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Common Stock Returns and Rating Changes: A Methodological Comparison

PAUL A. GRIFFIN and ANTONIO Z. SANVICENTE*

ABSTRACT

This paper examines the adjustments in a firm's common stock price during the eleven months before and during the month of announcement of a bond rating change. Based on several different measures of abnormal security return, the findings are consistent with the proposition that bond downgradings convey information to common stockholders. For bond upgradings, the price adjustments were statistically insignificant in the month of announcement, although in the eleven preceding months, upgraded firms exhibited positive abnormal returns. While the results do not fully support earlier research, we stress that the main contribution of this article lies in the scrutiny it gives to issues of methodology in assessing the possible price effects of bond reclassifications.

SEVERAL RECENT STUDIES HAVE investigated the effects of changes in company debt classifications by the major bond rating agencies.[1] These include four studies of the impact of a rating change on bond prices (Katz [19], Hettenhouse and Sartoris [14], Grier and Katz [12], and Weinstein [25]) and one study of the impact on common stock prices (Pinches and Singleton [22]). The findings of the bond price studies differ widely. Thus, while Katz, and Grier and Katz, suggest that trading rules based on the (announced) downgradings of industrial bonds may be moderately profitable, Hettenhouse and Sartoris, and Weinstein, analyzing bond yields and returns, respectively, find that there is no apparent opportunity to profit abnormally subsequent to the announcement of the change. Weinstein attributes these differences in results largely to issues of methodology, and proceeds to investigate the effects of rating changes on bond prices using more refined risk-adjustment techniques and portfolio estimation procedures.

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[1] Extensive discussion on the significance of bond ratings may be found in Pogue and Soldofsky [23], West [26], and Ross [24]. Bond ratings were "institutionalized" as part of the Banking Act of 1936 which uses the rating as one criterion for the acceptance of bond investments. Attempts to entrench further the bond rating have used the Securities and Exchange Commission's rule-making authority. In November 1977, for example, the SEC proposed that public companies tell investors what ratings their securities had been given by the agencies. [*The Wall Street Journal*, 7 November 1977].

This paper examines the adjustments in the common stock price of firms whose bonds are reclassified. The emphasis is on price changes in the eleven months preceding the announcement and during the month of announcement itself.[2] For reasons that are felt to be mostly methodological, the findings presented here do not fully support the conclusion of Pinches and Singleton, namely, that their "study provides strong support for the proposition that the information content of bond rating changes is *very small*" (emphasis added) [Pinches and Singleton, p. 42]. The Pinches and Singleton methodology is without a formal test of significance of their conclusion concerning "information content" and may be questioned for inadequately controlling for information and events known to be correlated with rating agencies' assessments of bond investment quality.[3] They use the "conventional" residuals approach, introduced by Ball and Brown [2] and Fama, Fisher, Jensen, and Roll [10]. Consequently, many developments in the methodology pertinent to their investigation that have been advanced in the past several years are ignored.

The event in this study—the announcement of a rating reclassification—provides an opportunity to compare approaches based on security residual returns with more refined procedures. The findings should be also relevant to the broad class of informational content studies contained in the accounting and finance literatures in the past decade.[4] Three approaches for measuring abnormal security price adjustments (i.e., abnormal return) are evaluated in this study. The first is to derive security residual returns from the one-factor market model. The second is to derive residual returns from the two-factor cross-sectional model described in Black [5] and Fama and MacBeth [9]. The third approach explicitly controls

[2] Knowledge of the nature and extent of price adjustment prior to reclassification would seem important for analysts, and others with information processing skills, who seek to identify and then purge (acquire) soon to be downgraded (upgraded) assets from (for) their (clients') portfolios based on models which attempt to replicate agency judgments. It is generally acknowledged that changes in bond ratings lag changes in the variables determining the ratings. Analysts' "second guessing," therefore, could be a profitable activity if price adjustments take place when the rating change is announced. Several firms (e.g., Paine, Webber, Jackson & Curtis, Inc.) provide services of this kind. [*The Wall Street Journal*, 26 October 1976 and 7 April 1980].

[3] It is impossible to isolate precisely the marginal informational content of a bond rating reclassification since, to a degree, the rating itself is an agency's reflection of already publicly available information. The same kinds of problems beset most earnings announcement studies due, for example, to the jointness of earnings, dividends, and analyst forecast signals. But a control sample, matched not only in terms of relative risk, but also in terms of financial variables that are, apparently, determinants of bond ratings and rating reclassifications, should exclude many specific informational effects that are external to the rating change *per se*.

[4] To the extent that this study can be viewed as an attempt to assess "informational effects," it should be recognized that there are those who would question whether a rating agency's purpose is to provide new information about outstanding bonds (Weinstein [25]). Reasons may be advanced, however, regarding why a rating change, and indeed the entire rating process, might convey nonpublic information to all classes of security-holders. One need only posit that managers have incentives (due to contractual arrangements, e.g., compensation) to obtain favorable and least-costly ratings for the firm. In their quest, managers may reveal internal accounting data and other statements about production, investment, and financing plans. To the extent that these data provide the agency with nonpublic information about the firm's future cash flows, the rating process becomes a vehicle for communication of private knowledge to investors and creditors. Examples of agency-manager discussions are provided in Ross [24]. Backer and Gosman [1] underscore the special role of the investment banker in those discussions.

for nonevent or extraneous factors by exploiting the properties of the two-factor model of equilibrium returns and constructing a control portfolio in which each asset is matched to an asset in the event (i.e., experimental) portfolio.[5]

The first section states hypotheses concerning the effects of rating changes on common stock prices under the three approaches. Data sources and sample selection criteria are described in the second section. The third section details the statistical methodology and portfolio test procedures. Empirical results are outlined in the fourth section, and tentative conclusions are presented in the final section.

