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Back to the Beginning: Persistence and the Cross-Section of Corporate Capital Structure

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ABSTRACT

We find that the majority of variation in leverage ratios is driven by an unobserved time-invariant effect that generates surprisingly stable capital structures: High (low) levered firms tend to remain as such for over two decades. This feature of leverage is largely unexplained by previously identified determinants, is robust to firm exit, and is present prior to the IPO, suggesting that variation in capital structures is primarily determined by factors that remain stable for long periods of time. We then show that these results have important implications for empirical analysis attempting to understand capital structure heterogeneity.

A FUNDAMENTAL QUESTION IN FINANCIAL ECONOMICS IS: How do firms choose their capital structures? Indeed, this question is at the heart of the "capital structure puzzle" put forward by Myers (1984) in his AFA presidential address. Attempts to answer this question have generated a great deal of discussion in the finance literature. Many studies, both before and after Myers's pronouncement, identify a number of factors that purport to explain variation in corporate capital structures. However, after decades of research, how much do we really know? More precisely, how much closer have previously identified determinants and existing empirical models moved us toward solving the capital structure puzzle? And, given this progress, how can we move still closer to ultimately providing a more complete understanding of capital structure decisions?

The goal of this paper is to address these questions. Specifically, we quantify the extent to which existing determinants govern cross-sectional and

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time-series variation in observed capital structures by examining the evolution of corporate leverage ratios. In doing so, we are not only able to assess the progress of existing empirical work, but more importantly, we are also able to characterize what existing determinants appear to miss—the gap in our understanding of what determines heterogeneity in capital structure. Our analysis, while shedding light on several issues, also presents some new challenges to understanding how firms choose their capital structures.

We begin by showing that leverage ratios exhibit two prominent features that are unexplained by previously identified determinants (e.g., size, profitability, market-to-book, industry, etc.) or changes in sample composition (e.g., firm exit). These features are illustrated in Figure 1 (see Section II), which shows the future evolution of leverage ratios for four portfolios constructed by sorting firms according to their current leverage ratios. The first notable feature in the figure is that leverage ratios exhibit a significant amount of convergence over time; firms with relatively high (low) leverage tend to move toward more moderate levels of leverage. The second feature is that, despite this convergence, leverage ratios are remarkably stable over time; firms with relatively high (low) leverage tend to maintain relatively high (low) leverage for over 20 years. Thus, leverage ratios are characterized by both a transitory and a permanent component that, as discussed below, have yet to be identified.

How important are these components? The adjusted R-squares from traditional leverage regressions using previously identified determinants range from 18% to 29%, depending on the specification. In contrast, the adjusted R-square from a regression of leverage on firm fixed effects (statistical "stand-ins" for the permanent component of leverage) is 60%, implying that the majority of variation in leverage in a panel of firms is time invariant and is largely unexplained by previously identified determinants.

One possible explanation for these findings is that commonly employed empirical models are misspecified because managers are more concerned with variation in long-run or equilibrium levels of leverage determinants, as opposed to short-run fluctuations. We test this hypothesis by estimating a distributed lag model of leverage. Two facts emerge from this exercise. First, the responses of leverage to short-run and long-run variation in its determinants often differ, highlighting the potential importance of accounting for lagged effects in empirical specifications. Second, even after allowing for an aggregate response that occurs over 8 years, variation in the traditional determinants still struggles to explain variation in capital structures. For example, a one-standard deviation change in the long-run equilibrium level of a firm's industry median leverage, the single most influential observable determinant of book leverage, results in a 6% change in expected leverage—a small fraction relative to the unconditional standard deviation of book leverage, 21%. Thus, regardless of whether one takes a short-run or a long-run perspective, existing determinants of capital structure appear to explain a relatively small fraction of the variation in leverage ratios.

We then examine the implications of these findings for empirical research in capital structure. We find that the estimated associations between leverage and previously identified determinants are highly sensitive to changes in model specification. The coefficient estimates on previously identified determinants experience an average decrease in magnitude of 86% (65%) in the book (market) leverage regression after incorporating firm fixed effects (i.e., accounting for the permanent component of leverage) and serially correlated errors (i.e., accounting for the transitory component of leverage). Given the importance of this unobserved heterogeneity in leverage, parameter estimates that do not account for the firm-specific effect (via within-transformation, differencing, structural estimation, natural experiments, etc.) and serial correlation (via lagged dependent variables, serially correlated errors, etc.) are suspect. That is, it is simply untenable to draw causal inferences in models ignoring these components of the data generating process because identification of the parameters of interest is questionable (Arellano (2003) and Hsiao (2003)).

Next, we turn to the question: What lies behind the unidentified components of capital structure revealed by Figure 1? Focusing first on the transitory component, we show that the convergence of leverage ratios revealed by Figure 1 is due, at least in part, to the role of leverage as an important state variable in firms' net issuance decisions. An analysis of security issuance behavior identifies debt policy as an important mechanism for controlling corporate leverage, while equity policy plays a secondary role. This finding suggests that active management of leverage ratios is at least partially responsible for the mean reversion in leverage ratios—findings consistent with a large recent literature examining this issue.[1]

Moreover, we also show that this dynamic rebalancing is directed toward a largely time-invariant target, namely, the unobserved permanent component of leverage. This finding is at odds with recent conclusions by Flannery and Rangan (2006) and Hovakimian, Opler, and Titman (2001), who suggest that firms appear to be adjusting toward time-varying targets. We show that accounting for time-varying factors in the target specification has a negligible effect on both the model fit and estimated speed of adjustment relative to a firm-specific, time-invariant specification for target leverage. This result fits nicely with our other findings highlighting the relative importance of cross-sectional, as opposed to time-series, variation in capital structures and the limited explanatory power of previously identified leverage determinants.

Finally, we turn to the permanent component of leverage, portrayed by the long-lived stability of leverage ratios in Figure 1. While identifying what factors are behind this feature of the leverage data generating process is beyond the scope of this paper, we take a step toward this goal by showing that persistent differences in leverage ratios exist among a sample of privately held firms in the United Kingdom for which we are able to obtain data. We also find that, among domestic firms, these differences persist *back* in time, predating the IPO. In other words, high (low) levered private firms remain so even after going

[1] Studies by Flannery and Rangan (2006), Hovakimian (2006), Kayhan and Titman (2007), Leary and Roberts (2005), and Liu (2005) find that firms gradually adjust their capital structure in response to various shocks.

public and despite the corresponding changes in the information environment, the distribution of control, and the access to capital. Thus, any explanation of capital structure must be reconciled with this long-lived stability of leverage ratios.[2]

The remainder of the paper is organized as follows. Section I discusses the data and sample selection. In Section II, we examine the dynamic behavior of capital structure from several perspectives. Section III examines the implications of our findings for existing determinants of leverage ratios. Section IV examines the implications of our findings for the empirical analysis of capital structure. Section V investigates what drives the convergence of leverage ratios over time (i.e., the transitory component). Section VI investigates what drives the long-lived stability of leverage ratios over time (i.e., the permanent component). Section VII concludes.

I. Data and Sample Selection

Our primary sample consists of all nonfinancial firm-year observations in the annual Compustat database between 1965 and 2003.[3] We require that all firm-years have nonmissing data for book assets, while all multivariate analysis implicitly requires nonmissing data for the relevant variables. We require leverage—both book and market—to lie in the closed unit interval. All other ratios are trimmed at the upper and lower one-percentiles to mitigate the effect of outliers and eradicate errors in the data. For some of our analysis, we also require an identifiable IPO date.[4] The construction of all of the variables used in this study is detailed in the Appendix.

Table I presents summary statistics for all of our firms, as well as a subsample of firms having at least 20 years of nonmissing data on book leverage. We refer to this latter sample as "Survivors," since selection is predicated on at least 20 years of existence. The potential for survivorship bias in our analysis motivates our examination of this subsample in all subsequent analysis as a robustness check; however, due to space considerations and the similarity of the findings, we sometimes suppress these results.

A quick comparison between the samples reveals several unsurprising differences. Survivors tend to be larger, more profitable, and have fewer growth opportunities (i.e., lower market-to-book), but more tangible assets relative to the general population. Survivors also tend to have higher leverage, especially in market value terms. This suggests that firm exits due to buyouts and acquisitions are potentially as important as those due to bankruptcy. Alternatively, the higher leverage of survivors may be an artifact of confounding effects—survivor

[2] Strebulaev and Yang (2006) show in a recent working paper that this stability and inexplicability are also common to firms with zero leverage ratios.

[3] We choose this horizon because of sample selection concerns associated with pre-1965 data in Compustat; however, none of our results are sensitive to this time frame, as suggested by unreported analysis examining 1950 to 2003 and 1971 to 2003 sample horizons.

[4] The IPO information is obtained from SDC and Jay Ritter, whom we kindly thank.

Table I
Summary Statistics

The sample consists of all nonfinancial firms in the Compustat database from 1965 to 2003. The table presents variable averages, medians (in brackets), and standard deviations (SD) for the entire sample (All Firms), as well as the subsample of firms required to survive for at least 20 years (Survivors). Variable definitions are provided in the Appendix.

	All Firms		Survivors	
Variable	Mean [Median]	(*SD*)	Mean [Median]	(*SD*)
Book leverage	0.27	(0.21)	0.27	(0.18)
	[0.24]		[0.26]	
Market leverage	0.28	(0.26)	0.32	(0.25)
	[0.23]		[0.28]	
Log(Sales)	4.43	(2.52)	5.49	(2.25)
	[4.54]		[5.59]	
Market-to-book	1.59	(1.87)	1.23	(1.19)
	[1.00]		[0.89]	
Profitability	0.05	(0.26)	0.12	(0.13)
	[0.12]		[0.13]	
Tangibility	0.34	(0.25)	0.39	(0.25)
	[0.28]		[0.33]	
Cash flow vol.	0.10	(0.14)	0.07	(0.08)
	[0.06]		[0.05]	
Median industry book leverage	0.24	(0.13)	0.26	(0.12)
	[0.24]		[0.25]	
Dividend payer	0.39	(0.49)	0.63	(0.48)
	[0.00]		[1.00]	
Intangible assets	0.05	(0.10)	0.04	(0.08)
	[0.00]		[0.00]	
Obs.	225,839		92,306	

firms are larger, and larger firms tend to have higher leverage (Titman and Wessels (1988)). At this point, we merely note that these summary statistics are broadly consistent with those found in previous studies and with intuition.

