Reassessment of Contagion and Competitive Intra-industry Effects of Bankruptcy Announcements

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We investigate the effects of bankruptcy on equity value of the bankrupt firm's competitors and find that these effects are unclear. Using a sample of bankruptcies with debt over $120 million, Lang and Stulz (1992) find competitive and contagion effects on other firms in the same industry. We replicate their stratification of industry portfolios by concentration and leverage but use a sample of bankruptcy filings from a single legal regime. Our tests do not support their conclusions. Furthermore, use of a debt screen on bankruptcies appears to confound the conclusions, because test significance depends on the debt screen applied.

Introduction

Bankruptcy has a strong negative impact on the filing company's shareholder wealth. The effect of bankruptcy news on competitors' stocks, however is not clear-cut.

If investors believe that a bankruptcy indicates industry-wide cash flow problems, then a contagion effect would decrease share prices of the bankrupt company's competitors. In their seminal paper on contagion effects in the banking industry, Aharony and Swary (1983) conclude that if bankruptcy is due to events specific to the failing firm, e.g., fraud, then no contagion effect occurs. On the other hand, bankruptcy associated with potential industry-wide cash flow problems, e.g., foreign exchange losses, has a negative contagion effect. Evidence exists for contagion effects of other types of bad news. For example, Akhigbe, Madura, and Whyte (1997) find that news of bond rating downgrades leads to a drop in rivals' share

0747–5535/99/1300/0045/$2.00

prices, especially if the downgrade is due to deterioration of financial strength. Similarly, Sun and Tang (1998) observe that downsizing announcements have a significant negative effect on rivals' share prices.

On the other hand, if investors perceive that a bankruptcy is an opportunity for competitors to improve profitability by increasing market share at the filing company's expense, then a competitive effect would increase rivals' share prices. A competitive effect might occur because the filing company bears costs that do not burden its non-filing competitors. It is well documented that the filer incurs substantial direct and indirect costs due to the bankruptcy process itself (Altman, 1984; Ang, Chua, and McConnell, 1982; Opler and Titman, 1994; Warner, 1977; Weiss, 1990). These costs deplete assets at the bankrupt company until the court approves its plan, a process that Eberhart, Moore, and Roenfeldt (1990) report averages about two years for Chapter 11 filings. The bankruptcy court focuses on an equitable division of assets among claimants rather than increasing shareholders' wealth. Finally, customers are reluctant to buy from bankrupt companies and shift their business to its competitors (Titman, 1984).

Empirical evidence on competitive effects is mixed. Cheng and McDonald (1996) focus on the effects of bankruptcy announcements in the airline and railroad industries. They find significant competitive effects in the airline industry but significant contagion effects in the railroad industry. Evidence is mixed concerning other news that might plausibly have a competitive effect. For example, no industry-wide effect is observed on news of large revisions in dividends (Laux, Starks, and Yoon, 1998) or dividend initiations (Howe and Shen, 1998). Erwin and Miller (1998), however, report that share repurchase announcements lead to a significant competitive effect: while share price of the repurchasing firm goes up, competitors' share prices decline significantly.

In their seminal study, Lang and Stulz (1992) argue that industry leverage and concentration are the keys to determining whether the negative contagion effect or positive competitive effect dominates when bankruptcy filing is news. First, they claim that if the competitors' leverage ratios are high, then the contagion effect will be strong. Lang and Stulz reason that higher leverage reduces flexibility of competitors to expand their market share at the expense of the filing company. Second, Lang and Stulz claim that if the industry lacks competitive market structure, then the bankruptcy will have a strong competitive effect. They reason that the less competitive the industry, the easier it should be for competitors to take advantage of the filing company's difficulties. Using data from 1970–1989, Lang and Stulz stratify industry portfolios by concentration and leverage. They find positive competitive effects in industries with high concentration (low competition) and low leverage, but negative contagion effects in industries with low concentration (high competition) and high leverage.

A major change in U.S. bankruptcy law in 1979 potentially confounds Lang and Stulz's (1992) analysis, because their sample time period, 1970–1989, straddles two different legal regimes. The legal regime for bankruptcy determines what happens to assets of the firm when it files. Rational investors should evaluate the economic impact of a filing in terms of outcomes likely under a given regime. Lang and Stulz hypothesize that, all else being equal, investors will respond positively when competitors of the filing firm are likely to gain a relative economic advantage due to bankruptcy costs at the filer. Boyes and Faith (1986) and White (1983) show that the Bankruptcy Reform Act of 1978 (which became effective in 1979) significantly changed the economic costs of bankruptcy. This institutional factor warrants reexamining Lang and Stulz's hypotheses using a sample collected under a single legal regime.

When we examine Lang and Stulz's (1992) stratification analysis under one legal regime, the results do not support their conclusions. Replication of their stratification approach over 1985–1993, a period not affected by major changes in bankruptcy law, does not produce significant evidence of any kind of shareholder wealth effect—positive or negative—at competitors of bankrupt companies. This analysis also indicates that Lang and Stulz overlooked a multiple comparison problem that caused them to overstate the statistical significance of their results. In addition, their bankruptcy debt screen could have introduced a positive bias in their statistical evidence of a contagion effect.

Sampling Design and Statistical Methods

The present study differs from Lang and Stulz (1992) in four important ways. The sample of bankruptcies is larger, the bankruptcy law is consistent (1985–1993), more industries are included, and the sample is not restricted to firms with debt greater than $120 million.

