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Article

Estimating the Speed of Adjustment of Leverage in the Presence of Interactive Effects*

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Abstract

Dynamic panel data regression models with fixed effects to account for unobserved heterogeneity are standard econometric tools. It is not until recently, however, that the problems involved when fitting such regressions to leverage data have been investigated. The main problem is that models of leverage are extremely noisy, much more so than what can be accommodated using fixed effects. The present article can be seen as a reaction to this. The purpose is to consider a more general interactive effects model in which there are multiple time effects, each with their own firm-specific sensitivities. Our empirical results suggest that proper accounting for the interactive effects and the bias that they cause leads to a marked increase in the estimated speed of adjustment to target leverage.

Key words: bias, capital structure, cross-section correlation, dynamic panel data, interactive effects, leverage

JEL classification: C23, G30

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Introduction

Dynamic Panel Data Models of Leverage

Consider the panel data variable $y_{i,t}$, observable for $t = 1, \ldots, T$ time periods and $i = 1, \ldots, N$ cross-sectional units. Recent years have witnessed an immense proliferation of research enquiring about the speed at which $y_{i,t}$ reverts back toward its long-run equilibrium or target level, typically referred to as the "speed of adjustment" (SOA). Formally, if we denote by $\mu_{i,t}$ the long-run target level of $y_{i,t}$, then the SOA is the slope coefficient, δ, in the following standard partial adjustment model:

$$\Delta y_{i,t} = \delta(\mu_{i,t} - y_{i,t-1}) + \varepsilon_{i,t}, \tag{1}$$

where $\varepsilon_{i,t}$ is a random error term. Examples of situations in which models like this arise are abound. Some of the most recent ones are found in the corporate finance literature. Here, t usually denotes a particular year, while i denotes a particular firm. Common examples of variables include payout, cash holdings, leverage, investments, debt maturity, and governance, to mention a few (see, e.g., Dang et al., 2015, and the references provided therein). In the present article, we focus on leverage, which is the most researched variable by far, and is in fact the topic of a separate literature (see Frank and Goyal, 2008, Graham and Leary, 2011, and Parsons and Titman, 2008, for surveys).[1]

The interest in the SOA in the capital structure literature can be motivated using both theoretical arguments and empirical observations. The apparent failure of the Modigliani–Miller irrelevance proposition has stimulated considerable research aimed at explaining how firms choose their capital structures. But while much progress has been made, there is as of yet no single workhorse theory that fits the stylized facts (see Frank and Goyal, 2008). However, most work centers on the so-called (dynamic) "trade-off" theory, which posits that firms have leverage targets and that they take action to offset deviations from target, leading to a partial adjustment model like the one in Equation (1). According to this theory, the SOA depends on the cost of adjusting leverage. If adjustment costs are zero, firms should never deviate from their target, and so the SOA should be one, whereas if transaction costs are infinite, there should be no adjustment toward a target, implying a SOA of zero. Empirically low estimates of the SOA would therefore go against the trade-off theory, favoring alternative explanations, which do not predict leverage targeting, such as the "pecking order," "market timing," and "stock price" theories. Hence, even if trade-off theory fails, by accounting for the potentially dynamic behaviour of leverage, Equation (1) provides a natural starting point for testing competing theories of leverage (see Flannery and Rangan, 2006). Indeed, as DeAngelo and Roll (2015, page 412) conclude, "[e]mpirically credible theories of capital structure will likely include some form of leverage targeting."[2]

1 We would like to point out, though, that the econometric results that we propose are not in any way specific to applications on leverage, but that they can be used to study dynamic panel data models in general.

2 In a previous version of this article, we considered not only the trade-off theory, but also the pecking, market timing and stock price theories. This version is available upon request from the corresponding author.

The empirical leverage literature based on Equation (1) is huge. Frank and Goyal (2008) survey this literature, and summarize the bulk of the empirical findings as follows: "Corporate leverage is mean-reverting at the firm level. The speed at which this happens is not a settled issue" (page 185). For example, while Fama and French (2002) report "snail's pace" SOA estimates by which deviations from target are closed at a rate of 7–18% per year, the SOA estimates of Flannery and Rangan (2006) are more than two times higher, about 35% per year. Hence, while the estimated SOAs differ quite markedly, there is substantial agreement in the literature over the usefulness of such estimates. In the words of Huang and Ritter (2009, page 239), the estimation of the SOA is "perhaps the most important issue in capital structure research today."

Limitations

As the above discussion makes clear, the model in (1) is the workhorse of the empirical capital structure literature. The model is not problem-free, though. In this subsection, we point to two of the most important limitations that to the best of our knowledge have not yet received a satisfactory solution.

One concern in practice is that target leverage is not observed, and that proper accounting for it necessitates potentially invalid assumptions. The most common assumption by far is that the target is a linear function of a $m \times 1$ vector of lagged "controls" related to the trade-off between the costs and benefits of debt, $x_{i,t-1}$ say, which can then be included into (1) as additional regressors (see, e.g., Huang and Ritter, 2009, Flannery and Rangan, 2006, and Lemmon, Roberts, and Zender, 2008).[3] Hence, by setting $\mu_{i,t} = \gamma' x_{i,t-1}$ and then adding $y_{i,t-1}$ to each side of the equality in (1), the model for leverage becomes

$$y_{i,t} = \rho y_{i,t-1} + \beta' x_{i,t-1} + \varepsilon_{i,t}, \tag{2}$$

where $\rho = 1 - \delta$ and $\beta = \delta \cdot \gamma$. The problem here is that the explanatory power of the controls in $x_{i,t}$ can be very low. Lemmon, Roberts, and Zender (2008) consider this issue. According to their results, the controls only explain 18% of the total variation in leverage, which means that most of the variation is unexplained. This is important because it means that controlling for unobserved heterogeneity is likely to be key in estimating the SOA. The reason is that said heterogeneity is likely to be correlated with the controls, which are then endogenous, thus rendering the ordinary least squares (OLS) estimator inconsistent. For example, many studies include total assets as a measure of firm size (see, e.g., Dang et al., 2015, and Flannery and Rangan, 2006). But firm size is likely to be (negatively) correlated with financial distress costs, which also affects leverage. Hence, unless we can control for financial distress costs, firm size is endogenous.

