Comments

Graciela L. Kaminsky: Contagion and the contagious nature of currency crises have been at the center of the economic discussion in recent years in both academic and policy circles. The stampede of investors out of emerging markets in the late 1990s and the explosion of international capital flows that preceded these crises have convinced many that we should turn back the clock to the times of capital controls. Many advocate the introduction of controls on capital inflows to reduce excessive euphoria among international investors.[1] Even controls on capital outflows, dismissed not long ago as ineffective, have become fashionable again. Krugman, for example, argues that restrictions on outflows may help to manage an otherwise disorderly retreat of investors from emerging markets in the aftermath of a crisis elsewhere.[2]

The question is whether there is, in fact, contagion. The debate is far from settled: the profession does not even agree on the definition of contagion. While many talk of contagion as interdependence not explained by market fundamentals, others label a massive retreat from emerging markets following increases in interest rates in the United States as a contagious crisis. The paper by Forbes and Rigobon contributes to clarifying the debate by reviewing the literature on contagion and re-examining the validity of some of the claims of the empirical research on the subject. It also contributes to the confusion, however, by adding yet another definition of contagion.

The paper largely summarizes previous research by both authors.[3] It begins with a discussion on the meaning of contagion, which they define as an increase in interdependence after a shock. If correlations in world financial markets do not change following a shock, it is business as usual and therefore not contagion (according to Forbes and Rigobon). By the same token, if correlations decline, contagion is eliminated. Does this definition help to clarify the nature of financial interdependence? I think

1. See, for example, Stiglitz (1999); Eichengreen (1999).
2. Krugman (1998).
3. For example, Forbes and Rigobon (1999, 2000).

not. During the Russian crisis, correlations between interest rates in Malaysia and the rest of the world declined substantially.[4] Did this represent the elimination of contagion? Perhaps, but the imposition of strict capital controls was one of the factors triggering this change in interdependence. Any effort to clarify whether the transmission of shocks across world financial markets is, in fact, contagion must thus start by identifying the channels (or elimination of the channels) of interdependence.

Still, not all is confusion in the paper. Forbes and Rigobon make an important contribution to our understanding of interdependence by pointing out that previous research on contagion (or interdependence) during crises might have been flawed. The idea is simple. Contagion and interdependence have traditionally been measured using correlation coefficients. However, the volatility of returns generally increases during crises, which biases the estimates of the correlation coefficient toward finding closer comovements of asset returns, even when interdependence has not changed. Forbes and Rigobon suggest using statistics that are not biased in the presence of heteroscedasticity.

This result is important. Previous research based on correlation coefficients highlights the increase in interdependence or contagion in times of crisis. In contrast, Forbes and Rigobon correct the correlation coefficient for heteroscedasticity, and they find that interdependence did not increase during the crises of the 1990s for most countries.[5] Prima facie, this implies that we can reject a number of models on crises and contagion, which indicate that crisis times are different from tranquil times. That is, as Forbes and Rigobon discuss, we can reject, for example, a number of papers based on the hypothesis that margin calls are at the core of crises. These models indicate that crises trigger massive sales by highly leveraged informed investors who need to cover margin calls. Uninformed investors may not recognize that margin calls are at the heart of these sales, believing instead that deteriorating market fundamentals in emerging markets are motivating the sale. They may therefore follow the actions of the informed investors, which can result in a major sellout that started because of a liquidity problem. This transmission mechanism does not occur during stable periods.

Having said that, the question is whether the corrections for heteroscedasticity guarantee unbiased estimates of the correlation coefficient.

4. Edison and Reinhart (1999).
5. See Forbes and Rigobon (1999, 2001).

I have two concerns on the implementation of the test. First, Forbes and Rigobon's correction for heteroscedasticity relies on an ad hoc identification of episodes of high and low volatility, mostly based on a chronology of news about the crises. This ad hoc identification can bias the results. I urge the authors to test for structural breaks in the volatility series and examine the robustness of their results. Second, the correction for heteroscedasticity relies on the identification of the source of the changes in volatility. This identification may be obvious in some episodes, such as the Mexican crisis in 1994–95, but it becomes quite blurred in others. For example, the episode of heightened volatility in the fall quarter of 1998 cannot easily be ascribed to a single factor. While the Russian default in August certainly triggered the turbulence in world financial markets in the fall of 1998, the imposition of controls on capital outflows in Malaysia in September greatly affected investors' reassessment of risk and contributed dramatically to this turmoil. Furthermore, the role of the Russian default cannot be clearly untangled from the news about liquidity and balance-sheet problems in international financial centers that started to sprout in the spring and summer of 1998.[6] In this context, further testing on the sources of market turbulence is warranted.

