Reconciling Estimates of the Speed of Adjustment of Leverage Ratios

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Abstract

A number of prominent papers in the literature have estimated the speed of adjustment (SOA) of firms' leverage ratios with estimators not designed for applications in which the dependent variable is a ratio. This made them detect mean reversion, which they mistakenly considered as readjustment. We propose a non-parametric way to model leverage ratios under the NULL, and a method of reconciling incongruous estimates. Using the earlier estimators, the best joint estimate for the true SOA is zero or negative. There is evidence for some heterogeneity in SOAs across firms.

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To A Reader

An earlier draft of our paper, called "How Quickly Do Firms Readjust Capital Structure?" was more comprehensive. In particular, it explored in great detail what is now only mentioned in Section 5. It tried a whole number of other processes, which in the end were no better than our NP process in this current draft. It covered the survival bias in Leary-Roberts in great detail. It derived the bias of the OLS and FE estimators under the SDPP process. It explained the advantages of the W estimator, and the interpretation of shocks in the capital structure context.

A reader who is interested in these details can look at the old draft on the SSRN website. If some omitted aspects (from the earlier draft or from elsewhere) should be explained better in the current draft, please let us know.

The earlier paper also made remarks that were too strident about the tradeoff theory. This was based on evidence that is no longer in this paper (specifically, about the second moment of capital structure changes). This has been removed in favor of a discussion that a low SOA is to be expected under these models.

This paper is now sharp in its focus on reconciliating existing estimators only. In 12 point font, with normal spacing, the main text of the paper is now 25 pages. We want to retain this sharp focus in any revision. We believe this makes our paper better.

The speed of readjustment (SOA) of leverage ratios to perturbations is an important input to models of capital structure. In the context of tradeoff models with frictions (Fischer, Heinkel, and Zechner (1989)), the SOA can reflect the tradeoff between capital structure adjustment costs on the one hand, and the current deviation from the best leverage ratio on the other hand. Huang and Ritter (2009) even go so far as to opine that determining the SOA is "perhaps the most important issue in capital structure today."

Despite the fact that it has been estimated in many papers, Frank and Goyal (2008) observe in their survey article that "the speed at which [corporate leverage is mean-reverting] is not a settled issue." On a scale where perfect non-readjustment is 0 and perfect instant readjustment is 1, the most prominent published estimates in the literature range from 34% (Flannery and Rangan (2006, Table 7)), to 25% (Lemmon, Roberts, and Zender (2008, Table 6)), to 23% (Huang and Ritter (2009, Table 8), to 7-18% (Fama and French (2002, Table 4)), to practically zero (Welch (2004, Table 3)). The economic interpretation of these estimates contrasts starkly. An SOA of 34% suggests a *half*-life for the influence of a shock of about $\log(0.5)/\log(0.66) \approx 1.7$ years, i.e., reasonably active managerial intervention. An SOA of 23% suggests a *half*-life for the influence of a shock of about 2.7 years, active but not quick managerial intervention. An SOA of 10% suggests a half-life of 6.6 years, a "glacial readjustment" (Fama and French (2002)). An SOA of 5% suggests a half-life of 14 years, in line with the "practically no-readjustment" views of Myers (1984) and Welch (2004). 14 years is about as long as the median lifetime of firms on Compustat.

The literature is now settled with these starkly different estimates. We want to understand why the existing methods in this literature have come to such different conclusions. Is it different data sets, or different leverage definitions, or different techniques, or something else altogether? Consequently, our paper does not propose a new estimator or a different measure of leverage. Adding yet more methods with yet more estimates would only confuse the matter even further. Instead, our paper's goal is to reconcile the existing estimates.

Our paper begins with some simple findings: First, we show that the aforementioned large discrepancies in empirical estimates remain even when we use the exact same data definitions and sample. Thus, these differences across papers are not the culprits.

Second, the data contains thousands of firms and many decades (a total of over 100,000 firm-years), and the year-to-year variation in leverage ratios is modest. We can show (in retrospect not surprisingly) that good estimators are able to produce almost perfectly accurate estimates in such an asymptotically large data set *even when the researcher ignores all firm-specific target information*. Thus, differences in control variables that identify firm-specific leverage targets are not the culprit, either.

Third, if leverage ratios followed the econometric standard dynamic panel process (henceforth **SDPP**), i.e., $\tilde{x}_{i,t} = \rho \cdot x_{i,t-1} + \alpha_i + \tilde{\epsilon}_{i,t}$ with iid normally distributed shocks $\tilde{\epsilon}_{i,t}$, the Lemmon-Roberts-Zender and Huang-Ritter papers should have provided roughly the same true estimate. Yet, their estimates are dozens of standard errors away from one another. Ironically, the main challenge in this literature could be viewed as not too much, but too little estimation uncertainty.

Our paper shows that the SDPP is simply not suitable within the leverage ratio context. The standard dynamic econometric panel estimators were never designed for it—they were designed for variables following an SDPP and only for cases in which the SOA is positive. Because leverage ratios do not follow the SDPP (and because we have theories that suggest negative SOAs), the estimators can become biased. For example, the dependent variable is by definition limited (feasible only between 0% and 100%). The estimators measure readjustment as mean reversion, so these papers report a speed of adjustment that is one minus an estimate of the rho. The estimators attribute the fact that leverage ratios remain inside the feasible domain as mean reversion. Therefore, their SOA estimates are too high. Moreover, when the true SOA is low, leverage ratios reach their domain limits more frequently. Thus, low SOA estimates are relatively more biased. In fact, we shall show that the mapping from the true SOA to estimated SOAs is non-monotonic for most estimators—estimated rhos are lower (and typically below 1) when the true rho is higher if this true rho is above 1.

To assess the bias quantitatively, we must offer a better underlying process to describe how leverage ratios can evolve. Our paper draws a sharp distinction between estimators (where we do not want to innovate) and feasible underlying processes (where we must innovate). For example, we will not (really) propose a new estimator of heterogeneous adjustment, but we will examine the performance of existing estimators if the underlying firms have heterogeneous SOAs.

Our paper makes three contributions:

1. We suggest a non-parametric process to model underlying true leverage ratios. In this process, debt changes and equity changes are joint processes. Under the NULL hypothesis that they are unrelated to the current capital structure or any other characteristics of the firm, both changes are sampled from another randomly drawn firm-year. The next leverage ratio is computed from the resulting new debt and equity levels and the firm's own stock returns. This preserves all the essential characteristics of a ratio and does not consider stock return caused changes to be an actively managed managerial capital structure mechanism. We submit that our process could be useful also in other investigations of capital structure (as in Chang and Dasgupta (2009)): it should be a minimum requirement that an ALTERNATIVE model of leverage should explain the *observed* evolution of the

leverage ratio process of firms better than those obtained from this *random* NULL leverage ratio process.

2. We suggest a methodology to reconcile earlier findings. Under the assumption of homogeneity in rho, the reconciled SOA is the single number that would best explain empirical estimates from different estimators under a given assumed process. The reconciliation can allow for heterogeneity. For example, we can assume that SOAs are normally distributed across firms, and we can then infer the best mean and variance distribution for the true SOA to fit multiple empirical estimates.

   Reconciliation under different process assumptions also makes it possible to compare how well different processes can fit the existing estimates. Our non-parametric process performs quite well in explaining the existing estimates, especially compared with the alternative processes we considered.

3. After correct bias adjustments, we find that the SOA estimate that best reconciles the empirical methods and estimates in the literature is about zero or negative. (A negative SOA is predicted by Baker and Wurgler (2002).) However, as noted, the average SOA need not be applicable to all firms. Depending on which estimators are reconciled, we find that heterogeneity in SOAs may be modest (a 4% cross-sectional standard deviation) or severe (10%).

A low speed of adjustment does not reject tradeoff theories. On the contrary, when augmented with frictions, this theory can predict no activity, which in turn can imply a zero speed of adjustments. Thus, the observed SOA is not a puzzle, but "only" an important moment in these models, pinning down the magnitude of frictions relative to the cost-benefit of adjusting. (For example, the parameters in Strebulaev (2007) match an SOA of just about zero.) However, Welch (2004) argued that tradeoff models with frictions (alone) cannot be responsible for the low speed of adjustment, based on the variance of leverage ratio changes. If true, this would change this low an SOA from an explained moment into a puzzle.

In either case, even if Huang and Ritter are too optimistic in their assessment of its importance, the SOA is still a readily observable and interpretable empirical fact about corporate capital structure behavior, whose magnitude is of interest not only to many academics, but also to many practitioners and students. It is important to learn which of the prevailing estimates is right.

Our paper now proceeds as follow: We describe the data and the estimators in Section 1. We successfully replicate the findings of earlier studies in our own data set. In Section 2, we explain the problems arising from the SDPP and what the reader can infer from the empirical estimates without adopting any other leverage ratio process. Section 3 proposes our non-parametric NULL and the process that models the underlying evolution of leverage ratios. Section 4 summarizes our simulation protocol, fits the estimators on our process, and shows how one would best reconcile the existing empirical estimates. Section 5 describes other specifications we considered.

1 Empirical Estimates and Replication of Findings

1.1 Data and Variables

[Table 1 here]

Table 1 explains our data and the construction of the leverage measures used in our paper. Our main data source is Compustat from 1963–2007, supplemented with CRSP stock returns without dividends. Depending on data requirements, the samples used in later analyses have up to 136,450 firm-year observations. The panel is irregular. The average number per firms in a given fiscal year is 3,299, with a range from 453 to 5,804. The average number of years per firm in the sample is 19, although the median is only 16.

We use L as our generic notation for leverage. Our (non-exclusive) emphasis will be on the market-value based financial debt-to-capital ratio, DC. It is the sum of long-term and current debt divided by the sum of long-term and current debt and the market value of equity. Table 1 shows that DC has a pooled mean of 27%, a pooled standard deviation of 25%, and an average cross-sectional standard deviation of 23%. In some papers, leverage ratios were defined differently. The most common correct[1] alternative to DC is LA, the ratio of total liabilities over total assets.[2] Table 1 shows that the LA ratio has a mean of 40%, which suggests more indebtedness than the DC ratio. Some papers calculate leverage ratios using the book value of equity. We denote these with lower-case letters. Table 1 shows that the means of book-value based ratios are similar to their market-value based equivalents, but their heterogeneity and variability is a little lower.

[1] The common "leverage" ratio of financial debt divided by total liabilities is not correct, because it considers non-financial liabilities the same as equity. Thus, it considers a company *less* levered when it has *more* non-financial liabilities. Chang and Dasgupta (2009) examine D/A ratios, although their conclusions would likely also hold if they used a correct leverage ratio (page 10).

[2] Huang and Ritter (2009) do not use our exact LA definition. They use as measure of liabilities "Total Liabilities + Preferred – Deferred Taxes – Convertibles" and divide by "Total Liabilities + Preferred – Deferred Taxes + MVE." The results are similar when we use their definition instead of our simpler LA definition.

The average leverage ratio for surviving firms in the sample drifted up at a rate of around 0.6% to 1.2% per firm-year, with a cross-sectional standard deviation of around 11% to 12% for the market-based ratios and 9% to 10% for the book-based ratios.

The IDC (implied debt-to-capital) ratio is a measure of what would happen to the leverage ratio if the firm did not change the quantity of its debt or equity instruments, the value of debt were to remain constant, and only the stock's rate of return (without dividends, though this matters little) changed the value of the equity. The difference between $\mathsf{IDC}_{t-1,t}$ and DC_{t-1} can be loosely interpreted as the leverage change that is *not* due to stock returns, but due to the intervention of management (net issuing of debt and equity). Comparing the 12.6% variability in leverage ratio changes with the 8.7% variability sans stock-return induced changes suggests that about 30% of the year-to-year change in capital structure is caused by stock returns. The remaining 70% is (almost entirely) due to debt and equity net issuing activity (including dividends). Stock returns are a first order determinant of capital structure changes, but firms are by no means passive or inert.

1.2 The SDPP Process and Leverage Ratio Targets

[Table 2 here]

Table 2 summarizes the methods that have been used in the relevant literature and that we are investigating in our current paper. The models are formulated in terms of one minus the speed of adjustment (SOA). This coefficient is named "rho." It is the parameter of primary interest to us. The typical model for the evolution of leverage in a dynamic panel is

$$\tilde{\mathsf{L}}_{i,t} - \mathsf{L}_{i,t-1} \quad = \quad (1-\rho)\cdot\left[\mathsf{T}_{i,t} - \mathsf{L}_{i,t-1}\right] + \tilde{\epsilon}_{i,t} \quad ,$$

where $\mathsf{T}_{i,t+1}$ is a firm-specific target leverage ratio, possibly itself a function of time and covariates. When rho is zero, the panel loses its dynamic aspect and becomes static. When the panel is dynamic, the true error and the next firm-year's independent leverage observation are correlated.