II. The Evolution of Leverage

We begin our analysis by studying the evolution of leverage for our cross-section of firms. Figure 1 presents the average leverage ratios of four portfolios in "event time." The figure is constructed in the following manner. Each calendar year, we sort firms into quartiles (i.e., four portfolios) according to their leverage ratios, which we denote: Very High, High, Medium, and Low. The portfolio formation year is denoted event year 0. We then compute the average leverage for each portfolio in each of the subsequent 20 years, holding the portfolio composition constant (but for firms that exit the sample). We repeat these two steps of sorting and averaging for every year in the sample period. This process generates 39 sets of event-time averages, one for each calendar year in our sample. We then compute the average leverage of each portfolio across the

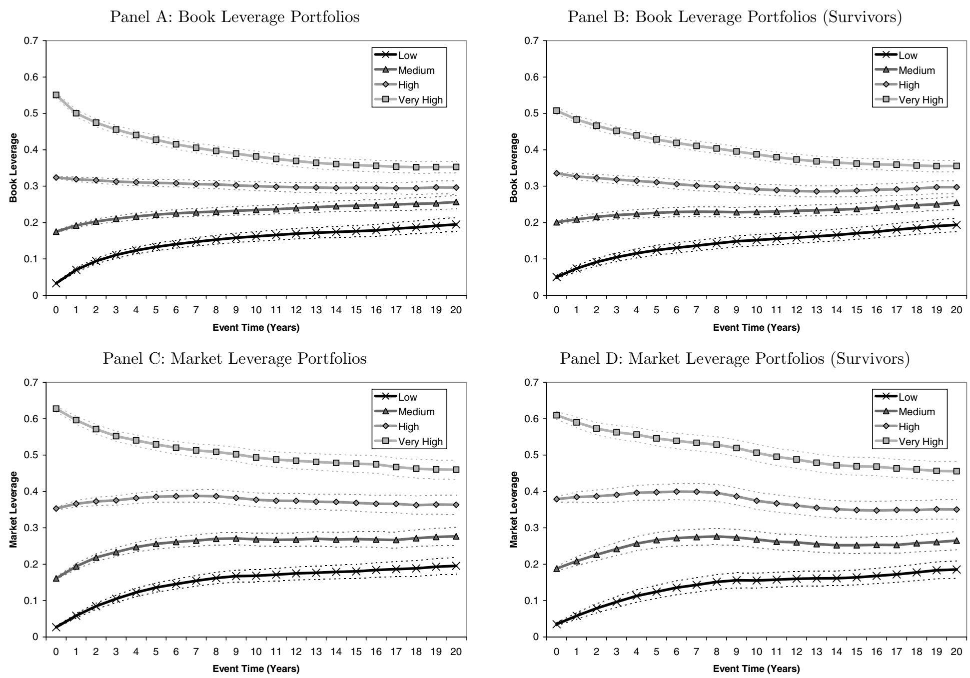


Figure 1. Average leverage of actual leverage portfolios in event time. The sample consists of all nonfinancial firms in the Compustat database from 1965 to 2003. Each panel presents the average leverage of four portfolios in event time, where year zero is the portfolio formation period. That is, for each calendar year, we form four portfolios by ranking firms based on their actual leverage. Holding the portfolios fixed for the next 20 years, we compute the average leverage for each portfolio. For example, in 1975 we sort firms into four groups based on their leverage ratios. For each year from 1975 to 1994, we compute the average leverage for each of these four portfolios. We repeat this process of sorting and averaging for every year in our sample horizon. After performing this sorting and averaging for each year from 1965 to 2003, we then average the average leverages across "event time" to obtain the bold lines in the figure. The surrounding dashed lines represent 95% confidence intervals. The results for book and market leverage are presented in Panels A and C, where book (market) leverage is defined as the ratio of total debt to total assets (sum of total debt and market equity). Panels B and D present similar results for book and market leverage, respectively, but for a subsample of firms required to exist for at least 20 years (consequently, we can only perform the portfolio formation through 1984 for this sample).

39 sets within each event year. We perform this exercise for both book leverage and market leverage, the results of which are presented as solid lines in Panels A and C, respectively. The dashed lines surrounding the portfolio averages represent 95% confidence intervals.[5]

[5] The confidence interval is defined as a two-standard error interval around the estimated mean. The standard error is estimated as the average standard error across the 39 sets of averages. Estimating the standard error using the standard error of the average across the 39 sets would underestimate the true standard error because of the overlapping observations. Thus, we use a conservative estimate that ignores the effects of averaging across the 39 sets, effectively treating each set as redundant.

Several features of the graphs are worth noting. First, there is a great deal of cross-sectional dispersion in the initial portfolio formation period. The range of average book (market) leverage is 52% (60%). Second, there is noticeable convergence among the four portfolio averages over time. After 20 years, the Very High book leverage portfolio has declined from 55% to 35%, whereas the Low portfolio has increased from 3% to 19%. (The market leverage portfolios display a similar pattern.) Third, most of the convergence occurs in the first few years after the formation period, as evidenced by the flattening slope over time in the Low and Very High portfolios. Finally, despite the convergence, the average leverage across the portfolios 20 years later remains significantly different, both statistically and economically.

The average book leverage ratios in the Very High, High, Medium, and Low portfolios after 20 years are 35%, 30%, 25%, and 19%, respectively, an average differential of over 5%. When compared to the average within-firm standard deviation of book leverage (12.9%), this differential is economically large. Therefore, a preliminary examination of leverage ratios suggests the presence of a transitory or short-run component that leads to a gradual convergence in leverage ratios, as well as a permanent or long-run component that leads to highly persistent cross-sectional differences in leverage.

One potential concern with interpreting Figure 1 is the effect of survivorship bias. First, as we progress further away from the portfolio formation period, firms will naturally drop out of the sample due to exit through bankruptcy, acquisitions, or buyouts. Second, from 1984 onward, the length of time for which we can follow each portfolio is censored because we only have data through 2003. To address this issue, we repeat the analysis described above for our sample of Survivors. The results for this subset of firms are presented in Panels B and D of Figure 1, and reveal negligible differences between the survivors and the general population in terms of the evolution of leverage.

Additionally, we examine the event-time evolution of leverage among the subsample of exiting firms. Specifically, we compute, in similar event-time fashion, the average of the last nonmissing leverage observation for each firm that exits the sample, conditional on the last observation occurring before 2003—the last year of our sample. In so far as the last observable leverage ratio is a reasonable proxy for future leverage ratios, this analysis provides for an additional examination of the sample selection issue. The figures, not presented, are nearly identical to those found in Figure 1 but for the larger standard errors due to the significant reduction in the degrees of freedom (approximately 10% of firms exit the sample each period). That is, the leverage ratios of firms just prior to exiting the sample look similar to the leverage ratios of the broader sample of firms. Thus, firm exit does not appear to be driving the patterns observed in Figure 1.[6]

[6] A more thorough investigation of the survivorship issue would entail a model of firm exit and an appropriate identification strategy, i.e., instrument(s), to disentangle the exit decision from the capital structure decision. Such an investigation is beyond the scope of this study, though a potentially fruitful area for future research.

A second potential concern with interpreting the figure is the effect of the bounded support of leverage. In other words, because leverage is defined on the unit interval, average leverage will have a natural tendency to reflect away from the extremes of zero and one (Chang and Dasgupta (2006)). To examine this possibility, we transform leverage using a logit transformation, which maps leverage from the unit interval onto the whole real line. Specifically, we construct

$$Logit(Leverage_{it}) = \ln\left(\frac{Leverage_{it}}{1 - Leverage_{it}}\right).$$

A limitation of this transformation is that it is only defined on the open unit interval, implying that it cannot be applied to values of leverage exactly equal to zero or one. To address this limitation, we perform two separate analyses. The first excludes leverage values equal to zero or one. Null leverage values comprise 10.6% (8.4%) of the observations on book (market) leverage, while unit values of leverage comprise less than 0.001% (less than 0.001%) of the observations on book (market) leverage. The second analysis adds 0.001 to each value of leverage and excludes the few observations that are equal to one. Plots of both transformed leverage series are virtually identical, but for scale, to those presented in Figure 1 and, as such, are not presented. Thus, the patterns observed in Figure 1 do not appear to be an artifact of the bounded domain of leverage.

A final potential concern with interpreting the figure is that the sorting of firms by leverage may simply be capturing cross-sectional variation in underlying factors associated with cross-sectional variation in leverage (e.g., bankruptcy costs, agency costs, etc.). For example, previous research (e.g., Titman and Wessels (1988)) finds that leverage is positively correlated with firm size, so that members of the Very High portfolio may simply correspond to large firms, while members of the Low portfolio correspond to small firms. To address this possibility, we modify the sorting procedure.

Each calendar year, we begin by estimating a cross-sectional regression of leverage on 1-year lagged factors that have been previously identified by the literature as being relevant determinants of capital structure (e.g., Titman and Wessels (1988), Rajan and Zingales (1995), Mackay and Phillips (2005), and others).[7] Specifically, we regress leverage on firm size, profitability, tangibility, market-to-book, and industry indicator variables (Fama and French 38-industry classification).[8] We then sort firms into four portfolios based on the residuals from this regression, which we term "unexpected leverage," and then track the average *actual* leverage of each portfolio over the subsequent

[7] We also examine the effect of using contemporaneous determinants. The results are unchanged.

[8] We also examine an alternative specification suggested by Frank and Goyal (2004) consisting of firm size, market-to-book, collateral, intangible assets, an indicator for whether the firm paid a dividend (Graham, Lemmon, and Schallheim (1998)), and year and industry indicators. The results are largely unchanged by these modifications and, as such, are not presented.