A major advantage of using panel as opposed to pure cross-section or time series data is the ability to deal with the presence of unobserved heterogeneity. Lemmon, Roberts, and Zender (2008) recognize this, and allow for heterogeneity in the form of additive firm and time fixed effects, which are shown to be much more important than the controls in $x_{i,t}$. In fact, the fixed effects alone account for no less than 61% of the variation in leverage, which should be compared to the 18% explained by the controls (see, e.g., Flannery and Rangan,

3 In the present article, we follow the previous literature (see, e.g., Iliev and Welch, 2010) and refer to the regressors in $x_{i,t}$ as "controls." The reason is that while the target is obviously important, we only use it here as a means to estimate the SOA.

2006, Graham and Leary, 2011, and Parsons and Titman, 2008, for some confirmatory empirical evidence). Their conclusion 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 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 (pages 1593–1594).

Of course, as pointed out by DeAngelo and Roll (2015), the type of heterogeneity that can be permitted using conventional fixed effects is very limited, and is unlikely to be enough to resolve the endogeneity problem. As a partial solution, they suggest including not only year and firm fixed effects, but also their interaction. The problem is that a full set of interaction dummies is not feasible, because of the number of parameters that has to be estimated. The authors therefore only include firm–decade interactions. According to the results, depending on the sample used, the interactions account for between 22.4% and 41.4% of the explained variation, and they are highly significant. Hence, as expected, time and firm fixed effects alone are not enough to account for the unobserved heterogeneity in leverage. Moreover, the proposed dummy variable approach is not only heavily parameterized, but the assumption of no within decade variation is also highly questionable. Observations like this led Dang et al. (2015, page 97) to conclude the following:

> [O]ur study does not account for a possibility that the (individual) firm fixed effects are time-varying or, more generally, that the error components are cross-sectionally correlated. Recent research shows that existing estimation procedures are significantly affected by cross-sectional dependence [. . .]. Thus, it would be important for future studies to examine the properties of alternative methods for estimating dynamic panel data models with cross-sectionally correlated errors.

The inability to control for general forms of unobserved heterogeneity is one of the main limitations of the literature.

The trade-off theory has been criticized on the grounds of not putting enough discipline on the data, and hence for being difficult to distinguish from other theories (see, e.g., Graham and Leary, 2011, and Welch, 2013). In particular, significant SOA estimates are not sufficient to show that firms target leverage, as leverage can be persistent for many reasons.[4] The basic problem here is that Equation (1) is not a structural equation but just a reduced form one. Of course, this problem is not in any way unique to (tests of) the trade-off theory, but applies to most theories in economics and finance. According to Welch (2013), however, there are good reasons to believe that the gap between the structural and reduced form equations is particularly large in the capital structure context. In particular, he argues that many theoretical variables are either poorly proxied or omitted altogether,

4 Shyam-Sunder and Myers (1999) argue that positive SOA is not necessarily incompatible with the pecking order theory. They argue that in a pecking order world in which firms do not have leverage targets, leverage may still appear to be mean reverting. This happens when capital investments are lumpy and positively serially correlated, free cash flows vary over the business cycle and average debt ratio is taken as target.

leading to "deafening" noise in empirical fits of Equation (2). This is the second limitation that we would like to highlight.

The Present Study

The current paper can be seen as a reaction to the above-mentioned limitations. The purpose is to relax the usual fixed effects assumption and to instead consider a so-called "interactive effects" specification, also known as a "common factor structure," in which there are multiple time and firm effects that enter in a multiplicative way. This specification contains the conventional fixed effects models as special cases, but is much more flexible since it allows the time effects (factors) to affect the cross-sectional units with individual specific sensitivities (factor loadings). In terms of the dummy variable approach of DeAngelo and Roll (2015), the interactive effects model considered here not only allows for a complete set of firm–year interactions, but there can also be more than one interaction. Moreover, by allowing both $y_{i,t}$ and $x_{i,t}$ to load on the same set of factors, the controls are not required to be exogenous. This flexibility of our interactive effects model will not only allow us to account for endogeneity due to measurement error and omitted variables, but will also reduce the noisiness of the regression errors, and in this way narrow the gap between the structural and reduced form models.

As pointed out by Iliev and Welch (2010), and Lemmon, Roberts, and Zender (2008) in the context of fixed effects, interactive effects cannot explain the unobserved heterogeneity. They do, however, allow us to control for the effect of this heterogeneity, which is enough if the focus is on estimating the SOA, or any of the other coefficients of (2) for that matter.[5] Of course, this does not really put any more discipline on the data, in the sense that it is still the same theory restrictions that are being tested. However, we believe that models like (2) are interesting enough in their own right, and they are, and continue to be, "among the most widely accepted reduced-form empirical models in leverage studies" (Zhou, Faff, and Alpert, 2014, page 495).

The reminder of this article is organized as follows. In Sections 1 and 2, we present the data and the econometric approach that we are going to use in the empirical analysis. The main econometric problem we face when introducing interactive effects into the dynamic model in Equation (2) is the presence of bias, which we deal with by proposing a new bias-corrected OLS estimator. Section 3 reports the empirical results. The main finding is that fixed effects are not enough to account for the unobserved heterogeneity, and that the estimated SOAs increase markedly when interactive effects are properly taken into account. This means that much of the evidence reported in the previous literature needs to be reevaluated, as the possibility remains that it is due in part to unattended heterogeneity. Section 4 provides some concluding remarks. All econometric details are presented together with a brief Monte Carlo study in the Supplementary Appendix.

5 As pointed out by Welch (2013), another possibility is to use tests that are quasi-experimental. Unfortunately, such tests are very difficult to come by, which explains why they have not attracted much attention in the literature (see Frank and Goyal, 2008, for a survey of some of the existing experimental studies). They will therefore not be considered here.

1 Data

The firm level data set that we will be using is taken from Compustat, which is the most common data source in the literature by far (see Dang et al., 2015, Elsas and Florysiak, 2011, 2015, Flannery and Rangan, 2006, Flannery and Hankins, 2013, Huang and Ritter, 2009, and Lemmon, Roberts, and Zender, 2008, to mention a few). Our particular data set contains observations on 18,843 firms from fifty-one countries covering the period from 1981 until 2015, and is larger than most existing data sets. We have an unbalanced panel. The smallest (largest) number of years per firm is two (34) with an average of 11.27 years per firm. The total number of observations per variable in the data set is 212,352.

