in the banking crisis late in 1934 and early in 1935, which more than any other was the factor immediately responsible for the devaluation of the belga...." (11)

Thus the weakness continued into 1935 and, indeed, intensified in February when it became clear that the Socialist Party would strongly
resist further deflation. In March, an attempt to persuade the French to give concessions in a bilateral trade and financial agreement (to help prop up the belga) failed. On March 19th, the government resigned and by March 26th planeloads of Belgium currency were being flown out of the country to be sold for what they would fetch. At the end of the month the newly formed "National Government" devalued the belga (by 28%). This sustained period of pressure on the belga obviously requires some kind of representation in the model and it was decided to use two speculative dummies (to try and take account of the differing degree of intensity of the speculation), BG3 (September, 1934 - January, 1935) and BG4 (February-March, 1935), both of which are expected to be associated with belga weakness.

The next year witnessed a dramatic change in Belgium as prosperity very rapidly returned and no more speculative dummies are required until June, 1936, when a variety of political influences may have led to uncertainty about Belgium and the belga (and hence weakness). Although the election in late May left the three major parties with the majority of the seats, the improved position of the extremist parties - the Communist and a new (fascist) party, the Rexist - gave a certain shock to confidence which was further undermined by the delay and difficulties of forming a new government (it took three weeks) and by an outbreak of strikes in June. A fifth speculative dummy will therefore be included for June, 1936.

The focus of attention in 1936 was, however, very much on the rest of the gold bloc which ultimately collapsed in late September. It seems reasonable, nevertheless, to suppose that there may have been some effect on the belga, probably a weakening, and at least
to include a variable in the October-December period to test for
this. In the first half of 1937 there is no evidence of
speculative effects but in July the belga apparently weakened due
mainly to events in France and the "psychological" link between the
two countries and also further internal political uncertainties (connected with the Nazis).

The real problems for the belga, though, came in the latter
part of the year and continued into 1938. In September there was an
internal political scandal involving the Prime Minister which had an
immediate effect on the belga:

"The crisis in Brussels gave rise to a transfer of funds
to New York". (16)

Nor was the situation helped by the troubles of the French franc.
In late October, the Prime Minister resigned which led to fears of a
more left wing government and hence the belga stayed weak. In fact,
it took four weeks to form a new government and confidence continued
to wane until the end of the year.

In the new year (1938), this decline was fuelled by more trouble
with the miners and a government decision to increase taxation. Matters
took a further turn for the worse in March, initially set off by fresh
weakness in the franc:

"The fall in the French franc has put a severe strain on
the belga. Capital is leaving the country". (17)

Budget difficulties were causing internal political problems and in
late April the Finance Minister resigned, quickly followed by the entire
government (on May 5th). Thus the reasons for the belga weakness in
March-May, 1938 were twofold:
"The unfortunate coincidence of a political crisis with a devaluation of the French franc has provided the required background for this attack on the belga to develop". (18)

Thus there are good reasons to believe that the belga was weakened by speculative forces in the whole of the September, 1937 - May, 1938 period. However, two separate dummies will be used - BG8 (September, 1937 - February, 1938) and BG9 (March-May, 1938) - partly to reflect the probability of differing intensities of the speculation and partly because, in a sense, the March-May, 1938 weakness had different causes. The Belgian speculative dummies are summarized in Table 6.1.

**TABLE 6.1: BELGIAN SPECULATIVE DUMMIES**

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>PERIOD ON</th>
<th>TENDENCY OF BELGA</th>
<th>REASON</th>
<th>INAPPLICABLE TO</th>
</tr>
</thead>
<tbody>
<tr>
<td>BG1</td>
<td>July-Oct. 1932</td>
<td>weak</td>
<td>Strikes, unrest, government resignation.</td>
<td>BG1</td>
</tr>
<tr>
<td>BG2</td>
<td>Nov.-Dec. 1932</td>
<td>strong</td>
<td>New government and return of confidence.</td>
<td>BG2</td>
</tr>
<tr>
<td>BG3</td>
<td>Sept. 1934 - Jan. 1935</td>
<td>weak</td>
<td>Loss of confidence for various reasons (including fall of the government and banking crisis)</td>
<td>BG3</td>
</tr>
<tr>
<td>BG4</td>
<td>Feb.-March 1935</td>
<td>weak</td>
<td>Accelerated loss of confidence (various reasons).</td>
<td>BG4</td>
</tr>
<tr>
<td>BG5</td>
<td>June, 1936</td>
<td>weak</td>
<td>Strikes, difficulties in forming a government (after elections).</td>
<td>BG5</td>
</tr>
<tr>
<td>BG6</td>
<td>Oct.-Dec. 1936</td>
<td>weak</td>
<td>Sympathy with French franc.</td>
<td>BG6</td>
</tr>
<tr>
<td>BG7</td>
<td>July, 1937</td>
<td>weak</td>
<td>Sympathy with French franc.</td>
<td>BG7</td>
</tr>
<tr>
<td>BG8</td>
<td>Sept. 1937 - Feb. 1938</td>
<td>weak</td>
<td>Loss of confidence for various reasons (including an internal political scandal).</td>
<td>BG8</td>
</tr>
<tr>
<td>BG9</td>
<td>Mar.-May 1938</td>
<td>weak</td>
<td>Accelerated loss of confidence (various reasons).</td>
<td>BG9</td>
</tr>
</tbody>
</table>
In many ways Holland had the most depressing experience in the 1930's of the countries in the study and unlike the others (such as Belgium) experienced only a very brief recovery after abandoning its old gold parity in 1936; the Economist's economic reports on Holland in the 1930's carry the heading "Unemployment Rising" with depressing regularity. More specifically there are two characteristics of the Dutch economy which might be highlighted in the present context (before examining events in detail), both of which may act to undermine the model developed here. In the first place, the Dutch supported the gold standard with a fervour that was almost religious, led by the Governor of the Central Bank who vowed, on taking office in October, 1931:

"With all the means within my power I shall maintain the standard of our coinage". (20)

Nor was this feeling in any way diminished by mid-1936:

"Holland may put up a longer resistance to the forces of devaluation. The authorities in that country are probably more rigidly orthodox and deflationist than anywhere else in the world". (21)

This extreme "orthodoxy" may have created pressures that caused the guilder to deviate from the path indicated by "economic fundamentals".

A second distortion may have been created by the strong protectionist policy of Holland, although in fairness this was largely retaliatory. Nevertheless:

"The Government of the Netherlands still pursues its active protectionist policy. The system of quota restrictions on imports is constantly being extended". (22)

"The quota policy of the Dutch government is being pursued unremittingly". (23)
Moreover, the Dutch were not prepared to backtrack on their protectionist policy after the devaluation (as the Belgians had done). Although it is true that Dutch protection was retaliatory and, insofar as it merely offset foreign protection, there could have been a cancelling out leaving the influence of relative prices and other "economic fundamentals" undistorted, this is rather tenuous and the applicability of the model to the guilder may turn out to be limited.

During the 1931-8 period Holland displayed remarkable political stability, much more so than Belgium (or France), being governed by the same right wing coalition and having the same prime minister (except for a short period in 1935). This may have been expected to instil confidence in the country and the currency but, nevertheless, there were still periods during which speculative influences may have been operative and these should be examined. The British abandonment of the gold standard had a minimal effect on the guilder which is not surprising; given the strong Dutch feelings about the gold standard, there was no question of the guilder following the pound and consequently no evidence of speculation to this effect either in late 1931 or throughout 1932. However, the American depreciation in 1933 did cause "fears that the guilder would have to follow the fate of the dollar"(24) and, in particular, the guilder experienced difficulties in May and June:

"In the early summer....Holland....suffered such a panic flight of capital that.../it was/...within measurable distance of having to suspend gold payments". (25)

Indeed, it was the "run on the guilder [that] precipitated a move towards the formal organisation of the 'gold bloc' "(26) in July which
seems to have brought this period of speculation to a halt. The first Dutch dummy, therefore, reflects guilder weakness in May-June, 1933.

The guilder suffered no further setbacks in 1933 but in 1934 there were two periods which may have been associated with guilder weakness. In February-March there was an influx of gold into the U.S. which exerted a deflationary effect on the gold bloc currencies and led to fears about their ability to remain on gold; this may have particularly affected the guilder, given the comparatively greater depth of the depression in Holland, and therefore may require a dummy variable. In July, there were Communist-led riots in Amsterdam and lesser disturbances in three other cities in protest against the policy of deflation and expenditure cuts (necessary to maintain the gold standard); in August a coal strike was narrowly averted and the budget deficit was causing concern; by September it was becoming increasingly clear that further wage cuts were out of the question as prices were actually rising.\(^{(27)}\) However, the gold bloc collaboration agreement at the end of September may have restored some degree of confidence, but a speculative dummy for the whole of the July-September, 1934 period would still seem appropriate.

The year 1935 continued in the same vein with intermittent "runs" on the guilder. The first of these, in early 1935, was related to the gold clause judgement in the U.S. and is therefore picked up by an American dummy (S6) but the others were more specifically related to Holland. The devaluation of the belga in late March, 1935, not surprisingly led to fears about the other members of the gold bloc, including the guilder. In fact Holland lost a great deal of gold in the first fortnight of April as the run on the currency reached alarming
proportions. The problems continued:

"During May the gold bloc currencies were subject to heavy pressure". (28)

The situation eased in June but an even worse crisis developed in July when Dr. Colijn, the Prime Minister, could not raise the support to get his retrenchment bill through Parliament:

"...this spelt the defeat of M. Colijn's deflationary policy, and therefore, in the eyes of the world, the end of the attempt to bolster up the value of the guilder. The exchange crisis followed inevitably". (29)

However, the opposition could not form a government and so within four days (July 30th) Colijn returned to office, and confidence (and gold) returned.

The problem was not so much that Holland lacked the technical position to stay on gold but rather whether the country could take the required deflation. For the time being it seemed that it could:

"At the expense of continued internal economic stagnation, the guilder weathered attacks against it in April and May and again in July, 1935". (30)

Unfortunately, this proved to be only temporary and the return of Parliament from its summer recess in September was awaited with some trepidation and its approach was sufficient to set off a fresh flight from the guilder. The budget deficit that Colijn had been trying to cover with his retrenchment bill had widened and, in addition, the devaluation movement was gaining ground having "derived stimulus from the proximity of Belgium to Holland". (31) It was only at the end of the month, following two statements by Colijn that his government would do everything in its power to maintain the gold standard, that
confidence began to return. Thus three speculative dummies are required for 1935 - April-May, July and September - all representing guilder weakness.

In 1936 the guilder's position remained technically strong but fears about the ability of Holland to tolerate further deflation grew and, in June, difficulties in France spilled over into Holland and there was another run on the guilder:

"The guilder has...been at the very centre of the storm". (32)

More statements of the commitment to the gold standard stemmed the flow but a Dutch speculative dummy for June, 1936, still seems appropriate. Finally, in late September, in the wake of the French and Swiss francs, the guilder succumbed and abandoned the gold standard, entering into its "floating" phase and depreciated. In the October, 1936 to December, 1938 period there was little evidence of any major speculative effect. For much of 1937 the guilder apparently moved in line with the dollar, although not all the time, and this year contains the only significant speculative influence in the late 1936 to end-1938 period: in May there was a large influx of gold which forced the Dutch Equalisation Fund to lower its buying rate for dollars which led to rumours of impending guilder revaluation; this will be represented by the final Dutch speculative dummy (H8), the only one associated with guilder strength. The Dutch dummies are summarized in Table 6.2.

Switzerland was the smallest of the gold bloc countries. It has a number of other characteristics though, relevant here, which it shared with Holland: in the first place, it embraced the gold standard with a similar (if slightly lesser) degree of intensity to that of Holland
TABLE 6.2: DUTCH SPECULATIVE DUMMIES

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>PERIOD ON</th>
<th>TENDENCY OF Guilder</th>
<th>REASON</th>
</tr>
</thead>
<tbody>
<tr>
<td>H1</td>
<td>May-June, 1933</td>
<td>weak</td>
<td>Adverse reaction to U.S. leaving gold.</td>
</tr>
<tr>
<td>H2</td>
<td>Feb.-April 1934</td>
<td>weak</td>
<td>Gold influx into U.S. leading to Dutch deflation.</td>
</tr>
<tr>
<td>H3</td>
<td>July-Sept. 1934</td>
<td>weak</td>
<td>Political disturbances.</td>
</tr>
<tr>
<td>H4</td>
<td>April-May, 1935</td>
<td>weak</td>
<td>Belgia leaving gold.</td>
</tr>
<tr>
<td>H6</td>
<td>Sept. 1935</td>
<td>weak</td>
<td>Budget deficit, general loss of confidence.</td>
</tr>
<tr>
<td>H7</td>
<td>June, 1936</td>
<td>weak</td>
<td>Loss of confidence.</td>
</tr>
<tr>
<td>H8</td>
<td>May, 1937</td>
<td>strong</td>
<td>Gold inflow and rumours of revaluation.</td>
</tr>
</tbody>
</table>

whilst, at the same time, pursuing a (similar) commercial policy which was far reaching but not particularly aggressive; in addition, Switzerland too exhibited great internal political stability and this together with the small size of the country, may diminish the likelihood of there being any internal events which could have been strong enough to exert speculative effects. There are also two aspects of the economy peculiar to Switzerland alone. Firstly, the importance of tourism, particularly the winter season, may give a strong seasonal variation to the Swiss exchange rate (although the number of tourists declined dramatically during the depression). Secondly, Switzerland had a role as a safe haven for funds in the 1930's (as it does now) because of its neutrality and this may distort the hypothesised effect of interest rates in the model; for example, in 1937-8, Switzerland was awash with foreign funds, so much so, that in early 1938 a scheme was devised to discourage further capital inflows by charging a commission on foreign deposits. Turning from general factors to more specific ones, it is
necessary to briefly examine the economic history of Switzerland to search for possible causes of speculative effects (even though, as indicated above, there might be some scepticism about their likely importance).

Britain's abandonment of the gold standard induced a "dumbfounded attitude"(34) in Switzerland and led to rumours about the country's ability to remain on gold but little else because:

"...Switzerland's financial position is stronger now than it has been for some years and deserves the confidence which Swiss and foreign investors have placed in it". (35)

However, the pound's depreciation did help to bring the depression to Switzerland, to which it had been to some extent immune, due to its comparatively low level of industrialisation, and the Swiss found their export and tourist trades increasingly in difficulties. The weakness in the economy ultimately transferred itself to the currency which, towards the end of 1932, was plagued by rumours of impending devaluation:

"The culminating point was reached in the closing weeks of the year, when the fall of the French government and rumours of Swiss banking difficulties caused...the Swiss franc...to fall to...an/appreciable discount...." (36)

Consequently, December, 1932 represents the first Swiss period of speculative influence which is expected to be associated with weakness.

The U.S. decision to leave the gold standard in 1933 created a brief adverse reaction in Switzerland in March but, as with Holland, the real difficulties came in May and June:

"During May and June the conviction gained ground that...Switzerland...would have to abandon gold, and as a result there was a serious flight of funds...." (37)
In July, following the formation of the gold bloc, the Swiss franc recovered but a second speculative dummy for May-June, 1933 seems appropriate. This renewal of confidence appears to have sustained the Swiss franc during the rest of 1933 and throughout 1934; indeed, it seems to have peaked in September-November, 1934 and therefore give cause to include a speculative dummy to take account of expected Swiss franc strength in these months. This was probably initially caused by the gold bloc collaboration agreement in September and the realisation that Switzerland was probably the "strongest" gold bloc member(38) and further fortified by the successful formation of new governments in France and Belgium in November allied with the predominance of pro-gold standard feeling in Switzerland itself:

"The devaluationists seem recently to have lost some ground and the government is stubbornly resolved to maintain the gold standard". (39)

The year 1935 began with a flurry of activity due to the impending gold clause judgement in the U.S. which may have weakened the Swiss franc to some extent but is more properly viewed as an American speculative influence. In any case, much more serious weakness occurred in the spring, initially set off by the defection of the belga from the gold standard in late March. The Swiss franc seemed to be particularly affected:

"There is a general lack of confidence in the gold bloc currencies, which is most marked in Switzerland". (40)

There were a number of reasons for this, most obviously the continued downward trend in the Swiss business situation and also the fact that the Swiss banking situation was known to be weak. (41) It could be argued, of course, that these problems were not unique to Switzerland
but a third element operating at this time certainly was and could be expected to lessen confidence in the currency. This was the referendum on the "Kriseninitiative", a Socialist proposal aiming "to assure to all Swiss citizens a sufficient livelihood", which was to be held on June, 2nd. Exactly how a "sufficient livelihood" was to be assured was not spelt out in detail but it was clear that the means would be inflationary and hence would threaten Switzerland's adherence to the gold standard. In fact, it was the rejection of the "Kriseninitiative" in early June which restored confidence, so much so that:

"By June 1935, the run on the Swiss franc subsided". (43)

Thus a dummy variable for April-May, 1935 seems appropriate.

The Swiss franc then entered into an uneasy period of respite during which any weakness elsewhere in the gold bloc tended to be transmitted to Switzerland. However, the situation did not deteriorate excessively until April, 1936, when many of the various influences that had been undermining the Swiss franc combined to cause sudden weakness, initially set off by events elsewhere in the gold bloc:

"The sudden weakness of the Swiss exchange was due to... a further efflux of capital from Switzerland, a movement prompted by the turn of [political] events in France.... This development happened to coincide....with reports of troop movements on the Austro-German frontier and thus exerted its maximum effect on somewhat frayed financial nerves....Quite apart from these external influences the situation in Switzerland has not been developing at all happily....a precarious budget situation has been thrown completely out of equilibrium by the decision to embark on....an exceptionally expensive programme of rearmament and frontier fortification. Further, the banking position....remains unsatisfactory and....the balance of payments is deteriorating". (44)

Any relief in May was short-lived because the situation flared up once
again in June:

"The...gold bloc exchanges were weak. Forward Swiss francs and guilders went to wide discounts against sterling...." (45)

These two months - April and June - are the only two months in 1936 that would seem to require speculative dummies because the devaluation of the Swiss franc in the autumn was not preceded by a period of panic; indeed the Swiss tended to gain gold rather than lose it in the twelve month period immediately preceding devaluation (admittedly mainly at the expense of France). In fact, the initial Swiss reaction to the French devaluation was a public announcement that the Swiss franc was to stay on the gold standard although this decision had to be reversed within a matter of hours as gold began to leave the country. Nevertheless, the effect of the devaluation on Switzerland was beneficial and fairly dramatic: the upturn had already begun in the spring of 1936 and the depreciation accentuated recovery considerably. The tourist trade improved to the extent of contributing to apparent Swiss franc strength (and the need for a dummy variable) in February, 1937:

"Swiss francs have been firm owing to further repatriation of capital and a successful winter sports season". (46)

For the first time, since the beginning of the depression, income from Swiss invisibles offset the visible trade deficit and this encouraged the inflow of funds even more. Capital continued to flow into Switzerland throughout 1937 to the extent that, by the end of the year, money was so abundant that it was being suggested that steps be taken to reverse the flow and get rid of some of the excessive amount already in Switzerland. In the first two months of 1938 (the
height of the tourist season) the influx became very excessive due mainly to the favourable balance of payments and a scheme was adopted for repelling unwanted capital by charging a commission on foreign deposits. This period - January-February, 1938 - will be represented by the eighth dummy variable, also associated with Swiss franc strength.

In April, however, the flow reversed itself:

"Swiss francs weakened...as a result of repatriation of funds to France". (47)

There were two main reasons for this: firstly, the German annexation of Austria caused much uneasiness about the neutral position of Switzerland; and secondly, there were rumours to the effect that the Swiss franc would be affected by the recent depreciation of the French franc. But, later in the year, the Swiss franc strengthened when it apparently moved with the dollar in the October-November period. These events provide the reasons for the final Swiss speculative dummies covering April and October-November, 1938. The Swiss dummies are summarized in Table 6.3.

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>PERIOD ON</th>
<th>TENDENCY OF FRANC</th>
<th>REASON</th>
<th>INAPPLICABLE TO</th>
</tr>
</thead>
<tbody>
<tr>
<td>SW1</td>
<td>Dec. 1932</td>
<td>weak</td>
<td>Rumours of banking difficulties, fall of French government.</td>
<td></td>
</tr>
<tr>
<td>SW2</td>
<td>May-June, 1933</td>
<td>weak</td>
<td>Adverse reaction to U.S. leaving gold.</td>
<td></td>
</tr>
<tr>
<td>SW3</td>
<td>Sept.-Nov. 1934</td>
<td>strong</td>
<td>Increased confidence (various reasons).</td>
<td></td>
</tr>
<tr>
<td>SW4</td>
<td>Apr.-May, 1935</td>
<td>weak</td>
<td>Belga leaving gold, &quot;Kriseninitiative.&quot;</td>
<td></td>
</tr>
<tr>
<td>SW5</td>
<td>April, 1936</td>
<td>weak</td>
<td>Capital outflow (various reasons).</td>
<td></td>
</tr>
<tr>
<td>SW6</td>
<td>June, 1936</td>
<td>weak</td>
<td>Capital outflow.</td>
<td></td>
</tr>
<tr>
<td>SW7</td>
<td>Feb. 1937</td>
<td>strong</td>
<td>Capital inflow, revival of tourism.</td>
<td></td>
</tr>
<tr>
<td>SW8</td>
<td>Jan.-Feb. 1938</td>
<td>strong</td>
<td>Excessive inflows of Foreign capital.</td>
<td></td>
</tr>
<tr>
<td>SW9</td>
<td>May, 1938</td>
<td>weak</td>
<td>War scare, French franc depreciation.</td>
<td></td>
</tr>
<tr>
<td>SW10</td>
<td>Oct.-Nov. 1938</td>
<td>strong</td>
<td>Sympathy with U.S. dollar</td>
<td></td>
</tr>
</tbody>
</table>

U.S.
There were three exchange rates examined for each of these (minor) gold bloc currencies giving a total of nine in all. These were estimated in the same way as the pound-dollar-franc triangle with two versions of each (WP and COL) and a series of estimates initially including all variables but gradually dropping all insignificant variables until "final equations" were produced. Before considering these, however, the speculative dummy variables should be examined for overlap with British, French and American dummies. There is a substantial amount of this which is not surprising given the relatively large number of dummy variables involved. Even so, it would not be particularly useful to catalogue and discuss every incidence of overlap. In some cases partially overlapping variables reflect different influences, sometimes working in the opposite direction, and it is therefore necessary to include them both and so there is nothing to discuss. Nevertheless, in many cases, overlapping dummies may have offset or reinforced each other but it would be more appropriate (and less tedious) to only discuss those cases where this actually seemed to happen, that is in the examination of the empirical results.

There is, however, one specific type of overlap that should be dealt with at this point: this is where two variables coincide exactly. There are four cases of this: BG2 with F1, BG6 with F9, H4 with F4 and SW4 with F4. Inclusion of both variables when they are perfectly collinear would lead to indeterminacy of the coefficients and so one must be excluded and the one remaining is implicitly testing for significance of both sets of influences. In the second case (BG6/F9)
there is no problem because BG6 represents a possible sympathy weakening of the belga with the franc and is therefore not applicable to the belga-franc exchange rate anyway. In the other three examples, all involving French franc exchange rates, the overlapping French dummies (F1, F4 and F4 respectively) are omitted.

The final equations involving the belga are presented in Tables 6.4A-6.4C. In general, the model seems to have been fairly successful in explaining fluctuations in the belga in the 1930's. There are significant (and correctly signed) relative price and income effects in the estimates of all three exchange rates whilst the $R^2$s are very high and there is no firm evidence of serial correlation (although the Durbin-Watson statistics are in the inconclusive range so that its existence cannot actually be rejected).

Turning to the belga-pound estimates first, not only is there clear evidence that the bi-lateral "economic fundamentals" - relative prices and, to a lesser extent, incomes - were important but there seems also to have been a highly significant ROW "economic" effect.

This includes the French-U.K. interest rate differential whose significance indicates that a rise in U.K. interest rates compared to French rates caused the pound to strengthen not only against the franc but also the belga; this could be evidence of the purported "psychological" link between the franc and the belga. However, the significance and "wrong" sign of the Belgian-U.K. interest rate differential is puzzling. It may indicate that a rise in Belgian interest rates did not attract funds but instead was interpreted as a sign of weakness. It may also be the case that changes in French interest rates were more important than changes in Belgian rates, for Belgian exchange rates; this is supported by the insignificance of the Belgian-U.S. differential and
TABLE 6.4A(1): BELGA-POUND FINAL ESTIMATES

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>COL</th>
<th>WP</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>18.4 (2.75)</td>
<td>48.6 (13.5)</td>
</tr>
<tr>
<td>U.K. prices/Belgian prices</td>
<td>-16.0 (2.22)</td>
<td>-28.5 (12.1)</td>
</tr>
<tr>
<td>U.S. prices</td>
<td>71.5 (10.4)</td>
<td>21.0 (4.18)</td>
</tr>
<tr>
<td>French prices</td>
<td>-13.5 (6.72)</td>
<td>-8.64 (7.51)</td>
</tr>
<tr>
<td>U.K. income/Belgian income(2)</td>
<td>-8.01 (4.89)</td>
<td>-0.83 (0.52)</td>
</tr>
<tr>
<td>U.S. income(3)</td>
<td>-17.3 (6.42)</td>
<td>0.22 (0.06)</td>
</tr>
<tr>
<td>Belgian-U.K. interest rates</td>
<td>0.72 (3.04)</td>
<td>0.45 (2.68)</td>
</tr>
<tr>
<td>French-U.K. interest rates</td>
<td>-0.21 (1.31)</td>
<td>-0.51 (3.71)</td>
</tr>
<tr>
<td>Seasonal Variables: A1</td>
<td>-0.62 (3.51)</td>
<td>-0.89 (5.82)</td>
</tr>
<tr>
<td>Seasonal Variables: B1</td>
<td>0.59 (3.76)</td>
<td>-0.05 (0.29)</td>
</tr>
<tr>
<td>Trend</td>
<td>0.06 (4.01)</td>
<td>0.06 (3.87)</td>
</tr>
<tr>
<td>Speculative dummies: K1</td>
<td>-0.71 (1.02)</td>
<td>-1.26 (2.07)</td>
</tr>
<tr>
<td>Speculative dummies: K3</td>
<td>-2.53 (4.54)</td>
<td>-1.93 (3.93)</td>
</tr>
<tr>
<td>Speculative dummies: BG2</td>
<td>-1.85 (2.82)</td>
<td>-1.31 (2.24)</td>
</tr>
<tr>
<td>Speculative dummies: BG3</td>
<td>-5.13 (11.3)</td>
<td>-2.19 (4.24)</td>
</tr>
<tr>
<td>Speculative dummies: BG4</td>
<td>-6.55 (9.77)</td>
<td>-3.65 (5.49)</td>
</tr>
<tr>
<td>Speculative dummies: BG6</td>
<td>0.49 (0.86)</td>
<td>0.53 (1.01)</td>
</tr>
<tr>
<td>Speculative dummies: BG8</td>
<td>0.60 (1.40)</td>
<td>1.29 (3.35)</td>
</tr>
<tr>
<td>Speculative dummies: BG9</td>
<td>1.59 (2.57)</td>
<td>0.81 (1.43)</td>
</tr>
<tr>
<td>R-SQUARED</td>
<td>0.95</td>
<td>0.96</td>
</tr>
<tr>
<td>DURBIN-WATSON STATISTIC</td>
<td>1.11</td>
<td>1.68</td>
</tr>
<tr>
<td>STANDARD ERROR OF THE REGRESSION</td>
<td>0.803</td>
<td>0.725</td>
</tr>
<tr>
<td>F-STATISTIC</td>
<td>68.9</td>
<td>83.4</td>
</tr>
</tbody>
</table>

Notes:  
1. T-statistics in brackets.  
2. U.K. Belgian employment indices.  
TABLE 6.4B: BELGA-DOLLAR FINAL ESTIMATES

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>COL</th>
<th>WP</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>10.4 (6.39)</td>
<td>11.9 (17.1)</td>
</tr>
<tr>
<td>U.S. prices/Belgian prices</td>
<td>-1.17 (1.28)</td>
<td>-1.65 (3.07)</td>
</tr>
<tr>
<td>U.K. prices</td>
<td>0.66 (0.42)</td>
<td>-1.02 (1.76)</td>
</tr>
<tr>
<td>U.S. income/Belgian income&lt;sup&gt;1&lt;/sup&gt;</td>
<td>-2.75 (5.01)</td>
<td>-2.27 (4.59)</td>
</tr>
<tr>
<td>U.S.-French interest rates</td>
<td>0.09 (3.42)</td>
<td>0.07 (2.59)</td>
</tr>
<tr>
<td>Pound-dollar ID</td>
<td>-0.06 (1.89)</td>
<td>-0.06 (2.21)</td>
</tr>
<tr>
<td>Seasonal variables:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>A1</td>
<td>-0.11 (3.34)</td>
<td>-0.07 (2.22)</td>
</tr>
<tr>
<td>A2</td>
<td>0.10 (2.86)</td>
<td>0.10 (3.10)</td>
</tr>
<tr>
<td>B1</td>
<td>0.12 (3.07)</td>
<td>0.10 (2.73)</td>
</tr>
<tr>
<td>B2</td>
<td>-0.07 (2.21)</td>
<td>-0.06 (2.13)</td>
</tr>
<tr>
<td>Trend</td>
<td>-0.01 (3.29)</td>
<td>-0.01 (3.90)</td>
</tr>
<tr>
<td>Speculative dummies:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>BG3</td>
<td>-0.31 (2.59)</td>
<td>-0.19 (1.59)</td>
</tr>
<tr>
<td>BG4</td>
<td>-0.87 (4.16)</td>
<td>-0.80 (3.98)</td>
</tr>
<tr>
<td>S4</td>
<td>-1.41 (11.9)</td>
<td>-1.33 (15.6)</td>
</tr>
<tr>
<td>S5</td>
<td>-1.77 (17.1)</td>
<td>-1.53 (13.6)</td>
</tr>
<tr>
<td>S6</td>
<td>-0.67 (3.71)</td>
<td>-0.39 (1.92)</td>
</tr>
</tbody>
</table>

| R-SQUARED                                     | 0.96      | 0.97      |
| DURBIN-WATSON STATISTIC                       | 1.95      | 2.01      |
| STANDARD ERROR OF THE REGRESSION               | 0.199     | 0.190     |
| F-STATISTIC                                   | 122.8     | 135.1     |

Notes: 1. U.S. and Belgian employment indices.
### TABLE 6.4C: BELGA-FRANC FINAL ESTIMATES

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>COL</th>
<th>WP</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>58.8</td>
<td>70.2</td>
</tr>
<tr>
<td>French prices/Belgian prices</td>
<td>-36.5</td>
<td>-23.9</td>
</tr>
<tr>
<td>U.K. prices</td>
<td>13.3</td>
<td>-12.6</td>
</tr>
<tr>
<td>French income/Belgian income (1)</td>
<td>-2.46</td>
<td>-5.09</td>
</tr>
<tr>
<td>Belgian-French interest rates</td>
<td>-1.21</td>
<td>-1.02</td>
</tr>
<tr>
<td>Belgian-U.S. interest rates</td>
<td>0.73</td>
<td>0.44</td>
</tr>
<tr>
<td>Seasonal variable: A1</td>
<td>-1.08</td>
<td>0.17</td>
</tr>
<tr>
<td>Trend</td>
<td>-0.11</td>
<td>-0.05</td>
</tr>
<tr>
<td>Speculative dummies: BG1</td>
<td>2.14</td>
<td>1.26</td>
</tr>
<tr>
<td>BG5</td>
<td>-4.10</td>
<td>-3.73</td>
</tr>
<tr>
<td>BG8</td>
<td>-1.15</td>
<td>-0.79</td>
</tr>
<tr>
<td>BG9</td>
<td>-1.54</td>
<td>-1.60</td>
</tr>
<tr>
<td>F4</td>
<td>8.19</td>
<td>6.47</td>
</tr>
<tr>
<td>F5</td>
<td>4.87</td>
<td>2.94</td>
</tr>
<tr>
<td>F6</td>
<td>3.83</td>
<td>2.67</td>
</tr>
<tr>
<td>F7</td>
<td>3.82</td>
<td>3.24</td>
</tr>
<tr>
<td>F8</td>
<td>7.43</td>
<td>6.52</td>
</tr>
<tr>
<td>F9</td>
<td>-0.50</td>
<td>-0.97</td>
</tr>
<tr>
<td>F11</td>
<td>-1.56</td>
<td>-1.77</td>
</tr>
<tr>
<td>F12</td>
<td>-4.12</td>
<td>-3.87</td>
</tr>
<tr>
<td>R-SQUARED</td>
<td>0.98</td>
<td>0.98</td>
</tr>
<tr>
<td>DURBIN-WATSON STATISTIC</td>
<td>1.31</td>
<td>1.34</td>
</tr>
<tr>
<td>STANDARD ERROR OF THE REGRESSION</td>
<td>0.99</td>
<td>1.08</td>
</tr>
<tr>
<td>F-STATISTIC</td>
<td>210.0</td>
<td>175.9</td>
</tr>
</tbody>
</table>

**Notes:** 1. French and Belgian employment indices.
the significance of the French-U.S. differential in the belga-
dollar estimates. No ID's were significant which is perhaps more
likely to reflect the inadequacy of the sources used to derive these
variables (particularly the ones for the minor currencies) rather
than to indicate that intervention was unimportant.

There is also evidence of a clear seasonal effect and a long
run trend for the belga to weaken against the pound. Although
different combinations of seasonal variables were significant in
COL and WP versions, the indicated seasonal effect is very similar
with the belga tending to be weak against the pound in the middle of
the year (March-August) and to be strong at the turn of the year
(September-February). To a large extent this is in line with the
"traditional" seasonal pattern of the pound, more usually associated
with its exchange rate against the dollar, of autumnal weakness and
spring strength. The trend variable may reflect a combination of
increasing war scare on the continent and higher British productivity
growth. At any rate its performance is consistent with that in the
estimates of the pound's exchange rate with the franc and the dollar
(although not with the guilder and the Swiss franc, discussed below).

Two British speculative dummies are significant (and correctly
signed), K1 and K3, as they were in the pound-dollar estimates;
furthermore, their insignificance in the pound-franc equations was
satisfactorily explained in terms of mistiming (K1) and overlap with
a significant French speculative dummy of the same expected sign
(K3 with F3). Five of the nine Belgian dummies were significant
and one of the others, BG6, had been significant and correctly signed
in earlier estimates but became insignificant in the final estimates;
this may be interpreted as a very tentative indication that the final
collapse of the gold bloc (in autumn, 1936) did cause the belga to weaken in sympathy. Two of the significant Belgian dummies are incorrectly signed - BG3 and BG4 - which cover the period immediately preceding the belga devaluation in March, 1935. The belga was under pressure at this time and therefore these variables were postulated to be associated with belga weakness; in retrospect, given that the belga was on the gold standard, they should probably have been expected to carry the "wrong" sign, showing belga strength, due effectively to official intervention to keep the belga on the gold standard during a period when it was under pressure (in part because "economic fundamentals" suggested that it was overvalued). The sign and significance of these two variables is therefore not surprising. The insignificance of BG1, BG5 and BG7, as with all insignificant speculative dummies, either suggests that the influences they represent were unimportant or of very brief duration.

The belga-dollar results in Table 6.4B are rather striking: they give very strong support to the hypothesis that exchange rates were determined primarily by "economic fundamentals". The coefficients of determination and Durbin-Watson statistics are high, there is strong evidence of relative price and relative income effects and only five speculative dummies are significant which are either associated with the period of U.S. dollar undervaluation in 1933-5 (S4, S5 and S6), or with the propping up of the belga in the period immediately before its devaluation in 1935 (BG3 and BG4), discussed above in connection with the "wrong" signs of BG3 and BG4. Thus "economic fundamental" effects predominate and principally bi-lateral ones at that, since only the U.K. WP index is significant of the main third country "economic" variables.
As in the belga-pound estimates the interest rate differential between the other country (the U.S., in this case) and France is significant and signed in such a way as to indicate that a rise in French interest rates caused the belga to strengthen (thereby supporting the idea of a belga-franc "psychological" link). The pound-dollar ID is also significant and, as might be expected, suggests that official support for the dollar against the pound caused it to appreciate against the belga. The significance of these latter two variables allied with the insignificance of their Belgian equivalents may indicate that it is the American "economic fundamentals" that are the most important component of the bi-lateral variables and hence that these dominated the belga-dollar exchange rate; this would seem to be quite plausible.

A comparatively large number of seasonal variables are significant (although they have the same sign and approximately the same values in both COL and WP versions). In fact the seasonal effect that emerges from adding them together is a rather simple one: the belga strengthened in the November to March period and weakened in April-October. This is virtually the opposite to what might be expected given the traditional tendency for the dollar to strengthen against European currencies during the crop-moving season (autumn). Nevertheless, the seasonal effect as described here is a significant one and is also a fairly smooth cycle and, moreover, it should be remembered, that it is virtually identical to, and hence perfectly consistent with, the belga's seasonal variation against the pound which may suggest that the explanation is of Belgian origin. The performance of the trend variable is also surprising: it suggests a tendency for the belga to appreciate against the dollar in the 1930's when a depreciation
(due to increasing war scare) might have been more reasonable. In conclusion, despite the unexpected direction of these effects, the belga-dollar estimates do, on balance provide a large measure of support for the model developed here.

To a large extent, this is also true of the belga-franc estimates. These are slightly different in that they relate to an "internal" gold bloc exchange rate and hence it may be expected that Belgian variables would be quite important. In fact, this turns out to be the case to the extent that these are the only belga exchange rate estimates in which Belgian interest rates are significant and also there are proportionately more Belgian speculative dummies significant than elsewhere. Once again there is strong evidence of a relative price effect and lesser evidence of a relative income effect but ROW effects are of only slight importance (U.K. prices). On the one hand, this is not surprising as this is an "internal" gold bloc exchange rate but, on the other, it is surprising that only one U.S. variable showed up - the Belgian-U.S. interest rate differential - and this is "wrongly" signed. However, whilst the scarcity of U.S. variables in Table 6.4C is perhaps unexpected, the "wrong" sign of the latter interest rate differential is not. This variable was "wrongly" signed in the preliminary belga-dollar estimates (and on one occasion significant at the 20% level) and the Belgian-U.K. differential was significant and "wrongly" signed in the belga-pound estimates where it was interpreted as indicating that a rise in Belgian interest rates was taken as a sign of Belgian weakness in the ROW. However, within the gold bloc itself, interest rate effects may have operated more conventionally as indicated by the correct sign of the highly significant Belgian-French interest rate differential.
There is no significant ID although to the extent that both countries were on the gold standard for much of the time, intervention must have taken place. The negative trend variable suggests a long run tendency for the belga to strengthen against the franc (as well as the dollar) and the single significant seasonal variable (A1) indicates belga strength in September-March and weakness in May-July. This is very similar to the seasonal movements of the belga against the pound and the dollar.

