Chameleons:
The Misuse of Theoretical Models in Finance and Economics

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March 2014

Abstract

In this essay I discuss how theoretical models in finance and economics are used in ways that make them "chameleons" and how chameleons devalue the intellectual currency and muddy policy debates. A model becomes a chameleon when it is built on assumptions with dubious connections to the real world but nevertheless has conclusions that are uncritically (or not critically enough) applied to understanding our economy. I discuss how chameleons are created and nurtured by the mistaken notion that one should not judge a model by its assumptions, by the unfounded argument that models should have equal standing until definitive empirical tests are conducted, and by misplaced appeals to "as-if" arguments, mathematical elegance, subtlety, references to assumptions that are "standard in the literature," and the need for tractability.

I thank Anat Admati, Jonathan Bendor, Jules van Binsbergen, Steve Ross, Mark Wolfson and Jeff Zwiebel for many helpful discussions and thoughtful comments.

1. Introduction

An engineer, a physicist and an economist are stranded on a deserted island with nothing to eat. A crate containing many cans of soup washes ashore and the three ponder how to open the cans.

Engineer: Let's climb that tree and drop the cans on the rocks.

Physicist: Let's heat each can over our campfire until the increase in internal pressure causes it to open.

Economist: Let's assume we have a can opener.

Theoretical models in economics and finance must of necessity be based on simplifying assumptions, assumptions that are in some senses "unrealistic." This does not mean that the connections between a model's assumptions and what we know about the real world do not matter and can be ignored. (An assumed but nonexistent can opener will not open a real can of soup.) In this essay I discuss how models are used in ways that make them "chameleons." A model becomes a chameleon when it is built on assumptions with dubious connections to the real world but nevertheless has conclusions that are uncritically (or not critically enough) applied to understanding our economy. Chameleons are not just mischievous they can be harmful − especially when used to inform policy and other decision making − and they devalue the intellectual currency.

Given my personal history, I am certainly not arguing against the importance of models in economics and finance. Economic phenomena play out in extremely complex, dynamic systems, in which outcomes are often determined by decisions made by millions of economic agents interacting in non-stationary environments. Our main hope for achieving any understanding of what is happening is to build simplified models that capture what is important and, when appropriate and possible, subject these models to rigorous empirical tests. There are no real alternatives. To put it quite simply, this is an article about the importance of using models properly and not an article against the use of models.

The proposition that theoretical models are necessary for understanding our economic system does *not* imply that having some particular theoretical model automatically means we understand anything useful. If one is reasonably clever and chooses the "right" set of assumptions, one can produce all kinds of results. Perhaps I would be going a bit too far if I claimed that one could produce *any* result through theoretical modeling, but it is certainly possible to produce a wide range of results. This potentially creates a problem that might be called "theoretical cherry picking." In empirical work it is well understood that biased and misleading results are obtained if one cherry picks the data, i.e., if one selects data that generally support a desired result and exclude those data that do not. This understandably is viewed by careful empiricists as a mortal sin.

Analogously in theoretical work it is often possible to cherry pick assumptions to produce a given result. Essentially it is a matter of reverse engineering: what do I need to assume to obtain the result that configuration X is optimal or that increasing Y will increase Z? Of course, the fact that "theoretical cherry picking" is possible doesn't necessarily mean that it is a problem. Perhaps it will be so obvious when a model is the result of cherry picking that the model will be quickly rejected or at least subjected to intense scrutiny. Won't cherry-picked assumptions with little foundation in the real world be immediately seen as being unreasonable? Won't it be clear that the theoretical argument is a contrived one designed to produce the result? In some cases, of course, it will be clear. If the assumptions critical to the result are patently false, the model will not be taken seriously. In many other cases, however, problems with a model (as something that can be applied to the real world) won't be quite so transparent. As is often observed, a model can be useful in understanding the real world even if the assumptions that are made are not "perfectly true." Indeed, any simplifying model − which of course means all models − will be based on assumptions that are not to be taken as perfect descriptions of reality.[1] Given this, cherry-picked assumptions won't necessarily stand out as contrived, and they can often be defended as simplifying assumptions that are made so that the model is tractable.[2] Since a model can't include all economic factors (e.g., every possible market "friction" and every possible incentive problem), the modeler needs to choose which factors are important. Results are determined not only by what is included but also by what is excluded.

I have introduced the notion of theoretical cherry picking not because I want to claim that theorists are intellectually dishonest and blatantly cherry pick assumptions. My focus here will be on how models are used and not on the incentives of those constructing them. Indeed, there is absolutely nothing wrong with reverse engineering *per se*, since

[1] In considering the various ways a model is unrealistic a useful distinction can sometimes be made between Aristotelian and Galilean idealization. The first involves ignoring certain aspects of a setting that are, at least for the problem at hand, not considered of first-order importance. For example, in certain settings one might ignore or "abstract from" transactions costs. Galilean idealization involves deliberate distortions that are designed to make the model simpler and easier to solve. For example, to make a model more tractable it might be assumed that agents are infinitely lived, although we know this is a distortion. This same sort of distinction is made in Gibbard and Varian (1978), where models are divided into those that are "approximations" (Aristotelian idealization) and "caricatures" (Galilean idealization). Gibbard and Varian explain that caricatures "seek to 'give an impression' of some aspect of economic reality not by describing it directly, but rather by emphasizing – even to the point of distorting – certain aspects of the economic situation."

[2] Cherry picking is not necessarily the result of conscious design. To guard against it a researcher may need to take a step back and ask whether the assumptions unintentionally are contrived and serve more to produce the particular result than to simplify the analysis.

identifying the set of assumptions that are either necessary or sufficient (or both necessary and sufficient) for obtaining a given result can be very important in guiding intuitions. The Modigliani and Miller (1958) results are important in this way, since they identify what we need to assume for capital structure to be irrelevant and what might make it relevant. What is important is that the nature of this type of argument be recognized for what it is.

My reason for introducing the notion of theoretical cherry picking is to emphasize that since a given result can almost always be supported by a theoretical model, the existence of a theoretical model that leads to a given result in and of itself tells us nothing definitive about the real world. Though this is obvious when stated baldly like this, in practice various claims are often given credence – certainly more than they deserve – simply because there are theoretical models in the literature that “back up” these claims. In other words, the results of theoretical models are given an ontological status they do not deserve. In my view this occurs because models and specifically their assumptions are not always subjected to the critical evaluation necessary to see whether and how they apply to the real world.

In all that follows I will make a clear distinction between what I call bookshelf models and models that are intended to be applied to the real world. Theoretical modeling is often undertaken simply to understand the implications of a given set of assumptions. For example, theoretical modelling might be used to identify the optimal contract to be used when one assumes that particular incentive problem exists between a principal and an agent and the only contracts that can be written are those within a certain set. The results obtained in analyzing this “bookshelf” model may give us some insights that are applicable to the real world – or they may not. Perhaps the posited incentive problem is one that does not arise in practice or, if it does arise, has immaterial consequences. Perhaps the assumed contract forms are not available and could not be made available in practice. Perhaps other much more effective contract forms are available. When we take a model “off of the bookshelf” and consider applying it to the real world, it is reasonable to ask whether it is based on assumptions that are generally in accord with what we know about the world and are capturing factors that are of first-order importance. In other words, we use the background knowledge that we have about the world we live in (knowledge that is based ultimately on empirical evidence) to filter out models that are not useful for understanding what happens in the economy or for making policy decisions.

In Section 2 I provide a more detailed discussion of how theoretical models should be used in understanding the world and how chameleons − models built on dubious assumptions that are uncritically given more credence than they deserve − arise. Section 3 and Section 4 give some examples of models that have become chameleons. My examples in these sections are drawn from finance, since this is my field, and specifically from the banking literature, since it is in this literature that I became most acutely aware of chameleons. In each of the cases I consider attempts that are made to apply the model's results to the real world without first applying the real-world filter. In Sections 5-9 I consider various arguments and views that lead to the notion that models should be allowed to avoid the real-world filter and thereby become chameleons. I show that these arguments and views are flawed and often based on wishful thinking. Milton Friedman's assertion that models should only be judged by their predictions and not by the realism of their assumptions is taken up in Section 5. In Section 6 I consider the argument that models should have equal standing until definitive empirical tests are conducted. In Section 7 I discuss flawed arguments based on the notion that if we see it, it must be optimal, while in Section 8 I consider the limitations of "as-if" arguments. In Section 9 I argue that while there is a need for tractability, while subtlety and mathematical elegance may be alluring, and while complexity can be a smokescreen that hides dubious assumptions, none of these should permit chameleons to circumvent real-world filters. In Section 10 I ask whether my position that economic models should be passed through real world filters before applying them to the real world is contradicted by the success of models in the hard sciences such as those used in quantum mechanics. Models in quantum mechanics seem to be based on unreasonable assumptions – ones that grossly violate "common sense" – and yet are accepted because their predictions are confirmed to many decimal places. As I explain in this section there are huge and instructive differences between the settings in which models in finance and economics are applied and those in which quantum mechanics are applied, making analogy between economics and physics inapt. Finally in Section 11 I provide some concluding remarks.

