MEASURING THE EFFECT OF RESTRUCTURING ON CORPORATE PERFORMANCE: THE CASE OF MANAGEMENT BUYOUTS

Scott B. Smart and Joel Waldfogel*

Abstract—Recent research has attempted to document that the financial gains associated with takeovers, LBOs and other types of restructuring are attributable to subsequent improvements in operating performance. In this paper we develop a more general framework for measuring the effect of corporate restructuring on performance and apply the framework to a sample of firms taken private by their management. We demonstrate that the estimation approaches employed in the literature embody restrictions on the general framework which the data can reject. However, our best estimates provide evidence that MBOs improve corporate performance, and the magnitudes of these improvements are similar to existing estimates.

I. Introduction

A growing body of research indicates that corporate restructuring generates value for stockholders, and recent empirical evidence points to improvements in operating performance as a primary source of these gains. Kaplan (1989) and Lichtenberg and Siegel (1989) study firms taken private in management buyouts (MBOs) and find that both financial (sales, income, etc.) and real (factor productivity) performance measures improve after the buyout. Healy, Palepu and Ruback (1990) also discover signs of asset productivity improvements in a sample of merged firms. The authors of these studies conclude that MBOs, mergers and other types of corporate restructuring generate operating efficiencies by altering managerial incentives, reducing agency costs and improving factor productivity through other means. In contrast, a number of other studies report results at odds with the view that corporate restructuring generates long-run improvements in value. For example, Ravenscraft and Scherer (1987a and 1987b) detect no evidence of improvements in post-takeover operating performance in a sample of merged firms.

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* Indiana University; and Yale University and National Bureau of Economic Research, respectively.

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Mueller (1985) reaches similar conclusions in his study of market share data.

Though their conclusions diverge, these studies share the common goal of measuring the effect of corporate restructuring on the performance of firms. Answering this question is complicated because the relevant benchmark against which post-reorganization performance should be measured is not preorganization performance. The preferred benchmark incorporates changes in expected performance for the restructuring firms prior to their decision to reorganize as well as unanticipated shocks to performance unrelated to the restructuring decision. It is difficult to find a control measure that convincingly answers the question, “what would have happened to performance at the restructuring firms in the absence of restructuring?”

In most cases, researchers attempting to address this issue have measured the ex post performance of restructuring firms relative to the results achieved by companies operating in the same industry.1 The effect of restructuring is thus measured as a “difference in differences,” or as the change in performance at the restructuring firm, minus the simultaneous change in performance at the control firm(s). The chief shortcoming of this approach lies in its assumption that, if not for the restructuring, the reorganizing firm would have experienced the same change in performance as its competitors. If individual firms have their own dynamic performance patterns, it may be unreasonable to assume that except when organizational structure changes, the performance measures of firms in the same industry move together.

An important reason to doubt that the change in performance at non-restructuring firms provides an appropriate control measure is that re-

1 In addition to matching firms according to industry, some studies have attempted to match firms according to size, leverage and other financial characteristics. In contrast, Jarrell (1991) employs an expectations-based methodology similar to the approach we propose.
structuring firms are a non-random sample of firms. The reasons for restructuring may depend, at least in part, on past and (expected) future performance. For example, suppose that firms’ performance patterns are positively autocorrelated and that restructurings occur when performance is low. Suppose, further, that at any point in time, the performance levels of different firms in an industry are at different levels relative to their own historical averages. Then, independent of the restructuring, we expect performance improvement at the restructuring firm. Equally important, we do not necessarily expect the same improvement at the “control” firms. Consequently, the difference in differences may provide a misleading measure of the effect of restructur- ing on performance.