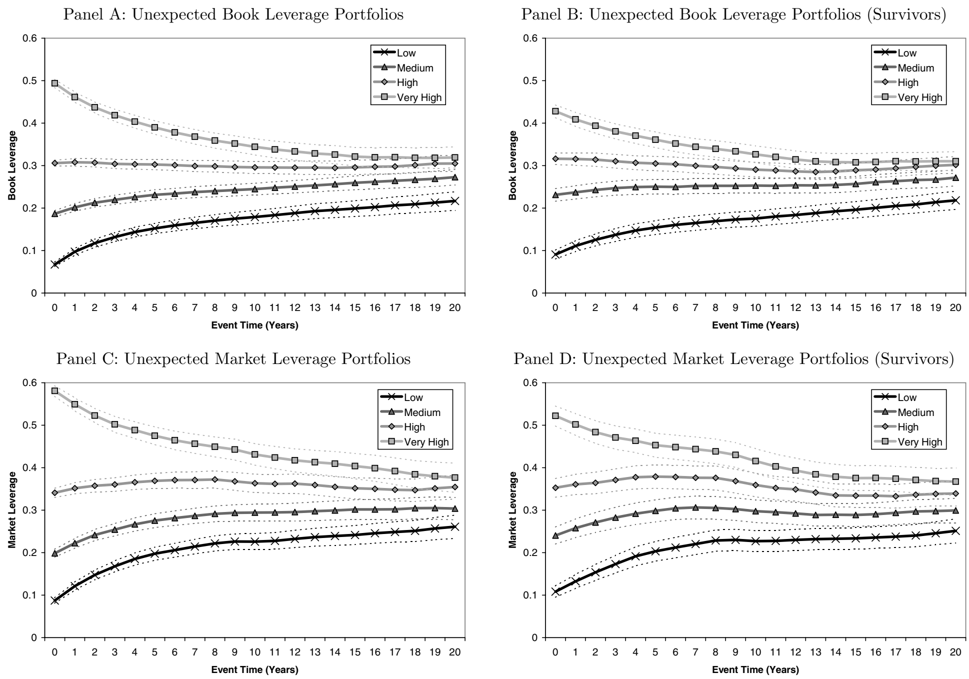


Figure 2. Average leverage of unexpected leverage portfolios in event time. The sample consists of all nonfinancial firms in the Compustat database from 1965 to 2003. Each panel presents the average leverage of four portfolios in event time, where year zero is the portfolio formation period. That is, for each calendar year, we form four portfolios by ranking firms based on their unexpected leverage (defined below). Holding the portfolios fixed for the next 20 years, we compute the average leverage for each portfolio. For example, in 1975 we sort firms into four groups based on their unexpected leverage ratios. For each year from 1975 to 1994, we compute the average leverage for each of these four portfolios. We repeat this process of sorting and averaging for every year in our sample horizon. After performing this sorting and averaging for each year from 1965 to 2003, we then average the average leverages across "event time" to obtain the bold lines in the figure. The surrounding dashed lines represent 95% confidence intervals. The results for book and market leverage are presented in Panels A and C, where book (market) leverage is defined as the ratio of total debt to total assets (sum of total debt and market equity). Panels B and D present similar results for book and market leverage, respectively, but for a subsample of firms required to exist for at least 20 years (consequently, we can only perform the portfolio formation through 1984 for this sample). Unexpected leverage is defined as the residuals from a cross-sectional regression of leverage on firm size, profitability, market-to-book, and tangibility, where all independent variables are lagged 1 year. Also included in the regression are industry indicator variables (Fama and French 38-industry classifications). Variable definitions are provided in the Appendix.

20 years. An attractive feature of this approach is that, by estimating the regressions each year, we allow the marginal effect of each factor to vary over time.

This approach homogenizes—in a linear sense—the sample with respect to previously identified determinants of leverage. The consequence is that each portfolio contains firms that are uncorrelated along the observable characteristics. To the extent that the regression model is well specified, the expectation is twofold. First, there should be less cross-sectional variation in the formation period as a result of sorting on the residual leverage. Second, any differences in the average leverage levels across portfolios should quickly disappear as the impact of the random shock dissipates. Neither outcome is the case. Figure 2 presents the graphs for the unexpected leverage portfolios and shows that the results are nearly identical to those presented in Figure 1.

In particular, leverage still varies over a large range (43% for book leverage, 49% for market leverage) in the portfolio formation period, suggesting that most of the variation in capital structure is found in the residual of existing specifications. As time progresses, we see similar patterns of convergence across the portfolios. Finally, while the spread in average leverage across the portfolios in each event year has decreased, there still remain significant differences for most periods. For example, even 20 years after the portfolio formation period, the average leverage of Low levered firms is significantly below that of all other portfolios, both in terms of book and market leverage. Additionally, the average leverage of Very High levered firms is significantly different from that of Medium levered firms. These differences are economically significant as well, with the range in leverage across the portfolios in event year 20 equal to 11% (12%) for book (market) leverage. Thus, even after removing all observable heterogeneity associated with traditional determinants of capital structure, leverage differences still remain highly persistent.

In sum, the figures reveal several interesting features of the data generating process for leverage. Figure 1 shows that extreme values of leverage—both very high and low—tend to converge significantly over time, on average. Further, this convergence appears to be concentrated in the short run. Finally, despite this convergence, differences in leverage ratios across firms are highly persistent, as indicated by the incomplete convergence even after 20 years.

Figure 2 shows that controlling for previously identified leverage determinants has two effects. The first effect is a short-run phenomenon that is seen in the decreased dispersion in the portfolio formation period. The second effect is a long-run phenomenon that is seen in the increased convergence of the portfolios over time. However, both of these effects appear, at first glance, to be relatively small. The variation across the leverage portfolios sorted by unexpected leverage is not much smaller than the variation across the leverage portfolios sorted by actual leverage. Similarly, the convergence among the unexpected leverage portfolios still leaves statistically and economically significant differences across some portfolios 20 years later.

This last result is both interesting and troubling. It is troubling because it suggests that traditional control variables do not appear to account for much of the variation in leverage. The result is interesting because it suggests that an important factor is missing from existing specifications of leverage and that this factor contains a significant, permanent, or time-invariant component, as well as a slowly decaying transitory component. The next section further examines these features of leverage with an eye toward quantifying their importance relative to existing capital structure determinants.

III. The Economic Importance of Persistence in Capital Structures

A prominent feature in Figures 1 and 2 is that leverage appears to be persistent. We investigate this feature of the data further using three sets of analysis. First, we examine the role that firms' initial leverage ratios play in determining future leverage ratios. Second, we perform a variance decomposition of leverage to quantify the explanatory power of existing determinants and test for the presence of unobserved firm-specific heterogeneity or, loosely speaking, firm fixed effects. Finally, we expand traditional empirical specifications using a distributed lag model of leverage to consider whether our findings are due to managers reacting to shifts in long-run or expected levels of previously identified determinants, as opposed to short-run fluctuations in their values.

A. The Role of Initial Leverage

One implication of the permanent component of leverage revealed by Figures 1 and 2 is that firms' future leverage ratios are closely related to their initial leverage ratios. However, the figures provide limited *quantitative* evidence of initial leverage ratios' economic importance. To measure the impact of initial leverage on future leverage, we estimate the following regression,

$$Leverage_{it} = \alpha + \beta X_{it-1} + \gamma Leverage_{i0} + \nu_t + \varepsilon_{it}, \tag{1}$$

where i indexes firms; t indexes years; X is a set of 1-year lagged control variables; $Leverage_{i0}$ is firm i's initial leverage, which we proxy for with the first nonmissing value for leverage; ν is a year fixed effect; and ε is a random error term assumed to be possibly heteroskedastic and correlated within firms (Petersen (2007)). To avoid an identity at time zero, we drop the first observation for each firm from the regression. The coefficient of interest is γ, which measures the importance of firms' initial leverage values in determining future values of leverage. In light of the figures, γ estimates the average leverage difference across firms over time. By incorporating the control variable X, we can also compare the importance of firms' initial conditions relative to those of near contemporaneous determinants.

The results from estimating equation (1) using book and market leverage are presented in Table II. Panel A presents the results using the full sample; Panel B presents the results using the subsample of survivors. In order to facilitate comparisons, we scale each coefficient by the corresponding variable's standard deviation. Thus, each reported estimate measures the change in leverage corresponding to a one-standard deviation change in X. The first column presents the results for a model consisting of solely initial leverage. Panel A reveals that a one-standard deviation change in a firm's initial book leverage ratio (*Initial Leverage*) corresponds to an average change of 7% in future values of book leverage. An even larger effect, 11%, is found for market leverage. These findings are consistent with the behavior illustrated in Figure 1.

Next, we incorporate two sets of determinants into the specification. The first set consists of those variables suggested by Rajan and Zingales (1995) and subsequently used in many capital structure studies (e.g., Baker and Wurgler (2002), Frank and Goyal (2003), and Lemmon and Zender (2007)), augmented with calendar year fixed effects. The coefficient estimates are largely consistent with previous evidence, in terms of sign and statistical significance. Nevertheless, *Initial Leverage* remains highly significant and reveals a small (economic) change from 7% to 6% in the case of book leverage and 11% to 9% for market leverage. Despite the change, however, the statistical and economic magnitudes of these effects are dramatic when compared to the marginal effects of the other determinants. *Initial Leverage* is the single most important determinant of future capital structure in this specification. These results are consistent

Table II

The Effect of Initial Leverage on Future Leverage

The sample consists of all nonfinancial firms in the Compustat database from 1965 to 2003. The table presents parameter estimates, scaled by the standard deviation of the underlying variable, from panel OLS regressions of book and market leverage on several different specifications. The interpretation of each measure is the change in leverage associated with a one-standard deviation change in the determinant. For example, in the first column, a one-standard deviation change in initial leverage is associated with a 7% change in book leverage. Panel A presents results using the entire sample (All Firms). Panel B presents results using a subsample of firms required to survive for at least 20 years (Survivors). All variables are trimmed at the upper and lower 0.5-percentiles. Variable definitions are provided in the Appendix. Year Fixed Effects denote whether calendar year fixed effects are included in the specification. The t-statistics are computed using standard errors robust to both clustering (i.e., dependence) at the firm level and heteroskedasticity.

Panel A: All Firms

Variable	Book Leverage			Market Leverage		
Initial leverage	0.07	0.06	0.04	0.11	0.09	0.07
	(41.57)	(38.1)	(28.63)	(52.27)	(43.16)	(33.15)
Log(Sales)		0.02	0.03		0.02	0.03
		(11.58)	(16.89)		(13.73)	(18.09)
Market-to-book		−0.02	−0.01		−0.06	−0.04
		(−20.31)	(−12.11)		(−40.49)	(−35.68)
Profitability		−0.03	−0.03		−0.05	−0.04
		(−22.88)	(−23.78)		(−30.89)	(−30.03)
Tangibility		0.04	0.03		0.04	0.03
		(27.7)	(17.94)		(24.55)	(15.92)
Industry median lev.			0.06			0.08
			(42.63)			(46.27)
Cash flow vol.			0.00			0.00
			(−1.81)			(−3.35)
Dividend payer			−0.03			−0.05
			(−24.16)			(−29.82)
Year fixed effects	No	Yes	Yes	No	Yes	Yes
Adj. R^2	0.13	0.21	0.30	0.20	0.34	0.42
Obs.	117,914	117,914	117,914	117,300	117,300	117,300

(*continued*)

Table II—*Continued*

Panel B: Survivors

Variable	Book Leverage			Market Leverage		
Initial leverage	0.07	0.05	0.04	0.10	0.07	0.05
	(28.56)	(24.13)	(18.55)	(30.86)	(25.25)	(19.19)
Log(Sales)		0.02	0.02		0.03	0.03
		(9.13)	(11.45)		(11.78)	(12.47)
Market-to-book		−0.02	−0.01		−0.06	−0.04
		(−10.75)	(−6.21)		(−26.22)	(−22.83)
Profitability		−0.03	−0.03		−0.06	−0.05
		(−14.91)	(−15.64)		(−19.8)	(−20.12)
Tangibility		0.03	0.02		0.05	0.02
		(17.3)	(10.59)		(18.86)	(10.65)
Industry median lev.			0.05			0.08
			(26.52)			(32.65)
Cash flow vol.			−0.01			−0.01
			(−3.34)			(−3.75)
Dividend payer			−0.03			−0.04
			(−15.53)			(−18.84)
Year fixed effects	No	Yes	Yes	No	Yes	Yes
Adj. R^2	0.15	0.23	0.32	0.17	0.37	0.45
Obs.	68,736	68,736	68,736	68,224	68,224	68,224

with those presented in Figure 2, although the regression specification here presents a more stringent test of persistence in leverage differences since the determinants (other than initial leverage) are allowed to update each period.

