

1st Referee's Report

Comment A

The paper clearly shows evidence of asymmetry in the volatility spillover effects. To do so, they extend existing models in the literature as to allow for this asymmetry, and further use a more flexible GARCH model. Although the model contains some (minor) methodological innovations, it does not succeed to explain why there is asymmetric behavior. Why do negative shocks have a higher impact than positive shocks? Can it be explained by business cycle variation, or is it contagion? The paper does suggest that the higher negative impact for Mexico (relative to Canada) can be explained by the liberalization process, making emerging stock markets more vulnerable to global market shocks, but no formal analysis is done to prove that. So basically, the model is not capable of extracting the driving economic forces behind the results.

Response: The referee is correct in noting that the model is incapable of extracting the driving force behind the results. However, any GARCH representation is most likely not capable of doing that. The main point of our paper, however, is to document the presence of asymmetry in cross-market linkages (both in mean and in variance) and their differential impact on a developed market (Canada) versus an emerging market (Mexico). The referee raises several explanatory hypotheses, and in particular the hypothesis of contagion. When contagion refers to a significant increase in cross-market linkages after a shock to one country then the difficulty in separating contagion from high volatility should not be underestimated. However, as pointed out by Boyer (1999) and Forbes and Rigobon (2002) and, incidentally, also noted by the referee, one cannot directly compare the correlations between tranquil periods and periods of turmoil since one would expect such correlations to increase during periods of high volatility even under the null hypothesis of no contagion.

Instead, we prefer to view the problem as one of liberalization, and document that the liberalization reforms of May 1989 and January 1992 made the emerging stock market of Mexico more vulnerable to shocks from the US. In the paper we only show the full sample results. These results indicate that the correlations increase in the post-liberalization period. Furthermore, since the correlations between the US and Mexican returns increase substantially following the Asian financial crisis of October 1997, we also re-estimated the model over the sub-periods Jan 1 1992 to the Asian stock market crash of October 27 1997 and October 28 1997 to December 31, 2003.

The results over the two sub-periods, which are not reported in the paper for economy of space, but are available upon request from the authors, confirm the results obtained using the full sample that there is an increase in correlations in the second sub-period.

References:

Boyer, Brian H., Michael S. Gibson, and Mico, Loretan, 1999, Pitfalls in tests for changes in correlations, *Board of Governors of the Federal Reserve System International Finance Discussion Paper* 597.

Forbes, Kristin J., and Roberto Rigobon, 2002, No contagion, only interdependence: Measuring stock market comovements, *Journal of Finance* 57, 2223–2261.

Comment B

Second, the spillover parameters are enforced to be constant. There is a recent paper by Baele and Inghelbrecht (2007) which clearly shows that exposures to global market shocks are time-varying, mainly driven by cyclical movements and the trade integration process. It could be that once allowing for these structural and economic driving forces, the asymmetric spillover effects disappear. In case of this study, you could expect integration en cyclical movements to have an effect on the transmission of shocks from the US to Canada and Mexico. This may be an interesting topic to explore.

Response: It is true that in our paper the estimates of the spillover parameters are enforced to be constant. However, the variance ratios and implied correlations between unexpected returns are not. This result is a familiar one in the spillover literature (Bekaert and Harvey, 1997; Baele, 2005; Ng, 2000, among others). We move this literature in a new direction, as our paper is the first in the literature to utilize results on the moments of censored normal distribution in deriving the conditional time varying variance ratios and the implied correlations of unexpected returns under conditions of asymmetry of spillover effects.

The referee also believes that if we had time varying parameters we would get different results. While this is certainly possible, we would like to note that we explored different sub-periods such as before and after the Asian financial crisis of October 1997 (results have not been reported for economy of space), but the presence of asymmetric spillover effects did not disappear.

