

A Critique of Quantitative and Deep-Structure Modeling in Capital Structure Research and Beyond

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Ivo Welch

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(If you have seen earlier drafts, this one is much better.)

- The study of the behavior of corporations.
 - Capital Structure (financing and payout)
 - Capital Budgeting (project choice)
 - Corporate Governance (incentives)
- Research challenges abound. Some are standard, some are unusual.

Research Challenges in CF

- Many different priors. Many different theories.
- Many (quasi-)endogenous variables.
- Murky evidence—specification dependent, sample dependent, small fraction of variance explained.
- Few or no arbitrage constraints—mistakes cannot be corrected by outsiders.
- Large cross-section, short low-frequency time-series, with selection and survivorship biases.
- Non-understood size, industry, time effects.
- Proxy issues, average vs. marginal

But

- + Many quasi-experiments (e.g., tax code, bankruptcy cost, productivity, resource cost changes)

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Capital Structure

- Let's focus on one area to animate arguments.
- Capital Structure:
 - how firms' leverage ratios come about.
- Although some critique points are specific to two prominent papers' hypotheses, almost all points are much broader.
 - I believe some specific problems with specific hypotheses apply to all papers in the genre.

Methods

- Choice 1: Include all forces. Let the data sort out which are important.
- Choice 2: Select a subset of forces. Pray you have the right ones, and/or that omitted forces are orthogonal.

Almost always choice 2. Choice 1 is way too difficult.

Other Misspec in CapStruct Research

- Empirical proxies (e.g., Tobin's Q: avg vs. marginal).
- Firm size?!?!
- Industry?!?!
- Time Effects?!?!
- Missing data for specific hypothesis, non-random.
- Firms are public on average for 10 years and then disappear. When they disappear (esp bad if leverage is high and stock returns are low), the **final financial statement is not available**.
- Scaling by firm assets, sales, net income, nothing?
- Everything seems sort of endogenous
- High correlations among financial statement variables.

Occasional remedies—residual diagnostics, more controls, fixed effects, differencing.

Empirical Test Approaches

- In-sample evidence—efficient if model is correct.
 - Good, quick assessment of what correlates highly.
 - **WARNING:** Leverage ratios are not normally distributed. Need resampling approach to establish properties of estimators

But what if the model is not correct? Now what?

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Empirical Test Approaches

- Out-of-Sample evidence.
 - Easy comparison of nested and unnested evidence based on objective criterion.
- In-Differences Evidence. (Predict changes in variables.)
 - If firms' (dependent) vars behave as they always have, this is no longer evidence in favor.
 - Helps against spurious evidence...
 - ...but not perfect: changes could also be affected by omitted variables.
- Quasi-Experimental Evidence (“Unusual, Identified Differences”)
 - (Natural experiments, Reg discontinuities, Diff-in-Diff, IV)
 - relies on unusual circumstances, where economics provides us with the situation.
 - If good experiment, then almost a direct causality tests.
 - Now “same behavior” rejects model.
 - But, is it the question or the experiments that now drive the research agenda?

Capital Structure QE

Capital structure offers many good quasi-experiments:

- Taxes have changed repeatedly.
- Transaction costs of issuing has changed repeatedly.
- Financial distress costs changed (prepacks, 1986)
- Productivity changed (sector specific)
- Personnel can die.
- Financial crisis selectively reduced access to capital.

Quasi-experimental tests are a “Gold Standard”

Now I will critique existing models in CF,
in light of the challenges above

Theory Models in CF

Quantitative and Deep Structure Models.

- Increasing Market Share
- Many Awards (Brattle prizes)

I will pick two of the best and most prominent ones, show what they do, and show how they fall short.

My critique is *not* about these two models, but about the approach more generally.

Preview of My Points

- 1 Ignored too many plausible forces based on priors.
- 2 Huge gap between unobservable mdl and measured tst.
 - 1 Many models map into same reduced-form evidence.
 - 2 Even orthogonal forces to mdl distort the inference.
 - 3 Needless—direct proxies seem better than FOC proxies.
 - 1 Don't use the CAPM and a distress model to tell you the cost of corporate borrowing.
 - 2 Use prevailing corporate borrowing rates.
- 3 Ignored all econometric issues mentioned above.
- 4 Not held to same high standards—no quasi-experimental or OOS evidence *to-date, ever.*

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- Tax Distress Model Base.
 - (Workhorse since Robichek-Myers, 1966.)
 - Taxes favor debt.
 - Distress-costs favor equity.
- Frictions.
 - (Since Fischer-Heinkel-Zechner, 1989.)
 - Transaction costs mean that firms don't constantly reoptimize.

