

Capitalizing the “Measure of Our Ignorance”:

A Pragmatist Genealogy of R&D

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Academic Abstract

As the dust of the Second World War began to settle, that which began life in the U.S. as an experimental space in early twentieth century firms became a knowable object of intervention for economics and accountancy alike. Jumpstarted by the war, research and development, or R&D, was pulled into the experimental forays of a new generation of experts, including macroeconomic growth theorists. By explaining growth outputs in terms of R&D inputs, postwar macroeconomists failed to learn the lesson that was so hard won by accountants, namely: that it is the very uncertainty of outcomes which makes experimental inquiry so valuable to society and yet so untenable as capital.

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General Audience Abstract

Over the course of the twentieth century, American “research and development” evolved from an experimental space within a handful of early twentieth century firms, into “R&D”—a knowable object of intervention for experts and regulators alike. Tracing this shift through the lens of accounting history, this dissertation draws out the debates involving R&D accounting which ultimately settled on the treatment of R&D as an ordinary and necessary business expense. Federal laws and regulations, however, continue to treat R&D as if it were a capital investment like any other. The implications of treating R&D as capital are discussed.

Dedication

To the memory of my grandfather, Ebrahim. Having spent the first part of his life as a beloved educator and community leader in northern Iran, in a town just off the Caspian Sea, Ebrahim passed away in his home in Baltimore, Maryland in December 2017, surrounded by family. A man who taught me the importance of self-respect and the human dignity common to us all, my grandfather began calling me “Dr. Lotfi” long before I would earn the title. This dissertation is dedicated in his honor.

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Chapter 1

Accountancy and Indeterminacy

“[T]o set up a problem that does not grow out of an actual situation is to start on a course of dead work... Problems that are self-set are mere excuses for seeming to do something intellectual, something that has the semblance but not the substance of scientific activity.” John Dewey, *Logic: The Theory of Inquiry* (1929, 112)

1.1 Background

This study began, as many do, with a problem. How an initial sense of unease developed into the motivation for this study will be illustrated by a brief anecdote, one which will also serve to demonstrate, by example, the generalizability of experimental inquiry as articulated by the early twentieth century American philosopher John Dewey.

Several years earlier, before I recognized the precise form of the problem before me, I was returning home from a trip to the global South where I joined a long line of travelers from the North, many of whom grasped after the particular network of resources, strategies, and morals known as global public health. I had spent the Summer observing the ins and outs of one of the leading globalized clinical trials research centers in the world, hoping to learn what it was that made the center so successful. Almost immediately upon arrival, my focus began shifting to the expat community, how its display of wealth, however unextraordinary by standards back home, stood in almost blinding contrast to the poverty endured by locals. If you're not prepared for it, the sheer magnitude of this contrast can leave you catching your breath. It can also lead you to dismiss global health and drug research as a necessarily destructive, neocolonial,

commercialization of science. Indeed, many critics of global pharmaceutical research within the social sciences have made claims along these lines.¹

Absent from these critiques was an approach that problematized R&D. In the pharmaceutical industry, clinical trials research constitutes a large proportion of the R&D costs incurred in the process of drug discovery. These are the costs that drug companies point to when justifying the high prices they charge. Even when tiered to accommodate the world's poorer nations, these prices effectively keep life-saving drugs out of the hands of those most in need but least able to pay.² To draw upon “the pattern of inquiry” expounded by John Dewey, I found myself in a situation permeated with doubt. The standard critical methods of the social sciences had been applied to every aspect of valuation involved in the globalized drug discovery process, except for R&D. For whatever reason, an unspoken consensus seemed to quietly set the terms of the debate, namely, that the costs of R&D determine the price of the resulting drugs. Certainly, drug

¹ Arguments range from the exploitation of patients' “surplus health” as a source of pharmaceutical capital (Sunder Rajan 2002); to the excessive investment in developing “me too” or copycat drugs (Mirowski & Van Horn 2005); to the redistribution of biomedical risks in service of “neoliberal science” (Petryna & Kleinman 2005); to the de-politicized distribution of treatments as an emerging form of “therapeutic citizenship” (Nguyen 2005); to “the increasing embeddedness of medical research within market forces and corporate structures” (Craddock 2007); to government complicity in permitting the sale of dangerous drugs (McGoey 2007); to pharmaceutical firm's profit-boosting marketing strategies, such as ghost-writing articles and the auditing of physicians' prescribing practices (Sismondo 2009); and to bypassing regulatory barriers by outsourcing clinical trials to contract research organizations (Petryna 2009), to name a few. Summarizing these works, Kelly and Geissler (2011, 5) point out that it is the value of global pharmaceutical research that, “in these studies, indexes a scandal – the incommensurability between the calculative logic of profit-maximization and the needs of populations.” More to the point, these studies occlude a serious look at the “different value-producing activities and the hidden costs these entanglements entail” (Kelly & Geissler 2011, 8). In other words, they presuppose that the only value harvested from these trials is used to line the pockets of the universities who lend their medical researchers to run them, the drug firms that financed them, and possibly also the government agencies, such as the FDA, who are de facto captured by the companies they are supposed to regulate. With this assumption, it is held that nothing of value accrues to the participants who enroll in these studies—and in so doing, human subjects are theorized as devoid of all agency, presented as mere victims who are oppressed by forces that only social scientists can reveal.

² It should go without saying that having access to (literally) life-saving drugs does not of itself translate into a thriving life.

companies and medical researchers did not problematize R&D, but neither did their critics.³ There were twists here and there, such as whether or not the cost of advertising or the cost of capital should be factored into price.⁴ But few so much as mentioned these factors of pricing, and those that did went no further in unpacking what made R&D not only valuable but knowable, as well.

This dissertation, however, is not about drug pricing or even of pharmaceutical R&D valuation; their importance lies in the contribution they made to an inescapable sense of doubt, a gnawing sense that something was off. Dewey, in step with fellow pragmatists William James and Charles Sanders Peirce before him, described this experience as a peculiar quality that pervades a situation and makes it indeterminate. This sense of dissonance, of being disturbed out of routine habits of thinking and acting, “is not just uncertainty at large,” wrote Dewey, but “a unique doubtfulness which makes that situation to be just and only the situation it is. It is this unique quality that not only evokes the particular inquiry engaged in but that exercises control over its special procedures.”⁵ The more I examined the indeterminate situation before me—one marked

³ The shortcomings of the standard social science critiques of globalized clinics trials are the same as those made by other critical theorists, as described by Bruno Latour in his important essay, “Why Has Critique Run Out of Steam? From Matters of Fact to Matters of Concern” (*Critical Inquiry*, Winter 2004)

⁴ In a widely cited study funded by the pharmaceutical industry, economists at Tuft University estimated the average, pre-tax cost of bringing a new drug to market in 2000 was \$802 million (DiMasi et al 2003). As media publicized the “\$800 million pill,” other calculations were quickly pulled together: internal industry estimates doubled the Tufts’ figure; Lehman Brothers Healthcare estimated the expense of taking a drug through Phase I, II and III of clinical testing cost the industry a total of \$169 million; a 1999 report by the Congressional Research Service estimated that for each of the three phases firms incurred on average \$10 million, \$20 million, and \$45 million, respectively (Brody 2007). With the highest estimate to date reporting \$1.8 billion, the range in these and other estimates have been wide and the differences between them in part a result of varying computational techniques. For example, some economists have argued the 11% cost of capital used in the Tufts estimate was a few percentage points too high; others argued it should not be included at all.

⁵ *Logic: The Theory of Inquiry*, 109

by a consensus towards R&D held by the pharmaceutical industry and its critics, alike—the more R&D itself came to the fore.

Pharmaceuticals constitute one site in which the accomplishment, measurement, and valuation of R&D come into play. There are many others. Among these are the pricing of computer software; the planning of municipal and state economic development; the terms of international trade deals; the determination of rate bases charged by public utilities to their customers; the incentive schemes built into the federal tax code; the harmonizing of academic activity with military needs; and the demonstration of patent validity. To consider how all of this is implicated in American life, take the example of a typical consumer and the cost of the prescriptions they need filled each month, the energy they rely on to heat their homes in winter, or the antivirus software they need to safeguard their files. Consider, too, a typical research-intensive company which uses the popular R&D tax credit to reduce its tax bill down to zero, or sets up shop in a commercial research park in response to the cajoling of local politicians.

All of this pointed to the question: how did the value of R&D come to be taken as given? With this question, I passed from what Dewey called “the antecedent conditions of inquiry,” into the first stage: the “institution of a problem.” “To see that a situation requires inquiry,” wrote Dewey, “is the initial step in inquiry.”⁶ In this first step, “the indeterminate situation becomes problematic in the very process of being subjected to inquiry.”⁷ Yet, as I soon found out, a problem instituted without, “in the very terms of its statement, reference to a possible solution,”⁸ is meaningless. Thus, even as the single

⁶ *Logic: The Theory of Inquiry*, 111

⁷ *Logic: The Theory of Inquiry*, 111

⁸ *Logic: The Theory of Inquiry*, 112

question that opens this paragraph multiplied into a triumvirate—What is R&D? For whom? And for what purpose?—I was no closer towards Dewey’s third stage of inquiry, the determination of a problem-solution, than I was before. This, because by posing the problem in such broad terms, my inquiry became overwhelmed by a constellation of actors, ideas, contexts, equipment, norms, and interactions that was simply too expansive to be tractable.

