The impact of currency risk on US MNCs: New evidence from returns and cross-border investment around currency crises

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This paper provides evidence on the valuation implications of currency risk. We analyze whether increases in currency risk for US MNCs following shifts from fixed to floating currency regimes in 23 countries led to increases in currency exposure and changes in cross-border investment decisions. To identify the effects, we use a difference-in-differences design that exploits the larger change in risk for currencies tied to the dollar during the fixed regime than for non-dollar-linked currencies. We conclude that the net exposure to currency risk is significant for US MNCs. However, factors other than currency risk such as strategic considerations are of first-order importance for firms' cross-border investment decisions.

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We provide new evidence that helps us understand the role of currency risk in the equity valuations of US Multinational Corporations (MNCs). Currency risk, defined as the probability that the real domestic currency purchasing power of a foreign currency will differ from expectations,[1] could affect valuations for MNCs, in theory, through a number of channels including assets in place, growth options, operating revenues and costs of operations, exports and imports, and the competitive environment. Despite years of research on this important question, the evidence is surprisingly mixed and weak. One commonly used approach attempts to measure MNCs' exposure to currency risk directly using market data, but has failed to reach a reliable consensus about its significance.[2] A second commonly used approach attempts to understand MNC exposure to currency risk less directly by investigating the valuation implications of cross-border acquisitions, which are a significant channel through which firms acquire exposure. In the cross-border investment studies, researchers seem to have reached a consensus belief that currency risk is of second order importance. A number of these studies find no consistent evidence of significant currency risk effects,[3] and indeed recent studies exclude exchange rates from at least some of their analyses.[4] Given the increasingly international scope of MNC operations, these mixed results, particularly across the two approaches, are troublesome.

We use the currency crisis setting to provide new evidence on the implications of currency risk for MNCs.[5] Since 1990, 23 countries shifted from fixed to floating exchange rates.

[1] See Adler and Dumas (1984), Pakko and Pollard (2003), and Rogoff (1996).
[2] For example, the following studies find that less than 20% of their sample firms have significant exposure estimates: Carter, Pantzalis, and Simkins (2004), Fraser and Pantzalis (2004), Miller and Reuer (1998), and Wei and Starks (2013).
[3] See, for example, Froot and Stein (1991), Harris and Ravenscraft (1991), Cebenoyan, Papaioannou, and Travlos (1992), Kang (1993), Dewenter (1995a, b), Cakici, Hessel, and Kishore (1996), Moeller and Schlingemann (2005), Bhagat, Malhotra and Zhu (2011), and Erel, Liao, and Weisbach (2012).
[4] See for example, Dos Santos et al. (2008); Ferreira et al. (2010); Francis et al. (2008); Karolyi and Liao (2013); and Martynova and Renneboog (2008).
[5] Other papers that use specific events to measure currency exposure include Bartov, Bodnar, and Kaul (1996), Bris, Koskinen, and Nilsson (2009), Forbes (2004), Dewenter, Higgins, and Simin (2005), and Ihrig and Prior (2005).

These regime shifts, often referred to as currency crises, facilitate research designs in the two commonly-used approaches for evaluating exposure that can yield new insights about the valuation implications of currency risk. The crisis events are advantageous because they are easily identifiable events that potentially cause a large and discreet change in currency risk, yet they are exogenous to the foreign firms doing business there. Crucially, some of the fixed-rate currencies were tied to the US dollar and some were not. For a US MNC doing business in one of the 15 countries where the currency was tied to the US dollar ("tied-$"), the shift to a floating regime causes a substantial, discreet jump in currency risk. Of the remaining eight countries, five were part of the Exchange Rate Mechanism (ERM) that governed the European Currency Unit (ECU), and three were tied to a basket of currencies. Regime shifts in these countries ("tied-other") merely shift the risk from uncertainty about changes in the original base currency (or basket) to the now-floating host country currency. The distinction between tied-$ and tied-other countries allows us to distinguish currency risk effects from other confounding factors that could affect returns on the regime shift date, referred to as the "peso problem."[6]

We capitalize on the inherent advantages of the currency crisis setting through several other sample selection and data measurement choices. First, we collect a comprehensive sample of firms that engage in cross-border acquisition activity into at least one of the crisis countries in the six-year periods around the regime shifts. This sample reduces noise by excluding firms with immaterial or indirect currency exposure and it also increases power by inducing important cross-sectional variation in exposure to individual country currencies. Second, we carefully identify when capital markets started to anticipate each regime shift using the literature on warnings of currency crises. We use these periods to improve identification. Third, we introduce

[6] See Evans (1996) for a formal discussion of the peso problem and Sill (2000) for specific applications in exchange rate tests. Similar conceptual issues are referred to as truncation dilemma and revelation bias in Bhagat, Dong, Hirshleifer, and Noah (2005).

to the currency risk literature a new method to identify operating presence derived from Exhibit 21 of the firm's 10-K, which lists subsidiaries by location at the country level. Finally, we identify whether the sample firms use derivatives to financially hedge their currency exposure. Thus, among our sample of firms with exposure to the crisis countries, we can use the distinction between hedgers and non-hedgers, and between tied-$ and tied-other regimes, to conduct difference-in-differences (DID) tests that increase our power to indentify currency risk effects.

When we analyze market data to infer the significance of currency exposure, we show that the estimated absolute value of the one-day abnormal return on the regime shift date is 1.78% for firms with exposure to countries that move from a tied-$ to a floating regime. This one day return is significantly different from zero and significantly larger than the estimate of 1.51% for firms with exposure to tied-other countries, consistent with a larger shift in currency risk for the tied-$ countries. Our DID analyses, pairing the tied-$ versus tied-other distinction with a split of firms based on proxies for operational and financial hedging and controlling for firm and country-level effects, suggest that approximately 40% of the regime shift abnormal return is due to the increase in currency risk. While hedging, either operationally or financially, offsets the exposure, the net currency risk positions are value-relevant.

These findings build on the evidence in existing returns-based studies that have produced a mixed set of findings. The early returns-based tests (e.g., Jorion, 1990) were unable to document significant exposures in the data, resulting in a "puzzle" given that currency risk, in theory, should affect firm value. A large literature developed to address this puzzle and pursued two broad explanations: 1) an economic one that firms effectively insulate themselves from currency risk through financial or operational hedges; and 2) a statistical one that the tests lack power. Studies have attempted to address the statistical issues by improving on the model

specification, sample identification, exchange rate measurement, control variable selection, and the like.[7] Bartram, Brown, and Minton (2010) make a significant contribution by showing that accounting for possible operational hedging activities (i.e., pass through to customers) and financial hedging leads to reasonable estimates of firm exposure predicted by theory, which diminishes the "puzzling" aspect of the earlier findings. Our evidence confirms the hedging effects in Bartram et al (2010), but also shows that on average, across all firms with exposure, shifts in currency risk are associated with statistically and economically significant shifts in multinational firm value.

When we use the second common approach to understanding the valuation implications of currency risk and attempt to infer implications indirectly based on cross border investment activity, we conduct two separate analyses. We first document that US MNCs are 1 to 1.7% more likely to establish a new overseas operating presence in tied-$ countries than tied-other countries during fixed exchange rate regimes. This finding is consistent with relatively lower currency risk in tied-$ countries for US MNCs, but it is economically modest and only weakly statistically significant. A falsification test during the floating rate regime suggests that our finding during the fixed period is not due to systematic differences across the tied-$ and tied-other countries. The second analysis is an event study of cross-border acquisition announcement returns. We find no significant evidence that currency risk affects the size of announcement date

[7] See Choi and Prasad (1995), Choi, Hiraki and Takezawa (1998), and Priestly and Odegaard (2007) for examples of studies that differentiate appreciating versus depreciating currencies; Dominguez and Tesar (2006), Chow, Lee and Solt (1997a,b), Chow and Chen (1998), Griffin and Stulz (2001), and Muller and Verschoor (2006, 2007) for examples of studies that estimate exposure over different time horizons; and Bartov and Bodner (1994) for a study that uses lagged exchange rates. See Linck (1999) for an example of using firm-specific currency indices and Fraser and Pantzalis (2004) and Williamson (2001) for an example using industry specific currency indices. For examples of studies where exposure estimates are related to hypothesized determinants of exposure, see: Bergbrant, Campbell, and Hunter (2014), Dominguez and Tesar (2001, 2006), Doidge, Griffin, and Williams (2006), and Marston (2001). Examples of studies that use conditional asset pricing tests include Jorion (1991), who does not find significant currency exposure; Dumas and Solnik (1995), De Santis and Gerard (1998), Choi, Hiraki and Takezawa (1998) for evidence from developed markets; and Carrieri, Errunza and Majerbi (2006a,b) and Carrieri and Majerbi (2006) for evidence from emerging markets.

abnormal returns. Overall, we interpret the evidence as indicating immaterial effects, consistent with the perceived wisdom that currency risk is of second order importance in cross-border investment decisions.

This "no-results" finding is noteworthy because our setting, sample, and test design mitigate concerns about power. We use a common sample of US MNCs to conduct both the regime shift event study tests and tests of cross-border investment activity. We use the same experimental setting and exploit the carefully measured time periods around the regime shift as well as the distinction between the tied-$ and tied-other countries. This sample and setting have sufficient power in the event study tests to find significant evidence of currency exposure, but find limited or no evidence of exposure in the cross border investment tests. Thus, we can interpret the lack of significant evidence in the cross border tests as indicating no relation, rather than the result of insufficient power or poor test design.

We interpret the combined evidence across the three sets of analyses as follows. Strategic considerations take precedence over currency risk in a firm's cross-border investment decisions. The investment, if made, nonetheless subjects the firm to currency risk. The risk exposure is mitigated for firms that hedge operationally or with financial derivatives. One explanation consistent with this combination of findings is that MNCs anticipate hedging currency risk when making their cross-border investment decisions, which is why the raw, unhedged risk is not a significant determinant of their investment strategy.[8]

[8] This conclusion is consistent with Sercu and Uppal (1995) who conclude in their textbook: "Note, however, that it is much cheaper and more efficient to hedge against exchange risk with financial claims than by changing operations. More importantly, it is much easier to undo financial hedges than to undo investment decisions. Thus, one should not make sub-optimal real investment decisions simply because they reduce exposure to exchange rates. Rather, one should choose the optimal real decisions and then use financial instruments to hedge these operations against exchange risk." (p. 509)

1. Overview of setting and sample

Between 1990 and 2008, 31 countries with a fixed par value currency experienced a speculative attack in the foreign exchange market ("currency crisis") that led the Central Bank to allow its currency's value to be set on the open market.[9] We use 23 of the 31 countries (Table 1), dropping eight with insufficient cross-border investment activity (Jamaica, 1990; Peru, 1990; Papua New Guinea, 1994; Algeria, 1996; Moldova, 1998; Democratic Republic of the Congo, 2001; Egypt, 2003; and Iceland, 2008). Column (2) reports the date on which each country's currency started to float. Column (3) reports the date in the local country that the Central Bank announced the regime shift. Column (4) provides the first trading date in the US following this announcement. For most countries, the announcement date and the first US trading date coincide. None of the first US trading dates occurred on the same day.

Prior to their respective crises, 15 of the 23 currencies were tied to the US dollar, five were part of the ERM, and three were tied to a basket of currencies (Table 1). For a US MNC with exposure to a currency tied to the US dollar ("tied-$"), currency risk during the fixed regime relates only to differentials in inflation rates, usually of minor magnitude. When the currency starts to float, the US firm faces the risk of continuous currency rate changes. Hence, the shift from a fixed-tied-$ regime to a floating regime causes a substantial, discreet jump in currency risk. In contrast, for a US MNC with exposure to a currency tied to a currency other than the US dollar or to a basket of currencies ("tied-other"), the regime change merely shifts the risk from uncertainty about changes in the original base currency (or basket) to the now-floating

[9] We used the IMF "De Facto Classification of Exchange Rate Regimes and Monetary Policy Frameworks" to identify countries with floating rate regimes as of 2008 (http://www.imf.org/external/np/mfd/er/2008/eng/0408.htm). We exclude nine countries from of the list of 31 fixed to floating regime shift crises because we cannot confirm a specific date for the shift; the shift was from a multi-tiered system with some transactions subject to fixed rates and some to floating; or the country quickly adopted a crawling peg or managed float. The nine excluded countries are Somalia, Uganda, Kenya, Romania, Pakistan, Paraguay, Burundi, Serbia, and Ukraine.

host country currency.[10] Because we rely on the tied-$ versus tied-other setting to conduct our analyses, systematic differences between the two types of countries could confound the results. Below, within the specification discussion for each test, we describe the variables that we include to control for any potential differences between the two sets of countries.