Following the bulk of the previous literature, $y_{i,t}$ is market leverage (MLEV) (see, e.g., Flannery and Rangan, 2006).[6] The controls included in $x_{i,t}$ are selected based on data availability and their importance in studies such as Dang et al. (2015), Elsas and Florysiak (2011, 2015), Flannery and Rangan (2006), Flannery and Hankins (2013), Huang and Ritter (2009), and Grahama et al., (2015), to mention a few. They are profit (EBIT), market-to-book ratio (MB), depreciation (DEP), logarithm of total assets (SIZE), tangibility (FA), industry median of MLEV (MMLEV), liquidity (LIQ), GDP growth (GDPG), inflation (INF). All controls are observed at the firm level, except MMLEV, which is observed at the industry level, and GDPG and INF, which are observed at the country level.[7] All variables are winsorized at the first and 99-th percentiles to remove the effect of outliers (as in, e.g., Dang et al., 2015, Elsas and Florysiak, 2011, 2015, Flannery and Hankins, 2013, and Flannery and Rangan, 2006). For a detailed description of each variable, we refer to Table 1. All controls enter Equation (2) with a lag, which reduces the risk of reversed causality (see, e.g., Dang et al., 2015, for a discussion).

In addition to the full sample of firms, we consider three subsamples. Studies such as Dang et al. (2015), Flannery and Hankins (2013), and Zhou, Faff, and Alpert (2014) remove all non-U.S. firms and therefore so do we. One of the subsamples is therefore the United States. Another split that we consider is between developing and developed countries, as firms' leverage decisions are likely to depend on the level of development of the country in which it operates (see Booth et al., 2001). The total number of firms per subpanel in each split can be found in Table 2.[8]

2 The Econometric Approach

2.1 The Interactive Effects Model

According to the trade-off theory, firms target leverage. The problem here is, as we pointed out in "Introduction" section, that the target is never fully known, and researchers therefore try to proxy it as accurately as possible. It is important to note here that the target itself is not of interest to us, but we only need it as a control in our quest to estimate the SOA

6 The results for book leverage are very similar and are available upon request.

7 The data for the country level variables are retrieved from the Global Financial Database.

8 In a previous version of this article, we also splitted the firms according to whether they are over- or under-leveraged, as in Flannery and Hankins (2013). Another split that we have considered is by firm size. The results for these splits were, however, very similar are therefore not reported.

Table 1 Variable description, expected signs and data source

Variables	Sign	Description	Source	Data item number
MLEV		Market value leverage: (long-term debt + short-term debt)/(total assets—book equity + market value of equity)	Compustat	$([9]+[34])/([6]-[216]+[199]\times[25])$
EBIT	–	Profit: (operating income + interest and related expense + current income taxes)/total assets	Compustat	$([18]+[15]+[16])/[6]$
MB	–	Market-to-book: (long-term debt + short-term debt + preferred capital + market value of equity)/total assets	Compustat	$([9]+[34]+[10]+[199]\times[25])/[6]$
DEP	–	Depreciation: total depreciation and amortization/total assets	Compustat	[14]/[6]
SIZE	+	Log of total assets	Compustat	[6] deflated by CPI
FA	+	Tangibility: fixed assets/total assets	Compustat	[14]/[6]
MMLEV	+	MLEV industry median: the median market value leverage ratio based on the 48 industry groupings of Fama and French	Compustat	
LIQ	–	Liquidity: total current assets/total current liabilities	Compustat	[4]/[5]
INF	+	Annual inflation rate: growth in consumer price index	Global Financial Database	
GDPG	+/–	Annual growth in nominal GDP	Global Financial Database	

Notes: All variables are winsorized at the 1st and 99th percentiles to eliminate the effects of outliers. Data at time t represents the data for the fiscal year $[t, t+1]$. For example, data for a fiscal year beginning on June 1, 2000, and ending on May 31, 2001 is reported as data year 2000. “Sign” refers to the expected sign of the coefficient of the variable when included in the model for leverage.

Table 2 Tests for residual cross-firm correlation and R^2

Sample	Firms	ABS	CORR	CD	p-value	LM	p-value	FE-R^2	PC-R^2
All	18,843	0.21	0.04	2511.6	0.00	7425.3	0.00	0.41	0.61
US	2162	0.18	0.04	328.0	0.00	495.5	0.00	0.42	0.60
Developed	8947	0.20	0.04	1356.3	0.00	2828.8	0.00	0.43	0.61
Developing	9896	0.22	0.05	1815.5	0.00	5081.5	0.00	0.40	0.62

Notes: "Firms" refers to the number of firms in each sample, "CORR" ("ABS") refers to the average (absolute) pairwise correlation coefficient, "CD" ("LM") refers to the CD (LM) test of null hypothesis of zero cross-correlation based on the level (squared) cross-correlation coefficient, and "FE-R^2" ("PC-R^2") refers to the overall R^2 when controlling for fixed effects (estimated PC factors).

(Iliev and Welch, 2010). This is how the previous literature has motivated the use of fixed effects; as a way to close the discrepancy between the true target and its proxy (see Huang and Ritter, 2009, Flannery and Rangan, 2006, Flannery and Hankins, 2013, Frank and Goyal, 2008, and Iliev and Welch, 2010, to mention a few). The fixed effects therefore enter the model through the target itself. Hence, instead of assuming $\mu_{i,t} = \gamma' x_{i,t-1}$, as we did in "Introduction" section, many researchers assume that $\mu_{i,t} = \alpha_i + \eta_t + \gamma' x_{i,t-1}$, where α_i and η_t are firm and time fixed effects, respectively. But as DeAngelo and Roll (2015) point out, fixed effects are unlikely to be enough to capture all of the unobserved target heterogeneity. This motivates the following target specification with interactive effects:

$$\mu_{i,t} = \theta_i' F_t + \gamma' x_{i,t-1}, \tag{3}$$

where F_t is an $r \times 1$ vector of unknown common factors with θ_i being the associated vector of factor loadings. The interactive effects are here given by $\theta_i' F_t$. This is the part of the leverage target that is unobserved. It is convenient to think of F_t as representing common shocks, such as liquidity shocks, financial crises, herd behavior, or financial and labour market frictions (see, e.g., Mitton, 2002; Rajan and Zingales, 1995), and to think of θ_i as the firm-specific exposure to these shocks. One possibility here is that $\theta_1 = \cdots = \theta_N = 1$, in which case $\theta_i' F_t = F_t$ is nothing but a common time fixed effect. The restriction that $F_1 = \cdots = F_T = 1$ is, however, more common (see, e.g., Frank and Goyal, 2003, and Huang and Ritter, 2009). This means that $\theta_i' F_t = \theta_i$, leading to a model with firm-specific fixed effects only. The most common specification, though, it to allow both time and firm fixed effects, which is tantamount to setting $\theta_i = [\alpha_i, 1]'$ and $F_t = [1, \eta_t]'$, such that $\theta_i' F_t = \alpha_i + \eta_t$ (see Dang et al., 2015, Flannery and Hankins, 2013, Flannery and Rangan, 2006, and Lemmon, Roberts, and Zender, 2008, to mention a few). The dummy variable specification of DeAngelo and Roll (2015) mentioned in "Introduction" section is more general, and is obtained by setting $\theta_i = [\alpha_i, 1, \gamma_i]'$ and $F_t = [1, \eta_t, g_t]'$, where g_t constant within decades, such that ${\theta_i}' F_t = \alpha_i + \eta_t + \gamma_i g_t$. The interactive effects specification considered here is even more general in that it does not put any restrictions on the within-decade variation of F_t, nor on r.

By substituting (3) into (1) and letting $\lambda_i = \delta \cdot \theta_i$, we obtain the following factor-augmented version of (2):

$$y_{i,t} = \rho y_{i,t-1} + \beta' x_{i,t-1} + \lambda_i' F_t + \varepsilon_{i,t}. \tag{4}$$

where $\rho = 1 - \delta$ and $\beta = \delta \cdot \gamma$, as before. This equation says that firms take action (issues securities) to close the gap between where they are ($y_{i,t-1}$) and where they wish to be ($\theta_i' F_t + \gamma' x_{i,t-1}$).[9] Because of adjustment costs, however, deviations from target will not be closed instantly but only gradually. However, provided $\delta \in (0, 1]$, so that $|\rho| < 1$, firms' actual leverage eventually converges to its target. Hence, if the trade-off theory holds, δ should be significantly different from zero, and the closer it is to one, the faster the convergence to target.

The expected sign of each control variable in $x_{i,t}$ is given in Table 1. Here we summarize some of the main arguments (see, e.g., Antoniou, Guney, and Paudyal, 2008, and Parsons and Titman, 2008, for a more extensive discussion). The sign on EBIT is expected to be negative because as firms' profits increase the need to employ leverage to finance investments declines. MB is used as a proxy for investment opportunities. As investment opportunities rise the need to employ leverage declines, implying that the effect of MB should be negative. Depreciation (DEP) reduces corporate tax burden and is therefore expected to have a negative effect on leverage. The expected sign on SIZE is positive. The reason is that as firms become larger they have capacity to carry more debt because the cost of carrying debt is lower. FA is expected to have a positive effect on leverage, as fixed assets act as a signal of confidence to creditors that firms have capacity to repay loans. LIQ is expected to have a negative effect on leverage as firms with higher liquidity have more liquid assets which can be used as a source of internal finance in place of debt. Finally, the sign on INF is expected to be positive, because inflation increases the cost of debt financing.

As mentioned in Section 1, while MMLEV is observed at the industry level, GDPG and INF are observed at the country level. Hence, while here we treat them as additional controls, an alternative way to think about these variables is as observed common factors for each industry/country. Huang and Ritter (2009) estimate the SOA while controlling for aggregate market conditions, as proxied by the implied market equity risk premium, the nominal interest rate, the default spread, the term spread, the corporate tax rate, market returns, and GDP growth. While not formulated in this way, the model of Huang and Ritter (2009) can be seen as a version of our interactive effects model in which F_t represents aggregate market conditions.[10]

2.2 Bias

A major obstacle when fitting dynamic models with fixed effects is the so-called "Nickell bias" (Nickell, 1981). This is a small-T bias that is due to the fact that the within transformation used to eliminate the fixed effects makes the regression error correlated with the

9 Hence, while one could in principle allow $x_{i,t}$ to enter (4) without lag, this would be inconsistent with the partial adjustment idea. Moreover, as pointed out in Section 1, by lagging $x_{i,t}$ we reduce the risk of reversed causality.

10 Grahama et al., (2015) consider a unique data set on unregulated and regulated U.S. firms. According to their results, starting at the end of World War II unregulated firms have experienced a dramatic increase in leverage that cannot be explained by firm-specific characteristics, but rather by changes in country-wide aggregates such as government borrowing, macroeconomic uncertainty and financial sector development. These aggregates are obvious candidates for known factors that may be included in F_t.

transformed lagged dependent variable. Hence, we would like to come to terms with the omitted variables bias caused by unattended heterogeneity. But when we account for fixed effects, we simultaneously incur another type of bias. However, unlike the omitted variables bias, the Nickell bias has a known structure, which means that we can correct for it.

The Nickell bias has attracted considerable interest among econometricians (see MoonPerron, and Phillips, 2015, for a recent survey); however, it is not until recently that the implications of this bias for models of leverage has been formally investigated. Recent examples of such studies include Flannery and Hankins (2013), Zhou, Faff, and Alpert (2014), and Dang et al. (2015), who all use calibrated Monte Carlo simulations as a means to study the small-sample performance of some commonly used estimators, such as instrumental variables (IV), generalized methods of moments (GMM), OLS and its bias-corrected counterpart (see Baltagi, 2008, chapter 8, for an overview). The main take-away is that the issue of bias cannot be ignored and that the relative performance of the estimators depends on the assumed data generating process used in the simulations. Specifically, while bias correction seems to work very well under the standard assumption of independent errors (see, e.g., Dang et al., 2015; Zhou, Faff, and Alpert, 2014), most estimators tend to suffer when this assumption is not met. The reason is that the bias depends on the properties of $\varepsilon_{i,t}$, and that the effectiveness of bias elimination procedures based on independent errors may be impaired if the errors are in fact dependent, which will be the case if the fixed effects assumption is violated. It follows that in order to allow for interactive effects, we have to address the issue of bias, and this is one of the contributions of our article. While we could in principle use IV or GMM, in the present article we focus on bias correction. The hope is that by doing so the estimator will inherit some of the good small-sample properties of bias-corrected OLS in the case of fixed effects. An additional advantage of using bias correction is that it removes the need for valid instruments, which are known to be difficult to come by and even if there are instruments available they tend to be weak, leading to poor estimation accuracy (see, e.g., Dang et al., 2015, Huang and Ritter, 2009, Lemmon, Roberts, and Zender, 2008, Parsons and Titman, 2008, and Zhou, Faff, and Alpert, 2014, for discussions and confirmatory results).