I. Hypotheses

This section states propositions regarding abnormal price adjustments to rating changes using measures of that adjustment based on one- and two-factor models, and a control firm approach. First, the null and alternative hypotheses about the effects of a rating change for firm i during time t on one-factor residual returns $\tilde{\epsilon}_{it}$ involve a comparison of the conditional and unconditional means $E(\tilde{\epsilon}_{it} \mid \tilde{\theta}_{it})$ and $E(\tilde{\epsilon}_{it})$, respectively, where $\tilde{\theta}_{it} = \theta^1_{it}$ denotes upgrading and $\tilde{\theta}_{it} = \theta^2_{it}$ denotes downgrading.

$$
\begin{aligned}
H_0\colon\ & E(\tilde{\epsilon}_{it} \mid \tilde{\theta}_{it}) - E(\tilde{\epsilon}_{it}) = 0 \quad \text{for} \quad \tilde{\theta}_{it} = \theta^1_{it} \text{ or } \theta^2_{it} \\
H_1\colon\ & E(\tilde{\epsilon}_{it} \mid \tilde{\theta}_{it}) - E(\tilde{\epsilon}_{it}) > 0 \quad \text{for} \quad \tilde{\theta}_{it} = \tilde{\theta}^1_{it} \text{ and} \\
& E(\tilde{\epsilon}_{it} \mid \tilde{\theta}_{it}) - E(\tilde{\epsilon}_{it}) < 0 \quad \text{for} \quad \tilde{\theta}_{it} = \theta^2_{it}
\end{aligned}
$$

Second, for the two-factor model H_0 and H_1 are identical to those for the one-factor model, except that $\tilde{\eta}_{it}$ replaces $\tilde{\epsilon}_{it}$.[6]

The third approach estimates the abnormal price adjustment for a given firm in a portfolio as the difference between the actual return and the return on a matched control firm. The test of hypothesis therefore involves a comparison of the common stock returns for event firm i and control firm j whose unconditional expected returns are equal. Control firms are selected so that industry (I_{jt}),

[5] Brown and Warner [7] also examine various methodologies comparing measures of abnormal performance, but using simulation techniques. While they include market model residuals, two-factor model residuals, and residuals derived from control portfolios in their analysis, a matching procedure of the kind utilized in this study (based on risk *and* other financial variables) is not addressed. Nonetheless, their results should provide an interesting benchmark against which to judge the present findings. Other studies comparing various abnormal performance methodologies include Brenner [6] and Langetieg [20]. Brenner compares alternative forms of the market model. Langetieg, on the other hand, evaluates a return-difference approach as well as more standard metrics.

[6] When the market model is used to construct residuals, $\tilde{R}_{it}$ (the return on common stock for firm i) and $\tilde{R}_{mt}$ (the return on the market portfolio) are assumed bivariate normal such that $E(\tilde{R}_{it} \mid R_{mt}) = \alpha_{it} + \beta_{it} R_{mt}$ where $\beta_{it} = \text{cov}(\tilde{R}_{it}, \tilde{R}_{mt})/\text{var}(\tilde{R}_{mt})$ and $\alpha_{it} = E(\tilde{R}_{it}) - \beta_{it} E(\tilde{R}_{mt})$. The market model residual is derived as $\tilde{\epsilon}_{it} = \tilde{R}_{it} - E(\tilde{R}_{it} \mid R_{mt})$, where $E(\tilde{\epsilon}_{it}) = 0$ and, by construction, $\text{cov}(\tilde{R}_{mt}, \tilde{\epsilon}_{it}) = 0$. The two-factor model requires that the joint distribution of $\tilde{R}_{it}$, $\tilde{R}_{mt}$, and $\tilde{R}_{zt}$ be trivariate normal, where z is a minimum variance portfolio whose return is uncorrelated with the market portfolio and all securities are priced in accordance with a two-factor model of equilibrium. The residual is $\tilde{\eta}_{it} = \tilde{R}_{it} - \tilde{R}_{zt} - \beta_{it}(\tilde{R}_{mt} - \tilde{R}_{zt})$, where $E(\tilde{\eta}_{it}) = 0$ and $\text{cov}(\tilde{R}_{zt}, \tilde{\eta}_{it}) = \text{cov}(\tilde{R}_{mt}, \tilde{\eta}_{it}) = 0$.

relative risk (β_{jt}), and other descriptors relevant to a firm's return generating process (ω_{jkt}, $k = 1, \cdots, K$) are the same as their event firm counterparts—I_{it}, β_{it}, and ω_{ikt}.[7]

In choosing among the three measures of abnormal returns, a primary issue is their variability, since, other things equal, the measure with the smaller variance will generate a more powerful test of the hypothesis. With "error-free" values of the relevant parameters and variables, it is straightforward to show, for β_{it} and $\beta_{jt} \neq 1$, that $\text{var}(\tilde{\eta}_{it} - \tilde{\eta}_{jt}) < \text{var}(\tilde{\epsilon}_{it} - \tilde{\epsilon}_{jt})$. (See Fama [8].) In other words, the two-factor model should yield more powerful tests than the single factor model. Whether, empirically, the control firm approach (i.e., the test based on return differences) yields a lower variance of abnormal return than either the one-factor or the two-factor model approach depends on the extent to which "common" variance factors are extracted by computing return differences and on the consequences of measurement error. Unfortunately, the relative impacts of these effects are too complex to be evaluated adequately by the present methodology. (See Beaver [3] for a detailed discussion.) Those who have favored a return difference approach (e.g., Gonedes [11], Harrison [13], and Beaver, Christie, and Griffin [4] claim that it obviates certain effects of measurement error, most notably error in estimation of the intercept term α_{it}, if the market model is posited, and $(1 - \beta_{it})E(\tilde{R}_{zt})$, if the two-factor model is posited as the underlying process generating returns. Accordingly, one might anticipate that conclusions regarding the statistical dependency between abnormal security returns and the information conveyed by rating reclassifications will depend on the approach used by the researcher.

However, even the use of several different abnormal return metrics cannot rule out the likelihood that common stock prices are adjusting to information which is correlated with the rating change process, but is not produced by the announcement itself. It is possible that firms with upgraded bonds are simply those that have been outperforming the market. Likewise, downgraded firms may be those that have been performing badly. Still, the use of a "well-matched" control sample is an attempt to control such correlated information. Increased confidence in the methodology may also be obtained by an analysis of the abnormal return metrics for samples which did *not* experience bond upgradings or downgradings.

