

Target Behavior and Financing: How Conclusive Is the Evidence?

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ABSTRACT

The notion that firms have a debt ratio target that is a primary determinant of financing behavior is influential in finance. Yet, how definitive is the evidence? We address this issue by generating samples where financing is unrelated to a firm's current debt ratio or a target. We find that much of the available evidence in favor of target behavior based on leverage ratio changes can be reproduced for these samples. Taken together, our findings suggest that a number of existing tests of target behavior have no power to reject alternatives.

A TRADITIONAL VIEW IN FINANCE IS that firms have debt ratio targets. For example, the notion of the weighted average cost of capital—taught to generations of finance students and recommended as the discount rate in capital budgeting—presupposes the existence of “target capital structure” weights. Tradeoff models that stress various costs and benefits of debt imply the existence of an optimal debt ratio, and it is assumed that firms make financing choices that minimize the cost of deviation from this optimum. In this paper, we evaluate some of the evidence in favor of the view that firms' financing choices reflect target behavior. We interpret target behavior to mean that a firm's financing choice at any point in time is a function of its current debt ratio as well as a “target” debt ratio. We find that a bulk of the evidence based on leverage ratio changes can

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be readily replicated if the choice of the security to be issued is dictated by the resolution of a state variable that has no relationship to either a firm's current debt ratio or any optimal debt ratio—such as the outcome of a coin toss.

Our results by no means contradict the existence of debt ratio targets, nor do we try to establish any specific alternatives to target behavior. While our results imply that some of the available evidence is consistent with alternative theories of financing, including even indifference, we do not try to discriminate between theories. In this, we differ from some other approaches that specifically question tradeoff theory by proposing alternatives.

Our methodology is similar to the one first adopted by Shyam-Sunder and Myers (1999) but, surprisingly, has been ignored by the subsequent literature. For a sample of Compustat firms for the period 1971 to 2004, we generate a new series of debt ratios under alternative assumptions about financing behavior. With one exception, the newly retained earnings and the financing deficit (i.e., the total amount of external financing) are as in the actual data.¹ We track the evolution of debt and equity under the assumption that firms choose the type of security to be issued (if the actual deficit is positive) or repurchased (if the actual deficit is negative) randomly. In one of our samples, the choice of the security to be issued or repurchased is determined essentially by a coin toss. In another sample, the choice is determined randomly in accordance with the empirical frequency of debt or equity issuance and repurchases in the actual data across all firm-years.² A third sample is selected by relaxing the assumption that the deficit is the same as in the actual data. Here, the deficit and the change in retained earnings, both scaled by the book value of assets, are also randomly drawn. We then examine the extent to which results of tests on the actual data that have been taken to be evidence of target behavior can be reproduced for the simulated data, in which no target behavior is assumed.

Why do we choose random financing as a benchmark? Target behavior requires that the probability of equity (debt) issuance be high when the debt ratio is above (below) target. By assuming fixed probabilities of debt and equity issuance, we want to make sure that our simulations do not mimic target adjustment behavior. As we discuss below, the simulated data are able to replicate a large body of empirical evidence that is generally associated with target behavior.

Fama and French (2002) and Flannery and Rangan (2006), among others, estimate target adjustment models of the leverage ratio. The empirical estimation of such models seems to indicate that the leverage ratio exhibits mean reversion. The mean is generally supposed to be a target leverage ratio.

¹ The financing deficit is equal to the difference between a firm's requirement for funds (due to investment and dividend payments) and internally generated funds, and is identically equal to the sum of net issue of debt plus net issue of equity. A negative deficit corresponds to a net repurchase of securities.

² In the actual data, conditional on the deficit being positive, the probability of a debt issue is about 0.75; conditional on the deficit being negative, the probability of a debt repurchase is around 0.8.

Shyam-Sunder and Myers (1999) and Chen and Zhao (2007) have argued that leverage ratios can be mean-reverting for purely mechanical reasons. We first show why these mechanical effects appear in simulated data in which no target behavior is assumed. We then demonstrate that for the simulated data, one can estimate speeds of mean reversion in the leverage ratio that are comparable to those for the actual data. For example, the speed of adjustment in a simulation sample where the debt versus equity choice is determined by a coin toss is only 10% slower than that for the actual data. However, our calibrations show that the latter speed of adjustment can be almost exactly replicated by a simulated “target behavior” sample in which firms’ issuance decisions move the debt ratio toward the target two-thirds of the time, which could be considered fairly vigorous target behavior. In other words, a move from random financing to vigorous target behavior produces only a 10% change in the estimated speed of adjustment. Therefore, these estimated speeds of adjustment are likely to provide a very imprecise picture of the extent of rebalancing going on in the data.

Empirical tests of target behavior are not limited to estimating speeds of target adjustment. While the possibility of mechanical mean reversion has been previously recognized (Shyam-Sunder and Myers (1999) and Chen and Zhao (2007)), what has not been shown previously is that a number of other widely employed tests are also susceptible to mechanical effects that could arise when firms do not follow target behavior. Leary and Roberts (2005), Hovakimian (2006), and Alti (2006) argue that firms experiencing “shocks” to their debt ratios due to major issuance activity or change in stock price subsequently rebalance. They demonstrate this by showing that the debt ratios of firms experiencing these shocks subsequently revert to their earlier levels or those of firms with no shocks. We show that similar results also hold in our simulated samples and are quantitatively of comparable magnitude. Kayhan and Titman (2007) show that the change in debt ratios over a 5-year period is negatively related to the deviation of the debt ratio from an estimated target at the beginning of the period and positively related to the change in the estimated target over the same period. These results also hold for our simulation samples. Therefore, our results show that these tests of rebalancing behavior do not have the power to reject mechanical effects associated with nontarget behavior.

Firms’ debt ratio targets are supposed to be outcomes of optimization exercises that trade off the costs and benefits of debt and equity. These costs and benefits are presumably related to firm characteristics. A large literature therefore tests target behavior by examining whether, in leverage regressions, firm-specific variables have signs consistent with the tradeoffs. We find that even for our simulation samples, several firm-specific variables show statistically significant coefficients in leverage regressions. The only exception is the sample in which the deficit and change in retained earnings are also random. Here, all firm-specific variables are insignificant. This suggests that firm characteristics affect the debt ratio in our *simulation samples* because the actual deficit and change in retained earnings themselves depend on firm characteristics and

can affect debt ratios mechanically.³ Thus, in the actual sample, it is unclear whether firm characteristics affect the debt ratio primarily because they affect the financing deficit and retained earnings, or because they have an independent effect.

The rest of the paper is organized as follows. Section I provides a brief discussion of how mechanical mean reversion can occur. Section II describes our actual data sample and the simulated data. Section III discusses further tests of target behavior and reports our empirical results on actual and simulated data. Section IV concludes the paper. An Internet Appendix⁴ available from the Journal's website and the authors' websites contains several supplementary results and tables referred to in the text.

I. Target Behavior versus Mechanical Effects

A. Target Behavior and Mean Reversion

One of the most traditional tests of target behavior was developed by Taggart (1977) and Auerbach (1985), and more recently extended by Fama and French (2002) and Flannery and Rangan (2006). This test consists of stipulating a target adjustment model in which leverage adjusts over time to a target. The target could be time-varying; the usual approach has been to model it as a function of both time-varying as well as time-invariant firm characteristics. The adjustment model is specified as

$$d_t - d_{t-1} = -\lambda(d_{t-1} - d_t^*) + \varepsilon_t, \quad (1)$$

where the target adjustment coefficient (λ) is greater than 0 if firms adjust toward the target, and it is strictly less than 1 if there exist some positive adjustment costs. The terms d_t and d_t^* denote, respectively, the debt ratio and the target at t . The expression $d_{t-1} - d_t^*$ is called the "deviation from the target." This model can be rewritten as

$$d_t = \lambda d_t^* + (1 - \lambda)d_{t-1} + \varepsilon_t. \quad (2)$$

The coefficient on the lagged debt ratio, $1 - \lambda$, is expected to be less than 1 if there is adjustment to the target. Moreover, the smaller the coefficient on the lagged debt ratio, the faster is the speed of adjustment. These two issues have been at the very core of tests of target behavior. Almost all tests of the target adjustment model find the coefficient on the lagged leverage ratio to be less than unity. Estimates of the coefficient on the lagged debt ratio vary

³ The change in retained earnings enters the denominator of book leverage and thus has a mechanical negative effect on the latter. If in any particular sample debt is issued and repurchased more frequently than equity (for example, due to pecking order behavior), then any variable that increases the likelihood of a positive deficit (as opposed to a negative one) will be positively related to the debt ratio. We confirm these conjectures by means of additional simulations that are not reported in this paper but are available on request.

⁴ An Internet Appendix for this article is online in the "Supplements and Datasets" section at <http://www.afajof.org/supplements.asp>.

depending on the estimation method; while Fama and French (2002) document an OLS estimate of about 0.9 and conclude that the speed of adjustment is slow (this estimate implies that it takes about 6.6 years for a deviation from target to be halved), more recently Flannery and Rangan (2006) find estimates of around 0.62 after incorporating firm fixed effects and conclude that the speed of adjustment is relatively fast (a “half-life” of 1.4 years).⁵

A related issue has been the determinants of target leverage. The leverage target is supposed to be the result of an optimization exercise in which firms trade off the costs and benefits of debt. These costs and benefits are related to firm characteristics. Therefore, the target d_t^* for firm i is stipulated to be of the form

$$d_{it}^* = \beta X_{i,t-1} + v_i, \quad (3)$$

where $X_{i,t-1}$ is a vector of time-varying firm characteristics that are predetermined at time t , and v_i represents a time-invariant firm characteristic. Tests involve estimating the model obtained from substituting (3) into (2), that is,

$$d_{it} = (1 - \lambda)d_{i,t-1} + \beta\lambda X_{i,t-1} + \lambda v_i + \varepsilon_{it}, \quad (4)$$

and examining whether the signs of the coefficients on the firm-specific variables conform to the theoretical predictions regarding how the target debt ratio should be related to these variables.