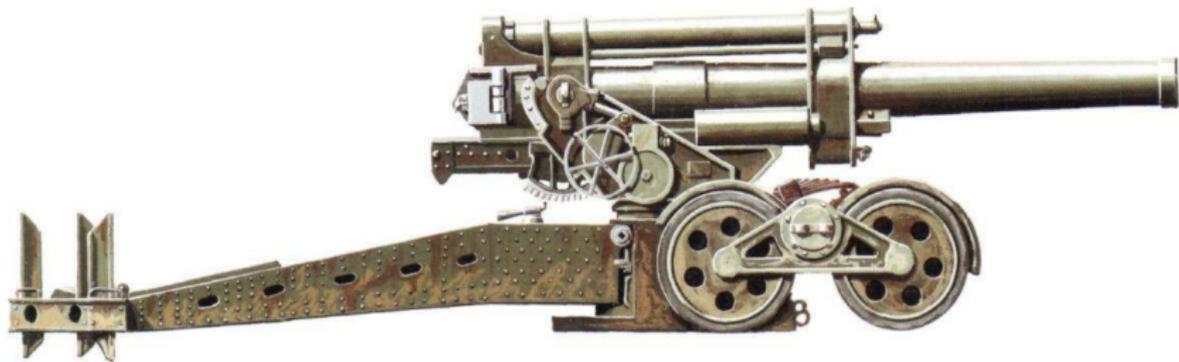
Innovations of Strebulaev:

- Points out that the fact that high-profit companies have lower debt ratios is not inconsistent if frictions are high enough. (Easy to illustrate with two-period sketch model, too.)
- Hallmark: **Quantitative dynamics** (not deep structure)

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- 1 PV of all future net payouts at time 0 (V_0).
- 2 The initial book value of firm assets (A_0),
- 3 The systematic risk of the firm's assets (β),
- 4 The volatility of monthly market returns (σ_E),
- 5 The volatility of monthly 10-year T-bills (σ_D),
- 6 The covariance between equity and debt returns (σ_{ED}),
- 7 the average leverage (L_{av}),
- 8 the volatility of idiosyncratic shocks (σ_I),
- 9 the volatility of the project's net cash flow (σ),
- 10 the proportional costs incurred in selling assets q_A ,
- 11 the proportional adjustment costs of issuing/retiring debt q_{RC} ,
- 12 the proportional direct costs of external equity financing (q_E),
- 13 the proportional restructuring costs (α),
- 14 the fraction of assets that remains after an asset sale (k),
- 15 the partial loss-offset boundary (κ),
- 16 the growth rate of book assets (g),
- 17 a shift parameter in the net payout ratio estimation (a),
- 18 the asset risk premium (RP_A),
- 19 the loss per dollar of full offset in the case of distress (τ_K),
- 20 the marginal corporate tax rate (τ_C),
- 21 the marginal personal tax rate on dividends (τ_d),
- 22 the marginal personal tax rate on interest income (τ_i),
- 23 the instantaneous after-tax riskless rate (r).

