Political Processes and Foreign Exchange Markets*

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Abstract: The efficient markets hypothesis implies that the forward exchange rate--the price of the currency deliverable 30 days in the future--should be an unbiased predictor of the future spot exchange rate. Empirical studies, however, conclude that the forward rate is biased. We argue that democratic political processes affect the forward rate bias. In particular, elections, cabinet negotiations, and cabinet dissolutions may generate uncertainty about the future composition of the government and its commitment to the exchange rate. Since economic agents will be less able to predict future exchange rate movements during periods of political uncertainty, the forward bias will increase during these periods. We test the effect of political events on the forward bias using data from eight industrial democracies between 1974 and 1995. Using both ordinary least squares and seemingly unrelated regression, we find that political uncertainty helps explain forward rate bias.

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The behavior of economic agents affects not only overall economic performance, but also the nature of voter choice and control in democratic societies (e.g., Block 1977; Lindblom 1977). The speed with which markets adjust to new information may affect the success of a new policy initiative. For instance, economic agents may anticipate a monetary expansion during periods of high unemployment and adjust their behavior accordingly. Because economic agents have already altered their behavior, the monetary expansion will not have the intended effect of stimulating employment growth. Instead, unemployment will remain the same while inflation will increase (Barro and Gordon 1983).

Market reactions also affect the quality of democracy. If, for instance, markets can anticipate policy changes, they can negate the ability of governments to pursue certain policies (e.g., Alesina and Roubini 1997). In fact, political economists (e.g., Alesina and Sachs 1988; Alesina and Rosenthal 1995) contend that political parties can achieve divergent economic outcomes only by exploiting the uncertainty surrounding elections and campaigns. Consequently, the less predictable an election, the more opportunity for divergent economic policies.

But if political events are predictable, markets may limit the partisan choices available to the electorate and, according to the most extreme view, thwart any meaningful democracy (Freeman 1997a; Freeman 1997b; Freeman, Hays and Stix 1999). With the internationalization of capital and product markets, economic agents have even a wider array of potential responses, and, as a result, can pressure governments to adopt certain policies and institutions, decreasing even further partisan options and democratic accountability. Therefore, the study of market reactions to political processes speaks to fundamental issues surrounding the relationship between democracy and capitalism.

We contribute to this debate by examining the relationship between political events and efficiency in forward exchange rate markets. The efficient markets hypothesis implies that the forward exchange rate--the price of the currency deliverable 30 days in the future--should be an unbiased predictor of the future spot exchange rate. That is, today's 30-day forward rate should, on average, accurately predict the spot exchange rate one month from today.

We argue that political uncertainty will make the forward exchange rate a less accurate predictor of future exchange rate movements. According to rational expectations arguments, economic agents incorporate all available information—both economic and political—when making economic decisions. However, political processes—elections, cabinet formations, cabinet dissolutions, etc.--may generate uncertainty about the future composition of the government and, as a consequence, the government's commitment to the exchange rate level. Therefore, economic agents will be less able to predict future exchange rate movements during these periods of political uncertainty.

The next section discusses forward exchange rates and the market efficiency hypothesis. Section two reviews the current literature on the forward exchange rate bias. The third section lays out our basic hypothesis. The fourth section explains our methodological approaches and results. The fifth section concludes.

Forward Exchange Rates and The Market Efficiency Hypothesis

Foreign exchange rate markets are both deep and wide. Technological innovations and market liberalization have made foreign exchange markets enormous--in 1992, the volume of foreign exchange transactions topped over $1 trillion *each day*. Moreover, economic agents can trade currencies with alarming speed—during the September 1992 E.M.S. crisis, British monetary

authorities expended over two billion dollars to support the pegged value of the pound in only a few *hours*. Similar crises have occurred in Sweden, Italy, France, Brazil, Hong Kong, Russia, and Thailand.

Economic agents can trade two types of contracts on the foreign exchange market: spot contracts and forward contracts.[1] The spot exchange rate is the price for a quantity of foreign currency that will be delivered within two days. Typically, economic agents use the spot foreign exchange market for (i) foreign travel; (ii) foreign investment (stocks, bonds, real estate); and/or (iii) the purchase of foreign imports. Spot prices reflect the exchange rate arrangement chosen by the government. With a fixed exchange rate, a government fixes the value of its currency and maintains that value through the purchase and sale of local currency on the foreign exchange market. Under a floating exchange rate, supply and demand on the foreign exchange markets determines the currency's value.

Forward contracts, on the other hand, specify a price today for delivery of a foreign currency at some point in the future—usually 30, 60, or 90 days ahead. Economic agents will purchase forward contracts in order to hedge against a rise in the price of foreign currency, to profit via interest arbitrage, or to speculate against future currency movements.

One of the fundamental ideas about the relationship between spot and forward exchange rates follows from the efficient market hypothesis (EMH). Fama (1970) defines an asset market as efficient if the price of an asset "fully reflects" all information available within that market. Since traders utilize all past and present information and asset prices reflect this, only "news" or new information can cause a change in the price of an asset. Therefore, profit via speculation or arbitrage should be impossible (assuming that risk-neutral actors populate the markets).

When applied to foreign exchange markets, the EMH implies that the forward rate should be a perfect predictor of the future spot exchange rate. That is, (with all variables expressed as logs) the forward exchange rate (*f*) at time t for delivery at time t+k should equal the expected spot exchange rate (*s*) at time t+k:

$$f_{t,t+k} = E_t(s_{t+k}) \tag{1}$$

where E(•) is the expectations operator given the information set available to actors at time t.

Economists have attempted to test efficiency in the forward exchange rate markets. These tests examine whether the expected exchange rate is the best predictor of the actual exchange rate:

$$s_{t+k} = \alpha + \beta[E_t(s_{t+k})] + \varepsilon_{t+k} \tag{2}$$

where ε_t is a random error which is orthogonal (uncorrelated) to the information set at time t and α and β are parameters to be estimated. Substituting (1) into (2) yields:

$$s_{t+k} = \alpha + \beta f_{t,t+k} + \varepsilon_{t+k}\,. \tag{3}$$

Typically, both the spot and the forward exchange rates are expressed as first differences to avoid problems associated with non-stationary variables:

$$s_{t+k} - s_t = \alpha + \beta[f_{t,t+k} - s_t] + \varepsilon_{t+k}\,. \tag{4}$$

[1] Economic agents can also purchase foreign currency futures.

The Forward Exchange Rate Bias

Given the assumption of risk neutral actors and competitive markets, the EMH implies the following joint hypothesis for equation 4: $(\alpha, \beta) = (0, 1)$. According to the literature, the assumption of rational expectations implies that $\alpha=0$. Additionally, if the forward premium (*f*-*s*) is an unbiased predictor of future changes in the spot rate (s_{t+k}-s_t)—that is, if the equilibrium condition between forward and spot rates (equation 1) holds--then estimates of β should equal one. Estimates of β greater than 1 signify that economic agents had unvalidated expectations of a devaluation; that is, the expected exchange rate (f) for time t+k is greater than the actual exchange rate at time t+k. Values of β less than 1, on the other hand, are explained by models emphasizing expectational errors, a peso problem or a time-varying risk premium on foreign exchange. We discuss these issues below.

Empirical studies of efficiency in the forward exchange market typically estimate equation 4 and treat $(\alpha, \beta) = (0, 1)$ as the null hypothesis. With few exceptions, the findings soundly reject the null hypothesis, indicating inefficiency in foreign exchange markets (e.g., Engel 1996; Lewis 1995). Review articles of the voluminous literature on the forward exchange markets report that hundreds of studies reject the null hypothesis of market efficiency. Moreover, a large portion of these studies find that β is not just statistically different from one but is also negative. Economists label this inefficiency the "forward exchange rate bias."