The sheer volume of paths down which I could explore possible solutions proved to be far larger than any single dissertation could contain. There was the economics of R&D; the taxation, incentivization, and regulation of R&D; the relation of R&D to price movements in and financial analyses of stock markets; the transformation of military procurement brought about by R&D contracts; the adaptation of organization and management to the vicissitudes of R&D; and other candidates for historical study. My research question(s), as posed, failed to be stated in such a way as to control the bounds of inquiry. To survey all these sites of study would only produce a very broad and superficial overview of how “research and development” became blackboxed as an end in and of itself.

For the sake of presenting a richer, more meaningful argument to round out my inquiry, the chapters that follow are therefore focused primarily on drawing out the history of accounting for R&D. The role of military procurement, taxation, economists, the stock market, etc. appears scattered throughout this history, but for reasons of space they cannot receive the full attention they are due. (Further discussion on the decision to pursue a more selective study of R&D with accountancy at its core is found in section II immediately below.) Thus, with respect to accountancy, this dissertation is addressed to the following question: Given that the value of R&D is presently taken for granted, what

were the practical or theoretical problems—i.e., Dewey’s “indeterminate situations”—out of which that value emerged?

1.2. Why Accountancy?

Aside from the constraints of space, there are at least three reasons for privileging the history of accountancy in this project. These range from (1) addressing the lack of attention given R&D by accounting historians, (2) reviving and extending studies of industrial research completed by historians of science and technology, and (3) experimenting with pragmatist genealogy as a historical method of critique well-suited to the study of accounting and R&D, both.

First, questions concerning R&D are rarely, if ever, asked by accounting academics who have otherwise made important strides since the early 1970s in developing a body of literature on the history of American accountancy.⁹ On very rare occasions, historians of different stripes have aided them in this work,¹⁰ but efforts to meet the need for accounting history has fallen largely on the backs of accounting educators. These educators work predominantly within the institutional context of US business schools, where their pleas for greater historical perspective in the business and accounting

⁹ Notable monographs on American accounting history include: Gary Previts and Barbara Merino, *A History of Accounting in America*; John L. Carey, *The Rise of the Accounting Profession*; Stephen A. Zeff, *Henry Rand Hatfield: Humanist, Scholar, and Accounting Educator*; David Allen and Kathleen McDermott, *Accounting for Success: A History of Price Waterhouse in America, 1890-1990*; O. Ten Have *The History of Accountancy*; M. Chatfield, *A History of Accounting Thought*; M. Chatfield and R. Vangermeersch *The History of Accounting: An International Encyclopaedia*; Previts, Walton and Wolonizer, *A Global History of Accounting, Financial Reporting and Public Policy*; Boyns and Edwards, *A History of Management Accounting*; Garner and Hughes *Readings on Accounting Development*; Edwards *The History of Accounting, Critical Perspectives on Business and Management*; Fleischmann *Accounting History*; Johnson and Kaplan, *Relevance Lost*; Dale Buckmaster, *Development of the Income Smoothing Literature, 1893-1998: A Focus on the United States*

¹⁰ Notable examples include business historian Paul Miranti and historian of science Theodore M. Porter.

curricula are rarely heard. Without demands placed upon them to provide historical insights into present-day problems, these scholars have been more inwardly focused, producing biographies of individuals, case studies of firms, and institutional studies of professional associations.¹¹

Beginning in the early 1990s, calls for a “new accounting history” were heard from outside the US and from outside accountancy proper.¹² By then, reform movements were already underway in various subfields of History, and the time had come for accounting history to fully confront its own legacy. Already in the 1980s, sociologically-trained critics had begun pushing back against the positivist ideals maintained and exported by mainstream American accountancy since the late 1960s and 1970s.¹³ What ensued was a multidisciplinary, historiographical critique of traditional accounting history, which stood accused of composing heroic narratives of “great men”; engaging in whig history that affirmed the status quo; failing to make use of theory and post-modern theory in particular; maintaining a false sense of objectivity; uncritically accepting such data as company financial records; and the related charge of making a fetish of the archive. In 2005, a retrospective by two active participants in these debates referred to the “roaring nineties” as the decade when “accounting history came of age,” suggesting that, by the early years of the new millennium, critical and traditional accounting historians had worked through some, though not all, of their methodological differences.¹⁴

¹¹ Thomas N. Tyson and Richard K. Fleischman, “Introduction” *Accounting, Business & Financial History*, Vol. 10, No. 2 (July 2000): 91-95.

¹² Peter Miller, Trevor Hopper and Richard Laughlin, “The New Accounting History: An Introduction” *Accounting, Organizations, & Society*, Vol. 16, No. 5/6 (1991): 395-504.

¹³ C. Richard Baker, “A Genealogical History of Positivist and Critical Accounting Research,” *Accounting History*, Vol. 16, No. 2 (2011): 2-7-221.

¹⁴ Robert K. Fleischman and Vaughan S. Radcliffe, “The Roaring Nineties: Accounting History Comes of Age” *Accounting Historians Journal*, Vol. 32, No. 1 (June 2005): 61-109; Garry D. Carnegie, “The

Until more recently, critical accounting history was predominantly an exercise in proving the universal applicability of Marxist labor theory or Foucault's disciplinary regimes of knowledge/power.¹⁵ The strength of these approaches has been in their effect. Where before questions of methodology remained largely unasked and unanswered, today they remain at the fore of the discipline. Furthermore, as a result of the pushback from critical theorists, the field has witnessed a dramatic expansion in its objects of study. No longer are questions of gender, class, and race perceived to be as uncouth as they once were. These issues help focus the field on contemporary problems that link accounting to broader society, while forcing it to defend its methodological leanings. For both these reasons, critical accounting historians deserve the unqualified thanks of their more traditional brethren.¹⁶

At the same time, traditional histories do serve a useful purpose. Accountancy's past is a vast reservoir of heterogeneous practices that have evolved in different times and

Roaring Nineties': A Comment on the State of Accounting History in the United States," *Accounting Historians Journal*, Vol. 33, No.1 (June 2006): 203-210; Stephen P. Walker, "Current Trends in Accounting History" *The Irish Accounting Review*, Vol. 13, Special Issue (2006): 107-121.

¹⁵ For summaries of these approaches, see S.C. Lodh and M.J.R. Gaffikin, "Critical Studies in Accounting Research, Rationalist and Habermas: A Methodological Reflection," *Critical Perspectives on Accounting*, Vol. 8, No. 5 (1997): 433-474; Garry D. Carnegie and Christopher J. Napier, "Accounting's Past, Present and Future: The Unifying Power of History," *Accounting, Auditing & Accountability Journal*, Vol. 25, No. 2 (2012): 328-369. For a reflection on the progress within this field, see C.J. Napier, "Accounts of Change: 30 Years of Historical Accounting Research," *Accounting, Organizations and Society*, Vol. 32, No. 4/5 (2006): 445-507. For the most recent discussion of the state of accounting history, see: Greg Patmore and Mark Westcott, "Special Issue: Interdisciplinary Historical Studies" *Accounting History Review*, Vol. 30, No. 1 (2020): 1-6; Cheryl S. McWatters, "Historians But Not Necessarily So," *Accounting Historians Journal*, Vol. 27, No. 3 (2017): 219-221; Cheryl S. McWatters, "Historical Accounts, Conversations and Contexts," *Accounting History Review*, Vol. 24, No. 1 (2014): 105; and Michael Gaffikin, "What is (Accounting) History?" *Accounting History*, Vol. 16, No. 3 (2011): 235-251.

¹⁶ Contrast this with a defense of traditional accounting history made by T. Tyson and D. Oldroyd in 2007: "We do not believe accounting historians have the authority or are they well situated 'to address the problems in practice and theory today.' That undertaking is better left to social activists, contemporary critics, and accounting regulators. Rather, historians should continue to examine, illuminate, and interpret the past"(188). "Straw Men and Old Saws: An Evidence-Based Response to Sy and Tinker's Critique of Accounting History," *Accounting Historians Journal*, Vol. 34, No. 1 (2007): 173-184. A decade later, Tyson and Oldroyd seem to have changed their stance very little; see "The Debate Between Postmodernism and Historiography: An Accounting Historian's Manifesto," *Accounting History*, Vol. 22, No. 1 (2017): 29-43.

places through the theoretical and practical contributions of engineering, auditing, economics, management, organization, and law—all of which plainly possess their own complex, evolutionary histories. For scholars untrained in accounting, such as myself, it is absolutely crucial that secondary source material continues to be produced that is less-heavily inflected by the latest fashions in critical theory. Otherwise, the intimidating obscurity of accounting’s past will continue to impede the multidisciplinary future that accounting historians have been so adamantly striving towards as of late.¹⁷

Although “antiquarians” and “activists” are no longer at each other’s throats, I will suggest an alternative form of critique below that embraces the former’s commitment to in-depth data collection and the latter’s orientation towards problems that have endured into the present.¹⁸ In appreciation of these scholars, in the hope of broadening exposure to their work, and with the intention of furthering the development of their profession, this dissertation will demonstrate and recommend a different approach to US accounting history. Rather than endeavor a description of events as they truly happened or people as they truly were, and rather than apply postmodern theory as a lens through which the past is made knowable, the pragmatist genealogical approach that will be deployed here suggests organizing historical inquiry as a means of grappling with contemporary problematic situations.¹⁹

Two more reasons substantiate the decision to narrow an inquiry of R&D to accounting. Each of these last two will require considerably more discussion than the first.

¹⁷ List examples of all of the editorials pleading for multidisciplinary.

¹⁸ I might also add that without the details found in traditional historical accounts, the obstacles facing an STS scholar untrained in accounting yet venturing into this area of American history would have been insurmountable. This accords with Norman B. Macintosh, “Effective’ Genealogical History: Possibilities for Critical Accounting History Research,” *Accounting Historians Journal*, 36, no. 1 (June 2009):1-27

¹⁹ This approach will be discussed more fully in section III.