A number of Belgian speculative dummies are significant although two (BG6 and BG7) are inapplicable and three (BG2, BG3 and BG4) are insignificant. The performance of these latter variables is a little surprising: BG2, associated with belga strength, overlaps exactly with F1 (therefore excluded), which is associated with franc weakness, and hence might be expected to be significant for two sets of reasons. However, F1 is insignificant in all French franc estimates (both in this and the preceding chapter) whilst BG2 is insignificant in the belga-dollar though not the belga-pound estimates, and, consequently, it is possibly just a straightforward case of the influences represented by BG2 (and F1) not being important. The failure of BG3 and BG4 is taken up in the discussion of the residuals later on. Of the four significant Belgian dummies, one (BG1) is correctly signed whilst the other three (BG5, BG8 and BG9) are not; these "wrong" signs are almost certainly due to the fact that the franc was also weak at these times, obviously more so than the belga.

Two-thirds of the French speculative dummies are significant, three of which - F9 (significant in earlier estimates), F11 and F12 are correctly signed. The other five are all "wrongly" signed: they are expected to be associated with franc weakness but all appear to
be associated with franc strength against the belga. One of them (F8) was "wrongly" signed in the pound-franc estimates (and insignificant in the dollar-franc); this was interpreted as indicative of official support for the franc, an explanation which can also be used here. Unfortunately, it cannot be used for the other dummies because they were found to be either insignificant (F4) or correctly signed (F5, F6 and F7) in the estimates of the pound-dollar-franc triangle. A plausible alternative may be found in terms of the "psychological" link between the franc and the belga along the lines of "anything that weakened the franc may have weakened the belga even more".

However, there is a more obvious explanation: three of these variables cover periods in 1935 after March (when the belga was devalued) and the other covers the first three months of 1936; it may well be that the period April, 1935 - September, 1936 represented one of belga undervaluation against the franc and that F8 (June-September, 1936) should also be interpreted in this light. An examination of the residuals may be instructive in this connection. In the whole of the April, 1935 - September, 1936 period there are only three months not covered by these significant (and positive) French dummies - December, 1935 and April, May, 1936. If the belga were "undervalued" against the franc in the 1935-6 period then large positive residuals might be expected for these months; this is precisely what happens. Thus, the argument that the "wrongly" signed French dummies indicate belga "undervaluation" in this period begin to look very plausible.

In general the contents of Table 6.4 give strong support to the model developed here. Before examining the guilder and Swiss franc, however, two tasks remain: firstly, the performance of variables appearing in more than one belga exchange rate should be examined to
test for consistency; and secondly, something might be said (very briefly) about the residuals. As far as "economic" variables are concerned, since no interest rate differential or ID appears in more than one equation there is little opportunity for inconsistency; furthermore the belga's seasonal pattern is virtually identical for all three exchange rates - weak in the middle and strong at the turn of the year - although different combinations of seasonal variables are involved in each case.

Only the trend variable appears to exhibit inconsistency with the belga apparently gradually weakening against the pound but strengthening against the dollar and franc. This is not entirely unacceptable but a similar trend of the belga against all other currencies might appear more plausible. The differences can be reconciled in that the pound tended to strengthen over time against both the dollar and the franc and a gradual weakening of the French franc might be expected (in view of its large depreciation in 1936-38) but the tendency for increasing belga strength against the dollar is less easy to explain, although it may simply represent a gradual change in tastes or some omitted variable (such as a gradual, relative increase in Belgian protectionism towards the U.S.) Turning to the speculative dummies there is no inconsistency since they either only appear in one final equation (BG1, BG2, BG5 and BG6) or in none (BG7) or in two but carry the same sign (BG3 and BG4) with the exception of BG8 and BG9 whose differences can be explained: they were associated with belga weakness (as expected) in the belga-pound equations but this was partly a sympathy movement with the franc and hence their association with belga strength against the franc was interpreted as indicating that the franc weakened comparatively more than the belga in these periods.
It is not proposed to examine the residuals in detail but it might be useful to briefly discuss the larger ones. In the belga-pound estimates there are only two periods of large residuals which indicate belga weakness in the first half of 1932 and strength in February-April, 1934. The former group can probably be explained by the tendency of the pound to strengthen in spring, 1932 and the latter possibly by a mistiming of K3 (or perhaps better an extension since it is significant). The majority of the large residuals in the belga-dollar estimates occur in the April, 1933 to April, 1935 period and can therefore mainly be explained in terms of S4, S5 and S6 (which cover this period and are significant) being of constant value and therefore of having to average out the effect of the dollar undervaluation even though it was an uneven affair in practice; this explanation was plausibly used to explain some of the residuals in the pound-dollar-franc estimates.

This type of argument can probably also be used for some of the residuals in the belga-franc estimates. In addition two residuals (December, 1935 and April-May, 1936) have already been accounted for above in terms of a belga undervaluation against the franc in 1935-36. The other residuals of specific interest are those from November, 1934 to March, 1935 which are particularly large in the latter three months and indicate unexplained belga strength. This would suggest that BG4 and possibly BG3 would be significant if they are included in the regression. These two variables were significant and associated with belga strength in the belga-pound and belga-dollar estimates. Strictly speaking, this is the "wrong" sign but a plausible explanation was that there was heavy official intervention taking place at this time to keep the belga on the gold standard. Thus it seems that
BG3 and BG4 been included in the final belga-franc equations they would have performed in a manner consistent with the other belga estimates. (51)

(IV)

On balance, the belga estimates seem to support the model; this is less true of the guilder estimates which are presented in Tables 6.5A - 6.5C. The guilder-dollar and guilder-franc results are reasonably supportive in terms of high R-squareds, significance of "economic fundamentals" and so on but the guilder-pound estimates are very poor: the COL ratio is significant but "wrongly" signed, the relative income variable is significant and "wrongly" signed in both versions and the regressions are serially correlated. These "wrong" signs are especially difficult to interpret; whilst insignificance can be taken as an indication that these variables had no effect, it is extremely difficult to provide precise reasons for actually perverse effects. Usually recourse has to be made to the existence of "distortions". Certainly there are two good candidates for this here: Dutch protectionism may be cited, especially in terms of retaliation against one of her two main trading partners, Britain, and also the intensity of the Dutch desire to remain on the gold standard and the technical ability to do so may have led the Dutch exchange rate to move in the opposite direction to that indicated by "economic fundamentals".

Thus it is possible that perverse effects actually did take place and there is some contemporary evidence to support this contention. The political difficulties of further deflation were becoming apparent
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<td>6.80 (12.5)</td>
<td>5.92 (12.6)</td>
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<td>U.K. income/Dutch income (2)</td>
<td>1.65 (2.12)</td>
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<tr>
<td>U.S. income (3)</td>
<td>2.95 (4.14)</td>
<td>3.37 (3.21)</td>
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<tr>
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<tr>
<td>A1</td>
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<td>-0.07 (1.50)</td>
</tr>
<tr>
<td>B1</td>
<td>0.16 (2.44)</td>
<td>0.14 (2.07)</td>
</tr>
<tr>
<td>Trend</td>
<td>-0.04 (9.96)</td>
<td>-0.03 (7.51)</td>
</tr>
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<td>Speculative dummies:</td>
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<td></td>
</tr>
<tr>
<td>H1</td>
<td>0.55 (2.43)</td>
<td>0.41 (1.89)</td>
</tr>
<tr>
<td>H2</td>
<td>-0.16 (0.78)</td>
<td>-0.37 (2.05)</td>
</tr>
<tr>
<td>H3</td>
<td>-0.32 (1.57)</td>
<td>-0.12 (0.65)</td>
</tr>
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<td>K1</td>
<td>-0.71 (3.52)</td>
<td>-0.52 (2.77)</td>
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<td>K3</td>
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<td>0.89</td>
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<tr>
<td>DURBIN-WATSON STATISTIC</td>
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<td>0.97</td>
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<tr>
<td>STANDARD ERROR OF THE REGRESSION</td>
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<td>0.265</td>
</tr>
<tr>
<td>F-STATISTIC</td>
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<td>47.0</td>
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**Notes:**
1. T-statistics in brackets
2. U.K. and Dutch employment indices.
### TABLE 6.5B: GILDER-DOLLAR FINAL ESTIMATES

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<td>-1.95 (6.43)</td>
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<td>French prices</td>
<td>0.92 (3.70)</td>
<td>0.002 (0.01)</td>
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<td>U.S. income/Dutch income&lt;sup&gt;(1)&lt;/sup&gt;</td>
<td>-0.38 (2.24)</td>
<td>-0.39 (3.07)</td>
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<td>U.K. income&lt;sup&gt;(2)&lt;/sup&gt;</td>
<td>2.09 (2.36)</td>
<td>0.74 (1.14)</td>
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<td>Dutch-U.S. interest rates</td>
<td>-0.05 (3.99)</td>
<td>-0.02 (2.60)</td>
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<td>Seasonal variables: A2</td>
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<td>-0.01 (5.61)</td>
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<td>Speculative dummies: H1</td>
<td>0.17 (2.43)</td>
<td>0.07 (1.19)</td>
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<tr>
<td>H2</td>
<td>-0.09 (1.58)</td>
<td>-0.12 (2.77)</td>
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<tr>
<td>S3</td>
<td>0.11 (2.03)</td>
<td>0.03 (0.69)</td>
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<tr>
<td>S4</td>
<td>-0.38 (6.38)</td>
<td>-0.32 (9.78)</td>
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<tr>
<td>S5</td>
<td>-0.57 (10.6)</td>
<td>-0.42 (15.5)</td>
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<tr>
<td>S6</td>
<td>-0.27 (5.64)</td>
<td>-0.18 (4.63)</td>
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<tr>
<td>S7</td>
<td>0.31 (5.24)</td>
<td>0.29 (6.15)</td>
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<td>S8</td>
<td>0.17 (3.86)</td>
<td>0.10 (2.69)</td>
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<td>S9</td>
<td>-0.03 (0.90)</td>
<td>-0.07 (2.33)</td>
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| R-SQUARED                                     | 0.97       | 0.98       |
| DURBIN-WATSON STATISTIC                       | 1.64       | 1.92       |
| STANDARD ERROR OF THE REGRESSION              | 0.078      | 0.063      |
| F-STATISTIC                                   | 121.3      | 191.5      |

**Notes:**
1. U.S. and Dutch employment indices.
2. U.K. employment index.
**TABLE 6.5C: Guilder-Franc Final Estimates**

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<td>French prices/Dutch prices</td>
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<td>U.K. prices</td>
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<td>-4.97 (5.04)</td>
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<td>0.15 (0.18)</td>
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<td>0.32 (0.98)</td>
<td>1.50 (4.56)</td>
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<td>U.K. income (2)</td>
<td>1.65 (1.05)</td>
<td>3.97 (2.05)</td>
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<tr>
<td>Dutch-French interest rates</td>
<td>-0.12 (5.40)</td>
<td>-0.13 (6.03)</td>
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<tr>
<td>Dutch-U.S. interest rates</td>
<td>0.09 (2.98)</td>
<td>0.12 (4.40)</td>
</tr>
<tr>
<td>Seasonal variables: A2</td>
<td>0.03 (1.19)</td>
<td>0.05 (2.29)</td>
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<td>-0.09 (2.58)</td>
<td>0.04 (1.21)</td>
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<td>-0.04 (9.54)</td>
<td>-0.05 (11.4)</td>
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<td>Speculative dummies: H3</td>
<td>0.16 (1.77)</td>
<td>0.09 (0.96)</td>
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<td></td>
<td>-0.40 (2.41)</td>
<td>-0.53 (3.14)</td>
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<td>-0.05 (0.49)</td>
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<td></td>
<td>0.20 (2.07)</td>
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<td></td>
<td>0.99 (10.1)</td>
<td>0.92 (9.20)</td>
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<tr>
<td></td>
<td>0.60 (6.39)</td>
<td>0.76 (8.07)</td>
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<td>0.93 (8.83)</td>
<td>1.21 (11.0)</td>
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<tr>
<td></td>
<td>0.90 (9.46)</td>
<td>0.98 (8.62)</td>
</tr>
<tr>
<td></td>
<td>-0.28 (2.65)</td>
<td>-0.06 (0.52)</td>
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</tbody>
</table>

| R-SQUARED                                              | 0.99    | 0.99    |
| DURBIN-WATSON STATISTIC                                | 1.08    | 1.37    |
| STANDARD ERROR OF THE REGRESSION                       | 0.132   | 0.132   |
| F-STATISTIC                                           | 704.2   | 696.3   |

**Notes:**

1. French production index and Dutch employment index.
2. U.K. employment index.
in Holland in autumn, 1934, so much so that prices were reported as rising (52) even though the guilder was actually appreciating against the pound at this time. Furthermore, trade flows between Britain and Holland in this period apparently moved in a somewhat perverse manner and in the opposite direction to that postulated in chapter four. Given relatively lower prices and income in Holland than in Britain, Dutch exports might be expected to hold up rather better than imports. In fact the opposite seems to have been the case: by 1934 Dutch imports had decreased to 62.7% of their 1931 value but exports had declined to an even lower level (42.4%); moreover, the ratio of imports to exports had also risen from 48% in 1931 to 71% in 1932, 83% in 1933 and 70% in 1934 (53).

To summarize then, there is some evidence to suggest that the model is not applicable to the guilder-pound exchange rate and that "perverse" effects are taking place. On the other hand, of course, these estimates are serially correlated which may indicate that the price and income ratios' significance is apparent rather than real. However, a re-estimation of the guilder-pound final equations using the Cochrane-Orcutt technique probably removed the serial correlation and left these variables significant although at lower levels. Finally, it is possible that the guilder-pound exchange rate was, in a sense, a "residual" or "cross" rate with the guilder's level being decided in other exchange rate markets and the pound-guilder rate simply the consequence of this, kept at its "artificial" level by protection and a strong affinity for the gold standard (which perhaps amounts to the same thing as saying that the model does not "work" because of distortions).

The performances of the other variables in the guilder-pound final
equations are more easily explained. There is some indication of a significant ROW (particularly American) effect, a seasonal effect that indicates guilder strength in November-March and weakness in May-September which is similar to that of the belga against the pound and is approximately in line with the pound's "traditional" seasonal pattern and the negative trend would indicate a tendency for the guilder to appreciate against the pound. Two British speculative dummies are significant and correctly signed and three Dutch dummies are significant, two of which are incorrectly signed: H2 coincides with a set of large residuals in the belga-pound estimates which were interpreted as indicative of a mistiming (or rather extension of) K3 and a similar interpretation seems reasonable here to explain H2's "wrong" sign; H3 may be related to the perversely signed economic fundamentals since it overlaps with the period (autumn, 1934) in which Dutch prices were reported as rising which should have caused the guilder to weaken when in fact it strengthened.

In sharp contrast to those for the guilder-pound, the guilder-dollar estimates are highly supportive of the model and indeed are reminiscent of the belga-dollar estimates (which may indicate that the major exchange rate for the minor gold bloc currencies was against the dollar and their other exchange rates were to some extent pre-determined by this). There is clear evidence of the expected relative price and relative income effects and the bi-lateral (U.S.-Dutch) interest rate differential is significant and correctly signed; in addition, there is limited evidence of a ROW effect, a surprisingly volatile seasonal effect - guilder weakness in March-April and September-October, guilder strength in December-January and June-July - and, as in the guilder-pound estimates, a negative trend.
A number of speculative dummies are also significant, two of which are Dutch (H1 and H2) and were significant in the guilder-pound equations, and hence can presumably be interpreted as they were then, and seven of which are American. Three of these (S4, S5 and S6) represent the period of dollar undervaluation in 1933-55 and S3 is "wrongly" signed, as it was in the pound-dollar estimates, where a plausible explanation was provided. The others - S7, S8 and S9 - are all correctly signed as they were in the pound-dollar-franc estimates. In conclusion, the model developed here would seem to satisfactorily "explain" variations in the guilder-dollar exchange rate and the hypotheses embodied within it receive substantial support.

This is broadly true of the guilder-franc estimates also. There is some suspicion of serial correlation but re-estimates of the final equations using the Cochrane-Occtt technique substantially raised the Durbin-Watson statistic without changing the signs or significance of most of the variables to any great extent. Of the major "economic fundamentals" two - relative prices and the Dutch-French interest rate differential - are significant and correctly signed but there were problems with the relative income variable. The original version of this, involving Dutch and French employment indices, was insignificant and the variable in Table 6.5C is based on Dutch employment and French production indices. A similar problem was encountered with the franc-pound estimates (and will be met in the Swiss franc-French franc estimates below) where an explanation was sought in terms of distortions due to trade controls and government intervention. In the guilder estimates this "wrong" sign is not entirely surprising and a similar explanation to that given of the "wrong" signs of both price and income ratios in the guilder-pound estimates seems appropriate: distortions have
been created by Dutch protectionism and the strong affinity to the gold standard.

A number of ROW "economic" variables are significant in Table 6.5C. A problem arises with the Dutch-U.S. interest rate differential whose sign is inconsistent with that in the guilder-dollar estimates; the suggestion is that a rise in Dutch interest rates relative to U.S. rates caused the guilder to strengthen against the dollar but weaken against the franc. An explanation could be sought in terms of rises in Dutch interest rates being interpreted as strength in the U.S. and weakness in France but this is not tenable because the Dutch-French differential is significant and negatively signed. The performance of the seasonal variables is equally unsatisfactory; some of them do show up but there are differences between the COL and WP equations and, in fact, this exchange rate is one of the few for which no clear seasonal pattern emerges. The negative trend variable indicating gradual guilder appreciation is not surprising given the tendency for the franc to depreciate from 1936 onwards.

Finally, several speculative dummies are significant although, with the exception of H3, F5 and F12, they are "wrongly" signed. The only Dutch dummy in this category (H7), expected to be associated with Dutch weakness, can probably be explained in terms of it overlapping with a French dummy representing French weakness, and consequently its "wrong" sign indicates that the French franc was even weaker. The "wrong" sign of F7 can also be plausibly explained: the (franc) strength it represents may have been due to official intervention to maintain the guilder-franc relationship, bearing in mind that both countries were on the gold standard.
However, the fact that F8, F9, F10 and F11 were associated with franc strength is difficult to interpret. The same difficulty was experienced in the pound-franc estimates and one possibility outlined there was that these variables coincided with the "Blum experiment" which, to the extent that it may have succeeded and improved economic conditions in France, could have led to an increase in confidence and hence franc strength. But this point of view appears an unlikely one to have been adopted by a country as steeped in gold standard orthodoxy as Holland. Fortunately an alternative explanation is available: the dummy preceding the September, 1936 devaluation (F8) can be interpreted as indicative of the presence of official intervention whilst the dummies after the devaluation (F9, F10, F11) could be due to the guilder depreciating by more than the franc, making the latter appear temporarily strong against the former. In fact, this turns out not to be the case but an equally valid argument would be that, even so, the franc had not depreciated enough (as is shown by the fact that it was to depreciate a great deal more in subsequent months) and hence, in a sense, was "overvalued" against the guilder in this period until it depreciated again in June, 1937, the end of the period covered by these "wrongly" signed dummies.

This completes the examination of individual guilder exchange rates. A brief look at the performance of variables appearing in estimates of more than one of them reveals one apparent inconsistency in the Dutch speculative dummies; this is provided by H3, apparently associated with guilder weakness against the franc (as expected) but strength against the pound. It may be that reactions on the continent to political disturbances (which is what H3 represents) were stronger than in Britain. On the other hand, H3 is only significant in one of
the two guilder-pound equations (and then only at the 20% level) and these estimates are unreliable because of serial correlation; consequently, the "wrong" sign and significance of H3 in the guilder-
pound estimates is perhaps best ignored.

As far as the other variables are concerned, an inconsistency of the performance of the Dutch-U.S. interest rate differential in the guilder-dollar and guilder-franc estimates has already been mentioned and there is no obvious explanation for this. The most likely one would seem to be that the choice of interest rate is inappropriate and this is taken up again when a similar difficulty arises in the Swiss estimates. Furthermore, there is no common seasonal pattern as occurred in the belga estimates although this may very well be less indicative of any inconsistency than of simply suggesting that no particular seasonal pattern exists which is not entirely surprising since none is expected. The trend variable is significant and suggests gradual guilder appreciation against all three major currencies and only one ID is ever significant.

Finally, a brief look at the large residuals for the guilder-
dollar and guilder-franc (but not the guilder-pound) estimates may be useful. In the former case the majority of the large residuals (and there were not that many) occur, as in the belga-dollar results, in the 1933-35 dollar "undervaluation period" and are therefore almost certainly caused by the averaging process of S4, S5 and S6 in their (unsuccessful) attempt to completely pick up an uneven influence. This averaging problem is also apparent in the guilder-franc residuals. There is, in addition, a strong case for explaining some of the residuals of the guilder estimates in terms of Dutch protectionism and strong affinity for the gold standard.
In conclusion, the application of the model to the guilder is not entirely successful. In particular, the guilder-pound exchange rate does not seem to have been determined by "economic fundamentals" or, to the extent that it was, these tended to behave in a way which is lacking in theoretical foundation. On the other hand, this may be explained in terms of the distortions created by Dutch retaliatory protection against British goods and the strong Dutch desire to remain on the gold standard. Certainly, the fluctuations of the guilder against the franc and, in particular, the dollar seem to be reasonably well explained by the model. In addition, the failure of the Dutch speculative dummies to show up very well is supportive (in a negative sort of way) although it may equally well suggest that events in a small country like Holland exerted little speculative influence on its exchange rate.

The final equations of the three Swiss franc exchange rates are given in Tables 6.6A-6.6C. The performance of the model is rather better than it was for the guilder, if not quite as satisfactory as for the belga. In the Swiss franc-pound estimates the relative price variable is correctly signed (although insignificant in the COL version) but the problem of a "wrongly" signed income ratio is once again apparent. This seems to be a recurring anomaly and, in the absence of any theoretical explanation (if the monetary approach is discarded), it may well be the case that the choice of income proxy is not a particularly good one; unfortunately, in the Swiss case, there is only one proxy available (unemployment data) and consequently alternatives cannot be tested. However, the Swiss-U.K. interest rate differential is correctly signed and significant, although the U.K.-U.S. differential is perversely signed, indicating that a rise
### Table 6.6A(1): Swiss Franc-Pound Final Estimates

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<td>U.K. prices/Swiss prices</td>
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<td>French prices</td>
<td>12.9 (4.28)</td>
<td>11.3 (4.66)</td>
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<tr>
<td>U.K. income/Swiss income (2)</td>
<td>20.2 (5.05)</td>
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<tr>
<td>French income</td>
<td>-9.75 (1.19)</td>
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<td>U.K.-U.S. interest rates</td>
<td>1.56 (4.53)</td>
<td>1.19 (3.56)</td>
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<td>Swiss-U.K. interest rates</td>
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<td>Swiss franc-pound ID</td>
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<td>R-SQUARED</td>
<td>0.92</td>
<td>0.93</td>
</tr>
<tr>
<td>DURBIN-WATSON STATISTIC</td>
<td>1.46</td>
<td>1.49</td>
</tr>
<tr>
<td>STANDARD ERROR OF THE REGRESSION</td>
<td>0.827</td>
<td>0.751</td>
</tr>
<tr>
<td>F-STATISTIC</td>
<td>43.9</td>
<td>54.9</td>
</tr>
</tbody>
</table>

Notes:
1. T-statistics in brackets.
2. U.K. employment index and Swiss unemployment index.
3. French employment index.
### TABLE 6.6B: SWISS FRANC-DOLLAR FINAL ESTIMATES

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>COL</th>
<th>WP</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>2.24 (0.89)</td>
<td>6.82 (7.32)</td>
</tr>
<tr>
<td>U.S. prices/Swiss prices</td>
<td>-4.27 (1.97)</td>
<td>-3.66 (5.59)</td>
</tr>
<tr>
<td>U.K. prices</td>
<td>3.67 (1.68)</td>
<td>2.41 (2.27)</td>
</tr>
<tr>
<td>French prices</td>
<td>4.36 (6.52)</td>
<td>1.65 (2.80)</td>
</tr>
<tr>
<td>U.S. income/Swiss income (1)</td>
<td>-0.24 (0.51)</td>
<td>-1.06 (2.33)</td>
</tr>
<tr>
<td>U.K.-U.S. interest rates</td>
<td>0.09 (1.71)</td>
<td>0.07 (2.03)</td>
</tr>
<tr>
<td>Pound-dollar ID</td>
<td>-0.04 (1.10)</td>
<td>-0.04 (1.60)</td>
</tr>
<tr>
<td>Seasonal variables: B2</td>
<td>-0.07 (2.17)</td>
<td>-0.04 (1.42)</td>
</tr>
<tr>
<td>Trend</td>
<td>-0.01 (2.86)</td>
<td>-0.01 (4.09)</td>
</tr>
<tr>
<td>Speculative dummies: SW3</td>
<td>0.20 (1.42)</td>
<td>0.22 (2.03)</td>
</tr>
<tr>
<td>S3</td>
<td>0.22 (1.76)</td>
<td>0.18 (1.85)</td>
</tr>
<tr>
<td>S4</td>
<td>-0.58 (4.67)</td>
<td>-0.63 (8.11)</td>
</tr>
<tr>
<td>S5</td>
<td>-0.93 (10.8)</td>
<td>-0.69 (10.3)</td>
</tr>
<tr>
<td>S6</td>
<td>-0.23 (1.97)</td>
<td>-0.01 (0.06)</td>
</tr>
<tr>
<td>S7</td>
<td>0.88 (5.73)</td>
<td>0.77 (5.99)</td>
</tr>
<tr>
<td>S8</td>
<td>0.42 (3.48)</td>
<td>0.23 (2.38)</td>
</tr>
<tr>
<td>S9</td>
<td>-0.45 (3.34)</td>
<td>-0.19 (1.77)</td>
</tr>
<tr>
<td>S10</td>
<td>-0.56 (3.13)</td>
<td>0.18 (0.11)</td>
</tr>
<tr>
<td>R-SQUARED</td>
<td>0.96</td>
<td>0.97</td>
</tr>
<tr>
<td>DURBIN-WATSON STATISTIC</td>
<td>1.45</td>
<td>1.71</td>
</tr>
<tr>
<td>STANDARD ERROR OR THE REGRESSION</td>
<td>0.191</td>
<td>0.155</td>
</tr>
<tr>
<td>F-STATISTIC</td>
<td>89.5</td>
<td>139.2</td>
</tr>
</tbody>
</table>

Notes: 1. U.S. employment index and Swiss unemployment index.
### TABLE 6.6C: SWISS FRANC - FRENCH FRANC FINAL ESTIMATES

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>COL</th>
<th>WP</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>23.5 (20.3)</td>
<td>21.7 (17.2)</td>
</tr>
<tr>
<td>French prices/Swiss prices</td>
<td>-2.96 (1.24)</td>
<td>-7.03 (4.78)</td>
</tr>
<tr>
<td>U.S. prices</td>
<td>-9.97 (4.24)</td>
<td>-3.56 (2.45)</td>
</tr>
<tr>
<td>French income/Swiss income (1)</td>
<td>7.76 (7.07)</td>
<td>6.25 (6.08)</td>
</tr>
<tr>
<td>U.S. income (2)</td>
<td>4.02 (7.08)</td>
<td>5.36 (8.93)</td>
</tr>
<tr>
<td>Swiss-U.K. interest rates</td>
<td>0.25 (4.93)</td>
<td>0.35 (8.58)</td>
</tr>
<tr>
<td>Pound-franc ID</td>
<td>-0.08 (2.08)</td>
<td>-0.10 (2.62)</td>
</tr>
<tr>
<td>Seasonal variable: A2</td>
<td>-0.06 (1.54)</td>
<td>-0.03 (0.84)</td>
</tr>
<tr>
<td>Trend</td>
<td>-0.07 (18.8)</td>
<td>-0.07 (20.5)</td>
</tr>
<tr>
<td>Speculative dummies: SW2</td>
<td>0.38 (2.12)</td>
<td>0.11 (0.64)</td>
</tr>
<tr>
<td>SW10</td>
<td>-0.21 (1.13)</td>
<td>-0.40 (2.14)</td>
</tr>
<tr>
<td>F3</td>
<td>0.42 (3.52)</td>
<td>0.37 (3.33)</td>
</tr>
<tr>
<td>F5</td>
<td>-0.39 (2.81)</td>
<td>-0.54 (3.91)</td>
</tr>
<tr>
<td>F6</td>
<td>-0.38 (1.50)</td>
<td>-0.70 (2.83)</td>
</tr>
<tr>
<td>F7</td>
<td>-0.24 (1.40)</td>
<td>-0.17 (1.08)</td>
</tr>
<tr>
<td>F8</td>
<td>0.72 (4.94)</td>
<td>0.90 (6.74)</td>
</tr>
<tr>
<td>F9</td>
<td>1.93 (12.6)</td>
<td>2.05 (14.0)</td>
</tr>
<tr>
<td>F10</td>
<td>2.79 (15.0)</td>
<td>3.03 (16.8)</td>
</tr>
<tr>
<td>F11</td>
<td>2.90 (20.4)</td>
<td>2.70 (17.6)</td>
</tr>
</tbody>
</table>

| R-SQUARED                                      | 0.99       | 0.99       |
| DURBIN-WATSON STATISTIC                       | 1.81       | 1.88       |
| STANDARD ERROR OF THE REGRESSION               | 0.232      | 0.222      |
| F-STATISTIC                                   | 712.1      | 778.7      |

**Notes:**
1. French employment index and Swiss unemployment index.
2. U.S. employment index.
in U.K. interest rates caused a depreciation in the pound. This may also be due to inadequacy of data and, indeed, the choice of the U.S. interest rate has already been criticised in the preceding chapter. Two other ROW "economic fundamentals" are also significant.

The Swiss franc-pound ID is significant but "wrongly" signed although whether this can be taken as showing that intervention was unsuccessful in this exchange rate, in that it was not sufficiently strong to overcome the factors that it was trying to offset, is perhaps rather dubious. The variable only actually takes on a value in three months and hence it is almost certainly not comprehensive enough to adequately test for the presence of official intervention. A clear seasonal pattern emerges with the Swiss franc strong against the pound in December-April and weak in May-November. This is (almost) in line with the "traditional" seasonal pattern of the pound and is similar to that of the belga and the guilder against the pound; furthermore, the Swiss franc strengthened during the winter tourist season (in the first quarter) which is precisely what is expected.

The majority of the British speculative dummies are significant and correctly signed. Half of the Swiss dummies are significant but three are "wrongly" signed. In the cases of SW5 and SW6 this might be explained by the fact that they cover two months in the six month period preceding the Swiss franc depreciation in 1936 and, as the franc was still on the gold standard, they may be picking up official intervention which was necessary to keep it there. This interpretation is borne out by an examination of the residuals: in the four of the six months immediately preceding depreciation not covered by dummy variables there are large negative residuals indicating that the Swiss franc was stronger than predicted by the model. The same
explanation in reverse can be used for the "wrong" sign of SW7: in the six months after leaving the gold standard the Swiss franc was "undervalued" as is indicated by large positive residuals in these months except for February, 1937, where the effect is picked up by SW7.

The performance of the model in "explaining" the Swiss franc-pound exchange rate is only adequate but for the Swiss franc-dollar it is extremely good. There are significant and correctly signed relative price and income effects and an important ROW effect. Furthermore the two "third country" variables that could be expected to have a given sign if their influence is to be plausible, do indeed behave as expected: the U.K.-U.S. interest rate differential indicates that an increase in the U.S. rate compared to the U.K. rate caused the dollar to appreciate against the Swiss franc and the pound-dollar ID suggests that official support for the dollar against the pound also caused it to appreciate against the Swiss franc. There is a significant seasonal variable which suggests a seasonal variation not dissimilar to that of the guilder-dollar exchange rate but the fact that it involves two periods of strength and two of weakness make it a rather unlikely seasonal pattern; however, as against the pound, the Swiss franc does strengthen during the winter tourist season (January-March). The negative trend is consistent with the other Swiss exchange rates examined.

A number of (mainly American) speculative dummies are significant. The single Swiss dummy is "wrongly" and, perhaps more important, inconsistently signed, given its performance in Swiss franc-pound estimates. However, an obvious explanation is at hand: SW3 covers a sub-period of (the significant) S5 and its effect is therefore clearly
distorted due to overlapping of dummy variables. With one exception, the U.S. dummies are correctly signed or where they are not (S3) are also "wrongly" signed in other dollar exchange rates and this has been plausibly explained in the preceding chapter. The exception is S10 which represents dollar strength in late 1938. In retrospect, its inclusion was probably a mistake since not only was the Swiss franc reported to have moved in sympathy but a dummy variable included to capture this effect (and specifically excluded from the Swiss franc-dollar estimates), SW10, was significant and correctly signed for the other two Swiss exchange rates. S10's incorrect sign is therefore not surprising and, as it is only significant in the COL equation, probably not important.

As with the guilder, the degree of support given to the model by the Swiss franc-French franc results lies somewhere in between that provided by the Swiss franc-pound and Swiss franc-dollar estimates. There is evidence of a relative price effect (58) but the relative income variable is significant and "wrongly" signed. This appears to be a recurring problem as far as French "income" is concerned and it was suggested above that it may indicate more about the choice of income proxy than about any relative income effect. There is a ROW effect but once again the problem of inconsistent interest rate differentials emerges. In this case it is the Swiss-U.K. differential which, having indicated that a (relative) increase in the Swiss rate caused the Swiss franc to appreciate against the pound, suggests here that the same change led to a depreciation against the franc. It may be that rises in Swiss interest rates were treated as a sign of weakness within the gold bloc and led to a decline in confidence but this is tenuous and a more plausible explanation may be an inappropriate choice of
interest rate. Another significant ROW variable is the pound-franc ID which is plausibly signed: official support for the franc against the pound apparently caused it to strengthen against the Swiss franc.\(^{(59)}\) There is very limited evidence of a rather strange (double peaked) seasonal effect which is probably best interpreted as indicative of no clear seasonal effect. The trend variable is once again negative and significant.

As with the Swiss franc-dollar estimates, the speculative dummies are mainly non-Swiss. Four of these, and the two significant Swiss dummies, are correctly signed but the other four — F8, F9, F10 and F11 — are not; they are unexpectedly associated with French franc strength. The same problem was encountered in the guilder-French franc estimates and the same explanation seems appropriate: F8 represents official support for the French franc and the other three suggest that in the period immediately following September, 1936 the franc was "overvalued" (and this was not corrected until 1937-8).

On balance, the Swiss franc seems to have fluctuated more or less in accordance with changes in "economic fundamentals" and a fair amount of support for the model is provided. Nevertheless, speculative effects are significant and although the contention that Switzerland, as a small country, would find that internal events which may have caused "speculation" would tend to be unimportant is borne out in the Swiss franc-dollar and Swiss franc-French franc estimates, the relatively large number of significant Swiss speculative dummies in the Swiss franc-pound estimates is rather surprising.

There is very little inconsistency of performance of any of the variables in different exchange rate estimates: there is evidence of
the expected seasonal pattern of the Swiss franc—strength in
the first quarter during the height of the winter tourist season—in two of the exchange rates, the trend was significant and
negative for all three and only one Swiss speculative dummy displayed
different signs for different exchange rates; this was SW3 and its
"wrong" sign was explained in terms of overlapping with another
speculative dummy (S5). Admittedly, there were problems with
interest rate variables, particularly the Swiss-U.K. differential,
but this has been encountered elsewhere and is perhaps best explained
in terms of the difficulties involved in selecting appropriate interest
rates and, indeed, by the fact that interest rate effects may have
been swamped and distorted by speculative effects. (60)

Finally, there were several clusters of large residuals in
each of the three Swiss exchange rates. In the Swiss franc-pound
estimates the majority of the residuals occur in the period immediately
preceding the abandonment of the gold standard in September, 1936 (and
have already been discussed above in the context of the performance
of the Swiss speculative dummies) or can be explained in terms of
mistiming of Swiss dummies, especially SW4 and SW8 (both of which are
insignificant). Obvious explanations of the residuals, with one
exception, also come to mind in the Swiss franc-dollar estimates.
These include the problem of averaging speculative dummies in the 1933-35
dollar undervaluation period (which seems to have affected most dollar
exchange rates), mistiming, or rather an extension of, S8 and, once
again, the fact that many of the residuals occurred in the periods
before and after the gold bloc collapse in September, 1936. This
factor can also be used to explain many of the residuals in the Swiss
franc-French franc estimates along with mistiming of some of the
speculative dummies (F2, SW1, SW4 and SW8), although it is not easy to rationally explain some of the others.

Before drawing some preliminary conclusions, reference should be made to an attempt to model speculative effects by use of a "speculative variable" (SV) based on forward exchange rate data instead of dummy variables. This could only be done for the three exchange rates involving the pound because of data limitations. The performance of an equivalent variable in the pound-dollar and pound-franc estimates led to pessimistic expectations about this exercise which turned out to be well founded: although the SV was significant with the correct (negative) sign in the belga-pound estimates, it was insignificant for the Swiss franc-pound and only significant in one out of the four guilder-pound equations, where it was "wrongly" signed. In view of this rather limited success, and in order to make the gold bloc results comparable to those for the pound-dollar-franc triangle, the use of "speculative variables" was not pursued.

In conclusion, the application of the model to the exchange rates of the minor gold bloc countries was undertaken with a certain amount of trepidation as these were clearly not freely floating exchange rates although all nine exchange rates were involved in a period of managed floating during part, and in some case most, of the 1930's. However, it was argued that the model should hold in a "permissive" sense to the extent that even these exchange rates could still not diverge too far from the path indicated by "economic fundamentals" if indeed "economic fundamentals" did determine exchange rates in this period.
In fact, this view turns out to be broadly supported by the results. Only in the guilder-pound estimates was there no evidence of a (correctly signed) relative price effect and for seven of the nine exchange rates the relative price variable was significant (or in two cases almost significant) at the 20 per cent level or higher in both COL and WP versions. The relative income variable was less successful although it was always significant and correctly signed in exchange rates involving either the belga or the dollar. Moreover, the incorrect sign and significance in the Dutch and Swiss exchange rates against the pound and French franc may raise more questions about the choice of income proxy rather than the direction of the relative income effect. The bi-lateral interest rate differentials also performed tolerably well: they were correctly signed and significant in half of the estimates and tended to be insignificant rather than significant and "wrongly" signed in the rest. However, there were some problems with the consistency of performance of third country interest rate differentials but these were put down either to plausible perverse effects - a rise in interest rates leading to a decrease in confidence rather than attracting funds - and the problems involved in choosing suitable interest rates. More generally, ROW effects did seem to play a part in determining these exchange rates as was expected.

