Aims and Objectives

This PhD course considers a number of topics that are at the center of ongoing research in corporate finance. The course builds on the financial first-year sequence, in particular on Corporate Finance Theory and Empirical Issues in Finance. In the first part of the course, we will cover dynamic corporate finance theory, specifically dynamic contingent-claim models, as well as several empirical topics, including endogeneity, cross-sectional methods, and structural estimation. In the second part of the course, we will cover topics depending on students’ interests.

I expect students to begin transforming themselves right from the start of the course, from being just “students”, who study and learn new material, to being “active researchers”, who come up with new ideas and are able to put these ideas into models and test them empirically. Thus, this is mostly a research-oriented course. This has several important implications. First, dynamic corporate finance theory requires substantial mathematical background. We will not have time to cover most of the background and knowledge of this will be pre-supposed (this material is given in other GSB and Stanford courses, and reading material is given in Session 1 description). More importantly, while there are a lot of subtle technical and mathematical issues, related to construction of models and solution methods, we will approach the issues rather informally and will intentionally skip over various technical problems. (At the same time, there are various mathematical “roadblocks” which slow down the development of these methods and they will be carefully identified – those with excellent mathematical background are encouraged to overcome this roadblocks).

Second, the course will include a number of projects instead of home assignments. There will be three different projects. (1) Each student (or most students, depending on the number of students in the class) will prepare a review and a presentation on a particular topic, and all students are expected to present at least once. (2) Each student will choose between (a) writing a referee report on one of the several suggested papers or (b) replicating the results of an empirical research study (of course, you can do both!).

(3) Finally, and most importantly, there will be a research project, either a theory-oriented or empirical that students should do to be successful in the course. Students may choose
topics for these projects from the suggested list; alternatively, they are welcome to come up with their own topics (subject to my approval). The outcome of each project should include a written paper that contains, at the very minimum, an intuitive and broad description of the topic/problem/idea, its importance and relevance relative to the field, a critical review of related literature, results and standing issues. In addition, a theory paper is expected to include the development of a fully-fledged model addressing one of the issues; an empirical paper is expected to include the description of the empirical hypotheses, data set and sample selection, empirical tests/methods, and results that have been/are expected to be obtained. Ideally, for those students who would like to concentrate on corporate finance, these projects should become a paper publishable in a top journal. (this course has quite good history: at least 3 papers which started as projects for this course have been published in top journals over the last three years). Students are expected to present the results of their projects (subject to time constraints).

Format and Teaching Methods
The first part of the course consists of methodological issues and new topics in modern corporate finance (both theory and empirics). The remainder of the course consists of discussions and student presentations of chosen topics and projects. Students will choose discussion topics from the suggested list and therefore the exact content of the course depends on the students’ preferences.

Student Responsibilities
The course is very intensive and will require students to prepare carefully for all classes, read, understand, and think deeply about a lot of materials, and participate actively in class discussion. Constructive class participation is very important and constitutes a significant portion of the grade. Readings are important for following the class discussion. Students are expected to devote a substantial amount of time to the course in addition to attending classes.

Regular class attendance is essential. Should a student be required to be absent, notification to Ilya, by e-mail, is required.

Assessment
Class participation 20%
Critical literature review 20%
Referee report or replication 20%
Research project 40%
Readings

There will be a number of readings that students should read before the class. These readings are marked with a start (★).


All remaining Lists (for exercises and research projects) will be given to you on the first day of the course.

Contacts

These are the contact details until April 11.

e-mail: istrebulaev@stanford.edu

Office hours: Any time (almost), by prior arrangement. Please stop by!

Assistant: Sandra Berg. Office: L318, phone: 723-4494
e-mail: berg_sandra@gsb.stanford.edu
Important Dates

Literature Review Deadlines

- APRIL 14 2011 Choose a topic from the List of Topics for Advanced Study and confirm your choice to Ilya by e-mail.

- MAY 4 2011: Submit the literature review on the chosen topic to Ilya; be prepared to get Revise & Resubmit. Be prepared to present the results of your critical assessment to the class.

Referee Report/Replication Deadlines

- APRIL 21 2011 Choose a paper to referee from the List of Papers to Referee and confirm your choice to Ilya by e-mail. Alternatively, choose a paper from the List of Suggested Papers for Replication and confirm your choice to Ilya by e-mail.

- MAY 12 2011: Submit the referee report/replication project to Ilya.

Research Project Deadlines

- APRIL 28 2011 Choose a research project from the List of Research Projects (or suggest your own), discuss it with Ilya, and confirm your choice to Ilya.

