

Finance research: What are the new frontiers?

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Prepared for *The Financial Review*. I gratefully
acknowledge helpful comments from the
editors, Michael Pagano and Tianxia Yang. All
errors and infelicities are solely my
responsibility.

Abstract

This paper provides a prospective look at the most exciting open research questions for future finance research in three important areas: (1) banking stability; (2) the intersection of medicine, healthcare, and finance; and (3) organizational higher purpose. In each case, a brief discussion of the existing literature is followed by a list of open research questions for future research to explore.

KEYWORDS

banking stability, future research directions, medicine and finance, organizational higher purpose

1 | INTRODUCTION

“Whatever you can do, or dream you can, begin it. Boldness has genius, power and magic in it.” – Johann Wolfgang von Goethe

Finance research over the past 60 years or so has redefined Economics. From the impressive yet almost nihilistic beginning of modern Finance with the Modigliani and Miller (1958) irrelevance theorems to its current state, the research journey has been bold, exciting, and transformational. What is striking about these research advances is that even though Finance is a subfield of Economics, it has made contributions that are distinct from other strands of economic thought, thus providing new first principles of analysis and expanding the boundaries of Economics to include models and methodologies previously outside the Economics domain.¹

A brief review of the research journey of the field reveals a staggering list of towering accomplishments:

- Markowitz (1952) and the efficiency frontier of the risk-return tradeoff.
- The Modigliani and Miller (1958) capital structure and dividend policy irrelevance theorems.
- The Market efficiency hypotheses and empirical approaches to examining market efficiency (e.g., Fama, 1970).

¹ Think of Harry Markowitz's dissertation on risk, return and diversification being considered by some as not being Economics. When Markowitz received the Nobel Prize in Economics, he famously remarked, "It is Economics now!"

- The Capital Asset Pricing Model (CAPM) developed by Lintner (1965) and Mossin (1966), Sharpe (1964), and the Arbitrage Pricing Theory (APT) of Ross (1976).
- The Black-Scholes-Merton Option Pricing Model (OPM); Black and Scholes (1973) and Merton (1973).
- Market Microstructure Theories (e.g., Kyle, 1985).
- The rich literature on why banks exist (e.g., Allen, 1990; Diamond, 1984; Ramakrishnan & Thakor, 1984), bank runs (e.g., Bryant, 1980; Diamond & Dybvig, 1983), and the role of deposit insurance as a put option whose value can be controlled via capital requirements and asset portfolio restrictions (e.g., Merton, 1977).
- The vast literature on agency costs (e.g., Jensen & Meckling, 1976) and other informational frictions and how they impact corporate financial policy (e.g., Bhattacharya, 1979; Myers & Majluf, 1984).
- Theories of debt overhang and associated inefficiencies (e.g., Myers, 1977).

Clearly, this list is not complete and many will quibble with my omissions. But my point here is to be illustrative rather than exhaustive.

Of all of these accomplishments, I view the Black-Scholes-Merton OPM as the single most important research contribution not only to Finance, but to all of social sciences, in terms of both its academic impact on subsequent research as well as its impact on practice. Think of how obscure options were as a financial market instrument before the OPM papers were published, and how ubiquitous they are today. In addition to traded options, the markets for asset-backed securities, credit default swaps, and hosts of other derivative claims provide ample evidence of the profound impact formal models of option pricing have had on both research and practice.

These path-breaking contributions of the past have provided Finance with an impressive and firm foundation of *first principles* upon which to build its edifice of future research.² What does the future hold for Finance research? That is the question I confront in this paper. Given the proliferation of excellent research in a variety of Finance subfields, this is a Herculean task if one were to try and do justice to all of the important research. So rather than attempt that, I will selectively focus on three areas – one well-trodden but two very early in their development and hence somewhat obscure – that I believe provide a rich set of research questions to explore. The choice of these three areas is purely a matter of personal preference and interest. The three areas are discussed below.

First, let me state the well-trodden area, which is banking stability, but my focus is on considering shocks to stability emanating from both familiar sources that have been extensively studied, as well as from non-traditional sources like fin tech, shadow banking, crypto currencies, government fiscal policy, national politics, geopolitical headwinds, climate change, cyber risk, and societal upheavals. The research question here is: What are the non-traditional (frontier) risks for banks, how will they interact with traditional risks, and what macroprudential regulation tools would be best to deal with these risks?