[7] The null and alternative hypotheses examined in this third approach may be (formally) written as a comparison of returns for an event firm i and a control firm j. That is:

$$H_0: E[(\tilde{R}_{it} \mid \tilde{\theta}_{it}, \beta_{it}, \omega_{it}, I_{it}) - (\tilde{R}_{jt} \mid \beta_{jt}, \omega_{jt}, I_{jt})] = 0$$
$$\text{for } \tilde{\theta}_{it} = \theta^1_{it} \text{ or } \theta^2_{it}$$
$$H_1: E[(\tilde{R}_{it} \mid \tilde{\theta}_{it}, \beta_{it}, \omega_{it}, \mathrm{I}_{it}) - (\tilde{\mathrm{R}}_{jt} \mid \beta_{jt}, \omega_{jt}, I_{jt})] > 0$$
$$\text{for } \tilde{\theta}_{it} = \theta^1_{it} \text{ and}$$
$$\mathrm{E}[(\tilde{\mathrm{R}}_{it} \mid \tilde{\theta}_{it}, \beta_{it}, \omega_{it}, \mathrm{I}_{it}) - (\tilde{\mathrm{R}}_{jt} \mid \beta_{jt}, \omega_{jt}, I_{jt})] < 0$$
$$\text{for } \tilde{\theta}_{it} = \theta^2_{it}$$
$$\text{where } \beta_{it} = \beta_{jt}, \omega_{it} = \omega_{jt}, \quad \text{and} \quad I_{it} = I_{jt}$$

II. Data and Sample Selection

A complete list of 732 announcements of bond rating changes for the period 1960–1975 was obtained from Moody's *Bond Survey* and Standard & Poor's *Bond Guide.* 503 changes were deleted from the sample because of the lack of return data on the CRSP tape (1977 version) for the period from 83 months before to 12 months after the date of reclassification.[8] Forty-nine other reclassifications were deleted because of lack of data on the annual industrial Compustat tape. Thus, the final sample consisted of 180 bond reclassifications.[9]

Table I summarizes the final sample. Duplications are defined to be rating changes of the same direction that occurred within six months of each other. They were grouped as (a) same month, (b) Moody's leads Standard & Poor's, or (c) Moody's lags Standard & Poor's.

Unconditional (marginal) distributions of returns were assessed using a control sample. The initial sample was selected by matching firms whose bonds were reclassified on the basis of relative risk, β_{it}, industry, I_{it}, and four financial variables comprising fiscal year-end, average payout, average asset size, and average debt to equity, which we denote ω_{ikt}, $k = 1, \cdots, 4$, respectively.[10] Since matching was done at the firm level, it also held for all portfolios formed by partitioning on realizations of the signal $\tilde{\theta}_{it} = \theta^1_{it}$ or θ^2_{it}. Relative risk for security i at month of reclassification (month zero) was estimated from the ordinary least squares regression $R_{it} = \hat{\alpha}_{i0} + \hat{\beta}_{i0}R_{mt} + \hat{\epsilon}_{it}$ for $t = -71, \cdots, 12$, where R_{mt} is defined as the CRSP tape's value-weighted index of dividend reinvested returns.[11]

The matching steps were as follows. Firms for possible matching purposes were selected from all firms, with the same four-digit standard industrial classification (SIC) code, that also had data on the Compustat and CRSP tapes. Firms in the original event sample were excluded. We next computed relative risk for these matching firms defining month zero as the month of reclassification for the firm in the event sample with which the matching firm might be compared. Then, for each firm in the event sample, we chose an equivalent fiscal year-end firm from the set of matching firms obtained from the preceding steps, provided that (a) estimated relative risk deviated by no more than 0.50 from the event sample firm's risk estimate; (b) asset size and debt to equity deviated by no more than 50 percent; and (c) payout was neither negative nor zero. Finally, if a control firm

[8] The earliest and latest return months were February 1953 and July 1975. All firms were listed on the New York Stock Exchange.

[9] Subsequent analysis, using the 1978 version of the CRSP tape, resulted in the loss of three more firms. The portfolio tests, for example, are based on a maximum of 177 firms.

[10] ω_{i2t}, ω_{i3t}, and ω_{i4t} are defined, respectively, as average payout: $\mathrm{PAY}_T = (1/3) \sum_{T=-2}^{0} (\mathrm{DATA}_{21,T}/\mathrm{DATA}_{20,T})$; average asset size, $\mathrm{TOTA}_T = (1/3) \sum_{T=-2}^{0} \ln(\mathrm{DATA}_{6,T})$; and average debt to equity, $\mathrm{DTEQ}_T = (1/3) \sum_{T=-2}^{0} (DATA_{9,T}/\mathrm{DATA}_{11,T})$, where $T = 0$ refers to the fiscal year in which the reclassification took place and the subscripts on DATA refer to the Compustat data item codes. For example, in the case of a reclassification as of (say) March 1970, the payout variable averaged the 1968, 1969, and 1970 fiscal year-end figures which, by Compustat convention, refer to end-of-year calendar dates between June 1970 and May 1971.

[11] From this point on in the paper, subscript t refers to a month *relative* to event date "0" and not to calendar time generally.

Table I

Data and Sample Selection

Total number of bond rating reclassifications					732
Less the union of					
Reclassifications deleted due to Compustat criterion			415		
Reclassifications deleted due to CRSP criterion			503		552
Number of reclassifications in final sample[a, b]					180
Broken down by					
Agency:	Standard and Poor's	99	Moody's	81	180
$\tilde{\theta}_{it}$:	Downgradings	94	Upgradings	86	180
Duplication:	Multiple	32	Single	148	
	Same month	10			
	Moody's first	18			
	Standard and Poor's first	4			180
Agency and $\tilde{\theta}_{it}$:	S & P: Downgradings	53	M: Downgradings	41	
	S & P Upgradings	46	M: Upgradings	40	180

[a] The final sample size was reduced to 128 reclassifications, comprising 63 downgradings and 65 upgradings, when the Fama and MacBeth [9] two-factor model estimates were used.