Finally, although the paper is entitled "Contagion in Latin America," I am not sure what I have learned about contagion in this region. Forbes and Rigobon demonstrate that Latin American bond and stock returns are highly correlated not only within the region, but also with returns in other regions. Is this contagion? Is this interdependence? They do not answer these questions in this paper, but they do address the issue in earlier works, in which they examine, for example, whether the so-called tequila crisis was contagious.[7] After correcting the bilateral correlation coefficient of stock returns with Mexico, they conclude that there was no contagion (defined as a change in comovements) from Mexico to other asset markets in Latin America or to asset markets in other regions of the world. Does this clarify the contagious nature of the Mexican crisis? Not really. According to Forbes and Rigobon's metric, Argentina and Chile look the same in the aftermath of the Mexican crisis, when in fact the countries could not look more different. While Argentina suffered serious bank runs and dramatic speculative attacks against the peso, Chile was left unscarred.

6. See, for example, Kaminsky and Reinhart (2000).
7. Forbes and Rigobon (1999).

The authors' conclusions on the Asian crisis also lack insight about contagion and the channels of Latin America's interdependence with other regions. For example, Rigobon concludes that the Asian crisis did not affect asset markets in Latin America differently than in tranquil times, but he finds that comovements between financial markets in Asia and Colombia did increase during the Asian crisis.[8] Does this constitute contagion? Perhaps, but it is unclear what the channels of interdependence are: Colombia does not have developed financial markets, foreign investors' exposure to Colombia is basically null, and Colombia's trade links with Asia are tiny. Could this change in comovements between assets markets in Colombia and Asia be explained by the fact that Colombia was brewing a crisis of its own? Surely, one cannot draw policy conclusions for Latin America without a better understanding of the nature of interdependence.

Andrea Repetto:[9] This work summarizes a series of earlier papers by Forbes and Rigobon and applies their results to contagion in Latin America. In these earlier works, the authors fundamentally revise standard statistical techniques; they find that after correcting for a variety of problems, asset price synchronization across countries does not increase during crises.[10] According to the authors, this evidence rules out theories that predict that contagion is crisis-contingent. When read in isolation, the current paper does not show how much progress the authors' work makes in revising the empirical literature and demonstrating how to carefully handle some commonly used statistical tools.

The basic idea behind their work is that correlation coefficients vary with the volatility of the underlying phenomenon. Correlation coefficients in periods of stability and periods of turmoil are therefore not comparable, and they cannot be used as a basis for testing for contagion. Their work further shows that the standard estimation techniques are subject to additional biases as long as asset prices around the world are jointly endogenous and as long as exogenous variables are omitted from empirical models. Fortunately, Forbes and Rigobon have developed quite ingenious

8. Rigobon (1999).
9. I am grateful to Rodrigo Valdés for many useful conversations.
10. Forbes and Rigobon (1999).

ways of dealing with these problems. Their methodological contribution can and should be applied to a wide variety of economic problems.

Since it is my role as a discussant to dissent, I take the opportunity to present a few quibbles. I begin with comments on methodological issues and then discuss their definition of contagion. Finally, I propose directions for future work.

Shift-Contagion Tests

Using Forbes and Rigobon's notation, assume that two stock markets comove as follows during tranquil periods:

$$x_t = \alpha y_t + \eta_t \text{ and}$$

$$y_t = \beta x_t + \varepsilon_t,$$

with $E(\varepsilon_t) = E(\eta_t) = E(\varepsilon_t \cdot \eta_t) = 0$, $E(\varepsilon_t^2) = \sigma_\varepsilon^2$, $E(\eta_t^2) = \sigma_\eta^2$, and $\alpha\beta \neq 1$.

Now suppose a crisis hits the stock market returns x_t and the structural model becomes

$$x_t = \alpha y_t + \eta_t + \lambda\eta_t \text{ and}$$

$$y_t = \beta x_t + \varepsilon_t + \phi\eta_t.$$

Forbes and Rigobon define contagion as $\phi \neq 0$ and interdependence as $\beta \neq 0$. The authors argue that it is impossible to estimate this model consistently using standard techniques: the stock market returns are endogenous, and volatility increases during a crisis.[11] In a very provocative series of papers, however, Rigobon constructs a method that allows us to consistently estimate β and at the same time test the hypothesis of no contagion ($\phi = 0$).[12] Let z_t^h and z_t^l represent the variable z_t during the high and low volatility periods, respectively ($z = x,y$), and let T^h and T^l represent the number of observations in each period. Rigobon shows that under the null hypothesis of no contagion, the instruments

11. They further argue that the standard models suffer from an omitted-variable bias, which I am not taking into account in this example.

