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Price Volatility, International Market Links, and Their Implications for Regulatory Policies

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Disastrous events sometimes engender redeeming social benefits. The international crash of equity markets in mid-October 1987 is a case in point, for it has provoked many scientific papers, both theoretical and empirical, on market volatility, international market links, and market structure, papers that may have never been written without the crash as a catalyst. The purpose of the present article is to provide an assessment of some of the most significant empirical studies and to summarize their implications about possible regulatory reform of the equity and futures markets. In addition, I will present some new evidence about volatility and its relation to existing market regulations across countries.

Specifically excluded from my survey of empirical papers will be reports provided by various "commissions." Some of these reports contain fine empirical work, but they are not really scientific studies in the sense of having been subjected to a thorough review by peers before publication. Kamphuis, Kormendi, and Watson (1989) provide excerpts that include the most important empirical results from six different commission reports.[1]

But even without such commission reports, there are plenty of good papers to examine, some of which have surprising new conclusions. Their technical sophistication is often quite advanced for the general reader, so one of my goals is to summarize their contributions in nontechnical language.

The October 1987 Crash posed three important scientific questions:

1. What were its causes?
2. Why and how did it propagate internationally?
3. Was it related to particular institutional practices, market arrangements, or regulatory policies?

The subsequent three sections of the article are devoted respectively to evidence about these three questions.

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1. Empirical papers on the causes of the crash

1.1. *Suggested causes and "triggers"*

The first question has been the most difficult to answer. Just after the crash, journalists and politicians (those experts) ascribed it to a variety of sources ranging from portfolio insurance to inadequate computer systems. However, empiricists have since found it difficult to confirm any of these contentions. For instance, though portfolio insurance is a popular culprit in some circles, I was unable to find any evidence that markets where portfolio insurance was widely used declined to a greater extent than markets where portfolio insurance was virtually absent. In fact, countries with portfolio insurance crashed *less* than countries without it, (Roll, 1989, table 4).[2] Bertero and Mayer (1989) confirm this finding.

Another allegation points to *all* uses of stock index futures or other related futures contracts, not just portfolio insurance. The argument seems to be that irrational speculators cause destabilization. However, I could find no evidence that stock markets with related futures markets crashed any more than stock markets in countries without futures exchanges. In a longer term study, Edward (1988a) concludes that the introduction of futures trading on a particular index, the S&P 500, coincided with a period of lower, not higher, volatility. However, a recent paper by Harris (1989) finds evidence that individual stocks that are included in the S&P 500 index have higher volatilities than a control group of similar stocks. The effect is not very large, but it appears to be statistically significant and conceivably could be related to institutional practice such as index arbitrage or widespread S&P 500 index fund investing.

There is better supporting evidence that a particular event in the United States triggered the worldwide crash. According to Mitchell and Netter (1989), the triggering event was the introduction in the U.S. Congress of a tax bill that would have severely penalized corporate takeovers, leveraged buyouts, and other similar activities. These authors provide empirical support in several forms. First, takeover candidates were more severely negatively affected in the week preceding October 19. Second, takeover candidates subsequently displayed price increases when congressional support for the tax bill waned. Third, in the week before the crash, the U.S. equity market declined more than a value-weighted average of other would markets.[3]

The Mitchell/Netter explanation of the crash, though indeed intriguing, depends on a chain of difficult-to-prove propositions. If the U.S. tax bill caused the worldwide crash (which was much worse in some countries than in the United States), we must accept (1) that heavier taxation of takeovers would cause *all* stocks to be affected; (2) that the U.S. decline on October 14–16 induced an even larger crash on October 19, although tax bill news had already been fully disseminated earlier; and (3) that a stock price decline in the United States *resulting from a proposed U.S. tax bill* caused at least as large a decline on average in other countries.

All of these propositions are possible, but are they really plausible? A persuasive argument can be advanced against each one. Regarding the first proposition, why would even the complete elimination of takeovers, not just a marginally higher tax, cause a 20 percent decline in the aggregate market value of *all* stocks? Takeovers have not often created such a dramatic increase in the combined value of two merging firms.[4] The second proposition is inconsistent with market efficiency; there was no news about the tax bill on October 19. The third proposition seems likely since past increases in U.S. taxes have had no memorable influence on stock markets in *other* countries.

A unique explanation of the crash is suggested by Amihud, Mendelson, and Wood (1989), who argue that investors became persuaded that the stock market was less "liquid" than they had believed previously. Somewhat ingenuously, these authors state that ". . . the main news which led to the prolonged decline in stock prices was the crash itself" (p. 1). In other words, the crash proved that the market was illiquid, and this caused a permanent downward revision in stock values.

This "explanation" is patently circular. The crash can't be explained by the crash! However, Amihud, Mendelson, and Wood might have an explanation for why stock prices failed to completely *recover* after the crash. Given a crash induced by some other influence, it seems quite possible that investors might revise their estimates of market liquidity and therefore permanently write down values, even if the original cause of the crash were reversed.

The absence of an obvious or fully persuasive triggering event has led some to suspect the existence of a speculative "bubble" prior to the crash. Miller (1989) surveys many of the arguments advanced in favor of a bubble but concludes that a fundamentals-based explanation is just as plausible; possible fundamental causes include a revision in risk attitudes or a minor trigger that induced a major shift in expected growth rates. Also, there is suggestive international evidence against a bubble. As Goodhart (1988) notes,

> . . . valiant—but not entirely convincing—efforts have been made to identify Stock Exchange bubbles developing and breaking, simultaneously, in New York, London and Tokyo . . . I would challenge anyone to find a bubble also in Frankfurt, and yet the Stock Market there fell in line with the rest in October (p. 5).

However, the anti-bubble evidence is not entirely convincing either, at least to many authors. Fama (1989) says that the most questionable aspect of 1987 was not the crash itself but the incredible market advance during the previous five years. Telser (1989, p. 102) states, "The most plausible [theory] is that a speculative bubble burst in October 1987." Siegal (1988) presents empirical evidence that fundamental factors from January through September of 1987 would not have been plausible support for such large price increases as were observed in the United States. I found a negative relation between the market increase before the crash in different countries and the size of the crash decline (Roll, 1988, figure 9).[5]

Suspicion about a bubble has been abetted by some theoretical developments, particularly the relatively new literature on "rational" bubbles.[6] Prior to recent papers, many financial economists dismissed bubbles because they seemed inconsistent with rational investor behavior in a realistic setting[7] (Tirole, 1982, 1985), and they were thought to be observationally equivalent to price episodes driven by fundamentals (Hamilton, and Whiteman, 1985). An outstanding survey of this literature is provided by Camerer (1989).

Some bubbles are rational from investors' points of view, although there is usually some other market inefficiency. For example, a rational finite bubble for finite-lived assets in a

market with a finite number of traders is demonstrated by Allen and Gorton (1988). In the Allen/Gorton model, all funds are managed by two different types of professionals, and only one type has investment skill. A labor market inefficiency exists in the sense that investors can never tell the difference between worthless and able managers.

An interesting empirical paper in this tradition is by Hardouvelis (1989).[8] He looks for rational bubbles in the United States, Japan, and the United Kingdom, and he finds them, at least to his satisfaction. His argument is that a rational bubble requires an increasing risk premium as the bubble becomes more advanced,[9] and he obtains seemingly impressive empirical evidence that this actually happened in the (approximately) two years ending in September 1987.

How does Hardouvelils obtain ex ante risk premia in this period and then measure their increase? He first builds a model to predict ex post risk premia[10] as functions of various predictor variables such as the price/earnings ratio, the price-to-book-value ratio, lagged values of the observed risk premium, and various interest rate variables. Using a test period ending in March 1985, he finds that the price/earnings ratio and price-to-book-value ratio explain a surprisingly large fraction of the actual stock return in excess of the short-term interest rate over the *subsequent* 12 months. In the United States, the adjusted R^2 is 36 percent, in the United Kingdom it is 32 percent, and in Japan it is an unbelievable 66 percent! The explanatory power is only slightly higher with the other variables included as regressors.

The word *unbelievable* is appropriate. If one could predict 66 percent of *next year's* actual return with the current price/earnings ratio and price-to-book-value ratio, the accumulation of wealth would be immodest. Of course, next year's return would be easier to predict with hindsight, with earnings and book values that are not actually announced to the public until long after the end of the previous year (to which they refer). I suspect that some of Hardouvelis's seeming predictive power may be attributable to using accounting numbers that were not actually available to the market on the forecast date.

Hardouvelis finds that the slope coefficients in his predictive models of ex post risk premia increase during approximately two years before the October 1987 crash. This is consistent with a simple explanation; viz., stock prices increased more than usual over those two years. The conclusion that risk premia actually increased before the crash might be regarded as a munificent interpretation of the results.

Although theorists have concocted models that could conceivably explain bubbles, no one has yet provided completely persuasive evidence that such models have applicability to the crash event. Nonetheless, a bubble is at least as good an explanation of the crash as any other explanation that has thus far been advanced. Further empirical testing for a possible bubble definitely seems worthwhile.

A potentially measurable characteristic of a bubble is positive serial dependence among successive price changes during the bubble's expansionary period. Santoni (1987) used this feature to develop a test for bubbles in 1924–1929 and in 1982–1987. He was unable to find enough evidence of postive serial dependence to conclude that bubbles actually existed during those episodes.

However, as Hardouvelis (1988a, 1989) argues in commenting on Santoni's results, there is so much noise in stock returns that serial dependence is often difficult to uncover. Thus, it might be worthwhile to examine more extensive data, stock returns from the major free market countries, for evidence of positive serial dependence before and after the 1987 crash, in the hope that more observations will overcome the noise problem. The data and some new tests are described in the next section.

1.2. Empirical tests for speculative bubbles in 23 countries before and after the October 1987 stock market crash

In a previous paper Roll (1988), I described and utilized a data set consisting of broad stock market indexes from 23 countries. Except for the absence of Korea and Taiwan, they are *the* major stock markets of the world.[11] Daily price indexes are now available from January 1987 through March 1989 inclusive. Table 1 presents mean daily returns[12] and standard deviations of daily returns for each of the 23 countries and for three periods around the October 1987 crash.[13] In the approximately nine months before the crash, most of the countries exhibited a substantial price increase. The simple average of mean returns in all countries, .1606 percent per day in local currency, can be translated into about 40 percent per annum (even without the outlier of Mexico, the average is about 31 percent per annum). Subsequent to the crash, average returns have been considerably smaller, only .0659 percent per day or about 16.6 percent per annum. Of course, the crash period displays extremely negative returns and extremely large volatilities relative to both the pre- and post-crash periods.

During a speculative bubble, the degree of serial dependence could be highly nonstationary, swinging up and down and yet still being positive during most of the bubble's expansion. Thus, a method robust to departures from a stationary dependence structure would be particularly desirable. Traditional methods for measuring serial dependence, such as autocorrelation methods, usually assume stationarity and, consequently, *may* have weak detecting power.

A volatility comparison test, or variance ratio test, will detect nonstationary serial dependence. A variance ratio test relies on the proposition that a sum of *T independent* identically distributed random variables has a variance *T* times the variance of a single random variable. *Any* dependence among the variables will make the variance of the sum either larger or smaller depending on whether the dependence is mostly positive or negative. Thus, if we look at multiple day returns (which are sums of daily returns), their volatilities should reveal the presence of any day-to-day dependence and thereby reveal the traces of a bubble.

Volatilities for 23 different countries over several different holding intervals and for pre- and post-crash calendar periods constitute a massive set of numbers. In order to present the essential results in a compact form, I used the following procedure:

First, for each of the 23 countries, volatilities (variances) were calculated for daily returns and for returns over *nonoverlapping* 2, 3, 5, 10, 15, and 20 trading day holding intervals, for calendar periods before and after the crash.