Sampling Design and Portfolio Construction

Using a two-stage sampling design similar to that in Lang and Stulz (1992), we first collect a sample of publicly traded companies subject to Chapter 11 filings from 1985–1993. We searched *The Wall Street Journal*, *The Bankruptcy DataSource*, and *Standard & Poor's Daily News* for news about Chapter 11 filings. To confirm bankruptcy filing dates, we searched Lexis/Nexis and *Disclosure Inc.* for filings with the Securities and Exchange Commission. A bankrupt firm is excluded from the sample if it does not have an industry match in Standard & Poor's Compustat. The current sample includes 76 bankruptcies from 57 industries, while Lang and Stulz examine 59 bankruptcies from 41 industries. As in Lang and Stulz, the current sample does not include banks, savings and loans, pure bank holding companies, and pure savings and loan holding companies due to regulator involvement in their failures. See Table 1 for descriptive financial statistics on the current bankruptcy sample.

Table 1—Descriptive Statistics on the Bankruptcy Sample for the Fiscal Year Preceding the Chapter 11 Filing

The sample of bankrupt firms consists of 76 Chapter 11 filings by publicly traded companies in 1985–1993 (excluding depository institutions). Compustat includes financial data on 51 of these companies for the fiscal year preceding bankruptcy

Descriptive Statistic	Total Sales ($ millions)	Total Long-Term Debt ($ millions)	Total Debt ($ millions)	Total Assets ($ millions)
Maximum	6,684.88	2,595.94	3,818.78	9,161.30
95 Percentile	3,383.09	1,365.17	2,090.15	3,011.58
Upper Quartile	567.76	153.25	406.62	514.73
Median	237.02	25.66	136.91	215.47
Lower Quartile	87.12	0.63	36.03	85.64
5 Percentile	17.25	0.00	7.04	21.31
Minimum	-0.05	0.00	0.46	2.77

The sampling method in the current study differs from Lang and Stulz's (1992) in two important ways. First, the sample is restricted to bankruptcy filings from a time period with a relatively fixed set of bankruptcy laws and precedents, while their sampling period covers two different legal regimes. Second, the current study places no restrictions on the debt level of included bankrupt companies, whereas Lang and Stulz restrict bankruptcies to those with debt greater than $120 million. In this paper, we explore how this constraint might have biased their results.

In the second stage of the sampling design, an industry portfolio of competitors is selected for each bankrupt company. This portfolio includes all companies in Compustat with the same primary four-digit SIC code as the bankrupt company. In this sample, 11 industries have more than one bankrupt company. Portfolios from the same industry include many of the same companies but usually have financial data from different fiscal years.

Lang and Stulz (1992) compute industry concentration for each portfolio using all companies in the industry but sometimes compute leverage using only a sample from the industry. For their 11 industries (19 percent of their sample) with more than 50 companies, Lang and Stulz randomly sample 50 companies to compute industry leverage. This approach introduces statistical sampling error in their second stage and thus adds statistical noise to these leverage measurements. To minimize sampling error, the current sample uses all available industry companies in Compustat to measure both industry concentration and leverage.

Lang and Stulz (1992) introduce potential bias when they calculate cumulative prediction error (CPE) because they use only those competitors with returns in the Center for Research in Security Prices (CRSP) database. Not all companies in Compustat have returns in CRSP. In our paper, zero to 57 percent of the companies in each industry portfolio do not have CRSP returns; the median rate is 26 percent. Industry portfolios formed from all available companies in Compustat have substantially different financial characteristics than subsets of companies with CRSP returns. For example, when calculating the Herfindahl index for industry portfolios and the corresponding CRSP-only subsets, we find that the ratio of the CRSP-only index to the full portfolio index ranges from 0.57 to 3.79. For the median total debt-to-total assets, the ratio of the CRSP-only median to the full portfolio median ranges from 0.32 to 2.19. To examine this source of bias in Lang and Stulz's study, we repeat the analysis on CRSP-only subsets as well as full portfolios.

Cumulative Prediction Error Calculations

As in Lang and Stulz (1992), industry portfolio abnormal returns are calculated as prediction errors for the portfolio return. (They note that this approach accounts for cross-sectional dependence among companies in each portfolio.) For each trading day in a portfolio's estimation and event windows, the portfolio return is computed using only those companies with returns in the 1993 CRSP files. To reduce survival bias, we include a company in the calculation even if it does not have reported returns for all days in the estimation and event windows.

Lang and Stulz (1992) use *The Wall Street Journal* publication of the Chapter 11 filing as their event day. This may not be the best choice for an event date. Average abnormal returns for bankrupt firms are significantly negative prior to the publication date. Lang and Stulz's bankruptcy sample has an average abnormal return of -18.93 percent on day -1 (z-statistic: -41.42) versus -2.50 percent on day 0 (z-statistic: -6.34). Due to the publication schedule of *The Wall Street Journal*, a bankruptcy filing usually precedes its publication date by one business day. Thus, Lang and Stulz's data suggest that the market responds to the bankruptcy on the filing date.

We use the filing date as the event day. We do not use an earlier date, because the filing date is the earliest published announcement of a filing for any firm in the current sample. (Generally, the first published news is the following business day.) To replicate Lang and Stulz (1992), the event date should be close to theirs yet also consistent with investor response in an efficient market. We select the filing date, because average abnormal returns on the bankruptcies spike downward on the filing date in both Lang and Stulz's sample and the current sample. (See Table 2.) To minimize the influence of confounding events, a bankruptcy is excluded from the current sample if any story concerning that firm (other than news about the filing itself) appeared in *The Wall Street Journal* during the 11-day event window centered on the filing date.