12. Rigobon (1999).

$$w_t^1 = \begin{bmatrix} \frac{x_t^{h'}}{T^h} \\ -\frac{x_T^{l'}}{T^l} \end{bmatrix} \text{ and } w_t^2 = \begin{bmatrix} \frac{y_t^{h'}}{T^h} \\ -\frac{y_T^{l'}}{T^l} \end{bmatrix}$$

are valid for x_t in the y_t equation.[13] He further shows that it is possible to construct a Hausman-type test of the no-contagion hypothesis based on the comparison of the two IV estimates of β. This is a very useful finding, since it indicates that there is no need to look for special instruments outside the data sets. Using this technique, both Rigobon and Forbes and Rigobon find that the tests do not reject the validity of the instruments, and hence they conclude that there is "no contagion, only interdependence."[14]

The proposed test is not strictly a shift-contagion test, but rather a test of the stability of the model in and out of crises. For instance, assume now that during the crisis the true model is instead

$$x_t = \alpha y_t + \eta_t + \lambda\eta_t \text{ and}$$

$$y_t = (\beta + \theta)\, \chi_t + \varepsilon_t,$$

such that the strength of the interdependence increases, but shift-contagion does not occur. Under the null hypothesis of no excess interdependence, w_t^1 and w_t^2 are still valid instruments, and the same Hausman-type test can be used to determine whether the null hypothesis that θ is equal to zero is true. If the null hypothesis is rejected, however, one might erroneously conclude that the process in question is shift-contagion ($\phi \neq 0$) rather than excess interdependence ($\theta \neq 0$). In other words, if the null hypothesis is accepted, then the model did not change during the crisis, but if it is rejected, then it is not possible to determine which underlying model is the true one. This example can be extended to shifts in α and σ_ε^2, such that the rejection of the null is evidence of either shift-contagion ($\phi \neq 0$) or changes in the parameters α, β, or σ_ε^2—or any combination of them. This is not a problem for Forbes and Rigobon, since they do not reject the null

13. Rigobon (1999).
14. Rigobon (2000a); Forbes and Rigobon (1999).

hypothesis.[15] However, not knowing where to turn if the null is rejected limits the applicability of the method. Future work should attempt to find instruments that are informative if the null is rejected.

What Does Contagion Mean?

Forbes and Rigobon define contagion as a situation in which the correlation between two stock markets increases during a crisis; they define interdependence as a correlation that does not depend on the occurrence of a crisis. From a policy point of view, is contagion what really matters? Should we be developing tests of contagion, or should we be looking into what lies behind the existing synchronization?

Forbes and Rigobon's definitions are quite misleading. First, countries that have very little in common exhibit a strong correlation in all periods. Emphasizing the lack of extra correlation during a crisis switches the focus away from the crucial issue: understanding the source of the comovement. Second, countries do make efforts to insulate their economies from external crises—with or without contagion à la Forbes and Rigobon. By revealed preference it does not seem to matter whether the transmission mechanism is contagion or interdependence. Third, the authors classify theories as crisis-contingent and non-crisis-contingent, and they argue that a contagion test gives us information on the underlying model. If the hypothesis of contagion is rejected, then the true model is a non-crisis-contingent one. The classification used is not very informative, however: whenever prices are forward looking, the mechanism that triggers the transmission of crises is relevant even if no crisis ever occurs. Many of the theories classified as crisis-contingent thus predict correlation during tranquil times. Finally, the contagion tests are crucial if the implied policy prescriptions depend on the event of a crisis. Different policy prescriptions have been advanced to help insulate countries from external shocks, including trade diversification, various exchange rates arrangements, the formation of monetary unions, and capital account regulations. The efficacy of these prescriptions depends on the exact source of the correlation, not on whether the true model is crisis- or non-crisis-contingent.

15. Forbes and Rigobon (1999).

Looking for Further Evidence

I propose three possible sources of additional information. First, the macroeconomic evidence indicates that quantities vary much more than prices. The finding that comovement in prices is the same in and out of crises does not necessarily mean that the correlation of quantities remains constant. The profession should develop and analyze models that study the effects on quantities as well as prices and build evidence on quantities as rich as the existing information on prices.

Second, empirical models should look for potential asymmetric responses to shocks. Some models do predict these effects, so the evidence might be helpful in disentangling the source of the correlation. Similarly, the importance of the size of the shocks should also be analyzed.

Finally, exchange rate flexibility and the maturity structure of the external debt are known to affect the transmission of shocks across countries and markets, but capital controls do not limit contagion.[16] Models need to be able to predict the circumstances under which a set of policies will or will not work. This evidence would also help understand the transmission mechanisms that underlie contagion.

Conclusions

Forbes and Rigobon have made an important methodological contribution, and they have highlighted the risks of using poor statistical techniques. Their work can and should be applied to a wealth of economic questions. The usefulness of their method would be enhanced, however, if they attempted to find instruments that are informative when the null hypothesis is rejected. Finally, future work should directly address the question of what lies behind the comovement of markets across countries.

16. Edwards (1998); De Gregorio and Valdés (2001).

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