For the most part, we will assume that each firm has its own *constant* target. The firm-specific targets themselves are not of interest to us. We only need them as controls in our quest to estimate rho. We will consider two cases. In the first case, we assume that the researcher knows them perfectly,

$$\tilde{\mathsf{L}}_{i,t} \quad = \quad \rho\cdot\mathsf{L}_{i,t-1} + (1-\rho)\cdot\mathsf{T}_i + \tilde{\epsilon}_{i,t} \quad .$$

In the second case, we assume that the researcher does not know anything about the targets, but the estimators can control for them, e.g., by allowing for firm-specific fixed effects or by differencing.[3] When each target is a firm-specific unknown constant (t_i'), then we can write this model in the form of the econometric *standard dynamic panel process* (SDPP),

$$\tilde{\mathsf{L}}_{i,t} \;=\; (1-\rho)\cdot t_i' + \rho\cdot \mathsf{L}_{i,t-1} + \tilde{\epsilon}_{i,t} \;=\; \rho\cdot \mathsf{L}_{i,t-1} + t_i + \tilde{\epsilon}_{i,t} \quad , \tag{SDPP}$$

where t_i are firm-specific intercepts. (The $1-\rho$ scalar on unspecified intercepts does not change rho estimates.) In real life, the target is never fully known or fully unknown. Instead, the researcher can proxy for the target with fixed effects and observable covariates, but this probably succeeds in capturing the true target for each firm only with limited accuracy. Fortunately, because we will show that target knowledge turns out to be unimportant in a data set as large as our's, we do not need to agonize over measurement error in the target.

1.3 Existing Estimators

We now discuss the five estimators that have been proposed to recover the rho.

OLS Estimator: The simplest panel estimator is a pooled OLS regression, in which leverage is regressed on past leverage and usually additional firm-specific variables. These covariates are included to capture the unknown firm-specific hetrogeneous targets. It is easy to verify that with thousands of cross-sectional observations, it also makes no difference whether the model is estimated in differences or in levels. However, OLS has two problems. The first problem is not too important in our context. The assumed orthogonality conditions between residuals and independent values does not hold. When an epsilon is high, the next year's independent variable will then also be high. In small samples, with few firms and time-periods, this causes the well-known "Hurwicz bias" in favor of finding mean reversion (estimated rhos less than 1) even when there is none in the underlying process (true rho of 1). Fortunately, our panel is primarily cross-sectional, with far more firms than years, and there is only one parameter to estimate. The Hurwicz bias vanishes asymptotically, and this violation of the orthogonality condition has almost no influence on the estimates in our context. The second problem is much more serious. The omission of firm-specific targets in the estimation creates a bias in favor of not finding mean-reversion if it is indeed present. We will show that this "omitted variable" bias is severe when firms have firm-specific targets unknown to the researcher.

Fixed-Effects (FE) Estimator: Flannery and Rangan (2006) in effect argue that researcher-supplied target covariates may not capture all the heterogeneity across

[3]We also considered random and/or time-varying targets. Our inference remained reasonably robust. See Section 5.

firms' targets. Thus, they suggest adding fixed effects into the estimator. This takes care of the omitted variables problem. However, as noted also in Huang and Ritter (2009), the FE estimator has desirable properties only in *static* panel processes, not *dynamic* ones. The Hurwicz bias reappears because the intercepts consume a large number of degrees of freedom. Thus, the fixed-effects estimator suffers seriously from the correlation between the error and the next independent variable. Put differently, the target fixed effect assumes the mean of the error realizations. Nickell (1981) derives the bias of this estimator.

GMM (BB) Estimators: To allow for fixed effects without suffering the Hurwicz bias, Lemmon, Roberts, and Zender (2008) suggest using the Blundell-Bond (BB) estimator (the use of which has since been adopted by other papers). It is a modified version of the well-known Arellano-Bond (AB) GMM procedure. An econometric derivation of this estimator is beyond our scope, but we can provide an intuitive explanation of some of their moment conditions to help readers unfamiliar with this technique (and to help them understand its failure in our context).

For exposition sake, assume that firm-specific targets are zero $t_i = 0$. The moment conditions are chosen to be cross-correlations, so that these means are irrelevant.[4] The following explanation is therefore valid even when firms have their own targets. Any specific rho implies a specific residual for each firm and year. Thus, the error e can be thought of as a function of ρ, i.e.,

$$e_{i,t}(\rho) \quad \equiv \quad \mathsf{L}_{i,t} - \rho \cdot \mathsf{L}_{i,t-1} \quad .$$

Arellano-Bond find their moment conditions by pointing out that these residuals should be uncorrelated with firms' own two-years-or-more-lagged leverage, $(\mathsf{L}_{i,t-2}, \mathsf{L}_{i,t-3}, ...)$.[5] For example, the expected sum over all firms of, say, $\sum_i e_{i,3}(\rho) \cdot \mathsf{L}_{i,1}$ should be zero. To improve on the intuition, consider an example with five years of level leverage data. Differencing eliminates one year. This leaves four years of residuals, of which two ($e_{i,3}$ and $e_{i,4}$) have at least one two-year lagged L. The three conditions that the AB estimator

[4]The fixed effects estimator imposes orthogonality conditions between these errors and the mean of the independent variables that are not likely to be satisfied. The GMM estimator ignores those (mean) conditions that are not likely to be orthogonal, and focuses only on those correlation conditions that are likely to remain valid (orthogonal). By doing so, the GMM estimator loses a large number of (linear) restrictions. With over 100,000 firm-years, this is not a big problem because the remaining conditions are sufficiently powerful.

[5]This also indicates why the BB estimator fails in our context. Intuitively, if the two-period lagged L is very low, e.g., zero, it is common that the one-period lagged L is also zero. For firm's with such zero leverage, the shock ϵ cannot be negative. It is this feature that is ultimately the problem for this estimator in our context. It was built upon the lack of correlation between ϵ and lagged L, which cannot hold here.

wants to match are $\sum_i(e_{i,3}\cdot \mathsf{L}_{i,1})=0$, $\sum_i(e_{i,4}\cdot \mathsf{L}_{i,2})=0$, and $\sum_i(e_{i,4}\cdot \mathsf{L}_{i,1})=0$. Thus, the GMM procedure seeks to find the rho that minimizes a *squared* penalty function of

$$w_{\mathrm{M1}}\left[\sum_i(\mathsf{L}_{i,4}-\rho\cdot\mathsf{L}_{i,3})\cdot\mathsf{L}_{i,1}\right] \;+\; w_{\mathrm{M2}}\left[\sum_i(\mathsf{L}_{i,4}-\rho\cdot\mathsf{L}_{i,3})\cdot\mathsf{L}_{i,2}\right] \;+\; w_{\mathrm{M3}}\left[\sum_i(\mathsf{L}_{i,3}-\rho\cdot\mathsf{L}_{i,2})\cdot\mathsf{L}_{i,1}\right] ,$$

where w_{M} are weights whose optimal values can be asymptotically derived. The intuition for their selection is that we want to weight moment conditions more when they have more to say about ρ—meaning that small changes in ρ would cause large changes in the particular moment sum. (We would not expect a residual to have much correlation with leverage from 20 years earlier regardless of what the actual rho is.)

Rearranging the penalty function yields

$$\begin{array}{rrcl} & (y & - & \rho\cdot x)^2 \\ \text{where} & y & = & w_{\mathrm{M1}}\cdot\sum_i \mathsf{L}_{i,1}\cdot\mathsf{L}_{i,3}+w_{\mathrm{M2}}\cdot\sum_i \mathsf{L}_{i,2}\cdot\mathsf{L}_{i,4}+w_{\mathrm{M3}}\cdot\sum_i \mathsf{L}_{i,1}\cdot\mathsf{L}_{i,4} \\ \text{and} & x & = & w_{\mathrm{M1}}\cdot\sum_i \mathsf{L}_{i,1}\cdot\mathsf{L}_{i,2}+w_{\mathrm{M2}}\cdot\sum_i \mathsf{L}_{i,2}\cdot\mathsf{L}_{i,3}+w_{\mathrm{M3}}\cdot\sum_i \mathsf{L}_{i,1}\cdot\mathsf{L}_{i,3} \quad . \end{array}$$

Consequently, one can obtain the GMM rho via a least-squares estimator by running a regression $y=\rho\cdot x+$ noise, in which the sums of these predetermined leverage products are regressed on one another. Intuitively, the GMM rho estimate is higher when the AR(t) cross-products are more similar to the AR(t+1) cross-products. Generalizing to an arbitrary number of years, Arellano-Bond provide $(T-1)\cdot(T-2)/2$ such moment conditions. It is not uncommon (as do Lemmon, Roberts, and Zender (2008)) only to use moment conditions in which e is close (in time) to L. After all, the optimal weight on moment conditions with more than a few years distance between residual and leverage is small anyway.

The Blundell-Bond "SYSTEM" statistic innovates over Arellano-Bond by adding another $T-2$ moment conditions: each residual should also not be correlated with lagged *changes* in leverage. For example, $e_{i,3}(\rho)$ should also not be correlated with $\mathsf{L}_{i,t-2}-\mathsf{L}_{i,t-1}$. Blundell and Bond (1998) prove that their GMM estimator is asymptotically efficient as $N\to\infty$ under the SDPP with independent normally distributed errors, *assuming* that rho is below 1.

Long Difference (LD) Estimator: Huang and Ritter (2009) use the Hahn-Hausman-Kuersteiner (HHK) "long difference instrumental variables" estimator. The LD approach suggests running an OLS regression on the longest differences that are available. For example, with 20 years of data per firm, it would estimate

$$(\mathsf{L}_{i,20}-\mathsf{L}_{i,2}) \;=\; \rho\cdot(\mathsf{L}_{i,19}-\mathsf{L}_{i,1}) \;+\; e_i^{\text{Estimation Stage}} \quad .$$

However, $(\mathsf{L}_{19} - \mathsf{L}_1)$ is still measured with error. Even though the average true epsilon error becomes smaller as the difference increases, higher error realizations still cause higher L_{19} values. This error-in-variables problem means that the estimate of rho would be biased towards 0 if simple OLS were used. The lagged difference in leverage is thus still not orthogonal to the (differenced) error. To reduce this problem, $(\mathsf{L}_{19} - \mathsf{L}_1)$ is replaced with its fitted value from an IV regression, which (given a rho estimate) is essentially

$$(\mathsf{L}_{i,19} - \mathsf{L}_{i,1}) = g1 \cdot \mathsf{L}_{i,1} + g2 \cdot (\mathsf{L}_{i,3} - \rho \cdot \mathsf{L}_{i,2}) + g3 \cdot (\mathsf{L}_{i,4} - \rho \cdot \mathsf{L}_{i,3}) + \ldots + e_i^{\text{IV Stage}} \quad .$$

This is then iterated a few times. Although HHK are quite concerned with choosing a good rho starting value, ultimately this matters little as long as the two equations are iterated more than a few times. Given an arbitrary starting value for rho between 0 and 1, iterating at least a half a dozen times yields the unique rho estimate. Hahn, Hausman, and Kuersteiner (2007) prove that their LD estimator is asymptotically efficient as $N \to \infty$ under the SDPP with independent normally distributed errors, *assuming* that rho is below 1.

Intuitively, as with the GMM estimators, the quality of the LD estimates in small samples depends on the strength of its moment restrictions (instruments) that are not ignored. Because the estimator requires a long time series, and because the data set is irregular, Huang and Ritter (2009) consider different length estimators (4, 8, 18 and 28 years). These trade off accuracy against data selection in different ways. Our replications focus on their overlapping 8-year estimates, but we also confirmed that the results are robust to different choices. (For example, in Table 3, the estimates would typically have varied by about 1%, with the largest discrepancy being 4%.)

Welch (2004) (W) "Implied Target" Estimator: This method is quite different from the tests above. It is *not* based on the statistical SDPP model. Instead, it is based on a specific economic identification. As already noted on Page 5, the implied leverage ratio, IDC, that comes about in the absence of corporate capital structure activity is $\mathsf{IDC}_{t,t+1} \equiv D_t/[D_t + (1 + r_{t,t+1}) \cdot E_t]$. The W method is a competitive OLS regression of

$$\widetilde{\mathsf{DC}}_{i,t+1} \quad = \quad a + b \cdot \mathsf{DC}_{i,t} + \rho \cdot \mathsf{IDC}_{i,t,t+1} + \tilde{e}_{i,t} \quad .$$

The lagged DC is intended to capture the most recent firm's own target (somewhat similar to a fixed effect), while the constant is intended to capture a common target. The coefficient ρ measures the degree to which stock-return-caused noise is or is not undone.