Starting with the second reason: building upon and expanding the scope of historical work on industrial research and development. This was once a lively area of study and debate among historians, but enthusiasm seems to have waned over the last few decades.²⁰ In the 1980s, especially, historians of technology seemed united in their efforts to examine the roles of scientists, engineers, universities, and government in shaping R&D. Most if not all of this body of work would easily qualify under the designation of “traditional” history, as the critical accounting historians have used the term, but for the fact that much of this work embraced the use of theory in its analyses. For example, it is hard to overlook the influence of business historian Alfred DuPont Chandler, whose organizational theories are heavily inflected by the structural-functionalism of Talcott Parsons, a fellow Harvard academic “who affected Chandler’s thinking more than any other scholar.”²¹ Other sources of theoretical equipment informing the histories of industrial research include David F. Noble’s Marxist analysis of the control of monopoly capital over the process of R&D, Thomas P. Hughes’ systems analytic thinking translated

²⁰ Exceptions include Karl Grandin, Nina Wormbs & Sven Widmalm (eds) *The Science-Industry Nexus: History, Policy, Implications* (USA: Science History Publications, 2004); Ann Johnson, “What If We Wrote the History of Science from the Perspective of Applied Science?” *Historical Studies in the Natural Sciences*, Vol. 38, No. 4 (Fall 2008): 610-620; David Edgerton, “Innovation, Technology, or History: What is the Historiography of Technology About?” *Technology & Culture*, Vol. 51, No. 3 (July 2010): 680-697; David Philip Miller, “The Political Economy of Discovery Stories: The Case of Dr Irving Langmuir and General Electric” *Annals of Science*, Vol. 68, No. 4 (January 2011): 27-60; Sean F. Johnston, “Making the Invisible Engineer Visible: DuPont and the Recognition of Nuclear Expertise,” *Technology & Culture*, Vol. 52, No. 3 (July 2011): 548-73.

²¹ Thomas K. McCraw, “Schumpeter’s Business Cycles as Business History,” *Business History Review*, Vol. 80, No. 2 (Summer 2006), 259. Also see: Thomas K. McCraw, “The Intellectual Odyssey of Alfred D. Chandler, Jr.” in McCraw, ed., *The Essential Alfred Chandler: Essays Toward a Historical Theory of Big Business* (Boston, 1988); and Richard R. John Jr., “Elaborations, Revisions, Dissents: Alfred D. Chandler’s The Visible Hand after Twenty Years,” *Business History Review*, Vol. 71 (Summer 1997). By 1995, historian of technology David A. Hounshell could make the observation that “the new scholarship on science-based industries and industrial research added markedly to both fields [i.e., business history and history of technology]” (1995, 211). “Hughesian History of Technology and Chandlerian Business History: Parallels, Departures, and Critics,” *History and Technology*, Vol. 12 (1995): 205-224. For a more demure discussion about the need for business history to move away from Chandler’s omnipresence, see the 2017 Business History Conference Presidential Address of Walter A. Friedman, “Recent Trends in Business History Research: Capitalism, Democracy, and Innovation.”

into a theory of technological momentum,²² and Edward T. Layton's notion of technological knowledge developed against the elitist fantasies of 1950s history of science.²³

Through engagement with these theories, the explosion of historical studies in the 1980s focused on telling the histories of individual R&D-intensive companies; on identifying technological systems of which R&D companies are one part; on theorizing the changing structure of these companies; on detailing the lives of entrepreneurs, inventors, and managers hired therein; on critiquing the commercial motivation involved; or on the distortions caused by government involvement.²⁴ Without question, these

²² In 1985, Hughes received an award from a committee of the Society for the History of Technology, whose chairman declared that, thanks to Hughes, "Historians now think in terms of 'technological systems,' 'national style,' 'reverse salient,' and 'technological momentum'" ("The Leonardo Da Vinci Medal," *Technology & Culture*, Vol. 27, No.3, July 1986, 570). At the time, Hughes insisted that, "Despite my having written a book that some associate with a system approach to history, I have never studied formal systems theory." As heavily-deterministic as system theory is in the abstract, Hughes sought to distance himself from such determinism. Curiously, though, immediately after making this statement, Hughes proceeded to explain how he *did* formally study systems theory and that "it made a lasting impression...because I saw that they incorporated systematic interconnections between the technical and the economic" (1986, 572). Hounshell concludes that "Hughes ultimately settles on a kind of soft determinism" (1995, 215).

²³ Edwin Layton, "Mirror-Image Twins: The Communities of Science and Technology in 19th-Century America" *Technology & Culture*, Vol. 12, No. 4 (October 1971): 562-580; Edwin T. Layton, Jr. "Technology as Knowledge," *Technology & Culture*, Vol. 15, No. 1 (January 1974): 31-41; and Edwin T. Layton, Jr. "American Ideologies of Science and Engineering," *Technology & Culture*, Vol. 17, No. 4 (October 1976): 688-701. The influence of Layton's work is palpable in efforts develop taxonomies that distinguish between various types of science and engineering knowledge. See: Ronald Kline "Construing 'Technology' as 'Applied Science': Public Rhetoric of Scientists and Engineers in the United States, 1880-1945," *ISIS*, Vol. 86 (1995): 194-221; and Nathan Rosenberg and W. Edward Steinmuller "Engineering Knowledge," *Industrial and Corporate Change*, Vol. 2, No. 5 (2013): 1129-1157. Aside from Layton's contributions, other work in this general category predating the 1980s include: Kendall Birr *Pioneering in Industrial Research: The Story of the General Electric Research Laboratory* (1957); Alfred Chandler *Strategy and Structure* (1962); Thomas Hughes *Elmer Sperry: Inventor and Engineer* (1971); Alfred Chandler & Stephen Salisbury *Pierre S. Du Pont and the Making of the Modern Corporation* (1971); Derek de Solla Price, "On the Historiographic Revolution in the History of Technology: Commentary on the Papers by Multhauf, Ferguson, and Layton," *Technology & Culture*, Vol. 15, No. 1 (January 1974): 42-48; Spencer R. Weart, "The Rise of 'Prostituted' Physics" *Nature*, Vol. 262, No. 5563 (July 1, 1976): 13-17;; Robert E. Kohler, "The Interaction of Science and Technology in the Industrial Age," *Technology & Culture*, Vol. 17, No. 4 (October 1976): 621-623; Alfred Chandler *The Visible Hand* (1977).

²⁴ George R. Wise, "A New Role for Professional Scientists in Industry: Industrial Research at General Electric, 1900-1916" *Technology & Culture*, Vol. 21, No. 3 (July 1980): 408-429; Thomas Hughes *Networks of Power: Electrification in Western Society* (1983) Graham Taylor & Patricia Sudnick, *Du Pont and the International*

perspectives all draw out important aspects of the R&D story, and the details they provide of the inner workings of early R&D pioneers clarifies important empirical questions, such as how were these experimental laboratories organized, who were they staffed by, and how did they manage open-ended inquiry under the constraints of time-sensitive corporate environments. However, common to much of this scholarship is the intentional or unintentional privileging of the “R” over the “D.”²⁵

In assessing the wave of corporate R&D histories that were published in the 1980s, Michael Aaron Dennis observed how, “the sociological view of the scientist impact in these studies denies that corporate researchers were genuine scientists. ... [These histories] define academic disciplines in largely intellectual terms, emphasizing abstract theory over practice.”²⁶ Others have reached similar conclusions. For example, in “Who Is the Industrial Scientist?” Steven Shapin examined the still-unchallenged sociological narrative of the 1960s which portrayed industrial scientists as unwilling victims of capitalism.²⁷ Observing two major “problems with the empirical adequacy of the academic story,” Shapin concluded that “certain obstinate facts remain.”²⁸

Chemical Industry (1984); Norman B. Wilkinson, *Lammont du Pont and the American Explosives Industry, 1850-1884* (1984); George R. Wise (1985); Leonard Reich (1985); John Kenley Smith, Jr. “R&D Pioneers Conference—Hagley Museum and Library, Wilmington, Delaware, October 7, 1985” *Technology & Culture*, Vol. 28, No. 2 (April 1987): 340-342; David Hounshell & John Kenley Smith (1988); Thomas Hughes *American Genesis: A Century of Invention and Technological Enthusiasm* (1989); John Kenley Smith, Jr. “The Scientific Tradition in American Industrial Research,” *Technology & Culture*, Vol. 32, No. 1 (January 1990): 121-131; John M. Staudenmaier, “Recent Trends in the History of Technology,” *The American Historical Review*, Vol. 95, No. 3 (June 1990): 715-725.

²⁵ Recently, historian David Edgerton has ironically (and astutely) suggested that, had the category been called “development and research,” that might “have helped avoid the standard narrative focusing on ‘research’ and...the tendency to see the pre-history of R&D only in research and not in the separate and larger realm of design and development. [A] new account is needed of the history and historiography of invention and innovation”(2017, 23-4).

²⁶ Michael Aaron Dennis. “Accounting for Research: New Histories of Corporate Laboratories and the Social History of American Science”(1987, 492).

²⁷ As is evident by the work of Paul Forman, for example.

