Structural Estimation Drawbacks
FMA

Ivo Welch

Oct 2011

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

- I want to challenge your perceptions.
- I want to provoke you at least a little.
- I want you to remember this seminar.
- The paper is (just a little) tamer.
Lots of Progress over the last 50 years.

- Predictive Ability (Friedman, 1966)
- Deeper Structural Modeling (Lucas, 1976)
- Quantitative Evaluation (Mehra-Prescott, 1985)
- **BIG BREAK** (time and area)
- Quasi-Experimental Studies (Angrist-Pischke, 2008; but many predecessors, such as event studies, Leamer, 1983, etc.)

Not covered: model esthetics (e.g., rational expectations, no first-order irrationalities, etc.) Modeling is both art and science. This version has very little on the subject.
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What is Top-Notch Economics?

- As a profession, we like “hard science.” Physics-like.
- Tight models. Tight predictions.
  - Show off how complex the models are that we can handle.
  - How well dressed is the emperor?
    - In some cases, well. In others not so.
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Plan

- Talk About a Specific Research Area (CF). Describe the Challenges. **SAME ISSUES ELSEWHERE**

  - How good are current top models in this area?
    - Look at a top Quant Model
    - Look at a top Structural Model

  At the end, you can state “while these are not good models, this does not mean that there could not be good one.”

  - Agreed,
  - ...but where are the good models?
  - Personally, I think the task is too difficult,
  - Please prove me wrong!
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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.
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)
Research Challenges in CF

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Let’s focus on one subarea 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.

Variations of my points apply to almost all papers in the genre and beyond, reaching as far as asset pricing and economics in general.

However, to describe failures, I need specific models.
Capital Structure—Possible Causes

- Distress Costs (e.g., Robichek-Myers 1966)
- Covenant Violations (Roberts Sufi 2009)
- Taxes (e.g., Graham 1996)
- Frictions (Fischer-Heinkel-Zechner)
- Adverse Selection (Myers-Majluf 1984)
- Risk Shifting (Parrino-Weisbach 1999)
- Unmitigated agency concerns
- ESOPs and acquisitions (Fama-French 2002)
- Clientele and credit ratings (e.g., Kisgen 2006)
- Tangibility (Rampini-Vishwanathan 2010)
- Stock Returns (Welch 2004)
- Market Timing (Baker-Wurgler 2002)
- Consensus Concerns (Dittmar-Thakor 2007)
- Acquisitions (Fama-French 2002)
- Industry peers (Welch 2004, Roberts-Leary 2009)
- Pension liabilities and industry (Shivdasani-Stefanescu 2010)
- Projects (DeAngelo-DeAngelo-Stulz 2010, DeAngelo-DeAngelo-Whited 2011?, Hennesy-Whited 2005, Shivdasani-Stefanescu 2009, etc.)
- Managerial identity (Bertrand-Schoar 2003)
- Crowding out by government (Greenwood-Hanson-Stein 2010)
- Precommitments (sinking funds)
- Hubris (Roll 1986)
- Investment Banking Conflicts
- Non-optimal behavior?
  - when heuristic band is violated?
  - asleep at switch? — PS: what would it take to convince you of this?
  - random? (I-bank influence)?

Not exhaustive or exclusive. Many papers presented favorable evidence.
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.
  - Great for quick assessment of what correlates highly.

**WARNING:** Leverage ratios are not normally or standard Tobit distributed (or handleable by subsets).
  - Need “placebo” evolution to test out estimators.
  - Good placebos are now becoming available.
  - I have sinned myself in past papers here, too.

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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 the economics itself 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 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”
Critique

How good is the approach to date (in CF)?

- Prima-Facie Evidence in CF:
  - Increasing Market Share.
  - Many Awards (Brattle prizes).

- Quasi-dominant Paradigm in AP.
Critique

- How good is the approach to date (in CF)?