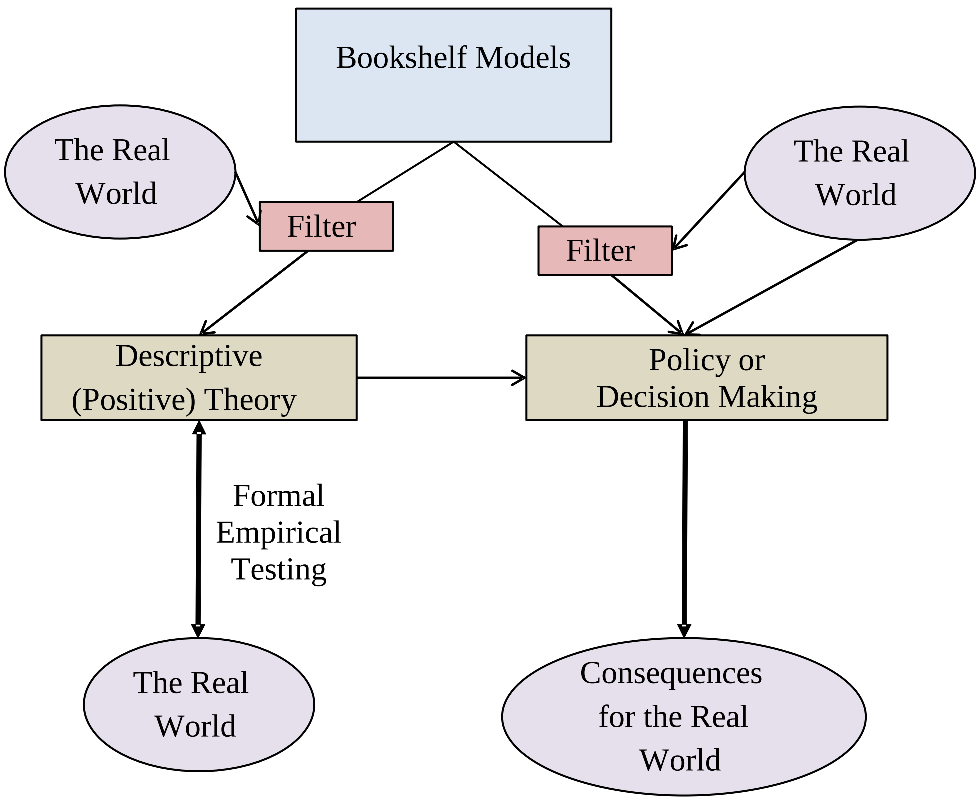
2. Chameleons

I begin by giving in Figure 1 a schema that shows in more detail the role theoretical models can play in helping us understand economic phenomena. At the top I place bookshelf models. I emphasize that this is not to be taken as a pejorative term. At this level I take a theoretical model to simply be an argument that a certain set of results follows from a given set of assumptions. If the argument is a valid one (the model contains no logical or mathematical flaws), then it *may* be useful in giving us intuitions or in being a building block that can be used to construct models we can apply to real world phenomena.[3] Bookshelf models might answer questions such as what security design

[3] Gilboa, Postlewaite, Samuelson and Schmeidler (2013) provide a number of insightful observations that explain the potential value of many bookshelf models. They suggest that theoretical models are often useful because they create cases that can be used analogically in analyzing real world problems. Theoretical models developed for this purpose can be considered thought experiments that enrich the set of results economists have available for making predictions and addressing policy questions. Akerlof's lemons model and Spence's signaling model are offered as good examples. In applying these thought experiment

minimizes adverse selection costs in a particular setting, what will be the Bayesian-Nash equilibrium in a given extensive form game, or how various risks will be priced if investors have a certain type of preferences. They may be toy models designed solely for intuitions or they may be the ambitious attempts to capture many elements of our economy.

Figure 1



As mentioned above, not all bookshelf models are equally applicable to the real world, and some may not be applicable at all. To take a decidedly extreme example, imagine an asset pricing model based on the assumption that there is no uncertainty about any asset's returns. Besides being a rather uninteresting model, such a model clearly has little or no relevance to the actual world, since the assumption is so transparently false. No serious person would suggest that the predictions of the model should be subjected to rigorous empirical testing before rejecting it. The model can be rejected simply on the basis that a critical assumption is contradicted by what we already know to be true.

As discussed above one can develop theoretical models supporting all kinds of results, but many of these models will be based on dubious assumptions. This means that when

models to specific issues in the real world, the key concern is the strength of the analogy. This depends on how well the model can pass through the real-world filters that I discuss below.

we take a bookshelf model off of the bookshelf and consider applying it to the real world, we need to pass it through a filter, asking straightforward questions about the reasonableness of the assumptions and whether the model ignores or fails to capture forces that we know or have good reason to believe are important.

I suspect that some people will be uncomfortable with this notion that a filter needs to be applied in this way at this stage of the process. Perhaps some will take the Friedman view that models should not be judged by their assumptions but only by their predictions. A related view is that any valid (logically consistent) model is potentially applicable to the real world and we should only reject such a model when we have decisive empirical evidence against it. I argue below (in Section 5) that any sensible interpretation of the Friedman view does not rule out the need for filters. I also argue (see Section 6) that claims that all models have equal standing until subjected to empirical tests are based on an overly optimistic view of our ability to test and discriminate among models using the types of data that we can collect and use in formal empirical studies and an overly pessimistic view of the role that real world filters can and should play. Since models are typically causal arguments, it is well known that an empirical test must be a direct test of the causal mechanism identified by the model. This generally means that we must find sources of variation that are truly exogenous before we can learn anything definitive from the data that would allow us to reject a model or decide in favor of one model over another. In an ideal world (or at least one much better than ours) we would be awash in natural experiments of all sorts, with each producing data that could be cheaply collected and analyzed. In such an ideal world we might be able to quickly test "every" possible theoretical model and no filters would be needed. This is obviously not our world.

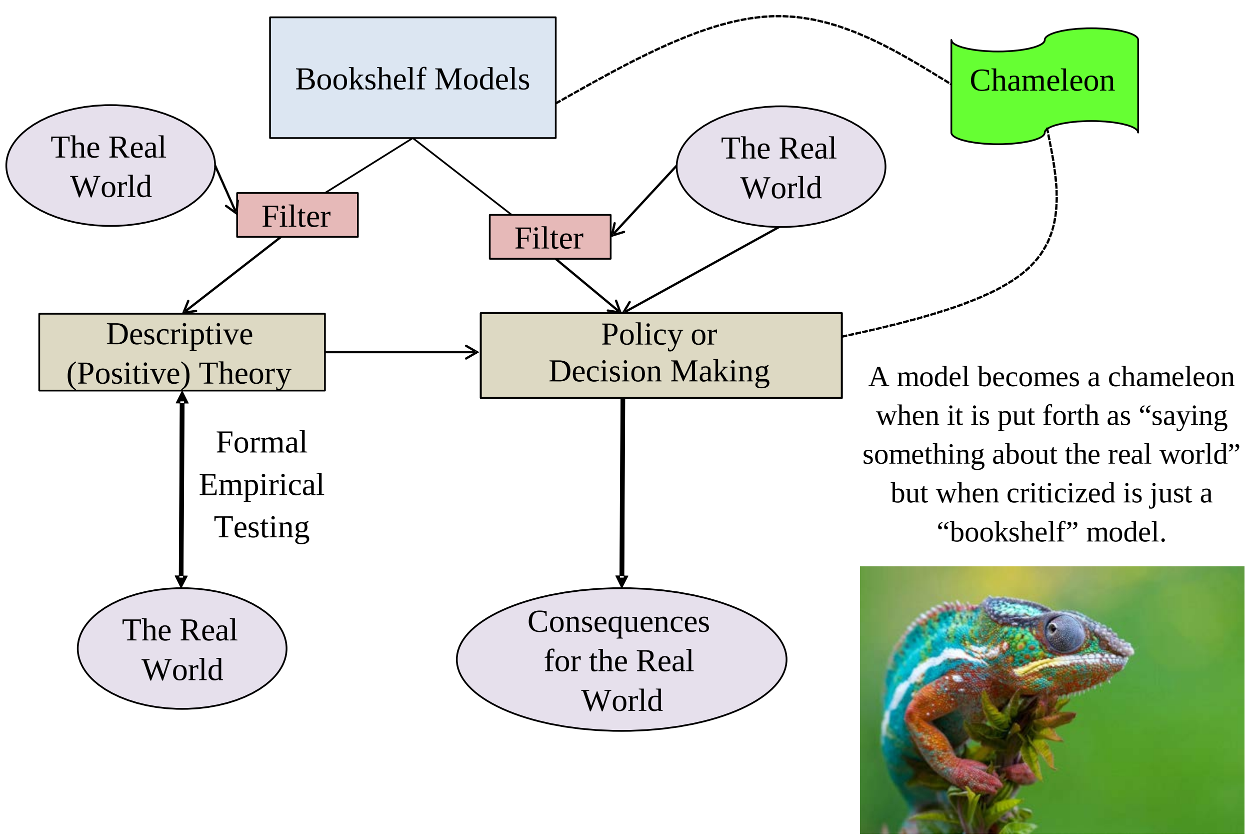
Figure 1 shows two tracks in which filters should be used in evaluating models we take off the bookshelf and attempt to apply to the real world. On the left-hand side, filters are employed when we are engaged in building models designed to describe how the world actually works (positive theory). As shown the filters that draw on our knowledge of the real world are applied to bookshelf models in the process of selecting those models (along with their insights and results) to be used in building a positive theory of the world. These theories are then subjected (when possible) to formal empirical tests.

On the right hand side we have what I will call policy theory.[4] Consider, for example, the policy question of how to structure a spectrum auction. The inputs include not only

[4] Policy questions generally involve two considerations. First one must determine what outcomes can be achieved and how these are achievable (what actions need to be taken to achieve them). Second, one must determine which outcome among the various achievable outcomes is the most desirable. The latter consideration, especially when social policy is involved, generally involves value judgments (e.g. fairness) and falls squarely in the realm of normative economics. What I mean to capture in the right-hand-side track is first consideration, which is independent of value judgments. In other words, I assume that the value judgments are given, and the focus is on the policy decision that will lead to the most desirable outcome as defined by these normative assumptions.

bookshelf models (e.g., theoretical results on the efficiency of various auction formats) but potentially also inputs from positive theory that have been subjected to empirical tests, since these contribute to our understanding of the environment in which the auctions will be conducted and perhaps to bidder behavior. A bookshelf theoretical model that was based on the assumptions that all bidders are identical and there are no information asymmetries would no doubt be “filtered out” as inapplicable to a spectrum auction. The outcome of this process is a policy recommendation, which potentially has consequences for the real world (how much revenue is raised by the government and whether licenses are efficiently allocated).

Figure 2



In Figure 2 I show how a model becomes a chameleon. In this case a “chameleon model” asserts that it has implications for policy, but when challenged about the reasonableness of its assumptions and its connection with the real world, retreats to being a simply a theoretical (bookshelf) model that has diplomatic immunity when it comes to questioning assumptions. In short, *a model becomes a chameleon when it attempts to bypass the relevant filters*. Of course I have used colorful language here in anthropomorphizing models. Models have no incentives. They become chameleons through the way they are used by either those who develop them or others who appeal to them.