The optimization is

$$\begin{aligned}
 c^* &= \arg \max_{c, \gamma_U, \gamma_{LU} \in \mathbb{R}_+^3} \frac{E^R(\delta_0) + (1 - q_R C) \cdot D(\delta_0)}{1 - \gamma_U \mathcal{E}_{\delta_0} [e^{-rT_U} | \phi_L^U = 0] - k \gamma_{LU} \mathcal{E} [e^{-rT_{LU}} | \phi_B^{LU} = 0]} \\
 D(\delta_0) &= D^R(\delta_0) + \mathcal{E}_{\delta_0} [e^{-rT_U} D_0 | \phi_L^U = 0] + \mathcal{E}_{\delta_0} [e^{-rT_U} w D_0 | \phi_B^{LU} = 0] \\
 \frac{\delta}{V_t} &= a + (1 - \tau_C) \cdot \frac{c}{V_0} \\
 \frac{\partial E(\delta_t)}{\partial \delta_t} \Big|_{\delta_t = \delta_B} &= 0 \\
 q(x) &= \begin{cases} x & \text{if } k\delta_S > wc \\ (1 + q_E)x & q_E > 0, \text{ otherwise} \end{cases} \\
 E^D(\delta_0) &= E^R(\delta_0) + \mathcal{E}_{\delta_0} [e^{-rT_U} \gamma_U E^D(\delta_0) | \phi_L^U = 0] + \mathcal{E}_{\delta_0} [e^{-rT_{LU}} \gamma_{LU} k E^D(\delta_0) | \phi_B^{LU} = 0] \\
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 E^R(\delta_0) &= \mathcal{E}_{\delta_0} \left[\int_0^{T'} e^{-rs} (1 - \tau) (\delta_S - c) ds \right] \\
 &\quad + \mathcal{E}_{\delta_0} \left[\int_{T_L}^{T''} e^{-rs} q \left((1 - \tau) (k\delta_S - wc) - \tau_I w c \mathbf{1}_{\delta_S < \delta_t} \right) ds \right] \\
 &\quad + \mathcal{E}_{\delta_0} \left[e^{-rT_B} \max \left[(1 - \alpha) \int_{T_B}^{\infty} e^{-rs} k (1 - \tau) \delta_S ds - w D_0, 0 \right] \Big| \phi_{LU}^B = 0 \right] \\
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 &\quad + \mathcal{E}_{\delta_0} [e^{-rT_B} \min \left[(1 - \alpha) \int_{T_B}^{\infty} e^{-rs} k (1 - \tau) \delta_S ds, w D_0 \right] \Big| \phi_{LU}^B = 0]
 \end{aligned}$$

No closed-form solution or comp statics. Only numerical. Not a model for intuition. BUT can use data even for comparative statics that are not unambiguous. Better predictions.

Agree: Great model **if** it predicts best.

Reevaluate the TDF Evidence

- Strebulaev put all eggs into one basket—the TDF model—*a priori*. **Problem is—Wrong Basket**
- TDF predicts inertia.
- The mustered empirical evidence is about non-adjustment.
- Inertia \Rightarrow Non-Adjustment.
- **NOT Non-adjustment \Rightarrow Inertia**
 - Firms randomly perturb their capital structures.
 - They can be very active, but still not readjust.
- My paper shows that there is a lot of activity.
 - Yes, non-adjustment.
 - No, inertia.

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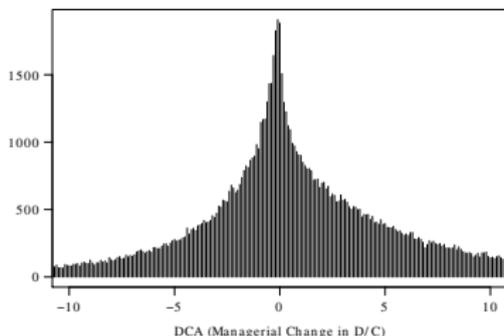
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Managerial Activity

- Take out stock returns (not subject to frictions).
- Prediction: 0 in 17 out of 20 times, 10-50% otherwise.



- Teeny spike is from truncated dist of lvg (0%).
- Managers are incredibly active. And my evidence was conservative (Rauh-Sufi 2010).

More Evidence—Tax-Distress Costs

Omitted due to time:

- Managers do not readjust *at all*. Instead, they add to the volatility of capital structure.

It ain't a slowly moving target

- Managerial leverage changes are *orthogonal* to stock-return induced changes.

"Target" must move mighty strangely.

- All TDF theories fail. Frictions are not important. Readjustment seems highly implausible.

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Deep Structure

- Unobserved Variable $r \neq$ Measured Variable M
- Inverting a behavioral theory (1-5%[?] marginal R^2) to get from r to M usually leaves huge gap.
 - Earlier Example: CAPM FOC vs. actual borrowing costs in the market.
 - Liquidity \Leftrightarrow Productivity Shocks; vs. Introduction of pagers and mobile phones for Fedex, UPS, USPS.

The Cost of Structure

- $r = \text{right}$
- $w = \text{wrong}$
- $y = \text{dependent}$
- $Q = r + w$
- $r \perp w$.

True Model:

$$y = r$$

Researcher falsely believes

$$y = w$$

Deep Structure

- If w is observable, researcher runs

$$y = a + b \cdot w$$

Expected \hat{b} is 0, because $w \perp r$.

- If w is unobservable (structural), and Q is, then researcher relies on reduced form

$$y = a + b \cdot Q$$

Expected \hat{b} is positive. Researcher falsely accepts theory.