The 11-day event windows in Lang and Stulz (1992) and the current study minimize the importance of whether the filing date or *The Wall Street Journal* publication date should be the event date. Both dates are at or near the center of either 11-day event window. Lang and Stulz report a significant negative cumulative average abnormal return (-28.25 percent) for their bankruptcies over an 11-day event window centered on the publication date. Similarly, we find a significant negative cumulative average abnormal return (-16.47 percent) for the current sample of bank-

Table 2—Abnormal Returns Associated with Bankruptcy Announcements

The prediction error (PE) is the market model residual in percent. The sample of bankrupt firms consists of 76 Chapter 11 filings by publicly traded companies in 1985–1993 (excluding depository institutions). For each bankruptcy we form portfolios of competitors with the same four-digit SIC code, using firms for which net annual sales are available in Compustat for the fiscal year preceding filing. We calculate abnormal returns for each portfolio using the subset of firms with daily returns in CRSP. The 11-day event window is centered on the filing date. Market-model abnormal returns, CAARs, and corresponding z-statistics follow the approach in Mikkelson and Partch (1985, 1988). Results below are for value-weighted industry portfolios, and the market is represented by the CRSP value-weighted NYSE/Amex/Nasdaq index. As in Lang and Stulz (1992), we define market-model estimation periods relative to first news of distress for the filing firm. Lang and Stulz use the filing announcement in *The Wall Street Journal* as their day 0. This publication date lags the filing date by one trading day. The event date (day 0) in the current study is the filing date and corresponds to Lang and Stulz's day -1. The event window (0, +1) shown below corresponds to the event window (-1, 0) in Lang and Stulz's Table 1.

Days Relative to Bankruptcy Filing	Average Abnormal Return for Bankrupt Firms			Average Abnormal Return for Industry Portfolios		
	#	PE	z-statistic	#	PE	z-statistic
-5	75	-0.74	-2.07	76	-0.20	-1.63
-4	75	-1.38	-3.32	76	-0.26	-2.22
-3	75	-0.63	0.49	76	0.03	0.62
-2	75	-1.13	-4.66	76	0.03	0.39
-1	75	-2.00	-6.18	76	0.09	0.41
0	74	-9.39	-18.20	76	0.03	0.28
+1	72	-10.83	-20.87	76	0.38	1.71
+2	73	7.13	15.04	76	-0.04	-0.54
+3	72	3.49	6.19	76	-0.05	-0.19
+4	73	-1.21	1.99	76	0.01	0.66
+5	73	1.60	3.82	76	-0.10	-0.14
-1, 0	74	-11.43	-17.21	76	0.12	0.61
-1, +1	71	-23.12	-25.77	76	0.50	1.53
0, +1	71	-20.29	-26.87	76	0.42	1.54
-5, +5	70	-16.47	-8.33	76	-0.08	-0.23

ruptcies over an 11-day event window centered on the filing date. (See Table 2.) An 11-day event window also captures market response to any unreported leakage of information about the eventual bankruptcy. To test the effect of the event window size on the results, we repeat the analysis with a three-day event window centered on the filing date.

The method that Lang and Stulz (1992) use to define their estimation periods introduces a bias whose magnitude and direction cannot be easily determined *a priori*. To estimate market-model parameters from a period uncontaminated by news of distress, they use a time period ending 51 trading days prior to the first news of distress in *The Wall Street Journal*. Gilson, John, and Lang (1990) observe that the time between news of distress and bankruptcy filing varies substantially. In the current sample, the time between the first distress announcement and bankruptcy filing ranges from zero to 808 trading days, with a median of 83 trading days. It seems likely that this time period would vary from filing to filing in Lang and Stulz's study, as well. As time between the estimation period and the event date increases, however, so too does the probability of a change in the risk profile of competitor companies—and hence in the true market-model parameters—for reasons unrelated to the bankruptcy filing. In addition, Lang and Stulz's precaution probably is unnecessary. *A priori*, news of distress should not contaminate the parameter estimates because financial distress generally does not lead to bankruptcy (Deakin, 1977; Gilbert, Menon, and Schwartz, 1990).

Market-model estimation periods are defined in the same way as in Lang and Stulz (1992). We check whether the variable length of time between estimation period and event date affects the results by repeating the tests with market-model parameters estimated on periods ending a uniform 51 trading days prior to the bankruptcy filing. The latter results are not significantly different and thus are not reported.

To compute the portfolio CPEs, we follow the same approach as in Lang and Stulz (1992) with one important exception. Lang and Stulz use the Scholes and Williams (1977) beta estimation technique in their market-model estimation procedures to overcome the bias that arises from infrequent trading of financially distressed companies (Gilson, John, and Lang, 1990). The Scholes and Williams technique is unnecessary in this study because the prediction errors are for portfolios of competitor companies, not the bankrupt companies themselves. Hence, the portfolio CPEs in this paper are calculated using formulas developed in Mikkelson and Partch (1985; 1988) but without the Scholes and Williams adjustment.

Portfolios are stratified into four categories by industry leverage and concentration as in Lang and Stulz (1992). We compute cumulative average abnormal returns (CAARs) and the corresponding z-statistics for each category. The estimation periods are sufficiently long so that the z-statistics are approximately normal random variables. Like Lang and Stulz, we repeat all calculations for both value-weighted and equally weighted industry portfolios.