Economists offer a number of explanations for these findings. Some economists view the forward exchange rate bias as primarily an econometric issue (e.g., Rolle and Yan 1998). The properties of exchange rate data make it difficult to estimate market efficiency accurately. The forward premium, for instance, tends to be more volatile than the actual change in the spot exchange rates. Additionally, exchange rates may enter into periods of high and low volatility, which can last for many years. Further, foreign exchange data may trend over significant periods of time, creating issues of non-stationary data. Economists, therefore, have used highly sophisticated techniques to solve the forward bias problem. Many of these approaches appear to be post-hoc data manipulation. Moreover, the results are often sample-specific.

A second set of arguments suggest that a forward exchange rate bias exists because economic actors' expectations are not be realized. That is, currency traders may expect a policy change that affects the exchange rate, but policymakers may not alter policy for an extended period (Krasker 1980). This explanation, referred to as the "peso problem," takes its name from studies of the Mexican peso. In the early 1970s, currency traders anticipated a devaluation, although policymakers remained committed to maintaining the peso's value. Consequently, the peso sold at a forward discount for a prolonged period. Although the peso was eventually devalued in 1976, the fact that it was not devalued immediately made the forward rate a biased predictor over a finite time period. In general, economists contend that peso problems exist because data samples are not sufficiently long for all possible "states of the world" to be observed.

A third set of explanations contends that the forward exchange rate bias actually represents a risk premium. Scholars have used three different approaches to estimate this risk premium. First, some economists argue that the expected variance of the returns on an asset can account for the existence of a risk premium. That is, if an asset exhibits higher volatility (higher risk), economic actors will demand a higher return for their investment (e.g., Domowitz and Hakkio 1985;

MacDonald and Taylor 1991). These models use ARCH and GARCH error specifications and include arch-in-mean terms. The arch-in-mean term specifies that the mean behavior of an asset is a function of its conditional variance. The inclusion of ARCH/GARCH terms, however, does not improve the performance of forward exchange rate bias models. In addition, these models do not exploit information regarding the variability or risk associated with underlying macroeconomic assets (Froot and Frankel 1990).

A second approach follows either a capital asset pricing model (CAPM) or a portfolio-balance model and examines how economic fundamentals vary or shift over time (e.g., Frankel and Engle 1984; Mark 1985). As with asset-based models of the real exchange rate, however, these models cannot outperform a naïve random walk in out-of-sample forecasting (e.g., Meese 1990).

Third, some scholars have modeled the risk premium using survey data about expectations of economic actors. Froot and Frankel (1989), for example, polled foreign exchange currency traders about their expectations and then included the measure as an independent variable in forward bias regressions. These models, however, have not had much success (Hallwood and MacDonald 1994, pp.244-52).

Interestingly, many of the studies that attempt to model the forward bias as a risk premium informally point to politics as a source of risk. MacDonald and Taylor (1991), for example, argue that the bias of the forward rate for the French franc during the interwar period reflected uncertainty about the government's commitment to the gold standard. Frequent government turnover during the period exacerbated concern that the government would not maintain the franc's value. In a study of the Greek drachma, Christodoulakis and Kalyvitis (1997) demonstrate that the forward bias increased (i.e., the estimated β became more negative) around the April 1992 election. However, few studies attempt to examine the political sources of exchange rate bias.

In contrast, Bachman (1992) does begin to incorporate political factors to explain the forward exchange rate bias. Adopting a variant of Krasker's (1980) peso problem model, Bachman argues that "elections provide investors with news about the country's *probability* of adopting different economic policies that alter the relative value of that country's assets" (1992, p.209; italics in original). Consequently, he contends that elections will change the short-term accuracy of the forward premium only if the "election results are a surprise."

To examine the effect of elections on the forward bias, Bachman modifies the standard efficiency equation (4) to test for differences in the accuracy of the forward premium in the pre- and post-election periods. Rewriting $y_t = \Delta s_t = (s_{t+k} - s_t)$ and $x_t = (f_{t,t+k} - s_t)$, Bachman estimates:

$$y_t = \alpha_0 + \beta_0 x_t + \alpha_1 A_t + \beta_1 x_t A_t + \varepsilon_{t+k} \quad (5)$$

where α_0 and β_0 are the slope and intercept for the twelve months prior to an election and A_t is a dummy variable taking the value 1 for the twelve months after an election. Bachman then tests the null hypothesis that the forward bias is not statistically different before and after an election by testing the joint hypothesis of $\alpha_1=0$ and $\beta_1=0$, which is equivalent to a standard Chow test.

Bachman estimates equation (5) for each election in his sample using monthly data for the 12 months before and after each election. His sample extends from 1974 to 1984 and includes Canada (4 elections), France (2 presidential elections), the United Kingdom (4 elections), and the

United States (3 elections).[2] Bachman rejects the null hypothesis for six out of the thirteen elections, indicating that the accuracy of the forward premium changed significantly from before to after the election. Of these six elections, three were instances of partisan change. But the seven other elections—where there was no evidence of a significant difference--also included four instances of partisan change. Bachman then engages in post-hoc explanation to account for why some election outcomes contained more "news" than other elections. His conclusions, however, are limited by the small sample size and by an understanding of politics that focuses only on elections and partisan change.

Political Processes and the Forward Exchange Rate Bias

The accuracy of the forward premium reflects the expectations of economic actors concerning future exchange rate movements. Economic actors must estimate the government's commitment to the level of the exchange rate. Anticipation that the government will implement policies that might lead to a change in the currency's value—e.g., an expansionary monetary policy—will be reflected in the forward rate. If those expectations are unrealized, then the forward rate will be a biased predictor of future exchange rate movements.

Economic actors incorporate information from a variety of sources into their expectations about the government's commitment to the level of the exchange rate. This information may be economic (e.g., data regarding unemployment or inflation) or political (e.g., the timing of elections or the policy preferences of governing parties). We contend that three political varibables will affect these expectations and, in turn, the forward bias: democratic political processes, government partisanship, and exchange rate commitments.

Democratic political processes can create uncertainty about the character of the government—its leaders, policy priorities, and competence. In presidential systems, the electorate directly selects the identity of the executive and the distribution of legislative seats. In parliamentary systems, voters choose the distribution of legislative seats. If no single party wins a majority of seats, political parties bargain with one another to form a new government.

During these political processes, the composition of the government has the potential to change. Opposition parties may win election or new individuals may be selected to occupy leadership positions. Economic actors may have difficulty identifying the next government and establishing its policy priorities. Consequently, during periods when the identity of the government is, at least in principle, uncertain, the forward exchange rate should be a more biased indicator of future exchange rate movements.

We identify two distinct periods of political uncertainty: the campaign and post-election negotiations. The campaign period includes the time immediately preceding the election. The post-election negotiation period lasts from election day until the new government assumes office. Many economists ignore the post-election period as a source of political uncertainty. But if no party wins a majority of legislative seats in a parliamentary system, post-election bargaining among the represented parties determines the composition of the government, the distribution of cabinet portfolios, and the cabinet's policy objectives. This bargaining typically occurs behind closed doors, out of the public's view. Even after a new government is announced, it may still be unclear what type of coalition bargain has been struck—whether parties made policy compromises or policy trade-offs. The vague language of public coalition agreements often does

[2] Bachman uses the thirty-day forward exchange rate. Both the spot and the forward rate data are for the last day of the month.

not clarify responsibility for policy (Laver and Schofield 1990; Laver and Shepsle 1996; Strom and Lijphart 1993). Consequently, economic actors may still not be able to predict the government's policy priorities even after an election. Even in majoritarian systems where a single party forms the government, the time immediately following the election can be a source of uncertainty if party leaders must still determine the personal identity of key cabinet ministers.

To test this argument, we divide the political process into campaign and post-election negotiations sub-periods. We expect the forward rate bias to be greater during these periods than when the government's identity is certain.

