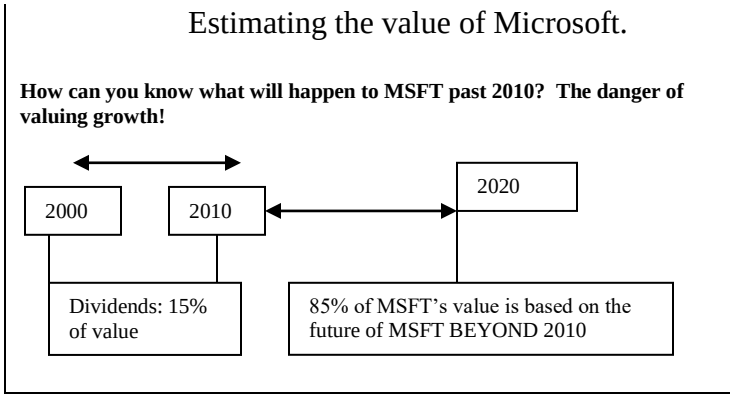
The Dangers of using DCF

The discounted cash flow model (DCF) is the correct way *theoretically* of valuing an asset. The problem with theory is that it faces two problems with reality: estimating cash flows and estimating a discount rate.

Human beings can't forecast, but that doesn't stop them from trying. Read David Dreman's book, *Contrarian Investment Strategies*, on how well analysts' forecast future earnings.

Once you pick a candidate, how do you value this candidate to know what you know? This will be the focus of the next Greenwald Class. Feb. 6th there will be a value investing panel at CIMA.



Since we can't forecast distant cash flows, the DCF leads us astray. Secondly, estimating the equity risk premium is difficult and the Capital Asset Pricing Model *(CAPM)* uses beta which is as flawed as the world is flat hypothesis.

Finally, **the terminal value where 90% of the final value resides in the DCF model is subject to wide swings in values based on tiny changes in assumptions.** It is the "Hubbell Telescope" problem; one slight turn of the telescope and you are looking at a different Galaxy. If one assumes a perpetual growth rate of 5% and a cost of capital of 9% then the terminal multiple is 25xs (or a 4% capitalization rate or 1/.04). However, if we are off by one percent on either or both of our inputs, then the terminal multiple can range from 16x to 50x which is too wide of a range to be useful for valuation.

Using a reverse engineered DCF avoids the need to forecast and avoids anchoring on the current market price.

Calculating IV:

Asset Value
Earnings Power Value.

Simpler, neater and more present based (as opposed to forecast based) methods are better at discovering investments.

CF can also be used with very conservative assumptions to check your assumptions.

Example.

Forecasting Error:

Analysts have no success with long-term forecasts. Analysts do particularly poorly in predicting growth stocks. Based on SG Equity Source: analysts expect growth stocks to generate around 17% pa but the actual delivered growth has been a meager 7% pa on average.

Analysts are prone to larger error on the stocks they are most optimistic about.

As Bruce Greenwald observes, "Profit margins and required investment levels, which are the foundations for cash flow estimates, are equally hard to project accurately into the far future."

Problems with the discount rate.

No one can agree on the equity risk premium (ERP). Beta is flawed and nonsensical. If a price plummets lower, the risk is assumed to have risen due to increased volatility but volatility does not equal risk of permanent capital loss. In fact, lower prices may lessen risk and increase future returns.

Beta is a flawed metric; it doesn't work.
The biggest problem concerns the interaction of the two previous sets of problems of forecasting growth and using a discount rate. When we take out ten-year forecast and then estimate a growth rate from year tens to forever, then capitalizing this via a multiple. The range of values render the valuations almost meaningless.

See Fernandez (2004) Are calculated betas worth anything? (www.ssrn.com).

Reverse engineer DCF

Mauboussin Book

Instead of trying to estimate the growth ten years into the future, this method takes the current share price and backs out what is currently implied. The resulting implied growth estimate can be determined by comparing the estimate with an empirical distribution of the growth rates that have been achieved over time, such as the one shown below. This allows one to assess how likely or otherwise the implied growth rate actual is.

This solves the problem of not being able to forecast the future, but it doesn't take the discount rate problems outlined above.

When you reverse engineer a DCF then you focus on growth potential rather than anchor on the current price.

To get around the DCF problem, you can use Ben Graham's approach of using asset valuation and it represents a liquidation value for the business.

As Graham wrote, "The fits rule in calculating liquidating value is that the liabilities are real but the assets are of questionable value."

Of course, in a fire sale items such as intangibles have no worth at all.

Graham was famous for finding net/nets which took CA and subtracted all liabilities. There is no forecasting of the future involved.

Earnings Power

What the investor wants to learn is the indicated earnings power under the given set of conditions, i.e. what the company might be expected to earn year after year if the business conditions prevailing during the period were to continue unchanged." It combines a statement of actual earnings, shown over a period of year, with a reasonable expectation that these will be approximated in the future, unless extraordinary conditions supervene. The record must over a number of years, first because a continued or repeated performance is always more impressive than a single occurrence, and secondly because the average of a fairly long period will tend to absorb and equalize the sitrorint influences of the business cycle.

Once EPV has been calculated it can either be capitalized at the cost of capital to give an estimate of value or it can be compared to the price to generate a PE of sorts which Graham suggested should be no more than sixteen times because that is as high a price as can be paid in an investment purchase of a common stock…ten times earnings ratio is suitable for the typical case."

Take an average EBIT margin over a reasonable time period of ten years, then multiply this by the average sales over the last ten years. This gives the normalized EBIT. Then subtract interest payments and remove taxes to end up with an estimate of earnings power—all done without any of the messiness of forecasting!

Study the Greenwald book.
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If you haven't read Cargo Cult Science, do it today. And if you have, then do it again.

Here is the article:

"Cargo Cult Science", by Richard Feynman

(Adapted from a Caltech commencement address given in 1974; HTML'ed from the book "Surely You're Joking, Mr. Feynman!")

During the Middle Ages there were all kinds of crazy ideas, such as that a piece of rhinoceros horn would increase potency. Then a method was discovered for separating the ideas -- which was to try one to see if it worked, and if it didn't work, to eliminate it. This method became organized, of course, into science. And it developed very well, so that we are now in the scientific age. It is such a scientific age, in fact, that we have difficulty in understanding how witch doctors could ever have existed, when nothing that they proposed ever really worked -- or very little of it did.

But even today I meet lots of people who sooner or later get me into a conversation about UFO's, or astrology, or some form of mysticism, expanded consciousness, new types of awareness, ESP, and so forth. And I've concluded that it's not a scientific world.