Table 3 shows CAARs for value-weighted portfolios of companies in each industry over the 11-day event window. Only a small number of CAARs have z-statistics of magnitude 1.96 or greater: for value-weighted industry portfolio CAARs, only four of 57 industries; for equally weighted industry portfolio CAARs (not shown here), only two of 57 industries. These results suggest that random chance might explain the z-statistics. In contrast, for value-weighted industry portfolio CAARs, Lang and Stulz (1992) report that six of 41 industries (or about 15%) have z-statistics greater than 1.96 in magnitude.

Table 3—Cumulative Average Abnormal Returns (Value-Weighted) by Industry Over the Event Window (-5, +5)

Sample of bankruptcies: 76 Chapter 11 filings by publicly traded companies in 1985–1993 (excluding depository institutions). The bankruptcies represent 57 industries defined by four-digit SIC code. For each bankruptcy, we form portfolios of companies with the same four-digit SIC code, using companies for which net annual sales are available in Compustat for the fiscal year preceding the bankruptcy filing. We calculate abnormal returns for each portfolio using the subset of companies with daily returns available in CRSP. N = number of industry portfolios. Avg # firms = average number of companies per portfolio. For each industry, we compute the cumulative average abnormal return (CAAR) from individual portfolio cumulative prediction errors over event windows centered on the bankruptcy filing date. z-stat = CAAR divided by its standard error.

SIC Code	N	Avg # Firms	CAAR (%)	z-stat	SIC Code	N	Avg # Firms	CAAR (%)	z-stat
1311	4	196	-0.49	-0.38	3861	2	19	1.50	0.86
1381	1	18	-11.09	-2.23	4213	1	31	2.46	0.51
1389	1	9	6.80	1.27	4512	4	21	2.69	1.00
1531	4	35	1.16	0.23	4700	1	5	-0.92	-0.16
2013	1	7	-4.28	-0.40	4911	1	48	0.11	0.10
2200	1	12	7.08	1.96	4922	2	12	2.02	0.98
2300	1	14	-0.02	-0.00	4923	1	17	-1.76	-0.67
2320	1	16	6.18	1.78	4953	1	19	1.16	0.25
2330	2	19	-0.81	-0.28	5065	1	29	2.76	0.85
2452	1	7	-4.84	-0.50	5211	1	15	-7.22	-1.87
2511	1	8	9.28	2.72	5311	1	14	3.15	1.71
2522	1	3	1.24	0.22	5331	4	25	0.33	0.28
2631	1	8	-4.99	-1.53	5399	1	2	7.20	0.71
2821	1	13	-0.50	-0.11	5412	2	5	-1.95	-0.44
2834	1	82	-3.32	-1.90	5812	1	72	-0.04	-0.01
3060	1	3	-0.20	-0.05	6099	1	6	-4.58	-0.49
3241	1	6	-2.82	-0.77	6282	1	16	-3.34	-1.33
3270	1	4	0.42	0.11	6411	1	31	0.38	0.15
3312	1	31	-3.43	-1.72	6510	1	6	-5.04	-0.81
3317	1	4	-0.52	-0.09	6532	1	11	1.28	0.84
3320	1	3	-1.40	-0.30	6552	1	20	-1.64	-0.40
3420	1	11	1.73	0.61	6798	1	133	-3.61	-2.80
3490	2	18	-0.15	-0.10	7011	2	21	-5.95	-1.72
3570	1	6	1.48	0.64	7373	1	71	-0.43	-0.15
3572	1	24	8.29	1.57	7812	2	29	-2.28	-0.77
3661	1	52	2.11	0.62	7822	1	7	-0.68	-0.10
3674	1	55	-0.02	-0.00	8062	1	9	-1.81	-0.42
3714	1	36	0.11	0.08	8351	1	3	-1.68	-0.20
3845	1	49	1.26	0.42					

Empirical Analysis Based on Stratification by Concentration and Leverage: Measures of Industry Concentration and Leverage

We measure industry concentration using the Herfindahl index, the sum of the squared market shares. As in Lang and Stulz (1992), market share is defined as the ratio of a company's net annual sales to the total for the industry. To test the sensitivity of the results to specification of industry concentration, we also apply the four-firm ratio, defined as total sales of the four largest companies in the industry divided by total industry sales (Samuelson and Nordhaus, 1989). To identify the four largest companies in each industry, we rank companies by net annual sales in the fiscal year preceding the bankruptcy filing. Results with the two measures do not differ materially, so the tables only include results with the Herfindahl index.

We investigate four leverage ratios: long-term debt to total assets, the measure used by Lang and Stulz (1992); total debt to total assets; total liabilities to total assets; and net property, plants, and equipment to total assets. Industry leverage is defined as median company leverage in the industry because the median is not sensitive to outliers and permits direct comparison to Lang and Stulz. Alternative leverage ratios permit tests of robustness of leverage measured as long-term debt. Many studies use total debt to total assets to measure financial leverage rather than long-term debt to total assets (Lang, Ofek, and Stulz, 1996). Total debt and total liabilities are more appealing than long-term debt because they are more comprehensive measures of claims with higher priority on assets in bankruptcy than shareholders' claims. The contagion and competitive effects might be more noticeable if we discriminate among industry portfolios in terms of leverage measured with total debt or total liabilities.

Empirical Results

Stratification by industry concentration and leverage follows Lang and Stulz (1992). We stratify industry portfolios by high and low concentration and by high and low industry leverage with respect to the median values among the 76 portfolios. Table 4 shows results based on the same measures as in Lang and Stulz: industry concentration measured by the Herfindahl index, and leverage measured by median long-term debt to total assets. None of the CAARs for the four stratification categories is significantly different from zero. Unlike Lang and Stulz, we find no evidence of either contagion or competitive wealth effects due to bankruptcy filing.