One group of variables that did not show up very well was the gold bloc official intervention dummies. Only the Swiss variable was ever significant (in the Swiss franc-pound estimates) and then it was wrongly signed. To interpret the failure of these variables as suggesting that official intervention was ineffective is, however, rather dangerous because they were constructed on the basis of very
limited and probably inadequate data\(^{(61)}\) and consequently the number of months in which they actually take on values is very small.\(^{(62)}\) Indeed, the more extensive pound-dollar and pound-franc ID's are significant and plausibly signed on three occasions. Therefore, a more reasonable assumption would be to say that no explicit evidence of effective official intervention has been found but it is not ruled out because of the probable inadequacy of the test.

This is not true of the seasonal and trend variables: there is ample evidence to suggest that the belga tended to weaken in the summer and strengthen at the turn of the year and that the Swiss franc strengthened during the winter tourist season (as expected) in the first quarter although the guilder's seasonal variation, if indeed there was any, is less certain. The trend variable suggests that the minor gold bloc currencies gradually strengthened (with the exception of one exchange rate). This is rather surprising as the opposite may have been expected due to increasing fear of war but at least the performance of the trend is consistent across exchange rates.

A number of speculative variables were significant and those of Belgium, Holland and Switzerland have been examined for consistency in different exchange rates and possible explanations have been provided for the (few) inconsistencies. It was anticipated that the potential speculative influence of such small countries may be swamped by other effects and not therefore be important. This was only clearly the case for Holland although it was also true for Switzerland except in the Swiss franc's exchange rate against the pound; however, the Belgian dummies were apparently quite important. Nevertheless, the U.K., U.S. and France did provide a lot of significant dummies which generally behaved in a manner consistent with their performance in
the pound-dollar-franc estimates. All this would suggest speculation did have a role in the determination of these exchange rates, but not an exclusive one. In addition, it appears that the belga may have been "undervalued" against the franc in the period between the belga devaluation (March, 1935) and the French devaluation (September, 1936), and that the franc may have been "overvalued" in the October, 1936 - June 1937 period.

Finally, there was a clear order of exchange rates in terms of the degree to which the model was supported: exchange rates involving the dollar were "best" followed by those involving the French franc and lastly, those with the pound. This may suggest that the principal exchange rate for these countries was that with the dollar and, to some extent, the rest simply followed. However, only one of the pound exchange rates - against the guilder - actually failed to provide any support for the hypothesis that exchange rates were determined by "economic fundamentals", and in this case distortions caused by Dutch protectionism and strong affinity for the gold standard can plausibly be put forward as reasons for this. Indeed, these results would seem to indicate that the degree of protection may have played a role in determining exchange rates in the 1930's: the "best" results in this chapter are provided by the belga, the currency of a country where protection was half-hearted, and the worst are those of the guilder, that of a country which pursued a vigorous protectionist policy.
FOOTNOTES TO CHAPTER SIX

1. In a sense this is the correct procedure since logically no country can proxy the ROW and therefore all other countries, in principle, should actually be included in the model. In practice, this is obviously impossible (even in the absence of data deficiencies) since it would substantially reduce the degrees of freedom and almost certainly lead to multicollinearity. However, this is the extreme case; where there are only two fairly equal contenders for the choice of "third country" it may be sensible to include both.

2. Dr. Trip, Governor of the Dutch Central Bank, reported in the Economist, 10/10/36, p. 70. The same source indicates a curious change in debating positions in Holland at this time: those who formerly favoured guilder devaluation now wanted it to be fixed at a new (lower) level while those who had advocated maintaining the gold standard wanted the guilder to find its own level.


4. For example, it is argued in an article in Lloyds Bank Review in November, 1934, that Belgium "would gain by a stable relationship to the pound... This is realised in Belgium; and it is generally admitted that Belgium should have followed sterling when the Scandinavian countries did so". (pp. 478-9).

5. Economist, 16/1/32, p. 121. Examples of this would include the Ouchy Convention negotiated in 1932, the attempts to save the belga by expanding the internal trade of the gold bloc, the U.S.-Belgian trade treaty (1935), the immediate adherence to the Tripartite Agreement in 1936 and, perhaps most clearly, the attitude following the devaluation in March, 1935, when strenuous efforts were made to show that the devaluation was not a competitive one.


8. Economist, 20/7/33, p. 20.


11. Statist, 16/11/35, International Banking Supplement, p. 10. The banking problems were caused largely by the "mixed" nature of the banks' business. In the 1926-29 boom period, the banks had issued a large number of long term loans to industry as well as granting unsecured current account advances on a lavish scale. Consequently, when the depression came, the banks found both a drop in the market value of their industrial participations and that they had a large number of unrealizable assets which led to liquidity shortages and a fear of insolvency. Indeed, in March, 1934, the Banque Belge du Travail had to close its doors and this was the fate of numerous small banks (although the larger banks managed to survive).
12. This was reported in the Wall Street Journal, 26/3/35, and is referred to in Shepherd (1936).

13. There were also political disturbances in Belgium, in the shape of Rexist agitation in October, which may have decreased confidence and hence caused weakness.

14. Obviously this variable is not applicable to the belga-franc estimates.

15. See the Economist, 24/7/37, pp. 187-8.


19. The Dutch unemployment rate was considerably higher than that of Belgium and Switzerland throughout the 1930's. It never fell below 20% after October, 1931.


25. Economist, 17/2/34, Commercial History and Review of 1933, p. 8. See also Lloyds Bank Review, June, 1933, p. 252: "Considerable nervousness has been felt during the month (May) lest Holland might be unable to maintain the gold standard".


27. See the Economist, 14/7/34, pp. 61, 4/8/34, pp. 222-3, 25/8/34, pp. 356-7 and 22/9/34, pp. 536-7 for an account of these difficulties.


33. Not surprisingly this step was taken with great reluctance. There was no panic in Holland after the French left the gold standard and, indeed, the initial reaction was to announce that Dutch monetary policy would remain unchanged. However, following the Swiss decision to leave the gold standard and an uncomfortably large gold outflow, this decision was reversed within twenty-four hours.
34. Economist, 26/9/31, p. 563.
36. Economist, 14/1/33, p. 59.
38. Reported in the Economist, 27/10/34, p. 784.
40. Economist, 6/4/35, p. 792. See also Economist, 20/4/35, p. 909: "Since the fall of the belga Switzerland has apparently become the weakest link in...the gold bloc".
41. This was because the two main forms in which Swiss banks held their assets were of dubious reliability; these were investments abroad, many of which were held in countries subject to transfer moratoria (mainly Germany), and mortgages and other long term advances, whose value depended very much on economic conditions and government policy.
44. Statist, 2/5/36, p. 714.
48. The only difference was that in the first stage estimates two variants of the two versions of each exchange rate were estimated, each using a different "third country". However, from the second stage onwards, these two variants were amalgamated and two countries (significant first stage variables only) were used to proxy the ROW.
49. This variable was significant and correctly signed in the pound-franc estimates.
50. The COL price ratio just fails to be significant at the 20% level. However, it was significant at this level in earlier estimates.
51. A re-examination of the first stage estimates showed that BG4 had been significant and had been excluded in error from later estimates.
53. These figures are all based on data for the first nine months of each year given in the Economist, 10/11/34, p. 876. They are calculated from data on trade flows in value terms (in guilders). This is the relevant measure since it is the quantity of currency not goods crossing the frontier that is important. (In fact, not surprisingly, the trend described in the text is much accentuated if data in volume terms are used.)

54. The Durbin Watson statistics rose to 1.85 (COL) and 1.84 (WP).

55. The main effect was that a number of the speculative dummies became insignificant which tends to give more rather than less support to the principal hypothesis being tested here.

56. Of course, it should be remembered that the "wrong" sign and significance of the relative income variable in the guilder-pound estimates is supportive of the monetary approach to exchange rate determination. Nevertheless, it is also true that the monetary approach (and any other) would always suggest a negative relationship between the relative price variable and exchange rates (as they are constructed here).

57. The effect should not, however, be given too great a weight in view of the performance of the same variable in the Swiss franc-pound estimates and its insignificance in the pound-dollar-franc estimates.

58. The COL price ratio had been significant in earlier estimates.

59. In fact, it is not quite as straightforward as this. The pound-franc ID is insignificant in the pound-franc estimates and "wrongly" signed in the pound-dollar estimates which, in a sense, makes its performance in the Swiss franc-French franc estimates actually inconsistent. This matter is taken up in Chapter 8.

60. Fears of capital loss due to devaluation may completely override the attraction from any gain to be had from interest rate differentials particularly in the case of the gold bloc currencies.

61. The sources for the ID's for the three main currencies (pound-dollar-franc) are much more extensive and reliable.

62. The Belgian, Dutch and Swiss ID's actually take on a value in four, seven and four months (out of eighty-eight) respectively.

63. An examination of the U.K., U.S. and French dummies' performance for consistency across all the exchange rates they were included in will be carried out in Chapter 8.
Canada is rather different from the other countries included in the study in a number of ways. Most obviously, it is the only Empire country examined, mainly because it is the only member of the Empire whose currency did not maintain a fairly rigid attachment to the pound and can be said to have floated in some sense. The reason for this was that Canada, unlike other Empire countries, had very close links not only with Britain but also with the U.S.; indeed, Canada is unique in that it maintained a very close relationship with both the U.K. and U.S. simultaneously. The importance of these two countries in Canadian economic affairs is made clear by considering firstly, that Canada was a very open economy - approximately thirty per cent of Canadian national income arose directly from external sources (1) - and secondly, that in the 1930's the U.S. and U.K. accounted for almost a half and a third, respectively, of Canadian trade whilst the rest of the world accounted for barely a quarter, as is shown by Table 7.1. (2) In fact, this rather underestimates

<table>
<thead>
<tr>
<th>TABLE 7.1: PERCENTAGE SHARES OF TOTAL CANADIAN TRADE (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>1928</strong></td>
</tr>
<tr>
<td><strong>U.S.</strong></td>
</tr>
<tr>
<td><strong>IMPORTS</strong></td>
</tr>
<tr>
<td>67.6</td>
</tr>
<tr>
<td>15.7</td>
</tr>
<tr>
<td>83.3</td>
</tr>
</tbody>
</table>
the importance of Britain and the pound in a sense, since the proportion of total Canadian trade with the Empire as a whole was rather larger than that with Britain itself, amount to 30.6% in 1931 and rising to 42.5% by 1936. (4)

In addition, Britain and the U.S. injected substantial amounts of capital investment into Canada during this period: for example, the value of U.S. assets accumulated in Canada reached $4,928 by 1930 and stayed around this level throughout the decade and the comparable figure for British assets held in Canada in this period was approximately $2,700. There was also substantial Canadian investment in the U.S. (though not so much in the U.K.) in the 1930's. This predominance of two countries in Canadian economic affairs, in terms of both trade and capital flows, clearly points to the two exchange rates which ought to be examined and makes the choice of third country to proxy the rest of the world equally obvious. Nevertheless, in spite of the importance of the U.S. and U.K. for Canada, external factors should not be emphasised to the point of totally excluding internal factors.

Even so, the course of the Canadian dollar in the 1930's does largely reflect the dominance of these two countries, particularly in the 1931-33 period. The Canadian dollar had left the gold standard during WW1 and had returned (at the old parity) in July, 1926. In early 1929, however, the exchange rate came under some pressure due to deficits on both current and capital accounts and, in January, 1929, the government abandoned the gold standard (de facto) by imposing an informal embargo on the export of gold. Surprisingly, the Canadian dollar did not drop to a substantial discount until October, 1929, (5) when the Wall Street Crash meant that Canadians who had speculated there had to transfer large amounts to cover previous purchases.
However, large (long term) capital inflows in 1930 pushed the Canadian dollar back to parity where it remained (more or less) until Britain left the gold standard in 1931.

Britain leaving gold created a problem for the Canadians. If the Canadian dollar were not depreciated with the pound then Canadian goods would become less competitive in a major export market. On the other hand, if it were depreciated with the pound then, although it is true that this would have given Canadian exports a competitive edge in the U.S., any advantage was not only slight, since demand for the main export good (newsprint) was (price) inelastic, but overwhelmingly offset by the disadvantage of an increased debt burden and cost of debt servicing since this would now have to be paid in more expensive U.S. dollars; this drawback was of great importance given the high level of Canadian indebtedness to the U.S. Caught between these two stools, Canada chose a middle path and the currency floated half-way between the depreciated pound and the American dollar until the U.S. left the gold standard in 1933:

"On the whole, it was considered wiser to take the buffets from both sides rather than a knock-down blow from one". (8)

For a short time the Canadian dollar stayed half-way between the French franc (still on gold) and the depreciated dollar and pound but it then followed the former and from January, 1934, onwards, the Canadian dollar, U.S. dollar and pound all stayed fairly steady at slightly over 40 per cent below their pre-1931 gold parity until the outbreak of WW2 when the Canadian dollar dropped to a 10 per cent discount against the U.S. dollar.
In terms of the model developed here and the independent variables that are included, several other aspects of the Canadian economy in the 1930's also require some discussion. These include the degree of official intervention in the foreign exchange market, the roles of protection and relative price effects (which are obviously related) and the seasonal aspects of an economy heavily dependent on wheat exports. The first issue can be dealt with very quickly; with the exception of the government's deliberate currency inflation in late 1932, which was one of several factors which may have caused the currency to weaken in this period and is therefore represented by a speculative dummy, there seems to have been very little attempt to influence the course of the exchange rate. In the first half of the 1930's (before the Central Bank was set up in 1935):

"Except for the above case, there was no attempt at control. The Minister of Finance announced that it was considered the best policy to allow the dollar to find its own level....The policy of drift was not regarded as satisfactory, but as the best thing possible in... the circumstances". (9)

This policy seems to have continued even after the setting up of a Central Bank and an exchange fund:

"...there does not appear to be any clear evidence that the Bank of Canada, the last government, or the new Government which took office in November 1935, have decided on any definite policy or any change from allowing the Canadian dollar....to find its own level". (10)

More specifically:

"The Canadian fund never operated at all....." (11)

In view of this evidence (or rather lack of it) no exchange fund dummy variable will be constructed for the Canadian dollar.
The role of relative prices in the determination of the Canadian exchange rate needs to be discussed in particular detail because there is some evidence to suggest that relative prices were of little importance in determining Canadian trade flows. The price inelasticity of American demand for Canadian newsprint has already been mentioned and it seems plausible to argue that European demand for Canadian wheat and flour was similarly inelastic. More generally, the trends in Canadian exports in the 1931-33 period have led some authors to conclude that it was income levels in Canada's major customers rather than relative price effects through changes in the level of the Canadian exchange rate that determined Canadian exports. More formal (econometric) evidence is provided by Thompson (1970) whose estimates of a Canadian export function for the period 1926-38 produced insignificant price variables and highly significant income variables. Thompson also estimates a Canadian import function which produces better results for the relative price variables but still finds that income fluctuations were the predominant influence.

The existence of various protective measures, which is particularly important in the Canadian case (and is discussed below), would, of course, undermine relative price effects, particularly in the import function, but whatever the reasons, the implications are clear: relative income effects are more likely to be important in the determination of the Canadian exchange rate. Fortunately, this does not really undermine the model being tested here since it is not completely wedded to the PPP hypothesis but rather stresses "economic" factors in general, including income levels. In any case there is some contemporary evidence to suggest that relative prices did play some role in determining Canadian trade flows on
occasion and so they should not be completely dismissed at this stage.

The "frankly protectionist policy" (13) of the 1930-35 Conservative government requires further examination. In fact the Bennett administration was largely elected on the basis of his advocacy of protectionism as a panacea and received much of its support from the protectionist manufacturing sector (which it rewarded handsomely). Indeed, within two years of the Conservatives taking office the "Economist's" Canada correspondent was moved to comment:

"We have reached the stage in Canada where a man ought to thank God every morning if no new industry has been born during the night, because every time a new Canadian industry is born it adds to the cost of living and increases the difficulty of doing business in Canada". (14)

However, the success achieved by this protectionism, in terms of steering Canada out of the depression, is rather doubtful. (15) Nevertheless, it was pursued by the Bennett government to the very end of its term of office and despite the policy of tariff reduction pursued by the Liberal government elected in 1935, protection remained at a high level in Canada throughout the 1930's; this must inevitably undermine the model used here.

Furthermore, any interpretation of the Canadian tariff is difficult because it was not just the height and coverage that was important, but the arbitrary and ungenerous way in which the tariff was interpreted by the Canadian authorities (16) and, in the present context, the use of special anti-dumping duties on imports from Britain. This was basically an extra duty aimed at counteracting the pound's depreciation and meant that imports from Britain were effectively
entering the country at an artificial exchange rate (higher than the market rate). For a while, this effectively operative exchange rate was £1 = $4.86\frac{1}{2}$ (September, 1931 to late November, 1931) – in other words the old par value – until it was reduced first to £1 = $4.40$ (late November, 1931 to March, 1933), then to £1 = $4.25$ and finally overtaken by events as the pound appreciated against the Canadian dollar in 1933-34 when this arbitrary valuation was dropped. An attempt was also made to impose a similar special anti-dumping duty on American goods at the end of 1933 following the U.S. depreciation but threats of retaliation by the U.S. led the Canadians to drop this proposal except for a very small number of goods.

The other important aspect of Canadian commercial policy relates to Canada's trade agreements with other countries including, of course, the Ottawa Agreements. Canada's policy towards trade negotiations not surprisingly mirrored that on tariffs, being much more generous after 1935. The effect of the Ottawa Agreements on Canada's balance of payments with her two major trading partners was to improve it in both cases. Canada's trade balance improved with the U.K. because Canadian tariff concessions and interpretations of tariffs were less generous than those of Britain. The Ottawa Agreements were also likely to improve the Canadian trade balance with the U.S. since their objective was to encourage members of the Empire to take each others' exports rather than those of third countries but, in addition, the capital account improved as American firms set up branch factories to circumvent the higher Canadian tariffs.

On returning to power in 1935, the Liberals set about reversing
the drift towards high protectionism and negotiated a commercial treaty with the U.S.\(^{(19)}\) which came into force in January, 1936. A new treaty with the U.K. followed in February, 1937 and, in late 1938, both these treaties were revised as the Liberal government continued its policy of freer trade.\(^{(20)}\) However, despite this slight relaxation of Canada's protectionist stance, this management of trade must inevitably undermine the model developed here (insofar as there is no specific allowance for protection). In particular, the special dumping duties on Canadian imports from the U.K. in 1931-34 must create a distortion since the exchange rate being used for this portion of Canadian trade was not the market rate (the dependent variable here), although it did at least move in the same direction (in discrete steps). Since these duties were not applied to Canada-U.S. trade, the model in general (and the relative price variables in particular) might be expected to perform less well for the Canadian dollar-pound exchange rate than for that between the Canadian and U.S. dollars. Nevertheless, as suggested above, the high level of Canadian protection may also affect the application of the model to the latter and, for this reason, it seems desirable to explore the possibility of devising a "protection" variable.

Fortunately, in the Canadian case where the use of such a variable seems especially important, its construction (in a fairly crude form) would appear to be relatively easy, since the direction of influence of Canadian protection is fairly obvious, as are the turning points in Canadian commercial policy.\(^{(21)}\) In fact, the "protection" variable takes the form of three dummy variables relating to Anglo-Canadian trade (TT1), American-Canadian trade (TT2) and a third simpler variable relating to either, which could
be used to replace TT1 and TT2. This latter variable carries a value of zero during the highly protectionist, Conservative government's period of office and one after the Liberals came to power, reflecting the relaxation of protectionism following the change in government in 1935.

TT1 and TT2 are more complex. The former will measure protective influences which might be expected to create pressure on the Canadian dollar to appreciate against the pound: from September, 1931, the variable takes a value of one to reflect Canadian protective measures (such as the anti-dumping duties) aimed at imports from Britain until the Ottawa Agreements, after which it takes a value of two up to the first Anglo-Canadian commercial treaty, since the effect of Imperial Preference seemed to favour Canadian exports to the U.K. more than vice-versa; finally, following the relaxation of Canadian protectionism after the first Anglo-Canadian trade treaty, the variable falls back to a value of one and then zero after the second Anglo-Canadian treaty. The U.S.-Canada dummy (TT2) is rather simpler and merely reflects the relaxation of Canadian protectionism (towards the U.S.) after the first U.S.-Canada commercial treaty signed at the end of 1935 (operative at the beginning of 1936). This might be expected to work in favour of the U.S. and so TT2 should carry the opposite sign to TT1. These Canadian trade policy dummies are summarized in Table 7.2.

The final area of particular interest in the Canadian case relates to seasonal factors. Given the importance of wheat in the Canadian economy (particularly as an export) it might be expected that there would be observable seasonal variations in the exchange rate. In addition, foreign tourism played a fairly important role
TABLE 7.2: CANADIAN TRADE POLICY DUMMIES

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>PERIOD ON</th>
<th>VALUE</th>
<th>EFFECT OF AN INCREASE ON CANADIAN DOLLAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>TT1 (U.K.-CANADA)</td>
<td>Sept. 31-Nov. 32</td>
<td>1</td>
<td>APPRECIATE</td>
</tr>
<tr>
<td></td>
<td>Dec. 32-Feb. 37</td>
<td>2</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Mar.-37-Nov. 38</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Dec. 38</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td>TT2 (U.S.-CANADA)</td>
<td>Sept. 31-Dec. 35</td>
<td>0</td>
<td>DEPRECIATE</td>
</tr>
<tr>
<td></td>
<td>Jan. 36-Dec. 38</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>TT3 (BOTH)</td>
<td>Sept. 31-Oct. 35</td>
<td>0</td>
<td>DEPRECIATE</td>
</tr>
<tr>
<td></td>
<td>Nov. 35-Dec. 38</td>
<td>1</td>
<td></td>
</tr>
</tbody>
</table>

in the Canadian economy and this too was of a seasonal nature with the inflow of (mainly American) tourists reaching its height in the summer. The tendency of Canadian business conditions to vary in a seasonal (and fairly predictable) manner may also have influenced the exchange rate: there was a general lull in mid-winter (December-January), followed by a recovery in spring, some decline in mid-summer - "Business is experiencing its usual mid-summer torpor..."(22) - and an important improvement in the crop moving season - "The general autumn expansion..."(23) For these reasons, it might be expected that the Canadian dollar would tend to be strong in the summer due to tourism and also in the autumn when exports of wheat to Europe were at their height but weak at the turn of the year during the lull in business activity.(24)

(III)

Having examined the special factors in the Canadian case it now remains to take account of the speculative influences on the Canadian
exchange rate by constructing appropriate dummy variables. The movements of the Canadian dollar in the 1930's have already been summarized and what follows is an elaboration of this and a chronological examination of the various speculative influences. Some of these only apply to movements of the Canadian dollar against the pound or the American dollar (when the Canadian dollar was moving in sympathy with the other major currency) and will only be included in the estimates for the appropriate exchange rate.

Although Britain's abandonment of the gold standard did cause problems for Canada particularly in its relations with the U.S. (discussed above), there appears to have been little to merit a speculative dummy until the latter half of 1932 with one exception. In the first quarter of 1932 the pound began to appreciate (having dropped to a very low level in November-December, 1931). This trend was, in part, due to speculative inflow of funds into Britain and reached its height in March, 1932 (25) at which point it seems to have dragged the Canadian dollar along in sympathy:

"During the past fortnight the Canadian dollar has strengthened considerably in New York...." (26)

This would suggest a first Canadian speculative dummy (for the Canadian dollar-U.S. dollar exchange rate only) in March, 1932.

A number of influences then began to operate in the latter half of 1932 and in early 1933. Initially, there was evidence of a modest strengthening of the Canadian dollar against both the pound and the U.S. dollar in August and September (27) although this may have been partially seasonal. This tendency, however, was very quickly reversed and the Canadian dollar entered a period of weakness in November which lasted until January. This was partly an inevitable consequence of
the earlier strength but was aggravated by the government's
decision "to embark on a deliberate policy of currency inflation"(28)
when, on November, 1st, following the partial failure of an internal
loan, the government borrowed $35 million from the banks. Despite
the apparent unwillingness of the government to go any further than
this these two factors, along with increased agitation for inflation
in Canada (especially amongst Western prairie farmers), "inevitably
aroused a feeling of alarm and caused heavy sales of Canadian
dollars"(29)

The position then becomes confused. The "Economist" reports
that:

"In the past fortnight the Canadian dollar has experienced
a sharp decline in New York, and its fall is attributed
partly to the pessimistic tone of Mr. Bennett's speeches
and partly to the inflationist campaign which is still
proceeding vigorously". (30)

But on the other hand:

"....the American banking crisis in February-March 1933
did not spread to the Canadian banks. Funds flowed in
from the United States to Canada for safety..." (31)

However, whilst the net effect of these two sets of factors on the
Canadian dollar-U.S. dollar exchange rate in February is not clear,
it does seem that the U.S. abandonment of the gold standard (and
the burst of rapid depreciation that followed) did encourage the
Canadian dollar to depreciate against the pound in March-June, 1933:

"...the sterling rate in Montreal has risen sharply" (32)

It would also seem likely that the second round of rapid U.S. dollar
depreciation, following the adoption of Roosevelt's gold buying policy
in November, 1933, may have triggered off another sympathy movement
in the Canadian dollar against the pound.
A number of dummy variables would therefore seem to be required in the mid-1932 to mid-1934 period: for August-September, 1932, for November, 1932 to January, 1933, for March-June, 1933, and for November, 1933 to May 1934, with the latter two dummies only for use in the Canadian dollar-pound estimates since they represent sympathy movements of the Canadian dollar with the U.S. dollar. As far as February, 1933 is concerned, given the conflicting direction of the reported speculative effects, it seems prudent to include no dummy at all for this month. Throughout the rest of the 1930's the Canadian dollar moved fairly closely with both the pound and the dollar and there were no obvious periods of extreme pressure on the Canadian currency. However, in virtually every year there was a month or two in which there was some evidence of potential speculative influences on the exchange rate and so a number of further dummy variables will be created whose importance is perhaps less certain.

The year 1935 marks a watershed for Canada in that it ended with the general election in which the protectionist Conservative government was routed by the Liberal opposition. Not surprisingly there was a certain amount of political uncertainty and agitation throughout the year which may have led to doubts about the Canadian dollar: in May, there was considerable labour unrest with a docker's strike in Montreal and a serious riot in Vancouver; in June, the "Economist" was moved to comment: "There is no lack today in Canada of political unrest born of economic troubles"; in July, a new party was formed by the ex-Minister of Trade (Stevens) and this month also witnessed the rise of the inflationist Social Credit Party in Alberta; in August, the latter were elected into office in local elections. Whether all or part of this was sufficient to cause any
speculative pressure against the Canadian dollar is perhaps debateable but a dummy variable for the May to August period would seem worth trying.

In 1936 the Canadian dollar moved against the pound (according to contemporary sources) in sympathy with U.S. dollar on at least two occasions. Firstly, in the first two months of the year when the dollar weakened against the pound due to a variety of factors:

"...the Canadian dollar has weakened on London in the company of its southern neighbour...." (34)

Then in October:

"The Canadian dollar is following the American dollar in its appreciation against sterling". (35)

This tendency of the U.S. dollar to strengthen continued into November (36) and therefore it seems plausible to assume that the sympathy movement of the Canadian dollar did the same. Thus there will be two Canadian speculative dummies in 1936, both of which are for use in the Canadian dollar-pound estimates only.

Only one month in 1937 is worthy of a speculative dummy and this is July when an appreciation of the Canadian dollar probably occurred due to "hot money" flows from Europe. This would clearly affect the Canadian exchange rate against the pound but in addition, although the U.S. was also a recipient of these funds, it may have influenced the Canadian dollar-U.S. dollar relationship to the extent that Canada was receiving "hot money" from the U.S. also because of fears of its safety there due to industrial unrest and various rumours:

"...there was...the influence of "hot money" flows from Europe, coming directly or via the New York markets..." (37)
"Stories have appeared in the Canadian papers that the Government and the banks are worried about a steady inflow of "hot money" on a large scale, due to the fears of European investors who, having placed their funds in the United States, have now become alarmed about industrial troubles and the possibility of new taxation directed against foreign investors". (38)

Finally, in autumn 1938, there were substantial capital movements out of Europe to America because of fears of war. These have already been discussed in the context of a potential strengthening of the U.S. dollar against the pound and the franc (in Chapter 5). It seems likely that the Canadian dollar may have strengthened against the pound (but obviously not the U.S. dollar) for the same reason and so a final Canadian dummy is created for the Canadian dollar-pound estimates only. The Canadian speculative dummies are summarized in Table 7.3.

TABLE 7.3: CANADIAN SPECULATIVE DUMMIES

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>PERIOD ON</th>
<th>TENDENCY OF C. DOLLAR</th>
<th>APPLICABLE TO</th>
<th>REASON</th>
</tr>
</thead>
<tbody>
<tr>
<td>DC1</td>
<td>Mar. 1932</td>
<td>strong</td>
<td>US</td>
<td>Sympathy with pound.</td>
</tr>
<tr>
<td>DC2</td>
<td>Aug.-Sept. 1932</td>
<td>strong</td>
<td>US, UK</td>
<td>Inflow of capital.</td>
</tr>
<tr>
<td>DC4</td>
<td>Mar.-June 1933</td>
<td>weak</td>
<td>UK</td>
<td>Sympathy with dollar.</td>
</tr>
<tr>
<td>DC5</td>
<td>Nov. 1933-May 1934</td>
<td>weak</td>
<td>UK</td>
<td>Sympathy with dollar.</td>
</tr>
<tr>
<td>DC6</td>
<td>May-Aug. 1935</td>
<td>weak</td>
<td>US, UK</td>
<td>Political unrest.</td>
</tr>
<tr>
<td>DC7</td>
<td>Jan.-Feb. 1936</td>
<td>weak</td>
<td>UK</td>
<td>Sympathy with dollar.</td>
</tr>
<tr>
<td>DC8</td>
<td>Oct.-Nov. 1936</td>
<td>strong</td>
<td>UK</td>
<td>Sympathy with dollar.</td>
</tr>
<tr>
<td>DC9</td>
<td>July, 1937</td>
<td>strong</td>
<td>US, UK</td>
<td>Inflow of &quot;hot money&quot; from U.S. and Europe.</td>
</tr>
<tr>
<td>DC10</td>
<td>Sept.-Dec. 1938</td>
<td>strong</td>
<td>UK</td>
<td>Inflow of &quot;hot money&quot; from Europe due to war scare.</td>
</tr>
</tbody>
</table>
The estimation of the C/P (Canadian dollar—pound) and C/D (Canadian dollar—U.S. dollar) exchange rates followed the pattern established for the pound-dollar-franc triangle. Two versions, using WP's and COL indices respectively, were tried and, in the first instance, employment indices proxied income and the "wide" version of the ID (intervention dummy) was used. The Canadian, U.K. and U.S. speculative dummies were checked for any close correspondence but the incidence of overlap was either partial or the variables concerned carried opposite expected signs; consequently, no speculative dummies had to be omitted on the grounds of overlap. The estimation proceeded in a number of stages with insignificant variables being omitted or, where appropriate, replaced by alternative proxies. The final equations are presented in Tables 7.4 and 7.5. In general the results are supportive of the model (although this is more true of the C/D than the C/P estimates). Many of the important variables are significant and plausibly signed, the R-squared is high and there is no positive evidence of serial correlation.

Examining the C/P equation in more detail would suggest ROW effects had a very significant influence (which is not surprising given the importance of the U.S. to the Canadian economy). U.S. "income" and, to a lesser extent, prices are clearly important while the significance and negative sign of the U.S.—U.K. interest rate differential would indicate that a fall in U.K. interest rates relative to those in the U.S. caused the Canadian dollar to appreciate against the pound. This is plausible in the sense that a decrease in U.K. interest rates should lead to a depreciation of the pound in general and, to the extent that the U.S. interest rate may have proxied the
TABLE 7.4: CANADIAN DOLLAR-POUND: FINAL ESTIMATES

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>COL</th>
<th>WP</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>8.42 (6.02)</td>
<td>6.87 (6.49)</td>
</tr>
<tr>
<td>U.K. prices/Canadian prices</td>
<td>-2.33 (1.38)</td>
<td>-1.08 (1.37)</td>
</tr>
<tr>
<td>U.S. prices</td>
<td>-2.73 (2.18)</td>
<td>-1.18 (1.27)</td>
</tr>
<tr>
<td>U.K. income/Canadian income (2)</td>
<td>-1.07 (0.87)</td>
<td>-2.28 (2.05)</td>
</tr>
<tr>
<td>U.S. income (3)</td>
<td>1.01 (3.95)</td>
<td>1.41 (2.95)</td>
</tr>
<tr>
<td>U.S. interest rate-U.K. interest rate</td>
<td>-0.20 (4.94)</td>
<td>-0.11 (3.52)</td>
</tr>
<tr>
<td>Seasonal variables:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>A1</td>
<td>-0.06 (2.00)</td>
<td>-0.03 (1.37)</td>
</tr>
<tr>
<td>B1</td>
<td>0.03 (1.50)</td>
<td>0.03 (1.61)</td>
</tr>
<tr>
<td>B2</td>
<td>0.02 (1.38)</td>
<td>0.02 (1.32)</td>
</tr>
<tr>
<td>Trend</td>
<td>0.02 (7.07)</td>
<td>0.02 (7.12)</td>
</tr>
<tr>
<td>Speculative dummies:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>K1</td>
<td>-0.28 (3.40)</td>
<td>-0.24 (2.88)</td>
</tr>
<tr>
<td>K2</td>
<td>-0.34 (4.00)</td>
<td>-0.30 (3.16)</td>
</tr>
<tr>
<td>DC2</td>
<td>-0.18 (1.97)</td>
<td>-0.23 (2.67)</td>
</tr>
<tr>
<td>DC3</td>
<td>-0.48 (6.82)</td>
<td>-0.44 (5.83)</td>
</tr>
<tr>
<td>DC4</td>
<td>-0.13 (1.80)</td>
<td>-0.11 (1.46)</td>
</tr>
<tr>
<td>DC5</td>
<td>0.30 (6.15)</td>
<td>0.31 (6.24)</td>
</tr>
<tr>
<td>DC7</td>
<td>0.14 (1.79)</td>
<td>0.19 (2.32)</td>
</tr>
<tr>
<td>DC10</td>
<td>-0.25 (3.10)</td>
<td>-0.21 (2.75)</td>
</tr>
<tr>
<td>Protection dummies:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>TT1</td>
<td>0.16 (4.06)</td>
<td>0.20 (6.70)</td>
</tr>
<tr>
<td>TT2</td>
<td>-0.21 (4.15)</td>
<td>-0.24 (3.42)</td>
</tr>
<tr>
<td><strong>R-SQUARED</strong></td>
<td>0.95</td>
<td>0.95</td>
</tr>
<tr>
<td><strong>DURBIN-WATSON STATISTIC</strong></td>
<td>1.60</td>
<td>1.38</td>
</tr>
<tr>
<td><strong>STANDARD ERROR OF THE REGRESSION</strong></td>
<td>0.096</td>
<td>0.099</td>
</tr>
<tr>
<td><strong>F-STATISTIC</strong></td>
<td>73.0</td>
<td>68.5</td>
</tr>
</tbody>
</table>

Notes: 1. T-statistics in brackets.
2. U.K. unemployment index and Canadian employment index.
Canadian interest rate, even more so; indeed the insignificance of this variable in the pound-dollar estimates would support this latter interpretation. Finally the apparently greater importance of U.S. incomes over U.S. prices is not surprising given the expectation that incomes rather than prices were the major determinant of the demand for Canadian exports.

The bi-lateral ratio variables perform less well. They are all correctly signed but both the relative price variables (WP's and COL indices) are only significant at low levels (20%) and the original relative income variable based on employment indices was insignificant, although when British employment index was replaced by the unemployment index the variable did become significant in the WP version (as shown in Table 7.4). This poor showing of relative prices is not unexpected and the likelihood is that it is British prices that are unimportant, and are causing the low levels of significance of the relative price variable, because of the existence of a variety of Canadian protective devices, discussed in Section II, which have undermined the expected effect of changes in British prices on Canadian demand for British imports (such as the artificially high exchange rate in 1931-33). This hypothesis is supported by the performance of the price variables in the C/D estimates where the Canada-U.S. price ratio is highly significant (thereby suggesting that Canadian prices did influence Canadian exchange rates) but U.K. prices are only significant at low levels. Moreover, a comparable study of the Canadian dollar-pound-U.S. dollar triangle which tested a similar model in non-ratio form found that U.S. and Canadian prices tended to be significant but not British prices. (40)

Unfortunately, the poor showing of the income ratio is less easy
to accept, particularly as it had been anticipated that relative
incomes would play a major role since income levels in the U.K.
seem largely to have determined the demand for Canadian exports
(according to the evidence examined earlier). However, there is
evidence to suggest that it is Canadian income that is unimportant
not U.K. income: in the C/D estimates the U.K. employment index is
significant (at the 1% level) and Ridpath found that the Canadian
employment index carried the wrong sign in his initial estimates and
was insignificant in his final equations. The final major quanti­
fiable independent variable, Canadian interest rates, could not be
examined because of the lack of any data. Ridpath did manage to
construct two series of Canadian three month interest rates based
on proxies but found them of little importance. This would
indicate that their omission from the present study is of little
consequence.

Of the non-quantifiable "economic" variables both the "narrow"
and the "wide" versions of the pound-dollar ID were found to be
insignificant whilst the trend variable was significant and
indicated a secular tendency for the pound to appreciate against the
Canadian dollar. There is clear evidence of a seasonal effect with
three significant variables. When these are combined it is quite
obvious that Canadian wheat exports were the dominant influence on
seasonal patterns in the C/P exchange rate (as expected) with the
Canadian dollar tending to strengthen during the crop moving season
in autumn (September-February) and weaken in the spring (March-
August).

The performance of the dummy variables representing changes in
protection is difficult to interpret: both TT1 and TT2 are significant
(and consequently TT3 was not tried) but they carry the wrong signs. However, the signing of the protection variables was based on an examination of Canadian bi-lateral trade flows with the U.S. and U.K. which, in retrospect, may have been a fairly tenuous exercise since it involved the implicit assumption that such changes were due solely (or at least mainly) to changes in commercial policy; not only may this assumption be incorrect but it is also, to some extent, inconsistent with one of the main hypotheses of the model, that trade flows were determined by prices and incomes. Perhaps the performance of TT1 and TT2 is best interpreted as providing a good example of the impossibility of adequately incorporating the effects of protection into the model, even in an apparently promising case.

Two of the British speculative dummies are significant but two are not (K3 and K4). K1 is correctly signed but the "wrong" sign of K2 is surprising; however, K2 overlaps with the significant DC3 and may be negatively signed for the same reasons (discussed below). Of the ten Canadian speculative dummies six are significant of which two are "wrongly" signed. The "wrong" sign and significance of DC3 is initially puzzling; however, a rational explanation may be found in terms of the Canadian dollar strengthening against the pound in sympathy with the U.S. dollar since the significance and sign in the pound-U.S. dollar estimates of an American speculative dummy (S3), which closely corresponds to DC3, indicated that the U.S. dollar did tend to strengthen against the pound in this period. Similarly the "wrong" sign of DC4, included to take account of expected Canadian dollar weakness in the period immediately after the U.S. left the gold standard in 1933, is difficult to explain. The most credible explanation
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would seem to lie in the possibility of (unreported) official intervention by the British authorities to hold the pound down in order to maintain some degree of competitiveness (with the U.S.) in the Canadian market.