In the Supplementary Appendix, we derive the bias when the fixed effects assumption is violated due to the presence of interactive effects. The main findings are twofold. First, the interactive effects impact the analytical bias formula for the estimator of the SOA, thereby invalidating the conventional Nickell formula and its predictions. Hence, not only do the interactive effects cause the conventional bias-corrected OLS estimator to break down, but we also do not know in which direction the bias is going. It follows that while correct in the fixed effects case, in the more general model considered here the conventional wisdom that fixed effects OLS is biased downwards is not necessarily true (see, e.g., Graham and Leary, 2011). Second, if the impact of the interactive effects on the SOA estimator is problematic, the consequences for the estimator of β is devastating in that it is not even consistent.

In the Supplementary Appendix, we show that the analytical bias can be estimated and subtracted, leading to a bias-corrected OLS estimator that is valid even in the presence of interactive effects. The estimator requires a (rotationally) consistent estimator of the unknown factors in F_t, which are then included in the model as additional controls.[11] There

11 Because in our model λ_i and F_t are both unknown, they are not separately identifiable, which means that consistent estimation of F_t is not possible. We can, however, estimate F_t consistently

are two popular approaches. The first is the principal components (PC) method of Bai (2009), which proceeds in an iterative fashion. The idea is to first ignore the interactive fixed effects and to estimate (4) by pooled OLS. By applying the PC method to the estimated residuals, we get a first-step estimator of F_t, which can be used to estimate the interactive effects model in (4). New residuals can be obtained on which the PC method can be applied to give an even better estimate of F_t. The model is then reestimated. This procedure is repeated until convergence.[12] The second approach is the cross-section average (CA) method of Pesaran (2006), which, as the name suggests and as we explain in detail in the Supplementary Appendix, is based on taking the cross-sectional averages of the observables as estimated factors.[13] In Section 3, we consider both approaches.

3 Empirical Results

3.1 The Fixed Effects Assumption

As already pointed out, fixed effects, which are special interactive effects, are standard in the literature. To get a feeling for the validity of the fixed effects assumption, in Table 2 we report some descriptives based on the pair-wise cross-firm correlation of the residuals obtained by fitting the model in (4) by bias-corrected firm and year fixed effects OLS. The idea here is that if fixed effects are enough the residuals should be uncorrelated across firms, whereas if fixed effects are not enough, then there should be some cross-firm correlation remaining. Two tests are considered. The first is the "CD" test discussed in Pesaran, Ullah, and Yamagata (2008), which is based on the average of all pairwise correlation coefficients. This test is very popular and is the workhorse of the empirical literature. Of course, the fact that the test is popular does not mean that it is problem free. One drawback of the CD test is that it lacks power when the average correlation is close to zero, and it is inconsistent when said average is exactly zero. This led Baltagi, Feng, and Kao (2012) to consider a Lagrange multiplier (LM) test based on the squared pairwise correlation coefficients, which does not suffer from the same averaging out problem. This is the second test that we consider. We also report the average pair-wise correlation coefficient itself as well as the average absolute correlation coefficient.

The results reported in Table 2 reveal that while the average correlations are all close to zero, the average absolute correlations are much larger and fall in the [0.18, 0.22] range. Hence, while there is a substantial degree of cross-correlation in the data, much of this correlation is averaged out in the CD test. Both tests are significant, though, suggesting that the cross-correlation is important even if we do not account for the averaging out effect. The main implication of this is that fixed effects are not enough to account for the cross-correlation in the regression errors, which casts doubt on much of the fixed effects-based

up to an $r \times r$ positive rotation matrix, which is enough as we are only concerned about controlling for the effect of the interactive effects. We therefore say that the estimated factors are "rotationally" consistent.

12 Another possibility is to follow, for example, Greenaway-McGrevy, Han, and Sul (2012), and to apply the PC method directly to the observed variables. However, unreported simulation evidence suggests that the PC method applied to the estimated residuals works best. In this article, we therefore only consider this latter approach.

13 Pesaran (2006) refers to his approach as "common correlated effects" or "CCE." We use "CA" because it provides a more accurate description of what the approach does.

results reported previously in the literature. Hence, as DeAngelo and Roll (2015, page 382) point out, "[u]se of a purely additive specification, that is, one that excludes interaction effects, is not a mandate of the data."

The CA approach requires that the average of the firm-specific factor loadings (λ_i) is non-zero. The fact that much of the cross-correlation averages out in the CD test suggests that this condition is not met and therefore that the CA-based factor estimates might not be reliable. In this section, we therefore focus on PC, although we still report the results based on CA for completeness.

Table 2 also reports the R^2 results obtained by fitting the model in (4) by bias-corrected OLS. We estimate two specifications, one with firm and year fixed effects, and one with PC factors, which, as we just pointed out, is our preferred interactive effects estimator. As expected given the descriptives for the fixed effects-based residuals, R^2 increases quite markedly as the PC factors are added to the model. The fixed effects R^2 fall in the [0.40, 0.43] range, which is largely consistent with the results of Flannery and Rangan (2006), whereas when the PC factors are included, the same range is [0.60, 0.62], which represents an improvement of around 50%.