Chameleons arise and are often nurtured by the following dynamic. First a bookshelf model is constructed that involves terms and elements that seem to have some relation to the real world and assumptions that are not so unrealistic that they would be dismissed out of hand. The intention of the author, let's call him or her "Q," in developing the model may to say something about the real world or the goal may simply be to explore the implications of making a certain set of assumptions. Once Q's model and results become known, references are made to it, with statements such as "Q shows that X." This should be taken as short-hand way of saying "Q shows that under a certain set of assumptions it follows (deductively) that X," but some people start taking X as a plausible statement about the real world. If someone skeptical about X challenges the assumptions made by Q, some will say that a model shouldn't be judged by the realism of its assumptions, since all models have assumptions that are unrealistic. Another rejoinder made by those supporting X as something plausibly applying to the real world might be that the truth or falsity of X is an empirical matter and until the appropriate empirical tests or analyses have been conducted and have rejected X, X must be taken seriously. In other words, X is innocent until proven guilty. Now these statements may not be made in quite the stark manner that I have made them here, but the underlying notion still prevails that because there is a model for X, because questioning the assumptions behind X is not appropriate, and because the testable implications of the model supporting X have not been empirically rejected, we must take X seriously. Q's model (with X as a result) becomes a chameleon that avoids the real world filters.

The best way to illustrate what chameleons are is to give some actual examples, which I do in the next two sections.

3. Models becoming Chameleons: Example 1

In April 2012 Harry DeAngelo and René Stulz circulated a paper entitled "Why High Leverage is Optimal for Banks." The title of the paper is important here: it strongly suggests that the authors are claiming something about actual banks in the real world. In the introduction to this paper the authors explain what their model is designed to do:

> To establish that **high bank leverage** is the natural (distortion-free) result of intermediation focused on liquid-claim production, the model rules out agency problems, deposit insurance, taxes, and all other distortionary factors. By positing these idealized conditions, the model obviously ignores some important determinants of bank capital structure in the real world. However, in contrast to the MM framework – and generalizations that include only leverage-related distortions – it allows a meaningful role for banks as **producers of liquidity** and **shows clearly** that, if one extends the MM model to take that role into account, it is **optimal for banks to have high leverage**. [emphasis added]

Their model, in other words, is designed to show that if we rule out many important things and just focus on one factor alone, we obtain the particular result that banks should be highly leveraged. This argument is for all intents and purpose analogous to the argument made in another paper entitled "Why High Alcohol Consumption is Optimal for Humans" by Bacardi and Mondavi.[5] In the introduction to their paper Bacardi and Mondavi explain what their model does:

> To establish that **high intake of alcohol** is the natural (distortion free) result of human liquid-drink consumption, the model rules out liver disease, DUIs, health benefits, spousal abuse, job loss and all other distortionary factors. By positing these idealized conditions, the model obviously ignores some important determinants of human alcohol consumption in the real world. However, in contrast to the alcohol neutral framework – and generalizations that include only overconsumption-related distortions – it allows a meaningful role for humans as **producers of that pleasant "buzz" one gets by consuming alcohol**, and **shows clearly** that if one extends the alcohol neutral model to take that role into account, it is **optimal for humans to be drinking all of their waking hours.** [emphasis added]

The Deangelo and Stulz model is clearly a bookshelf theoretical model that would not pass through any reasonable filter if we want to take its results and apply them directly to the real world. In addition to ignoring much of what is important (agency problems, taxes, systemic risk, government guarantees, and other distortionary factors), the results of their main model are predicated on the assets of the bank being riskless and are based on a posited objective function that is linear in the percentage of assets funded with deposits. Given this the authors naturally obtain a corner solution with assets 100% funded by deposits. (They have no explicit model addressing what happens when bank assets are risky, but they contend that bank leverage should still be "high" when risk is present.) Given all of this, a much more accurate title for the paper would be:

Why "High" Leverage is Optimal for Banks in an Idealized Model that Omits Many Things of First-order Importance

The DeAngelo and Stulz paper is a good illustration of my claim that one can generally develop a theoretical model to produce any result within a wide range. Do you want a model that produces the result that banks should be 100% funded by deposits? Here is a

[5] As the reader has no doubt surmised, this is a fictional paper.

set of assumptions and an argument that will give you that result. That such a model exists tells us very little. By claiming relevance without running it through the filter it becomes a chameleon.

One might ask, "So what if the DeAngelo/Stulz model is presented in a way that makes it a chameleon? What's the harm?" Won't people who read the paper realize its limitations? Some will, but not all. For example, in an article entitled "Capital punishment: Forcing banks to hold more capital may not always be wise" in the September 14, 2013 issue of *The Economist*, it was written:

> In a new paper Harry DeAngelo of the University of Southern California and René Stulz of Ohio State University show that this premium means that banks, unlike other firms, are not indifferent to leverage, as the Modigliani-Merton theorem suggests. Mr DeAngelo and Mr Stulz show that it is better for banks to be highly levered even without frictions like deposit insurance and implicit guarantees.

At the very least chameleons add noise and contribute to public misunderstanding and confusion about important issues.[6]

4. Models becoming Chameleons: Examples 2 and 3

My next examples are also taken from the banking literature and relate to the idea that the demandable debt that banks issue (e.g. deposits) prevents managerial malfeasance or disciplines the bank managers.[7] In "The Role of Demandable Debt in Structuring Optimal Banking Arrangements," Calomiris and Kahn assume that a bank's manager can abscond with the bank's funds and not be caught doing so. In their model "any promise to pay the depositor an amount P is actually an option of the banker to either pay P or to leave town with his assets diminished by the proportion A." In their model the incentive for the manager to run off with the money is greater when bank assets have lower value, and they show that in the context of their model this can create an incentive for depositors to monitor the value of bank assets and withdraw their money just before the bank manager can take the money and board a plane for South America.

A more involved argument for the role of demandable debt disciplining managers is found in a paper by Diamond and Rajan that is quite properly titled "A Theory of Bank Capital." (The title of the paper is proper since it clearly suggests that what follows is a

[6] For additional discussion of the misuse of the DeAngelo and Stulz paper, see Admati (2013).
[7] A much more extensive discussion about the various problems encountered in applying the notion that debt has a disciplining effect to modern banking in the real world can be found in Admati (2013), Admati et al. (2013) and Admati and Hellwig (2013).

bookshelf theory: a set of assumptions that leads to some results. Admirably, and perhaps unusual for the literature, the title does not claim direct applicability to the actual world of banking.) Under the Diamond and Rajan theory “banks can create liquidity precisely because deposits are fragile and prone to runs.” The theory they develop is based on the assumption that significant holdup problems exist between a bank and those who borrow from the bank and also between bank managers and those who fund the bank. Behind the first holdup problem is the notion that an entrepreneur who has borrowed from the bank can threaten to cease working unless the bank makes the terms of its loan more favorable to the entrepreneur. Behind the second is the assumption that a bank manager can threaten to cease using his skills to collect on loans the bank has made unless the funders of the bank agree to compensate the manager more for his special abilities. Diamond and Rajan write:

> The relationship lender is an intermediary who has borrowed from other investors. In the same way as the entrepreneur can negotiate his repayment obligations down by threatening not to contribute his human capital, the intermediary can threaten not to contribute his specific collection skills and thereby capture a rent from investors.

Diamond and Rajan argue that the way to solve these holdup problems in their model is for the bank to make sure that its funding structure is very fragile. For example, if the bank is funded by depositors who can withdraw their money any time they want on a first-come first-served basis, then the threat of a run on the bank reduces the ability of both the entrepreneur and the bank manager to make their threats. The bank manager cannot renegotiate with the depositors since each depositor will run if he or she fears that there might be a transfer from depositors to the manager.

This notion that short-term debt and fragility disciplines bank managers has been cited as a reason for banks not to have “too much capital.” For example, in the Squam Lake Report we find the statement:

> The disciplining effect of short-term debt, for example, makes management more productive. Capital requirements that lean against short-term debt push banks toward other forms of financing that may allow managers to be more lax.

It is not clear what the authors of the Squam Lake Report are relying on in making this strong claim (they make no reference to any theoretical or empirical work), but in all conversations I have had with people who assert that short-term debt disciplines managers, the work Calomiris and Kahn and of Diamond and Rajan is put forward as major arguments for their position.

But, if these theoretical models are to be taken off the bookshelf and applied to modern banking in the real world, will they pass through a filter of reasonable questions about their assumptions? I list below some questions that I think should be asked to assess the applicability of these models to modern banking – these are examples of reasonable empirical filters:

- How much money could managers at a bank run away with, and is this of a magnitude that makes it worthwhile for depositors to closely monitor the bank?
- Precisely which short-term creditors are vulnerable to the manager absconding with money as in the Calomiris/Kahn model and which would be threatening to call the manager's hold-up bluff by running as in the Diamond/Rajan model? Presumably it is not the *insured* depositors. The insurance is in place to protect the depositors and prevent runs. Could it be the repo lenders? The problem is that these lenders have collateral in their possession and are not subject to the sequential service (first come, first served) constraint.
- Which employee at a major bank like JP Morgan or Barclays might threaten not to use his skills if investors don't cut him a better deal as called for in the Diamond and Rajan model? Is it the CEO? Or is it a loan officer who has made some commercial real estate loans? Which loan or asset in a trillion dollar balance sheet is so big that this threat is of such consequence that it would cause creditors to run?
- Isn't the Diamond/Rajan hold-up problem a potential problem at other firms and why don't we see fragile funding structures in these firms? Can't the General Partners (GPs) in a private equity fund threaten not to do their work in realizing value from their portfolio firms unless the Limited Partners (LPs) cut them a better deal? Can't Microsoft employees threaten not to finish the operating system unless they are cut a better deal? Why is fragility not used for other firms? How do they solve the problem?

Most importantly, in both these models we can also ask questions concerning how much "fragility" might be necessary to achieve the salutary results posited. For example, for the purpose of evaluating the Diamond and Rajan model consider what happened to JP Morgan in the so-called "London Whale" scandal of 2012. The trading losses due to Bruno Iksil's ill-advised positions reportedly amounted to $6.2 billion. It is important to recognize that these huge losses did *not* create a run on JP Morgan. One reason they did not is that whereas these losses are huge in absolute terms, they were not enough by themselves to create any panic among creditors given JP Morgan's overall financial position at the time. Of course, creditors were probably also kept from panicking by their well-founded beliefs that the government was quite likely to support JP Morgan if the bank became truly distressed. What does all this mean? It means that Jamie Dimon could make the following speech to JP Morgan investors (shareholders and creditors):

> As you know, my executive team and I are absolutely critical to JP Morgan's future success. If you do not pay us an extra $6.2 billion in bonuses this quarter, we will refuse to execute on all of the complex positions we have taken at the bank,

and he would suffer no consequences of the sort posited in the Diamond/Rajan model due to short-term fragility. This suggests that to prevent Jamie Dimon from holding up the bank for $6.2 billion using the mechanism envisioned in the Diamond and Rajan paper, JP Morgan would need to be *much* more fragile than it is.[8] It would need an extremely thin equity cushion and the depositors and short-term creditors must be in vulnerable positions and prepared to run. Clearly something kept and continues to keep Jamie Dimon from making the speech given above, but it is just as clear that it is not the mechanism discussed in Diamond/Rajan. If there is a hold-up problem at JP Morgan and other similar banks, it is not being solved by fragility.[9]

The notion that short-term debt plays a critical role in disciplining bank managers in the actual world seems to be based on chameleons: theoretical results have been taken off the bookshelf and applied to the real world without passing them through a reasonable filter. If a model's result critically depends on a particular mechanism working and we want to apply this model to the real world, we should be able to see how the foundations for that mechanism are actually in place and working in the real world. It must be more than a story that can be told.

