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The Capital Structure Puzzle: What Are We Missing?

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Abstract

An important piece of the capital structure puzzle has been missing, and it is not a contracting friction. It is recognition that managers do not have sufficient knowledge to optimize capital structure with any real precision. The literature critique in this paper i) identifies the conceptual sources of the main empirical failures of the leading models of capital structure and ii) shows how those failures can be repaired by taking into account imperfect managerial knowledge and several other factors. The analysis yields a compact set of principles for thinking about capital structure in an empirically supported way.

I. Introduction

The Holy Grail of corporate finance is a theory that explains the capital structure behavior of real-world firms. It's been 63 years since Modigliani and Miller's (MM) (1958) landmark paper and we still do not have a model that explains even the broad-brush features of observed capital structures (e.g., Fama and French (2005), Graham and Leary (2011)). Nor have we had much success in descriptive work, with a wide-ranging set of studies struggling to pin down significant empirical drivers of capital structure (e.g., Rajan and Zingales (1995), Lemmon, Roberts, and Zender (2008), and Frank and Goyal (2009)).

The unfortunate reality is that the literature has stagnated without any real clarity about what is the truly important "cake" and what is peripheral "frosting" for understanding capital structure behavior. What we have instead is a laundry list of frictions—taxes, distress costs, asymmetric information, agency costs, etc.—that might someday be sutured together (in some as-yet-unspecified way) into a model that can explain observed capital structures. It takes a theory to beat a theory and so, given the vacuum of empirically credible competing models, the trade-off and pecking-order models of capital structure continue to dominate the empirical literature and textbooks, even though both have serious empirical shortcomings.

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This paper distinguishes the "cake" from the "frosting" for analyzing capital structure and yields a viable path out of the literature's stagnation. I identify the conceptual sources of the empirical failures of the leading models, and delineate model features that would repair those failures. In the process, I explain why we should largely ignore Miller's (1977) "horse-and-rabbit-stew" view of the tax incentive to lever up and Jensen's (1986) view of the disciplinary role of debt. The analysis yields a compact set of foundational principles for building an empirically credible theory of capital structure.

I argue that our failure to solve the capital structure puzzle reflects a major Catch-22: The formal analytical (optimization) approach that is used in our leading models inherently ignores—and therefore implicitly rules out—the key to explaining real-world capital structure behavior. By insisting that our models be framed in a way that implicitly precludes a key element of the solution, we have inadvertently ensured that the literature has stagnated far short of a solution to the capital structure puzzle.

What we have mistakenly ruled out is the role of *imperfect managerial knowledge*: Managers do not have sufficient knowledge to optimize capital structure with any real precision.

The imperfect-knowledge view departs radically from the prevailing paradigm in which all of the leading capital structure models are framed with *full-knowledge optimization*: Managers are endowed with perfect knowledge of how to optimize financial policy without any cost or effort on their part. Managers know the "correct" model and the exact parameter values for the stochastic investment opportunity set, contracting costs, all other relevant frictions, and capital-market-pricing conditions. That information enables them to calculate the state- and date-contingent path of optimal capital structure decisions.

The full-knowledge-optimization paradigm has had a long trial period and its ability to explain observed capital structures has been quite disappointing. Despite 60-plus years of research using state-of-the-art empirical methods and highly sophisticated theoretical analysis, financial economists remain clueless about how to identify whether any given firm has a uniquely optimal capital structure, much less how to isolate with precision what that unique optimum might be. To be clear, it's not that we haven't identified many plausible determinants of capital structure in the last 63 years. We surely have had many interesting theoretical insights. It's that we have been unable to apply those insights in a way that comes anywhere close to explaining real-world capital structures. As Graham and Leary ((2011), p. 381) aptly conclude in the summary section of their comprehensive literature review: "It's not clear what it all adds up to."

In any case, my point here for motivating this paper's argument is: It makes no sense to cling to the belief that full-knowledge optimization is a good approximation to actual managerial behavior given that, despite 6 decades of careful study, financial economists know virtually nothing about the precise nature of optimal capital structures. On the contrary, our inability to come close to solving the capital structure puzzle makes a compelling case for treating imperfect managerial knowledge as a factor of first-order importance when analyzing financial policy.

The case becomes even more compelling if we listen to what managers say about their inability to optimize financial policy with precision. Consider the view of Alfred P. Sloan, Jr., the iconic President of General Motors, who is honored in the names of MIT's Alfred P. Sloan School of Management and the Memorial Sloan Kettering Cancer Center. Sloan, who is almost surely the most influential corporate manager of the modern industrial era, stated (with emphasis added):

> The strategic question in industrial finance, assuming you have something to work with in the way of a going business, is how to optimize its elements. *The latitude for opinion, or subjective judgment here, is wide.*

The quote is reported in DeAngelo (2021), a clinical study of the financial policies articulated (and implemented) by Sloan at GM and by Henry Ford at Ford Motor Co. That study documents widely different financial policies adopted simultaneously by Sloan and Ford, even though their firms were in the same business. Such differences cannot be explained by any model in which financial policies are uniquely determined by economic fundamentals. That includes the leading capital structure models, all of which assume full-knowledge optimization, with financial choices determined solely by fundamentals.

What is a sensible alternative to full-knowledge optimization? A truly holistic approach would treat the production of knowledge about how to optimize capital structure as an *economic problem itself*, not something that can be assumed away, as it is in the leading models, which all assume that managers are endowed with all the information needed to solve an analytically complex optimization problem.

This holistic view points to a process of financial policy experimentation by managers in the face of imperfect knowledge that leads to the culling of policies that are revealed to be clearly inferior. This process seems a far more plausible foundation (than full-knowledge optimization) for thinking about managerial decisions about financial policy. It has overtones of Darwinian selection, which has clear precedent in economics generally with Alchian's (1950) foundational contribution and with applications in the finance literature, most notably Miller's (1977) view that neutral mutations are important sources of capital structure variation and Lo's (2017) adaptive-markets view.

I use the term "baseline model" as the shorthand label for my answer to the question: What sort of economically sensible model features would repair the empirical failures that plague the leading models and thus provide a framework that explains the main capital structure regularities?

I arrived at the features of the baseline model *inductively*, that is, I followed the "invert, always invert" dictum of Jacobi, the famous German mathematician, whose general approach to problem-solving has been championed by Charlie Munger. I knew the main facts about capital structure, including the failures of the leading models, and I understood the conceptual structures of those models. I then "backed out" the baseline model by thinking about economically sensible features of frameworks that wouldn't suffer from those failures. I don't present the analysis in that order, but that expositional decision reflects a desire for clarity, not an attempt to portray the baseline as derived de novo from a set of axioms.

This process made it clear that an empirically credible model of capital structure must:

i) Emphasize reliable access to funding, not optimizing the debt–equity mix;
ii) Recognize that chosen financial policies are not pinned down uniquely by economic fundamentals;
iii) Include incentives for a connection between debt issuance and investment;
iv) Incorporate a role for firms having, using, and replenishing "dry powder" (untapped debt capacity and cash balances) to meet funding needs;
v) Accommodate managers acting on a belief they can time the capital markets; and
vi) Include a nontrivial role for costly financial intermediation in providing reliable access to funding for operating firms.

These six properties motivate the specific features of the baseline model, with ii) and v) key to why imperfect managerial knowledge is so important. When managers have imperfect knowledge about how to optimize capital structure, they will be indifferent among multiple feasible choices not because capital structure is literally irrelevant, but rather because they can't reliably detect material differences among those choices. Such cases entail what I call an indeterminacy, which I define as a situation in which there is no unique mapping from a firm's economic fundamentals to its chosen financial policy. Indeterminacies leave room for choices to reflect managerial judgment—including judgment about market timing—so that actual choices are not fully determined by fundamentals. As the discussion of the evidence (in Section VI) makes clear, breaking the functional link running from fundamentals to chosen capital structures is important for repairing the empirical failures of the leading models.

Viewed at the broadest level, the baseline picture that emerges from this analysis is: Firms focus on reliable access to funding rather than on optimizing the debt–equity mix because i) managers do not have knowledge of even a rough approximation of the "correct" (empirically relevant) model of optimal capital structure, yet ii) there is no doubt that funding is needed to produce value.

In constructing the baseline framework, I sidestep a methodological pitfall that has contributed to the literature's stagnation: I do not follow the usual methodology of starting with MM's (1958) perfect-markets set-up and examining the effect of adding a new friction. The usual approach informs us about the impact of perturbations from frictionless conditions. That would make sense if we knew that financial markets operate close to the zero-friction ideal, and have always done so. But we know otherwise. Many firms do not have access to low-cost, deep, and informationally efficient securities markets. Moreover, most models that consider perturbations from frictionless conditions imply perfectly integrated pricing of securities, which makes it irrational to engage in financial intermediation that entails real-resource costs. Yet costly intermediation is massively important as a source of funding for real-world firms.

In developing the baseline, I accordingly start with a simple banking-based framework with imperfect managerial knowledge in which i) operating firms raise equity only from founders' initial infusion and do not repurchase shares and ii) all future external funding comes from bank loans, with operating firms also able to keep cash balances on deposit at banks to be drawn down as internal sources of funding.

I show that the simple banking-based version of the baseline model can explain the main known failures of the leading capital structure models, except of course those failures that concern external-equity financing or the repurchase of shares, which are ruled out in the banking-based version. I go on to show that the equity-related behavior that extant models fail to explain is readily explained by a generalization of the banking-based structure that includes shadow banking and the development of stock and bond markets.

The upshot is that the baseline model has a clear-cut empirical edge over the trade-off and pecking-order models that dominate the literature and textbooks. It is better able to explain a variety of well-documented aspects of observed capital structures, including the relation between proactive leverage increases and investment, the nature of deleveraging and its empirical connection with cash accumulation, and the role of costly financial intermediation in supplying funding to operating firms.

The baseline model is a stripped-to-the-basics framework that explains the main known facts about capital structure and that can be used as a foundation for a more complete theory. The objective is a parsimonious foundation that gets the big pieces right, *not* a complete theory that gets all the details. The baseline excludes agency costs, collateral benefits, control-related contracting provisions, the nature of legal regimes, managerial uniqueness ("managing with style"), cultural norms about debt use, and behavioral biases. For a credible comprehensive theory, these factors will need to be added to the baseline.

My baseline argument is conceptual analysis in the spirit of Miller (1977) and Jensen (1986). These venerable studies used a combination of empirical observations and economic reasoning to challenge received wisdom about capital structure. Miller's "horse-and-rabbit-stew" argument indicated that the existence of low-leverage firms like IBM and Kodak—prominent firms that were viewed much like Apple and other tech titans are today—contradicted tradeoff models, given the high corporate tax rate and realistically low estimates of direct distress costs. Miller juxtaposed low leverage against a high corporate tax rate to motivate the idea that personal tax differences on debt versus equity would fully offset the corporate tax incentive to lever up. Jensen used the existence of highly leveraged takeovers to support his argument that firms take on debt to discipline managers to pay out, not waste, free cash flow.

Before getting into the details, I want to emphasize that I use the "model" structure as an expositional device to convey an argument whose objective is *not* to provide a stand-alone theory of capital structure or to "sell" the baseline as such a theory. My objective is to critique the literature in the most constructive way, which means identifying elements of a solid foundation for a comprehensive theory.

One needs a conceptual blueprint to build a better theory, and such a blueprint is what the paper delivers, with the baseline model serving as the expositional vehicle to convey the supporting economic logic.

Readers who simply want a compact summary of the key principles in that blueprint can "fast forward" to the paper's final paragraph.

While this paper was under submission, I received from John Graham a not-for-public-circulation draft of his AFA presidential-address-in-progress (Graham (2021)) with a request for comments. I am referencing that work here (with permission) because it is relevant to the argument I advance in this paper. Specifically, Graham (2021) examines many real-world corporate finance practices, including the choice of capital structure, and concludes that a model of "satisficing" rather than precise optimization aligns with many of the practices he documents. His evidence is important and fully compatible with the imperfect managerial knowledge argument that I present here, as is his interpretive point about managers plausibly "satisficing" instead of strictly optimizing when making decisions about financial policy. I discuss his most striking new findings in Section VI.

Here is a roadmap to the current paper. Section II isolates the conceptual shortcomings of the leading capital structure models. Sections III, IV, and V present the banking-based version of the baseline model. Section VI discusses the main known regularities about capital structure and the failures of the leading models. Section VII generalizes the baseline framework to include stock and bond markets and shadow banking. Section VIII discusses implications for capital structure analysis.