Table 1. Daily returns during the pre-crash period, the crash period, and the post-crash period by country (percent/day)

Calendar Period	Trading Days	Mean (Local Currency)	Std. Dev. (Local Currency)	Mean (Dollars)	Std. Dev. (Dollars)
		Australia			
1/ 2/87–10/ 9/87	199	0.2239	0.8496	0.2661	1.0887
10/12/87–10/30/87	15	−3.5160	8.3145	−3.9796	8.9462
11/ 2/87– 3/31/89	364	0.0475	1.2160	0.1008	1.4902
		Austria			
1/ 2/87–10/ 9/87	198	−0.0202	0.7363	0.0093	0.9360
10/12/87–10/30/87	15	−0.8255	1.6634	−0.5071	1.7087
11/ 2/87– 3/31/89	361	0.0699	0.5571	0.0453	0.8144
		Belgium			
1/ 2/87–10/ 9/87	198	0.0808	0.8135	0.1156	1.0316
10/12/87–10/30/87	15	−1.6531	4.3161	−1.3836	3.9137
11/ 2/87– 3/31/89	363	0.0906	0.9649	0.0652	1.0516
		Canada			
1/ 2/87–10/ 9/87	200	0.1143	0.6890	0.1421	0.7793
10/12/87–10/30/87	15	−1.5150	5.4126	−1.5613	5.6947
11/ 2/87– 3/31/87	365	0.0405	0.7721	0.0672	0.8576
		Denmark			
1/ 2/87–10/ 9/87	199	0.0710	0.9198	0.0978	1.1514
10/12/87–10/30/87	15	−1.1254	2.6782	−0.8392	3.0185
11/ 2/87– 3/31/89	366	0.1518	0.7152	0.1250	0.8940
		France			
1/ 2/87–10/ 9/87	199	0.0114	0.9177	0.0383	1.0491
10/12/87–10/30/87	15	−1.6526	4.5678	−1.4625	4.3250
11/ 2/87– 3/31/89	365	0.1018	1.2542	0.0784	1.2242
		Germany			
1/ 2/87–10/ 9/87	199	−0.0296	1.2508	0.0000	1.2469
10/12/87–10/30/87	15	−1.5913	4.1776	−1.2696	4.0512
11/ 2/87– 3/31/89	366	0.0254	1.2924	0.004	1.2859
		Hong Kong			
1/ 2/87–10/ 9/87	199	0.2218	1.1213	0.2206	1.1209
10/12/87–10/30/87	11	−5.4174	12.0719	−5.4121	12.1229
11/ 2/87– 3/31/89	362	0.1083	1.3530	0.1089	1.3536
		Ireland			
1/ 2/87–10/ 9/87	199	0.2121	1.1746	0.2364	1.2815
10/12/87–10/30/87	15	−2.6303	5.2557	−2.3895	5.5859
11/ 2/87– 3/31/89	367	0.0852	1.3906	0.0617	1.3317
		Italy			
1/ 2/87–10/ 9/87	200	−0.0338	1.0171	−0.0230	1.1966
10/12/87–10/30/87	15	−1.3943	3.1849	−1.2208	2.9911
11/ 2/87– 3/31/89	367	0.0293	1.1486	0.0064	1.2087
		Japan			
1/ 2/87–10/ 9/87	200	0.1543	1.2743	0.2020	1.4567
10/12/87 10/30/87	15	−0.9777	5.5671	−0.7203	5.8080
11/ 2/87– 3/31/89	366	0.0810	0.9460	0.0929	1.0775
		Malaysia			
1/ 2/87–10/ 9/87	201	0.2821	1.1707	0.2937	1.1869
10/12/87–10/31/87	15	−3.6080	6.0258	−3.5503	6.0397
11/ 2/87– 3/31/89	363	0.1426	1.1363	0.1178	1.1216
		Mexico			
1/ 2/87–10/ 9/87	197	0.9831	2.5085	0.7018	2.6053

10/12/87–10/31/87	15	−3.4050	6.8924	−3.6194	6.9043
11/ 2/87– 3/31/89	360	0.0128	2.7538	−0.0924	2.8612
		Netherlands			
1/ 2/87–10/ 9/87	199	0.0672	0.9928	0.1028	0.9258
10/12/87–10/30/87	15	−1.5985	5.6771	−1.2690	5.6945
11/ 2/87– 3/31/89	366	0.0633	1.3012	0.0374	1.1533
		New Zealand			
1/ 2/87–10/ 9/87	199	0.0291	1.0908	0.1400	1.2773
10/12/87–10/30/87	15	−2.0473	5.2956	−2.8215	5.9162
11/ 2/87– 3/31/89	363	−0.0755	1.3659	−0.0640	1.5104
		Norway			
1/ 2/87–10/ 9/87	199	0.2473	1.2061	0.2997	1.2476
10/12/87–10/30/87	15	−2.5942	7.4383	−2.5134	7.6799
11/ 2/87– 3/31/89	366	0.1063	1.7726	0.0933	1.6964
		Singapore			
1/ 2/87–10/ 9/87	201	0.2508	1.0754	0.2685	1.1171
10/12/87–10/30/87	15	−3.9675	10.1823	−3.8900	10.2357
11/ 2/87– 3/31/89	364	0.1004	1.3265	0.1152	1.3033
		South Africa			
1/ 2/87–10/ 9/87	197	0.1636	1.0775	0.3243	2.5348
10/12/87–10/30/87	15	−2.0414	3.6262	−2.5031	5.1156
11/ 2/87– 3/31/89	362	0.0610	1.3823	0.0185	2.0956
		Spain			
1/ 2/87–10/ 9/87	199	0.2143	1.2761	0.2599	1.4270
10/12/87–10/30/87	15	−2.4154	3.2863	−2.1014	3.7311
11/ 2/87– 3/31/89	367	0.055	0.9266	0.0482	1.0377
		Sweden			
1/ 2/87–10/ 9/87	199	0.1272	1.0092	0.1561	1.0296
10/12/87–10/30/87	15	−1.8998	4.5347	−1.7039	4.5496
11/ 2/87– 3/31/89	366	0.1202	1.2421	0.1098	1.2393
		Switzerland			
1/ 2/87–10/ 9/87	199	0.0156	0.9166	0.0494	1.0046
10/12/87–10/30/87	15	−2.0706	5.4091	−1.7146	5.2403
11/ 2/87– 3/31/89	364	0.0025	1.3053	−0.0388	1.2560
		United Kingdom			
1/ 2/87–10/ 9/87	199	0.1852	0.8654	0.2393	1.1134
10/12/87–10/30/87	15	−2.0759	4.9469	−1.7951	5.0693
11/ 2/87– 3/31/89	367	0.0524	0.9621	0.0470	1.0120
		United States			
1/ 2/87–10/ 9/87	199	0.1213	0.9645	0.1213	0.9645
10/12/87–10/30/87	15	−1.4128	7.2527	−1.4128	7.2527
11/ 2/87– 3/31/89	358	0.0428	1.0937	0.0428	1.0937
		Mean Returns Over All Countries (Simple Cross-Country Average and Standard Deviation)			
1/ 2/87–10/ 9/87	201*	0.1606	0.2041	0.1853	0.1528
10/12/87–10/30/87	15*	−2.2363	1.1032	−2.1583	1.2292
11/ 2/87– 3/31/89	367*	0.0659	0.0500	0.0516	0.0588
		Standard Deviations Over All Countries (Simple Cross-Country Average and Standard Deviation)			
1/ 2/87–10/ 9/87	201*	1.0834	0.3523	1.2510	0.4453
10/12/87–10/30/87	15*	5.5555	2.3729	5.7215	2.3811
11/ 2/87– 3/31/89	367*	1.2251	0.4264	1.3031	0.4414

*The maximum number of trading days available for any country.

For each country and calendar period, a T-day holding interval sample variance s_T^2 can be written approximately as:

$$s_T^2 = Ts_1^2 + T(T-1)Rs_1^2, \tag{1}$$

or,

$$(s_T^2/T)s_1^2 = 1 + R(T-1), \tag{2}$$

where s_1^2 is the computed one-day holding interval sample variance and R is a particular *average* of sample autocorrelation coefficients of daily returns (averaged over lags running from 1 to $T-1$).[14] Note that R need not be stationary; it is merely an average autocorrelation during the sample calendar period.

Second, for each country and calendar period, regressions were fit in the following form:

$$(s_T^2/T)s_1^2 = b_0 + b_1(T-1), \ \{T=2,3,5,10,15,20\}. \tag{3}$$

The coefficient b_0 should be unity, and the slope coefficient b_1 should be the average sample serial correlation R.[15]

Table 2 gives the cross-country means and standard deviations of the coefficients b_0 and b_1 and t-statistics (from testing b_0 against 1.0 and b_1 against 0). Also shown are the simple averages across countries for mean returns and standard deviations of returns, by holding interval.[16]

Although the mean pre-crash value of b_1 is small (only .0251), the average t-statistic for testing its deviation from zero is substantial. Of the 23 countries, 18 displayed t-statistics for b_1 greater than zero (11 were greater than 2.776)[17] using local currency returns, and 15 were greater than zero using dollar-denominated returns. This seems to imply the existence of small, positive, statistically significant serial dependence in the returns of many countries during the first nine months of 1987. Note that the intercept b_0 is not significantly different from unity, its theoretical value.

The presence of positive serial dependence is revealed also in the pattern of return standard deviations across holding intervals. Note in table 2, pre-crash period, that the average standard deviation is 1.0834 for one-day holding intervals and that it increases to 1.3439 (per day) for 20-day holding intervals. The same pattern was observed in 19 of the 23 individual countries (results not shown).

The contrast is rather striking with the post-crash period. After the crash and up through March of 1989, the average value of b_1 is very close to zero (average t-statistic = .0797). In the post-crash period, the cross-sectional standard deviation of the t-statistic for b_1 is 1.87 (for local currencies), slightly higher than the null value of 1.414.[18] Out of 23 countries, only three (five) of the individual country local currency (dollar-denominated) t-statistics exceeded in absolute value the .05 critical value 2.776, and one of these was negative.[19]

One difficulty with a serial dependence test of a speculative bubble has been recently discovered by Ross (1987). Suppose that prices actually fluctuate randomly. There are no

Table 2. Multiple-day returns and volatilities over 23 countries.