The large number of statistical tests raises the question of what the appropriate level of significance should be. We set aside the broader issue of judging significance across all reported (as well as unreported) tests and simply address the multiple comparison problem for each four-category set of results.[1] The equation $p = 1 - (1 - \alpha)^n$ defines the relation between overall level of significance (p) and the significance level for each individual test (α) in a multiple comparison test with n categories. In a four-category test, if the desired overall level of significance is 0.05, then the significance level for each individual test is 0.0127. The p-value for each test in Panel B of Table 4 exceeds 0.0127. Hence, no CAAR is significantly different

[1] Assessing significance is complicated because reported statistics are not independent across all tests; e.g., we use the same value-weighted CPEs to calculate CAARs in all left side two-by-two sections of Tables 4 and 5. Within each four-category test, however, CAARs are statistically independent because portfolio CPEs are independent.

Table 4—CAARs over 11-Day Event Window for Industry Portfolios Stratified by Industry Concentration and Industry Leverage Measured by Median Long-term Debt to Total Assets

Each cell of each two-by-two table shows CAAR for the category, z-statistic for the CAAR (square brackets), p-value for z-statistic (parentheses). The full sample of bankrupt firms consists of 76 Chapter 11 filings by publicly traded companies in 1985–1993 (excluding depository institutions). For each bankruptcy we form portfolios of competitors with the same four-digit SIC code, using firms for which net annual sales are available in Compustat for the fiscal year preceding filing. We calculate abnormal returns for each portfolio using the subset of firms with daily returns in CRSP. Results are based on industry portfolio CPEs over an 11-day event window centered on the filing. Market-model abnormal returns, CAARs, and corresponding z-statistics follow the approach in Mikkelson and Partch (1985, 1988). As in Lang and Stulz (1992), we define market-model estimation periods relative to first news of distress for the filing firm. We stratify industry portfolios by median industry concentration and median industry leverage. Financial measures are based on all industry firms available in Compustat. Industry concentration is measured by the Herfindahl index. For comparison, we list the corresponding results from Lang and Stulz.

Panel A: Results in Lang and Stulz (1992)

	Value-Weighted Returns	
Concentration:	Below Median	Above Median
Below	-1.60%	2.21%
Median	[-0.90]	[1.81]
Leverage	(0.37)	(0.070)
Above	-3.22%	-2.46%
Median	[-3.27]	[-2.04]
Leverage	(0.001)	(0.041)

Panel B: Results from the Replication of Lang and Stulz

	Value-Weighted Returns		Equally Weighted Returns	
Concentration:	Below Median	Above Median	Below Median	Above Median
Below	-0.65%	-0.30%	-1.44%	-1.08%
Median	[-1.07]	[0.13]	[-1.76]	[-1.30]
Leverage	(0.28)	(0.90)	(0.078)	(0.19)
Above	1.38%	-0.51%	-0.89%	-0.33%
Median	[0.76]	[-0.10]	[-1.59]	[-0.99]
Leverage	(0.45)	(0.92)	(0.11)	(0.32)

from zero in any category. Lang and Stulz (1992) provide significant evidence for a contagion effect (at low concentration and high leverage) but not for a competitive effect (at high concentration and low leverage).

We check robustness of the analysis by repeating it with alternative measures of concentration and leverage. None of the CAARs in these tests is significantly different from zero. See Table 5 for results with concentration measured by the Herfindahl index. Tests for contagion effects (at low concentration/high leverage) are not significant, with p-values of at least 0.42 for value-weighted returns and at least 0.09 for equally weighted returns. Tests for competitive effects (at high concentration/low leverage) are not significant, with p-values of at least 0.33 for value-weighted returns and at least 0.20 for equally weighted returns.[2] The evidence does not support the

[2] In many cases these CAARs have the wrong sign, i.e., the signs are not consistent with Lang and Stulz's (1992) hypotheses of a competitive effect at high concentration and low leverage.

Table 5—CAARs Over 11-Day Event Window for Industry Portfolios Stratified by Industry Concentration and Industry Leverage: Alternative Measures of Leverage

Each cell of each two-by-two table shows CAAR for the category, z-statistic for the CAAR (square brackets), p-value for z-statistic (parentheses). The full sample of bankrupt publicly traded companies consists of 76 Chapter 11 filings in 1985–1993 (excluding depository institutions). For each bankruptcy we form portfolios of competitors with the same four-digit SIC code. We calculate abnormal returns for each portfolio using the subset of firms with daily returns in CRSP. Results are based on industry portfolio CPEs over an 11-day event window centered on the filing. Market model abnormal returns, CAARs, and corresponding z-statistics follow the approach in Mikkelson and Partch (1985, 1988). We stratify industry portfolios by median industry concentration and median industry leverage. Financial measures are based on all industry firms available in Compustat. Industry concentration is measured by the Herfindahl index.