The final specification incorporates additional variables motivated, in part, by Frank and Goyal (2004), who perform an exhaustive analysis of capital structure determinants. Despite statistically significant marginal effects, the inclusion of these additional variables does little to diminish *Initial Leverage*'s relative importance. The estimated coefficient on *Initial Leverage* is still highly significant and larger in magnitude than all other determinants but for *Industry Median Leverage*. The results emphasize that historical leverage is an important determinant of future leverage, even after controlling for traditional sources of variation.

The results for the Survivor sample in Panel B produce qualitatively similar findings. This similarity is reassuring as one concern with the results of Panel A is that initial leverage simply proxies for leverage lagged only a few periods. However, the median number of time-series observations for firms in our Survivor sample is 30 years. This statistic implies that initial leverage corresponds to a median lag time of 15 years. Thus, our estimate of γ for the Survivor sample implies that leverage 15 years ago is one of the most important determinants of leverage today, second only to a firm's near contemporaneous industry median leverage.

In concert with Figures 1 and 2, the importance of firms' initial leverage in determining future values of leverage suggests the following. First, an important component missing from existing specifications of leverage is a time-invariant factor(s). Second, most existing determinants seem to contain relatively little information about corporate leverage relative to this time-invariant factor. However, the possibility exists that while each variable has an economically small impact of its own, the existing determinants as a whole have an economically important impact on capital structure. Further, it is unclear precisely how important this latent firm-specific effect is, beyond its importance relative to other measurable determinants. The next section addresses these issues.

B. Variance Decomposition of Leverage

We begin with a nonparametric variance decomposition of book and market leverage. More precisely, we compute the within- and between-firm variation of leverage. For book leverage, these estimates are 12.9% and 19.9%, respectively. For market leverage, these estimates are 15.5% and 22.9%, respectively. Thus, the between-firm variation is approximately 50% larger than the within-firm variation for both measures of leverage. Intuitively, this suggests that leverage varies significantly more across firms, as opposed to within firms over time, consistent with the patterns observed in Figures 1 and 2.

We now turn to a parametric framework, analysis of covariance (ANCOVA), which enables us to decompose the variation in leverage attributable to different factors. We do so by estimating the following model of leverage:

$$Leverage_{it} = \alpha + \beta X_{it-1} + \eta_i + \nu_t + \varepsilon_{it}, \tag{2}$$

where η is a firm fixed effect and the other variables are as defined in equation (1). Because our goal at this stage is only to understand the relative importance of various determinants in capturing leverage variation, we focus on static specifications for this analysis. We relax this restriction below.

Table III presents the results of the variance decompositions for several specifications.[9] Each column in the table corresponds to a different model specification for leverage. The numbers in the body of the table, excluding the last row, correspond to the fraction of the total Type III partial sum of squares for a particular model.[10] That is, we divide the partial sum of squares for each effect by the aggregate partial sum of squares across all effects in the model. This provides a normalization that forces the columns to sum to one. Intuitively, what the values in the table correspond to are the fractions of the model sum

[9] Because of the large number of firms (19,700) in our panel and computer memory limitations, performing the variance decomposition on the entire sample is infeasible. As such, we randomly sample 10% of the firms in the panel and perform the analysis on this subsample. To minimize sampling error, we repeat the process of sampling and performing the variance decomposition 100 times and average the results.

[10] We use Type III sum of squares for two reasons. First, Type I sum of squares is sensitive to the ordering of the covariates because the computation involves sequentially projecting the dependent variable onto each variable. Second, our data are "unbalanced" in the sense that the number of observations corresponding to each effect is not the same (some firms have more observations than others). For a discussion of the methods used here, see Scheffe (1959).

Table III

Variance Decompositions

The sample consists of all nonfinancial firms in the Compustat database from 1965 to 2003. The table presents a variance decomposition for several different model specifications, with adjusted R-squares at the bottom. We compute the Type III partial sum of squares for each effect in the model and then normalize each estimate by the sum across the effects, forcing each column to sum to one. For example, in model (d) for book leverage, 4% of the explained sum of squares captured by the included covariates can be attributed to *Log*(*Sales*). *Firm FE* are firm fixed effects. *Year FE* are calendar year fixed effects. Variable definitions are provided in the Appendix.

	Book Leverage							Market Leverage						
Variable	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(a)	(b)	(c)	(d)	(e)	(f)	(g)
Firm FE	1.00	.	0.98	.	0.95	.	0.92	1.00	.	0.94	.	0.89	.	0.85
Year FE	.	1.00	0.02	0.11	0.01	0.05	0.01	.	1.00	0.06	0.21	0.06	0.12	0.05
Log(Sales)	.	.	.	0.04	0.01	0.07	0.02	.	.	.	0.02	0.02	0.04	0.03
Market-to-book	.	.	.	0.09	0.00	0.03	0.00	.	.	.	0.31	0.01	0.19	0.01
Profitability	.	.	.	0.11	0.01	0.06	0.01	.	.	.	0.08	0.01	0.05	0.01
Tangibility	.	.	.	0.27	0.01	0.08	0.01	.	.	.	0.09	0.01	0.03	0.01
Industry med lev	.	.	.	.	.	0.46	0.02	.	.	.	.	.	0.35	0.03
Cash flow vol	.	.	.	.	.	0.00	0.00	.	.	.	.	.	0.01	0.00
Dividend payer	.	.	.	.	.	0.16	0.01	.	.	.	.	.	0.13	0.01
Industry FE	.	.	.	0.38	.	0.09	.	.	.	.	0.29	.	0.08	.
Adj. R^2	0.60	0.01	0.61	0.18	0.63	0.29	0.65	0.61	0.06	0.65	0.31	0.68	0.41	0.70

of squares attributable to particular "effects" (e.g., firm, year, size, market-to-book, etc.). When only one effect is included in the model, the entire explained sum of squares is attributable to that effect. For example, when we examine only the firm effect (η_i) in column (a), the reported estimate takes on a value of 1.00, or 100%. The same is true in column (b), which examines the year effect in isolation.

The last row of Table III presents the adjusted R-square corresponding to each specification. Firm-specific effects alone, as displayed in column (a), capture 60% of the variation in book leverage, while the time effects capture 1% of the variation, as shown in column (b). Thus, consistent with our previous evidence, the majority of the total variation in capital structure is due to time-invariant factors.[11] This finding is important because it suggests that theories of capital structure based on volatile factors, in a time-series sense, are unlikely explanations for capital structure heterogeneity. Rather, leverage ratios are relatively stable over time.

Column (d) presents the results from the specification inspired by Rajan and Zingales (1995) and shows that asset tangibility (*Tangibility)* and industry fixed effects (*Industry FE*) account for most of the explanatory power in this specification. However, the adjusted R-square is 18% for book leverage (31% for market leverage), significantly lower, both statistically and practically speaking, than

[11] An early working paper version of Frank and Goyal (2005) provides evidence that aggregate leverage in the U.S. has been remarkably stable over time.

the adjusted R-square from the initial firm fixed effect regression. In column (e), we augment the specification in column (d) with firm fixed effects and note that the adjusted R-square for book leverage more than triples from 18% to 63%, highlighting the significant incremental contribution of the firm fixed effects.

Columns (f) and (g) present similar results using the specification inspired by Frank and Goyal (2004). Including firm fixed effects into the book leverage specification again leads to a substantial increase in the adjusted R-square from 29% to 65%. The results using market leverage are analogous. In unreported results, we also examine the effect of further expanding the specification to include measures of asset volatility (Faulkender and Petersen (2006)), proxies for the marginal tax rate (Graham (1996)), and higher-order polynomial terms of each determinant to capture potential nonlinear associations. All of these modifications have little effect on our results. In fact, a kitchen sink model that includes linear, quadratic, and cubic terms for all of the determinants mentioned increases the adjusted R-square by less than 8% for book leverage.

At this point, it is worth clarifying precisely what the results of the variance decomposition imply. First, the results show that leverage contains an important unobserved firm-specific component that is not fully captured by existing determinants. In other words, controlling for previously identified determinants does not alleviate the concern over heterogeneous intercepts—a result with potentially important implications for empirical studies that we elaborate below. Second, the variance decomposition reinforces the finding that the majority of the total variation in leverage is due to cross-sectional differences, as opposed to time-series variation.

The variance decomposition does *not* necessarily imply, however, that existing determinants are of little value in explaining variation in leverage ratios. If much of the explanatory power of existing determinants comes from cross-sectional variation, as opposed to time-series variation, then the importance of these determinants will necessarily fall as the firm fixed effects remove all such variation.[12] A comparison of adjusted R-squares across specifications suggests that this is indeed the case.

Column (a) of Table III reveals that a specification of only firm fixed effects yields an adjusted R-square of 60%. Column (e) suggests that including existing determinants in this specification boosts the adjusted R-square by only 3%, yet existing determinants alone explain 18% of the variation in leverage (column (d)). Thus, much of the explanatory power of existing determinants comes from cross-sectional, as opposed to time-series, variation. However, the explanatory power of existing determinants falls well short of accounting for the variation captured by the firm fixed effects (18% vs. 60%). This finding raises the possibility that time-series variation in the standard determinants is simply "noise" around their long-run equilibrium levels, about which managers are really concerned. We examine this possibility in the next subsection.

[12] See the study by Griliches and Mairesse (1995) for a critical discussion of fixed effects estimators.