In conclusion, the performance of the model in representing the influences that determined the C/P exchange rate is perhaps not entirely satisfactory although, to a large extent, the problems were correctly identified a priori (particularly with respect to relative prices). However, the results did indicate some measure of support for the model and the results of the C/D estimates (Table 7.5) are highly supportive. There is clear evidence of both strong relative income and (a little surprisingly) strong relative price effects. In addition, the third country (U.K.) economic variables show up well although the U.K.-U.S. interest rate differential is not significant as it was in the C/P equations. (This would tend to indicate more support for the argument, presented above, that U.S. interest rates proxied Canadian interest rates to some extent).

There is also evidence of a seasonal effect with the Canadian dollar tending to weaken against the U.S. dollar in the first half of the year (January-June) but strengthen for the remainder (July-December). As the effects of the crop moving season should have been important in both Canada and the U.S., the reason for the autumnal strengthening of the Canadian dollar is not immediately obvious although, given the comparatively much greater importance of wheat in the Canadian economy, it may be that the tendency for the currency to strengthen in the autumn was more marked in Canada. The strength of the Canadian dollar against the U.S. dollar in the summer was expected and is due to the influx of American tourists into Canada.
which reached its height at this time. The negative trend variable would indicate a secular tendency for the Canadian dollar to appreciate vis-à-vis the U.S. dollar in the 1930's; there is no obvious reason for this. All three protection dummies (TT1, TT2 and TT3) were tried and found to be insignificant which would seem to reinforce the earlier conclusion (in connection with their performance in the C/P estimates) that this indicates the impossibility of adequately modelling protection.

Seven of the American speculative dummies were significant and three were not. The insignificance of S1 and S7 is not surprising given the poor performance of the former in estimates of other U.S. dollar exchange rates and the fact that the latter represents an inflow of "hot money" into the U.S. following the final collapse of the gold bloc; consequently, this influence may also have affected the Canadian dollar and, in any case, S7 clearly represents an effect that is really only applicable to U.S. dollar exchange rates against European currencies. The final insignificant American dummy - S4 - represents the first phase of the undervaluation of the U.S. dollar in 1933; it may be that the Canadian dollar tended to move with the U.S. dollar in this first phase of depreciation but not in the second (and even more undervalued) phase after the U.S. gold buying in November, 1933. It is certainly the case that the U.S. dollar appears to have been undervalued against the Canadian dollar in the November, 1933 to December, 1934 period as S5 was both significant and correctly signed.

Of the seven significant U.S. speculative dummies five are correctly signed (S2, S5, S8, S9 and S10). This is not inconsistent with their performance in estimates of other dollar exchange rates.
TABLE 7.5: CANADIAN DOLLAR-U.S. DOLLAR: FINAL ESTIMATES

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>COL</th>
<th>WP</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>0.93 (6.13)</td>
<td>0.96 (5.42)</td>
</tr>
<tr>
<td>U.S. prices/Canadian prices</td>
<td>-0.46 (3.61)</td>
<td>-0.22 (3.56)</td>
</tr>
<tr>
<td>U.K. prices</td>
<td>0.28 (1.68)</td>
<td>-0.13 (1.51)</td>
</tr>
<tr>
<td>U.S. income/Canadian income (2)</td>
<td>-0.28 (4.74)</td>
<td>-0.34 (6.96)</td>
</tr>
<tr>
<td>U.K. income (3)</td>
<td>0.75 (4.09)</td>
<td>0.90 (4.09)</td>
</tr>
<tr>
<td>U.S. interest rate-U.K. interest rate</td>
<td>-0.003 (0.87)</td>
<td>-0.004 (1.10)</td>
</tr>
<tr>
<td>Seasonal variable:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>A1</td>
<td>-0.02 (5.40)</td>
<td>-0.01 (4.02)</td>
</tr>
<tr>
<td>B1</td>
<td>-0.02 (4.52)</td>
<td>-0.02 (6.46)</td>
</tr>
<tr>
<td>B2</td>
<td>-0.004 (1.83)</td>
<td>-0.003 (1.14)</td>
</tr>
<tr>
<td>Trend</td>
<td>-0.004 (7.42)</td>
<td>-0.004 (6.68)</td>
</tr>
<tr>
<td>Speculative dummies:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>S2</td>
<td>-0.05 (4.13)</td>
<td>-0.04 (3.77)</td>
</tr>
<tr>
<td>S3</td>
<td>0.08 (7.11)</td>
<td>0.08 (7.07)</td>
</tr>
<tr>
<td>S5</td>
<td>-0.08 (13.7)</td>
<td>-0.08 (16.4)</td>
</tr>
<tr>
<td>S6</td>
<td>-0.03 (2.68)</td>
<td>-0.04 (4.22)</td>
</tr>
<tr>
<td>S8</td>
<td>0.01 (0.95)</td>
<td>0.01 (1.49)</td>
</tr>
<tr>
<td>S9</td>
<td>-0.02 (2.55)</td>
<td>-0.03 (2.87)</td>
</tr>
<tr>
<td>S10</td>
<td>0.02 (1.19)</td>
<td>0.02 (1.55)</td>
</tr>
<tr>
<td>DC1</td>
<td>-0.05 (2.99)</td>
<td>-0.04 (2.55)</td>
</tr>
<tr>
<td>DC3</td>
<td>-0.06 (4.45)</td>
<td>-0.06 (4.23)</td>
</tr>
</tbody>
</table>

R-SQUARED              | 0.96                  | 0.96                  |
DURBIN-WATSON STATISTIC| 1.82                  | 1.93                  |
STANDARD ERROR OF THE REGRESSION | 0.014 | 0.014 |
F-STATISTIC            | 103.6                 | 101.9                 |

Notes: 1. T-statistics in brackets.
   2. U.S. employment index and Canadian employment index.
and would therefore seem to require little comment other than to note the clear indication that American speculative influences were important in determining the C/D exchange rate. Moreover, the incorrect signs of S3 and S6 were perhaps to be expected given their performance elsewhere and possible reasons for their incorrect sign were examined in detail in the discussion of the pound-U.S. dollar estimates. It was concluded that the wrong sign of S6 represented a continuation of the U.S. dollar's undervaluation into 1935 and, less plausibly, that S3 reflected a combination of confidence engendered by Roosevelt's victory in the Presidential election and (or) unreported official support for the dollar. However, there are complications relating to S3 in the Canadian context because of its overlap with a Canadian dummy (DC3) which are discussed below.

Only five of the ten Canadian speculative dummies were applicable to the U.S. and only two of these were significant. The insignificance of DC6 and DC9 is consistent with the C/P estimates in which they were also insignificant. Moreover, when the Canadian dummies were constructed, there was some suspicion that the influences represented by DC2 were seasonal and so this variable's insignificance is not too surprising. DC1 was significant and represented a sympathy movement with the pound in March, 1932. Its significance posed the question as to whether a Canadian speculative dummy should also have been included for the final quarter of 1931 to reflect potential sympathy depreciation of the Canadian dollar with the pound and an examination of the residuals suggests that such a movement probably did occur. The final Canadian dummy, DC3, is significant and wrongly signed.

This is difficult to explain since a similar occurrence in the C/P estimates had been interpreted in terms of a sympathy movement
with the U.S. dollar; therefore DC3 might have been expected to be insignificant in the C/D equations. Furthermore, S3 (which covers much the same period) carries the opposite sign to DC3; thus the former is indicating a Canadian dollar appreciation and the latter an appreciation at (approximately) the same time. A rather complicated explanation in terms of a longer term tendency for U.S. dollar appreciation in the November, 1932-March, 1933 period (picked up by S3) containing a sub-period, November, 1932-January, 1933 (picked up by DC3), in which there was a panic flow of American capital into neighbouring Canada because of Roosevelt's election, thereby causing a Canadian dollar appreciation (U.S. dollar depreciation) is possible, but unlikely. A more plausible explanation may lie in the three month overlap between the two variables (the three month DC3 fits into the five month S3) and it may be that this is causing the rather odd result and that this overlap should have been considered sufficiently large to merit the exclusion of one of these two variables.

(v)

This completes the analysis of the Canadian dollar. Before drawing some general conclusions something should be said about consistency of performance of the variables in the two Canadian exchange rates examined. This has already been referred to in numerous cases and, in fact, there are not many cases where inconsistency could arise (especially since half of the Canadian speculative dummies were not applicable to the C/D exchange rate). Of the four Canadian speculative dummies that appeared in the estimates of both exchange rates, three were insignificant in both and the other was significant and carried the same sign (albeit the "wrong" one). There were a few other variables
appearing in both exchange rate equations which could have displayed inconsistency but, in the event, none of them did: the third country interest rate differential was significant in the C/P and not the C/D estimates but this was interpreted as perhaps indicating that Canadian and U.S. interest rates moved together and so the latter proxied the former, and third country official intervention appeared to have no effect on either Canadian exchange rate.

Besides considering the possibility of inconsistency it would be instructive to examine the residuals. This was done and revealed eleven clusters of large residuals for each of the two Canadian dollar exchange rates and these are listed in Table 7.6. Explanations of

<table>
<thead>
<tr>
<th>Period</th>
<th>Sign</th>
<th>Period</th>
<th>Sign</th>
</tr>
</thead>
<tbody>
<tr>
<td>July 1932</td>
<td>-</td>
<td>Nov. 1931</td>
<td>-</td>
</tr>
<tr>
<td>Dec. 1932</td>
<td>-</td>
<td>April 1932</td>
<td>-</td>
</tr>
<tr>
<td>Feb. 1933</td>
<td>-</td>
<td>May–Aug. 1932</td>
<td>+</td>
</tr>
<tr>
<td>April 1933</td>
<td>-</td>
<td>April–May 1933</td>
<td>+</td>
</tr>
<tr>
<td>May–Oct. 1933</td>
<td>+</td>
<td>June–Oct. 1933</td>
<td>-</td>
</tr>
<tr>
<td>Oct.–Dec. 1934</td>
<td>+</td>
<td>Jan. 1935</td>
<td>-</td>
</tr>
<tr>
<td>Mar.–April 1936</td>
<td>+</td>
<td>May–July 1935</td>
<td>-</td>
</tr>
<tr>
<td>Feb. 1938</td>
<td>+</td>
<td>July–Oct. 1936</td>
<td>+</td>
</tr>
<tr>
<td>July–Aug. 1938</td>
<td>-</td>
<td>Oct. 1937</td>
<td>+</td>
</tr>
</tbody>
</table>
varying degrees of plausibility can be provided for most of these residuals, mainly in terms of mistiming or extensions of speculative dummies and the difficulties involved in trying to use constant value dummy variables to pick up uneven effects.

Approximately half the C/P residuals can be explained by mistiming of Canadian speculative dummies: an extension of DC2 would have removed the July and September-October, 1932 residuals and similarly extensions of DC7 and DC10 would account for the March-April, 1936 and July-August, 1938 residuals; also the October, 1936-February, 1937 residuals could be caused by a mistiming of DC8 (which would explain its insignificance in the final estimates). A number of other residuals would seem to be related to the U.S. dollar depreciation and undervaluation in 1933-34. DC5 was significant indicating a sympathy depreciation of the Canadian dollar with the U.S. dollar and the May-October, 1933 residuals would seem to provide further evidence of this. Furthermore, it was suggested in the examination of the pound-dollar-franc triangle that the U.S. dollar depreciation was an uneven affair and the April, 1933 and October-December, 1934 residuals here would seem to bear this out. Similarly, the December, 1932 and February, 1933 residuals quite clearly suggest that the significant (and negatively signed) DC3 is inadequately picking up an uneven effect. Finally, there is no obvious explanation for the February, 1938 residuals.

Similar explanations for most of the residuals in the C/D estimates are possible. Mistiming of S1, DC6 and S8 would seem to explain the residuals in November, 1931, September, 1935 and July-October, 1936 respectively. An identical mistiming of S1 with dollar weakness occurring not in October, 1931 (as S1 supposes) but in November
was discovered in the examination of the pound-dollar-franc
residuals (in Chapter 5). Two other sets of residuals were also
similar to those in the pound-dollar-franc estimates: the negative
January, 1935 residual once again indicates the continuation of the
U.S. dollar devaluation into 1935 and that January, 1935 is possibly
more properly considered as part of S5 rather than S6, and the May-
July, 1935 residuals suggest continued dollar undervaluation well
into 1935. Moreover, the April and May-October, 1933 residuals,
allied with the insignificance of S4, may also be caused by the U.S.
dollar undervaluation, in that it was an uneven affair that cannot
be adequately handled by a constant value dummy variable. Finally,
the December, 1931-January, 1932 residuals may reflect a U.S. dollar
recovery (following its November weakness) or a sympathy weakening
with the pound whilst the April, 1932 residual almost certainly
reflects a sympathy strengthening with the pound (and an extension
of DC1). This leaves two sets of large residuals - May-August, 1932
and October, 1937 - for which there is no obvious explanation. Thus,
it is possible to adequately explain most of the large residuals in
the Canadian dollar exchange rates.

Moreover, in general terms it would seem that the model developed
here and the hypotheses embodied within it, largely explain the
fluctuations of the Canadian dollar in the 1930's, particularly against
the U.S. dollar. There were some problems with the relative price
variables in the C/P estimates but these had been anticipated. The
expected seasonal effects materialised and there was some evidence of
an important speculative effect as indicated by the significance of a
number of (particularly the American) speculative dummies. The
significance of the third country variables (proxying the ROW) bears
out the contention that the Canadian economy was heavily influenced by events in two countries, the U.K. and U.S. Finally, the major disappointment was the performance of the protection dummies which were, at best, insignificant and, at worst, wrongly signed; however, it was concluded that these problems were more likely to be due to the methodology of these variables' construction rather than to any actual perverse "protection effects".
FOOTNOTES TO CHAPTER 7

1. Marcus (1954) p. 2. Also Hodson (1938) talks of "Canada's peculiar dependence upon wheat exports". (p. 286.)

2. Of course trade was much reduced in 1935 and 1938 compared with 1928. In value terms Canadian trade flows in 1935 were only 30% of their 1928 value and by 1938 had only risen to 38% (although the decline in volume terms was not so great). A further change, which is made clear by Table 7.1, is the increase in trade with the U.K. (as a proportion of total Canadian trade) in 1935; this presumably reflects the impact of the Ottawa Agreements.

3. Calculated from the League of Nations' "Network of World Trade". Canada accounted for a much smaller proportion of U.S. and U.K. trade, 15% and 5-7% approximately.


5. The average discount against the U.S. dollar in January-September, 1929 was less than 1%.

6. In practice, this problem was aggravated by the fact that two of the three rival producers (Australia and Argentina) of the main product in question (wheat) did depreciate their currencies by more than the Canadian dollar. On the other hand, Canada did not suffer too much from the increase in competitiveness of British exports to Canada (caused by the fact that the pound depreciated by more than the Canadian dollar) because of the imposition of special "anti-dumping" duties (discussed below).

7. Much of the Canadian debt incurred in the U.S. contained clauses giving the creditors the choice of currency in which the debt was to be repaid and serviced. The American creditor would obviously choose the U.S. dollar in preference to a depreciated Canadian dollar.

8. Economist, 18/1/36, Special Review of Canada, p. 47.

9. Ibid. p. 47.


15. For example, Marcus (1954), p. 110, states: "As for the resulting effects on the cyclical course of the Canadian economy it is doubtful if the tariff and other protective measures had any important effect". See also Safarian (1959) pp. 56-7 and Brecher (1957) Ch. XI.
16. An example of this would be the long running and ultimately unsuccessful attempts of British woollen producers to get the tariff on their products reduced which are reported in various issues of the Economist (1934-35). The Canadian Tariff Board's final report - see Economist 6/4/35 p. 788 - more or less justified the existing tariffs and only made some slight cuts on woollen fabrics.

17. It was also due to the comparatively earlier and stronger recovery in the U.K. which meant that British import demand held up better than Canadian demand for imports.

18. This trend was also encouraged, in the September, 1931-April, 1933 period, by the fact that the Canadian dollar was depreciated against the U.S. dollar.

19. There was also a demand for this treaty on the American side. The U.S. Secretary of State, Cordell Hull, had been engaged in a policy of negotiating bi-lateral trade treaties for some time.

20. During the same period the commercial treaties with New Zealand and Australia were revised (October, 1937) and, following a minor trade war, Japan (December, 1935).

21. Canada is therefore a special case where some kind of plausible "protection" variable can be constructed. However, in general, the problems outlined in Chapter 4, Section III, make such an exercise impossible.


24. For example, the Economist, 4/2/33, p. 231, talks about "the development last month (January) of a certain seasonal weakness in the (Canadian) dollar...." This would suggest that the implicit assumption of the analysis here - that business activity decreased because of a decrease in foreign demand for Canadian goods - is correct. If it were due, instead, to a fall in Canadian demand then this would presumably also affect Canadian demand for imports and the Canadian dollar would strengthen.

25. This is discussed in more detail and in relation to the construction of the British speculative dummies in Chapter 5.


29. Economist, 4/2/33, p. 231.


34. Economist, 8/2/36, p. 308.
35. Economist, 17/10/36, p. 118.
36. Indeed it was felt necessary to construct an American speculative dummy (S7) for October-November, 1936 to reflect this tendency of the U.S. dollar.
38. Economist, 24/7/37, p. 183.
39. The overlapping variables were as follows (with the expected signs of the variables in brackets): for the C/P relationship there was a partial (one month) overlap between DC3(+) and K2(+) and between DC5(+) and K5(-) and, in addition, K4(-) fitted into DC6(+); for the C/D DC6(+) and S6(+) overlapped partially (two months) and DC3(+) fitted S3(-).
40. See Ridpath (1975) pp. 145-6 and p. 149. This study applies the same basic model to the C/P and C/D exchange rates although there are important differences. Unfortunately it was only discovered at a very late stage in the writing of Chapter 7 of the present study and therefore had little influence on it. However, Ridpath's results are referred to wherever they are of interest in terms of either supporting or contradicting those of the present study.
41. Ibid, p. 115 and pp. 145-6 respectively. It should be pointed out that Ridpath also found that the British employment index tended to be insignificant although this may have been due to multi-collinearity between income and money supply variables since Ridpath included the latter in his regressions (unlike the present study).
42. Ibid. See Tables D7(a) and D7(b), pp. 363-4, for the interest rate data and pp. 145-6 for their performance in the final equations.
43. This is very much in line with Ridpath's results. See especially pp. 137-140.
44. The results of Ridpath's study would support this. He uses dummy variables to capture seasonal effects and in his C/P final equation (p. 145) he finds one seasonal dummy, S3, which covers September-November, is significant and negative, indicating that the Canadian dollar strengthened against the pound in this period.
45. Furthermore, the belief that the relaxation of Canadian protection embodied in the Canada-U.S. commercial treaty of 1935 would tend to increase Canadian demand for U.S. exports proportionately more than U.S. demand for Canadian exports, expressed above, may have been false even though it is true that: "During the 1930's Canada experienced....a growing net debit current balance of payments with the U.S." (Ridpath, 1975, p. 53). It could be argued that this was due to other factors and that changes in protection were working in the opposite direction as the "wrong" sign of T22 suggests.
46. Indeed it was felt necessary to include a Canadian speculative dummy (DC8) in the C/P estimates to allow for a sympathy movement with the U.S. dollar (of the Canadian dollar) at this time.

47. Unfortunately there are some difficulties attached to this interpretation because a Canadian dummy (DC4) included in the C/P estimates, which was intended to reflect a sympathy depreciation of the Canadian dollar, carried the wrong sign indicating that the Canadian dollar tended to appreciate against the pound (whilst the U.S. dollar depreciated as indicated by the correct sign and significance of S4 in the pound-U.S. dollar estimates). However, it may be possible to reconcile this apparent conflict by recognising that DC4 and S4 cover different periods, March-June, 1933 and April-October, 1933 respectively, and it could be that the Canadian dollar tended to move with the U.S. dollar after a lag, thereby appreciating in the March-June period but depreciating with the U.S. dollar thereafter; this could explain the insignificance of S4.

48. A negative residual implies that the actual Canadian dollar was stronger than that predicted by the model and hence indicates the presence of unexplained Canadian dollar strength. Conversely positive residuals imply unexplained Canadian dollar weakness.

49. Either in the sense of speculative dummies being insignificant because they are constructed for the wrong months or in the sense of significant dummies that should have been extended.

50. It was not possible to construct a "blanket" speculative variable based on forward exchange rates and interest rates because of lack of data.


The last three chapters have attempted to test the hypothesis that exchange rates were relatively "stable" (or at least not violently "unstable") in the sense of being determined by "economic fundamentals" for fourteen bi-lateral exchange rates (involving seven currencies) which can be said to have been "floating" in some sense for part, if not most, of the 1930's. From the onset it was clear that, in addition, speculative influences probably played some role and so these were explicitly modelled and therefore the test is also, in a fairly crude way, for the presence of "instability" as well. The purpose of the present chapter is principally to examine the performance of the different variables across all the exchange rates in which they were included to discover to what extent (if any) a given variable can be said to appear to have determined bi-lateral exchange rates generally and to look for consistency of performance of variables in estimates of different exchange rates.

The model used has been presented, on occasion, as an extended PPP hypothesis and it is encouraging (though not surprising given the results of comparable studies) to discover that relative prices stand out as the most important individual independent variable in the study. In all but one of the fourteen sets of estimates there is evidence of a significant and correctly signed relative price effect and in over half the price ratio is significant at the 5% level or less in both the WP and COL versions of the tests. The only "wrongly" signed
and significant price ratio occurs in the COL version of the
guilder-pound estimates and a rational explanation was provided
in terms of retaliatory Dutch protection and the strong Dutch
affinity for remaining on the gold standard even during sub-periods
of 1931-36 when prices were actually rising.

In general, as was expected, the WP index is the "best" price
index with relative prices in the WP versions being significant at
the 1% level and correctly signed in eleven out of the fourteen
equations and only actually failing to be significant at the 20%
level or less in one case (the guilder-pound) where the variable is
at least correctly signed. Given that WP indices are dominated by
traded goods, doubts were expressed (in chapter two) about the
validity of any tests based on WP's and an extreme view would be
that interpreting their performance here as supportive of PPP only
compounds the mistakes of earlier authors who have mainly tended to
use this type of index.

However, whilst it is true that COL indices generally perform
less well, they are still significant and correctly signed for half
the exchange rates at the 5% level or less (and at the 1% level in
two legs of the pound-dollar-franc triangle) and are significant at
the 20% level or less in nine of the fourteen exchange rates; in the
other five, they are almost significant at the 20% level (and were in
some of the earlier stage estimates) for two exchange rates and in one
case (the pound-dollar) another largely non-traded goods-based index
(wages) was significant and correctly signed at the 1% level. There
is therefore overwhelming evidence of a short run bi-lateral relative
price effect in the 1930's. This would seem to indicate some support
for the PPP hypothesis and furthermore, the short run nature of the
relative price effect highlighted does not, of course, preclude a longer term, more "traditional" PPP relationship.

Unfortunately, two other "economic fundamentals" - relative incomes and interest rate differentials - were less successful although it should be remembered that problems were always likely as these variables are based on proxies. The relative income variable is always significant in at least one version of each exchange rate but for five of the fourteen exchange rates it is "wrongly" signed. Whilst this could be optimistically interpreted as indicating the existence of the expected relative income effect for two-thirds of the exchange rates the "wrong" signs require some comment.

One possibility is to invoke the monetary approach to exchange rate determination: instead of people reacting to an increase in income by buying more goods, including imports (the "traditional" approach), they attempt to hold more money, some of which they obtain from abroad by selling more goods (increasing exports). If the income ratio were always "wrongly" signed this might be satisfactory but it is not. However, it is possible to argue that at different times in different places the reaction to an increase in income might have differed and this possibility might be explored. In this connection it is worth observing that the "wrong" signs occur (with one exception) in exchange rates involving the guilder or the French franc; in fact, these currencies are more often than not associated with "wrongly" signed bi-lateral income ratios (two out of three and three out of five occasions, respectively).(1) It might therefore be possible to argue that a "monetary approach" to the relative income effect is suitable for the guilder and the franc but a "traditional approach" should be used for the other currencies in the study, especially the dollar.
This would suggest that the relative income effect is extremely important as it is significant in all fourteen exchange rates. However, this attempt to argue that significance of the income ratio - whatever its sign - supports the hypothesis of "stable" exchange rates seems rather dubious. An alternative explanation would be to notice that the "wrong" signs occur either in guilder exchange rates or in exchange rates where there have been problems with the income proxy and an alternative has had to be used (or in the Swiss case is unavailable). Dutch protection and extreme gold standard orthodoxy have already been used to explain perverse relative price effects and this argument may also be applicable to relative income effects; it might also be extended to Swiss exchange rates (although the relative price effects were not distorted). The other "wrong" signs can be explained in terms of poor choice of income proxy; it is possible that the proxy is distorted in some way or that it simply does not adequately proxy income and so it is not an "income effect" that is being picked up at all. In conclusion, it is therefore plausible to point to the fact that the relative income variable is significant and correctly signed for nine exchange rates and to interpret this as (at least tentatively) indicating the existence of the expected bi-lateral income effect.

It is less easy to argue a case for the expected bi-lateral interest rate differential effect. Half of these (six)² are simply insignificant. (In addition there are problems of inconsistency when some of these interest rate differentials are used as ROW variables; this is discussed below). However, this is not entirely unexpected and some doubts were expressed in chapter two about the importance of interest rate differentials, mainly because of the possibility
that when a capital loss was feared differences in such differentials might well become relatively unimportant. This may have led to a situation where a rise in the interest rate was interpreted as a sign of weakness and therefore led to a capital outflow and hence a perverse effect; another possible reason for a perverse effect occurred in the Swiss case since Switzerland was, to some extent, viewed as a "safe haven" for capital.

In fact, such perverse effects seem, by and large, not to have occurred; there is only one "wrongly" signed significant bi-lateral differential and on the other five occasions the variable was significant and correctly signed. This would suggest that interest rate differentials did influence exchange rates in the expected manner to some extent, especially as the insignificance in half the estimates can plausibly be attributed to an inappropriate choice of interest rate to proxy "the interest rate". This seems to be particularly true of the U.S. interest rate which was involved in four of the six insignificant bi-lateral differentials and, in fact, variables incorporating the American interest rate were only significant in one out of the five exchange rates in which they were involved.

Moving on from bi-lateral variables, there is evidence of a significant ROW "economic" effect and that the use of a third country to proxy the ROW has been reasonably successful with the ROW price or income variable (or both) being significant in at least one version of each exchange rate equation. As with the ratio variables it is prices more so than income which seem to be important, although this may of course reflect the inadequacies of the income proxies; indeed, proxies based on data other than employment indices are used on several occasions. This argument is likely to be particularly
relevant with regard to the "invisibles effect" (discussed in Chapter 4, Section II) for which a more comprehensive ROW income proxy is probably required than one based on a single country.

ROW interest rate differentials have a less obvious effect and, on the occasions that they are significant, their sign is often inconsistent with the one they carry in estimates of other exchange rates. A variety of explanations were suggested, most of which were rather unsatisfactory. Perhaps it is best remembered that not only are all the problems associated with the bi-lateral interest rate differentials applicable (discussed above) but the ROW differential is effectively a proxy based on two other proxies.⁴

To some extent the performance of the ID's can also be interpreted in terms of the inadequacy of the proxies used: where ID's were based on a number of extensive sources they performed reasonably well but where they were not, they were insignificant. This latter problem occurred for all the minor gold bloc currency ID's for which information was scant and the number of months in which they took a non-zero value was small. The probability is that such variables inadequately represented official intervention and, indeed, the presence of unreported intervention was used (with some plausibility) to explain some of the residuals.

Given these data deficiencies, the only real test for the presence of official intervention therefore occurred in the estimates of the pound-dollar-franc triangle and the results there were very encouraging: the pound-dollar ID and franc-dollar ID⁵ were significant and correctly signed in the pound-dollar and franc-dollar estimates and, although the pound-franc ID was insignificant in the OLSQ estimates of the pound-franc,
it is worth noting that this too became significant (and correctly signed) in the CORC estimates. As ROW variables these ID's achieved, perhaps not surprisingly more limited success but, on the few occasions they were significant, they were plausibly signed. Thus there is fairly strong evidence to the effect that official intervention was an important influence on exchange rates in the 1930's although it should, of course, be observed that significance of these variables does not indicate anything about whether such intervention was stabilising, merely that it was effective.

On balance, the seasonal variables also perform quite well. Their unconventional nature - they are sine and cosine waves as opposed to dummy variables - might have caused problems but the incidence of significant seasonal variables with more than one peak and trough is, in fact, fairly small. In most cases there was evidence of a smooth and, where there were any expectations (particularly the pound and Swiss franc), plausible seasonal variation in the exchange rate. The trend variable is also mainly significant and indicates a tendency for the pound and the "minor" gold bloc currencies to strengthen over time whilst the French franc and U.S. dollar weakened. To some extent this is surprising as the only expectation about this variable was that the pound and other European currencies might tend to weaken against the U.S. dollar over time due to increasing fears of war; indeed, this argument may actually apply to the U.S.-Canadian dollar estimates (since Canada was part of the Empire), the one currency against which the U.S. dollar did strengthen. However, from the beginning, two other possibilities were suggested - changes in productivity (which has been applied to the pound in Chapter 5) and simply changes in tastes - and two further arguments were introduced.
in the discussion of the empirical tests; firstly, the trend variable may be picking up the effects of protection (which caused a "change in tastes") and secondly, the French franc did tend to move only in one direction (depreciate) and the trend may reflect this.

(II)

On balance it seems reasonable to conclude that "economic fundamentals" did play a major role in exchange rate determination in the 1930's. Nevertheless, it is also clear that there was an important speculative effect in the sense that many of the speculative dummies were significant. Their performance was examined in detail and they were checked for consistency of performance across exchange rates (as indeed were the "economic fundamentals") in the preceding chapters. As far as the four "minor" currencies were concerned speculative effects due to events in these "small" countries seem to have been of limited importance in the case of Holland and Switzerland. However, this is less true of Belgium and Canada and, in any case, a number of British, American and French dummies were significant in the exchange rates of all four "minor" currencies which suggests that speculative effects, in general, were of some importance; this conclusion is strengthened, to some extent, by the existence of occasionally large residuals. Finally, there was no inconsistency of performance of the speculative dummies of Canada and the three smaller gold bloc countries.

The dummies relating to the three "major" currencies were also discussed earlier but only in the context of the pound-dollar-franc
triangle. The performance of these variables across all the exchange rate regressions in which they were included therefore requires discussion and is summarized in Table 8.1. The most striking feature of this comparison is the consistency of performance of the British and American variables. It is clear that the influences expected to cause the pound to depreciate in 1931 (K1) and mid-1934 (K3) were important but this was not true in 1935 (K4), although it should be pointed out that in the latter case the expectations were weaker, as the weaker pound largely reflected the stronger dollar due to the gold clause judgement. The insignificance of K1 and K3 in the pound-franc estimates was due to "misting" and overlap with a significant (and similarly signed) French dummy (F3), respectively. There is also limited evidence to support the contention of a speculative appreciation of the pound in early 1933. (8)

The most obvious conclusion to be derived from the performance of the American dummies is that the dollar was clearly undervalued in the 1933-34 period (S4 and S5) (9) which was widely suggested by many sources and, furthermore, that this undervaluation continued into 1935 (S6); indeed, the residuals of some of the regressions were interpreted as possibly indicating that it continued well into 1935. All the other American dummies were significant and plausibly signed to a greater or lesser degree. This was especially true of S8 and S9 which only failed to show up in the dollar-belga estimates. (10) Thus the gold and dollar "scare" of 1937 appear to have induced an appreciable speculative effect. The effects of S2 and S7 seem only to have occurred in non-gold bloc and gold bloc exchange rates against the dollar respectively; in the latter case this is unsurprising and it was conceded during the construction of the American dummies that
### Table 8.1: Summary of the Performance of the Major Speculative Dummies Across All Exchange Rates

<table>
<thead>
<tr>
<th>Exchange Rate of Pound Against</th>
<th>Pound Expected To</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pound</td>
</tr>
<tr>
<td></td>
<td>U.S. Dollar</td>
</tr>
<tr>
<td></td>
<td>French Franc</td>
</tr>
<tr>
<td></td>
<td>Canadian Dollar</td>
</tr>
<tr>
<td></td>
<td>Belg.</td>
</tr>
<tr>
<td></td>
<td>Guilder</td>
</tr>
<tr>
<td></td>
<td>Swiss Franc</td>
</tr>
</tbody>
</table>

#### Key
- **D**: Depreciate
- **A**: Appreciate
- **X**: None
- **n.a.**: Not applicable

<table>
<thead>
<tr>
<th>Exchange Rate of Dollar Against</th>
<th>Dollar Expected To</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pound</td>
</tr>
<tr>
<td></td>
<td>U.S. Dollar</td>
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<tr>
<td></td>
<td>French Franc</td>
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<td></td>
<td>Canadian Dollar</td>
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<td>Guilder</td>
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<td></td>
<td>Swiss Franc</td>
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<table>
<thead>
<tr>
<th>Exchange Rate of Franc Against</th>
<th>Franc Expected To</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pound</td>
</tr>
<tr>
<td></td>
<td>U.S. Dollar</td>
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<td></td>
<td>French Franc</td>
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<tr>
<td></td>
<td>Canadian Dollar</td>
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<td>Belg.</td>
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<tr>
<td></td>
<td>Guilder</td>
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<tr>
<td></td>
<td>Swiss Franc</td>
</tr>
<tr>
<td>EXCHANGE RATE OF POUND AGAINST</td>
<td>EXCHANGE RATE OF DOLLAR AGAINST</td>
</tr>
<tr>
<td>--------------------------------</td>
<td>---------------------------------</td>
</tr>
<tr>
<td>F1 (Nov.-Dec. 32)</td>
<td>D</td>
</tr>
<tr>
<td>F2 (Nov.-Dec. 33)</td>
<td>D</td>
</tr>
<tr>
<td>F3 (Mar.-Oct. 34)</td>
<td>A</td>
</tr>
<tr>
<td>F4 (Apr.-May 35)</td>
<td>D</td>
</tr>
<tr>
<td>F5 (June-Oct. 35)</td>
<td>? (D)</td>
</tr>
<tr>
<td>F6 (Nov. 35)</td>
<td>D</td>
</tr>
<tr>
<td>F7 (Jan.-Mar. 36)</td>
<td>D</td>
</tr>
<tr>
<td>F8 (June-Sept. 36)</td>
<td>D</td>
</tr>
<tr>
<td>F9 (Oct.-Dec. 36)</td>
<td>? (D)</td>
</tr>
<tr>
<td>F10 (Jan-Feb. 37)</td>
<td>D</td>
</tr>
<tr>
<td>F11 (Mar.-June 37)</td>
<td>? (D)</td>
</tr>
<tr>
<td>F12 (July-Oct. 37)</td>
<td>D</td>
</tr>
</tbody>
</table>
the "hot money" flows represented by S7 were, to some extent, a gold bloc phenomenon.

The remaining three dummies are less straightforward. S1 is never significant but some of the residuals suggested that this was mainly due to "mistiming" (particularly in the relationship of the U.S. dollar with the French franc and Canadian dollar): the main impact of the shift of speculative attention from the pound to the dollar in late 1931 seems to have been felt in November rather than October. "Mistiming" may also be a problem with S10, especially in the pound-dollar-franc triangle. Similarly the "wrong" sign of S10 in the dollar exchange rates with the Canadian dollar and Swiss franc (and also its insignificance in the Belgian case) may reflect a tendency for these currencies to move in sympathy with the dollar for part of the period that it covers. (11) Finally, S3 is mainly significant but "wrongly" signed. This covers the period in 1932-33 between Roosevelt's election and his taking office and there was some doubt about its expected sign in the first place. The conclusion reached in the earlier discussion of the empirical results seems quite reasonable: the sign of S3 probably reflected a combination of a capital inflow due to confidence engendered by Roosevelt's election and (more likely) official intervention to maintain the value of the dollar in the period immediately preceding its devaluation.

Of the three groups, the speculative dummies that perform least well are those of France; in particular there are several cases of unexpected and apparently inconsistent signs. Two variable - F1 and F2 - are never significant and F4 is significant only once (in the belga-franc estimates) where it is "wrongly" signed. However, this "wrong" sign, it was argued, is due to a belga undervaluation against
the franc in the period between the Belgian and French devaluations (April, 1935–September, 1936) which also explains the apparently inconsistent signs of F5, F6, F7 and possibly F8 in the belga-franc estimates. However, the apparent failure of any speculative weakness of the franc to show up in other French exchange rates following the Belgian devaluation is surprising, although it may reflect a mistiming of F4. In any case, F5 and, to a lesser extent, F6 do indicate that the franc weakened later in 1935; indeed, the performance of F5 (ignoring the belga) could be interpreted as reflecting both a lack of confidence in the success of the deflation undertaken by the Laval administration (whose policy it represents) and a continuation of doubts about the franc due to the Belgian devaluation.

Three variables, F3, F6 and F12, are significant and correctly signed in some franc exchange rates whilst F7 is ambiguous; its "wrong" sign has already been discussed in the belga-franc context and it may be that the overvaluation of the franc implied there also applied to the franc-guilder exchange rate, or that there was official intervention which, since it would be required to maintain any overvaluation, amounts to the same thing. This leaves F8, F9, F10 and F11 which are "wrongly" signed being associated with a strong franc except in the case of the dollar and the belga. In the dollar-franc estimates this apparent inconsistency, it was suggested, was caused by the fact that these variables overlapped with American dummies and were, in fact, picking up (plausibly signed) American effects; thus these French variables were, as in the Belgian case, really insignificant.

Ozmun (1976) had similar problems and suggested that the "Keynesian flavour" of the Blum experiment may have increased confidence and attracted capital. This was dismissed as being unlikely, particularly in the Dutch case, and the only plausible explanation that remained
was that the franc was receiving heavy official support in the mid-1936 to mid-1937 period because it was overvalued (even after its devaluation). Such an explanation is supported by the fact that the franc depreciated substantially further in mid-1937.