II. What's Wrong with the Leading Models of Capital Structure?

Standard static trade-off models fail empirically because they ignore funding (after the initial capital infusion) and hold investment policy fixed, which means the only capital structure decision they consider is how to divide future earnings into *payouts* flowing to debt (as interest and principal) versus equity (as dividends and share repurchases). In short, they consider only the narrow problem of optimizing the debt–equity mix of cash flowing out of the firm, while ignoring the problem of access to funding, that is, accessing cash to cover investment outlays, operating earnings shortfalls, and distributions to security holders. With future investment/operating decisions held fixed, these models ignore decisions about infusions over time and the connections among security issuances, cash buildups/drawdowns, and funding needs. These models are best reserved for understanding firms at a stage of lifecycle where funding is not important.

For understanding the preponderance of capital structure behavior by operating firms, standard trade-off models thus focus on what amounts to a very thin layer of frosting and ignore a very large cake, which is funding because, without adequate funding, firms cannot generate value. In this blanket indictment, I include all of the many static corporate tax/distress cost trade-off models that follow Robichek and Myers (1966) as well as more sophisticated tax-based models such as DeAngelo and Masulis (1980). I also include all dynamic leverage-rebalancing models that hold investment fixed (e.g., Leland (1994)) and all empirical work that focuses on gauging speeds of rebalancing to estimated target leverage ratios (see Yin and Ritter (2019) and studies cited therein). Given the evidence detailed in Section VI, there is simply no empirically tenable case for ignoring funding and focusing solely on the debt–equity mix.

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The pecking-order model of Myers and Majluf (1984) focuses on funding, which was a huge advance in the literature. However, it is a one-shot financing model, which means that firms choose the source of funds with the lowest current cost and ignore preserving and/or rebuilding debt capacity and cash balances for future use. The one-shot structure is why the model predicts that funding decisions follow a strict pecking order, which they clearly do not (Fama and French (2005), (2012), Frank, Goyal, and Shen (2020), and Denis and McKeon (2021)).

I would distinguish carefully between i) the pecking-order *model* as a stand-alone theory of capital structure and ii) pecking-order *behavior* as a general tendency for firms to favor internal over external financing and to favor borrowing over raising outside equity. The empirical failures concern the model, that is, i), not ii). There is no doubt that pecking-order behavior in the general tendency sense is a reasonable characterization of the financing decisions of real-world firms. That tendency is just not strong enough to dictate that the model does an adequate job of explaining capital structure regularities.

Dynamic trade-off models in the spirit of Hennessy and Whited (HW) (2005) repair the latter shortcoming by treating investment as endogenous in all future periods, which dictates that firms now have incentives to preserve and/or rebuild debt capacity and cash balances for future use. Introduction of the latter feature was a major advance for the capital structure literature, just as was the case with Myers and Majluf's (1984) decision to put the spotlight on funding rather than on optimizing the debt–equity mix.

Path-dependent financing behavior is another advantage that both the pecking-order and dynamic HW-style models have over standard trade-off models. Path dependency related to funding is quite clear in the data, which reinforces the idea that standard trade-off models need to be reserved only for use in special situations in which funding issues are of little or no concern.

Importantly, however, the predicted nature of the path dependencies in the pecking-order and dynamic models is itself empirically problematic. The conceptual source of the problem is that these models assume that managers have complete knowledge to select the best financial policies, which leads to the empirically problematic prediction of a functional (unique) mapping from underlying economic fundamentals to chosen financing decisions.

What is needed is a model with path dependencies, but with the exact path of a firm's financing decisions *not* uniquely determined by fundamentals. That is a critical reason why imperfect managerial knowledge has a central role in the baseline model described beginning with Section III.

With one minor exception, dynamic HW-style models have equilibrium security price regimes that rule out costly financial intermediation, and the same is true of the pecking-order model. Such pricing regimes are incompatible with the massive scale of such intermediation activity in the real world and its prominent role in funding nonfinancial firms. There are other empirical issues with these models; see Section VI.

Although the leading models all have significant shortcomings, they do (collectively) highlight three features that are important for an empirically credible theory of capital structure: i) an aversion to default (and financial distress) that encourages firms to limit leverage, ii) a focus on funding with path-dependent leverage ratios, and iii) recognition that the world is dynamic so that, in addressing funding needs, firms have incentives to preserve, utilize, and rebuild debt capacity and cash balances.

III. Basic Premise: Managers Are Concerned with Reliable Access to Funding

The baseline framework is grounded in a principle that is almost surely essential to any empirically credible theory of capital structure: Managers inherently know they need reliable access to funding since, without funding, firms cannot generate value from investment policy.

In an MM (1958) world, reliable access to funding is always available at zero real-resource cost because there are no frictions of any type and firms' equity and debt are priced and traded in strong-form efficient markets. MM's analysis ignores funding and focuses instead on whether altering the debt–equity mix of future payouts affects firm value, with the probability distribution of future earnings held fixed.

That is not to say that MM (1958) in any way imply that funding is unimportant. On the contrary, the empirically most relevant way to interpret MM is as indicating that the overwhelmingly dominant generator of value for nonfinancial firms is investment/operating policy. That principle, in turn, indicates that funding is critical since, absent adequate funding, firms cannot make the investments that generate value.

I assume throughout that reliable access to funding is not freely and universally available. The most familiar way to think of such a situation is in asymmetric information settings (Myers and Majluf (1984), Gorton and Pennacchi (1990)). Asymmetric information is not required, however. Even when everyone sees the same data, they may not agree on what those data imply about security valuation. Perhaps such situations should be called symmetric *ignorance* rather than symmetric *information*, since the latter term is often used to connote situations in which everyone agrees on the value of a security.

Terminology aside, the point is that the impediments that firms face in accessing funding do not require that some parties have an informational advantage over other parties. Even when everyone sees the same data, the difficulties of reaching agreement on asset valuation will encourage firms to use debt for external financing, much as they do in Myers and Majluf (1984). The reason is that it is easier for firms and suppliers of capital to reach a mutually agreed price on a fixed-income claim than on an equity-like claim, as the latter depends more strongly on residual profits, which are especially difficult to estimate.

The expectation is for debt issuance to tend to dominate stock issuance, but not for complete dominance of debt over equity as in the strict financing hierarchy in Myers and Majluf's formal model. The reason is that debt capacity is a scarce and valuable resource when firms face dynamic (multi-period and time-varying) funding considerations. Consequently, once we move beyond the basic one-shot funding-decision framework of Myers and Majluf, firms generally have incentives to avoid using up all of their debt capacity today, so that they have some ability to issue debt to meet future funding needs.

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IV. Basic Premise: Managers Cannot Identify Optimal Financial Policies with Precision

The foundational emphasis placed here on the informational difficulties and knowledge limitations that managers confront in selecting a capital structure is consistent with the general view of Knight (1921), Keynes (1936), and Kay and King (2020) that massive uncertainty plagues financial decision-making, but with the spotlight squarely on understanding capital structure behavior, which has been the main unresolved puzzle in academic corporate finance since at least Modigliani and Miller (1958).

A. Limited Knowledge of How to Gauge Value and of the "Correct" Model

The informational difficulties that managers face in choosing a capital structure include, but are not limited to, factors that make it difficult to gauge the fair values of the debt and equity claims a firm might issue. Such difficulties are central to Myers and Majluf (1984) and Gorton and Pennacchi (1990).

Managers also face informational difficulties that relate to the characteristics of specific policies that they might adopt. These difficulties arise from i) lack of knowledge of the "correct" or of even a reasonably accurate model that spells out the consequences of choosing particular financial policies, ii) noise in the available data (e.g., stock prices) that prevents accurate managerial inferences about the consequences of different policy choices, or, most realistically, iii) both limited knowledge and noise in the data.

There is a natural temptation to treat valuation difficulties as independent of imperfect knowledge of the "correct" or of even a reasonably accurate model of financial policy. However, as a practical reality, the two sources of informational imperfections are not separable. If one doesn't know the "correct" model that specifies the exact costs and benefits of all feasible financing decisions, how can one possibly have noise-free estimates of what firm value would be under each hypothetical financial choice?

The "uncertainty effect" in Black and Scholes ((1974), p. 4) is the earliest corporate finance discussion of informational difficulties managers face in discovering whether or how financing decisions matter. By uncertainty effect, they mean managers' inability to infer reliably whether dividend policy affects equity value from a regression linking stock returns to dividend yields. Noise in the data is the cause of the uncertainty for Black and Scholes, a theme that Black (1986) revisited in more detail.

While "noise" is surely relevant for the issues of concern in this paper, the term gives far too narrow an impression of the difficulties managers face in figuring out the best financing decisions for their firms. Noise gives the misimpression that managers know the relevant model structure and are impeded from getting precise estimates of parameter values only by poor data. Noise puts too much emphasis on what is measurable as opposed to what is economically important (Barzel (1982), Gorton ((2012), chapter 7)).

I have in mind a much larger knowledge (ignorance) gap than filtering out "noise," with navigation through a "pea-soup fog" a far more descriptive metaphor,

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and one suggested by Stew Myers in comments on an early draft of this paper. Managers do not have a map—never mind an accurate topographical map or radar—to help them find the best path through the fog, although they likely have figured out from the misfortunes of prior travelers that they should try to stay away from areas known to have quicksand, for example, capital structures with high risks of financial distress.

B. Experimentation with Alternative Financial Policies, Not Full-Knowledge Optimization

Full-knowledge optimization occurs in models in which managers have all information needed to select the uniquely optimal capital structure. Models of the latter type include all standard static tax/distress cost trade-off models, dynamic extensions of those trade-off models, and the pecking-order model.

The literature's approach to optimization is simply infeasible in a world in which managers do not have complete knowledge to select the best financing decisions. Instead, experimentation becomes the natural path for managers who proactively seek better financial policies for their firms.

If experimentation yielded refined and highly reliable results, then we would expect managers to converge rapidly on choosing capital structures that approach what they would adopt under full-knowledge optimization. Such rapid convergence essentially tells us that incomplete knowledge is simply not that important, as ignorance can be overcome with an easy set of experiments.

If "easy ways out" are available through experimentation, then incomplete knowledge is not a material issue for choosing financial policies. In that case, we are stuck with the current view in the literature, with models that fail badly. In constructing the baseline framework, I accordingly ignore "easy ways out" so that managers face significant impediments to identifying policies that are truly the best.

Consistent with this general view, Myers (2020) notes that noise in share prices makes it is difficult for managers to figure out whether a particular capital structure change is responsible for changing firm value.

For example, consider the fact that Fama and French (1998) find no reliable evidence that taxes systematically affect value. Given that state-of-the-art statistical methods (in the hands of the most accomplished researchers in the business) are unable to isolate a clear effect in real-world data of a factor that we *know* (from reading the tax code) exists, it would seem almost delusional to assume that corporate managers have an "easy way out" that effectively lets them gauge with precision the value impact of alternative capital structures. (There is an additional issue about the use of value maximization as a decision heuristic; see Sections V.B and VII.F.)

In general, when there is no "easy way out," experimentation will still be valuable, but its usefulness will come more from lessons learned about avoiding some unquestionably bad financial policies. The process will not converge tightly or rapidly toward a unique optimum. It is useful to think of this process in Darwinian terms, with a culling of the weakest decisions as ongoing and never ending.

For example, it seems clear that managers of real-world firms have figured out that they should avoid policies that have a firm teetering on the brink of financial distress because such situations entail seriously impaired access to funding. It's not that firms don't wind up near distress at times. It's that managers have figured out that distress is something they should try to avoid.

In sharp contrast, it will generally be much tougher for managers to gauge, for example, whether the tax savings are worth increasing the debt/assets ratio from, say, 0.100 to 0.200 because of the uncertainty about future funding needs that could be addressed if the firm had greater unused debt capacity.

In any case, a critical premise of the baseline framework is that financial policy experimentation will not easily reveal a clearly uniquely best capital structure for a firm.

C. The Focus on Reliable Access to Funding in the Baseline Model

Even though managers are unsure how to optimize capital structure with precision, they never doubt that they need a financial policy that provides reliable access to funding. The reason is that they inherently know that funding is essential to generate value (per Section III). As a practical matter, then, the knowledge (ignorance) gap in the baseline model is ultimately about the best way to arrange funding access.