3.2 SOA

We consider no less than seven estimators. They are (i) simple OLS with no effects (NE), (ii) firm and year fixed effects OLS (FE), (iii) bias-corrected firm and year fixed effects OLS (BCFE), (iv) PC-augmented OLS (PC), (v) bias-corrected PC-augmented OLS (BCPC), (vi) CA-augmented OLS (CA), (vii) bias-corrected CA-augmented OLS (BCCA). NE is a naive estimator that does not account for any type of unobserved heterogeneity. FE and BCFE are better but are still not expected to perform very well, as the fixed effects assumption do not seem to hold. CA and BCCA do allow for interactive effects and are in this sense more general than NE, FE and BCFE. However, CA and BCCA require that the factor loadings do not average to zero, and the results reported in Section 3.1 suggest that they do. The PC estimator allows for interactive effects and it does not place any conditions on the average loadings. It does, however, suffer from bias, which BCPC does not. Moreover, the bias is larger the smaller is T, suggesting that in our small-T sample bias correction is likely to be important. It follows that among the seven estimators considered here, BCPC is expected to lead to the most reliable results. The other estimators are included for comparison purposes. PC requires estimation of the number of factors, r. Here we treat this as a model selection problem, and estimate r using the information criterion of Bai (2009). As for the implementation of CA, we note that unlike in the static model considered by Pesaran (2006), here it is not enough to augment with the cross-sectional averages of $y_{i,t}$ and $x_{i,t-1}$, but we also need the cross-sectional average of $y_{i,t-1}$ (see De Vos and Everaert, 2019, for an extended discussion).[14]

Tables 3–5 contain the main results, which are split by the parameters of interest. Table 3 reports the results of the estimated SOA, which are presented together with the

14 De Vos and Everaert (2019) assumes that $x_{i,t}$ is in itself a dynamic process, which requires augmentation by the cross-sectional averages of the lags of $x_{i,t-1}$. Here we assume that the serial correlation in $x_{i,t}$ emanates from its common and idiosyncratic components (as in Pesaran, 2006), which removes the need for the cross-sectional averages of the lags of $x_{i,t-1}$. Unreported results suggest that the results are not sensitive to this assumption.

Table 3 Estimated SOA and half-lives

Sample		NE	FE	BCFE	PC	BCPC	CA	BCCA
Controls included								
All	SOA	0.126***	0.381***	0.257***	0.435***	0.355***	0.641***	0.614***
		(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.007)	(0.003)
	Half-life	5.16	1.45	2.33	1.21	1.58	0.68	0.73
US	SOA	0.135***	0.358***	0.247***	0.406***	0.333***	0.602***	0.576***
		(0.003)	(0.005)	(0.005)	(0.006)	(0.006)	(0.018)	(0.008)
	Half-life	4.79	1.57	2.45	1.33	1.71	0.750	0.81
Developed	SOA	0.121***	0.367***	0.251***	0.392***	0.310***	0.560***	0.529***
		(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.009)	(0.004)
	Half-life	5.40	1.52	2.40	1.39	1.87	0.84	0.92
Developing	SOA	0.128***	0.398***	0.274***	0.440***	0.357***	0.532***	0.487***
		(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.007)	(0.004)
	Half-life	5.05	1.37	2.17	1.20	1.57	0.91	1.04
Controls excluded								
All	SOA	0.122***	0.348***	0.209***	0.344***	0.156***	0.345***	0.229***
		(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
	Half-life	5.34	1.62	2.95	1.65	4.10	1.64	2.67
US	SOA	0.133***	0.343***	0.226***	0.352***	0.195***	0.330***	0.226***
		(0.003)	(0.004)	(0.004)	(0.007)	(0.007)	(0.006)	(0.006)
	Half-life	4.85	1.65	2.70	1.60	3.19	1.73	2.70
Developed	SOA	0.118***	0.343***	0.217***	0.338***	0.169***	0.348***	0.244***
		(0.001)	(0.002)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)
	Half-life	5.53	1.65	2.84	1.68	3.74	1.62	2.48
Developing	SOA	0.128***	0.348***	0.201***	0.349***	0.143***	0.349***	0.215***
		(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
	Half-life	5.07	1.62	3.09	1.62	4.50	1.62	2.86

Notes: "NE", "FE", "PC" and "CA" refer to OLS with no effects, fixed effects OLS, OLS augmented with estimated PC factors, and OLS augmented with cross-sectional averages, respectively. "BCFE", "BCPC" and "BCCA" refer to the bias-corrected versions of FE, PC and CA, respectively. The SOA is computed as $1-\hat{\rho}$, where $\hat{\rho}$ is the relevant estimator of ρ and the numbers within parentheses are the standard errors. The half-life is computed as $\ln(0.5)/\ln(\hat{\rho})$. Because the half-life only depends on $\hat{\rho}$, we do not report its standard error. "***", "**" and "*" denote significance at the 1%, 5%, and 10% level, respectively.

associated "half-life" measure, which is the number of years it takes for a one-unit leverage shock to reduce by half (see, e.g., Huang and Ritter, 2009, Lemmon, Roberts, and Zender, 2008, and Zhou, Faff, and Alpert, 2014). Iliev and Welch (2010) argue that knowledge of the true target is inconsequential, provided that fixed effects are included. In Table 3, we therefore report SOAs and half-lives not only for our benchmark specification with the controls in $x_{i,t}$ included, but also for a pure dynamic model without controls. The results for the effects of the controls are reported in Tables 4 and 5. Table 4 contains the results for all firms, while Table 5 contains the results for our three subsamples.

Huang and Ritter (2009), and Zhou, Faff, and Alpert (2014) discuss to what extent estimated half-lives are low enough for a meaningful long-run leverage targeting level. According to their results, while half-lives of 2.5 to three years can be considered as

Table 4 The estimated effect of the controls for all firms

Regressor	NE	FE	PC	CA
EBIT	0.007***	−0.031***	−0.020***	−0.026***
	(0.002)	(0.002)	(0.003)	(0.006)
MB	0.000*	0.000**	0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
DEP	−0.175***	−0.219***	−0.202***	−0.139***
	(0.008)	(0.013)	(0.015)	(0.038)
SIZE	0.017***	0.316***	0.300***	0.302***
	(0.000)	(0.005)	(0.005)	(0.015)
FA	0.028***	0.027***	0.028***	0.046***
	(0.001)	(0.002)	(0.003)	(0.007)
MMLEV	−0.148***	0.014	0.213***	0.185***
	(0.004)	(0.013)	(0.007)	(0.039)
LIQ	−0.001***	0.000	0.000	−0.001
	(0.000)	(0.000)	(0.000)	(0.001)
GDPG	0.167***	0.052***	0.009	0.196***
	(0.006)	(0.011)	(0.008)	(0.032)
INF	0.055***	0.108***	0.182***	0.107***
	(0.007)	(0.012)	(0.011)	(0.037)