Second, the government's partisanship is likely to influence the policy expectations of economic actors. The party label serves as an informational cue for economic actors, giving them a sense of the government's policy objectives. The partisanship literature traditionally assumes that right parties are more concerned with controlling inflation while left parties place more emphasis on employment and wealth redistribution (Alesina 1989; Alesina and Sachs 1988; Hibbs 1987; Havrilesky 1987). According to the literature, left parties possess little anti-inflation credibility with financial and capital markets (Garrett 1995). Economic actors, therefore, may expect that left governments are less committed to maintaining the level of the exchange rate than right governments. Consequently, the forward bias should be greater under left governments than under right governments.

Changes in the partisan composition of the government will also affect the forward bias. When a new government enters office, economic agents are likely to have some uncertainty about its commitment to the exchange rate, especially if left parties compose the new government. As the government makes policy choices, economic agents will learn about the government's commitment and have more accurate expectations about future exchange rate fluctuations. On the other hand, if the incumbent government is returned to office, economic agents will most likely have a clear sense of the government's policy priorities. Therefore, after an election, we expect the forward rate bias to be greater if the partisanship of the government changes.

To examine how partisanship affects the forward rate bias, we employ a measure of left government strength based on Cameron (1984). The measure multiplies the percentage of cabinet seats held by left parties by the percentage of a legislative majority held by left parties in the legislature for each year in each country. Higher values indicate increased left party influence.

Finally, exchange rate commitments can increase the predictability of future exchange rates. In particular, a fixed exchange rate will decrease exchange rate volatility and consequently, should decrease exchange rate risk (Mundell 1961; McKinnon 1962). More specifically, we examine the effect of participation in the European Monetary System (E.M.S.), a multilateral exchange rate arrangement among the countries of the European Community.[3] Participation committed members to maintaining the value of their currency against participating currencies, particularly the German mark. As a result, the E.M.S. decreased exchange rate volatility within E.M.S. members (Gros and Thygesen 1992). Since E.M.S. membership should increase the predictability of currency movements, we expect that participation in the E.M.S. will decrease the forward bias.

[3] The EMS was founded in 1979. Participation in the E.M.S. was restricted to member states of the European Community, although member states were not required to join. Indeed, Britain chose not to participate in the E.M.S. until 1990 and then withdrew in 1992.

Testing the argument

We employ a two part testing strategy to gauge how political uncertainty, government partisanship, and exchange rate commitments affect the forward exchange rate bias.

Strategy 1: Campaigns and Elections

The first strategy draws on Bachman (1992) and focuses only on the periods surrounding political events. We investigate whether market behavior changes across different sub-periods of the political process. Specifically, we estimated forward bias tests (30-day forward rate) for each election using weekly data for 12 months before and after each election. Thus, for each election we observe 102-104 weeks.

Within each two-year period, we identify four sub-periods: pre-election, the campaign, post-election negotiations, and post-government formation. The pre-election period begins 12 months before the election and lasts until the start of the campaign period. The campaign period begins either the week the election was announced, the week that the government collapsed, or, if these dates were unavailable, four weeks prior to the election date. The campaign period includes the week of the election.

The post-election negotiations commence the week after the election and last until the week that new government takes office. In systems where the new government is easily identifiable from the election results (e.g., a single party holds a majority of legislative seats), this period is often too short for meaningful statistical tests. But where no party holds a legislative majority, this negotiation period can be quite lengthy. Finally, the post-government formation period begins the week after the new government is sworn-in and lasts until 12 months after the election date.

Adding dummy variables for the different sub-periods, we estimated the following equation for each election: (Recall that $y_t = \Delta s_t = (s_{t+k} - s_t)$ and $x_t = (f_{t,t+k} - s_t)$ and that s and f are logged).

$$y_t = \alpha_0 + \alpha_1 C + \alpha_2 N + \alpha_3 A + \beta_0 x_t + \beta_1 x_t * C + \beta_2 x_t * N + \beta_3 x_t * A + \varepsilon_{t+k} \qquad (6)$$

where C=campaign period; N=post-election negotiation period; and A=post-government formation period.

With estimates from each election, we then test two hypotheses. First, we examine whether the efficiency hypothesis can be rejected for each period:

$$H_0: \alpha_0 = 0; \beta_0 = 1$$
$$H_0: \alpha_1 = 0; \beta_1 = 1$$
$$H_0: \alpha_2 = 0; \beta_2 = 1$$
$$H_0: \alpha_3 = 0; \beta_3 = 1$$

In situations where the efficiency hypotheses is rejected, we identify the direction of the forward bias, whether it was positive--indicating that there are unvalidated devaluation expectations—or negative--evidence of a time-varying risk premia.

The second test examines whether the short-term predictive accuracy of the forward premium differs across political sub-periods. We test for structural breaks across the political periods using a Chow test:

$$H_0: \alpha_1 = 0; \beta_1 = 0$$
$$H_0: \alpha_2 = 0; \beta_2 = 0$$
$$H_0: \alpha_3 = 0; \beta_3 = 0$$

Rejection of the null hypothesis indicates that the forward bias changes across the different political sub-periods.

We then examine the number of times we reject the two null hypotheses and tabulate them with different political conditions based on the partisanship of the incumbent government, whether the election resulted in significant partisan change, and whether the country was a member of the E.M.S.

Sample

We estimate equation (6) on 49 elections in eight countries : Belgium (7 elections), Britain (6), Canada (6), France (5 parliamentary elections), Germany (6), Italy (6), Japan (7), and the Netherlands (6). The data include elections after January 1974 and prior to January 1994. Data availability limited the sample size.

Data

The spot and forward exchange rate data consist of weekly prices of the Belgian franc, Canadian dollar, English pound, French franc, German mark, Italian lira, Japanese yen and Dutch guilder denominated in U.S. dollars. Both spot and forward prices are from the Wednesday close to avoid problems associated with bank/national holidays and increased volatility on Mondays and Fridays (Baillie and McMahon 1989). The data are from Harris Bank's weekly review. The authors filled in missing observations with data from Bloomberg Online.

The political data on elections and partisanship were collected by the authors from *Keesing's Archives* and various other sources. The data on exchange rate arrangements is from the International Monetary Fund's *Annual Report on Exchange Arrangements and Exchange Restrictions* (various years), Gros and Thygesen (1992) and Cobham (1994).

Methodology

We estimate the model in equation (6) using weekly data, but the forward rate is quoted for a month in advance. Consequently, we have an overlapping contract/overlapping data problem (Hansen and Hodrick 1980). Each week of the month will produce a forecast error and these errors are likely to be serially correlated.[4] As a result, estimation of the standard errors is not straightforward.

[4] An example will clarify the situation. Suppose two forward contracts are quoted, one at time t and the other at t-1, where t denotes weeks. Although both contracts mature in one month time, the former contract matures at time t+4 and the latter contract at time t+3. The forecast error for the contract signed in period t will be influenced by the forecast error for the time t-1 contract. Hallwood and MacDonald (1994, pp.216-7) provide a discussion.

Most researchers follow Hansen's (1982) strategy of estimating the parameters in (6) via OLS (which will be unbiased and consistent) and correct the variance-covariance matrix using the approach suggested by Newey and West (e.g., Rolle and Yan 1998; Lewis 1995). The Newey-West (1987) estimator of the standard error has an asymptotically consistent covariance matrix that uses a finite number of autocorrelated lags to approximate residual dynamics. As a result, it provides standard errors that are robust to autocorrelated and heteroscedastic disturbances. With weekly data and 30-day forward contracts, we expect a four period moving average error. Consequently, we allow the Newey-West estimator to use four lags. Increasing the number of lags does not change the results.

Results

Appendix table one reports the results of estimating equation 6 for all 49 elections in the sample. The columns labeled Pre-Election, Campaign, Negotiate, and Post-Election show the estimates of β for each sub-period. A star indicates that the null hypothesis of market efficiency, (α, β) = (0, 1), was rejected for that sub-period. Consider the first row. For the 1974 Belgian election, in the pre-election period, the null hypothesis of market efficiency could not be rejected and the estimated β equals –1.28. During the campaign period, however, the null hypothesis was rejected, as indicated by the star. The estimate of β during that campaign was 10.94. For both the negotiation and post-formation periods, the null hypotheses could not be rejected.