Most people believe so many wonderful things that I decided to investigate why they did. And what has been referred to as my curiosity for investigation has landed me in a difficulty where I found so much junk that I'm overwhelmed. First I started out by investigating various ideas of mysticism and mystic experiences. I went into isolation tanks and got many hours of hallucinations, so I know

something about that. Then I went to Esalen, which is a hotbed of this kind of thought (it's a wonderful place; you should go visit there). Then I became overwhelmed. I didn't realize how MUCH there was.

At Esalen there are some large baths fed by hot springs situated on a ledge about thirty feet above the ocean. One of my most pleasurable experiences has been to sit in one of those baths and watch the waves crashing onto the rocky slope below, to gaze into the clear blue sky above, and to study a beautiful nude as she quietly appears and settles into the bath with me.

One time I sat down in a bath where there was a beautiful girl sitting with a guy who didn't seem to know her. Right away I began thinking, "Gee! How am I gonna get started talking to this beautiful nude woman?"

I'm trying to figure out what to say, when the guy says to her, "I'm, uh, studying massage. Could I practice on you?"

"Sure", she says. They get out of the bath and she lies down on a massage table nearby.

I think to myself, "What a nifty line! I can never think of anything like that!" He starts to rub her big toe. "I think I feel it", he says. "I feel a kind of dent -- is that the pituitary?"

I blurt out, "You're a helluva long way from the pituitary, man!"

They looked at me, horrified -- I had blown my cover -- and said, "It's reflexology!"

I quickly closed my eyes and appeared to be meditating.

That's just an example of the kind of things that overwhelm me. I also looked into extrasensory perception, and PSI phenomena, and the latest craze there was Uri Geller, a man who is supposed to be able to bend keys by rubbing them with his finger. So I went to his hotel room, on his invitation, to see a demonstration of both mind reading and bending keys. He didn't do any mind reading that succeeded; nobody can read my mind, I guess. And my boy held a key and Geller rubbed it, and nothing happened. Then he told us it works better under water, and so you can picture all of us standing in the bathroom with the water turned on and the key under it, and him rubbing the key with his finger. Nothing happened. So I was unable to investigate that phenomenon.

But then I began to think, what else is there that we believe? (And I thought then about the witch doctors, and how easy it would have been to check on them by noticing that nothing really worked.) So I found things that even more people believe, such as that we have some knowledge of how to educate. There are big schools of reading methods and mathematics methods, and so forth, but if you notice, you'll see the reading scores keep going down -- or hardly going up -- in spite of the fact that we continually use these same people to improve the methods. There's a witch doctor remedy that doesn't work. It ought to be looked into; how do they know that their method should work? Another example is how to treat criminals. We obviously have made no progress -- lots of theory, but no progress -- in decreasing the amount of crime by the method that we use to handle criminals.

Yet these things are said to be scientific. We study them. And I think ordinary people with commonsense ideas are intimidated by this pseudoscience. A teacher who has some good idea of how to teach her children to read is forced by the school system to do it some other way -- or is even fooled by the school system into thinking that her method is not necessarily a good one. Or a parent of bad

boys, after disciplining them in one way or another, feels guilty for the rest of her life because she didn't do "the right thing", according to the experts.

So we really ought to look into theories that don't work, and science that isn't science.

I think the educational and psychological studies I mentioned are examples of what I would like to call cargo cult science. In the South Seas there is a cargo cult of people. During the war they saw airplanes with lots of good materials, and they want the same thing to happen now. So they've arranged to make things like runways, to put fires along the sides of the runways, to make a wooden hut for a man to sit in, with two wooden pieces on his head to headphones and bars of bamboo sticking out like antennas -- he's the controller -- and they wait for the airplanes to land. They're doing everything right. The form is perfect. It looks exactly the way it looked before. But it doesn't work. No airplanes land. So I call these things cargo cult science, because they follow all the apparent precepts and forms of scientific investigation, but they're missing something essential, because the planes don't land.

Now it behooves me, of course, to tell you what they're missing. But it would be just about as difficult to explain to the South Sea islanders how they have to arrange things so that they get some wealth in their system. It is not something simple like telling them how to improve the shapes of the earphones. But there is one feature I notice that is generally missing in cargo cult science. That is the idea that we all hope you have learned in studying science in school -- we never say explicitly what this is, but just hope that you catch on by all the examples of scientific investigation. It is interesting, therefore, to bring it out now and speak of it explicitly. It's a kind of scientific integrity, a principle of scientific thought that corresponds to a kind of utter honesty -- a kind of leaning over backwards. For example, if you're doing an experiment, you should report everything that you think might make it invalid -- not only what you think is right about it: other causes that could possibly explain your results; and things you thought of that you've eliminated by some other experiment, and how they worked -- to make sure the other fellow can tell they have been eliminated.

Details that could throw doubt on your interpretation must be given, if you know them. You must do the best you can -- if you know anything at all wrong, or possibly wrong -- to explain it. If you make a theory, for example, and advertise it, or put it out, then you must also put down all the facts that disagree with it, as well as those that agree with it. There is also a more subtle problem. When you have put a lot of ideas together to make an elaborate theory, you want to make sure, when explaining what it fits, that those things it fits are not just the things that gave you the idea for the theory; but that the finished theory makes something else come out right, in addition.

In summary, the idea is to give all of the information to help others to judge the value of your contribution; not just the information that leads to judgement in one particular direction or another.

The easiest way to explain this idea is to contrast it, for example, with advertising. Last night I heard that Wesson oil doesn't soak through food. Well, that's true. It's not dishonest; but the thing I'm talking about is not just a matter of not being dishonest; it's a matter of scientific integrity, which is another level. The fact that should be added to that advertising statement is that no oils soak through food, if operated at a certain temperature. If operated at another temperature, they all will -- including Wesson oil. So it's the implication which has been conveyed, not the fact, which is true, and the difference is what we have to deal with.

We've learned from experience that the truth will come out. Other experimenters will repeat your experiment and find out whether you were wrong or right. Nature's phenomena will agree or they'll disagree with your theory. And, although you may gain some temporary fame and excitement, you will not gain a good reputation as a scientist if you haven't tried to be very careful in this kind of work.

And it's this type of integrity, this kind of care not to fool yourself, that is missing to a large extent in much of the research in cargo cult science.

A great deal of their difficulty is, of course, the difficulty of the subject and the inapplicability of the scientific method to the subject. Nevertheless, it should be remarked that this is not the only difficulty. That's why the planes don't land -- but they don't land.

We have learned a lot from experience about how to handle some of the ways we fool ourselves. One example: Millikan measured the charge on an electron by an experiment with falling oil drops, and got an answer which we now know not to be quite right. It's a little bit off because he had the incorrect value for the viscosity of air. It's interesting to look at the history of measurements of the charge of an electron, after Millikan. If you plot them as a function of time, you find that one is a little bit bigger than Millikan's, and the next one's a little bit bigger than that, and the next one's a little bit bigger than that, until finally they settle down to a number which is higher.