An accurate measure of the effect of restructuring on firm performance is important because it sheds light on organizational efficiency. In addition, the influence of corporate control events on private sector productivity is directly linked with a number of public policy concerns. For example, corporate restructuring affects the tax revenue collected from corporations. When companies privatize, they typically replace equity with debt. While the cash flow previously paid to equity holders faced corporate income taxation, the interest payments paid to post-buyout debtholders escape corporate-level taxation. Jensen, Kaplan, and Stiglin (1989) estimate that the U.S. Treasury has actually gained from management buyouts (MBOs) because the tax revenues generated from improved corporate cash flows have been large enough to offset corporate tax losses associated with the increased usage of debt. Bulow, Summers, and Summers (1990) show that these gains to the Treasury disappear without the improvements in operating performance. Thus, it is necessary to determine whether increased corporate cash flows are attributable to restructuring or to other factors.

This paper presents a framework for estimating the effect of restructuring on performance. This framework embraces as special cases the difference in differences measure, as well as others. Each special case entails a set of testable restrictions on the general framework. Using data on performance at firms undertaking management buyouts (MBOs), we present tests that distinguish between various estimators for the effect of MBOs on performance, as well as our best estimates of the MBO effect.

The paper is organized as follows. Section II describes how the circumstances surrounding corporate restructuring affect the measurement of its effect on firm performance. Section III develops a general empirical framework for measuring the effect of reorganization on firm performance and discusses the special cases of this framework that have been employed in existing studies. Section IV describes data and empirical estimation of the components of the framework. Section V presents results. A conclusion follows.

II. The Reorganization Decision

The number and size of corporations undertaking significant restructuring activities in the form of mergers, share repurchases, and leveraged buyouts grew during the last decade, motivating economists to study the effects of corporate reorganization on various aspects of firm performance. Jensen (1986) and others advance the theory that some types of restructuring create value by altering the incentives of managers and owners in a way that enhances efficiency and by reducing agency costs. Shleifer and Summers (1988) suggest that corporate takeovers merely transfer value from employees and other stakeholders of firms to shareholders by breaking implicit contracts. Similarly, Lowenstein (1985) discusses the possibility that managers who take their companies private in leveraged buyouts have private information about their firm’s future prospects. This information allows managers to buy the company from public shareholders at a price below that which an informed investor would pay. Though this hypothesis implies that financial performance improves after the buyout, the source of the value increase is not related to the buyout itself.

Whatever the motivation for corporate reorganization, theory provides reasons to suspect that these firms are systematically different from other companies, and empirical evidence supports the theory. Neither the types of firms that choose to

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2 There are other tax effects of these transactions including extra capital gains taxes paid by selling shareholders, increased revenues from more efficient use of capital, taxes on debtor’s incomes and asset sales and so on. Some of these tax effects (e.g., the added capital gains taxes) are not contingent upon future operating performance. However, the net effect on the Treasury’s revenues is negative without the operating performance improvements.
go private, nor the timing of the decision is random. Palepu (1986), Smart (1992), and others find that firms involved in corporate control transactions have characteristics that distinguish them from other firms. If these characteristics are correlated with future returns, then one must account for these factors in order to obtain an unconditional estimate of the effect of the restructuring decision on returns.

Firms that are experiencing unusually low current performance may be more likely than other firms to restructure. If this is the case, then simply calculating pre- and post-buyout performance changes may overstate the effect of the reorganization. In addition, part of the change in performance at the restructuring firms is also experienced at other firms. For example, part of the improvement at a firm that restructures during an industry slump is experienced by all firms in the industry and is not attributable to the reorganization.

Thus, there are two adjustments that must be made to raw changes in performance to isolate the effect of restructuring. Both the firm’s expected change in performance and contemporaneous shocks felt at the firm and elsewhere must be removed. Adjusting the change in performance at a restructuring firm with the change in performance at non-restructuring firms will not yield a correct estimate unless both the control and restructuring firm have the same expected performance improvement. The following section lays out a framework for measuring the effect of restructuring on performance that is valid even if control and restructuring firms have divergent performance expectations.