[b] The sample size was reduced to 177 reclassifications, comprising 92 downgradings and 85 upgradings, when the 1978 version of the monthly CRSP file was used.

could not be found using the four-digit SIC code, the entire process was repeated using the first three digits of the SIC code. A repetition using the first two digits was unnecessary.

III. Methodology

Measurement of Security Performance

Residuals for firm i from the one-factor model were computed as $\hat{\epsilon}_{it} = R_{it} - \hat{\alpha}_{it} - \hat{\beta}_{it} R_{mt}$ with estimates $\hat{\alpha}_{it}$ and $\hat{\beta}_{it}$ derived from the regression

$$R_{it'} = \hat{\alpha}_{it} + \hat{\beta}_{it} R_{mt'} + \hat{\epsilon}_{it'} \quad \text{where} \quad t' = t - 48, \cdots, t - 1 \quad \text{and} \quad t = -35, \cdots, 12$$

For each period $t' = t - 48, \cdots, t - 1$, $E(\hat{\epsilon}_{it'}) = 0$ by the linear regression constraint. Residuals from the two-factor cross-sectional model were computed as $\hat{\eta}_{it} = R_{it} - \hat{\gamma}_{1t} - \hat{\gamma}_{2t} \hat{\beta}_{it}$ using $\hat{\beta}_{it}$ from the equation above. $\hat{\gamma}_{1t}$ and $\hat{\gamma}_{2t}$ are the Fama and MacBeth least squares estimates of the ex post relationship between risk and return with the properties that $E(\hat{\gamma}_{1t}) = E(\tilde{R}_{zt})$ and $E(\hat{\gamma}_{2t}) = E(\tilde{R}_{mt}) - E(\tilde{R}_{zt})$. The procedure used to obtain the two-factor residuals was similar to that used by Mandelker [21] except that β_{it} was estimated using the last 48 months up to month t and applying a value-weighted index of market returns (see footnote 17). Average residuals were therefore defined as

$$\bar{\epsilon}_{it} = 1/N(\textstyle\sum_{i=1}^{N} \hat{\epsilon}_{it}) \quad \text{for the one-factor model}$$

and

$$\bar{\eta}_{it} = 1/N(\textstyle\sum_{i=1}^{N} \hat{\eta}_{it}) \quad \text{for the two-factor model,}$$

where N is the number of firms that have residuals in month t, $t = -35, \cdots, 12$. Average returns were similarly defined as

$$\bar{R}_{it} = 1/N(\textstyle\sum_{i=1}^{N} R_{it})$$

Testing for Significance: The Portfolio Method

The portfolio method of testing for significance of the various security performance measures followed the procedures used by Jaffe [16], Mandelker [21], Weinstein [25], and Hong, Kaplan, and Mandelker [15]. Calendar-month portfolios were formed by including the securities of all firms with reclassifications between months $t = L_1$ and $t = L_2$. In this study, $L_1 = -1$ and $L_2 = 1$. Separate portfolios were constructed for firms with upgraded and downgraded bonds. Several calendar months, of course, contained only one security.[12]

We then computed the calendar-month portfolio return R_{pt} as an equally-weighted average of the returns of firms in portfolio p. Similarly, $\hat{\epsilon}_{pt}$ and $\hat{\eta}_{pt}$ were computed as equally-weighted averages of the residuals in that portfolio. ($t = 0$ is the month in which an event takes place.) Note that the portfolio residuals were always based on regression parameters estimated from 48 preceding monthly return observations. Equivalent portfolio residual and portfolio return measures were also computed for the control sample.

Denoting R_{qt}, $\hat{\epsilon}_{qt}$, and $\hat{\eta}_{qt}$ as control sample portfolio returns, market-model residuals, and two-factor residuals, respectively, the variables

$$(R_{pt} - R_{qt}),\ (\hat{\epsilon}_{pt} - \hat{\epsilon}_{qt}), \quad \text{and} \quad (\hat{\eta}_{pt} - \hat{\eta}_{qt})$$

then define the differences between the conditional and unconditional (marginal) distributions. The portfolio test statistic examines whether the expected value of these differences is greater or less than zero, depending on whether the conditioning agent $\tilde{\theta}_{it}$ is a bond upgrading or downgrading. Let $\hat{u}_{pt}$ be that portfolio return difference or portfolio residual difference.

The portfolio test statistic was derived according to the following procedure:

1. To standardize $\hat{u}_{pt}$, we calculated measures of the standard deviation of $\hat{u}_{pt}$ as

$$\hat{s}_K(u_p) = \sqrt{(1/K - 1) \textstyle\sum_t [\hat{u}_{pt} - (1/K) \sum_t \hat{u}_{pt}]^2}$$

$$\text{where } K = 24 \quad \text{for} \quad t = -35, \cdots, -12,$$

$$\text{and} \quad K = 36 \quad \text{for} \quad t = -35, \cdots, -12, 1, \cdots, 12.$$

The standardized portfolio residual or return difference is thus denoted:

$$u_{pt}^*(K) = \hat{u}_{pt}/\hat{s}_K(u_p)$$

Under the null hypothesis, $E[u_{pt}^*(K)] = 0$ and the individual $u_{1t}^*(K)$, $u_{2t}^*(K)$, etc., represent independent drawings from a t distribution with K degrees of freedom.

2. Average standardized residual or return differences, u_t^{**}, were then computed. The mean of $u_{pt}^*(K)$ is denoted as $\bar{u}_t^*(K) = (1/M) \sum_{p=1}^{M} u_{pt}^*(K)$, where M is the

[12] Also, in forming the portfolios, duplicate changes are ignored. Only the first announcement in calendar time is placed in the portfolio.

number of portfolios (M less than N). The following statistic u_t^{**} behaves as a t statistic with $(K - 1)M$ degrees of freedom.

$$u_t^{**} \times \bar{u}_t^{*}(K)/(1/\sqrt{M})$$

Since $(K - 1)M$ is large, a standard normal distribution can be used to approximate the distribution of u_t^{**}.