Why didn't they discover the new number was higher right away? It's a thing that scientists are ashamed of -- this history -- because it's apparent that people did things like this: when they got a number that was too high above Millikan's, they thought something must be wrong -- and they would look for and find a reason why something might be wrong. When they got a number close to Millikan's value they didn't look so hard. And so they eliminated the numbers that were too far off, and did other things like that. We've learned those tricks nowadays, and now we don't have that kind of a disease.

But this long history of learning how to not fool ourselves -- of having utter scientific integrity -- is, I'm sorry to say, something that we haven't specifically included in any particular course that I know of. We just hope you've caught on by osmosis

The first principle is that you must not fool yourself -- and you are the easiest person to fool. So you have to be very careful about that. After you've not fooled yourself, it's easy not to fool other scientists. You just have to be honest in a conventional way after that.

I would like to add something that's not essential to the science, but something I kind of believe, which is that you should not fool the layman when you're talking as a scientist. I am not trying to tell you what to do about cheating on your wife, or fooling your girlfriend, or something like that, when you're not trying to be a scientist, but just trying to be an ordinary human being. We'll leave those problems up to you and your rabbi. I'm talking about a specific, extra type of integrity that is not lying, but bending over backwards to show how you're maybe wrong, that you ought to have when acting as a scientist. And this is our responsibility as scientists, certainly to other scientists, and I think to laymen.

For example, I was a little surprised when I was talking to a friend who was going to go on the radio. He does work on cosmology and astronomy, and he wondered how he would explain what the applications of his work were. "Well", I said, "there aren't any". He said, "Yes, but then we won't get support for more research of this kind". I think that's kind of dishonest. If you're representing yourself as a scientist, then you should explain to the layman what you're doing -- and if they don't support you under those circumstances, then that's their decision.

One example of the principle is this: If you've made up your mind to test a theory, or you want to explain some idea, you should always decide to publish it whichever way it comes out. If we only publish results of a certain kind, we can make the argument look good. We must publish BOTH kinds of results.

I say that's also important in giving certain types of government advice. Supposing a senator asked you for advice about whether drilling a hole should be done in his state; and you decide it would be better in some other state. If you don't publish such a result, it seems to me you're not giving scientific advice. You're being used. If your answer happens to come out in the direction the government or the politicians like, they can use it as an argument in their favor; if it comes out the other way, they don't publish at all. That's not giving scientific advice.

Other kinds of errors are more characteristic of poor science. When I was at Cornell, I often talked to the people in the psychology department. One of the students told me she wanted to do an experiment that went something like this -- it had been found by others that under certain circumstances, X, rats did something, A. She was curious as to whether, if she changed the circumstances to Y, they would still do A. So her proposal was to do the experiment under circumstances Y and see if they still did A.

I explained to her that it was necessary first to repeat in her laboratory the experiment of the other person -- to do it under condition X to see if she could also get result A, and then change to Y and see if A changed. Then she would know the the real difference was the thing she thought she had under control.

She was very delighted with this new idea, and went to her professor. And his reply was, no, you cannot do that, because the experiment has already been done and you would be wasting time. This was in about 1947 or so, and it seems to have been the general policy then to not try to repeat psychological experiments, but only to change the conditions and see what happened.

Nowadays, there's a certain danger of the same thing happening, even in the famous field of physics. I was shocked to hear of an experiment being done at the big accelerator at the National Accelerator Laboratory, where a person used deuterium. In order to compare his heavy hydrogen results to what might happen with light hydrogen, he had to use data from someone else's experiment on light hydrogen, which was done on a different apparatus. When asked why, he said it was because he couldn't get time on the program (because there's so little time and it's such expensive apparatus) to do the experiment with light hydrogen on this apparatus because there wouldn't be any new result. And so the men in charge of programs at NAL are so anxious for new results, in order to get more money to keep the thing going for public relations purposes, they are destroying -- possibly -- the value of the experiments themselves, which is the whole purpose of the thing. It is often hard for the experimenters there to complete their work as their scientific integrity demands.

All experiments in psychology are not of this type, however. For example, there have been many experiments running rats through all kinds of mazes, and so on -- with little clear result. But in 1937 a man named Young did a very interesting one. He had a long corridor with doors all along one side where the rats came in, and doors along the other side where the food was. He wanted to see if he could train the rats to go in at the third door down from wherever he started them off. No. The rats went immediately to the door where the food had been the time before.

The question was, how did the rats know, because the corridor was so beautifully built and so uniform, that this was the same door as before? Obviously there was something about the door that was different from the other doors. So he painted the doors very carefully, arranging the textures on the faces of the doors exactly the same. Still the rats could tell. Then he thought maybe the rats were smelling the food, so he used chemicals to change the smell after each run. Still the rats could tell. Then he realized the rats might be able to tell by seeing the lights and the arrangement in the laboratory like any commonsense person. So he covered the corridor, and still the rats could tell.

He finally found that they could tell by the way the floor sounded when they ran over it. And he could only fix that by putting his corridor in sand. So he covered one after another of all possible clues and

finally was able to fool the rats so that they had to learn to go in the third door. If he relaxed any of his conditions, the rats could tell.

Now, from a scientific standpoint, that is an A-number-one experiment. That is the experiment that makes rat-running experiments sensible, because it uncovers that clues that the rat is really using -- not what you think it's using. And that is the experiment that tells exactly what conditions you have to use in order to be careful and control everything in an experiment with rat-running.

I looked up the subsequent history of this research. The next experiment, and the one after that, never referred to Mr. Young. They never used any of his criteria of putting the corridor on sand, or being very careful. They just went right on running the rats in the same old way, and paid no attention to the great discoveries of Mr. Young, and his papers are not referred to, because he didn't discover anything about the rats. In fact, he discovered all the things you have to do to discover something about rats. But not paying attention to experiments like that is a characteristic example of cargo cult science.

Another example is the ESP experiments of Mr. Rhine, and other people. As various people have made criticisms -- and they themselves have made criticisms of their own experiments -- they improve the techniques so that the effects are smaller, and smaller, and smaller until they gradually disappear. All the para-psychologists are looking for some experiment that can be repeated -- that you can do again and get the same effect -- statistically, even. They run a million rats -- no, it's people this time -- they do a lot of things are get a certain statistical effect. Next time they try it they don't get it any more. And now you find a man saying that is is an irrelevant demand to expect a repeatable experiment. This is science?