Second, healthcare is increasing as a percentage of GDP and an aging population in developing countries is creating a huge upsurge in demand for more and more biomedical innovations to be financed by non-governmental sources. While our universities rely heavily on NIH funding for biomedical research, this source will continue to decline as a percentage of the total investment in biomedical research in the future. A major problem is much of the early stage innovation in this area needs large investments for further development but is subject to substantial risk. This makes raising external financing challenging (e.g. Hall & Lerner, 2010) and calls for *financial* innovation to help close the “funding gap” (e.g., Jorring et al., 2022). The broad research question is: What role can financial innovation play in closing the R&D funding gap in biomedical research, and how can financial intermediaries facilitate this role?

Third, there is a growing recognition that the traditional firm objective function of shareholder value maximization needs to be appended with some pro-social “higher purpose” goals that also guide corporate decision-making (e.g.,

² So, the first principles provide valuable discipline. One sometimes sees a paper by researchers outside Finance where it is assumed, as part of the analysis, that here is an optimal capital structure. But when you ask which Modigliani-Miller assumption is violated to generate an optimal capital structure, you discover that the capital structure irrelevance theorem holds, calling for a change in the model.

Gartenberg et al., 2019; Hart & Zingales, 2022; Henderson & Van den Steen, 2015; Quinn & Thakor, 2018, 2019). The broad research question here is: What are the effects of adopting a higher purpose on contracting outcomes, investments and corporate financial policy in general?

Note that these are very broad questions. Underneath each is a plethora of interesting, bite-sized questions that are more specific and yet have the potential for weighty contributions. And the last two questions are in areas that are not crowded, at least not yet. This means it is easier to uncover previously-untouched questions to address.

In the rest of the paper, I provide my thoughts on these three big research questions. The discussion raises more questions than it answers, but my hope is that it stimulates rigorous explorations of a multitude of issues that we do not currently have a good understanding of.

The rest of the paper is organized as follows. Section 2 takes up the first question about frontier risks and banking stability. Section 3 addresses the biomedical innovation question. Section 4 turns to organizational higher purpose. Section 5 concludes.

2 | BANKING STABILITY

Banking stability has been a preoccupation of researchers and policymakers for centuries (e.g., Reinhart & Rogoff, 2009), and the literature on it is vast, with a variety of theories, offered for why and how financial crises arise (e.g., Allen & Gale, 1998; Calomiris & Haber, 2014; Gennaioli et al., 2015; Lo (2012); Thakor, 2012, 2015a, 2016).³ One reason why financial crises attract so much attention is that they are very disruptive and costly, with damage caused not only by the inability of failed banks to provide a host of intermediation services like *funding liquidity creation* (e.g., Donaldson et al., 2018; Thakor, 2018) and reliable continuation financing (Banerjee et al., 2021), but also due to associated negative shocks to GDP. According to Atkinson et al. (2013), the 2007–2009 financial crisis cost the U.S. an estimated 40%–90% of one year's economic output, which is \$6–\$14 trillion.

While the root causes of financial crises are endlessly debated, recent research has done a lot to clarify the picture. Reinhart and Rogoff's (2009) analysis of 800 years of global financial crises reveals that, while every crisis is different, there are two factors that show up in almost every crisis – an asset price bubble somewhere in the economy and excessive leverage either on bank or household balance sheets or on both.⁴ While many crises, including the 2007–2009 crisis, appear initially to be caused by a precipitous drying up of aggregate liquidity, it turns out in the end that the real culprit is elevated *counterparty risk* triggered by heightened concerns about *insolvency*. In other words, when the asset price bubble bursts, banks have to write down the values of assets on their balance sheets, leading to an erosion of equity capital and increasing insolvency risk. For an extensive discussion of why, in light of this, regulators need to focus on insolvency risk and not illiquidity risk in coming up with measures to thwart the onset of a crisis, see Thakor (2018).

Extensive research has established that the most effective solution to reducing insolvency risk and thereby diminishing the likelihood of a financial crisis – insofar as banks are concerned – is to increase bank capital, either through the voluntary actions of banks or via higher capital requirements; see Thakor (2014) for a more complete discussion. But banks often resist higher capital requirements, arguing that they hurt their competitiveness *vis a vis* banks in regulatory jurisdictions with lower capital requirements and thus damage their market values. The argument, however, is specious, as research has shown. Mehran and Thakor (2011) show that banks with higher book equity capital ratios have higher equity values in the cross-section.⁵ Berger and Bouwman (2013) provide evidence that better-capitalized banks were able to improve their competitive positions and acquire less-capitalized banks during the 2007–2009 financial crisis. Perignon et al. (2018) document that high-capital European banks had *greater* access to short-term

³ See Lo (2012) and Thakor (2015b) for reviews.