Another important feature of our analyses is careful specification of the time period over which the crisis was anticipated.[11] We designate a specific calendar month when anticipation started. The details of the process are described in Appendix A. In summary, we rely on the literature on warnings of currency crises to identify when capital markets started to anticipate the regime shift, assuming that this timing reflects the beliefs of MNC managers and investors. The warning signs include changes in Central Bank reserves, stock price levels and volatility, GDP, and inflation.[12] We also read press accounts during and after the crises to reach a consensus on the month when markets could begin to anticipate a shift. We define the period from the anticipation start date to the regime shift date as the "*anticipation*" period. The length varies by country, ranging from 10 days for the Philippines to about 12 months for Chile and Uruguay (Table A1). The mean anticipation periods for tied-$ and tied-other countries are 154 days and 123 days, respectively (untabulated), which are not significantly different.

As a simpler measure of anticipation, we also assume greater anticipation when a country's regime shift followed other countries within a regional crisis. Table 1 reports the countries by regional cluster and crisis date. The first country within the region to have a crisis is the leader (designated with an "*" in Table 1); the other countries in the region are followers.

[10] We measure currency volatility over one-year and six-month windows at the end of the fixed neutral and the beginning of the float neutral periods (defined below), and find that the change for tied-$ currencies is significantly larger at the 10% level.

[11] Controlling for anticipation is necessary to address peso risk effects. Sill (2000) defines peso risk as "the possibility that some infrequent or unprecedented event may occur (that) affects asset prices (and), the event must be difficult, perhaps even impossible to accurately predict…" (p4). We discuss peso risk in more detail in section 2.2.

[12] See Eichengreen, Rose, and Wyplosz (1995), Kaminsky, Lizondo and Reinhart (1998), Osakwe and Schembri (1998), Edison (2003), and Fiess and Shanker (2009).

Figure 1 summarizes periods around the crisis event date, in addition to the anticipation period, that we use in various tests. The periods are: (1) The fixed "*neutral*" period, which is the two-year period during the fixed regime preceding the anticipation period; (2) the anticipation period; (3) the announcement date; (4) the "*stabilization*" period during which markets were adjusting to the new currency regime; and (5) the floating "*neutral*" period, which is the two year period following currency stabilization. We define the end of the stabilization period as the first peak of a rolling thirty day local currency volatility time series starting at the regime shift date, with volatility falling afterwards. The stabilization periods range from about 1.5 months for Norway to five months for Indonesia and Poland. The mean stabilization periods for the tied-$ and tied-other countries (92 and 71 days, respectively) are not significantly different.

An important feature of our analysis is the use of a common sample across the tests and our sample selection procedures. We use the SDC database to obtain a sample of all US MNCs that announce an acquisition in one of the 23 crisis countries in the three years before and two years after the regime shift year. We eliminate acquisitions by (or of) financial institutions. The data collection and cleaning process yields a sample of 687 unique US firms that make 1,164 acquisition announcements in the six years surrounding the 23 shifts from fixed to floating exchange rates.

2. Regime shift announcement date event study

2.1 Event study sample

For each crisis, we use a sub-sample of the 687 US MNCs, previously described, that are likely to have exposure to that crisis country on the date the country announces its regime shift. Including only the US MNCs with exposure reduces noise in the estimates of the currency risk

effects. We use Exhibit 21 of firms' annual 10-K filings to determine operating presence, which is a new way to indicate exposure.[13] Regulation S-K §229.601 requires firms to "List all subsidiaries of the registrant, the state or other jurisdiction of incorporation or organization of each, and the names under which such subsidiaries do business." For each crisis, we include any of the 687 MNCs that list a subsidiary in the crisis country as of the last 10-K filed during the fixed rate regime.[14] The resulting sample has 1,314 firm-country pair observations.

Operations in a country are not the only source of exposure for US firms. Exposure can result from monetary transactions denominated in the foreign currency, such as receipts on export sales or payments for imports, or from changes in the value of domestic real assets due to changes in international demand and supply conditions caused by currency changes. Limiting the sample to US MNCs with an operating presence is thus overly restrictive. Having consistently reported data, however, results in a reasonable-sized sample with a reliably measured probability of having exposure to the crisis country currency.

Table 2 Panel A reports characteristics of the MNCs. Of the 1,314 observations, 913 or 69.5% are associated with crises in countries with currencies tied to the dollar, dominated by Mexico, Brazil, and Argentina. The remaining 30.5% are associated with crises in tied-other countries, primarily the UK, Italy, and Poland. The MNCs with operating presence in the tied-$ countries are not significantly different at the 5% level from the MNCs with operating presence in tied-other countries in terms of size, market-to-book ratio, sales growth, market leverage, cash flow, R&D, foreign sales percentage, financial hedging activity measured by an indicator for

[13] We are grateful to Scott Dyreng for providing Exhibit 21 data. Missing data were supplemented with hand data collection. We also tried to identify operating presence using annual report geographic segment data. Because most firms report segment information at the regional level, we are only able to identify exposure at the regional level, defining a crisis country's region using the MSCI regional indices. This alternative classification system resulted in a sample of only 217 firm-region pair observations, an insufficient number to conduct reliable DID analysis.

[14] We examine potential presence in each of the 23 crisis countries for all 687 MNCs (i.e., 23 x 687 pairs where financial statements are available), regardless of whether the firm made a cross border acquisition into that country.

derivatives use, or operating hedging activity as measured by diversification, specifically the number of geographic segments.[15] (Appendix B describes the measurement of these firm characteristics. Internet Appendix Table IA.1 provides the country-level values of the Table 2 variables.) Because these characteristics do not differ between the MNCs that invest in tied-$ and tied-other countries, they should not induce systematic differences in measurement error in the abnormal return metric.

2.2 The model

To generate estimates of the change in firm value associated with the change in currency risk due to the regime shift, we separately estimate a short window and long window version of the following OLS model:

$$|AR_{ij}| = \alpha_1 TIED\$ + \alpha_2 TIED_{OTHER} + \sum_c \theta_c Control_j + \epsilon_{ij} \quad (1)$$

where $|AR_{ij}|$ is the absolute value of the abnormal return of sample firm *i* in the regime shift window for country *j*. *TIED$ (TIED$_{OTHER}$)* = 1 if the observation is for a regime shift in a tied-$ (tied-other) country and equals 0 otherwise. $Control_j$ is a vector of firm- and country-specific variables (described in Section 2.3). We estimate eqn. (1) using the panel of firm-country pairs from the 23 regime shift dates. Because each firm can enter eqn. (1) multiple times, we cluster standard errors by firm.

Our goal is to be able to interpret the coefficient estimates on *TIED$* and *TIED$_{OTHER}$* ($\alpha_1$ and α_2) as the valuation effects associated with the change in exposure to currency risk due to the change from a fixed to floating regime. To this end, we need to deal with confounding effects in the return window, which we refer to as the "peso problem." The abnormal return

[15] Cash flow, R&D, and hedging are weakly significantly different at the 10% level.

reflects changes in expectations about the exposure to currency risk, which is the effect we want to isolate, but it also reflects changes in expectations about the probability that the currency regime will shift, and changes in expectations about real effects on cash flows and discount rates. Real effects include cash flows from existing assets, investment opportunities, and growth, which can change due to changes in expectations about the country's *current* economic conditions and/or the magnitude of the country's *upcoming* economic problems.

Changes in expectations about the probability that the currency regime will shift are a concern in the short window analysis but not in the long window analysis. In the long window analysis, the abnormal return is computed over the entire anticipation period such that the expectation of a regime shift goes from close to zero and stable (at the end of the fixed neutral period) to one on the regime shift day, thereby encompassing any effects from changes in the expectation of a regime shift. In fact, the long window analysis is undertaken primarily as a means to address the peso risk problem. In the short window analysis when the event window equals 1-day, we add specific controls for the change in the market's expectation of the probability of a regime shift as discussed below. We present results for both windows, but place more weight on the long window results because we are more confident that using the anticipation period window mitigates the peso problem without inducing additional measurement error relative to the short window controls for regime shift anticipation.[16]

To control for changes in expectations about real effects, we include a comprehensive set of country- and firm-level characteristics (described in Section 2.3) that proxy for the magnitude of real effects. We include the variables primarily so that we can interpret the magnitudes of the intercepts (α_1 and α_2) as the conditional average valuation effect associated with the expected

[16] Also, exposure estimates are stronger with longer horizons, e.g., Dominguez and Tesar (2006), Forbes (2004), and Griffin and Stulz (2001).

change in exposure to currency risk. We do not expect changes in expectations about real effects to be a significant concern for the inferences for two reasons. First, as previously noted, none of the country- and firm-level control variables we examine are systematically different across the tied-$ and tied-other countries. Second, because we use a DID approach, we only need to control for factors that potentially *changed differently* across the tied-$ and tied-other countries within the event window, putting less pressure on the control variables.

2.3 Variable specifications

In the short window specification, $|AR_{ij}|$ is the absolute value of the event day abnormal return, computed as the raw return minus the return of the corresponding CRSP cap-based-decile index return. Bodnar and Wong (2003) argue that value weighted market portfolios overweight large firms, which tend to be exporters, but equal weighted portfolios overweight smaller firms, which tend to be importers. They recommend using market-capitalized-matched portfolios.

The event day is the first US trading date on or after the regime shift announcement as reported in column (4) of Table 1. The average absolute value of the one-day abnormal return is 0.0169.[17] This amount reflects all of the effects we discussed above; i.e., changes in currency risk, the probability of a regime shift, and expectations about current and future economic conditions. Our DID experimental design is constructed to isolate the effect of the shift in currency risk.

In the long-window specification, the absolute value of the abnormal return is computed from the beginning of the anticipation period through the regime shift date. Because the length

[17] This value is comparable in daily magnitude to the two-day absolute cumulative abnormal return of 2.71% for US firms with operations in Mexico and Thailand around their regime shift announcements (Dewenter, Higgins, and Simin, 2005). As further context, Forbes (2005) reports 60 day CARs between -20% and -35% during the Asian/Russian currency crises for firms with direct trade exposure to those countries. Ihrig and Prior (2005) find returns of -1.7% during "crisis" months compared to -0.74% during periods of "normal" exchange rate fluctuations.

of the anticipation period varies across countries, we normalize each firm's long window abnormal return by the number of days in the respective country's anticipation period. The average absolute value of the long window returns is 0.002 and for the unadjusted long window returns is 0.146. For robustness, we also normalize returns using the method outlined in Dodd and Warner (DW, 1983) and used in Hess and Bhagat (1986). Inferences with this method are similar. We point out any differences in the text.[18]

We analyze absolute values of the abnormal returns because the regime shift could have a positive or negative effect on the US MNC's value depending on the source of the firm's exposure. Using absolute values of exposure estimates is common in the literature because it is difficult to determine whether a given firm is a net exporter or importer from available disclosures in the annual 10-K about existing operations and transactions.[19] In addition, US MNCs are likely to have exposure due to unobservable competitive effects.[20]

Table 2 Panels B-D provides descriptive statistics for the control variables. We estimate two versions of eqn. (1) – a "Parsimonious" model and a "Full" model – that include different control variables as discussed below. All control variables are measured as of the fiscal- or calendar-year-end prior to the crisis date.