B. Mechanical Mean Reversion

In this section, we show that tests of target behavior based on the coefficient on lagged leverage in a specification similar to equation (4) are flawed. Specifically, we generate data to show that mean reversion can arise due to a number of mechanical reasons—even when no target behavior is assumed.

We illustrate two ways in which mean reversion can occur even in the absence of target behavior. The first has to do with the fact that the debt ratio is a fraction bounded between 0 and 1. Chen and Zhao (2007), among others, have emphasized that this can drive mean reversion. The second, suggested by Shyam-Sunder and Myers (1999), has to do with the possibility that firms' need for external financing (described by the financing deficit) and their internally generated funds may have time-series properties that lead to mean reversion of the debt ratio when firms follow a pecking order of financing.

B.1. Mechanical Mean Reversion I (MMR I)

Assume that the firm's need for external finance is given by $y_t > 0$. The firm follows the following type of financing: with probability p it issues debt, and

⁵ We compute the half-life, the time required for a deviation from target to be halved, as $\ln(0.5)/\ln(1 - \lambda)$, where $1 - \lambda$ is the coefficient on the lagged leverage ratio in equation (2). See Huang and Ritter (2008), Table 8, for a comparison of speeds of adjustment and the implied half-lives obtained from different methods of estimation in previous studies.

with probability $1 - p$ it issues equity, to finance y_t . We refer to this type of financing as *random financing*.

Notice that the financing choice is not dictated by the sign of the deviation from the target. While equation (1) is not stated in terms of firms' financing choices, the implicit assumption is that if the deviation from target on the right-hand side is negative, the firm is underlevered and will issue debt. If, on the other hand, the deviation from target is positive, the firm is overlevered and will issue equity. A weak form of target behavior will stipulate that the probability of debt issuance will be lower if the deviation from target is positive than if it is negative. In contrast, random financing assumes that the probability of debt issuance is independent of the deviation from target.

Note that the debt ratio is $d_t = \frac{D_t}{D_t + E_t}$, where D_t and E_t are, respectively, the book debt and equity at t . With random financing and zero newly retained earnings, the debt ratio evolves according to

$$d_{t+1} - d_t = \begin{cases} \frac{D_t + y_t}{D_t + E_t + y_t} - \frac{D_t}{D_t + E_t} & \text{with probability } p \\ \frac{D_t}{D_t + E_t + y_t} - \frac{D_t}{D_t + E_t} & \text{with probability } 1 - p, \end{cases} \quad (5)$$

which simplifies to

$$d_{t+1} - d_t = \begin{cases} \frac{y_t}{A_t} \cdot \frac{E_t}{A_t + y_t} & \text{with probability } p \\ -\frac{y_t}{A_t} \cdot \frac{D_t}{A_t + y_t} & \text{with probability } 1 - p, \end{cases} \quad (6)$$

where $A_t = D_t + E_t$ is the book value of assets. Assume that the deficit as a proportion of book value of assets is constant, that is, $\frac{y_t}{A_t} = k$. Then, the above process becomes

$$d_{t+1} - d_t = \begin{cases} k \cdot \frac{1 - d_t}{(1 + k)} & \text{with probability } p \\ -k \cdot \frac{d_t}{(1 + k)} & \text{with probability } 1 - p. \end{cases} \quad (7)$$

First, note that even with $p = 1$, which corresponds to a strict pecking order behavior (i.e., the firm always issues debt), we will have $d_{t+1} = \frac{k}{1+k} + (1 - \frac{k}{1+k})d_t$. Similarly, if the firm always issues equity, we have $d_{t+1} = (1 - \frac{k}{1+k})d_t$. Thus, the coefficient on the lagged leverage ratio is less than 1, exactly as the target adjustment model would suggest. However, the debt ratio in these extreme cases will monotonically diverge further from an initial value without reverting back—quite the opposite of target behavior.

Second, observe from (7) that when the debt ratio is close to 1, the positive increment (the first expression to the right of the curly bracket) is much smaller than the negative decrement (the expression immediately below). In

other words, if the flip of the coin dictates a debt issuance, the debt ratio will increase little; however, if the flip of the coin dictates an equity issuance, the debt ratio will fall a lot. As a result, the debt ratio will tend to decrease when it is close to 1. The opposite happens when the debt ratio is close to zero. This mechanical mean reversion is similar to that first discussed by Chen and Zhao (2007).

To see this more succinctly, note that the expected change in leverage ratio can be written as follows:

$$E(d_{t+1} - d_t) = pk \frac{1 - d_t}{(1 + k)} + (1 - p) \frac{-kd_t}{(1 + k)} = \frac{k(p - d_t)}{(1 + k)}. \quad (8)$$

When the current leverage is low ($p - d_t > 0$) we have $E(d_{t+1} - d_t) > 0$. Similarly, when the current leverage is high ($p - d_t < 0$), we have $E(d_{t+1} - d_t) < 0$. Thus, in a sample of firms following random financing, the leverage ratio has a tendency to mean revert: in cross-sectional regressions with the change in the leverage ratio on the left-hand side and the lagged leverage ratio on the right-hand side, one would obtain a negative coefficient for the latter. Moreover, this coefficient should be increasing in k , suggesting a faster speed of mean reversion when the financing deficit is higher.

In regressions with firm fixed effects, a similar argument applies. Incorporating firm fixed effects in a model such as in equation (4) is equivalent to mean differencing, so that one is estimating

$$d_{i,t+1} - \bar{d}_i = (1 - \lambda)(d_{i,t} - \bar{d}_i) + \lambda\beta(X_{i,t} - \bar{X}_i) + \varepsilon_{i,t+1} - \bar{\varepsilon}_i, \quad (9)$$

where variables with bars denote firm-specific sample means. A test of mean reversion amounts to $0 < 1 - \lambda < 1$. Now notice that from (7), we have

$$d_{i,t+1} - \bar{d}_i = \begin{cases} k \cdot \frac{1 - d_{i,t}}{(1 + k)} + (d_{i,t} - \bar{d}_i) & \text{with probability } p \\ -k \cdot \frac{d_{i,t}}{(1 + k)} + (d_{i,t} - \bar{d}_i) & \text{with probability } 1 - p. \end{cases} \quad (10)$$

Hence, ignoring the firm-specific variables (X) for the moment, if we neglect the first terms on the right-hand side of (10) and try to estimate the slope coefficient on $(d_{i,t} - \bar{d}_i)$ in regressions similar to (9) for data generated on the basis of random financing, we will estimate a coefficient of less than unity because of the negative relation between the term $(d_{i,t} - \bar{d}_i)$ and the omitted first terms.⁶ Therefore, in regressions involving firm fixed effects, we will also find mechanical mean reversion around the firm-specific means.

A final point to note is that MMR I requires the deficit to be positive; if k is negative (but less than 1), it is easy to check that the average debt ratio will diverge away from p . Therefore, MMR I will be stronger in a sample for which positive financing deficits outnumber negative financing deficits.

⁶This is an example of the omitted variables bias (see, for example, Pindyck and Rubinfeld (1998), chapter 7).

B.2. Mechanical Mean Reversion II (MMR II)

Shyam-Sunder and Myers (1999) propose an explanation of mean reversion based on the pecking order theory of capital structure. In the pecking order theory, there is no well-defined optimal debt ratio. According to Shyam-Sunder and Myers (1999, p. 221),

Debt ratios change when there is an imbalance of internal cash flow, net of dividends, and real investment opportunities. Highly profitable firms with limited investment opportunities work down to low debt ratios. Firms whose investment opportunities outrun internally generated funds borrow more and more. Changes in debt ratios are driven by the need for external funds, not by any attempt to reach an optimal capital structure.

In other words, firms issue debt when they have external financing needs, and the debt ratio rises; firms repurchase debt when they have financial surplus, and the debt ratio falls—equity is rarely issued or repurchased. Given this, the authors argue that the time patterns of capital expenditures and operating income create mean-reverting debt ratios even under the pecking order.

Recall that even if the financing deficit is always positive, a firm following a pecking order of financing would have an estimated coefficient on lagged leverage that is less than unity. However, if the financing deficit also mean reverts, estimated mean reversion speeds under pecking order behavior can be faster, as we shall see below.

B.3. Preliminary Simulation Results

We generate data under alternative assumptions of nontarget behavior, and then estimate the coefficient on lagged leverage in a specification similar to equation (2) that includes firm fixed effects. Essentially, this is the specification in a target adjustment model in which the firm's target is invariant over time.