A number of people have said to me that the "jury is still out" when it comes to determining whether short-term debt disciplines bank managers. They claim that it is an "empirical issue," implying that the jury will only come in once we have some definitive empirical test of the proposition. I asked these people what such a test would be and asked them to describe how it would be carried out. In most cases I got blank stares. In a few cases I was told that we could look for a natural experiment where there was an exogenous shock to the amount of short-term debt funding and we could measure the "performance" of the bank after the shock, with the presumption being that any change in performance will reveal the effects of a change in discipline or prevention of malfeasance. The problems involved in such a test are obvious. Even if we could find an

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[8] The reader might object that if Jamie Diamond tried to hold the bank for $6.2 billion, the bank shareholders (through the board of directors) would simply hire another management team. The amount that Jamie Diamond could hold up the bank for is bounded by the loss that the bank would incur in hiring an outside team and this may be less than $6.2 billion. All this is true and simply reinforces the point I wish to make. If the maximum value that Jamie Diamond could hold up the bank for is, say $400 million, then he can clearly extract this value without generating a run since $400 million is a lot less than $6.2 billion. To prevent this $400 million holdup the bank must be even more fragile.

[9] Note that the argument I am making here also applies to the Calomiris and Kahn Model, since we can assume that depositors in JP Morgan would have little or no incentive to monitor the bank to make sure that the top executives are not absconding with $6.2 billion.

exogenous shock affecting how the bank is funded, it is not clear that any subsequent change in performance can be unambiguously associated with the posited discipline channel, since the shock may affect a number of factors, many of which are difficult to account for. Now it is possible that a clever empiricist may identify some way to make significant headway on this, but given the obstacles involved, this doesn't seem likely. Does this mean that we must be completely agnostic? Complete agnosticism requires that we ignore all the background knowledge we have (e.g., Jamie Dimon doesn't hold up JP Morgan shareholders for hundreds of millions of dollars even though this would apparently not cause a run on the bank; private equity firms don't rely on fragile funding structure even though one might think that general partners might be in a position to hold up limited partners; etc.) Completely ignoring this background knowledge seems like a strange way to reason about the world.

5. You *Should* Judge a Model's Assumptions

In his 1953 essay "The Methodology of Positive Economics," Milton Friedman famously argued that we should not judge a model based on the realism of its assumptions but only by the accuracy of its predictions. This claim has generated considerable controversy over the years and many have rejected Friedman's arguments, although some have come to his defense.[10] It is not my intention to give a full critique of Friedman's position, but I will argue it cannot be used to give cover to chameleons by allowing them to avoid the real-world filter.

If Friedman is only claiming that a model should not be automatically rejected whenever its assumptions fall short of "complete realism," then on this simple level Friedman is correct. Since all economic models are simplifications of the actual world, their assumptions will not and cannot be completely realistic. If we must reject any model that has unrealistic assumptions in this sense, then we must reject all models. Friedman is, no

[10] See Bear and Orr (1967), Boland (1979), Caldwell (1982 and 1984), Hausman (1989), Machlup (1955), Mäki (2009), Melitz (1965) and Samuelson (1963). The controversies sparked by Friedman's paper are related to the controversy between a "realist" view of science and an "instrumentalist" view. The former takes the goal of scientific inquiry to be uncovering the underlying reality of the world as it is, while the latter takes the goal as the less ambitious one of producing a set of tools that allow useful predictions to be made. Reiss (2012), for example, makes an argument for the instrumentalist approach in economics. It is not my intention to address in a systematic way the many issues raised in realist/instrumentalist debate. My argument here is simply that that a literal interpretation of the instrumentalist view put forth by Friedman is not tenable, and Friedman's arguments do not provide cover for models becoming chameleons. Whereas it is true that many who have written on methodological issues in economics in the past few decades do not defend Friedman's views (Hausman claims that it "will be a step forward when economists come to regard Friedman's essay only as an historically interesting document"), the notion that the realism of a model's assumptions are not important, only its predictive powers, seems to still have some currency and I believe needs to be addressed here.

doubt, arguing for something beyond this trivial point, but it is not clear exactly what his argument is and how far he is willing to take it.

An absolutely literal interpretation of Friedman's claim that models should be judged only by their predictions leads to nonsense. As I will argue below, the only way the extreme Friedman view can be supported requires that we come to any prediction problem with *completely* agnostic Bayesian priors about the world. For example, assume that we are interested in economic models designed to predict the U.S. inflation rate in year *T* based on information known in year *T-1*. Let us take literally the notion that we should judge these models *only* by the success of their predictions and we should completely ignore the realism of these models' assumptions. In this context we would simply be looking for the model with the best inflation prediction record. If we allow any assumptions to be made no matter how fanciful they are, we can use any data that are available at *T-1* to predict the inflation rate in period *T*.

The number of models we could develop that make (within sample) extremely accurate predictions is extremely large if not infinite. Friedman, of course, recognizes that within sample fit is meaningless in this context and states that the goal is to obtain "valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed."[11] However, if we wait a few years and use the new out-of-sample data to "test" all of our contending inflation prediction models, it is not the case that the one "true" model will emerge victorious and all the "false" models will crash and burn. We will, of course, find that the performance of most of the models that we identified as stellar within-sample performers is severely degraded in the out of sample period, but an extremely large number will still survive. Many of the surviving ones will no doubt be based on fanciful and unrealistic assumptions. For example, we may find that in one of the many surviving models the inflation rate in year *T* is determined by a simple function of the average height of male Oscar award winners in year *T-1*. Of course, I am well aware this is *argumentum ad absurdum*, but I am taking Friedman's claim that we should ignore the realism of the assumptions quite literally. In fact, from what Friedman writes it is difficult to know how he separates the absurd from the merely unrealistic. He writes:

> "Truly important and significant hypotheses will be found to have "assumptions" that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense)."[12]

[11] Friedman, "The Methodology of Positive Economics," page 7.

[12] To be fair, Friedman does in a footnote state that "the converse of the proposition does not of course hold: assumptions that are unrealistic (in this sense) do not guarantee a significant theory." Note that despite this qualification, Friedman is still suggesting that a very unrealistic set of assumptions *can* be the basis for a "significant" theory and seems to hold that unrealism is a virtue. None of this gives us any guidance for determining which (if any) of the extremely large number of models that make "accurate" predictions with very unrealistic assumptions should be taken seriously. In other parts of his essay

He also writes

> "Complete 'realism' is clearly unattainable, and the question whether a theory is realistic 'enough' can only be obtained by seeing whether it yields predictions that are good enough for the purpose in hand or that are better than predictions from alternative theories."

It will almost always be the case that the seemingly "best" models for prediction (based on the data we have available) will be "fantasy" models like the "Oscar Award" model. These "winners" will appear better at predicting inflation (again given the data we have available) than alternative models that are based on what are transparently more reasonable assumptions involving, for example, central bank policies, past inflation rates, and consumers' expectations of future inflation. In other words, if we consider ALL possible models, the ones with the best track records are virtually guaranteed to be hugely unrealistic ones like the Oscar Award model. Simply identifying those that yield predictions that are "good enough for the purpose in hand" will not eliminate these silly models. Note that one can always concoct an implausible story (an internally consistent model with highly unrealistic assumptions) that "explains" the prediction success of a model. For example, with enough imagination one can dream up some far-fetched assumptions and use them to construct a model that "explains" how people who set prices in the economy are influenced by the stature of male actors in well-made films. However, the absolute implausibility of this story and stories like them will mean that no thinking person will take them seriously. No matter what the success of their predictions, these models *will be and should be* judged by the realism of their assumptions.

Since little or nothing in the Oscar Award model is in accord with our background knowledge about the world, we reject it. It does not pass through any sensible real world filter. This is because our Bayesian prior on the Oscar Award model is effectively zero and a track record of prediction successes does essentially nothing to change that prior. This Bayesian prior is based on our knowledge of the world and the only way to give the Oscar Award model any standing would be to ignore our knowledge of the world and have a "completely uniform" or "agnostic" Bayesian prior, assuming that this could be defined.

We can also think of the Oscar Award model as making other predictions than the inflation rate. If there is a causal chain between actor heights and inflation rates, we

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Friedman suggests that we should value simple models over more complex ones, but this does not seem to get us very far since some of the best predicting models may be quite "simple," perhaps depending on only one variable, e.g., the height of male actors.

would expect to see further evidence for this chain than just the resulting inflation rates. For example, do we see economic agents who make purchasing and price-setting decisions actually checking on the heights of the leading male Oscar contenders? For anyone who takes the Friedman claim quite literally, this is a probably an illegitimate question to ask since it seems to be addressed to the realism of the model's assumptions not the accuracy of its predictions, which is a no-no. For the rest of us, this is quite a sensible question to pose, since it is part of passing the model through the real world filter.

The question is not whether we should judge the practical utility and applicability of models by the realism of their assumptions. We should (and we routinely do). The question is what types of unrealism are relatively innocuous – simplifications that make the model tractable and useful but do not create a serious disconnect with the real world or take us into fantasy land – and what types should lead us to reject the model. This is a question that is not easily answered and no doubt one that involves judgment. Friedman's claim might be viewed as a valiant attempt to completely avoid having to address this difficult question, since his claim seems to suggest that the realism of a model's assumptions is of no import, only the accuracy of its predictions. But Friedman's claim cannot be used to avoid these questions, since it gives life to the Oscar Award model and myriad other models like it. We cannot avoid the need to run models through the real-world filter. The literal interpretation of Friedman's claims cannot be taken as an argument for allowing chameleons to bypass these filters.