In the short window analysis, we include four variables to control for anticipation of a

[18] Inferences from results using non-normalized abnormal returns are the same. See Internet Appendix Table IA.5.2.
[19] Approximately half of our sample firms have directionally positive abnormal returns on the regime shift date. Carter, Pantzalis, and Simkins (2004), Choi and Prasad (1995), Dewenter, Higgins, and Simin (2005), Faff and Marshall (2005), and Pantzalis, Simkins and Laux (2001) all use absolute values. Other studies, such as Bartov and Bodnar (1994) and Pritamani, Shome and Singal (2004) use screening methods to limit their samples to firms with only net positive or net negative exposures. Internet Appendix Table IA.4 reports additional tests based on attempts to distinguish importers from exporters based on the sign of exposure estimates, but the resulting sub-samples of firms with clear classifications were too small to conduct meaningful analyses.
[20] Internet Appendix Table IA.5.1 shows that a square root transformation of the AR does not affect our inferences, as suggested by the practice of using the square root of the absolute value of traditional beta estimates of exposure to control for truncation bias (Dominguez and Tesar, 2006). Internet Appendix Table IA.5.3 provides Table 3 results with the DW normalization.

regime shift (Table 2 Panel B).[21] The first proxy is an indicator variable equal to 1 if the Reuters report of the regime shift described it as anticipated or expected, and zero otherwise. The second proxy is an indicator variable equal to 1 if the country is designated as a regional follower (Table 1), assuming that regime shifts in the follower countries were more likely to be anticipated.[22] The third proxy is an indicator variable equal to 1 if the country experienced an unusual currency reserve drop in the three months prior to the crisis. Prior literature shows this condition can signal an impending crisis, although not necessarily a regime shift. These three proxies are specified such that they are positively associated with regime shift anticipation. The final proxy is an indicator variable equal to 1 if there was at least one currency devaluation in the two months prior to the regime shift announcement date.[23] The effect of devaluations on investors' expectations about a regime shift is ambiguous. Prior devaluations could indicate the likelihood of a regime shift is higher because there is clear pressure on the currency and the Central Bank is showing some flexibility. Alternatively, prior devaluations could suggest a lower likelihood of a regime shift if the Central Bank's new par value accurately reflects market conditions. As shown in Table 2 Panel B, we cannot reject that any of the four pairs of means of the proxies for the change in the probability of a regime shift for the tied-$ and tied-other countries are equal.[24]

[21] See Berg et al. (2005) for a review of models that predict currency crises. See Osakwe and Schembri (1998), Edison (2003), Kaminsky, Lizondo, and Reinhart (1998), and Fiess and Shanker (2009) for empirical evidence on warning signs. See Eichengreen, Rose, and Wyplosz (1995) for evidence suggesting that there are no consistently reliable warning signs that a Central Bank's response to crisis conditions will be a shift in its exchange rate regime.

[22] The average absolute values of the short window returns are significantly higher for firms in leader countries (untabulated). The difference suggests that the short window returns in follower countries understate the full revision in firm value associated with the change in currency risk due to greater anticipation of the announcement.

[23] Robustness tests related to these proxies include the following: Internet Appendix Table IA.6.2 presents results with an indicator variable = 1 if the anticipation period is greater than two months as an alternative control for the reserve change. Internet Appendix Table IA.6.3 presents results with an indicator variable = 1 if there was at least one devaluation in the year prior to the regime shift announcement date as an alternative control for devaluation. Internet Appendix Table IA.6.1 shows that results are robust to including these four proxies in the long window analysis.

[24] See Internet Appendix Table IA.1.2 for country-level values.

In the long window analysis, neither model includes the four proxies for the change in the probability of a regime shift on the event date. As discussed previously, an underlying assumption of this analysis is that the probability of a regime shift changes from zero (or close to zero) and stable to one during the event window.

Table 2 Panel C summarizes the proxy variables intended to control for changes in expectations about the magnitude of real effects on cash flows and discount rates. The Parsimonious model includes two summary measures. In the short window analysis, *LOCALRET1* is the change in the value of the local stock exchange index on the regime shift announcement date, which is intended to summarize all value-relevant news other than the change in currency risk. The second proxy, *CURRΔ1,* is the one-day change in currency value on the first day that the currency floats (multiplied by -1).[25] We expect that larger first-floating-day movements away from the fixed par value may signal the economy has a larger adjustment ahead than the government and investors originally anticipated and cause investors to update their prior expectations about the magnitude of the country's current and upcoming economic problems and the associated real effects on firm cash flows and discount rates. The change of 0.097 for the tied-$ countries is significantly larger (at the 5% level) than the one-day change of 0.022 for the tied-other countries. In the long-window analysis, we use similarly constructed summary measures, *LOCALRETFULL* and *CURRΔFULL,* computed as the change in the value of the local stock exchange index and the change in currency over the anticipation period.[26]

The Full model includes two additional proxies for changes in expectations about the

[25] Another commonly used metric to gauge the magnitude of the crisis is ex post changes in interest rates. However, a consistent and comparable interest rate (or index) for which we can measure the one-day change across countries is not available.

[26] See Internet Appendix Table IA.6.4 for a discussion of whether these measures should be normalized and for results when we use normalized measures. The local stock exchange index is not available for Ecuador or Uruguay resulting in a loss of 52 observations in the Full model specifications that include this variable.

country's current and upcoming conditions.[27] The first is the inflation rate for 12 months prior to the regime shift announcement date. According to Purchasing Power Parity (PPP), this rate provides an indication of probable on-going strength or weakness of the new currency vis a vis the dollar. The second proxy is an indicator variable that directly controls for reforms announced concurrently with the regime shift announcement. We conducted an in depth review of the announcements for the 23 regime shifts. We identified two categories of significant changes that were announced concurrently: 1) other government reforms such as revised fiscal spending or new interest rate policies; and 2) senior government personnel changes. We create an indicator variable equal to 1 if either change is announced concurrently with the regime shift announcement (*CONCANNCS*).

The Full model also includes country-level and firm-level control variables. The country characteristics are drawn from the cross-border investment literature: economic freedom, legal system origin, investor protection, liquidity in the takeover market as measured by M&A volume and frequency, and GDP growth (Bris and Cabolis, 2008; Ferreira et al., 2010; Martynova and Renneboog, 2008; and Moeller and Schlingemann, 2005).[28] (See Table 2 Panel D). These characteristics control for the possibility that similar crises will have different valuation implications as a function of the country's institutions or macro-economic conditions. The only difference is observed in average cross-border M&A frequency measure, which is significantly lower for tied-$ countries. The firm-level controls are the nine variables that were discussed in section 2.1 (see Table 2 Panel A). These firm characteristics do not differ across the tied-$ versus tied-other countries and as such should not introduce systematic differences in measurement error which could bias the *TIED$* and $TIED_{OTHER}$ coefficient estimates. Nonetheless, we include

[27] The results are robust to alternative specifications for both variables (Internet Appendix Tables IA.6.5 and 6.6).
[28] Some of these papers also use indices for anti-corruption and credit ratings in their cross-border investment analyses. Since both variables are highly correlated with the economic freedom measure, we do not include them.

them in the specifications as proxies for the firm's underlying currency exposure, directly as in the measure of foreign/total sales, and indirectly through their effect on incentives to hedge (e.g., leverage and R&D).

2.4 Event study results

As a benchmark, Table 3 Panel A presents the unconditional intercepts (no control variables). The mean abnormal returns for US MNCs with exposure to the tied-$ countries are larger than for the firms with exposure to the tied-other countries, weakly significant in the short window analysis. Panel B presents the conditional intercepts from the Parsimonious and Full models. Coefficient estimates on the control variables are not presented in Table 3 for brevity; estimates are available in the Internet Appendix Table IA.3.[29] The conditional average tied-$ values are significantly larger than the tied-other values in one specification: Long window/Parsimonious model. With the DW normalization, the Panel A (B) long window test(s) of difference is significant at the 10% level (5% for both).

Table 4 reports the results of four cross-sectional DID tests of the regime shift abnormal returns across firms with high and low exposure using ex ante proxies for firm-level exposure. We predict that in response to the increase in currency risk the firms with high exposure will have larger regime shift abnormal returns than the low exposure firms, and that these differences will be more pronounced in the tied-$ countries where the shift in currency risk is larger.

For each of the four pairings, we report the mean conditional long window abnormal returns estimated in the Full model (corresponding to Table 3, Panel B, column 4) and the number of firm-year observations. In each analysis, we can compare the tied-$ and tied-other

[29] In the Short window/Parsimonious model, the coefficient estimate on *CURRΔ1*, which is the one-day change in currency value on the first day that the currency floats (multiplied by -1), is insignificant. In the Short window/Full mode, the coefficient estimate is 0.0486, significant at < 5%.

abnormal returns (i.e., the conditional intercepts) in rows 1 and 2, respectively, and the abnormal returns of firms with relatively high and low exposure as measured by one of the four ex ante proxies in columns 1 and 2, respectively. The bottom right number in each panel is the DID value with the F-test *p*-value for significance. The conditional abnormal return model includes proxies for the four partitioning variables, mitigating concerns that the changes in abnormal returns across the splits reflect systematic differences in real effects.

Panel A reports results for two commonly-used proxies for currency exposure: firm size and the ratio of foreign/total sales. For size, we divide the firms by median size within the tied-$ and tied-other countries. Prior findings suggest that larger firms have less exposure.[30] The explanations are that larger firms are more operationally hedged due to more diversified operations and (or) they are more likely to financially hedge using derivatives. For the ratio of foreign/total sales, we expect that firms with a greater portion of foreign sales have more exposure.[31] The results using firm size and foreign sales as proxies for exposure are similar. First, reading down the columns within each ex ante exposure category, firms with exposure to tied-$ countries have a larger abnormal return when the country switches to a floating exchange rate regime relative to firms with exposure to tied-other countries, consistent with the results in Table 3. Second, reading across the rows, in 3 of 4 cases the firms classified as having high exposure (i.e., smaller firms and firms with relatively high ratios of foreign to total sales) have higher exposure estimates than the low-exposure firms. As predicted, the difference in the difference between the returns in the tied-$ and tied-other countries, reported in the lower right-

[30] Bris, Koskinen, and Nilsson (2008), Choi and Kim (2003), Chow, Lee, and Solt (1997b), Dominguez and Tesar (2006), Hutson and Stevenson (2010), and Wei and Starks (2013) provide evidence that exposure estimates are negatively related to firm size. Chow and Chen (1998), Doidge, Griffin, and Williamson (2006), Faff and Marshall (2005) and Pantzalis, Simkins, and Laux (2001) provide alternative evidence. In our cross-sectional regressions, we find that both short and long window abnormal returns are significantly negatively related to firm size.

[31] Gao (2000), Jorion (1990), and Wei and Starks (2013) are examples of studies that find a positive relation between exposure estimates and firm-level foreign/total sales. Allayanis, Ihrig, and Weston (2001), Dominguez and Tesar (2006), and Faff and Marshall (2005) provide conflicting evidence.

hand side corner of the table, is larger for firms with more exposure, but the positive DID is insignificant.

The next two exposure proxies measure financial and operational hedging activities of the firms. We designate a firm as a financial hedger if it reports using currency derivatives in its latest 10-K (item 7a) prior to the crisis (*DERIVSUSE* = 1). We designate a firm as an operational hedger if it is internationally diversified, defined as reporting greater than or equal to three geographic segments in its latest 10-K.[32] We assume that hedged firms have less net currency exposure than non-hedged firms, which is generally consistent with prior studies that show that exposure estimates are lower for hedgers.[33]

Panel B reports results using our two hedging proxies for exposure. Both comparisons show a significant positive difference in the difference between the returns in the tied-$ and tied-other countries for firms with less hedging (no derivatives use and lower diversification). If we take the DID value in the upper quadrant of Panel B of 0.0012 (rounded) and divide it by the *TIED*$ coefficient estimate in Table 3, Panel B, column 4 of 0.003, we get an estimated currency risk effect of 0.387. The comparable ratio from the diversification DID equals 0.443. Thus, the results suggest that approximately 40% of the abnormal returns around currency regime shifts are due to the jump in currency risk.

We subject the conditional return models to numerous robustness tests given the many measurement and design issues raised in the extensive literature that uses returns-based models to estimate currency risk. Table 4 Panel C summarizes the results of the difference-in-

[32] This approach to identify foreign operations is similar in spirit to Allayannis, Ihrig, and Weston (2001) who use level-1 subsidiary data from the National Registry and Carter, Pantzalis and Simkins (2004) and Dewenter, Higgins, and Simin (2005) who use Who Owns Whom.

[33] Evidence from prior studies that measure financial hedging and exposure by derivatives use are more mixed than relative to studies of operational hedging and exposure. Allayanis and Ofek (2001) and Carter, Pantzalis, and Simkins (2004) provide evidence that derivatives users have smaller exposures, while Bartram, Brown, and Fehle (2009) and Faff and Marshall (2005) provide conflicting evidence. Two explanations for the mixed results are inadequate controls for the underlying exposure being hedged and endogeneity of the hedging decision.

differences tests for our three measures of abnormal returns (long window normalized by number of days, long window with the DW normalization, and short window), our 4 measures of exposure (size, foreign sales, derivatives use, diversification), and the three specifications (univariate, parsimonious, and full). For each specification, we provide the DID value (corresponding to the lower right hand number in the matrices of Panels A and B), the p-value for the DID test, and an estimate of the portion of the abnormal return due to the change in currency risk. This value equals the DID value divided by the univariate average abnormal return for firms in the tied-$ countries. Results from Panel B are repeated in column 3 for comparison. The table shows that for both hedging proxies, but especially for our measure of operational diversification, the DID results are significant. Across the 13 DID measures significant at the 5% or better level, the share of the abnormal return attributable to currency risk ranges from 16.6% to 68.5% with an average of 41%.