3. The final step was to calculate the cumulative portfolio t statistic, U^{**}. For the $\tau + 1$ month period $t = -\tau, \cdots, 0$, the t statistic was computed as

$$U^{**} = (1/\sqrt{\tau + 1})\,[\textstyle\sum_{t=-\tau}^{0} u_t^{**}]$$

To summarize, portfolio t statistics and cumulative portfolio t statistics were generated for bond rating upgradings and bond rating downgradings for 11 months prior to and including the event month. Portfolio standard deviations were calculated over 24 and 36 observations. The test statistics should indicate whether the abnormal returns in the months preceding and during the month of the bond reclassification are statistically different from zero, and hence whether such reclassifications might convey new information to market participants.

The abnormal return metrics based on return differences or residual return differences, however, may not result in equally powerful tests for the reasons described in the second section. To examine further this aspect of the research design, abnormal return measures and portfolio t statistics were generated for calendar-month portfolios constructed so that *no* firm included experienced a reclassification event in that month. This was accomplished by comparing the original control portfolios with a set of "hypothetical event" portfolios constructed by using the same matching procedures described earlier. The same calendar months were used to construct the abnormal return measures, except that such dates, of course, were purely fictitious. No reclassification event occurs on those dates for the "hypothetical event" firms. It is expected that the expected value of the portfolio test statistic should be zero for all abnormal return measures, that is, the null hypothesis should not be rejected.

IV. Results

Preliminaries

The control portfolios used in this study were constructed by matching on beta, industry, and key financial variables, each potentially relevant to a measure of relative risk and, hence, portfolio expected return. With the exception of year-end, these key variables include payout, leverage, and size. By virtue of the matching procedures, differences in the mean values for these variables between the event and control portfolios should be zero. We also examined several other data items not employed in the matching process, but which capture additional financial dimensions reported as determinants of a firm's rating in the literature on the replication of agency judgments.[13] (For recent evidence and a summary of relevant work, see Kaplan and Urwitz [18].)

[13] The variables are defined on a fiscal year-end (T) basis as:

Average asset growth, $\text{AGTH}_T = (1/3) \sum_{T=-2}^{0} 1/3[\ln(\text{DATA}_{6,T}/\text{DATA}_{6,T-2})]$

Table II

Financial Profile of Event (E) and Control (C) Samples: Univariate Tests of Differences—*t* Test and Mann-Whitney *U* test

			Levels			Change[b]		
Dimension[a]	Sample	Number of Firms	Mean	*t* Test 2-Tail Prob.	M-W *U* Test 2-Tail Prob.	Mean	*t* Test 2-Tail Prob.	M-W *U* Test 2-Tail Prob.
DTEQ	E	155	0.869	0.346	0.352	0.230	0.249	0.212
	C	155	0.779			0.064		
TOTA	E	179	6.255	0.133	0.270	0.141	0.840	0.660
	C	178	6.077			0.138		
CACL	E	172	2.197	0.351	0.972	−0.108	0.091	0.154
	C	171	2.308			−0.019		
REEQ	E	155	0.107	0.052	0.166	0.004	0.179	0.523
	C	155	0.076			−0.044		
EASA	E	176	0.152	0.462	0.516	−0.006	0.264	0.881
	C	177	0.161			−0.001		
INTC	E	175	10.781	0.039	0.942	−0.220	0.176	0.938
	C	168	31.365			−12.055		
PAYT	E	179	0.513	0.807	0.538	0.188	0.780	0.791
	C	178	0.504			0.134		
AGTH	E	179	0.182	0.874	0.683	0.106	0.254	0.185
	C	178	0.178			0.089		
BETA	E	180	1.055	0.930	0.867	—	—	—
	C	180	1.051			—		

[a] Details of the calculations, including BETA, are given in the text on pp. 7, 28, and 29.

[b] The change in a variable is defined as the difference between the data item reported at the fiscal end of the year in which the reclassification took place and the average of the two preceding years.

Table II compares the mean values for the matching variables and the above additional variables, first, in terms of the items themselves and, second, in terms of the change between the item (reported at the end of the fiscal year in which the reclassification took place) and an average of the two preceding years. It is evident from the Mann-Whitney (and t) probability values that, for the variables identified, the event and control samples are not significantly different at less than a five percent level of probability. Linear discriminant analysis, using all nine variables, was also conducted as a further check on the similarity of the financial profiles. For neither levels nor changes, were the discriminant functions able to distinguish significantly between the two samples. The models classify only 56.7 percent (levels) and 58.0 percent (changes) of the combined sample.

Average liquidity, $\mathrm{CACL}_T = (1/3) \sum_{T=-2}^{0} (\mathrm{DATA}_{4,T}/\mathrm{DATA}_{5,T})$

Average return on common equity, $\mathrm{REEQ}_T = (1/3) \sum_{T=-2}^{0} (\mathrm{DATA}_{20,T}/\mathrm{DATA}_{11,T})$

Average earnings before taxes and interest to sales revenue, and

$$\mathrm{EASA}_T = (1/3) \sum_{T=-2}^{0} [(\mathrm{DATA}_{18,T} + \mathrm{DATA}_{15,T} + \mathrm{DATA}_{16,T})/\mathrm{DATA}_{12,T}]$$

Average earnings before taxes and interest to interest,

$$\mathrm{INTC}_T = (1/3) \sum_{T=-2}^{0} [(\mathrm{DATA}_{18,T} + \mathrm{DATA}_{15,T} + \mathrm{DATA}_{16,T})/\mathrm{DATA}_{15,T}].$$

Table III

Portfolio Betas

Period	Downgrading Event Sample	(No.)	Downgrading Control Sample	(No.)	Upgrading Event Sample	(No.)	Upgrading Control Sample	(No.)
A: 1960-1975								
All Firms[a]	1.106	(92)	1.112	(92)	1.003	(85)	0.985	(85)
Exclude Duplications	1.131	(83)	1.136	(83)	1.029	(78)	1.005	(78)
Portfolios	1.104	(68)	1.103	(68)	1.049	(65)	1.031	(65)
B: 1960-1971								
All Firms	0.940	(63)	0.986	(63)	0.929	(65)	0.928	(65)
Exclude Duplications	0.951	(57)	0.998	(57)	0.948	(59)	0.947	(59)
Portfolios	0.919	(46)	0.968	(46)	0.969	(49)	0.968	(49)

[a] The downgrading and upgrading samples were reduced by two and one observations, respectively, when the 1978 version of the monthly CRSP file was used. The number of firms is shown in parentheses.