- To avoid false inference, we need all other forces to be orthogonal to proxy Q , not to underlying variable w .
- Interpretation of *ceteris paribus* is very different.
- The wider the gap between r and Q , the harder it is to trust inference. w can wedge easier.

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Remedies

- 1 Reduce the gap between r and Q .
 - Analogy = finding a better instrument.
 - Even a lousy direct measured proxy is usually better than a behavioral-theory FOC proxy.
- 2 Increase test stringency.
- 3 Enumerate alternative explanations for Q . If too many plausible ones, don't believe your reduced form is good evidence for your structural model.

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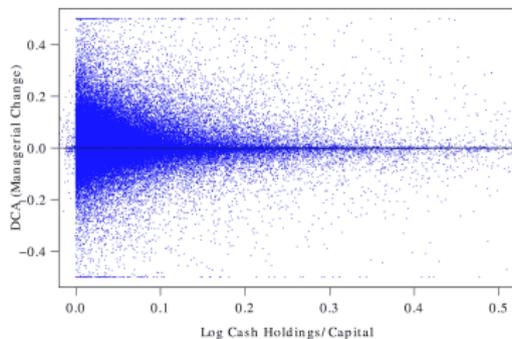
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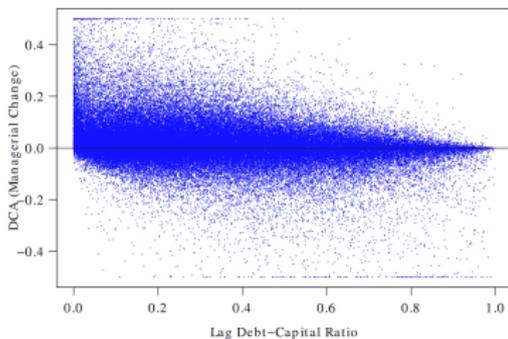
Hennessy-Whited 2005

- Tough theory to understand. Complex. (No closed forms.) But clearly built on good, plausible forces.
- Taxes and underlying profitability (the structural variable) determine the capital structure dynamics.
- Let's make it easy on us—use their stated implications.
-

Liquidity - Mngrl CapStruct Δ



Lagged Lvg - Mngrl CapStruct Δ



Gap And More

- Huge gap between reduced-form and underlying model.
- Not plausible that nothing else drives liquidity +.
- Fit is poor. Not first-order for capital structure.
- HenWhit theory might have marginal power. Just no convincing empirical evidence *anywhere* that it does.
- What about other omitted plausible forces?
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Their Assessment

- Hennessy-Whited 2005:

our theoretical and empirical results underline the importance of understanding corporate financial decisions in dynamic settings, as well as the importance of having a tight connection between theory and empirical work. Given the power of our theoretical and empirical framework to explain observed leverage phenomena, it appears likely that similar success is possible in other areas of corporate finance.

- Strebulaev 2007:

Research that combines these two strands [real cash flow models and capital structure models] is likely to be a fruitful avenue for future research in capital structure, and more generally, corporate finance.

- Too many potential forces ignored. No confidence that we have the right ones. We need to know what the first-order correlations are.
- Too many (ignored) other misspecification issues.
- Existing quantitative and structural test standards have been too low. We need quasi-experimental tests for theories.

Editorializing even further

- It would have been a miracle if these models had worked.

Beyond Corporate Finance

- Macroeconomics, labor, etc., have similar problems.
 - Angrist-Pischke and Caballero make similar points.
- Asset Pricing:
 - Arbitrage can identify correct mechanism. Good in derivatives. Miserable in equities (similar problems).
 - Out-of-sample tests have been more common.
 - Higher data frequency and less survivorship data problems.
 - No QE evidence. Reluctance to measure constructs (e.g., habit) directly. ⇒ Be more skeptical.

Conclusion

- Current quantitative and deep structure models have failed empirically—badly.
- Managerial capital structure activity remains a mystery.
OPINION. WE NEED
 - First Order Effects (Correlations). Theory-informed.
 - Quasi-Experimental Evidence
- **I have little hope that this will change. The setting is too complex and mechanisms are not well understood.**
 - Quantitative and deep structural models can be more useful if they explain the data better.
 - I would love to be proven wrong, i.e., that such models could help explain the data.

Reader—Keep Perspective

- A critique paper is not unbiased.
- It is a whole lot easier to critique than to innovate.
- The papers were excellent and worthwhile attempts, but I think we should now have learned that this approach has failed and is not likely to succeed.

The literature should go a different route in the future.

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