For other robustness tests, see Internet Appendix Section IA.6 for discussion and results including alternative: (1) samples; (2) model specifications; (3) abnormal return variable specifications; and (4) control variable specifications. All results are robust to these alternatives; only one analysis is worth noting here. We provide cross-sectional evidence that, across the sub-set of tied-$ countries, the ARs are significantly larger for firms with exposure to the countries that experienced the largest jumps in currency risk. This analysis (reported in Internet Appendix Section IA.2.) mitigates concerns that higher abnormal returns in the tied-$ countries are due to omitted variables that differ systematically between the tied-$ and tied-other countries and not to the larger jump in currency risk.

In sum, we find that changes in currency risk have statistically and economically significant value effects on US MNCs with exposure, and that firm hedging activities, both

financial and operating, affect the size of the exposure estimates.

3. Tests using cross-border investment activity

In this section we conduct two separate analyses, both about cross-border investment activity, as an alternative way to understand how currency risk affects US MNCs.

3.1 Tests of US MNC entry decisions

Most models predict that higher exchange rate volatility will be associated with lower levels of FDI, but various model assumptions can reverse this prediction. The empirical evidence on the relation between currency risk and US MNC entry decisions is also mixed. (See the discussion in Abbott, Cushman, and De Vita, 2012). We again exploit the tied-$, tied-other distinction to provide new evidence on this question. We predict that, all else equal, if currency risk is a significant factor in a firm's entry decision, then during fixed exchange rate regimes we will observe that US MNCs are more likely to enter, i.e., establish a new operating presence, in countries with a currency tied to the US dollar than in countries with a currency tied to something other than the US dollar.

We use a logit model to examine entry decisions during the *fixed* currency regimes that preceded a crisis:

$$ENTRY_{ij} = \beta TIED\$_j + \lambda TIED_{OTHER_j} + \Psi CONTROL_i + \varepsilon_i \quad (2)$$

where $ENTRY_{ij}$ is an indicator variable that equals one if firm *i* enters country *j* during the fixed rate regime. In principle, we could analyze entry into any country with a fixed exchange rate; our pre-crisis setting is not necessary. The crisis setting, however, allows for a falsification test: we examine entry by the same sample of US MNCs during the first year of the floating rate regime, which is in close time proximity to the fixed regime. The effect of factors other than the

tied-$ distinction on entry propensity should be similar, but the distinction between tied-$ countries and tied-other countries should be irrelevant to a US MNC's entry decision after the currency starts to float. If we find greater entry propensity into tied-$ countries in the fixed regime but not in the floating regime, this analysis mitigates the concern that systematic differences other than currency risk between the tied-$ and tied-other countries explain our results in the fixed period.

We examine entry by the same sample of 687 US MNCs described in Section 1 into each of the 23 crisis countries in the last year of the fixed exchange rate regime. To determine entry, we use changes in reported subsidiaries in the 10-K Exhibit 21. For each firm-country pair, we require the Exhibit 21 to be available for three years: the year-end prior to the crisis year, the year end of the crisis year (i.e., the last fixed exchange rate regime year), and the year end of the first floating regime year. Exhibit 21 data are available for 4,854 firm-country observations. We estimate the logit model over the 3,873 observations where the firm did not report a presence in the crisis country in Exhibit 21 the year before the crisis. We set the dependent variable equal to one for the 174 firms that reported (i.e., added) a subsidiary in their post-crisis Exhibit 21. More details of this process are reported in Internet Appendix Section IA.7. Of the 3,873 fixed regime observations, 2,819 (72.8%) are in tied-$ countries; 133 of the 174 that enter (76.4%) are in tied-$ countries.

Table 5 columns (1) through (3) report the logit model estimates during the fixed rate regime. The table reports the marginal effects of the regressors calculated at their means. Across all specifications, our logit model delivers a predicative accuracy of approximately 80% for both the entrants and non-entrants. Column (1) presents a benchmark model that includes only an intercept and controls for acquirer characteristics (i.e., size, market-to-book, sales

growth, market leverage, cash flow, R&D, and hedging using derivatives), defined previously. In addition, we control for the acquirer's business exposure in the crisis region, measured as the number of countries in the crisis region in which the firm has a subsidiary (*REGIONAL*), and for general experience in international operations, measured as the number of countries outside of the crisis region where the firm has a subsidiary (*GLOBAL*).[34] All firm-specific variables are measured as of the last 10-K filing date in the fixed rate regime.

The coefficient estimates on the indicators for regional and global exposure are positive and significant, suggesting that firms with more international experience are more likely than firms with less international experience to engage in cross-border investment during fixed rate regimes. The only other significant coefficient estimate is on sales growth, confirming that faster growing firms are more likely to engage in cross-border investment to expand into new countries.

The model in column (2) reports separate intercepts for tied-$ and tied-other countries. The marginal probability of entry by a US firm into tied-$ countries is 1.0% higher than the probability of entry into tied-other countries (-0.119 vs. -0.129), weakly significant with a *p*-value of 0.064.

Column (3) reports results for the model that also includes country characteristics measured as of the calendar year of the crisis. The characteristics are the same as those used in the regime shift event study with three exceptions. First, we add trade flows (imports and exports) between the crisis country and the US relative to total trade flows across all countries and the US to reflect the linkages between trade and FDI.[35] Second, we exclude the proxy for

[34] These variables are measured at the country level using the Exhibit 21 data, in contrast to the measure of diversification used in the regime shift event study tests, which is measured based on annual report segments, due to the constraints of the sample.

[35] For example, the Cushman (1985) model focuses on the substitution effect between exports and FDI.

the expected magnitude of the crisis, which was the one-day change in currency value on the first day of the regime shift (*CURRΔ1*). Although the expected magnitude of the crisis could have distorted entry decisions during the fixed regime, the currency change on the regime shift announcement date was not available information that US MNCs could use to form expectations. Third, we add the proportion of the entry window during which US MNCs might have anticipated the crisis, computed as the percentage of days in the anticipation period (as previously defined) that overlaps with the fixed rate regime entry period, truncated at 100%. Anticipation of a significant change in currency risk, or of other crisis-related effects, could distort firms' entry decisions during the fixed regime.

Lastly, we include two indicator variables equal to one for targets located in the UK (UK) and Mexico (MX) respectively. The UK indicator is included based on evidence that cultural similarity is associated with strong cross-border investment flows and that US investment into the UK, in particular, is distinct from US investment into countries with similar legal systems, capital market restrictions, and takeover market activity.[36] The Mexico indicator is included because it is the only crisis country contiguous to the US, which might distort entry propensity there. The results show that US MNCs are more likely to enter countries with greater shareholder rights, a higher frequency of cross-border acquisitions, and higher trade flow, and less likely to enter Mexico and countries of German legal origin (i.e., South Korea).

With these controls, the marginal probability of entry by a US firm into countries with exchange rates tied to the dollar is 1.7% higher than into countries with currencies tied to a basket, weakly significant with a *p*-value of 0.079.

[36] Rossi and Volpin (2004) find that the cultural similarity effect is eliminated when they control for bilateral trade flows, which also have a positive relation with cross-border investment. The US-UK combination has both cultural similarity and high bilateral trade flows. The Moeller and Schlingemann (2005) results relate to bidder returns, rather than investment flows, but still suggest that a separate control for the UK is warranted.

We next estimate eqn. (3) in the floating rate period. The logit model is estimated over the 3,799 observations that do not report a subsidiary in the crisis country at the beginning of the floating rate regime. The procedure we use to identify entry is the same as the procedure used in the fixed regime. We set *ENTRY* equal to one for the 190 observations that report a subsidiary in the post-regime-shift Exhibit 21, indicating that they entered the country in the year following the regime shift, and equal to zero for the other observations that do not add a subsidiary. Of the 3,799 observations, 2,754 (72.5%) are in tied-$ countries; 132 of the 190 entrants (69.5%) are in tied-$ countries. The firm-specific variables in this analysis are measured as of the first 10-K filing date in the floating rate regime, and the country characteristics are measured as of the first full post crisis calendar year.

The model in Table 5 column (6) includes an additional country control variable for the magnitude of the crisis equal to the total change in the currency value from the beginning of the anticipation period through the end of the stabilization period (*CURRΔTOT*, defined in Appendix B). This variable controls for potential systematic differences between the magnitudes of the tied-$ and tied-other country crises that could distort firms' entry decisions during the floating regime. The results of interest do not change if we exclude this variable from the analysis.

As expected, the coefficient estimates on *TIED$* and $TIED_{OTHER}$ are *not* significantly different from each other in columns (5) and (6)[37] with *p*-values of 0.391 and 0.540, respectively. The no-results finding during the floating regime supports the inference that it is currency risk during the fixed regime, not omitted country characteristics, that (weakly) affects entry decisions.

[37] Coefficient estimates on most of the control variables are similar in the fixed (columns 1-3) to floating (columns 4-6) regime estimations, suggesting the specification is reasonable. In five instances, the coefficients on target country control variables flip sign, but in none of these cases does the coefficient change from being significantly positive to significantly negative, or vice versa.

3.2 Acquisition announcement date return tests

In this section, we analyze US bidder short window abnormal returns to cross-border acquisition announcements of targets in the crisis countries to test whether changes in currency risk affect the valuation of cross border acquisitions by US MNCs. Our hypothesis is that, all else equal, abnormal returns to cross border acquisitions will be lower for acquisitions in floating rate regimes than for acquisitions in fixed rate regimes due to higher currency risk. In the cross-section, we predict this difference will be greater for firms that do not hedge than for firms that do hedge, and these differences will be larger in tied-$ countries than in tied-other countries because of the larger jump in currency risk.

We estimate the following seemingly unrelated model (SUR) of announcement date abnormal returns that yields a DID estimate of the change in cross border acquisition valuation from fixed to floating regimes between hedge versus no-hedge firms, allowing us to compare these differences between tied-$ versus tied-other countries:

$$\begin{aligned} CAR_{Tied\$,i} &= \beta_1 Fixed_j * Hedge_j + \beta_2 Fixed_j * NoHedge_j + \\ &\quad \lambda_1 Float_j * Hedge_j + \lambda_2 Float_j * NoHedge_j + \\ &\quad \sum_t \Psi_t TARGET + \sum_a \Phi_a ACQ + \sum_d \Psi_d DEAL + \sum_c \Theta_c COUNTRY + \varepsilon_i \\ CAR_{Tied\_Other,i} &= \beta_3 Fixed_j * Hedge_j + \beta_4 Fixed_j * NoHedge_j + \\ &\quad \lambda_3 Float_j * Hedge_j + \lambda_4 Float_j * NoHedge_j + \\ &\quad \sum_t \Psi_t TARGET + \sum_a \Phi_a ACQ + \sum_d \Psi_d DEAL + \sum_c \Theta_c COUNTRY + \varepsilon_i \end{aligned} \quad (3)$$

where CAR_i is the cumulative acquisition announcement abnormal return of firm *i*. CARs are estimated using a two factor market-industry model for a five-day event window around the acquisition announcement day (-1 to +3). The market and industry model parameters are estimated over the period from 11 to 265 days before the announcement using returns from a

value-weighted market portfolio and a value-weighted industry portfolio based on the target's Fama French 48 industry classification. The industry adjustment is important because of periods of industry clustering among the target firms.[38]

We derive the sample of announcements from the cleaned sample of 1,164 acquisition announcements described in Section 1. We combine deals that SDC reports as separate but that are partial purchases of the same target by the same acquirer on the same day. We exclude announcements that are *not* deals (e.g., purchase options and rumors of deals). We verify every acquisition announcement date, effective date, and deal value in newswires or financial press articles, eliminating 52 deals that were not completed.[39] We further eliminate 20 deals by bidders that have negative book equity and 192 deals with insufficient data to estimate abnormal returns. Data requirements for the control variables, particularly the bidder firm-level financial variables, further constrain our sample size. The final sample contains 760 deals that occurred in the window from three calendar years prior to two calendar years after the regime shift year.

Eqn. (3) is estimated across acquisition announcements that occur during the fixed neutral and float neutral periods as defined in Appendix A (596 of the total 760 deals).[40] Including observations during only the neutral period of the fixed regime mitigates concerns that markets had started to anticipate the regime shift and thus to incorporate its expected effects into the valuation of acquisition decisions. Including observations during only the neutral period of

[38] For example, almost half of the telecomm and natural resources acquisitions in South America took place in a nine month period in 1999.