III. An Empirical Framework

This section introduces a series of approaches to estimating the effect of restructuring on performance, including measures which have been advanced in the literature. Each of the estimation approaches examined is a special case of our theoretically preferable general approach, and each embodies testable restrictions on the general framework.

The simplest possible measure of the effect of restructuring on firm performance is simply the raw change in performance after the buyout occurs:

$$X_{t+k}^R - X_t^R,$$  \hspace{1cm} (1)

where $X$ is some measure of firm performance, $R$ denotes a restructuring firm, and subscripts $t$ and $t + k$ refer to pre- and post-restructuring time periods. We will refer to the raw changes as estimator (1). The problem with this estimator is that it attributes the entire change in performance to the reorganization even though some portion of the change at the firm would have occurred in the absence of any reorganization. For example, observable measures of firm performance may be autocorrelated. If a firm’s performance pattern is cyclical and restructuring occurs when the performance measure is below its historical mean, then some improvement is expected and is not properly attributable to the reorganization.5 We can test whether performance changes are expected at restructuring firms.

HYPOTHESIS A: \[ E(X_{t+k}^R | t) = X_t^R \]

Prior to the restructuring, performance is expected to remain at current levels.

Rejection of the hypothesis that no changes are expected indicates that the raw changes estimator is an inadequate measure of the effect of restructuring because significant performance changes are expected absent restructuring.

A natural way to deal with the problem of anticipated performance changes is to adjust the actual change in performance that occurred for (an estimate of) the expected change that would have occurred without the buyout. Suppose we have a measure of expected performance at the restructuring firm in period $t + k$, $E(X_{t+k}^R | t)$. (Note that this expectation is taken at time $t$, 5 Survivorship bias can generate the same type of effect. Consider the population of firms experiencing below average performance at time $t$. Some of these firms will survive in one form or another, but others will cease to exist. Because any sample of firms with continuously observable performance data over some time interval will exclude those that go bankrupt, we would expect to observe performance improvements among those firms in the sample initially doing poorly.
prior to the restructuring decision, and it presupposes that no organizational change will take place). Then the effect of reorganization is only the part of the change in performance that was not expected prior to the buyout, or the restructuring surprise:

\[ X_{t+k} - E(X_{t+k}^R | t). \]  

(2)

Interpreting this as a measure of the effect of restructuring relaxes the assumption of the first approach that the expected change in performance in the absence of restructuring is zero.

While the restructuring surprise offers an improvement over the raw change (1), it suffers from the potential shortcoming that it attributes to the buyout any unforecastable change experienced at the restructuring firm even though nonrestructuring firms may experience the same performance shock. That is, the surprise in expression (2) will typically include both firm-specific and more general shocks.\(^6\) If the restructuring firm's industry does surprisingly well following the firm's reorganization, then it would be incorrect to attribute all of expression (2) to restructuring. We can test whether industry shocks undermine the interpretation of estimator (2).

HYPOTHESIS B: \( E(X_{t+k} | t) = X_{t+k} \) (for controls)

There are no performance shocks at control firms.

Rejection of hypothesis B indicates that estimator (2), while better than (1), still gives a misleading estimate of the effect of restructuring. Only the component of the restructuring surprise not experienced by similar nonrestructuring firms is properly attributed to the reorganization.

The possibility of a performance shock experienced by both restructuring and control firms suggests that the effect of restructuring should be measured as the performance surprise experienced at restructuring firms, less the performance surprise at comparable non-restructuring firms.\(^7\) Our most general measure of the effect of a restructuring is then the restructuring surprise minus the "control surprise," or the difference in surprises:

\[ [X_{t+k}^R - E(X_{t+k}^R | t)] - [X_{t+k} - E(X_{t+k} | t)]. \]  

(3)

The difference in surprises measure has the following intuitive interpretation. Suppose that we have accounted for every statistically controllable aspect of the change in performance at restructuring and non-restructuring firms predictable prior to the reorganization, except the restructuring itself. What we have left is a set of change-in-performance surprises for the restructuring and non-restructuring firms. The difference in surprises estimator is the average difference in performance surprises between restructuring and nonrestructuring firms and may properly be interpreted as an estimate of the effect of restructuring on firm performance.