6. A weak defense: "The Empirical-test Jury is Still Out"

A claim quite closely related to the Friedman argument is the claim that one shouldn't reject a model until the definitive empirical tests (those that examine the model's predictions) have been run. The claim would be that as long as the "Empirical-test Jury is Still Out," we must take the model as seriously as any other. In other words, a model should not be judged on the realism of its assumptions, only on the success of empirical work that is yet to be carried out.

This "corollary" to the Friedman argument leads to a closely related view that the role of a theorist is simply to produce internally consistent models that will sometime in the future be tested empirically. These models should be developed without much regard for whether the assumptions on which they are based are in any sense "true" or applicable to the real world, since all this will ultimately be sorted out by empiricists who will test the models' predictions. Those with this view seem to believe that the more models we have on the bookshelf, the better equipped we are in understanding the world. Taken to its extreme this "more models the better" view would lead us down the path to creating a

"Library of Babel."[13] There is an infinite number of models that can be created and analyzed in logically and mathematically correct ways, but most are built on unrealistic assumptions and useless. Building models without any serious regard to the realism of their assumptions and simply assuming that empirical testing will sort things out in the end, is clearly a silly way of conducting research.

The problem with these views is that they do not recognize the difficulties we generally face in testing theories. It is easy to come up with theories that are falsifiable (in the sense of Popper) and therefore not vacuous or tautological, but at the same time are difficult to test. This means that the claim that all theories are equally good until definitive empirical tests separate the "winners" from the "losers" does indeed put us on the path to a Library of Babel.[14]

To illustrate this, I present below the outlines of a model of managerial discipline in banking that "explains" why banks are so complex. Here are the assumptions:

1. Because of the great complexity and opacity of modern financial institutions, bank managers have wide scope to take actions that benefit them and hurt shareholders and others. Call these opportunities "scams."

2. The opportunities for managers to engage in scams arrive randomly according to a Poisson process with intensity λ.

3. The bank manager has bounded processing abilities (limited "bandwidth") and this means that the probability that the manager will recognize any given scam opportunity and be able to take advantage of it depends on the amount of "bandwidth" he has available at the moment the scam opportunity presents itself.

4. The most efficient way to prevent the manager from engaging in scams is to make sure that he has limited bandwidth available.

5. The amount of bandwidth the manager has available depends on the complexity of the bank's capital structure. A more complicated capital

[13] In Jorge Luis Borges short story "The Library of Babel" we are told of a useless library that contains all K^N books that can be written using a set of K characters (letters and punctuation marks) and have a given length of N characters.

[14] Another problem involved in relying on empirical tests to sort the good models from the bad models in a long list of competing models is related to the Quine–Duhem thesis of underdetermination in scientific theories. Since one typically must make a number of additional assumptions beyond those made by a theory when subjecting that theory to a test, negative results do not necessarily disconfirm the theory since they may be explained by one of these ancillary assumptions not holding. A good illustration of this from finance is found in attempts to test the efficient market hypothesis. As is well known, tests of the efficient markets hypothesis have generally been tests of the joint hypotheses of market efficiency and a particular asset pricing model. Any negative results could always be interpreted as a rejection of the asset pricing model rather than the market efficiency hypothesis.

structure requires more of the manager's attention and leaves less bandwidth for identifying scams.

6. Equity is simple (it is only common stock) but debt can be quite complicated (it varies in maturity, priority, and a host of other features). In other words, debt is more complicated than equity.

This gives rise to the following conclusion:

> It is efficient for banks to have a complex capital structure and this is achieved by using a lot of debt and making the structure of liabilities very complicated. This will occupy more of the manager's bandwidth and reduce his ability to recognize and take advantage of scam opportunities.

This model clearly provides *theoretical* foundations for thinking about important issues related to bank regulations and capital requirements. How seriously should we take it? Should we take it just as seriously as any other model until it is rejected by some empirical test? The proposed model is not vacuous: it is falsifiable in the Popperian sense. For example the fully fleshed out model predicts that as the opportunities for scams increase, bank capital structures should become more complex. One could imagine a natural experiment in which there is some exogenous increase in scam opportunities that would allow for a test of this. If we saw that bank capital structures systematically became simpler after exogenous increases in scams, we would have grounds to reject the model.

Of course it is not likely that conditions for a decisive test will arise and that such a test could be effectively carried out. One can imagine many practical problems. This statement applies not to just the "manager bandwidth theory" given above, it unfortunately applies to many theories in the economics and finance literature as well. This doesn't mean that we should stop developing theories and trying to test them. It only means that since our ability to directly test theories is limited, we must use other means to judge the applicability and usefulness of a given model or theory. The "bandwidth" model simply does not pass through any filter about how the real world works. It is true that some of its assumptions are plausible,[15] but no thinking person who is aware of the world would take the story seriously. First, the assumption that the most efficient way to prevent scams is to occupy managerial bandwidth is highly dubious. Second, if managers are the ones determining the complexity of the banks liabilities, why would they choose

[15] For example, managers, being human, do have "limited bandwidth." The assumption that scam opportunities are possible cannot be ruled out, although one can question how important this is. It should be noted that whether or not managerial malfeasance is important or not in actual practice, this is an assumption that is made in various forms in many papers in banking, most notably Calomiris and Kahn (1991), which is discussed above. The assumption that more complex liability structures require more managerial attention is plausible.

something that prevents them from profiting through scams or, more generally, why should they chose something that disciplines them? Third, if the board of directors forces the complex capital structure on the manager to prevent scams, why have we never heard this given by board members as an explanation for actions taken? And so on. When I began writing this essay I was determined not to use the phrase “common sense,” but it is unavoidable: simple common sense (based on knowledge of the world we live in) tells us that the bandwidth model is not one to be taken seriously. It would not be an efficient allocation of theorists’ time to develop the bandwidth model or models like it, nor would it be efficient for empiricists to allocate time and effort to test such a model.

The bandwidth model is, by design, manifestly silly. It clearly does not pass through the real world filter. However, if one insists that we shouldn’t reject models based on the reasonableness of their assumptions and also insists that unless the testable hypotheses of a theoretical model are rejected in a formal empirical test, that model has the same standing as any other theoretical model, then one has no grounds for treating the bandwidth model as a silly model. For someone taking this sort of view, the bandwidth model must be taken “seriously.” The fact that most – I hope all – people would not take the bandwidth model seriously, means that at least for extreme cases like this one we do apply the real world filter. The problem, and my reason for writing this essay, is that there are chameleons: theoretical models that, while not as silly as the bandwidth model, nevertheless have dubious connections with the real world and yet seem to be given a pass around the real world filters and are taken as serious descriptions of reality, at least by some.

7. Just Because We See It, Doesn’t Make it Optimal

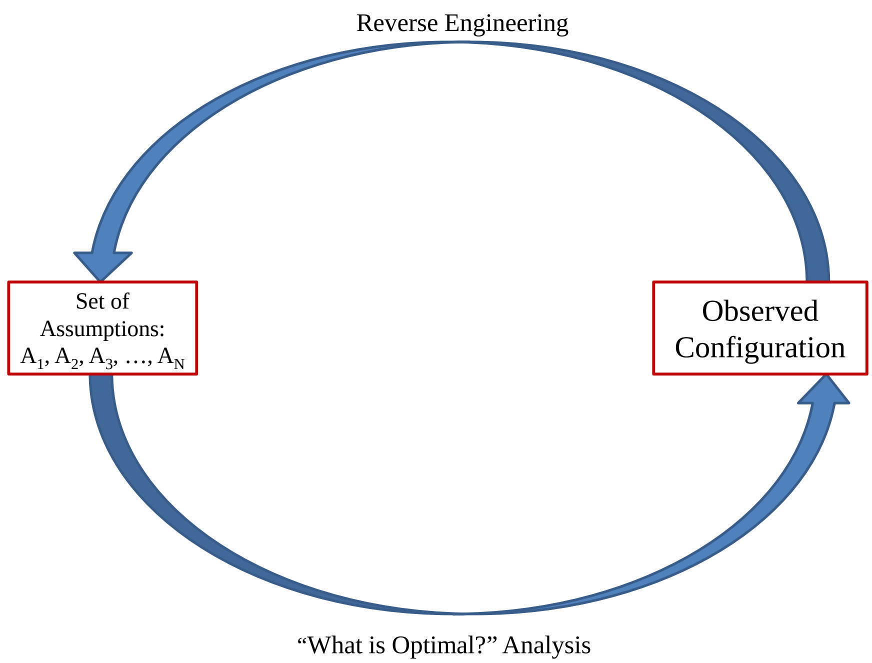
As economists we have the strong and generally well-founded belief that vast amounts of money will not be left on the table. In many situations economic agents will find ways to change inefficient organizational structures or practices or overcome various obstacles that reduce the size of the total pie. This means that when we see contracts written in particular ways or a set of transactions carried out in some manner, it is reasonable to think that these are not random outcomes but are instead solving some potentially important problems. Whereas the money-will-not-be-left-on-the-table hypothesis has a very strong predictive track record, it certainly does not follow that everything we see is optimal.[16]

[16] It is important to distinguish between what is privately optimal (i.e., what maximizes the size of the pie available to those parties directly involved in contracting) versus what is socially optimal (where all parties, not just those involved in contracting, are taken into account). Whereas it is uncontroversial that private contracting does not always lead to a social optimum, the distinction between private and social is sometimes glossed over and at times it seems to be implicitly assumed that what emerges will be socially optimal. My point here is that even the assumption that the outcome will be the privately optimal one is questionable in many contexts.

Assume we observe something in the real world, such as a contract or an organizational structure. Call this an "observed configuration." As shown in figure 3, it is quite often possible for someone to "reverse engineer" a set of assumptions (A_1, A_2, A_3, …, A_N) under which the observed configuration will be optimal.

Now if we start by assuming that the reverse-engineered assumptions hold and ask what is optimal, the analysis will necessarily show that it is the observed configuration that is optimal.