C. Short-Run versus Long-Run Effects

If managers are concerned only about changes in the long-run equilibrium level of leverage determinants, then it is perhaps unsurprising that existing specifications, such as equation (2), explain relatively little of the variation in leverage. Equation (2) assumes that the effect of X on *Leverage* is delayed by one period and is "complete," in the sense that there are no persistent effects of changes in X on *Leverage*. However, if managers are slow to respond to changes in X or if managers gradually adjust *Leverage* to changes in X, then equation (2) provides an incomplete description of the leverage data generating process. Alternatively, if managers ignore short-term or transitory fluctuations in the factors that determine leverage, then, again, equation (2) provides an incomplete description of capital structure. Given the relative infrequency of capital structure adjustments (Leary and Roberts (2005)), these alternative behaviors seem plausible.[13]

To examine these alternatives, we estimate a distributed lag model of leverage, effectively expanding the traditional specification in equation (1) to incorporate deeper lags of the independent variables:

$$Leverage_{it} = \alpha + \sum_{s=1}^{n} \beta_s X_{it-s} + \gamma Leverage_{i0} + \nu_t + \varepsilon_{it}. \tag{3}$$

Here, n corresponds to the lag order of each independent variable contained in the vector X. Again, we assume that ε is potentially heteroskedastic and correlated within firms, and we drop the first observation for each firm to avoid an identity at time zero.

To determine the appropriate lag length, we undertake two specification searches using the Akaike Information Criterion (AIC) and the Bayesian Information Criterion (BIC). Both searches suggest a lag length of eight periods, though there is little difference, statistically speaking, between 4 and 10 periods. As such, we present results corresponding to $n = 8$; however, in unreported analysis, we also examine the effect of alternative lag orders (e.g., 4 through 10). The results are qualitatively similar and therefore are not presented.

Rather than presenting all of the estimated coefficients, Table IV presents two summary measures for each independent variable in X. The first measure is the scaled short-run impact, defined as the product of the estimated coefficient on the 1-year lag and the corresponding variable's standard deviation. This measure presents the short-run change in leverage associated with a one-standard deviation change in the determinant. For example, a one-standard deviation increase in a firm's profitability has a near-immediate (one-period lag) impact of lowering book leverage by 2%.

The second measure is the scaled long-run impact, defined as the sum of the eight estimated slope coefficients times the standard deviation of the corresponding variable. This measure is interpreted as the change in the expectation (or long-run, equilibrium level) of *Leverage* given a one-standard deviation

[13] We thank the associate editor for suggesting this possibility and inspiring this analysis.

Table IV

A Distributed Lag Model of Leverage

The sample consists of all nonfinancial firms in the Compustat database from 1965 to 2003. The table presents summary measures from panel OLS regressions of book and market leverage on distributed lag specifications. Each independent variable contains eight lags. We present two scaled summary measures: the short-run multiplier and long-run multiplier, each multiplied by the standard deviation of the corresponding determinant. The short-run multiplier is the coefficient on the 1-year lag variable and presents the change in leverage associated with a one-standard deviation change in the determinant. The long-run multiplier is the sum of the coefficients on all lags. The interpretation of each measure is the change in the expectation (or long-run equilibrium level) of leverage associated with a one-standard deviation change in the expectation (or long-run equilibrium level) of the determinant. For example, a one-standard deviation change in *Profitability* is associated with a short-run change in book leverage of 2% and a long-run change of 5%. All variables are trimmed at the upper and lower 0.5-percentiles. Year Fixed Effects denote whether calendar year fixed effects are included in the specification. The *t*-statistics are computed using standard errors adjusted for clustering (i.e., dependence) at the firm level. Variable definitions are provided in the Appendix; however, for this analysis, *Log*(*Sales*) is first detrended by using the residuals from a regression of *Log*(*Sales*) on a time trend.

	Book Leverage		Market Leverage	
Variable	Short Run	Long Run	Short Run	Long Run
Initial leverage	0.03		0.04	
	(12.92)		(14)	
Log(Sales)	0.04	0.02	0.04	0.03
	(4.74)	(9.3)	(4.47)	(9.3)
Market-to-book	0.00	−0.01	−0.03	−0.06
	(−3.81)	(−5.44)	(−17.92)	(−20.05)
Profitability	−0.02	−0.05	−0.03	−0.08
	(−10.98)	(−15.46)	(−14.84)	(−21.71)
Tangibility	0.04	0.02	0.04	0.03
	(10.19)	(8.59)	(11.11)	(10.39)
Industry median lev.	0.04	0.06	0.07	0.08
	(21.41)	(22.01)	(29.65)	(23.48)
Cash flow vol.	−0.03	−0.01	−0.09	−0.02
	(−2.77)	(−6.09)	(−7.54)	(−6.36)
Dividend payer	−0.03	−0.03	−0.04	−0.04
	(−14.71)	(−11.12)	(−18.37)	(−14.21)
Year fixed effects	Yes		Yes	
Adj. R^2	0.29		0.43	
Obs.	55,355		54,963	

change in the expectation of X_k, where k corresponds to a particular determinant.[14] For example, a one-standard deviation increase in expected *Profitability* is associated with a 5% decrease in expected *Leverage* in the full sample—an

[14] For this interpretation to be strictly true, we require two conditions. First, *Leverage* and its determinants must be jointly stationary. With the exception of *Log*(*Sales*), this condition is true according to the testing strategy proposed by Levin and Lin (1993). To ensure the stationarity of firm size, we use the residuals from a regression of *Log*(*Sales*) on a time trend in our regressions. The second condition requires that the marginal effect of determinants lagged more than 8 years is zero. Casual inspection of higher-order regressions suggests that this is a reasonable assumption.

effect that is more than twice as large as the short-run impact of shocks to profitability. Moreover, the sign of the effect indicates that firms with persistently high profitability tend to have even lower leverage than those firms experiencing transitory shocks to their profitability—consistent with dynamic contracting theories (DeMarzo and Fishman (2001)).

In many instances, the long-run multiplier is not necessarily greater than, or even different from, the short-run multiplier. This feature is a result of an unrestricted lag distribution. That is, we place no constraints (e.g., polynomial, geometric) on the slope coefficients. Consider the marginal effect of cash flow volatility. The short-run multiplier suggests that a one-standard deviation increase in cash flow volatility coincides with an initial 3% decrease in book leverage, whereas the long-run multiplier suggests that the long-run impact of this change is only 1%. This pattern suggests an initially large response of leverage to cash flow volatility and an eventual reduction of the impact. Overall, the results are somewhat mixed in the sense that some determinants exhibit a stronger short-run sensitivity to changes in leverage determinants, whereas others exhibit a stronger long-run sensitivity.

For our purposes, the implications of this analysis are as follows. First, despite expanding the dynamic structure of the model along each determinant dimension, initial leverage is still economically and statistically significant in every specification. Thus, even when firms are allowed to respond to both short-run and long-run shifts in leverage determinants, initial leverage ratios still play an important role in determining future leverage ratios.

Second, the long-run impact of each determinant is relatively small when compared to the unconditional standard deviation of leverage, 21% for book and 26% for market. For example, a one-standard deviation permanent change in median industry leverage, the single most important determinant of leverage, corresponds to a 6% change in expected, or long-run, book leverage. This estimate is less than 29% (6%/21%) of the typical unconditional variation in leverage. In other words, large changes in the long-run equilibrium levels of the determinants lead to relatively small changes in the unconditional expectation of leverage.[15]

In sum, previously identified determinants account for relatively little of the variation in leverage, regardless of whether one takes a short-run or a long-run perspective. Coupled with the presence of a significant unobserved firm-specific effect, these findings suggest that the identification problem in capital structure studies is more difficult than implied by previous research.

IV. Implications for Empirical Studies of Capital Structure

One implication of our analysis in the previous section is that simple pooled ordinary least squares (OLS) regressions of leverage ratios, the workhorses of the empirical capital structure literature, are likely to be misspecified because

[15] Unreported estimates from the sample of firms that survive for at least 20 years reveal qualitatively similar findings.

they ignore a significant time-invariant component of leverage ratios that is likely correlated with traditional right-hand side variables. The presence of this unobserved component of leverage suggests that one potential concern with existing estimates of capital structure regressions is that the parameter estimates and inferences drawn from the data may be tainted by omitted variable bias (Arellano (2003) and Hsiao (2003)). For example, differences in technologies, market power, and/or managerial behavior have long motivated the incorporation of firm fixed effects in investment (e.g., Kuh (1963)) and production function regressions (e.g., Mundlak (1962) and Hoch (1962)) as estimates of such factors are difficult to obtain. In so far as these unobserved factors are slowly changing, if not strictly time-invariant, their impact on capital structure is mostly absorbed by the firm-specific effect.

Similarly, the unidentified transitory component revealed by Figures 1 and 2 may also have consequences for traditional OLS leverage regressions. The presence of this component will lead to inefficient estimates and can adversely affect statistical inference (e.g., Greene (1993)). In so far as financial adjustment is costly, economic shocks are persistent, or autocorrelated independent variables are omitted from the specification, one might suspect the presence of serial correlation in the error structure.

To explore the importance of these considerations, Table V presents the results of estimating capital structure regressions using a pooled OLS approach that ignores firm-specific effects and serial correlation in the error structure, with the results from using a fixed effect specification and potentially serially correlated errors. More precisely, the pooled OLS approach estimates equation (1), excluding initial leverage, and assumes that the errors are possibly heteroskedastic and equicorrelated within firms (Petersen (2007)). The fixed effect estimation estimates

$$Leverage_{it} = \alpha + \beta X_{it-1} + \eta_i + \nu_t + u_{it}, \tag{4}$$

where

$$u_{it} = \rho u_{it-1} + \omega_{it},$$

u is assumed to be stationary, and ω is assumed to be serially and cross-sectionally uncorrelated but possibly heteroskedastic.

The results illustrate that most determinants are highly statistically significant, regardless of the model specification. However, the estimated magnitudes are very sensitive to the specification. Additionally, the estimated serial correlation coefficient, while bounded well below one, is statistically large for both book (0.66) and market (0.65) leverage, consistent with a gradual decay in the impact of leverage shocks. Focusing on the book leverage results, every coefficient estimate experiences fairly large declines in magnitude moving from the pooled OLS specification to the fixed effect specification. The coefficients in the book (market) leverage specification decline by approximately 82% (62%), on average.

Table V

Parameter Sensitivities to Model Specification

The sample consists of all nonfinancial firms in the Compustat database from 1965 to 2003. The table presents parameter estimates and t-statistics in parentheses for book and market leverage regressions. For each measure of leverage, two sets of parameter estimates are presented corresponding to a pooled OLS regression and a firm fixed effects regression. The standard errors for the pooled OLS regression are robust to heteroskedasticity and within firm equicorrelation. The standard errors for the firm fixed effects regression are robust to heteroskedasticity and within-firm serial correlation. Also presented is the percent change in the magnitude of the coefficient when changing the model specification from Pooled OLS to Firm FE. AR(1) is the estimated first-order serial correlation coefficient. Variable definitions are provided in the Appendix.