Table 1 reports the results of testing for "market efficiency" across different political periods for the 49 elections in the sample. For the pre-election periods, the null hypothesis was rejected 30 times or in 61% of the sample. In the campaign periods, however, the null hypothesis was rejected 42 times or in 86% of the sample. In the post-election negotiations periods, the null hypothesis was rejected 31 out of 38 times (recall that some of the negotiate periods were too short for meaningful statistical tests) or in 82% of the sample. Finally, the null hypothesis was rejected 28 times in the post-government formation periods, about 57% of the sample. While the forward premium is infrequently an accurate predictor of future exchange rate movements, the results indicate that the forward premium is accurate less often during periods of political uncertainty—campaigns, post-election negotiations—than under periods when the partisan and policy composition of the government is clear. This suggests that democratic political processes make it more difficult for economic actors to predict future policy choices.

We divided the 49 elections into three categories based on the partisanship of the incumbent government. We classified governments as Left if they scored a 0.7 or higher on our measure of partisanship; Right if they scored less than 0.3; Center otherwise. Consider the pre-election periods. For left governments, the null hypothesis is rejected in 8 out of 10 cases. For right incumbent governments, the null hypothesis is rejected 19 out of 32 cases or in only about 59% of the observations. This finding suggests that economic actors are less confident that left governments will maintain the value of the exchange rate. During the campaign periods, however, the null hypothesis is rejected about 90% of the time for both left and right governments. While there are only a few center governments in the sample, it is interesting to note that the null hypothesis is rejected 5 out of 5 times for the post-election negotiation periods. Center governments tend to form in non-decisive electoral systems, where post-election negotiations determine the composition of the government. Consequently, the post-election negotiations are often periods of intense political uncertainty as parties bargain to determine the composition of the government.

We also classified the elections based on the outcome—whether there was a partisan shift to the left, to the right, or no significant change. We defined a partisan shift as a change in the partisan

score of more than than 0.2. A partisan change might be taken as a post-hoc indicator of the uncertainty surrounding the campaign. However, the number of times that the null hypotheses was rejected in the campaign period if there was a partisan shift is actually lower—about 79% of the time—than when the incumbent government was returned to office—about 89% of the time. But if we compare the number of times the null hypothesis was rejected in the post-government formation periods, the difference between partisan change and non-change is clear and in the expected direction. In elections where partisan change occurred, the null hypothesis was rejected 10 of 14 times—about 71%. For elections that returned the incumbent, the null hypothesis was rejected in only 51% of the elections (18/35 times). The results suggest that economic actors have more confidence about the policy priorities of continuing governments rather than new governments.

The third comparison divides the sample into elections that were held while countries participated in the E.M.S and elections held outside the E.M.S. (i.e., either in non-participating countries or before the inception of the E.M.S.).[5] Since participation in the E.M.S. represented a commitment to maintain the level of the exchange rate, economic actors should be able to predict future levels of the exchange rate more accurately. Consequently, the null hypothesis should be rejected less often in E.M.S. countries. In fact, this turns out to be true across all political periods. The null hypothesis is rejected less often under the E.M.S. than outside the system. The results indicate that participation in the E.M.S. did make future exchange movements more predictable.

We also evaluate these results by comparing the accuracy of the forward premium across the political sub-periods (Table 2). That is, we examined periods that were "temporally adjacent" to one another: the pre-election and campaign periods (Row 1); the campaign and negotiate periods (Row 2); the negotiate and post-government formation periods (Row 3); the campaign to post-government formation periods (Row 4); and the pre-election to post-government formation periods (Row 5). We then tabulated the number of elections where the null hypothesis was not rejected in both periods (Column 1); where the null hypothesis was not rejected in the antecedent period, but rejected in the subsequent period (Column 2); where the null hypothesis was rejected in the antecedent period, but was not rejected in the following period (Column 3); and, when the null hypothesis was rejected in both periods (Column 4). That is, we want to know if the forward premium became a more or less accurate predictor of future exchange rates from sub-period to sub-period. Entries in column 2 indicate that the forward exchange rate became less accurate in the following periods. Entries in column 3 indicate that the forward exchange rate became more accurate. Entries in column 4 indicate that the null hypothesis of market efficiency was rejected in both periods. Given the poor accuracy of the forward premium, this last column unsurprisingly contains the bulk of the elections.

Nevertheless, the results in Table 2 are highly suggestive. Consider the first row, the transition from the pre-election to campaign periods. In 16 elections—or about 1/3 of the total cases--the forward rate becomes less accurate after the campaign has commenced. Put differently, of the 18 cases where the null hypothesis was not rejected in the pre-election period, the forward rate became less accurate once the campaign commenced in 16 cases—nearly 90% of the time. Further, the accuracy of the forward premium improves from the pre-election to campaign periods in only four cases, a finding that is also consistent with our expectations.

[5] We did not include Germany as an E.M.S. country. Since the German mark represented the "anchor" currency of the system, German economic policy was not constrained by an external exchange rate commitment. Consequently, the E.M.S. did not make German exchange rates any more predictable than if it had been outside the system.

The comparison between the campaign and negotiate is relatively unsurprising. In most cases, the null hypothesis is rejected for both periods. Since both represent periods of political uncertainty, we expect the forward premium to be less accurate.

The negotiate-post government formation and campaign-post government formation comparisons also are consistent with the argument. In 14 cases, the forward bias declines once the new government assumes office. Once a new government takes power, economic actors should have a better sense of the government's commitment to the exchange rate. Overall, the results from table 2 provide support for our intuitions. While the results are not conclusive, the results fit patterns that we expect.

Table 3 reports the number of structural breaks between sub-periods—that is, instances when the estimates of α and β in one sub-period jointly differ from estimates in another sub-period. This test is another way of examining whether the accuracy of the forward premium changes across sub-periods, although it does not indicate whether the accuracy improves or diminishes. This test replicates Bachman (1992), except that we a) employ higher frequency data and b) categorize different sub-periods.[6] As in table 2, we make the same comparisons between sub-periods.

The results are not particularly enticing. For the campaign period, the estimates of α and β significantly differ from the pre-election period 22 times, or in about 45% of the sample. The estimates from the campaign periods differed from the negotiation periods 20/38 times, or in about 53% of the sample. Between the negotiation and post-government formation periods, there are structural breaks 19 out of 38 times or about 50% of the time. The estimates from the campaign period differed from the post-government formation period in 18 instances, about 37% of the time. Finally, if we compare the pre-election and post-government formation periods, the estimates differ significantly for 11 elections, or about 22% of the sample.

Interestingly, the rest of the table reveals no strong patterns based on the partisanship of the incumbent government, the electoral results, or membership in the E.M.S. If we compare the pre-election and post-government formation periods, the estimates are significantly different 20-30% of the time, regardless of how the elections are classified.

The results suggest that politics does affect the accuracy of the forward premium. Economic agents do appear to be less sure about future exchange rate movements during periods of political uncertainty and change.

Strategy 2: Seemingly Unrelated Regression

The last section estimated forward rate bias tests for each election in the sample and limited the period under investigation to the 24 months surrounding the election. In this section, we estimate a model similar to equation 6 for each country in the sample over the period January 1974 – December 1994. Using the continuous time period helps insure that extreme short-term values of the dependent or independent variables do not overly influence the results. Additionally, the continuous sample decreases the risk of obtaining results that reflect peso-type problems. The longer sample period also allows us to include other political variables beyond elections. We estimate these forward bias models simultaneously for all eight countries using seemingly-

[6] Bachman uses Chow tests to see if the accuracy of the forward premium differs in the years prior to and after an election. Using his periodization but our higher frequency data, we were not able to replicate Bachman's findings (results not reported).

unrelated-regression. This technique controls for factors (shocks) in the world economy that may affect the expectations of economic agents in all the major foreign exchange markets.