In general, a sufficient number of the speculative dummies (of all seven countries) are significant and consistently signed to suggest that speculative effects had an important (though not overwhelming, as Nurkse would have it) role in exchange rate determination in the 1930's. However, this conclusion requires heavy qualification. In the first place, the modelling of speculation has proved difficult and the use of dummy variables has not been entirely satisfactory in numerous respects; in particular an "averaging" problem has often occurred (and is apparent from the examination of residuals) due to the inability of a constant value dummy variable to pick up an influence of differing degrees of intensity in different months. Nevertheless, attempts to model speculation by use of a proxy variable based on forward market data were rather less successful and are, in any case, subject to theoretical objections. Furthermore the significant speculative dummies are, in some cases, not picking up speculative influences at all but are "wrongly" signed and it was often argued that they were picking up official intervention aimed at offsetting the speculation or, particularly in the case of the dollar in 1933-35, their significance represented an over- or under-valuation of a currency (which amounts to the same thing). In this sense, the significance of a speculative dummy is not indicative of effective speculation but quite the opposite - that is, successful official intervention.

Another qualification relates to the question as to whether
the significance of dummies picking up capital flows due to speculation (as opposed to official intervention) is really indicative of de-stabilising speculation. (15) To the extent that speculators were acting on the basis of expectations that were correct (in the sense that their behaviour was likely to push the exchange rate towards the level it should have been at, given the values of the "economic fundamentals") and the level of the exchange rate was being artificially maintained by the authorities, then the speculation was stabilising rather than destabilising. Even so, having made these qualifications, to the extent that unexplained exchange rate variations and low Durbin Watson statistics still exist, this may indicate the presence of unmodelled and (genuinely) destabilising speculation. Nevertheless, these qualifications about the speculative dummies would seem to turn the balance of the interpretation of the regressions to the conclusion that the most important role in exchange rate determination in the 1930's was played by "economic fundamentals"; it would also indicate that more importance should be attached to official intervention than is suggested by the performance of the ID's.

A final "variable" that is probably of importance is protection; this may also weaken the above argument that large residuals may imply destabilising speculation since trade controls constitute another omitted variable and hence they may account for some of the unexplained variation in exchange rates. It is virtually impossible to quantify protection and the only attempt to do so (in the Canadian case) was rather unsuccessful. But quite apart from the residuals, it was specifically concluded on two occasions (for Holland and Canada) that protection played some role in influencing the flow of goods and services and hence currency flows and the exchange rate. It was
also suggested (above) that significant trend variables may have
represented the effects of protection. Whilst this may lessen the
role of "economic fundamentals" in exchange rate determination in
the 1930's, it certainly does not strengthen the Nurkse view of the
predominance of destabilising speculation.

(III)

Having reviewed the performance of individual variables across
different exchange rates, it might be useful to briefly consider the
success of the model on an individual currency basis. In general, with
the possible exception of the guilder, the model can be said to have
satisfactorily "explained" much of the variation of all seven currencies.
The model is perhaps most applicable to the U.S. dollar and especially
so in its relationship with the four "minor" currencies. Although
it is true that the dollar did go through a sustained period of
(deliberately engineered) undervaluation, its underlying trend does
seem to have largely followed the path indicated by "economic
fundamentals" with a slight interruption due to the gold and dollar
"scarens" in the latter part of the decade.

The model performed slightly less well in the case of the pound
although even here the importance of speculative effects (in the shape
of significant speculative dummies) was very limited and the source
of deviations from the course indicated by "economic fundamentals"
appears more likely to have emanated from the EEA than destabilising
speculation. In the case of the French franc, a relatively large
number of speculative dummies were significant but the extent to
which these represented genuinely destabilising speculation is suspect:
the franc seems to have undergone periods of overvaluation but the highly significant relative price variables – whether WP or COL indices are used – would indicate the underlying importance of "economic fundamentals". Given the political instability and uncertainty in France in this period, this is probably rather more than could have been expected and the performance of the model in successfully "explaining" French franc variation is perhaps particularly noteworthy.

The smaller gold bloc currencies were expected to be less suited to the model and this did turn out to be the case. However, with the exception of the guilder whose path, particularly against the pound, was distorted by protection and extreme gold standard orthodoxy, these currencies did, by and large, follow the course indicated by "economic fundamentals", especially in their relationship with the dollar. Finally, movements in the Canadian dollar were satisfactorily "explained" by the model, especially in view of the fact that it was expected that the role of "economic fundamentals" may have been diminished because of Canadian protectionism and various other factors.

Two final tasks remain: firstly, to weigh up the evidence as to the relative importance of "economic" and "speculative" effects and, secondly, to consider the results presented here in the context of other studies examined in chapter three. A fairly crude attempt was made to compare the relative importance of "economic" and "speculative" influences in the examination of the pound-dollar-franc triangle using F-tests. Whilst this perhaps indicated that "economic fundamentals" were more significant (in the sense of having a higher F-statistics), a more considered interpretation would probably be that both were important. In any case, such an exercise is made rather
tenuous by the fact that whereas the group of "economic fundamentals" is fairly comprehensive, this is not true of the speculative variables which probably only include part of the speculative effect. On the other hand, the discussion of the speculative dummies (above) did suggest that on numerous occasions when these variables were significant, they did not necessarily indicate the presence of speculation and, where they did, it was not necessarily of the destabilising variety. In addition alternative explanations (to speculation) of residuals were found in many cases. On balance, therefore, it seems reasonable to conclude that, although destabilising speculation may have occurred on some occasions in the 1930's, it was certainly not dominant and a greater role in exchange rate determination should be given to "economic fundamentals" and official intervention.

Finally, the results and conclusions presented here are in broad agreement with those in the studies of Hudgins (1973), Ozmun (1976) and Ridpath (1975), which constitute the only comprehensive empirical studies of exchange rates in this period that have been found. Whilst their agreement with the present study is perhaps to be expected, given the similar methodology, the much wider range of the model developed and tested here, in terms of bi-lateral exchange rates examined and variables used (particularly the use of COL indices), would suggest that interpretations of exchange rate movements in the 1930's would perhaps be improved by reference to these studies and the present one, rather than to the assertions of Nurkse (1944) and others. In a wider context the results presented here would indicate that the use of the 1930's experience as a good example of the inherent problems involved in adopting a system of floating exchange rates is erroneous. Furthermore, this issue cannot be dealt with satisfactorily without an examination of multilateral exchange rates which is undertaken in the next section.
FOOTNOTES TO CHAPTER 8

1. Where they are not, either the level of significance is low (belga-franc) or the other currency involved in the exchange rate is the dollar (dollar-franc and guilder-dollar), and in eleven out of the twelve equations involving dollar exchange rates the relative income ratio is significant and correctly signed.

2. No Canadian interest rate is available.

3. The problems are discussed in detail in Chapter 4, Section III.

4. This is also true of the ROW income variable.

5. It must be conceded that the "wide" version was insignificant and it is the "narrow" version that is included in the final regression.

6. That is, variables that suggest two or more periods of seasonal strength and two or more of seasonal weakness in each year.

7. In fact, Ridpath (1975, p. 143) does find a "war scare dummy" significant and correctly signed in his estimates of the pound-Canadian dollar exchange rate but this variable is only operative after Hitler's annexation of Austria in March, 1938 and is of constant value. Therefore, it is rather different to the trend variable used here.

8. The "wrong" sign of K2 in the pound-Canadian dollar estimates is probably caused by an overlap with DC3, a Canadian dummy.

9. The insignificance of S8 in the U.S.-Canadian dollar estimates is probably explained by the depreciation of the Canadian dollar in 1933.

10. The non-applicability of S8 in the dollar-franc estimates was due to overlap with two French dummies (F10 and F11), the significance, coefficients and signs of which (in the dollar-franc estimates), were interpreted as indicating that they were actually picking up not French influences (which they represented) but the American influences associated with S8. Hence S8 was also significant, in this sense, in the dollar-franc estimates.

11. Indeed, in the Swiss case a dummy variable (SW10) was constructed specifically to capture this sympathy movement and was, in fact, significant in the Swiss exchange rate against the pound and the franc. (It was excluded from the Swiss franc-dollar estimates).

12. The obvious solution is to assign different monthly values to proxies covering a number of months. However, not only is it often difficult to do this because of lack of information, but it would be a highly subjective exercise anyway, which is probably best avoided.

13. See Chapter 4, Section IV.

14. It must be conceded that this conclusion is, on occasion, based solely on one possible interpretation of the regression results. However, in other cases, the dummy variable coincides with an observation in one of the ID's (based on actual evidence of intervention).

15. This was discussed in Chapter 4, Section IV.
SECTION III: MULTILATERAL EXCHANGE RATES
A detailed examination of bi-lateral exchange rates must provide an integral component of any study of exchange rate movements in the 1930's. However, in the 1970's, following the collapse of the Bretton Woods adjustable peg system, it has become widely accepted that, in a period of floating exchange rates, any individual bi-lateral rate can only partially reflect the overall movement of any particular currency. Consequently, a new concept - the effective or multilateral exchange rate - has been developed, which seeks to take account of the movements of a currency against all others in a single measure. The importance of this indicator in the 1970's has been acknowledged to the extent that the Bank of England now pays more attention to the pound's effective exchange rate than to any individual bi-lateral rate. This section of the present study seeks to apply this concept to the 1930's and this chapter begins the exercise by examining how effective rates are (and should be) calculated.

The need for such an application becomes clear when it is realised that, in the 1930's, the three major currencies - the pound, the dollar and the franc - followed radically different paths (particularly in the 1931-36 period). This implies that the bi-lateral exchange rates of these (not to mention other) currencies, four of which are included here, inevitably follow a different path according to the choice of numeraire currency. When allowance is
also made for "minor" currencies and therefore other bi-lateral rates the necessity of examining effective exchange rates in this period is self-evident. Examples of differences in bi-lateral exchange rate movements for all seven currencies are provided in Table 9.1. Thus, for example, the U.S. dollar depreciated 40.8% against the French franc in the period from January 1931 to January 1936 whilst depreciating only 17.7% against the Belgian franc and 2.1% against the pound, appreciating 20.6% against the Danish krone and 1.9% against the Australian pound and so on.

Clearly therefore, the basic rationale involved in calculating an effective exchange rate (EER) is to provide an average (in some sense) of the movements of the currency concerned against all other currencies which allows for the fact that bi-lateral exchange rate movements against different individual currencies may differ in degree and even in direction. Unfortunately, moving from this general definition to a more precise definition of an EER creates various difficulties and rather depends on the purpose for which it is being calculated. In other words there is presumably an interest in the effect of exchange rate changes on some variable or policy objective and the exact definition of an effective exchange rate (and its weights) is determined by the variable or policy objective which is chosen as being of primary interest or importance. A fairly obvious choice would be the competitiveness of exports which would imply that the effective exchange rate of, for example, the dollar would be defined as follows: a change in the dollar's effective exchange rate in a given period should imply the same change in U.S. export competitiveness that actually did occur as a consequence of all the exchange rate changes that actually did take place. If the
TABLE 9.1: PERCENTAGE CHANGE IN EXCHANGE RATES (Jan. 1931 to Jan. 1936) (1)

<table>
<thead>
<tr>
<th>Appreciation (+) against that of:</th>
<th>U.S.A.</th>
<th>Canada</th>
<th>France</th>
<th>Belgium</th>
<th>Holland</th>
<th>Switz.</th>
<th>U.K.</th>
<th>Argentina</th>
<th>Denmark</th>
<th>Spain</th>
<th>Australia</th>
<th>Composite Effective Rate</th>
</tr>
</thead>
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<td>-17.7</td>
<td>-41.0</td>
<td>-40.7</td>
<td>-2.1</td>
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<td>+20.6</td>
<td>-24.3</td>
<td>(+1.9)</td>
<td>-7.5</td>
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<tr>
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<td>+67.9</td>
<td>-</td>
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<td>+65.4</td>
<td>(+73.7)</td>
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Note: (1) Figures in parenthesis are based on cross-rates.
variable of interest were the cost of imports (in national currency) then the effective exchange rate would have to be redefined with the words "export competitiveness" replaced by "cost of imports" and this would imply a different set of weights.

However, whilst such definitions are appropriate when specific variables are of interest, a more general definition would relate the EER to a more generally important variable and the most obvious is the balance of payments, although there are still some problems with this in that it implicitly involves defining "the balance of payments": is it the trade balance, current account, basic balance, total currency flow or something else? In fact most EER calculations focus on the current account and ignore the relationship between exchange rate changes and capital flows; indeed most go even further and concentrate only on merchandise trade, thereby also ignoring invisible trade. This tendency to concentrate on the merchandise trade balance is usually justified in terms of its usefulness in providing some indicator of changes in competitiveness due to exchange rate movements.

Nevertheless, it is important to realise that insofar as an effective exchange rate concentrates on the merchandise trade balance and yet is treated as if calculated in relation to the balance of payments as a whole, it is inaccurate or, at best, an approximation and the weights derived from merchandise trade flows are strictly speaking incorrect (no matter how sophisticated the manner in which they are calculated). For this reason the use of a complex model - along the lines of the IMF's multilateral exchange rate model (MERM), for example - to calculate effective exchange rates, whilst it may provide a sounder basis for calculating weights derived from merchandise
trade flows than simpler methods, still suffers from the fundamental weakness that it ignores invisible trade and (especially) capital flows, and its application is therefore strictly limited. This is not to say that those who calculate EER's from MERM are unaware of these difficulties but rather to imply that in comparing MERM - type weighting with much simpler weighting schemes such limitations should always be remembered.

Having recognised the conceptual necessity of relating an EER to the balance of payments as a whole and defining a change in a country's EER as the proportionate change in the value of its currency against all the other currencies (expressed as an index) that would have had the same effect on its balance of payments as all the changes in its bi-lateral exchange rates that actually did occur, there is, unfortunately, no way in which this can be put into practice for the 1930's. The reason for this relates mainly to the lack of any suitable data. Whilst data on invisible trade and capital flows are available for some countries, they are unavailable, inaccessible or unreliable for many others and, consequently, to use a weighting scheme based on the whole balance of payments would reduce the countries included to an unacceptably small number. Therefore for purely technical reasons merchandise trade flows only will be used as a basis for the weighting scheme. However the weights will be varied and a number of EER's calculated for each country and so the possibility of invisibles and capital flows having some effect on the weights will be allowed for in a sense (although the effective rates presented must remain in principle subject to the limitations discussed above).

This means that some criteria are required to determine how
exactly the weights should be varied and what has been done is to
use trade flows for three different years which it was found gave a
fairly large variance in the size of the weights, probably enough to
allow for the effects of invisibles and capital flows. The remaining
question to be answered is whether a simple or MERM-type weighting
scheme should be used and the likelihood is that the large amount of
extra work involved in calculating weights based on the latter may
well not be worthwhile since the extra degree of accuracy may be
spurious as neither weighting scheme avoids these more fundamental
objections. Consequently a simple weighting scheme with fairly
widely varied weights seems more appropriate. The choice of weights
is obviously of crucial importance and is taken up again later.

(II)

Having defined the effective exchange rate (and noted the
limitations implied by the definition) the next step is to deal with
three technical aspects of the calculation: these are the selection
of the countries to be included, the choice of base year and the
precise form of weighting scheme (having already opted for weights
based on merchandise trade flows). If it were possible to include
all countries then this would obviate the need to choose countries
for inclusion. However it is not feasible because of lack of
complete data (certainly for the 1930's) nor is it necessarily
desirable. For example if the main interest is the effect of exchange
rate fluctuations on international competitiveness then the countries
included should compete with each other to a significant extent and
the inclusion of countries that are not competitors may be actually
misleading.
More importantly a small open economy may well find itself a price-taker as far as its exports are concerned with prices denominated in a foreign currency and, in the case of many developing countries, its supply of exports may be very inelastic; consequently an exchange rate change of this small country may have little effect on the trade balance of other countries since the price of its exports (denominated in foreign currency) will stay approximately the same as the exchange rate change will be largely reflected in changes in export prices in local currency. Unfortunately a simple trade share weighting scheme implies that price elasticities of demand for all goods in all countries are equal which is obviously incorrect in this context whilst a weighting scheme based on trade balance effects of exchange rate changes (like the MERM-weights) would have to calculate elasticities (which may not be easy) or make assumptions about them. For these reasons it may be better actually to exclude some countries - probably the developing countries - and concentrate on industrial countries where, if the former weighting scheme is used the assumption of equal price elasticities is more plausible and if the latter is used reliable estimates of price elasticities of different countries are more likely to be readily available.

The present calculation largely ignores these difficulties and is clearly, therefore, subject to criticism on the above grounds. However, it was felt that some of the more questionable countries included were so important in terms of trade flows that, in the absence of any conclusive evidence that they did adjust their export prices in local currency and maintain them at the same level in foreign currency, their large share of trade (particularly bi-lateral trade with some of the countries whose effective exchange rates are being
calculated) justified their inclusion. In any case observed changes in export prices in local currency may not be entirely passive but may represent a deliberate choice to maintain export prices in foreign currency terms (presumably for reasons of competitiveness). The lack of a trade balance effect is therefore due to deliberate policy aimed at offsetting exchange rate effects which would presumably otherwise have taken place. Given that an effective exchange rate seeks to isolate exchange rate effects only, then adjusting a country's weights (or excluding it altogether) to allow for a deliberate policy would be invalid.

The position adopted by Honohan (1979) in his discussion of the treatment of agricultural and primary producers would seem to be very much in this spirit:

"The idea of modifying indices to take account of special factors, such as the response of agricultural trade to exchange-rate changes has considerable merit. However, such modifications can be multiplied to the point where the 'index' is more like the prediction of a complex model, based on many unproven assumptions, than a simple summary measure of a range of exchange rate movements. Such models will always be subject to misunderstanding and controversy...and will not attract universal acceptance. For this reason there is merit in retaining simple, albeit crude, indices...." (6)

Thus the indices presented here have merely concentrated on maximising the number of countries included subject to availability of data. This has meant that all major trading countries are included along with a large number of less important ones with the result that countries accounting for approximately 90 per cent of bi-lateral trade and three-quarters of total (global) trade flows are included. (7)

The choice of base year for an effective exchange rate index is not particularly important except insofar as there is sometimes
a tendency to treat the base period as in some sense "normal" or more specifically a period in which the exchange rate was at an equilibrium level. This is mistaken as the choice of base year is usually rather more pragmatic:

"The base date is normally chosen so as to make the index show deviations from a set of rates to which general interest attaches, say from par values in existence at the time when they were last generally observed". (8)

Thus for the 1970's the date of the Smithsonian Agreement (December 1971) or the month preceding the shift to generalised floating (March 1973) is appropriate. In the absence of anything better this convention of choosing the last period of fixed par values was followed in the present calculations and the year 1929 was chosen as the base as it represents a year in which most of the countries were still on the gold standard. (9) At a more simplistic level the calculations are of effective rates for the 1930's and so the last full year preceding the decade is an obvious choice of base. So long as the lack of significance of the base period is remembered then the period actually chosen is not important. (10)

The third technical aspect of the calculations which requires attention is the most difficult one and relates to the choice of weights. This has already been discussed and the limitations of basing the weights on merchandise trade flows only and excluding invisible trade and capital flows have been indicated. The present discussion mainly relates to the choice of the different weighting schemes that can be derived from merchandise trade flows. However, an initial difficulty relates to the choice of period from which to take data on trade flows on which the weights are to be based; this
will generally differ from the base period of the effective exchange rate itself. The practice for the calculation of current effective rates is to use recent data and periodically update this to allow for the fact that trade patterns may change over time for reasons other than changes in exchange rates and in fact the IMF has recently done this so that the weights it uses are now based on data in 1977. (11)

The problem of finding up-to-date data on trade flows is in principle easier for past periods. However, in practice the data may not exist, be incomplete or at least extremely difficult to gather for the number of countries involved. Therefore use has been made of the League of Nations' "Network of World Trade" which has complete data on visible trade flows for a large number of countries for three years: 1928, 1935 and 1938. This still leaves the question of which year to choose and it would be possible to calculate both a Laspeyres index based on all three years and a Paasche index with weights (for example) for 1930-32 based on 1928 trade flows, for 1933-36 on 1935 trade flows and for 1937-39 on 1938 trade flows.

The theory of index numbers indicates that a Laspeyres index would overstate the importance of an appreciating currency and underestimate the importance of a depreciating currency (since no allowance would be made for the changes in quantity caused by the price changes) whilst a Paasche index would do the opposite. This would suggest that some average of Laspeyres and Paasche indices would be appropriate. (12) However, the former is preferred for a number of reasons: firstly, since separate indices based on trade flows from each of the three years are calculated then a Paasche index has effectively been calculated and, by examining the range of variation between the different indices, is being implicitly compared; (13) secondly, the sole Paasche index
which is explicitly calculated (for the pound) was very similar to the Laspeyres version anyway and, indeed, the major difficulty (and difference) probably relates to reconciling bi-laterally and globally weighted indices; finally, Paasche indices would have to be based on limited data (for only three years, one of which is outside the 1930's) and would consequently be very crude. For these reasons, and also for the sake of simplicity, the weights chosen for all the main indices presented below are an average of the weights derived from all three years for which suitable data are available.

However, the most important difficulty to be overcome, having decided that the weights are to be based on visible trade flows and having chosen the years for which the trade flows are to be taken, is to decide which of the different ways of calculating weights from trade flows is to be used. The simplest and most obvious method is to use weights based on bi-lateral trade shares: for example, the weights for the dollar's EER would be calculated on the basis of the share of each country in U.S. trade (imports and exports).\(^\text{(14)}\)

Unfortunately this is open to three fundamental objections: firstly, such weights ignore competitive relations in third markets between the U.S. and other countries — for example, the U.S. may have very little bi-lateral trade with a particular country but may compete intensively with that country in the rest of the world and consequently a change in the exchange rate of the dollar against that country's currency may have extensive effects on the U.S. balance of payments and these would not be picked up by a bi-lateral trade-weighted effective exchange rate of the dollar; secondly, bi-lateral weights take no account of the different price elasticities for different countries and different goods which will imply, for example, that the effect of
changes in the exchange rate of the dollar against the currencies of two countries with identical shares in U.S. trade on the balance of payments will differ if, as is very likely, these countries have different price elasticities of demand and supply; finally, bi-lateral weights do not allow for changes in the prices of traded goods induced by changes in exchange rates which may offset the effect on the balance of payments of the original exchange rate changes.\(^{(15)}\)

The first of these objections can be overcome by basing the weights on global trade shares (country shares of total world trade) although this has the disadvantage of ignoring important bi-lateral trading relationships such as that between the U.S.A. and Canada. However an acceptable compromise might be achieved by using an average of the bi-lateral and global weights. Unfortunately this does not deal with the two latter objections. This is because such a simple trade-weighting scheme does not directly deal with the question of primary interest which is what proportionate change in the dollar's EER would imply the same effect on the U.S. balance of payments as that caused by the changes in individual bi-lateral dollar exchange rates which actually did occur. To answer this question, account must be taken of the different price and supply elasticities of different countries (and goods); this is precisely what the IMF's MERM tries to do.

This would seem to provide a good case for basing the weights on a multi-lateral exchange rate model for the 1930's. The problem is that in order to do this a great deal of information about price elasticities of different goods in different countries along with a number of other basic parameters (including supply and expenditure elasticities) are required. The task of obtaining reliable estimates
of these parameters is difficult enough for the calculation of current effective exchange rates and frequent recourse to assumptions is required where such estimates are not available. A critique of MERM (in 1976) pointed to some of the more questionable assumptions made in the model which in some ways are only more acceptable than the assumption of equal price elasticities implied by a simple trade weighted index because of the exclusion of non-industrial countries. The provision of estimates of similar parameters for the 1930's would be a Herculean if not impossible task and the actual weights derived would only be as reliable as the parameters included and consequently:

"While this approach is, for many purposes, an improvement over the simpler weighting schemes from a conceptual viewpoint, it has the practical limitation that existing techniques for estimating elasticities, as well as data problems, are such as to raise the question of whether the inevitable wide margin of uncertainty surrounding the elasticity figures offsets the advantages of the more elaborate technique for deriving the weights." (18)

This is particularly true for the 1930's. Moreover, the limitations due to ignoring invisible trade and capital flows apply with equal force to any weighting scheme based solely on merchandise trade flows.

However, the decision to use a simple weighting scheme creates its own problems: the choice between bi-lateral and global (or multi-lateral) weights is conceptually difficult since both are subject to major drawbacks. Not only do the former ignore effects in third markets but they implicitly assume that the home country (the one whose effective rate is being calculated) has the same elasticity of demand for all imports, whatever their source; whilst the latter underemphasise important bi-lateral trading links and implicitly assume identical elasticities of demand for the home country's exports in all foreign countries. (19) Thus both weighting schemes ignore
important trade effects and make unrealistic assumptions about elasticities.

Not surprisingly, therefore, different authorities favour different weighting schemes. The Federal Reserve Board apparently prefers multilateral weights in the case of the U.S. dollar partly because bi-lateral weights give an excessive (and unwarranted) importance to Canada and partly because empirical tests suggest that a multilaterally weighted index is marginally better at predicting changes in the American trade balance.\(^{(20)}\) On the other hand, the Morgan Guaranty Trust Company favours bi-lateral weights because they tend to be more stable and relate directly to the exchange rate effects on prices which actually confront buyers and sellers in each national market and also because multilateral weights inadequately reflect third market effects anyway.\(^{(21)}\)

Consequently, given the inherent weaknesses in both types of weighting scheme, it would seem plausible to argue that the choice rather depends on the purpose for which the effective rate is being calculated and indeed, possibly for which country. Unfortunately, whilst this may be true, it provides no guidance for selecting an acceptable weighting scheme to produce an approximation of a "general purpose" effective rate which can be used for all countries. The matter is further complicated by the fact that there are substantial differences between indices calculated both for the current period\(^{(22)}\) and the 1930's\(^{(23)}\) (using the different weighting schemes). Since there is no theoretical reason for preferring either (in general), as both have major drawbacks, some kind of averaging procedure seems reasonable.

This raises the question as to how exactly this should be done.
There are two obvious methods — arithmetic and geometric averaging — and a third possibility is to use the harmonic average. The main difference would seem to be that geometric (and to an even greater degree harmonic) averaging would tend to reduce the weights (compared to the arithmetic average) of countries which had a large share of bi-lateral trade but a small share of multilateral trade (or vice versa)\(^\text{(24)}\) and increase the weights of those which had similar shares of bi-lateral and multilateral trade. This may be seen as a desirable property to the extent that it increases the weights of countries which tend to be equally important in bi-lateral and global (total) trade, although it may tend to give disproportionately large weights to countries which have similar but small shares of both bi-lateral and global trade.\(^\text{(25)}\)

Partly for the above reasons, geometrically averaged weights were preferred for the pound's effective rate.\(^\text{(26)}\) This was the first to be calculated and consequently was subject to most experimentation (in the same way as the pound-dollar exchange rate was the first bi-lateral rate to be estimated and was therefore subject to most experimentation). On the basis of comparing different indices for the pound, it quickly became clear that this degree of sophistication and complication was unnecessary and so a simple arithmetic average was used for the subsequent calculations of the effective rates of the other six currencies. In any case, it is not unreasonable to argue that there are strong a priori grounds (in the shape of data deficiencies and other difficulties) for regarding this as a relatively trivial problem.

Finally, it should be remembered that the choice between bi-lateral and multi-lateral weights is, to some extent, an empirical one. Fortunately,
if it is accepted that MERM-weights are likely to be fairly accurate (given that the effective rate is being calculated with reference to the trade balance only), there are two studies whose results can be interpreted as suggesting that the "correct" weights lie somewhere between the bi-lateral and multilateral weights. Rhomberg (1976) calculates a number of different indices for a variety of currencies in the 1970's and concludes:

"...the MERM-weighted index more often than not (in 9 of 15 cases) falls between the two trade-weighted indices...It assumes the largest or smallest of the calculated index values in only 5 of the 15 countries and even then it does not deviate very much from the trade-weighted index that is closest to it". (27)

Much the same can be said about a similar study in World Financial Markets (1979). (28) In eight out of fifteen cases the MERM index lies between the bi-laterally and globally weighted indices and in five others it is within 2\% of the nearest trade-weighted index. This leaves only two countries (Denmark and Sweden) for which the MERM-index is appreciably outside (in both cases above) the range between the two similar indices. Nor is it clear that the bi-laterally and globally weighted indices are always related in a systematic way. (29) Thus, the (fairly limited) empirical evidence would suggest that, on the basis that the MERM-index is the "best" one - admittedly not an unquestionable assumption (and, indeed, its fallibility has already been indicated) but perhaps the only one available - an average of bi-lateral and multilateral weights is more likely to produce a reasonable approximation than either system used on its own.

(III)

There is one final problem which relates to the high degree of
protectionism in the 1930's. Tariffs and quotas could distort an effective exchange rate in that even if the weights are "correct" and derived from a MERM-type model which incorporated invisibles and capital flows, given exchange rate changes would not lead to the expected changes in the balance of payments and consequently weights that did not allow for trade controls would be incorrect in a sense. Unfortunately there is no totally satisfactory method of incorporating this adjustment. However, it is, in effect, partially allowed for in the following way: an increase in a country's level of protection is tantamount to an appreciation (in that it is an alternative way of relieving balance of payments pressure to depreciation) and, although its exchange rate will not change, its share of trade (both bi-lateral and global) will decline and so the country's weight in any other's effective exchange rate index will also fall; thus, in spite of the quoted exchange rate remaining the same, the total contributed to other countries' indices by the country increasing import controls will fall, thereby (to a certain extent) allowing for its higher level of protection. This provides an argument for ignoring 1928 trade flows and basing the weights on 1935 (or 1938) trade flows although, in any case, bi-lateral exchange rates are subject to the same distortion and so perhaps such adjustment is unnecessary. More fundamentally, it may be argued that an effective exchange rate should more properly be concerned with balance of payments effects due solely to exchange rate changes and adjusting weights to allow for deliberate acts of policy is inappropriate. (30)

To summarise then, the EER's calculated here can be defined as follows: a change in a country's EER equals the proportionate change in the value of its currency against all other currencies (expressed
as an index) that would have the same effect on its trade balance as all the changes in its bi-lateral exchange rates that actually did take place. The weighting system is based on data on trade flows in three years - 1928, 1935 and 1938 - contained in the League of Nations's "Network of World Trade" (1942). Specifically the weights are an average (either geometric or arithmetic) of two sets of weights: one based on the average of weights derived from bi-lateral trade flows in each of the three years, the other on the average of weights derived from global trade flows (country shares of total world trade) in each of the three years.

APPENDIX TO CHAPTER 9: THE IMF'S MULTILATERAL EXCHANGE RATE MODEL (MERM)

The MERM has been developed in the IMF's Research Department to analyse the effects of exchange rate changes on trade flows in the medium term (that is, after an adjustment period of two or three years). It is, therefore, inappropriate to use MERM to consider either the short run (one year or less) or the long run effects of such changes. One of the major applications of MERM has been to develop a weighting pattern for simultaneous changes in a number of exchange rates, in order to derive an indicator of the extent to which the external value of a given currency has changed in relation to other currencies in general; this indicator is the effective (or multilateral) exchange rate.

The MERM is continually being further developed and two versions of it have been described in the IMF Staff Papers: MERM I in
Artus and Rhomberg (1975) and MERM II in Artus and McGuirk (1981).

The critique of MERM by Dixon (1976) discussed earlier in the chapter related to MERM I (although much of it is still valid if, in some cases, to a slightly lesser extent). The description of MERM in this appendix, however, is of MERM II, the current version, and draws heavily on Artus and McGuirk (1981) who summarise it thus:

"The...theoretical structure of MERM is basically the Walrasian general equilibrium framework, simplified to a great extent by the use of input-output relationships. As far as possible, the numerical values for the various behavioural parameters were derived from econometric studies; however, emphasis was placed on maintaining the structural relationships derived from economic theory rather than modifying these relationships according to data availability. A priori judgement was used, when necessary, in the choice of parameter estimates". (31)

The MERM is based on three major assumptions: firstly, that costs and prices (in local currency) are somewhat inflexible. If this were not the case then, since costs and prices could be adjusted easily and quickly by monetary policy, exchange rate changes would not be needed to maintain balance of payments equilibrium; the assumption therefore implies that an exchange rate change will help to restore equilibrium (by changing the relative price structure).

The second assumption is that "many of the countries, in particular the industrial countries, produce differentiated goods that are faced by finite, in fact sometimes small, price elasticities of demand in world markets;"(32) this implies that changes in exchange rates, through relative price effects, cause a reallocation of demand among similar, but differentiated goods produced by different countries.

The third major assumption is that "the overall level of nominal final domestic demand can be influenced by the central authorities."(33) This allows the model to concentrate on what is believed to be more
relevant, namely the effect of exchange rate changes on the allocation of demand rather than on its overall level (in real terms).

There are eighteen countries and two groups of countries (the major oil exporters and the rest of the world) in MERM and six groups of goods (five traded and classified according to the SITC, one non-traded). Each good is both an intermediate and a final product\(^{(34)}\) and is differentiated by its country of origin creating 120 "products" \((20 \times 6)\). Each "product" has a single supply function but a separate demand function in each market (country), with the exception of the non-traded good which has only one (domestic) market, creating a total of 120 supply functions and 2,020 demand functions \((5 \text{ goods } \times 20 \text{ countries } \times 20 \text{ markets plus one non-traded good } \times 20 \text{ countries})\). Demand for each product is a function of expenditure on the good and the prices of all products. Supply of each product depends on prices in the supplying country; shift parameters are also incorporated to allow for changes in wages and indirect taxes induced by the effect of changes in exchange rates on the cost of living. The model is closed by assuming equality of supply and demand for each product and that the level of real output in each country remains constant. The various basic parameters - price elasticities, expenditure elasticities, trade shares, supply elasticities and supply shift factors - have to be either calculated, estimated or assumed for the model to be used.

A number of reservations about the suitability of MERM for calculating EER's might briefly be expressed at this point.\(^{(35)}\) Firstly, it takes no account of invisible trade and capital flows; this deficiency has already been highlighted and it must be conceded that it is equally
true of all the weighting schemes discussed (and indeed used) in the present study. Secondly, as indicated above, it is concerned with the reallocation of demand following exchange rate changes and assumes that (real) output and employment remain unchanged; short run output effects can occur because of exchange rate changes and the model ought to recognise this and its implications for the balance of payments (and hence for the weights used to compute EER's). Thirdly, the assumption that exchange rates are determined exogenously (by governments) is not acceptable, especially in the context of the present study which is largely based on the contention that exchange rates are primarily determined by prices.

Nevertheless, in spite of these problems, it might well be true (as Honohan, 1979, points out) that:

"...the weaknesses of the MERM for modelling balance-of-payments developments may not seriously impair its ability to calculate effective exchange-rate changes. It may be quite suitable for finding the unilateral depreciation which, in terms of balance-of-payments effects, is equivalent to a set of actual bilateral exchange rate changes, even if its estimates of these balance-of-payments effects may be inaccurate". (36)

if it were not for the final criticism of MERM which has already been extensively stated: the elasticities employed to give numerical content to the model are either assumed or based on limited empirical evidence. However, all calculations of EER's make assumptions about elasticities and at least MERM does this explicitly rather than implicitly. Consequently, it may be the case that, whilst less than ideal, MERM provides the best available means for calculating EER's in the current period (although its potential application to the inter-war period is doubtful).
FOOTNOTES TO CHAPTER 9

1. Effective exchange rates are always defined in terms of change. This is because the concept of an EER cannot be expressed in absolute terms. For example, in January, 1931 the pound was worth $4.855 but it is not possible to "quote" the pound's EER in a similar fashion; this can only be done in terms of how the EER has changed since an earlier period.

2. An alternative "general definition" would involve using the EER as an indicator of changes in the overall asset value of a currency. This is not explored here mainly on the grounds that it is more usual to calculate EER's with reference to balance of payments changes, presumably because this is considered to be more important. However, it should be pointed out that such an index could be calculated but that data on trade flows would not provide a suitable basis for the weights; GDP data may be most appropriate for this purpose (and indeed is used to calculate the value of the ecu, the "reserve asset" of the European Monetary System). For this reason the EER's presented here are not really suitable for this purpose and, indeed, most indices calculated for current EER's are similarly "less than ideal" (Honohan, 1979, p. 84).

3. One study - Hirsch and Higgins (1970) - concentrates solely on trade in manufactures and uses this as a basis for the weights.

4. Indeed, the limitations of the EER's calculated from the IMF's MER M are made quite clear in Artus and Rhomberg's (1975) description of MER M (p. 597). A summary of MER M is provided in the appendix to this chapter.

5. Thus, changes in a country's EER (calculated here) are more accurately defined as: the proportionate change in the value of its currency against all other currencies (expressed as an index) that would have the same effect on its trade balance as all the changes in its bi-lateral exchange rates that actually did occur.


7. The figures for the bi-lateral trade of the U.K. (86%) and the U.S. (80%) are slightly lower than for the other five countries.


9. For the pound's EER the base period is slightly different; it is the average for 1929-30. The difference arises because this calculation was undertaken (much earlier) as a separate exercise and an average of the 1929-30 period was chosen because these were the last two full years that Britain was on the gold standard. In fact this change of base makes little difference: on the basis of 1929 = 100 the pound's EER in 1930 only increased to 101.916. Therefore to rebase the pound's EER from 1929-30 = 100 to 1929 = 100 would simply involve a multiplication by 1.00958. (Consequently, a 1929-based version of the pound's EER would be approximately one percentage point higher).
10. For example, a change in the base year of the current effective exchange rate of the pound (to 1975) was announced in January, 1981. This is of little importance. The base year does, however, become important in the next chapter when multilateral PPP tests are carried out.

11. Previously, data for 1972 and, before that, 1969 were used. Indices using weights based on 1977 trade flows appeared for the first time in the January, 1981 issue of "International Financial Statistics". (This updating also applies to other aspects of the IMF's MERM).

12. Indeed, in a very early calculation of the pound's EER (not presented here), a Fisher ideal index was calculated (involving a geometric average of the Laspeyres and Paasche indices).

13. In fact, this is a more severe test for the extent to which Laspeyres and Paasche indices may vary since the latter would not have, for example, used weights based on 1928 data for 1938 and weights based on 1938 data for 1931, as is done in some of the indices calculated here.

14. Some indices use either exports or imports. However, all trade weighted indices discussed in this study are based on imports plus exports.

15. This point is particularly relevant in the case of developing countries with inelastic supply schedules and has already been discussed in this context above.


17. These include the assumptions that supply elasticities are the same for all countries and that expenditure elasticities are equal to one. This latter point received particularly severe criticism: "Expenditure elasticities are comparatively easy to measure, and there would seem to be little justification in simply assuming them all to be 1. For example, elasticities in the food group (SITC 1) have systematically been found to be less than 1." (Dixon, op.cit., p. 74). MERM is also criticised for its rigid definition of final and intermediate goods and for recognising only one type of user of the former.

18. Blackhurst and Tumlir (1980), p. 64. A similar view is taken by the Morgan Guaranty Trust Company: "Although the theoretical cogency of....MERM....is attractive, the model's empirical realism is less certain. Many assumptions and guesstimates of real world parameters are required". (World Financial Markets, 1979, p. 9).