In Table I, we report the coefficient on lagged leverage under three alternative types of financing for a panel of 500 hypothetical firms for 15 years and 100 years, respectively. Initial leverage ratios are assumed to be uniformly distributed over the unit interval. Parameter k is the absolute value of the deficit to assets ratio, and p is the probability of debt being issued (repurchased) when the financing deficit is positive (negative). For the top two panels, the financing deficit is assumed to be positive in all firm-years. In Panel A, p takes the value of 0.5, so that the firms issue either debt or equity with equal probability. In Panel B, we change the value of p to 1, i.e., firms follow a strict pecking order. In Panel C, the deficit is positive for the first 3 years, and subsequently changes sign (with the same absolute value k) every 3 years. Firms are assumed to issue (repurchase) debt with probability $p = 1$ when the financing deficit is positive (negative).

Several observations are worth noting. First, consistent with our arguments in Section B.1 concerning MMR I, we estimate a mean reversion parameter of

Table I
Mechanical Mean Reversion

Data are generated under three alternative types of financing for a panel of 500 hypothetical firms for 15 and 100 years. Retained earnings are assumed to be zero for all firms. Firms' initial total assets (A) are unity. Initial leverage ratios (D/A) are uniformly distributed on the unit interval. From the second year onward, leverage ratios are updated based on the financing deficit (y) and the type of issuance activity (debt or equity). k is the ratio of the absolute value of deficit to beginning-of-period assets (y/A), and p is the probability of debt being issued (repurchased) when financing deficit is positive (negative). In Panels A and B, the deficit to assets ratio k is assumed to be positive in all firm-years. In Panel A, p takes the value of 0.5, so that firms issue either debt or equity with equal probability. In Panel B, p is set equal to 1, that is, firms follow a strict pecking order behavior. In Panel C, the deficit is positive ($+k$) for the first 3 consecutive years, and subsequently changes sign (with the same absolute value) every 3 years. Firms are assumed to issue (repurchase) debt with probability $p = 1$ when financing deficit is positive (negative). The regression model is written as $(D/A)_t = a_0 + a_1(D/A)_{t-1} + \varepsilon_t$. The model is estimated with firm fixed effects for the first 15 years and for the entire 100 years. The reported parameter estimates are the average coefficients on lagged leverage ratios, a_1 , obtained from 500 replications of the simulation.

	Absolute Value of Financing Deficit to Assets Ratio (k)			
	$k = 5\%$	$k = 10\%$	$k = 15\%$	$k = 35\%$
Panel A: $y/A = k$ and $p = 0.5$				
$t \in [1, 15]$	0.879	0.814	0.766	0.626
$t \in [1, 100]$	0.941	0.887	0.852	0.714
Panel B: $y/A = k$ and $p = 1$				
$t \in [1, 15]$	0.952	0.909	0.869	0.740
$t \in [1, 100]$	0.952	0.909	0.869	0.740
Panel C: Alternating Deficit and $p = 1$ (y/A Equals $+k$ in the First 3 Years and Subsequently Changes Sign Every 3 years)				
$t \in [1, 15]$	0.489	0.509	0.527	0.629
$t \in [1, 100]$	0.516	0.727	0.821	0.878

less than unity in Panel A, and this parameter becomes smaller (mean reversion becomes faster) as the financing deficit size (k) increases. Second, in Panel B, we obtain comparable parameter estimates to those in Panel A (especially for 100 periods of data), although in this case, the debt ratio monotonically increases from its initial value for each firm.⁷ This suggests that it may be difficult to distinguish mean-averting from mean-reverting behavior by means of this test. Third, for the case in which the deficit itself mean reverts and firms either issue or repurchase debt, the parameter estimates for the coefficient on lagged leverage are also less than 1. Interestingly, in this case, in contrast to the other cases, we estimate very rapid speeds of adjustment when the deficit is small in absolute value.

⁷ In fact, as noted above, the process in this case is given by $d_{t+1} = k/(1+k) + [1 - k/(1+k)]d_t$, and we recover the coefficient $1 - k/(1+k)$ exactly. The estimated firm-specific mean for all firms is unity: The debt ratio asymptotically converges to this value.

II. Data and Samples

A. Actual Data and Variables

Our actual data sample, $S(\text{actual data})$, consists of firms listed in the Compustat Industrial Annual Files at any point between 1971 and 2004. We obtain data on stock prices and returns from the Center for Research on Security Prices (CRSP) files. All dollar values are converted into 2,000 constant dollars. We exclude financial, insurance, and real estate firms (SIC code 6000–6900), regulated utilities (SIC code 4900–4999), and firms with missing book values of assets. Following previous studies of target adjustment models, we restrict the sample to firms with at least 5 years of continuous balance sheet items.⁸ This also ensures that in simulated samples, the debt ratio evolves via random financing for a sufficiently long period of time so that the resulting simulated leverage ratios do not closely resemble the actual leverage. The final data set is an unbalanced panel consisting of 112,035 firm-year observations. Firm characteristics, such as the market-to-book assets ratio and EBITDA to assets ratio, are winsorized at the 0.5% level at both tails of the distribution to mitigate the impact of outliers or misrecorded data.

Book leverage is defined as the ratio of book debt to total assets. Book debt is the sum of total liabilities and preferred stock minus deferred taxes and convertible debt.⁹ When preferred stock is missing, we replace it with the redemption value of preferred stock. Book equity is then defined as total assets minus book debt. We drop firm-year observations where book leverage is negative or exceeds 1.

We define net equity and net debt issues using balance sheet data.¹⁰ Following the accounting identity that book equity equals balance sheet retained earnings plus paid-in share capital, we define net equity issues (Nei) as the change in book equity (ΔE) minus the change in retained earnings (ΔRE). Net debt issues (Ndi) are then defined as the change in total assets less the change in retained

⁸ Among others, Jalilvand and Harris (1984), Titman and Wessels (1988), Leary and Roberts (2005), Fama and French (2002), and Flannery and Rangan (2006) exclude companies for which continuous data are not available. In the earlier draft of the paper, we only include firms that have no gaps in data on the relevant balance sheet items for 20 years and obtain similar results. As a robustness check, we drop firms involved in large asset sales and significant mergers (identified by Compustat footnote code AB). The results are essentially the same.

⁹ The definition of the leverage ratio follows Fama and French (2002), Baker and Wurgler (2002), and Kayhan and Titman (2007). Our results are robust to alternative definitions of debt, for example, when total debt is defined as the sum of short-term debt and long-term debt. These results and results of other robustness checks that are not tabulated are available from the authors on request.

¹⁰ As a robustness check, we also define debt and equity issues using the cash flow statements. Following Shyam-Sunder and Myers (1999) and Frank and Goyal (2003b), we define equity issues as the sale of common and preferred stock less the purchase of common and preferred stock. Debt issues are defined as long-term debt issuance minus long-term debt reduction plus changes in current debt. Financing deficit is then defined as the sum of the change in net working capital, investments, and cash dividends, net of internally generated cash flow. These alternative definitions have little impact on our results. We prefer the measures constructed from balance sheets as they offer more nonmissing observations than those defined using cash flow statement data.

earnings and net equity issues. One key variable of our interest, the financing deficit (y), is the difference between the change in total assets and the change in retained earnings. This variable is positive ($y > 0$) when the firm invests more than it internally generates, and by definition, this deficit must be filled by the net issues of debt and/or equity. In contrast, the financing deficit takes a negative value ($y < 0$) when the firm internally generates more funds than it invests, in which case the resulting financing surplus (or the negative financing deficit) has to be used to repurchase debt and/or equity. In other words, the financing deficit is equal to the sum of the net debt and the net equity issued. This accounting identity can be written as follows:

$$y = \Delta A - \Delta RE = Nei + Ndi. \quad (11)$$

Untabulated summary statistics show that in roughly two-thirds (68.9%) of the firm-years, firms have positive financing deficits. Roughly three-quarters (75.1%) of the positive financing deficits are financed with debt issues. In contrast, roughly 80% (79.7%) of the financing surpluses (negative deficits) are associated with debt reductions. Table IA.I in the Internet Appendix to this paper available at the “Supplements and Dataset Section” of the Journal’s website (www.afajof.org) reports detailed summary statistics for the actual data sample.

B. Simulation Samples

Our main objective in this paper is to examine the extent to which existing tests of target behavior have the power to reject alternatives. To this end, we generate data under a number of alternative assumptions about financing behavior. Our simulated samples are based on the actual sample of firms in Compustat described in Section II.A. We assume that all firms in the simulation sample have the same initial debt ratio as in the actual data, and, importantly, we assume—in all cases except one to be discussed below—that the financing deficit and newly retained earnings are the same as in the actual data for every firm-year. All other variables are also as in the actual data with the exception of the mix of debt and equity. The mix of debt and equity is determined by the initial debt ratio and the outcome of successive “coin toss” experiments described below.

We take the initial book leverage ratio of each firm from Compustat.¹¹ From the second year onward, we update leverage according to the financing rule that firms follow. For each simulated sample, the simulated end-of-period total equity is the sum of the simulated beginning-of-period total equity plus net

¹¹ Even though we assume no apparent target for simulated samples, since we start off the simulations at the initial debt ratio of the firms in the actual data, it is conceivable that this initial debt ratio is a target debt ratio that is related to firm characteristics (Lemmon, Roberts, and Zender (2008)). To ensure that the use of actual initial leverage ratios is not driving our results, we replicate all empirical tests using simulation samples where all firms start their leverage ratios at 0.2 or 0.8 and document essentially the same findings.

equity issued and the change in retained earnings. The simulated end-of-period debt is the beginning-of-period debt plus the new net debt issued.