In our current paper, we implement the estimation with a simple pooled regression (clustered standard errors) rather than with the Fama-Macbeth-style method used in

Welch (2004).[6] The rho estimates are almost identical. The W estimator nests the simple pooled OLS estimator. In Table 3 below, we also report the rho estimate from a simple extension that allows for fixed effects. This can nest the FE estimator.

Not being an estimator derived under the general SDPP assumption, the W estimator cannot be used in other contexts. It is unsuitable even if leverage is defined by book value. It also has the disadvantage that its properties are not known under *any* underlying process. Moreover, it is not clear whether the past leverage ($\mathsf{DC}_{i,t}$) term in the regression can be successful in controlling for firm-specific targets, as interpreted by Welch (2004). Flannery and Rangan (2006) in particular express reservations about how this estimator performs if each firm has its own constant target. The main advantage of W is that it uses the fact that a large part of the true shock (the stock return) is observable under the NULL. (This would be analogous to knowing the true epsilon, not the estimated residual, in the OLS generating process.) Moreover, W has very precise predictions under the NULL.

1.4 Replication of Findings

[Table 3 here]

Table 3 shows the results of the estimators applied to the empirical data. The coefficient that is to be interpreted as the influence of past leverage is in bold. For the OLS, FE, BB, and LD methods, this is simply the coefficient on lagged leverage (DC). For the W estimators, with its interest in the role of shocks from stock returns, this is the coefficient on IDC. Different sections in Table 3 consider different definitions of leverage. Table 4 repeats Table 3 in the 40,708 firm-years that can be used in all estimations. This sample is mostly limited by the LD estimator's need for long time series. In such a "constant sample," differences in the firm-years that are used by different estimators cannot be responsible for differences in estimates.[7]

[Table 4 here]

The first observation from both tables is that it does not matter greatly which definition of leverage is used. The rho estimates are remarkably similar, with differences caused by leverage definitions of less than 0.02. The often heated arguments about whether book-value or market-value based leverage ratios are more appropriate seem unimportant *in this context.*

The second observation is that the estimation methods themselves are responsible for the ordering of inference. The W estimate for rho is always highest, followed by

[6] The Fama-Macbeth method in Welch (2004) was unjustified, but served to show that standard errors remain small even when one sacrifices power. The standard errors in Welch (2004) were wider (around 1.4%) than those reported in our current paper (around 0.4%).

[7] We follow the literature in allowing both samples to suffer from survivorship bias. We exclude firm-years in which the firm experiences a negative return shock and then disappears before the fiscal year end (which is more likely if the leverage ratio is high). Imputing a high leverage ratio to such observations increases the estimate of rho further.

OLS, BB, LD, and FE.[8] As in the literature, the range of estimates is uncomfortably large, with FE seemingly suggesting a rho of less than 0.7, LD suggesting 0.78, BB suggesting 0.85, OLS suggesting 0.9, and W suggesting 1.

Comparing Table 3 and 4 shows how the DC leverage ratio estimates change in a constant sample: the OLS estimate decreases by 0.1%, the FE estimate decreases by 1.6%, the BB estimate increases by 1.0%, the LD estimate decreases by 0.04%, and the W estimate increases by 1.2%. Thus, all in all, Table 4 estimates are similar to those in Table 3. Differences in the sample cannot explain the discrepancies in estimates.

Our estimates are also similar to those reported by our predecessors. Lemmon, Roberts, and Zender (2008, Table 6) explain book-leverage and report a coefficient of 87% in an OLS estimation (a), 64% in an FE estimation (d), and 78% in their own BB estimation. This is just 4% lower than the rho estimate for the book-value based *dc* that we obtain in our sample. Huang and Ritter (2009, Table 8) even include a similar summary table, although it is based on reports by earlier papers and not based on an independent replication. They report rhos of between 93% (dividend paying firms) and 85% (non-dividend paying firms) for the OLS method in Fama and French (2002); 91.7% for the OLS method in Kayhan and Titman (2007); 64.5% for the fixed-effects method in Flannery and Rangan (2006); 67.8% for the fixed-effects method in Antoniou, Guney, and Paudyal (2008); and 76.8% for their own long-difference method.

Half-Lives: The common way to gain intuition into the meaning of these SOA estimates is to translate them into "half-lives." (1-ρ) is the expected percentage by which the gap between the past leverage and the target closes in one period. Half-life is the time that it takes a firm to adjust back one-half the distance to its target leverage after a one unit shock to the error term ($\epsilon_{i,t}$). For an AR(1) process, half-life is $\log(0.5)/\log(\rho)$. The discrepancies in these methods' rho estimates cause stark contrasts in interpretation. The fixed-effect estimate of 0.66 suggests a half-life of less than 2 years, while the W estimate of 1 suggests a half-life of infinity. Half-life is particularly sensitive when rho is close to 1. Rhos of 0.8 or lower suggest half-lives of around 3 years, in line with the limited trade-off based view of capital structure in Flannery and Rangan (2006), Huang and Ritter (2009), and Lemmon, Roberts, and Zender (2008). (If projects appear with much higher frequency, and if they need to be financed quickly, even this adjustment seems slow, however.) Rhos above 0.9 suggest half-lives of 6 years, more in line with the "glacial readjustment" view Fama and French (2002). Rhos above 0.95 suggest half-lives in excess of 13.5 years, in line with the "practically no-readjustment" view of Myers (1984) and Welch (2004). 13.5 years is only slightly lower than the 16-year median life of firms in our sample.

[8] Estimating the OLS or FE in changes instead of in levels yields practically identical results.

2 The Problem With The Infeasible Leverage Process

The estimated standard errors in Tables 3 and 4 are very low. This should not be surprising. The observation count is thousands of firms multiplied by dozens of years, noise is modest (leverage ratios can only change so much), and there is really only one parameter of interest. Put simply, if the SDPP assumption is correct, the BB and LD estimators should have standard errors that are close to zero.

Yet despite the fact that these estimators should uncover the true rho with perfect accuracy, their actual estimates remain far from one another. One way to view this problem is that different estimators can be viewed as "SDPP diagnostics" for one another, in the same sense that a Durbin-Watson statistic is a diagnostic on an OLS regression. We would expect its value to be around 2; and if the value is below 1, there is good cause to consider the standard OLS assumptions to be violated. In our case, we should simply not expect to observe distances between the rho estimates that are as large as those observed if the SDPP assumptions are correct. Even the smallest difference—from the 0.847 and 0.772 for the unbiased LD and (almost) unbiased BB estimators in the full sample—is still over 10 standard deviations. We can conclude from this that the underlying process assumptions probably do not hold.

2.1 Reconciliation under the SDPP Process

One can extend this insight to reconcile multiple estimates under the SDPP process. Consider the following thought experiment. Assume that the true rho has a value of 0.791 for each and every firm. Simulate an SDPP with a 12.5%[9] standard error, beginning for each firm with the true leverage ratio observed in the data. The average OLS estimate of rho in this simulated data set will then be 0.925 (with a true [sampling] sd of 0.001), the FE estimate will be 0.646 (sd of 0.003), the LD estimate will be 0.791 (sd 0.004), and the BB estimate will 0.814[10] (sd of 0.004). Now compare these expected values to the empirical estimates in our data. They were 0.895, 0.681, 0.772, 0.847, and 1.003, respectively. Thus, if the true rho is 0.791, its empirical OLS estimate of 0.895 was $(0.895 - 0.925)/0.0011 \approx 26.3$ standard errors away from what we would have expected. Analogously, the FE estimate was –12.7 standard errors off, the LD estimate was 4.6 standard errors off, and the BB estimate was –8.7 standard errors off. Each of these values is of course a simple t-statistic. We can now define our overall measure of (lack

[9] 12.5% is in line with the estimated residual variation. The inference does not change if we choose values of 8% or 15%. Table 5 summarizes other aspects of our simulation protocol. The reconciliation in this section excludes the W estimator below. The inference does not change if we include it.

[10] We have confirmed that the BB estimator has this 2% bias because of the starting leverage ratios of firms in our sample. Thus, it reflects the unusual capital structure of new firms (IPOs) entering the sample. Many time-series estimators are sensitive to initial conditions.

of) fit as the equal-weighted root mean squared t-statistic.[11] The reconciliated rho is the rho that minimizes the function that maps any assumed true rho to this *penalty*:

$$\min_{\rho_{\text{True}}} \text{Penalty}(\rho_{\text{True}}) \quad = \quad \sqrt{\frac{\sum_{\text{Est}} \; [t_{\text{Est}}(\rho_{\text{True}})]^2}{\text{Number of Est's}}}$$

$$\text{where } t_{\text{Est}}(\rho_{\text{True}}) \equiv \frac{\rho_{\text{sim}}(\rho_{\text{True}}) - \rho_{\text{empirical}}}{\text{Stderr}(\rho_{\text{sim}}(\rho_{\text{True}}))} \; ,$$

and $\rho_{\text{sim}}(\rho_{\text{True}})$ is the simulated mean estimate given a presumed rho ρ_{True}. Admittedly, although we promised not to derive new estimators for the speed of adjustment, one can view this minimum-penalty rho as its own estimator. It is unbiased and consistent, but it is not optimal. In a data set as large as our's, the first two qualities ensure that it should uncover the true rho with almost no error.

As it turns out, the rho estimate of 0.791 that we used for our example *is* the minimum penalty estimate. Thus, it represents the best reconciliation of the four estimators given the empirical estimates. The penalty is 15.4, which is obviously poor. It seems that under the SDPP process, the different estimates are difficult to reconcile. We can reject with high confidence the hypothesis that leverage ratios follow this process.

2.2 Problems Due To The Additive Normal Shocks in an SDPP

An obvious reason why the SDPP is flawed in our context is that the dependent variable is a ratio. It cannot possibly follow an SDPP. For example, assuming additive independent normal shocks would regularly induce impossible values in the leverage ratio context. Leverage ratios have to be between 0 and 1. Thus, when the initial leverage ratio is 0%, a shock to leverage can only be positive. This is not rare. Figure 1 shows that firm-years with low leverage are quite common. Although leverage ratios have a mean of 27%, their mode is zero. And, as Table 1 showed, the average year-to-year change in leverage **[Figure 1 here]**

[11] One could also weight the estimators more if they have lower estimated standard errors or steeper slopes of the inference function at the estimated values. Further, it is unlikely that the key problem would be correlations among unknown normally distributed errors (ϵ's) that we have not corrected for—again, there are so many observations that we believe that the rho point estimates are very accurate even under reasonably high cross-correlations. The failure is more likely in the functional process specification itself, and/or in the symmetry and zero assumption of the errors.

ratios, even though limited by the domain, is 12%, of which 8.7% is due to managerial intervention.[12]

Even dynamic panel estimators that are unbiased under the SDPP with normal errors may well attribute the inability of the leverage ratio to exceed its domain of [0,1] as mean-reversion. This predictably biases them towards finding positive SOAs. The domain limits seem *a priori* important: When we simulate SDPP processes without truncating leverage ratios, the number of firm-years with impossible leverage ratios is large. Figure 2 shows that even when the true rho is 0, normally distributed random shocks of 12.5% per year (the average change in the data) would still push around one-fifth of all firm-years into the infeasible region under the SDPP. When rho is 1, this figure rises to about one quarter. When rho is above 1, the funtion rises very quickly. There are also many leverage ratios that are just absurdly high. With a rho of 1.0, the 95 percentile range of observed leverage ratios reaches from –72% (unreasonable) to +112% (far from the 27% DC average, but still close to the feasible range). With a rho of 1.1, this range increases to –350% to +400%. Under the SDPP, the evolution of true bounded leverage ratios cannot deliver such large ranges. **[Figure 2 here]**

Truncation is an obvious, but not the only possible violation of the SDPP in a ratio context. For example, a shock to equity has a marginal influence of 0 if the firm is financed fully with equity, a marginal influence of 0.25 if the firm is financed equally with both debt and equity, and a marginal influence of 0.1875 if the firm is financed with 75% debt and 25% equity. Therefore, when firms of different initial leverage ratios experience same-sized shocks (changes to equity), their debt ratio changes will be of different magnitudes. Second, if a firm starts out with $25 in debt and $100 in equity, then a +$25 shock to equity reduces the leverage ratio to $25/$125=20%, while a –$25 shock to equity increases its leverage ratio to $25/$75=33%. The average ratio of 26.67% is above the initial ratio of 25%. This bias is positive when the debt ratio is below 50%, which is the majority of our observations. Both effects can contribute to indicated mean-reversion even when there is no managerial activity.