Panel A: Industry Leverage Measured by Median Total Debt to Total Assets Ratio

	Value-Weighted Returns		Equally Weighted Returns	
Concentration:	Below Median	Above Median	Below Median	Above Median
Below	-0.51%	-0.52%	-1.63%	-0.86%
Median	[-0.88]	[-0.25]	[-1.85]	[-0.85]
Leverage	(0.38)	(0.80)	(0.064)	(0.39)
Above	1.02%	-0.32%	-0.76%	-0.47%
Median	[0.47]	[0.26]	[-1.51]	[-1.42]
Leverage	(0.64)	(0.79)	(0.13)	(0.16)

Panel B. Industry Leverage Measured by Median Total Liabilities to Total Assets Ratio

	Value-Weighted Returns		Equally Weighted Returns	
Concentration:	Below Median	Above Median	Below Median	Above Median
Below	-0.41%	-0.07%	-1.29%	-1.14%
Median	[-1.16]	[0.32]	[-1.66]	[-1.29]
Leverage	(0.25)	(0.75)	(0.096)	(0.20)
Above	1.00%	-0.74%	-1.09%	-0.23%
Median	[0.80]	[-0.29]	[-1.70]	[-0.99]
Leverage	(0.42)	(0.77)	(0.090)	(0.32)

Panel C: Industry Leverage Measured by Median Operating Leverage

	Value-Weighted Returns		Equally Weighted Returns	
Concentration:	Below Median	Above Median	Below Median	Above Median
Below	0.40%	0.23%	-1.59%	-0.67%
Median	[-0.36]	[0.98]	[-2.41]	[-0.71]
Leverage	(0.72)	(0.33)	(0.016)	(0.48)
Above	0.10%	-1.00%	-0.76%	-0.65%
Median	[-0.04]	[-0.92]	[-0.90]	[-1.54]
Leverage	(0.97)	(0.36)	(0.37)	(0.12)

existence of either a contagion or competitive effect in scenarios with alternative measures of industry concentration and leverage.

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which requires a positive sign for the CAARs, and a contagion effect at low concentration and high leverage, which requires a negative sign for the CAARs. Signs of the CAARs are not meaningful, however, because they are not significantly different from zero.

Alternative Market Model Specifications

None of the alternative market model specifications provide significant CAARs. Tables 4 and 5 report replications of each analysis for both the value-weighted and equally weighted portfolio returns. The p-values of the z-statistics for equally weighted returns tend to be smaller than for the corresponding value-weighted returns but not small enough to conclude that the equally weighted CAARs are significantly different from zero.[3] We repeat all of the stratification tests for three-day event windows. Although the corresponding statistics for 11-day and three-day event windows often differ in magnitude and sign, none are significant. Finally, we repeat all tests using market model parameters estimated on periods a uniform number of trading days (51) prior to the bankruptcy filing. None of the resulting CAARs is significantly different from zero.

Alternative Sampling Designs for Competitors

In both the current study and Lang and Stulz (1992), availability of stock returns fundamentally constrains sampling in a manner that can introduce bias. To test the robustness of sampling methods and ascertain the effect of any bias, we repeat the stratification analysis using only those Compustat companies with returns in CRSP. While these CAARs and z-statistics differ in magnitude compared to the results reported earlier, they usually have the same sign and statistical significance. Thus, it does not appear to matter whether industry leverage is measured based on all Compustat companies or only the subset with CRSP returns.

We also check for sampling bias by replacing portfolio CPE with portfolio change in return on equity (ROE) because ROE is available for most companies in Compustat, even when CRSP returns are not. To compute portfolio change in ROE, we first calculate change in ROE for each company with ROE available for fiscal years ending just before and immediately after the bankruptcy filing month. Portfolio change in ROE is defined as the median change for companies in the portfolio. As before, we then stratify portfolios by industry concentration and leverage.

For each of the four stratification categories, we test whether the category median change in ROE is significantly different from zero using the quartile test (Conover, 1980). All of the category medians are less than zero. Support for a contagion effect is strongest in the low concentration/high leverage category, as Lang and Stulz (1992) predict, although we also find a contagion effect at high concentration. On the other hand, these results provide no evidence of a competitive effect. Furthermore, profitability ratios, such as ROE, do not directly measure changes in shareholder wealth. Thus, implications of these results for wealth effects are limited.

[3] Bankruptcy is more likely for small companies, so equally weighted returns might be more sensitive to a shareholder wealth effect than value-weighted returns. The CAARs based on equally weighted returns are not significant, either.

Alternative Sampling Designs for Bankruptcies

Only competitors' characteristics affect the results. But statistics in Lang and Stulz (1992) also depend on characteristics of the bankrupt companies because they delete bankruptcies with debt less than \$120 million. They argue that this screen restricts attention to cases with a potential industry-wide effect. In the current paper, total debt in the fiscal year preceding bankruptcy filing ranges from a low of \$0.464 million to a high of \$3,818.8 million for the 51 of the 76 bankruptcies with financial data in Compustat. Thus, a debt level screen as in Lang and Stulz could introduce a size-related bias. We wish to see if the statistical significance of Lang and Stulz's results depends on application of a debt screen to bankruptcy filers.

Before examining the effects of screens based on bankruptcy debt, we first check for bias that might arise if we omit the 25 competitor portfolios for bankruptcies that have no financial data in Compustat. We compare median industry concentration and leverage for the remaining 51 portfolios with the medians for the full sample of 76 portfolios and find no significant difference. This result implies that omitting competitor portfolios for bankrupt companies without financial data would not significantly change the original results. To test this implication, we stratify the 51 portfolios for which the bankruptcy has financial data in Compustat. While the CAARs are different from those reported earlier for all 76 portfolios, these differences are not meaningful. In almost all cases, the CAARs are not significantly different from zero. Thus, the general conclusions in the current paper do not change when competitor portfolios for bankruptcies with no financial data in Compustat are omitted from the study.

We use three separate debt screens to test for bias introduced when bankruptcy filers with low debt are screened from the sample. The three screens are Lang and Stulz's (1992) long-term debt greater than \$120 million, total debt greater than \$120 million, and total debt greater than 1 percent of total industry debt. The screened portfolios are stratified by the measures that Lang and Stulz use for concentration and leverage.