	Mean	Std. Dev.	Mean	Std. Dev.
	---Local Currency---		------Dollars------	
Pre-crash period, 1/2/87–10/9/87 Regression of Variance Ratio on (Holding Period-1) [Equation (3) of Text]				
Slope:	0.0251	0.0328	0.0160	0.0349
t-statistic (from zero):	2.6410	3.8122	3.0393	7.1361
Intercept:	1.1478	0.2474	1.0982	0.2094
t-statistic (from 1.0):	0.7762	1.6858	0.5582	2.2861
Cross-Country Simple Averages (percent per day)				
Calendar Period / Holding Interval (days)				
1/ 2/87–10/ 9/87 1				
Mean Returns:	0.1606	0.2041	0.1853	0.1528
Standard Deviations:	1.0834	0.3523	1.2510	0.4453
1/ 2/87–10/ 8/87 2				
Mean Returns:	0.1616	0.2019	0.1850	0.1513
Standard Deviations:	1.1454	0.4060	1.2927	0.4729
1/ 2/87 -10/ 9/87 3				
Mean Returns:	0.1587	0.2004	0.1833	0.1503
Standard Deviations:	1.2019	0.4095	1.3461	0.4870
1/ 2/87–10/ 8/87 5				
Mean Returns:	0.1616	0.2019	0.1850	0.1513
Standard Deviations:	1.2015	0.4594	1.3338	0.5044
1/ 2/87–10/ 8/87 10				
Mean Returns:	0.1616	0.2019	0.1850	0.1513
Standard Deviations:	1.2543	0.5770	1.3795	0.6452
1/ 2/87–10/ 1/87 15				
Mean Returns:	0.1604	0.2001	0.1791	0.1512
Standard Deviations:	1.2817	0.5598	1.4072	0.6052
1/ 2/87–10/ 8/87 20				
Mean Returns:	0.1616	0.2019	0.1850	0.1513
Standard Deviations:	1.3439	0.4796	1.4266	0.6065
Post-crash period 11/2/87–3/31/89 Regression of Variance Ratio on (Holding Period-1) [Equation (3) of Text]				
Slope:	0.0024	0.0160	0.0022	0.0180
t-statistic (from zero):	0.0797	1.8719	-0.0650	2.2838
Intercept:	1.1058	0.1590	1.0424	0.1554
t statistic (from 1.0):	0.9334	1.7863	0.0223	2.2396
Cross-Country Simple Averages (percent per day)				
Calendar Period / Holding Interval (days)				
11/ 2/87– 3/31/89 1				
Mean Returns:	0.0659	0.0500	0.0516	0.0588

(Continued)

Table 2. (*Continued*)

	Mean	Std. Dev.	Mean	Std. Dev.
	---Local Currency---		-------Dollars-------	
Standard Deviations:	1.2252	0.4264	1.3030	0.4414
11/ 2/87– 3/31/89 2				
Mean Returns:	0.0650	0.0493	0.0510	0.0578
Standard Deviations:	1.3230	0.4946	1.3475	0.4934
11/ 2/87– 3/30/89 3				
Mean Returns:	0.0641	0.0488	0.0503	0.0574
Standard Deviations:	1.2624	0.4414	1.3227	0.4907
11/ 2/87– 3/31/89 5				
Mean Returns:	0.0649	0.0493	0.0509	0.0578
Standard Deviations:	1.2495	0.4399	1.3247	0.5277
11/ 2/87 3/31/89 10				
Mean Returns:	0.0649	0.0493	0.0509	0.0578
Standard Deviations:	1.2704	0.4646	1.3137	0.5721
11/ 2/87– 3/17/89 15				
Mean Returns:	0.0652	0.0497	0.0531	0.0590
Standard Deviations:	1.3087	0.5204	1.3734	0.6896
11/ 2/87– 3/17/89 20				
Mean Returns:	0.0652	0.0497	0.0531	0.0590
Standard Deviations:	1.2763	0.4974	1.3389	0.6767

true bubbles. Every so often in such a random series, a local peak will occur by chance; interocular examiners have a tendency to focus on the peak as an indication of something unusual in the time series. Conditional on selecting the (random) peak as a sample separation point, Ross shows that the segregated data leading up to the peak will exhibit positive *sample* serial dependence. There is actually no dependence in the true process, and there was actually no bubble prior to the peak, yet choosing such a point ex post has the effect of creating seemingly significant serial dependence in the data preceding the peak.

Given Ross's result, how is one to distinguish a true bubble from a random series that just happened to be selected for study because it was followed by a crash? We are in this very predicament, trying to determine whether a bubble expanded before October 1987. Clearly, attention has been directed to this period because of the crash.

A key to determining the difference between a spurious relation and a true bubble can be developed from Ross's formulae. He shows that the spurious local serial dependence arises from three factors: (1) the height of the peak relative to the starting point in the sample, (2) the volatility of the underlying [random] series, and (3) the time difference between the current data point and the peak. Thus, we can examine the cross-country differences in calculated serial dependence to ascertain whether they are related to these factors. Since all countries crashed at the same time, the third factor can be ignored in a cross-country comparison. As for the first two factors, we can measure the height of the peak by the country's total return from the beginning of 1987 through the week before October 19 and the volatility can be measured by the sample daily standard deviation of returns. Thus, Ross's theory would imply the cross-country model,

$$b_{ij} = a_0 + a_1 r_j + a_2 s_j, \{j=1, \ldots, 23\}, \tag{4}$$

Table 3. Slope from variance ratio regression (3) regressed on mean daily return and volatility during the calendar period [equation (4) of text]

	Mean Return	Standard Deviation	Intercept	Mean Return	Standard Deviation	Intercept
	Local Currency			Dollars		
	Pre-Crash Period 1/2/87 through 10/9/87					
Coefficients:	−0.0384	0.0043	0.0266	−0.0203	0.0015	0.0179
t-statistics:	−0.5662	0.1098	0.7712	−0.2774	0.0598	0.7188
	Post-Crash Period 11/2/87 through 3/31/89					
Coefficients:	−0.0073	−0.0070	0.0114	−0.0724	0.0081	−0.0046
t-statistics:	−0.1002	−0.8241	0.8828	−0.9774	0.8255	−0.2977

where b_{1j} is the coefficient estimate of serial dependence from regression (3) for country j, r_j is the mean daily return from January 2, 1987, through October 12, 1987, for country j, and s_j is the standard deviation of daily returns for country j during the same time period. Coefficient a_1 (a_2) should be positive (negative) and statistically significant.

Table 3 presents the results of fitting (4). Somewhat surprisingly, there is no evidence that the serial dependence detected before the crash was related across countries to the size of the price runup or to volatility during the pre-crash period. Both coefficients have the wrong sign, although neither is significant. There are (at least) three possible explanations consistent with this result: (1) there was not enough of a difference among countries in the extent of the bull market before the Crash;[20] (2) the coincidental timing across countries of both the crash and the pre-crash price increase may render a cross-country test problematic;[21] (3) there really was a speculative bubble that burst in October 1987. Further empirical work is required to sort out the appropriate conclusion.

2. International linkages

Given the improvement in electronic coordination across world markets, most casual observers presume that markets are becoming more related. The free flow of capital to locales with the most favorable risk/return tradeoff is certainly a strong force for the alignment of price innovations. Prior to the crash, however, direct empirical estimates have found cross-country correlations to be statistically significant but surprisingly weak even during periods that were clearly affected by international shocks.[22] Indeed, one of the most unusual aspects of the crash was its simultaneous occurrence in every major market. As I pointed out previously,[23] October 1987 was the only month during the decade of the 1980s when every market moved in the same direction.

A recent empirical paper by Dwyer and Hafer (1988) examined the short-term relations among four big markets (Germany, Japan, the United States, and the United Kingdom). The

authors find no evidence that correlation has increased *except* for the period immediately around the crash. Over a longer term, they find only a slight increase in correlation from the fixed exchange rate period (January 1957 through April 1973) to the flexible rate period (after April 1973). Bennett and Kelleher (1988) document an increase in international comovements in the 1980s compared to the 1970s, but it was of only a small magnitude.

Parhizgari, Dandapani, and Bhattacharya (1988) provide some interesting evidence about possible lead/lag relations among four markets (London, New York, Singapore, and Tokyo). They find evidence of "bi-directional feedback" between New York and Tokyo and New York and London. However, the temporal "causation" between New York and Singapore goes in only one direction, with New York being the causative force. The strength of the relation is also found to be stronger from New York to both London and Tokyo than the reverse feedback *to* New York.[24]

Three of these markets (London, New York, and Tokyo), were also studied by Hamao, Masulis, and Ng (1989), who had data on opening and closing prices for each exchange. This enables them to measure more accurately the "volatility spillover" from one market to another by examining only those cases where the opening occurs in a market *after* the close in another market. Quoting the authors,

> If the impact of a foreign market is completely absorbed in the subsequent foreign market to open trading, then there should be no significant effect from adding the spillover . . . from the foreign market which trades earlier. [Nonetheless] . . . for the full sample period [which includes the crash], all three markets are affected by the volatility surprises of the two previously open foreign markets, with the exception that Tokyo has no significant influence on New York. . . . The spillover . . . from New York is larger than the effect from the other . . . markets. [Excluding the crash] there is no significant spillover onto the London and New York markets, but there is an equally significant spillover effect from London and New York onto the Tokyo stock market. The Japanese market is most influenced by spillovers from foreign markets, but the other two major stock markets are only moderately affected, it at all, by volatility spillovers from foreign stock markets (p. 17).

This evidence is consistent with investors evaluating international news events and taking some time to rebalance their portfolios in response. Note that the item being predicted from one market to another is the *volatility* of returns, not the direction of returns. Indeed, for the direction of returns, Hamao et al. find

> . . . that there is strong evidence that the most recent open-to-close return in one market has a very large positive influence on the opening price in the next market. . . . even the Japanese market return influences the London opening price, which is consistent with informational efficiency across stock markets (p. 20).

Aside from the transmission of international price changes during normal times, several papers have studied the transmission specifically around the October 1987 crash. Bennett and Kelleher, for example, first estimated the usual relations among volatilities across countries[25] and the relation between volatility in one country and approximately contemporaneous correlation of returns between that country and a second country. In the pre-crash period, they found that daily return volatility within a given month *did* tend to be significantly positively related across countries. Also, higher volatility was associated with a larger degree of intercountry correlation.

Then, using the cross-country relations estimated from pre-crash data, Bennett and Kelleher predicted the October 1987 volatility and correlation and compared them with the actual level. For instance, given the observed level of daily volatility during October 1987 in the United States, they predicted the correlation between the United States and Japan, Germany, and the United Kingdom. The predicted value of the correlation was higher than the historical value by a considerable margin; e.g., for Japan, the historical value was .26 while the *predicted* October 1987 value was .97! The actual observed correlation during October was generally lower than predicted (it was actually only .77 with Japan). Nonetheless, the actual correlation was indeed higher for most countries than the historical level.

In contrast, the actual level of volatility during October 1987 was *much* higher than that predicted based on historical estimates of the usual relation between one country's volatility and another's. Although the predicted volatility was roughly twice as high as the historical norm, the actual level was about five times as large. Bennett and Kelleher interpret this evidence as follows:

> These results are consistent with the common view that a wave of panicky selling circled the globe, with traders paying an unusually large amount of attention to price developments in foreign markets in the absence of fundamental news sufficient to account for the disruption [p. 26].

This is a rather bold statement, given the fact that the fitted predictive models were subject to certain econometric problems. To understand these problems let's consider the model used to predict the volatility in one country by the volatility in another country; it was,

$$\ln(s_k) = A + B\ \ln(s_j) + e \tag{5}$$

where s_j is the observed standard deviation of daily returns in country j, and s_k is the standard deviation in country k, j preceeding k by the partial day by which country j trades before country k.[26] The fitted value of B was uniformly less than unity, no matter which way the regression was run; for instance, when j = U.S. and k = Japan, the fitted value of B was .33 (for data in the 1980s). Conversely, when j = Japan and k = U.S., the fitted value was .13.

There appears to be a classic errors-in-variables problem; indeed, the explanatory variable $\ln(s_j)$ is only a *sample estimate* of the population log standard deviation in returns. In a two-variable model, errors-in-the-variable causes attenuation bias. The coefficient B is biased toward zero. Thus, the model *should be expected* to grossly underpredict, particularly when the explanatory variable is several standard errors outside its historical range, as it was in October 1987.

Based on other tests relating the direction of movement to that predicted by other

countries' movements, Bennett and Kelleher conclude that "... the basic degree of linkage among monthly average prices in different stock markets during the crash was neither clearly stronger nor weaker than it had been prior to October" (p. 26), a finding curiously at odds with the interpretation of "panicky selling" with traders paying "an unusually large amount of attention ... to foreign markets."

In the same issue of *The Federal Reserve Bank of New York Quarterly Review*, Aderhold, Cumming, and Harwood (1988) examine the role of cross-border equity investing and stock trading in "centers outside the home market." They conclude that such "direct international linkages cannot explain the worldwide decline in mid-October" (p. 34). According to their data, cross-border selling during the crash was immaterial except in Tokyo (and Tokyo was one of the least affected markets in the world).