- MAY 18 2011 Submit a detailed outline of your plan for how you are going to do the project (as well as a report of what you’ve done up to now) to Ilya, and then schedule a meeting with him to discuss progress.

- JUNE 20 2011 Submit the final project to Ilya.
Referee Report Guidelines


There are two roles of a referee report. First, it is to give advice to an editor whether to reject or invite to resubmit/accept the paper to a journal. When you start writing your own papers (hopefully soon!) you will send them one day to the best journals in the field and the editor will send your paper to between one and three referees who would make a recommendation. Second, it is to give advice to the authors on how to improve the paper. It usually contains the description of assumptions/results which are wrong/not important/need to be redone/checked for robustness/generalized. It also contains an overall impression of the paper, its importance and relation to the field. The exact nature of what the referee writes to the authors depends on whether the referee wants the paper to be resubmitted or rejected.

Why do I ask you to do it:

- One day you are likely to become a referee for a top journal and it is good if you have some prior experience.
- Refereeing makes you think critically about somebody else’s work and look at research from a different perspective.

The quality of referee reports varies [based on my own experience and that of my colleagues]. What I want from you here is for you to write an ideal referee report, which I define myself as follows: (1) clear judgement on what you like/dislike in the paper and why; (2) clear advice on whether you think the paper should be accepted (with or without major revisions) by a top journal; (3) clear summary of what the authors should do to make the paper more interesting/important/novel relative to the field; (4) clear identification of problems and whether/how they can be solved; (5) in case you recommend a revision, a clear identification of items that you would like to see re-done/improved/thought about (but not asking the impossible). You may be surprised, but I received referee reports like that in my (so far) short research career!

In addition, I would like you to investigate to what extent the model is robust to a variety of changes in assumptions (this includes solving the model for various cases).

Important: None of the papers in the List of Papers to Referee have (to my knowledge) been accepted for a publication as yet. Therefore:

- If you have any questions about the paper or you do not understand something about the paper, you may write an e-mail to one of the authors (you can say that the paper was
given for you to study and provide a report/feedback as a part of this course). Finance professors are usually busy people so try to put all questions you have in one e-mail. If you do not get an answer within a reasonable period, let me know and I can send questions on your behalf.¹

- If the referee report contains constructive comments on how to improve the paper, I will send the report (or a part of it) to the authors on your behalf (only with your permission, of course).

A referee report usually (but not always) consists of several parts:

- **Introduction.** The first couple of paragraphs contain a short abstract of the paper that summarizes, *in your own opinion* and *in your own language*, the main research question and results of the paper (as the authors claim them). It is of no use to replicate the paper’s abstract – the editor can read it herself.

- **Overall view of the paper.** Overall thoughts on the paper’s importance/novelty/relationship to other papers in the field. What you like in the paper. What major concerns you have. If you are fairly negative on the paper, justify it.

- **Main part: how to improve the paper.** This is a main part of the report. It contains all your suggestions on how to improve the paper/questions you have for the authors to explore more/problems you have with the paper. You do not need to answer questions, but you have to ask them in a constructive way (if the paper is deficient in something that is not rectifiable this is better put in the overall view of the paper).

- **Recommendation.** Usually referees write a separate letter to the editor (which the authors do not see) in which they summarize their thoughts, offer some comments they would like only the editor to hear, and make their recommendation. Include this part at the end of your referee report, for this exercise.

**Length of the report:** Most (good) reports are between 2 and 7 pages. Try to be concise (do not go over 10 pages unless you absolutely must). In addition, you can add an Appendix with the solution of robustness check models you investigated.

¹I am sure I don’t need to say this, but remember that it helps to be polite and positive (even if you think there are some problems in the paper).
List of papers to referee

I selected several papers that satisfy all of the following criteria: (1) theoretical contribution; (2) to the best of my knowledge, these papers have not been published as of now; (3) They are relatively recent (it also means that the authors are still actively engaged in working on these projects); (3) I think the papers are interesting and deserve a closer look and discussion.

You have to choose one paper from this list [I will restrict the number of students working on one paper so choose several in case you first choice is not available]


Literature review/discussion guidelines

You should choose a topic from the list (choose an alternative in case your first choice is not available). You can also suggest your topic to Ilya.

Your literature review should:

1. Contain a concise and critical assessment of the research question and the field.

2. Say, in particular, what has been done, how it has been done and what is not done/important to be done, both theoretically and empirically.

3. Avoid giving abstracts of each paper separately: your task is to think about the overall state of research and link all these papers.