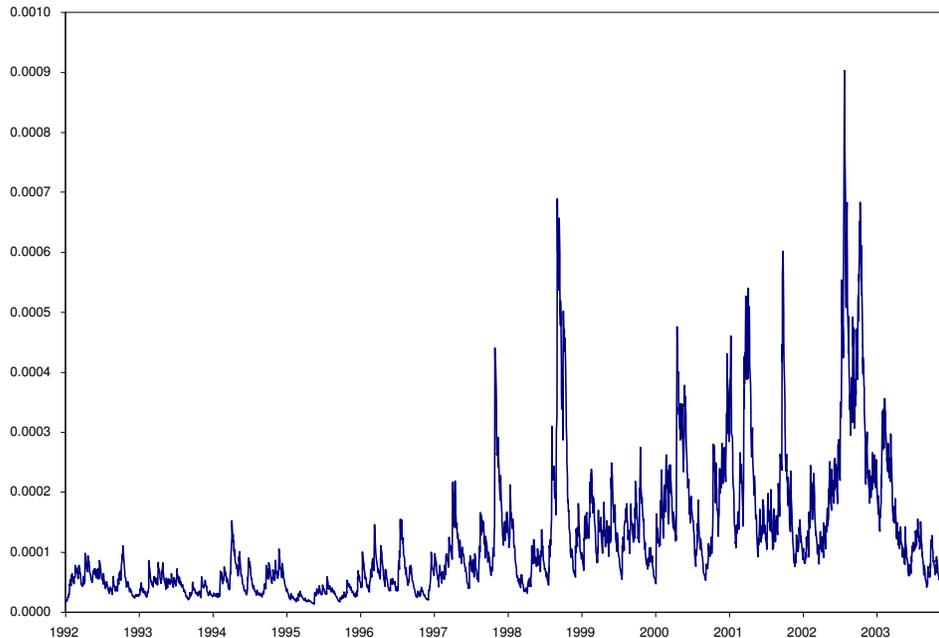
Comment C

The paper shows in figure 3 the implied correlations between the unexpected returns. Given the existing literature, we could expect that the correlation are higher in times of higher global (US) market volatility. The paper, however, does not investigate this link, although I could reveal interesting information.

Response: Exploring the issue raised by the referee on point C is straightforward. The correlations are indeed higher in times of higher global (US) volatility, as one can see from Figure (below), which depicts the variance of unexpected returns in the US, and Figure 2, which depicts the variance ratios using the estimates from model 2, as well as Figure 3, which depicts the correlations between US/Canada and US/Mexico unexpected returns. As noted in the paper, the variance ratio is the proportion of the variance of the

unexpected returns of Canada and Mexico that is driven by US volatility. As US volatility increases, we note that the variance ratios increase too. Alternatively, the correlations between unexpected returns are higher when the variance of the unexpected returns in the US is higher.

Figure 4. Variance of US Unexpected Returns. January 1, 1992-December 31, 2003



Comment D

The model used in the paper has some nice features, for instance allowing for three sorts of asymmetric effects. Moreover, the authors praise the APARCH specification for its functional flexibility. This may be somehow overstated. First, the lag structure of the specification is enforced to be AR(1)-APARCH(1,1). No formal tests are done to come to this specification. The only flexibility of the model is through the power term, which is not enforced (as is the case in the traditional GARCH models), but is estimated. I am, however, not convinced that this flexibility really makes a difference. Moreover, significance tests show that each estimated model is in line with one of the traditional models.