19. Moreover, neither weighting scheme makes any allowances for differences in supply elasticities.


21. World Financial Markets (1978), p. 7. The third argument is rather a curious one since it would seem equally valid to argue that bi-lateral weights inadequately reflect bi-lateral effects (for exactly the same kind of reason).

23. See Chapter 10 below.

24. For example, a country accounting for 1% of bi-lateral (or global) trade and 9% of global (bi-lateral) trade would have an arithmetically averaged weight of 4.5, a geometric weight of 3.0 and a harmonic weight of 1.8. In practical terms, it would reduce the weights of "smaller" countries which can often be important in the bi-lateral trade of certain countries but relatively insignificant in global trade flows. Good examples are provided by colonies of many European countries and by Canada (arithmetic weight = 11.38, geometric weight = 9.36) in the case of the U.S. dollar.

25. Consider a country with 0.95% of bi-lateral and 1.05% of global trade. The arithmetic weight is 1.0 and the geometric weight is 0.999. The latter is apparently smaller but once this has been "scaled up" to compensate for the fact that geometrically averaged weights sum to less than a hundred, it will actually be larger than the arithmetically averaged weight.

26. The weights summed to less than a hundred leaving a small residual which could be allocated to the U.S. to increase the weight of that country which was felt to be desirable; this was an additional reason for adopting a geometric average.

27. Rhomberg (1976), p. 100. It should be pointed out that Rhomberg himself interprets these results rather differently: "It cannot be assumed, therefore, that one of the trade-weighted indices, or any average of them, is an acceptable substitute for the MERM-weighted index". (op.cit., p. 101). However, this comment relates to a slightly different context and assumes that finding plausible values for MERM parameters is not impractical.

28. This discussion is based on World Financial Markets (1979), Table 3, p. 7, which compares nominal EER's for March, 1979 using bi-lateral multilateral and MERM weighting schemes (March, 1973 = 100).

29. Although there is a tendency for the global index to be the higher (in eleven cases), the difference between the two simple indices varies considerably (and is often small) and the four countries for which the bi-lateral index is the higher include the U.S. and Japan.

30. This point has already been made in connection with countries deliberately maintaining prices of their exports in foreign currency (and adjusting them in local currency) in order to offset exchange rate effects.


33. Ibid.

34. This is a modification of MERM I which goes some of the way to disarming one of Dixon's (1976) criticisms referred to in footnote 17 above.

35. This paragraph and the next draws heavily on Honohan (1979), pp. 82-4. See also Dixon (1976).

CHAPTER 10

EFFECTIVE EXCHANGE RATES FOR THE 1930's

AND SOME APPLICATIONS

The calculations of the EER's proceeded as follows: for each currency twelve indices (plus a few extra in the case of the pound) were calculated. This was done for three reasons: firstly, to make full use of the available data; secondly, there was no way of deciding which set of data (available for three separate years) was most appropriate and therefore it seemed reasonable to try all possible permutations; and thirdly, varying the weights illustrated whether or not basing them solely on visible trade flows introduced a substantial degree of bias, to the extent that the variation in each country's weight was sufficiently large to accommodate any change that would be required to the visible trade based weights to allow for invisible and capital flows of a feasible size (although this is admittedly a rather tenuous method of dealing with this problem).

The twelve indices can be divided into three groups of four. Four bi-lateral indices were calculated with weights based on bi-lateral trade flows (imports plus exports) in three different years - 1928, 1935 and 1938 - and on an average of the weights derived from trade flows in these three years. Four parallel global indices used countries' shares of total (global) trade and four "average" indices used the average of the bi-lateral and global weights in 1928, 1935 and 1938 and the average of the weights used in the average bi-lateral and average global EER's (the composite index). In the case of the pound,
a Paasche index \(^{(2)}\) and two indices in which the weight of the U.S. were increased (to follow for the fact that the dollar is important for reasons other than trade \(^{(3)}\) were also calculated.

There are several other differences between the calculation for the pound and those for the other six currencies, mainly because the pound's EER was calculated first and was subject to most experimentation, in the light of which the method of calculation was modified. One difference has already been referred to in the preceding chapter and relates to the fact that the "average" in the calculation of the pound's EER is geometric whereas in the others it is arithmetic. This difference largely arises from the fact that experimentation with the pound's EER indicated that differences between variants were relatively small and consequently use of geometric averaging represented unnecessary sophistication.

Two further differences exist but are relatively minor and relate to the different choice of base period for the pound and the inclusion of one less country (28 compared to 29 for the others). The pound's EER uses 1929-30 as its base period while the later calculations use 1929 only (and in fact calculate monthly values of EER's from the beginning of 1930). However, the final (composite) version of the pound's EER can quite easily be adjusted (to make it comparable) and rebased on 1929 by simply multiplying any given value by 1.00958. \(^{(4)}\) The reason why there is one less country in the calculation of the pound's EER is that when the second EER - that for the U.S. dollar - was examined it was felt necessary to add Mexico since it had a significant share in U.S. bi-lateral trade in the 1930's (approximately 3 per cent) and Mexico was subsequently included in all EER calculations. Given the small share of Mexico in world trade in general and British
bi-lateral trade in particular (3%), it hardly seemed worthwhile to recalculate all the variants of the pound's EER to incorporate this extremely minor adjustment.

Having noted these differences, the purpose of the rest of the chapter is twofold: firstly, to examine the empirical aspects of the problem of choosing an index which can be put forward as the most representative EER of those calculated for any particular currency, which basically involves looking at the consequences of varying the weighting pattern; secondly, to undertake a very brief consideration of some of the implications of EER's in terms of a reinterpretation of some economic aspects of the 1930's.

(II)

A graphical comparison of the different types of index for the seven currencies is presented in Figs. 10.1-10.7. With the exception of the pound only the average bi-lateral and average global (and composite) indices are shown since the differences within the two "families" of indices tends to be relatively small and it is differences between indices based on the different types of weighting scheme that are important. This limited variation within the two "families" is indicated by the two additional graphs for the pound's EER's (Figs. 10.1A and 10.1B) which plot the range of variation within the families of bi-lateral and global indices. The range of variation is seen to be small with the possible exception of the lower limit of the bi-lateral indices where the point at which the pound regains its 1929-30 value is much later, and is nearer the point indicated by the global indices (1937), than that of the other bi-lateral indices.
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indices where the point at which the pound regains its 1929-30 value
is much later, and is nearer the point indicated by the global indices
(1937), than that of the other bi-lateral indices.
FIG. 10.1A  U.K. BILATERAL EER WITH RANGE OF VARIATION
FIG. 10.1B U.K. GLOBAL EER WITH RANGE OF VARIATION
FIG. 10.1C U.K. EER's COMPARED
FIG. 10.2  U.S. EER's COMPARED

FIG. 10.2  U.S. EER's COMPARED

Bilateral EER
Global EER
Composite EER
FIG. 10.3 CANADIAN EER'S COMPARED
FIG. 10.4 FRENCH EER's COMPARED
FIG. 10.5  BELGIAN EER'S COMPARED

- Bilateral EER
- Global EER
- Composite EER
FIG. 10.6 DUTCH EER'S COMPARED

- Bilateral EER
- Global EER
- Composite EER
FIG. 10.7  SWISS EER's COMPARED
However, there is an appreciable difference in absolute levels between the pound's average global and average bi-lateral indices (Fig. 10.1C) in the period 1932-36, in which the latter is significantly higher. In particular, the bi-lateral index suggests that the pound had appreciated back to its 1929-30 level by mid-1934 whereas the global index indicates that the pound stayed at least 5% below its 1929-30 level (and nearly 10% below its pre-devaluation level) until as late as mid-1937. The first possibility that comes to mind is that the former may give a larger weight to the U.S.A. which devalued its currency by approximately a third in 1933-4, but this does not seem to have been the case since, in fact, it is the global index which gives the U.S.A. the larger weight. A more likely explanation is to be found in the fact that the bi-lateral system allocates higher weights to the Commonwealth countries, three of which, substantially depreciated their currencies against the pound early in 1933.

Three additional indices were also calculated for the pound: a Paasche index and two indices in which the U.S. weight was raised to take account of the possibility that the U.S. should be given a higher weight than indicated by trade shares alone because this did not adequately reflect the dollar's importance. The results are presented in Table 10.1 on an annual basis. It is clear that the Paasche index is very similar to the Laspeyres (composite) index and also that increasing the American weight (even from 20.063 to as high as 35.251) had little effect. Furthermore the point at which the pound regained its 1929-30 level is much the same for all four indices.

The remaining six countries' EER's are presented and compared
TABLE 10.1: ALTERNATIVE INDICES OF THE POUND'S EER

<table>
<thead>
<tr>
<th>Year</th>
<th>Composite (Laspeyres)</th>
<th>Paasche</th>
<th>A1</th>
<th>A2</th>
</tr>
</thead>
<tbody>
<tr>
<td>1929/30</td>
<td>100</td>
<td>100</td>
<td>100</td>
<td>100</td>
</tr>
<tr>
<td>1931</td>
<td>100.08</td>
<td>100.56</td>
<td>99.40</td>
<td>98.78</td>
</tr>
<tr>
<td>1932</td>
<td>86.75</td>
<td>86.73</td>
<td>85.29</td>
<td>83.97</td>
</tr>
<tr>
<td>1933</td>
<td>91.28</td>
<td>92.07</td>
<td>90.82</td>
<td>90.40</td>
</tr>
<tr>
<td>1934</td>
<td>95.92</td>
<td>96.36</td>
<td>96.70</td>
<td>97.39</td>
</tr>
<tr>
<td>1935</td>
<td>95.44</td>
<td>95.91</td>
<td>95.99</td>
<td>96.48</td>
</tr>
<tr>
<td>1936</td>
<td>97.51</td>
<td>97.98</td>
<td>97.99</td>
<td>98.42</td>
</tr>
<tr>
<td>1937</td>
<td>100.75</td>
<td>100.88</td>
<td>100.84</td>
<td>100.92</td>
</tr>
<tr>
<td>1938</td>
<td>105.05</td>
<td>104.57</td>
<td>104.61</td>
<td>104.21</td>
</tr>
<tr>
<td>1939(4)</td>
<td>105.35</td>
<td>104.43</td>
<td>104.43</td>
<td>103.60</td>
</tr>
</tbody>
</table>

Notes: 1. Weights for 1931-2 based on 1928 data, for 1933-6 on 1935 data and for 1937-9 on 1938 data.

2. U.S. weight = 28.056; all other weights are \(\frac{9}{10}\) of their value in the composite index.

3. U.S. weight = 35.251; all other weights are \(\frac{9}{10}\) of their value in A1.

4. January-August only.

in Figs. 10.2-10.7. Unlike the pound, it is the global index that is the highest in the case of the four gold bloc currencies (with the exception of France after the third quarter of 1937). This indicates the absence of any general tendency of either global or bi-lateral indices to be highest (which is not surprising) and, more importantly, suggests that the bi-lateral version is least variable: the multi-lateral pound was depreciated during most of the 1930's whilst the gold bloc currencies were appreciated and, what seems to emerge, is that the bi-lateral version of the former depreciated less and those
of the latter appreciated less (than the global versions). Similarly, the Canadian bi-lateral index is the least variable, rising less than the global in appreciated periods (early and late 1930's) and falling less during the depreciated period (mid-1930's). The only ambiguous case is the U.S. dollar whose bi-lateral index remains above the global index even during appreciated periods. However, a comparison of standard deviations (Table 10.2) does show the bi-lateral version was (marginally) less variable. Furthermore, Table 10.2 also gives more precise (statistical) evidence of the lesser variability of bi-lateral indices generally.

**TABLE 10.2: STANDARD DEVIATIONS OF EFFECTIVE EXCHANGE RATES**

<table>
<thead>
<tr>
<th></th>
<th>U.K.</th>
<th>U.S.</th>
<th>Canada</th>
<th>France</th>
<th>Belgium</th>
<th>Holland</th>
<th>Switzerland</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bi-lateral Index</td>
<td>5.337</td>
<td>11.928</td>
<td>2.283</td>
<td>20.769</td>
<td>12.828</td>
<td>12.368</td>
<td>12.039</td>
</tr>
</tbody>
</table>

This lesser variability is interesting in that, whilst both global and bi-lateral indices have drawbacks, an argument can be presented for preferring the latter in the 1930's. Although it has been argued (in Chapter 9) that acts of deliberate policy should be ignored and EER's should simply reflect the effect of exchange rate changes on the balance of payments, the high level of protection in the 1930's may be so important as to demand attention. Since protection, and later its relaxation, were pursued in such a way as to give (usually reciprocal)
preferential treatment to major trading partners then any exchange rate of the latter was likely to have more effect on the trade balance of a given country than a similar change by a trading country important in global terms but not bi-laterally (and therefore not a recipient of preferential treatment). Hence bi-lateral indices are more important and the fact that they exhibit lesser variability emphasises the inference (drawn below) that multi-lateral exchange rates are more "stable" than bi-lateral exchange rates in this simplistic sense.

However, such an argument is fairly tenuous and probably only applicable to the 1930's. Consequently, the most striking features of Figs. 10.1-10.7 remain the fairly large differences between bi-lateral and global indices in terms of levels and similarities in terms of trends. In the absence of any theoretical justification and on the basis of empirical evidence suggesting that the most accurate EER lies somewhere between the two, the composite index is preferred. Nevertheless, the difference in levels of the different types of index should be remembered: it is much safer to deal with trends and examine levels only insofar as a range is being considered and this range is clearly above or below something else (or at least most of it is). For this reason the composite index, when compared with bi-lateral exchange rates in Figs. 10.8-10.14, is flanked by its upper and lower limits (based on the highest and lowest values of all twelve indices calculated) and, although something will still actually be said about the levels of the composite indices, such observations should be treated with caution.

(III)

The (composite) EER's and major bi-lateral exchange rates are compared in Figs. 10.8-10.14. There are two general points which might
FIG. 10.8  U.K. BILATERAL AND EFFECTIVE EXCHANGE RATES
FIG. 10.9 U.S. BILATERAL AND EFFECTIVE EXCHANGE RATES

- Composite EER
- Upper Limit
- Lower Limit
- Pound-Dollar
- Dollar-Franc
FIG. 10.10 CANADIAN BILATERAL AND EFFECTIVE EXCHANGE RATES
FIG. 10.11  FRENCH BILATERAL AND EFFECTIVE EXCHANGE RATES
FIG. 10.12  BELGIAN BILATERAL AND EFFECTIVE EXCHANGE RATES
FIG. 10.13  DUTCH BILATERAL AND EFFECTIVE EXCHANGE RATES
FIG. 10.14  SWISS BILATERAL AND EFFECTIVE EXCHANGE RATES
usefully be made: firstly, and most obviously, the EER's of all seven currencies exhibit much less variability than the bilateral rates and secondly, six of them show a tendency to appreciate from the end of 1936 onwards, with the exception being the French franc (whose depreciation in 1936-38 obviously contributed to the appreciation of the others). A possible implication of these appreciations is that, in the absence of WW2, overvalued exchange rates may once again have become a problem. Of course, this is a fairly tentative inference, since there may be good reasons for the appreciations, but the rise in most EER's in the late 1930's is still quite striking and worthy of comment.

However, it is the other general characteristic - the lesser variability of the EER's - that is probably most important. This is because there is some tendency to treat exchange rate variability and instability as being the same thing. That this is incorrect is, of course, central to the present study and the stability (more properly defined) of EER's is taken up in the next chapter; but, if this incorrect definition of stability were accepted, then it is clear, both from an examination of Figs. 10.8-10.14 and from a comparison of the standard deviations (Table 10.3), that EER's were relatively "stable" in the 1930's compared to bilateral exchange rates and so

<table>
<thead>
<tr>
<th>EXCHANGE RATE OF AGAINST</th>
<th>U.K.</th>
<th>U.S.</th>
<th>Canada</th>
<th>France</th>
<th>Belgium</th>
<th>Holland</th>
<th>Switzerland</th>
</tr>
</thead>
</table>
Nurkse's contention of extreme exchange rate volatility (based on the latter) loses much of its force when EER's are examined.

In addition to these general points, some specific observations relating to individual currencies might be of interest. In the case of the pound, it would seem that the tendency to concentrate on the pound-dollar exchange rate has not only led to an overstatement of the variability of the pound but has also produced a sort of contradiction in discussions of the effects of the 1931 devaluation in much of the literature: while the existence of the gold bloc and the deflation required by its members to maintain the gold standard is acknowledged, the potential effects on British trade and the balance of payments, and the general exchange rate implications of these countries maintaining their currencies at the old gold parities are not brought out; the effects of the British devaluation are usually dismissed as being over by 1933. (10) However, the EER (composite index) suggests that the pound remained 4-5 per cent below its 1929-30 level and 8 per cent below its level in August, 1931 until well into 1936. Thus it would seem that concentration on the pound-dollar exchange rate has led to the benefits of freeing the exchange rate and their role in Britain's recovery being overstated in the period 1931-33 and understated in 1934-36.

The contention that the pound remained below its 1929 value is strongly supported by two other independently calculated sterling EER's that have recently become available. The three alternative indices are compared in Table 10.4. It is quite clear from the Dimsdale and Andrews indices that the pound's EER remained considerably below its 1929 level in the 1930's. However, whilst these two indices are very similar to each other, they are rather different to the composite index presented here and this requires some explanation.
### TABLE 10.4: ALTERNATIVE STERLING EFFECTIVE EXCHANGE RATES (1929–38)

<table>
<thead>
<tr>
<th></th>
<th>Redmond (Composite)</th>
<th>Dimsdale</th>
<th>Andrews</th>
</tr>
</thead>
<tbody>
<tr>
<td>1929</td>
<td>100</td>
<td>100</td>
<td>100</td>
</tr>
<tr>
<td>1930</td>
<td>101.9</td>
<td>99.6</td>
<td>100</td>
</tr>
<tr>
<td>1931</td>
<td>101.3</td>
<td>95.7</td>
<td>91</td>
</tr>
<tr>
<td>1932</td>
<td>87.7</td>
<td>75.2</td>
<td>77</td>
</tr>
<tr>
<td>1933</td>
<td>92.3</td>
<td>77.0</td>
<td>77</td>
</tr>
<tr>
<td>1934</td>
<td>97.1</td>
<td>75.4</td>
<td>81</td>
</tr>
<tr>
<td>1935</td>
<td>96.6</td>
<td>74.5</td>
<td>77</td>
</tr>
<tr>
<td>1936</td>
<td>98.7</td>
<td>77.7</td>
<td>79</td>
</tr>
<tr>
<td>1937</td>
<td>101.9</td>
<td>84.7</td>
<td>86</td>
</tr>
<tr>
<td>1938</td>
<td>106.4</td>
<td>86.9</td>
<td>91</td>
</tr>
</tbody>
</table>

Sources: (1) Redmond: the 1929 based composite index (for comparability) and not the 1929-30 version discussed in the text. (2) Dimsdale: from Dimsdale (1981) Table 3, p. 317 and Table 9, p. 333. (3) Andrews: approximate values from a graphical presentation of an index calculated by B.P.A. Andrews (Balliol College, Oxford); I am indebted to Mr. Andrews for his permission to refer to this unpublished material.

It would be possible to adopt a defensive posture and to attempt to criticise these alternative indices: Dimsdale uses multi-lateral weights thereby ignoring important bilateral relationships and his data for calculating weights is taken from only one year (1929), even though trading patterns clearly changed in the 1930’s following the shift to protectionism; Andrews includes only five countries, uses bilateral weights (and so ignores third country effects) and his use of a Paasche index can also be criticised. However, the index presented here could also be criticised on various technical grounds and to
It is quite clear that the main reason for the differences in levels between the indices is the inclusion of a large number of colonies and primary producers in the composite index, calculated here. The arguments for and against this practice have already been discussed. (12)

Ultimately the differences in Table 10.4 may well stem from the initial definitional problem of EER's: how an EER is constructed depends on its purpose. An EER required to examine changes in competitiveness (such as that of Andrews) should quite rightly be based on countries which produce mainly manufactures but in calculating a "general purpose" EER (such as that presented here) the inclusion of at least some primary producers can probably be justified. To this extent then, the EER's presented in Table 10.4 are not "competitors" but represent different "solutions" to the same "problem" (how to measure exchange rate changes multi-laterally) and it is useful to consider a range of EER's for a given country, during a given period, using different weighting schemes and different countries. It is therefore encouraging to note that the main inferences drawn from the composite index for the pound could equally well be drawn from Dimsdale's and Andrews' indices.

This is especially true of the tendency for the pound to remain below its 1929-30 level and also, to a lesser extent, of the other interesting feature of the pound's EER, namely its steady upward trend (common to composite, global and bilateral indices after 1932). This may shed some light on the widely held belief that the EEA was holding the pound down in the 1930's. (13) On the one hand, this could be taken as an indication that the EEA was simply doing its job of smoothing out the external value of the pound; on the other, it could suggest that
had not the EEA intervened then the pound would have quickly risen to its old level or above.\(^{(14)}\) Certainly there were clear motives for holding the pound down in terms of boosting the export trades and freeing Bank Rate from its external responsibilities to allow a "cheap money" policy to be pursued.

Some additional evidence is provided by an examination of the capital inflow into Britain in the 1930's. In the period 1931-37 official gold holdings increased by approximately £600 million, sterling balances increased by £350 million and there was, in aggregate, a (net) current account deficit of £270 million; this implies a capital inflow of the order of £520 million.\(^{(15)}\) At the same time, there was a total increase in new overseas issues of about £400 million\(^{(16)}\) which indicates a gross capital inflow of nearer £900 million. This large capital inflow, combined with only a moderate appreciation of the pound, does lend some support to the hypothesis that the pound was being deliberately held down. The EER does not actually prove anything in this respect but it does indicate that while the EEA was dealing in the few main currencies in which it operated, the pound, on average, was appreciating against all other currencies; thus, there was a much stronger motive for holding the pound down than is indicated by simply examining the pound-dollar exchange rate.

Thus the calculation of a sterling EER and the data presented above provide both a motive for and evidence of an official attempt to hold sterling down. However, although this is highly suggestive, it is hardly conclusive and so it may be appropriate, at this point, to digress briefly and consider more directly how the EEA's activities in the 1930's should be interpreted. This was briefly examined in chapter 4 (appendix) where neither contemporary and more recent accounts

(parge 358A follows)
of the EEA's early years nor the econometric tests of Hudgins and Ozmun (17) were found to provide any direct evidence of competitive depreciation in the sense of the EEA and another exchange fund each trying simultaneously to depress the value of its currency against that of the other. Moreover, it was further argued that, although the EEA may have sometimes deliberately prevented sterling appreciation, this was incidental rather than deliberately aggressive in that it was a bi-product of internal policy rather than a major policy in itself. (18)

The views expressed in three recent examinations of the EEA in the 1930's (19) (two of which had access to official sources) would suggest that this conclusion is perhaps a little too kind to the British authorities. While Sayers is not directly concerned with the motives behind official intervention, the impression given by his account of events is that the EEA did, on occasion, do rather more than simply smooth out trends in the pound. It is true that the pound was supported as well as depressed at certain times (20) and, indeed, there were occasions when intervention was precluded by lack of funds (21) but the general tenor of Sayers' comments appears to be accepted by Howson (both implicitly and explicitly) in the only extensive study of the EEA's activities in this period:

"...the U.K. authorities in the 1930's certainly had exchange rate targets..." (22)

"...in the mid-1930's...the authorities intervened heavily whenever the pound threatened to rise..." (23)

However, Howson's principal conclusion is rather more liberal:

"At various times...the EEA's management of sterling in the 1930's included aggressive intervention...etc...Yet of none of these episodes can it be said that the EEA would...have violated the principles recently adopted by the IMF (designed to prevent aggressive intervention)." (24)
A rather less generous view is taken by Dimsdale who, whilst accepting Howson's general position, nevertheless concludes:

"It appears, however, that in some years, such as 1932 and 1936, intervention went beyond the smoothing of market fluctuations and that a deliberate attempt was made to prevent the appreciation of sterling". (25)

Although these views expressed by Howson and Dimsdale are, on the surface, at variance, the difference between them would seem to be one of emphasis rather than substance. It is clear that intervention of a competitive nature - in the sense of preventing an appreciation of the pound that would otherwise have occurred - did take place from time to time but such activity was not as endemic as is often suggested (or seemingly was believed to be by many Americans at the time) (26) and, by any standard, to portray the EEA in its early days as primarily a device to competitively depreciate the pound seems unfair. (27)

The major implication of the EER's of the four gold bloc currencies is that they were not as overvalued in the 1931-35/6 period as their bi-lateral exchange rates against the major currencies suggests. (28) This is an interesting, although on reflection unsurprising observation. The reason why the effective rates were lower than the main bi-lateral rates is that a fairly large proportion of their trade was with each other and with colonies. (29) Nevertheless,
this point needs to be made and the appreciation of these currencies in the first half of the 1930's should clearly not be overstated. These lower effective rates would suggest that the decline in exports of the gold bloc countries should be blamed less on a high exchange rate and more on protection and furthermore that competitive depreciation was not as much a problem in the 1930's as is often implied.

In the second half of the 1930's the paths of the gold bloc countries' exchange rates diverge, particularly that of France. Nevertheless, two generally applicable observations can be made: firstly, and fairly obviously, their EER's depreciated by rather less than their major bilateral exchange rates (against the pound and the dollar) when they were finally forced to abandon their old gold parities; this is not surprising as the former take account of exchange rates with other members of the gold bloc whose currencies were also depreciating and this parallel movement is, of course, an important reason why effective rates did not appreciate so much in the first place. This smaller movement of effective rates would imply that exchange rate depreciation had a lesser role in stimulating recovery. However, it is not clear that this is the case because the second observation would seem to imply the opposite: despite their greater decline the bilateral rates still remain well above their 1929 levels and their levels immediately preceding the first major breach of the inter-war gold standard (August, 1931) and the onset of depression in Europe but the effective rates, whilst not dropping back to their earlier levels, came rather closer to doing so (especially in relation to their August, 1931 levels). Consequently, this latter point would suggest that the exchange rate did perhaps play a greater role in stimulating recovery in these countries than bilateral rates suggest.
This is particularly true of Belgium whose EER dropped to within 8% of its 1929 level when the belga was devalued in March, 1935 and, in fact, almost all the way back to its August, 1931 level (within 2%) whilst the Belgian bilateral exchange rates against the pound and the dollar remained some 20% above their August, 1931 (and 1929) levels. Switzerland also provides a good example of this: its effective rate fell to within 14% of its 1929 level in 1936 and was only 7% above its August, 1931 level compared to bilateral rates some 18-19% above their earlier levels. Moreover, although both Dutch bilateral and effective rates remained well above their earlier levels, in spite of large depreciations, the argument would seem to apply with equal force in this case also since the latter dropped within 17% of its August, 1931 level whilst the former remained more than 30% above their levels in the corresponding period. The difference between the relationships of the levels of the French effective rate and French bilateral rates after September, 1936 with those in the earlier periods is less marked but is appreciable even here: in October, 1936 the effective rate was less than 10% above its August, 1931 level and the bilateral rates 18-19% higher although the fact that both French effective and bilateral exchange rates soon fell well below their earlier levels undermines the validity of this general argument as far as France is concerned. In fact the French effective and bilateral exchange rates move fairly closely together from late 1936 onwards and the latter is, in this particular case, a reasonably good proxy for the former in the 1936-9 period.

A final point, relating mainly to the three smaller gold bloc countries, is that there was a marked upward trend in their effective rates after 1936 which was also true of the two North American effective
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A final point, relating mainly to the three smaller gold bloc countries, is that there was a marked upward trend in their effective rates after 1936 which was also true of the two North American effective
rates and has already been discussed. Thus to summarise, as far as the four gold bloc countries are concerned, the EER's indicate, firstly, that reference to bilateral exchange rates overstates the extent (and therefore influence) of the appreciation of their currencies in the 1931-35/6 period and secondly, that the latter do not show how close the exchange rates of these countries came to returning to their early 1931 (and to a lesser extent 1929) levels following their devaluations in 1935/6.

The major divergences between the U.S. dollar's EER and its exchange rate against the pound (its main bilateral rate) occur in 1931-33 and 1937-39 with it being lower in the former period and higher in the latter. As with the gold bloc currencies the lower level of the effective rate in 1931-33 would indicate that any role the exchange rate had in contributing to the depression was rather less important than the dollar-pound exchange rate suggests. Of more importance perhaps, is the relationship between the exchange rate and the recovery. It has been argued that the dollar devaluation in 1933-4 was a deliberate attempt to stimulate internal recovery on the basis of the (mistaken) belief that there was a direct relationship between the price of gold and internal commodity price levels and that if one rose so would the other. The same author goes on to argue that prices rose for different reasons and that the existence of beneficial effects of devaluation on internal recovery (in terms of prices and employment) is rather debatable. The effective rate would seem to add some measure of support to this in that the actual depreciation of the dollar in effective terms (from March, 1933 to January, 1934) was less (29.5%) than the dollar-pound rate suggests (32%). On the other hand in the 1934-6 period the effective rate maintained a value of
between 3% and 6% less than its 1929 level and as much as 11-14% below its August, 1931 level which may well have facilitated American recovery.

Finally, it is interesting to note the movement of the effective rate during the 1937-8 recession. In September, 1937 this stood at 105.2 and then rose progressively until it reached 113.3 in July, 1938. What, if any, contribution this made to the recession may be doubtful but it is nevertheless true that the possibility of the exchange rate having any effect is not made clear by referring only to the bilateral rate which remained fairly stable, at a level of 97.9 in September, 1937 and 98.5 in July, 1938 varying between 96.8 and 98.5 in this eleven month period. Moreover, whilst it is true that the effective rate continued to rise even after the upturn in the second half of 1938, this rise did not match the 7.7% increase of the September, 1937 to July, 1938 period reaching only 116.8 — a further rise of only 3.4% — in June, 1939 (although it had reached as high as 118.4 in January, 1939) with the effective rate not really taking off until July, 1939 (119.5) and August, 1939 (125.2) when the calculation becomes rather unreliable and distorted.

The most notable feature of the Canadian EER is its lack of variation (an issue which has already been discussed): all twelve versions fluctuated within a 30% band around the base level (+20% to -10%) whilst the composite index fluctuated within only 17% compared with as much as 55% for the two major bilateral rates. Perhaps a more important point, however, relates to the 1933-36 period when the EER was several percentage points below both major bilateral rates and, indeed, its 1929 level, giving Canada, a country heavily dependent on exports, a competitive edge on the rest of the world(32) until the 1937-39 appreciation (common to all countries except France).
The Canadian calculation is interesting because its provides a good opportunity to take up once again the possibility of giving the U.S. a higher weight (previously discussed in relation to the pound). At the same time, it allows an empirical test (of sorts) of the importance of capital flows since much of the Canadian capital inflow came from the U.S. (and so the two issues amount to the same thing). Canadians were acutely aware of the importance of American capital and, indeed, its large volume (and the consequent large debt repayments) were a major reason why the Canadian dollar only followed the pound part of the way in its 1931 depreciation. Thus U.S. capital flows influenced the Canadian exchange rate but causation also ran the other way since any depreciation of the Canadian dollar against the U.S. dollar encouraged U.S. capital to flow to Canada (because it became cheaper to invest there).

Both these arguments would indicate a need for a higher U.S. weight for the Canadian EER: in the first case, if the U.S. dollar-Canadian dollar exchange rate is being deliberately held back because of U.S. capital flows, then it is not changing as much as it ought and so a larger U.S. weight should be used to offset this; and, in the second case, if a depreciation of the Canadian dollar attracts U.S. capital then this improves the Canadian balance of payments and an EER calculated for general purposes should allow for this effect.

It was argued (in chapter 9) that varying the weights by calculating different indices was one method, in the face of inadequate data on international capital flows, of testing for the effect of using weights based on the complete balance of payments. The Canadian case would seem to bear out the validity of this approach while throwing light on the influence of increasing the U.S. weight at the same time.
The effect of increasing the U.S. weight is apparent in Fig. 10.4 where the average bilateral and global indices are charted; in the former the U.S. has a weight of 51.9, in the latter 12.9.\(^{(34)}\) Of course, this test is not strictly valid in that the weights of other countries differ also but it is highly suggestive: the effect of increasing the U.S. weight (and allowing for capital flows) would seem to be that the variability of the Canadian EER is considerably reduced. Furthermore, Fig. 10.11 implies that the effect of allowing for capital flows is also fairly minimal, at least in the Canadian case, as despite the extremely large variation of the U.S. weight (and of that of the U.K., the other main supplier of Canadian capital) in the twelve indices, the gap between the highest and the lowest of them is never more than twenty percentage points (and the trend is unaffected).\(^{(35)}\)

Finally, it would be useful to compare the 29 country EER's calculated and presented above with the 7 country EER's calculated for regression purposes in the next chapter. The latter could be considered as indices that exclude primary producers, although it should be remembered that they also exclude numerous industrial countries (since they were calculated with a view to the availability of price data and income proxies and whether or not the exchange rates of the included countries were "free"). However, for the sake of completeness, a comparison is made in Table 10.5.

It is clear that the two EER's for each country are broadly similar, particularly in terms of trends, for all seven currencies. This is especially true of the four gold bloc countries. The other major feature of Table 10.5 is the fact that the 7 country EER's tend to be lower in all cases (probably because they exclude primary
TABLE 10.5: COMPARISON OF 7 COUNTRY AND 29 COUNTRY EFFECTIVE EXCHANGE RATES

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producers with depreciating currencies). As far as Canada and the gold bloc countries are concerned, this gives further support to the inferences drawn from the 29 country EER: in the former case, the 7 country version indicates that the comparative edge given to Canadian imports in the 1933-36 period was somewhat larger than previously suggested; in the latter case, the argument that the gold bloc currencies were not as overvalued as indicated by the major bilateral exchange rates is reinforced (particularly for Belgium and Switzerland).

The U.K. 7 country EER, not surprisingly, lies somewhere between the 29 country version and the Dimsdale and Andrews EER's (discussed above) and, as it was concluded that the latter tended to reinforce rather than contradict the inferences drawn from the 29 country EER, no further comment is required. The U.S. country EER is also markedly lower and this would tend to emphasize the role of the exchange rate in the American recovery which has been tentatively suggested. In conclusion, the 7 country EER's would not seem to contradict any of the inferences drawn from the 29 country EER's.

This completes the examination of some of the more obvious implications of the movements of the seven EER's that were calculated. To some extent this discussion is peripheral to the main concerns of the study, but it was felt necessary to do a little more than merely present the EER's without comment. There are numerous problems involved in the calculations (discussed in chapter 9) and the EER's presented here are by no means completely free of them (although data deficiencies make some of the problems virtually unavoidable). Consequently, the results that have been presented and the conclusions derived should be treated with caution.
Nevertheless, some of the points made do carry some authority, since they can be derived from any of the twelve indices calculated for a particular country; for example, it has been argued that the main bilateral exchange rates overstate the extent of overvaluation of the four gold bloc currencies in the 1931-35/6 period, and this is apparent whichever variation of any particular country's EER is considered since the whole range of indices calculated - from the highest to the lowest values - lies below the main bilateral rates in all four cases. The next chapter returns to the central theme of the study and examines the extent to which EER's can be considered to have exhibited stability in the 1930's (in the sense of being determined by "economic fundamentals").
FOOTNOTES TO CHAPTER 10

1. The total value of world trade is derived by adding together total trade of each of the 29 countries with the other 28. (It would lead to incorrect weights if each of the 29 countries total trade with all other countries were used to calculate total world trade in this context).

2. All the other EER's presented here are Laspeyres indices.

3. Such a reason could be that the index was intended to reflect the asset value of the pound to some extent; this is a potential application of EER's that trade-weighted indices are not well suited for (see Chapter 9, n. 2) and raising the weight of the U.S. above that suggested by trade flows could be a first step in modifying trade-weighted EER's for this purpose.

4. Given the relatively small degree of this adjustment - it raises the EER by about 1% - it seems unimportant. In fact a 1929 based index was calculated (and indeed is used in the next chapter) but only the composite version. Therefore, in the context of the present chapter, it is necessary to discuss the 1929-30 based sterling EER's since all the indices using different weighting patterns, calculated for comparison, used a 1929-30 base. It would have been possible to recalculate all the different variations with a 1929 base but, as this would have had little effect on either the values of sterling EER's or on the ensuing comparisons, it did not seem worthwhile.

5. The upper (lower) limit value is the highest (lowest) value of any index of that family calculated. Therefore, it is not an index in itself since its values are not all provided by the same index. Consequently, the range of variation in the diagrams is, in a sense, overstated because no individual index deviates from the average as much as the upper and lower limits plotted in the graphs.

6. South Africa, Canada and New Zealand. (The combined bilateral rates of these countries are nearly twice as great as the combined global weights).

7. Monthly values are given in Appendix III.

8. This evidence was surveyed in the previous chapter. See Rhomberg (1976) and World Financial Markets (1978) (1979).

9. In the case of the pound this is part of a much longer period of gradual appreciation, beginning in early 1933. However, it was not until after 1936 that the pound regained and then surpassed its base year level.

10. For example, see Aldcroft (1977), p. 281 and Lewis (1949), p. 82.

11. See Chapter 9, Section II.

12. Ibid.

13. This possibility is extensively discussed in the appendix to Chapter 4.
14. This latter argument is less applicable to the Dimsdale and Andrews indices but not inapplicable; they both began a rising trend in 1935.

15. These figures are taken from Table N in the L.C.E.S.'s "The British Economy Key Statistics, 1900-1970". More recent data in Sayers (1976), Appendices, Table C, pp. 312-3 suggest a slightly lower gold and foreign currency inflow (£500 million) but larger current account deficit (£310 million). These changes do not affect the conclusions being drawn.

16. This is calculated from data given in Richardson (1967), p. 58. Aldcroft (1977), p. 264 and Sayers (1976), Appendices, p. 313 suggest a similar figure while Youngson (1968), p. 164 gives data for 1932-36 which would imply a lower figure, probably around £250 million.


18. See p. 125 above. The argument is that the British authorities did not deliberately set out to depreciate the pound to make British exports more competitive and so on but, rather, having decided on a cheap money policy to stimulate internal recovery, wished to free Bank Rate from its external responsibilities and to avoid the recessionary influence of an overvalued pound (which was perceived as a major problem in the 1925-31 period). In a sense, therefore, the downward pressure on sterling was not an attempt to undervalue the pound artificially but, rather, to prevent it from becoming overvalued.


20. For example, in July - October, 1933 (Howson, 1980, p. 1938-9 (Sayers, 1976, pp. 561-7).

21. This happened in November, 1932, as reported in Sayers (1976), p. 453.


23. Ibid., p. 27.

24. Ibid., p. 56.


27. Sayers (1976), pp. 474-5, gives an account of an instance, in late 1936, when a suggestion to engineer a sterling depreciation deliberately was discussed and rejected.