We do not find statistically significant evidence for either contagion or competitive effects among the screened portfolios. Furthermore, the results highlight the sensitivity of the tests to the debt screen applied. (See Table 6.) Lang and Stulz (1992) show significant evidence only for a contagion effect. Their CAAR for the low concentration/high leverage category is significant and negative. In the current study, when bankruptcy filers are screened by long-term debt greater than \$120 million, the CAAR based on equally weighted portfolio returns for the low concentration/high leverage category has a relatively small p-value (0.024) and is negative. When the other two screens are applied or when the CAAR is based on value-weighted returns, however, the marginal evidence of a contagion effect vanishes.

Table 6—CAARs Over 11-Day Event Window Centered on Filing Date for Industry Portfolios Stratified by Industry Concentration and Industry Leverage: Effects of Debt Screens on the Filing Firms

Each cell of each two-by-two table shows the CAAR for the category, z-statistic for the CAAR (square brackets), p-value for z-statistic (parentheses). The full sample of bankrupt publicly traded companies consists of 76 Chapter 11 filings in 1985–1993 (excluding depository institutions). For each bankruptcy we form portfolios of competitors with the same four-digit SIC code. We calculate abnormal returns for each portfolio using the subset of companies with daily returns in CRSP. Results are based on industry portfolio CPEs. Industry concentration is measured by the Herfindahl index. Industry leverage is measured by median long-term debt to total assets ratio. The number of bankruptcies passing each debt screen is listed in parentheses after the screen.

Panel A: Value-Weighted Returns on Industry Portfolios Corresponding to High Debt Bankruptcies

Bankruptcies with:	Long-Term Debt > \$120 Million (16)		Total Debt > \$120 Million (26)		Total Debt > 1% of Total Industry Debt (30)	
Concentration:	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
Below	1.20%	-0.01%	-4.98%	1.09%	-1.95%	0.45%
Median	[0.13]	[0.47]	[-3.54]	[1.11]	[-1.13]	[0.39]
Leverage	(0.90)	(0.64)	(0.0004)	(0.27)	(0.26)	(0.70)
Above	-0.07%	-2.98%	1.44%	-2.01%	2.18%	-1.37%
Median	[-0.48]	[-0.83]	[0.39]	[-0.71]	[0.89]	[-0.38]
Leverage	(0.63)	(0.41)	(0.70)	(0.48)	(0.37)	(0.71)

Panel B: Equally Weighted Returns on Industry Portfolios Corresponding to High Debt Bankruptcies

Bankruptcies with:	Long-Term Debt > \$120 Million (16)		Total Debt > \$120 Million (26)		Total Debt > 1% of Total Industry Debt (30)	
Concentration:	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
Below	1.00%	-2.47%	-2.24%	-0.36%	-2.40%	-0.71%
Median	[0.76]	[-1.65]	[-1.29]	[-0.56]	[-2.09]	[-0.29]
Leverage	(0.45)	(0.10)	(0.20)	(0.58)	(0.037)	(0.77)
Above	-3.38%	-1.70%	-2.26%	-1.45%	-1.78%	-0.23%
Median	[-2.25]	[-0.60]	[-1.94]	[-0.73]	[-1.20]	[-0.57]
Leverage	(0.024)	(0.55)	(0.053)	(0.46)	(0.23)	(0.57)

These results could be explained by lower sensitivity that competitors have to bankruptcy filings by smaller companies. Conrad, Gultekin, and Kaul (1991) find that volatility surprises at large-cap companies are important for predicting volatility at all companies, but volatility surprises at small-caps have no impact on mean or variance of returns at larger companies. Their results suggest that a bankruptcy filing by a large-cap might affect shareholder wealth at competitors, but filing by a small-cap would not. A company with a small absolute quantity of debt is more likely to be a small company by other measures, such as market cap or total assets, as well. Thus, Conrad, Gultekin, and Kaul's finding could explain why results in this paper differ from Lang and Stulz's (1992) results. Even when the analysis is restricted to bankruptcies with large absolute debt, however, the p-values for the CAARs are greater than 0.0127 and thus provide no support for Lang and Stulz's shareholder wealth effects.

Empirical Analysis Based on Weighted Least Squares

Lang and Stulz's (1992) four-category stratification is equivalent to a linear regression in which category membership is represented by dummy variables. That approach loses information about the magnitude of the industry concentration and leverage because the value of each variable is used only to determine whether it is above or below the median.

To address this weakness, we estimate the following model using weighted least squares:

$$CPE_j = \beta_0 + \beta_1 \cdot C_j + \beta_2 \cdot L_j + \beta_3 \cdot C_j \cdot L_j + \varepsilon_j, \quad (1)$$

where:

- CPE_j = The cumulative prediction error for industry portfolio j over the 11-day event window;
- C_j = The industry concentration for portfolio j; and
- L_j = The industry leverage for portfolio j.

Weighted least squares are appropriate because the market-model estimate of standard error for CPE differs widely among the industry portfolios.

Coefficient estimates generally are significant with value-weighted returns but not with equally weighted returns. (See Table 7.) These results are consistent with Conrad, Gultekin, and Kaul's (1991) finding that volatility surprises at large-cap stocks affect other companies, while volatility surprises at small-caps do not. Coefficient estimates generally are consistent in sign and magnitude across all four measures of industry leverage.