⁴ Donaldson et al. (2019) show theoretically that excessive household leverage contributes to unemployment.

⁵ They also have higher total values.

uninsured deposit funding and thus expanded this funding during the 2007-09 crisis, whereas low-capital banks shrank.

Thus, to deal with the kinds of risks that have spawned banking crises, it appears that banks need to have higher capital ratios, and this will not only be good for society due to the reduced risk of financial crises, but may also benefit the *banks themselves*. That is, at least at the current bank capital ratios, there should be no serious tension between bank shareholder value and social welfare in insisting on higher capital in banking.

Of course, banks are just one – albeit the most important – part of the financial system. As we were reminded during the 2007–2009 crisis, as well as by more recent events, banks and many other parts of the financial system are joined at the hip.⁶ This means that as innovations in other, perhaps less regulated, parts of the financial system generate new types of risks, they hit banks too. So, in addition to the interest rate and credit risks that have been largely responsible for imperiling banks, a host of “frontier risks” – non-traditional risks that may not be on the risk management radar for most banks – must also be considered.

What are some of these frontier risks that future research should study? I list a few below:

Fin-tech

There has been some discussion of the competitive threat that banks face in the lending business from non-bank lenders like P2P platforms (see de Roure et al., 2022 for evidence). While this threat may be real, I am skeptical that it will undermine banks in any significant way. Relationship banking will continue to be valuable for a variety of reasons, including improved credit terms (Boot & Thakor, 2000), better collateral utilization by banks (e.g., Cerqueiro et al., 2020) and more favorable continuation lending terms for borrowers (e.g., Banerjee et al., 2021). Moreover, if borrowers find borrowing from P2P platforms more attractive than traditional bank loans, banks will simply create subsidiaries that engage in P2P lending (e.g., see Thakor, 2020).

The more ominous threat comes from crypto currencies and cyber attacks. The recent FTX collapse has highlighted the manner in which cryptos can pose a risk for banks if they have relationships with firms that are either engaged in crypto trading or are long in crypto currencies. Cyber security has been presented as one reason the US Federal Reserve has been cautious in introducing central bank digital currency (CBDC), although other countries (e.g., China) have, and the Federal Reserve has been considering the idea. See Thakor (2019) for a discussion of the pros and cons of CBDC.

Politics and government fiscal policy

Politics and banking have been intertwined for centuries (e.g., Calomiris & Haber, 2014), so it is not surprising that politicians attempt to influence banking. However, this influence is not always benign, and it can not only have allocational consequences but also potentially increase systemic risk in banking. In addition, recent events have highlighted how banks also face risk from the fiscal policy choices of the government. If the government undertakes spending policies and/or restricts some sectors like oil and gas, it can contribute to inflation that necessitates a tighter monetary policy by the central bank, which in turn can generate a negative shock for banks.⁷

⁶ For a theoretical analysis of how banks and financial markets arise endogenously and segment the resolution of different types of informational frictions, see Boot and Thakor (1997).

⁷ Another example may be imposing fees on low-credit-risk borrowers to subsidize risky mortgages, creating credit risk for banks.

Geopolitical headwinds

Geopolitical tensions and wars invariably strain international trade and can pose risks for banks engaged in international lending and borrowing. This can lead to not only direct costs experienced by banks, but also their borrowers, with possibly significant real-sector implications.

Pandemics

A pandemic like COVID-19 can shut down economies and engender unforeseen risks for banks. There has been an explosion of research on the impact of COVID-19 on banks and their borrowers. But the research is entirely retrospective, and there is a crying need for normative theoretical research on how banks can best prepare to cope with risks created by future pandemics.

Climate change

This is a politically divisive issue that has attracted considerable recent research attention. For banks, the risk is primarily twofold. One is that more extreme weather can adversely affect borrowers who reside in coastal areas and increase default risk. This can be dealt with by using adequate loan loss provisioning and higher levels of capital. The other risk inheres in how *regulators* will react to climate risk. If they attempt to use the tools of prudential regulation – say capital requirements – to influence banks to make more “green” loans at the expense of “brown loans”, then there may be complex interactions that generate unforeseen consequences. This is a source of risk that is difficult to hedge against just with more capital, and research is sorely needed on the issue.

The open research questions:

Based on the discussion in this section, I believe that exploring the following research questions (RQ) holds substantial promise.

RQ1: Why are banks resistant to higher capital requirements when they strengthen their competitive position and may generate higher risk-adjusted returns for their shareholders?

