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The Lessons of Michael C. Jensen

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April 2025

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Abstract

This paper assesses the contributions of Michael C. Jensen to financial economics and to American business. His work on agency theory is the cornerstone of modern corporate finance. He influenced how American business operates by helping make the internal and external governance of firms more efficient. He changed how knowledge in financial economics is diffused both through the founding of the Journal of Financial Economics and of the Social Science Research Network. I question the claim made by some that he recanted his ideas in the 2000s.

Keywords: Agency costs, managerial ownership, free cash flow, governance, Jensen's alpha

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The lessons of Michael C. Jensen

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Abstract

This paper assesses the contributions of Michael C. Jensen to financial economics and to American business. His work on agency theory is the cornerstone of modern corporate finance. He influenced how American business operates by helping make the internal and external governance of firms more efficient. He changed how knowledge in financial economics is diffused both through the founding of the *Journal of Financial Economics* and of the Social Science Research Network. I question the claim made by some that he recanted his ideas in the 2000s.

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1. Introduction

From the 1950s to the middle of the 1970s, a few scholars built the foundations for a new field of scholarship, the field of financial economics. Michael C. Jensen is one of these scholars. Most of these scholars have been rewarded with a Nobel prize, but the award escaped Jensen. Yet, among the fathers of our field, he ranks second in citations, below only his advisor, Eugene Fama. He also has the distinction of having written the most highly cited paper in our field, his paper with William Meckling on the theory of the firm. The impact of this paper is so broad that the agency theory of the firm is described as "the dominant theory of modern corporate law" (Hill, 2025).

Over his career, Jensen had a constant goal as a scholar and teacher: Make the world a better place. He believed that to succeed he had to relentlessly and fearlessly pursue the truth. He was not writing papers to build a resume, but to change the world. After making important empirical contributions to asset pricing that provided support for market efficiency, he decided that he could have more of an impact through theoretical work in corporate finance and the theory of organizations. He thought that agency theory was important for other fields that he wanted to influence, such as accounting, economics, law, and management. He also sought to change the world outside academia by helping make the corporate world more productive by fighting obstacles that made it harder to monitor, control, and reduce agency costs. In pursuit of the truth, prodded by empirical observation, he paid increasing attention to behavioral factors and market inefficiencies. His most important lesson for corporate finance is that the productivity of firms depends directly on corporate finance, so that corporate financial policy is not a side-show but a critical factor in the success of corporations.

In this paper, I assess how Jensen impacted the field of financial economics and academia more broadly, as well as the world outside academia. I also examine how his thinking evolved. Jensen was controversial throughout his career. *The New York Times* published an article following his death that highlighted both his accomplishments and the controversy that followed him. The article was titled "Michael C. Jensen, 84, who helped reshape modern capitalism, dies." The title captures his enormous influence, but then the article blames him for the "greed-is-good era." Some have argued that Jensen late in his career changed his mind

on a number of issues. Wooldridge (2024), after calling him "Gekko with a doctorate," goes on to say that in the late 1990s "he underwent the equivalent of a religious conversion," leading him to regret or recant some of his earlier ideas. He started talking about what he called "enlightened value maximization," a concept described as having "some remarkable similarities with ESG" (Smith, 2024). Jensen cared more about the truth than consistency. He was willing to change his mind and did so when new facts convinced him to do so, but I will argue that claims of recantation are not accurate.

The central idea that motivated him was that individuals pursue their own objectives, so they have different interests. These different interests create conflicts that impose costs on organizations. Organizations must manage these conflicts to minimize their costs. Different organizational forms have different impacts on the costs of conflicts of interest, so that different activities will be organized using different organizational forms. His most famous article, "Theory of the Firm: Managerial Behavior, Agency Costs and Ownership Structure," co-authored with Meckling (Jensen and Meckling, 1976, *Theory of the firm*), shows how financial policy helps corporations to reduce the costs of conflicts of interest. The article focuses on conflicts of interest between the manager of a firm and its shareholders, as well as conflicts of interest between bondholders and shareholders. It uses the language of agency costs to characterize the costs of these conflicts of interest.

Theory of the Firm was a contribution to positive economics (Friedman, 1953). It helps explain the existence of debt and the role of managerial ownership of shares. This was a seminal contribution as it showed that there is an optimal level of debt even in the absence of corporate taxes. Previous theories of an optimal level of debt were tradeoff theories, which traded off the tax benefit of debt against the costs of financial distress potentially resulting from leverage. Tradeoff theories have no explanation for why firms have debt even when there are no corporate taxes. Much of the subsequent work of Jensen is more normative in that it points out instances where agency costs are poorly controlled and actions that can control them better. Poorly managed agency costs make corporations less efficient through misallocation of resources. Hence, to make corporations more efficient, obstacles to controlling agency costs must be removed and solutions must be found to reduce agency costs. Throughout his career, Jensen studied agency costs caused

by a variety of factors, such as entrenched management, free cash flow, and overvalued equity. He also investigated how agency costs could be controlled through devices internal to the corporation such as the choice of capital structure, the choice of payout policy, the distribution of ownership, the role of corporate boards and compensation policy, as well as through devices external to the corporation, such as the market for corporate control.

In this paper, I first assess the impact of Jensen's work within academia. To measure this impact, I focus on citations. Jensen was always a strong believer in citations as a measure of impact. For him, in any endeavor, it was critical to be able to define success in a way that could be measured. I show the citations of his most cited articles. I then compare his lifetime citations to the citations of winners of the Nobel Prize for Economics[1] and of Fellows of the American Finance Association (AFA). Citation distributions are extremely skewed. Not surprisingly, Jensen is at the extreme right-tail of the distribution for both benchmarks I consider. Only a handful of financial economists have more total citations than Jensen's most cited paper.

I then examine Jensen's main contributions, focusing on their impact on the finance field, on other fields, and on practice. Throughout his career, Jensen focused on big impactful ideas. Identifying these ideas makes it possible to divide his career into distinct periods. Before *Theory of the Firm*, Jensen published papers that, viewed from today, are asset pricing papers. Yet, these papers are important both for their contribution to asset pricing (see Fama and French, 2025) and for how they helped make his ideas in corporate finance more impactful. His dissertation showed how what became known as Jensen's alpha makes it possible to measure the performance of mutual funds. He is a co-author of "The Adjustment of Stock Prices to New Information" with Eugene Fama, Lawrence Fisher and Richard Roll. The paper introduced the event study method. These papers became instrumental in the use of short-run and long-run event studies. Event studies are the main method used to assess how corporate actions impact shareholder wealth and, for Jensen, shareholder wealth is the metric of success for corporate actions.

[1] I abbreviate the formal name of the Nobel prize for economics is the Nobel Memorial Prize in Economic Sciences as is common.

I turn next to *Theory of the Firm*. This paper underpins all of Jensen's subsequent thinking in financial economics. Though Jensen thought of himself as an organization theorist, the main contribution of that paper is how a firm's equity ownership and capital structure can be used to reduce agency costs. While *Theory of the Firm* is focused on corporations with residual claims owned by shareholders, three papers jointly authored by Fama and Jensen use a broader lens to show how conflicts of interest are inherent in activities that involve many participants and how the choice of organizational form and of control and monitoring devices for these activities minimizes agency costs.

An important theme in these agency papers is the role of the market in controlling agency costs. In the early 1980s, Jensen's focus was on the role of the market for corporate control. He argued forcefully that this market had to be left to do its work, so that capital would not be wasted by being put to suboptimal uses. Mergers and acquisitions are valuable in allocating resources to the firms that could have the best use for them. They also play an important role in displacing management that is incompetent or entrenched, when the board of directors fails in disciplining and replacing such management. Eventually, one expects firms with incompetent or entrenched management to fail. However, many firms have a cash cow, a set of assets that produces high cash flows. Unfortunately, managers can use these high cash flows wastefully with little risk of having their firm fail. In one of his most famous papers (Jensen, 1986), Jensen calls this issue the agency costs of free cash flow. He points to the example of oil companies that earn large cash flows because of high oil prices and use these cash flows to make unrelated acquisitions that destroy shareholder wealth. He argues that such companies should be forced to pay out their free cash flow. One way to have such firms commit to paying out free cash flow is to have them issue large amounts of debt, so that they have to use the free cash flow for debt service.

While the use of debt is one prong in *Theory of the Firm* to reduce managerial agency costs, there is a second prong, namely aligning managerial incentives with shareholder incentives. Having managers have more exposure to equity payoffs is one way for managers to have better incentives. Jensen was a vocal defender of paying CEOs according to their creation of shareholder wealth. In a famous paper, Jensen and Murphy (1990) made the case that CEOs were paid like bureaucrats and should not be paid that way.