Some earlier papers had recognized the issue of truncation and mechanical mean reversion due to leverage being a ratio, but either handled it differently or corrected insufficiently. (And most papers are continuing to ignore it entirely.) For example Shyam-Sunder and Myers (1999) and Welch (2004) mention the presence of mechanical mean reversion. Chen and Zhao (2007) show how the non-linear nature of leverage ratios can relate issuing activity to an estimate of a deviation from the target. Rajan and Zingales (1995) employ a Tobit regression. Kayhan and Titman (2007) in one specification focus

[12]Note that one option is to abandon the definition of leverage that has been used in the literature. For example, the profession could explain leverage that counts cash as negative debt or the (far more volatile) interest coverage ratios. However, this would change the meaning of the inference. More important to our own paper, it would defeat its purpose. We want to reconcile the existing findings on the existing leverage measures, and not propose yet more methods to assess the SOA.

only on firm-years with leverage ratios above 10%. We have confirmed that these are not adequate solutions.

In sum, we conclude that assuming an unconstrained leverage process as a model for observed leverage ratios is not just conceptually wrong, but also difficult to defend as a reasonable empirical approximation that is only violated in rare firm-years.

3 A Feasible Process

In order to understand the quantitative properties of our estimators better, we must propose a reasonable alternative process for leverage ratios. Our best suggestion is a non-parametric process, which makes minimal assumptions under our NULL hypothesis (that firms do not change their leverage ratios with regard to their prior leverage ratios or any other own firm-specific characteristics).

3.1 The NULL

Without loss of generality, the following identity describes the evolution of corporate leverage ratios:

$$\begin{aligned}
\widetilde{\mathsf{DC}}_{i,t+1} &= \frac{D_{i,t+1}}{D_{i,t+1} + E_{i,t+1}} = \frac{D_{i,t} \cdot (1 + \tilde{\nu}_{i,t,t+1})}{D_{i,t} \cdot (1 + \tilde{\nu}_{i,t,t+1}) + (1 + \tilde{\eta}'_{i,t,t+1}) \cdot E_{i,t}} \\
&= \frac{\frac{D_{i,t}}{D_{i,t}+E_{i,t}}}{\frac{D_{i,t}}{D_{i,t}+E_{i,t}} + \frac{1+\tilde{\eta}'_{i,t,t+1}}{1+\tilde{\nu}_{i,t,t+1}} \cdot \frac{E_{i,t}}{D_{i,t}+E_{i,t}}} = \frac{\mathsf{DC}_{i,t}}{\mathsf{DC}_{i,t} + \frac{1+\tilde{\eta}'_{i,t,t+1}}{1+\tilde{\nu}_{i,t,t+1}} \cdot (1 - \mathsf{DC}_{i,t})},
\end{aligned}$$

where $\eta'_{i,t,t+1} \in [-1, \infty]$ is the fractional change in equity and $\nu_{i,t,t+1} \in [-1, \infty]$ is the fractional change in debt. We can further decompose $\tilde{\eta}'_{i,t,t+1}$ into a shock due to stock returns ($r_{i,t,t+1}$) and a change due to managerial equity issuing and repurchasing ($\tilde{\eta}_{i,t,t+1}$), i.e., $(1 + \tilde{\eta}') = (1 + r) \cdot (1 + \tilde{\eta})$. Each firm undergoes its own specific eta and nu changes in debt and equity. Presumably, under an active hypothesis, these changes should reflect the firm's own concerns—such as a leverage ratio that the firm deems to be too low or too high. In contrast, under our NULL hypothesis, leverage ratios could evolve as if this firm's characteristics had had no influence on its managerial debt and equity activities.

We have no theoretical guidance that tells us the joint distribution of changes to debt and equity under the no-readjustment NULL hypothesis, so we cannot identify the shocks parametrically. However, with this large a sample, a non-parametric alternative works well. We randomly draw some other firm-year, and adopt the sampled (percent) debt and equity changes (excluding stock returns) from this firm-year as our shocks. (By keeping stock returns separate from the evolution of equity, the alternative hypothesis has to explain a net equity response by the firm and not stock returns.) It is important for the meaning of this test that this match be chosen without regard to any variable related to the hypothesis (for us, the match's existing leverage ratio). We then combine these two randomly chosen shocks ($\tilde{\eta}^R, \tilde{\nu}^R$) with the firm's known own existing stock returns to compute its next "placebo" leverage ratio.[13] Note that under our NULL, firms that have a zero leverage ratio cannot change. Thus, if there are firms in the real sample with zero leverage that are issuing debt, it will favor rejection of this NULL. To remedy this would require a non-multiplicative perturbation of debt levels.

Because $\tilde{\nu}^R$ and $\tilde{\eta}^R$ were not from this specific firm and unrelated to any characteristic of the firm that is of interest, one can consider the resulting evolving leverage ratio to be a NULL hypothesis—a leverage ratio independent of the firm itself, including its own current leverage ratio or any other own firm-specific characteristic.

3.2 Embedding With An ALTERNATIVE

Our ALTERNATIVE is determined by the conjecture in this literature: that a firm has a leverage ratio target that it wants to return to, and a parameter rho that governs how slowly this occurs. This is a natural though not an innocuous assumption about the ALTERNATIVE—but it is the assumption made in the papers that we want to reconcile, so we have no choice. The SDPP process fits this structure. Our complete process is therefore

$$\widetilde{\mathsf{DC}}_{i,t+1} \quad = \quad \rho \cdot \widetilde{\mathsf{SDC}}_{i,t+1} \ + \ (1-\rho) \cdot \mathsf{T}_i \qquad \text{(Process NP)}$$

where

$$\widetilde{\mathsf{SDC}}_{i,t+1} \quad \equiv \quad \frac{\mathsf{DC}_{i,t} \cdot (1+\tilde{\nu}^R_{i,t,t+1})}{\mathsf{DC}_{i,t} \cdot (1+\tilde{\nu}^R_{i,t,t+1}) + (1+\tilde{\eta}^R_{i,t,t+1}) \cdot (1+r_{i,t,t+1}) \cdot (1-\mathsf{DC}_{i,t})} \; .$$

where $\tilde{\nu}^R$ and $\tilde{\eta}^R$ are the debt and equity net issuing activities from a randomly chosen firm-year. For rhos between 0 and 1, the evolution of leverage ratios is guaranteed

[13] For example, say a firm has debt of \$100 and equity of \$50 in year 1, for a DC ratio of $\$100/(\$100+\$50) = 2/3$; debt of \$120 and equity of \$40 in year 2, for a DC ratio of 75%; and a stock return of 50%. Therefore, its debt increased by 20% and its equity (absent stock returns) changed by $\$40/(\$50 \cdot 1.5) - 1 \approx -47\%$. Now draw a random firm-year. Say this "match" had debt of \$20 and equity of \$80 in year 5; and debt of \$15 and equity of \$90 in year 6, accompanied by a stock rate of return of 10%. The match's debt decreased by 25%, its equity (sans stock return) increased by $\$90/(\$80 \cdot 1.1) - 1 \approx 2.27\%$. Under the NULL, our firm would then be imputed as having debt of $(1-25\%) \cdot \$100 = \$75$ and equity of $(1+2.27\%) \cdot (1+50\%) \cdot \$50 \approx \$76.70$, for a DC ratio of $\$75/[\$75+\$76.70] \approx 49.4\%$ in year 2.

to remain in the feasible domain. For rhos above 1, it can happen that the weighted average exceeds the domain. In this case, we winsorize the *observed* leverage ratio.

3.3 Applications

The proposed behavior under the NULL should be compatible with a wide range of structural and non-structural models. Specificity arises primarily in how the ALTERNATIVE can compete with the NULL. This alternative—a specific model proposed by our predecessors—should be able to predict the evolution of firms' actual leverage ratio better than the random evolution under the NULL.

Naturally, a custom-designed test of a specific model can deliver more power to the alternative *if the model is correct.* Conversely, the closer the ALTERNATIVE hypothesis is to the NULL hypothesis, the less power the competitive test has to distinguish between them. For example, a tradeoff theory with high frictions shares much of the prediction of non-adjustment. A rho of 1 practically *is* the prediction of the tradeoff model with large frictions—testing non-adjustment vs. no-activity (the tradeoff-theory with frictions) via rho has no power.[14]

A similar analysis of estimation problems appears in Chang and Dasgupta (2009). Our own paper developed independently of their's and thus there are numerous differences between the two.[15] Note that our own NULL process could also be used in their context. For example, one could test whether observed funding deficits can explain actual leverage ratios better than the placebo leverage ratios obtained under our NULL of

[14] Iliev and Welch (2010) suggest a test to distinguish between non-adjustment and non-activity, based on the variance of leverage ratio changes. The tradeoff theory with frictions predicts non-activity, which in turn causes non-adjustment.

[15] Chang and Dasgupta (2009) focus on the role of financing deficits. Their NULL process is conditional and parametric. First, they compute or randomly draw a financing deficit, which is the change in assets net of the change in retained earnings. If it is positive, they draw randomly either an equity or a debt issue equal to this funding deficit. If it is negative, they draw randomly either an equity or a debt repurchase equal to this funding deficit. They impose a structure which is plausible but not necessary in our context. (The deficit may well be endogenous—firms can increase or decrease assets and retained earnings *because* they issue debt or equity.) We would argue that our non-parametric process is a simpler and perhaps more attractive NULL. They examine only the book value of leverage divided by assets and thus have/need little consideration of stock returns. Our papers do not share a single test specification. The closest overlap appears in their Section III.A., where they replicate a Flannery-Rangan fixed-effects regression. They do not explore the BB, LD, and W estimators that are a major focus of our study. They do not consider the location of the empirical estimate within the distribution of random leverage ratio evolutions. (They only show that random evolution can produce statistically significant results, too.) They do not reconcile multiple estimators. They do not offer a single best SOA estimate or distribution thereof, but show only that no-readjustment cannot be rejected. In their Section III.C, they explore studies that predict issuing behavior, which we do not. Their critique here seems to focus primarily on Alti (2006) and Kayhan and Titman (2007), possibly also Leary and Roberts (2005) and Hovakimian (2006).

random evolution. In sum, we believe that Chang and Dasgupta (2009) is an important paper, and that our paper complements theirs. We believe that our findings together suggest that evidence of active target readjustment behavior, reported in many papers in this literature over the decades, could have been spurious.

4 Fitting Estimators On the NP Process

[Table 5 here]

Table 5 summarizes our simulation protocol. *Our simulations remain as close to the empirically observed leverage data as possible.* This is important, because we are interested in assessing the quantitative magnitude of any biases, not just their qualitative presence.

4.1 Estimation

[Figure 3 here]

Figure 3 shows the performance of the estimators under Process NP (Page 16) when the researcher has no information about the firm-specific target. The true rho is on the x-axis; the inference is on the y-axis. The figures plot the mean and the (true simulated) two standard error bounds. With so many observations, the estimates are so precise that the bounds lie almost on top of the means.

The figure shows that the inference function for OLS is so flat that it is uninformative. In the data set, its estimate should range between about 0.9 and 0.94, almost regardless of the true underlying rho. This is because it ignores firms that have distinct own capital structure targets.

The FE, BB, and LD estimators are increasing when rho is below 1, and decreasing when it is above 1. Rhos greater than one may or may not be plausible for long series, but they are certainly plausible in our context for a limited number of years and non-linear shocks (and they are predicted by Baker and Wurgler (2002)). (The negative slope beyond 1 is not an artifact of the NP process, but also occurs under the SDPP process.) Thus, the inference functions produce multiple inverse mappings from a given empirical statistic to the true rho, one below 1 and one above 1.

- The FE empirical estimate of 0.68 is severely biased (see also Huang and Ritter (2009, Figure 5)). However, it has a good slope, and thus a bias-adjusted version makes a good estimator. After such a bias-adjustment, the FE estimator suggests a true rho of either **0.92** or **1.09**.
- The BB empirical estimate of 0.85 suggests a true rho of either **0.91** or **1.04**.
- The LD empirical estimate of 0.77 suggests a true rho of either **0.84** or **1.08**.