The Model

We are interested in testing the effect of political processes on the accuracy of the forward premium. In this longer period estimation, we again include categories for the campaign and post-election negotiation periods. These periods are defined as before. We include dummy variables for each period as well as terms interacting the dummy variables and the forward premium.

We also want to gauge the influence of partisanship and participation in the EMS on the accuracy of the forward premium. For partisanship, we include three variables: the partisanship term, an interactive term multiplying partisanship and the forward premium, and a variable measuring the change in partisanship after a change in government. The partisan change measure was created by subtracting the value of the partisanship variable of the incoming government from the partisanship value of the outgoing government.

We expect the partisan parameter to have a positive estimate: according to the literature, left governments are more likely to allow the currency to depreciate. The interactive term should be negative, indicating that economic agents have less certain expectations about future exchange rate fluctuations under a left government. Finally, the change in partisanship variable should have a positive coefficient—the literature suggests that a move toward the left should cause depreciation.

We also argue that participation in the E.M.S. should affect the accuracy of the forward premium. To test this, we include a dummy variable for participation in the E.M.S. and an interactive term multiplying the E.M.S. dummy variable and the forward premium, E.M.S.*(f_{t+4}-s_t). The E.M.S. dummy variable should be negative—participation in the exchange rate arrangement should slow depreciation. The interactive term, on the other hand, should be positive. Participating in the E.M.S. should make exchange rates more predictable in the near future. We also include a dummy variable for realignments and devaluations of the exchange rate within the E.M.S, coded 1 during the week of the devaluation.[7] This variable should have a positive effect on volatility.

In addition to these variables, we also include variables capturing the political uncertainty surrounding a cabinet dissolution. In parliamentary systems, elections are not the only source of political change. A cabinet may dissolve at any time. After a collapse, parties can negotiate to form a new government without calling for new elections. Political economy studies that focus only on elections ignore these important events. To investigate if the uncertainty over a cabinet collapse affects market behavior, we identified cabinet collapses and government formations without an election.[8] The dissolution period begins the week the cabinet dissolved and ends the week a new government is sworn-in. We include a dummy variable, Dissolution, for these periods. We also interacted this dummy variable with the forward premium, Dissolution*(f_{t+4}-s_t). As with the campaign and post-elections negotiations sub-periods, we expect the forward premium to be less accurate during the dissolution sub-period.

[7] Data are from the International Monetary Fund's *Annual Report on Exchange Arrangements and Exchange Restrictions* (various years), Gros and Thygesen (1992) and Cobham (1994).

[8] We defined a cabinet collapse as a change in the party composition of the cabinet or the identity of the prime minister.

Economic actors do not incorporate information about political process into their economic expectations only during discrete periods of political change. Clearly, a cabinet collapse or an election can create tremendous uncertainty about the partisan identity of the next cabinet and, as a result, the future course of economic policy. Consequently, economic actors have an interest in estimating when these events are likely to occur. The expectations of a potential change in the nature of the cabinet—either through a cabinet dissolution or an election--will affect their economic behavior. As economic actors anticipate a cabinet collapse or an election, their expectations about the future course of economic policy will become less certain. Consequently, the forward rate bias should increase as the anticipation of a political event increases.

Elsewhere we have generated a variable that estimates the weekly probability that a cabinet will end due to a cabinet dissolution or election (Bernhard and Leblang 1998; Leblang and Bernhard 1999). The probability that the cabinet will end is a function of five sets of variables: the duration of the cabinet to that point, the time remaining before constitutionally mandated elections, whether the system has exogenous electoral timing, government type, and party system attributes. We include this variable, Political Expectations, in the model and interact it with the forward premium, Political Expectations*(f_{t+4}-s_t). We expect that as expectations of a cabinet collapse increase, the forward exchange rate will become a less accurate predictor of future exchange rate movements.

The model to be estimated is:

$$y_t = \alpha_0 + \alpha_1 C_t + \alpha_2 N_t + \alpha_3 D_t + \alpha_4 E_t + \beta_0 x_t + \beta_1 x_t * C + \beta_2 x_t * N_t + \beta_3 x_t * D_t + \beta_4 x_t * E_t + \gamma_1 EMS_t + \gamma_2 REAL_t + \gamma_3 PART_t + \varepsilon_{t+k}$$

Where C=campaign period, N=negotiation period, D=dissolution period, E=political expectations, EMS=member of the European Monetary System, REAL=currency realignment, and PART=partisan change.

Methodology

We estimate equation (7) using seemingly unrelated regression (Zellner 1962). Seemingly unrelated regression permits the simultaneous estimation of several different equations. It would be possible to estimate single equation models for each country, but we expect that the contemporaneous residuals are correlated across countries. If the error terms are correlated across equations, least squares estimates are no longer unbiased and efficient (Kmenta 1986).

We anticipate cross-equation correlation among residuals for two reasons. First, all spot rates are quoted against the U.S. dollar. If the U.S. dollar experiences a shock, then all of the dependent variables will be affected. Second, the currencies can be considered substitutes. Changes in the supply of or demand for one currency will likely affect the supply of or demand for other currencies.

Estimating these equations together, therefore, provides better statistical efficiency and a theoretically more appealing specification. Breush and Pagan's LM test is used to test the null hypothesis that the correlation matrix across equations is diagonal.[9]

Results

The results from estimating equation (7) using seemingly unrelated regression are contained in table four. The Breush-Pagan LM test strongly rejects the null hypothesis that the residuals are not correlated across countries. Thus, the seemingly unrelated regression approach is preferable to single country OLS. The chi-squared statistic also allows us to reject the null hypothesis for each model that, taken together, the variables are not significantly different from zero.[10]

Before discussing the forward bias tests, first consider the partisanship and E.M.S. membership variables. For partisanship, the left variable and the left*(f_{t+4}-s_t) variables never attained statistical significance (equations including these variables not reported). These results indicate that the government partisanship does not affect the forward rate bias. The partisan change variable, does attain statistical significance for three countries. As expected, leftward partisan change is associated with a currency depreciation in Italy and France. In Japan, however, a leftward shift in the government is associated with a currency appreciation. Nevertheless, government partisanship in Japan was very stable throughout the sample period, so the substantive significance of this result is negligible.

The E.M.S. variables also present mixed results. Membership in the E.M.S is statistically significant in only two cases, Belgium and Italy. But these two estimates have opposing signs: E.M.S. membership led to an appreciation in the Belgian franc versus the U.S. dollar, but a depreciation in the Italian lira. The E.M.S. membership*premium variable never attained statistical significance and was dropped from the equation. Unlike the results from the previous section, participation in the E.M.S. does not seem to affect the forward bias. Surprisingly, the only statistically significant realignment occurred in Britain--during the week of the September 16, 1992 E.M.S. crisis.

Next consider the political sub-periods and their interactions with the forward premium. Here, we perform two tests. First, we test the joint significance of each sub-period dummy variable and its corresponding interaction term using an F-test. This test is similar to the tests for structural breaks in the forward rate bias across political sub-periods we performed above. Columns 1-4 of table five contain the results of these F-tests. Column entries are F-statistics; p-values for rejection of the null hypothesis are in parentheses. Consider first the campaign period. In all countries except Japan and the Netherlands, the campaign period is statistically different from other periods. That is, the forward rate bias during the campaign period differs from other periods. This result supports the findings of the previous testing strategy. The forward bias in the

[9] The seemingly unrelated regression estimator we use iterates over the estimated error and parameter estimates until the parameter estimates converge. This is equivalent to maximum likelihood results. Estimation was performed using the sureg command and the isure option in STATA version 6.0.