The event and control samples, therefore, seem reasonably homogeneous in terms of certain specified determinants of bond ratings.[14]

A related set of comparisons is presented in Table III for average beta for the various sample partitions. It is apparent from this table that the betas for the control and event portfolios are essentially equal and close to one.[15]

Table IV details a third set of comparisons. Here the statistics examine differences in the samples partitioned by upgrading vs. downgrading. Control firms are partitioned with reference to their event sample counterparts. The differences reported are consistent with the prior literature. Concentrating on changes in the variables, for example, the Mann-Whitney probability values suggest that, relative to upgraded firms, leverage (DTEQ) increased, profitability (REEQ and EASA) deteriorated, and interest coverage (INTC) dropped significantly for downgraded firms. Moreover, linear discriminant functions were able to distinguish significantly between the upgraded and downgraded firms. Applied to the event sample, the functions were able to classify correctly 67.8 percent and 74.7 percent of the reclassifications into their upgrading and downgrading categories. Hence, the profiles for upgraded and downgraded firms are not alike. Such differences, possibly reflecting changed production, investment, and financial conditions, should account for a part of the security return behavior that precedes a rating change. Indeed, the matched portfolios, based on historical beta and financial profiles, are designed to control explicitly for this anticipatory price movement.

[14] The control sample was also compared to the second "hypothetical event" sample—with similar results. Only 2 out of 18 Mann-Whitney U statistics were significant at less than a five percent level of probability. These were for the AGTH (levels) and TOTA (changes) variables.

[15] Pairwise differences (event firm minus control firm) in beta were also calculated. They are shown below in distributional form.

Decile	.10	.20	.30	.40	.50	.60	.70	.80	.90
Difference in BETA	−.31	−.17	−.13	−.06	.00	.03	.06	.13	.29

Table IV

Univariate Tests of Differences between Firms in Combined Sample Classified by Downgradings (D) and Upgradings (U)—*t* Test and Mann-Whitney *U* Test

			Levels			Change[b]		
Dimension[a]	Sample	Number of Firms	Mean	*t* Test 2-Tail Prob.	M-W *U* Test 2-Tail Prob.	Mean	*t* Test 2-Tail Prob.	M-W *U* Test 2-Tail Prob.
DTEQ	D	166	0.852	0.533	0.687	0.295	0.027[b]	0.000[d]
	U	144	0.792			−0.023		
TOTA	D	188	6.300	0.017	0.004[c]	0.142	0.772	0.651
	U	169	6.018			0.137		
CACL	D	182	2.291	0.485	0.252	−0.041	0.366	0.904
	U	161	2.208			−0.089		
REEQ	D	166	0.068	0.001	0.000[c]	−0.044	0.142	0.001[c]
	U	144	0.119			0.008		
EASA	D	185	0.134	0.000	0.000[c]	−0.011	0.001	0.000[c]
	U	168	0.181			0.004		
INTC	D	183	16.184	0.316	0.006[c]	−3.441	0.529	0.000[c]
	U	160	26.214			−8.963		
PAYT	D	188	0.538	0.100	0.004[c]	0.311	0.106	0.276
	U	169	0.475			−0.005		
AGTH	D	188	0.261	0.543	0.837	0.095	0.717	0.218
	U	169	0.145			0.100		
BETA	D	188	0.447	0.019	0.051	—	—	—
	U	172	0.406			—		

[a] Details of the calculations, including BETA, are given on pp. 7, 28, and 29.

[b] The change in a variable is defined as the difference between the data item reported at the fiscal end of year in which the reclassification took place and the average of the two preceding years.

[c] Average rank for U sample greater than average rank for D sample.

[d] Average rank for D sample greater than average rank for U sample.

Residual and Return Differences

The null hypothesis tested in this study is that, for a given measure of abnormal security return, there is no difference between the expected value of that measure conditional on signals $\tilde{\theta}_{it} = \theta^1_{it}$ or θ^2_{it} and a measure of (unconditional) expected value assessed using an otherwise equivalent control portfolio. The initial results are summarized in Table V which presents (1) mean cumulative residual/return differences; (2) cumulative portfolio t statistics U^{**}; and (3) average standard deviations $\bar{s}$ for the individual $\hat{s}_K(u_p)$ where $K = 24$ and 36 are the number of months used in estimating $\hat{s}_K(u_p)$. It is clear from this table that the mean cumulative residual/return differences are negative for downgradings and positive for upgradings. For example, during the 12 months prior to reclassification, the returns of downgraded firms in 1960–1975 declined by 5.89 percent on average relative to an otherwise equivalent control sample. Similarly, the returns of upgraded firms outperformed a control group by 3.24 percent in that same period. But are such results significant?

The statistical significance of these results would appear to depend on whether the researcher employs market-model residuals instead of residuals from the two-

Table V

Portfolio t Statistics: Differences in Cumulative Residuals/Returns between Event and Control Samples

Common Stock	Downgrading					Upgrading				
Price Adjustment	Mean[a]	$U^{**}(24)$[b]	$\bar{s}(24)$	$U^{**}(36)$	$\bar{s}(36)$	Mean	$U^{**}(24)$	$\bar{s}(24)$	$U^{**}(36)$	$\bar{s}(36)$
A: 1960-1975										
Market Model Residuals	−0.1129	−1.088	.0303	−1.286	.0329	0.0322	0.298	.0311	0.531	.0317
Security Returns	−0.0589	−2.914	.0303	−2.987	.0324	0.0295	1.464	.0309	1.574	.0313
Number of Portfolios	68	68		68		65	65		65	
B: 1960-1971										
Market Model Residuals	−0.1248	−1.163	.0258	−0.357	.0273	0.0553	1.121	.0286	1.048	.0284
Two-Factor Model Residuals	−0.1656	−1.598	.0258	−1.772	.0273	0.1383	2.128	.0279	2.084	.0278
Security Returns	−0.0817	−1.641	.0254	−1.797	.0269	0.1285	2.084	.0279	2.037	.0278
Number of Portfolios	46	46		46		49	49		49	

[a] Mean cumulative residual/return difference, cumulated over months −11, ⋯, 0, i.e., $\sum_{t=-11}^{0}(1/M)\sum_{p=1}^{M}\hat{u}_{pt}$ where $\hat{u}_{pt}$ is defined in the text on p. 10 and M is the number of portfolios.