4. Give a brief introduction to the field (choose yourself the level of technicality and depth).

5. Contain research ideas in as much detail as possible, as well as explaining how working on them would improve our knowledge of the field.

Optimal length of the review. Be as concise as possible. 10-15 pages is the ideal length. Your review must not be over 20 pages (excluding figures, tables, and references).

You (or some of you) will be asked to present the results to the class. Here are the guidelines:

1. The exact timing will be decided later but most likely we will have 45-60 minutes for each topic. You should plan on giving a short presentation of about 25 minutes, leaving the remaining time for discussion.

2. One week before the discussion you should identify two-three papers that you would like everybody to read and e-mail the list to everybody.

3. Briefly identify the field and what it is about. Indicate major papers and major results in the field.

4. Discuss the field critically, based on your review. Have in mind that most of your classmates will not read all the papers assigned to you.

5. Be ready to answer questions.

6. Give an overview of research ideas and what are the most important questions still to be done.
IMPORTANT!

For all other topics (on which you do not present), you should read all the papers identified by a presenter and look through other papers mentioned in the report.
“Replication” project

Choose one paper from the list of suggested empirical projects. All these papers have been published in top journals. All these papers are empirical corporate papers that use a data set that is publicly available (for Stanford students) so that you are asked to replicate smartly many results in the paper. By “smart replication” I mean the following:

1. Produce all the results in the tables I asked you to replicate. Describe in sufficient detail how you did it (your data set, your sample, restrictions on the sample).

2. If you can reproduce all results very closely, go to the next item. If you fail to reproduce the results, you should do your best to understand why. There are many reasons for this, some of which may be: (1) the paper does not provide sufficient information about how it got the final sample or its econometric procedure. In this case you should try to understand how the paper got the results (working backwards from the results to the procedures/sample); (2) there is a mistake in the paper: not impossible but not likely so please make sure you identify, check, and recheck any supposed mistake. Make sure you explain why your results are different from the authors’.

3. Ask yourself a number of critical open-ended questions and answer at least some of them (and report them in your piece). Here is a partial list: How robust are the authors’ results? Do you agree with their methodology? How important are their results? Any other tests you can think of (on this data set (then do it!) or other data sets). Describe the data set. Do you agree with the authors’ econometric procedure (in particular, based on what you have learnt in the course)? How to improve it? What is the relationship to the rest of the field? Any research ideas you developed while working on this project?

4. Using more recent data (whenever possible), extend the results. Has anything changed? Would you expect any results to change?

Length of the report: An ideal length would be 5-10 pages. The report must not be more than 15 pages (excluding tables). Do not give the computer codes (I will not read them) or software-related information (apart from very basic information).
List of papers for replication project

I selected well-known empirical corporate finance papers from several related fields. One important point is that most of these papers use standard (in some cases modified) cross-sectional methodology. In addition to all the questions that follow these papers below, I would like you to think seriously about all the empirical problems/methods/solutions we have been talking/reading about. In doing so, try to come up with the identification of major problems (if any) and plausible solutions. For example, endogeneity/simultaneity problem is pervasive in corporate finance studies. In other fields of economics (see e.g. references for Class 4) many ingenious solutions were proposed. Can any of them be applied to these problems?

Most importantly, keep in mind that most successful research is built on the shoulders of past research. It is very likely that I have not outlined the most interesting question about the papers below that can lead to new striking empirical results. Keep your eyes wide open.


A well-known and frequently cited paper both on cash policy and on financial constraints. Cash decisions are very important and cash policy has not been completely understood by financial economists. Financial constraints have been understood perhaps even less. The findings of the paper are interesting and important. To what extent are these findings robust?

(a) Replicate Tables I–IV and VI both on their data set (both their data period and the period available now) and on your chosen sample.

(b) To what extent do the authors’ results depend on the definition of financially constrained firms? Would the results change if you vary these definitions?

(c) To what extent are the authors’ definitions of financial constraints robust? First, come up with an economic justification of all of their definitions. What are the main drawbacks of each definition. Any other measures/methods that could classify firms with respect to “the extent of financial constraints”? [Note that the issue of financial constraints is by itself an important question and has been studied separately in a number of studies]

(d) What are the main potential problems with the baseline model is Equation (8). Does the IV model in Equation (9) resolve these problems?
(e) When talking about $Q$, theoretical research usually means marginal $Q$. Empirical research in most cases uses average $Q$. In addition, $Q$ could be measured incorrectly which can substantially complicate the problem. Does this apply to the authors’ results. How can this issue be addressed?

(f) Analyze the authors’ empirical methodology. Econometric specification, robustness, endogeneity.