[39] We include firms that have withdrawn deals when identifying the sample of US MNCs, but we exclude the withdrawn deals in the acquisition announcement tests. We also exclude Malaysian deals after September 3, 1998 because Malaysia returned to a fixed currency regime on this date, approximately one year after the shift to a floating regime.

[40] The average five-day acquisition announcement date return for the 596 deals is 0.262%, which is comparable to the three-day return of 0.148% for a (-1,+1) window for US acquisitions of foreign targets from 1990 to 1995 (Moeller and Schlingemann, 2005). See also Doukas and Travlos (1988) and Martynova and Renneboog (2008).

the floating regime reduces noise in the return measure for deals during the stabilization period.[41] Standard errors are clustered by firm.

Eqn. (3) includes four vectors of control variables: firm-specific characteristics of the target and acquirer; deal-specific variables; and crisis country characteristics. All variables are measured as of the fiscal- or calendar-year-end prior to the deal announcement as defined in Appendix B. The acquirer characteristics include size, market-to-book, sales growth, market leverage, cash flow, R&D, and hedging using derivatives, as described previously, plus an indicator variable equal to one for firms that have only one acquisition in our sample to control for possible differences in single versus multiple acquirers. The target characteristics include four indicator variables for organizational form – public, private, subsidiary, and JV – and a continuous measure of investment opportunities based on industry sales growth. The deal characteristics include the level of the acquirer's prior ownership of the target; an indicator for whether the acquisition results in full control; the size of the transaction relative to the acquirer's asset value; an indicator if SDC classifies the transaction as hostile; an indicator if SDC indicates that there were competing offers; an indicator if the transaction was a tender offer; and a measure of industry relatedness between the acquirer and the target. The country characteristics are economic freedom, legal system origin, investor protection, liquidity in the takeover market, and GDP growth, described previously.

Table 6 presents the coefficient estimates of interest when we estimate equation (3) without distinguishing across acquirers based on hedging activity.[42] The coefficient estimates reported in the table reveal no significant differences between acquisitions into the tied-$ versus tied-other countries or between acquisitions announced during the fixed neutral versus float

[41] Internet Appendix Tables IA.10.1 and 10.2 report robustness tests, including the use of alternative windows, that do not change our inferences.
[42] Coefficient estimates on the control variables are available in the Internet Appendix Table IA.8.

neutral periods.

Table 7 provides the cross-sectional DID matrices for acquisition announcements in the tied-$ countries (top panels) and tied-other countries (bottom panels), partitioning the deals based on the acquirer's hedging activities. We report results for the two proxies for hedging that showed significant effects in the regime shift event study: derivatives use and geographic diversification.[43]

If higher currency risk in the floating period is (negatively) priced in acquisition announcement returns, we expect to observe a positive difference between abnormal returns in the fixed vs. floating period, especially for non-hedgers relative to hedgers. That is, the (a) – (b) difference should be positive, and this difference should be larger in the top row (non-hedgers), which suggests a positive DID in the lower right of each matrix. The DID value should be larger for acquisitions into the tied-$ countries because they experience a larger jump in currency risk than the tied-other countries. Thus, the difference between the tied-other DID and tied-$ DID, which is reported in the bottom row of the table, should be positive. None of the differences are significant at the five percent level. Overall, the data do not reveal a robust relation between changes in currency risk and cross-border acquisition announcement abnormal returns.

4. Conclusions

Using the setting of fixed-to-floating exchange rate regime shifts, we provide new evidence on the impact of currency risk on US MNCs. Our event study of US MNCs' abnormal returns when countries announce a shift from a fixed to a floating exchange rate regime provides evidence on the magnitude of US MNCs' net exposure. We find larger abnormal returns for US firms with exposure to countries that shift from a fixed currency tied to the US dollar than for

[43] Internet Appendix Table IA.9 reports similar results for the other two hedging proxies, size and foreign/total sales.

those with exposure to countries whose currency is fixed to a different currency (or basket). Difference-in-differences analyses suggest approximately 40% of a US firm's crisis date reaction is due to the shift in currency risk.

Two separate tests provide evidence on the impact of currency risk on cross-border investment. We observe (weakly) statistically significant but economically marginal evidence that currency risk affects entry, suggesting that currency risk has a minimal impact on the trade-offs in a firm's cross-border investment location decision. Tests of entry propensity show that US MNCs are only 1.0% to 1.7% more likely to enter a dollar-linked fixed exchange rate country than a non-dollar linked fixed exchange rate country. An analysis of cross-border acquisition announcement date abnormal returns provides no evidence of a significant relation between currency risk and announcement returns. This "no-results" finding occurs using a sample and setting that is powerful enough to detect evidence of currency risk effects on exposure in our regime shift event study. Therefore, we interpret the lack of significance as evidence of an economically minimal impact rather than a weak test.

We interpret the combination of results as follows. While factors such as strategic considerations may take precedence over currency risk in cross-border investment decisions or acquisition valuations, the net exposure to currency risk of US MNCs is still significant. Our novel setting allows us to confirm that currency exposure is a significant factor for multinational firms. Our test design, including the use of the same sample across tests, allows us to conclude that while international operations are subject to currency risk in general, this factor is of second order importance when considering cross border investments.

Appendix A. Dating the stages of the currency crises

We identify four periods that surround the regime shift date. In the fixed rate regime, we identify the month when markets could begin to anticipate that the Central Bank might move to a floating currency (the "anticipation date"). The *anticipation* period starts at the anticipation date and ends at the regime shift date. The *fixed neutral* period begins two years prior to the anticipation date. The determination of the anticipation date is described below. In the floating rate regime, we identify a *stabilization* period immediately following the regime shift that ends when the crisis country's currency volatility has stabilized. The *floating neutral* period begins at the end of the stabilization period and ends two years later. The dating of the end of the stabilization period is described below. Table A1 summarizes the starting and ending dates of the four periods. Table A2 provides supporting evidence for the start of the anticipation period.

To define the anticipation period, we first consider changes in Central Bank reserves. Columns (1) – (3) of Table A2 report data on reserve drops prior to the regime shift for each crisis country. We report the month in the year preceding the regime shift in which there was a 5% or greater drop in reserves (column 1), and the month in which the change in monthly reserves was greater than one (column 2) and two (column 3) standard deviations of the monthly reserve changes calculated from the prior year.

We also consider pre-crisis dates that prior literature has characterized as indicators or "warning" signals of a developing crisis. Column (4) reports months of extreme market pressure identified in Osakwe and Schembri (1998). Column (5) reports months of pressure identified by Edison (2003). Based on the methodology developed by Kaminsky, Lizondo and Reinhart (1998), Edison (2003) identifies pressure months as those in which an index of the weighted average of exchange rate changes and reserve losses is 2.5 standard deviations or more above the mean. Column (6) reports high exchange rate policy intervention regimes identified in Fiess and Shanker (2009) in the two year period prior to the crisis. Fiess and Shanker (2009) estimate quarterly and monthly regime switching models based on hypothesized determinants of exchange rate policy intervention including changes in reserves, stock price levels and volatility, GDP, inflation, and other hypothesized determinants. Column (7) reports months of devaluation episodes prior to each regime shift from Edison (2003).

We supplement the analysis of the factors presented in Table A2 by reading press accounts at the time of the events as well as *ex post* summaries of the crises to reach a consensus on the month that markets could begin to anticipate that the Central Bank might move to a floating currency. Column (8) reports our consensus decision on the month that the anticipation period begins, which also represents the end of the fixed neutral period. The anticipation date is the first day of the designated month.

To define the stabilization period, we compute a rolling thirty day local currency volatility time series starting at the regime shift date. The end of the stabilization period is the first peak of this series, with volatility falling afterwards. The end of the stabilization period (reported in Table 1) is the beginning of the floating neutral period. This dating method is intended to reflect a stabilization of expectations with respect to the currency value, not necessarily with respect to the macro-economy or government policy.

	Regime shift date						
	Start of fixed neutral period	End of fixed neutral period; start of anticipation period	Anticipation period days	End of anticipation period; start of stabilization period	End of stabilization period; start of floating neutral period	Stabilization period days	End of floating neutral period
Country	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Finland	11/1/89	11/1/91	312	9/8/92	11/10/92	63	11/30/94
UK	8/1/90	8/1/92	47	9/17/92	11/9/92	53	11/30/94
Italy	7/1/90	7/1/92	78	9/17/92	11/6/92	50	11/30/94
Sweden	8/1/90	8/1/92	110	11/19/92	1/13/93	55	1/31/95
Norway	9/1/90	9/1/92	100	12/10/92	1/27/93	48	1/31/95
Mexico	12/1/92	12/1/94	21	12/22/94	3/16/95	84	3/31/97
Czech Rep.	4/1/95	4/1/97	55	5/26/97	7/30/97	65	7/31/99
Thailand	5/1/95	5/1/97	62	7/2/97	9/17/97	77	9/30/99
Philippines	7/1/95	7/1/97	10	7/11/97	10/1/97	82	10/31/99
Malaysia	7/1/95	7/1/97	41	8/11/97	10/6/97	56	10/31/99
Indonesia	7/1/95	7/1/97	44	8/14/97	1/28/98	167	1/31/00
South Korea	9/1/95	9/1/97	106	12/16/97	3/31/98	105	3/31/00
Russia	11/1/95	11/1/97	289	8/17/98	12/19/98	124	12/31/00
Slovak Rep.	8/1/96	8/1/98	61	10/1/98	12/2/98	62	12/31/00
Brazil	9/1/96	9/1/98	136	1/15/99	4/7/99	82	4/30/01
Ecuador	5/1/96	5/1/98	287	2/12/99	6/25/99	133	6/30/01
Chile	9/1/96	9/1/98	367	9/3/99	11/5/99	63	11/30/01
Colombia	6/1/97	6/1/99	118	9/27/99	12/20/99	84	12/31/01
Poland	9/1/97	9/1/99	224	4/12/00	9/27/00	168	9/30/02
Turkey	11/1/98	11/1/00	113	2/22/01	4/18/01	55	4/30/03
Argentina	9/1/99	9/1/01	163	2/11/02	4/3/02	51	4/30/04
Venezuela	8/1/99	8/1/01	196	2/13/02	6/28/02	135	6/30/04
Uruguay	7/1/99	7/1/01	354	6/20/02	9/13/02	85	9/30/04

Table A1. Periods surrounding regime shift

Description: Countries are listed in calendar date order of the regime shift. Columns (1) and (2) report the beginning and end of the 2-year fixed neutral period. The end of the fixed neutral period is the start of the anticipation period. Column (3) reports the number of days in the anticipation period. Column (4) reports the first day the currency floated. Column (5) reports the end of the stabilization period that follows the regime shift, which is the beginning of the floating neutral period. Column (6) reports the number of days in the stabilization period. Column (7) reports the end of the two-year floating neutral period that follows the stabilization period.