²⁸ Shapin 2008, 68

First, the majority of American scientists did not work in universities by the middle of the twentieth century. Historian of technology Ann Johnson agreed: “Looking over the historiography of science in the twentieth century, one might think that academic scientists overwhelmingly dwarf their industrial siblings in number. ... But statistics show that most scientists work in the private sector, both today and in the 1960s.”²⁹ Second, since the start of the twentieth century on through today, most American universities have been “under-resourced; most had a primary commitment to teaching;... And it is good to remember the restrictions on free research problem-choice that were, and remain, endemic in even the best universities.”³⁰ Another historian of technology, David Edgerton, builds on this point: “Rare have been the historians and sociologists of science who have pointed to the limitations of the academic-centered view; rarer still are those who note that the implied asymmetry is misplaced.”³¹ It is not that a history of Development is needed to complement a history of Research in industry. Instead, the work that remains to be done is a historical study of R&D, of the two taken together.

In some firms, it is true, there existed research labs, departments, or divisions isolated from development labs, departments, or divisions. Sometimes these organizational forms were known by other names, such as technical labs, engineering departments, pioneering research labs, or experimental stations. But it is a mistake to read too much into the names given and to conclude that R&D *per se* cannot be subjected to

²⁹ Johnson 2008, 611.

³⁰ This “autonomy-restricting states of affairs are not at all uncommon in academia, nor are they inconsistent with saying that universities have an in-principle association with the ideal of research freedom.”(Shapin 2008, 67-8).

³¹ Edgerton 2012, 326

historical analysis because to do so would represent, at best, a kind of sully of the truth of research and the truth of development. In fact, these organizational forms were not fixed once and for all but malleable and subject to change. Once created, they could be relocated, renamed, funded differently, assigned new priorities, or even have their staff shuffled around. This came through in the 700+ pages of *Science and Corporate Strategy: Du Pont R&D, 1902-1980* (1988), a celebrated company biography by David Hounshell and John K. Smith. In fact, Du Pont's experimental approach to the organization of R&D was precisely one of the core arguments in their text. However, the malleability of R&D seems not to have inspired Hounshell, Smith, or any other authors of the 1980s to question their assumption of a foundational distinction between science and engineering.

In *The Making of American Industrial Research: Science and Business at GE and Bell, 1876-1926* (1985), Leonard S. Reich concluded that “science and technology” became united in the activities of R&D labs, forming “a complex, integrated activity that defied the usual characterizations. ... Industrial research was in this sense more in tune with the American conception of science as ‘useful knowledge’ than with the ‘pure science’ ideal or with academic science generally.”³² In praising this aspect of the text, Dennis (1989) noted how Reich had argued “persuasively that trying to distinguish between science and technology in the new labs is an impossible task.”³³ But, again, neither Reich, Hounshell, Smith, nor the other authors of the 1980s challenged the idea that had been so clearly expounded by George R. Wise at the start of the decade: “While industry is a natural habitat for the engineer, with his emphasis on the use of knowledge and technique, it is less obviously the

³² Reich 1985, 240

³³ Michael Aaron Dennis (1989), 490

home for the seeker of knowledge.”³⁴ As I see it, the message in all of these works is clear: science and engineering united effectively in the R&D lab because the latter developed an appreciation of the former. Or, to put it more bluntly, How marvelous that engineers can become scientists, too!

It is the act of first differentiating, then elevating science over engineering, that hurts—rather than helps—the authors of the 1980s interpret their source materials. Since historians have seemed to turn away from R&D as an object of study, the body of literature on this topic remains stuck in time. Without updating this literature, scholars outside of history, such as Benoit Godin, continue to insist upon the unique essence of science that distinguishes it from engineering. Taking Godin’s work as an example, the result has been nostalgic efforts to locate in the past a science free from “external” constraints. This continues even though texts covering such far-spanning periods of time as that of Mario Biagioli’s *Galileo Courtier* and Robert E. Kohler’s *Partners in Science* make plain that patronage in some form or another has always factored into the equation, for better and for worse.³⁵

Another, less obvious consequence has been the wholesale absence of accountants in the historical literature on R&D. To Shapin’s argument “that much academic social science which dealt with ‘unhappy industrial scientists’ was in large part (not wholly) *deducing its objects from theory*,”³⁶ I would add that the neglect of accountancy from extant R&D histories has arisen for the same reason. Accounting in these works is treated, if at

³⁴ George R. Wise *Technology & Culture* (July 1980), 409

³⁵ Mario Biagioli’s (1993) *Galileo Courtier*. The Royal Societies for science in Britain offer up another example.

³⁶ Shapin 2008, 68-9

all, as a passive compilation of figures, of quantities, rates, and ratios. But just as R&D resists fixed epistemological categories, so too does accountancy. The exercise of judgment in navigating the indistinct boundary between capital and revenue expenditures, the struggle for professional recognition in the face of employers who dismissed accountants as mere “bean-counters”, and the distinct experiences of cost or company accountants and public or independent accountants all factor into the evolution of R&D. By examining these and other features of accountancy, this dissertation seeks to enrich the present state of historical research on R&D. Such a project, it is well to note, should be approached *without* endeavoring to develop epistemological taxonomies of R&D and accounting knowledge, the result of which would amount to solutions without a problem.

The third and final reason for studying R&D through the lens of accounting history has to do with advancing a relatively recent trend in my own field of Science and Technology Studies (STS). In recent years, scholars such as Bruno Latour, Michel Callon, Casper Bruun Jensen, David Stark, Mario Biagioli, Isabelle Stengers, Fabian Muniesa, Ian Hacking, John Law, Steven Shapin, and David Edgerton, among others, have been experimenting with the approach of classical pragmatists William James and, to a lesser extent, John Dewey. The evolution in the work of Bruno Latour, a foundational figure in STS, offers a good case study to make this point. According to Latour, it was in the 1980s when it occurred to him that a project “of systematically comparing truth productions” begun years earlier, would require “a double-edged critique of Nature and Society.” To critique Society, he “had to delve into social theory and to propose, with Michel Callon, under the horrible name of actor network theory [ANT], an alternative possibility.” The

history of ANT is well known.³⁷ Despite this, few recognize in Latour's critique of Nature "a rethinking of much philosophy, and, as I discovered completely by surprise, of politics as well." This critique, Latour added, "required a somewhat drastic overhaul of ontology, in order to have enough room, so to speak, to accommodate so many different types of existing entities." It was through this latter effort, his critique of Nature, that Latour arrived at Jamesian and Deweyian pragmatism.

Recalled Latour: "Having started out with semiotics as my toolbox...I had to get into more hard-core philosophy. The breakthrough was made possible for me through the work of Isabelle Stengers – certainly the longest lasting influence on my overall thought – especially her introduction to Alfred North Whitehead." Whitehead, it should be noted, explicitly sought to revive James and Dewey's work.³⁸ Latour continues:

It was through Whitehead, and also William James, that I discovered...the reason why it was so difficult to find an ontological status for all those other entities I was slowly adding to my list of modes of existence. If, in the eyes of Whitehead, William James had put an end to...the 'Bifurcation of Nature' (Whitehead, 1920), it is because James had made a shambles of the distinction between primary and

³⁷ Since the development of Actor-Network Theory is well-known, I relegate a summary to this footnote. Latour and Callon began by rejecting mainstream sociology's notion of the "social," arguing that it does not consist in agency and structure. In place of this dualism, the two men offered up the actor, one who is not an agent "with predefined characteristics, but rather underscores the many ways in which actors bestow agency on one another, thereby enabling subjectivity, intentionality, and so forth to emerge in network processes." As for "network," this was not to be taken as an abstraction, but an assemblage of concrete things, inscriptions, objects, sentient being, etc. in specific contexts. As Christopher Gad and Casper Bruun Jensen aptly explain, "ANT is certainly not a method of telling researchers what to do. If it can be considered a method, it is so only in a counterintuitive sense of providing a crash course in learning to recognize the limitations imposed on research by an overly reverential attitude to theories and methods. The sole 'method' of ANT, then, is to provide a constant reminder that research is always likely to encounter conglomerates or hybrids of action rather than pure entities"(2010, "On the Consequences of Post-ANT").

³⁸ In *Process & Reality*, published in 1929 as a revision of his Gifford Lectures of 1927-8, Whitehead explained: "I am also greatly indebted to Bergson, William James, and John Dewey. One of my preoccupations has been to rescue their type of thought from the charge of antiintellectualism, which rightly or wrongly has been associated with it"(xii). See also: *Thinking with Whitehead and the American Pragmatists: Experience and Reality* (2015), edited by Brian G. Henning, William T. Myers, and Joseph D. John.

secondary qualities. Relations are not what is *added* to a world of meaningless matters of fact, but what are empirically *given* in the world of experience.

While Latour's observation is crucial to his post-ANT work, similar moves have been made by others already mentioned, although for reasons of space, their paths towards Jamesian and Deweyian pragmatism will not be traced. Suffice it to say that the present dissertation hopes to further this methodological reorientation within STS. Furthermore, this trend within STS is particularly amenable to a study of R&D and accountancy both. The relevance of John Dewey for historical inquiry into R&D will be examined in the next section. As for accountancy, it is no small feature of the field that much of its conceptual toolkit has been thrown against the test of experience.

Throughout its development, accounting thought has been challenged by the experience of those who put the products of accountancy to use. A whole host of credit granters, financial analysts, investors, bankers, unions, managers, executives, and regulators rely on the products of accounting practice. The figures appearing on inward-facing progress reports and outward-facing financial statements are the result of methods that have been deliberated over and contested for more than a century, since accountancy took hold in the US. At various moments in history, the figures etched on paper have ceased to be of use for one or more user groups, and it is precisely these moments of contestation, of how accountants could and did respond to their own "indeterminate situations," that pragmatic analysis is most sensitive to. Since accountancy is where the rubber meets the road, where theoretical abstractions are negotiated at the limits of experiences, there is much to recommend it as an object of study for STS scholars of a pragmatist bent.