This man also speaks about a new institution, in a talk in which he was resigning as Director of the Institute of Parapsychology. And, in telling people what to do next, he says that one of things they have to do is be sure to only train students who have shown their ability to get PSI results to an acceptable extent -- not to waste their time on those ambitious and interested students who get only chance results. It is very dangerous to have such a policy in teaching -- to teach students only how to get certain results, rather than how to do an experiment with scientific integrity.

So I have just one wish for you -- the good luck to be somewhere where you are free to maintain the kind of integrity I have described, and where you do not feel forced by a need to maintain your position in the organization, or financial support, or so on, to lose your integrity. May you have that freedom.

Since then, whenever I read Feynman, and watched his videos, I was stuck by two things: (1) How similar were his mental habits to those I talk about in the classroom; and (2) how naive are some of the investment research reports that come across my table created by analysts who seem to know more about the world from a *social science* perspective than even the greatest scientists know about the world from a *science* perspective! Surely, the degree of certainty about the worldview of a great scientist should be more than the degree of certainty exhibited by social "scientists"?

Let me illustrate this. Go and watch this video clip — Feynman on Uncertainty and Doubt—an excerpt: "I can live with doubt and uncertainty and not knowing. I prefer that to knowing answers that might be wrong. I don't have to know and I am not frightened by not knowing. "

Does it not bother you that one of the greatest men of science that the world has seen is quite happy to accept uncertainty (*"I have approximate answers, and possible beliefs, and different degrees of certainty about different things, but I'm not absolutely sure of anything, and there are many things*

I don't know anything about."), while you are studying how to use the DCF model to value businesses? That's a model which requires you to predict cash flows more than thirty years out requiring "degrees of certainty" that Feynman would have laughed at. Does it not bother you that DCF models use "scenario analysis" where sometimes all scenarios are the functional equivalents of "groping in the dark"? And does it not further bother you that the investment community typically values a stock based on various future scenarios by applying subjective probabilities to those scenarios and then computing the "expected value" by simply taking the weighted average thereby becoming functional equivalents of the statistician who forgot about ranges of depth (2 feet to 10 feet) and drowned in a river which was, on average, only 4 feet deep?

Does it not bother you that a man of science can dare to say *"I don't know"* while investment analysts almost never say that? Feynman once said, *"The first principle is that you must not fool yourself — and you are the easiest person to fool."* How true! How we analysts fool ourselves so often! How often we just believe what we are told by companies and their self interested agents and convert that nonsense into very nice-looking reports with plenty of pie charts and tables which make the whole thing look so believable!

In one of his famous lectures, Feynman was talking about the scientific method of inquiry. He said, *"Looking back at the worst times, it always seems that they were times in which there were people who believed with absolute faith and absolute dogmatism in something. And they were so serious in this matter that they insisted that the rest of the world agree with them. And then they would do things that were directly inconsistent with their own beliefs in order to maintain that what they said was true."*

Are we sure that Feynman was referring to the worst scientific practices of the past instead of current "best" practices in investment analysis? After all, that particular excerpt was from a lecture titled *"The Uncertainty of Values."* Earlier I mentioned that Feynman had a great sense of humor, and once when he was talking of rare events he said: *"You know, the most amazing thing happened to me tonight. I was coming here, on the way to the lecture, and I came in through the parking lot. And you won't believe what happened. I saw a car with the license plate ARW 357. Can you imagine? Of a) the millions of license plates in the state, what was the chance that I would see that particular one tonight? Amazing!"*

Feynman, of course, was having fun at the expense of people who think that rare things don't happen very often. Yet the world of finance is full of models of risk like VAR and models of capital allocation like the CAPM which are based on the assumption that rare events are so rare that they can virtually be ignored.

Four sigma? Six sigma? Seven sigma? Do you realize that if the world is best described by a bell curve then, a seven sigma event is likely to happen once every 3 billion years? And do you realize that the world in the past few decades has experienced many more seven sigma events than those models can explain? What can this mean?

Feynman knew what it meant when he said, *"It doesn't matter how beautiful your theory is, it doesn't matter how smart you are. If it doesn't agree with the experiment, it's wrong."* You already know of my skepticism about the utility of using Microsoft Excel in creating elaborate models of valuation using DCF.

We discussed that some time ago. Part of the reason for my skepticism arises from the biases of human nature which have a tendency to find their way into the models thru the biased man creating an illusion of precision. Man is not a rational animal, but a rationalizing one — there I said it again — many of you will be pulling your hair by now!

Feynman was aware of this problem when he wrote: *"There is a computer disease that anybody who works with computers knows about. It's a very serious disease and it interferes completely with the work. The trouble with computers is that you 'play' with them!"*

Feynman had "The Pleasure of Finding Things Out" by having the very same mental habits that you need to develop to have the pleasure of becoming a great analyst.

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Don't forecast so admit your ignorance. Be humble.

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Mouboussin

A Return to First Principles

Say you had to come up with a fair offer to buy your local dry cleaner and the seller limited the extent of your financial information to the answers to five questions. Which questions would you ask?

Chances are you wouldn't ask how the quarter is progressing or about last year's earnings, but you would focus on the prospects for cash coming in versus cash going out over time. Sole proprietors understand intimately that the value of their business hinges on the cash flow the business generates. No distributable cash, no value. Cash puts food on the table and pays the mortgage; earnings do not.

Equity investors are business buyers. While most shareholders own only a small fraction of a company, they are owners nonetheless. The source of shareholder value, and value changes, is no different than the sole proprietor's: it's all about the cash.

Most investors don't think this way. In part, this is because market exchanges readily allow investors to trade cash today for claims on future cash flows, and vice versa, encouraging them to forget they are evaluating, buying, and selling businesses. Yet investors, as opposed to speculators, should never lose sight of their objective: buying a stream of cash flows for less than what it is worth.

Given that cash inflows and outflows are the lifeblood of corporate value, you might expect investors to be intent on measuring and valuing cash flows. Indeed, valuation in the bond and commercial real estate markets is all about cash. In practice, however, very few equity investors dwell on cash. Proxies for value, like earnings and multiples, dominate Wall Street valuation work.

Because markets are mostly efficient, investors can get away with using value proxies without awareness of what the proxies actually represent. The result is complacency and a false sense of understanding. As a consequence most investors don't do fundamental valuation work; when they do, they often do the work incorrectly.

First principles tell us the right way to value a business is to estimate the present value of the future cash flows. While most Wall Street professionals learned about discounted cash flow (DCF) models in school, in practice the models they build and rely on are deeply flawed. Not surprisingly, the confidence level in these DCF models is very low. This faint confidence is not an indictment of analytical approach but rather of analytical methods.