[10] We checked the robustness of the results in two ways. First, we re-estimated the Belgian, British, French, Italian, and Dutch equations using the German mark as the center currency and estimated the German, Canadian, and Japanese equations using the U.S. dollar as the center currency. Second, we estimated the system including only European countries using the mark as the center currency. The substantive results did not change in either case.

post-election negotiations period, on the other hand, is significant only for half of the sample: Canada, France, Italy, Japan. The dissolution period constitutes a break for five of the eight currencies: the Canadian dollar, the British pound, the French franc, the German mark, and the Dutch guilder. Finally, the expectations variable and its interaction with the forward premium are jointly significant for all cases.

Second, we test the joint hypothesis that $(\alpha, \beta) = (0, 1)$ for each political sub-period. Columns 5-7 of table five show the estimated beta coefficient. The p-values are for the rejection of the efficiency hypothesis. A star indicates rejection of the market efficiency hypothesis for the particular sub-period. During the campaign period, the null hypothesis is rejected for five of the eight countries: Britain, France, Germany, Italy, and the Netherlands. Again, this result is consistent with the findings in of our previous tests. During the post-election negotiations period, the null hypothesis is rejected in five countries: Belgium, Canada, Italy, Japan, and the Netherlands. These results indicate that the negotiation period also entails political uncertainty.

Finally, the null hypothesis is rejected only twice during the dissolution period, for Germany and the Netherlands. This result is not too surprising in that the dissolution period may not entail significant uncertainty about the government's partisan identity and its commitment to the value of the exchange rate. In Britain, Japan and Italy, for instance, none of the dissolution periods led to a major change in the partisan composition of the government.

Interpreting the effect of the Political Expectations variable on the forward bias is a bit tricky since it is a continuous measure. Rather than summarizing its effect in a table, we display the effect of Political Expectations on the forward premium graphically. In figure one, we plot the expected value of the forward premium across values of political expectations for each country. The x-axis runs between zero and 0.06, since the bulk of the values for the expectations variable lies in that range.[11] The y-axis represents the value of the coefficient on the forward premium (i.e., the β). The plotted values are the estimated value of β given the level of the expectations variable. The graphs also indicate the 95% confidence interval for the expected value of the forward premium.[12] Since the null hypothesis under investigation is $\beta=1$ and not $\beta=0$, we include a horizontal line in each graph indicating the location of B=1.

The expectations variable has a negative effect on the forward bias for all countries. That is, as economic agents become more uncertain about government survival, the estimated value of β becomes more negative. This result is consistent with the argument about political uncertainty.

The confidence intervals allow us to test the null hypothesis that $\beta=1$. When the confidence interval contains 1 (i.e., when it is above the horizontal line), we cannot reject the efficiency hypothesis. We fail to reject the hypothesis only when the expectations of a government end are very high in Belgium and Britain and when the expectations of a government end are very low in France. The results for Belgium and Britain, however, are likely artifacts of the data. In both countries, the expectations variable takes on high values for only a few observations. Consequently, the confidence interval is artificially inflated. The results for France, on the other hand, are more plausible since it has many observations where the expectations variable has low

[11] Only Japan and the Netherlands had values that exceeded 0.06 (.13 and .15 respectively).

[12] The standard error for the interaction is calculated as $[\text{var}(b1)+Z^2*\text{var}(b3)+2*Z*\text{cov}(b1,b3)]^{1/2}$ where b1 is the coefficient on the Political Expectations variable, b3 is the coefficient on the Expectations*$(f_{t+4}-s_t)$ variable, and Z is the Political Expectations variable.

values. When the probability of a government end is low—that is, when economic agents can expect the incumbent cabinet to remain in office—then foreign exchange markets operate efficiently.

Conclusion

Foreign exchange markets are among the deepest and widest financial markets in the world. The volume and speed of these markets may overwhelm any evidence that politics affects economic behavior. Consequently, these markets should be a difficult test to gauge the influence of politics on market behavior. Drawing on an extensive economics literature, we examine how political information influences the relationship between the spot and forward exchange rates. The efficient markets hypothesis states that the forward exchange rate should be an optimal predictor of the future spot rate. A vast majority of studies examining the relationship between forward and spot exchange rates finds that the forward rate is a biased predictor of the spot rate and attribute this to variety of factors.

We argue that political factors help explain the forward rate bias. During periods of political uncertainty, economic agents have less clear expectations about future economic policies. Consequently, the forward bias should be worse during periods of potential political change. Our results largely support the argument. During campaigns, post-election negotiations, and the periods surrounding cabinet dissolutions, the forward premium was less accurate than when the tenure of political leaders was secure. In contrast, we found little evidence that partisanship affected the accuracy of the forward premium. Instead, economic agents may have more information about the policy priorities of specific administrations, rather than simply relying on the party label. Our findings on the importance of the E.M.S. were mixed, but overall seemed to indicate that exchange rate commitments limited exchange rate volatility and made the forward rate less biased.

Our findings lend support to those who argue that politics still matter in a world of global capital. While markets may reduce a government's room to maneuver (Garrett 1998), economic agents respond to potential changes in the composition of the government. Democratic events—elections, cabinet changes, party bargaining—still make economic agents less certain about the future course of economic policy.

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Table 1: Tests of Market Efficiency by Political Sub-Period

		Not Efficient (Null Hypothesis Rejected)				Not Efficient and B<1			
	N	Pre-Election	Campaign	Negotiation	Post-Formation	Pre-Election	Campaign	Negotiation	Post-Formation
All Elections	49	30 [61]	42 [86]	31 [82]	28 [57]	21 [43]	33 [67]	19 [50]	22 [45]
Left Incumbent	10	8 [80]	9 [90]	5 [71]	7 [70]	5 [50]	7 [70]	2 [29]	6 [60]
Right Incumbent	32	19 [59]	29 [91]	21 [81]	19 [59]	12 [38]	23 [72]	11 [42]	14 [44]
Center Incumbent	7	3 [43]	4 [57]	5 [100]	2 [29]	4 [57]	3 [43]	5 [100]	2 [29]
Partisan Change to Left	6	3 [50]	5 [83]	3 [60]	4 [67]	2 [33]	5 [83]	3 [60]	3 [50]
Partisan Change to Right	8	6 [75]	6 [75]	4 [100]	6 [75]	3 [38]	5 [63]	3 [75]	4 [50]
No Partisan Change	35	21 [60]	31 [89]	24 [83]	18 [51]	16 [46]	23 [66]	13 [45]	15 [43]
E.M.S. Member	18	10 [55]	13 [72]	11 [79]	9 [50]	5 [27]	11 [61]	8 [57]	5 [28]
Not E.M.S. Member	31	20 [65]	29 [94]	20 [83]	19 [62]	16 [52]	22 [71]	11 [46]	17 [55]

Cell entries are the number of times the null hypothesis, $(\alpha, \beta) = (0, 1)$, was rejected. Numbers are raw frequencies; entries in square brackets are percentages.

Table 2: Changes in Market Efficiency from Political Sub-Period to Sub-Period

	N	Efficient to Efficient	Efficient to Not Efficient	Not Efficient to Efficient	Not Efficient to Not Efficient
Pre-Election to Campaign	49	2 [4]	16 [33]	4 [8]	27 [55]
Campaign to Negotiation	38	1 [3]	3 [8]	6 [16]	28 [74]
Negotiation to Post-Formation	38	3 [8]	4 [11]	14 [37]	17 [45]
Campaign to Post-Formation	49	5 [10]	4 [8]	14 [29]	26 [53]
Pre-Election to Post-Formation	49	10 [20]	9 [18]	11 [22]	19 [39]

Efficient indicates failure to reject the null hypothesis $(\alpha, \beta) = (0, 1)$. Not Efficient indicates rejection of the null hypothesis. Cell entries are raw frequencies; entries in square brackets are percentages.