Separate from the progression of estimators above is a measure of the restructuring effect that has been used in the literature (Kaplan, 1989; Lichtenberg and Siegel, 1989, and others), the change in performance at restructuring firms minus the change in performance for a control sample of firms,

\[ (X_{t+k}^R - X_t^R) - (X_{t+k} - X_t) \]  

(4)

or the difference in differences. A shortcoming of this estimation approach is that it neglects the possibility, discussed above, that some parts of the changes in performance at the restructuring and non-restructuring firms may be forecastable. Our general estimator (3) reduces to expression (4) when the expected changes in performance are equal at restructuring and control firms.\(^8\) We can test whether this condition obtains, and hence whether the difference in differences measure is appropriate, by examining the following hypothesis:

HYPOTHESIS C: \[ [E(X_{t+k}^R | t) - X_t^R)] - [E(X_{t+k} | t) - X_t] \]

Expected improvements at restructuring and control firms are identical.

An example will help illustrate the difference among the estimators. Suppose that performance at XYZ Corp. is expected (in the absence of a restructuring) to improve by 10% over the next two years.\(^9\) Suppose a buyout occurs and performance improves 15%. The restructuring surprise

\(^6\) For example, a petroleum firm that went private in 1990 would have experienced a performance improvement in 1991 due to the Gulf Crisis, as did firms remaining public.

\(^7\) Of course, finding a non-MBO control firm is not a trivial matter, but we leave that concern for the empirical section of the paper.

\(^8\) An intuitive special case of this arises if no performance improvements are expected at either type of firm.

\(^9\) In the empirical section we use the change in the ratio of operating income to sales as our performance measure. Thus, if the current income to sales ratio for XYZ is 0.10, an expected improvement of 10% implies an expected income to sales ratio of 0.11 in two years.
IV. Data

Estimation of the effect of restructuring on performance using the framework above requires us to create empirical analogues to each of the components of expression (3). These are comprised of two types of information, performance and expected performance, for two different types of firms, restructuring firms and control firms. Expected performance measures are obtained from both time series methods and analysts' forecasts.\(^\text{10}\)

The performance measure \((X)\) used in this study is the ratio of operating income to sales. Performance changes are reported in percentage terms. We use a scaled measure to allow comparability across firms and to control (at least partially) for post-buyout divestitures. In addition, the income to sales ratio may be loosely interpreted as a measure of the efficiency with which firms utilize a given amount of sales. We scale by sales rather than assets because the accounting value of assets typically changes at the time of a buyout, making pre- vs. post-buyout comparisons difficult. Finally, our measure of performance has been used elsewhere in the literature (Kaplan, 1989; Jarrell, 1991), so we are able to make rough comparisons between our results and the results of previous studies.

The data on actual performance of MBO firms \((X_p)\) come from two sources. Pre-buyout data are primarily from COMPUSTAT's Research File, although data unavailable on COMPUSTAT were obtained from 10-k's directly. Post-buyout data for 48 MBOs are obtained from the data set developed by Kaplan (1989).\(^\text{11}\) These firms were subject to public disclosure requirements after their buyouts either because they continued to have public securities (debt or preferred stock), or because they sought additional public funding.\(^\text{12}\) Up to three years of post-buyout data, not including the fiscal year of the reorganization, are available for each firm. In what follows, the first, second, and third years after the buyout are referred to as \(t + 1\), \(t + 2\), and \(t + 3\).