Neumark, Tinsley, and Tosini (1988) examine U.S. stocks that are also listed in Tokyo and London. The previous price change in either Tokyo or London is used to predict the New York change. If markets are informationally efficient, the slope coefficient should be unity in the prediction equation; i.e., the price change in Tokyo should be an unbiased predictor of the price change in New York since the previous close, the prediction error reflecting any news between Tokyo's close and the next New York close. Although Neumark et al. find that the estimated coefficients are close to unity (indicating market efficiency), during the crash period, they are significantly less than unity later.

This is consistent with trading costs preventing completely effective arbitrage across countries *except* when volatility is quite large, which it certainly was during the crash period. Their findings for directly arbitragible stocks imply that one might very well observe increased international correlation during every volatile period, without such an increase indicating an augmentation of international linkages. In other words, when markets are highly volatile, transaction costs are less of an impediment to simultaneous price co-movements in response to fundamental factors.

Bertero and Mayer document that return correlations across geographical regions increased dramatically during the period around the crash, and they also interpret the empirical evidence to indicate that the correlations "... remained higher *after* the crash" [p. 12, emphasis in original]. Their table 8 shows that three of six geographical region index pairs displayed *lower* correlation, not higher, in the latest reported period (April–May 1988), relative to the crash period, although the correlations do appear somewhat higher on average than in the pre-crash period. However, as was shown in table 1, volatility also has been somewhat higher since the crash.

Bertero and Mayer also find that the degree of intercorrelation between two markets is related to the trading of overseas securities on the domestic market. They reach this finding by regressing a given country's return on (1) a world market index, and (2) indices from other individual countries that have some stocks traded on the local exchange. An F test of significance indicates, for 13 countries out of 22,[27] that foreign individual markets with cross-listed shares explain a significant portion of the local return *beyond* that explained by a world index (using monthly data over the period January 1981 through May 1988).

Using daily data, Bertero and Mayer purport to document a significant increase in the influence of cross-listed share markets during and after the crash. Before the crash, indices from countries with cross-listed shares are statistically significant regressors for 11 countries out of 22.[28] From October 13, 1987, through December 31, 1987, the number of significant countries rises to 19, where it remains from January 1 through May 31, 1988.

Although there seems to be little doubt that empirical connections across markets have increased since the crash, one would be justified in a bit of skepticism about Bertero and Mayer's proposition that cross-listing is an indicator of connectivity. They showed that *individual* foreign country indices improved on the power of a world index in explaining contemporaneous local returns, where the particular indices chosen were from foreign countries with cross-listed shares. One has to wonder whether *any* collection of randomly chosen foreign regressors might not have done just as well. There might simply be more precise information in individual country indices than in a single world index.[29] A better way to test that cross-listing indicates a strong intercountry link would be to regress each country on *all* the other foreign countries and check whether the coefficients are larger and more significant for countries with cross-listed shares.

One of the most innovative and interesting new papers about international linkages is by King and Wadhwani (1989). They develop a "contagion" model of international volatility transmission. The underlying idea is that rational traders in one country *should* use price movements in another country to deduce changes in underlying economic fundamentals, even in the absence of any public news. This implies that a price "mistake" in one country will be transmitted to others almost as if it were an infectious disease. King and Wadhwani point out that weak evidence for contagion in normal times does not preclude contagion being rampant in more volatile times.

King and Wadhwani are motivated by the puzzling uniformity of the crash across countries. As they rightly state,

> . . . It is difficult to come up with a credible story that links "fundamentals" to the crash; . . . moreover, it is extremely hard to imagine that any [fundamentals based] explanation would be consistent with the uniform decline in equity prices in different countries. . . . In a non fully revealing equilibrium, price changes in one market will . . . in a real sense depend on price changes in other countries through structural contagion coefficients. Mistakes or idiosyncratic changes in one market may be transmitted to other markets, thus increasing volatility. It is this feature that appeals to us as an alternative to "news" as an explanation of the contemporaneous fall in all major stock markets in October, 1987 (pp. 1–2).

An important part of their theory is that contagion increases with volatility. If this be true, a contagion-based model is consistent with the generally low cross-country dependencies exhibited in "normal" times and with a much greater degree of dependence in periods of major disruption. As mentioned earlier, increased correlation around the crash period has been observed, but King and Wadhwani present a more refined empirical analysis of the phenomenon.

They first develop an explicit bi-country contagion model whose coefficients can be estimated using noncontemporaneous data; an example of noncontemporaneous data point would be the close-to-open London price change and the price change on the previous day in New York *after* the London market closed. Using hourly data from New York, London,

and Tokyo from September through November 1987, they fitted the contagion model with cash prices in London and Tokyo and futures prices for the S&P index from the United States.[30] The evidence is quite striking that increased volatility around the crash coincided with increased contagion coefficients. King and Wadhwani conclude, "The pattern of correlations between markets that is revealed by the data seems easier to reconcile with the contagion model than with a fully revealing or purely 'fundamental' model" (p. 24).

Perhaps so, but regardless of the fine quality of their empirical work and the ingenuity of their contagion model, there remain some unanswered questions about the King/Wadhwani interpretation of the crash episode; viz., if the crash was caused by a mistake or perturbation in a given market that went on to infect other markets, where did it originate? If the United States was the original infectee, its price decline on October 14–16 was transmitted to other countries in sequence. Yet this seems inconsistent with the very large declines in October 19 in the Far East (excluding Japan), in continental Europe, and in the United Kingdom, declines that were far larger than the U.S. decline during the previous week and that preceded in time the big U.S. decline on its October 19. Indeed, in deciphering the global sequencing of declines in the context of the contagion model, one would be obliged to conclude that the crash was caused by investor "mistakes" in Hong Kong, Malaysia, and Singapore! This seems a bit far-fetched.

A second question involves whether contagion as measured by King and Wadhwani is really inconsistent with fundamental news. Imagine, for instance, that many different and important news items happened to arrive at random intervals during the crash period, some arriving when London was open and New York and Tokyo were closed, and vice versa in all combinations. Then, to the extent that these news items had importance for all markets, a price change in, say, New York would indeed be highly related to the next opening price change from the previous close in Tokyo, and so on. And, to the extent that there were more important news events around the crash than in a quieter period, the King/Wadhwani contagion coefficients would be larger. The coefficients are clearly a positive function of the number of news items arriving per unit of calendar time and of the international significance of the news relative to the background news idiosyncratically concentrated in each local country.

Unfortunately, such a conjecture may not help very much, for, as King and Wadhwani argue, if such news items really had been forthcoming during the crash period, one might have thought they would stand out[31] as possible candidates for causing the crash.

Searching for possible triggering mistakes, King and Wadhwani examine a practice that is often indicted by the popular press, computer-driven portfolio insurance in the United States. During periods of high volatility, they argue that portfolio insurance might induce negative serial dependence in very short-term price movements; thus, during the crash period, there should have been more detectable negative dependence in the United States, with its widespread use of portfolio insurance, than in the United Kingdom where formal portfolio insurance schemes were not common. Both U.S. and U.K. cash prices and U.K. futures prices showed more negative dependence during the crash, but U.S. futures prices did not (see their table 6). Their conclusion: " . . . time-honoured practices such as stop-loss orders had as significant an effect on share prices as formal dynamic hedging strategies" (p. 25); i.e., whatever the source of the "mistake," if any, it wasn't U.S. portfolio insurance.

This conclusion is buttressed with results by Goodhart (1988), who constructs a number of different tests exploiting both foreign exchange and stock returns. His overall conclusion: "... once the Crash week itself is past, ... the main increase in the strength of linkage [among international markets] appear to have been from the rest of the world to asset price changes in New York" (p. 22).

3. Market regulations and volatility

The third important question arising around the crash episode concerns the influence, if any, of various regulations and institutional rules on price volatility. Three that are prominently mentioned are: (1) some form of circuit breaker, (2) margin requirements, and (3) transaction taxes.[32] To be sure, other possible regulations are sometimes mentioned, such as short-sale rules, restrictions on the DOT system, enhanced capital requirements for market makers, and so on,[33] but margin requirements, circuit breakers, and trading taxes are discussed most often, they are actually in place in many markets and countries, and they undoubtedly are the most likely to be implemented.

3.1. *Empirical literature on margin requirements*

Margin requirements are one of the most intensely studied market regulations. In the United States, initial margin requirements for equity purchases are set under law by the Federal Reserve using regulations G, T, U, and X. The regulations differ depending on whether the lender is a broker/dealer, a bank, or another type of lender. They apply only to the *initial* margin, (at the time of original purchase), and do not apply to all stocks. Maintenance margins on stocks (after original purchase) are set by the exchanges and the National Association of Securities Dealers with the approval of the Securities Exchange Commission (SEC). Stock index futures, stock options, stock index options, and stock index futures options are set by various authorities, mostly the exchanges and clearing houses (with the approval of regulatory agencies). A good description of the various margins is provided by Sofianos (1988).

Many of the earlier studies of margin requirements failed to find a significant impact on anything. A classic paper of this genre is by Largay and West (1973). Using data from January 1933 through January 1969 on the S&P 500 index, Largay and West fail to uncover an impact of changes in margin requirements on stock price levels. They do, however, uncover striking evidence of what induces the Fed to change margins; decreases in margin tend to occur after price declines and increases in margins follow price rises, a tendency confirmed and discussed by Garbade (1982). Note that this typical Federal Reserve behavior before 1970 seems to conform to what many persons thought (with hindsight) might have been a good policy in the pre-crash period of 1987.

Officer (1973) also found evidence that the Fed responded to changes in the market, concluding that margins were increased *after* stock volatility had fallen. In connection with a comprehensive study of stock return volatility over more than a century, Schwert (1988)

put together the Largay and West and Officer conclusions into a coherent explanatory package. Noting that there is a general tendency for increases in stock price levels to be associated with decreases in volatility, and vice versa, Schwert pointed out that a tendency for the Fed to increase (decrease) margin requirements after a price increase (decrease) would induce an inverse correlation but *not a causation* between return volatility and margin requirements.

Schwert also conducted empirical tests that confirmed that margin requirements were increased "*after* prices have risen and volatility is relatively low ... [but that] There is no evidence that stock return behavior is different from normal in the 12 months following a change in margin requirements" (p. 28, emphasis in original).

Grube, Joy, and Panton (1979) examine almost the same set of data as Largay and West (there are four additional changes in margin requirements), and they also investigate abnormal volume around margin changes. The pattern of prices during days before and after margin changes is very similar to that found by Largay and West. However, Grube, Joy, and Panton place more emphasis on the price increase observed in just the few days around a margin decrease. The average pattern prior to a margin decrease is generally downward, but there is a sharp reversal just prior to the margin decrease announcement, which Grube, Joy, and Panton interpret to imply that margin decreases are good news (and that there is either anticipation or leakage about the decrease a few days in advance). After the margin decrease announcement date, there is no perceptible change in either prices or volume. No abnormal price movements were found around margin increases. However, there is evidence of abnormal volume both before and after an increase.

The exchanges are free to impose margin requirements in excess of those required by the Federal Reserve and on some occasions in the past, the exchanges *have* chosen higher margins for given individual stocks. The impact of exchange-imposed 100 percent margin requirements was studied by Largay (1973), who found a significant price impact:

> ... the restricted stocks all rose in price prior to 100% margins being imposed. On the imposition date, the restricted stocks declined in price and over 70% of the individual price relatives were less than one ... [A control sample of] non margin stocks also rose in price but did not decline on what would have been the imposition date if [100%] margins had been imposed (p. 982).