[b] Cumulative Portfolio t Statistic, cumulated over months −11, ⋯, 0. Details of the calculations for U^{**} and $\bar{s}$ are given in the text on pp. 10 and 11. The parenthesized numbers (24) and (36) indicate the two ways by which portfolio standard deviations were computed: over 24 and 36 observations, respectively.

factor model or security return differences as the measures of abnormal security return. At a five percent level of probability, the null hypothesis is rejected for two-factor residual and return differences. Differences in the market-model estimates, however, are essentially zero and suggest that rating changes had no statistically detectable impact on common stock prices.

Also of interest is the result that the standard deviations used in the t statistics are of approximately equal magnitude. Whereas, in theory, it is expected that residual differences would exhibit lower variability than return differences, this was not so, perhaps because the residuals approaches, especially those that utilize the one-factor market model, introduce estimation error. As stated earlier, such error may have implications for the power of the tests applied.[16]

These results, however, do not lead unambiguously to a conclusion that reclassifications *per se*, and not other publicly available information, are responsible for the statistically significant t values. As indicated earlier, it is simply not possible to isolate precisely the marginal impact of the rating change with the present methodology. Even the use of a "well matched" control portfolio does not rule out completely the presence of correlated information that might be introduced by some unidentifiable factor.

To complement the basic design, the analysis was extended to focus on the month zero effects of bond reclassifications and evaluate differences in residuals/ returns between the control sample and a second (control) sample, i.e., one not subject to rating reclassifications. Obtained using matching procedures identical to those used earlier, the latter group of firms was denoted the "hypothetical event" sample. The purpose of this extended analysis was two-fold. We wanted, first, to address the substantive issue of whether the rating change itself "produced" the significant price adjustments or whether the adjustments were due to correlated information released independently. A cumulative test tends to obscure any possible distinction between such alternative explanations. Second, we wanted to examine the performance of the three abnormal return metrics when the null hypothesis was known to be true. Evidence on relative efficiencies and possible biases due to noninformational factors may become apparent.

To evaluate the month zero effects of the bond reclassification, mean residual/ return differences and their associated portfolio t statistics u_0^{**} are reported in Table VI. The latter numbers were calculated with the same estimates of standard deviation employed in computing the cumulative statistics. It is evident that the mean price adjustment that occurs in the reclassification month is small, and probably of little economic value for practical purposes. Apparently, the returns of downgraded firms during the 1960–1975 period differed from the returns of a control sample by 1.4 percent on the average. (The price adjustments surrounding upgradings in the month of reclassification are essentially zero.) However, as indicated by the portfolio t values u_0^{**}, the slight decline in common stock price

[16] It should be noted, however, that a more powerful test using return differences will not be obtained if the matched control portfolio is not sufficiently highly correlated with the event portfolio. To check whether this condition held in the context of this study, product moment correlations between each event and control portfolio were calculated for returns, market-model residuals, and two-factor model residuals for the period $t = -35, \cdots, 12$. In all cases the return correlations were significantly positive and higher than the residual return correlations.

Table VI

Portfolio t Statistics: Differences in Month 0 Residuals/Returns between Event and Control Samples

Common Stock Price Adjustment	Downgrading			Upgrading		
	Mean[a]	$u_0^{**}(24)$[b]	$u_0^{**}(36)$	Mean	$u_0^{**}(24)$	$u_0^{**}(36)$
A: 1960-1975						
Market Model Residuals	−0.0182	−1.470	−1.223	−0.0131	−0.203	−0.238
Security Returns	−0.0140	−1.319	−1.150	0.0039	0.207	0.183
Number of Portfolios	68	68	68	65	65	65
B: 1960-1971						
Market Model Residuals	−0.0091	−1.778	−1.671	−0.0039	−0.781	−0.846
Two-Factor Model Residuals	−0.0067	−1.995	−1.939	−0.0011	−0.380	−0.440
Security Returns	−0.0070	−1.861	−1.823	−0.0018	−0.388	−0.446
Number of Portfolios	46	46	46	49	49	49

[a] Mean residual/return difference, i.e., $(1/M)\sum_{p=1}^{M}\hat{u}_{p0}$, where $\hat{u}_{p0}$ is defined in the text and M is the number of portfolios.

[b] Details of the calculation for u_0^{**} are given in the text. The parenthesized numbers (24) and (36) indicate two ways by which portfolio standard deviations were computed: over 24 and 36 observations, respectively.

is statistically significant. The test statistics are uniformly negative for the 1960–1971 period. All statistics for upgradings are insignificant. This leads us to reject the null hypothesis that the marginal and conditional return distributions are equal for *all* signals $\tilde{\theta}_{it} = \theta_{it}^1$ or θ_{it}^2 during month 0.[17] (See footnote 7 for a precise statement of the null and alternative hypotheses examined.)

The second phase of the extended analysis involved an examination of relationships among the alternative security return metrics derived from samples for which there was *no* reclassification event. Since the null hypothesis of absence of informational effects is known to be true, any apparent rejection might be due to selection bias or other statistical aberrations inherent in the methodology. To the extent that such spurious effects are not observed, greater confidence may be placed in the methodology.