Notes: "NE", "FE", "PC" and "CA" refer to OLS with no effects, fixed effects OLS, OLS augmented with estimated PC factors, and OLS augmented with cross-sectional averages, respectively. These are not bias-corrected, because it is only the SOA that requires bias-correction. See Table 1 for an explanation of the variables. All regressions include one lag of the dependent variable (MLEV). The results for this variable are reported in Table 3. The numbers within parentheses are the standard errors, with "***", "**" and "*" denoting significance at the 1%, 5%, and 10% level, respectively.

moderately low, half-lives of 0.9 to 1.3 years are generally considered as low enough. Half-lives over three years are considered as too high for meaningful leverage targeting. According to the results reported in Table 3 for the benchmark specification with controls, the NE-based SOA estimates vary from 0.121 to 0.135, which translates into half-lives of between 4.79 and 5.4 years. These are similar to the half-lives reported by Fama and French (2002), who also use OLS without any effects, and are too high for meaningful leverage targeting. Of course, the NE estimator does not allow for any form of unobserved heterogeneity, and is therefore prone to be misleading. FE is more reliable. The FE-based SOA estimate for the full sample is 0.381, which is highly significant when compared to normal critical values, and the resulting half-life is 1.45 years. These are close to the fixed effects results reported by Flannery and Ragnan (2006). The results for the three subsamples are very similar. The estimated SOAs range between 0.358 and 0.398, and the resulting half-lives lie between 1.37 and 1.57 years. Hence, based on this evidence, one would conclude, as much of the previous literature has done (see, e.g., Flannery and Rangan, 2006, Leary and Roberts, 2005, and Huang and Ritter, 2009), that the half-lives are low enough, and therefore that trade-off theory is supported by the data.

As explained in Section 2, however, fixed effects render the OLS estimator downwards biased, which means that the bias of the estimated SOA is going in the other direction. That is, the bias will make the SOA upwards biased, implying that the estimated half-life will be

Table 5 The estimated effect of the controls by subsample

	US		Developed		Developing	
Regressor	FE	PC	FE	PC	FE	PC
EBIT	–0.019***	–0.010**	–0.014***	–0.009***	–0.076***	–0.037***
	(0.004)	(0.004)	(0.002)	(0.003)	(0.004)	(0.005)
MB	0.000	0.001	0.000	0.000	–0.001***	0.000
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
DEP	–0.019	–0.031	–0.079***	–0.063***	–0.385***	–0.344***
	(0.028)	(0.034)	(0.015)	(0.019)	(0.022)	(0.024)
SIZE	0.197***	0.144***	0.264***	0.196***	0.397***	0.329***
	(0.011)	(0.011)	(0.006)	(0.006)	(0.008)	(0.007)
FA	0.020***	0.039***	0.023***	0.029***	0.031***	0.039***
	(0.006)	(0.008)	(0.003)	(0.004)	(0.003)	(0.004)
MMLEV	–0.019	–0.041**	0.032**	0.106***	0.039*	0.312***
	(0.029)	(0.019)	(0.015)	(0.010)	(0.021)	(0.012)
LIQ	0.001*	0.001***	0.001**	0.001***	–0.001	–0.001**
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
GDPG	–	–0.003	0.014	0.020*	0.061***	0.042***
		(0.025)	(0.020)	(0.010)	(0.015)	(0.011)
INF	–	0.381***	–0.162***	–0.011	0.101***	0.094***
		(0.046)	(0.023)	(0.020)	(0.014)	(0.011)

Notes: "NE", "FE", "PC" and "CA" refer to OLS with no effects, fixed effects OLS, OLS augmented with estimated PC factors, and OLS augmented with cross-sectional averages, respectively. These are not bias-corrected, because it is only the SOA that requires bias-correction. See Table 1 for an explanation of the variables. GDPG and INF are observed at the country level. Their effect is therefore absorbed by the time fixed effects. The numbers within parentheses are the standard errors, with "***", "**" and "*" denoting significance at the 1%, 5%, and 10% level, respectively.

biased downwards. This bias effect is quite clear when looking at the results of the BCFE estimator. In fact, the bias correction brings most SOA estimates down by about 30%, which is partly expected given the smallness of T. The new SOA range is [0.247, 0.274] and the resulting half-life range is [2.17, 2.45], which is just below the moderately low three-year mark. Hence, while very close to the mark, these half-lives still represent a major improvement when compared to those obtained based on NE. In fact, accounting for fixed effects and the bias they cause bring the half-lives down by about one half. Hence, allowing for fixed effects is just as important as accounting for bias. But then again we know from Section 3.1 that fixed effects are not enough to capture all of the unobserved heterogeneity. We therefore put the FE results aside and focus instead on our preferred BCPC estimator. The estimated SOAs based on this estimator lie between 0.31 and 0.357, leading to half-lives that are in the [1.58, 1.87] range, which is consistent with Flannery and Rangan (2006), and Antoniou, Guney, and Paudyal (2008), who document half-lives of 1.6 and 1.8 years, respectively. Hence, allowing for interactive effects leads to another marked reduction in the estimated half-lives. When compared to the BCFE results the reduction is roughly 30%. According to Huang and Ritter (2009), and Zhou , Faff, and Alpert (2014), half-lives like these are low enough for a meaningful long-run leverage level, which provides support in favor of the trade-off theory.

The above discussion is for our benchmark specification with controls. In order to assess the importance of these, consider next the results reported in Table 3 for the specification without controls. According to the results of Iliev and Welch (2010), provided that fixed effects are included, knowledge of target leverage does not substantially improve the accuracy of the estimated SOA, which is taken to imply that the SOA can be estimated even without good controls for target leverage. Consistent with this, we see that the results for FE and BCFE do not change much when the controls are excluded. The picture is, however, quite different if we instead look at the results for our preferred BCPC estimator, which is partly expected given that the fixed effects assumption is violated and because many of the controls are in fact significant, as we explain in detail in Section 3.3. The logic here is the following: Because they are significant, we cannot just ignore the controls, as is clear from the BCPC results. FE and BCFE are not affected to the same extent, but then these estimators are not reliable anyways, as the fixed effects assumption is not met. Hence, apparently failure to account for unobserved heterogeneity partly masks the effect of the controls. This is important, because it casts doubt on the conclusion of Iliev and Welch (2010, page 22) that "[a]gonizing over controls is misplaced."