The first two simulation samples, denoted as $S(p, \text{actual deficit})$, are generated using *actual* financing deficit and newly retained earnings; p represents the probability of debt issue/repurchase.

- $S(p = 0.5, \text{actual deficit})$: If the financing deficit is positive, we assume that firms decide whether to issue debt or equity by tossing a coin, that is, there is a 50% chance for equity issuance and a 50% chance for debt issuance. Similarly, firms are assumed to retire debt or equity with equal probability when the financing deficit is negative. This sample is generated because it is the most intuitive form of “random financing”: A firm flips a fair coin to decide whether it should issue (repurchase) debt or equity, without any regard to the current debt ratio or a target.
- $S(p = \text{empirical frequency}, \text{actual deficit})$: Here, we assume that conditional on the actual deficit being positive (negative), the probability of debt and equity issuance (repurchase) corresponds to the empirical frequencies in the overall actual data when dual issues are excluded. The probability of debt issuance is approximately 0.75 (equity issuance 0.25), and the probability of debt retirement is approximately 0.80 (equity repurchase 0.20) as discussed in Section II.A. This sample is generated because if, in fact, target behavior is not followed widely, then a sample in which debt and equity are issued and repurchased with fixed probabilities in accordance with the actual empirical frequencies is likely to reproduce results similar to those obtained using the actual data. Moreover, notice that the actual empirical probabilities conform reasonably closely to what pecking order behavior would suggest: Both for issuance and repurchase activities, debt appears to be the preferred security. Thus, if much of the observed empirical regularities can be reproduced for this sample, it follows that pecking order behavior is a reasonable description of actual behavior.

The decision to preserve the actual financing deficit for the purpose of generating the simulation samples may appear not to be innocuous. One immediate advantage is that the size of the firm remains the same in each firm-year in the simulated and the actual samples, since the book value of assets increases by the amount of the financing deficit plus newly retained earnings. This makes it easier to interpret variables such as the market-to-book ratio in the context of the simulation samples. For example, if the market value of the firm is not affected by the choice of financing, the market-to-book ratio in the actual data is still the right one for the simulated data. Since our simulations do not stipulate an optimal debt ratio, we might as well pretend that Modigliani and Miller’s (1958) result holds. One could also argue that target behavior is less related to the magnitude of the deficit (especially, whether it is positive or negative) than the form of financing chosen. For example, a firm that is underlevered (i.e., has a negative deviation from the target) could either issue debt or buy back equity to move closer to the target. Shyam-Sunder and Myers (1999) also seem to take the view that the primary determinants of the financing deficit,

namely investment opportunities and internal funds, are exogenous. However, the magnitude of the deficit does determine the extent to which a firm is able to adjust its debt ratio—so for a firm following target behavior, the financing deficit must be at least to some extent endogenous.¹²

To eliminate the possibility that any replications of evidence consistent with target behavior in the *simulated* samples could be related to endogeneity of the actual financing deficit, we also construct a sample $S(p = 0.5, \text{random deficit})$ described as follows:

- $S(p = 0.5, \text{random deficit})$: Here we assume both the financing deficit scaled by total assets (y/A) and the change in retained earnings scaled by total assets ($\Delta RE/A$) are randomly drawn from normal distributions with the same means and standard deviations as those in the actual data (the means are 0.162 and -0.004 , and the standard deviations are 0.495 and 0.225 for y/A and $\Delta RE/A$, respectively). Firms are assumed to issue (retire) debt or equity with equal probability when the financing deficit is positive (negative).

It is also worth pointing out that even if the deficit size were correlated with the deviation from the target in the actual data, such a relationship is unlikely to persist in our simulations as the leverage ratio evolves in response to random financing. Further, even if such a correlation persists, as financing is random, it is unlikely that this should generate a move necessarily *toward* the target in the same way as in the actual data. Therefore, taken together, it does not appear as though the endogeneity of the financing deficit could explain replication of evidence consistent with target behavior in our simulation samples.

C. Book versus Market Leverage and Stock Returns

Much of our analysis is in terms of leverage ratios. We focus exclusively on book leverage. There are good reasons for this. A financing mix that differs from the actual data at any point in time will also have implications for subsequent stock returns if other firm characteristics did not change, even if we were to assume that financing has no impact on firm value. Under the assumption that the valuation consequences of departures from the actual financing choice are not of first-order importance, it is straightforward to adjust the stock returns when the financing mix changes. However, our results would be less convincing if we were to rely on a market debt ratio based on these adjusted returns. Therefore, we work in terms of the book debt ratio.

A second reason for focusing on the book debt ratio is that changes in the book debt ratio reflect what might be called “active rebalancing,” that is, the

¹² To address the issue of endogeneity more thoroughly, we replicate our subsequent analysis by generating a time series of the predicted deficit in a variety of ways and recreate the simulated samples on the basis of these predicted deficits. In one of the robustness checks, we predict the financing deficit by running a cross-sectional regression of the actual financing deficit on the log of total assets, sales growth, and the change in the net PPE to assets ratio. Our results are similar to those obtained with the actual deficits.

effect of debt and equity issuances and repurchases, and retention and payout policy. Changes in the market debt ratio reflect unanticipated changes in the stock price that may not be in the control of management. Survey evidence by Graham and Harvey (2001, p. 215) finds that “few firms . . . state that changes in the price of equity affect their debt policy.”

D. Why Use Random Financing as a Benchmark?

It is important to point out that our objective in using samples generated under the assumption of random financing as a benchmark is not to propose that, in reality, financing is random. Rather, this choice is motivated by the fact that we do not know a great deal about the reasons underlying firms' financing choices. Actual financing behavior is presumably affected by the resolution of state variables that are observed by firm insiders but unobserved or imperfectly observed by researchers; therefore, modeling financing as random reflects the probabilities with which these state variables are jointly realized. Random financing with fixed probabilities of issuance or repurchase of a particular type of security thus incorporates a wide class of theories of financing but excludes tradeoff behavior. The latter requires that the probability of equity (debt) issuance (repurchase) be high when the debt ratio is above the target, and that the probability of debt (equity) issuance (repurchase) be high when the debt ratio is below the target. Our simulations are constructed so that whatever the patterns occurring in the simulated data, we know they are not caused by debt ratio targeting.

III. Tests of Target Behavior

In this section, we discuss the extent to which various tests of target behavior have the power to reject other types of financing behavior. We begin with the mean reversion test introduced in Section I.A.

A. Mean Reversion in Actual and Simulated Samples

We estimate the following model in a panel setting using firm fixed effects as in Flannery and Rangan (2006):

$$\begin{aligned} \left(\frac{D}{A}\right)_t = & a_0 + a_1 \left(\frac{D}{A}\right)_{t-1} + a_2(M/B)_{t-1} + a_3(EBITDA/A)_{t-1} + a_4Size_{t-1} \\ & + a_5(PPE/A)_{t-1} + a_6(R\&D/S)_{t-1} + a_7RDD_{t-1} + a_8StkRtn_{t-1} + \varepsilon_t. \end{aligned} \quad (12)$$

The dependent variable is the leverage ratio (D/A). The independent variables are the lagged leverage ratio and firm-specific control variables lagged one period, including the market-to-book asset ratio (M/B), profitability ($EBITDA/A$), firm size ($Size$) measured as the log value of total assets, tangibility of assets (PPE/A), research and development expense to sales ratio ($R\&D/S$), a

dummy variable (*RDD*) that takes the value of 1 when R&D is missing in Compustat and 0 otherwise, and annual stock return (*StkRtn*) obtained by compounding monthly stock returns over the entire fiscal year. These variables are chosen because they have been shown by prior literature to influence capital structure (see, among others, Frank and Goyal (2003a) and Flannery and Rangan (2006)). We estimate equation (12) for the actual data as well as a number of simulation samples. For simulated samples, the reported parameter estimates are the average coefficients obtained from 500 replications of the simulation.

In column (1) of Table II, we report the coefficient estimates for equation (12) for the actual data sample $S(\text{actual data})$. The estimated coefficient on lagged leverage is 0.622, which is almost identical to that obtained by Flannery and Rangan (2006). This is fortuitous, since the samples, while comparable, are not exactly the same. More importantly, for the $S(p = 0.5, \text{actual deficit})$, $S(p = \text{empirical frequency, actual deficit})$, and $S(p = 0.5, \text{random deficit})$ samples, the estimated coefficient on lagged leverage appears comparable (0.688, 0.670, and 0.690, respectively). Mechanical mean reversion can give rise to comparable estimated speeds of adjustment as in the actual data. Moreover, since this is true even when the financing deficit is random, these adjustment speeds do not reflect some properties of the actual deficit that could be driving mean reversion. These are two of the most important messages of the paper.

In the last column of Table II, we report the results of a calibration exercise using a family of simulated samples, $S(p = \text{Target}(\pi), \text{actual deficit})$, defined as follows:

- $S(p = \text{Target}(\pi), \text{actual deficit})$: We assume a firm issues debt (equity) with probability $\pi > 0.5$ if its debt ratio is below (above) the target leverage ratio and the financing deficit is positive, and repurchases equity (debt) with probability π if its debt ratio is below (above) target and the financing deficit is negative. The target leverage is estimated from the actual data by regressing the leverage ratio on firm characteristics and firm dummies as in equation (3).¹³ The financing deficit and the change in retained earnings are as in the actual data.