Response: The AR(1)-APARCH(1,1) specification of the model is not as arbitrary as stated by the referee, which we are willing to make clearer in the paper. We did investigate other lag structures, but those results were not reported. First, we experimented with both the autoregressive AR(p) and the moving average MA(q) version of the mean equation, with p and q varying from 1 to 4. In all cases, the AR(1) specification appeared superior, based on the t-ratio of the higher terms. Second, we also experimented with APARCH (i,j) for i,j=1,2,3,4. In this case too the APARCH(1,1) proved superior as the higher terms were insignificant. Furthermore, the Ljung-Box

statistics based on the standardized and squared standardized residuals as well as the ARCH-LM test from the AR(1)-APARCH(1,1) indicated that this specification was satisfactory and adequate. The referee does not see the use of the APARCH model. Clearly, as the referee notes, the flexibility of the model is limited to the power term. Still, the model encompasses several other GARCH extensions as special cases. We do note, however, where tests of individual parameters do not result in the rejection of these same values as those for specific GARCH models. However, such tests do not invalidate the joint significance tests which reveal a clear rejection of these traditional GARCH models (Bollerslev and Taylor/Schwert) and thus provide for our selection and the relevance of the APARCH specification.

Comment E

Some data issues. I believe you should work with return indices, i.e. including dividends. Moreover, as a robustness check, you should do the analysis for indices expressed in a common currency (US dollar). From the point of view of the investor (for instance US investor), working in a common currency is in place.

Response: As noted in the paper on p. 15, the indices utilized in the paper are provided by Commodity Systems, Inc. and are downloaded from the Yahoo Finance portal. The web site reports for each day that the market is open the price at the close of the day and the adjusted close price (adj. close), which is the price at the close adjusted for dividends and splits. In the paper we used the latter, although there is no difference between the two. We erroneously stated we were using the closing price rather than the adjusted closing price. We corrected this in the revised paper. As for doing the analysis using US dollars indices as a robustness check, we agree that from the viewpoint of the US investor this is quite relevant. However, the approach opens up the risk of potentially confounding stock market volatility with exchange rate volatility which is beyond the scope of our paper.

2nd Referee's Report

Comment 1

The authors claim to “extend the standard shock spillover model of Bekaert and Harvey (1997), Baele (2003) and Ng (2000) to account for asymmetries”. This affirmation could be misleading. Those studies also account for asymmetries, though they are asymmetries in volatility spillover effects. As far as I understand it, the current study innovates by including asymmetry modeling in mean returns.

Response: Revised abstract to clarify the intent of the paper.

Comment 2

I do not fully agree with the statement on page 2: “correlations in volatility and returns appear to be causal from the US market while none of the other markets explains US stock market movements”. I would rather include some references or be less strict in the second part of the sentence.

Response: We revised this in the paper: “correlations in volatility and returns appear to be causal from the US market.”

Comment 3

Similarly, I do not agree with the statement on page 3: “Yet, there is no evidence in the literature documenting that the international transmission of stock returns and volatility also exhibits asymmetric behaviours”. There is empirical evidence on that (see, for instance, Booth, Martikainen and Tse (1997)). The same applies to: “This generality in the modeling of spillover effects has thus far been absent in the literature studying dependencies in national stock markets”, on page 4.

Response: We revised in the paper by adding Booth, Martikainen and Tse (1997) on page 3 and 4. : “Yet, there is little evidence in the literature documenting that the international transmission of stock returns and volatility also exhibits asymmetric behaviors”. The same applies to: “This generality in the modeling of spillover effects has thus far been scant in the literature studying dependencies in national stock markets”, on page 4.

Comment 4

Would it be possible to model volatility transmission among the three countries considered using a multivariate GARCH specification? This way, one could analyze as well interactions between Canada and Mexico and reverse spillover effects from the Canadian and Mexican markets to the US.

Response: The multivariate GARCH model could be extended to handle positive and negative US shocks in modeling volatility transmissions among the three countries. Assuming a truncated/censored multivariate normal distribution for the innovations could handle this, but it has not been done in the literature. The likelihood function will get complicated due to the presence of multiple integrals. However, a simulated EM algorithm could be written to compute maximum likelihood estimates. The difficulty is that EM is likely to be too slow to converge and may only find local maxima. This approach will be explored in future research by the authors.