Figure 3

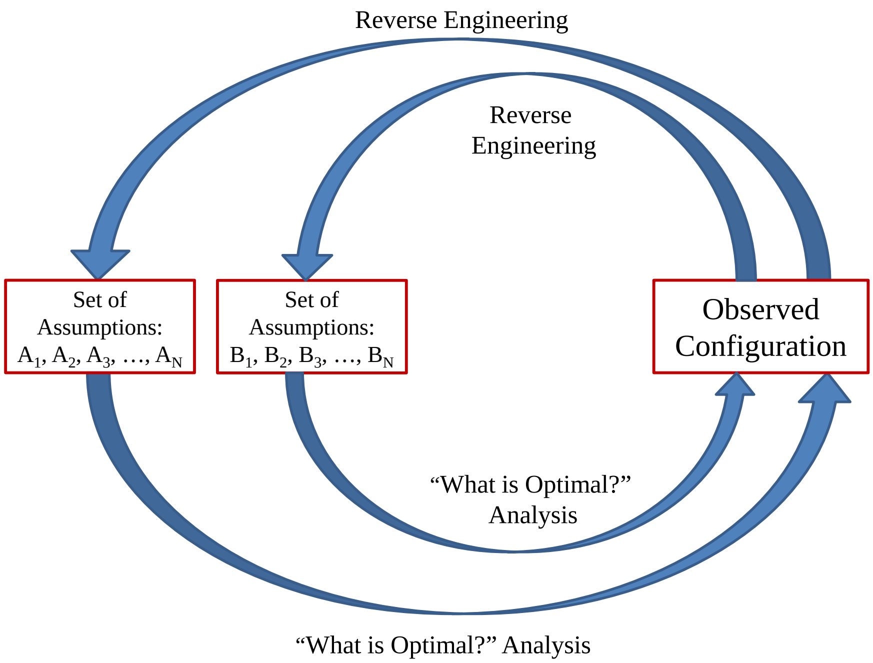


This can give rise to the following flawed argument. Starting with the observed configuration we loosely "infer" that assumptions (A_1, A_2, A_3, …, A_N) must hold under the notion that what we see must be optimal, which means that assumptions (A_1, A_2, A_3, …, A_N) "must" hold otherwise we wouldn't observe the observed configuration. Having accepted the assumptions, we then have further support for the notion that economic agents find optimal solutions, since the observed configuration is a fine example of how agents have optimized. When all this is stated in the same paragraph, as I have done here, it is easy to see the "bootstrapped" nature of the argument, which makes it fallacious.[17] The problem is that the argument is never presented in a single paragraph. Rather it is loosely developed with planks of the argument not necessarily made in the

[17] The argument involves the fallacy of affirming the consequent. From the two claims "if P then Q" and "Q", we cannot conclude "P." Thus even if (A_1, A_2, A_3, …, A_N) implies the observed configuration, the fact that observed configuration holds does not allow us to conclude that (A_1, A_2, A_3, …, A_N) hold.

same paper or same context. For example, as was discussed above, Diamond and Rajan develop a model in which they essentially ask what set of assumptions will make short-term debt and fragility an optimal solution (at least in a second- or third-best sense). Once their theoretical model is developed and published, people in other contexts assume that these assumptions must in a rough way hold (since we see short-term debt being used) and then loosely argue that if banks forced to rely less on short-term funding this will be moving them away from what is an optimal configuration and will potentially impose a cost. The circularity is still there, but not laid out all in the same place.

Figure 3A



Even if we can legitimately assume that the observed configuration is optimal, we won't generally be able to infer what set of assumptions "must hold," since in most cases there will be several sets under which the observed configuration would be optimal, as illustrated in Figure 3A, in which two sets are identified as possible candidates. The assumptions are sufficient but not necessary.

Given all this it is should be clear that a person who comes up with a set of assumptions under which an observed configuration is optimal cannot legitimately claim to have *explained* the observed configuration. In figure 3A we have two competing explanations and potentially there are many others. We clearly can't "test" the model that incorporates assumptions (A_1, A_2, A_3, …, A_N) by asking if the optimal configuration is observed – the model was built to "explain" why an *already observed* configuration is "optimal." What we can do, of course, is ask whether assumptions (A_1, A_2, A_3, …, A_N) are reasonable. Do they pass through the filter when we hold them up to all the background knowledge we have about the real world?

8. The Limitations of "As If"

In Friedman's "Essay on Positive Economics" we are told that we can understand the actions of an expert billiard player by assuming he

> "made his shots *as if* he knew the complicated mathematical formulas that would give the optimum directions of travel, could estimate accurately by eye the angles, etc., describing the location of the balls, could make lightning calculations from the formulas, and could then make the balls travel in the direction indicated by the formulas."

The strength of Friedman's "as-if" argument as applied to the billiard player rests on two very important features of his example:

1. In learning to play, any expert billiard player has taken many shots and for each one he has received almost immediate feedback on whether the shot was successful or not. This has allowed him to experiment with and fine tune the angles he takes, the amount of top spin he applies, and so on. If, as it is sometimes alleged, it takes 10,000 hours to become an expert and if a billiard player takes approximately 100 shots in an hour, then an expert billiard player has run about one million of these experiments, almost immediately seeing the result in each case.

2. We know all the important details of the game the billiard player is playing. We know the weight of the balls, the size of the table, and importantly the objective of the player (to win the game according to its rules). This means that if we presented all this information to a physicist and asked the physicist to predict, for example, the angle the expert billiard player would hit the cue ball to put a particular ball in the side pocket, the physicist could derive and successfully use the appropriate formulas to make the prediction.

Now consider the "as-if" argument as it is used in some models in finance and economics. As an example consider dynamic models of a firm's capital structure. In one paper on this topic[18] we encounter the following formula for the firm's equity at a particular node on a grid used to numerically solve the stochastic control problems that arise in the model:

[18] "A Dynamic Model of Optimal Capital Structure" by Sheridan Titman and Sergey Tsyplakov, *Review of Finance*, 2007, pages 401-451

$$E_{(t-\Delta t)}(p,A,d)=\max_{i\geq 0}\left[CFE_{(t)}(i)\Delta t+e^{-r\Delta t}\mathrm{E}_Q\left[E_{(t)}\right]\right]$$

$$=\max_{i\geq 0}\left[CFE_{(t)}(i)\Delta t+E_{(t-\Delta t)}(p,A,d)+\Delta t\hat{L}\left[E_{(t)}(p,A-\gamma A\Delta t+i\Delta t,d)\right]\right]$$

where

$$\hat{L}[Z]=\frac{1}{2}\sigma_p^2 p^2 Z_{pp}+(r-\alpha)Z_p+(-\gamma A+i^*)Z_A-rZ$$

and where i^* is the investment strategy that solves the maximization problem above.

The model is obviously quite complicated and the "formulas" that economic agents are assumed to solve are quite complex. Not only must agents be able to (in some sense) solve the dynamic programming problem, they must also be able to estimate values for the various parameters (e.g., α, σ_p^2 and γ). One could take this as a normative or policy model (i.e., this is the problem corporate managers *should* be solving). However, in this particular paper and in papers like it, the model is presented as a contribution to positive theory (the model is intended to help explain what we see in practice). Indeed, the authors of this paper attempt to calibrate their model and compare results with empirical regularities. They claim that "regressions estimated on simulated data generated by our model are roughly consistent with actual regressions estimated in the empirical literature."

If this model is to be taken as a contribution to positive theory, then it is clear that the argument must be an "as if" argument. The assumption is that chief financial officers (CFOs) and corporate boards are making capital structure decisions "as if" they are calculating, among other things,

$$\max_{i\geq 0}\left[CFE_{(t)}(i)\Delta t+e^{-r\Delta t}\mathrm{E}_Q\left[E_{(t)}\right]\right]$$

Although few, if any, CFOs are well versed in the principles of dynamic programming, perhaps we can assume that, like expert billiard players, CFOs play the capital structure game *as if* they are numerically solving stochastic control problems. But at this point we must be mindful of the huge differences between the contexts in which CFOs operate and the contexts in which billiard players become experts. Unlike an expert billiard player, the CFO takes only a few significant capital structure "shots" in a year and unlike the billiard player the CFO does not get immediate feedback on whether he has made the right decision (no equivalent of a ball going into the side pocket two seconds later occurs when the CFO raises debt and buys back equity). Indeed, a good case can be made that the CFO essentially receives no feedback or at best the feedback is very noisy. How does the CFO learn to solve in an “as if” manner these complex equations when he is able to run so few experiments and receives so little feedback?[19]

Another critical difference between CFOs and billiard players relates to the fact that we know in quite precise detail the game the expert billiard player is playing, but we don’t know all the details about the “game” the CFO is playing. A group of physicists all working independently will come up with basically the same explanation (set of formulas) for how an expert billiard player will hit the ball in a given circumstance. This is because the table dimensions are precisely given along with all of the other details of the physical setting and what counts as success is also well known since it is given by the explicit rules of the game. A group of financial economists all working independently to predict what an “expert” CFO is doing will come up with many different and conflicting descriptions and formulas. This is because the “game” a CFO is playing (even if we assume he is a superhuman “expert” and able to solve the most complex programming problems) is played in an environment that is not precisely defined (certainly not in the same way as the billiard table) and we don’t reliably know what the objectives are (is shareholder value being maximized or are the conflicting preferences of the managers playing a role?).

Those who put forth dynamic capital structure models must first conjecture the nature of the game that is being played and the dimensions of the table it is being played upon. They must then assume − if the model is to be taken as a positive model − that the CFOs and other decision makers who are playing the postulated game make decisions *as if* they are able to solve the complex dynamic programming problems.

The question one should ask is whether these are reasonable assumptions. What basis do we have for believing that CFOs make decisions “as if” they have solved complex stochastic control problems? I have heard claims that we might be able to rely on some “wisdom of the crowd” phenomenon or similar effect that would make “as if” true “on average” for CFOs solving stochastic control problems. It is true that if 100 people are asked to guess how many jelly beans have been placed in a jar, the average of their guesses is likely to be close to the true number and will be better than most individual

[19] One might be tempted to argue that whereas an individual CFO runs only a few experiments with limited feedback, more information is potentially available if we consider all the experiments run by CFOs. Perhaps over time this information somehow gets aggregated into “rules of thumb” that become part of the lore passed down to CFOs. Theorists who formulate, calibrate and solve complicated dynamic programming problems are thus shedding light on phenomena that emerges through CFOs following these rules of thumb. There are two problems with using this as a justification for taking an “as-if” approach. First and most important, complicated solutions to dynamic programming problems that are based on multiple parameters generally can’t be encapsulated in simple rules of thumb. Second, if we are interested in predicting how capital structure decisions *are* made (and not on how they *should* be made) and we believe that they are made by CFOs following rules of thumb, wouldn’t it be easier (and more reliable) to survey CFOs and ask what rules of thumb they are following?

guesses. However, if we ask 100 or even 1,000,000 people to guess the 10^{100}th digit in the decimal expansion of pi, the average guess will be no better than any individual guess. We have good reason (and evidence) to believe that an individual (even a 10 year old) can come up with a reasonable guess of the number of jelly beans in a jar and that by averaging these guesses the errors tend to be "cancelled out." We could assume that people can somehow estimate the 10^{100}th digit of pi "as if" they were super computers vastly more powerful than what we have available today, but clearly this "as if" argument should give us no confidence in the average guess for that digit. We simply have no foundation for assuming that individuals have any ability to divine the identity of the 10^{100}th digit of pi. What foundation do we have for believing that CFOs and other managers can solve extremely complex programming problems that ultimately must be solved numerically on a computer using programs that took weeks or months for a researcher to write? At the very least it would be reasonable to ask some CFOs if these are the problems they are solving and ask how they go about solving them. [20] Solving these problems is admittedly less difficult than determining the 10^{100}th digit of pi, but it is still far from obvious that it is sensible to assume that CFOs are solving them, even heuristically.