The objective of calibration is to find the value of π for which the estimated speed of adjustment in the simulation sample $S(p = \text{Target}(\pi), \text{actual deficit})$ is the same as that in the actual data. Our calibration exercise reveals a value of π very close to 0.65. In other words, *if firms in the actual sample were rebalancing*, the estimated speed of adjustment would be consistent with a financing behavior where firms adjust in the direction of the target about two-thirds of the time. This can be considered reasonably vigorous target behavior: Whereas

¹³ The firm characteristics (control variables) are the same as those in equation (12). Our results are robust to alternative target measures, including the three-digit SIC industry median leverage, the firm-specific mean leverage, or the 3-year rolling-average leverage ratio from the actual data. Estimates of the speed of adjustment for different values of π are reported in Table IA.II of the Internet Appendix.

Table II
Target Adjustment Model with Firm Fixed Effects

Actual data are collected from Industrial Compustat and CRSP for the years 1971 to 2004. Sections II and III discuss how the simulated data are generated. The dependent variable is book leverage (D/A). Independent variables include the lagged leverage ratio, the market-to-book asset ratio (M/B), profitability ($EBITDA/A$), firm size ($Size$) measured as the log value of total assets, tangibility of assets (PPE/A), research and development expense to sales ratio ($R\&D/S$), a dummy variable (RDD) that takes the value of 1 when R&D is missing in Compustat and 0 otherwise, and annual stock return ($StkRtn$) obtained by compounding monthly stock returns over the entire fiscal year. The regression models are estimated with firm fixed effects. For $S(actual\ data)$, t -statistics in parentheses are calculated from the Huber-White sandwich heteroskedasticity consistent errors, which are corrected for correlation across observations for a given firm. Coefficients significant at the 10%, 5%, and 1% levels are respectively marked with *, **, and ***. For simulated samples, the reported parameter estimates are the average coefficients obtained from 500 replications of the simulation. 95% confidence intervals are included in square brackets and coefficients are marked with ^a if 95% confidence intervals do not span zero.

Dependent Variable (D/A) _{<i>t</i>}	(1) $S(actual\ data)$	(2) $S(p = 0.5,$ actual deficit)	(3) $S(p = empirical\ frequency,$ actual deficit)	(4) $S(p = 0.5, random$ deficit)	(5) $S(p = Target(0.65),$ actual deficit)
$(D/A)_{t-1}$	0.622*** (148.6)	0.688 ^a [0.680, 0.695]	0.670 ^a [0.664, 0.677]	0.690 ^a [0.683, 0.697]	0.627 ^a [0.620, 0.633]
M/B	-0.004*** (-11.4)	-0.001 ^a [-0.002, -0.000]	0.002 ^a [0.001, 0.003]	-0.000 [-0.001, 0.000]	-0.004 ^a [-0.005, -0.003]
$EBITDA/A$	-0.062*** (-10.6)	-0.069 ^a [-0.077, -0.061]	-0.072 ^a [-0.082, -0.062]	-0.001 [-0.008, 0.006]	-0.084 ^a [-0.095, -0.073]
$Size$	0.008*** (17.9)	0.001 [-0.000, 0.002]	0.010 ^a [0.008, 0.012]	0.000 [-0.001, 0.001]	0.006 ^a [0.005, 0.007]
PPE/A	0.013*** (2.6)	0.021 ^a [0.010, 0.032]	0.030 ^a [0.018, 0.042]	0.004 [-0.007, 0.016]	0.026 ^a [0.014, 0.038]
$R\&D/S$	-0.001 (-0.6)	0.001 [-0.003, 0.006]	0.003 ^a [0.000, 0.006]	-0.001 [-0.004, 0.002]	0.002 [-0.003, 0.007]
RDD	-0.001 (-0.5)	-0.000 [-0.004, 0.003]	0.001 [-0.001, 0.003]	-0.001 [-0.004, 0.002]	0.001 [-0.002, 0.004]
$StkRtn$	-0.007*** (-12.4)	-0.004 ^a [-0.006, -0.002]	0.004 ^a [0.003, 0.005]	-0.001 [-0.005, 0.003]	-0.005 ^a [-0.006, -0.004]]
R^2	0.79	0.79	0.81	0.68	0.78
N	112,035	112,035	112,035	112,035	112,035

for the sample $S(p = 0.5, \text{actual deficit})$ a firm is as likely to move in the direction of the target as away from the target, for the sample $S(p = \text{Target}(0.65), \text{actual deficit})$ a firm is twice as likely to move in the direction of the target as away from the target. However, estimating adjustment speeds appears to be a very imprecise way of gauging the extent of target behavior: The difference in the speed of adjustment between the samples $S(p = \text{Target}(0.65), \text{actual deficit})$ and $S(p = 0.5, \text{actual deficit})$ is only 10%.¹⁴

B. Firm-Specific Variables in Leverage Regressions

In Section I.A, we noted that a traditional way of thinking about target behavior has been to examine the determinants of target leverage. The leverage target is supposed to be the result of an optimization exercise in which firms trade off the costs and benefits of debt. These costs and benefits are related to firm characteristics. Therefore, considerable attention has been paid to the issue of finding proxies for the costs and benefits of leverage, and examining whether these proxies have consistent signs in leverage regressions such as equation (4).¹⁵

In Table II, except for the sample in which the deficit is random (column (4)), most of the firm-specific variables are significant in the actual and the other simulated samples. The significance of firm-specific variables is usually taken as evidence that the firm's optimal or target debt ratio is related to firm characteristics. Yet, in columns (2) and (3), where the data are generated under random financing, many of the firm-specific variables are still significant. Therefore, our results show that even when a firm does not follow target behavior, firm characteristics can affect the mean debt ratio.¹⁶

To understand why, it is useful to consider the sample $S(p = 0.5, \text{random deficit})$ and the estimation results reported in column (4). Except for lagged leverage, all firm-specific variables are insignificant. Recall that while in this sample the deficit and retained earnings are randomly drawn in each firm-year, simulated samples in columns (2) and (3) are based on the *actual* deficit and retained earnings. Hence, it appears that the firm-specific variables affect the debt ratio partly because they are related to the *actual* deficit and the *actual* changes in retained earnings. The change in retained earnings enters

¹⁴ The estimated coefficients on the lagged leverage ratio are 0.622 and 0.688 for the actual data and $S(p = 0.5, \text{actual deficit})$, respectively. They are statistically different at the 1% level of significance. However, they imply very similar half-lives (1.45 years and 1.85 years, respectively). To put things in perspective, Huang and Ritter (2008) report estimated half-lives of between 1.6 and 3.7 years from four recent studies that incorporate firm fixed effects in the estimation.

¹⁵ This literature is huge. Titman and Wessels (1988), Rajan and Zingales (1995), and Frank and Goyal (2007) are some representative papers. See also Harris and Raviv (1991), who point out another aspect of the problem, namely, that a variety of theories are consistent with the observed empirical regularities, and call for methods to better distinguish between theories.

¹⁶ Note that for our simulation samples, the actual stock returns (*StkRtn*) are not meaningful measures of returns to shareholders. However, as we argue below, the financing deficit and newly retained earnings are related to stock returns, and stock returns thus potentially affect leverage ratios in the simulation samples through these channels.

the denominator of book leverage and thus has a mechanical negative effect on the latter. Further, if in any particular sample debt is issued and repurchased more frequently than equity (for example, due to pecking order behavior), then any variable that increases the likelihood of a positive deficit (as opposed to a negative one) will be positively related to the debt ratio. This is true, for example, for the sample $S(p = \text{empirical frequency, actual deficit})$ in column (3) of Table II, in which debt is issued or repurchased more than 75% of the time.¹⁷

Finally, for the sample $S(p = \text{Target}(0.65), \text{actual deficit})$ in column (5), coefficients on most firm-specific variables are very similar in magnitude and sign to those in the actual sample. The difference between this sample and that in column (2) lies in the fact that here, firms move in the direction of an assumed target (estimated from the actual data) roughly two-thirds of the time, whereas in the sample in column (2), they are as likely to move in the direction of any assumed target as away. Clearly, this difference in financing behavior has an important effect on the coefficients on the firm-specific variables. The fact that the coefficients in column (5), rather than the ones in column (2) or (3), are somewhat more similar to those in column (1) suggests that there are some patterns in the actual data that are more consistent with target behavior than random financing or pecking order behavior.¹⁸

Overall, however, the results in Table II indicate some difficulties in interpreting coefficients on firm-specific variables in leverage regressions. Our simulations show that these variables indirectly affect the debt ratio through their effect on the financing deficit and newly retained earnings. This makes it difficult to conclude that the firm-specific variables affect debt ratios because they are related to a target. Specifically, we do not know whether the observed relationship between a particular firm-specific variable and the leverage ratio in the actual sample is because (1) the variable causes the target to change, and the deficit and retained earnings themselves represent an endogenous response to this change in the target, or (2) the variable affects the debt ratio mechanically through the financing deficit and retained earnings, and firms do not rebalance. So far, research in capital structure has not been successful in distinguishing between these alternative channels, and the “evidence” must be regarded as inconclusive.

¹⁷ In the working paper version of this paper, we explore further the reasons for the particular signs of the firm-specific variables in our simulation samples in columns (2) and (3). In the actual data, the financing deficit and the change in retained earnings are related to the set of firm-specific variables in Table II. Consistent with the mechanical effects of the deficit and retained earnings on the debt ratio, in general, we find that variables that affect the change in retained earnings positively (negatively) and the financing deficit negatively (positively) have a negative (positive) sign in these two columns.