Comment 5

Regarding the data, are the indices used comparable? Why not using indices from a common database (DataStream?) so that they are more easily comparable? Would results be different by using indices measured in a common currency?

Response: As noted in the paper on p. 15, the indices utilized in the paper are provided by Commodity Systems, Inc. and are downloaded from the Yahoo Finance portal. The web site reports for each day that the market is open the price at the close of the day and the adjusted close price (adj. close), which is the price at the close adjusted for dividends and splits. In the paper we used the latter, although there is no difference between the two. We erroneously stated we were using the closing price rather than the adjusted closing price. We corrected this in the revised paper. As for doing the analysis using US dollars indices as a robustness check, we agree that from the viewpoint of the US investor this is quite relevant. However, the approach opens up the risk of potentially confounding stock market volatility with exchange rate volatility which is beyond the scope of our paper.

Comment 6

On page 17, an explanation to justify the selection of two sub-periods would certainly help.

Response: At page 18 we state:

"Table 2 shows that the cross-market correlations are substantially different before and after the US stock market crash of October 1997"

The same applies to Table 1. The choice of the dates is defined by the stock market crash of October 1997.

Comment 7

In the notation used through the paper, is $d = \delta$? A more complete definition/interpretation of δ would also help, both in the methodology and the empirical results sections.

Response: The issue of $d = \delta$ has been resolved. On pages 7-8, the power term is denoted by δ in equation (4) and can be given by any positive values. In particular, Ding, Granger and Engle (1993) conclude that when $\delta = 1$ the long memory property of stock returns is the strongest compared to other values of δ .

Comment 8

The results found by some other studies analyzing volatility spillovers between US and Canada could have been mentioned in the literature review (see Karolyi (1995), Darbar and Deb (1997), Ramchand and Susmel (1998) and Susmel (2000)).

Response: We revised in the paper and added these references.

Comment 9

Why variance ratios are obtained under the assumption that “volatility spillovers originating from the US equity market have symmetric effects on the volatility of the Canadian and Mexican returns”? See page 13.

Response: At p. 23 and 24 we state the reason: the Wald tests fail to reject the hypothesis that $\theta_k^+ = \theta_k^-$, while do not fail do reject the hypothesis that $\varphi_k^+ = \varphi_k^-$.

We show that Eq. 22 shows the general formulation of volatility spillovers originating from the US equity market. Under the assumption of symmetric effects, eq. 22 reduces to eq. 23. We are just showing the simplification of volatility spillovers when symmetry is present.

Comment 10

On page 16, Why the fact that “the mean of the returns is higher for Mexico, as is the standard deviation” is “an indication of unconditional variance in returns, compared to Canada and the US.”?

Response: We revised in the paper and the sentence on page 16 to now read as follows: The results indicate that the mean of the returns is higher for Mexico, as is the standard deviation, compared to Canada and the US.

Comment 11

Finally, some minor and formal remarks:

Baele (2003) has already been published in the Journal of Financial and Quantitative Analysis, Vol. 40, No. 2, June 2005.

The complete title for Ding, Granger and Engle (1993) is “A long memory property of stock market returns and a new model”.

Response: These references are corrected in the revised paper.

Comment 12

The figures and tables design and format could be improved in order to make it more easily readable (Some examples: title in bold and include notes in all tables).

Response: We revised in the paper.

Comment 13

Why do you use Q(12) and Q2(12) in Table 1 and Q(6) and Q2(6) in the rest of the paper?

Response: No special reason. We revised in the paper so that in Table 1 we now use Q(6) and Q2(6) to maintain this uniformity. The discussion on page 17 reflects this change as well.

Comment 14

Why do you use a 10% level of significance on page 20 instead of 5% or 1%?

Response: The p-values for this test are all greater than 10%.

Comment 15

On page 7, last line, “equation (4)” should be “equation (5)”.

Response: We revised in the paper.

Comment 16

Revise formulas, as some characters appear as “?”.

Response: We revised in the paper.