28. This mirrors the depreciated level of the pound's EER in the same period.
29. In the composite index the combined weight of the other three
gold bloc countries plus colonies was 29.02 for France, 26.48 for
Belgium, 22.02 for Holland and 19.95 for Switzerland. (In the
bilaterally weighted indices it was obviously higher: 42.29,
36.59, 27.51 and 21.98 respectively in the average bilateral
indices).

30. This is not true of Belgium where the depreciation of effective
and bilateral rates was more or less the same (26-28%) but in the
other cases - France, Holland and Switzerland - the bilateral rate
depreciations were respectively 5%, 5% and 10% (approximately)
greater than the effective rate depreciations.


32. A priori it was felt that, in the Canadian case, relative prices
were of less importance than generally expected because the major
determinant of demand for the main Canadian imports was income
levels (Chapter 7, Section II). This would indicate that this
point about extra Canadian price competitiveness is unimportant.
However, the regression results presented in Chapter 7 did suggest
that relative price effects were of some importance in Canadian
trade.

33. See Chapter 7, Section I.

34. The range of variation of the U.S. weight is actually slightly
larger when all twelve Canadian indices are taken into account:
from 11.97 to 55.16.

35. All these arguments also apply to the Canadian weight in the U.S.
EER, although capital flows between the two countries were obviously
much less important to the U.S. than to Canada.
CHAPTER 11
EFFECTIVE EXCHANGE RATES AND STABILITY

(I)

In the preceding chapters the methodology of constructing EER's was discussed, they were calculated for the seven currencies in the study and some preliminary observations were made about their implications. The present chapter seeks to place EER's in the wider context of the thesis, specifically to examine the hypothesis that, like bilateral exchange rates (to some extent at least), multilateral exchange rates were also principally determined by "economic fundamentals". In fact, what follows can only be a first approximation in that a thorough examination of this hypothesis would require not a chapter but an extensive study in itself. The conclusions drawn will therefore be preliminary and based on fairly simple tests. There are a few studies which have carried out a similar exercise but these are mainly concerned with different time periods and confine themselves to the relationship between exchange rates and prices only.\(^1\) The tests described below try and go a little further.

However, it would be useful to begin by considering the results of these other studies to examine, in particular, to what extent they found a relationship between exchange rates and relative prices. Those of Genberg (1978), Thygesen (1978) and World Financial Markets (1978, 1979) deal with recent periods and so only the more general conclusions are of interest here. Thygesen is summarizing the OPTICA Reports and strongly supports the contention that there is an important relationship between relative prices and exchange rates:
"...changes in the exchange rates of EC member currencies during the period between the early 1960's and the mid-1970's predominantly reflect inflation differentials". (2)

Interestingly enough, he also concludes that:

"Conformity to PPP was considerably closer multilaterally than bilaterally against the DM (Deutchemark)". (3)

The EER's are based on an eighteen country model and are consequently very comprehensive. On the other hand the tests used wholesale prices which are widely condemned as a biased indicator of PPP (as suggested in Chapter 3).

Genberg's results are rather less encouraging: using a fourteen country model to calculate EER's for the 1957-76 period and, (perhaps ominously) consumer prices, he finds that deviations of exchange rates were larger in the floating rate period (1973-76) and presents evidence to suggest a bias in PPP calculations using consumer prices. Specifically he discusses:

"...the possibility that there has been a shift in PPP relationships, as measured by CPI's, due to changes in relative prices of traded goods combined with intercountry differences in the weights they are accorded in national indexes. The obvious candidate for such a relative price change would be the increase in oil prices in late 1973". (4)

Nevertheless, Genberg does concede that:

"...evidence indicates that actual exchange rates are not likely to be far off their PPP levels if a simple adjustment for structural change is allowed for". (5)

He also admits that the period of floating examined may not be very representative as it comprises the first four years of floating following the collapse of the Bretton Woods system. To the extent that relative income and trend variables can pick up structural changes
and dummy variables can capture abnormal factors following the
collapse of the gold standard, the second exercise undertaken below,
involving regression analysis, makes some allowance for Genberg's
criticism.

The World Financial Markets articles do not explicitly test the
PPP hypothesis but do calculate real EER's for the 1970's, compare
these to nominal EER's and conclude:

"...it is remarkable that the floating rate system has
brought so little real effective exchange rate variation..." (6)
"the variability of effective exchange rates in real terms
is empirically much less than the variability of the nominal
indices...". (7)

This lack of variability of real effective rates clearly suggests a
strong relationship with relative prices. Two other points are of
interest: firstly, when EER's are adjusted for inflation differentials
the choice between the different possible weighting patterns for EER's
becomes less important as the differences become smaller; secondly,
real EER's adjusted with wholesale prices are more stable than those
adjusted with consumer prices. (8) In the present context, the former
point would appear to reduce the importance of a potentially awkward
problem whilst the latter reinforces the claim that it is important to
consider PPP tests using prices other than WP's (made in earlier
chapters).

The Dimsdale (1981) and Officer (1980) studies are of more
interest because the period they cover includes at least part of
the 1930's. This is particularly true of Dimsdale's real EER index
for the 1921-38 period (Table 11.1) since it uses the same base (1929)
as that chosen here and the nominal EER is adjusted by consumer (retail)
prices. Whilst the real EER still depreciates substantially in the
mid-1930's the fact that the degree of this depreciation is much reduced, along with the lesser variability of the real EER, (9) clearly suggests that there was a relationship between the multilateral exchange rate and relative prices of the type postulated here; that PPP does not hold especially well is not surprising in view of the results presented in Section II of the present study which indicated that relative prices, though important, were not the only determinant of (bilateral) exchange rates.

Officer's (1980) study is a fairly comprehensive test of the long run PPP theory for fourteen EER's, using a variety of base periods, and calculates PPP for a "current" period (1975) and two intervening periods (1938 and 1963-66). Whilst his choice of price index (GNP deflator) is methodologically sound, his EER's have a limited country coverage (between two and six). Calculations are made using up to four different base periods (as data availability permits) and six of the seven countries in the present study are included. (The exception is Belgium). The relevant results are summarized in Table 11.2. Officer's conclusions are fairly optimistic:

"For any sample and time period the average absolute percentage error is less than 30%". (10)

"Regression analysis suggests that deviations from PPP can be explained in terms of structural changes in economies". (11)
TABLE 11.2: PERCENTAGE OVER (+) OR UNDervaluation (-) OF THE EFFECTIVE EXCHANGE RATE IN 1938 ACCORDING TO OFFICER'S (1980) PPP CALCULATIONS

<table>
<thead>
<tr>
<th>Base Country</th>
<th>Period</th>
<th>1879-88</th>
<th>1910</th>
<th>1905-13</th>
<th>1913</th>
</tr>
</thead>
<tbody>
<tr>
<td>U.K.</td>
<td></td>
<td>+2.05</td>
<td>+10.42</td>
<td>+10.28</td>
<td>+5.79</td>
</tr>
<tr>
<td>U.S.</td>
<td></td>
<td>-27.03</td>
<td>-11.34</td>
<td>-9.98</td>
<td>-10.98</td>
</tr>
<tr>
<td>Canada</td>
<td></td>
<td></td>
<td>-7.63</td>
<td></td>
<td></td>
</tr>
<tr>
<td>France</td>
<td></td>
<td></td>
<td>-25.56</td>
<td></td>
<td>-21.87</td>
</tr>
<tr>
<td>Holland</td>
<td></td>
<td></td>
<td>-22.05</td>
<td></td>
<td>-1.81</td>
</tr>
<tr>
<td>Switzerland</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Source: Officer (1980), Tables 2, 3, 4 and 5, pp. 214-7.

However, a rather less encouraging interpretation of the results for 1938 may seem appropriate: given the tendency for structural changes to become increasingly pronounced over time, the larger average absolute error in 1975 (up to 28%) than in 1963-66 (up to 16%) is perhaps to be expected; for the same reason, the error in 1938 ought to be the smallest but, in fact, it is almost the largest (up to 24%). Fortunately, although this is disturbing, it undermines any exact application of a simple PPP hypothesis rather than the hypothesis being tested here, which does not suggest that relative prices are the only determinant of exchange rates. Moreover, the structural changes involved after a world war, a major world-wide depression and the collapse of the established system for regulating exchange rate values would certainly be large; consequently, the fact that four of the six currencies in Table 11.2 apparently stayed within approximately 10% of their PPP value (ignoring the 1879-88 based U.S. calculation) might be interpreted as a "good" result. (12)
This completes a brief review of relevant empirical work. The major conclusion would seem to be, both in a general sense and in relation to the 1930's in particular, that multilateral exchange rates were determined partially, but by no means only, by relative prices. This does not contradict the view of bilateral exchange rates taken earlier in the study. Consequently, the hypothesis that relative prices played a role in the 1930's in determining the multilateral (as well as bilateral) exchange rates of the seven currencies examined in Section II is considered below. Two separate tests are carried out: the first involves a simple PPP test and takes the form of calculations of inflation differential adjusted (or real) EER's; the second employs regression analysis which allows the inclusion of non-price explanatory variables (although, because of data deficiencies, at the expense of substantially reducing the country coverage of the EER's used as the dependent variables).

(II)

As a preliminary to constructing a real EER it was necessary to calculate an effective or "rest of the world" price index (using the same countries and weights as in the EER). The real EER can then be calculated:

\[
\text{Real EER} = \sqrt{\frac{\text{Nominal EER} \cdot (\text{EP}/\text{PD})}{7}} 
\]

\[\text{where EP} = \text{an effective (ROW) price index} \]
\[\text{PD} = \text{a domestic price index} \]

This variable is constructed in such a way that if PPP held exactly then the real EER would be one hundred, if it is greater then the
multilateral exchange rate is overvalued (in PPP terms) and if it is less then the exchange rate is undervalued; in addition the degree of "misvaluation" is clear because the real EER is effectively the nominal EER expressed as a percentage of its PPP value. Finally, as in the examination of bilateral exchange rates, prices are lagged by one month.

Two versions of the real EER were calculated for each country, one adjusted by WP's, the other by COL indices. This creates an immediate data problem due to limitations in the availability of price data but, in fact, the number of countries that had to be excluded turned out to be very small: the 29 country model used to construct EER's in Chapter 10 was reduced to 28 countries for tests involving WP's and 24 for those involving COL indices. Thus the nominal EER's were recalculated using scaled up values of the weights used in the 29 country versions. This is not quite correct as, strictly speaking, the global "half" of the composite weights is based on a model including the trade of the countries excluded from the 28 and 24 country versions. However, any discrepancy is likely to be small given the size of the excluded countries (in global trade terms) and the fact that the results presented in the previous chapter indicated that the weights of EER's could be varied quite substantially with little effect.

Real versions of the 7 country EER's used in the regression analysis below were also calculated. This was desirable for several reasons, most importantly because they are indices which are free of primary producers and therefore any associated criticisms but also, at a more trivial level, the global half of the composite weights are based only on trade between the seven countries thereby avoiding the criticism of the previous paragraph. The four versions of
each country's real EER are presented in Figs. 11.1-11.7.\(^{(17)}\)

A recurring problem in this kind of exercise is deciding just how large a deviation has to be before it indicates that the PPP hypothesis is discredited. There is no objective method of resolving this issue and so it becomes a matter of judgement and hence potentially of dispute.\(^{(18)}\) In the turbulent 1930's, as portrayed by Nurkse (1944), a similarly extreme view (to his) might be adopted and it could be argued that relatively large deviations - perhaps 10% or even 15% - from PPP values might still be interpreted as encouraging for the PPP hypothesis and somewhat larger deviations might be expected. However, this is an extreme view. Nevertheless, the evidence relating to bilateral exchange rates (presented above) did suggest an important role for non-price variables and consequently, it may be reasonable to ignore deviations of up to 5% and only treat those greater than 10% as being excessive. A further complication concerns the adequacy of 1929 as a base year. Whilst it has been argued that the pound, for example, was possibly at equilibrium in some general balance of payments sense in 1929, this was probably not the case in terms of relative prices, both for the pound and for several other currencies. Thus a deviation from the 1920-based PPP may, in fact, represent a movement towards, rather than away from, equilibrium if 1929 were a disequilibrium year. This possibility must be remembered.

Despite these problems the results for the pound are basically encouraging particularly for the "wide" (28 and 24 country) versions: the wide COL-adjusted index is within 5% of a hundred for 70% of the February, 1930 - August, 1939 period and the WP-adjusted version for 60% of the time. The "narrow" (7 country) versions tend to be rather lower but still remain within 10% for much of the time. Moreover,
FIG. 11.1  U.K. REAL EER's
FIG. 11.2  U.S. REAL EER's

- 28 country EER (WP adjusted)
- 24 country EER (COL adjusted)
- 7 country EER (WP adjusted)
- 7 country EER (COL adjusted)
FIG. 11.4  FRENCH REAL EER's
FIG. 11.5 BELGIAN REAL EER's
FIG. 11.6  DUTCH REAL EER's
FIG. 11.7 SWISS REAL EER's
in spite of the differences in levels, the trend of all four real EER's is virtually identical: the pound became undervalued in autumn, 1931 and remained so until late 1936 but for the rest of the period was approximately at an equilibrium level. It is not difficult to explain this undervaluation. The most obvious explanation lies in the activities of the EEA which is widely believed to have been deliberately holding the pound down in this period; this possibility has already been extensively discussed earlier in the study. Secondly, the fact that a recovery was beginning to take place in Britain at this time, whilst many other countries (notably those in the gold bloc) were still in a state of depression, may provide an additional explanation in terms of relative income effects. Finally, if the pound were indeed overvalued in 1929 and that year is an unsuitable base, then at least part of this "undervaluation" is apparent rather than real.

It may also be possible to explain some specific sub-periods by reference to the speculative dummies (or to periods characterised in the bilateral exchange rate regressions by large residuals). The extent of the undervaluation at the turn of 1931 and its decrease in spring, 1932 clearly falls into this category; this period is covered by K1, a highly significant British speculative dummy (in the bilateral regressions), associated with sterling weakness due to various speculative factors. Similarly, a slight tendency towards more undervaluation in early 1933, which occurs in the two COL adjusted indices, may be associated with the influences represented by K4 and the decrease in the degree of undervaluation in early 1933 of all four indices is quite clearly related to K2. This does not account for all the deviations of the pound from its PPP value - in particular
there is no obvious reason for the real effective pound to be at its lowest level in late 1932 — but it is highly suggestive: much of the effective pound's deviation from its PPP level can be adequately explained. Finally, it is interesting to note that the conclusion reached on the basis of the pound's nominal EER — that the EEA deliberately depressed the pound in the 1930's — is supported by the movements of the pound's real EER.

Unlike the pound's real EER's, there are significant differences between the WP and COL adjusted indices for the dollar, particularly in the wide version. The conclusions that should be drawn from the COL versions are quite clear: for the period between the pound leaving gold in 1931 and the beginning of the dollar depreciation in 1933 the dollar was overvalued, possibly by as much as 15% (22) from the spring of 1933, however, the dollar became undervalued until the end of the decade, particularly in 1933-34 and by more than 10% until late 1936. The implications of the WP versions are equally clear: the dollar was overvalued for most of the decade, excessively so in the late 1931 to early 1933 period, with the exception of 1933-34 and possibly early 1938.

Expectations (and probably the truth) lie somewhere between these two views and a compromise based on both indices would suggest a middle position consisting of three main propositions. Firstly, the dollar became overvalued in the October, 1931 — March, 1933 period (possibly reflecting the undervalued pound) reaching two peaks: the first of these at the turn of 1931 coincides with K1 and clearly, therefore, does reflect the undervalued pound and the other, at the turn of 1932, coincides with a U.S. speculative dummy (83), significant in the bilateral regressions and associated with dollar strength. (23)
second proposition is that the dollar was undervalued in 1933-34; this was expected and the fact that it is true multilaterally as well as bilaterally (as strongly suggested by the significance of $S4$ and $S5$ in the bilateral regressions) is no surprise.

Thirdly, from 1935 onwards the position of the dollar is not clear: the WP version indicates that it became slightly overvalued but remained fairly close to its PPP level but the COL version showed a persistent undervaluation which, for 1935, is more consistent with the bilateral results (bearing in mind the significance and signs of $S6$ in the latter). A common feature of all four of the American indices in this final sub-period is the increase in the extent of the undervaluation in early 1938. This could have been caused either by the 1937/38 recession and consequent relative income effects or by the influences associated with $S9$, an American speculative dummy which was significant in the bilateral regressions and covers this period. In general, the course of the real EER of the dollar gives less support for the PPP hypothesis than that of the pound but, nevertheless, deviations did tend to occur where expected and after 1935 the WP version does seem to indicate that PPP held approximately.

The Canadian real EER's are rather similar to those of the U.S. in that the COL adjusted versions are appreciably lower but dissimilar in that there is no important conflict between the latter and the WP adjusted indices. The WP indices would, in fact, suggest that PPP held quite well for the Canadian dollar in the 1930's, remaining within 5% of its PPP value for three-quarters of the time (24) and always within 10%. The COL versions are less encouraging: although they concur with the apparent equilibrium in the first three years or so, from mid-1933 onwards they indicate undervaluation (of up to 14%)
until the end of the 1930's. However, this does not actually contradict the WP adjusted EER's because they too indicate an undervaluation for much of this period, particularly during 1933-36. Indeed, fairly obvious explanations for this tendency to be undervalued after mid-1933 (and especially until late 1936) are available in the shape of the apparent overvaluation of the gold bloc countries for much of this period (discussed below) and also the fact that the Canadian dollar may have moved in sympathy with the undervalued U.S. dollar. (Two Canadian speculative dummies, DC5 and DC7 specifically included to represent this latter possibility were significant in the bilateral regressions).

It is also possible (as with the pound and the dollar) to look to dummy variables and large residuals in the bilateral regressions to explain specific sub-periods: the increase in overvaluation at the turn of 1932 coincides with a Canadian dummy (DC3) which was significant and associated with Canadian dollar strength in the bilateral regressions (although admittedly "wrongly" signed); the increase in the degree of undervaluation at the turn of 1933 also corresponds to a significant Canadian dummy (DC5); finally, the tendency towards greater undervaluation for much of 1936 can be associated with a combination of speculative effects (DC7, January-February), positive residuals in the Canadian dollar-pound estimates (March-April) and in the Canadian dollar-U.S. dollar estimates (July-October). (25)

Moreover, on the basis of a priori expectations, the role of relative incomes might also be expected to be important in the Canadian case although it is difficult to pick out specific examples of this. Indeed, it was anticipated that, for a number of reasons (including the high level of Canadian protection), relative price effects would
be weak and in the Canadian dollar-pound regressions this did turn out to be the case. Consequently, the lack of extreme deviations from PPP (particularly in the WP adjusted case) could be construed as indicative of a rather greater role for relative prices than might have been expected.

The gold bloc currencies all show a tendency towards overvaluation in the 1930's. This is particularly true of France with the COL adjusted version suggesting that a peak of 70% overvaluation was reached in 1934. The path of the French real EER is very clear since, despite differences in levels, the WP and COL adjusted versions (both wide and narrow) exhibit the same trend: the franc gradually became progressively overvalued in the early 1930's reaching a peak in the first half of 1934, it became less overvalued for a short while after this but the degree of overvaluation resumed its upward trend in autumn 1935 until the September, 1936 devaluation, after which, the degree of overvaluation was reduced (and in fact almost disappeared in the WP version); however, the upward movement still continued until the trend reversed itself and the overvaluation eventually became an undervaluation.

This tendency towards persistent (and large) overvaluation and the short period of undervaluation in the late 1930's (with very little equilibrium in between) clearly indicates that a simple PPP approach is inappropriate in the case of the French franc. Nevertheless, it is possible to argue that, after allowing for a number of fairly obvious explanations of this undervaluation, the remaining deviation from PPP would be much less pronounced, especially in the WP version. These explanations include the apparent undervaluations of the pound and the dollar in much of the 1930's (due, in part to the influence
of the EEA in the former case), the continued recession in France leading to a lower level of demand for foreign goods and currency (a relative income effect) and also the fact that 1929 may be an inappropriate base year for the franc in that it was widely believed that it had returned to gold at an undervalued level in 1928; hence much of the franc’s apparent overvaluation in the 1930’s may actually represent a movement towards PPP equilibrium.

In addition to these general arguments, some of the changes in the degree of overvaluation in specific sub-periods can be explained by speculative influences as represented by the French dummy variables and the residuals in bilateral regressions. The peaking of French overvaluation in early 1934 coincides with residuals in both the pound-franc and dollar-franc estimates (for January-February, 1934) which indicated franc strength and were tentatively attributed to official intervention. Subsequently, although all the real EER’s drop back immediately after this, they still remain at their highest levels in the 1930’s and this, in part, could well be due to speculative influences causing the franc to appreciate in 1934 and represented by F3.

Obviously speculative influences cannot explain all the deviations and dummies which were significant in the bilateral regressions do not always find their counterpart in the paths of multilateral exchange rates (as will be made even clearer by the regression results presented in the next section). Nevertheless, it does seem that some of the movement of real EER’s in general, and that the French franc in particular, can be attributed to the influences represented by the speculative dummies. In the latter case, it is also possible to suggest that the apparent overvaluation of the franc for much of the 1930’s can be accounted for and that an underlying relative price
influence still exists, even though it is not dominant.

The Belgian real EER also tends to be overvalued for much of the 1930's although to a much lesser degree than that of France. Another similarity is that the COL-adjusted version is consistently higher. Furthermore, each of the four EER's indicates the same pattern: the belga gradually became increasingly overvalued until its devaluation in March, 1935, after which it became undervalued until the French franc devaluation in Autumn, 1936, when the overvaluation resumed. In accounting for this, all the explanations used for the pre-devaluation overvaluation of the French franc - initial undervaluation in 1929, British and American undervaluations and relative income effects - can also be used in the Belgian case, whilst the middle period of belga undervaluation may well support the contention (expressed in Chapter 6) that the belga was undervalued against the franc for the April, 1935 - September, 1936 period; indeed this may be extended to other currencies of the gold bloc (and, effectively, amounts to saying that they were all overvalued).

Similarly, the return to an overvalued position in the latter part of the decade may also reflect French franc disequilibrium (in this case, undervaluation). Finally, despite the fairly good showing of the Belgian speculative dummies in the bilateral regressions, there is no obvious period during which the real EER was influenced by the speculative factors which they represent. In conclusion, it is perhaps worth noting that, in spite of the general tendency towards overvaluation the WP adjusted EER's suggest that the belga was rarely more than 10% above or below its PPP level.

The Dutch real EER indicates overvaluation for most of the 1930's
although it was approximately at equilibrium until Britain left gold in 1931 and for a brief period immediately after the Dutch followed in 1936. It should be remembered, of course, that a combination of protection and extreme gold standard orthodoxy was expected to undermine relative price effects in Holland and this did indeed turn out to be the case for the guilder-pound exchange rate. Consequently, a close correspondence of the Dutch EER to its PPP value would have been rather surprising. The Swiss franc is notable in that all four versions of the real EER are not only in agreement in terms of trends, but also levels. The Swiss franc, like the guilder became increasingly overvalued as the 1930's progressed, although the devaluations in autumn, 1936 seems to have restored equilibrium at least until mid-1938.

To a great extent the reasons suggested for the French and Belgian overvaluations in the first half of the decade clearly apply to all the gold bloc currencies, including the guilder and the Swiss franc; moreover, the overvaluation in the late 1930's could be attributed to the French undervaluation at that time. Specific reference to Dutch and Swiss speculative dummies, however, is not useful (as in the Belgian case) but, since they were of limited importance in the bilateral regressions, their inability to "explain" movements in the (real) multilateral EER is unsurprising. In general, it must be conceded that, in PPP terms, the gold bloc currencies were certainly overvalued to some extent in the 1930's (although the real EER's presented here may overstate it for the reasons outlined above). Nevertheless, this does not preclude a role for relative prices in effective exchange rate determination in the 1930's but, rather, it could be interpreted as indicating that relative prices were not the only determinant and other
variables were important; indeed, the results of the regressions involving bilateral exchange rates would support this view.

Consequently, a simple test of the "pure" PPP hypothesis was unlikely to be successful. However, it has been indicated that relative prices have some role and more precise (statistical) support for this contention is provided by Table 11.3 which compares the standard deviations of nominal and real EERS.

### TABLE 11.3: STANDARD DEVIATIONS OF NOMINAL AND REAL EERS

<table>
<thead>
<tr>
<th></th>
<th>U.K.</th>
<th>U.S.</th>
<th>Canada</th>
<th>France</th>
<th>Belgium</th>
<th>Holland</th>
<th>Switzerland</th>
</tr>
</thead>
<tbody>
<tr>
<td>29C: Nominal</td>
<td>6.42</td>
<td>12.15</td>
<td>4.60</td>
<td>25.71</td>
<td>14.30</td>
<td>15.91</td>
<td>15.46</td>
</tr>
<tr>
<td>29C: Real (WP)</td>
<td>5.76</td>
<td>7.07</td>
<td>3.86</td>
<td>12.05</td>
<td>5.80</td>
<td>10.10</td>
<td>9.63</td>
</tr>
<tr>
<td>29C: Real (COL)</td>
<td>5.71</td>
<td>9.67</td>
<td>5.89</td>
<td>24.92</td>
<td>11.63</td>
<td>14.72</td>
<td>13.21</td>
</tr>
<tr>
<td>7C: Nominal</td>
<td>9.29</td>
<td>10.75</td>
<td>4.09</td>
<td>29.06</td>
<td>13.91</td>
<td>17.53</td>
<td>15.19</td>
</tr>
<tr>
<td>7C: Real (WP)</td>
<td>7.38</td>
<td>7.08</td>
<td>4.02</td>
<td>12.42</td>
<td>6.20</td>
<td>12.09</td>
<td>10.85</td>
</tr>
<tr>
<td>7C: Real (COL)</td>
<td>8.41</td>
<td>8.15</td>
<td>5.55</td>
<td>25.23</td>
<td>11.41</td>
<td>16.17</td>
<td>13.44</td>
</tr>
</tbody>
</table>

**Key:**
- 29C = 29 country model
- 7C = 7 country model
- (WP) = WP adjusted
- (COL) = COL adjusted

If relative prices influence exchange rates then real EER's should be less variable and hence have smaller standard deviations. This is clearly the case for all countries, whether 29 or 7 country EER's are used, with the exception of the COL-adjusted Canadian real EER, although even here the increase in variation of the real EER is relatively small.

In many ways these simple tests, involving calculating real EER's, indicate more about the role of non-price variables in multilateral
exchange rate determination than relative price effects. It is likely that relative prices had some effect (since real EER's are less variable than nominal EER's and also some countries' real EER's do stay quite close to their PPP equilibrium levels for sustained periods) but it is equally obvious that PPP did not hold even approximately for much of the time and other variables were important; in particular, the occasions on which official intervention (especially that of the EEA), relative income levels and speculative factors (as represented by the speculative dummies) probably had some effect have been illustrated. More generally, expectations about deviations from PPP were usually realised: the pound did become undervalued because of the EEA's activities, the dollar was deliberately undervalued in 1933-34 and the gold bloc currencies did become increasingly overvalued (until they were devalued in 1936).

However, the highlighting of the importance of non-price variables does point to the general unsatisfactory nature of these tests and the exclusion of such variables constitutes a major aspect of this. The extent to which the base year represented equilibrium (in simple PPP terms) might also be questioned and the lack of reliability of some of the price data used in the wide version of the real EER criticised. More fundamentally, the inclusion of countries which practiced widespread exchange controls (such as Germany) and, indeed, were excluded from the examination of bilateral exchange rates for precisely this reason, must surely reduce the likelihood of the EER following its PPP path. Fortunately, these two latter problems are minimised by also calculating a 7 country version of the real EER but this still does not adequately deal with the exclusion of non-price variables. Consequently, in an attempt to remedy this, a second series of tests
was undertaken involving regression analysis, thereby allowing such variables to be incorporated. This exercise is described in the next section.

(III)

An initial problem of the regression analysis was to decide which of the variables used in the bilateral regressions could be "converted" into multilateral versions. Clearly multilateral price variables are available because they have already been calculated and it is not difficult to extend this calculation to income (although there are problems with data availability, discussed below). In addition, seasonal and trend variables can be easily incorporated along with the speculative dummies of the country whose EER is being examined. However, it is not really possible to construct acceptable "ROW speculative dummies" or a "ROW interest rate", since these variables are already proxies and to extend them further would be an extremely tenuous exercise. This latter argument also applies to official intervention and there are, in addition, less obvious reasons why an appropriate variable to represent this cannot be included. The intervention dummies used in the bilateral regressions reflect intervention to influence bilateral exchange rates not the activities of individual exchange funds and the construction of a multilateral ID for an individual currency would be difficult for two major reasons: firstly, subjective judgement would be required to decide whether, for example, British intervention to support the pound against the dollar merited the same weight as intervention to support the pound against the minor gold bloc currencies; secondly, to the extent that different exchange funds operated on each other's behalf, then incorporating
all a given country's intervention would not capture all intervention aimed at influencing its exchange rate anyway. (28)

Having decided on which variables to include in the multilateral regressions, a further problem relates to the selection of the countries to be included in the calculation of multilateral variables. Obviously, any country which did not have a relatively "free" exchange rate should be excluded as the model of exchange rate determination developed here would not apply to such countries. (This was a problem in the PPP tests using the 29 country model in the previous section). Moreover, a balance has to be struck between the availability and reliability of data, on the one hand, and maximising country coverage, on the other. A high country coverage is desirable but there is no income data and it is difficult to find suitable proxies for some countries. Consequently, it was decided to include only the countries whose exchange rates had been examined bilaterally thereby producing a 7 country model. (29) This is a relatively small number but, nevertheless, it does include the three most important countries - the U.K., U.S. and France (30) - certainly in terms of the size of weights in the 29 country EER's. (31)

In the first instance, then, OLSQ estimates for each country's EER were produced with relative prices, relative incomes, the country concerned's speculative dummies, seasonal variables and a linear trend as the independent variables. Unfortunately, with the possible exception of the WP version of the U.S. dollar (where the Durbin-Watson statistic was in the indeterminate range) all these estimates were serially correlated. Consequently, although, by and large, relative price and income variables were significant and correctly signed, the OLSQ regressions had to be discounted and the equations were re-estimated using the Cochrane-Orcutt (CORC) technique. It is the CORC estimates that are presented in Tables 11.4-11.10; all variables that were not significant (at the 20% level at least) have been excluded.
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Having decided on which variables to include in the multilateral regressions, a further problem relates to the selection of the countries to be included in the calculation of multilateral variables. Obviously, any country which did not have a relatively "free" exchange rate should be excluded as the model of exchange rate determination developed here would not apply to such countries. (This was a problem in the PPP tests using the 29 country model in the previous section). Moreover, a balance has to be struck between the availability and reliability of data, on the one hand, and maximising country coverage, on the other. A high country coverage is desirable but there is no income data and it is difficult to find suitable proxies for some countries. Consequently, it was decided to include only the countries whose exchange rates had been examined bilaterally thereby producing a 7 country model. (29) This is a relatively small number but, nevertheless, it does include the three most important countries - the U.K., U.S. and France (30) - certainly in terms of the size of weights in the 29 country EER's. (31)

In the first instance, then, OLSQ estimates for each country's EER were produced with relative prices, relative incomes, the country concerned's speculative dummies, seasonal variables and a linear trend as the independent variables. Unfortunately, with the possible exception of the WP version of the U.S. dollar (where the Durbin-Watson statistic was in the indeterminate range) all these estimates were serially correlated. Consequently, although, by and large, relative price and income variables were significant and correctly signed, the OLSQ regressions had to be discounted and the equations were re-estimated using the Cochrane-Orcutt (CORC) technique. It is the CORC estimates that are presented in Tables 11.4-11.10; all variables that were not significant (at the 20% level at least) have been excluded.
TABLE 11.4: ESTIMATES FOR THE MULTILATERAL POUND

<table>
<thead>
<tr>
<th></th>
<th>EXPECTED SIGN</th>
<th>WP</th>
<th>COL</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td></td>
<td>58.2 (3.14)</td>
<td>91.8 (3.66)</td>
</tr>
<tr>
<td>World prices/U.K. prices</td>
<td>+</td>
<td>3.01 (0.20)</td>
<td>-31.7 (1.39)</td>
</tr>
<tr>
<td>Trend</td>
<td></td>
<td>0.36 (5.90)</td>
<td>0.39 (6.91)</td>
</tr>
<tr>
<td>Seasonal variables: A1</td>
<td>?</td>
<td>-0.62 (1.34)</td>
<td>-0.84 (1.74)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>0.56 (1.19)</td>
<td>0.60 (1.31)</td>
</tr>
<tr>
<td>Speculative dummies: K1</td>
<td>-</td>
<td>-4.35 (4.06)</td>
<td>-4.36 (4.11)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>-2.28 (2.18)</td>
<td>-2.35 (2.26)</td>
</tr>
<tr>
<td>Rho</td>
<td></td>
<td>0.89 (18.5)</td>
<td>0.89 (18.0)</td>
</tr>
<tr>
<td>R-SQUARED</td>
<td></td>
<td>0.98</td>
<td>0.98</td>
</tr>
<tr>
<td>DURBIN-WATSON STATISTIC</td>
<td></td>
<td>1.55</td>
<td>1.49</td>
</tr>
<tr>
<td>STANDARD ERROR OF THE REGRESSION</td>
<td></td>
<td>1.39</td>
<td>1.37</td>
</tr>
<tr>
<td>F-STATISTIC</td>
<td></td>
<td>319.4</td>
<td>327.4</td>
</tr>
</tbody>
</table>

Notes: 1. T-statistics in brackets
TABLE 11.5: ESTIMATES FOR THE MULTILATERAL U.S. DOLLAR\(^{(1)}\)

<table>
<thead>
<tr>
<th></th>
<th>EXPECTED SIGN</th>
<th>WP</th>
<th>COL</th>
</tr>
</thead>
<tbody>
<tr>
<td>World prices/U.S. prices</td>
<td>+</td>
<td>86.8 (9.48)</td>
<td>12.0 (0.38)</td>
</tr>
<tr>
<td>World income/U.S. income</td>
<td>+</td>
<td>27.1 (4.82)</td>
<td>20.4 (2.25)</td>
</tr>
<tr>
<td>Seasonal variables:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>A1</td>
<td>?</td>
<td>1.27 (2.34)</td>
<td>0.09 (0.14)</td>
</tr>
<tr>
<td>B1</td>
<td>?</td>
<td>-0.52 (0.92)</td>
<td>-1.56 (2.21)</td>
</tr>
<tr>
<td>B2</td>
<td>?</td>
<td>-1.00 (2.27)</td>
<td>-0.32 (0.88)</td>
</tr>
<tr>
<td>Speculative dummies:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>S2</td>
<td>-</td>
<td>-2.88 (1.40)</td>
<td>-1.66 (1.09)</td>
</tr>
<tr>
<td>S3</td>
<td>-</td>
<td>1.20 (0.71)</td>
<td>4.25 (2.20)</td>
</tr>
<tr>
<td>S4</td>
<td>-</td>
<td>-11.1 (7.54)</td>
<td>1.75 (0.73)</td>
</tr>
<tr>
<td>S5</td>
<td>-</td>
<td>-13.9 (12.0)</td>
<td>-4.07 (1.66)</td>
</tr>
<tr>
<td>S6</td>
<td>+</td>
<td>-3.73 (2.08)</td>
<td>-1.34 (0.70)</td>
</tr>
<tr>
<td>S7</td>
<td>+</td>
<td>1.47 (0.74)</td>
<td>2.89 (1.90)</td>
</tr>
<tr>
<td>S9</td>
<td>-</td>
<td>-2.53 (1.52)</td>
<td>-0.75 (0.49)</td>
</tr>
<tr>
<td>S10</td>
<td>+</td>
<td>3.05 (1.40)</td>
<td>1.09 (0.50)</td>
</tr>
<tr>
<td>Rho</td>
<td></td>
<td>0.33 (3.20)</td>
<td>0.97 (34.4)</td>
</tr>
<tr>
<td>R-SQUARED</td>
<td></td>
<td>0.97</td>
<td>0.98</td>
</tr>
<tr>
<td>DURBIN-WATSON STATISTIC</td>
<td></td>
<td>1.99</td>
<td>1.99</td>
</tr>
<tr>
<td>STANDARD ERROR OF THE REGRESSION</td>
<td></td>
<td>2.33</td>
<td>2.05</td>
</tr>
<tr>
<td>F-STATISTIC</td>
<td></td>
<td>134.5</td>
<td>173.1</td>
</tr>
</tbody>
</table>

Notes: 1. Omitted (insignificant) variables: constant, trend, A2, S1, S8.
TABLE 11.6: ESTIMATES FOR THE MULTILATERAL CANADIAN DOLLAR(1)

<table>
<thead>
<tr>
<th>Expected Sign</th>
<th>WP</th>
<th>COL</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>76.7 (4.21)</td>
<td>90.8 (3.73)</td>
</tr>
<tr>
<td>Trend</td>
<td>0.16 (1.21)</td>
<td>0.20 (1.36)</td>
</tr>
<tr>
<td>Seasonal variables: A1</td>
<td>0.78 (1.78)</td>
<td>0.90 (2.08)</td>
</tr>
<tr>
<td>Seasonal variables: A2</td>
<td>0.37 (1.89)</td>
<td>0.34 (1.71)</td>
</tr>
<tr>
<td>Speculative dummies: DC8</td>
<td>1.87 (2.15)</td>
<td>1.85 (2.13)</td>
</tr>
<tr>
<td>Rho</td>
<td>0.96 (34.1)</td>
<td>0.97 (34.8)</td>
</tr>
<tr>
<td>R-SQUARED</td>
<td>0.92</td>
<td>0.92</td>
</tr>
<tr>
<td>Durbin-Watson Statistic</td>
<td>1.92</td>
<td>1.90</td>
</tr>
<tr>
<td>Standard Error of the Regression</td>
<td>1.16</td>
<td>1.16</td>
</tr>
<tr>
<td>F-Statistic</td>
<td>49.1</td>
<td>49.0</td>
</tr>
</tbody>
</table>

Notes: 1. Omitted (insignificant) variables: relative prices, relative incomes, B1, B2, DC1, DC2, DC3, DC4, DC5, DC6, DC7, DC9, DC10.
TABLE 11.7:  ESTIMATES FOR THE MULTILATERAL FRENCH FRANC

<table>
<thead>
<tr>
<th></th>
<th>EXPECTED-SIGN</th>
<th>WP</th>
<th>COL</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>?</td>
<td>196.4 (4.20)</td>
<td>21.4 (0.39)</td>
</tr>
<tr>
<td>World prices/French prices</td>
<td>+</td>
<td>26.7 (1.70)</td>
<td>179.6 (4.29)</td>
</tr>
<tr>
<td>Trend</td>
<td>?</td>
<td>-1.76 (3.83)</td>
<td>-0.95 (2.43)</td>
</tr>
<tr>
<td>Seasonal variables:</td>
<td>A1</td>
<td>?</td>
<td>0.06 (0.06)</td>
</tr>
<tr>
<td>Speculative dummies:</td>
<td>F4</td>
<td>-</td>
<td>4.63 (1.62)</td>
</tr>
<tr>
<td></td>
<td>F8</td>
<td>-</td>
<td>8.51 (2.96)</td>
</tr>
<tr>
<td></td>
<td>F9</td>
<td>-</td>
<td>-18.0 (4.95)</td>
</tr>
<tr>
<td></td>
<td>F10</td>
<td>-</td>
<td>-10.0 (2.61)</td>
</tr>
<tr>
<td></td>
<td>F12</td>
<td>-</td>
<td>-9.39 (3.24)</td>
</tr>
<tr>
<td>Rho</td>
<td></td>
<td>0.97 (37.7)</td>
<td>0.97 (34.5)</td>
</tr>
<tr>
<td>R-SQUARED</td>
<td></td>
<td>0.99</td>
<td>0.99</td>
</tr>
<tr>
<td>DURBIN-WATSON STATISTIC</td>
<td></td>
<td>2.15</td>
<td>2.14</td>
</tr>
<tr>
<td>STANDARD ERROR OF THE REGRESSION</td>
<td></td>
<td>3.01</td>
<td>2.76</td>
</tr>
<tr>
<td>F-STATISTIC</td>
<td></td>
<td>396.7</td>
<td>473.2</td>
</tr>
</tbody>
</table>

Notes: 1. Omitted (insignificant) variables: relative incomes, A2, B1, B2, F1, F2, F3, F5, F6, F7, F11.
### Table 11.8: Estimates for the Multilateral Belga

<table>
<thead>
<tr>
<th></th>
<th>Expected Sign</th>
<th>WP</th>
<th>COL</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>?</td>
<td>90.4 (1.74)</td>
<td>91.5 (1.55)</td>
</tr>
<tr>
<td>Speculative dummies:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>BG3</td>
<td>-</td>
<td>12.4 (4.72)</td>
<td>12.6 (4.66)</td>
</tr>
<tr>
<td>BG4</td>
<td>-</td>
<td>24.9 (9.66)</td>
<td>25.0 (9.39)</td>
</tr>
<tr>
<td>BG6</td>
<td>-</td>
<td>5.60 (2.47)</td>
<td>5.60 (2.47)</td>
</tr>
<tr>
<td>Rho</td>
<td></td>
<td>0.97 (39.8)</td>
<td>0.98 (41.4)</td>
</tr>
<tr>
<td>R-SQUARED</td>
<td></td>
<td>0.96</td>
<td>0.96</td>
</tr>
<tr>
<td>Durbin-Watson Statistic</td>
<td></td>
<td>1.97</td>
<td>1.89</td>
</tr>
<tr>
<td>Standard Error of the</td>
<td></td>
<td>3.02</td>
<td>3.04</td>
</tr>
<tr>
<td>Regression</td>
<td></td>
<td>94.6</td>
<td>94.0</td>
</tr>
</tbody>
</table>

Notes: 1. Omitted (insignificant) variables: relative prices, relative income, trend, A1, A2, B1, B6, B7, B8, B9, B10, B11.