¹⁸ However, other types of financing behavior could also produce coefficients similar to those in column (1) for some of the variables. In an earlier version of the paper, we generated a simulation sample assuming mechanical market-timing behavior (i.e., issuing equity (debt) when market-to-book ratio is high (low)), which also produced a negative coefficient on the market-to-book ratio similar to that in column (1).

C. The Pervasiveness of Mechanical Effects

Up to this point, our focus has been the estimated adjustment speed in partial adjustment models such as equation (12). What we have shown is that even if data are generated assuming random financing, one can estimate “adjustment speeds” of comparable magnitude to what we have for the actual data. However, in other tests of tradeoff behavior, the adjustment speed is not the focus. Rather, these tests try to identify direct evidence of rebalancing behavior.

It is important to apply these tests to our simulation samples for several reasons. First, some (but not all) of these tests recognize the possibility of mechanical mean reversion and attempt to control for this possibility. At issue is whether these attempts at mitigating the effect of mechanical mean reversion are adequate. For example, several authors argue that mechanical mean reversion is relevant only when the debt ratio is close to the boundaries of 0 or 1. Since mechanical mean reversion is the *only* reason for mean reversion to occur in our simulation samples, repeating such tests on these samples is an ideal way to address this question. Second, even though mechanical mean reversion might be at work, it remains to be seen whether its quantitative effects are of the same order of magnitude as what is observed in the actual data. Finally, some of these tests are predicated on comparisons of the debt ratios of different groups of firms—for example, issuers of equity versus nonissuers—and it may not be obvious why the analysis in Section I.B applies.

C.1. Nonparametric Analysis of Rebalancing Behavior

C.1.1. Debt Ratio Dynamics. Several studies document “rebalancing behavior” by examining how the debt ratio behaves after large movements or shocks caused by issuance activity or, in the case of market debt ratios, stock price changes. In particular, the studies examine whether the debt ratio tends to revert back to levels observed before the shock, or to debt ratios of firms that were not subject to the shocks. Examples of studies in this spirit are Leary and Roberts (2005), Hovakimian (2006), and Alti (2006).

We follow Leary and Roberts (2005) to examine firms’ response to four major corporate finance events related to capital structure changes:

- Large equity issues: A large equity issue occurs if a firm’s net equity issue divided by total assets exceeds 5%.¹⁹
- Large debt issues: A large debt issue occurs if a firm’s net debt issue divided by total assets exceeds 5%.
- Positive equity shocks: A firm is defined to experience a positive equity shock if its annual stock return is one standard deviation above the firm-specific mean return.

¹⁹ The same cutoff for large debt/equity issues (5%) is used in many previous studies, including Hovakimian, Opler, and Titman (2001), Hovakimian (2004), Leary and Roberts (2005), and Chen and Zhao (2005a, 2005b). The alternative cutoff of 1% or 10% makes little difference to the results that follow.

- Negative equity shocks: A firm is defined to experience a negative equity shock if its annual stock return is one standard deviation below the firm-specific mean return.

Leary and Roberts (2005) consider the first, third, and fourth of these events to study dynamic rebalancing behavior. While we consider all four types of events, only the results for large equity issues are discussed here for brevity. Unlike Leary and Roberts (2005), however, we consider the book rather than the market leverage ratio.

Panel A of Figure 1 shows the difference in book leverage between date 0 equity issuers and nonissuers for the actual data and three simulation samples (the latter are event-year averages of 500 replications of each simulation). Panel B separately shows the leverage ratio patterns for equity issuers and nonissuers. The book leverage ratio is lower immediately after the equity issue for the equity issuers compared to the nonissuers for all four samples. The difference becomes smaller over the next 5 years, although it does not completely vanish.

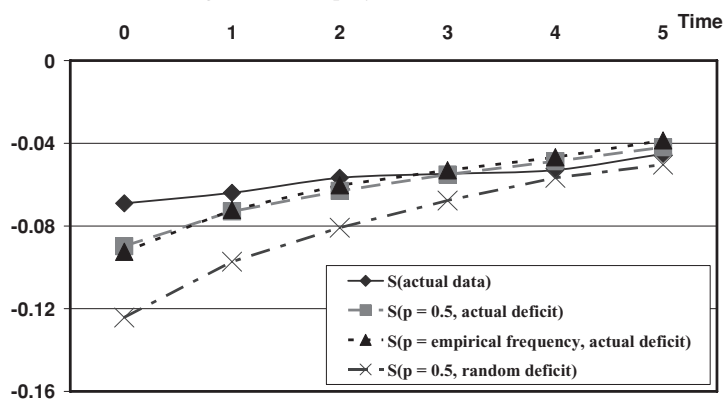
For the simulation samples, the behavior of average debt ratios is a consequence of mechanical mean reversion identified in equation (8).²⁰ Since average debt ratios for issuers are lower immediately after issuance than those for nonissuers, a higher fraction of issuers are likely to have debt ratios below p than nonissuers. Therefore, average debt ratios of the issuers move up more than those for nonissuers.

The general features of large debt issues are similar to the case of large equity issues. Again, all four samples exhibit a tendency for the debt ratios of the date 0 debt issuers to decline relative to nonissuers. Date 0 debt issuers' debt ratios tend to fall, while those of nonissuers tend to rise, consistent with equation (8). To save space, these figures are not shown here but are reported in the Internet Appendix Figure IA.1.

Positive equity shocks are taken to be shocks to the market debt ratio in Leary and Roberts (2005) and Welch (2004). However, they can also affect book leverage as some of these firms are likely to take advantage of market conditions and issue equity; profits are also likely to be positively correlated with positive equity shocks and increase retained earnings, thereby lowering the book debt ratio. Consistent with this observation, we find that book debt ratios fall when firms have positive equity shocks. Similarly, during negative equity shocks, book debt ratios rise. While we do not report these cases in detail, the patterns we observe are very similar to those noted for large equity issues and large

²⁰ The derivation of (8) assumes that there is no change in retained earnings. With newly retained earnings ΔRE , it is easy to check that the average debt ratio of a group of firms with a positive financing deficit ($y > 0$), given by $p(D + y)/(A + \Delta RE + y) + (1 - p)D/(A + \Delta RE + y)$, will be higher than the initial debt ratio D/A if and only if $p > (D/A)(1 + \Delta RE/y)$. Similarly, if the deficit is negative ($y < 0$), the required condition is $p < (D/A)(1 + \Delta RE/y)$. Since the fraction of positive deficits in the actual data is 69%, the effect of the positive deficit dominates. The mean value of $\Delta RE/y$ for the actual data sample is 0.22.

Panel A: Difference in book leverage between equity issuers and nonissuers



Panel B: Book leverage of equity issuers and nonissuers

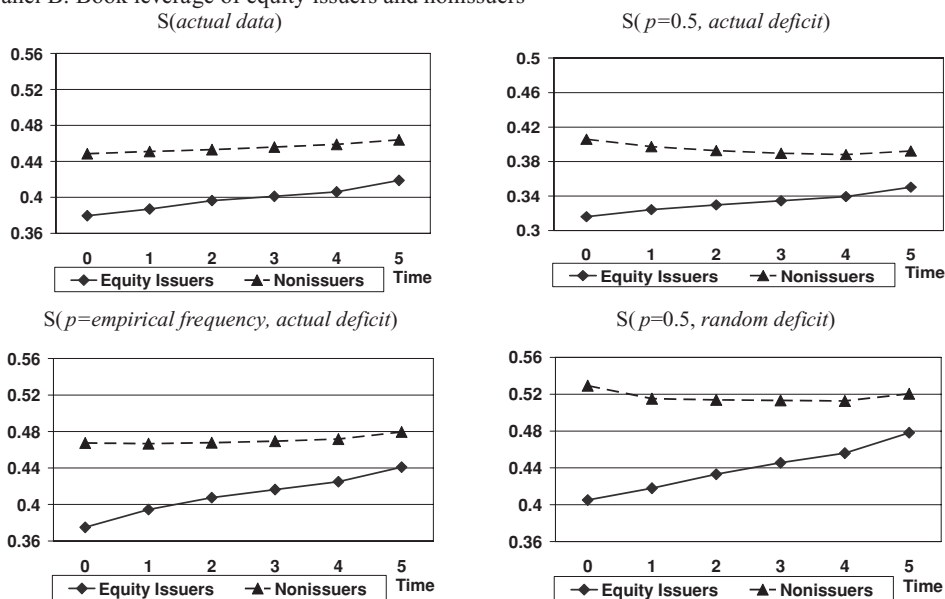


Figure 1. Book leverage following large equity issues. Actual data are collected from Industrial Compustat and CRSP for the years 1971 to 2004. Section II describes how the simulated data are generated. For each data set each year, large equity issuers (nonissuers) are identified if firms' net equity issues divided by total assets is higher (lower) than 5%. Net equity issues equal the change in book equity minus the change in retained earnings. Both equity issuers and nonissuers are then followed over the next 5 event years. The average book leverage is computed across event times over the entire sample period for both equity issuers and nonissuers, respectively. Date 0 corresponds to the end of the issue period. Each simulated sample is generated 500 times, and the average leverage ratio in each event year over 500 simulations is presented. Panel A reports the differences in book leverage between equity issuers and nonissuers for actual and simulated data. Panel B presents the book leverage of equity issuers and nonissuers separately.

debt issues: Debt ratios behave similarly for the actual and simulated samples. These cases are reported in an earlier working paper version of this paper.