	Book Leverage			Market Leverage		
Variable	Pooled OLS	Firm FE	% Change	Pooled OLS	Firm FE	% Change
Log(Sales)	0.013	0.008	−41%	0.016	0.024	52%
	(16.96)	(8.37)		(16.86)	(21.17)	
Market-to-book	−0.010	−0.003	−74%	−0.039	−0.001	−96%
	(−14.01)	(−7.14)		(−36.16)	(−3.23)	
Profitability	−0.159	−0.032	−80%	−0.232	−0.053	−77%
	(−23.93)	(−10.41)		(−30.38)	(−14.13)	
Tangibility	0.160	0.076	−52%	0.156	0.099	−36%
	(20.05)	(15.07)		(16.7)	(15.76)	
Industry median lev.	0.569	0.046	−92%	0.544	0.065	−88%
	(48.31)	(8.05)		(52.53)	(12.59)	
Cash flow vol.	−0.031	0.020	−163%	−0.099	0.010	−110%
	(−2.71)	(1.11)		(−8.14)	(0.48)	
Dividend payer	−0.084	0.000	−100%	−0.111	0.002	−102%
	(−28.22)	(0.02)		(−31.42)	(0.98)	
Year fixed effects	Yes	Yes		Yes	Yes	
Adj. R^2	0.262			0.376		
AR(1)		0.660			0.653	
Obs.	106,097	106,097		105,532	105,532	

The magnitudes of these differences are striking; however, one must take care with their interpretation, much like the results of the variance decomposition (Griliches and Mairesse (1995)). The fixed effects transformation removes the between variation, some of which is captured by existing determinants. Nonetheless, the variance decomposition shows that the fixed effects capture significantly more of the variation in leverage than can be attributed to existing determinants. Additionally, the distributed lag model shows that neither short-run fluctuations nor long-run equilibrium considerations are likely responsible for this discrepancy. Therefore, existing determinants do not adequately proxy for either the permanent or the transitory omitted components of leverage.

The implication of ignoring the permanent component of the data in empirical models is summarized in Hsiao (2003, p. 8): "Ignoring the individual [i.e., firm] specific effects that exist among cross-sectional units but are not captured by the included explanatory variables can lead to . . . inconsistent or meaningless estimates of interesting parameters." While somewhat less severe, the finite sample implications of serially correlated errors can be significant for statistical inference (Greene (1993)).

To be clear, this is not to say that all empirical specifications should employ the firm fixed effects (a.k.a., within, or least-squares dummy variable) estimator with serial correlated errors. These assumptions relegate to the error structure prominent features of the leverage process that, ultimately, need to be understood. Worse, the within estimator sweeps out all of the cross-sectional variation so that the model cannot identify what is responsible for the majority of the variation in leverage ratios. Rather, the model specification decision must depend on the goal of the research. If the goal is to identify the marginal effects of a particular determinant, then firm fixed effects offer one of several alternatives (e.g., differencing, structural estimation, quasi-structural estimation as in Olley and Pakes (1996), natural experiments, etc.) to address concerns over omitted variables. Similarly, serially correlated errors are but one of several alternatives (e.g., lagged dependent variable, differencing, "new" serially correlated explanatory variables, etc.) for addressing autocorrelation in leverage. Ultimately, what these approaches provide is greater confidence in the identification of marginal effects.

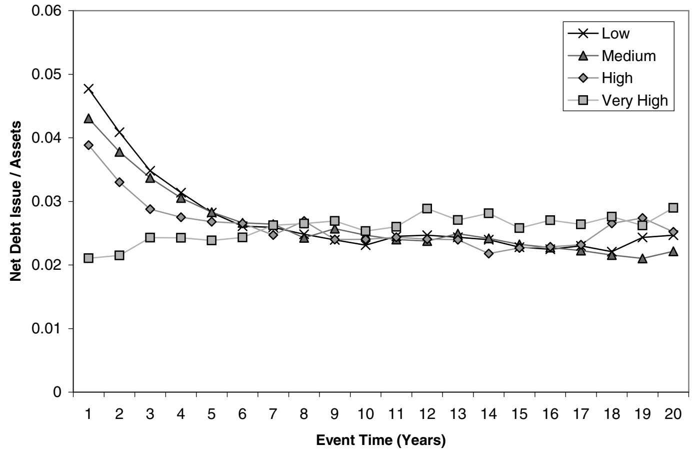
V. What Lies behind the Transitory Component?

A. Is "Active" Financial Management behind the Convergence?

Figure 2 illustrates that leverage ratios in the tails of the distribution converge significantly toward more moderate levels of leverage. That is, there is an important unobserved transitory component of leverage ratios. A number of recent studies suggest that firms actively manage their leverage ratios to maintain an optimal or target level of leverage (e.g., Graham and Harvey (2001), Hovakimian, Opler, and Titman (2001), Leary and Roberts (2005), Hovakimian (2006), Flannery and Rangan (2006), Kayhan and Titman (2007)). Simulation evidence in Shyam-Sunder and Myers (1999) and Chang and Dasgupta (2006), however, suggests restraint in equating mean-reversion with active leverage management. In this section, we provide additional evidence on whether the dynamics apparent in our figures are a function of active management toward desired leverage ratios or of more passive behavior.

To distinguish between these two possibilities, we examine the net security issuance activity of firms in the four unexpected leverage portfolios described earlier. The results are presented in Figure 3, Panels A and B. To ease the presentation, we suppress the confidence intervals. Focusing first on net debt issuance activity (Panel A), we find that, initially, the tendency to issue debt noticeably differs across the portfolios. What is interesting is that the propensity to issue (net) debt is monotonically negatively related to firms' leverage ratios. Firms undertake progressively more net debt issuing activity as we move from the Very High portfolio to the Low portfolio. These differences remain for 3 to 5 years before becoming largely indistinguishable. This finding is consistent with Leary and Roberts (2005) and Hovakimian (2006), who suggest that

Panel A: Net Debt Issuing Activity



Panel B: Net Equity Issuing Activity

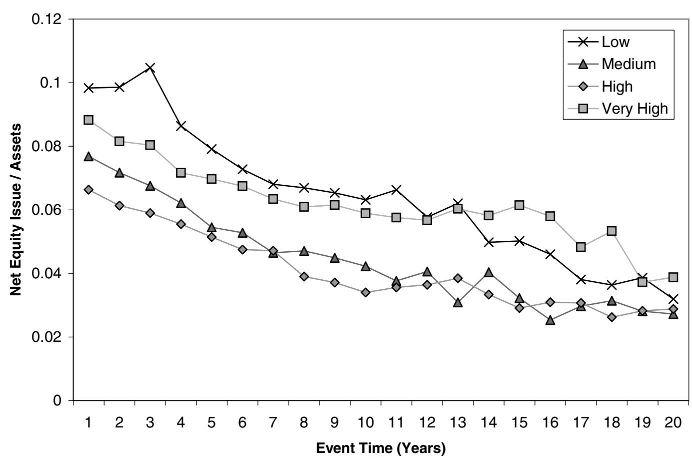


Figure 3. Financing behavior of unexpected leverage portfolios in event time. The sample consists of all nonfinancial firms in the Compustat database from 1965 to 2003. Panel A (B) presents the average net debt (net equity) issuance activity scaled by beginning of period assets for each of four unexpected leverage portfolios. That is, for each calendar year, we form four portfolios by ranking firms based on their unexpected leverage (defined below). Holding the portfolios fixed for the next 20 years, we compute the average net debt (net equity) issuances scaled by assets for each portfolio. For example, in 1975 we sort firms into four groups based on the unexpected leverage ratios. For each year from 1975 to 1994, we compute the average net debt (net equity) issued for each of these four portfolios. We repeat this process of sorting and averaging for every year in our sample horizon. After performing this sorting and averaging for each year from 1965 to 2003, we then average the average net issuances across "event time" to obtain the lines in the figure. Net debt issued is defined as the change in total debt from period $t-1$ to t divided by total assets in period $t-1$. Net equity issued is defined as the split-adjusted change in shares outstanding from $t-1$ to t times the average share price during the year divided by total assets in period $t-1$. Unexpected leverage is defined as the residuals from a cross-sectional regression of leverage on firm size, profitability, market-to-book, and tangibility, where all independent variables are lagged 1 year. Also included in the regression are industry indicator variables (Fama and French 38-industry classifications). Variable definitions are provided in the Appendix.

an important motivation behind debt policy is capital structure rebalancing. It also helps identify the mechanism behind the initial convergence of leverage ratios (i.e., the transitory component of leverage) observed in Figures 1 and 2.

Panel B presents the average net equity issuing activity across the four portfolios and reveals a different story.[16] In particular, firms with Low leverage are significantly more likely to issue equity. This result appears counterintuitive for a rebalancing story; however, because these firms have low leverage to begin with—in many cases zero leverage—net equity issuances have little or no effect on their capital structure and therefore are largely irrelevant in terms of their impact on the cross-sectional distribution of leverage. We also note that firms with Very High leverage appear to issue a significant amount of equity, on average, relative to the Medium and Low portfolios. This finding is consistent with (very) highly levered firms using equity to reduce their leverage (Lemmon and Zender (2007)) and also helps to further explain the initial decline in leverage for the Very High portfolio in Figures 1 and 2. There is little systematic difference between the Medium and High portfolios, though relative to the other two portfolios these firms appear to use less equity on average.

This analysis indicates that, even after controlling for the traditional determinants of capital structure, current leverage is an important state variable in net security issuance decisions. That is, the transitory component corresponding to the convergence of leverage ratios in Figures 1 and 2 appears to be driven, at least in part, by active rebalancing. Therefore, although random (Chang and Dasgupta (2006)) or pecking order (Shyam-Sunder and Myers (1999)) financing schemes coupled with mean reversion in cash flows can generate leverage paths similar to those observed in Figures 1 and 2, they are ultimately inconsistent with an important feature of the data—financing decisions are not random nor do they appear to follow the pecking order (see Helwege and Liang (1996), Frank and Goyal (2003), Fama and French (2005), and Leary and Roberts (2006)). Overall, our results illustrate that firms' financial policies are at least partially geared toward maintaining their leverage ratios relatively close to their long-run mean values, suggesting that managers are concerned with their leverage ratios.

B. Are Firms Adjusting Toward a Moving Target?

Several recent studies have emphasized the importance of time-varying leverage targets (e.g., Hovakimian, Opler, and Titman (2001) and Flannery and Rangan (2006)). However, this result would appear to contradict our findings, which instead suggest that any desired level of leverage, indeed leverage in general, is largely time-invariant. As such, we take a closer look at this claim.