All of the arguments I have heard for why we can assume CFOs have extraordinary "as if" abilities seem quite weak, certainly much, much weaker than the "as if" argument applied to the billiard player whose "as if" abilities are based on thousands of hours of play and immediate feedback. I am certainly not asserting that the "as if" argument never applies in understanding what economic agents are doing, but I am asserting that it requires more support than hand waiving or wishful thinking. "As if" assumptions must be held up against what we know about the real world. They must pass through the filter.[21]

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[20] When I have suggested that it might be reasonable to ask CFOs what stochastic control problem they think they are solving, the response I get is often one that either invokes Friedman's claim that you shouldn't judge a model by the realism of the assumptions or uses a Friedman-like "as-if" argument that CFOs behave (on average) as if they are able to solve these complicated problems.

[21] While I have focused on capital structure decisions to provide examples where the applicability of "as-if" arguments is questionable, there are many other cases where we assume that economic agents are solving very complex problems with limited feedback. In many cases the "as-if" assumptions behind these assumptions deserve more scrutiny than they routinely get. For example, in the context of asset pricing theory models it is often assumed that investors are able to solve complicated dynamic optimization problems involving complex preferences over lifetime consumption streams and various risks that are hard to measure. While it is true that many investors turn these problems over to professional investment managers, who perhaps are a bit more likely to have the ability to solve these problems than these "naive" investors, it is still questionable whether these financial advisors and professional money managers are solving the investment problems posited in many of these asset pricing models even if they had the ability to do so. First, these managers have incentives of their own and it is not clear that these are aligned with the principal investors. Second, they must ultimately market their services to investors in ways that these investors can understand and generally have their performance measured in simpler ways than would be consistent with the posited models. For more discussion see Cornell and Hsu (2014).

9. Subtlety, Mathematical Elegance, Complexity and Tractability

Many important results in economics and finance are not obvious. To a trained economist the theory of comparative advantage in trade is transparently simple; to many people it is puzzling and hard to accept. The Modigliani & Miller irrelevance results are quite difficult to explain even to the best students and are fully internalized by only a few. Many results in finance and economics are counterintuitive and hinge on complicated interactions and tradeoffs.

Theories that are "deep" and produce surprising results are intrinsically interesting and no doubt are fun to develop and present to others. Explanations that involve subtle and complex reasoning seem to have, at least in some people's minds, a higher status than those that are simpler and more straightforward. Of course, subtlety in and of itself should not be taken as a virtue when it comes to judging how much a model or theory contributes to our understanding of the world. There is no reason to assume that the explanation for something must be subtle and no reason to assume that a subtler and deeper model is necessarily better than a less deep and less subtle competitor.

Since not much prestige and honor come to those who put forth simple and obvious models to explain or understand economic phenomena, there plausibly is a distorting bias toward models that are deep and subtle. Closely allied with this is a preference for mathematical "elegance" or "sophistication".[22] Mathematics has provided powerful tools for analyzing difficult problems arising in finance and economics, and I am certainly not suggesting that we should restrict ourselves to "verbal" reasoning. I am also not suggesting that subtlety and mathematical elegance are things to be avoided: the correct explanation for something may indeed be quite subtle and even mathematically elegant.[23] My simple point is that the clever use of mathematical tools and the attainment of subtlety should not be the driving forces in developing models. Most importantly, when we take a model off of the bookshelf with the intention of applying it to the real world,

[22] Related to this is Ricardo Caballero's observation (Caballero, 2010) that a potentially misplaced premium has been put on precision, at least among macroeconomists using DSGE models. His claim is that there is a tendency to confuse the precision these models have achieved about their own world with a precision that can be applied to real one. I take this as a claim that these models have become in a sense chameleons, where the precision of their results is falsely applied to the real world since a full appreciation of the limits of the models that comes from passing them through real world filters has not been kept in mind.

[23] Among some physicists there seems to be a belief (perhaps just a hope) that the ultimate explanations of our physical world will be mathematically elegant and simple. Explanations that lack this quality are often viewed with suspicion. I am not aware of anyone who makes the same sort of claim that the ultimate explanations in the social sciences will necessarily be mathematically elegant and simple and there seems to be little reason to believe they will be. In the social sciences it seems clear that mathematical elegance is neither necessary nor sufficient and can't be a guiding criterion.

subtlety and mathematical elegance have no bearing on how well the model captures what is important in the actuality. The quotation “Make things as simple as possible, but no simpler” is often attributed to Albert Einstein (although the evidence for this is less than compelling).[24] In my view an important corollary of this is: “Make things as subtle as necessary, but no subtler.”

In the same way that subtlety and mathematical sophistication do not − in and of themselves − make a model better for explaining our actual world, neither does complexity. Whereas a model may need to involve quite a few “moving parts” to capture the important economic factors at play, models that are built on a larger number of dubious assumptions are not any more useful than those built on fewer. Although the actual world is complex, this doesn’t mean that any move toward complexity in modeling necessarily moves us closer to understanding the economic phenomena we are considering. I am not aware of anyone who explicitly argues that complexity is a virtue unto itself, but I do see complexity sometimes serving as a smokescreen that allows chameleons to emerge. A simple model with a dubious assumption that drives the results is likely to be recognized for what it is. A much more complex model with many assumptions, including the aforementioned dubious assumption, is more difficult to evaluate. Not only will the dubious assumption not stand out as prominently as it does in the simple model context, it will often be more difficult to determine how important it is in driving results. This is particularly true in complex models that are numerically calibrated to approximate what we observe. The importance of the dubious assumption to the success of the calibration exercise will not always be apparent and the success of the calibration exercise may be used in an offhand way to gloss over the dubious nature of the assumption. Of course, these problems are only magnified when the model is composed of many dubious assumptions.

Finally, assumptions are often defended as being necessary for tractability. Everyone realizes, of course, that an assumption made for tractability is quite possibly an assumption that takes us away from what might be important in the real world and potentially reduces the applicability of the model and its results. We all know that it makes no sense to hunt for lost keys under the bright streetlight when we know the keys were lost many yards away in the darkness. Despite this and perhaps arising out of sympathy for the theorist and the knowledge of how hard it is to come up with tractable models, there is quite often a willingness to downplay the gap between tractable assumptions and what we know about the real world. If the goal is to build a bookshelf model that through its logical argument might give us some intuitions, these tractable (“or

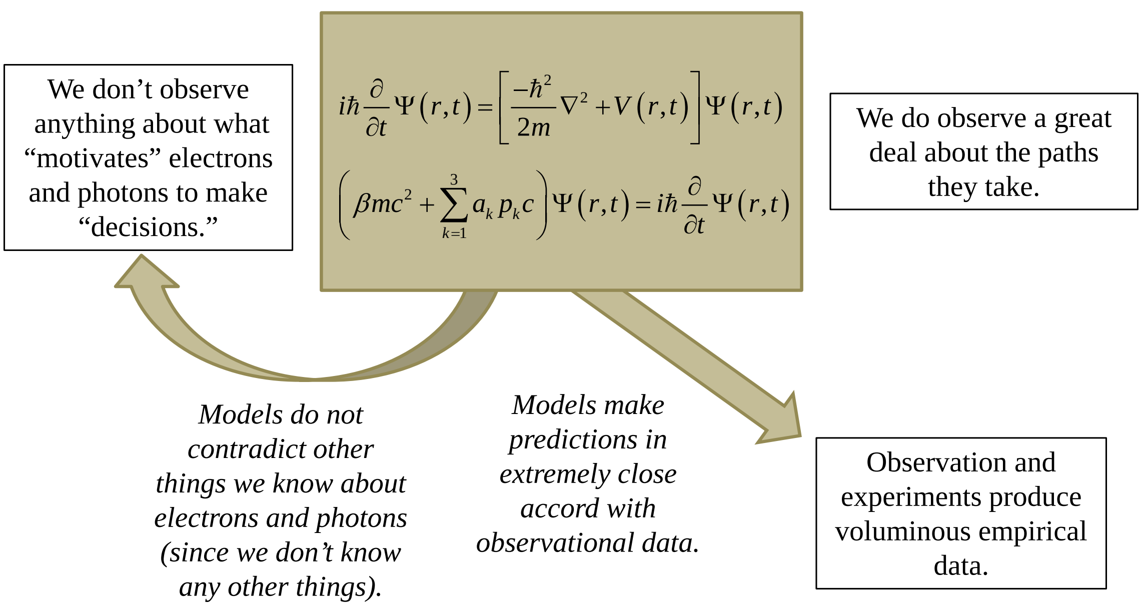
[24] The notion that simplicity is desirable can be traced back to Occam’s Razor and the dictum that “entities should not be multiplied beyond necessity.”

stylized") assumptions may be a virtue and not a problem. But if the model is to be applied to the real world, tractability, like subtlety, mathematical elegance and complexity, should not be a free pass that allows the filter to be circumvented.

10. Why Models in Finance and Economics are not Like Models in Quantum Mechanics

I have argued above that before applying theoretical economic models to the real world we need to critically assess them by seeing how well they pass through real world filters. It might be thought that this is not the practice in the hard sciences. In particular it might seem that the success of models in fields such as quantum mechanics shows that we should not judge models by the reasonableness of their assumptions or the model's plausibility. After all, almost everything in quantum mechanics seems to defy common sense. Richard Feynman is reputed to have said, "If you think you understand quantum mechanics, you don't understand quantum mechanics." The models of quantum mechanics are well accepted because of the strength of their predictions, not the "reasonableness" of their assumptions.