Managerial incentives can be better aligned with those of shareholders through compensation contracts or through ownership of shares. In *Theory of the Firm*, more debt at the firm level makes it possible for managers to own a greater proportion of the equity and hence to have incentives better aligned with those of shareholders. More debt has a potential cost, as it makes distress and its associated costs more likely. *Theory of the Firm* focuses on the costs of asset substitution, namely the incentives that shareholders have to increase the risk of the firm at the expense of the debtholders. Leveraged buyouts lever up firms, so that managers cannot waste free cash flow and must focus on generating enough cash flow to service the debt. These buyouts also make it possible to have concentrated equity ownership. Concentrated equity ownership facilitates monitoring of managers by shareholders. This led Jensen to argue for the benefits in the allocation of resources of leveraged buyouts and private equity at a time when these practices were under attack. He concluded that the public firm organization form is not well suited to allocate resources efficiently for some types of firms, namely those that have potentially high agency costs that could be reduced through monitoring of management and with sharper constraints on management through debt. His presidential address to the American Finance Association shows that firms in markets that are in decline are especially vulnerable to agency costs (Jensen, 1993).

I then turn to Jensen's last act. Much of what he said before was controversial, even among financial economists. The controversies surrounding Jensen and his positions played out at center stage in financial economics as well as in the business world. After his presidential address and after the Jensen and Murphy papers, Jensen developed a research agenda focused on two connected ideas. The first idea was the concept of integrity. He did not have a normative view of integrity. Instead, he sought to understand practices and attitudes that, using contemporaneous language, are sustainable. As a result, he viewed his inquiry as a positive rather than normative inquiry. He wanted to understand what type of behavior a corporate leader could have that would lead to outcomes consistent with the outcomes that the corporate leader is seeking. The second idea was the concept of enlightened firm value maximization. His basic point was that value maximization is "not a vision or a strategy or even a purpose, it is the scorecard for the organization." Hence, to actually succeed in achieving a high firm value, managers have to do better than saying that they

will maximize firm value. They have to offer a vision or a strategy towards which the organization is working and by doing so they will maximize firm value. These two ideas have been viewed by some as a renunciation of his earlier ideas or, at least, a radical departure from his earlier ideas. I will argue, however, that these ideas are just a deepening of his thinking about how to make organizations achieve their goals efficiently.

Jensen was so impactful because he focused on developing big ideas and on pushing these ideas relentlessly. He made his ideas known through refereed academic papers, but also through wide-ranging proselytization. He wrote articles for practitioners, gave speeches to business audiences, and interacted with leading practitioners. He organized what may be the most influential conference in the history of financial economics, namely the conference organized under the auspices of the *Journal of Financial Economics* on the market for corporate control. This conference focused on how acquisitions and mergers impact shareholder wealth. At the time of the conference, Boone Pickens was at the height of his career in making unsolicited acquisition offers through his company Mesa Petroleum. Jensen strongly argued that unsolicited offers create value when agency costs are excessive because of entrenched management. Boone Pickens was one of the attendees at the conference.

Jensen understood that he could not change the world alone. When he realized that established journals were not sufficiently receptive to what at the time was modern finance, he founded the *Journal of Financial Economics* (*JFE*) with the help of Robert Merton and Eugene Fama. As editor of *JFE*, he viewed part of his role to encourage research on topics that he thought important. With the arrival of the World Wide Web, he understood quickly that the Web would offer a way to share scientific research that would be much more efficient. He became an entrepreneur and, with Wayne Marr, started the Social Science Research Network (SSRN). SSRN completely changed how research in financial economics is diffused. Anybody anywhere in the world could learn about new working papers and could post new working papers. The market for ideas in financial economics became global and access to new ideas instantaneous.

An important way for Jensen to present and develop his ideas was through his teaching. His teaching was focused on how to make organizations more effective. He was recruited by the Harvard Business

School to teach the course on the economics of organizations that he and Meckling had developed in Rochester. At HBS, this course was enormously successful. Even though it was an elective, at its peak it was taken by almost all MBA students. It was taught by Jensen and Professors Baker, Baldwin, Salter, and Wruck. A rough estimate is that more than 5,000 students took that course.

The paper is organized as follows. Section 2 focuses on impact metrics. Section 3 is a brief history of the birth of modern financial economics and Jensen's place in that history before *Theory of the Firm*. Section 4 is about the development of agency theory in Jensen's work with Meckling and then Fama. Section 5 is about Jensen's work on identifying agency problems and solutions to reduce the associated agency costs in the 1980s and 1990s. Section 6 is about Jensen's last act. I then discuss in Section 7 Jensen's contributions through his founding and editing of the *Journal of Financial Economics*, founding and managing of SSRN, and his teaching. I conclude in Section 8 by summarizing Jensen's lessons.

2. Measuring impact

Jensen's work emphasizes the importance of metrics to measure success. He often asked the question: How do we know how well we have done? When it comes to academic papers or to academic journals, his metric was the number of citations. This metric became available two years before the first issue of *JFE*. Merton, one of the co-founders of *JFE*, told Jensen about Eugene Garfield and the Social Science Citation Index that he established in 1972.[2] Jensen saw the value of citations to guide his editorial policy and measure the success of the *Journal*. Jensen's focus on citations was at times criticized. Colleagues pointed out that a paper that is wrong can have many citations. They also pointed out that review papers often have large numbers of citations. They further argued that different subfields have different citation patterns. The top papers in governance have more citations than the top papers in microstructure. All these caveats should be taken into account when comparing citations across scholars. At the same time, what is the alternative? Colleagues often argue that citations do not measure quality. However, what is quality if not impact? What

[2] The father of Robert C. Merton, Robert K. Merton, wrote several theoretical papers on citations as a measure of impact and importance that guided Garfield as he created the Social Science Citation Index.

is the value of a paper that some readers find to be a tour-de-force but nobody cites? Jensen wanted to change the world and an uncited perfect paper has no impact on the world. It would have been easy for Jensen to argue for using citations as a metric of scholarly achievement after he became a huge winner in citations, but he was consistent throughout his life in arguing that citations are the proper metric.

To assess Jensen's citation count, I follow the approach used by Schwert and Stulz (2017) in their paper on Fama's impact. The two main sources for citations are the Social Sciences Citation Index (SSCI) and Google Scholar. The number of citations for a paper is higher in Google Scholar than in the SSCI. At least one factor contributing to this difference is that Google Scholar counts citations in working papers and journals that are not included in SSCI. I use Google Scholar for the simple reason that rankings using Google Scholar are generally highly correlated with rankings using SSCI but are much easier to obtain than rankings using SSCI. For lifetime citations, I start with a scholar's Google Scholar page. Google Scholar pages can include papers that the scholar did not write and fail to include papers that the scholar did write. I try to correct these issues. I also use *Semantic Scholar* and *Publish or Perish* as additional sources.[3] These efforts can make large differences. An extreme example is Stewart Myers, whose total Google citations are roughly 50% higher than the total citations on his Google Scholar page.

Table 1 shows the citation counts for Jensen's ten most cited papers. Not surprisingly, the most cited paper is *Theory of the Firm* with 137,380 Google cites. To understand the significance of this number, it is useful to note that only 13 of the 53 winners of the Nobel Prize for Economics from 2000 to 2024 have more lifetime citations than the number of citations of *Theory of the Firm*. It is not the case of an old paper that is at the tail end of its influence. In Google Scholar, the largest number of citations for the paper is in 2022. It collected 9,361 citations that year. From 2019 to 2023, the paper had more than 8,000 citations each year. Further, out of 74 AFA Fellows, only 12 have more lifetime citations than *Theory of the Firm*. Kim, Morse, and Zingales (2006) used SSCI citations instead of Google Scholar citations to rank economics

[3] A difficulty with using Google Scholar is that citation counts change continuously. With the number of counts used in this article, it would be impossible to have all counts collected the same day. All counts I use are collected during a two-month window from November 15, 2024, to January 15, 2024.

articles published from 1970 to 2005. Jensen and Meckling (1976) is the third paper in that ranking. Kahneman and Twersky (1979) ranks just above Jensen and Meckling (1976). Most of the other top ten papers are econometric methods papers. The exception is Black and Scholes (1973) that is the sixth paper. A major reason for the citation success of *Theory of the Firm* is its multidisciplinary appeal. The article is widely cited in law reviews, in accounting journals, in general economic journals, as well as in management journals.

The second most cited paper in Jensen's top ten list is the paper titled "Agency Costs of Free Cash Flow, Corporate Finance and Takeovers." Few finance papers have more cites than that paper. It is published in the Papers and Proceedings of the *American Economic Review*, so it was not refereed as it would have been had it been published in a regular issue of that journal or of a top finance journal. As mentioned, Fama and Jensen wrote three papers on agency theory. Two of these three papers are in the top ten list. The most cited is titled "Separation of Ownership and Control" and was published in the *Journal of Law and Economics*. The second one was also published in the same *Journal* and is titled "Agency Problems and Residual Claims."[4] The fourth most highly cited paper of Jensen is his AFA presidential address, titled "The Modern Industrial Revolution, Exit, and the Failure of Internal Control Systems." The presidential address has 15,750 citations. It is the second most cited presidential address. Myers gave the most cited address. His "Capital Structure Puzzle" has 17,240 citations (Myers, 1984). The third most successful address is Merton's address titled "A Simple Model of Capital Market Equilibrium with Incomplete Information" with 8,303 citations (Merton, 1987). The paper with Murphy on "Performance Pay and Top-Management Incentives" is next with 11,010 cites (Jensen and Murphy, 1990). Jensen has six papers with more than 10,000 citations. Very few financial economists have one paper that exceeds the 10,000 citation threshold. Using this metric, Jensen ranks third among financial economists, following Fama with 8 and Shleifer with 7.