Largay also found an effect on volume. "The imposition of the margins dampens trading volume while their removal is associated with a revival of somewhat higher levels of trading activity" (p. 984). However, the Largay evidence on volume is not all that clear-cut.[34] In his figures 7 and 8 (p. 985), which show plots of average volume around 100 percent margin imposition, it appears that post-imposition volume is roughly on a level with previous volume *except* for a few days just preceding the imposition. But this could imply that the exchange imposes 100 percent margin *in response* to a sudden and inexplicable flurry of trading, not that margin has a causative influence.

Of course, volume is merely a proxy for price volatility and it may not be a very good one. Largay does report that price volatility decreased after the imposition of 100 percent margins, but the empirical results are not given.[35]

The direct effectiveness of the Fed's margin requirements on reducing borrowing (in broker margin accounts) was presented by Luckett (1982). Luckett does find evidence that increased margin requirements reduced borrowing in the 1966–1979 period; his empirical estimates indicate "... that a 10 percent change in the Federal Reserve's margin requirement will change investor equities on stocks held in margin accounts at security dealers by about 1 1/2 to 2 percent" (p. 794). Of course, this does not necessarily imply that it has a significant impact on volatility, but Luckett argues that reducing volatility is not the appropriate goal of margin regulations anyway. In an interesting passage that remains germane for present purposes, Luckett states,

> The Federal Reserve is not (nor should it be) in the business of influencing volume of credit, volume of trading, or stock prices *per se*. ... none of [these] taken by itself, is inherent cause for official concern. Rather, the power over the margin requirement given the Federal Reserve in 1934 was meant to prevent a repetition of the disastrous events that culminated in the Crash of '29 --specifically, the pyramiding of margin credit in a rising market and margin calls in a falling market.... [Even] pyramiding *per se* is not troublesome; its significance lies, rather, in the derivative fact that it makes the market *vulnerable* to margin calls" (p. 787, emphasis in original).

Yet the crash of 1987 is unlikely to have been prevented by the Fed's having imposed higher initial margin requirements (than the current 50 percent) during the pre-crash expansion. Most of the selling in the cash market during the crash episode was by institutions, who rarely use margin, and the actual percentage of stock held on margin was very small.

Negative conclusions about the actual or likely efficacity of margin requirements can be found in a number of other papers, including the Federal Reserve's own "Evaluation" (1984). Hartzmark (1986) goes even further, arguing that margin requirements can "backfire," driving sophisticated investors from the market and leaving unsophisticated traders who might cause even more "aberrant price movements" (p. S148).[36] Grossman (1988) makes a similar point in discussing portfolio insurance:

> These [margin] requirements make it more difficult ... to take the opposite side of portfolio insurance trades. These requirements grew out of an effort to curb speculation, but it is exactly speculation by traders that can provide the other side of portfolio insurance trades and serve to lessen volatility (p. 8).

Because of this uniformly negative literature about margin requirements, recent papers by Hardouvelis (1988b, 1988c) came as quite a surprise. Hardouvelis claims to have found a surprisingly large and significant effect,

> ... over the entire sample, [1934–1987], an increase in the margin requirement by 10 percentage points from, say 50 percent to 60 percent decreases the monthly volatility of large stocks by 1.10 percentage points. The effect ... on small stocks is even greater (1.91 percentage points). To put these numbers in perspective, observe that the average monthly

> volatility of large stocks ... is 4.8 percentage points and of small stocks, 7.4 percentage points (1988b, p. 85).

According to this, if margin requirements were raised to 100 percent from their current 50 percent level, large stock volatility would decline to zero!

The second paper by Hardouvelis (1988c) is more complete than the one quoted above (1988b) and contains fancier econometrics. The conclusions, too, are somewhat different. For instance, the 10 percentage point increase in margin now only causes a reduction in small stock volatility of .77 percentage points, but the effect is still highly significant. From a rather complex set of tests, Hardouvelis concludes that margin requirements also reduce "excess" volatility (defined as volatility "that cannot be explained by the variation of current and future dividends and discount rates" (p. 11); see also 1988b, p. 88).

Finally, Hardouvelis studies long-term swings in stock prices, presumably occasioned by long-term changes in expected returns. He finds no effect of margin requirements. Everything together, he concludes, "... is consistent with the hypothesis that an increase in margin requirements mitigates the presence of fads, while a decrease in margin requirements exacerbates the presence of fads and excessive speculation (p. 19); i.e., long-term trends are not affected very much by higher margin requirements, but speculatively driven short-term swings are reduced.

Hardouvelis's results are mighty suspicious on their face, not only because he finds a large and significant margin effect where no one else ever did but also because his results are difficult to understand owing to their econometric complexity. There have been only 23 changes in margin requirements since the Federal Reserve system received its margin authority in 1934, yet Hardouvelis uses *monthly* data 14 years of which are included since 1974, the last time margin requirements were changed! He also controls for the influence of nuisance variables, whose impact might mask the influence of margin requirements, variables such as lagged changes in industrial production and in stock prices. He measures volatility by a moving standard deviation of returns over the past year, updated monthly. Thus, each successive estimate of volatility shares 11/12ths of the underlying monthly returns data with its neighbors. It would take an econometric wizard to figure out what is really going on.

Fortunately, two wizards have stepped forward. Hsieh and Miller (1989) have replicated Hardouvelis's results and found them spurious. First, Hsieh and Miller conduct a detailed study of their own using the 23 historical margin regulation changes and estimated volatilities before and after each change. They calibrate their test statistic with a "bootstrap," or simulation procedure, so that the measured level of statistical significance is reliable. Their conclusion:

> We find only three occasions in which the modified Levene statistic lie in the upper 5% tail of the bootstrap distribution. In one case, the volatility increased when margins declined. In the other two cases, the volatility declined when margins declined. This absence of strong and consistent impact effects of margin changes is particularly relevant for policy discussions. Margin requirements are not [at] all like the beryllium rods used to control nuclear reactors (p. 7).

Hsieh and Miller also present a "long-term" investigation that comes to the same conclusion.

Give these result, Hsieh and Miller then set out to solve a detective problem: viz., how did Hardouvelis get such different results with the same data? After an extensive examination of Hardouvelis's methods, Hsieh and Miller conclude that the basic problem is autocorrelation in the moving standard deviation that is not adequately expunged by the anti-autocorrelation method employed by Hardouvelis. Using a simulation, they show that the coefficient in a simplified version of Hardouvelis' model "... converges to a non-degenerate distribution, so that *any test of the hypothesis of no correlation between* [volatility and margin] *will be rejected with probability 1* (as the sample size goes to infinity)" (p. 15, emphasis in original), even if there is no true relation between the two variables.

Hardouvelis recognizes that his overlapping observations are subject to autocorrelation problems, and he employs the well-accepted Newey/West method of correction. However, again using simulation, Hsieh and Miller show that Hardouvelis's implementation of Newey/West employs an insufficient number of lags to remove the danger of spuriously finding "significance." As a final tribute to Occam, they run the Hardouvelis model in first difference form and find "... that margins and volatility are positively rather than negatively correlated, although the coefficients are not reliably different from zero" (p. 16)!

Hardouvelis's results were found wanting also by Schwert (1988) (cited already above) and by Salinger (1989).[37] Salinger presents new evidence that there is a connection between volatility and the level of margin *debt*, but that margin requirements appear to have no direct effect on volatility. Both debt and requirements meet head-to-head in Salinger's regressions, and debt is the winner during most periods. Of course, a proponent of margin requirements would probably assert that increased margin requirements reduce margin debt so that such requirements actually are effective in reducing volatility. Furthermore, Salinger finds that inclusion of the pre-1934 period (before the Fed had margin authority), reverses the relative strength of margin debt and margin requirements: if the regressions include data from January 1927 through December 1987, margin requirements have a significant negative impact on volatility above and beyond the influence of margin debt.[38]

It seems to me, however, that Salinger's results are subject to some of the same criticisms that Hsieh and Miller and Schwert aimed at Hardouvelis. There is a danger of spurious regression. Also, there seems every reason to think a priori that Salinger's measure of margin debt, dollar amount of debt *divided by NYSE market value*, would decrease (increase) *after* a rise (fall) in stock prices. Since a rise (fall) in stock prices is related to a (fall) rise in volatility. Schwert's argument about Hardouvelis's results applies with equal force to Salinger's results. Both margin requirements *and* margin debt display a lagging and spurious relation to volatility changes. They are not causes.

3.2. *Empirical evidence on price limits and volatility*

In futures markets, price limits are often employed, but they do not appear to be directed toward dampening volatility; instead, limits seem to be useful for ensuring contract

compliance. A theory of price limits developed by Brennan (1986) focuses on their effectiveness in preventing futures traders from reneging on contracts. Brennan's theory predicts that limits will disappear in futures markets that have closely correlated cash markets, a prediction more or less satisfied by the existing markets in the United States. This is suggestive evidence against the proposition that limits are used to reduce volatility.

There is a relative paucity of empirical literature on circuit breakers. Price limits in commodity markets have sometimes been studied in connection with other phenomena, and they have displayed perceptible influences on short-term price behavior.[39] If a market has limits on price movements that are occasionally encountered, *measured* volatility over a short enough interval is bound to be affected. During the October 1987 crash, daily volatility in Japan could have been reduced to the extent that some stocks hit their daily limit. Certainly in Hong Kong, which closed during the day of the crash, measured daily volatility was lower than it would have been without the closure.

Of course, most investors would see little difference between a market that went down 20 percent in one day and a market that hit a 5 percent down limit four days in a row. Indeed, the former might very well be preferable. The measurement problem with price limits and circuit breakers is to detect their *long*-run impact on volatility, if any. A reduction in short-term volatility could be spurious and immaterial.

An illustration of this pitfall is given by Bertero and Mayer (1988), who find a mitigating influence of "circuit breakers" on the cross-country extent of the crash. They say,

> The results reported here are similar to those in Roll (1988) in finding no relation [between the size of the Crash and futures trading or portfolio insurance] but it [sic] differs in finding some influence from circuit breakers and capital controls on domestic residents. This may be because Roll's results relate to the *month* of October, not the days immediately surrounding the crash. Repeating our regression for the month of October, we too find that most variables become significant ... (p. 11).

In their introduction, however, they state that their study differs from mine partly because they used daily data. "This allows the period of the crash to be identified more precisely and ... suggests a different interpretation of the role of some of the structural characteristics of markets."

Actually, I used and reported daily data too, but in evaluating the impact of various market arrangements, returns were calculated over a longer period around the crash *explicitly to assure the inclusion of all crash-related returns.* It seems sensible that any investigation of factors relating to the crash should include *all* related price movements. Again using the example of Hong Kong, its observed price decline October 19 grossly understates the true impact of the crash.

Several past studies, though not directly about price limits, may bear on their potential effectiveness. Hopewell and Schwartz (1978) examined trading suspensions on individual New York listed stocks and concluded that post-suspension price movements offered no profit opportunities and that the behavior of prices around the suspension was consistent with significant news. They could not ascertain whether the suspension per se had any influence on volatility.

Kryzanowski (1979) examined trading halts effected by Canadian exchange officials who were suspicious that corporate information had been withheld from the market. On average over all such halts, stock prices declined more than 26 percent from the last transaction price prior to the suspension. Some of this drop occurred between the last transaction and the first trade after resumption, but the greatest part of the drop, more than 24 percent, occurred even later. Although we cannot know whether prices might have declined even more had there *not* been a cooling-off period, such a period evidently does not preclude an impressive price change.

A recent study by Ma, Rao, and Sears (1989) of price limits in futures markets contains some striking and surprising results. Using minute-by-minute data, they seem to find that limit moves are *not* followed by an increased probability of price moves in the same direction during subsequent periods,[40] this result is hard to believe since any limit move induced by significant news (and there surely must be some occasional important news) almost certainly alters the subsequent conditional expected return in the same direction as the limit move.