The data on actual performance at control firms \((X_c)\) come from two basic sources, COMPUSTAT and Value Line. We use three different control samples. From COMPUSTAT, we construct an annual time series of the aggregate ratio of operating income to sales at the 3-digit SIC level. These 3-digit industries are chosen to match the 3-digit industries of the MBO firms. In any year, we include all firms that have data for both variables (COMPUSTAT data items 12 and 13). From Value Line, we obtain two different control measures of performance. First, for each MBO firm, we choose up to five individual Value Line firms of similar size in the same SIC industry. We aggregate these firms' operating income and sales figures and use the aggregate ratio as our second control measure of performance. Alternatively, we associate each MBO firm with a Value Line industry grouping, and we use Value Line's aggregate industry-grouping data as a control measure of performance for that firm.\(^\text{13}\)

For both control and restructuring firms, we must also derive measures of the expected change in performance at the time of the restructuring.

\(^{10}\) We employ both types of forecasts because of conflicting evidence on the quality of time series and analysts' forecasts. See, for example, Brown et al. (1987) and O'Brien (1986).

\(^{11}\) See Kaplan (1989) for a detailed description of his data.

\(^{12}\) A few firms disclosed their financial data when they were sold to existing public companies.

\(^{13}\) Value Line industry categories correspond only loosely to SIC codes and group together firms that operate in vastly different markets on dramatically different scales. It is not uncommon for Value Line to group two firms whose SIC codes are dissimilar even at the two-digit level. We prefer on a priori grounds using firm-specific data to construct our control measures because we can match control and restructuring firms more closely on the basis of firm size and industry.
firm’s buyout. We derive expected changes in performance for the MBO firms \(E(X_{t+k}^R|t) - X_t^R\) in two ways. First, we estimate a dynamic performance regression on the MBO firms’ annual performance history up to the last pre-buyout year using the ratio of operating income to sales as the dependent variable, and we use this regression to generate forecasts for post-buyout performance. This regression includes one lagged dependent variable and allows each firm a different intercept. Our second measure of expected change in performance at the restructuring firms is the ratio of operating income to sales predicted by Value Line. Value Line publishes a multi-year forecast covering the second through fourth years following the buyout (years \(t + 2\) to \(t + 4\)). We obtain the expected change in performance as the last forecast of performance before the announcement of the buyout. Thus, these expected changes are the expectations of performance changes in the absence of a buyout.

We derive expected changes in performance for control measures \(E(X_{t+k}^R|t) - X_t\) in three ways: (1) using autoregressions on SIC 3-digit aggregate COMPUSTAT history (in a fashion analogous to the regressions on MBO history), (2) by aggregating Value Line’s performance forecasts for the 5 individual firms that we match with each MBO firm, and (3) using Value Line’s forecasts of industry aggregate performance. In all three cases, the control forecasts are made given information available prior to the buyout at their matched MBO firms.

V. Empirical Results

In this section we estimate the effect of restructuring on performance using data on MBOs. We test between the measures discussed in section III, and we present our best estimates of the MBO effect.

A. Choosing an Estimator

In principle, the most general measure of the effect of restructuring is the best measure, since it relaxes as many assumptions as possible. Yet, in choosing empirically among estimators for the effect of restructuring, what matters is whether the restrictions embodied by a particular estimator indeed hold. In what follows, we test the restrictions implied by choices of various estimators over others. In particular, we test hypotheses that allow the data to distinguish between raw changes and restructuring surprises (1 vs. 2), between restructuring surprises and differences in surprises (2 vs. 3), and between differences in differences and differences in surprises (4 vs. 3).

Table 2 presents Wilcoxon statistics (in parentheses) for these three hypotheses, estimated over the three different control groups’ data. Figures in column A represent the median expected performance improvements at restructuring firms measured as a percentage change in the operating income to sales ratio.\(^{14}\) For example, using COMPUSTAT data we estimate that the median expected change in the ratio of operating income to sales is 4.0% by the first year after the buyout, 5.3% by the second year, and 7.0% in the third. Value Line forecasts show even larger expected performance improvements for these firms, 7.5% by year two and 17.0% by year three, and they are significantly different from zero.\(^{15}\) Because we consistently reject hypothesis A, it is inappropriate to use raw changes to measure the MBO effect as they will give an upwardly biased estimate of the true effect.