D. Market Timing and Imperfect Managerial Knowledge

In constructing the baseline framework, I took as given the idea that attempts to time the capital markets are pervasive in the real world and must be accommodated by any credible theory of capital structure. The imperfect-knowledge premise of the baseline opens the possibility of pervasive attempted timing activity, even when we cannot detect (using the best empirical methods) systematic profits due to that activity. Section V presents a simple version of the baseline that does not include market-timing activity per se, while Sections VI and VII discuss how the argument generalizes to include such activity.

V. The Baseline Model: Banking-Based Version

In this section, I present a simple banking-based version of the baseline model. I start (in Section V.A) by detailing assumed restrictions of sources of funding for operating firms that effectively translate the external funding spotlight to banks. I relax these assumptions in Section VII, which generalizes the baseline to incorporate a broad array of real-world funding alternatives.

I consider capital structure decisions in a simple setting with three distinct types of economic actors: i) nonfinancial (or operating) firms that have real investment projects and need capital to undertake them, ii) financial firms (banks) that engage solely in intermediation between nonfinancial firms and households, and iii) households (including single individuals) that possess resources and are willing to supply some of those resources for investment in exchange for a return in the future.

This functional division is for analytical simplicity only. In real-world situations, some firms may engage in both real investment and the supply of financial products, for example, by having a captive-finance subsidiary that makes loans to customers and/or dealers of its products. For the current discussion, the functional division helps clarify the symbiotic relationship between the financial policies of nonfinancial firms and other firms that earn profits by creating privately and socially valuable financial products and services for nonfinancial firms and households. For more on this view of financial firms, see Diamond and Dybvig (1983), Gorton and Pennacchi (1990), and Diamond and Rajan (2001).

A. Bank Financing Before Trading Markets in Stocks and Bonds Exist

Suppose that stock and bond markets have not yet emerged and consider the financing problem that a firm faces: It needs funding if it has attractive projects, but inadequate resources to launch them.

Suppose also that the founder-managers of a given firm have contributed capital to the business, but it is not enough to fund all of the investment they would like to make. I assume for the time being that external-equity financing is not available to the firm from outside investors or from additional cash infusions by the founders made after their initial contribution to get the business up and running.

The founder-managers could approach households to raise debt, but that would be costly because of the large number of contacts to be made (and contracts to be negotiated) with many households, each of which typically has only a modest amount of capital to invest and limited skill to assess investment opportunities. I assume (again for the time being) that direct attempts to raise debt capital from households are prohibitively costly if undertaken by operating firms at any meaningful scale.

With the firm's funding sources limited in this fashion, financial intermediation in the form of commercial banks is the natural response to economize on the costs of marshaling large amounts of capital and allocating it efficiently. Think specifically about banks that collect funds (deposits) from households and decide how to supply funds (make loans) to operating firms, while covering their intermediation costs out of the spread between the returns on loans and deposits.

The specification of deposits as the source of capital for banks is, of course, realistic, but it also reflects the objective of building a theory from the ground up. The focus here is on the capital structures of firms, but one could take the same build-from-the-ground-up approach to the construction of a portfolio theory for individuals and households. In the latter case, it would make sense not to start with either long-term saving or diversification as the foundational focus. The evidence on the portfolio behavior of the poor suggests that liquidity—reliable access to funds when they are needed—is the bedrock concern of households that are building portfolios; see Collins, Morduch, Rutherford, and Ruthven (2009).

Deposit debt that is redeemable on demand is, of course, the quintessential liquid claim. Long-term saving and diversification are surely important, but the evidence says that they should come into play only after the foundational liquidity issue has been addressed. In any case, the use of deposit debt (at banks) in the simple model is consistent with a fundamental role for liquid claims in the portfolios of households.

For two reasons, banks have incentives to extend loans rather than take equity stakes in operating firms. First, proper risk management of a bank's assets will have its managers focused on limiting left-tail risk because that is the critical risk for being able to support deposit debt (DeAngelo and Stulz (2015)).

Second, managers of banks and managers of operating firms both face difficulties assessing the value of different types of financing. Debt claims (loans, in this case) are easier to value than equity claims to residual profit streams (Myers and Majluf (1984), Gorton and Pennacchi (1990)), and so it will be easier for the bank and an operating firm to come to agreement on loan terms than on terms for an equity infusion.

B. Transitory Debt Use and Indeterminacy of Debt and Cash Dynamics

How should the managers of an operating firm think about capital structure after they have raised enough debt (bank loans) to expand beyond their own initial equity capital contributions?

For the time being, let's ignore any motive to distribute cash (e.g., pay dividends) to fund consumption by the firm's owners or to cover wage or other expenses due to erosion of operating cash flows. In general, an ability to make payouts and to cover operating shortfalls are perfectly legitimate reasons why a firm wants reliable access to funding; see, for example, DeAngelo (2021) for evidence of clientele-based demands by a controlling shareholder for payouts. For now, it's just easier to see the concern with reliable funding access by thinking about funding needs that involve investment outlays.

One sensible policy for the operating firm is to use earnings to repay (some or all) debt and restore its ability to borrow again if new funding needs emerge. This response is the way many people manage their credit card debt. After using the credit card to meet a funding need, they use their earnings to repay the debt and restore their ability to borrow.

Alternatively, the firm could split earnings realizations between debt repayment and corporate saving (retention of cash earnings), with the resultant increase in bank deposits enabling it to have cash on demand for use later when funding needs emerge. There is an infinite number of earnings "splits" that fall between full debt repayment and full retention of earnings that the firm could choose.

In principle, the best choice among the infinite number of feasible policy responses depends on calculations that weigh the trade-offs among the loan-deposit spread, the perceived safety of bank deposits, the chance that the firm's borrowing capacity will erode, the uncertainty surrounding the scale of such erosion, and the likelihood of needing more funds for new investment (including, but not limited to, the correlation between cash flow and investment, as in Acharya, Almeida, and Campello (2007)).

Under the posited conditions, the best cash-versus-debt repayment choice also depends on the preferences of the founder-managers and how they resolve policy disagreements. I elaborate on this issue in Section VII.F and ignore the details for now, focusing instead on the core intuition of the baseline model.

The critical point: Managers will not be able to calculate with precision which choice—only debt repayment or the saving of a specific fraction of earnings—is the best use for a given earnings realization.

The reason is that they will typically be able to make only rough estimates of future funding needs, the likelihood that their firm's debt capacity will erode by varying degrees, and the safety of leaving the firm's money in the bank. This estimation problem goes well beyond gauging the parameters of probability distributions of investment needs (and returns) to include forecasts of how safe the bank is and uncertainty about how suppliers of capital will view the firm's creditworthiness.

Perhaps the biggest problem that managers of nonfinancial firms face in selecting the best financing decision is the presence of "unknown unknowns" that will almost surely come into play eventually. The important point is that the possibility of "unknown unknowns" inherently makes the choice between only debt repayment and some amount of corporate saving a crapshoot rather than a decision that has a clear-cut uniquely optimal analytical solution.

In any case, with or without "unknown unknowns," as long as there is a nontrivial chance of the firm needing funds in the future, the prudent response is to follow one of these approaches to make it more likely the firm can cover its future funding needs. And because managers cannot distinguish among multiple feasible choices, they behave as though they are "approximately indifferent" to those choices.

General implication. Firms select capital structures to provide reliable access to funding, with firms that face similar economic fundamentals sometimes arranging such capital access in different ways, all of which seem roughly equivalent from what managers can tell with any real degree of confidence.

Deleveraging implication. After borrowing to meet current funding needs, firms deleverage as earnings permit, but there is no clear-cut uniquely best deleveraging strategy. Firms sometimes use future earnings to pay down debt so that they are prepared to meet new funding needs. Other times they build cash balances under the same circumstances. And in yet other instances they do some of both, with debt repayment and cash accumulation both helping to rebuild the ability to meet future funding needs. In all cases, leverage is path dependent, with decisions to lever up typically followed by deleveraging through some combination of debt repayment and cash accumulation.

In no case do firms take on debt with the intention of keeping that debt in the capital structure on a permanent basis. Debt is purely a *transitory* funding vehicle at this point in the analysis.

Importantly, in this simple version of the model, firms do not have positive leverage targets of the type in standard trade-off models where all proactive financing decisions move the firm *toward* its target.

However, there is a different sense in which they do have a target: Ideally, firms would like to have no debt outstanding because that gives them the maximum feasible capacity to issue new debt to meet future funding needs. The difference with the latter type of target is that firms sometimes exercise the *option* to borrow and move deliberately, but temporarily, *away* from being ideally positioned to address new funding needs that may arise. To be clear, I am speaking here of a firm's ideal in terms of the *gross* amount of debt it has outstanding, while holding cash balances fixed. If we allow cash to vary and focus on *net* debt (gross debt minus cash), then there is nothing necessarily special, that is, desirable in a target sense, about zero gross debt. Viewed in the latter way, firms will have option-inclusive target net-debt ratios below zero, with abundant dry powder in terms of cash balances and unused debt capacity. For more on option-inclusive targets, see DeAngelo and DeAngelo (2007) and DeAngelo, DeAngelo, and Whited (2011).

Levering-up implication. Although this discussion emphasizes indeterminacies between cash buildup and debt paydown when a firm is deleveraging, the same set of concerns implies indeterminacies between cash drawdown and debt issuance when meeting funding needs.

C. Qualifications and Clarifications

An important qualification is that the deleveraging prediction here applies only to firms that have nontrivial chances of material future funding needs. Throughout this discussion, I have focused on what seems to be the most common (by far) situation in which firms have nontrivial prospective funding needs due to investment outlays, operating cash flow shortfalls, and (explicit and/or implicit) commitments to make payouts to holders of its debt and equity.

What if a firm's future funding needs are nil or nearly so? In that case, managers should not worry much, if at all, about access to funding. They should focus instead on distributing cash, with debt becoming much more attractive, perhaps even on a permanent or nearly permanent basis. In such cases, there is no real downside to i) giving up the option to borrow by taking on more debt and ii) shrinking cash balances. Firms will want to do those things because of taxes, which are ignored in the simple baseline model, but clearly exist in the real world and can be readily added to the baseline without changing its main features.

Miller (1977) on horse-and-rabbit stew. In such an extension of the baseline model, why don't firms lever up aggressively to capture the value of corporate tax savings? The reason is the existence of potentially large future funding needs that could not be addressed if the firm had largely spent its "dry powder" on pursuing the interest tax shield.

That is the key to understanding why Miller's (1977) horse-and-rabbit-stew argument does not, in fact, raise a puzzle about the conservative leverage of real-world firms. Miller raised that argument in a critique of the standard trade-off model which, as noted above, ignores funding. He compared the corporate tax benefit of having high debt in *perpetuity* with a *one-shot* deterrent of direct distress costs. Yes, as he duly noted, he should have included indirect distress costs. But even if he had included such costs, his comparison would still be problematic.

The right comparison is of i) the *perpetuity* value of tax benefits from being levered to the hilt with ii) the *perpetuity* value of lost profitable projects (and distress costs) when the firm is permanently hamstrung in its ability to raise funds because it is out of "dry powder" (unused debt capacity and excess cash).

The horse-and-rabbit-stew argument loses all force once one recognizes the importance of untapped debt capacity and cash holdings for addressing funding needs over a corporate lifetime.

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Jensen (1986) on the disciplinary role of debt. The importance of untapped debt capacity to meet funding needs is also why the baseline model does not assign a role for Jensen's (1986) argument that firms should lever up aggressively to create a legal obligation that forces managers to distribute rather than waste cash. If firms have high debt and low cash balances, managers won't have resources to waste, but they also will face perpetual problems getting funds for profitable use. A disciplinary role for debt does make sense for firms in the endgame stage of the lifecycle, which has a real danger of "inefficient continuation" as managers have incentives to use retained cash to fund diversification experiments in desperate hopes to keep their firms economically viable; see, for example, Lambrecht and Myers (2007).