	Reserve drops			Periods of pre-crisis pressure			Devaluation	Consensus beginning of anticipation period
	5%	1σ	2σ	OS98	Edison03	FS09	Edison03	
Country	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Finland	AUG-91	OCT-91	OCT-91	NOV-91, AUG-92, SEP-92	SEP-92	N/A	SEP-92	11/1/91
United Kingdom	SEP-92	None	None	AUG-92, SEP-92	N/A	N/A	N/A	8/1/92
Italy	DEC-91	DEC-91	JUL-92	JUL-92, AUG-92, SEP-92	N/A	N/A	N/A	7/1/92
Sweden	NOV-91	NOV-91	APR-92	DEC-91, AUG-92, SEP-92, NOV-92	NOV-92	N/A	None	8/1/92
Norway	SEP-92	SEP-92	SEP-92	SEP-92, NOV-92, DEC-92	NOV-92	N/A	No data	9/1/92
Mexico	NOV-93	MAR-94	MAR-94	DEC-94	DEC-94	No monthly, Q4-93	DEC-94	12/1/94
Czech Republic	None	None	None	N/A	N/A	N/A	N/A	4/1/97
Thailand	MAY-97	MAY-97	None	MAY-97, JUL-97	JUL-97	JUL-97, Q2-97	AUG-97	5/1/97
Philippines	JUL-97	None	None	JUL-97	DEC-97	N/A	SEP-97	7/1/97
Malaysia	JUL-97	MAY-97	JUL-97	JUL-97	JUL-97	FEB-95, Q2-96	OCT-97	7/1/97
Indonesia	AUG-97	None	None	JUL-97, AUG-97	DEC-97	No monthly, Q2-97	SEP-97	7/1/97
South Korea	AUG-97	AUG-97	NOV-97	OCT-97, NOV-97, DEC-97	NOV-97	JAN-97, Q1-97	NOV-97	9/1/97
Russia	NOV-97	NOV-97	None	N/A	N/A	N/A	N/A	11/1/97
Slovak Republic	SEP-98	AUG-98	SEP-98	N/A	N/A	N/A	N/A	8/1/98
Brazil	SEP-98	SEP-98	SEP-98	None	JAN-99	Mar-96, Q4-98, Q2-99	JAN-99	9/1/98
Ecuador	JAN-98	MAY-98	AUG-98	N/A	N/A	N/A	N/A	5/1/98

	Reserve drops			Periods of pre-crisis pressure			Devaluation	Consensus beginning of anticipation period
	5%	1σ	2σ	OS98	Edison03	FS09	Edison03	
Country	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Chile	JUN-99	JUN-99	None	N/A	None	No monthly, Q4-98, Q2Q3Q4-99	None	9/1/98
Colombia	None	None	None	N/A	SEP-97, SEP-98, JUL-99	No monthly, Q3-98, Q1Q2Q3-99	NOV-97	6/1/99
Poland	None	SEP-99	None	N/A	N/A	N/A	N/A	9/1/99
Turkey	NOV-00	NOV-00	NOV-00	N/A	N/A	N/A	None	11/1/00
Argentina	MAR-01	MAR-01	MAR-01	N/A	N/A	JAN-02, Q4-01	None	9/1/01
Venezuela	FEB-01	AUG-01	None	N/A	N/A	JUN-00 to FEB-01, Q3-01 to Q4-02	None	8/1/01
Uruguay	JUL-01	JUL-01	JAN-02	N/A	N/A	N/A	None	7/1/01

Table A2. Factors to determine the anticipation period

Description: Column (8) reports the beginning of the anticipation period (the "anticipation" date). The dates are established by consensus of the authors based on the factors in columns (1) – (7) and based on press descriptions of the crises. Columns (1) through (3) report the month in the year preceding the regime shift in which there was a 5% drop in Central Bank reserves and the month in which the change in monthly reserves was larger than one and two standard deviations of the monthly reserve changes calculated from the prior year. Columns (4) through (6) report periods of pressure prior to the regime shift. Column (4) reports months of extreme market pressure identified in Osakwe and Schembri (1998, OS98). Column (5) reports months of pressure identified by Edison (2003). Based on the methodology developed by Kaminsky, Lizondo and Reinhart (1998), Edison (2003) identifies pressure months as those in which an index of the weighted average of exchange rate changes and reserve losses is 2.5 standard deviations or more above the mean. Column (6) reports high exchange rate policy intervention regimes identified in Fiess and Shanker (2009, FS09) in the two year period prior to the crisis. Quarterly and monthly regime switching models are estimated based on hypothesized determinants of exchange rate policy intervention including changes in reserves, stock price levels and volatility, GDP, inflation, and others. Column (7) reports months of devaluation episodes from Edison (2003). In columns (4) – (7), N/A indicates that the country was not listed in the report. None indicates that no periods were identified by the models used.

Appendix B. Summary of control variables

Variable	Name	Description
Panel A: Country-level variables		
Ancments of gov't personnel changes	ANNC_PERS	Indicator variable = 1 if the regime shift event day Reuters reports included announcements of key government personnel or senior policy makers that quit or were fired.
Announcements of other reforms	ANNC_REFS	Indicator variable = 1 if the regime shift event day Reuters reports included announcements of other economic reforms, and = 0 if the Reuters reports said either that the government/Central Bank would not change policies or did not make reference to other reform changes.
Concurrent announcements	CONCANNCS	Indicator variable = 1 if concurrent with the regime shift announcement were announcements of other reforms (ANNC_REFS) or key government personnel changes (ANNC_PERS).
Magnitude of crisis	CURRΔ1	% change in currency value on regime shift day, multiplied by -1
	CURRΔFULL	% change in currency value from the beginning of anticipation period to the regime shift date, multiplied by -1
	CURRΔTOT	% change in currency value from the beginning of anticipation period to the end of the stabilization period, multiplied by -1
Devaluations	DEVALUE_P2M	Indicator variable = 1 if there was at least one devaluation in the two months prior to the regime shift announcement date.
Economic freedom	EFW	Economic Freedom of the World (EFW) Index (www.freetheworld.com)
Crisis expected	EXPECT_NEWS	Indicator variable = 1 if the Reuters report of the regime shift indicated that markets were "shocked", "surprised", or "confused" by the announcements, and equals zero if the Reuters report indicated markets "anticipated" the shift, or that it was "long awaited" or "inevitable."
Regional follower	EXPECT_REGION	Indicator variable = 1 if the country was the follower in a cluster of regional crises as indicated in Table 1.
Unusual reserve drop	EXPECT_ΔRES	Indicator variable = 1 if the drop in reserves in the three months prior to the regime shift month is more than 1 standard deviation of monthly changes in the year prior to the regime shift. Due to lack of data, this variable is equal to zero for Slovak and Poland.
GDP growth	GDPCHG	% change in real GDP
Inflation	INFL	Average of the 12 monthly annual inflation rate observations from Global Insight (or four quarterly observations if monthly not available) for the year prior to the regime shift announcement month (or quarter). We set the rate = to the Datastream rate for Italy and Indonesia, which are missing on Global Insight.
Legal origin	LEGALO-ENG	Indicator variable = 1 if legal origin is English.
Legal origin	LEGALO-FR	Indicator variable = 1 if legal origin is French.
Legal origin	LEGALO-GER	Indicator variable = 1 if legal origin is German.

Table B1, Panel A, country-level variables, continued		
Local market return	LOCALRET1	The one day return to the local market stock exchange on the regime shift announcement date, using Datastream's local market index in the local currency. Data for Ecuador and Uruguay are missing from Datastream; we set the returns at zero. Ecuador news reports stated that the index was "flat" on that date. Uruguay newspapers did not report any news about the stock market returns (which we presume they would if it was material). The one day return is scaled by the standard deviation of the daily returns to the index measured over the six months prior to the regime shift announcement date to get an "abnormal" return. One day returns for the following countries are significantly different from zero: Finland, Sweden, Norway, Czech Republic, Mexico, Thailand, Philippines, Malaysia, Indonesia, S. Korea, Brazil, Turkey and Venezuela. The return for Venezuela is measured on 2/13/2002 because 2/12/2002 is a Venezuelan national holiday. The return for the Czech Republic is computed over the two-day window from 5/26/97 and 5/27/1997. May 26, 1997 is Memorial Day in the US, but it is a trading day in the Czech Republic. The two day window captures all information available to US traders.
	LOCALRETFULL	The return to the local market stock exchange computed over the anticipation period. All other calculation methods are the same as for LOCALRET1.
Cross-border takeover frequency	MAFREQ	Number of completed cross-border deals divided by the total number of publicly listed firms in the country.
Takeover volume	MAVOL	Total transaction value of all cross-border and domestics deals in a country listed on SDC divided by the country's GNP.
Overlap between entry and anticipation periods	OVERLAP	The proportion of days in the anticipation period that overlap with the fixed rate regime entry period, truncated at 100%.
Investor protection	SHRIGHTS	Indicator variable = 1 if the shareholder rights variable in La Porta et al. (1998) or Pistor et al. (2000) is three or above, and = 0 otherwise.
Trade flow	TRADEFLOW	The trade flows (imports and exports) between the crisis countries and the U.S. relative to total trade flows across all countries for the U.S.
Panel B: Firm-level variables		
Exposure (in entry propensity tests)	GLOBAL	The number of countries outside of the crisis region where the firm has a subsidiary
	REGIONAL	The number of countries in the crisis region where the firm has a subsidiary
Exposure (in acq. announcement tests)	EXPOSED	Indicator variable = 1 if the firm reports either assets or sales for the crisis country (e.g., UK), the region (e.g., Europe), or other countries in the region (e.g., Germany) in its required annual report geographic segment data, and = 0 otherwise.
Cash flow	CF	Ratio of total cash flow to total assets

Table B1, Panel B Firm-level variables, continued		
Currency hedging activities	DERIVSUSE	Indicator variable = 1 if the acquirer reports any indication (quantitative or qualitative) that it uses currency derivatives in its 10K, and = 0 otherwise. For each sample firm and for each fiscal year ending between 1/97 and 6/02, we searched Item 7a in the 10-K report, "Quantitative and Qualitative Disclosures about Market Risk." We built indicator variables equal to 1 if the firm reported any currency hedging. (We also tracked whether the firm reported currency hedging specifically for any of the crisis countries or countries in the same region as our crisis countries, but these samples were too small to conduct meaningful statistical tests.) We classify a firm/crisis observation as being in the group of financial hedgers if the firm reported using currency derivatives in the 10-K report in the fiscal year prior to that crisis. Section 7a is not available prior to 1997. For pre-1997 observations, we assume there was no financial hedging activity.
Diversification	DIVERSE	A firm is defined as "more diversified" if it reports greater than or equal to three geographic segments in the annual report and "less diversified" if it reports less than three segments.
Foreign sales	FSALE	Ratio of foreign sales to total sales
Geog. segments	GEOSEG	The number of geographic segments
Sales growth	GROWTH	Two year geometric average of annual growth rate in net sales
Market leverage	LEV (LEV2)	Ratio (squared ratio) of total debt to total market value of assets
Market-to-book	MTB	Ratio of book equity and market equity
R&D	RD	Ratio of R&D expense and total sales
Size	SIZE	Logarithm of the firm's total assets adjusted for US inflation (base year 2005)
Panel C: Deal-level variables		
Full control	100PERC	Indicator variable = 1 if the acquirer fully acquires the target and hence holds 100% of the share capital after the completion of the deal, and = 0 otherwise.
Competition	COMPETE	Indicator variable = 1 from SDC classification, and = 0 otherwise.
Industry relatedness	DISTINCT	Indicator variable = 1 if the acquirer and target operate in different industries (their primary 2-digit SIC codes are not equal), and = 0 otherwise.
Attitude	HOSTILE	Indicator variable = 1 from SDC classification, and = 0 otherwise.
Investment opps	INVOPP	Country-industry average of 2-year geometric average of annual growth rate in net sales prior to announcement.
Organizational form	ORG_JV	Indicator variable = 1 if the target is a joint venture.
	ORG_PRIVATE	Indicator variable = 1 if the target is a private company.
	ORG_PUBLIC	Indicator variable = 1 if the target is a public company.
	ORG_SUB	Indicator variable = 1 if the target is a subsidiary.
Relative size	RELSIZE	Ratio of the transaction value to the acquirer's market value of assets. Deal values are from SDC supplemented by hand collection. If deal values are not available, we use the target's asset value. We assign an adjusted deal value of zero if no financial information of the target is available.
Single	SINGLE	Indicator variable = 1 if the acquirer makes a single acquisition in our sample, and = 0 otherwise.
Tender-offer	TENDER	Indicator variable = 1 if transaction is a tender-offer, and = 0 otherwise.
Prior ownership	TOEHOLD	Indicator variable equal to 1 if the acquirer has a pre-existing stake in the target firm, and = 0 otherwise.

Description: Table provides description of explanatory variables used throughout the paper. Table B1 Panels A, B and C describe the country-level, firm-level, and deal-level variables, respectively.

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Country	Tied to: (1)	Regime shift date (2)	Regime shift announcement date (in local country) (3)	First US trading date on or after regime shift announcement[44] (4)
Finland*	ECU	9/8/92	9/8/92	9/8/92
UK	ECU	9/17/92	9/16/92	9/16/92
Italy	ECU	9/17/92	9/17/92	9/17/92
Sweden	ECU	11/19/92	11/19/92	11/19/92
Norway	ECU	12/10/92	12/10/92	12/10/92
Mexico	$	12/22/94	12/22/94	12/22/94
Czech Republic	basket	5/26/97	5/26/97	5/27/97
Thailand*	$	7/2/97	7/2/97	7/2/97
Philippines	$	7/11/97	7/11/97	7/11/97
Malaysia	$	8/11/97	8/9/97	8/11/97
Indonesia	$	8/14/97	8/14/97	8/14/97
South Korea	$	12/16/97	12/15/97	12/15/97
Russia	$	8/17/98	8/17/98	8/17/98
Slovak Republic	basket	10/1/98	10/1/98	10/1/98
Brazil*	$	1/15/99	1/15/99	1/15/99
Ecuador	$	2/12/99	2/12/99	2/12/99
Chile	$	9/3/99	9/2/99	9/3/99
Colombia	$	9/27/99	9/25/99	9/27/99
Poland	basket	4/12/00	4/11/00	4/11/00
Turkey	$	2/22/01	2/22/01	2/22/01
Argentina*	$	2/11/02	2/3/02	2/4/02
Venezuela	$	2/13/02	2/12/02	2/13/02
Uruguay	$	6/20/02	6/20/02	6/20/02

Table 1: Regime shift announcement dates for the twenty-three currency crises

Description: This table provides a country-level summary of the regime shift announcement dates. Countries are listed in calendar date order of the regime shift and are grouped by possible regional relationships. Column (3) reports the local country date the Central Bank announced a shift to a floating rate currency regime and column (4) reports the first trading date in the US on or after the announcement. * Designates "Leader" countries, the first crisis country within a regional grouping of countries.