Table 3: Structural Breaks across Political Sub-Periods

	N	Pre-Election to Campaign	Campaign to Negotiation	Negotiation to Post-Formation	Campaign to Post-Formation	Pre-Election to Post-Formation
All Elections	49	22 [45]	20 [41]	19 [39]	18 [37]	11 [22]
Right Incumbent	32	19 [59]	14 [44]	10 [32]	11 [34]	7 [22]
Center Incumbent	7	1 [14]	4 [57]	5 [72]	2 [29]	1 [14]
Left Incumbent	10	2 [20]	2 [20]	4 [40]	5 [50]	3 [30]
Partisan Change Any Direction	14	3 [21]	3 [21]	6 [43]	5 [36]	3 [21]
Partisan Change to Left	6	3 [50]	2 [33]	2 [33]	1 [17]	0 [0]
Partisan Change to Right	8	0 [0]	1 [13]	4 [50]	4 [50]	3 [38]
No Partisan Change	35	19 [54]	17 [49]	13 [37]	13 [37]	8 [23]
E.M.S. Member	18	6 [33]	5 [28]	10 [56]	4 [22]	4 [22]
Not E.M.S. Member	31	16 [52]	15 [48]	9 [29]	14 [45]	7 [23]

Cell entries are raw frequencies; entries in square brackets are percentages.

Table Four: Seemingly Unrelated Regression

	Belgium	Britain	France	Italy	Nether	Germany	Canada	Japan
Constant	0.0003 (0.0011)	-0.0033* (0.0012)	0.0029* (0.0013)	-0.0059* (0.0016)	0.0024* (0.0010)	0.0028* (0.0010)	-0.0023* (0.0007)	0.0064* (0.0011)
$(f_{t+4}\text{-}s_t)$	-0.1804 (0.2204)	-0.5960* (0.2819)	0.9272* (0.1736)	0.3491* (0.1818)	-0.1846 (0.1192)	-0.1983* (0.1168)	-1.1377* (0.3671)	0.4723 (0.2848)
Campaign	-0.0012 (0.0012)	-0.0090 (0.0066)	-0.0004 (0.0048)	0.0029 (0.0027)	0.0007 (0.0009)	0.0026* (0.0014)	0.0009 (0.0019)	0.0002 (0.0049)
Campaign*$(f_{t+4}\text{-}s_t)$	0.8542* (0.3129)	-5.7162* (2.4133)	-1.0810 (0.7568)	-0.2678 (0.3235)	-0.0777 (0.3220)	-0.2915 (0.7441)	-1.674 (1.9174)	-1.5827 (1.5932)
Negotiation	0.0010 (0.0009)	----- -----	0.0067 (0.0047)	-0.0049 (0.0039)	-0.0004 (0.0007)	-0.0009 (0.0009)	-0.0027 (0.0024)	-0.0254* (0.0056)
Negotiation*$(f_{t+4}\text{-}s_t)$	-0.3554 (0.2608)	-0.0063 (0.0199)	1.7984 (1.5901)	-1.694* (0.6089)	0.2871 (0.2522)	0.6706 (0.4072)	-2.3462 (1.9252)	-4.4025* (1.3254)
Dissolution	0.0010 (0.0021)	-0.0172 (0.0553)	0.0010 (0.0046)	0.0006 (0.0027)	0.0045 (0.0047)	-0.0005 (0.0171)	0.0009 (0.0059)	-0.0029 (0.0064)
Dissolution*$(f_{t+4}\text{-}s_t)$	0.1687 (0.7583)	0.3856 (10.6658)	-0.6743 (0.7881)	0.2059 (0.4708)	1.2804 (1.1802)	-1.5401 (4.7592)	4.8063 (4.0226)	1.6266 (1.8812)
Political Expectations	0.1503* (0.0511)	0.1138 (0.1408)	-0.1502* (0.0612)	-0.0772 (0.0560)	-0.0342* (0.0131)	-0.0384 (0.0269)	-0.0764 (0.0658)	-0.0750 (0.0506)
Political Expectations*$(f_{t+4}\text{-}s_t)$	-10.9234 (26.7981)	-49.2547 (47.4056)	-45.577* (12.7436)	-31.350* (9.8927)	-16.637* (5.9804)	-2.4670 (11.3522)	-41.6402 (42.0296)	-90.356* (24.5742)

Change in Partisanship	0.0016	-0.0266	0.0123*	0.1632*	0.0021	-0.0032	-0.0043	-0.2678*
	(0.0097)	(0.0203)	(0.0041)	(0.0857)	(0.0068)	(0.0054)	(0.0051)	(0.1426)
Realignment	-0.0011	-0.0492*	-0.0023	-0.0026	0.0017	-----	-----	-----
	(0.0027)	(0.0201)	(0.0036)	(0.0043)	(0.0021)	-----	-----	-----
Member of the E.M.S.	-0.0015*	-0.0034	-0.0008	0.0042*	-0.0001	-----	-----	-----
	(0.0006)	(0.0026)	(0.0008)	(0.0011)	(0.0003)	-----	-----	-----
Root Mean Square Error	0.0309	0.0293	0.0304	0.0291	0.0304	0.0310	0.0300	0.1143
Chi-Squared	48.5701	42.3508	54.8459	73.9288	75.8700	23.9138	65.2211	48.4186
Probability	0.0000	0.0000	0.0000	0.0000	0.0000	0.0078	0.0000	0.0000

Parameter estimates and standard errors are estimated via seemingly unrelated regression. The dependent variable is the monthly change in the nominal exchange rate versus the U.S. dollar using weekly data. (f_{t+4}-s_t) is the forward premium.
The sample period is from March 13, 1974 through November 30, 1994 (1056 observations).
*p<0.05, two-tailed test.
Breusch-Pagan test that the variance-covariance matrix of the residuals is diagonal: chi^2 = 13726.70, Prob. = 0.0000.

Table Five: Hypothesis Tests Based on Table Four

	Campaign	Negotiation	Dissolution	Political Expectations	Campaign	Negotiation	Dissolution
Belgium	11.87*	6.33	0.80	28.17*	0.67	-0.535*	-0.011
	(0.0087)	(0.0966)	(0.8502)	(0.0000)	(0.6111)	(0.0000)	(0.2428)
Canada	11.82*	15.16*	15.45*	34.14*	-2.807	-3.475*	3.662
	(0.0082)	(0.0017)	(0.0015)	(0.0000)	(0.1320)	(0.0083)	(0.1463)
Britain	10.58*	-----	8.54*	20.89*	-6.346*	-----	-0.211
	(0.0142)	-----	(0.0360)	(0.0001)	(0.0078)	-----	(0.2710)
France	36.74*	31.08*	29.46*	35.16*	-0.153*	2.717	0.253
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0018)	(0.1252)	(0.1155)
Germany	7.78*	4.89	9.19*	11.79*	-0.490*	0.472	1.73*
	(0.0508)	(0.1800)	(0.0268)	(0.0000)	(0.0076)	(0.2933)	(0.0164)
Italy	11.28*	15.10*	3.99	16.74*	0.082*	-1.341*	0.554
	(0.0103)	(0.0017)	(0.2629)	(0.0000)	(0.0456)	(0.0006)	(0.1532)
Japan	3.72	45.20*	3.90	18.52*	-1.108	-3.928*	2.10
	(0.2929)	(0.0000)	(0.2727)	(0.0000)	(0.2455)	(0.0000)	(0.4050)
Nether	2.92	2.99	27.25*	44.68*	-0.262*	0.103*	1.093*
	(0.4041)	(0.3938)	(0.0000)	(0.0000)	(0.0008)	(0.0015)	(0.0022)

Notes:

1. Columns 1-4 test for the joint statistical significance of the political variable and its interactions. Column entries are F-statistics; p-values are in parentheses.
2. Columns 5-7 test the efficiency hypothesis. Column entries are the estimated beta coefficient; p values are for the rejection of the efficiency hypothesis.