[4] Oddly, the second Fama-Jensen paper is not on the Google Scholar homepage of Jensen and the number of citations in the homepage to the first paper is 41,003. The number when looking up the paper individually in Google Scholar is the number used in the table.

I now compare Jensen's lifetime citations to two benchmark groups. The first group is the group of scholars awarded the Nobel Prize for Economics from 2000 to 2024 and the second group consists of fellows of the AFA.[5] For both groups, I show the top ten scholars in Table 2. Panel A shows the top ten scholars awarded the Nobel prize from 2000 to 2024. Half of these scholars have a connection with finance. The top scholar on the list is Kahneman, who has 558,277 citations. He is followed by Fama who has 393,623 citations and Stiglitz who has 389,098. These scholars are the only ones out of 53 winers of the Nobel Prize for economics from 2000 to 2024 who have more lifetime citations than Jensen. Turning to the fellows of the AFA in Panel B, the top two fellows are Shleifer, with 432,029 citations, and Fama, with 393,623 citations. The third on the list is Jensen. The list has six winners of the Nobel prize. Overall, the average number of citations for an AFA fellow is 88,127 and the median is 62,277.

3. The birth of financial economics and Jensen's contributions

Markowitz's paper titled "Portfolio Selection" was published in the *Journal of Finance* in 1952. The paper came from Markowitz's dissertation in the economics department at the University of Chicago. His advisor was Marshak, but Friedman was on the committee in front of which Markowitz had to defend his dissertation. Friedman argued that the dissertation should not be accepted because portfolio theory was not economics. As Markowitz stated when he received his Nobel prize in economics, "at the time I defended my dissertation, portfolio theory was not part of Economics. But now it is." (Markowitz, 1990). Friedman was right that Markowitz's dissertation was a radical departure, but that departure involved the application of economics to understand finance. Markowitz's paper is widely credited as the start of modern finance or financial economics. As Rubinstein (2002) points out, the "Portfolio selection" paper seemed to come from nowhere.

Earlier authors had talked about the benefit of diversification. They generally took the view that diversification would eliminate risk. Markowitz (1952) showed that risk cannot be eliminated through

[5] The fellows of the AFA include all former presidents and since 2000 elected fellows.

diversification, but that for a given expected return, there is a minimum-variance portfolio. As a result, the risk of a security depends on the other securities held in the portfolio. This portfolio theory forms the basis for many important developments in financial economics over the next twenty years. Still in the 1950s, Tobin (1958) enriched the theory by adding a risk-free asset to the choice set and established his separation theorem according to which mean-variance investors with different attitudes towards risk will all hold the same portfolio of risky assets. In the 1960s, Sharpe (1964) was the first to publish a paper showing that in a mean-variance setting, the expected return of a security is linearly related to the contribution of the security to the variance of the return of the efficient portfolio that all mean-variance investors hold when there is a risk-free asset. Sharpe (1964) called his model the capital asset pricing model (CAPM). Sharpe already had the key results in his dissertation in 1961 at UCLA. Publication took time as the paper was first rejected by the *Journal of Financ*e but was published after the editor of the *Journal of Finance* changed. Lintner (1965) published a paper with similar asset pricing results five months after Sharpe. Mossin added his own version in 1966. Treynor had these key asset pricing results in a paper that he wrote in 1961, but that was published many years later. As editor, Jensen was always emphasizing that how a paper is written is almost as important as its results. As Rubinstein (2006) points out, the Sharpe paper is easy to read, but the Lintner paper is not. Sharpe received the Nobel Prize in 2002 with Markowitz and Miller.

For the CAPM to apply, it must be that existing information about a stock is incorporated in the price of the stock. Otherwise, the stock's expected return would also depend on the future incorporation of already available information in the stock price. A market in which available information is incorporated in security prices quickly is an efficient market. The theory of market efficiency became a critical pillar of financial economics in the 1960s through the work of Fama. Fama (1965) starts from the insight that market equilibrium in securities markets has implications for the statistical properties of security returns. Specifically, if unexpected returns arise only because of new unexpected information, as should be the case in an efficient market, prices should follow a random walk.[6] He found evidence supporting this random

[6] Subsequent developments in the field enriched this view by allowing for predictable changes in risk premia and interest rates.

walk theory. If stock prices follow a random walk, then investment funds should follow a random walk as well. In his study, he finds no evidence that the average fund outperforms the market.

A growing literature attempted to assess the performance of mutual funds in the 1960s, but it did not directly assess whether mutual funds performed better than the market taking into account risk. Jensen's dissertation introduced a direct measure of the performance of mutual funds relative to the market accounting for the risk of mutual funds. He wanted to test whether mutual funds could "predict security prices well enough to outperform a buy-the-market-and-hold policy" (Jensen, 1968). This measure is what is now called Jensen's alpha. It measures the average excess return of a fund over an investment in the risk-free asset and the market that has the same beta. Jensen (1968) estimates the risk-adjusted performance of more than one hundred funds. He finds no evidence of a positive alpha for mutual funds on average and no convincing evidence that there were individual funds that beat the market (one fund in his sample has a t-statistic greater than 2 for alpha).

Jensen's approach to evaluating the performance of mutual funds has been used repeatedly by scholars and practitioners. The literature has evolved in statistical sophistication as well as in the quality and comprehensiveness of the data used. One of the most highly cited papers is Carhart (1997) that examines the persistence of performance and has 24,135 citations. He concludes that evidence of persistence is almost completely explained by common factors in stock returns and investment expenses. In 2010, Fama and French published an analysis of mutual fund performance concluding that "few active funds produce benchmark-adjusted expected returns that cover their costs" (Fama and French, 2010).

Almost sixty years after Jensen's paper, his assessment of the performance of mutual funds mostly still holds. The evidence on the poor performance of active mutual funds has played a large role in the growth of indexed funds. As the history of indexed funds from Wigglesworth (2021) shows, the birth of indexed funds was closely tied to academic research. In particular, John McQuown, an MIT graduate, was hired by Wells Fargo to set up and manage a Management Sciences department that would use computers to solve problems and design products for the bank. At MIT, he had worked as an assistant for a professor who wanted to forecast stock prices. In 1968, Scholes recommended to McQuown that he start a passive

investment vehicle that would hold the market. McQuown wanted groundwork to be done to establish how such a fund would work. At one time, Jensen consulted for Wells Fargo and so did Sharpe, Markowitz, Miller, Treynor, Black, and Scholes. The bulk of the consulting work was done by Black and Scholes – after Jensen introduced Black to his classmate Scholes. Fama did not consult, but he did send David Booth to work for McQuown. In July 1971, the first-ever passively managed index-tracking fund was born. It was housed in a new unit within the bank called Wells Fargo Investment Advisors that eventually was bought by Barclays, which introduced the iShares brand of ETFs, and then was acquired by BlackRock in 2009. David Booth subsequently founded a highly successful asset management firm focused on building passive portfolios inspired by Fama's work and the work of his students, Dimensional Fund Advisors. By now, the assets under management by indexed mutual funds and ETFs are as large as those under management by active mutual funds and ETFs.

Jensen's alpha turns out to be useful not just to evaluate the performance of mutual funds but also to evaluate the performance of firms following corporate actions. For instance, starting in the 1980s, much research examines the long-run performance of firms following an acquisition. This research belongs to what is often called long-run event studies (Kothari and Warner, 2008) and uses, among other approaches, Jensen's alpha. An event study "measures the impact of a specific event on the value of a firm" (MacKinlay, 1997). The literature on short-run event studies is much larger than the literature on long-run event studies. The availability of stock return data through CRSP at the University of Chicago in the 1960s made possible much empirical research. It led to the development of the event study method. Fama, Fisher, Jensen, and Roll (1969) study the impact on firm value of stock splits. Though the paper was published in 1969 in the *International Economic Review*, it was accepted by the *Review* on May 31, 1966 – the *Review* had roughly a three-year publication lag. The idea for the paper originated from Lorie (as stated in footnote 1 of the paper), who was then director of CRSP, and suggested the idea as a test of the Miller and Modigliani (1961) proposition on the irrelevance of dividends (see Ball, 2017). That paper is the first statistical event study published in an academic journal. It shows how to conduct such a study estimating abnormal returns using the market model. The study uses monthly returns. Eventually, event studies started to use daily returns.

The literature using event studies is extremely large and includes papers with considerable influence both in financial economics and in the business world. It is likely the empirical method most used by financial economists. Searching for "event study" with "stock" under Google Scholar returns 143,000 results. As we will see, event studies play a critical role in the work of Jensen in the 1980s and 1990s.