### Table 11.9: Estimates for the Multilateral Guilder

<table>
<thead>
<tr>
<th></th>
<th>Expected Sign</th>
<th>WP</th>
<th>COL</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>?</td>
<td>187.0 (4.44)</td>
<td>166.5 (2.37)</td>
</tr>
<tr>
<td>Rho</td>
<td></td>
<td>0.96 (30.6)</td>
<td>0.95 (28.7)</td>
</tr>
<tr>
<td>R-SQUARED</td>
<td></td>
<td>0.95</td>
<td>0.95</td>
</tr>
<tr>
<td>Durbin-Watson Statistic</td>
<td></td>
<td>1.73</td>
<td>1.87</td>
</tr>
<tr>
<td>Standard Error of the</td>
<td></td>
<td>3.18</td>
<td>3.21</td>
</tr>
<tr>
<td>Regression</td>
<td></td>
<td>88.4</td>
<td>86.7</td>
</tr>
</tbody>
</table>

TABLE 11.10: ESTIMATES FOR THE MULTILATERAL SWISS FRANC(1)

<table>
<thead>
<tr>
<th></th>
<th>EXPECTED SIGN</th>
<th>WP</th>
<th>COL</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>?</td>
<td>69.6 (1.32)</td>
<td>239.1 (2.68)</td>
</tr>
<tr>
<td>World prices/Swiss prices</td>
<td>+</td>
<td>94.2 (2.34)</td>
<td>-93.2 (1.16)</td>
</tr>
<tr>
<td>Seasonal variables: B2</td>
<td>?</td>
<td>1.05 (1.51)</td>
<td>1.05 (1.46)</td>
</tr>
<tr>
<td>Rho</td>
<td></td>
<td>0.96 (29.9)</td>
<td>0.96 (30.9)</td>
</tr>
<tr>
<td>R-SQUARED</td>
<td></td>
<td>0.94</td>
<td>0.94</td>
</tr>
<tr>
<td>DURBIN-WATSON STATISTIC</td>
<td></td>
<td>1.89</td>
<td>1.70</td>
</tr>
<tr>
<td>STANDARD ERROR OF THE</td>
<td></td>
<td>3.60</td>
<td>3.70</td>
</tr>
<tr>
<td>REGRESSION</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>F-STATISTIC</td>
<td></td>
<td>62.1</td>
<td>58.4</td>
</tr>
</tbody>
</table>

Notes: 1. Omitted (insignificant) variables: relative incomes, trend, A1, A2, B1, SW1, SW2, SW3, SW4, SW5, SW6, SW7, SW8, SW9, SW10.

It is clear that the regression results are rather poor and, in some cases, the EER seems to have been principally determined by some combination of the constant term, the trend variable, speculative effects (as represented by the dummy variables) and seasonal factors. On the other hand, relative price effects are apparent in three cases (the U.S., France and Switzerland) and in two others - Canada and Holland - were expected to be weak anyway. Moreover, if it is accepted that the model is less applicable to the minor gold bloc currencies (since their exchange rates were heavily influenced by the pound-dollar-franc triangle) then the only major surprise is the failure of any correctly signed relative price effects to show up in the case of the pound. The "wrong" sign in the COL version is particularly puzzling since the pound was only once associated with perverse bilateral price effects (in its
relationship with the guilder). However, these problems may well be due to the exclusion of any variable to represent the activities of the EEA which probably "distorted" the path of the pound's EER and, on occasion, pushed it in the opposite direction to that indicated by "economic fundamentals". (In any case the "wrongly" signed COL ratio is only significant at the 20% level).

Nevertheless, to the extent that it was usually relative prices based on WP's, not COL indices, that were important - on two out of the three occasions when the relative price variable was significant, in fact - and relative income was only significant in one case (the U.S. dollar), the results could be construed as being supportive of the Nurkse hypothesis, at least in the negative sense of not supporting the alternative hypothesis put forward here. However, the speculative dummies do not perform especially well either (and where they do it is usually in the regressions in which relative prices actually are significant) and so the regression results might be interpreted as providing strong support for neither hypothesis. This view is supported by the fact that the generally poor performance of the relative income variables can probably be explained in terms of data inadequacies. Indeed, this variable performed only tolerably well in the bilateral regressions since, though usually significant, it was "wrongly" signed for a third of the time; consequently, this could be expected to lead to a cancelling out effect in the multilateral relative income variable.

Despite the generally poor quality of the results, it might be useful to consider the variables that are significant to see if they perform as expected and also whether their performance is consistent with that in the bilateral regressions. In the case of the pound the trend variable indicates a gradual appreciation (as indeed it did in
the bilateral regressions) and the "traditional" seasonal pattern of autumnal weakness and spring strength was very much apparent in the shape of the significance and sign of A1. The importance of B1 is less certain but, in any case, taken in conjunction with A1 it modifies the seasonal pattern rather than changes it. Two speculative dummies are significant: K1's appearance was expected but that of K4 is surprising as it was never significant in any bilateral regressions, although it is correctly signed. K2 and K3 might have been expected to show up, given their performance in the bilateral regressions, and their lack of significance rather supports the contention (made earlier) that the speculative dummies did not perform especially well.

The model seems to work tolerably well for the U.S. dollar and the estimates for U.S. EER are probably the "best" of the seven, in terms of supporting the hypotheses embodied in the model. The significance and correct signs of the relative price and income variables have already been referred to and it is clear from Table 11.5 that the speculative dummies' performance is almost entirely consistent with a priori expectations and the results of the bilateral regressions: where variables were "wrongly" signed in the latter (S3 and S6), they were "wrongly" signed here, where they were insignificant, they are also insignificant here (S1) - the exception is S8, which was significant in most of the bilateral regressions - and the performance of the variables tends even to match up in terms of degree of significance, in that those which were only significant in some of the bilateral regressions are significant at low levels or in only one version of the multilateral regressions (S2 and S7, for example). Finally, the sign of the trend variable is the same as in the bilateral regressions and, since no clear seasonal pattern emerged in the latter, the slight
difference in seasonal variations indicated in the WP and COL versions is not surprising. In fact, the former version displays a seasonal pattern for the dollar exactly in keeping with expectations, that is strong in the autumn, weak in the spring; the pattern in the COL version is similar, but from the point of view of expectations, mistimed since it suggests that the dollar remained weak until as late as October and then became strong until April.

Whilst the model performs rather less well for the Canadian dollar, the actual results produce few surprises. Doubts were expressed (in Chapter 7) about the likelihood of relative price and income effects showing up in the Canadian case (due to trade controls) and, to some extent, these were borne out in the bilateral regressions. In addition, the uncertain performance of the trend variable here - it is only significant in one version at the 20% level - is not unexpected as it carried opposite signs in the bilateral regressions against the pound and the U.S. dollar. Also the expectation that the Canadian dollar would be influenced by seasonal factors - particularly because of the importance of wheat exports to the economy - is strongly confirmed. Only the speculative dummies perform unexpectedly as they are mainly insignificant and the only exception (DC8) was insignificant in the bilateral regressions. However, it is correctly signed here and the examination of the bilateral Canadian dollar residuals indicated that its insignificance was due to mistiming.

The French franc regressions are, on balance, encouraging. A clear relative price effect emerges and the insignificance of the relative income variable is not unsurprising given the difficulties encountered with this variable in the bilateral regressions. The tendency for the franc to weaken over time, as indicated by the trend
in the bilateral regressions, is confirmed here and the limited (COL version only) evidence of a seasonal effect is consistent with the hypothesis that European currencies weakened in the autumn and strengthened in the spring (although no particular seasonal pattern had been apparent in French bilateral exchange rates).

The performance of the speculative dummies is a little erratic: the insignificance of F1 and F2 is expected, as they had been insignificant in the bilateral regressions, but since all the other dummies were significant (to varying degrees) in the latter, the insignificance of some of these others in the multilateral regressions is surprising. This is particularly true of F3 and F5 but less so of F6, F7 and F11 since their sign varied across the different bilateral exchange rates and a cancelling out effect may have occurred. Of the variables that did show up here, F8 and F12 were mainly significant in the bilateral regressions (and F8 is consistently "wrongly" signed) although this was only true of F4 in the belga-franc estimates. F9 and F10 are most interesting: they were mostly significant but usually "wrongly" signed in the bilateral regressions but are correctly signed in Table 11.7 suggesting that multilaterally the franc was indeed weak in the October, 1936 – February, 1937 period.

With the partial exception of Switzerland (for which the WP ratio is significant) the results for the rest of the gold bloc EER's are rather poor, although not actually inconsistent with the bilateral results. Indeed, only the constant term is significant in the Dutch regressions. However, it should be recalled that both speculative and "economic fundamental" variables tended to fare relatively badly in the Dutch bilateral regressions, particularly in the guilder-pound estimates. This raised some questions about the applicability of the
model to the guilder and the multilateral guilder regressions would seem to do much the same.

The Swiss regressions, as already indicated, are the "best" of the three: not only is one of the relative price variables significant, but the only other significant variable (B2) indicates the expected seasonal strength in the Swiss franc in the early part of the year at the height of the winter tourist season. Moreover, the failure of potential speculative influences to show up is not unexpected, as Switzerland is a small country, although the insignificance of the trend variable is perhaps surprising in view of the bilateral regression results. This latter point can also be made in connection with the other two minor gold bloc currencies. In fact, in the case of the belga, the only significant variables are the constant term and the three speculative dummies which are all "wrongly" signed. B63 and B64 were similarly signed in the bilateral regressions but B66 had been insignificant (except in earlier regressions of the belga-dollar) and its presence in Table 11.8 is rather surprising. The overall impression from the regressions involving the minor gold bloc currencies is that there is little evidence that their multilateral exchange rates were determined by either the "economic" or the "speculative" variables included in the model.

On balance the poor quality of the results is perhaps not surprising. Clearly there are problems in calculating the multilateral variables and questions might be raised about the adequacy of their construction. In addition, there are a number of omitted influences including official intervention, interest rate differentials, ROW speculative effects and also, as in the bilateral regressions, the effects of protection. Consequently, whilst these regression results
cannot be said to indicate general support for the hypothesis that multilateral exchange rates were determined by "economic fundamentals", it is too harsh an interpretation to suggest that they show that they were not. Moreover, in at least one case (the French franc), there is firm evidence that relative prices were an important determinant of the multilateral exchange rate and more limited evidence to suggest the same for the U.S. dollar and the Swiss franc. Indeed, of the three major currencies of the 1930's, only the multilateral pound shows no signs of being determined by the "economic fundamentals" that were included in the regressions.

(IV)

In general, testing the hypotheses developed in chapter four for EER's has not been very successful in terms of providing support for these hypotheses; the results have tended to be ambiguous and difficult to interpret. To a large extent, this may be due to the crudity and inadequacies of the tests actually performed (although some of these are probably unavoidable). For example, the calculation of real EER's is essentially an examination of a simple PPP hypothesis which effectively tests directly for the importance of only one "economic fundamental". It was also subject to the criticism that too many countries which did not have exchange rate regimes that even approximated the "free" exchange rate system, to which the model is applicable, were included. Furthermore, whilst the second test, using regression analysis, incorporated more variables, the criticism of not including all important explanatory variables is valid here also, if to a lesser degree. The construction and country coverage of the
EER's might also be questioned. Therefore, there are a number of good reasons for the rather inconclusive results.

However, on the positive side, further evidence to support the contention that the explanatory variables included in the model were important determinants of exchange rates was provided in some cases, in particular for seasonal effects, some of the speculative effects and indirectly (and more tenuously) for the role of official intervention. In fact the consistency of performance of variables that were significant in both bilateral and multilateral regressions is a notable feature of the results. The contention that some of the exchange rates were distorted by various factors, especially protection, which is not the same as arguing that they were subject to destabilizing speculation, is also supported. Moreover, the conclusions concerning "economic fundamentals" were far from wholly negative: interest rate differentials unfortunately had to be excluded and the problems of finding suitable income proxies appeared to continue but positive evidence of a relative price effect was provided by the calculation of the pound's real EER, the comparison of the standard deviations of nominal and real EER's (Table 11.3), the French franc regressions and, to a lesser extent, by those for the U.S. dollar and Swiss franc.
FOOTNOTES TO CHAPTER 11

1. Long run PPP tests are carried out by Officer (1980) for the periods 1938, 1963-66 and 1975 using a variety of base periods (to maximise country coverage) going back up to a hundred years (1879-88, 1905-1913, 1910 and 1913). Similar tests were carried out by Genberg (1978) and by the OPTICA Reports, as described in Thygesen (1978), for the 1960's and 1970's. It is also becoming increasingly common to calculate "real" EER's (that is, EER's adjusted to allow for inflation differentials). See Dimsdale (1981) for 1921-38 and World Financial Markets (1978, 1979) for the 1970's.

9. The standard deviations are 5.57 (real EER) and 8.58 (nominal EER).
12. There is one other feature of Table 11.2 of interest in the shape of the (approximately) 10% overvaluation of the (effective) pound in 1938. This conflicts with Dimsdale's real EER (Table 11.1) which apparently suggests that the pound was undervalued by 10% in 1938. However, since different base periods were used this may indicate more about the choice of base period than the level of the pound. Nevertheless, given the use of 1929 as a base period in the present study, this divergence is disturbing and, if 1929 were a poor choice of base year and Officer's calculations were correct, then Dimsdale's index could be construed as suggesting that the pound was overvalued by about 20% in 1929. Of course, Officer's choice of base may be equally unreliable and, in any case, this undervaluation is in relative price terms and if allowance is made for other determinants of exchange rates then it is possible to take the view that in 1929 the pound was at equilibrium in a wider sense. (This issue was discussed at length in Chapter 2, Section IV).
13. The excluded country in the WP version was Mexico. In fact, in the U.K. case, since Mexico was not included in the original calculation (a 28 country model was used), the nominal EER used is the same as that discussed in Chapter 10. In the COL version the excluded countries were Mexico, Brazil, Japan, Russia and Spain. (The exclusion of the latter is probably no great loss since Spanish exchange rate data was unavailable during most of the Civil War period and had had to be interpolated).
14. This was done by multiplying the weights of the remaining countries by \(100 / (100 - \text{combined weights in the 29 country EER of the excluded countries})\) to make the remaining weights sum to one hundred.

15. These were extensively discussed in Chapter 9, Section II.

16. In fact a comparison between scaled up versions of the 29 country model weights and those actually used in the 7 country model (and properly calculated) revealed that this latter problem is fairly trivial. There are, of course, differences but the only significant one was that the "properly calculated" weighting system tended to give a greater weight to Canada mainly at the expense of the U.K. (In the U.K. case it was at the expense of the U.S.)

17. It would have been possible to simply compare the nominal EER with its PPP value but use of a real EER allows the results to be presented as a single figure (and, furthermore, one which clearly shows the degree of over- or undervaluation).

18. Indeed Balassa (1964) and Yeager (1958) did dispute this point. The figure in question was ±25\% (for long run tests of PPP).

19. The trend of the (comparable) Dimsdale index (given in Table 11.1) is also similar. The main difference is that it suggests the real effective pound had not regained its 1929 value even by 1938. Nevertheless, the arguments concerning the reasons for the pound's undervaluation in the next paragraph are still applicable.

20. See Chapter 5, Section II.

21. This variable was never significant in the bilateral exchange rate regressions but it was significant in those using the multi-lateral pound (presented in the next section) in both WP and COL versions.

22. To some extent this must reflect the apparent undervaluation of the pound in the corresponding period.

23. It was, in fact, "wrongly" signed. This is discussed in Chapter 5, Section III.

24. The figure is slightly higher (80\%) for the wide version than for the narrow version.

25. See Table 7.6, Chapter 7, Section V.

26. Chapter 5, Section IV, Table 5.9.

27. Price indices for some countries were based on one city or region; for others they were only available on a quarterly basis and the gaps had to be filled by interpolation.

28. Of course, all the problems of constructing intervention dummies discussed in the appendix to Chapter 4, are equally applicable here.
29. It would have been possible to construct an 11 country model by including the four Scandinavian countries but their combined weights (in the 29 country model) is small.

30. In fact, there are four major countries (in terms of importance in world trade) but the fourth - Germany - must obviously be excluded because of its use of trade and exchange controls.

31. The combined weights of the other six countries in the composite weights of the EER's (29 country model) of the seven countries were as follows:

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<th>Country</th>
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<td>U.K.</td>
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<td>U.S.</td>
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<td>Canada</td>
<td>69.6%</td>
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<td>France</td>
<td>45.3%</td>
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<tr>
<td>Holland</td>
<td>48.1%</td>
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<tr>
<td>Belgium</td>
<td>54.6%</td>
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<tr>
<td>Switzerland</td>
<td>47.7%</td>
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</table>

32. For some French exchange rates the relative income variables using the original proxy (employment), which is used in the multilateral regressions, were insignificant and an alternative proxy (production) had to be used. The relative income variables involving French "income" were then always significant, to varying degrees, although often "wrongly" signed. This may suggest that a cancelling out process has occurred in the multilateral variable.

33. However, the appreciating trend was against the pound, dollar and franc, not each other, and there may be a cancelling out process taking place in these multilateral regressions.
SECTION IV: CONCLUSIONS
CHAPTER 12

SUMMARY AND CONCLUSIONS

(I)

The purpose of the present chapter (and effectively the final section) is to summarize the findings of the earlier chapters and bring together the results (for both bilateral and multilateral exchange rates) in order to draw some general conclusions relating to the main themes of the study. A detailed analysis of the results of the tests involving bilateral exchange rates - particularly the technical aspects such as consistency of performance of variables across different exchange rates - has already been provided (in Chapter 8) and some preliminary conclusions about multilateral exchange rates were suggested in the previous chapter. These have now to be related to the wider issues.

The basic objective of the thesis has been to test what might be called an "anti-Nurkse" hypothesis for the 1930's: specifically, that exchange rates were generally "stable", determined by "economic fundamentals" and not subject to excessive variation due to destabilising speculation. This has involved examining various aspects of exchange rates; for example, the concepts of stability and of speculation, the role of expectations and the extension of the concept of multilateral or effective exchange rates to the 1930's. It has also generated evidence relating to a number of other issues, particularly the role of official intervention in the 1930's, the PPP hypothesis and some of the implications of using multilateral as opposed to bilateral exchange rates for this period.
The study begins by examining the relevant theoretical and empirical literature with a view to identifying the major variables that determine exchange rates. On the basis of these surveys, a model of exchange rate determination was developed which could be viewed either as an extended PPP hypothesis - that is, the PPP hypothesis is accepted but it is realised that deviations from PPP can occur for non-speculative (or "economic") reasons and the relevant variables should be identified and added - or as being based on the premise that the variables that determine the balance of payments also determine the exchange rate (since in determining the flow of goods and services they implicitly determine the flow of currencies). It was also recognised that, realistically, some allowance should be made for speculative effects and that they ought to be explicitly modelled, and this was done by including dummy variables to represent anything that may have caused speculative capital flows.

This model was then tested, with varying degrees of success, for the bilateral exchange rates of seven countries whose currencies were relatively "free" in the 1930's, in terms of not being subject to extensive exchange controls. Attention was then turned towards multilateral exchange rates. These had never been calculated for the inter-war period and so, after an examination of the relevant methodology, EER's were calculated for all seven currencies. Given that they had never been calculated before, it was felt necessary to draw some general inferences from the EER's before using them to provide a more limited test (because of data deficiencies and methodological problems) of the model for multilateral exchange rates.
The central hypothesis - that exchange rates were principally determined by "economic fundamentals" in the 1930's - receives a fair measure of support, although this is rather less true of the tests involving EER's. The most important explanatory variable was relative prices but it was noticeable (in both the bilateral and multilateral tests) that WP's performed "better" and this confirmed, to some extent, the suspicion that verification of this extended PPP hypothesis by an index of mainly traded goods might be spurious. However, whilst their performance was clearly "worse", the COL indices did show up reasonably well in the bilateral tests; in particular, in two legs of the pound-dollar-franc exchange rate triangle they were significant (at the 1% level) and in the other (pound-dollar) a relative "price" variable based on indices of wages, therefore containing even less traded goods, was significant. Furthermore, despite the poor results when multilateral exchange rates were examined, the inflation differential-adjusted (real) EER's were less variable than the nominal EER's with only one exception.

On obtaining similar results for a similar model, Hodgson (1971) and Hudgins (1973) suggested that this indicated support for the PPP hypothesis. In a sense, this is true but in another, in taking the significance of relative prices in isolation and ignoring the rest of the model, it is not, since the rest of the model suggests that the PPP hypothesis did not hold because numerous other variables obviously did partially determine exchange rates. Moreover, the single explicit test of the "pure" PPP hypothesis, carried out multilaterally (in Chapter 11) when real EER's were calculated, clearly demonstrated that
in most cases "pure" PPP did not hold. However, if the PPP hypothesis is interpreted as simply arguing that exchange rates were primarily, but not solely, determined by relative prices then support is provided for this version. Nevertheless, to the extent that some would interpret the PPP hypothesis more rigidly, the results here do not really support it but rather suggest that relative prices were one of several important determinants of exchange rates.

Other "economic fundamentals" of importance were relative incomes, interest rate differentials, seasonal effects and probably the influences represented by the trend.\(^2\) It must be conceded, however, that in the case of the first two (and most important) variables the bulk of the positive evidence was contained in the section on bilateral exchange rates,\(^3\) although the multilateral tests did confirm the importance of seasonal effects and the trend variable in some cases. Furthermore, on the basis of the performance of the more comprehensive version of the model in the bilateral tests, it could be argued that some of the unexplained variation in EER's was probably due to the (excluded) "economic fundamentals". Nevertheless, the performance of the one variable of this group included in the multilateral tests – relative income – was disappointing although it was significant in the U.S. dollar regressions and the construction of the variable was beset by methodological difficulties.

A final set of "economic" variables - ROW effects (only applicable to bilateral rates) - were also generally significant. In retrospect, it might have been advisable to test for ROW speculative effects. Logically, ROW ("third country") speculative dummies should have been included along with ROW "economic fundamentals". However, given the subjective nature of such variables, then whilst it is acceptable to
use them directly (in regressions involving their country of origin),
to use them indirectly (in regressions involving exchange rates between
two other countries) is probably rather tenuous, although third country
speculative effects may have provided rational explanations for some
of the residuals.

The second major purpose of the thesis or, perhaps better,
another way of approaching the first, was to consider the Nurkse
charge that:

"If currencies are left free to fluctuate, 'speculation'...
...is likely to play havoc with exchange rates...." (4)

Of course, to the extent that it has been demonstrated that "economic
fundamentals" determined exchange rates, this has already been refuted.
However, more specific and positive evidence was also provided in the
shape of the inclusion of speculative dummies and the examination of
the residuals and their explanation in terms of non-speculative
influences. The general impression of such tests was that, on
balance, exchange rates were not subject to destabilising speculation.
The issue is mainly examined in relation to bilateral exchange rates
but, since speculators operated on the basis of expectations about
bilateral not multilateral rates, then this is where the tests for
destabilising speculation should be concentrated. (5)

It is true that numerous speculative dummies were significant,
particularly those of Britain, France and the U.S.A. and, in some
cases, the exchange rate was clearly overshooting due to genuinely
destabilising speculation; for example, the contentions that de-
stabilising speculation caused excessive weakening of the pound at the
turn of 1931 and the gold and dollar "scare" affected the dollar in
1937-8 receive both bilateral and multilateral support. However, such dummies tend to be in the minority since many variables are either insignificant (implying no speculative effect) or are probably picking up something else, such as official intervention, and are consequently often "wrongly" signed. In addition, a few of the variables are not really testing for destabilising speculation in that the influences they represent are not of this type or, alternatively, the speculation being modelled is not destabilising in the sense that speculators were correct.

Of course, it can be suggested that these dummies provide inadequate tests for destabilising speculation but it is difficult to devise alternative and superior tests - a "speculative variable" based on forward market data was also tried with limited success - and the inclusion of no speculative variables and their replacement by an examination of the residuals is inadequate in that it makes the implicit (and demonstrably incorrect) assumption that all the residuals are due to speculation. At least dummy variables are based on attempts to identify and model destabilising speculation a priori.

Furthermore, some of the dummies (particularly S4 and S5) and some of the residuals (especially those relating to the belga in 1935-6 and the French franc in 1936-7) indicated periods of under- or over-valuation (in the sense of the exchange rate not being at the level indicated by "economic fundamentals"). Whilst this indicates that the influence of the latter was being undermined, it does not suggest that this was due to destabilising speculation but rather to official intervention. The exchange rate was not at its "extended PPP level" because of government interference (6) but it was not "unstable" in the Nurkse sense of the deviations being due to destabilising speculation;
indeed, speculation was being thwarted by the authorities as indicated by the frequent "wrong" signs of speculative dummies representing speculation in these periods (such as the French dummies in 1935-6). When large movements ultimately took place - for example, the deviations of the gold bloc currencies in 1935-6 - these were not due to destabilising speculation but, in a sense, quite the opposite: the pressure of "economic fundamentals" became irresistible. The actual devaluation may have been accompanied by a brief bout of speculation but it was not destabilising because the speculators were correct and it was the government that had made the mistakes.

This argument would imply that official intervention played an important role in the 1930's and testing specifically for the presence of intervention was one of the main secondary objectives of the study. Evidence of the importance of the exchange funds' activities had been provided by Hudgins (1973) and Ozmun (1976) who were specifically concerned with this issue. The results presented here would seem to bear their conclusions out. No tests were carried out for EER's although it was suggested that the path of the pound's real EER did indicate the presence of intervention. In the bilateral tests, the ID's of the minor currencies were insignificant but this was not entirely surprising given the limited "data" on which they were based. On the other hand, the ID's for the pound-dollar-franc triangle were more complete and were clearly important. On the basis of this, the misvaluation (already discussed) of some currencies and the interpretation of some of the residuals, two main conclusions can be drawn: firstly, that the major exchange rates were influenced by official intervention and secondly, that the evidence does tend to support the widely held view that the pound was deliberately held down in the early 1930's.
Another possible interpretation of large residuals is that they are caused by the (unmodelled) effects of protection. This was widespread in the 1930's and, just as it affected the flow of goods, it would also have affected the flow of currencies. Trade controls are important because they are widely acknowledged to cause deviations from PPP. No positive evidence relating to the significance of this influence was presented: a rather crude attempt to model the effects of protection in the Canadian case was unsuccessful and it was not possible to derive even a crude indicator for protection in any other cases. To the extent that relative prices did show up and residuals could be satisfactorily explained, it might be argued that trade controls did not influence exchange rates (with the exception of the guilder-pound exchange rate where the poor showing of "economic fundamentals" was attributed in part to the effects of protection).

In the face of no direct evidence though, a more cautious conclusion may be appropriate: namely, that the effects of the widespread trade controls on exchange rates seem to have been rather weaker than expected.

The aspect of the study that is most innovative is the calculation of EER's for the 1930's. The use of EER's to test the central hypothesis of the study was probably the least successful part of the thesis although the contention that "economic fundamentals" were important did receive some support in the shape of the lesser variability of real EER's and the regression results for two of the three major currencies in the 1930's (the dollar and the franc); moreover, there was no positive evidence that destabilising speculation was dominant. In any case, the most important contribution of the section on multilateral exchange rates probably lies in the examination of the methodology, its application to the 1930's (especially) and the drawing of a few general inferences concerning the course of multilateral as opposed to bilateral exchange rates.
Ultimately, the study is principally concerned with whether or not the 1930's can justifiably be portrayed as a period of exchange rate near-anarchy, more specifically whether or not exchange rates were determined primarily by "economic fundamentals" or were determined by the activities of speculators. The results here would give qualified support to the former interpretation. Exchange rates do seem to have been largely determined by "economic fundamentals" and were relatively "stable" in that sense. Speculation, sometimes destabilising, did play a minor role but such "instability" that did occur was due, in a large part, to government interference. Indeed, it might be argued that this latter observation provides the most important conclusion of the study. The central hypothesis has been that exchange rates were determined by economic fundamentals and much effort has gone into examining the traditional counter-argument (put forward by Nurkse for the 1930's) that this was not so because of private destabilising speculation. However, the results are consistent with the view that such instability as occurred had government rather than private origins and that the real threat to exchange rate stability in the 1930's was provided by the activities of governments rather than private individuals. This is an interpretation that might usefully be applied to other periods of allegedly "unstable" floating exchange rates.

continued over/......
FOOTNOTES TO CHAPTER 12


2. These latter two variables are not obviously "economic fundamentals" but they are included in that group for two reasons: firstly, they almost certainly do not reflect speculative influences and secondly, and most important, they represent genuinely "economic" effects to the extent that seasonal variation takes place because of the unevenness of crop movements and tourism during the year and the trend variable represents changes in tastes and/or productivity.

3. Interest rate differentials were not even included in the multilateral tests of the model because of the difficulties involved in constructing a suitable variable.


5. Indeed, it is probably inappropriate to look at multilateral exchange rates in this context in some cases. For example, a feeling during the 1933-36 period that the French franc was going to leave the gold standard and depreciate could have led to an outflow of funds, but almost certainly not to other members of the gold bloc or to a small country like Canada. Consequently, speculative capital flows would only affect some of the bilateral components of the multilateral exchange rate and there may even be offsetting forces at work within the latter if the "minor" gold bloc currencies weakened in sympathy (due to a capital outflow).

6. This is not an uncommon phenomenon. Similar situations where government interference caused such problems were described in Chapter 2. Examples are the French franc in 1924-26 and the Canadian dollar in 1960-1.

7. Two other important issues - whether intervention was competitive and whether it had a stabilising or destabilising effect on exchange rates - have not been properly addressed and no specific conclusions are offered.
## APPENDIX I

### DETAILS OF CONSTRUCTED VARIABLES

(1) *Seasonal Variables (Sine and Cosine Waves)*

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### (2) Speculative Dummies

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British Speculative Dummies are given in Table 5.1, p. 144.

American " " " " " " 5.2, p. 150.

French " " " " " " 5.3, p. 157.

Belgian " " " " " " 6.1, p. 220.

Dutch " " " " " " 6.2, p. 226.

Swiss " " " " " " 6.3, p. 231.

Canadian " " " " " " 7.3, p. 289.
(3) Official Intervention Dummies

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APPENDIX II

DATA SOURCES

(1) Spot Exchange Rates

These were taken from several sources in order to obtain direct quotations as far as possible. (Any exchange rate used in the study but not listed below has been based on cross-rates). For some exchange rates two sources had to be used and the respective dates are given in these cases; where no dates are given the source gave the exchange rate for the entire 1929-38 period.

(i) Sterling exchange rates

L.C.E.S. (London and Cambridge Economic Service) Bulletin: U.S.A., Brazil, Argentina, India, Japan, Czechoslovakia, France, Germany, Italy, Netherlands, Sweden, Switzerland.

Bank of England Statistical Summary (all 1932-8 only): S. Africa, Canada, China, Belgium, Denmark, Finland, Norway, Spain, Australia, New Zealand.

Economist: Egypt, Canada (1929-31), China (1929-31), Russia, Austria, Belgium (1929-31), Denmark (1929-31), Finland (1929-31), Norway (1929-31), Poland, Spain (1929-31).


(ii) U.S. Dollar exchange rates

Federal Reserve Bulletin: Argentina, Austria, Belgium, Brazil, Canada, China, Czechoslovakia, Denmark, Egypt, Finland, France, Germany, India, Italy, Japan, Mexico, Netherlands, Norway, Poland, Portugal, Spain, Sweden, Switzerland.

(page 421A follows)
(iii) Canadian Dollar Exchange Rates

The Canadian Yearbook (all 1930-38): Argentina, Australia, Austria, Brazil, China, Czechoslovakia, Denmark, Finland, France, Germany, India, Italy, Japan, Mexico, Netherlands, Norway, Spain, Sweden.

(iv) French Franc Exchange Rates

Bulletin de la Statistique Générale de la France: Norway (1933-8), Spain (1931-8).

Ministère de l'Economic Nationale, Annuaire Statistique: Czechoslovakia, Finland (1935-8), Germany, Italy, Poland (1933-8), Netherlands, Spain (1929-30), Sweden.

(v) Belga Exchange Rates

Ministère de L'Intérieur, Office Centrale de la Statistique, Annuaire Statistique de la Belgique et du Congo Belge: Canada, Czechoslovakia, Denmark, France, Germany, Italy, Netherlands, Norway, Poland, Spain, Sweden, Switzerland.

(vi) Swiss Franc Exchange Rates

Annuaire Statistique de la Swisse: Austria, France, Germany, Italy, Netherlands, Spain, Sweden.

(vii) Dutch Guilder Exchange Rates

These were all based on cross-rates.

(2) Forward Exchange Rates

1931-6: Einzig (1937), Appendix I.
1937-8: Economist.

(page 421B follows)
(3) Prices


Economist, Statist, Board of Trade: U.K. wholesale price index is an average of three completely separate indices calculated by these sources.


(4) Wages


(5) Income Variables (Employment, Unemployment and Production Indices)

International Labour Review: S. Africa (1932-8), Czechoslovakia (1933-8), Germany (1931-8), Italy (1931-8), Japan (1931-8), Poland (1931-8).

League of Nations Monthly Bulletin: all other "income" indices.

(6) Interest Rates

1931-6: Einzig (1937), Appendix III.
APPENDIX III

EFFECTIVE EXCHANGE RATES:

METHODS OF CONSTRUCTION AND DATA

Twelve indices were calculated for each of the six countries based on trade flows in 1928, 1935 and 1938: four indices were based on bilateral trade flows, one for each year and one (the average bilateral) averaging the weights of each year; four were based on shares of total world trade ("global" weights), one for each year and one (the average global) averaging the weights for each year; and four were based on averaging the bilateral and global weights, one for each year and the "composite index" which averaged the average bilateral and average global weights. All the averages were geometric in the case of the pound and arithmetic for the other six countries. The indices were calculated according to the simple formula:

\[ \text{Index in time period } i = \frac{1}{n} \sum w_i R_i^i \]

where \( n \) = the number of countries

\( R \) = the index of the value of a bilateral exchange rate (\( R^0 = 1.00 \))

\( R_i^i \) = the index for an exchange rate in time period \( i \)

\( w \) = a weight

The data presented in the following pages is based on the following key:

\[ \begin{align*}
K & = \text{U.K.} \\
S & = \text{U.S.} \\
C & = \text{Canada} \\
F & = \text{France} \\
H & = \text{Holland} \\
B & = \text{Belgium} \\
W & = \text{Switzerland}
\end{align*} \]
The following indices are 1929 based using a 29 country model except for the pound (1929-30 based, 28 countries). The weights follow the indices.

1 = 1928 weights, bilateral
2 = 1935 " "
3 = 1938 " "
4 = average bilateral
5 = 1928 weights, global
6 = 1935 " "
7 = 1938 " "
8 = average global
9 = average of bilateral and global weights, 1928
10 = " " " " " " " 1935
11 = " " " " " " " 1938
12 = Composite index

The following data is also presented:

13 = (WP)28 country EER
14 = ROW 28 country WP index (used to calculate WP adjusted EER)
15 = (COL) 24 country EER
16 = ROW 24 country COL index (used to calculate COL adjusted real EER)
17 = 7 country EER
18 = ROW 7 country WP index
19 = " " " COL index
20 = " " " "income" index
21 = 7 country real EER (WP adjusted), quarterly.
22 = " " " " (COL adjusted), "
23 = Weights used for all 7 country variables

In addition one further index is presented for the pound only:

24 = 1929 based composite EER (i.e. 1929 based version of K12).
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Argentina: 5.2.1500 1.3.000 4.6.543 5.265 5.347 4.26500

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**Additional Notes:**
- The data seems to be a series of numbers, possibly indicating measurements or values over time. The entries are likely years, with '99' indicating 1999 and '10' indicating 2010.
- The values are not clearly defined without additional context, but they could be averages, totals, or other statistical measures.
- The columns are labeled with letters (C1, C2, etc.), which might correspond to different categories or variables.
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ADDITIONAL BIBLIOGRAPHY
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