DCF models should be economically sound and transparent. Economically sound means the

company's return and growth patterns are consistent with the company's positioning and the ample empirical record supporting reversion to the mean. Transparent means you understand the economic implications of the method and assumptions you choose. Most DCF models fail to meet the standards of economic soundness and transparency.

The List

Here's our list of the most frequent errors we see in DCF models. We recommend you check your models, or the models you see, versus this list. If one or more of the errors appear, the model will do little to inform your business judgment.

1. *Forecast horizon that is too short.* One of the most common criticisms of DCF models is that any forecast beyond a couple of years is suspect. Investors, therefore, are alleged to be better off using more certain, near-term earnings forecasts.

Such reasoning makes no sense, for at least two reasons. First, a key element in understanding a business's attractiveness involves knowing the set of financial expectations the price represents. The market as a whole has historically traded at a price-to-earnings multiple in the mid-to-high teens. Simple math shows today's stock prices reflect expectations for value-creating earnings and cash flows many years in the future.

To make the point more concrete, imagine you are a restaurant industry executive in charge of finding new store locations. When assessing the attractiveness of a prospective site, would you consider only two years of earnings because "any beyond that is guessing"? Of course not. You'd base your judgment on the location, past results for similar sites, and other value-relevant factors. Intelligent capital allocators take a long-term view.

The mismatch between a short forecast horizon and asset prices that reflect long-term cash flows leads to the second problem: investors have to compensate for the undersized horizon by adding value elsewhere in the model. The prime candidate for the value dump is the continuing, or terminal, value. The result is often a completely non-economic continuing value. This value misallocation leaves both parts of the model—the forecast period and continuing value estimate—next to useless.

Some investors swear off the DCF model because of its myriad assumptions. Yet they readily embrace an approach that packs all of those same assumptions, without any transparency, into a single number: the multiple. [3] Multiples are not valuation; they represent shorthand for the valuation process. Like most forms of shorthand, multiples come with blind spots and biases that few investors take the time and care to understand.

John Maynard Keynes famously said, "I'd rather be vaguely right than precisely wrong." His message applies here. We recommend explicit forecast periods of no less than five years, and note many companies require over ten years of value-creating cash flows to justify their stock prices. Ideally, the explicit forecast period should capture at least one-third of corporate value with clear assumptions about projected financial performance.

While the range of possible outcomes certainly widens with time, we have better analytical tools to deal with an ambiguous future than to place an uncertain multiple on a more certain near-term earnings per share figure. We address the uncertainty issue below.

captures the firm's value for the time beyond the explicit forecast period, which can theoretically extend into perpetuity.

While developing a DCF model's structure, investors must mind one of microeconomics's most powerful lessons: competitive forces assure that return on investment will approximate the cost of capital over time. Numerous empirical studies document this reversion to the mean, and life cycle theories express the process. [2] Accordingly, one reasonable way to structure a DCF model uses

the explicit forecast period to capture the excess returns on new investment (value creation) with the continuing value component reflecting value after the company exhausts its incremental value-creation opportunities. See Exhibit 1.

Exhibit 1: A DCF Model Should Be Economically Sound
Source: LMCM analysis.

We rarely see this structure in practice. Based on a sample of sell-side models we gathered, we observed forecast periods that are too short, generally five years or less. The crucial consequence of a too-short forecast period is the modeler has to heap the value burden on the continuing value estimate in order to have any consonance with the market price. As a consequence, the model fails to reflect a clear sense of the company's pattern or timing of value creation. In reality, most analysts understand little about the implications of their continuing value assumptions.

For instance, modelers commonly apply a multiple to ending-period earnings before interest, taxes, depreciation, and amortization (EBITDA) to estimate continuing value. The assigned multiple rarely has a solid economic foundation, and most analysts have no idea what the multiple implies about financial performance.

As an example, given certain assumptions a 13.0 times EBITDA multiple implies 6 percent earnings growth and a 150 percent return on incremental capital *in perpetuity*. [4] For the record, no company in the history of mankind has achieved such financial performance. A high percentage of value accorded to the continuing value almost always reflects an improper forecast period. In contrast, segregating the model into a period of value creation and value neutrality not only makes sound economic sense, but allows for greater model clarity. We can capture the

continuing value with a perpetuity assumption, capitalizing the last year's net operating profit after tax by the cost of capital. This value, even discounted to the present, often represents 60-70 percent of corporate value.

Moreover, a perpetuity assumption neatly captures the reversion-to-the-mean phenomenon. The approach assumes *incremental* returns on investment equal the cost of capital. Over time, naturally, depreciation of old investments and addition of new investments assure a company replaces its invested capital base. After the company depreciates all its old, value-creating investments, the remaining invested capital earns exactly the cost of capital. A company's asset life determines the length of this reversion process. A short-asset-life company will rapidly replace its invested capital base and revert to cost-of-capital returns quickly. The opposite is true for a long-asset-life company.

Many analysts incorrectly assume the perpetuity approach does not reflect growth beyond the explicit forecast period. This assumption is wrong. The perpetuity assumption does not rule out growth. But since growth has no necessary link to value creation, companies can continue to grow without creating any shareholder value. The essential assumption of the perpetuity approach relates to incremental value creation, not incremental growth. Appendix A demonstrates this point.

Most DCF models fail the economically sound and transparent test because of poor structure: the explicit forecast periods are too short and the continuing value estimates carry too much value. An investor should have a clear handle on the economic assumptions or implications behind whatever continuing value approach they choose.

3. *Cost of capital.* You'll rarely see a great equity investor point to an ability to judge the cost of capital better than others as the source of meaningful edge. But you do see many DCF models debilitated by a nonsensical cost of capital estimate.

The cost of capital is an estimate of the rate of return an investor demands to hold an asset or, said differently, an investor's opportunity cost. As such, the cost of capital is the proper rate for discounting future cash flows to a present value.

Most companies finance their operations largely through debt and equity. The cost of debt, especially for large companies, is generally transparent because companies have contractual obligations to make coupon payments and return principal on a timely basis. Some yield premium over risk-free securities is appropriate, with the size of the premium reflecting the company's creditworthiness. The large and generally liquid corporate bond market makes comparisons between fixed-income securities relatively straightforward.

Estimating the cost of equity is more challenging. Unlike debt's explicit cost, the cost of equity is implicit. The cost of equity is higher than the cost of debt because equity's claim is junior. But no simple method exists to estimate the cost of equity.

By far the most common approach to estimating the cost of equity is the capital asset pricing model (CAPM). The CAPM says a company's cost of equity equals the risk-free rate plus the product of the equity risk premium and beta. Government-issued notes generally provide a good proxy for the risk-free rate. Estimates for the equity risk premium and beta prove more challenging.
Let's start with beta, which attempts to reflect the sensitivity of a stock's price movement relative to the broader market. A beta of 1.0 means the stock tends to move in line with the market. A beta below 1.0 suggests a stock moves less than the market, while a beta above 1.0 implies moves greater than the market. All things equal, finance theory associates a higher beta with higher risk and reward.