RQ2: What approaches can regulators use to overcome this resistance from banks and infuse more capital into banking? What are the welfare implications of a “capital insurance” program – along the lines of federal deposit insurance – wherein banks pay an on-going fee during normal times for access to non-dilutive equity capital from the government during a crisis? The fee can be risk-sensitive and dependent on the bank’s risk and capital ratio.

RQ3: How should banks deal with cyber risk and risks from crypto currencies?

RQ4: What are the optimal risk mitigation approaches for banks to deal with political risk? How does political risk affect banks?

RQ5: How should banks deal with new emerging risks like geopolitical risks and pandemic risks?

RQ6: What are the potential consequences of governments using central bank regulatory tools to influence banks in an attempt to deal with climate risk?

3 | MEDICINE, HEALTHCARE, AND FINANCE

Aggregate healthcare spending in the U.S. is predicted to reach \$6.3 trillion by 2028 (see Lo and Thakor 2023)). This is in line with the critical and growing importance of healthcare therapeutics to the world economy. Nonetheless, a major problem is that there is underinvestment, relative to the social optimum, in the biomedical R&D needed to create these therapies, and it is caused by what is called the R&D “funding gap” (e.g., Arrow, 1963; Hall & Lerner, 2010; Schumpeter, 1942). This funding gap is especially acute for biomedical R&D. Lo and Thakor (2023) propose that it is due to financing frictions generated by the fact that these projects have long durations, large capital outlays, and involve technical complexity in the drug development process.

This underinvestment in biomedical R&D has huge implications for both the real economy as well as the financial system. Lack of progress in curing diseases leads to an overall decline in the productivity of the population, and it also reduces the ability to deal effectively with pandemics. The adverse implications of this for the real economy are self-evident. But the financial system suffers too as the diminished real-sector productivity not only directly affects financial institutions but also makes their borrowers worse credit risks.

What can Finance research do to address this problem? Existing research suggests at least two distinct ways. One is for financial innovations to help attenuate the problem. For example, Jorring et al. (forthcoming) propose a new financial instrument – the “FDA hedge” – to reduce the biomedical R&D funding gap by insuring against the risk that a drug may not receive FDA approval. It pays off the firm that purchases the hedge if there is failure in obtaining FDA approval. The paper develops a theory to show that the FDA hedge can help eliminate the funding gap, and then uses novel project-level data to provide empirical evidence that FDA hedge risk is idiosyncratic. Karaca-Mandic et al. (2023) examine interventions to address social drivers by health (SDH), such as food insecurity, transportation and housing that can reduce future healthcare costs but require up-front funding. They propose a financial innovation, an “SDH bond”, that would increase investments in SDH interventions. The idea behind the bond is that it would be issued by multiple managed care organizations within a Medicaid coverage region and involve coordination across these organizations.

The second approach is for banks and other financial intermediaries (e.g., venture capital firms) to get directly involved in financing biomedical innovation. This issue is squarely addressed in Lo and Thakor 2023) who argue that banks can potentially resolve some financing frictions through relationship lending without significantly adding to their risk exposure.⁸ In particular, banks can use their special expertise in screening and monitoring, and can also resolve incentive problems related to the non-pledgeability of certain assets in a way that markets cannot. Goldman and Peres (2023) provide evidence that when the firm’s financiers have better information about the firm, it invests more in R&D. This means getting banks—which have the expertise to access better information about their borrowers—more involved in R&D financing can potentially enhance R&D investments. Moreover, some of the R&D underinvestment may be due to disagreement between firms and investors about R&D payoffs. As Coval and Thakor (2005) show, financial intermediaries can provide a “beliefs bridge” and facilitate investment. Banks can also protect the proprietary information about borrowers’ R&D that they obtain as part of their relationships (e.g., Bhattacharya & Chiesa, 1995). Lo and Thakor (2023) develop a theoretical model that fleshes out these ideas.

This discussion suggests the following open RQs.

RQ1: What new banking theories can we develop in which the factors that provide the *raison d’être* for the bank also facilitate the bank’s role in closing the biomedical R&D funding gap?

RQ2: What regulatory policies may impede banks’ ability to do this? How should future bank regulation be designed to facilitate, rather than impede, banks’ role in closing the R&D funding gap?

RQ3: What financial innovations can help banks and markets to help close the biomedical R&D funding gap and what innovations can help improve the delivery of healthcare?

⁸ See Boot and Thakor (2000) for a theory of relationship lending by banks when they face competition from other banks as well as the capital market.