1.3. Experimental Knowing and the Quest for Certainty

To understand the relevance of Dewey's theory of inquiry for our study of R&D, we had better to first examine his critique of classical philosophy and its legacy upon the Western analytic tradition. That which we might recognize as an object of everyday, existential life, was to philosophers of antiquity a mere illusion, relegated to a lesser status of a phenomenal world. Such objects are rank ordered by the extent to which they approximate their true, celestial, ideal ends, or *telos*. For our purposes, this cosmology has two notable features. First, knowledge is *a priori* truth and is fixed to its specific object as its own unique essence. Objects of knowledge are, quite literally, knowledge-embodied objects. Second, to know an object is to instantly recognize its fixed truth. Those whose minds more closely correspond to the ideal realm of superior Being (ie, philosopher-kings) are able to immediately possess the truth of the objects they behold. From this cosmology was borne what Dewey referred to as "the spectator theory of knowing," which, impressively, has managed to withstand even the demythologizing efforts of the scientific revolution.³⁹

By positing a knower apart from the known, this theory introduces a gulf between man and nature and makes possible a whole host of *a priori* distinctions that are every bit as pervasive today as they were in Dewey's time—and for the simple fact that they continue to instill a sense of certainty about an otherwise uncertain world. Said Dewey: "Any philosophy that in its quest for certainty ignores the reality of the uncertain in the ongoing processes of nature ... is marked at some point by a division of its subject-matter

³⁹ STS scholars have made similar references to what Dewey refers to as "the spectator theory of knowing." Donna Haraway's "God trick" is one prominent example.

into the truly real and the merely apparent, a subject and an object, a physical and a mental, an ideal and an actual, that have nothing to do with one another, save in some mode which is so mysterious as to create an insoluble problem.”⁴⁰ Throughout his career, Dewey devoted considerable energy to reminding his audience of the historical development of Western philosophy, encouraging them to examine the rampant use of *a priori* binaries that seemed always to be used in lieu of more careful explanation. By using history in this way, Dewey sought to reconstruct philosophy away from mere contemplation and upon a new basis of inquiry usable for clarifying, evaluating, and ameliorating a wide range of experiences in everyday life.

As Dewey expressed it, inquiry is a process of knowing developed through the resolution of a problematic situation. Because humans are cosubstantial with their environment, not apart from it, the transition from the experience of an indeterminate situation to the settling of a determinate one is transformative, not additive. Knowing is, to borrow from Latour, *given* in the experience of this transition, rather than *added* to some external, antecedent “Nature.” As such, this process is marked at every stage by action. There is the act of responding to a sense of dissonance, followed by the act of thinking through the dissonance to formulate a problem in a way that allows for an answer. Next, there is the act of testing out emergent ideas or hypotheses against experience, the act of refining earlier ideas so as to resolve the indeterminacy of the original problematic situation, and ultimately the act of instituting a warranted assertability that is usable for

⁴⁰ *The Quest for Certainty: A Study of the Relation of Knowledge and Action* (Gifford Lectures, University of Edinburgh, 1929)

future experience. This process he summarized as “the pattern of inquiry,” the early stages of which were identified and purposefully demonstrated in section I above.

For Dewey, studying the past is an experimental inquiry. Rather than study the past for its own sake or to uncover facts which prove one’s own theory, historical research qualifies as inquiry when it is launched by a felt need or problem experienced in a present situation, and executed with an eye towards establishing a “warranted assertion” that is useable for experience. History as one site of experimental inquiry enables us to examine our long-standing habits so that we can see them as being capable of transformation in the service of new purposes in the future. This is where I find a productive overlap in Michel Foucault’s methodological essay, “What Is Enlightenment?” In this essay, Foucault describes his approach as the adoption of a “historico-critical attitude” that motivates a different kind of Nietzschean genealogy, namely: an approach to historical inquiry that “will separate out, from the contingency that has made us what we are, the possibility of no longer being, doing, or thinking what we are, do, or think.” This approach, says, Foucault, *must* be experimental: “I mean that this work done at the limits of ourselves must, on the one hand, open up a realm of historical inquiry and, on the other, put itself to the test of reality, of contemporary reality, both to grasp where change is possible and desirable, and to determine the precise form this change should take.”⁴¹ Ian Hacking appropriately calls Foucault’s approach historical ontology.⁴² Taken

⁴¹ Foucault, “What is Enlightenment?”

⁴² Hacking 2002. Whether you call it genealogy or historical ontology, it is Foucault’s experimental approach that I put to use in the present project, rather than the conceptual products of his research, such as knowledge/power or the microphysics of power. Aside from these concepts, a more important albeit subtle difference between Foucault and Dewey lies in the emphasis they place on the initiation of a study. Where Dewey identifies the experience of doubt as the antecedent condition of inquiry, Foucault encourages a more deliberate seeking out of discomfort through his notion of problematization.

together, pragmatist genealogy offers what the philosopher Colin Koopman calls “a middle way between epistemological foundationalism on the one hand and epistemological relativism on the other.”⁴³

Contrast this “middle way” against the historiographical debates that raged not only in accounting history but in business history, economic history, and elsewhere. As Koopman has shown well, antiquarian or traditional historians see themselves as champions of objectivity. Instantiating a sharp separation between knower and known allows them to view truth as correspondence to the reality of the past. Critical historians, or relativists as Koopman calls them, deny a separation between history and fiction, and “see history as largely driven by the subjective preferences of the discipline’s leading practitioners.”⁴⁴ Thus, whereas objectivists tend to exhibit optimism about the past, relativists maintain a pessimistic view in regards to changes over time. In either case, a subject-object dualism is maintained.

Objectivist historians, for example, see “pure research” as the essence of R&D, and in so doing, have instantiated what Edgerton rightfully identified as a misplaced asymmetry between research and development. By contrast, the critical theorist take on R&D would insist that, in lieu of *essences*, the truth of the matter is that there is no truth nor any good to be discerned. In this equally unproductive view, R&D is treated as epistemologically plural, thereby rendering its meaning as being merely “in the eyes of the beholder.” Certainly, it is all to the better that, in the hands of critical theorists, “pure research” is no longer fixed as the essential truth of R&D. However, a new problem arises

⁴³ Colin Koopman *Pragmatism as Transition* (2010), 697

⁴⁴ Colin Koopman *Pragmatism as Transition* (2010), 697

when what was once the essential knowledge of the object, is then reassigned to the mind of the subject, i.e. the scholar or analyst. By making this move, any and all understandings of R&D become equally valid and equally invalid. Thus, writes Koopman, the “common assumption of optimists and pessimists alike is that freedom is truth’s consequence. This thesis renders superfluous any effort in experimentation.” By contrast, “pragmatism refocuses attention on the possibilities of our efforts in holding that the truth does not make us free.”⁴⁵

Only with a Deweyian understanding of inquiry has it been possible for me to analyze accountancy’s active role in negotiating R&D value (as opposed to the passive, apolitical role usually assigned accountants); to construct a history of R&D (as opposed to the history of “pure” science and “applied” engineering); and to recommend a way forward for STS that does not engage in what Dewey called “a quest for certainty.” A pragmatist genealogy of R&D lays bare decades of untenable assumptions about differences between pure science and industry technology. Observes Johnson: “neither applied nor industrial science is adequately reflected in the dominant picture of the scientific enterprise that has been constructed by historians and philosophers of science.”⁴⁶ Taking a step back, we might ask: could it be that the qualities separating pure (academic) science from applied (engineering) science are not as natural nor fixed as we usually presume? Dewey cautioned of the “superstitious awe reflected in the current estimate of science. If we could free ourselves from that abject emotion,” he urged, “it would be clear enough that what makes any proposition scientific is its power to yield

⁴⁵ Colin Koopman. *Pragmatism as Transition*. p.20

⁴⁶ Johnson 2008, 612.

understanding, insight, intellectual at-homeness, in connection with any existential state of affairs, by following events with coherent and tested meaning.”⁴⁷

Rather than attempting to divine the essence of science or engineering, the more pragmatically productive question with respect to R&D is: How are they *related*? One possible answer proffered in this paper is “the experimental method.” This is the means by which all inquiry—regardless of the setting in which it occurs, regardless of the individual inquirer’s professional or personal identity—achieves its end. Depending on how you define it, another answer may very well be to use the concept of “technology.” Anthropologists such as George Herbert Meade, who himself exerted a profound influence on Dewey’s thinking, have long used this term in an expansive way, inclusive of a wide range of conceptual and tangible equipment that are, quite simply, *of use* for particular ends. This is the approach recommended by Dewey scholar Larry Hickman.⁴⁸ Ultimately the trick is to avoid reinscribing the historical association of science with purity, and to advance an alternative concept that effectively communicates science as an experimental process executed towards the resolution of an initially identified problem. In the present analysis, then, engineers are analytically treated no differently than scientists, industrial technologies no differently than academic laboratory discoveries, and R&D no differently than experimental inquiry.