Beta is wonderful theoretically but fails practically and empirically. The practical failure surrounds what beta to actually use in the CAPM. Ideally, we want forward-looking betas, which we cannot reliably estimate. Beta's empirical failure reflects studies showing beta does a poor job explaining returns. [5]

A visit to the Bloomberg terminal shows the problem with blithely using the CAPM. In mid-March 2006, General Motors had a beta of 1.3 while Yahoo! had a beta in excess of 1.6. It would be hard for a businessperson to argue that Yahoo is likely to be significantly more risky than General Motors over an appropriate forecast time horizon. Indeed, the implied option volatilities suggest more risk at GM than YHOO. While gauging the relative risks of businesses is clearly valuable, investors have to impose judgment on the figures the various services produce.

The second important input into the CAPM is the equity risk premium, the return above and beyond the risk-free rate an investor expects to earn as compensation for assuming greater risk. Like beta, the equity risk premium is ideally a forward-looking estimate. Most analysts rely on past equity risk premiums, which, depending on the time frame, may not give a reasonable sense of the return outlook.

Most of the problems with the cost of capital come from stale inputs for beta and the equity risk premium. For example, the geometric average equity risk premium was 1.9 percentage points from 1982-2005, 3.7 percentage points from 1962-2005, and 6.2 percentage points from 1926-2005. [6] The arithmetic average equity risk premiums during the same time frames were higher. One area of debate in valuation is whether the geometric or arithmetic average is more appropriate. We favor geometric returns for long term models and arithmetic averages for shortterm return forecasts. [7]

In addition, research suggests the equity risk premium is probably nonstationary, which means using past averages may be very misleading. Specifically, variables shaping the equity risk premium—like past stock returns, stock price volatility, and business conditions—clearly change, making it likely the ex-ante equity risk premium changes as well. [8]

Whether you add 200 or 600 basis points to the current risk-free rate of 4.7 percent (mid-March 2006) will make a significant difference in the model's output. Exhibit 2 shows a sample of equity risk premiums assumed in recent sell-side analyst reports. Our best advice is to settle on a cost of capital that makes business and economic sense. Some academics suggest the equity risk premium is in the 3 to 4 percentage point range, which strikes us as reasonable. [9] The midpoint of this range implies a market return of roughly 8 percent over time.

Finally, while some sensitivity analysis around the cost of capital can be useful, we would argue investors are much better off considering alternative scenarios for the key operating value drivers (sales growth, margins, capital intensity). Sometimes asset mispricings do show up as high discount rates, as we saw in the high-yield bond market in late 2002. But even there, you could argue great investors have historically generated an edge by understanding the value drivers better than the market.

4. *Mismatch between assumed investment and earnings growth.* Companies invariably must invest in the business—via working capital, capital spending, acquisitions, R&D, etc.—in order to grow over an extended period. Return on investment (ROI) determines how efficiently a company translates its investments into earnings growth. Since ROI links investment and growth, and ROIs tend toward the cost of capital over time, investors must treat the relationship between investment and growth carefully.

DCF models commonly underestimate the investment necessary to achieve an assumed growth rate. This mistake often comes from two sources. First, analysts looking at companies that have been highly acquisitive in the past extrapolate an acquisition-enhanced growth rate while only reflecting capital spending and working capital needs for the current business. You can mitigate this error by carefully considering the growth likely to come from today's business—which will be less than an acquisition-fueled rate.

The second reason for underestimating investment stems from a simple failure to explicitly link growth and investments via ROI. Analysts frequently project growth (sales and margins) independent of investments. A simple way to check for this error is to add a ROI line in the model. If you see ROIs rising or dropping sharply without a thoughtful strategic underpinning, the model is likely unreliable. The vast majority of the models we see make no effort to reflect a link between growth and investment.

5. *Improper reflection of other liabilities.* In the widely-used free cash flow to enterprise approach, an analyst determines the corporate value based on the present value of future cash flows. The analyst then adds cash and any other nonoperating assets and subtracts debt and any other liabilities to arrive at shareholder value.

Most liabilities, including debt and many pension programs, are relatively straightforward to determine and reflect in the model. Some other liabilities, like employee stock options, are trickier to capture. Not surprisingly, most analysts do a very poor job capturing these liabilities in an economically sound way.

We would note that other liabilities tend to be important for only a handful of companies. For example, other postretirement employee benefit plans tend to concentrate in manufacturing industries (e.g., autos) while employee stock options occur most frequently in knowledge and service industries (e.g., technology and financial services). Investors must properly recognize other liabilities in the sectors where they have a large impact on corporate value.

Employee stock options are a good illustration of this common shortcoming. Most DCF models simply reflect past option grants through fully diluted shares. Of course, since fully diluted shares only reflect in-the-money options, the solution does an awful job of capturing the magnitude of the liability. [10] While accounting standards now require companies to expense options, we have found

few DCF models that explicitly treat future option grants as an expense. Often, analyst models show rising shares outstanding (dilution) and models sometimes include option proceeds without reflecting any dilution.

We can deal with ESOs appropriately by treating already-granted options as a contingent liability, using basic shares outstanding, and reflecting future option grants as an expense. This approach allows for a dynamic appraisal of past option grants and considers future grants as an economic expense.

Discount to private market value. In what we'd characterize as an unfortunate hangover from the 1980s and 1990s investment-banker mentality, we still see DCF models that calculate a value, only to modify the amount by a "public market discount" of 20-25 percent. This practice seems most prevalent in the telecommunications industry.

This practice fails the transparency test. To see the point, we have to invert the discount mindset and ask why a private (strategic or financial) buyer would pay a *premium* to public market value. Perhaps the private buyer believes the stock is undervalued (in which case the premium wipes out the benefit). More likely, the buyer believes it can generate a higher stream of cash flows from the acquired assets than the target company can by itself.
Synergies, the benefits of putting two companies together, often justify most if not all of the premium. Alternatively, a buyer may allocate capital differently or use financial leverage to enhance after-tax cash flows.

To be sensible, a DCF model reflecting a public market discount must already incorporate synergies or some other catalyst for higher cash flows the company cannot achieve on its own. Modeling possible deal synergies is fine, though the exercise should remain separate from valuing the standalone business. The discount-to-private-market-value model lacks sufficient transparency because it conflates the base and synergy cash flows.