Appendix Table One: Results from Estimating Equation Six on Individual Elections

Country	Date	Pre-Election	Campaign	Negotiation	Post-Formation
Belgium	6/3/74	-1.28	10.94*	-2.32	2.55
Belgium	4/13/77	-1.21*	7.82*	0.487*	-4.26*
Belgium	12/13/78	2.53	2.19*	-5.19*	-0.02
Belgium	11/18/81	-3.43*	-0.13	-2.24*	-0.23
Belgium	10/9/85	39.72	134.55	-69.00*	4.19*
Belgium	12/16/87	0.0007	-24.12*	-12.00	-51.01*
Belgium	11/20/91	-5.57	19.48*	61.86*	3.29
Canada	7/3/74	-2.27*	0.163*	6.42*	0.153*
Canada	5/23/79	-4.77*	-25.16*	-0.21*	-4.47*
Canada	2/13/80	-7.72*	11.05*	-47.67*	-1.66
Canada	9/5/84	5.84*	-3.26	Ns	-19.52*
Canada	12/23/88	-4.86	28.18*	1.83*	-3..37
Canada	10/27/93	-5.33	-31.02*	Ns	-6.78*
Britain	2/27/74	-1.39	-8.47*	-2.22*	-3.37*
Britain	10/9/74	-2.94*	-4.46*	-6.39	-5.81*
Britain	5/2/79	-14.03*	-3.49*	Ns	1.83
Britain	6/8/83	1.82	-23.40*	Ns	-7.86*
Britain	5/10/87	-8.77*	-64.30*	Ns	-13.16*
Britain	4/8/92	23.88*	-21.89*	Ns	11.69
France	3/8/78	-1.49*	-15.81*	-29.54*	-4.04
France	5/17/81	-1.43*	-1.61	Ns	1.02
France	3/12/86	1.29*	-9.10*	Ns	-2.26*
France	6/8/88	1.11	-7.75*	-286.21	-32.43
France	3/24/93	21.42*	2.01*	Ns	-4.12*

Country	Date	Pre-Election	Campaign	Negotiation	Post-Formation
Germany	10/6/76	-14.22*	-34.26*-	-6.56*	4.25
Germany	10/1/80	0.62	-13.12*	22.66*	-2.94*
Germany	3/2/83	-3.14	-7.52*	8.02*	-28.72
Germany	1/28/87	-0.01*	-118.82*	29.24*	-3.11
Germany	11/28/90	-2.13*	-88.16*	-82.0	-57.33*
Germany	10/12/94	8.68*	-128.32*	68.42	97.17*
Italy	6/23/76	-1.90*	0.09*	-0.70*	0.43*
Italy	5/30/79	-0.69	-8.87*	8.89*	0.96
Italy	5/22/83	-1.43	-5.25*	2.42*	1.01
Italy	5/17/87	-3.29*	-10.56*	-22.64*	-9.63*
Italy	4/1/92	5.86	-5.87	-17.49*	-0.41
Italy	3/23/94	-16.62	-4.76*	-25.89*	-16.01*
Japan	12/8/76	-0.93	-1.73*	122.72*	2.87*
Japan	10/3/79	0.66*	-12.36*	1.88*	-1.96*
Japan	5/18/80	-12.54*	-4.18*	32.74*	-2.87*
Japan	12/14/83	-5.43	-19.92*	-------	-5.01*
Japan	7/2/86	7.23*	700.46*	86.48*	-4.26
Japan	2/14/90	-7.33	55.21	62.21*	10.34
Japan	7/21/93	7.25*	158.79*	-----	10.14
Nether	5/25/77	-0.78*	-4.22*	-17.36*	-8.12*
Nether	5/27/81	-9.40*	-5.04*	-23.47*	7.11*
Nether	9/8/82	-7.78*	-0.37*	-12.84*	-14.20*
Nether	5/21/86	71.64*	-57.63*	-39.86*	23.41*
Nether	9/6/89	9.53*	-257.43*	-10.64*	18.27*
Nether	5/4/94	0.84*	-12.63	15.94	-15.82

Cell entries are the estimated beta coefficient. An asterisk indicates rejection of the efficiency hypothesis.

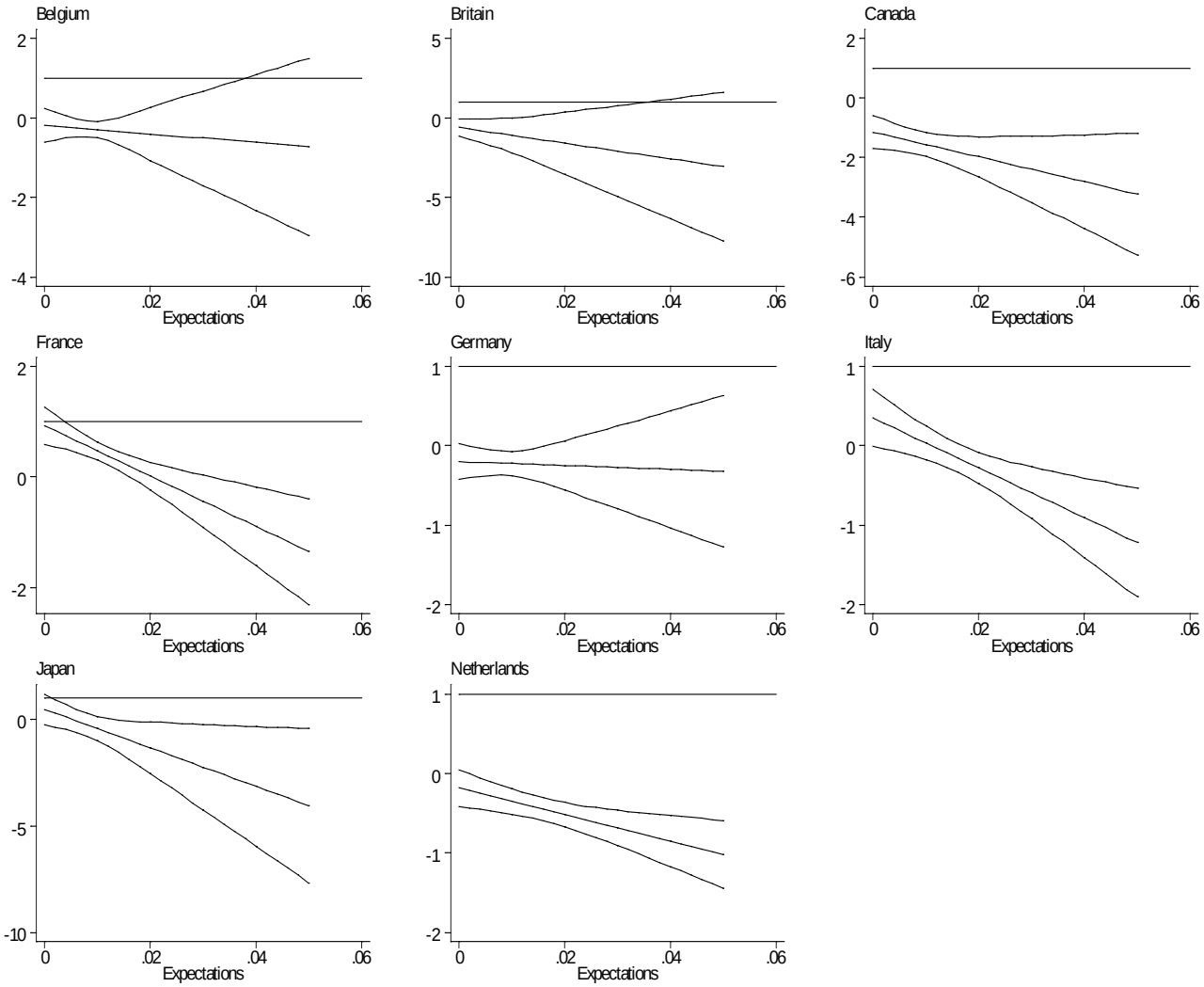

Figure One: Effect of Expectations on the Forward Bias