The popularity of the debt discipline story as a generally important phenomenon rests largely on the observation that LBO firms use a lot of debt. But that usage is not plausibly due to a disciplinary role because, in LBOs, private-equity investors hold a major equity stake, control the board of directors, and tightly monitor operating managers. No one would take seriously the idea that a 100% owner-manager of a firm (e.g., as in Jensen and Meckling (1976)) needs a high debt load to have incentives to avoid wasting cash. Because of their large equity stakes and board control, private-equity investors are in approximately the same incentive (and power) position as such an owner-manager, and that is why the case of LBOs fails to substantiate an important disciplinary role for debt. In such cases, debt is largely redundant as a device to limit agency costs. LBO firms are thus among those least in need of debt to create a legal obligation to force cash to be distributed instead of retained and wasted on unprofitable activities.

The point here is that high equity ownership by private-equity investors is responsible for the pressure on operating managers to improve efficiency in LBOs. Those investors could, of course, *use* the need to repay debt as an incentive device to motivate managers, for example, by borrowing so much that managers would have to sell assets to avoid default. However, if the firm did not have high debt, it would be easy for private-equity monitors simply to tell managers to sell assets or they will be fired (or punished in other ways). For example, compensation contracts could reward managers for freeing up and distributing cash and punish them if they fell short of specified cash targets. The implication is that high debt is not necessary to motivate operating managers in LBOs to sell assets or take other actions to generate cash because private-equity investors have non-debt ways to motivate the cash-generating behavior they want.

I would add that, if debt per se actually played an important disciplinary role in forcing cash payouts at firms in general, we should observe firms with widely dispersed share ownership operating pervasively with high leverage so that managers are contractually forced to pay out cash. We do not. Instead, what we see is many dispersed-ownership firms with little or no debt voluntarily making large equity payouts when they have substantial earnings; for a discussion of the funding access and financial flexibility benefits of these commonly observed financial policies, see DeAngelo and DeAngelo (2007).

One final point on LBOs: If their high debt load is not due to the need to force managers to disgorge free cash flow, why do LBO firms have so much debt in their capital structures? One possibility is that LBO firms have been screened to be firms with little chance of having material funding needs in the foreseeable future, and so the tax benefits of debt loom large for them; see immediately above. Another possibility is that private-equity firms are acting as intermediaries that produce levered equity returns for investors who are willing to pay for such returns, for example, pension funds that are limited in their legal ability to use leverage. A third possibility is that the high debt in LBOs is simply a dramatic manifestation of transitory debt financing that is an integral element of a large-scale restructuring of a firm's assets and operations—a possibility that is more than simply a conjecture given the real-world prevalence of reverse LBOs in which firms acquired by private-equity investors are subsequently taken public.

Why Miller (1977) and Jensen (1986) are important. While I have criticized aspects of the stories told by Miller (1977) and Jensen (1986), both papers make other points that put them among the most important contributions to the post-MM literature.

Jensen (1986) clarified the importance of actually making payouts. It seems unbelievable today, but a reading of the pre-Jensen literature on equity payouts, especially Black's (1976) classic "dividend puzzle" article, reveals an almost exclusive focus on the (tax) benefits of retention. Although Easterbrook (1984) made some progress against that problematic view, Jensen (1986) changed the course of the literature in a flash with his intuitive argument about the value of actually sending cash out of the firm.

Miller (1977) brought general equilibrium considerations to the analysis of the net (corporate minus personal) tax incentives to lever up. His "neutral-mutation" argument was insightful early recognition of the fact that it is hard to explain many observed capital structure features in terms of real costs and benefits. In that respect, it is an important antecedent of the imperfect-knowledge argument in this paper.

VI. What the Data Say: Baseline Model Versus Trade-Off and Pecking-Order Models

This section details how the baseline model explains the main known capital structure regularities, and identifies the regularities that the (static and dynamic) trade-off and pecking-order models cannot explain. The contrast, of course, establishes that the baseline repairs the failures of the leading models.

Section VI.A discusses evidence that supports the idea that managers focus on reliable access to funding rather than optimizing the debt–equity mix. Section VI.B discusses evidence that supports the view that imperfect managerial knowledge is an important factor in real-world capital structure behavior. Section VI.C discusses a variety of other factors that support the baseline model.

A. A Managerial Focus on Funding, Not on Optimizing the Debt-Equity Mix

Basic evidence that firms do not focus on optimizing the debt–equity mix. If managers were mainly focused on remaining close to an optimal (possibly firm-specific) debt–equity mix, then firms should proactively issue and repurchase debt and equity to counteract mechanistic changes in leverage ratios that were induced by changes in their stock-market values. The evidence in Welch (2004) indicates that they do not systematically do so. He finds ample issuing activity, but it is not focused on reversing the changes in leverage from stock price changes.

Consistent with the latter inference in Welch (2004), DeAngelo (2021) documents that, in choosing financial policies, Henry Ford and Alfred P. Sloan, Jr. both focused on access to funding per se and both thought of debt as a funding tool, with neither concerned about pursuing a target debt–equity mix.

The fact that CFOs say that financial flexibility is the most important feature of capital structure (Graham and Harvey (2001)) similarly indicates an emphasis not on maintaining a given debt–equity mix, but rather on the firm's ability to move to other financing arrangements as circumstances dictate.

In the baseline model, firms do not seek to attain (or maintain) a particular leverage ratio. Some degree of proactive time-series variation is to be expected in the baseline model because debt is used as a transitory funding vehicle. The potential is present for substantial time-series variation in leverage, which will be a function of the extent of transitory debt financing—an issue discussed in detail below. The idea that a given firm can have a wide range of capital structures is supported by DeAngelo and Roll's (2015) evidence that substantial time-series instability in leverage is the norm at nonfinancial firms. Substantial instability is also the norm internationally, as reported by He, Hu, Mi, and Yu (2021), who study the time-series properties of leverage at firms in 43 countries.

Deliberate movements away from ostensible leverage targets. Standard trade-off models predict that firms seek to keep leverage at a target optimum. However, many proactive debt issuances move real-world firms above estimated target leverage ratios, often well above (Hovakimian (2004), Denis and McKeon (2012)). And real-world deleveraging pervasively entails moving below any possible leverage optimum in standard trade-off models (see the discussion a bit later in this section). The funding focus and the transitory debt property of the baseline model readily explain these empirical regularities.

Debt issuance and funding. The baseline model predicts a relation between debt issuance and a firm's need for funding. For evidence of the connection between borrowing spikes and the funding of investment, see Mayer and Sussman (2005), Harford, Klasa, and Walcott (2008), DeAngelo, DeAngelo, and Whited (2011), Uysal (2011), Denis and McKeon (2012), DeAngelo and Roll (2015), (2016), Bargeron, Denis, and Lehn (2018), Im, Mayer, and Sussman (2020), Korteweg, Schwert, and Strebulaev (2022), DeAngelo (2021), Huang and Ritter (2021), and DeAngelo, Gonçalves, and Stulz (2021).

A dramatic and historically prominent example of the debt-funding link is the transformation of General Motors from an essentially unlevered firm at the end of World War II to a firm with a debt-to-assets ratio above 0.300 in the mid-1950s, with a huge debt increase taken on in steps to fund GM's huge postwar expansion (DeAngelo (2021)). GM was by no means alone among prominent firms in levering up to fund expansion after WWII. In DeAngelo and Roll ((2016), Table 4), see the entries for General Electric, IBM, Procter & Gamble, Allied Chemical (Honeywell), Union Carbide, Sears Roebuck, International Harvester (Navistar), and Caterpillar.[1]

[1]For evidence of a general increase in leverage during the post-WWII boom, see Taggart ((1985), Table 1.1), DeAngelo and Roll ((2015), p. 386), and Graham, Leary, and Roberts (2015). I am not

GM's post-WWII levering up might sound like the pecking-order model at work, but it was not. GM increased its dividend payments by large amounts in tandem with its massive investment outlays. That violates both the strict pecking-order model and the modified version which assumes dividends are "sticky."

While it is most natural to think of funding as related to investment outlays, the dividend behavior of GM makes it clear that firms treat equity payouts as a funding need per se, not simply as a funding-neutral way of rebalancing the debt–equity mix. To be sure, there are cases of firms rebalancing through exchange offers (Masulis (1980)) and, given the advent in recent decades of repurchases as a payout vehicle, many firms now increase debt while repurchasing shares (Farre-Mensa, Michaely, and Schmalz (2020)). DeAngelo, Gonçalves, and Stulz (2021) find that equity payouts contribute to cash squeezes that lead firms to increase debt. The importance of equity payouts as a funding need is perhaps most convincingly established by the emphasis that managers attach to avoiding reductions in their dividend payouts (DeAngelo, DeAngelo, and Skinner ((2009), Section 5.4)), even if preserving the dividend possibly required a reduction in investment (Brav, Graham, Harvey, and Michaely (2005)).

Deleveraging and cash-balance build-ups. Because standard trade-off models hold investment policy fixed, they have no role for funding decisions per se. The pecking-order model does focus on funding. However, because it is formulated as a one-shot financing model, it cannot explain proactive corporate deleveraging and cash accumulation, which are central features of real-world financing dynamics and which are readily explained by the baseline model.

The baseline model's predictions about cash build-ups and deleveraging occurring in parallel are borne out for nonfinancial firms. The median market leverage (ML) ratio is 0.543 at the all-time firm-specific ML peak and 0.026 at the later firm-specific ML trough, with the median cash ratio (cash/total assets) almost tripling from 0.050 to 0.132 in a sample of 4,476 firms with at least 5 years of post-peak data on Compustat; see DeAngelo, Gonçalves, and Stulz ((2018), Table 2).

Proactive moves to negative-net-debt and zero-gross-debt capital structures. For the latter sample, the median deleveraging takes 6 years and 60.3% of firms wind up with negative net debt (more cash than debt), while 33.2% of firms move to zero gross debt. In short, we pervasively see firms selecting capital structures that should never be chosen according to standard (static) trade-off models. Adjustment costs (in ad hoc dynamic adaptations of such models) cannot repair that model failure, as avoiding the overshooting into the negative-net-debt zone could be done at trivial cost simply by distributing cash.

In the baseline model, the straightforward explanation for decisions to move to negative net debt is that firms have incentives to stockpile cash when they anticipate possible future funding needs.

claiming that the broad-based post-WWII increase in leverage documented in these studies is fully explained by debt-financed expansion. My point is simply that the latter factor was clearly at work at the firms reported in DeAngelo and Roll ((2016), Table 4). A comprehensive explanation of the full set of factors underlying the postwar secular increase in leverage awaits future research.

Transitory debt. Transitory debt is a central feature of the baseline model, but has no role in either standard trade-off or pecking-order models. Korteweg, Schwert, and Strebulaev (KSS) (2022) use hand-collected data to document the remarkable fact that 39% of long-term debt changes on Compustat are actually due to credit-line changes. They conclude that "numerous studies of capital structure confound the costly issuance of long-term debt with transitory credit-line usage."

KSS (2022) also find that many firms in the retail and wholesale industries are active users of lines of credit, with transitory leverage ratios that vary with seasonal operating needs.

Bargeron, Denis, and Lehn (2018) document strong post-event deleveraging by firms that use debt to fund investment around World War I, that is, the debt taken on to fund investment was a temporary component of the capital structures of most firms they study.

Credit lines, fortress balance sheets, and dry powder. Credit-line usage is pervasive and focused on funding (Sufi (2009), Lins, Servaes, and Tufano (LST) (2010)). Credit lines offer a method of altering leverage substantially at trivial adjustment costs, yet the evidence is that credit lines are used for funding, not for rebalancing the debt–equity mix per se.

The pervasive use of, and payment of fees for, lines of credit establishes that firms value the option to borrow. The value of saving "dry powder" (untapped debt capacity and/or excess cash holdings) to meet *future* funding needs is a central feature of the baseline model and is missing from the pecking-order model and from all standard static trade-off models because they are one-shot financing models.

Credit lines typically provide the option to borrow large amounts (Sufi (2009), Lins, Servaes, and Tufano (2010)). LST find that the median credit line allows new debt of up to an additional 15% of assets, while 39% of credit lines allow new debt of 20% or more of assets. LST also find that holdings of nonoperational cash balances are typically far larger at firms that do not have credit lines than at firms that have credit lines. The latter fact indicates that firms view untapped debt capacity and cash balances as alternative ways of arranging dry powder to meet funding needs.

The notion that dry powder is valuable is abundantly clear from the fortress balance sheets maintained by prominent managers ranging from Henry Ford (DeAngelo (2021)) to Warren Buffett and Bill Gates.