7. *Double counting.* Models should not count a dollar of value (or liability) more than once. Unwittingly, DCF models often double count the same source of value.
Take share repurchase, for instance. Companies generating strong free cash flow often have a record of buying back stock that is likely to continue. Analysts, recognizing both a proclivity toward buybacks and strong cash flow, sometimes build buybacks into their models by assuming the company uses free cash flow to shrink shares outstanding over time. This double counts because the model values the cash flow (once) and the model uses the same cash flow to reduce shares outstanding (twice). This error of double counting leaves aside the analytical challenge of judging the future stock price (the only way to properly determine how much stock a company might buy).

Another less frequent example of double counting involves the practice of including interest income in the cash flow calculation and adding the cash balance to corporate value. Alternatively, some analysts subtract financing costs from cash flow and then deduct debt from corporate value to come up with shareholder value.

8. *Scenarios.* Probably the most-often-cited criticism of a DCF model is that small changes in assumptions can lead to large changes in the value. We addressed part of that concern, which can be mitigated by lengthening the forecast horizon, in our discussion of the first two errors. Going one step further, the large majority of reports we see offer *one* DCF scenario, and analysts often peg their target prices on that scenario. Given investing is inherently probabilistic, one scenario—often backed by shaky assumptions—does not constitute thorough analysis. An intelligent investor needs to consider multiple scenarios.

Though many DCF models do incorporate sensitivity analysis (typically a grid of values driven by alternative cost of capital, growth, or terminal valuation assumptions), these grids provide little relevant information for anyone trying to understand the prospects of the business. Investors should look to the value drivers—sales, margins, and investment needs—as sources of variant

perception.

Even sensitivity analysis based on the value drivers is generally flawed because it fails to consider the *interactivity* between value drivers. Proper scenario analysis considers how changes in sales, costs, and investments lead to varying value driver outcomes. [11] See Exhibit 3.

Scenario analysis also addresses concerns about an uncertain future. By considering "if, then" scenarios and insisting on a proper discount to expected value—or margin of safety—an investor can safely and thoughtfully weigh various outcomes.

Conclusion

Theory and practice tell us the value of a company is the present value of future cash flows. Investors primarily seek to buy a stream of cash flow for less than it's worth—or sell a stream for more than it's worth. Accordingly, an investor needs to be able to model cash flows intelligently and identify a variant perception: a well-founded belief the market has placed an incorrect value on a company.

Business school students learn all about DCF models, and they often practice building them in a classroom setting. But when applying the models to the real world, an investor must ensure the models are economically sound and transparent. In practice, very few models pass these tests. We have tried to identify the key areas of failure, and offer some thoughts about how to address the shortcomings.

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We predict by extrapolation. People remember the recent past better than the distant past, and they informally generalize from the few cases that are memorable rather than incorporate the full body of data into their analysis.

The PV of current and future cash flows.

The value of any asset is determined by the PV of the distributable cash flows that the assets provides to its owner. All inflows and outflows are discounted back to the present at an appropriate discount rate.

What seems perfect in theory fails in practice.

The typical way to estimate IV is to estimate the relevant cash flows for the current and future years out to a reasonable date, perhaps 10 years in the future. Then one estimates a rate for the cost of capital that is appropriate to the riskiness of the asset in question.

See below: Table

The way of dealing with the cash flows in the distant future is to come up with what is called a terminal value. From VI Greenwald

The PV of Current and Future Cash Flows.

There is wide agreement in theory that the intrinsic value of any investment is determined by the pv of the distributable cash flows that the asset supplies to its owner. PV is calculated as the sum of present and future cash flows, both outlays and receipts, with each dollar of future cash flow appropriately discounted to take into account the time value of money.

The standard way of calculating present values, and hence intrinsic value, is to begin by estimating the relevant cash flows for the current and future years out to a reasonable date, perhaps 10 yeas in the future. Then one estimates a rate for the cost of capital that is appropriate the riskiness of the asset in question.

Start-up to government bond. With these two figures it is possible to calculate the present value of each annual cash flow. Summing them gives us the present value of all the cash flows for the years in question. Way of calculating pre;setn values, and hence iv is to begin by estimating the relevant cash flows for the current and future years out to a reasonable date, perhaps 10 years in the future.

The practice for dealing with the cash flows in the distant future is to come up with what is called a terminal value. The terminal value is invariably calculated by assuming that beyond year 10 or the last year for which we have done annual cash flow calculations.

And the fact that this (stock or search result) is not a *MSFT*, it is not followed by 200 analysts and it is not very hard to get your arms around, this is a very important part of the search strategy and investment style. It is unloved, cheap, obscure, which should give you confidence in evaluating these opportunities.

II) Valuation (Valuation in Principle, Valuation in Practice

In theory, the value of any asset can be determined by the PV of the distributable CFs that the asset supplies to its owner.

There is a glaring inconsistency between the precision of the algebra and the gross uncertainties infecting the variables that drive the model. We estimate growth rates for years 1-10, then from the end of year 10 to forever. Too much uncertainty.

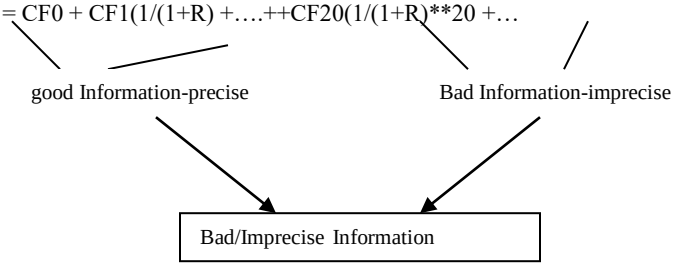
Secondly, the valuations vary significantly if the underlying assumptions are off by only small amounts.

Investors try to simplify the valuation process by relying on multiple-based calculations of earnings or EBIT or EBITDA. But here errors depend on secondary origins of the valuation, dependent on someone else's uncertain projections for other companies; the noncomparability of the companies chosen to provide the multiple factor. **The key shortcoming of this approach is that multiple based valuations are nothing more than present value calculations with some simplifying assumptions tacked on.**

Shortcomings of NPV Approach in Practice (however correct in theory)

(1) Method of combining information: Results in BAD information

NPV = CF0 + CF1(1/(1+R) +….++CF20(1/(1+R)**20 +…



(2) Sensitivity Analysis is based on difficult-to-forecast parameters

Valuation Approaches: Ratio Analysis

Cash Flow Measure	x	Multiple
Earnings (Maint Inv = Depr. + A)		Depends on: ❑ Econ Position ❑ Cyclical Situation ❑ Leverage ❑ Mgt. Quality ❑ Cost of Capital (risk) ❑ Growth
EBIT (Maint Inv = Depr + A; Tax = 0)		
EBIT - A (Maint Inv = Depr Only)		
EBIT-DA (Maint Inv = 0)		

Range of Error (100%). Note: NPV Analysis encompasses ratio analysis (NPV diseases are ratio analysis diseases)

How is valuation done on Wall Street?