4. Corporate finance and agency theory

In Section 3, I discussed the revolution in the study of investments and asset pricing that followed Markowitz (1952). During that time, the field of corporate finance lived through its own revolution. John Burr Williams, in his dissertation at Harvard written under the direction of Schumpeter, already incorporated key ideas that are at the heart of the revolution in corporate finance started in the 1950s. He published his dissertation in 1938 under the title of "The theory of Investment Value." In this book, Williams presented a theory of fundamental value based on the present value of dividends and a theory of leverage irrelevance. The Harvard University publishing company asked him to help pay the cost of producing the book because it had too many formulas. Williams never had a regular academic appointment.

Though Williams was an important pioneer, he left plenty of room for Miller and Modigliani to make their famous contribution (Modigliani and Miller, 1958; Miller and Modigliani, 1961). The fundamental difference between the work of Williams and the subsequent work of Modigliani and Miller is the role of equilibrium and arbitrage. With Modigliani and Miller (1958), a firm cannot create value if investors can manufacture on their own the same payoffs that the firm could manufacture through changes in financial policies. The concept of arbitrage led to the leverage irrelevance proposition. However, the leverage irrelevance proposition seemed a supremely elegant result hard to reconcile with reality. Most firms have some debt. To address this issue, Modigliani and Miller focused on the tax benefits of debt, but their work was incomplete as it suggested that firms would be extremely highly levered and they could not explain why firms are generally levered even when taxes are not an issue. Robichek and Myers (1965, 1966) formulate what is now known as the tradeoff theory of capital structure, where firms trade off the tax

benefits of debt against costs of financial distress.[7] In Robichek and Myers (1965), the disadvantage of debt is that a more levered firm might not be able to carry on its investment plans ("The firm may not undertake all planned investment."),[8] and such interruptions will be costly if the firm was planning to invest in positive NPV projects. With Modigliani and Miller as well as with Robichek and Myers (1965), there is no advantage of debt other than the tax advantage.

Jensen joined the faculty of the business school at the University of Rochester in 1967. The dean of the business school was Meckling. Meckling had been a graduate student in the economics department at the University of Chicago. He left without earning a Ph.D. and joined the Rand Corporation. At the Rand Corporation, he was a colleague of Alchian. Alchian eventually joined UCLA. Meckling and Alchian remained close over time. Alchian's work was not in finance. Instead, he was focused on how firms are organized and why they exist. Coase (1937) had developed a theory of firms where transactions are organized within firms when it is too expensive to carry out these transactions in markets. Alchian and his colleague Demsetz thought that Coase's approach was flawed. They published in 1972 a highly cited paper that addresses the issue. The paper's main claim is that firms exist when joint production or team production is involved. In the case of team production, team members have incentives to shirk. Monitoring is required to decrease the incidence of shirking. At the heart of Alchian and Demsetz (1972) is the view that individuals in teams pursue their own interests. This divergence of interests is costly and must be managed well for team production to be profitable.

Jensen and Meckling (1976) develop a theory of the firm and a theory of firm ownership based on the role of conflicts of interest. Their approach builds on Alchian and Demsetz (1972) but differs in fundamental ways. They do not focus on team production and conflicts of interest. Instead, they consider a firm that has an entrepreneur with limited resources who must raise capital to organize production. This setup creates agency costs in that the providers of capital cannot costlessly ensure that the entrepreneur will

[7] Myers, in an interview with Emery (Emery, 2013), says that the tradeoff theory "was first laid out in a 1966 article by Alex Robicheck and me."
[8] See Robichek and Myers (1965), p. 40.

act fully to their benefit. As a result, the providers of capital must monitor the entrepreneur, which imposes on them monitoring costs, and the entrepreneur may expend his own resources in the form of bonding costs to commit to some course of action favorable to the capital providers. However, neither monitoring nor bonding results in a situation where the entrepreneur does exactly what the capital providers would like him to do, so that there is a residual loss. In Jensen and Meckling (1976), the agency costs are the monitoring costs, the bonding costs, and the residual loss.

As Ross (1973) states in the first sentence of his paper, "The relationship of agency is one of the oldest and commonest codified modes of social interaction." Jensen and Meckling (1976) did not introduce agency to economics. Ross (1973) formulates the agency problem and investigates contracts that solve the principal's problem and are Pareto efficient. The fundamental difference between Jensen and Meckling (1976) and Ross (1973) is that they use agency theory to derive a positive theory of corporate ownership that is empirically testable while Ross (1973) constructs a normative theory of principal-agent contracts.

In Jensen and Meckling (1976), there are no agency costs of equity if the entrepreneur finances the firm with his own funds. As the entrepreneur issues equity to outside shareholders, his interests will in general differ to some extent from the interests of the outside shareholders. Specifically, the entrepreneur may indulge in perks that are valuable to him but costly to outside shareholders. More generally, the entrepreneur may derive private benefits from his role in the firm. If the entrepreneur consumes \$1 of private benefits which reduces the value of the firm's equity, he pays \$α of these private benefits if he owns a fraction α of the firm's shares. Hence, the lower the stake of the entrepreneur, the more outside shareholders pay for the private benefits of the entrepreneur. It follows that having the entrepreneur have a greater stake in the firm reduces the agency costs, but the stake of the entrepreneur is intrinsically limited by his wealth.

One way that the ownership of the firm can be organized so that the entrepreneur has a larger stake is by using debt. As a firm borrows, it has to issue less equity and the entrepreneur can own a larger fraction of the equity. It follows that debt is valuable in making it possible to reduce the magnitude of the agency costs of equity. However, there are agency costs associated with debt financing. Jensen and Meckling (1976) focus on what they call agency costs of asset substitution. These costs arise because shareholders

can increase the value of equity by increasing its volatility, but by doing so they decrease the value of the debt. When a firm becomes highly levered, the incentives to take riskier projects that benefit equity but hurt the debtholders increase. Jensen and Meckling (1976) were aware of drafts of the theory of Myers (1977) showing that shareholders may be unwilling to raise funding for positive net present value projects for the firm when the firm is highly levered because the funds raised will mainly benefit the debtholders. They viewed this well-known underinvestment problem as another agency costs of debt.

Jensen and Meckling (1976) is an extremely rich paper. Bolton (2025) shows how the paper influences the theoretical corporate finance and governance to this day and explains why. Whole literatures are devoted to investigating predictions from the paper. What is perhaps the most cited empirical corporate finance paper testing predictions from the paper is the study by Morck, Shleifer, and Vishny titled "Management Ownership and Market Valuation: An Empirical Analysis" with 12,700 citations (Morck, Shleifer, and Vishny, 1988). The success of Jensen and Meckling (1976) is in part explained by the literatures it spawned in corporate finance. However, the paper's influence is not limited to corporate finance. Lawyers debate "What Jensen and Meckling really said about the public company" (Cheffins, 2020) almost fifty years after publication of Jensen and Meckling (1976). The paper contributes to the theory of corporate finance, but it is broader in that it proposes a theory of what makes organizations efficient.

The important limitation of the *Theory of the Firm* is that it is a theory of the entrepreneurial firm. It does not address the issues of large firms where management has small stakes and where shareholders are atomistic. Jensen and Meckling (1976) were aware of this limitation and stated it explicitly (p. 356). They thought that the broad principles presented in the *Theory of the Firm* apply to the large diffuse corporation, but they explain that the issues "remain to be worked out in detail and will be included in a future paper." No such paper was written by Jensen and Meckling. However, Jensen collaborated with his advisor on three agency papers. The first one of these three papers addresses the issues of the large diffuse ownership corporation. That paper distinguishes carefully between the various tasks that must be carried out in corporations in the presence of agency costs. These tasks involve decision management and decision control. Management takes decisions but, because of agency costs, these decisions may not be the ones that

the outside shareholders would want. One solution to this problem is to concentrate decision rights and residual claims, which corresponds to the entrepreneurial firm studied in *Theory of the Firm*. This solution is limited by the wealth of the managers and forces them to bear more risk. Another solution is to separate ownership of residual claims from decision management. In this case, decision control to limit the extent to which decisions by management can hurt the interests of the outside shareholders becomes essential. The "apex of the decision control system" is the board of directors. Hence, for the corporation to work efficiently, the board has to do its job well. Another way to put it is that good governance is essential for corporations to be as productive as they can be.

5. Identifying and reducing agency costs

Theory of the Firm is a study in positive economics in that it explains why corporations exist and are organized as they are. It is also at the start of a new epoch in the governance literature. As Ma and Shleifer show, in the 40 years before Jensen and Meckling, the focus was on the ideas of Berle and Means about the importance of the law to solve the corporate governance problem that the providers of capital should be receiving a return on their investments (Shleifer and Vishny, 1997). Jensen and Meckling is the starting gun for a new epoch in corporate governance centered on the importance of private, market solutions, to the problem of governance.

Different firms can manage agency costs differently and some approaches are more efficient than others. This issue is especially important when it comes to large firms where management has a relatively small stake in the firm. In such firms, management may have more ability and interest to pursue courses of action detrimental to the interests of outside shareholders. It was, therefore, a natural evolution for Jensen to focus on identifying agency costs, finding obstacles to the reduction of agency costs, and highlighting approaches that control agency costs better. All of this work is grounded in empirical work from financial economists and the conclusions have a strong empirical basis. Jensen did not conduct this work isolated in academia. It would have been nonsensical from his perspective to identify issues that called for actions to control agency costs better and not communicate his findings to the business world to make the world a

better place with corporations made more efficient because they control agency costs better. Therefore, he set out to not only impact academia, but also the world outside academia.