1.4. Getting Their Hands Dirty

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⁴⁸ Larry A. Hickman, *John Dewey’s Pragmatic Technology* (Bloomington, IN: Indiana University Press, 1990); Hickman, *Philosophical Tools for Technological Culture: Putting Pragmatism to Work* (Bloomington, IN: Indiana University Press, 2001)

Having just arrived at an understanding of R&D as one instantiation of the experimental method, we can now consider how the institutional development of R&D was itself experimental, as well. In the early twentieth century, most businesses had no pressing need to set up their own internal research organizations. The late-nineteenth century consolidation of industry accelerated the transformation of the United States as a nation of small-scale businesses producing a range of products, to one dominated by large-scale manufacturers that mass-produced a much more limited range of products with a set method of manufacturing.⁴⁹ For very few firms was the potential benefit of hiring full-time researchers worth the expense. Should the occasional need arise for a new piece of equipment or manufacturing process, firms would commonly acquire patent rights from outside inventors or else buy whatever devices they needed in the market. Sometimes, inventors would be hired for brief stints on short-term contracts, only to leave the business once a particular manufacturing problem had been resolved. None of these actions presented any problems for accountancy. Whether in the marketplace, the contract, or the transfer of rights, the price paid was verifiable either by receipt or by stipulation. For most corporations, such events occurred infrequently. Neither the manufacturing processes nor the products manufactured changed all that often. When tweaks had to be made, they were considered routine and usually paid for out of profits of the period. In fact, even General Electric, which opened the nation's first, researcher-populated industrial laboratory in 1900, did not cease acquiring patents from outside inventors.

⁴⁹ Naomi Lamoreaux (1985) *The Great Merger Movement in American Business, 1895-1904*. pp.30-34.

Gradually, however, other large manufacturing corporations followed General Electric's lead and began paying salaries to men whose duties explicitly included experimental work.⁵⁰ This would present a methodological problem for accountancy, although it would take many years before the proliferation of internal research organizations gave urgency to this problem. In the meantime, R&D labs of all shapes and sizes were opened up at such firms as E.I. du Pont de Nemours, Westinghouse Electric, Corning Glass Works, Eastman Kodak, and American Telephone and Telegraph (Bell Labs).⁵¹ As already noted, these early organizational forms would prove to be quite malleable over the years, and it is in this sense that R&D, as an institutional configuration, was very much itself an experiment.

By the 1920s, a growing number of faculty and students had left their academic posts for careers in industrial research, much to the disbelief of their former colleagues and mentors. The latter viewed research performed outside the university as destined by nature to become corrupted, impure, and this seemed only to be confirmed by the severity of the Great Depression the following decade. As the 1930's wore on, research directors facing charges of waste and "technological unemployment" delivered impassioned speeches defending the improved quality and reduced cost of products resulting from R&D. Even as calls for a "moratorium on research" gathered steam,⁵²

⁵⁰ In a January 1911 issue of *Industrial & Engineering Chemistry*, M.C. Whitaker explained that the "largest industrial corporations...have established and are maintaining research laboratories. These laboratories are...equipped and maintained at great expense [and] are being established because there are no existing equipments, on a suitable scale, for solving the problems necessary for their industrial advancement"(38).

⁵¹ Others companies to open R&D labs in the first quarter-century or so include: Dow Chemical, Parke Davis, Goodyear, General Motors, E.R. Squibb, B.F. Goodrich, Monsanto, Shell Oil, Atlantic Refining Company, American Cyanamid, U.S. Rubber, Standard Oil of New Jersey, of California, and of Indiana, and US Steel.

⁵² Carroll W. Pursell, Jr. "Government and Technology in the Great Depression," *Technology & Culture* Vol. 20, No. 1 (January 1979): 162-174.

these directors continued to emphasize the social benefits of research, while downplaying development. This defensive strategy relied upon an artifact of Western culture trailing back to the philosophers of antiquity and their elevating of pure, true science over impure, ephemeral technology or engineering. The war, however, made such positioning moot. R&D's tarnished image was restored by the game-changing devices generated and manufactured by the nation's R&D labs. Firms that had never considered opening their own R&D organizations were now convinced of its value, especially when government contracts made it easy to do so. Accelerated by the demands of the Second World War, R&D thus emerged at the end of 1945 a permanent feature of American political and economic life.

Through the eyes of Cold War-era policymakers, R&D became a knowable object of intervention that could be measured, managed, and incentivized. In this process, R&D would shift from a *means* to an end, to an end itself. Crucial to this shift was the role of macroeconomists who, confronted by an anomaly in national income data, began experimenting with R&D as a distinct form of capital appearing in a type of mathematical formula known as a production function. In the present analysis, this anomaly—which one economist in the 1950s referred to as the “measure of our ignorance”⁵³—constitutes an indeterminate situation which macroeconomists resolved by theorizing R&D as capital. Accountants, however, were not so easily convinced. To them, treating R&D as capital implied a guarantee that money spent on R&D *predictably* generated income for the firm. Capital, after all, was by convention an income-producing

⁵³ This quote is from Moses Abramovitz whose contributions to the what became the economics of R&D are discussed in later chapters.

asset. It was something invested for the sake of generating income for the foreseeable future as one would expect, for example, of manufacturing equipment; but it also had two additional features.

First, capital assets designated items that could be liquidated (or converted to cash through sale) and used to repay debts. This was especially important in the event a firm should fail to generate enough income to keep itself afloat. Second, the sum of capital, including not only tangible property but also cash on hand, shares of stock, and other liquid assets, could be used to demonstrate the sum value of a firm in the event of a potential sale or in applications for credit. If expenditures on R&D could not be counted on to produce income, then it ought not be accounted for as capital. This was hardly up for debate, and neither was the question of whether or not R&D expenditures represented an asset that could be liquidated to meet debts. The indeterminacy that remained for accountancy, however, was whether R&D expenditures could be used to enhance the value of the firm by including it among capital assets in publicly-facing financial statements.

From the beginning of the century onward, accountants alert to the many failures and dead ends involved in R&D, vacillated between a treatment of R&D as a capital expenditure, on the one hand, and an ordinary business expense on the other. Bridging these two poles was the deferred charge, an accounting category that emerged on the American industrial landscape at just about the same time as R&D itself. As the number of firms with R&D labs multiplied during and after World War II, no one doubted that the expenditures of these organizations could be charged to income account as they were incurred. The difficulty came in determining whether or when R&D expenditures might be treated as deferred charges to income, to be publicly reported among a company's

assets. Few would go so far as to suggest that such expenditures qualified as direct charges to capital account; as an experimental activity, there was just no guarantee that money spent on R&D, unlike with the purchase of manufacturing equipment, would contribute to firm value in any sort of measurable, predictable fashion.

Eventually, a decade of out-of-control earnings manipulation made possible in part by corporate managers' freedom to defer R&D expenditures, forced leadership of the public accounting profession out of complacency. By 1974, a pronouncement backed by the force of law was issued by an independent body of accounting standard setters requiring the charging of non-contracted R&D expenditures to income as incurred. No longer could non-contracted R&D expenditures be held in limbo as deferred charges awaiting final disposition as assets or losses. After nearly three-quarters of a century, the profession finally had a mandate reflecting the years of experience accumulated by company cost accountants. Unlike public accountants, cost accountants were salaried employees of the firms they served, and as such, they constitute the frontlines of R&D accounting. For decades, cost accountants used their own annual conventions, local chapter meetings, and professional journal to deliberate the benefits and disadvantages of accounting for R&D either as a current charge to income or as a quasi-asset in the form of deferred charges. Unlike many of their counterparts in the nation's leading public accounting firms, cost accountants got their hands dirty. This did not make them morally superior in the slightest, but it did mean that they could not speak only in aggregates, like public accountants, or in abstractions, like accounting theorists in academia. Just as academic researchers scoffed at the work of industrial researchers, public accountants maintained the same disposition towards cost accountants. In both cases, the proximity to commerce was seen as a corruption of a more pure, untainted practice.

By the time the public accounting leadership took the problem of R&D head on, economists were many years into a campaign that would define the scope of national economic policies for generations to come. In 1980, a temporary incentive to stimulate growth in R&D expenditure was introduced into the federal tax code. Later that decade, the R&D tax credit was renewed with a few tweaks. Since then, the credit has been reauthorized by Congress each time it has come up for renewal, and always on the basis of macroeconomic arguments in which R&D features as a capital asset. Indeed, in 2017, as part of President Trump's Tax Cuts and Jobs Act, the once temporary credit has now been made a permanent feature of the US tax code. In these arguments, the aggregate sums spent on R&D are treated as input into the US economy, with growth in the economy as its inevitable output. By explaining growth outputs in terms of R&D inputs, postwar macroeconomists failed to learn the lesson that was so hard won by the accountants, namely: that it is the very uncertainty of outcomes which makes experimental inquiry so valuable to society and yet so untenable as capital.

Chapter 2

Industrial Accountancy in the New Century

2.1. The Fuzzy Boundaries of Corporate Capital

Heading into the twentieth century, British public accountancy traditions continued their reign over American practice. This occurred not simply as a source of precedents in the same way that American lawyers and justices drew upon British common law decisions, but also directly through English émigrés.⁵⁴ Attractive opportunities for investment following the Civil War had led to a rise in British capital exports across the Atlantic, with particular interest shown in American railroads and breweries. With this outflow of capital grew the need for auditors whose reports from overseas helped British investors track how effectively their investments were being put to use. Towards the end of the nineteenth century, the increasingly frequent boom-and-bust cycles of American industry compelled British auditing houses to set up permanent satellite offices, an exemplar of which was the New York City branch of Price Waterhouse which began work in 1890. Soon to arrive at the New York office of Price Waterhouse was the young George O. May and his mentor Arthur Lowes Dickinson.