Change in value-weighted portfolio CPE with respect to change in industry concentration depends on the level of leverage. When leverage is measured as median long-term debt to total assets,

$$\partial CPE_j / \partial C_j = 0.2189 - 0.6907 \cdot L_j . \quad (2)$$

Table 7—Weighted Least Squares Regression of Industry Portfolio CPE on Industry Concentration and Leverage

Data are for 76 industry portfolios matched by four-digit SIC code to publicly traded bankruptcy filers in 1985–1993. We calculate abnormal returns for each portfolio using the subset of firms with daily returns in CRSP. Results are based on industry portfolio CPEs over an 11-day event window centered on the filing. Market-model abnormal returns follow the approach in Mikkelson and Partch (1985, 1988). Financial measures are based on all industry firms available in Compustat. Industry concentration is measured by the Herfindahl index. *t*-statistics are in parentheses below each coefficient estimate.

Characteristics of the Data Set Analyzed		Results from Weighted Least Squares Regression					
		Coefficient Estimates					
Industry Leverage	Weighting of Portfolio Returns	Constant Term	Industry Concentration	Industry Leverage	Interaction Term	p-value for F-statistic	Adjusted R-squared
Median Long-Term Debt to Total Assets	Value	-0.0370	0.2189	0.1065	-0.6907	0.027	0.09
		(-2.85)	(3.17)	(2.11)	(-2.57)		
	Equal	-0.0067	0.0291	-0.0219	-0.1045	0.53	-0.01
		(-0.56)	(0.45)	(-0.47)	(-0.43)		
Median Total Debt to Total Assets	Value	-0.0389	0.2456	0.0698	-0.4868	0.083	0.06
		(-2.21)	(2.39)	(1.55)	(-1.93)		
	Equal	0.0031	-0.0057	-0.0422	0.0227	0.51	-0.01
		(0.20)	(-0.06)	(-1.01)	(0.09)		
Median Total Liabilities to Total Assets	Value	-0.0881	0.3741	0.1311	-0.5417	0.025	0.09
		(-2.94)	(2.29)	(2.61)	(-2.01)		
	Equal	0.0212	-0.1115	-0.0527	0.1880	0.68	-0.02
		(0.76)	(-0.66)	(-1.15)	(0.70)		
Median Property, Plant, Equipment to Total Assets	Value	-0.0199	0.1293	0.0307	-0.2528	0.070	0.06
		(-2.41)	(2.88)	(1.66)	(-2.03)		
	Equal	-0.0219	0.0618	0.0299	-0.1577	0.44	-0.004
		(-2.56)	(1.19)	(1.72)	(-1.33)		

Portfolio CPE is higher for greater industry concentration when industry leverage is low ($L_j < 0.32$) but is lower for greater concentration when leverage is high ($L_j > 0.32$). That is, when leverage is low, a competitive effect appears at higher levels of industry concentration as predicted by Lang and Stulz (1992). When leverage is high, however, competitors are unable to take advantage of low competition.

Change in value-weighted portfolio CPE with respect to change in industry leverage depends on the level of concentration. For example, when leverage is measured as median long-term debt to total assets,

$$\partial CPE_j / \partial L_j = 0.1065 - 0.6907 \cdot C_j . \tag{3}$$

Portfolio CPE is higher for greater industry leverage when industry concentration is low ($C_j < 0.15$) but is lower for greater leverage when concentration is high ($C_j > 0.15$). This result does not support Lang and Stulz (1992) because it implies a competitive effect rather than a contagion effect under the low concentration/high leverage scenario.

Conclusions

Lang and Stulz's (1992) stratification test results support a contagion effect for industries with low concentration and high leverage. Replications of their stratification tests—using a sample of bankruptcy filings from a time period with only one legal regime—do not support either a contagion or a competitive effect. Stratification tests in the current paper are more robust than Lang and Stulz's tests: the sample includes more industries, and bankrupt companies are not required to have a minimum level of debt.[1] The debt screen appears to influence the test results because significance appears and disappears depending on the debt screen applied. Also, Lang and Stulz apparently overlooked the multiple comparison problem and thus overstate the significance of their test statistics.

Other analytical approaches produce results that are significant but not consistent with Lang and Stulz's (1992) conclusions. For example, when we replace industry portfolio CPE with portfolio ROE, we find significant evidence of a contagion effect among high leverage industries regardless of industry concentration—a broader effect than Lang and Stulz predict. On the other hand, the ROE analysis provides no evidence of a competitive effect under any circumstance.

Lang and Stulz's (1992) propositions are logically appealing. Lack of empirical support, however, implies that investors see the impact of bankruptcy news on competitors as minor or absorbed in subtle complex changes in the financial and real asset markets. One possible explanation for the current results is that the industry portfolios are sufficiently diversified to mask effects of differences in concentration

[1] We also repeat our analysis with different paired combinations of industry market-to-book, leverage, and concentration. Those results also are not significant. We thank an anonymous referee for this suggestion.

and leverage. If so, then research on more homogeneous industry subgroups or individual industry rivals might uncover the predicted contagion and competitive effects. For example, Cheng and McDonald (1996) examine the airline and railroad industries and find significant intra-industry bankruptcy announcement effects. The opposite signs of the two industries' responses lead them to propose that market structure determines whether a contagion or competitive effect occurs. Another possible explanation for the current results is that industry-to-industry differences in concentration and leverage are secondary to other factors, such as business risk, that affect competitors' market values. For example, Laux, Starks, and Yoon (1998) find that the ratio of a rival's market value to its replacement value (Tobin's q) is an important factor in determining whether the firm has a positive or negative share price response to a competitor's dividend revision announcement. Thus, examination of the role of other business risk measures would be appropriate in future research on contagion and competitive effects due to bankruptcy announcements.

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