4 | ORGANIZATIONAL HIGHER PURPOSE

There has been considerable research interest in the past few years in the idea that the firm's objective should not only be to maximize shareholder value, but also to do good for society, although one could argue that the basic concept has a rather hoary tradition stretching all the way back to Adam Smith who introduced the idea of society benefitting from consumer surplus created by the firm. A recent strand of the literature refers to this as organizational "higher purpose", which is a prosocial goal that transcends the usual business goals but calls for decision-making to be at the intersection of business goals and that higher purpose (e.g., Bartlett & Ghoshal, 1994; Besley & Ghatak, 2005; Bunderson & Thakor, 2022; Gartenberg et al., 2019; Quinn & Thakor, 2018, 2019).⁹

While this way of thinking seems to conflict with the pristine and unambiguous goal of shareholder value maximization, what the theoretical and empirical research has shown is that the adoption of an *authentic* organizational higher purpose can potentially lower agency costs, boost employee morale and improve economic outcomes for firms in the long run., although this is subject to risk and not guaranteed in every case. However, this pursuit often requires short-term financial sacrifices, thereby creating a tension for top management, and the basic question of whether the firm's objective should include higher purpose is not settled yet. We need more research on this issue.

One major obstacle to "diluting" the shareholder-value-maximization objective with higher purpose goals is that it opens the door to some (a priori unidentifiable) CEOs pursuing private benefit projects dressed up to look like higher purpose investments. Thus, there is the potential for both greater adverse selection and moral hazard, creating a need to think more carefully about how incentive contracting would need to be modified to cope with it.

The other problem is that with political divisions and different points of view about what enhances social welfare, a firm that embraces a particular higher purpose may find itself being criticized by groups whose special interests may be orthogonal to that purpose.

Finally, there is the question of social pressure. Some prosocial goals (e.g., racial justice, climate change, etc.) may be so compelling for some groups that they may exert pressure on firms to adopt these as higher purpose goals. This may be counterproductive. Song and Thakor (2023) show that even when the authentic and voluntary adoption of a higher purpose enhances social welfare, mandating it through excessive social pressure or regulation can lower welfare for all agents.

The nascent body of research on higher purpose seems to have had much greater visibility in the management literature (e.g., Bartlett & Ghoshal, 1994) and in Economics (e.g., Benabou & Tirole, 2006) than in Finance. But it is time for Finance research to tackle this topic. I see at least three promising avenues for future research. Each is associated with research questions.

Theoretical research on the appropriate objective function for the firm

Thus far the papers that have modeled higher purpose have simply attached a utility associated with purpose pursuit to the firm's and/or employees' objective function (e.g., Henderson & Van den Steen, 2015), and different papers adopt different approaches to doing this. But we need a consistent approach, which leads to:

RQ1: How should we formally modify the firm's objective function to account for higher purpose?

RQ2: What are the potential moral hazard and adverse selection problems created by including higher purpose in the firm's objective function and how should incentive contracting deal with these problems?

⁹ As an example of higher purpose, consider *4Ocean*, a firm that pays fisherman to dredge plastic garbage from ocean floors and then uses the plastic to make consumer products that it sells. Its higher purpose is to end the global ocean plastics crisis. Another example is Patagonia, whose founder donated controlling interest in the firm to a foundation focused on environmental protection and preservation.

Implications for financial policy

If we modify the firm's objective function, it stands to reason that this will impact the firm's financial policy.

RQ3: How will including higher purpose as a firm objective affect the capital structure and dividend policy decisions of the firm?

This needs to be explored both theoretically and empirically.

Causal research on the impact of higher purpose

We need to have more causal evidence on how higher purpose affects behavior and economic outcomes. This will require field studies and corporate interventions. The evidence that exists at present is outside Finance (e.g., Grant et al., 2007). That is:

RQ4: What is the causal empirical evidence related to the impact of adopting an organizational higher purpose on employee behavior and firm output?

5 | CONCLUSION

In this paper, I have speculated about the possible new frontiers in Finance research. Unlike the usual retrospective approach in a review paper, the focus here has been prospective. I have identified three major areas: banking stability, the intersection of medicine and Finance, and organizational higher purpose. This is by no means intended to be exhaustive. Many important areas where significant future contributions are likely to be forthcoming are excluded – market microstructure, asset pricing (especially intermediary-based asset pricing), macro finance, and household finance are notable examples.

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How to cite this article: Thakor, A. V. (2023). Finance research: What are the new frontiers? *Financial Review*, 1–10. <https://doi.org/10.1111/fire.12348>