5.1. The market for corporate control

The US had a spectacular period of economic growth after World War II. This period extended to the end of the 1960s. However, after that, the US started experiencing high inflation, lower growth, and a collapsing stock market. While US firms faced little competition from abroad for many years after World War II, the situation changed and many firms were not ready for that competition. Further, firms in some industries were protected by regulation which prevented price competition. Anti-trust actions had been extremely aggressive, so that firms could not grow through acquisitions within their industry and chose often to acquire outside their industry to grow. This led to inefficiencies. It was clear by the second half of the 1970s that the rules of the game had to change. The Carter administration started the deregulation process with the deregulation of air travel. The Reagan administration kept this process going and also changed the rules concerning anti-trust actions. A new playing field was put in place.

Many firms did not understand the new state of play or hoped that they could operate without changes. However, greater economic efficiency required a reallocation of assets across firms and actions that pushed management to increase shareholder wealth instead of wasting it. Assets can be reallocated across firms through mergers and acquisitions. The market for corporate control can increase discipline in management by threatening to take away control from existing management. The early 1980s saw the emergence of so-called "raiders", namely investors who saw sources of value in corporations that were being suppressed by existing management and were not afraid to attempt hostile takeovers of companies led by such management. Considerable opposition to hostile takeovers emerged quickly. State governments started helping firms in their states by adopting laws that gave them additional tools to defend themselves against hostile takeovers.

With this evolution, there was much interest in understanding better the implications of mergers and acquisitions. Jensen and Ruback (1983) defined the market for corporate control as one where managerial teams compete for control of corporate resources. In their view, shareholders have a passive role. This

definition was well-suited for the 1980s as acquisition battles were fights between managerial teams. The main question was then whether such fights ended up benefitting shareholders of the acquiring and acquired companies. Jensen had a toolkit that could be used to answer the question. It was the event study method introduced in Fama, Fisher, Jensen, and Roll (1969) discussed in Section 3. This toolkit was used extensively to measure the gains to shareholders of the target firm and shareholders of the acquiring firm in acquisitions. A striking result of this literature is how much target shareholders generally gain from acquisitions.

Jensen took to the pages of the *Harvard Business Review* to make the case that takeovers create value and pointed out that the existing evidence at the time suggested that target shareholders gained on average 30% from an acquisition (Jensen, 1984). This is a huge increase in the value of corporations, suggesting that the assets of acquired corporations were seriously misused unless the acquirers made an equivalent loss. Much of the literature has presented results that are less clear for acquirers. As I showed in a paper titled "Wealth destruction on a massive scale?," acquirers do at times make acquisitions that destroy substantial amounts of wealth for their shareholders (Moeller, Schlingemann, and Stulz, 2005). However, when considering both the acquirer and the target, the evidence is generally that acquisitions create wealth in total, so that potential losses incurred by the acquirer, if any, do not offset the gains to the target. Another way to put this in a language that became rapidly almost universal: acquisitions increase shareholder wealth. Not surprisingly, Jensen's views were controversial. The *Institutional Investor* magazine devoted an article to his views saying that Jensen "has come out in favor of corporate raiders and greenmailers to the point of developing an economic rationale for takeovers" and went on to call the *Journal of Financial Economics* Jensen's "fan-club newsletter" (VerMuelen, 1985).

In a world of efficient markets, increases in shareholder wealth result from increases in expected future cash flows generated by firms. As long as these increases in expected future cash flows do not result from redistribution of resources away from stakeholders, but instead grow the pie, the increase in shareholder wealth represents an increase in welfare. However, even at the time, some papers raised concerns about potential redistribution (Shleifer and Summers, 1988). Jensen made sense of an increasing amount of

evidence using the framework of agency theory. In so doing, he created a research agenda within academic finance and helped the worlds of business, law, and policy understand that takeovers have an important role to play in making the firms, and thereby the economy, more productive.

5.2. The agency costs of managerial discretion

In *Theory of the Firm*, the entrepreneur's incentives were rather narrowly defined. The owner-manager could choose more attractive perquisites and work less hard than outside shareholders would want him to. Such a view is rather limiting for managers of large corporations. These managers choose what projects the firm invests in. They may choose projects that are valuable to them but may be wealth-destroying to outside shareholders. In Fama and Jensen (1983), managers have decision rights, but there are control mechanisms. For management to destroy substantial wealth, the internal control mechanisms as well as the market for corporate control have to fail. Jensen wrote important papers where he identified situations where managers used their discretion for projects that destroyed wealth. These papers have in common situations where management commands large resources but does not have good uses for these resources.

Absent agency costs, management would return resources that cannot be used productively within the firm to shareholders, but with agency costs, such an outcome may not happen unless control mechanisms are put in place that limit management's discretion. Jensen's second most highly cited paper is "Agency Costs of Free Cash Flow, Corporate Finance, and Takeovers" (*Agency costs of free cash flow*, Jensen, 1986). The *Agency costs of free cash flow* paper is focused on poor acquisitions by oil companies in industries outside of oil and gas. These acquisitions arise because these companies benefit from extremely large cash flows, but do not have valuable investment opportunities in oil and gas. To make use of the funds they accumulated, these companies decided to make acquisitions outside of their industry and become conglomerates. In this case, had these companies had valuable investment opportunities, they would likely not have made such diversifying acquisitions and the problem Jensen focuses on would not have arisen. It follows that the importance of agency costs of managerial discretion depends on the growth opportunities the firm has as well as on the cash flows generated by assets in place. If the assets in place generate little

cash flow, management has little ability to make expensive investments of dubious quality. In that case, the firm would have to raise cash in capital markets and Easterbrook (1984) explains why being forced to resort to the capital markets for funding involves monitoring that can prevent the adoption of poor projects.

In the *Agency costs of free cash flow*, Jensen makes the case that firms that generate large cash flows but do not have good investment opportunities should pay out the cash flow to shareholders, thereby, as shown by DeAngelo, Kahle, and Skinner (2025) providing a foundation for theories of capital allocation, and should be forced to do so if they do not do so voluntarily. Doing so would prevent management from investing in poor projects. One way for managers to pay out cash flows is to use dividends or repurchases. Another way, which is likely more binding, is to increase the firm's leverage. With debt, management is forced to make payouts since otherwise the firm would default on debt. Consequently, the firm can issue debt and pay out the proceeds from the debt to shareholders in a leveraged recapitalization. In *Theory of the firm*, leverage makes it possible for the entrepreneur to issue less equity to outside shareholders, so that the agency costs of outside equity are less. In Jensen (1986), the advantage of leverage is that it constrains management by reducing the resources at its command. Again, this is a theory of debt that does not require a tax benefit of debt for leverage to exist. However, it is a theory where the benefit of debt can be increased through risk management (Stulz, 1990).

At the heart of Jensen (1986) is the problem created by the existence of large cash flows from assets in place in firms that do not have valuable growth opportunities internally. Instead of paying out these cash flows, managers may try to use them to invest in projects outside of the firm's industry where they have no particular expertise, so that these projects are quite likely to destroy shareholder wealth. In his AFA presidential address titled "The Modern Industrial Revolution, Exit, and the Failure of Control Systems" (*The Modern Industrial Revolution*, Jensen, 1993), Jensen embeds the free cash flow problem in a much broader setting, namely shocks to productivity from innovation and globalization. Globalization saw the emergence of low-cost producers in low wage countries. Though *The Modern Industrial Revolution* was written years before the China shock, Jensen was prescient about the effects of globalization on the US industry. He argues that the arrival of low-cost producers makes it necessary for high-cost producers to exit

an industry. He also reviews a vast array of technological innovations that changed the cost structure of industries and led to overcapacity. One example is the improvement in tires that extended considerably the life of tires, so that existing production capacity became too large. With both innovation and globalization, firms that were profitable and successful with their assets in place become high-cost producers that, absent the ability to change into low-cost producers, should shrink and exit. Jensen (1993) makes the case, however, that too many firms destroy shareholder wealth instead of shrinking and exiting. As evidence, he estimates the productivity of firms with their R&D and capital expenditures. He finds a large number of firms with valuation changes that do not reflect the vast amounts they spent on R&D and capital expenditures. For instance, over the 1980s, he finds that General Motors spent $100 billion on R&D and capital expenditures in excess of the change in its value.

Jensen (1993) argues that four control forces can resolve the problems caused by the divergence of interests between managers, who value continuation, and those of shareholders and society as a whole. These forces are capital markets, through the market for corporate control, the legal/political/ regulatory system, product and factor markets, and the internal control system headed by the board of directors. He thought that the role of capital markets had been somewhat neutered at the end of the 1980s, in part by the collapse of the investment bank Drexel Burnham Lambert in 1990, state antitakeover statutes, and various regulations. Part of the paper discusses the problems of board of directors and why they were not performing well. However, Jensen (1993) concludes that a new form of organization can replace public corporations to make possible the restructuring that is necessary to cope with technological and globalization shocks. This type of organization is the LBO organization. These organizations are structured so that they can only reap their rewards if they succeed in the necessary restructuring.