(g) What are the sources of changes in cash levels? Does it matter for the present research?

(h) What are other theoretical motivations for firms to keep and change cash levels? How to disentangle various theories?


An influential paper that is often cited in capital structure, contracting, and signalling literature when empirical evidence on maturity choice is needed. Maturity decisions are important in practice and the issue is still largely unresolved theoretically. To what extent are their findings robust?

(a) Replicate Tables I–IV both on their data set (both their data period and the period available now) and on your chosen sample.

(b) Decompose debt maturities differently. In particular, COMPUSTAT allows to consider long-term debt maturing within one year and short-term debt.

(c) Is there any way to separate the effect of private/public debt [can it be that the effects they find are driven by public/private rather than by short/long]?

(d) Analyze their empirical methodology. Econometric specification, robustness, endogeneity.

(e) Other proxies that can be considered? [For example: firm age, tangibility]

(f) Is the ratio of long-term debt to debt (their dependent variable) a good proxy? Any other suggestions?

(g) Any other ways to test the implications of Diamond (1991, 1993)?

(h) One important limitation of their analysis is that they analyze the levels of maturity: leverage levels reflect past financing decisions as we know by now. A better procedure would be to analyze the joint decision of marginal changes in debt (debt issuance) and debt maturity choice.

(i) Related to above, what could we say about the relation between leverage and maturity choices?

The priority structure of corporate debt has been important in theoretical research. Empirical evidence is rare/mixed, and this is one of early and influential papers. To what extent are the findings of the paper robust? Can the analysis be extended to cover some new predictions on the priority structure?

(a) Replicate Tables II-IV, VI-VII both on their data set (both their data period and the period available now) and on your chosen sample.

(b) Is there any way to separate the effect of private/public debt [can it be that the effects they find are driven by public/private rather than by senior/junior]?

(c) Analyze their empirical methodology. Econometric specification, robustness, endogeneity.

(d) Other proxies that can be considered? [For example: firm age, tangibility]

(e) One important limitation of their analysis is that they analyze the firm level of seniority: leverage levels reflect past financing decisions as we know by now. A better procedure would be to analyze the joint decision of marginal changes in debt (debt issuance) and seniority choice (given the seniority structure of existing debt).

(f) Related to above, what could we say about the relation between leverage and seniority choices.

(g) Search for recent theory studies on the priority structure/seniority of corporate debt. Derive empirical implications from those studies.

(h) Related studies investigate covenants of corporate debt (e.g. Roberts and Bradley (2003)). Draw relation between these studies and the priority structure.


This is a paper that synthesizes cross-sectional research in capital structure and payout policy.

(a) Replicate Tables I-V both on their data set (both their data period and the period available now) and on your chosen sample.

(b) Firms change leverage infrequently. Strebulaev (2007) showed that cross-sectional tests lead to misleading results e.g. with respect to profitability. What about other explanatory variables that these authors use?

(c) And how does this critique affect dividend regressions?
(d) What would be your solution to the above-mentioned problems? Implement it.
(e) Analyze their empirical methodology (apart from the discussion above). Econometric specification, robustness, endogeneity. What would be your econometric approach to study related questions?
(f) The authors avoid investigating the issue of standard errors. One possibility why standard errors can be overblown in such regressions is the presence of common shocks in the cross-section. What would be your approach to estimating reliable standard errors?
(g) One innovation in the paper is that it recognizes that leverage and payout policies are dependent. What then is the basic message of the paper: are these policies found to be dependent? How would you approach this problem.
(h) A number of firms are zero-levered and they create mass point at the extreme of available leverage choices. How would you control for it and does it change any of the results?


A well-known and often-cited paper that analyzes firms’ cash policy. A number of interesting results. To what extent are they robust?

(a) Replicate Tables 1–5 and 9–11, as well as Figures 2 and 3 both on their data set (both their data period and the period available now) and on your chosen sample.
(b) Think carefully about all their definitions. Compare them with similar variables used in other studies. Any differences? How do results change if you modify various variables? What is the economic justification of using any particular variables?
(c) A number of firms are zero-levered and they create mass point at the extreme of available leverage choices. How would you control for it and does it change any of the results?
(d) Their result on the relationship between cash policy and leverage (Table 4): would it be consistent with models of both optimal cash policy and optimal leverage? How could you modify/check this result?
(e) The authors use different empirical methods including fixed-effects regressions. When you replicate your results, do you get similar results for various methods? What is your explanations of differences between cross-sectional and fixed effects regressions? Any possibility to substantiate these hypotheses?
(f) Firms change leverage infrequently. What about cash policy? Is the critique of capital structure regressions in Strebulaev (2007) applied here?