Most valuation is done by essentially taking a measure of cash flow and applying a multiple to that cash flow measure.

Where does the multiple come from? The multiple is based on what comparable securities are trading at relative to their measure of cash flow.

What does the CF measure consist of? Either recent cash flow numbers or a projections of 1 or 2 years of future cash flow. It is a ST projection of CF. The most common is projecting earnings (what most Wall Street Analysts do) and then apply a multiple on those earnings based on comparable companies

Or you could say you don't want it distorted by leverage—I want it to be leverage free so no tax advantage distortion—look at EBIT and multiples of EBIT to prices for comparable companies and you take an average and you multiple by 8 or 9 or whatever that average multiple was or is.

Operating earnings really includes amortization, so they include an EBITA multiple to that.
Increasingly, everyone is using EBITDA—not a good idea because it assumes no capex.
Inevitably, this is a **relative value approach**.
So you are looking at a media company, you don't look at multiples for a steel company.
This is NOT value investing, this is the way most people on Wall Street look at valuation.

Value investors are only 7% of all investors, but these value investors represent a high percentage disproportionately of successful investors.

Comparable companies used in comparable analysis should be comparable in management quality, risk quality, comparable cyclicality, economic position and leverage. Almost no company satisfies those conditions as a comparable.

There are very large errors of up to 100% in the multiples that get selected. Apart from that, the whole sector could be overvalued or the whole market overvalued.

There is a lot of uncertainty buried in the supposed simplicity and robustness to the errors of complexity embedded in that approach.

What are analysts taught to do?

CGBS Course 6301 project of valuing a company—estimates of future cash flows. Start off with estimate revenues x market growth rate x margin then profits then subtract taxes + required investment = FCF.
Then a sequence of CFs out to 5 to 7 years, then add in a terminal value. Terminal value: CFlast/(r-g).

Or you take a terminal earnings' number or terminal EBIT and multiply it by a terminal price/earnings ratio.
Is that last step sound?

Big problem with the multiple is straightforward. What were your valuations dominated by? **The terminal value.**

Terminal Cash Flow of $100 million, g = 5% which is the world GDP growth rate and
Cost of capital r is 10%. $100 million/(10% - 5%) = $2 billion terminal value.

Your error range? R= 9% and g = 6%, divide by 3% so $3.3 Billion
R = 11% and g = 4%, divide by 7% so $1.4 billion—these are not big changes in input Assumptions, which result in errors. The range is $1.4 Billion to $3.3 Billion! A huge range.

Look at your range of possible values that you are looking at within the precision of your mode of estimation. It is laughable and equally laughable when you do the ratio valuations. This is a 2 to 1 or 3 to 1 error or uncertainty in the estimation/valuation. **The terminal value dominates the valuation and causes the error.**

Sensitivity analysis has the virtue of making explicit the unreliability of PV estimates.
The PV approach relies on information—parametric values for operating values—that is often simply not knowable, especially in the far distant future.

Two things you should know about the CF formula:

1) Theoretically DCF valuation is sound.
2) It does encompass the ratio valuation.

However, for practical purposes, it is a *dumb way* to do things.

When you do the sum of DCFs you are summing good and bad information together, which results in bad information—you don't end up with half bad information. This is where all the errors are.

You want to segregate what you know reliably from what you really know or don't know from things that you are estimating based on a hope or a dream. There is no way to do that in a NPV calculation. You were mis-taught to do valuations that way.

Greenwald, *"I used to give this lecture at MIT and Yale and at this point the hate vibes were high." This argument applies to most of your attempts to value companies.*

II. 2nd argument: What is a valuation rule supposed to do? It is like a box where you pour in the judgments you think you can make about the future. You turn the crank and what goes on inside the box--which is the valuation rules that uses the assumptions you put in to generate a value.

What are the assumptions you have to put in the Excel model to generate the DCF analysis? You have to put in Cost of Capital, margins, growth rates, required investments. Do you have a good idea what they are? No. They are subject to **substantial degree of uncertainty**. These numbers are not things in which you have a lot of confidence in. Right away you are not using the best information that you have.

How do you get around that? Do a million sensitivity analyses. On the other hand, the information varies together. Not a solution!

What you would like to do: Is this a judgment you can effectively make? For example, if in 20 years, the return on sales for *Ford* is 6%? NO. But, if I ask you, "In 20 years, do you think the auto industry will be viable?" Yes. That is a judgment you make based on past history. Trends in transportation. It is that second assumption you would like to build into your valuation?

There are *no competitive advantages* in auto industry. *Mercedes* has no competitive advantage—no one has a competitive advantage? Is that going to be true 10 years from now? Probably.

No competitive advantage or alternatively the competitive advantages that are there will continue. Does anyone think in 20 years that *Coke* will no longer have a competitive advantage over its competitors? Probably no. That is a judgment you can make.

The broad strategic judgments are easier to make such as whether the auto industry will be viable rather than predicting operating margins or the costs of capital. Yet the PV approach cannot be adapted to incorporate the implications of these judgments for a valuation of the company.

It is those assumptions that you want to feed into your valuation box, not those parametric assumptions of r and g. And that is the shortcoming with the way you have been taught to value companies using DCF.

The **Graham and Dodd Approach** to valuation avoids both of these problems. It segregates information affecting valuation by reliability class, so that good information is not contaminated by poor information. It uses the valuation implication of broad strategic judgments.

Try to triangulate on valuations with as many perspectives as you can.

Look at the value inherent in the:

- assets
- economic position of the firm
- present and future earnings of the firm.

Liquidation value of assets is usually a joke.

You want an alternative valuation approach that has three characteristics:

1) It organizes elements of value from most-to-least reliable. What is really there, what is problematically there, what is a hope and a dream.
"Whereof one cannot speak, thereof one must be silent." Wittgenstein.

2) You would like a valuation approach that organizes value by strategic assumptions—we will talk about competitive advantages. But from situations like we described in the auto business where nobody has a competitive advantage. To situations of non viable industries. Or the industry is viable but no one has a particular advantage over anybody else—free entry. Situations of non-viable industries. You want to think about those situations separately and say, look, this value is inherent in the viability of the industry. The judgment only depends on the viability of the industry.

   The stability of competitive advantage.

3) The competitive advantages being sustainable in growing markets.

Organize by strategic advantages/assumptions.

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On DCF Models: We just don't think it's worth the effort except expert for very stable businesses such as toll roads or utilities –Francisco Garcia Parames.