The W estimator is monotonic over the plotted domain. Its empirical estimate of 1.00 suggests a true SOA of **1.04.**

[Figure 4 here]

In contrast to Figure 3, Figure 4 assumes that the researcher has access to covariates that perfectly identify the target leverage ratio, T_i. Comparing the figures shows that this makes a difference for the previously badly biased and practically useless OLS estimate. Knowing the target perfectly compensates for the omitted variables problem, i.e., the fact that different firms have different targets. However, none of the other estimates are changed by knowledge of the target. The best point estimates for the true rho that correspond to the empirically observed measures remain virtually the same. Their standard errors are only mildly perturbed, too. In retrospect, this is not surprising. After all, Compustat provides over 130,000 firm-years. This provides for almost perfect accuracy, even in the absence of knowledge about the target.

In sum, with the exception of the OLS estimator, knowledge of firms' target leverage ratios does not substantially improve the inference. The question of what the best SOA estimate is can thus be answered even without good covariates for firms' target leverage ratios. Agonizing over controls is misplaced.

If the true SOA is a single homogeneous number for all firms, then there seem to be two plausible scenarios for rho: either a value around 0.85–0.92 (mean 0.89) or a value around 1.04–1.08 (mean 1.07). The lower value suggests a *half*-life of around 4–8 years (mean 6 years)—more in line with a slow adjustment view than with some of the interpretations in the papers that suggested the estimators. The higher value suggests no readjustment but amplification, in line with the timing theory of Baker and Wurgler (2002).

Even without an optimized reconciliation, the figure already hints which is better: First, ignoring the W estimator, the true rho estimates above 1 are closer to one another than their counterparts below 1. This makes it relatively more likely that the true estimate is the higher one. Second, adding the fairly monotonic W estimator with its 1.04 estimate points even more strongly towards the value of rho that is above 1.

4.2 Reconciliation Under The Feasible Process

The graphical reasoning ignored the slopes of the inference functions—for example, any far off true rho would add very little penalty to the OLS component of the objective function, because the OLS function is almost flat. It is better to find the true rho that best fits the estimates by repeating the reconciliation from Section 2.1 under our non-parametric process. The objective is again finding the rho with the lowest mean-squared T-statistic.

The upper panel in Table 6 shows that the best reconciliated estimate of an underlying true rho for estimators OLS, FE, BB, and LD is 1.066. It has a penalty of 3.43. This is quite good, given the large data set that pins down each estimator with great accuracy. Even a small and economically unimportant mismatch can produce high t-statistics. The penalty of 3.43 under the NP process is clearly much better than the penalty of 15.4 under the SDPP process. Not reported in the table, it is also much better than the second local minimum at a true rho of 0.89, which has a penalty of 8. Because the W inference function is more linear than those of the other estimators, it provides further powerful identification for disambiguating the local minima of 0.89 and 1.08. If we add the W estimator into the objective function, the best rho increases from 1.066 to 1.070, and the local minimum below 1 becomes even worse.

We also want to know how sensitive our objective function is to changes in the true reconciliating rho. A common measure for the sensitivity of an optimized parameter is the inverse Hessian at the minimum. Under the additional assumption that the penalty function is approximately normally distributed at its minimum, the inverse Hessian is the asymptotic standard error. For our purposes, we prefer to view it as a generic sensitivity measure for our inferred estimates, rather than as a formal standard error. The table shows that the inverse Hessian was 0.010. The best reconciling estimate of the true rho of 1.066 seems quite accurate.

4.2.1 Heterogeneity

However, firms may not be homogeneous in their SOAs. When firms have heterogeneous SOAs, a non-monotonic inference function does not make it easy to interpret its statistic as an "average" SOA. For example, consider a homogeneous case in which the true rho is 1 vis-a-vis a heterogeneous case in which half the firms have a true rho of 0.9 and the other half have a true rho of 1.1. In both cases, the average true rho is therefore 1.

- Figure 3 suggests that the non-monotonic FE statistic in the homogeneous case would be about 0.75. In the heterogeneous case, an estimation on the first half would yield an FE statistic of about 0.65. An estimation on the second half would yield about the same 0.65 statistic. An estimation using both samples would likely (though not surely) also yield 0.65. The heterogeneity around the true rho of 1.0 thus reduced the observed FE statistic by 0.10.

- In contrast, Figure 3 suggests that the more linear W statistic would have a value of about 0.93 in the homogeneous case, and an almost identical value of about $(0.85 + 1.00)/2 \approx 0.93$ in the heterogeneous case. The heterogeneity around the true rho of 1.0 thus did not reduce the observed W statistic.

This reasoning suggests that the W estimator is a more “trustworthy” measure of the *average* SOA in the presence of heterogeneity.

The difference in how the estimators respond to heterogeneity also makes it possible to estimate the amount of heterogeneity. As noted, a combined minimum-penalty rho can be viewed as an estimator, and it can assess not only a homogeneous true rho, but also heterogeneity in rho. Its estimates are unbiased and consistent, suggesting that the estimates may work well in a sample as large as our’s. (Of course, the estimator is not optimal, but its inaccuracy in such a large sample is already tiny, as is. As noted, the goal of our paper is reconciliation, not deriving estimators.)

There is no conceptual novelty in allowing the true rhos to be heterogeneous. We assume that each firm has its own true SOA, and that the cross-sectional distribution of rhos over firms has a normal distribution with mean μ_ρ and standard deviation σ_ρ. (Assumptions other than the normal distribution may fit better, but normality yields an appealingly simple two-parameter characterization.) Each simulation then draws different rhos for the firms based on the two parameters μ_ρ and σ_ρ, and the objective function finds the minimum penalty over them.

The middle panel in Table 6 shows that this extra degree of freedom improves our ability to explain the evidence. The best explanation for the empirical estimates provided by the OLS, FE, BB, and LD methods is an average rho of 0.986 with a cross-sectional heterogeneity of 0.109. The penalty at the optimum is a modest 2.09—again, in a sample as large as our’s, where even small deviations generate large t-statistics, this is a surprisingly good fit. The OLS, FE, and LD estimates are right on the money. Only the BB estimate remains too high and about 2% thereof are likely due to the initial conditions.[16] In sum, the best estimator of the *average* speed of adjustment for these four estimators under the assumption of heterogeneity is practically zero. However, there seem to be many firms that have a significant positive one and many firms that have a significant negative one.

The power to assess heterogeneity comes from differences in how estimators respond to it. Figure 3 shows that the four considered estimators in the middle panel have similar inference functions. This suggests that our estimate would be greatly improved by adding the W estimator to the objective function. Moreover, the middle panel also shows that if rho was 0.986 with a standard deviation of 0.109, the W estimate of 1.003 in the empirical data would be much too high. (It should have been around 0.900.)

[16]Recall Footnote 10, which pointed out that the BB estimate was also too high in the reconciliation under the SDPP process due to the initial conditions. If we consider this 2% a BB bias due to unusual IPOs that we should correct for, then the penalty under this process drops to 1.09. Reconciliation beyond the initial ratios is practically perfect.

The lower panel in Table 6 adds the W estimator into the objective function. With W in the objective function, the best SOA estimate has a true rho average of 1.066, with only modest heterogeneity of 0.039. Note that the objective function now has to reconcile five estimators, not four—a much more difficult task. Nevertheless, the objective function indicates an average penalty of only 3.3 (and, again, it would drop to 2.5 if we accounted for the 2% initial condition IPO bias of the BB estimator).

5 Alternatives and Robustness

Our paper has examined the estimators proposed in the papers referenced above. We have also investigated a number of other processes and issues, but are omitting them in order to keep this paper concise. Among them were the following:

A Modified Non-Parametric Process: We examined a process in which the target was $\tilde{\mathsf{T}}_{i,t+1} = \mathsf{T}_{i,t+1} + \tilde{u}_i$, where $\tilde{u}_i$ is normal noise with standard deviation of 9.5%. In contrast to process NP, this implies that the leverage ratio variance is unaffected by rho. The inference functions look similar to those in Figure 4. The mapped empirical estimates under NP' are: OLS=0.77 or 1.05, FE=0.91 or 1.10, LD=0.84 or 1.09, BB=0.92 or 1.04, and W=1.11. Repeating Table 6 would show that the best reconciling rho estimates are 1.07, 0.98, and 1.06, but the modified NP process has higher penalties (by about 1) than its NP cousin.

Parametric Process I: We examined an SDPP like process, in which leverage ratios outside the unit domain are assumed to be reported as being on the boundary. This process corrects the SDPP only with respect to its feasible domain, but leaves additive errors in the process intact.[17] The inference functions again look similar to those in Figure 4. However, the penalty is in excess of 10, so this process is best ignored.

Parametric Process II: In another SDPP like process, we allowed the non-adjustment leverage ratio to be IDC and then again reported ratios beyond the domain as if the researcher had observed a ratio at the border. The mapped empirical estimates are: OLS=0.91 or 1.09, FE=0.94 or 1.16, LD=0.82 or 1.13, BB=1.00, and W=1.04. The reconciliated rhos under this process fit the data better than under the parametric process I, but still not as well as the non-parametric processes. Repeating Table 6 would show that the best reconciling rho estimates are 0.94, 0.99, and 1.06, but this process has higher penalties (by about 2) than its NP cousin.

[17] Remarkably, even if we restrict the regression to past leverage ratios greater than 20% and less than 80%, contrary to the intuition in Kayhan and Titman (2007, page 26), the bias does not diminish much.

Structural Processes: If we can get Ilya's programs to work, then we can add a Strebulaev process, which is simulated given the parameters suggested by Strebulaev (2007). Otherwise, we can say that we consider only statistical rather than fully structural models. They should be viewed as processes that are locally robust to alternatives, in the same sense in which OLS assumptions are never fully satisfied. The alternative of estimates from structural models may be more powerful IF their stronger economic and functional assumptions hold.

Evolving Targets: Randomly evolving targets make little difference. Intuitively, we already know that the best fitting rhos are generally close to 1. Thus, given that the target was almost irrelevant, random perturbations or paths to it do not make it more important. We also explored targets that change in a manner that is correlated with shocks. The W estimator offers a specific quantitative prediction under the NULL that is extremely sensitive to even small changes in the dependent variable. This gives it the power to distinguish between a pure non-adjustment and another hypothesis (such as one in which a correlated-target is the cause). The evidence suggests that it is non-adjustment, and not optimal target changes that are responsible for the evidence. Of course, our evidence does not imply that the true target does not move in the same direction—just that it does not move enough to be detectably different (i.e., causing firms to actively adjust). The evolution of leverage ratios is simply too perfect to non-adjustment to be entirely due to changes in the target in the absence of another influence that makes exact non-adjustment optimal.

Lumpiness: We explored a process in which we presumed that firms adjust only when their evolved ratio deviates by at least 5% from their current ratio. For example, an actual leverage ratio sequence of (15%, 12%, 13%, 8%, 11%, 20%) would became an observed leverage ratio sequence of (15%, 15%, 15%, 8%, 8%, 20%). Again, the inference is virtually unchanged.

In sum, in addition to its appealing simplicity, Process NP seems to be the best description of the evolution of leverage ratios, in the sense that it can best reconcile the existing estimates.

We close with some observations about earlier work.

Heterogeneity: We believe the empirical evidence strongly suggests that lack of adjustment is the first-order behavior of publicly traded firms. This is not at all inconsistent with the view that some firms do adjust (or are culled), at least in some years. We even provided evidence in favor of some unconditional heterogeneity in the SOA. Our test was unbiased and consistent, although it was not an estimator designed to be optimal. It was the availability of ample data that made its inference plausible.

Improving the test for unconditional heteroskedasticity may well be less interesting than identifying conditional heteroskedasticity. If we can correctly identify which firms should adjust more quickly, then we can improve the power of the test. For example, Leary and Roberts (2005) suggest that firms which have experienced a negative value shock and thus obtain a higher leverage ratio should be more inclined to rebalance. Their Figure 4 indeed suggests a fairly speedy SOA for such firms, with roughly a half-life of around 2–3 years. For about the 5th and 20th percentiles of negative stock return shocks, i.e., firms having experienced a –66% (–35%) rate of return, we could replicate half-lives of about 4 (7) years. However, as conjectured (but not shown) by Huang and Ritter (2009, page 24), we found that about 1/3 (1/10) of firms with such negative returns disappear from Compustat before they reported their same-year-end leverage ratio. If we attribute a 100% leverage ratio (bankruptcy) to these firms at the end of the fiscal year, the estimated SOA half-lives more than double. The fact that these firms tend to disappear clearly supports the notion that firms do face a trade-off (a cost to too much debt). The support is less strong for the interpretation that managers actively adjust, even those that survive such dire situations. Firms that experience more than a –66% rate of return had their leverage ratios increase from a mean of 25% to 42.9%. If managers had done nothing, stock returns would have moved it to 42.4%. In the following year, stock returns would have moved them to 39.9% in the absence of capital structure activity (due to their stock returns). Instead, active capital structure management reduced the leverage ratio to 38.5%. In sum, any active managerial readjustment seems modest. The data can barely suggest that the managers of surviving firms' adjusted—it primarily suggests that some firms with disastrous stock returns disappear.