(g) Analyze their empirical methodology (apart from the discussion above). Econometric specification, robustness, endogeneity. What would be your econometric approach to study related questions?

(h) How would you analyze the question of what firms do with excess cash?

(i) Any other variables that could be important in determining cash policy? How would you measure them? What effect would you expect?


One of the first papers on structural estimation in corporate finance. Their 2007 paper is a continuation and can be considered as well. Great many interesting results and methodological contributions. If you choose this paper, your task is more difficult (but also more exciting?): there are no specific questions – replicate all the results and using the free flow of your research creativity, suggest future developments, problems, uncovered puzzles, etc.
**Research project guidelines**

Unlike for the other exercises/projects for this course, I do not give you a lot of guidelines here. Ideally, these projects of yours (or at least one of them) should be on the track to become an academic paper publishable in a top journal. You have read and will read a lot of good papers to enable you to understand what it means by a paper to be good.

At the very minimum, your research projects should each contain a very detailed outline of the project: literature review, detailed discussion of your research ideas, the difference between what you are proposing and what has been done already, and how to do it (for empirical projects: the list of conjectures, data sets you would need and how to get the data/collect the sample).

At the maximum, I would like to see your research projects published in a top journal soon and have a non-trivial impact on the field!
# Summary of Class Schedule

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<tr>
<td>April 6 2011</td>
<td>2</td>
<td>Ilya</td>
<td>Dynamic Capital Structure.</td>
</tr>
<tr>
<td>April 13 2011</td>
<td>3</td>
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<td>Industry Competition. The role of uncertainty and information asymmetry</td>
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Also [potentially to be discussed on some of the days or after the course]: discussion of replication/referee reports/literature reviews/overall feedback

We are meeting in M107 (Knight Management Center) 8.15-11.45am. **Note: on June 1st, the meeting time is 8-11am**
Session 1
Setting the scene. Real options

Day: March 30

Topic of lecture and discussion

Setting the scene

1. Introduction/logistics. My expectations from students/what students can expect from me.
2. Survey of students: background, interests.
3. What will be covered in the course. Dynamic Quantitative Models (DQM)
4. The state of contemporary corporate finance research. Theory and empirics.
5. The list of most important unresolved questions/issues/topics in corporate finance.

Real options

2. Investment timing decision. The case of an individual firm. GBM and discussion of other stochastic processes. Single threshold case.
4. Applications

Readings for class

General pre-requisite reading for Session 1–4.

As mentioned in the intro to the syllabus, we will concentrate on economic intuition rather than technical details of solution methods to dynamic problems. The following two sources provide ample material for background mathematical reading (which includes stochastic processes, solution to SDE, optimal control, optimal stopping times):


Real options


Session 2
Dynamic Capital Structure

Day: April 6

Topic of lecture and discussion

1. Intuition. Trade-off vs other capital structure models.
4. Asset pricing and corporate finance: pricing of corporate debt
5. Unresolved issues and problems.

Readings for class


Session 3
Industry Competition. Uncertainty and Information Asymmetry

Day: April 13

Topic of lecture and discussion

1. Investment under uncertainty and strategic industry competition.
2. Investment under uncertainty in competitive industries
3. Investment when information is asymmetric.
5. Optimal capital structure with information asymmetry: unresolved issues.

Readings for class


⊚ Grenadier, Steven and Andrey Malenko, 2010, Real Options Signaling Games with Applications to Corporate Finance, Journal of Finance.
Session 4
Other finance applications of DCCM. Future research discussion.

Day: April 20

Topic of lecture and discussion

1. Macrofinance. optimal capital structure and business cycles.
2. Optimal capital structure and permanent vs temporary shocks.
4. Managerial preferences and agency issues.
5. Important unresolved issues. Overall discussion.

Readings for class


Session 5
Endogeneity. Cross-sectional issues

Day: April 27

Topic of lecture and discussion

2. Application of DCCM to quasi-experimental approach.
3. Issues with reduced-form cross-sectional and panel empirical methods.

Readings for class

Endogeneity

The best way to appreciate endogeneity issues is to read economic rather than financial applications as the approach has been more developed in applied economics. See below for economic applications. Corporate finance-related papers will be supplied to you later.


Cross-sectional methods


Session 6
Structural Estimation

Day: May 4

Topic of lecture and discussion

1. Neoclassical investment model in discrete time. Differences with DCCM.

2. Corporate finance applications.

Readings for class