The control sample was compared to the hypothetical event sample. In both cases the hypothetical event was assumed to have taken place during calendar month zero. Table VII reports the t statistics and average standard deviation measures. The null hypothesis cannot be rejected for all three performance measures at a five percent level of probability. Note that, since the alternative hypothesis involves a one-sided test, only a failure to report significant positive

[17] Subsequent to the initial analysis, and upon the sugestion of an anonymous reviewer, the results were replicated with an equally-weighted index of market returns. Little sensitivity of the results to the choice of a market index was observed. For example, the month zero portfolio t statistics for the two-factor model residuals in Table VI when based on an equally-weighted index, are −0.919, $\bar{s}$ (24) and −0.819, $\bar{s}$ (36) for upgradings, and −1.827, $\bar{s}$ (24) and −1.955, $\bar{s}$ (36) for downgradings. The $\bar{s}(\cdot)$ values refer to the number of months used to compute the portfolio standard deviation. For the market model residuals in Table VI, computed using an equally-weighted index, the corresponding statistics are −0.815, $\bar{s}$ (24) and −0.750, $\bar{s}$ (36), for upgradings, and −1.509, $\bar{s}$ (24) and −1.538, $\bar{s}$ (36), for downgradings. Both sets of t statistics relate to the common 1960–1971 period.

Table VII

Portfolio t Statistics: Differences in Cumulative Residuals/Returns between "Hypothetical Event" and Control Sample

Common Stock Price Adjustment	Downgrading				Upgrading			
	$U^{**}(24)$[a]	$\bar{s}(24)$	$U^{**}(36)$	$\bar{s}(36)$	$U^{**}(24)$	$\bar{s}(24)$	$U^{**}(36)$	$\bar{s}(36)$
A: 1960-1975								
Market Model Residuals	−0.207	.0289	−0.371	.0300	−2.106	.0323	−1.641	.0340
Security Returns	0.479	.0291	0.183	.0305	−2.701	.0333	−2.384	.0345
Number of Portfolios	68		68		65		65	
B: 1960-1971								
Market Model Residuals	0.370	.0257	0.145	.0264	−0.991	.0289	−0.893	.0298
Two-Factor Model Residuals	0.220	.0259	−0.803	.0267	−0.823	.0287	−0.908	.0295
Security Returns	0.231	.0262	0.001	.0271	−0.915	.0288	−0.973	.0295
Number of Portfolios	46		46		49		49	

[a] Cumulative portfolio t statistic cumulated over months −11, · · ·, 0. Details of the calculations for U^{**} and $\bar{s}$ are given in the text. The parenthesized numbers (24) and (36) indicate the two ways by which portfolio standard deviations were computed: over 24 and 36 observations, respectively.

(negative) t values for upgradings (downgradings) is consistent with its rejection. In other words, in the *absence* of signals θ_{it}^1 and θ_{it}^2, none of the measures can be associated with a potentially spurious rejection of the null hypothesis.[18]

Also, the average variance estimates do not appear to display appreciable differences. No single performance measure has a consistently lower average variance than another, for both time periods and both classes of signals. Hence, regarding the variability of residual vs. return differences, the results of Tables V and VII are basically equivalent.

These findings underscore the potential for estimation error to imply conclusions which are ambiguous with respect to whether the price adjustments to rating reclassifications are significant and thus consistent with the hypothesis of informational effects. But while certain problems inherent in a one-factor or two-factor model approach tend to be circumvented by approaches using matched control samples (e.g., difficulties in the estimation of an intercept term), no approach is totally exempt from error. Return differences and two-factor model residuals are influenced by error in measuring relative risk; two-factor model residuals, in addition, are subject to error in the estimation of $E(\tilde{R}_{zt}) = \gamma_{1t}$. Nevertheless, these results should raise questions about the adequacy of studies that employ just *one* method of estimating abnormal security return. Kaplan [17] identifies and reviews many of these studies.[19]

[18] Concerning the "hypothetical event" sample, portfolio t statistics were also calculated for month zero residuals and returns. None were statistically significant. The t values ranged from 0.131 to −1.210.

[19] Brown and Warner [7] indicate that the chances of one measure of security performance failing to reject the null while another rejects the null (at a five percent level of probability) ranges from 22

V. Concluding Remarks

This paper has examined the adjustments in common stock price of firms with reclassified bonds in the eleven months before and during the month of the rating change. While previous research has suggested that the impact of a reclassification on stock prices is "very small," this research examines that conclusion using several measures of security price performance together with portfolio estimation procedures. Since a rating change, to a degree, is also a reflection of publicly available market and accounting data, such public information should be carefully controlled for if any observed price adjustments at or before the time of announcement are to be attributed to the rating *per se*. That control was provided by analyzing differences between the returns or residual returns on a portfolio of firms experiencing bond rating changes and the returns or residual returns on a portfolio of firms individually matched to firms in the "event" sample on the basis of expected security return. Expected return was estimated as a function of relative risk and various accounting-determined descriptors.

The findings, especially those based on two-factor model residuals and return differences, are consistent with the proposition that bond downgradings convey new information to common stockholders pertinent to the assessment of security return. The null hypothesis was rejected for downgradings focusing on either the price adjustments in the month of announcement or on the cumulative price adjustments in the preceding eleven months. The significant negative response appeared robust to several methodological alternatives. For bond upgradings, however, the price adjustments were statistically insignificant in the month of announcement, although, in the preceding eleven months, upgraded firms experienced positive abnormal returns.

However, while common stock prices appear to adjust to the change in bond rating, we cannot, unfortunately, rule out a major competing explanation—that upgraded firms have been doing better than normal and vice versa for downgraded firms—even by using an approach based on a comparison of reclassified firms with otherwise equivalent control firms. Information correlated with, but not produced by, the rating process may be confounding the results. Moreover, since neither the control nor the "event" samples were chosen randomly, a selection bias could be adding further contamination.

Still, confidence in the results was obtained by comparing a second "hypothetical event" sample with the original control sample. Differences in returns and residuals between those two samples were largely insignificant. Moreover, return differences were no less variable than estimated return differences notwithstanding the fact that common variance factors are removed in the one- or two-factor residual analyses. Finally, we stress that the main contribution of this paper lies in the close scrutiny it gives to issues of methodology in assessing possible security price effects of bond rating reclassifications. Future research should be directed to an analysis of daily or even intraday price adjustments.

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percent to 34 percent for an induced level of abnormal performance of 1 percent. They raise the question that there may be pairs (or sets) of security performance measures that, when applied together, minimize the likelihood of conflicting results. In the present study, only two-factor residuals and return differences produce such nonconflicting results (although only for the twelve-month cumulative residual/return differences).

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