Ma et al. also produce evidence that limit moves are followed by reduced volatility and by normal levels of volume but the results may have been affected by novel methods. Even if their results were impeccable, however, they would not constitute unambiguous evidence that price limits reduce volatility. Reduced volatility after a limit move is equally consistent with a reduction in the amount of news received relative to the pre-limit move period and the limit move period. We really need information about whether the imposition of a limit move *system* reduces *overall* volatility in (all periods.)

3.3. *Transaction taxes*

Transaction taxes are the least studied of the three most serious proposals for dampening volatility. In fact, two recent papers by academics urging such a tax, Stiglitz (1989) and Summers and Summers (undated) cite *no* empirical studies bearing directly on this question. However, both Stiglitz and the Summers cite empirical work which allegedly finds that stock prices are excessively volatile, too volatile to be explained by "fundamental" determinants of value.

Accepting this alleged excess volatility as fact, Stiglitz develops a theory based on "noise" traders who believe (irrationally) that trading systems, horoscopes, etc., are beneficial in forecasting prices. (Arbitrageurs are unable to completely remove the noise induced by such traders.) In a taxonomy of traders, Stiglitz portrays the noise-causing group as " . . . dentists and doctors in the midwest and the retired individuals in the sunbelt . . . " (p. 7) who are essentially using the stock market to amuse themselves, as they would in a casino. Why do such traders not eventually lose all their money and disappear? Because, says Stiglitz, quoting P.T. Barnum, " . . . a fool is born every moment. For every fool that is weeded out, a new one enters the market" (p. 9).[41]

Other traders such as those who possess no information and even those who do possess valuable information trade less frequently than noise traders and would therefore be less affected by a transaction tax. Indeed, such a tax would be a smaller fraction of total return

the longer the holding period, and this would supposedly induce investors to take a more long-term view. Finally, according to Stiglitz, a transaction tax might actually increase liquidity since it would reduce the influence of noise traders and lower volatility. Not only can the government raise revenue but it can promote the efficacity of capital markets in the process! (This conclusion is based entirely on theory.)

Summers and Summers argue that there is too much trading, too much volatility to be explained by fundamentals, and too many "resources devoted to financial engineering." Furthermore, many other countries have transaction taxes in place, so why should the United States be different? The authors complain that "talented human capital is devoted to trading paper assets rather than actually creating wealth. . . . one fourth of the Yale senior class [applied] for a job at First Boston" (p. 13).

Summers and Summers seem to regard securities transactions and the entire securities industry as a pernicious activity that should be taxed heavily along with other vices such as gambling, alcohol, tobacco, and the teaching of economics. No evidence is offered that a tax would actually be beneficial to investment and saving, but that seems secondary to raising revenue anyway.

3.4. *Conclusions about the existing empirical literature on market regulations and volatility*

My overall interpretation is that the empirical literature has failed to uncover any solid evidence that margin requirements are effective in reducing price volatility, at least for U.S. data, nor has it found uncontrovertible evidence that price limits are effective in reducing true (as opposed to measured) volatility. There have been demonstrable effects of margin requirements on the equity levels in margin accounts (Luckett and Hsieh and Miller), and possibly on trading volume (Largay), but volatility itself has thus far escaped unscathed.[42] Perhaps this is not too surprising given how easily an investor can evade the initial Federal Reserve margin requirements (by borrowing from someone other than a broker/dealer or allegedly for some purpose other than equity investing).[43]

However, I retain some sympathy for the view that any effect might be awfully difficult to detect, given the enormous background noise in equity markets. A cross-country comparison could possibly help overcome a locally weak signal-to-noise ratio, and we now turn to the international data to find out if it will. In the process of examining the international data, some new evidence will be presented concerning the influence on volatility of price limits and transaction taxes.

3.5. *The cross-country influence of margin requirements, price limits, and transaction taxes on market volatility*

Official margin requirements for cash equity positions and official price limits on cash equity transactions[44] are given by country in table 4 for the pre-crash and post-crash periods. (They are mostly unchanged in the two periods.) Taxes on round-trip transactions are also shown.

Table 4. Official price limits, margin requirements, and transaction taxes

	Price Limits	Margin Requirements	Price Limits	Margin Requirements	Roundtrip Transaction Tax
	---------Before Crash-------------		---------After Crash--------------		
	Percent				
Australia	none	0.	none	0.	.6
Austria	5.	100.	5.	100.	.3
Belgium	10.	100.	10.	100.	.375
Canada	none	50.	none	50.	0
Denmark	none	0.	none	0.	1.0
France	7.	20.	7.	20.	.3
Germany	none	0.	none	0.	.5
Hong Kong	none	0.	none	0.	.6
Ireland	none	100.	none	100.	1.0
Italy	15.	100.	15.	100.	.3
Japan	10.	70.	10.	50.	.55
Malaysia	none	0.	none	0.	.03
Mexico	35.	0.	35.	0.	0
Netherlands	24.	0.	24.	0.	1.2*
New Zealand	none	0.	none	0.	0
Norway	none	100.	none	100.	1.0
Singapore	none	71.	none	71.	.5
South Africa	none	100.	none	100.	1.5
Spain	10.	40.	20.	50.	.11
Sweden	none	40.	none	40.	2.0*
Switzerland	5.	0.	12.5	0.	.9
United Kingdom	none	0.	none	0.	.5
United States	none	50.	none	50.	0

Notes about Transaction Taxes:
Belgium: tax on forward contracts is .195 percent.
France: .15 percent above FF 1 million.
Japan: .18 percent on dealers
Netherlands: tax on nondealers only.
Sweden: additional .5 percent if transaction is through a market maker.
Switzerland: different tax rate applies to foreigners.
*Does not agree with tax rate in Summers and Summers, 1989.

In many countries, there is a different margin requirement for futures as opposed to cash transactions. Given the variety of margin rules, a single number may be an inadequate portrayal of reality, yet it *would* be a policy variable that could conceivably be altered. As mentioned in the table's notes, a similar situation exists in some countries with respect to transaction taxes, different rates being applied to different instruments and/or traders. Thus, if we find an influence of margin requirements, price limits, or taxes in the tests below, we could legitimately interpret this as indicating the potential impact of a particular policy change, *but only while holding constant the plethora of other rules and regulations.*

The first results are shown in table 5, which contains a series of cross-country OLS regressions of the following form:

Table 5. Standard deviation of returns regressed on price limits, margin requirements, and trading taxes

Holding Period (Days)		Local Currency Pr Limits	Local Currency Margin	Local Currency Taxes	Dollar Pr Limits	Dollar Margin	Dollar Taxes
Estimated values of price limits, margin, and taxes used as regressor [equation (6) of text]							
Pre-Crash Period 1/2/87 through 10/9/87							
1	Coefficients:	−0.0088	−0.1049	−0.1127	−0.0117	0.1092	0.0264
	t-statistics:	−0.7225	−0.5598	−0.7417	−0.7476	0.4519	0.1347
2	Coefficients:	−0.0041	−0.1114	−0.2154	−0.0092	0.0784	−0.1095
	t-statistics:	−0.2990	−0.5233	−1.2479	−0.5521	0.3035	−0.5230
3	Coefficients:	−0.0066	−0.0840	−0.1949	−0.0134	0.1097	−0.0761
	t-statistics:	−0.4719	−0.3871	−1.1082	−0.7812	0.4145	−0.3547
5	Coefficients:	−0.0098	−0.0915	−0.2598	−0.0143	0.0241	−0.1883
	t-statistics:	−0.6314	−0.3823	−1.3392	−0.8141	0.0890	−0.8585
10	Coefficients:	−0.0085	−0.0508	−0.3165	−0.0131	0.0681	−0.1756
	t-statistics:	−0.4288	−0.1667	−1.2818	−0.5776	0.1937	−0.6164
15	Coefficients:	−0.0009	−0.0011	−0.3538	−0.0045	0.0300	−0.2236
	t-statistics:	−0.0452	−0.0038	−1.4924	−0.2121	0.1003	−0.8387
20	Coefficients:	−0.0074	−0.1089	−0.2126	−0.0210	0.0725	−0.0791
	t-statistics:	−0.4489	−0.4269	−1.0282	−0.9940	0.2215	−0.2981
Post-Crash Period 11/2/87 through 3/31/89							
1	Coefficients:	−0.0205	−0.0852	−0.0067	−0.0208	0.0125	0.0204
	t-statistics:	−1.1365	−0.3638	−0.0360	−1.1023	0.0511	0.1051
2	Coefficients:	−0.0210	−0.0572	−0.0198	−0.0210	−0.0110	−0.0495
	t-statistics:	−0.9908	−0.2074	−0.0911	−0.9902	−0.0398	−0.2266
3	Coefficients:	−0.0243	−0.0004	−0.0306	−0.0200	−0.0073	−0.1046
	t-statistics:	−1.3020	−0.0017	−0.1597	−0.9440	−0.0264	−0.4819
5	Coefficients:	−0.0222	0.0476	−0.0821	−0.0188	−0.0031	−0.1735
	t-statistics:	−1.1823	0.1950	−0.4255	−0.8277	−0.0106	−0.7437
10	Coefficients:	−0.0200	0.0307	−0.1059	−0.0217	0.0035	−0.2207
	t-statistics:	−1.0024	0.1182	−0.5155	−0.8882	0.0111	−0.8781
15	Coefficients:	−0.0216	0.0952	−0.1393	−0.0115	−0.0107	−0.3247
	t-statistics:	−0.9634	0.3265	−0.6046	−0.3878	−0.0279	−1.0679
20	Coefficients:	−0.0098	−0.0683	−0.0641	−0.0078	−0.1765	−0.2286
	t-statistics:	−0.4475	−0.2409	−0.2864	−0.2665	−0.4655	−0.7626
Dummy Variables for Price Limits, Margin, and Taxes used as Regressors [Equation (7) of Text]							
Pre-Crash Period 1/2/87 through 10/9/87							
1	Coefficients:	0.1868	−0.1673	−0.2969	0.0963	−0.0437	−0.2014
	t-statistics:	1.2620	−1.1565	−1.5769	0.4697	−0.2178	−0.7719
2	Coefficients:	0.2961	−0.1658	−0.3958	0.1970	−0.0592	−0.3200
	t-statistics:	1.8128	−1.0385	−1.9043	0.9342	−0.2874	−1.1929
3	Coefficients:	0.2410	−0.1317	−0.3778	0.1303	−0.0119	−0.3035
	t-statistics:	1.4042	−0.7853	−1.7300	0.5900	−0.0551	−1.0796
5	Coefficients:	0.2434	−0.1334	−0.4553	0.1752	−0.0676	−0.4218
	t-statistics:	1.2660	−0.7100	−1.8611	0.7896	−0.3117	−1.4945
10	Coefficients:	0.3986	−0.1052	−0.6880	0.3065	0.0017	−0.6099
	t-statistics:	1.7494	−0.4724	−2.3729	1.1083	0.0064	−1.7330
15	Coefficients:	0.4534	−0.0731	−0.6663	0.3687	−0.0218	−0.6261
	t-statistics:	2.1002	−0.3466	−2.4254	1.4687	−0.0889	−1.9600

Table 5. (*Continued*)