[44] For six countries, the first US trading day on or after the regime shift announcement (column 4) does not coincide with the regime shift announcement date in the local country (column 3). US exchanges were closed on the announcement dates for the Czech Republic (Memorial Day), Malaysia, Colombia, and Argentina (weekends). The announcements by Chile and Venezuela came after the close of the US exchanges.

		Tied-$	*Tied-other*	Tied-$ - Tied-other	*t*-test for diff.
Panel A:Means of sample firms					
N		913	401		
% of sample		69.5%	30.5%		
Size	SIZE	8.462	8.178	0.284	[0.155]
Market-to-book	MTB	4.805	4.305	0.500	[0.458]
Sales Growth	GROWTH	0.088	0.096	-0.008	[0.686]
Market Leverage	LEV	0.249	0.226	0.024	[0.101]
Cash Flow	CF	0.008	0.015	-0.007*	[0.050]
R&D	RD	0.041	0.047	-0.006*	[0.079]
Foreign Sales %	FSALE	0.606	0.594	0.012	[0.528]
Currency hedging	DERIVSUSE	0.153	0.063	0.090*	[0.054]
Geographic Segments	GEOSEG	2.076	0.932	1.143	[0.117]
Panel B: Proxies for the change in the probability of a regime shift					
Expected = 1 based on news report	EXPECT_NEWS	0.400	0.375	0.025	[0.912]
Expected = 1 for regional followers	EXPECT_REGION	0.667	0.500	0.167	[0.458]
Expected = 1 if unusual reserve drop in prior 3 months	EXPECT_ΔRES	0.333	0.250	0.083	[0.696]
Devaluations over prior two months	DEVALUE_P2M	0.333	0.125	0.208	[0.300]
Panel C: Proxies for value-relevant concurrent effects of the regime shift					
|1-day local market return scaled|	LOCALRET1	2.096	1.764	0.332	[0.642]
|1-day currency change|	CURR*Δ1*	0.097	0.022	0.075**	[0.021]
|Full local market return|	LOCALRETFULL	0.279	0.165	0.113	[0.292]
|Full currency change|	CURR*Δ*FULL	0.227	0.076	0.151***	[0.009]
Inflation for prior 12 months	INFL	0.120	0.045	0.075	[0.176]
Other reforms announced	ANNC_REFS	0.533	0.250	0.283	[0.209]
Sig. gov't personnel changes announced	ANNC_PERS	0.133	0.125	0.008	[0.957]
Concurrent announcements	CONCANNCS	0.667	0.375	0.292	[0.195]
Panel D: Proxies for institutional structures and macro-economic conditions					
Economic Freedom	EFW	6.182	6.764	-0.582	[0.139]
Investor Protection	SHRIGHTS	0.467	0.750	-0.283	[0.209]
M&A Volume	MAVOL	0.049	0.034	0.015	[0.588]
X-Border M&A Freq.	MAFREQ	0.027	0.079	-0.052**	[0.027]
GDP Growth	GDPCHG	-0.019	0.013	-0.031	[0.195]

Table 2: Means of sample firm characteristics and control variables

Description: Panel A reports sample sizes for the regime shift announcement date event study and the equal-weighted means of characteristics of the sample firms. Panels B through D present the equal-weighted means of the control variables, measured at the country-level, that are included in the regime shift announcement date conditional return model (eqn. (1)). The variables are described in Appendix B. Country-level means are reported in Internet Appendix Table IA.1. *** {**} (*) indicate significance at the 1% {5%} (10%) level in a two-sided test based on p-values in final column.

Note: Legal Origin: All countries are of French Legal origin with the following exceptions: English origin (Thailand, Malaysia, UK); German origin (South Korea); Scandinavian origin (Finland, Sweden, Norway); and other origin (Russia, Czech Republic, Slovak Republic, Poland).

Interpretation: Results in Panel A indicate that firm characteristics do not differ between the tied-$ and tied-other sample firms and thus should not introduce any biases into the regressions.

Panel A: Univariate abnormal returns around regime shift announcement dates

	N	SHORT WINDOW |AR|	LONG WINDOW |AR|
Full sample			
Mean	1,314	0.0169	0.0020
Median	1,314	0.0116	0.0014
Std. Dev.	1,314	0.0237	0.0021
Tied-$			
Mean	913	0.0178	0.0020
Median	913	0.0121	0.0014
Tied-other			
Mean	401	0.0151	0.0019
Median	401	0.0109	0.0014
Tied-$ - Tied-other			
Mean		0.0027*	0.0001
t-test for diff.		[0.058]	[0.139]
Median		0.0012	0.0000
Wilcoxon-test for diff.		[0.192]	[0.784]

Panel B: Event study of conditional absolute abnormal returns around regime shift announcement date

Dependent Variable:	SHORT WINDOW |AR|		LONG WINDOW |AR|	
Model:	PARSIMONIOUS	FULL	PARSIMONIOUS	FULL
Tied-$	0.0234***	0.0530***	0.0026***	0.0030***
Tied-other	0.0209***	0.0534***	0.0021***	0.0027**
Tied-$ - Tied-other	0.0025	-0.0004	0.0005***	0.0003
F-test for difference across coefficients [*p*-value]	[0.149]	[0.893]	[0.001]	[0.167]
Control variables				
Δ event probability	Included	Included	–	–
Concurrent events	Selected	Included	Selected	Included
Country institutions	–	Included	–	Included
Firm characteristics	–	Included	–	Included
N	1,314	1,314	1,262	1,262
Adjusted R^2	34.9%	37.7%	49.6%	54.3%

Table 3: Regime shift announcement date event study of conditional abnormal returns

Description: Panel A presents the mean and median for event window absolute values of abnormal returns (|AR|) for the full sample, and separately for *Tied-$* and *Tied-other* sub-samples. The computation of AR is described in Section 2.3. Event windows for each country are reported in Table 1. In Panel B, the event window abnormal returns (|AR|) are the intercept estimates from models of the absolute value of abnormal returns on separate intercepts for countries tied to the dollar (*Tied-$*) and tied to other currencies (*Tied-other*). The Parsimonious short window model includes EXPECT_NEWS, EXPECT_REGION, EXPECT_ΔRES, DEVALUE_P2M and uses LOCALRET1 and CURRΔ1 as summary measures of non-currency-risk news on the event date. The Parsimonious long window model includes CURRΔFULL and CURRΔ1 as summary measures of concurrent events. The Full models augment the Parsimonious models with the following control variables: CONCANNCS, INFL, EFW, LEGALO-ENG, LEGALO-FR, LEGALO-GER, SHRIGHTS, MAVOL, MAFREQ, GDPCHG, SIZE, MTB, GROWTH, LEV, CF, RD, FSALE, DERIVSUSE, and GEOSEG. Variables are described in Appendix B. *** {**}

(*) indicate significance at the 1% {5%} (10%) level (two-sided). Standard errors are clustered at the firm level. Note that p-values are in brackets

Interpretation: Absolute values of abnormal returns in response to regime shift announcements are always larger for firms with exposure in the tied-$ countries than for firms with exposure in the tied-other countries (univariate in Panel A and conditional in Panel B). Although, the differences are significant at the five percent level in only one case (multivariate, long window, parsimonious model).

		Panel A: Traditional proxies for exposure				*Panel B: Our proxies for financial and operational hedging*			
		Small vs. big firms				*Not-hedge vs. hedge with derivatives*			
		Small size (a)	Big Size (b)	Difference (a) - (b)	F-test for diff. across coefficients [*p*-value]	No derivatives (a)	Uses derivatives (b)	Difference (a) - (b)	F-test for diff. across coefficients [*p*-value]
Tied-$	(i)	0.0021* *n = 441*	0.0018 *n = 420*	0.0003*	[0.053]	0.0027** *n = 736*	0.0025** *n = 125*	0.0002	[0.137]
Tied-other	(ii)	0.0018 *n = 198*	0.0016 *n =203*	0.0002	[0.316]	0.0024* *n = 382*	0.0034*** *n = 19*	-0.0010*	[0.082]
Difference	(i) - (ii)	0.0003	0.0002	0.0001	[0.633]	0.0003	-0.0009	0.0012**	[0.030]
F-test for diff. across coefficients [*p*-value]		[0.211]	[0.416]			[0.144]	[0.115]		
		High vs. low foreign/total sales firms				*Less vs. more diversified*			
		High For. Sales (a)	Low For. Sales (b)	Difference (a) - (b)	F-test for diff. across coefficients [*p*-value]	Less diversified (a)	More diversified (b)	Difference (a) - (b)	F-test for diff. across coefficients [*p*-value]
Tied-$	(i)	0.0024** *n = 433*	0.0024** *n = 428*	0.0000	[0.665]	0.0028*** *n = 510*	0.0024** *n = 351*	0.0004**	[0.014]
Tied-other	(ii)	0.0020* *n = 201*	0.0023* *n = 200*	-0.0003	[0.107]	0.0023** *n = 349*	0.0033*** *n =52*	-0.0009***	[0.002]
Difference	(i) - (ii)	0.0004*	0.0001	0.0003	[0.267]	0.0005**	-0.0008***	0.0013***	[0.000]
F-test for diff. across coefficients [*p*-value]		[0.080]	[0.601]			[0.039]	[0.008]		

Table 4. Cross-sectional difference-in-differences analysis for long-window regime shift event date returns

Panel C: Summary of DID results

AR Measure	Long Window			DW Long Window			Short Window		
Model	Uncond'l	Parsim.	Full	Uncond'l	Parsim.	Full	Uncond'l	Parsim.	Full
SIZE: Small vs. Big									
DID estimate	0.0002	0.0000	0.0001	0.0534	0.0460	0.0235	0.0040	0.0034	0.0037
F-test [*p*-value]	[0.481]	[0.726]	[0.633]	[0.519]	[0.584]	[0.782]	[0.114]	[0.190]	[0.188]
Currency exposure effect	8.9%	3.4%	4.0%	7.8%	6.1%	2.7%	22.6%	14.3%	6.9%
FSALE: High vs. Low									
DID estimate	0.0003	0.0003	0.0003	0.1652**	0.1593*	0.1462*	0.0024	0.0017	0.0029
F-test [*p*-value]	[0.320]	[0.294]	[0.267]	[0.049]	[0.062]	[0.082]	[0.334]	[0.522]	[0.328]
Currency exposure effect	13.2%	10.4%	9.3%	24.0%	21.1%	16.8%	13.7%	7.3%	5.5%
DERIVSUSE: No vs. Yes									
DID estimate	0.0011**	0.0010**	0.0012**	0.3699**	0.3186*	0.1836	0.0005	-0.0045	-0.0106*
F-test [*p*-value]	[0.015]	[0.026]	[0.030]	[0.042]	[0.088]	[0.392]	[0.922]	[0.453]	[0.084]
Currency exposure effect	53.5%	37.7%	38.7%	53.6%	42.2%	21.2%	3.0%	-19.4%	-20.0%
DIVERSE: Less vs. More									
DID estimate	0.0010***	0.0010***	0.0013***	0.2831***	0.2425**	0.2003	0.0122***	0.0091**	0.0088**
F-test [*p*-value]	[0.000]	[0.000]	[0.000]	[0.001]	[0.017]	[0.115]	[0.001]	[0.016]	[0.028]
Currency exposure effect	49.9%	35.0%	44.3%	41.2%	32.3%	23.1%	68.5%	38.8%	16.6%

Table 4. Cross-sectional difference-in-differences analysis for long-window regime shift event date returns

Description: This table reports difference-in-differences tests for the absolute values of abnormal returns on the first US trading date on or after a country's Central Bank announces that it will allow its currency to float. Panel A presents results partitioning firms based on firm size (SIZE) and proportion of foreign sales (FSALE). Panel B presents results partitioning firms based on currency hedging activity (DERIVSUSE) and level of geographic diversification (DIVERSE). Panels A and B report the intercept estimates from the full conditional return model that regresses the absolute value of long-window abnormal returns on intercepts and controls for crisis country characteristics described in Table 3. The model is estimated with four separate intercepts, one for each currency-regime/cross-sectional variable combination. Below each intercept estimate, we report the number of observations in that currency-regime/cross-sectional variable combination. Panel C provides the DID estimate, with F-test and currency exposure effect for 9 returns model specifications and four proxies for hedging. The computation of AR is described in Section 2.3. *** {**} (*) indicate significance at the 1% {5%} (10%) level (two-sided). Standard errors are clustered at the firm level.