The value of dry powder is also evident at the nontrivial set of less famous firms that have zero debt outstanding. Not only do these firms have the option to use their full debt capacity to raise new funds, but they also typically also have high cash-balance ratios (Strebulaev and Yang ((2013), Table 2)). Sufi (2009) reports that, in about 30% of firm-year observations in which a firm has zero debt outstanding, it also has a credit line with untapped debt capacity.

Highly significant firm fixed effects in panel leverage analysis. Lemmon, Roberts, and Zender (LRZ) (2008) find that firm-fixed effects (FFEs) are highly significant determinants of leverage in panel analyses, a finding also reported by Mackay and Phillips (2005).

LRZ interpret FFEs "as statistical "stand-ins" for the *permanent* component of leverage" (p. 1576, emphasis added) and conclude that their findings imply the existence of "an unobserved time-invariant effect that generates surprisingly *stable* capital structures" (abstract, emphasis added).

Although these interpretations have gained considerable traction in the literature, significant FFEs do not establish either that i) leverage ratios are stable or that ii) firms have permanent (time-invariant and positive) debt components in their capital structures.

Regarding i), the stability of firm dummies (FFEs) can and should be tested using standard ANOVA methods that assess the significance of firm-time interactions. The label "firm *fixed* effect" might seem to imply that a firm-specific factor is constant (fixed) over time in the data. Not so. The point is moot when analyzing a cross-sectional snapshot of firms. But when analyzing dynamic behavior and drawing inferences about stability, it is important to *test* for changes over time in the firm-specific factor. Such tests strongly reject time-series stability of firm dummies; see DeAngelo and Roll ((2015), Section II).

Regarding ii), significant FFEs indicate only that there are material differences across firms in their time-series average leverage ratios. Such differences will exist if there are two firms that use debt to differing degrees of intensity (e.g., to meet differing funding needs).

That does *not* require permanent debt. Such heterogeneous debt usage can be *purely transitory*, as in Section V's baseline framework. For example, consider two firms, one that uses its credit line to borrow 10% of assets in odd-numbered years and then repays all of its debt in even-numbered years, and a second firm that borrows 30% of assets in odd years and repays all debt in even years. The time-series average leverage for the first firm is 5% of assets and 15% for the second. Panel leverage analysis will show significant FFEs simply because the second firm uses debt to a greater intensity on a transitory basis.

The implication: Significant FFEs do not establish that firms have permanent debt components or that they have anything akin to positive leverage targets. Indeed, counterexamples abound in which FFEs per se yield high R^2s, yet firms have no targets and their debt use is 100% transitory. For example, DeAngelo and Roll ((2015), p. 395) find R^2s of 77.1% for FFEs and of 1.0% for year dummies in panel analyses of leverage data generated by unit-root models that have no target ratio or permanent debt.

Moreover, there is good reason to doubt that firms pervasively have permanent debt components: Many firms proactively deleverage from their historical peak leverage to zero debt (see the deleveraging discussion earlier in this section).

The important general point: Transitory debt usage of the type in the baseline model clearly arises in the real world and can fully explain what seemed to be a major puzzle: Highly significant FFEs in panel leverage analyses that researchers could not link to any known economic motive.

I would add that the baseline model is easily generalized (e.g., by adding taxes) so that firms do value having some permanent debt. In the latter case, significant FFEs would reflect cross-firm differences in both i) permanent debt components in the capital structure and ii) the typical intensity with which firms take on transitory debt.

B. Imperfect Managerial Knowledge and Indeterminacies in Financial Policy

In the pecking-order and (static and dynamic) trade-off models, there is a unique functional mapping from a firm's economic fundamentals to its choice of capital structure. In the baseline model, there are nontrivial indeterminacies due to imperfect managerial knowledge.

Direct evidence of imperfect managerial knowledge. DeAngelo (2021) documents the testimony of Alfred P. Sloan, Jr. that there is wide latitude for managerial opinion and judgment in optimizing financial policy; see the introduction to this paper for Sloan's statement.

Academic surveys of managerial views of financial policy generally provide laundry lists of factors managers consider, but do not yield precise statements about how managers go about choosing particular capital structures. If managers knew how to optimize capital structures with precision, we'd expect that knowledge to be revealed at least in the interview portions of survey research. It is not.

Graham (2021) reports important evidence from a recent large-sample survey that documents the forecasting and planning horizons managers use when choosing financial policies. He finds that managers are typically confident about their forecasts and plans for a 2-year (or maybe 3-year) horizon, but not longer. That means that most managers have nothing close to the knowledge assumed in extant dynamic capital structure models, which posit a complete understanding of investment opportunities and capital-market conditions over an infinite horizon. There is no tenable case that the massive volume of managerial knowledge assumed in the leading extant models of capital structure are good approximations to the remarkably short forecasting horizons about which real-world managers are actually confident.

Graham's (2021) findings on planning and forecasting horizons thus provides compelling evidence that the norm is far from perfect managerial knowledge of how to optimize capital structure.

The same conclusion is implied by the 63-year-old-and-counting failure of financial economists to solve the capital structure puzzle. If real-world managers knew how to optimize capital structure with any real precision, it seems highly doubtful that participants in executive programs would not have set their professors straight after hearing capital structure lectures dominated by the clearly flawed models that dominate the academic literature and textbooks.

Estimated flat leverage-value relation. Korteweg's (2010) estimate of a nearly flat relation between firm value and leverage indicates that it is difficult empirically to isolate a uniquely optimal capital structure. The critical word here is *estimate*, which dictates that the peak (value maximum) in his fitted relation is subject to estimation error. And the fact that the fitted function is nearly flat points to nontrivial scope for the fitted peak to fall a good distance away from the true peak. Using a very different empirical approach, Binsbergen, Graham, and Yang (2010) report estimates that similarly support this inference, with the leverage-value function "fairly flat" within +/-20% of the estimated optimum for a typical firm.

These papers employ state-of-the-art empirical methods, which are surely beyond the grasp of the vast majority of corporate managers. If those methods can't pin down the peak with reliable precision, it taxes credulity to think real-world managers can do so. Their estimates thus strongly suggest at least perceived "approximate indifference" across a nontrivial range of leverage choices.

When the flat (or nearly flat) leverage-value function estimates of Korteweg (2010) and Binsbergen, Graham, and Yang (2010) are juxtaposed against the findings of DeAngelo and Roll (2015) that wide time-series variation in leverage is the norm, it becomes all the more plausible to believe that real-world managers are unable to pin down a uniquely optimal capital structure with any real precision. Conversely, it becomes untenable to accept the premise of the leading capital structure models that there is a unique and fully understood (by managers) functional mapping from a firm's underlying economic fundamentals (investments, earnings, etc.) to its financing choices.

Two other empirical considerations strongly reinforce the importance of indeterminacies in financial policy. One is managerial uniqueness in the "managing with style" sense emphasized by Bertrand and Schoar (2003). If fundamentals determined uniquely optimal financing decisions, there would be zero scope for such uniqueness to shape capital structure, yet it is clear that the zero-scope prediction is grossly violated in the real world. The other (closely related) consideration is the observation of pervasive attempts by managers to use their personal judgments about valuations to time the capital markets. I next elaborate on both of these considerations.

Financial policy indeterminacies and managing with style. Graham and Narasimhan (2004) find a greater reluctance to use debt among managers of firms that had high leverage ratios during the Great Depression. This regularity echoes Bertrand and Schoar's (2003) findings that older CEOs tend to be more conservative in their use of leverage and, more generally, that the identity of managers is a significant determinant of a firm's financial policy.[2]

Two aspects of DeAngelo's (2021) clinical evidence on the financial policies of Ford Motor Company and General Motors strongly support the implication of the baseline model that there is no unique functional mapping from economic fundamentals to chosen financial policies. First, the quote from Alfred P. Sloan, Jr. that is reproduced in the introduction to this paper indicates that Sloan believed there are many possible financial policies that could reasonably be viewed as optimal for a firm. In his discussion of GM's financial policy, Sloan does not mention the idea of a uniquely optimal capital structure, or even a target debt–equity mix. (As noted earlier, Sloan clearly thought funding access was essential.)

Sloan's influence on the structure and functioning of large corporations is legendary. If we are going to take CFO survey answers seriously as evidence about

[2]Fee, Hadlock, and Pierce (2013) present large-sample analysis that challenges the robustness of Bertrand and Schoar's (2003) conclusions about the importance of managerial style effects, while adding the caveat that their empirical measures may miss some empirically important style effects. The caveat is important because requiring the experimental design to rely on Compustat data effectively stacks the deck against identifying policy choices that are unique to particular managers. The Compustat file includes variables that are of general interest to customers who will then be willing to pay for access, which means that the file tends to exclude idiosyncratic variables that are more likely to capture unique elements of the approaches managers take to running firms.

our theories, we should give nontrivial weight to what Sloan said, given the unquestionably large effect he has had on corporate practices.

The second important observation from DeAngelo (2021) is that the articulated (and implemented) financial policies of Sloan and Henry Ford differed radically despite simultaneously being in the same business. Radical *intra*industry differences in financial policies are hard to reconcile with the universal predictions of the leading models that economic fundamentals map *uniquely* into chosen financial policies.

The clinical evidence in Denis (1994) points in the same direction. It makes no sense to think that fundamentals uniquely determine financial policies when one sees the sharp simultaneous differences in policies among Kroger, Safeway, and other firms in the grocery business.

The same conclusion applies when examining the almost-overnight transformation of Coca-Cola's financial policies by Roberto Goizueta from a conservatively levered firm to one that was willing to—and did—use debt aggressively to make acquisitions (DeAngelo and Roll ((2016), pp. 46-47)).

The literally overnight capital structure transformations of firms that take on large amounts of debt and go private similarly point to ample scope for managerial judgment in the choice of capital structure.

In a similar vein, any informed observer of the corporate world can easily find convincing examples of such scope at work with prominent managers like Warren Buffett and Steve Jobs.

Market timing and financial policy indeterminacies. Baker and Wurgler (BW) (2002) note that the critical condition for a market-timing based theory of capital structure is "that managers *believe* they can time the market" (p. 28, emphasis added). BW focus on equity-market timing, but their logic indicates that managers look at "capital-market conditions" generally in making their financing decisions. See also Titman (2002) on the importance of capital-market conditions for understanding capital structure.

In timing theories of the type discussed by BW, the concern with capital-market conditions does not mean that managers always accurately understand the value (or cost-of-capital) consequences of different financing decisions. Rather, the theories recognize that managers often use rough heuristics about security valuations and interest rates to decide whether to issue or buy back debt and equity securities. And it's not just managers who use rough heuristics and put their faith in rules of thumb and financial practices that seem reasonable. The same is true of suppliers of funding to firms and, indeed, of academic specialists who seek to understand the financing decisions that firms make.

A prominent case that supports the latter claim is the mistaken acceptance in both the real and academic worlds of the junk-bond marketing efforts of Drexel and Michael Milken. Those of us who are old enough to be working in finance in the 1980s will recall the serious misunderstanding about junk-bond default rates that Asquith, Mullins, and Wolff (1989) rectified in a Smith-Breeden-Prize-winning paper—a paper that I can't resist pointing out clarified the mistaken view using only simple descriptive statistics and no formal tests. Financial economists missed the boat badly, as did the real-world marketplace. The reason is that they failed to appreciate that the fact that borrowers rarely default soon after issuance made the (widely publicized) low default rates quite misleading due to the explosive rate of growth in junk-bond issuance in the 1980s. The fact that academics missed the boat badly reflects their widespread (unwarranted) faith that managers and investors have a precise understanding of the funding landscape.

Baker and Wurgler (2002) discuss variants of market-timing theories in which managers *actually* have the ability to outwit the capital markets and earn abnormal trading profits systematically. There is an enormous literature on the latter question and that literature provides reason to think that managers do not have a great deal of actual timing ability. However, the extent of managers' actual ability to outwit the capital markets is not important for the main points in this paper.

What is important here: There is an enormous range of real-world financing behavior that plausibly reflects managers' *beliefs* that they can time the capital markets. Baker and Wurgler ((2002), p. 1) open their paper with a summary of four types of studies that support the existence of such beliefs. Classic examples are attempts to sell what managers believe are overvalued shares (Ritter (1991), Loughran and Ritter (1995)) and repurchase shares they believe are undervalued (Dann (1981), Vermaelen (1981)). Baker and Wurgler add their own evidence in support of market timing, and the subsequent two decades have seen many more studies that reinforce the idea that managers attempt to time the capital markets.