Column B contains median estimates of control firm surprises which should theoretically be subtracted from restructuring surprises to yield the true MBO effect. Note that according to all of our firm-specific data, control firms perform worse than expected, and significantly so by year three. In other words, both COMPUSTAT and Value Line forecasts of the performance of individual control firms prove to be overly optimistic, although the Value Line data on aggregate industry performance fail to reject the hypothesis that control surprises are zero. To the extent that control surprises are significantly below zero, the restructuring surprise estimates underestimate the MBO effect because they neglect simultaneous negative performance surprises at control firms.

Column C presents median estimates of the difference between restructuring firms’ expected performance improvements and that of control firms. A positive estimate indicates that expected improvements at restructuring firms exceed those at control firms, and therefore that the differences in differences estimates will exceed the true

\(^{14}\) These changes are measured relative to the year immediately prior to the buyout.

\(^{15}\) The bottom panel of column A is blank because to test this hypothesis we need to measure expected performance improvements only at restructuring firms, not control firms.
MBO effect. Our evidence on this hypothesis is mixed. **COMPSTAT** data suggest that there are significant differences between expected performance at restructuring and control firms in the short run (the first and second post-buyout years), but that these differences disappear by year three. The **Value Line** firm-specific data indicate that MBO firms have greater expected performance improvements, but they are significant at conventional levels only in the third year. The industry aggregate data also indicate higher expected performance for MBO firms, but the differences are not significant. The erratic estimates for the latter years probably reflect our small samples (fewer than 15 firms by the final year). In summary, all but one of our estimates indicates that MBO firms have higher expected performance improvements than control firms. Because these estimates are not consistently significant, we conclude that there is only weak evidence that the difference in differences estimator overstates the true MBO effect and that the difference in surprises estimator offers an improvement over the difference in differences.

### B. Choosing an Estimate

The tests reported above indicate that conventional measures of the effect of restructuring on performance embody restrictions that the data sometimes reject. The pattern of test results guides our choice of estimates, presented below. Table 3 reports median MBO effect estimates as well as Wilcoxon tests of the hypotheses that these effects are zero. Our finding of positive expected improvements at restructuring firms—and concomitant rejection of hypothesis A—suggests that the raw changes (1) gives larger estimates than the restructuring surprises (2). Results in table 3 confirm this. With the exception of the **COMPSTAT** estimate for the first post-buyout year, all of the restructuring surprise esti-

<table>
<thead>
<tr>
<th>Control</th>
<th>N</th>
<th>A⁺</th>
<th>B⁺</th>
<th>C⁺</th>
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<td><strong>COMPSTAT</strong> data used to forecast expected performance</td>
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<tr>
<td>t + 1</td>
<td>35</td>
<td>4.0</td>
<td>-1.5</td>
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<td></td>
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<td>(2.19)</td>
<td>(-1.03)</td>
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<td>t + 2</td>
<td>31</td>
<td>5.3</td>
<td>-4.7</td>
<td>6.0</td>
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<td></td>
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<td>(-1.29)</td>
<td>(1.96)*</td>
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<tr>
<td>t + 3</td>
<td>14</td>
<td>7.0</td>
<td>-28.7</td>
<td>-6.7</td>
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<tr>
<td></td>
<td></td>
<td>(1.6)</td>
<td>(-2.54)</td>
<td>(-0.41)</td>
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<tr>
<td><strong>Value Line</strong> data used to forecast expected performance</td>
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<tr>
<td>1. Individual firms</td>
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</tr>
<tr>
<td>t + 2</td>
<td>25</td>
<td>7.5</td>
<td>-7.9</td>
<td>1.0</td>
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<td></td>
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<td>(3.43)</td>
<td>(-2.52)</td>
<td>(0.96)</td>
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<td>t + 3</td>
<td>11</td>
<td>17.0</td>
<td>-18.3</td>
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<td>(2.22)</td>
<td>(-1.96)</td>
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<tr>
<td>b. Industry aggregate data²</td>
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<tr>
<td>t + 2</td>
<td>24</td>
<td>2.3</td>
<td>1.6</td>
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² Wilcoxon statistics are in parentheses. Critical values for two-tailed tests are 1.96 and 1.65 for 95% and 90% significance levels, respectively.