- Prima-Facie Evidence in CF:
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- Quasi-dominant Paradigm in AP.
Do existing published structural papers address above collection of issues?
- **Omitted forces** and econometric challenges?

If not, do you consider the omissions problematic, *a priori*?
Challenges

Name me one structural model (in CF [and beyond]) that predicts well *out of sample*. This was the whole point of the Lucas critique.

Name me one structural model (in CF [and beyond]) that predicts well *in a quasi-experiment*. This was the whole point of the Lucas critique.
Name me one structural model (in CF [and beyond]) that predicts well *out of sample*. This was the whole point of the Lucas critique.

Name me one structural model (in CF [and beyond]) that predicts well *in a quasi-experiment*. This was the whole point of the Lucas critique.
Absent better models, I will have to take shots at the best models if I want to curb the blind infatuation with the approach.

- 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.

The specific models critiqued does not mean that future models cannot do better, in principle...perhaps.

- I doubt it, but intelligent people can disagree.

One contribution of this paper—point out where we should get better.
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One contribution of this paper—point out where we should get better.
1. Ignored too many plausible forces based on priors.

2. Huge gap between unobservable mdl and measured tst.
   - Many models map into same reduced-form evidence.
   - Even orthogonal forces to mdl distort the inference.
   - Needless—direct proxies seem better than FOC proxies.
     - Don’t use the CAPM and a distress model to tell you the cost of corporate borrowing.
     - 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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Big Structural Break

Advance warning:

- The papers that I will critique were great attempts at the time.
- The issues in them are not unique to them, either.

My critique is about where we should head in the future.
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Strebulaev 2007

- 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 t=2 sketch model, too.)
- Hallmark: Quantitative dynamics (not deep structure)
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- Hallmark: **Quantitative dynamics** (not deep structure)
PV of all future net payouts at time 0 ($V_0$).
The initial book value of firm assets ($A_0$),
The systematic risk of the firm’s assets ($\beta$),
The volatility of monthly market returns ($\sigma_E$),
The volatility of monthly 10-year T-bills ($\sigma_D$),
The covariance between equity and debt returns ($\sigma_{ED}$),
the average leverage ($L_{av}$),
the volatility of idiosyncratic shocks ($\sigma_I$),
the volatility of the project’s net cash flow ($\sigma$),
the proportional costs incurred in selling assets $q_A$,
the proportional adjustment costs of issuing/retiring debt $q_{RC}$,
the proportional direct costs of external equity financing ($q_E$),
the proportional restructuring costs ($\alpha$),
the fraction of assets that remains after an asset sale ($k$),
the partial loss-offset boundary ($\kappa$),
the growth rate of book assets ($g$),
a shift parameter in the net payout ratio estimation ($a$),
the asset risk premium ($RP_A$),
the loss per dollar of full offset in the case of distress ($\tau_K$),
the marginal corporate tax rate ($\tau_C$),
the marginal personal tax rate on dividends ($\tau_d$),
the marginal personal tax rate on interest income ($\tau_i$),
the instantaneous after-tax riskless rate ($r$).
The optimization is

\[
\begin{align*}
  c^* &= \arg \max_{c, \gamma_U, \gamma_{LU} \in \mathbb{R}_+^3} \quad c \cdot \gamma^{U} \cdot \gamma^{LU} \in \mathbb{R}_+^{3+} \\
  &\quad \frac{E^R(\delta_0) + (1-q_RC) \cdot D(\delta_0)}{1-\gamma^{U} \cdot \gamma^{LU} \cdot \gamma^{\phi_B(LU)=0} - k \cdot \gamma^{LU} \cdot \gamma^{\phi_B(LU)} = 0} \\
  D(\delta_0) &= D^R(\delta_0) + \gamma^{U} \gamma^{\phi_B(LU)} D(\delta_0) = 0 + \gamma^{LU} \gamma^{\phi_B(LU)} D(\delta_0) = 0 \\
  \frac{\partial E(\delta)}{\partial \delta} |_{\delta = \delta_B} &= 0 \\
  q(x) &= \begin{cases} 
    x & \text{if } k \cdot \delta_S > wc \\
    (1 + q_E) x & \text{if } q_E > 0, \text{ otherwise}
  \end{cases} \\
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  &\quad + \gamma^{LU} \gamma^{\phi_B(LU)} \left[ \int_0^{T'} e^{-r \cdot (1-\tau)(k \cdot \delta_S - wc) - \tau w c (\delta_S < \delta_t)} ds \right] \\
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\end{align*}
\]