Managers' concern with market timing does not mean they are unconcerned with funding issues. On the contrary, timing issues come into play most plausibly when managers have other reasons to be raising funds and/or making payouts.

The way the baseline model (as expanded in Section VII) incorporates timing behavior is through the premise that managers don't know the precisely best way to set capital structure, and so they use judgment to make financing decisions, with that judgment including their heuristic-based assessments of capital-market conditions. As long as timing-based decisions do not entail large and systematic valuation mistakes by managers, a managerial belief in market-timing ability can survive indefinitely.

The key point here: The indeterminacy property of the baseline model leaves scope for the enormous set of real-world financing decisions that have overtones of managerial attempts to time the capital market. Trade-off models have no such scope and, while the pecking-order model does include timing of share issuances due to asymmetric information, it has no role for timing of share repurchases or timing in any other form.[3] For more on market timing in the context of the extended baseline model, see Section VII.E.

Heuristics and financial policy indeterminacies. The unique mapping from economic fundamentals to financing decisions in the leading models of capital structure is also incompatible with the pervasive use of financial management heuristics in the real world. Heuristics are inherently needed when managers have imperfect knowledge about how to optimize financial policy, as in the baseline

[3]The dynamic trade-off model of Warusawitharana and Whited (WW) (2016) includes issuance and repurchase of misvalued equity. This exception to my general statement about market timing in trade-off models is not important as WW's model has other empirical limitations, including an inability to explain costly financial intermediation and the nature of interactions between debt and cash balances at real-world firms.

model. Their pervasive use in the real world is strong prima facie evidence of indeterminacies in optimal financial policies.

Disagreement among owners. In static and dynamic trade-off models, optimal capital structures maximize firm value, an assumption that can be read as implicitly invoking the principle from frictionless finance theory that, regardless of the details of their personal preferences, all shareholders want managers to maximize value; see Fama and Miller's ((1972), pp. 70–71) discussion of the Fisher Separation Theorem.

The empirical problem, however, is that disagreement among owners about financial policy is pervasive at real-world firms, with lawsuits (e.g., DeAngelo (2021)) and proxy fights just the tip of the iceberg. Such disagreements arise naturally in the baseline model; see Section VII.F for more on this issue.

The point is important in this discussion of evidence on imperfect managerial knowledge because it highlights another factor that indicates managers do not have easy access to knowledge of the optimal policies they should adopt. Simply put, there is no obvious or easy way for managers to identify policies that satisfactorily balance the generally heterogeneous desires of the owners of the firm. Academics have not derived robust solutions to this problem even in relatively simple economic settings in which the Fisher Separation Theorem fails and there is no unanimously agreed upon corporate objective (e.g., the asymmetric information settting of Myers and Majluf (1984)). Why then would anyone reasonably expect managers to have done so, given the complexity of the real world?

C. Other Evidence that Favors the Baseline Model

Costly financial intermediation and segmented capital-market pricing. In the baseline model, banks supply loans to firms, which they fund through deposits, and they cover the costs of intermediation by the loan-deposit interest-rate spread. In the trade-off and pecking-order models, there is no role for costly intermediation because of fully integrated capital-market pricing.

The problem with these models is that, with fully integrated financial asset pricing, if an intermediary buys financial assets and repackages their cash flows for sale, the net value generated for the intermediary is zero. Consequently, there is no "spread" left to cover the costs of financial intermediation, and so costly intermediation cannot survive. Yet it has survived and thrived for centuries and continues to do so.

In the real world, of course, enormous amounts of resources are spent on banking and other forms of financial intermediation. Such expenditures are fully compatible with the baseline model and with the generalizations of that model that I discuss below.

The inability to accommodate a role for costly financial intermediation is a serious shortcoming of all models in which equilibrium rules out market segmentation.[4]

[4]The dynamic model of DeAngelo, DeAngelo, and Whited (DDW) (2011) is consistent with costly intermediation. However, this exception to my general statement about trade-off models is not important for the argument in this section because of other empirical shortcomings of DDW's model, including its predicted unique mapping from fundamentals to chosen financing decisions.

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Segmented-market pricing. Titman (2002) discusses a broad set of nonbanking examples in which a firm's capital structure reflects managerial attempts to arbitrage segmented-market pricing of securities. His findings point to a capital structure role for segmentation that extends beyond that related to banking per se and that reflects the limits of arbitrage, as in Shleifer and Vishny (1997). The evidence in Ma (2019) points in the same general direction.

The point here is that the trade-off and pecking-order models cannot explain behavior in which capital structure changes reflect attempts to arbitrage segmented-market pricing of financial assets. The baseline model accommodates both actual cross-market arbitrage transactions and transactions in which managers think they are capturing arbitrage profits, but cannot tell with precision that they are.

Bank leverage. The trade-off and pecking-order models do not explain why banks have high leverage. In the baseline model, that phenomenon is easily explained because banks create social (and private) value through liquid-claim production and, specifically, by issuing deposit debt to fund the loans they make.

Equity issuance when firms have low leverage. The standard trade-off and pecking-order models are unable to explain why firms with little or no debt would issue equity (see, e.g., Fama and French (2005) and Denis and McKeon (2021)). Such issuances are explained by dynamic trade-off models in the spirit of Hennessy and Whited (2005) and by the baseline model as soon as we consider financial innovation and the development of equity markets (see Section VII). In both cases, firms issue equity so that they have more dry powder (untapped debt capacity and/or cash) to address funding needs that may arise in the future.

Mean reversion in leverage. The leverage dynamics of the baseline model imply mean reversion in leverage, with decisions to borrow (lever up) tending to be followed by deleveraging through debt repayment and/or the accumulation of cash balances. Weak mean reversion in leverage is a well-known feature of the leverage ratios of nonfinancial firms (Yin and Ritter (2019), Huang and Ritter (2009)).

Profitability and leverage. Early critiques of the standard trade-off model highlighted the model's inability to explain the observed inverse relation between profits and leverage. The transitory debt property of the baseline model readily explains that empirical relation, as firms with high earnings are in a better position to fund investment outlays internally and thus have less reason to borrow.

Debt overhang and the ratchet model. While this empirical section focuses on why the baseline model beats the trade-off and pecking-order models, I would add that the baseline model also has an empirical edge over the recently developed leverage-ratchet model of Admati, DeMarzo, Hellwig, and Pfleiderer (2018). The ratchet model is a sophisticated, dynamic version of a familiar debt-overhang wealth-transfer argument, and its signature prediction is a pervasive and strong aversion to deleveraging (because leverage reductions would transfer wealth from shareholders to existing creditors) and a concomitant desire to increase leverage.[5]

[5]Admati et al. ((2018), abstract) state: "With debt in place, shareholders pervasively resist leverage reductions no matter how much such reductions may enhance firm value." They reinforce this main carry-away by making three similar statements in the next few pages, including the following where they add italics to highlight the unbounded (potentially massive) overhang-related distortions they have in mind with the ratchet model: "…shareholder resistance to leverage reductions is pervasive and

The empirical problem is that proactive leverage reductions are widespread at real-world firms, for example with innumerable IPOs, SEOs, and private-placements of equity by firms with debt outstanding; reverse LBOs that undo the high leverage effected in LBOs; equity-for-debt exchanges; calls of convertible debt to force conversion to common stock; and voluntary early repayment of credit-line and other debt. If the incentives to avoid deleveraging were as strong as in the ratchet model (see footnote 5), these leverage-reducing financing actions would be observed rarely at best. In reality, they are commonplace.

Moreover, as detailed earlier in this section, the typical publicly held firm deleverages aggressively from peak leverage to a near-zero market-leverage ratio while accumulating substantially higher cash balances so that it reaches a negative-net-debt capital structure. The baseline model explains such deleveraging as building dry powder—untapped debt capacity and ample cash holdings—that can be used for future funding, while the ratchet model indicates such deleveraging should not occur.

Another empirical problem for the ratchet model is the existence of excess cash holdings by real-world firms with debt that has even the tiniest degree of risk. The reason is that cash payouts can be made at negligible transactions cost and would increase leverage and the riskiness of outstanding debt, thereby transferring wealth from creditors to shareholders. However, one look at the debt and cash positions on the balance sheets of Apple, Microsoft, Berkshire-Hathaway, Alphabet/Google, and Amazon reveals a serious problem for the hypothesis that the pursuit of debt-overhang-based wealth transfers is a generally important driver of capital structure behavior.

The latter point draws further support from the widespread "dash for cash" in financial crises (Campello, Graham, and Harvey (2010), Ivashina and Scharfstein (2010), and Acharya and Steffen (2020)). Indeed, as I write this section, the COVID-related dash to obtain cash (through credit-line drawdowns) and to hold on to it as a safety cushion continues, as the financial press is reporting huge stockpiling of cash by levered firms ("Investors Circle Largest Corporate Cash Hoard Ever," *Wall Street Journal*, Dec. 4, 2020).

The clear implication is that real-world deleveraging behavior and cash-management activity are at strong odds with what the ratchet model predicts.

That is not to say that debt-overhang problems, as first discussed by Myers (1977), are completely unimportant at real-world firms.[6] Overhang problems are no

persists *no matter how much the leverage reduction would increase the total value of the firm*" (p. 149, emphasis in original).

[6]Why do Admati et al. (2018) and Myers (1977) reach different conclusions about the extent of overhang-related distortions? One important reason is that the former paper effectively assumes (in its footnote 2) infinite contracting costs for negotiations to avoid overhang distortions, while the latter paper indicates (on p. 158) that the extent of distortion depends on the magnitude of contracting costs. Infinite contracting costs are needed for the ratchet model's conclusion about shareholders resisting deleveraging regardless of the gain in firm value (see footnote 5). Shareholders will not bypass unbounded—arbitrarily large and infinite in the limit—value gains from deleveraging if negotiation costs are finite. There is always a feasible bargain (with side payments) that makes everyone better off by deleveraging to capture (the otherwise lost) gains when those gains are high enough that they exceed the finite contracting costs.

doubt important at troubled firms, as the common use of debtor-in-possession financing shows. The reasonable bottom line here is that debt-overhang problems are important in some circumstances involving financial trouble, but the evidence does not support the idea that they are important general drivers of capital structure behavior.

VII. Baseline Model Generalization: Financial Innovation, Experimentation, and Funding Access

In the banking-based model presented in Section V, all external funding flows from households through banks to nonfinancial (operating) firms. The Section V analysis is only the first step in an evolutionary process that, with competition and innovation, will become more refined (and efficient) in providing reliable capital access to nonfinancial firms. More avenues for funding will accordingly emerge over time.

However, there is no reason to think that this process will eventually converge on a clearly obvious single best capital structure for a given firm. The choice problem that managers face in Section V is complex despite the stripped-to-the-basics setting. The addition of more financing options will add complexity to the choice problem which, if anything, should make it less likely that managers will be able to isolate a capital structure that is obviously the uniquely optimal choice.

A. Evolutionary Changes in Financing Arrangements

To understand the evolutionary process of financial innovation away from the banking-based version of the baseline model, consider first the vulnerabilities of the bank-financing arrangement in Section V.

Nonfinancial firms will want i) larger loans to be able to fund more investment, ii) banks to tolerate greater risks of repayment so that more credit is extended, iii) lower rates on loans they take out, iv) higher rates on deposits they put in banks as they earn cash from investments, and v) risk-sharing opportunities (access to investors who are willing to buy residual profit claims at values current owners find reasonable).

Existing banks will resist for two reasons. First, the reduced diversification in the asset portfolio (from i), ii), and v)) would increase the risks faced by a given bank, which will then want to charge higher loan rates. If the risk increase on loans is large enough to be noticed by depositors, they will demand a higher interest rate or withdraw their deposits and place them with a different bank.

Second, downward pressure on the loan-deposit spread (from iii) and iv)) will reduce bank profits by making the spread less able to cover the costs of making prudent loans and collecting/servicing deposits.

Although incumbent banks will resist actions that reduce their profits, there are also clear incentives for entry by newly formed bank competitors that can do a more efficient job evaluating loan risks, managing portfolios of loans and other financial assets to support deposit debt, and physically collecting deposits.

The entry and experimentation with financial innovations will come from all quarters, not just from new banks seeking to displace incumbents. It will come from shadow banks, that is, entities that seek to perform the functions of banks without being formally labeled as such (Gorton (2010), (2012)).