Other conditional heterogeneity has been identified elsewhere, too. For example, Kisgen (2006) finds that firms near a change in credit ratings issue less debt. (Of course, leverage ratio increases and debt issuances are not one-to-one.) Roberts and Sufi (2009) find that firms issue less debt when they violate a covenant.

Five-Year Changes: There is evidence that firms show more adjustment behavior when we use longer intervals to measure changes. The estimated rhos become much less accurate and very biased. They can be corrected, just as those in Figure 4. The corrected evidence suggests that the best empirical estimates for a true 1-year underlying rho based on 5-year changes are 90% for the BB estimator, 96% for the W estimator, and 99% for the LD estimator.

In sum, the evidence is not that no firm adjusts, but that firms *on average* do not readjust very quickly. Non-adjustment is the first-order effect. Non-activity which implies non-adjustment is consistent with a tradeoff theory with frictions, in which the curvature of the objective function is too mild to overcome the costs of readjusting.

6 Conclusion

Our paper found [a] that discrepancies in SOA estimates in the literature, given the almost asymptotically large sample, suggest violations of these papers' underlying process assumptions; [b] that knowing the firm-specific leverage ratio target should be relatively unimportant (except for the OLS estimator); and [c] that the inference function of many estimators in the literature is non-monotonic, which can lower the estimated statistics if SOAs are heterogeneous.

The main contributions of our paper were three-fold:

- We suggested a non-parametric process to model leverage ratios under the NULL. In this process, managerial debt changes and equity changes are joint processes. A random matching firm provides these changes under the NULL hypothesis that the firm changes its debt and equity randomly. Applying the firm's own stock returns and these random debt and equity changes results in the next leverage ratio under the NULL. Our method preserves the essential characteristics of leverage as a ratio. To reject the NULL of no readjustment, the firm's actually observed debt and equity changes should be different from those of the randomly chosen firm.

  This process is not only suitable to our own investigation of the SOA, but also to modeling the NULL hypothesis in many other papers (as in Chang and Dasgupta (2009)).

- We suggested a methodology to diagnose and reconcile estimators obtained from different estimators. Under a given process, one searches for a single true rho (or the mean and variance of a distribution of true rhos across heterogeneous firms) that best explains the empirical estimates. The reconciliation also allows comparing how good underlying process assumptions are.

- We showed that the best homogeneous estimate of the SOA is zero or negative. Depending on the estimators that are being reconciled, heterogeneity may be modest (4%) or severe (10%). To reconcile all estimators requires a negative SOA, as suggested by Baker and Wurgler (2002).

Non-readjustment is not evidence against tradeoff theories with frictions. On the contrary—this models predict very little activity which in turn induces a very low SOA. This was noted by Myers (1984), Fischer, Heinkel, and Zechner (1989), Welch (2004), and others, and quantified by Strebulaev (2007).

We wish to close with an opinion. Replication and critique is so rare in financial economics that it has acquired a stigma, both for the original paper and the critique. This is regrettable. We believe that we need more reconciliations of seemingly conflicting results. It is hard to see how finance as a field can make progress if the published literature cannot understand why earlier results are different, and thereby converge on a set of facts that can become generally accepted. We believe that the speed of adjustment should be among them.

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Table 1: Measures of Leverage Ratios and Sample Characteristics

		Levels			Changes		
Variable	Explanation	Mean(i,t)	Sdv(i,t)	$\overline{\text{Sdv(t)}}$	Mean(i,t)	Sdv(i,t)	$\overline{\text{Sdv(t)}}$
DC	Financial Debt/Financial Capital, Market Value $\frac{\text{DLTT+DLC}}{\text{DLTT+DLC+CSHO} \cdot \text{PRCC\_F}}$	27%	25%	23%	1.2%	12.6%	11.4%
LA	Liabilities/Assets, Market Value $\frac{\text{LT+PSTKL–TXDITC–DCVT}}{\text{CSHO} \cdot \text{PRCC\_F+LT+PSTKL–TXDITC–DCVT}}$	40%	25%	24%	1.5%	12.4%	11.2%
dc	Financial Debt/Financial Capital, Book Value $\frac{\text{DLTT+DLC}}{\text{AT}}$	24%	20%	19%	0.6%	9.7%	9.0%
la	Liabilities/Assets, Book Value $\frac{\text{LT+PSTKL–TXDITC–DCVT}}{\text{AT}}$	45%	21%	20%	0.8%	10.2%	9.5%
IDC	Stock-Return Implied DC (and non-stock return changes) $\frac{\text{DLTT+DLC}}{\text{DLTT+DLC+(CSHO} \cdot \text{PRCC\_F)} \cdot (1+r_{i,t})}$	26%	25%	23%	$(\text{IDC}_{t-1,t} - \text{DC}_{t-1}) =$ –1%	8.7%	8.1%

(continued)

Table 1: Measures of Leverage Ratios and Sample Characteristics (continued)

where Compustat defines variables as

Variable	Explanation	Mean(i,t)	Sdv(i,t)	$\overline{\text{Sdv(t)}}$
DLTT	Long-Term Debt, Total	\$352	\$2,629	\$1,655
DLC	Debt in Current Liabilities, Total	\$122	\$1,847	\$1,122
CSHO	Common Shares Outstanding	\$50	\$244	\$136
PRCC_F	Price Annual Close, Fiscal Year End	\$25	\$748	\$357
LT	Liabilities, Total	\$1,011	\$7,582	\$4,736
PSTKL	Preferred Stock, Liquidating Value	\$13	\$329	\$159
TXDITC	Deferred Taxes and Investment Tax Credit	\$72	\$591	\$406
DCVT	Debt, Convertible	\$19	\$208	\$128
AT	Assets, Total	\$1,632	\$10,239	\$6,677

and

	Mean	Sdv	Min	Median	Max
Number of Firms Per Year	3,299	1,436	453	3,307	5,804
Number of Years Per Firm	19	12	1	16	45

Explanation: In addition to the data from Compustat, we use the rate of return $r_{i,t}$ without dividends from CRSP. We confirmed that using the CRSP market value of equity instead of CSHO·PRCC changes empirical alpha estimates by no more than by 1%. Mean and Sdv are pooled statistics. $\overline{\text{Sdv(t)}}$ is the average cross-sectional standard deviation per year. The observation count includes firm-years from 1963 to 2007 with non-missing DC, excluding financial firms and utilities (SIC codes 6000–6999 and 4900–4949), and prior year total assets of more than \$10 million. This leaves a total of 14,615 firms with 148,464 firm years. The change in IDC is an exception, in that it subtracts lagged DC. (It is the non-stock return caused change in capital structure.)

Table 2: Estimation Procedures

Procedure	Explanation
OLS, Auto (OLS) Lemmon, Roberts, and Zender (2008), Fama and French (2002), etc.	$\mathsf{DC}_{i,t} = c + \rho \cdot \mathsf{DC}_{i,t-1} \;\; [+(1-\rho) \cdot \gamma \cdot \mathsf{T}_{i,t-1}] + \epsilon_{i,t}$ (Note that estimations in which the dependent variable is measured in differences come to virtually identical inferences.)
Firm Fixed Effects (FE) Flannery and Rangan (2006)	$\mathsf{DC}_{i,t} = c_i + (1-\rho) \cdot \mathsf{DC}_{i,t-1} \;\; [+\rho \cdot \gamma \cdot \mathsf{T}_{i,t-1}] + \eta_i + \epsilon_{i,t}$ Similar to "OLS, Auto," but each firm receives its own intercept.
Blundell Bond GMM (BB) Lemmon, Roberts, and Zender (2008)	Stata: **xtabond2 dc L.dc [T], gmm(L.(dc), lag(1 3)) [iv(T)] robust** System GMM estimation (Blundell and Bond (1998)), for estimating dynamic panel data models in the presence of firm fixed effects. Includes variable levels, as well as differences, in the instrument set to address the problem of persistent regressors. (Uses book values and exact GMM stata specification, as noted.)
Long Difference Estimator (LD) Huang and Ritter (2009)	$\mathsf{LA}_{i,t} - \mathsf{LA}_{i,t-8} = \rho \cdot (\mathsf{LA}_{i,t-1} - \mathsf{LA}_{i,t-9}) \;\; [+\delta \cdot (\mathsf{T}_{i,t-1} - \mathsf{T}_{i,t-9})] + \epsilon_{i,t} - \epsilon_{i,t-8}$ Uses iterative two-stage least squares instruments $(\mathsf{LA}_{i,t-1} - \mathsf{LA}_{i,t-9})$ with $\mathsf{LA}_{i,t-9}$. Obtains initial values for $\tilde{\rho}$ and $\tilde{\delta}$. Uses the residuals $\mathsf{LA}_{i,t-1} - \tilde{\rho} \cdot \mathsf{LA}_{i,t-2} - \tilde{\delta} \cdot \mathsf{T}_{i,t-2}, \ldots, \mathsf{LA}_{i,t-7} - \tilde{\rho} \cdot \mathsf{LA}_{i,t-8} - \tilde{\delta} \cdot \mathsf{T}_{i,t-8}$ as additional instruments. (HR perform 3 iterations overall.) This estimator was suggested by Hahn, Hausman, and Kuersteiner (2007).
Implied Target (W) Welch (2004)	$\mathsf{DC}_{i,t} = c + \rho \cdot \mathsf{IDC}_{i,t} + (1-\rho) \cdot \mathsf{DC}_{i,t-1} \;\; [+\gamma \cdot \mathsf{T}_{i,t-1}] + \epsilon_{i,t}$ where $\mathsf{IDC}_{i,t}$ is the implied financial debt to capital ratio (leverage), which is the past leverage adjusted for the change in the stock price. One can also extend this method to use fixed effects, $\mathsf{DC}_{i,t} = c_i + \rho \cdot \mathsf{IDC}_{i,t} + (1-\rho) \cdot \mathsf{DC}_{i,t-1} \;\; [+\gamma \cdot \mathsf{T}_{i,t-1}] + \epsilon_{i,t}$

Explanation: Variables are described in Table 1. ($\mathsf{DC}_{i,t}$ is financial debt to capital ratio, our main measure for leverage.) $\mathsf{T}_{i,t-1}$ is a proxy for the firm target, usually instrumented by observable variables. $\epsilon_{i,t}$ is a random disturbance term. ρ is the autocoefficient, our main parameter of interest, which is also 1- SOA.

Table 3: Estimates With Full Sample

Measure ↓	Method →	**OLS**	**FE**	**BB**	**LD**	**W**	**WFE**
	Lagged Leverage (DC)	**0.895**	**0.681**	**0.847**	**0.772**	–0.075	–0.191
		(0.002)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)
	Implied Debt Ratio (IDC)					**1.003**	**0.971**
Market DC						(0.004)	(0.004)
	Constant	0.040	many	0.053		0.029	many
		(0.001)		(0.001)			
	Observations	132,412	132,412	132,412	55,967	128,943	128,943
	R-Squared	0.76	0.81	.	.	0.88	0.90
	Lagged Leverage (LA)	**0.886**	**0.660**	**0.828**	**0.779**	–0.026	–0.122
		(0.002)	(0.004)	(0.005)	(0.004)	(0.003)	(0.003)
	Implied Debt Ratio (ILA)					**0.969**	**0.942**
Market LA						(0.003)	(0.003)
	Constant	0.147	0.147	0.082		0.035	0.082
		(0.002)	(0.002)	(0.002)		(0.001)	(0.001)
	Observations	120,827	120,827	120,827	44,783	117,626	117,626
	R-Squared	0.77	0.82	.	.	0.92	0.93
	Lagged Leverage (*dc*)	**0.893**	**0.677**	**0.829**	**0.772**		
		(0.002)	(0.005)	(0.008)	(0.004)		
Book *dc*	Constant	0.032	many	0.047			
		(0.001)		(0.002)			
	Observations	136,450	136,450	136,450	58,345		
	R-Squared	0.77	0.81	.	.		
	Lagged Leverage (*la*)	**0.888**	**0.683**	**0.864**	**0.789**		
		(0.002)	(0.005)	(0.009)	(0.004)		
Book *la*	Constant	0.0582	many	0.0688			
		(0.0009)		(0.0040)			
	Observations	119,919	119,919	119,919	43,694		
	R-Squared	0.77	0.82	.	.		