C.1.2. Issuance and Repurchase Activity. Another way in which rebalancing behavior has been documented is to examine firms' issuance and repurchase activities subsequent to shocks to the debt ratio. The idea is that if firms are actively rebalancing, then after a shock that lowers (raises) the debt ratio, debt (equity) issuance will increase for firms experiencing the shock compared to other firms. Leary and Roberts (2005) and Altı (2006) examine the subsequent issuance patterns of firms making large equity or debt issues (or experiencing positive or negative equity shocks). We perform a similar analysis to Leary and Roberts (2005) for the actual and simulation samples.²¹ We find that in the actual as well as all simulation samples except $S(p = 0.5, \text{random deficit})$, a higher proportion of firms that issue equity (debt) at event time $t = 0$ subsequently issue more debt (equity) than the nonissuers at $t = 0$. We find that this happens because of serial correlation in the financing deficit: A higher proportion of $t = 0$ issuers (of either debt or equity) have positive deficits in the subsequent years than nonissuers. This implies that relative to nonissuers, not only are they more likely to issue the opposite security to the one that was issued at $t = 0$ (which gives the appearance of rebalancing behavior) but also the *same* security. This is exactly what we find.²² Chen and Zhao (2005a) also find that equity issuers issue more debt (as in Leary and Roberts (2005)), but that they also issue equity, and Welch (2004) notes that although firms are active in issuance after equity shocks, they do not attempt to "correct for" these shocks and move back toward preshock levels.

C.2. Regression Analysis

C.2.1. Persistence versus Reversal. Altı (2006) examines whether firms that "time" the equity market subsequently rebalance. Altı (2006) argues that firms that go public in hot issue markets are market timers. The size of the issue (IPO proceeds from primary issues as well as total proceeds scaled by book value of assets) is higher in hot markets than in cold markets, and the short-run impact of such market timing is to lower the debt ratio of hot market issuers further below the pre-IPO levels than for cold market issuers (the pre-IPO debt ratios are similar for hot and cold market issuers). He then examines whether the

²¹ We only focus on nonparametric analysis on debt (equity) issuance similar to Leary and Roberts (2005). Using quarterly data from Compustat, Leary and Roberts (2005) present other evidence that is consistent with target behavior in the presence of adjustment costs. We do not attempt to replicate those results here.

²² Please see Figures IA.2 and IA.3 in the Internet Appendix for details. For $S(p = 0.5, \text{random deficit})$, there is no serial correlation in the deficit, so we find no difference in the frequency of issuance among issuers and nonissuers. Issuance activity for large debt issues and positive equity shocks, and repurchase activity for negative equity shocks, conform to the pattern of the case discussed here, and appear to be largely driven by the serial correlation in the deficit. Please see an earlier working paper version of this paper.

“hot-market” effect is persistent. He finds that timing effects are not persistent: Firms rebalance within 3 years of the IPO.

Following Altı’s methodology of examining the persistence of the effects of leverage changing events, we estimate the following regression:

$$\begin{aligned} \left(\frac{D}{A}\right)_t - \left(\frac{D}{A}\right)_{pre-event} &= b_0 + b_1 Event + b_2(M/B)_{t-1} + b_3(EBITDA/A)_{t-1} \\ &+ b_4 Size_{t-1} + b_5(PPE/A)_{t-1} + b_6(R\&D/S)_{t-1} \\ &+ b_7 RDD_{t-1} + b_8 \left(\frac{D}{A}\right)_{pre-event} + \varepsilon_t. \end{aligned} \quad (13)$$

The dependent variable is the cumulative change in book leverage (D/A) from its pre-event level to that t years after the event. The dummy variable *Event* equals 1 if a corporate leverage-changing event occurred t years ago and 0 otherwise.²³ If firms rebalance in response to leverage changing events, then the coefficient on *Event* should eventually become smaller in absolute value as t increases. For example, if the event is large equity (debt) issuance, then rebalancing would imply that the coefficient on *Event* will be negative (positive) but become weaker over time. Complete rebalancing would imply that the coefficient would eventually become insignificant or even change sign. The control variables are those employed by Altı (2006).

To save space, in Panel A of Table III, we only report results on the simulation sample $S(p = 0.5, actual\ deficit)$ for the case of large equity issues.²⁴ The results in Panel A show that the coefficient on *Event* is negative but becomes smaller in absolute value from year “*Event* + 1” to year “*Event* + 5”. Thus, if the date 0 deviations for the equity issuers were akin to “shocks” to the debt ratio, that is, movements away from close-to-target levels, these results appear to suggest that these deviations are not persistent. However, by construction, there is no actual rebalancing going on in the data.

We now turn to evidence of reversal in the debt ratio by estimating models of leverage change as in Altı (2006). In particular, we estimate the following leverage change regressions:

$$\begin{aligned} \left(\frac{D}{A}\right)_t - \left(\frac{D}{A}\right)_{t-1} &= c_0 + c_1 Event + c_2(M/B)_{t-1} + c_3(EBITDA/A)_{t-1} + c_4 Size_{t-1} \\ &+ c_5(PPE/A)_{t-1} + c_6(R\&D/S)_{t-1} + c_7 RDD_{t-1} + c_8 HighLev \\ &+ c_9 LowLev + \varepsilon_t, \end{aligned} \quad (14)$$

²³ In Altı (2006), the corresponding dummy variable takes the value of 1 if the firm is a hot market issuer, and 0 otherwise.

²⁴ Results on the actual data and the other simulation samples, as well as for large debt issues and positive and negative equity shocks, are qualitatively similar and consistent with the nonparametric results discussed above. These results are available in an earlier working paper version of this paper.

where $t = \text{event year} + 1$ or $\text{event year} + 3$. Reversal entails the dummy *Event* having a positive (negative) significant coefficient if equity (debt) is issued at $t = 0$. Notice the difference between equations (13) and (14): Equation (13) is about the cumulative change in the leverage ratio but (14) concerns the year-to-year changes.

Of particular interest are the high leverage ratio dummy *HighLev*, which equals 1 if the lagged leverage ratio is above 0.8 (and 0 otherwise), and the low leverage ratio dummy *LowLev*, which equals 1 if the lagged leverage ratio is below 0.1 (and 0 otherwise). These variables, suggested by Altı (2006), are employed to control for mechanical mean reversion, or the fact that debt ratios have a tendency to “rebound” when close to 0 or 1.

Panel B of Table III shows the results for the sample $S(p = 0.5, \text{actual deficit})$. If the high and low leverage dummies are not included, the results indicate that leverage changes that are in the opposite direction to the initial shock are stronger for the date 0 large equity issuers than the nonissuers. However, the magnitude of the coefficient on *Event* becomes somewhat smaller once the high and low leverage dummies are included. So these dummies do mitigate the mechanical effect. Unreported results show that the actual data exhibit slightly weaker reversal and the coefficient on *Event* becomes insignificant in event year +3 when high and low leverage dummies are controlled for. The high leverage dummy has a negative sign and the low leverage dummy has a positive sign for both the actual and the simulated samples, and the coefficients on these dummies are economically large compared to the coefficients on *Event*. This is consistent with equation (8), which suggests that high leverage firms will reduce leverage on average and low leverage firms increase leverage on average, that is, mechanical mean reversion.

However, importantly, controlling for these dummies does not remove all mechanical adjustment, since in the simulated sample, *Event* remains significant even after controlling for these dummies.²⁵

C.2.2. Deviation from Target and Leverage Change over Longer Time Periods.

Kayhan and Titman (2007) examine a number of issues related to the history of firm-specific variables on capital structure. Specifically, they estimate the model

$$\begin{aligned} (D/A)_t - (D/A)_{t-5} = & d_0 + d_1(Def/A)_{[t-5,t]} + d_2StkRtn_{[t-5,t]} \\ & + d_3(EBITDA/A)_{[t-5,t]} + d_4LevDef_{t-5} \\ & + d_5\Delta Target_{[t-5,t]} + d_6 \sum Ind + \varepsilon_t, \end{aligned} \quad (15)$$

where the dependent variable is the change in leverage ratio over a 5-year period, that is, from $t - 5$ to t . Here, *LevDef* is the deviation from target (defined as leverage minus an estimated target) at time $t - 5$, and $\Delta Target$ is the change

²⁵ It should be pointed out that Altı (2006) provides additional evidence based on the issuance activities of “hot” and “cold” market issuers that is consistent with the notion that hot market IPO firms rebalance more actively compared to cold market IPO firms.

in the estimated target (between $t - 5$ and t). A negative coefficient on $LevDef$ and a positive coefficient on $\Delta Target$ are deemed as evidence in support of target behavior.