Entry and experimentation will also come from parties willing to i) bear residual-claim risk directly, ii) facilitate trade in such claims, and/or iii) find others who are willing to bear residual risk.

In other words, incentives exist for individuals to buy equity claims directly from firms, for trading markets to emerge, and for hedge funds, mutual funds, private-equity firms, and investment banks to be formed and compete with commercial banks in supplying funding.

B. Experimentation In, and Evolution Of, Financial Practices

New financing techniques should emerge over time, as operating firms, intermediaries, and households experiment with different financial arrangements and gauge their efficacy. As the historical record shows, this experimentation eventually came to include markets not just for common stock and conventional (high-credit-quality) bonds, but also for junk bonds, leveraged loans, and other forms of more risky debt.

Experimentation should also include an ongoing search for higher returns on bank deposits by both households and nonfinancial firms that would like to have fortress balance sheets that provide highly assured access to capital. As the record also shows, the latter ongoing search led to money-market funds, repo transactions, and other such alternatives to deposits at traditional banks.

Nonfinancial firms will also experiment with methods of fostering reliable access to funding through the asset side of the balance sheet. *Asset modularity* refers to a situation in which assets can be separated out from other assets and liquidated to raise cash without damaging the overall productivity of the firm. Cash balances (held in a bank, money market fund, or other liquid form) are the quintessential example of a modular asset. Other modular-asset arrangements that provide viable ways of addressing a firm's funding needs include: i) holdings of marketable financial assets that are more risky than cash balances (Duchin, Gilbert, Harford, and Hrdlicka (2016)), ii) the ability to separate out and sell operating divisions, iii) equity carve-outs of subsidiaries, and iv) conglomerate structures in which cash transfers across technologically independent divisions effectively represent transactions in an internal capital market.

Operating flexibility refers to the ability to free up cash by adjusting the scale and form of productive activity without triggering large adjustment costs. Examples include the ability to effect, at low cost, the shutdown of a money-losing factory or the furloughing of workers when demand falls.

Asset modularity and operating flexibility are important reasons why one should look beyond leverage ratios to credit ratings to gauge the extent to which firms have reliable access to funding. See Kisgen (2006), (2009) and Hovakimian, Kayhan, and Titman (2009).

C. The Culling of Inferior Financial Practices

This process of financial innovation and experimentation is best viewed as continuous, never-ending, and focused on the culling of policies that experience has shown to be undesirable.

The process is Darwinian in the sense that Charles Darwin came to natural selection in part by extrapolating from the fact that farmers bred crops and animals by culling of the worst manifestations of desired traits and focusing on the best (Browne ((2006), p. 45)). He coupled knowledge of such deliberate "domestic selection" with critical insights from economics and geology. In reading Thomas Malthus' essay on population, he came to see how competition in nature would cull the weakest under conditions of extreme scarcity. His familiarity with the geology theory of Charles Lyell (a very close friend) opened his mind to gradualism, which held that the earth's surface was transformed through "innumerable, tiny, accumulative changes, the result of natural forces operating uniformly over immensely long periods" (Browne ((2006), p. 31)). Culling of the weakest leaves the stronger surviving and propagating offspring better able to compete, with improvements occurring gradually over very long horizons and with Darwin ((1859), p. 202) carefully adding that "natural selection will not produce absolute perfection…"

To what extent is it reasonable to extrapolate Darwin's reasoning to economics generally and to capital structure specifically? We need to be cautious about such extrapolation, given that the firms whose capital structures (and other behavior) we seek to explain are almost never operating on the Malthusian margin of survival. That, in part, reflects first-mover advantages in their product markets. It also reflects the fact that technological change and Schumpeterian destruction often proceed slowly, thus allowing firms to survive for extended periods while being less than fully efficient.

We should still expect the culling of demonstrably bad financial policies, as managers receive feedback about their capital structure choices. But culling of bad policies does not translate to survival of only the very best, especially not over the horizons that we examine in corporate finance empirical work. We have 150 or so years of experience with the financing of large corporations. That is less than a blink of an eye viewed in terms of geological/biological time as in Lyell's and Darwin's analyses.

What about the possibility that ongoing survival-related pressures will eventually lead managers to converge on the globally optimal capital structure for their firm? I find that conjecture highly implausible, as it is hard to make progress toward the global optimum when the time frame is short and, worse yet, the competitive environment keeps being hit with shocks. Shocks to productive capacity and demand can easily undermine all the progress a firm has made on converging on the capital structure that is best, conditional on the pre-shock environment.

My point here echoes Alchian's (1950) general view of the limitations of "trial-and-error" behavior and the disruptive effects of changing (unstable) environments in preventing economic agents from converging on the specific decisions that would be globally optimal conditional upon having perfect knowledge. See Alchian's discussion of conscious adaptive behavior (p. 219) and the bottom line he emphasizes in the conclusion (p. 220) of his paper:

> The economist may be pushing his luck too far in arguing that actions in response to changes in environment and changes in satisfaction with the existing state of affairs will converge as a result of adaptation or adoption

toward the optimum action that should have been selected, if foresight had been perfect.

For the current paper, the important implication is that it takes a prodigious leap of faith to think of capital structures as having been subjected to long periods of vigorous competition in which firms die off—fail to survive—because they have failed to find their globally optimal capital structures.

Moreover, our job is to construct theories that do a good job explaining the capital structures we see *at all points along the evolutionary path.* Even if a given firm were able to discover its globally optimal capital structure in the really long run, there is a huge amount of financial policy behavior that we need to explain along the evolutionary path of firms that exist for any appreciable amount of time.

The bottom line for corporate finance: We should expect clearly inferior financial policies to be shunned once they are convincingly shown to be poor, and for better practices to survive and be improved upon over time through further experimentation. Culling of clearly inferior policies does not mean that the currently surviving set of financial policies contains only the very best practices. On the contrary, numerous policies can co-exist for long periods of time, with the key condition for continued survival of a given policy being that it is not perceived as substantially inferior to other feasible choices.

D. Managers' Inability to Identify Optimal Financial Policies with Reliable Precision

The question then is how the efficacy of financial practices can be gauged with reliability by managers who have only a limited understanding of the choices they face. They have neither a solid theoretical framework for choosing financial policies, nor empirical work that tells them how to weigh the possibly relevant theoretical determinants so that they can confidently select the uniquely optimal capital structure for their firm. Nor is there much raw data for such empirical work, given that there have been only 100 or so years in which more than a few firms have had public equity. And there are many fewer years of data to judge the efficacy of innovations such as original-issue junk-bond financing and LBOs.

Under these conditions, firms will not converge quickly on uniquely optimal financial policies unless there is a single choice that confers a detectably sharp advantage over all of the other alternatives (because then the alternatives would be quickly exposed as inferior and rapidly culled).

But if there are at most only second-order differences among numerous feasible choices, then it will take a lot of experience to figure out which are (slightly) better and which are (slightly) worse. Managers will therefore be unable to isolate with reliable precision the specific features of uniquely optimal choices.

This view draws empirical support from Korteweg's (2010) and Binsbergen, Graham, and Yang's (2010) estimates of a nearly flat empirical relation between leverage and value (see Section VI) and Myers' (2020) general assessment that noise in stock prices makes it hard for managers to infer whether changes in capital structure affect value.

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E. Market Timing and Imprecise Ability to Identify Optimal Capital Structure

The imperfect-knowledge view also draws support from the large amount of financing activity that has overtones of managerial attempts to time the capital market; see Section VI's discussion of timing. Consider the fact that finance academics—who have state-of-the-art empirical techniques for gauging abnormal investment performance—have difficulty nailing down the extent of actual timing ability that managers have. The only reasonable interpretation is that there is considerable latitude for managers to make market-timing bets while generating neither systematic serious damage nor clear-cut benefits for shareholders. In other words, the observed extent of timing attempts supports the idea that imperfect knowledge implies that managers cannot pin down uniquely optimal capital structures with any real precision.

An example that I find particularly convincing is that many managers seem to believe that that a low absolute level of interest rates is a good reason to issue debt. I have yet to see any managerial expression of such sentiment accompanied by a discussion of the weighted-average-cost-of-capital rationale for, or the firm-value impact of, such attempts to exploit low interest rates. While there is some evidence that managers can time the market for their own firms' shares, it is much more difficult to believe that they can systematically outwit the credit markets, and especially not en masse when everyone can see that the general level of interest rates is low by historical standards.

My reading is that decisions to issue debt when interest rates are low are plausible examples of what Miller (1977) called "neutral mutations," which are capital structure variations that bring no clear benefit, but do no clear harm. There is no room for neutral mutations to arise in the trade-off and pecking-order models which imply a unique functional mapping from economic fundamentals to financing decisions.

F. Decision Complexity for Managers: Disagreement About the Corporate Objective

The foregoing discussion of the baseline model only briefly mentions (in Section V.B) an issue that adds to the complexity that managers face when seeking to identify optimal financial policies. That issue is the failure of the Fisher Separation Theorem (FST) in virtually all interesting economic settings, including that of the baseline model. The FST indicates that, regardless of the details of their preferences, all shareholders of a firm unanimously agree that maximization of market value is the proper objective for managers, that is, value (current wealth) maximization is utility maximizing for all owners (Fama and Miller ((1972), pp. 70–71)). The FST holds in perfect-market settings, with perfection defined to include price-taking behavior, frictionless exchange, and strong-form informational efficiency.

Value maximization, either market value or intrinsic value maximization, is the centerpiece of the leading models of capital structure, and it is a pervasive foundational premise in the literature and textbooks that is treated as the unquestionably correct (and preference-free) objective for the firm.

However, outside of perfect-markets conditions, value maximization is no longer the clearly correct and unanimously supported (by shareholders) corporate objective. Indeed, there generally will not be a preference-free corporate objective: Absent homogeneity restrictions on the tastes of shareholders, there will generally not be an objective that is unanimously supported by shareholders. The problem is *not* market incompleteness, as the FST continues to hold in incomplete markets, given an intuitive framing of the notion of perfection in such markets; see DeAngelo (1981) and papers cited therein.

The empirically important reasons for FST failure are factors that impede *exchange*, that is, that impede shareholders from trading out of their value-maximized portfolio positions to positions they would personally prefer. For example, risk-averse shareholders will prefer value-maximizing decisions that entail high risk as long as they can take their maximized wealth and use it to trade to a lower-risk position for their personal portfolios. If such exchanges are impeded to any material degree, then high-risk corporate decisions will be unattractive to risk-averse shareholders, even when they are value-maximizing.

The general point: When shareholders can't trade easily out of risks they do not want to bear personally, they will not view value maximization as the proper objective for the firm and, more generally, they will typically disagree about the objective that managers should pursue.

These sorts of disagreements arise for a variety of plausible reasons, including asymmetric information and endogenously agreed-to contracts.

For example, consider an asymmetric information situation in which there are two managers who have the same inside information that the firm's shares are quite undervalued, with both holding on to their shares to capture the future market price increase when the undervaluation is corrected. Manager A is much more risk averse than Manager B, and so A will prefer safer projects; in other words, Manager A will be more willing to trade off intrinsic value for safer returns. Managers A and B will need to negotiate a resolution of their disagreement over which projects to take, but their ultimate resolution will generally not maximize intrinsic value.

For another example, consider intergenerational disagreements at family-controlled firms, with older family members wanting higher dividends and safer projects than the younger folks want. It's a feud among owners, not agreement on firm-value (shareholder-wealth) maximization. What will resolve the feud so that the family converges on value maximization as the unanimously agreed-upon objective? That will happen if family members are able and willing to trade out of their equity positions at approximately fair prices. But if they want to keep control in the family, such solutions are often simply not feasible.

Given the importance often attached to keeping control generally as well as the prevalence of family-controlled firms in the world, this second example indicates that owner conflicts and collective-action problems are likely to be of broad-based importance in determining real-world capital structures. Control-related capital structure decisions can thus be added the list of regularities that the baseline model explains better than the trade-off and pecking-order models.

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For the main theme of this paper, the implication is that collective-action problems exacerbate the complex decision problems that managers face, but they do not alter the main takeaway from the earlier analysis. Managers still have good fundamental reasons to seek capital structures that provide reliable access to funding, and the added complexity of collective-action problems means they will find it even more difficult to isolate with any real precision a uniquely optimal way to provide such access.