Holding Period (Days)		Local Currency Pr Limits	Local Currency Margin	Local Currency Taxes	Dollar Pr Limits	Dollar Margin	Dollar Taxes
20	Coefficients:	0.3108	-0.1228	-0.6282	0.1098	0.0571	-0.5252
	t-statistics:	1.6855	-0.6811	-2.6770	0.4095	0.2179	-1.5396
		Post-Crash Period 11/2/87 through 3/31/89					
1	Coefficients:	0.0917	-0.2031	-0.3280	0.0522	-0.1537	-0.3329
	t-statistics:	0.4965	-1.1252	-1.3961	0.2679	-0.8074	-1.3439
2	Coefficients:	0.1169	-0.2185	-0.3343	0.0752	-0.1948	-0.3664
	t-statistics:	0.5374	-1.0271	-1.2074	0.3470	-0.9188	-1.3281
3	Coefficients:	0.0710	-0.1729	-0.3121	0.1446	-0.1968	-0.4402
	t-statistics:	0.3644	-0.9075	-1.2584	0.6880	-0.9579	-1.6463
5	Coefficients:	0.1029	-0.1400	-0.3361	0.2010	-0.2063	-0.5067
	t-statistics:	0.5300	-0.7376	-1.3608	0.9018	-0.9469	-1.7868
10	Coefficients:	0.0966	-0.1853	-0.2857	0.1856	-0.2380	-0.4888
	t-statistics:	0.4667	-0.9160	-1.0850	0.7551	-0.9907	-1.5630
15	Coefficients:	0.1444	-0.1640	-0.4046	0.4022	-0.2782	-0.6824
	t-statistics:	0.6305	-0.7330	-1.3889	1.4186	-1.0037	-1.8914
20	Coefficients:	0.2358	-0.2392	-0.3358	0.4078	-0.3508	-0.6043
	t-statistics:	1.0989	-1.1399	-1.2296	1.4682	-1.2921	-1.7099
		Dummy Variables for Price Limits, Margin, and Taxes used as Regressors [Equation (7) of Text] With Mexico Deleted from Sample					
		Pre-Crash Period 1/2/87 through 10/9/87					
1	Coefficients:	0.1010	-0.1806	0.2139	0.2399	-0.0166	0.2606
	t-statistics:	0.6041	-1.1426	0.8987	1.1463	-0.0840	0.8747
2	Coefficients:	0.1681	-0.1737	0.2240	0.2819	-0.0001	0.2253
	t-statistics:	0.8722	-0.9533	0.8165	1.2679	-0.0007	0.7119
3	Coefficients:	0.1386	-0.1559	0.3015	0.2677	0.0174	0.3074
	t-statistics:	0.7183	-0.8542	1.0975	1.1798	0.0810	0.9518
5	Coefficients:	0.1200	-0.1706	0.2916	0.2228	-0.0349	0.3094
	t-statistics:	0.5414	-0.8143	0.9246	0.9250	-0.1535	0.9025
10	Coefficients:	0.2130	-0.1501	0.2488	0.3327	0.0001	0.2357
	t-statistics:	0.7518	-0.5602	0.6167	1.0624	0.0003	0.5288
15	Coefficients:	0.2798	-0.0287	0.2442	0.3558	0.0533	0.2485
	t-statistics:	1.0289	-0.1115	0.6307	1.2329	0.1952	0.6049
20	Coefficients:	0.2983	-0.1871	0.0618	0.4093	0.0211	0.0224
	t-statistics:	1.2945	-0.8589	0.1885	1.3953	0.0760	0.0536

$$s_{jT} = c_0 + c_L L_j + c_M M_j + c_{TX} TX_j \quad \{j=1, \ldots, 23\} \tag{6}$$

where s_{jT} is the computed sample standard deviation of returns for country j using T-day nonoverlapping holding intervals, L_j is an inverse measure of country j's price limits,[45] M_j is the level of margin requirements in country j, TX_j is country j's tax rate on transactions (in %/100), and c_0, c_L, c_M, and c_{TX} are regression coefficients.

All of the tests are repeated using zero/one dummy variables for countries that that do not/do have price limits, official margin requirements (whatever the size), and

transaction taxes. These regressions, shown in the second part of each table, have the following form:

$$s_{jT} = h_0 + h_L D_{jL} + h_M D_{jM} + h_{TX} D_{jTX}, \quad \{j=1, \ldots, 23\} \tag{7}$$

where D_{jK} is one (zero) if country j has (does not have) feature K, $\{K=L, M,$ and TX for price limits, margin requirements, and transaction taxes, respectively.$\}$

For the pre-crash and post-crash periods, table 5 contains absolutely no evidence that margin requirements have an influence on volatility, regardless of whether returns are measured in local currency or in dollars and regardless of the length of the holding interval. In local currency, c_M from regression (6) is negative for all holding intervals during the pre-crash period while it is positive for three of seven of the holding intervals in the post-crash period (the holding periods are obviously not independent). In a common currency (dollars), the margin requirements coefficient is uniformly positive in the pre-crash period and mainly negative in the post-crash period. The dummy variable margin coefficient, h_M, is similarly insignificant, though it is mostly negative.

The results for price limits are also insignificant, though their sign pattern makes at least some sense. Price limits *should* have a larger impact on *measured* volatility, the shorter the holding interval. Using the L_j measure of limits, we do observe slightly smaller (absolute) values of c_L the longer the holding interval, particularly in the post-crash period, although there is undoubtedly no statistical significance in the pattern. The dummy variable coefficient, h_L, is mostly positive.[46]

Transaction taxes display negative coefficients as they should if taxes reduced volatility, and there are even a few significant t-statistics, for the dummy variable regressions using local currency in the pre-crash period. However, these results are somewhat questionable because of a possible outlier. Notice in table 4 that only four countries out of 23 have a zero transaction tax and thus a zero valued dummy variable D_{jTX}; these countries are Canada, Mexico, New Zealand, and the United States. The fact that the coefficient is significant only in the pre-crash period, only for local currency, and only for the dummy variable version of the regression makes one suspect that Mexico is the culprit, the reason being that Mexico had an extremely large and volatile *local* currency return in the pre-crash period.

To check for a possible overweaning influence of Mexico, these regressions were repeated with Mexico excluded, and the results are shown in the final panel of table 5. The tax effect is no longer significant, and the coefficient has changed signs. This seems to be evidence against taxes really having any material impact on volatility during relatively normal periods.

There remains a possibility that margin requirements, price limits, or taxes might have an influence on volatility during particularly disruptive periods. To check this out, regressions (6) and (7) were repeated for all possible nonoverlapping holding intervals during the 1987 crash period, the 15 trading days between October 12 and October 30 inclusive.[47] Table 6 gives the results.

Margin requirements still display no statistically significant effect, though both c_M and h_M, the fitted coefficients using numerical values and dummy variables, respectively, are

Table 6. Standard deviation of returns regressed on price limits, margin requirements, and trading taxes

Holding Period (Days)		Local Currency			Dollars		
		Pr Limits	Margin	Taxes	Pr Limits	Margin	Taxes
Estimated values of price limits, margin, and taxes used as regressors [equation (6) of text] Crash Period 10/12/87 through 10/30/87							
1	Coefficients:	−0.1444	−0.8544	−0.5257	−0.1658	−0.7156	−0.3830
	t-statistics:	−1.9117	−0.7321	−0.5558	−2.2382	−0.6253	−0.4129
2	Coefficients:	−0.1302	−0.8080	−1.0769	−0.1544	−0.3827	−0.9718
	t-statistics:	−1.6033	−0.6442	−1.0592	−1.8593	−0.2982	−0.9341
3	Coefficients:	−0.1252	−0.1924	−0.8451	−0.1376	−0.1241	−0.6516
	t-statistics:	−1.8634	−0.1854	−1.0044	−2.0470	−0.1195	−0.7740
5	Coefficients:	−0.1483	−0.6151	−0.4713	−0.1509	−0.6535	−0.3364
	t-statistics:	−2.0726	−0.5565	−0.5261	−2.0757	−0.5817	−0.3695
Dummy Variables for Price Limits and Margin Used as Regressors [Equation (7) of Text]							
1	Coefficients:	−1.5440	−0.7787	−0.4707	−1.8726	−0.6986	−0.4967
	t-statistics:	−1.5159	−0.7820	−0.3632	−1.8856	−0.7196	−0.3931
2	Coefficients:	−0.8665	−0.8624	−1.8705	−1.2746	−0.5905	−1.8792
	t-statistics:	−0.8059	−0.8205	−1.3671	−1.1682	−0.5536	−1.3535
3	Coefficients:	−1.4077	−0.1722	−1.3586	−1.6943	−0.1535	−1.2931
	t-statistics:	−1.6185	−0.2025	−1.2276	−1.9896	−0.1843	−1.1933
5	Coefficients:	−2.2196	−0.9887	0.7574	−2.2852	−0.9577	0.8086
	t-statistics:	−2.5376	−1.1564	0.6805	−2.5737	−1.1034	0.7157

uniformly negative as they should be if margin requirements reduced volatility. Transaction taxes are similarly negative but insignificant.

Price limits are another matter. Using both the numerical limits and dummy variables, more stringent limits are associated with lower volatility, and the effect is significant for some holding intervals. Remember that very short-term intervals might display a significant impact of price limits for a spurious reason, yet the five-day intervals in table 6 are generally more significant than the shorter intervals.

This unexpected pattern is suspicious and made me wonder whether the results had been unduly influenced by the Hong Kong case. Notice in table 4 that Hong Kong does not have official price limits, yet the closing of the Hong Kong market on October 19 after about an 11 percent decline (cf. footnote 13), had the same effect as a price limit since the registered return on that day was smaller than it otherwise would have been. Hong Kong's estimated crash period volatility is extremely large; see table 1.

This unusual situation suggests that the tests be repeated with Hong Kong counted as if it had had price limits. Thus, Hong Kong was assigned a price limit of 11 percent, the price decline before trading was suspended on October 19. The results are given in table 7 which shows that the significance for price limits has disappeared. This is consistent with Hong Kong's closure (for five days) being entirely responsible for the seemingly significant long-term effect of price limits found in table 6.

Table 7. Standard deviation of returns regressed on price limits and margin requirements with Hong Kong assumed effectively to have had price limits (crash period, 10/12/87 through 10/30/87)

Holding Interval (Days)		Local Currency			Dollars		
		Pr Limits	Margin	Intercept	Pr Limits	Margin	Intercept
Estimated Values of Price Limits and Margin Used as Regressors [Equation (6) of Text]							
1	Coefficients:	−0.0821	−1.1181	−0.3806	−0.1038	−0.9990	−0.2356
	t-statistics:	−1.0345	−0.9112	−0.3796	−1.3179	−0.8197	−0.2366
2	Coefficients:	−0.0711	−1.0516	−0.9404	−0.0933	−0.6537	−0.8276
	t-statistics:	−0.8477	−0.8098	−0.8863	−1.0767	−0.4875	−0.7554
3	Coefficients:	−0.0751	−0.4130	−0.7272	−0.0870	−0.3578	−0.5308
	t-statistics:	−1.0713	−0.3806	−0.8202	−1.2309	−0.3273	−0.5943
5	Coefficients:	−0.0924	−0.8694	−0.3386	−0.0960	−0.9084	−0.2052
	t-statistics:	−1.2264	−0.7456	−0.3554	−1.2555	−0.7676	−0.2122
Dummy Variables for Price Limits and Margin Used as Regressors [Equation (7) of Text]							
1	Coefficients:	−0.3721	−0.9977	−0.6343	−0.7027	−0.9513	−0.6399
	t-statistics:	−0.3535	−0.9602	−0.4608	−0.6710	−0.9202	−0.4672
2	Coefficients:	0.2563	−1.0092	−2.0644	−0.1079	−0.7815	−2.0580
	t-statistics:	0.2397	−0.9563	−1.4765	−0.0976	−0.7159	−1.4229
3	Coefficients:	−0.4240	−0.3674	−1.4891	−0.0979	−0.3789	−1.4090
	t-statistics:	−0.4694	−0.4121	−1.2605	−0.7733	−0.4252	−1.1936
5	Coefficients:	−1.2271	−1.2679	0.6742	−1.3039	−1.2431	0.7319
	t-statistics:	−1.2913	−1.3517	0.5425	−1.3520	−1.3058	0.5803

4. Conclusions

The October 1987 stock market crash spawned an abundance of research papers, as scholars attempted to explain what seemed at the time, and to some extent remains, an inexplicable event.