An important conclusion from *The Modern Industrial Revolution* is that to change the inefficiencies of internal control systems in corporations, we have to understand better how and why decisions are made. Jensen points out that there is much normative research about how to compute NPVs. However, such research does not help to understand why firms do not follow the NPV rule in their decisions.

All the Jensen papers we have discussed so far implicitly or explicitly assume that financial markets are efficient. In what appears to be his last paper published in a finance academic journal in 2005, he explores what he calls the agency costs of overvalued equity (Jensen, 2005). In that paper, equity can be misvalued. He argues that overvaluation gives incentives to management to engage in tactics that destroy value in the long run but help maintain the overvaluation. One such tactic is earnings manipulation to meet analyst expectations. He was also concerned that in some cases managers could have incentives to misrepresent a firm's prospects to increase their pay when the firm is not misvalued.

5.3. Politics, providing sharper incentives to managers, and LBOs

The papers discussed in the previous section are hugely influential. Together, they have more than 50,000 citations. Not a single of these papers crossed the desk of a referee. In 1990, Jensen and Kevin Murphy published in the *Journal of Political Economy* a paper titled "Performance Pay and Top-Management Incentives" (*Performance Pay*, Jensen and Murphy, 1990) that has an extremely large number of citations, but the citation total vastly understates the impact of the paper. The paper is closely linked to the analysis in *The Theory of the Firm*. In that paper, Jensen and Meckling focus on the entrepreneur's fractional ownership of equity and argue that higher fractional ownership results in better alignment between the interests of outside shareholders and the entrepreneur. In *Performance Pay*, Jensen and Murphy find that CEO wealth changes by $3.25 for every $1,000 change in shareholder wealth. They further show that most CEOs "hold trivial fractions of their firm's stock."

Providing sharper incentives for managers reduces agency costs. Hence, a carefully designed compensation plan that provides proper incentives, even if it involves large payments to the CEO if some outcomes arise, benefits outside shareholders because it makes it more likely that their shares will gain value. Given that paying executives well when they perform well is generally in the interest of shareholders, the important question to answer is why CEO wealth had such low sensitivity to firm value creation in the 1980s. The answer put forth by Jensen and Murphy is politics. The argument is that political forces make it costly for CEOs to have large pay even when they have earned it. Specifically, they conclude that "Public

disapproval of high rewards seems to have truncated the upper tail of the earnings distribution of corporate executives."

Politics can get in the way of actions that reduce agency costs and can directly worsen agency costs by providing cover for managers to take actions that reduce shareholder wealth. The impact of politics affects public firms in a way that it does not affect private firms. When it comes to compensation, the compensation of executives of private firms is typically not public, while the compensation of executives of public firms is subject to disclosure rules. More generally, private firms can take actions without the glare of public opinion. In the famous *Eclipse of the Public Corporation* and then in his presidential address, Jensen explained why public corporations are ill-suited to drastic restructuring to respond to technological and globalization shocks. As Chew (2021) has justly pointed out, Jensen did not predict in 1989 that public corporations would disappear. He explicitly stated that "The current trends do not imply that the public corporation has no future. The conventional twentieth-century model of corporate governance—dispersed public ownership, professional managers without substantial equity holdings, a board of directors dominated by management-appointed outsiders—remains a viable option in some areas of the economy, particularly for growth companies whose profitable investment opportunities exceed the cash they generate internally. (…) The public corporation is not suitable in industries where long-term growth is slow, where internally generated funds outstrip the opportunities to invest them profitably, or where downsizing is the most productive long-term strategy."

LBOs bring together themes from *Theory of the Firm* in that with their high leverage they increase the fractional stake of management and from Jensen and Murphy (1990) in that they give managers high incentives outside the glare of public opinion. Jensen's *Eclipse* paper predates the recent trends of a decrease in the number of listed firms and of firms often wanting to stay private.

6. Did Jensen recant?

Jensen retired from the Harvard Business School and became a professor emeritus in 2000. He then joined the Monitor Group where he worked until 2009. The *New York Times* obituary of Jensen asserts that

"Later in life, after Wall Street had been besieged by corporate stock option scandals and politicians derided excessive compensation packages, Professor Jensen acknowledged that his ideas had spiraled out of control." The book by Nicholas Lemann titled "Transaction Man" contributed much to this view (Lehman, 2019). Jensen's work after 2000 includes a highly cited paper titled "Value Maximization, Stakeholder Theory, and the Corporate Objective Function" (*Value Maximization*, Jensen, 2001). The paper puts forth the view of enlightened value maximization. Chew (2021) argues that we can see "these precepts at work everywhere in today's burgeoning 'ESG' movement." Besides that paper, Jensen spent considerable time and energy developing a research agenda centered on the concept of integrity. This research had minimal impact on the finance profession as, in total, the papers related to integrity have less than 500 citations. I discuss first the *Value Maximization* paper and then the integrity work, focusing on whether Jensen recanted after leaving Harvard.

6.1. Value maximization and stakeholder theory

Though there has been much attention paid to the purpose of corporations in recent years, especially following the Business Roundtable statement supporting a stakeholder model in 2019 and the fiftieth anniversary of Friedman's famous *New York Times* article stating that the objective of management should be to maximize profits, the debate on corporate purpose has been going on for decades.[9] In a post titled "Stakeholder governance and the eclipse of shareholder primacy," Martin Lipton, a lawyer credited with the invention of the poison pill, blames Friedman, Jensen, and Easterbrook for the shareholder-primacy narrative, argues that it constitutes an existential threat to society, and points out that he has "supported stakeholder governance for more than 50 years." (Lipton, 2024). Many observers in recent years have argued that the period of shareholder dominance has ended. In his paper, Jensen reiterated that value-increasing actions by managers are welfare enhancing in the absence of externalities. As he had discussed

[9] "A Friedman doctrine – The Social Responsibility of Business Is to Increase Its Profits," by Milton Friedman, *The New York Times*, September 13, 1970.

in earlier papers, he pointed out that value maximization offers an unambiguous metric of performance for a corporation that aligns with welfare for the economy. After all, generating higher profits means that more value is created out of inputs, which benefits the economy. Jensen goes on to argue that stakeholder objectives suffer from a fundamental problem, which is that they do not tell managers how to make tradeoffs. It is not possible to take actions that necessarily benefit all stakeholders, so decisions have to be made. Stakeholder models have no metric for success. One can draw a straight line through his earlier papers to the statement that "By failing to provide a definition of better, stakeholder theory effectively leaves managers and directors unaccountable for their stewardship of the firms resources." To make sure that his point would be understood, he also added that "Stakeholder theory plays into the hands of special interests that wish to use the resources of the corporations for their own ends."

After reiterating why value maximization is the best way to keep score for corporations, Jensen goes on and argues that maximizing value does entail paying attention to stakeholders. It is not clear why he felt the need to use the language of "enlightened value maximization" to argue that corporations have to pay attention to stakeholders to maximize value. In fact, his main statement of enlightened value maximization that "it is a basic principle of enlightened value maximization that *we cannot maximize the long-term market value of an organization if we ignore or mistreat any important constituency*" would be equally correct without the use of the word "enlightened." To succeed, a corporation must have a motivated workforce, it must have satisfied customers, and it must have good relationships with communities. The crux of the issue is how managers make decisions when the interests of the various stakeholders have to be traded off against each other. Value maximization provides a way to make these tradeoffs. Stakeholder theory does not.

There is a theme in the *Value Maximization* paper that would not have shown up with a much younger Jensen. It is related to some of the discussion in the *Agency Costs of Overvalued Equity*. Jensen argues for maximizing long-term value. The *Agency Costs of Overvalued Equity* paper allows for misvaluation. The arguments in *Value Maximization* are deeper. He does allow for misvaluation, but in part because the market does not have all the information that management has. He points out that "financial markets, although forward-looking, may not understand the full implications of a company's policies until they begin to show

up in cash flows over time" and that the "market is inevitably ignorant of many managerial actions and opportunities, at least in the short run. In those situations where the financial markets clearly do not have this private competitive information, directors and managers must resist the pressures of those markets while making every effort to communicate their expectations to investors." A younger Jensen might have worried that management arguing for a course of action because it knows more than the markets might just be entrenching itself. It is without doubt one of the toughest issues in evaluating the actions of management whether management does something that appears counterproductive for firm value because it has information it cannot credibly communicate that justifies the course of action or because it values that course of action for reasons unrelated to maximizing shareholder wealth.