Interpretation: Using traditional measures of exposure, firm size and foreign to total sales in Panel A, the difference-in-differences results are insignificant. Using two measures of hedging, a dummy variable for derivatives use and a measure of diversified operations in Panel B, the diff-in-diff tests show that the regime shift announcement abnormal returns were larger for firms with more exposure/less hedging, and these differences were larger for firms with exposure to the tied-$ countries than for those with exposure to tied-other countries, consistent with currency risk affecting firm value. Panel C estimates show that the currency risk effects are economically large.

	Fixed rate regime			Floating rate regime		
	(1)	(2)	(3)	(4)	(5)	(6)
Tied-$		-0.119***	-0.087***		-0.131***	-0.107***
		(-9.48)	(-3.86)		(-8.98)	(-4.31)
Tied-other		-0.129***	-0.104***		-0.135***	-0.101***
		(-9.28)	(-4.58)		(-8.73)	(-4.60)
Acquirer characteristics:						
Regional exposure	0.005***	0.005***	0.004***	0.004***	0.004***	0.003***
(REGIONAL)	(5.08)	(5.34)	(4.32)	(4.54)	(4.53)	(3.78)
International experience	0.002***	0.002***	0.001***	0.002***	0.002***	0.002***
(GLOBAL)	(7.59)	(7.25)	(6.85)	(8.52)	(8.43)	(7.89)
Firm size	0.002	0.002	0.001	0.002	0.002	0.002*
(SIZE)	(1.10)	(1.11)	(1.11)	(1.59)	(1.60)	(1.76)
Market-to-book	0.000	0.000	0.000	-0.000**	-0.000**	-0.000***
(MTB)	(0.19)	(0.17)	(0.27)	(-2.31)	(-2.23)	(-2.93)
Sales growth	0.003***	0.003***	0.002***	0.004**	0.004**	0.003**
(GROWTH)	(3.43)	(3.64)	(3.61)	(2.11)	(2.08)	(2.24)
Market leverage	-0.032	-0.031	-0.014	0.009	0.009	0.011
(LEV)	(-1.08)	(-1.08)	(-0.63)	(0.29)	(0.27)	(0.47)
Market leverage squared	0.038	0.036	0.020	-0.021	-0.020	-0.013
(LEV2)	(1.07)	(1.06)	(0.76)	(-0.52)	(-0.51)	(-0.47)
Cash flow	0.051	0.047	0.034	0.005	0.003	0.011
(CF)	(1.34)	(1.25)	(1.20)	(0.10)	(0.06)	(0.29)
R&D	0.000	0.000	0.000	-0.046	-0.046	-0.025
(RD)	(0.07)	(0.05)	(0.49)	(-0.98)	(-0.99)	(-0.85)
Currency hedging	0.001	0.001	0.007	-0.012	-0.012	-0.002
(DERIVSUSE)	(0.20)	(0.13)	(1.40)	(-1.42)	(-1.47)	(-0.29)
Crisis country characteristics:						
UK			-0.018			-0.029**
MX			-0.099***			-0.033
EFW			-0.001			-0.006
LEGALO-ENG			-0.011			0.023
LEGALO-FR			-0.011			0.006
LEGALO-GER			-0.044**			0.007
SHRIGHTS			0.009*			0.014
MAVOL			-0.060			0.071
MAFREQ			0.120***			0.207***
GDPCHG			-0.058			0.010
TRADEFLOW			1.999***			1.183**
OVERLAP			-0.019			n/a
CURRΔTOT			n/a			0.009
Tied-$ - Tied-other		0.010*	0.017*		0.004	-0.006
χ^2-test for difference across coefficients [*p*-value]		[0.064]	[0.079]		[0.391]	[0.540]
N	3,873	3,873	3,873	3,799	3,799	3,799
Predictive accuracy	80.22%	79.60%	81.90%	81.47%	80.76%	81.02%

Table 5: Logit analysis of entry into the crisis countries

Description: This table reports results from logit model estimations of the probability that a firm enters a crisis country. Columns (1)-(3) estimate entry propensity in the fixed rate regime; columns (4)-(6) estimate entry propensity in the floating rate regime. The table reports the marginal effects of the regressors calculated at the means of the regressors across all available observations. All variables are defined in Appendix B. Values for Intercepts suppressed for columns (1) and (4). The z-statistics for the marginal effect estimates are reported in parentheses and

based on standard errors that are clustered at the firm level. *** {**} (*) indicate significance at the 1% {5%} (10%) level (two-sided).

Interpretation: Controlling for firm and country characteristics, US MNCs are 1.0 to 1.7% more likely to establish a new operating presence in a fixed exchange rate regime country with a currency tied to the dollar than with a currency tied to something else (weakly significant at ten percent).The falsification test, columns 4-6, which shows no differences in entry propensity across the same two sets of countries during the initial year of floating exchange rates, suggests the difference during the fixed regime is not driven by systematic differences in the two sets of countries. Nevertheless, the difference in entry probability during fixed regimes is economically and statistically small, suggesting a minimal effect of currency risk on FDI location choice.

	(1) FIXED Neutral	(2) FLOATING Neutral	Difference (1) – (2)	χ^2-test [*p*-value]
Tied-$	-0.016	0.023	-0.039	[0.683]
Tied-other	-0.007	0.031	-0.038	[0.699]
Tied-$ - Tied-other	-0.009	-0.007	-0.002	[0.923]
F-test for diff. across coefficients [*p*-value]	[0.625]	[0.670]		
Control variables				
Acquirer characteristics	Included	Included		
Deal characteristics	Included	Included		
Country characteristics	Included	Included		
N	253	343		
Adjusted R^2	16.5%	15.5%		

Table 6: Announcement date returns to cross-border acquisition announcements

Description: This table reports US MNC acquirer abnormal returns in the five day window from -1 to +3 around the announcement of an acquisition into one of the crisis countries. Cumulative abnormal returns (CARs) for each firm *i* equal the residual from a two factor market-industry model. The market-industry model parameters are estimated over the period from 11 to 265 days before the announcement using returns from a value-weighted market portfolio and a value-weighted industry portfolio based on the target's Fama French 48 industry classification. The CARs are measured separately for acquisitions into countries that were ending a fixed rate regime with the currency tied to the dollar and into countries that were ending a fixed rate regime with the currency not tied to the dollar and separately for announcements in the fixed neutral and floating neutral periods. Control variables include acquirer characteristics (i.e., EXPOSED, SIZE, MTB, GROWTH, LEV, LEV2, CF, RD, DERIVSUSE), deal characteristics (i.e., 100PERC, COMPETE, DISTINCT, HOSTILE, INVOPP, ORG_JV, ORG_PRIVATE, ORG_PUBLIC, ORG_SUB, RELSIZE, SINGLE, TENDER, TOEHOLD), and country characteristics (i.e., EFW, LEGALO-ENG, LEGALO-FR, LEGALO-GER, SHRIGHTS, MAVOL, MAFREQ, GDPCHG) described in Appendix B. Coefficient estimates on the control variables are not presented. *** {**} (*) indicate significance at the 1% {5%} (10%) level (two-sided), respectively. Standard errors are clustered by firm.

Interpretation: The results show no significant differences in the US MNC cross border announcement abnormal returns between acquisitions in tied-$ versus tied-other countries, or between acquisitions during the fixed neutral versus floating neutral periods.

Panel A: Not-hedge vs. hedge (Tied-$)

		FIXED Neutral (a)	FLOATING Neutral (b)	Difference (a) - (b)	χ^2-test for diff. across coefficients [*p*-value]
No Derivatives	(i)	-0.019 *n = 54*	0.027 *n = 38*	-0.046	[0.644]
Use Derivatives	(ii)	-0.018 *n = 75*	0.044 *n =85*	-0.062	[0.533]
Difference	(i) - (ii)	-0.001	-0.017	0.016	[0.370]
F-test for diff. across coefficients [*p*-value]		[0.940]	[0.309]		

Panel B: Not-hedge vs. hedge (Tied-other)

		FIXED Neutral (a)	FLOATING Neutral (b)	Difference (a) - (b)	χ^2-test for diff. across coefficients [*p*-value]
No Derivatives	(i)	-0.007 *n = 68*	0.030 *n = 102*	-0.037	[0.698]
Use Derivatives	(ii)	-0.012 *n = 56*	0.053 *n =118*	-0.065	[0.520]
Difference	(i) - (ii)	0.005	-0.023*	0.028*	[0.070]
F-test for diff. across coefficients [*p*-value]		[0.656]	[0.058]		
Comparing diff-in-diffs Tied-$ - Tied-other				-0.012	[0.613]

Panel C: Less vs. more diversified (Tied-$)

		FIXED Neutral (a)	FLOATING Neutral (b)	Difference (a) - (b)	χ^2-test for diff. across coefficients [*p*-value]
Less		-0.026 *n = 69*	0.054 *n = 89*	-0.080	[0.432]
More		-0.018 *n = 60*	0.017 *n =34*	-0.035	[0.741]
		-0.008	0.038*	-0.046*	[0.064]
		[0.597]	[0.070]		

Panel D: Less vs. more diversified (Tied-other)

		FIXED Neutral (a)	FLOATING Neutral (b)	Difference (a) - (b)	χ^2-test for diff. across coefficients [*p*-value]
Less		-0.016 *n = 115*	0.059 *n = 215*	-0.075	[0.476]
More		-0.004 *n =9*	0.030 *n =5*	-0.034	[0.711]
		-0.012	0.028	-0.040	[0.161]
		[0.584]	[0.176]		
				-0.006	[0.871]

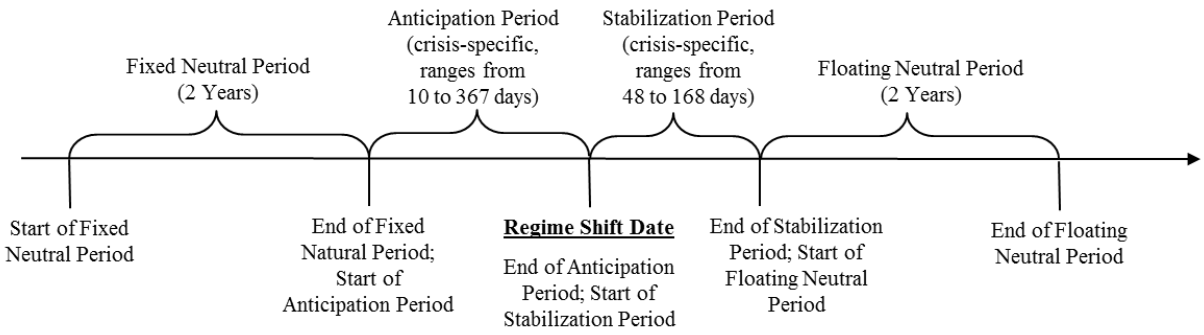
Table 7. Cross-sectional difference-in-differences analysis for announcement date returns to cross-border acquisition announcements

Description: This table reports difference-in differences values for acquirer cumulative abnormal returns (CARs) in the five day window from -1 to +3 around the announcement of an acquisition into one of the crisis countries. Panels A and B present results partitioning firms based on currency hedging activity (DERIVSUSE). Panels C and D present results partitioning firms based on the level of geographic diversification (DIVERSE). The panels report the intercept estimates from the full return model that regresses the CARs on intercepts and controls for acquirer, deal and crisis country characteristics described in Table 6. The model is estimated with four separate intercepts, one for each currency-regime/cross-sectional variable combination, and separately for announcements in the fixed neutral and floating neutral periods. Below each intercept estimate, we report the number of observations in that currency-regime/cross-sectional/period

variable combination. The computation of CAR is described in Section 3.2. *** {**} (*) indicate significance at the 1% {5%} (10%) level (two-sided). Standard errors are clustered at the firm level.

Interpretation: Triple difference-in-differences tests (fixed versus floating periods, hedged versus not hedged, and targets in tied-$ versus tied-other countries), show no significant differences in US MNC cross border acquisition announcements abnormal returns, suggesting currency risk does not affect cross border acquisition announcement abnormal returns.

Figure 1. Timeline of periods surrounding regime shift



See Table 1 for regime shift dates.