² H0: \( \text{E}(X^{\text{MBO}}_{k}(t) - X^{\text{A}}_{k}) = 0 \), or that the expected performance improvement at restructuring firms is zero. Rejection indicates that estimator 2 provides an improvement over estimator 1.

² H0: \( X^{\text{MBO}}_{k} - \text{E}(X^{\text{MBO}}_{k}(t)) = 0 \), or that control surprises are zero. Rejection indicates that estimator 3 is more appropriate than estimator 2.

² H0: \( E(X^{\text{MBO}}_{k}(t) - X^{\text{A}}_{k}) - [E(X^{\text{MBO}}_{k}(t) - X^{\text{A}}_{k})] = 0 \), or that the control and restructuring performance measures have the same expected improvements prior to the restructuring. Rejection indicates that estimator 3 provides an improvement over estimator 4.

² Note that since there are an even number of firms in this panel and in year \( t + 3 \) of panel 1, there is no unique median, so we report both medians. Since hypothesis B does not require industry adjustment, we do not repeat the results from panel 2.

² Significant at 95%.

² Significant at 90%.
7.7% in restructuring for estimates with zero buyout reject performance. The median increase is 11.9%.

While performance improvements persist even after removing expected improvements at restructuring firms, we reject the hypothesis that these improvements are zero with less confidence than for raw changes.

Table 3—Median Estimates of the MBO Effect Using Alternative Estimators

<table>
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<th>3^d</th>
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<td>N</td>
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<tr>
<td>t + 1</td>
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<td>4.1</td>
<td>5.7</td>
<td>6.8</td>
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<tr>
<td></td>
<td></td>
<td>(1.97)^g</td>
<td>(0.95)</td>
<td>(1.39)</td>
</tr>
<tr>
<td>t + 2</td>
<td>31</td>
<td>11.9</td>
<td>7.4</td>
<td>11.5</td>
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<tr>
<td></td>
<td></td>
<td>(2.62)^g</td>
<td>(1.87)^h</td>
<td>(2.00)^g</td>
</tr>
<tr>
<td>t + 3</td>
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<td>22.8</td>
<td>18.1</td>
<td>33.6</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(2.42)^g</td>
<td>(1.66)^h</td>
<td>(2.86)^g</td>
</tr>
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2. Value Line data used to forecast expected performance
   a. Individual firms
   t + 2                | 25  | 18.0| 7.7 | 21.4| 15.7|
   |                      |     | (2.65)^g | (0.98) | (1.98)^g | (2.19)^g |
   t + 3                | 11  | 23.9| 1.4 | 33.9| 33.1|
   |                      |     | (2.31)^g | (0.80) | (2.13)^h | (2.84)^g |

   b. Industry aggregate data^f
   t + 2                | 24  |     |     | 13.1| 12.0|
   |                      |     |     |     | 12.4| 7.5 |
   |                      |     |     |     | (0.71) | (1.31) |
   t + 3                | 10  |     |     | 21.0| 28.7|
   |                      |     |     |     | 4.6  | 23.1|
   |                      |     |     |     | (1.38) | (1.17) |

Note that since there are an even number of firms in this panel and in year t + 3 of panel 1, there is no unique median, so we report both medians. Columns 1 and 2 are blank for this panel because these estimators are based only on restructuring firm data.