6.2. Integrity: An incomplete project

Jensen was a superb communicator. This made him a great writer, a great teacher, and a great mentor. However, his last major research project was cut short before he could successfully apply these skills to the project. Starting in 2004, he embarked on a project that focused on what he viewed as a missing factor of production that financial economics had to take seriously and incorporate in its models. He called this missing factor integrity. Jensen stated that there is a "straight line from where I started back with Bill Meckling working on efficiency in organizations and people" to his integrity work (Walkling, 2011). The idea of the integrity work is that people and organizations have to behave in a way that they are in "the state of being whole, complete, unbroken, unimpaired, sound, in perfect condition". He thought that he needed to develop a new language to build his theory. However, this language constituted a formidable obstacle to interested readers. The first reaction of readers would be to think of integrity as a moral, normative value. This is not what he was after. He viewed his definition as reflecting a purely positive approach. To use his analogy, a car with a flat tire has no integrity. As a result, the car cannot perform its function as expected. This is not an issue of morality. One can observe the car and know that it will not perform well. One can do a study of cars that lack integrity and find the impact of the lack of integrity on production. He viewed his theory of integrity as a factor of production as testable. It is possible to investigate how productivity is

positively related to integrity. With his theory, "integrity is a necessary but not sufficient condition for long-run value maximization." He argued that "As integrity (whole and complete) declines, workability (capable of producing the desired effect or result) declines, and as workability declines, the opportunity for performance declines (however one chooses to define performance). Thus value maximization requires integrity."

Jensen thought he was onto something really important, perhaps as important as his agency theory. However, his theory might have been much more successful using a simpler and more intuitive language. To see this, it helps to relate his theory to work on corporate culture, organization capital, and sustainability. There is much discussion about the importance of corporate culture. The difficulty with work on corporate culture is how to measure corporate culture and how to assess its impact on firms' value. Jensen's integrity work offers a way to assess corporate culture and its impact on value. A culture that produces integrity contributes to value maximization. What does integrity mean in that context? A better term might be sustainability. What Jensen defines as integrity is well described as practices that are sustainable. Practices are sustainable when they can be repeated without damage to value. Sustainability has no normative or moral dimension. It is a quality that is observable. Using sustainability instead of integrity bypasses the normative baggage that comes with integrity. A corporation that has practices with a high degree of sustainability is one with high organization capital. It has developed these practices and maintains them.

The bottom line of Jensen's integrity work is that there are behaviors and practices that increase corporate value. It is important to understand what these practices are, to be able to measure them, and to be able to show that they affect corporate value. Such knowledge is important to managers and shareholders. It offers a scientific basis for theories of leadership and corporate culture.

7. Editor, teacher, and entrepreneur

A brilliant paper that nobody knows about is useless. It is not evidence of a superior mind. It is evidence of a wasted effort. Jensen throughout his life believed that good research changes the world. Research that draws the admiration of a small circle of scholars does not change the world. For research to change the

world, it has to be known broadly. This means that it has to be published in journals that draw attention, that it has to be taught, and that it has to be accessible. In his career, he worked hard on these three dimensions.

7.1 The Journal of Financial Economics

The development of financial economics faced an important obstacle in the 1960s and early 1970s, which is that existing journals were not receptive to financial economics contributions. The CAPM paper of Sharpe was rejected by the *Journal of Finance* before a new editorial team was more welcoming. The paper by Black and Scholes on the option formula only got published after the intercession of Fama and Miller at the *Journal of Political Economy* (Scholes, 1998). Some important papers were published in the *Journal of Busine*ss and not the *Journal of Finance*. Some of the most highly cited papers, such as Fama's paper on market efficiency and Jensen's paper on mutual fund performance, were published in the Papers and Proceedings of the American Finance Association rather than in the *Journal of Finance*. There was an enormous need of a new journal that would be welcoming to financial economics papers. Together with Fama and Merton, Jensen started the *Journal of Financial Economics (JFE)*. After securing a contract with what was then North-Holland to publish the journal, he led *JFE* from its first issue in 1974 until the early 1990s. Schwert replaced him formally as editor in 1997. He documents the history of *JFE* in Schwert (2021).

It is hard to understate the immense success of *JFE* in the 1970s and the 1980s. This success is obvious using the metric of citations, as the gap in the most commonly used metric, the citation impact factor, between *JFE* and the *Journal of Finance* is simply enormous in the 1980s as can be seen in Figure 1.[10] No finance journal has such a leadership position after 1990. Among interesting statistics, Alexander and Mabry (1994) show citations from 1987 to 1991 for the top fifty papers cited in the top four finance journals. None of the top ten papers were published in the *Journal of Finance*, but five of the top ten were published

[10] I am grateful to Bill Schwert for making available the data from 1977 to 2019 from Schwert (2021).

in the *JFE*. Nasseh and Saporoschenko (2009) rank the twenty-five papers that garner the most citations in the top four finance journals from 2003 to 2008. Ten papers on the list were published before 1990. Five of these papers are in *JFE*. None are in the *Journal of Finance*. Though the landscape of the top finance journals became more competitive after the late 1980s, *JFE* stayed among the top three finance journals competing for the top spot until today. Fittingly, in the last calendar year that Jensen was alive, Figure 1 shows that *JFE* had the highest citation impact factor among finance journals.

What explains the success of *JFE*? Some factors are simply the practices that *JFE* introduced in journals that are now common. Jensen wanted to compete on turnaround time and quality of service. He started the now common system of paying referees if their reports met the deadline set by the *Journal*. He viewed the time of referees to be valuable, so that they had to be compensated and authors had to pay submission fees. The goal of submission fees was in part to keep the volume of papers manageable so that authors could get the attention of knowledgeable referees. He introduced transparency concerning submissions and turnaround times. He published statistics for turnaround times in each issue. Jensen also had the view that articles had to be written so that they could have an impact. This meant that the quality of the writing was important. As editor, he would often carefully annotate articles to help improve the writing as well as the content. He had a hands-on approach. This approach helped grow the financial economics profession and its influence. He was vocal that what is important about a paper is what it teaches us about the world, not its mathematical or econometric sophistication. This view helped launch a huge literature on governance that had considerable impact on the business and legal world. To attract attention to this growing literature, he organized conferences in the 1980s. The first one, with papers published in 1983, may be the most important conference in the history of financial economics as it established the foundations for our understanding of the market for corporate control.

It may well be, however, that the most important attribute of Jensen as editor was his judgement about what he thought was important research and what was not. He was not shy about exercising that judgement. That judgement guided the evolution of the field. It affected what scholars worked on and how they wrote papers. Absent that judgement, the field would be different today. His views about what was important and

what was not were controversial. I doubt that there is a day in his life that he shied from controversy. However, his strong and well-defined views about what is important also led to the creation of a competitor to *JFE* and he should get credit for that development as well. The *Review of Financial Studies* was created explicitly as a journal that would not espouse the values of Jensen. Jensen never thought much of most theory work. He was vocal about that – at times using language not suitable for an academic journal. The birth of the *Review* was motivated by the need to create a journal that would be more welcoming to theory than *JFE* was.

7.2. Teacher

Rochester had the distinction of being a business school without a traditional management faculty – by choice. Jensen and Meckling introduced and developed a course at Rochester that eventually became called "Coordination, control, and the management of organizations" (CCMO). The course was first taught in 1974. Initially, it was mostly a price theory for managerial issues course, but rapidly evolved in a richer course focused on how to manage organizations from an agency theory perspective. Eventually, after Jensen moved from Rochester to Harvard part-time in 1985 and then full-time in 1989, the course was summarized as:

> "CCMO presents a new theory of organizations that draws on economics, finance, psychology, neuroscience, and human resource management. In developing our theory, we focus particularly on four interrelated areas: 1) the nature of human beings and their behavior; 2) compensation, career systems, and performance measurement; 3) task structure, organizational boundaries, and technology; and 4) governance, corporate finance, and organizational performance." (Jensen, Baker, Wruck and Baldwin, 1997).

Most faculty members anywhere could not possibly attract a crowd with a course that has such ambition and reach, or a course with material that includes the statement "We do not just teach; we are devoted to making a difference in peoples' lives." (Jensen, Baker, Wruck and Baldwin, 1997). However,

CCMO had an incredible success at the Harvard Business School as an elective. At its peak, in 1996, it had six sections and about 600 students. This means that most students at the Harvard Business School chose to take that course. Five faculty members were involved in the teaching and the course was then the product of a new interdisciplinary unit within the School, Organizations and Markets, led by Jensen. A large number of future leaders of American business were exposed to the course and were directly influenced by Jensen's thinking. In an interview for an article posted on the Alumni page of the Harvard Business School website, Jensen summarized the course by saying that "This course changes how people view the world in fundamental ways. It gives them a set of general principles for managing and for living."[11]

7.3. Entrepreneur

When I was a graduate student at MIT in the late 1970s, the way to learn about working papers was through a sheet distributed weekly that listed the new working papers the library received. We could then go and fish out papers from boxes where they were kept. If we were aware of a working paper that the library had not received, we could write to the author (which meant we put a letter in a stamped envelope) and hope that he would send the paper. My main recollection of doing so is writing to a well-known financial economist and never receiving a response. Things did not change much until the arrival of the internet. The internet offered a wonderful opportunity to make access to working papers easy and global. Jensen realized that early and created SSRN with another financial economist, Wayne Marr.