A caveat: Although the FST does not hold in virtually all realistic economic settings, it does not follow that managers should abandon concerns with the market- and/or intrinsic-value consequences of their decisions. Because managers face serious knowledge impediments to identifying optimal capital structures with precision, decision heuristics become sensible tools for the pursuit of reasonable funding policies.

Viewed in this light, a focus on enhancing shareholder value is better viewed as a useful heuristic for financial management than as a criterion for strictly optimal corporate decisions as in the sense of the FST.

G. Ambiguity About Optimal Policies: The Distinction Between Operating Firms and Banks

The ambiguity about optimal policies applies more strongly to nonfinancial (operating) firms than to banks (the two corporate entities in Section V's baseline model).

With operating firms, production of real goods and services will almost surely be the main source of value generated for owners. Their financial policies are important for getting adequate funds to support production and so, as long as the desired capital can be obtained, managers will be more focused on using it wisely for production than on fine-tuning the debt–equity mix.

With banks, on the other hand, capital structure is central to the business model. Banks generate value for their owners from the return spread on their sources and uses of funding. So, banks should focus more than operating firms on fine-tuning the liability side of their balance sheets. Banks will need to focus on risk management of assets, too, because of the importance of tailoring their assets to support the liquid financial claims (e.g., deposit debt) they produce to earn their return spread; see DeAngelo and Stulz (2015).

For banks, the better the job that managers do in optimizing the funding mix, the more profitable they become. That does not mean that real-world banks will necessarily converge over time to a sharply delineated single best capital structure. Managers of banks have imperfect knowledge and face evolutionary pressures, just like managers of operating firms.

Consequently, as new attractive financial products and strategies (e.g., money-market funds and repo transactions) emerge, banks will tend to gravitate away from deposit funding (as in Section V's initial bank capital structure) simply because that source of funding has become more difficult to attract.

It seems doubtful that bank managers in the real world will ever know with certainty the single strictly best capital structure for their institution. However, because of their history of making money by focusing on return spreads, bank

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managers should be better able (than managers of operating firms) to identify a more narrow range of attractive capital structures.

VIII. Summary and Implications for Capital Structure Analysis

In this paper, I critique the capital structure literature with a focus on understanding why, despite 63 years of intense research effort, we have fallen far short of identifying a model that credibly explains real-world capital structure behavior. The critique yields a simple baseline framework that repairs the empirical failures of the leading models of capital structure and provides a foundation for a credible comprehensive theory. I also challenge some important ancillary elements of received wisdom about capital structure, including the incentive to lever up in Miller's (1977) horse-and-rabbit-stew argument about corporate taxes and Jensen's (1986) view of the disciplinary role of debt.

The baseline framework abandons the literature's pervasive paradigm of full-knowledge optimization and instead recognizes the importance of imperfect knowledge when managers make financing decisions. Imperfect knowledge implies there is no single clearly best capital structure for a firm.

Managers accordingly focus on arranging reliable access to funding by choosing among a set of financial policies that all appear to be roughly equivalent in terms of providing such access, while avoiding policies that experience has shown to be clearly inferior, for example, those that risk financial distress and the impaired access to funding that comes with distress.

Instead of (instantaneously and effortlessly) solving a complex formal optimization problem to select a uniquely optimal capital structure, managers experiment with different financial policies and cull those that are clearly revealed to be inferior. Because reliable funding access is valuable to operating firms, costly financial intermediation is valuable, as is financial innovation generally, with banks and shadow banks helping to produce such access, such as with corporate credit lines and deposit debt.

Ironically, the baseline takes us almost, but not quite, full circle to the view of capital structure in MM (1958) and Miller (1977). The baseline eschews MM's frictionless foundation, but it nonetheless implies that there are substantial indeterminacies in capital structure because managers operate in a "pea-soup fog" of limited knowledge about how to optimize financial policy.

I use the "pea-soup fog" metaphor deliberately to emphasize the existence of large costs of knowledge production that deter managers from reducing to negligible levels their ignorance about how to optimize financial policy. When such costs are present, many feasible financing choices appear (roughly) equally attractive, and managers treat them as such even if there are nontrivial actual cost differences that could be detected by someone who hypothetically had perfect knowledge.

Capital structure still matters because funding is essential to generate value from investment—a view of funding implied by MM (1958)—but there is no clearly single best way to arrange reliable funding access. In this sense, the baseline has more in common with Miller's (1977) Darwinian view that neutral mutations account for much of observed capital structure variation than it does with the trade-off and pecking-order models in which managers have perfect knowledge to optimize financing decisions with precision.

What are the important general messages from this paper for capital structure analysis?

Indeterminacies in the choice of financial policies. The data clearly indicate that we need to break the functional (unique) mapping from fundamentals to financing decisions that exists in all of the leading models. The point is *not* that fundamentals are irrelevant to financial policy. Fundamentals clearly matter, especially through the importance of providing reliable access to funding. The problem is that there is considerable slack in the system, and so models in which fundamentals map *uniquely* into financing decisions do poorly empirically. The unequivocal implication is that empirically credible theories need to recognize indeterminacies in the choice of financial policies.

Indeterminacies: Imperfect knowledge versus literal indifference. One conceptual way to break the strict functional mapping would be to posit full-knowledge optimization coupled with settings in which financing-related costs imply literal indifference (in the MM (1958) sense) among a subset of financing choices.

This approach yields fragile theories that are empirically problematic, as all such theories counter-factually predict that financing decisions will change in response to new small changes in contracting frictions. The imperfect-knowledge approach does not suffer from the same empirical shortcoming and it robustly eliminates the clearly counter-factual unique mapping from fundamentals to financing decisions.

Imperfect-knowledge versus perfect-knowledge paradigms. There is a natural desire to cling to full-knowledge optimization models in our research and teaching because they are the core foundation of the training received by virtually all of us. But financial economists have spent 6 decades working on the capital structure puzzle and, despite that intense research effort, we cannot credibly claim to have figured out how to identify real-world optimal capital structures with any precision. If it were truly easy to do so, we would have solved the capital structure puzzle many years ago.

The only reasonable conclusion is that full-knowledge optimization is a poor approximation to how capital structures are set in the real world.

The imperfect-knowledge approach accordingly seems far more deserving to be the a priori maintained (null) hypothesis, with financial economists who instead favor the full-knowledge approach bearing the burden of proof to establish its empirical superiority.

Key concept to move the literature forward. The key to an empirically credible theory of capital structure is the principle that managers cannot identify optimal financial policies with any real precision. While this principle might sound nihilistic, it is in fact realistic, and it provides a robust and plausible foundation to repair important empirical failures of the leading models of capital structure.

Promising research avenues. *Intra*industry variation in capital structure is largely uncharted territory and is an especially promising focal point for new empirical work. In the leading extant models, economic fundamentals determine uniquely optimal financing decisions, which means that firms in the same industry should have similar financial policies. With imperfect managerial knowledge, there is wide scope for differences in capital structure among firms that have similar (or even essentially identical) underlying fundamentals. Consequently, analysis of intraindustry heterogeneity in capital structure has great potential for gauging the importance of the imperfect-knowledge argument advanced here.

Another promising line of research would gauge managers' cognitive abilities to identify optimal financial policies with precision. This issue could sensibly be approached through the use of detailed interviews. Survey questions alone are of limited usefulness to the task because their phrasing often leaves them open to multiple materially different interpretations. For example, survey responses that a firm has a leverage target are not informative without a clear understanding of what is meant by a target. It's one thing for managers to say that a firm has a leverage target as a rough financial planning guide. It's quite another to say that a firm has a leverage target that managers confidently view as strictly optimal, so that deviations from target imply material value losses. Detailed cross-examination in interviews would seem to be important for drawing reliable inferences, as simply hearing managers express confidence in their estimates of how firm value would vary with leverage is unconvincing absent backup for that claim. The opportunity for checking managerial claims about optimal capital structure against actual behavior would be another important feature of research focused on managerial cognition.

General message for research. Stop emphasizing models that focus solely on optimizing the debt–equity mix, and call a halt to the closely related empirical work that estimates the speed of adjustment to a target leverage ratio. Those issues are sideshows for understanding capital structure. Drop the pecking-order model per se, and treat pecking-order behavior as a rough empirical tendency. Instead, focus on how firms go about arranging reliable funding access and on the role of funding in the path-dependent dynamic link between levering up and deleveraging. Abandon the assumption of full-knowledge optimization. Instead, recognize that fundamentals do not strictly determine financing decisions because managers cannot identify optimal financial policies with any real precision. Drop models in which equilibrium requires fully integrated capital-market pricing of securities and, relatedly, recognize a major role for costly financial intermediation in the funding of nonfinancial firms.

General message for teaching. Start as usual with MM (1958), but instead of highlighting the irrelevance of the debt–equity mix, emphasize that investment policy is the foundational source of value for operating firms. Point out that prudent managers will focus on having reliable access to funding since, without funding, they cannot create value from operating policy. Recognize that managers (and finance professors) have imperfect knowledge, and thus cannot optimize capital structure in a fine-tuning sense. Recognize that managers still must choose financial policies, and so it makes sense to pursue policies that position their firms to address funding needs that may arise. Discuss the prevalence of managerial attempts to time the capital markets and the link to imperfect managerial knowledge. Explain the incentive for firms to use debt for transitory funding, just as individuals use credit cards, with firms borrowing to meet a funding need and then deleveraging to have "dry powder" for future funding needs.

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TABLE 1

Capital Structure: Basic Principles

1. The foundational problem of corporate finance is arranging reliable access to funding.
2. There is no single clearly best way to arrange reliable funding access. The reason is that managers, like financial economists, cannot identify optimal financial policies with any real precision.
3. There are numerous combinations of debt, cash, and payout policies that appear (roughly) equally attractive, with optimality requiring that they fit together in a way that provides reliable funding access.
4. Optimizing leverage per se is too narrow a way to think about capital structure. It ignores the funding role of the asset side of the balance sheet and interdependencies among debt, cash, and payout policies.
5. Reliable access to funding is a valuable asset, with banks (and shadow banks) creating social and private value by producing it, e.g., by supplying credit lines and issuing deposit debt.
6. Financial distress: Avoiding it is important, since funding access is especially difficult in distress.
7. Transitory debt: Firms use debt the way individuals use credit cards. They borrow (lever up) to address funding needs and then deleverage (as earnings permit) so they can borrow to meet new funding needs.
8. Dry powder and the option to borrow: That option is valuable and comes from untapped debt capacity and includes the unused portion of a corporate line of credit.
9. Permanent debt: Debt in the capital structure generates tax savings, but uses dry powder, per #8. So, having permanent debt is more attractive when funding needs can more likely be covered from earnings.
10. Cash balances: Cash holdings are a valuable source of dry powder to meet funding needs, but firms limit them due to taxes and a liquidity discount on returns earned on cash.
11. Payouts: They are funding needs, just as investment/operating outlays are. Payouts per se matter because they, not free cash flows that might not ever reach shareholders, determine equity value.
12. Leverage targets: They include the option to borrow and move temporarily away from being optimally positioned to address funding needs. Firms do not move inexorably toward a leverage target (see #7).
13. Credit-rating targets: They are more relevant than leverage targets because they are more indicative of the extent to which a firm has reliable access to funding (see #4).
14. Market timing: Managers believe they can time the markets for their firm's securities and often act on that belief. It is unclear that they often actually capture gains from attempts to time the market.
15. Debt overhang: It is important at troubled firms, but pervasive attempts to capture overhang-related wealth transfers (leverage-ratchet effects) are not important.
16. Disciplinary role of debt: It is not generally important, even in LBOs. It is plausibly important near the corporate endgame to help ensure efficient liquidation.

Bottom line: An empirically sound way to think about capital structure. Table 1 provides a compact summary of principles for capital structure analysis that emerge from the arguments in this paper. I view the principles in two complementary ways. First, they are core concepts for thinking about capital structure that I would want students to carry away from a corporate finance class. Second, they provide a foundation for a theory that not only explains the main previously documented facts, but would move beyond the baseline model to explain the nuances of real-world capital structure behavior. The critical foundation for thinking about capital structure: Firms focus on reliable access to funding rather than optimizing the debt–equity mix because i) managers do not have knowledge of even a rough approximation of the "correct" (empirically relevant) model of optimal capital structure, yet ii) there is no doubt that funding is needed to produce value.

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