Kayhan and Titman (2007) are particularly interested in the question of whether the effect of firms' financing deficits, past profits, and stock returns have a more-than-fleeting effect on debt ratios. Accordingly, the specification includes the cumulative financing deficit (Def/A) between $t - 5$ and t , cumulative profitability ($EBITDA/A$) between $t - 5$ and t , 5-year cumulative stock returns ($StkRtn$) from $t - 5$ to t , and three-digit SIC industry dummies (Ind).²⁶

Kayhan and Titman (2007) find that, consistent with target behavior, the deviation from target has a significant negative effect, and the change in the estimated target has a significant positive effect, on the change in leverage between $t - 5$ and t . These findings could be interpreted as evidence of target behavior. In columns (1), (3), (5), and (7) of Table IV, we replicate Kayhan and Titman's results for the actual sample and our simulation samples. For the actual data, we are able to closely replicate their results. However, for our simulated samples, we are also able to replicate almost all their results. The sign of the deviation from target in the simulation samples is consistent with the mechanical mean reversion. A possible reason for the positive coefficient on the change in the estimated target in these samples could be that firms that experience large increases (decreases) in leverage over the 5-year period are also likely to have high (low) estimated target changes, since the estimated target at the end of the 5-year period will be higher (lower) for such firms, especially if the changes are somewhat persistent.

In the rest of Table IV, we consider the issue of how the possibility of mechanical mean reversion is addressed. In their robustness tests (Section 7.4 in their paper), Kayhan and Titman (2007) mention two ways of dealing with this issue. First, they drop all firms with debt ratios under 10%. Second, they include the beginning-of-period debt ratio in addition to the leverage deficit. When we replicate these tests for our samples after removing observations with leverage ratios lower than 0.1, we obtain essentially the same results (reported in columns (2), (4), (6), and (8) in Table IV) as those obtained using the full sample. Unreported results show that adding the beginning-of-period debt ratio has no material effect on the results either. In particular, mechanical mean reversion remains.

The signs of the remaining firm-specific variables in the simulation samples are also similar to those for the actual data. This is not surprising, and is consistent with Kayhan and Titman's interpretation. According to the authors, the reason why these "history" variables are significant for the actual data is because firms do not rebalance away immediately. There is no rebalancing in the simulation samples anyway; hence, these variables are related to changes in the debt ratio in the same way as in the actual data. In fact, there is a close

²⁶ They also consider two variables representing a decomposition of Baker and Wurgler's (2002) external finance-weighted market-to-book ratio, and an interaction term between the financing deficit and a positive financing deficit dummy variable, which captures the asymmetric effect of the financing deficit. We do not include these variables in our reported specifications. However, our main results hold when these variables are included.

Table IV
Change in the Leverage Ratio and Deviation from Target

Actual data are collected from Industrial Compustat and CRSP for the years 1971 to 2004. Section II discusses how the simulated data are generated. The dependent variable is the change in the leverage ratio between year t and $t - 5$. Financing deficit, $(Def/A)_{t-5,t}$, is the sum of financing deficit between year $t - 5$ and t divided by total assets at $t - 5$. Five-year cumulative stock return, $StkRtn_{t-5,t}$, is the cumulative stock return between $t - 5$ and t . Five-year cumulative profitability, $(EBITDA/A)_{t-5,t}$, is the cumulative return on assets between $t - 5$ and t . Deviation from target $(LevDef)$ is the difference between leverage ratio and target leverage ratio at $t - 5$. The target leverage is the predicted value of the regression where the leverage ratio is regressed on the same set of control variables as in Kayhan and Titman (2007). Change in target $(\Delta Target)_{t-5,t}$ is the difference between target leverage at t and target leverage at $t - 5$. The constant term and three-digit SIC industry dummies (Ind) are included in the regression but not reported. For $S(actual\ data)$, the reported coefficients significant at the 10%, 5%, and 1% levels are respectively marked with *, **, and ***. t -statistics in parentheses are calculated from the Huber–White sandwich heteroskedasticity consistent errors. For simulation samples, the reported parameter estimates are the average coefficients obtained from 500 replications of the simulation. 95% confidence intervals are included in square brackets and coefficients are marked with ^a if 95% confidence intervals do not span zero. For each sample, we report coefficients estimated using the full sample and those estimated with beginning-of-period leverage ratios greater than 0.1.

Dependent Variable $(D/A)_t - (D/A)_{t-5}$	$S(actual\ data)$		$S(p = 0.5, actual\ deficit)$		$S(p = empirical\ frequency, actual\ deficit)$		$S(p = 0.5, Random\ Deficit)$	
	(1) Full Sample	(2) $(D/A)_{t-5} > 0.1$	(3) Full Sample	(4) $(D/A)_{t-5} > 0.1$	(5) Full Sample	(6) $(D/A)_{t-5} > 0.1$	(7) Full Sample	(8) $(D/A)_{t-5} > 0.1$
$(Def/A)_{t-5,t}$	0.006*** (9.9)	0.005*** (8.2)	0.001 ^a [0.000, 0.002]	0.000 [-0.001, 0.001]	0.004 ^a [0.003, 0.005]	0.003 ^a [0.002, 0.003]	0.010 ^a [0.007, 0.013]	0.006 ^a [0.003, 0.009]
$StkRtn_{t-5,t}$	-0.006*** (-22.3)	-0.006*** (-21.5)	-0.002 ^a [-0.003, -0.001]	-0.002 ^a [-0.003, -0.001]	0.006 ^a [0.005, 0.007]	0.005 ^a [0.004, 0.006]	0.000 [-0.001, 0.001]	0.000 [-0.001, 0.002]
$(EBITDA/A)_{t-5,t}$	-0.027*** (-28.5)	-0.029*** (-28.9)	-0.057 ^a [-0.060, -0.054]	-0.057 ^a [-0.061, -0.054]	-0.058 ^a [-0.061, -0.056]	-0.061 ^a [-0.063, -0.058]	-0.001 [-0.005, 0.003]	-0.001 [-0.005, 0.003]
$LevDef_{t-5}$	-0.463*** (-107.7)	-0.470*** (-112.2)	-0.412 ^a [-0.426, -0.399]	-0.411 ^a [-0.422, -0.399]	-0.430 ^a [-0.441, -0.419]	-0.433 ^a [-0.449, -0.417]	-0.577 ^a [-0.591, -0.562]	-0.577 ^a [-0.591, -0.563]
$\Delta Target_{t-5,t}$	0.591*** (33.9)	0.611*** (34.1)	0.580 ^a [0.540, 0.620]	0.592 ^a [0.564, 0.621]	0.712 ^a [0.681, 0.744]	0.727 ^a [0.689, 0.765]	0.472 ^a [0.354, 0.590]	0.450 ^a [0.279, 0.622]
R^2	0.28	0.28	0.25	0.24	0.25	0.25	0.28	0.25
N	61,147	59,119	61,147	56,496	61,147	56,605	61,147	55,058

relationship between these results and those in Section III.B where we find several firm-specific variables to be significant in leverage regressions in the simulation samples. We argued in Section III.B that these variables affect the leverage ratio through their effect on the deficit and retained earnings. The results in Table IV directly show that the deficit and retained earnings have more than transitory effects on capital structure.²⁷

IV. Conclusion

We show that existing tests of target behavior based on leverage ratio changes are largely inconclusive. Mean reversion tests of the debt ratio do not distinguish between target behavior and mechanical mean reversion; moreover, estimates of “speeds of adjustment” toward a target are insensitive to the type of financing policy followed by firms. For example, one obtains comparable estimates when target behavior in simulation samples is fairly vigorous as when financing is random. Several other tests that identify rebalancing behavior in the actual sample based on leverage ratio changes also produce very similar results on simulation samples generated via random (nontarget) financing.

Our results have a very clear implication regarding which tests are useful in identifying target behavior. Looking at leverage ratios is not enough, and even possibly misleading. We need to look at financing behavior, that is, debt versus equity choices. If firms follow target behavior, then the deviation from the target variable should have a negative effect on the probability of debt issuance (as opposed to equity) conditional on a positive financing deficit, and a positive effect on the probability of debt retirements (as opposed to equity repurchases) conditional on a negative financing deficit. No such effects, of course, should be present in simulated data when financing is random. Hence, tests based on issuance activity clearly have the power to reject alternative, nontarget, behavior.

The right recommendation for future research is probably a focus on financing decisions as the dependent variable, and then a separate assessment of economic significance. In other words, it might be the case that an equity issue is more likely conditional on high leverage, but the effect on leverage convergence could be small. It is worth noting in this context that existing evidence from firms’ issuance activities is somewhat mixed, indicating a need for research to focus more on issuance decisions and reconcile the facts. Logit or Probit models of debt versus equity choice do not strongly support target behavior. For example, Hovakimian (2004) finds that when combined issue and repurchase transactions are taken out, the deviation from target does not have

²⁷ It is worth emphasizing that Kayhan and Titman (2007) present additional results that our tests here do not address. For example, they show that the same set of “history variables” measured over $t - 10$ to $t - 5$ have weak effects on the change in the debt ratio from $t - 5$ to t , and in some cases (notably, for profitability and the positive financing deficit dummy) the direction of the effects reverses. The authors conclude that their results show that while the effects of deficit, profitability, and stock returns on leverage persist for some time, they eventually tend to reverse.

a significant effect on the choice between pure debt and equity issues. There is somewhat stronger evidence on repurchases, but this mostly comes from debt repurchases by highly levered firms. Chen and Zhao (2005b) also show that the main empirical evidence on financing decisions supporting tradeoff theory comes from highly levered troubled firms because these firms cut debt. This evidence is also consistent with Myers (1984), who realizes that if one excludes debt reduction activities of highly levered firms, the remaining evidence in favor of tradeoff theory is not very strong. On the other hand, Leary and Roberts (2005), who use quarterly data from Compustat, estimate hazard function models to understand the duration between successive debt issues, equity issues, debt retirements, and equity repurchases, and find evidence consistent with dynamic rebalancing in the presence of adjustment costs.

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