Looking upon the state of American accountancy in his time, Dickinson reported to the London office of Price Waterhouse that a “great deal yet remains to be done before

⁵⁴ Gary Previts and Barbara Merino. *A History of Accountancy in the United States: The Cultural Significance of Accounting* (Columbus, OH: Ohio State University Press, 1998); M. Chatfield, *A History of Accounting Thought* (London: Dryden Press, 1974); T. A. Lee “US Public Accountancy Firms and the Recruitment of UK Immigrants: 1850-1914,” *Accounting, Auditing & Accountability Journal*, Vol. 14, No. 5 (2001): 537-64; T.A. Lee “UK Immigrants and the Foundation of the US Public Accountancy Profession,” *Accounting Business & Financial History*, Vol. 12, No. 1 (2002): 73-94; S.E. Loeb and Paul J. Miranti, *The Institute of Accounts: Nineteenth-Century Origins of Accounting Professionalism in the United States* (London: Routledge, 2004).

the [New York] business can in any way be compared with yours in England.”⁵⁵ With a domestic accountancy profession yet to take shape, Price Waterhouse represented one of, if not the, largest accounting firm in America, and as such, it was capable of handling the books of many of the largest businesses, including US Steel and Du Pont. May and Dickinson were thus viewed by many as the upper crust of the British-expatriate elites, the latter of which included such figures as Arthur Young, Thomas Higgins, John Ballantine Niven, James Marwick, Roger Mitchell, John Barrow, and George Touche, among others.⁵⁶ Simultaneously resented and admired by accountants in various corners of the country, these men introduced standards of practice where none existed before.

As accountancy developed in the United States, a key concept inherited from the British tradition was that of capital as a value of income-producing property, or wealth. But there were important differences that accountants, whether British- or native-born, had to be mindful of in their thinking.⁵⁷ Rooted in the practices of aristocracy, whose capital had long been represented by land bound up in estates passed down from one generation to the next, the older English conception of capital was a poor fit for the more socially-fluid and heavily-industrializing American context.⁵⁸ Equally, if not more,

⁵⁵ As quoted in David Grayson Allen and Kathleen McDermott *Accounting for Success: A History of Price Waterhouse in America 1890-1990* (Boston: Harvard Business School Press, 1993), 25.

⁵⁶ Those familiar with the names of the largest public accounting firms in the US, will note that many of the men named here were founding partners of such firms as Peat, Marwick, Mitchell, Arthur Young & Co., and Touche, Ross. For a more detailed discussion on the dominance by British-Americans in late 19th, early 20th century accountancy, see Derek Matthews “The Social Class, Educational Background, Gender and Recruitment of American CPAs: An Historical Profile” *Accounting Historians Journal*, Vol. 43, No. 1 (June 2016): 121-156; and Dale L. Fleisher & Gary Previts “Preparing an Accounting Professional: The Articles of Clerkship (1892-1897) of George Oliver May,” *Accounting Historians Journal*, Vol. 41, No. 1 (2014): 61-78.

⁵⁷ For a discussion of some of the efforts made by British accountants to adapt to American culture, see Thomas A. Lee “Assimilation and Americanisation in the Progressive Era: Price, Waterhouse & Company in the US, 1890-1914” *Accounting History*, Vol. 19, No. 1-2 (2014): 13-30.

⁵⁸ The eminent accounting historian Barbara D. Merino adds that, “By 1901 American accountant warned that their English brethren had failed by continued reliance on classical economics to recognize the changing nature of the accountant’s role in an industrial society. Wage-fund theory emphasized that the

important was the increasing number of businesses operating in the corporate form. The industrial landscape of America was changing from a nation of self-owned proprietorships and partnerships, to corporations.⁵⁹

In the case of proprietorships and partnerships, any revenue (or income) remaining at the end of a fiscal period are transferred to the capital accounts of the owners (i.e., the proprietors or partners), thereby merging the end-of-year balance with capital previously contributed to the firm.⁶⁰ The boundary between capital and revenue was thus temporary if not altogether fuzzy. With corporations, though, the line between capital and income was believed to be somewhat permanently defined by state incorporation laws.⁶¹ Developed separately by each state, these incorporation statutes governed the process by which businesses could be organized in corporate form.

Organizers would compose charters in accordance with state laws usually requiring an explicit statement somewhere in the proposed charter, of the amount of capital upon

amount of capital wealth determined the amount of industry possible. Both accountants and economists noted that this was obviously not the case in the United States. Efficient credit markets had appeared which added to the productivity of industry and increased the efficiency of money. Facilitating capital flows into these markets became the prime function of the accountant rather than the traditional duty of detecting fraud. While English accountants continued to stress the complete balance sheet audit, American accountants began developing testing procedures, less effective in detecting fraud but far more efficient in promoting fuller disclosure that could promote capital flow”(Merino 1976, 34). “Development of American Accounting from 1876 to 1976.” *The CPA Journal*, Vol. 46, No. 6 (July 1976): 35.

⁵⁹ Berle & Means, ch. 2 “The Appearance of the Corporate System.” “Thus, in field after field, the corporation has entered, grown, and become wholly or partially dominant. The date of its appearance and the degree of its dominance have in general varied with two factors, the public character of the activity in question and the amount of fixed capital necessary to carry on business. It came first in the fields of public utilities, common carriers, banks and insurance companies (which even in the 1840’s were conceded to perform public functions) and last in the areas of personal service and agriculture;—early with the high fixed capital costs in railways and mines; late, in mercantile pursuits where capital consists to such a large extent of stock on hand. On the basis of its development in the past we may look forward to a time when practically all economic activity will be carried on under the corporate form. ... It does not simply give a legal clothing to the private enterprise of individuals. It adds a new quality to enterprise—the quality of multiple ownership”(B&M, 17).

⁶⁰ Littleton 1933, 217

⁶¹ This belief rested on precedents set by British courts in the late-nineteenth century which held that “capital and revenue were separate.” Richard P. Brief “The Origin and Evolution of Nineteenth-Century Asset Accounting,” *Business History Review*, Vol. 40, No. 1 (Spring 1966), 15.

which a company would be registered and operate as a corporation. Known as the par value of a company's capital stock, the declaration of this amount constitutes an important site in which the distinction between capital and income was governed by law.

In the most simplified of terms, whenever investors contributed wealth to a newly-forming corporation, the money or property they committed thereto, or “paid in,” became permanent capital that the business would put to work in generating earnings. The expectation was that, in each fiscal period, the earnings generated through production would be used 1) to ensure that the company's opening level of capital did not diminish, 2) to cover the costs of any and all expenses incurred in running the business, and 3) to pay out regular dividends as profits to investors. In a text that remains, to this day, a profoundly influential study of the evolution in corporate governance,⁶² law scholar Adolf A. Berle and economist Gardiner C. Means explained that, in a legalistic sense, capital “is that amount of purchase price [for a share of company stock] which the law requires the corporation to segregate on its books, and over which the law throws certain restrictions: *viz.*, that it cannot be used for the purchase of the corporation's own stock and that it cannot be paid out as dividends.”⁶³

⁶² The influence of *The Modern Corporation and Private Property* since its publication in 1932 upon every single generation of law and corporate governance scholars, cannot be denied. However, as Harwell Wells rightly points out, the ideas in that text—such as the famous separation of [share] ownership from [managerial] control—were already objects of debate in the 1890s, if not earlier. Notable examples given by Wells include Louis Brandeis, Walter Lippman, and Thorstein Veblen before World War I, and William Z. Ripley in the 1920s. See Wells' “The Birth of Corporate Governance” *Seattle University Law Review*, Vol. 33, No. 1 (2010): 1247-1292.

⁶³ “The economist and the accountant,” Berle and Means observed, “quarrel violently and with reason over this legalistic interpretation, some holding that the restrictions which the law applies to ‘capital’ as so interpreted, should be applied equally either to the assets which the corporation intends to devote permanently to the conduct of its business (as distinguished from those which it expects to distribute from time to time) or else to the entire amount of contributions made by individual shareholders for their participations. Accountants lean peculiarly to the latter view. To the lawyer, however, ‘capital’ means that amount which by reason of some statute or rule of common law, the corporation was obliged to segregate, and maintain intact, save for possible impairment through business operations” (Berle & Means, 149-150).

If a corporation sought to expand its capital base, it could do so by retaining earnings, selling additional shares or bonds, or by incurring debts from outside lenders, all of which usually required approval by voting shareholders. Over time, however, the retention of earnings came to be seen more and more as a decision that ought to remain in the hands of management.⁶⁴ This occurred in at least two fronts. Through a series of court cases in the late nineteenth and early twentieth century, minority shareholders alleging the mismanagement of company resources were denied relief by U.S. judges on the grounds that evidence presented was insufficient to prove flagrant abuses of power. As economic historians Naomi Lamoreaux and Jean-Laurent Rosenthal explain,

directors of corporations large and small frequently negotiated contracts with other companies in which they had a financial interest, elected themselves to corporate offices at lucrative salaries that they themselves set, arranged mergers that earned themselves impressive capital gains while leaving other shareholders in the lurch, and engaged in a wide variety of other actions from which they benefitted at the expense of their associates. Examples included lending themselves corporate funds, issuing themselves additional shares of stock, and settling lawsuits against their companies that they had helped to bring in the first place.⁶⁵

Lacking compelling evidence to prove their case, shareholders were up against the “business judgment” rule, which granted benefit of the doubt to corporate directors. As it emerged through nineteenth century court decisions, this rule set a standard that assumed managements acted prudently and in the best interest of investors and creditors alike.

⁶⁴ In time, how well managements put retained earnings to use to generate greater income came to be seen as a proxy for managerial effectiveness or efficiency.