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.
Strebulaev put all eggs into one basket *a priori*,

The TDF model

- TDF predicts inertia.
  - Frictions explain inertia.
- Inertia $\Rightarrow$ Non-Adjustment.

**BUT NOT:** Non-adjustment $\Rightarrow$ Inertia
  - Firms could randomly perturb capital structures.
  - They could be very active, but still not readjust.
  - Frictions are inconsistent with activity.
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Theory: Inertia vs. Non-Adjustment

Inertia

Non-Adjustment
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The Problem is—the basket has no bottom.

- All mustered empirical evidence is about non-adjustment.
- I will show that there is a lot of activity.
  - Yes, non-adjustment.
  - No, inertia.

(I am partly to blame. In 2003, I thought firm behavior was inertia. I discovered I was wrong during the revision to my 2004 JPE paper, and fixed it all.)
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If you are interested in determinants of managerial capital structure activity, (often) use dca, not dct!

Avoids noise and stock-market anomalies (such as B/M).

“Conservative”: You might want to lump more into “passive.”
Take out stock returns (not subject to frictions).
Prediction: 0 in 17 out of 20 times, 10-50% otherwise.
Cannot be normally distributed, of course.
Yes—spiky at zero. from truncated dist of lvg (0%).
No—mode is zero, but lots of activity at, say, −3% and +3%.
Compare to typical *theoretical* magnitudes (> 10%)

(Next draft—use just long-term debt for predetermined.)
(Stock-return induced change dcp adds to weight left of zero.)
Managers are incredibly active. And my evidence was conservative (Rauh-Sufi 2010).
Prediction: dca counteracts dcp

<table>
<thead>
<tr>
<th>Equation</th>
<th>Mean</th>
<th>Sdv</th>
</tr>
</thead>
<tbody>
<tr>
<td>( dct_{t-1,t} = \frac{D_t}{D_t+E_t} - \frac{D_{t-1}}{D_{t-1}+E_{t-1}} )</td>
<td>1.15</td>
<td>12.9</td>
</tr>
<tr>
<td>( dcp_{t-1,t} = \frac{D_{t-1}}{D_{t-1}+E_{t-1} \cdot (1+x_{t-1,t})} - \frac{D_{t-1}}{D_{t-1}+E_{t-1}} )</td>
<td>0.17</td>
<td>9.3</td>
</tr>
<tr>
<td>( dca_{t-1,t} = \frac{D_t}{D_t+E_t} - \frac{D_{t-1}}{D_{t-1}+E_{t-1} \cdot (1+x_{t-1,t})} )</td>
<td>0.97</td>
<td>8.7</td>
</tr>
<tr>
<td>( dca^{+} ) (with divs)</td>
<td>1.26</td>
<td>8.7</td>
</tr>
</tbody>
</table>

Units are “in percent per single year” here. \( x \) is pct capital gain. Winsorized at \(|0.5|\) gives identical results.

- \( \text{sd}(dct) \nless \text{sd}(dcp) ! \)
- dca (active) \( \perp \) dcp (passive).
  \( \text{var}(dct) \approx \text{var}(dca) + \text{var}(dcp) \) \( (\sqrt{2} \cdot 9 \approx 12.7) \)
- What are managers thinking??
- Note: no misspecification tests. mea culpa. (focus is noise.)
Evidence: dca against Stock Returns

Capital Structure Change (dct)

Stock-Return Induced (dcp)

Managerial Net Response (dca)

Managerial Net Response (dca$^+$ [with div])
Evidence: Pruned Managerial Net Response (dca⁺);

- Managers do not undo shocks. (“consistent” with TDF)
- No evidence of inertia. (not consistent with TDF)
Managers do not readjust \textit{at all}. Instead, they add to the volatility of capital structure.