^a Wilcoxon statistics are in parentheses. Critical values for two-tailed tests are 1.96 and 1.65 for 95% and 90% significance levels, respectively.
^b Raw changes: X_{t+k}^M - X_t^P. All measures are reported as percentage change in the ratio of operating income to sales.
^c Restructuring surprise: X_{t+k} - E(X_{t+k})|t).
^d Difference in surprises: [X_{t+k} - E(X_{t+k})|t)] - [X_{t+k} - E(X_{t+k})|t)]
^e Difference in differences: [X_{t+k} - X_t] - [X_{t+k} - X_t].
^f Significant at 95%.
^g Significant at 90%.

estimates are smaller and less significant than the median raw changes. For example, the raw change in the operating income to sales ratio for the median firm is 11.9% for the sample of 31 firms with COMPSTAT data in the second post-buyout year. Adjusting these figures for time series forecasts of expected performance yields an increase in the ratio of just 7.4%. While performance improvements persist even after removing expected improvements at restructuring firms, we reject the hypothesis that these improvements are zero with less confidence than for raw changes.

The result of hypothesis B, that control firms experienced negative performance shocks, indicates that the difference in surprises estimates should be larger than the restructuring surprise estimates which ignore these shocks. Again table 3 is consistent with our earlier findings. In every instance, median differences in surprises exceed restructuring surprises and are more significant. For example, the median restructuring surprises in the second post-buyout year are 7.4% and 7.7% (using COMPSTAT and Value Line forecasts, respectively), while the associated median difference in surprises are 11.5% and 21.4%.

Finally, we compare our theoretically preferred set of estimates, the difference in surprises, to the difference in differences approach commonly used in the literature. The estimates differ when expectations of future performance at restructuring and control firms diverge. The positive and significant performance improvements expected at MBO firms (documented above) are somewhat offset by the expected changes in performance at control firms. As a result, the medians of the difference in differences and difference in surprises are in most cases quite similar, even though we sometimes reject hypothesis C, that the expected changes at restructuring and control firms are identical.

The reader may find it puzzling that although in some cases we reject hypothesis C, the median estimates of (3) and (4) are virtually identical. The answer relates to the distribution of differences in expected performance changes for both types of firms in this sample. The adjustments
(which distinguish between estimators (3 and 4)) are close to zero for firms near the median for estimator (3) and more substantial in the tails. Hence, the medians are virtually unaffected while the significance levels drop somewhat.

In summary, our best estimate of the MBO effect leads us to conclude that there are large positive increases in the ratio of operating income to sales after the buyout. This measure of performance improves by 6.8% between the last pre-buyout year and the first post-buyout year. Performance improvements by the second post-buyout year range from 11.5% to 21.4%. Our estimates of performance in the third post-buyout year indicate improvements of up to 20% to 30%. The median ratio of operating income to sales in the last pre-buyout year is 10.6%. Therefore a 10% improvement in performance corresponds to a post-buyout operating income to sales ratio of 11.7%. Given a median pre-buyout sales figure of $570 million, this improvement translates to an increase in operating income of about $6 million. If MBOs improve performance by, say, 30% over longer horizons, and if these improvements persist, then the present value of the firm’s increased operating income would be $180 million (discounted at 10%).

VI. Conclusion

Recent developments have focused attention on the difficult question of whether corporate restructuring affects performance. The principal obstacle to measuring the effect of restructuring on performance is accounting for what would have happened to performance if the reorganization had not occurred. Existing studies have generally measured the effect as the change in performance at the restructing firm, less the change in performance at a control firm.

We have shown in this paper that this conventional measurement approach is one of various special cases of a more general measurement framework. Moreover, the restrictions on the general framework required to justify the use of each special case are testable. Using data on MBOs, we test and sometimes reject these restrictions, suggesting that more flexible estimators of the MBO effect may be preferable to more commonly used estimators.

The results of our hypothesis tests guide our selection of MBO effect estimators and estimates. Even though we find some evidence that our estimator provides an improvement over the conventional approach, our best estimates of the MBO effect lend added support to the view that MBOs enhance operating performance.

REFERENCES