Except for the period immediately around the crash, there is only meager evidence that international linkages across markets have become tighter over time. Yet the crash was worldwide in scope, and its similarity across countries was uncanny. Just on the face, this international similarity puts doubt to such explanations as particular macroeconomic events in one country, failure of a given country's market system, or simultaneous changes in underlying fundamentals (which were quite different across countries).

Assigning the origination of the crash to one country cannot be entirely ruled out, however, because of the possibility of a non-fully revealing equilibrium "contagion" process of the type suggested by King and Wadhwani (1988). Such a process *would* allow a world-wide crash to begin by a particular news event or even by a market "mistake" in one country. Evidence in favor of this process is that international correlations of returns increased dramatically during the crash period. However, this increase is consistent with other explanations, such as transaction costs hindering international arbitrage except during periods of high volatility.

Was the crash the bursting of a bubble? Some evidence seems to support this proposition: for example, in the majority of countries, the pre-crash period displayed significant serial dependence in stock returns, dependence that was definitely *not* present in the post-crash period. However, further work is necessary to ascertain whether this *measured* serial dependence is unusual relative to what one would have expected to find, even in a perfectly random process, by choosing a sample period that happened to culminate in a random peak. Ross (1987) shows that such ex post sample period selection will induce upward bias in estimates of serial dependence. Cross-country tests failed to detect this bias, but there are several ambiguities in the tests that will have to be resolved in future work.

The crash is history. What implications, if any, does it have for regulatory policy? Is there evidence that popular regulations or rules would have mitigated the crash, or that they would decrease price volatility in general? There is very little evidence in favor of the efficacity of margin requirements, price limits, or transactions taxes. Despite a large number of empirical studies, no one has provided evidence that margin requirements have an impact on volatility. There has been at least one recent paper claiming the contrary, but a careful examination of its methods have uncovered enough problems to cast those results into doubt.

As for price limits, there *must* be a very short-term impact on *measured* volatility, for the measured market price at a trading halt is likely to understate the direction of movement. Yet even for daily data, the cross-country evidence is slim that price limits reduce volatility, and there is no evidence at all that they work over periods as long as a week. In other words, trading halts caused by limits seem to have no effect on true volatility.

Transaction taxes are inversely but insignificantly correlated with volatility across countries, and the effect is too questionable for taxes to be used with confidence as an effective policy instrument.

Notes

1. The reports covered by Kamphuis, Kormendi, and Watson include those by the Presidential Task Force on Market Mechanisms, the Chicago Mercantile Exchange, the International Stock Exchange of London, the Commodity Futures Trading Commission, the Securities and Exchange Commission, and the U.S. General Accounting Office.

2. However, no one (including me) argues that the existence of portfolio insurance actually *mitigated* the extent of the crash.

3. The authors show that other potentially damaging news events, such as a worse-than-expected U.S. trade deficit announcement on October 14, were unable to explain the extent of the price decline.

4. However, as my colleague David Hirshleifer has pointed out, even a seemingly small event such as increased tax on takeovers could conceivably trigger a large crash if it were expected to foster more management malfeasance (because of the decreased threat of takeover).

5. However, this cross-country relation is also consistent with some countries simply being more sensitive to fundamental factors than other countries (and with such factors increasing over the first part of 1987 and falling in October).

6. This finance literature had its antecedents in some of the earlier literature on optimal growth along unstable paths. Samuelson (1967) speaks of the "tulip mania" in this context, and Ross (1975) gives an example of a bubble under a dynamic jump process.

7. Bubbles were known to be possible even under rationality with unrealistic assumptions such as an infinite time horizon or an infinity of traders.

8. Some of the results in Hardouvelis (1989) have been published in Hardouvelis (1988a).

9. As the bubble inflates, the probability of a crash increases; this increase in potential volatility (or risk) must be accompanied by an increased reward.

10. That is, the observed difference between actual stock returns and short-term interest rates.

11. The data are from Goldman Sachs & Co. and are widely publicized as the Goldman Sachs/FT-Actuaries World Indices.

12. A daily return is defined as $\log_e(P_t/P_{t-1})$ where P_t is the price index level on trading day t. Dividends are *not* included; dividend yields are available in the data base but their addition to the return makes virtually no difference in volatility. The log first difference was used so that returns over multiple days would be additive.

13. Table 1 brackets the major crash day of October 19 by a week before and two weeks afterward. This window is somewhat arbitrary, of course, but it is justified by allowing the inclusion of substantial and perhaps related price movements in the week prior to October 19, and it allows the capture of crash-related price movements after October 19 in countries with price limits or closures. In Hong Kong, for example, the observed major price decline did not occur until October 26 because the Hong Kong market was closed for the rest of the week after substantial price declines on October 19. The Hong Kong index declined 11.4 percent on October 19 and then the market was closed. On the day the market reopened, October 26, the index fell an *additional* 39.7 percent.

14. R is actually the simple average of the off-diagonal elements of the $(T \times T)$ sample autocorrelation matrix.

15. The regression is not well specified under all possible serial dependence structures. For example, if there were stationary first-order dependence, the coefficient b_1 would be a known function of T. However, the regression is simply meant to be a compact way to present a lot of evidence, and it should not be regarded as anything more meaningful, such as a method of testing for a given dependence structure.

16. For the cross-country simple averages given in table 2, everything is scaled to percent per day. Thus, for multiday returns, every mean return and variance of returns is divided by T, the number of days in the multiday holding period.

17. The regression has 4 degrees of freedom. A two-tailed test for significance at the 5 percent level has a critical value of 2.776 (i.e., the .975 fractile of Student's t distribution with four d.f.).

18. The t distribution with d degrees of freedom has variance $d/(d-2)$; see Johnson and Kotz (1970, p. 96).

19. The negative significant coefficient, indicating negative serial dependence, was for the United States. The two positive significant coefficients were for Austria and Belgium.

20. However, this explanation seems unlikely. As shown in table 1, there were large differences across countries in pre-crash mean returns.

21. Imagine, for instance, that returns in each country were driven by some common world factor, but that the degree of response to that factor differs among countries. Suppose in addition that the pre-crash period was simply a realization of a random process for the factor. Then the degree of measured [spurious] serial dependence would be the same in every country because it would be the same as the serial dependence in the factor itself. The observed serial dependence would not be cross-country related to the magnitude of the price rise in the pre-crash period.

22. See the paper by Hilliard (1979), which examined international stock price comovements during the OPEC crisis, July 1973–April 1974.

23. See Roll (1989, p. 39).

24. The empirical results of Parhizgari et al. are somewhat implausible because they find lead/lag relations from two to eight days. This would seem to imply the possibility of predicting one market's return up to eight days in advance by using the return in another market as a predictor. Their test involves a complicated multivariate statistic and it is not clear (at least to me) how to transform their results into a practical trading rule.

25. Using Germany, Japan, the United States, and the United Kingdom.

26. If, for example, j = U.S. and k = Japan, and the standard deviation is calculated from March 1 through March 31 in the U.S., it would be calculated from March 2 through April 1 in Japan.

27. Bertero and Mayer also use the Goldman Sachs/FT Actuaries data which cover 23 countries, but Mexico is excluded from the tests for the effect of cross-listed countries because no Mexican shares are traded abroad and no foreign shares are traded in Mexico.

28. At the 1 percent level of significance.

29. This is probably true for several reasons. For example, if there is more than one world "factor" in the

Arbitrage Pricing Theory context, just about any set of multiple regressors will beat a single regressor. Also, Bertero and Mayer *are* undoubtedly justified in thinking that some countries are much more closely attuned than others, and, as a consequence, a global index masks individual country relations.

30. King and Wadhwani present evidence that the contemporaneous correlation during hours when both London and New York are open for trading was much higher between U.S. futures prices and London cash prices than between New York and London cash prices. They interpret this to imply that cash prices in New York were out of date part of the time during the crash period, and thus they use the apparently more accurate U.S. futures prices in all subsequent tests.

31. However, Miller (1989) argues that the triggering event might not be all that obvious; an analogy to the crash is "... an avalanche ... an increasingly unstable buildup of snow on a mountainside, triggered finally by some trivial and normally harmless event like the snapping of a twig (p. 3)."

32. A transaction tax should be distinguished from the currently-in-place tax on trading profits. The former is levied on every trade at the time it occurs. Less credible proposals have also been made about the latter; for instance, a 50 percent surtax on short-term trading profits is proposed by Rohatyn (1988) to curb volatility induced by the "proliferation of speculative financial instruments" wielded by institutional investors who "... no longer invest ... they *speculate*" (p. 50, emphasis in original). With a 100 percent surtax, volatility might be *entirely* eliminated, along with trading!

33. See Edwards (1988b). Also, an interesting survey of many suggested reforms is given from a legal viewpoint by Becker and Underhill (1988).

34. There is a very clear-cut impact on price *level:* imposition by the exchange of 100 percent margin causes an immediate price decline. But this could be attributed to the conveyance of information about the company implicit in the imposition of such a severe margin. For instance, investors could interpret the exchange's move to imply that the exchange itself possesses privileged information to the effect that previous price advances have been unwarranted.

35. Largay's price impact and volume results were confirmed with a bigger sample in a replication study by Eckardt and Rogoff (1976).

36. Hartzmark's paper is unique in several respects. He studies the impact of margins on the *type* of trader and he uses data for commodity futures. He finds no systematic influence of margin changes on price volatility (p. S177), but he points out that much larger margins than are currently in effect in the futures market could conceivably have an effect (though perhaps in the "wrong" direction). Merton Miller pointed out to me, however, that empirical studies of margin requirements with futures data might not be entirely applicable to cash markets because, unlike the Federal Reserve, futures exchanges explicitly tie margin levels to volatility. The Fed does the opposite since it decreases (increases) margin requirements when price levels decrease (increase), and there is a well-documented inverse relation between volatility and price level.

37. Salinger argues that Schwert's criticism of Hardouvelis "... has several problems ... it suggests that the Federal Reserve reduces margins in response to volatility increases, which would be odd" (p. 17). Actually, Schwert finds that the Fed responds to price *levels* and that levels happen to be inversely related to volatility (evidently unbeknownst to the Fed). Salinger also says that Schwert's test "arguably has low power" since it is based on first differences. On the other hand, a test based on levels such as Hardouvelis's is subject to gross bias induced by the spurious regression fallacy, as documented by Hsieh and Miller.

38. However, when 12 lags of volatility are included in the model, neither debt nor margin requirements remains significant in the 1928–1987 period.

39. For instance, in studying orange juice futures (Roll, 1984), I found that limits impeded prices from responding to weather.

40. The Ma et al. results using daily *do* show a tendency for continuation of price movements in the same direction.

41. One might ask if Barnum's proposition applies also to economists.

42. This conclusion is supported in the recent detailed examination provided by Salinger (1989).

43. Easy avoidance is related to the problem of harmonization of margin rules across markets, although the latter also a question of inter-market competition. An interesting analysis of avoidance and harmonization is given by Estrella (1988).

44. In the single exception of France, the margin requirements and price limits are given for the short-term futures market. There most stock is traded for delivery within the month (i.e., the purchase contract is technically a future).

45. If there are no price limits in country j, $L_j = 0$, while if there are price limits, L_j = 100/(Price Limits in %). The rationale for L_j is that the absence of price limits is essentially a wider band than any level of imposed limits; thus, L_j represents an attempt to combine countries without limits into the same data set as countries with limits having numerical values.

46. If price limits reduced volatility, the coefficient would be negative.

47. Holding intervals are limited to five trading days. Intervals of 10 and 20 days used in previous results are not feasible here because there would be only one complete nonoverlapping interval during the 15-day crash period and thus the standard deviation of returns could not be computed.

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