Explanation: The variables are defined in Table 1. The estimation techniques are defined in Table 2. The dependent variable is always the same as the first independent variable (lagged) (except in the LD column, where it is a change.) The table reports results for two measures of market leverage and two measures of book leverage. Standard errors are reported in brackets, and adjusted to be robust to heteroscedasticity and clustered within firms.

Interpretation: The estimates seem uncomfortably far from one another.

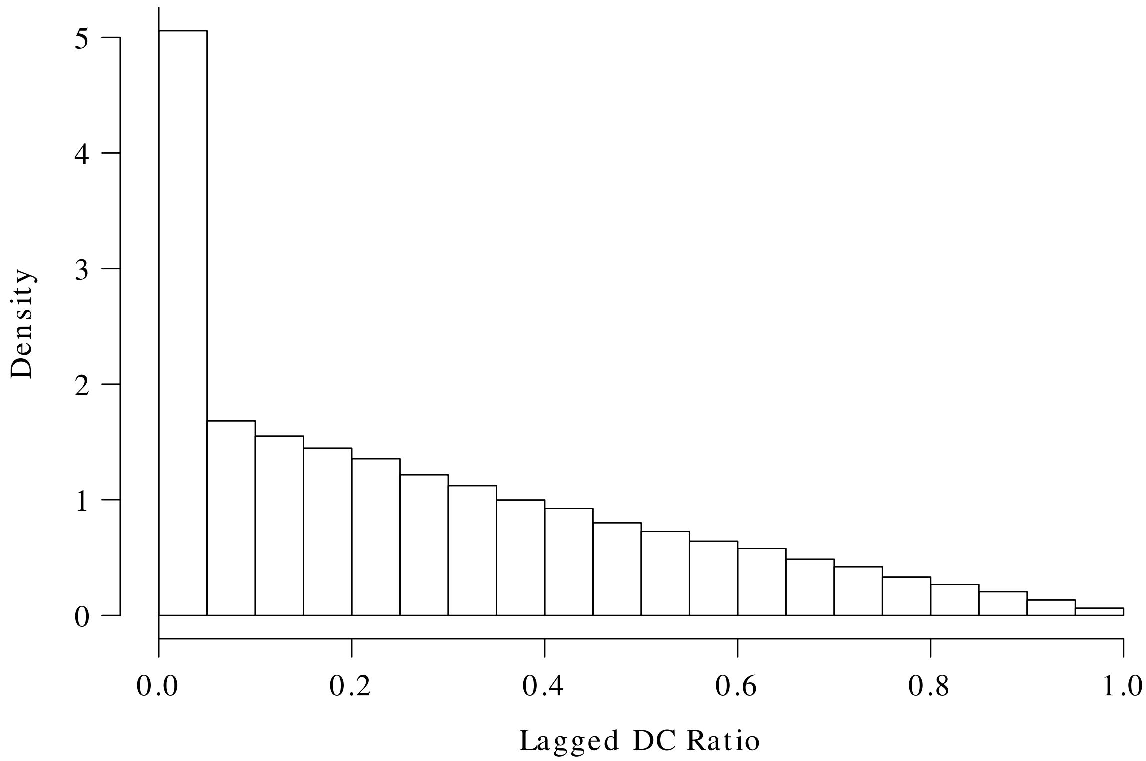
Table 4: Estimates With Constant Sample (40,708 Firm-Years)

Measure ↓	Method →	**OLS**	**FE**	**BB**	**LD**	**W**
Market DC	Lagged Leverage (DC)	**0.894**	**0.665**	**0.857**	**0.768**	-0.081
		(0.0027)	(0.0066)	(0.0070)	(0.0039)	(0.0075)
	Implied Debt Ratio (IDC)					**1.015**
						(0.0076)
	Constant	0.027	many	0.037		0.025
		(0.0008)		(0.0019)		(0.0006)
	R-Squared	0.79	0.83	.	.	0.88
Market LA	Lagged Leverage (LA)	**0.902**	**0.673**	**0.851**	**0.775**	-0.020
		(0.0023)	(0.0063)	(0.0072)	(0.0035)	(0.0052)
	Implied Debt Ratio (IDC)					**0.969**
						(0.0050)
	Constant	0.129	many	0.059		0.032
		(0.0025)		(0.0029)		(0.0007)
	R-Squared	0.80	0.84	.	.	0.92
Book *dc*	Lagged Leverage (*dc*)	**0.903**	**0.677**	**0.850**	**0.771**	
		(0.0029)	(0.0077)	(0.0095)	(0.0043)	
	Constant	0.024	many	0.035		
		(0.0007)		(0.0022)		
	R-Squared	0.79	0.83	.	.	
Book *la*	Lagged Leverage (*la*)	**0.921**	**0.730**	**0.908**	**0.787**	
		(0.0026)	(0.0072)	(0.0097)	(0.0040)	
	Constant	0.041	many	0.046		
		(0.0012)		(0.0044)		
	R-Squared	0.81	0.85	.	.	

Explanation: This table repeats Table 3 but restricts the sample to observations that are used in all methods. The principal reason why the number of firm-years is so low is that the LD estimator requires long sample time-series.

Interpretation: The estimates remain uncomfortably far from one another. Sample differences are not the reason.

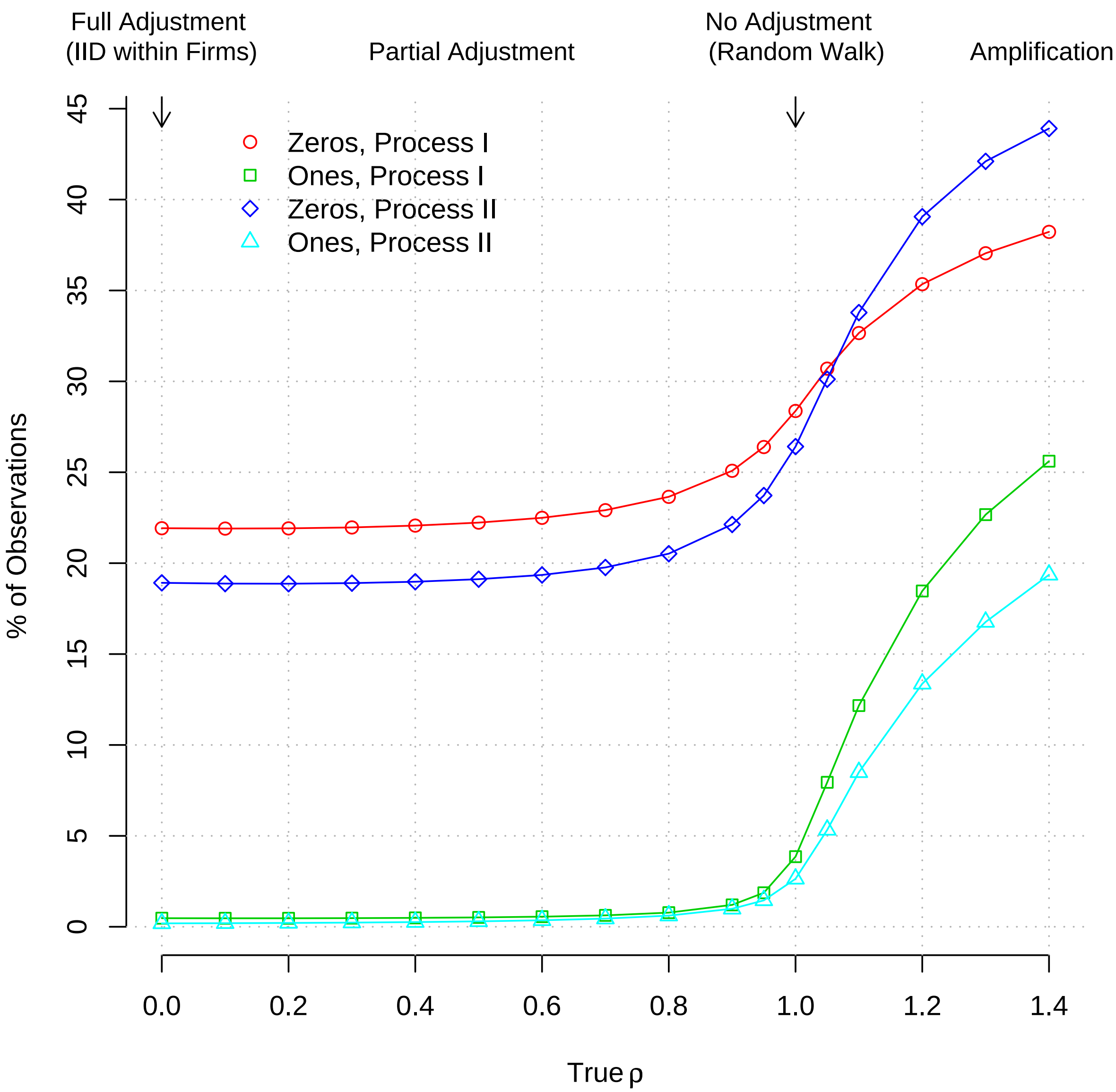
Figure 1: Histogram of Debt-to-Capital Ratios (DC)



Explanation: This figure plots a histogram of (lagged) leverage ratios. This is typically an independent variable in our regressions predicting its own future value. The figure shows that the distribution is skewed to the right, and there is a large number of firms with leverage ratios less than 10%. With a mean of 27% and a standard deviation of 25%, the truncation is about one standard deviation from the mean. Moreover, not shown in this figure, the year-to-year standard deviation of changes in DC in the sample is 12.5%.

Interpretation: With an additive error process, firms are likely to run especially into the lower domain limit quite frequently.

Figure 2: Percentage of Leverage Ratios Below 0% and Above 100%



Explanation: This figure shows the percentage of firm-years in which an unconstrained AR(1) process produce infeasible leverage ratios. The simulated true leverage processes are Process I, i.e., $\mathrm{DC}_{i,t+1} = \rho \cdot \mathrm{DC}_{i,t} + t_i + u_{i,t+1}$ or Process II, i.e., $\mathrm{DC}_{i,t+1} = \rho \cdot \mathrm{IDC}_{i,t,t+1} + t_i + u_{i,t+1}$.

Interpretation: When rho is high, firms would run into the domain limit imposed by additive error processes much more frequently.

Table 5: Summary of Simulation Protocol

- The dimension of the firm-year matrix is the same in the simulations as in the actual data. That is, each sample draw is equivalent in dimension to that in Table 3. In each run, this means that we have 132,412 firm-years with at least one lagged leverage ratio.

  When a firm first appears in the data, it also first appears in simulated data sets, where it is assumed to start out with its actually observed first target leverage ratio.

  When a firm disappears from the Compustat data, it also disappears from the simulated data set.

- The simulated process is

$$\widetilde{\mathsf{DC}}_{i,t+1} = \rho \cdot \mathsf{SDC}_{i,t,t+1} + (1-\rho) \cdot \mathsf{T}_i$$

$$\mathsf{SDC}_{i,tt+1} \equiv \frac{\mathsf{DC}_{i,t} \cdot (1+\tilde{\nu}^R_{i,t,t+1})}{\mathsf{DC}_{i,t} \cdot (1+\tilde{\nu}^R_{i,t,t+1}) + (1+\tilde{\eta}^R_{i,t,t+1}) \cdot (1+r_{t,t+1}) \cdot (1-\mathsf{DC}_{i,t})}$$

  The random matches, $\tilde{\nu}^R_{i,t,t+1}$ and $\tilde{\eta}^R_{i,t,t+1}$, are the percent changes in debt and equity of a random matching firm-year, drawn without regard to the firm's or the match's capital structure. $\tilde{\eta}^R_{i,t,t+1}$ is the match's percent change in the value of equity that is *not* due to stock returns, and $r_{i,t,t+1}$ is the firm's own realized firm stock return returns from CRSP. Drawing $\tilde{\eta}^R$ and $\tilde{\nu}^R$ shocks jointly preserves the correlation structure of non-stock related debt and equity changes.