Studies such as Flannery and Rangan (2006), and Lemmon, Roberts, and Zender (2008) argue that the previously documented low SOAs are partly due to the use of estimation techniques that do not control properly for unobserved heterogeneity. Our results support this argument, and show that it applies also outside the fixed effects environment, where only a few have yet ventured. As pointed out already in "Introduction" section, fixed effects are special interactive effects. The fact that the estimated PC factors are consistent therefore means that if the true model is a fixed effects one, then FE and PC are expected to lead to very similar results, at least in large samples. But then this is not what we observe. The difference between the FE- and PC-based SOA estimates can therefore be seen as a measure of the importance of allowing for interactive effects as opposed to regular fixed effects. The results reported here therefore strengthen the evidence reported in Section 3.1 against fixed effects.

3.3 The Effects of the Controls

Let us now consider the effect of the controls, whose expected signs are given in Table 1. We begin by considering the results for all firms reported in Table 4. The first thing to note is that the PC-based results are quite different from the FE-based ones, which are in turn very similar to the results reported by Elsas and Florysiak (2011), Flannery and Hankins (2013), and Flannery and Rangan (2006). Note in particular that while the sign of the estimated coefficients are the same, their magnitude differ quite substantially depending on whether we include fixed effects or estimated PC factors. This is not surprising, given the inconsistency of the fixed effects OLS estimator of β in the presence of interactive effects, and the fact that the fixed effects assumption is refuted by the data. Hence, consistent with the SOA results of Section 3.2, the previously documented sensitivity of the effects of the controls with respect to fixed effects (see, e.g., Lemmon, Roberts, and Zender, 2008, and Frank and Goyal, 2008) does not stop here, but extends to interactive effects. Because of this and the already documented bias of the fixed effects-based SOAs, in what follows, we focus on the results for the PC-based OLS estimator. We see that the PC-based point estimates on all variables have their expected signs. In particular, the estimates are negative for EBIT, MB, DEP and LIQ, and positive for SIZE, FA, MMLEV, TAX, GDPG and INF, although LIQ and GDPG are insignificant. This reinforces the already reported evidence in favor of trade-off theory.

Consider next the FE and PC results for the three subsamples reported in Table 5.[15] Looking first at the U.S. subsample, we see that the results are quite different from those reported in Table 4 for all firms. The magnitude of most estimates changes markedly, and the estimates of MMLEV, LIQ and GDPG even change sign. The fact that U.S. firms appear to behave rather differently from firms in other countries is consistent with the results reported by Antoniou, Guney, and Paudyal (2008), who consider the G5 countries (France, Germany, Japan, the UK, and the United States). Note in particular how the relatively weak effect of DEP and SIZE is consistent with the results of this other paper. The fact that the effect of DEP differs depending on the sample is expected, because the implication of tax on the dept depends on the design of the tax system, which varies across countries. Due to lower information asymmetry, larger firms have easier access to debt markets and can borrow at a lower cost. Of course, being a highly developed country, such information asymmetry is much less of a problem in the United States than in many other countries, which explains the relatively weak effect of SIZE.

The results for the U.S. subsample can be compared to other studies that focus on the United States. One such study is that of Flannery and Hankins (2013). Except for MB, the effects of the controls that are common to both studies have the same sign. There is, however, a difference in magnitude, and this is true even if we look at FE, which is included in both studies. Dang et al. (2015) and Zhou, Faff, and Alpert (2014) also focus on the U.S. and report FE results. Again, while similar in sign, the estimates differ in magnitude. This is true not only when compared to our study but also when we compare across all four studies. One possibility here is that since the estimator and the definition of the variables are the same, the differences in the results are due to differences in the time span of the data.[16] Either way, the estimated SOAs, which are of course the main objects of interest here, are very similar across studies, which is reassuring. Similar to the full sample of firms, it matters whether we allow for interactive effects or just fixed effects.

Let us now consider the results for the subsamples of developed and developing countries. The most striking difference is that the estimated effects are generally larger in absolute value for the developing countries than for the developed countries. The fact that the estimated effects of SIZE, FA, and EBIT are relatively larger for developing countries means that larger, more tangible and more profitable firms use relatively less debt. This finding is consistent with the relatively large agency and informational asymmetry problems in developing countries, and the relatively undeveloped nature of their bond markets (see Booth et al., 2001). Financial market imperfections of this type are central in the pecking order theory. Transaction costs and asymmetric information link the firm's ability to undertake new investments to its internally generated funds. If the firm

15 The NE and CA results, which have been omitted to save space, are available upon request.

16 Dang, Kim, and Shin (2015), and Zhou, Faff, and Alpert (2014) consider roughly the same time span. But while the first study considers book leverage, the second considers market leverage, just as we do. Hence, again the results are not directly comparable.

must rely on external funds, then it prefers debt to equity due to the lesser impact of information asymmetries. The results for the subsamples of developed and developing countries are therefore consistent with not only the trade-off theory, but also the pecking order theory.

4 Conclusion

The present article is motivated by the presence of unobserved heterogeneity in the form of interactive effects and their likely detrimental effect when estimating the SOA of leverage. The main problem we face is that when we try to account for the interactive effects we simultaneously incur a bias that cannot be ignored and that must also be accounted for. An analytical expression for this bias is provided and used as a basis for a new bias-corrected OLS estimator that is valid even in the presence of interactive effects. Our empirical results suggest that the usual fixed effects specification is not supported by the data, and that there is a need for more general interactive effects. We also find that proper accounting for interactive effects leads to marked increase (decrease) in the estimated SOAs (half-lives) when compared to estimates based on fixed effects. Our interpretation of these results is that the fixed effects assumption is violated and that this renders the resulting fixed effects-based OLS estimator misleading. The bias-corrected SOA estimates that account for interactive effects are around 0.35, which corresponds to a half-live of around 1.5 years. The implication is that firms are in fact targeting leverage, as predicted by the trade-off theory.

Supplemental Data

Supplemental data is available at *Journal of Financial Econometrics* online.

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