Specifically, a direct way to quantify the importance of time-varying effects for identifying target leverage is to examine the impact that they have on the estimated speed of adjustment (SOA) in a partial adjustment model. In particular, if time-varying characteristics are a crucial component of firms' target leverage,

[16] See Dittmar (2000) and Dittmar and Thakor (2007) for recent investigations into why firms retire and issue equity, respectively.

as previous studies suggest, then omitting them will substantially reduce the estimated SOA since firms will be adjusting to a target that is different from the one modeled by the econometrician. This exercise is equivalent to adding measurement error to the target, precisely as Flannery and Rangan (2006) do to illustrate the reduction in adjustment speed that accompanies decreases in the signal-to-noise ratio of the target estimate.

Table VI presents the raw (i.e., unscaled) parameter estimates and t-statistics from estimating several different partial adjustment specifications for leverage,

Table VI

Speed of Adjustment

The sample consists of all nonfinancial firms in the Compustat database from 1965 to 2003. The table provides estimates for the following autoregressive model of book leverage:

$$\Delta Leverage_{it} = \alpha + \lambda(\beta X_{it-1} - Leverage_{it-1}) + \eta_i + \nu_t + \varepsilon_{it},$$

where X is a set of pre-determined covariates, η is a time-invariant effect potentially correlated with X, ν is a firm-invariant effect potentially correlated with X, and ε is a random error term that is potentially heteroskedastic and autocorrelated within firms. Columns (a) through (c) present ordinary least squares estimates that restrict $\eta_i = 0$. Columns (d) and (e) present OLS estimates after transforming the original data into deviations from firm-specific means. Columns (f) and (g) present system GMM (Blundell and Bond (1998)) estimates. The speed of adjustment, or SOA, is given by λ. The table presents estimates of the β's, where standard errors are computed using a Taylor approximation (i.e., the delta method). Leverage half-life is defined as the time (in years) that it takes a firm to adjust back to the target leverage after a one-unit shock to ε, $\ln(0.5)/\ln(1 - \hat{\lambda})$. Variable definitions are provided in the Appendix. The t-statistics are in parentheses and are robust to heteroskedasticity and within-firm correlation.

	Pooled OLS			Firm Fixed Effects		GMM	
Variable	(a)	(b)	(c)	(d)	(e)	(f)	(g)
SOA	0.13	0.15	0.17	0.36	0.39	0.22	0.25
	(63.8)	(63.31)	(62.92)	(80.05)	(79.19)	(27.44)	(29.36)
Initial leverage		0.25	0.17				
		(23.97)	(18.34)				
Log(Sales)			0.00		0.01		0.00
			(−2.6)		(5.98)		(−4.87)
Market-to-book			−0.01		0.00		−0.01
			(−4.89)		(−4.47)		(−6.02)
Profitability			−0.18		−0.10		0.01
			(−11.97)		(−9.19)		(0.63)
Tangibility			0.09		0.11		0.10
			(10.05)		(8.81)		(12.36)
Industry median lev.			0.42		0.13		0.32
			(24.11)		(9.01)		(18.79)
Half-life	4.96	4.35	3.62	1.53	1.42	2.82	2.41
	(59.45)	(58.4)	(57.09)	(63.32)	(61.42)	(24.2)	(25.34)
Year fixed effects	No	No	Yes	No	Yes	No	Yes
R^2	0.72	0.72	0.72	0.72	0.71	.	.
Obs.	145,726	145,726	145,726	145,726	145,726	145,726	145,726

$$\Delta Leverage_{it} = \alpha_0 + \lambda\left(\mu^*_{it} - Leverage_{it-1}\right) + \varepsilon_{it}, \tag{5}$$

where

$$\mu^*_{it} = \beta X_{it-1} + \eta_i + \nu_t$$

is "target" leverage and all other variables are as defined earlier. The parameter λ, the SOA, measures the fraction of the gap between last period's leverage and this period's target that firms close each period. The β parameters measure the sensitivity of the target to variation in the determinants, and the standard errors for these parameters are computed using a Taylor expansion (the "delta" method).[17]

We present results from three estimation techniques (pooled OLS, firm fixed effects, and generalized method of moments GMM) to benchmark our results with previous studies and reinforce our conclusions. Column (a) shows that the speed of adjustment to a population constant (i.e., a single intercept term in the target specification) is approximately 13% per year. Columns (b) and (c), under the Pooled OLS heading, present coefficient estimates using firms' initial leverage ratios as a determinant of target leverage. These results show that, even in the presence of one-period lagged leverage, firms' initial leverage ratios are statistically significant determinants of future leverage ratios or, alternatively, of firms' leverage targets. In fact, column (c) shows that initial leverage is also an economically significant determinant of future leverage, more important than most other factors, including the market-to-book ratio, asset tangibility, and firm size. (This can be seen more clearly by multiplying the coefficient of each regressor by its standard deviation found in Table I.) Thus, again, we see that initially high (low) levered firms tend to remain as such even after controlling for the current level of leverage, as well as changes in near contemporaneous determinants.

Next, we note that the Pooled OLS and firm fixed effects estimated adjustment speeds (SOA) closely match those found in Fama and French (2002) and Flannery and Rangan (2006), respectively, suggesting that firms close 13% to 17% or 36% to 39% of the gap between last period's leverage and this period's target. Moving from the Pooled OLS to the firm fixed effects regression results in an increase in the speed of adjustment of approximately 22% (39–17%) in absolute terms and over 129% in relative terms ((39–17%)/17%), consistent with the importance of firm-specific effects for identifying leverage targets. This finding is Flannery and Rangan's (2006) insight.

However, within these two estimation methods we see that the difference in adjustment speeds arising from the inclusion of time-varying determinants in the target specification is negligible. Consider the change in the SOA moving from column (b) to (c). Column (b) assumes that all firms target a fraction of their initial leverage ratio, which, by definition, does not change over time.

[17] Because we are not using maximum likelihood, the estimation procedure estimates the product of λ and β, from which we must derive β by dividing by the estimated λ. Because of this nonlinearity, we are forced to use a Taylor expansion to derive the appropriate standard error and t-statistic.

Adding time-varying determinants increases the speed of adjustment from 15% to 17%, a difference of only 2%. In the firm fixed effects regressions, including time-varying determinants increases the speed of adjustment from 36% to 39%, a difference of only 3%. Relative to the differences in adjustment speeds resulting from incorporating firm fixed effects into the target, these differences are negligible. Thus, what is most important for identifying target leverage is simply firms' long-run mean leverage ratios, as opposed to firm characteristics or macroeconomic factors, consistent with our earlier results highlighting the relatively small time-series variation in capital structures and the low explanatory power of traditional determinants.

While the Pooled OLS and firm fixed effects regressions enable us to directly compare our results with previous studies, they both suffer from potentially severe biases, particularly with respect to the speed of adjustment (and, consequently, with respect to β, which is derived from the estimated SOA). Specifically, pooled OLS regressions that (incorrectly) ignore the presence of fixed effects will result in an estimated SOA that is biased downward, while firm fixed effects regressions will result in an estimated SOA that is biased upward (Hsiao (2003)). As such, columns (f) and (g) in Table V present results using system GMM estimation (Blundell and Bond (1998)), which is specifically designed to address the econometric concerns associated with estimating dynamic panel data models in the presence of firm fixed effects.[18] The results confirm that including time-varying characteristics in the target specification has little effect on the estimated speed of adjustment (22% vs. 25%) and, consequently, contributes little toward the identification of firms' target leverage ratios. Thus, these results further reinforce our finding that any target leverage is largely time-invariant and that existing determinants are of limited use in explaining cross-sectional variation in leverage.

The GMM method also reveals a less extreme estimate of the speed of adjustment, approximately equal to the midpoint of the Pooled OLS and firm fixed effects estimates. This finding is comforting in that the biases in the pooled and fixed effects estimates go in opposite directions, suggesting that the true parameter lies somewhere between these two extremes. This distinction from Flannery and Rangan's (2006) estimate is due to differences in the instruments used, as well as in the estimation strategies, and highlights the difficulty in obtaining accurate parameter estimates in these models.[19]

In sum, these results suggest that leverage is mean-reverting around a largely time-invariant, firm-specific level, contrary to the conclusions of

[18] In particular, the system GMM approach includes variable levels, as well as differences, in the instrument set to address the problem of persistent regressors, which, when differenced, contain little information for parameter identification.

[19] The system GMM utilizes lagged levels and differences of the dependent variable and exogenous variables. Flannery and Rangan rely on book leverage as an instrument, which we suspect is a weak instrument. Not surprisingly, their results are very close to those found using the within transformation (i.e., Firm Fixed Effects). We also note that small sample biases (Huang and Ritter (2007)) and nonlinearities (Caballero and Engel (2004)) may be relevant for the specification and estimation of these models, though these issues fall outside the scope of this study.

Hovakimian, Opler, and Titman (2001) and Flannery and Rangan (2006). While we are hesitant to draw any inferences based solely on this mean-reversion—for reasons discussed earlier— our results in Section V.A (and the evidence from recent studies cited therein) suggest that this mean-reversion is accomplished, at least in part, by active management of leverage ratios via net debt issuing activity.

VI. What Lies behind the Permanent Component?

The key question at this point is: What is the economic mechanism generating the unobserved permanent component revealed by Figure 2? Or, less formally, why do some firms always seem to have high leverage, while others always seem to have low leverage, despite being similar along many dimensions (e.g., size, profitability, industry, etc.)? While a complete answer to this question is beyond the scope of this paper, we take a step toward this goal by investigating whether differences in leverage persist *back* in time.

A. How Far Back Does the Firm-Specific Effect Go?

To answer this question, we form unexpected leverage portfolios at the time of the IPO. Because of a significant reduction in the number of observations (approximately 5,000 IPO firms), we modify slightly the analysis in Figure 2. We begin by computing initial leverage as the average of the first three public observations on leverage in event years 0 (year of the IPO), 1, and 2. We compute initial values of the corresponding determinants (size, market-to-book, profitability, and tangibility) in a similar manner. Averaging helps minimize noise and mitigate the effect of extreme observations in this smaller sample. We then estimate a regression of initial leverage on the initial values for the determinants, as well as indicator variables for calendar year and industry as of the time of the IPO. The calendar year variables help address the differential effect of IPOs in hot versus cold markets in the regression. The residuals from this regression are then used to sort firms into four unexpected leverage portfolios, as described earlier.

Figure 4 plots the average actual leverage and 95% confidence interval for each of the four portfolios in event time, where event year 0 is the year of the IPO. We note the following. First, the regression eliminates any meaningful difference between the Low and Medium portfolios. Second, the results are somewhat noisier compared to those in Figure 2, primarily because of the reduced number of observations. Overall, however, the general patterns and implications are quite similar. Despite some initial convergence, differences in leverage across firms persist for quite some time. More importantly, for book leverage, we see that these differences are established prior to the IPO. Firms with high (low) leverage as private firms also have high (low) leverage as public firms.[20] It therefore appears that the significant changes in the distribution of

[20] Unreported results for the sample of IPO firms surviving for at least 20 years reveal similar patterns.