\textit{It ain’t a slowly moving target}

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

\textit{If there were a target, it must move mighty strangely.}

All TDF theories fail. Frictions are not important.
Readjustment seems highly implausible.
It’s not just that noise hides readj, though dct has a lot.
Complex estimators can obscure simple truths.
This is not to say either that managers optimize something—they probably do—or that some variables may have large coefficients.
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Wish I could get to first base...

- Unfortunately, we only got to first base, theory.
- We struck out before we reached second base, IS performance.
- Thus, we never got to try to run to third base, OOS performance.
- And we definitely never scored, QE performance.
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.

Your theory is about the impact of financing costs on capital structure.

Earlier Example: CAPM FOC vs. actual borrowing costs in the market.

Your theory is about the impact of productivity shocks on capital structure.
Liquidity ⇔ Productivity Shocks; vs. Introduction of pagers and mobile phones for Fedex, UPS, USPS.

Your theory is about habit impact on asset prices.
Measure habit (how?) and aggregate, vs. infer it from poor consumption to return links. Can individual habit behavior explain aggregate habit??? If not, what is this theory's base? How did we get from poor consumption-return links to inference of habit of an aggregate consumer?
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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

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y = a + b \cdot Q
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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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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—we use their stated implications.

Not Direct Tests—Heuristic Assessments. I will not attempt to reject the model.
We highlight the main empirical implications.

First, absent any invocation of market timing or adverse selection premia, the model generates a negative relationship between leverage and lagged measures of liquidity, consistent with the evidence in Titman and Wessels (1988), Rajan and Zingales (1995), and Fama and French (2002).

Second, even though the model features single-period debt, leverage exhibits hysteresis, in that firms with high lagged debt use more debt than otherwise identical firms. This is because firms with high lagged debt are more likely to find themselves at the debt versus external equity margin.
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- Unobs. Productivity ($r$) drives liquidity ($M$) and capstruct ($y$)

- Patrick Bolton pointed out that it’s really what it says:
  
  $\textit{cash flows go up } \Rightarrow \textit{debt goes down}.$

- Huh?

- But, this is predicted by any theory in which managers use some extra cash flows to take down debt, whether there are any productivity shocks or not.

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Contemp Operating Net Cash Flow

\[ b = -0.060 \ ( -34.22), \quad R^2 = 1.1457\% \]
$b = -0.003 \ (\text{1.60}), \ R^2 = 0.0016\%$
Contemp Cash Changes

\[ b = 0.012 \ (4.21), \ R^2 = 0.0114\% \]
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$b = -0.050 \quad (-82.99), \quad R^2 = 3.7\%$

Ignore selection bias.
$b = 0.002 \ (0.71), \ R^2 = 0.0003\%$
Huge gap between reduced-form and underlying model.

Not plausible that nothing else drives liquidity plus. If anything, non-productivity shock related influences on measured liquidity seem bigger than productivity shock related influences.

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?

What about econometric misspecifications?

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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.*
My Assessment

- 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.
Don’t many papers in corporate finance that are not structural and not quantitative share the same weaknesses??

Absolutely. Many do.

No paper is perfect. The best non-structural non-quantitative papers are better, though.
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Macroeconomics, labor, etc., have similar problems.
- Angrist-Pischke and Caballero make some 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.
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. 
  “*more pictures of managerial activity*”

- Quasi-Experimental Evidence

I have little hope that this will change soon. setting is too complex and motives are poorly understood/measured.

- 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 behavior—in QE settings!
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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