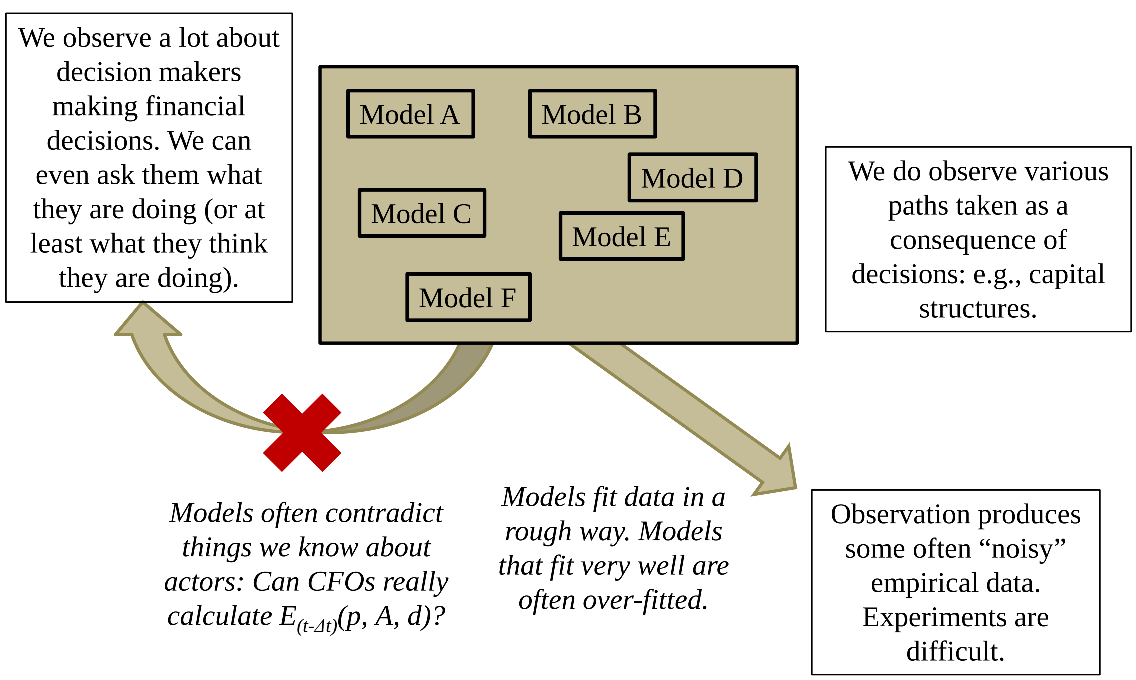
Figure 4



There are, however, huge differences between the settings in which the models of quantum mechanics are applied and those in which we attempt to apply economic models. Figure 4 depicts how models are used in quantum mechanics. We don't get information about what "motivates" electrons and photons to make "decisions," but we do see the outcomes of those "decisions" in the paths they take and other observational evidence. Of course, it is silly to anthropomorphize electrons and photons and think of them as making decisions. The main point is that all of our observational evidence is based on interactions these particles have with other particles and we can produce voluminous data through observation and experiments. Models in quantum mechanics have been extraordinarily successful in making predictions, with these predictions being consistent with what is measured in experiments to many decimal places.

Most importantly, the models in quantum mechanics don't blatantly contradict anything we know about electrons and photons. They are basically consistent with all that is observed. If a systematic inconsistency between what the models assume and what is observed were to emerge, the models would generally be viewed as incomplete or flawed and efforts would be made to make corrections.[25]

Figure 5



[25] It is, however, well understood that in their current states quantum theory cannot be unified with the theory of general relativity and this has led to a search for a quantum theory of gravity that describes gravity in quantum mechanical terms.

Now consider models as they are often used in finance or economics. Figure 5 is, I believe, a fair depiction of what we face in many cases. We often have competing models (e.g., models A through F) that are all roughly consistent with *some* aspects of what we observe (e.g., certain patterns of capital structure decisions). The amount of data we have is often (but not always) quite limited and endogeneity problems generally make it challenging to test and discriminate among models. (We are justifiably suspicious of models that fit the data extremely well, since we suspect they may be over-fitted.) A major issue is that the models are often based on assumptions or have implications that contradict things we know about the economic decision makers or other economic phenomena we observe. As I have argued at length above, simply ignoring or dismissing these contradictions is not justified and is not a good practice for developing models and theories that are useful for advancing our understanding of our economy.

Economic agents (i.e., human beings) are much more complicated than electrons and photons and they interact in environments that are much more complex and constantly changing. It is even possible that the development of an economic theory or the publication of an academic paper in economics can change how these economic agents behave. The same cannot be said about electrons or photons changing their behavior based on something written by a physicist. This means that developing useful models in economics and finance involves challenges not found in quantum mechanics and other research areas in physics and the hard sciences, where much more data are generally available and, at least in many cases, controlled experiments are possible.

Because of these greater challenges, models in economics and finance will of necessity be simplifications and will abstract from much of the complexity of the domain they are designed to explain. This means that there will be tensions between what these models assume and things we know are true. The fact that this will be the case does not mean that we should ignore these tensions. When the major drivers of a model's results cannot be connected to things we see in the real world, we are surely justified in questioning how useful the model is as an explanation of what we see. This is true even when the predictions of the model are supported by some empirical tests. When the model ignores some factors that we have good reason to believe are of first-order importance, we likewise are justified in questioning the model's usefulness. Confronting these tensions head on is surely a better way of making progress than glossing over them.

11. Conclusion

The arguments that I make in this essay are all based on a simple and − I believe − uncontroversial claim: *when using economic models to understand the economy we should not ignore or dismiss important background knowledge we have about the real world.* Whereas some theoretical models can be immensely useful in developing intuitions, in essence a theoretical model is nothing more than an argument that a set of conclusions follows from a given set of assumptions. Being logically correct may earn a place for a theoretical model on the bookshelf, but when a theoretical model is taken off the shelf and applied to the real world, it is important to question whether the model's assumptions are in accord with what we know about the world. Is the story behind the model one that captures what is important or is it a fiction that has little connection to what we see in practice? Have important factors been omitted? Are economic agents assumed to be doing things that we have serious doubts they are able to do? These questions and others like them allow us to filter out models that are ill suited to give us genuine insights. To be taken seriously models should pass through the real world filter.

Chameleons are models that are offered up as saying something significant about the real world even though they do not pass through the filter. When the assumptions of a chameleon are challenged, various defenses are made (e.g., one shouldn't judge a model by its assumptions, any model has equal standing with all other models until the proper empirical tests have been run, etc.). In many cases the chameleon will change colors as necessary, taking on the colors of a bookshelf model when challenged, but reverting back to the colors of a model that claims to apply the real world when not challenged.

While I have given some examples of chameleons above and cases where I think it is quite clear that assumptions are not subjected to the real world filter as much as they should be (e.g. some "as-if" assumptions), I am certainly not claiming that filters are never employed in practice. My experience has been that when papers are presented at seminars and conferences, the assumptions behind these papers' models and theories are generally held up to the real world and challenged, often aggressively, by a skeptical audience. Although this is true, I still see numerous cases where chameleons are given a pass. For example, I have often heard an assumption defended with the claim "this is the standard assumption in the literature." Indeed, the speaker will sometimes attempt to preempt any need to defend the assumption by introducing it with the phrase "as is standard in the literature." If the model is offered simply as a bookshelf model, then assuming what is standard in the literature is not a major problem. If the model is to be used *only* as a bookshelf model, it can viewed as facilitating further exploration of the "logically possible" terrain in the vicinity of what other authors have explored when they made the same "standard-in-the-literature" assumptions. However, if we are asked to take the model as something that can be applied to the real world, an assumption cannot be simply defended with the claim that others have assumed it.

At one conference I attended not too long ago, I heard a discussant of a paper say that the paper's author had done a very poor job trying to defend one of his model's assumptions. The discussant then said that the author should "just make the assumption and move on." The implication seemed to be that since the assumption in question was difficult to defend, it would be best not to call a reader's attention to this by mounting a very weak defense. Perhaps this was "good" advice for building a bookshelf model and sneaking the paper past a referee, but I would characterize it as a strategy to make the paper's model a chameleon, a model that is intended to circumvent the filter.

As I have argued above, although a model may be internally consistent, although it may be subtle and the analysis may be mathematically elegant, none of this carries any guarantee that it is applicable to the actual world. One might think that the applicability or "truth" of a theoretical model can always be established by formal empirical analysis that tests the model's testable hypotheses, but this a bit of a fantasy. Formal empirical testing should, of course, be vigorously pursued, but lack of data and lack of natural experiments limit our ability in many cases to choose among competing models. In addition, even if we are able to formally test some hypotheses of these competing models, the results of these tests may only allow us to reject some of the models, leaving several survivors that have different implications on issues that we are not able to test. The real world filters will be critical in all these cases.

I suspect that some people will be uncomfortable with the idea of applying real-world filters to models, mainly because there are no readily available formal criteria for determining what these filters should be and no unambiguous way to set the threshold that determines what should be allowed to pass through them. For some this will seem too subjective and therefore inadmissible into what we like to think of as a field of inquiry that uses "objective" methods. This is not a tenable objection. As I mentioned above, the filters I am discussing can be interpreted as Bayesian priors. Any reasonable person puts an extremely low (essentially zero) prior on the height of male actors in year $T\text{-}1$ determining the U.S. inflation rate in year T. It would take an extraordinary amount of data to the contrary to move these priors in a material way. Ruling out the use of filters on the basis of them being too subjective, means that we are effectively saying that we must evaluate all models based on an agnostic (uniform) Bayesian prior, whatever that might mean. (It is "equally likely" that U.S. inflation is determined by the height of male actors as it is that inflation is determined by actions taken by central banks.) Obviously we don't in practice conduct our research as if we have these agnostic or uniform priors. We routinely take these priors into account. Now it is true that people don't have exactly the same priors and “reasonable people can disagree,” but this doesn’t open the door for taking seriously models whose assumptions are severely athwart what we see in the real world and ignore factors we know are of first order importance. A model that claims that “high” leverage is optimal for banks while ignoring almost everything that we know is important is a bookshelf model. It is not a model that can pass through any reasonable real-world filter because it is at odds with all of the background information we have about the world, information that we use to form reasonable priors.

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