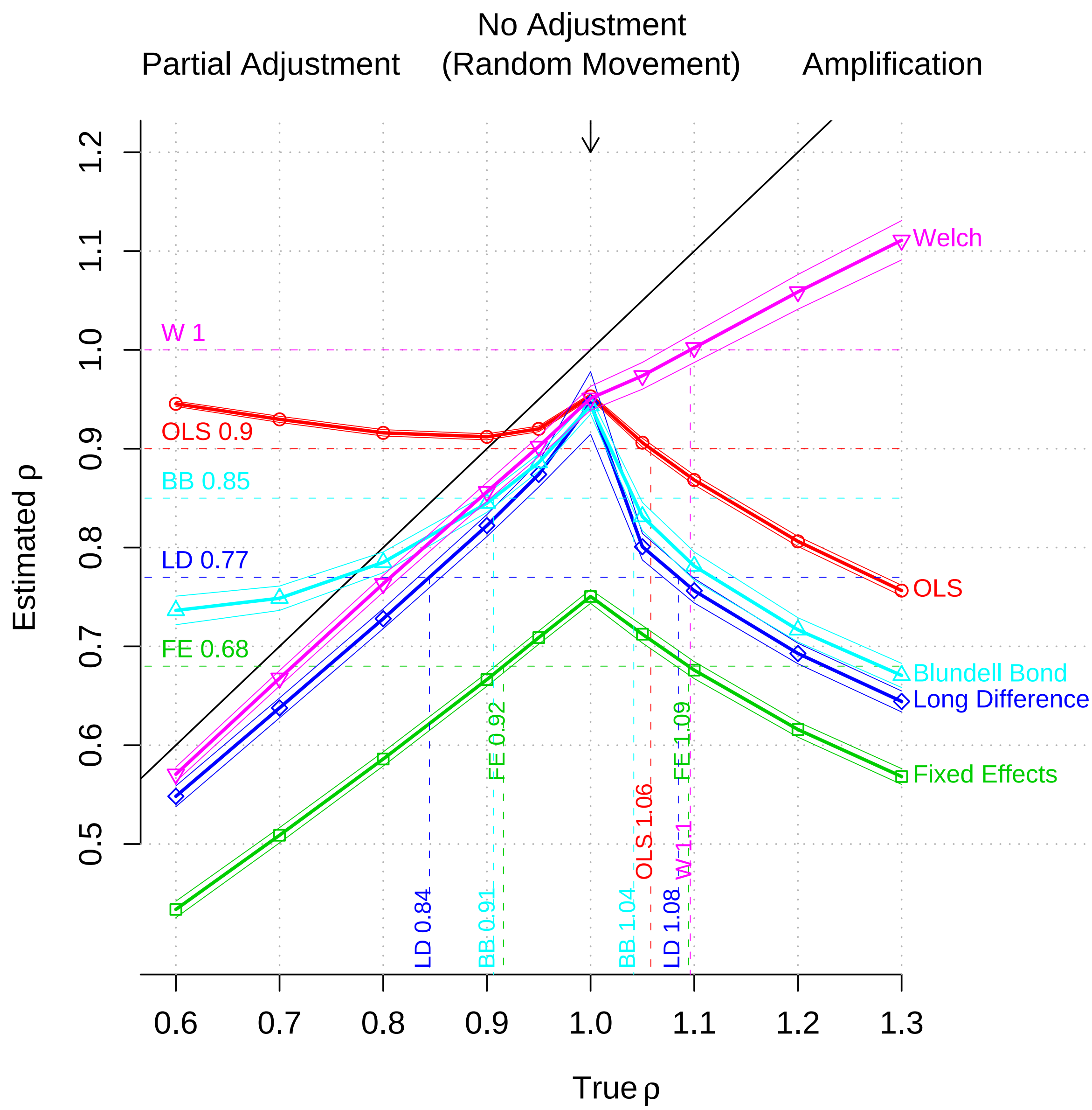
  Thus, the hypotheses tested is that these changes are dependent or independent of the firm's current leverage ratio.

- In this simulation, each firm's target is one-shock away from the firm's initial leverage. Here, $\mathsf{T}_i = \frac{\mathsf{DC}_{i,0} \cdot (1+\tilde{\eta}^R_{i,t-1,t}) \cdot (1+r^R_{i,t-1,t})}{\mathsf{DC}_{i,0} \cdot (1+\tilde{\eta}^R_{i,t-1,t}) \cdot (1+r^R_{i,t-1,t}) + (1+\tilde{\nu}^R_{i,t-1,t}) \cdot (1-\mathsf{DC}_{i,0})}$, where $\tilde{\nu}^R_{i,t-1,t}$, $\tilde{r}^R_{i,t-1,t}$ and $\tilde{\eta}^R_{i,t-1,t}$ are drawn as a vector from the complete sample distributions of observed triplets. (Note: this is the only place where a stock return is drawn rather than retained.) We have confirmed that the results are similar if we assume that each firm's first leverage ratio is also its target, or when we draw random targets for each firm.

- Each simulation is a set of 132,412 times two draws from the two error distributions, one for each firm-year.

- We simulate the capital structure processes and use the procedures from Table 2 to estimate rho with different techniques. Our inference is based on 500 simulations of each process, with each simulation exploring values of true rho from 0.0 to 1.4.

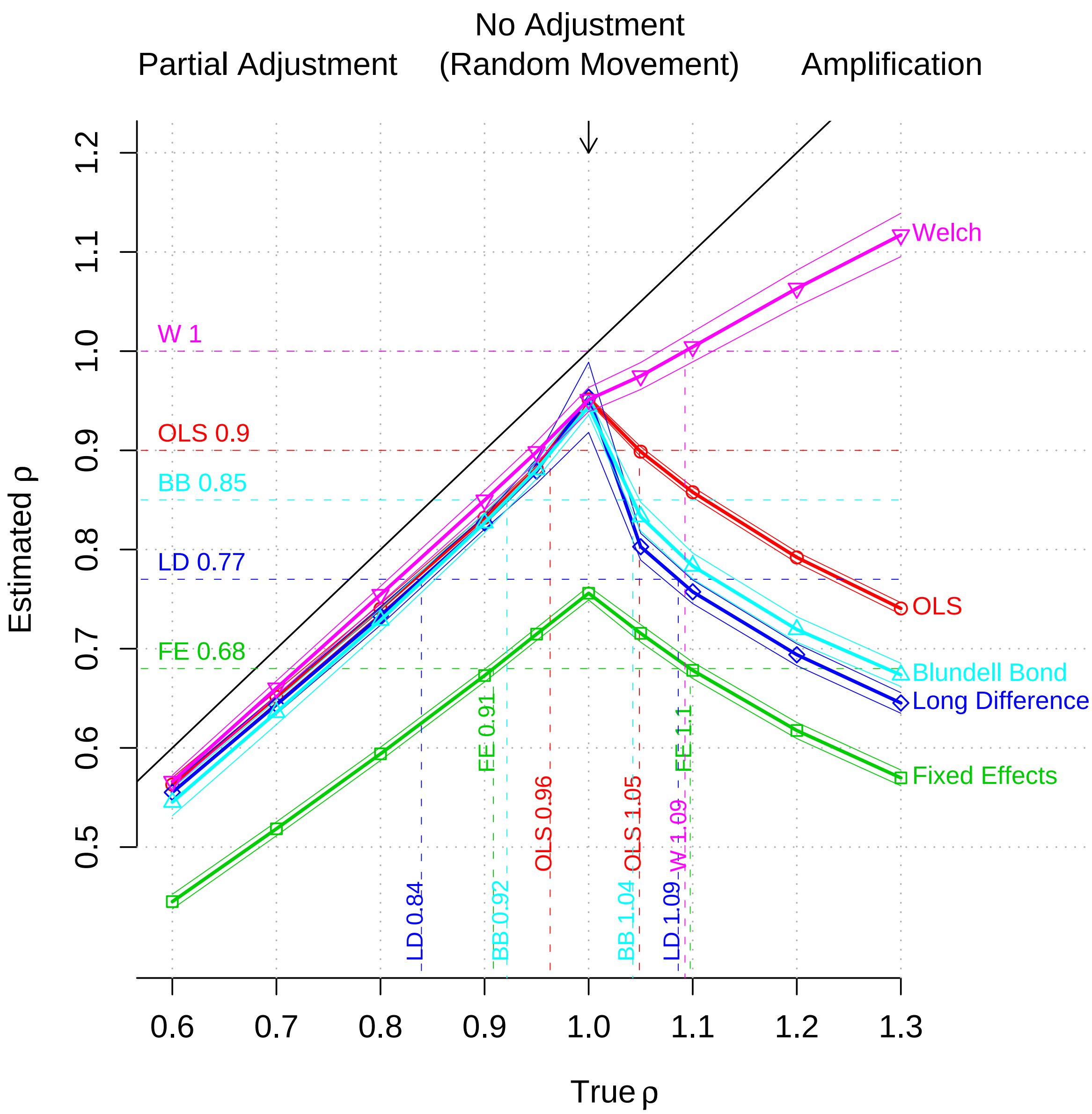
Figure 3: Inference Functions (Simulated Means With Two Standard Error Bounds) for Estimators under Process NP with Unknown (Firm-Specific) Target



Explanation: Variables are described in Table 1. Estimators are described in Table 2. The simulation process is described in Table 5. In brief, the simulated process is $\mathrm{DC}_{i,t} = \rho \cdot \mathrm{SDC}_{i,t-1}(\nu_{i,t}, \eta_{i,t}, r_{i,t}) + (1-\rho) \cdot \mathrm{T}_i$, where $\mathrm{SDC}_{i,t-1}(\cdot) \equiv D_{i,t-1} \cdot (1+\nu_{t-1,t})/[D_{i,t-1} \cdot (1+\nu_{t-1,t}) + (1+\eta_{t-1,t}) \cdot (1+r_{t-1,t}) \cdot (E_{i,t-1})] \equiv \mathrm{DC}_{i,t-1} \cdot (1+\nu_{t-1,t})/[\mathrm{DC}_{i,t-1} \cdot (1+\nu_{t-1,t}) + (1+\eta_{t-1,t}) \cdot (1+r_{t-1,t}) \cdot (1-\mathrm{DC}_{i,t-1})]$. The SDC value models debt and equity to be joint random draws (from a randomly chosen firm) and only then computes the leverage ratio. If rho is between 0 and 1, leverage ratios are guaranteed to be between 0 and 1, too.

Interpretation: The OLS method is uninformative. The FE method has a strong bias, and the BB and LD methods have a modest bias for true rhos below 1. The W method retains its monotonicity over the plotted region. The other inference functions are non-monotonic.

Figure 4: Inference Functions (Simulated Means With Two Standard Error Bounds) for Estimators under Process NP with **Known** (Firm-Specific) Target



Explanation: This figure is identical to Figure 3, except the researcher knows each firm's true target precisely.

Interpretation: Knowing the target is not important with so many observations, except in the OLS process (where it cures the omitted variable problem).

Table 6: Reconciliation: Multiple Estimators' Best Fitting True Rho

Included	Best Fitting True Rhos			Individual Fits					Overall Penalty
		Avg ρ	Sd(ρ)	Method	Empirical	Fitted	S.E.	Penalty	
Homogeneous				OLS	0.895	0.893	0.0024	−0.68	
OLS, FE, LD, BB	Best Estimate	1.066	(Forced 0)	FE	0.681	0.700	0.0043	+4.43	3.43
	Inverse Hessian	(0.010)		LD	0.772	0.785	0.0063	+2.03	
				BB	0.847	0.813	0.0072	−4.77	
				~~W~~	~~1.003~~	~~0.983~~	~~0.0070~~	~~−2.89~~	
		Avg ρ	Sd(ρ)	Method	Empirical	Fitted	S.E.	Penalty	
Heterogeneous				OLS	0.895	0.897	0.003	−0.92	
OLS, FE, LD, BB	Best Estimate	0.986	0.109	FE	0.681	0.682	0.005	−0.11	2.09
	Inverse Hessian	(0.022)	(0.014)	LD	0.772	0.783	0.008	−1.36	
				BB	0.847	0.814	0.009	+3.85	
				~~W~~	~~1.003~~	~~0.900~~	~~0.006~~	~~+17.14~~	
		Avg ρ	Sd(ρ)	Method	Empirical	Fitted	S.E.	Penalty	
Heterogeneous				OLS	0.895	0.891	0.002	+1.58	
OLS, FE, LD, BB, W	Best Estimate	1.066	0.039	FE	0.681	0.695	0.004	−3.18	3.30
	Inverse Hessian	(0.022)	(0.014)	LD	0.772	0.780	0.007	−1.19	
				BB	0.847	0.809	0.007	+5.35	
				W	1.003	0.979	0.007	+3.42	

Explanation: Any (true) rho produces a measure of fit of its empirical value for each estimator. This measure of fit is the t-statistic which tests for equality of the empirically observed estimate and the average simulated estimate under the given process with the given true rho. We then find the (true) rho that minimizes an objective function that is the square-root of the average squared t-statistic. This is the best reconciliation. W is included in the objective function ony in the final panel. (Note that the minimized penalties [t-stats] do not average to zero, because the penalty function does not have a constant.) The inverse Hessians are a measure of fit of the penalty function around the optimum. For numerical stability, these are calculated from a distance of 0.02 off the maximum. In a normally distributed context, they could be interpreted as pseudo standard errors.