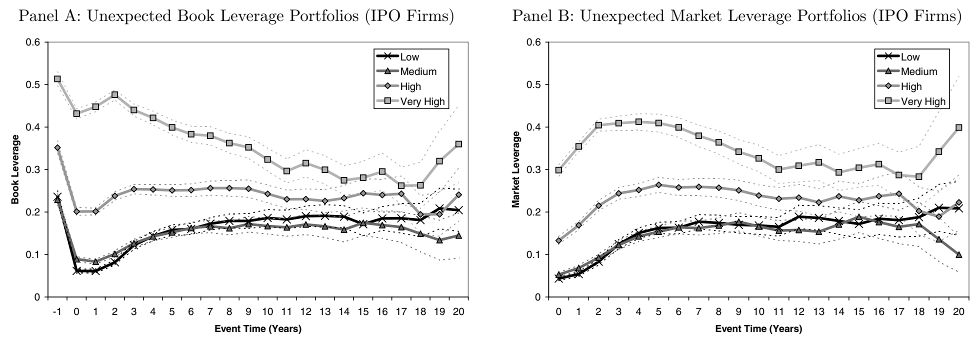


Figure 4. Average leverage of unexpected leverage portfolios in event time (IPO subsample). The sample consists of all nonfinancial firms in the Compustat database from 1965 to 2003 for which we have an IPO date. Each panel presents the average leverage of four portfolios in event time, where year 0 is the firm's IPO. Portfolios are formed based on quartiles of firms' unexpected initial leverage ratios, defined as the residuals from a cross-sectional regression of initial values for leverage on initial values for firm size, profitability, market-to-book, and tangibility. Also included in the regression are year and industry indicator variables (Fama and French 38-industry classifications). Variable definitions are provided in the Appendix. The initial values for continuous variables are computed as the average over the first 3 years of the firm's public existence (including the IPO year). Binary variables in the regression are measured at the IPO date. The average leverage for each quartile and event year, measured from the IPO, is plotted in the figure. Panels A and B present results for book and market leverage, respectively.

control, the information environment, and the access to capital markets that accompany the IPO do little to alter the relative costs and benefits that determine firms' preferred leverage ratios.

While the evidence suggests that firms maintain their pre-IPO rankings, we acknowledge the following potential concern. Leverage in the year prior to the IPO may not be representative of leverage as a private firm, more generally. That is, in the year prior to the IPO, the capital structure of the firm may have been altered in anticipation of this event. However, we make two remarks in response to this concern. First, the differences in book (as well as market) leverage across the portfolios in event year –1 are very large: 18% between the Very High and High portfolios and 12% between the High and Medium portfolios. We believe it is unlikely that firms alter their capital structure by these magnitudes in the year or two preceding the IPO. Second, this belief is supported by the evidence in Kaplan, Sensoy, and Stromberg (2005), who show that, other than human capital, the operations, assets, and investment policy of firms remain very stable from the literal birth of a firm to several years after the IPO.

Although data on private firms in the United States are limited, the FAME database from Bureau van Dijk contains data on a significant number of private firms in the United Kingdom. Rajan and Zingales (1995) argue that, except for differences in the rights of creditors in bankruptcy (driven by differences in bankruptcy laws between the United States and the United Kingdom), U.K. firms operate in an environment that is similar to that of U.S. firms. This fact

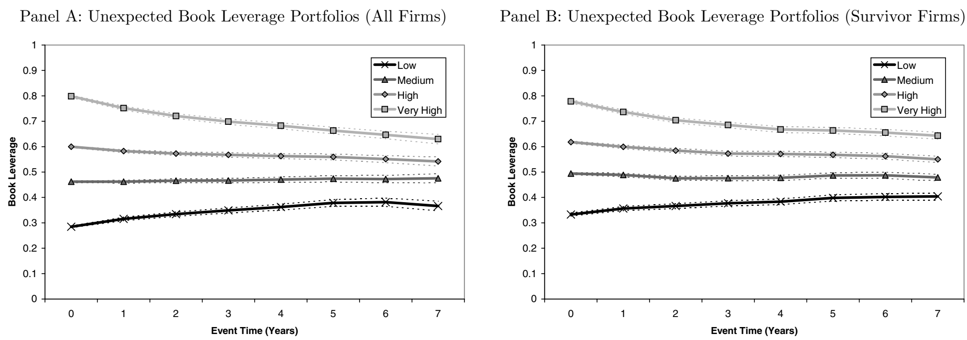


Figure 5. Average leverage of unexpected leverage portfolios in event time (privately held U.K. firms). The sample consists of nonfinancial privately held (unlisted) firms in the United Kingdom in the FAME database during the period from 1993 to 2002. Each Panel presents the average book leverage of four portfolios in event time, where year 0 is the portfolio formation period. That is, for each calendar year, we form four portfolios by ranking firms based on their unexpected leverage (defined below). Holding the portfolios fixed for the entire sample period, we compute the average actual book leverage for each portfolio. Unexpected leverage is defined as the residuals from a cross-sectional regression of leverage on firm size, profitability, market-to-book, and tangibility, where all independent variables are lagged 1 year. Also included in the regression are industry indicator variables (Fama and French 38-industry classifications). Variable definitions are provided in the Appendix. The average leverage for each quartile and event year is plotted in the figure. Panel A presents the results for the sample of all private firms; Panel B presents the results for the sample of all private firms that have nonmissing data for the entire 20-year panel.

is echoed and extended by Acharya, John, and Sundaram (2004) and Allen, Carletti, and Marquez (2006), who note the many similarities between the U.S. and U.K. economic and, in general, legal environments. We recognize, however, that other differences across the two countries, sample selection concerns, and other potential confounding effects limit the extrapolation of any inferences to U.S. firms. Nonetheless, given the paucity of information on the capital structures of private firms, we believe the following analysis can provide new insight into the behavior of capital structures for private firms, as well as provide an out-of-sample robustness check for our results based on U.S. data.

Figure 5 replicates the analysis in Figure 2 using data on privately held firms from the United Kingdom contained in the FAME database for the period 1993 to 2002.[21] Panel A presents results for all firms, while Panel B restricts attention to those firms with nonmissing data for the duration of the panel. The results illustrate a pattern for U.K. firms, which is virtually identical to that of

[21] Specifically, we focus only on unquoted (i.e., private) firms with a market capitalization of at least £700,000 or classified as medium or large by the Companies House. Since market-to-book is unobserved for private firms, we use the percentage change in sales as a proxy. The remaining variables in the portfolio formation regressions are similar to those used for the Compustat sample: the ratio of tangible assets to total assets, the log of total assets deflated by the United Kingdom's CPI, the ratio of operating profits to total assets, year dummies, and Fama and French 38 industry dummies. For further details regarding the FAME database see Michaely and Roberts (2006).

the public U.S. firms in Figure 2. As can be seen in Figure 5, there are relatively large differences between the average leverage of the unexpected leverage portfolios in the formation period (event date 0), the average leverage ratios for the portfolios converge over time, and significant differences in average leverage across the portfolios persist throughout the entire period.

Figure 5 suggests that the stability of leverage ratios documented above for public firms in the United States also characterizes the financing behavior of private firms in the United Kingdom. Overall, the findings indicate that differences in firms' capital structures are partially determined prior to the time that they go public and that these differences persist for many years.

VII. Conclusion

The last several decades have produced a wealth of information about corporate financial policies. However, our results suggest that the value of this information may be more limited than previously thought. We find that corporate capital structures are stable over long periods of time: Firms that have high (low) leverage tend to remain as such for over 20 years. This feature of the leverage data-generating process is present after controlling for firm entry and exit, as well as for previously identified determinants of capital structure. Thus, our findings show that the majority of variation in capital structure is time-invariant and that much of this variation is unaccounted for by existing empirical specifications.

While our findings paint a somewhat dim picture of existing empirical models of capital structure, they also narrow the scope for future investigations. The stability of capital structure over time suggests that the factors driving cross-sectional variation in leverage ratios are stable over long horizons as well. Additionally, because we also observe this persistence among private firms and after initial public offerings, these factors appear to be largely unaffected by the changes in capital market access, distribution of control, and information environment that occur at the time of the IPO.

Of course, the other implication of our findings is that the identification problem is more difficult than previously portrayed by much of the empirical literature. Static pooled OLS regressions of leverage ratios appear inadequate for dealing with the unobserved heterogeneity present in corporate capital structures. The presence of a significant unobserved transitory component suggests that dynamic specifications are necessary (e.g., Hennessy and Whited (2005, 2007)). The presence of a significant unobserved permanent component requires more creative identification strategies, such as fixed effects estimates (e.g., Bertrand and Schoar (2003) and Frank and Goyal (2006)), natural experiments (e.g., Leary (2006) and Lemmon and Roberts (2006)), instrumental variables (e.g., Faulkender and Petersen (2006)), and structural estimation (e.g., Hennessy and Whited (2005, 2007)). We look forward to future research in this vein.

Appendix

This appendix details the variable construction for analysis of the Compustat sample. All numbers in parentheses refer to the annual Compustat item number.

Total Debt = short-term debt (34) + long-term debt (9).

Book Leverage = total debt/book assets (6).

Firm Size = log(book assets), where assets are deflated by the GDP deflator.

Profitability = operating income before depreciation (13)/book assets.

Cash Flow Volatility = the standard deviation of historical operating income, requiring at least 3 years of data.

Marginal Tax Rate = simulated marginal tax rates obtained from John Graham.

Market Equity = stock price (199) $*$ shares outstanding (54).

Market Leverage = total debt/(total debt + market equity).

Market-to-Book = (market equity + total debt + preferred stock liquidating value (10) – deferred taxes and investment tax credits (35))/book assets.

Collateral = Inventory (3) + net PPE (8)/book assets.

Z-Score = 3.3 $*$ pre-tax income (170) + sales (12) + 1.4 $*$ retained earnings (36) + 1.2 $*$ (current assets (4) – current liabilities (5))/book assets.

Tangibility = net PPE/book assets.

Net Debt Issuance = the change in total debt from year $t-1$ to year t divided by the end of year $t-1$ total assets.

Net Equity Issuance = The split-adjusted change in shares outstanding (data25_t – $\text{data25}_{t-1} * (\text{data27}_{t-1}/\text{data27}_t)$) times the split-adjusted average stock price (data199_t + $\text{data199}_{t-1} * (\text{data27}_t/\text{data27}_{t-1})$) dividend by the end of year $t-1$ total assets.

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