SSRN fundamentally changed how new ideas are diffused in financial economics. Now, anybody interested in working papers on a given topic can go to the internet and do a search. Most of the time, if the paper is unpublished, the search will lead to SSRN. Anybody anywhere on the globe with the ability to access the internet efficiently is in the same situation in terms of finding the most recent work on a given topic and in making papers accessible to the global community. With regular newsletters listing new

[11] "Organizations and Markets: A Challenging View of the World" by Susan Young, December 1, 1996. See https://www.alumni.hbs.edu/stories/Pages/story-bulletin.aspx?num=6173

working papers, academics and non-academics can easily know what the new working papers are. This evolution has reduced the geographic segmentation of the profession. An academic from Boston who visits a university in China ends up talking about working papers from her University that are a few weeks old with faculty members there who are knowledgeable about them. Forty years ago, the academic would have had the unique advantage of knowing about working papers at her university and being able to enlighten her hosts on these papers. This advantage is gone.

More than one million papers have been uploaded on SSRN. The papers on the site have been downloaded more than 200 million times. SSRN was sold in 2016 to Elsevier. While the site is a huge success, it has also not lived up to expectations. The famous saying of Peter Thiel about the internet ("We wanted flying cars, instead we got 140 characters.") applies here as well. The hope was that the site would create a way for researchers to communicate and comment on new papers, so that it could lead to a form of crowdsourced referee reports. It has not led to a decrease in the importance of journals. On the contrary, publication in the top three journals has become the only way for academics across the world to get tenure in the better departments or those departments that aspire to become better departments.

8. Conclusion

This paper attempts to evaluate Jensen's contribution to the field of financial economics and to American business. This contribution is large compared to the contribution of almost all financial economists. Few others have made contributions at that level. Of these few, almost all were rewarded by Nobel prizes. However, Jensen's story is a uniquely American story that should be celebrated as such. He was the son of a blue collar worker in Minnesota who, through hard work, intelligence, and the force of his personality, made an indelible mark on financial economics and American business. As Wooldridge (2024) wrote in Bloomberg, "Nobody can deny his extraordinary influence."

Throughout his academic career, he was insisting that research worth doing was research that had the objective of changing the world. For him, research was not an avocation. It was a matter of life and death. Research that impacted the world could make the world a better place. Few others in the field of financial

economics have had this burning ambition to impact the world through research. Even fewer have succeeded. Compared to other Nobel-caliber financial economists, he is in many ways unique in that much of his work did not go through the crucible of the refereeing process. I leave it to others to draw lessons from this. No doubt, there are financial economists who take a dim view of his papers because they typically have neither a conventional theory model nor conventional empirical work. However, none of these critiques have impacted how scholars and businesspeople think about the main issues of financial economics in the way that Jensen's work has. If he were still with us, he would argue vigorously with those critiques, trying to convince them that scholars should develop new ideas in financial economics to help make the world a better place rather than meet some tenure threshold. After all, he did give his dean an undated signed resignation letter that the dean could put a date on anytime.

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Table 1. Jensen's Top 10 Cited Papers Using Google Scholar Citations

Citations	Authors	Title	Year	Publication
138,678	MC Jensen, WH Meckling	Theory of the firm: Managerial behavior, agency costs and ownership structure	1976	*Journal of Financial Economics*
41,414	MC Jensen	Agency costs of free cash flow, corporate finance and takeovers	1986	*American Economic Review*
35,400*	EF Fama, MC Jensen	Separation of ownership and control	1983	*Journal of Law and Economics*
15,904	MC Jensen	The modern industrial revolution, exit, and the failure of internal control systems	1993	*Journal of Finance*
11,094	MC Jensen, KJ Murphy	Performance pay and top-management incentives	1990	*Journal of Political Economy*
10,371*	MC Jensen	Value maximization, stakeholder theory, and the corporate objective function	2001	*Journal of Applied Corporate Finance*
9,653	MC Jensen	The performance of mutual funds in the period 1945-1964	1968	*Journal of Finance*
9,349	EF Fama, MC Jensen	Agency problems and residual claims	1983	*Journal of Law and Economics*
9,155	EF Fama, L Fisher, MC Jensen, R Roll	The adjustment of stock prices to new information	1969	*International Economic Review*
8,296	MC Jensen, RS Ruback	The market for corporate control: The scientific evidence	1983	*Journal of Financial Economics*

Note: Citations marked with an asterisk (*) include multiple versions in Google Scholar.

Table 2. Comparison of Jensen's Lifetime Citations to Two Benchmark Groups

The table uses Google Scholar citations. For each scholar, the starting point is the scholar's Google Scholar page if available. The total count from the page is cleaned up to exclude papers the scholar did not author and papers that are missing are added. I use Semantic Scholar and Publish or Perish as additional sources to make adjustments to the scholar's Google Scholar page or as the starting point if the scholar does not have a Google Scholar page.

Panel A. Lifetime Citations of Michael C. Jensen

Citations	Name
327,099	Michael C. Jensen

Panel B. Economics Nobel Prize Laureates since 2000 (Top 10)

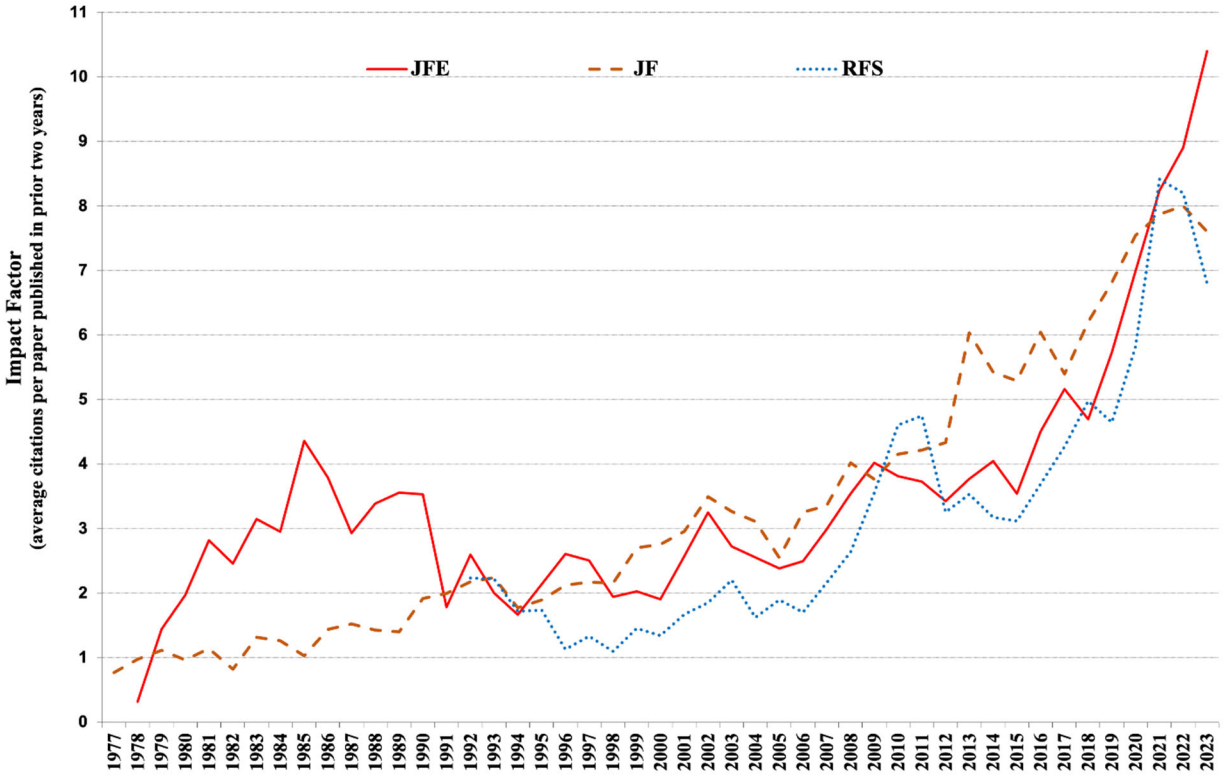
Citations	Name	Nobel Prize Year
558,277	Daniel Kahneman	2002
393,623	Eugene F. Fama	2013
389,098	Joseph E. Stiglitz	2001
289,315	Paul Krugman	2008
281,290	Oliver E. Williamson	2009
263,798	Elinor Ostrom	2009
263,275	James J. Heckman	2000
250,449	Daron Acemoglu	2024
215,411	Richard H. Thaler	2017
210,105	Clive W.J. Granger	2003

Panel C. AFA Presidents and Fellows (Top 10)

Citations	Name	AFA President or Fellow Election Year
432,029	Andrei Shleifer	2012
393,623	Eugene F. Fama	2001
327,099	Michael C. Jensen	1992
261,896	Kenneth Arrow	2003
217,387	Robert W. Vishny	2018
215,886	Richard Thaler	2009
208,251	Robert F. Engle	2005
198,666	Jean Tirole	2022
187,574	Kenneth R. French	2007
166,261	Paul A. Samuelson	2002

Figure 1. Impact Factor for Three Finance Journals: *JFE*, *JF*, and *RFS*

The data from 1977 to 2019 is from Schwert (2021). The data for 2020-2023 was obtained from the *Journal Citation Reports*.



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