⁶⁵ Naomi Lamoreaux and Jean-Laurent Rosenthal “Corporate Governance and the Plight of Minority Shareholders in the United States before the Great Depression” in *Corruption and Reform: Lessons from America’s Economic History*, edited by Edward L. Glaeser and Claudia Godin (Chicago & London: University of Chicago press, 2006): 126-127.

Unless shareholders could prove managements were not acting in good faith in the ordinary course of business, they could not complain.

Without this assumption, judges feared that what Lamoreaux and Rosenthal have called “the problem of untimely dissolution” would befall corporations the way it had partnerships. If corporations were easy targets for shareholder lawsuits, then internal disagreements could quickly lead to mounting business failures, as had happened with partnerships. “For this reason,” Lamoreaux and Rosenthal conclude, “the courts were very conservative in defining what constituted an abuse of trust by those in control.”⁶⁶ Others, such as Stanford Law School professor and American legal historian Lawrence M. Friedman, have been less forgiving in their analysis.⁶⁷ Friedman, for example, described the decades following the Civil War as “an age of vultures” in which the “investing public was unmercifully fleeced,” and that legislators and the “courts were corrupted, too.”⁶⁸

Expansion of managerial discretion was also brought about by reforms to incorporation laws that swept the nation in the late nineteenth and early twentieth century as states competed among themselves to attract business and thereby raise tax revenues. States, argued Friedman, had “found the lure of easy money from chartering businesses irresistible.”⁶⁹ Through these reforms, greater flexibility was introduced in the

⁶⁶ “Lamoreaux & Rosenthal 2006, 142

⁶⁷ Lawrence M. Friedman, *A History of American Law, Second Ed.* (New York: Simon & Schuster, Inc., 1985/[1973])

⁶⁸ “Justice was a whore of the rich,” argued Friedman. “In New York, judges like George Barnard and Albert Cardozo did what the robber barons wished; they issued injunction for a price, sold the public interest down the river out of ignorance or greed”(Friedman 1985/[1973], 513). Following the Civil War, a series of high profile scandals tainted the law profession. In New York City, growing public outrage over corrupt judges and lawyers led to the formation of the nation’s first bar association in 1870: the Association of the Bar of the City of New York, predecessor to today’s New York City Bar Association.

⁶⁹ Friedman (1985/[1973]), 524.

provisions of corporate charters, including the very meaning of the term “capital” and how it might be put to use on a day to day basis by the men running the firm. “Basically,” concludes Friedman, “the corporation had torn free of its past—it could be formed almost at will, could do business as it wished, could expand, contract, dissolve.”⁷⁰

Since at least the late nineteenth century, directors of newly-forming corporations enlisted the services of corporate organizers, known as promoters, who were given shares of a company’s stock to sell to the public. Towards this end, promoters often included bonuses in their offers as an enticement to potential buyers, or else sold shares at a discount. By sweetening the pot in this way, promoters were all the more successful at drawing in investors. Too successful, perhaps: as a result of these sales tactics, there emerged widespread outcry that condemned such practices as “stock watering.”⁷¹ The value of a company was thus said to be “watered” if the amount contributed by shareholders fell short of the par value stated in the company charter. In such a situation, a company that became insolvent could take shareholders and creditors down with it. All this seemed anathema to the idea of limited liability which was intended to make the corporate form of business so attractive to investors in the first place. When lawsuits ensued throughout the country, state legislators legalized “no-park” stock on the hope that formalizing such under-funded stock would help mitigate “the evils of stock watering.”⁷² Observers viewed the competition among states as a “race to the bottom,”

⁷⁰ Friedman (1985/[1973]), 525.

⁷¹ David L. Dodd, *Stock Watering: The Judicial Valuation of Property for Stock-Issue Purposes* (New York: Columbia University Press, 1930)

⁷² Find citations for this phrase

and its history has been described as one marked by “drastic reduction of legal restrictions on management and of the legal rights of shareholders.”⁷³

When the new corporation statutes sought to protect investors by restricting the payment of dividends to that made out of *surplus*, rather than capital, the distinction introduced between capital and surplus was vague at best. It was generally urged that capital must not be used to pay dividends, but what exactly counted as surplus was a point of contention. To accountants, this seemed the obvious result of charters having been penned by attorneys unfamiliar with the importance of specifying whether expenditures of profits were capitalized, and therefore capital expenditures, or not.⁷⁴

“The whole point of issue is one of morals rather than law,” wrote Robert H. Montgomery, a leading accountant in his time, “since dishonest or unscrupulous directors are the only ones who pay dividends out of premiums received on capital stock. I am afraid that if accountants do not take a firm stand regarding the whole subject of surplus, particularly as related to no-par stock, the dishonest corporations will take advantage of the situation and stockholders will receive dividends, believing they are out of profits, whereas the dividends are out of either contributed capital or capital contributed to surplus, in either of which cases the stockholders will surely be deceived.”⁷⁵

⁷³ Ralph Winter “State Law, Shareholder Protection, and the Theory of the Corporation” *Journal of Legal Studies*, Vol 6 (June 1977): 255. See also Daniel R. Fischel’s “Race to the Bottom Revisited: Reflections on Recent Developments in Delaware’s Corporation Law” *Northwestern University Law Review*, Vol. 76, No. 6 (1982): 913-45; Roberta Romano “Competition for Corporate Charters and the Lesson of Takeover Statutes” *Fordham Law Review*, Vol. 61 (March 1992): 843-64; William Carey “Federalism and Corporate Law: Reflections Upon Delaware” *Yale Law Journal*, Vol. 83 (March 1974): 663-705.

⁷⁴ “When a new company is organized,” explained Seymour Walton, “he may stipulate that certain things shall be provided for ‘out of profits,’ without specifying whether or not they shall be charged to profits. If he uses the term as synonymous with a charge against profits, he is liable to make trouble for those who later have to follow out the letter of the by-laws and who may know the distinction between what is provided for out of profits as a source of income and what is chargeable against profits as an actual loss or expense” (“Relation of the commercial lawyer to the cert pub acctnt” Jan 1909, *JofAcc*, Vol. 7, No. 3, 208).

⁷⁵ Montgomery *The Accounting Review* (June 1927): 192-193

Such divergence between the value of equities sold and the value of the firm itself was compounded by the passage of laws allowing company directors to determine, for themselves, the value of assets they had acquired through mergers and paid for out of own capital stocks.⁷⁶ The result was a slew of corporate combinations issuing more shares than the acquired properties were actually worth. This practice, combined with stock-watering, meant that large numbers of firms across the country were overcapitalized; these corporations, in other words, “pretended to have capital, which they had not.”⁷⁷ By convention, a firm was held to be no more valuable than the amount of capital it had on hand at any given moment, and many, including investors, creditors, and judges, continued to assume as much on through the first couple decades of the twentieth century. It was by taking advantage of this assumption that unscrupulous promoters and directors of newly formed corporations successfully valued capital shares above par. When taken to court, their defense rested on the assumption that differences in price reflected future earning power, i.e. the notion that the present total value of a company’s capital reflected the earnings it would return in the future. Promoters, it was claimed, recognized this future value where others didn’t, and so they should be compensated for the unique services they brought to the table.

⁷⁶ As one prominent accounting academic recalled in 1937, “It is a well-known fact that such securities [as those issued by one firm in the purchase of another], with their par or stated values largely within the discretion of the officers and directors issuing them, have been handed out far more freely than cash would have been, and no experienced person is under any delusions as to an identity between a security price and a cash price paid for the properties”(Thomas H. Sanders, “Accounting Aspects of the Securities Act,” *Law and Contemporary Problems*, Vol. 4, No. 2 (April 1937): 196).

⁷⁷ William C. Cook, *A Treatise on the Law of Corporations Having a Capital Stock*, 8th ed. (1923/[1887]), 292.

This defense of stock-watering was not entirely inimical to public accountancy conventions of the time, as the Columbia University finance professor David L. Dodd explained:

In some respects the accepted practices of accounting seem to support the position that a corporation may properly issue stock for services to be rendered. In listing the assets of an enterprise the accountant sees no valid reason for excluding payments that have been made in exchange for promises to render future services. Hence his inventory of assets will include such items as prepaid insurance, prepaid rent and prepaid interest. All of these items are valuable to a going concern, though their value may be wholly unrealized. The promoter regards in the same light his promise, or the promise of others, to render future services in promoting the welfare of the corporation [i.e., in getting it up and running as a viable enterprise].⁷⁸

However, unlike the future value of a promoter's services, prepaid rent or prepaid insurance were "competitively sold in the market place and thus have a value to the enterprise which is fairly determinate."⁷⁹ Since promoters' future services "are not usually the subject of organized bargain and sale,"⁸⁰ they simply could not be capitalized on the same, verifiable basis as other assets.

The dilemma facing accountants was profound and fractured the nascent discipline, with those measuring firm value in terms of its tangible assets or current profits, on the one side, and those who determined that value on the basis of future earning power, on the other. The debate would eventually find resolution not as a result of curbing or eliminating promoters' value-inflating practices, nor as a result of writing off that excess value quickly enough to remove it from the books entirely. Instead, the debate

⁷⁸ David L. Dodd, *Stock Watering: The Judicial Valuation of Property for Stock-Issue Purposes* (New York: Columbia University Press, 1930), 47-48.

⁷⁹ Dodd (1930), 49.

⁸⁰ Dodd (1930), 49.

found resolution only by reframing the inflated value as a legitimate basis for goodwill—a so-called “intangible asset” that had previously referred only to the unique earning ability of shop owners to curry favor with their customers, either through the owner’s agreeable demeanor, the particularly convenient location of his store, or through the high quality of his products. According to Eric Hake, the “creation of this new asset expanded the value of capital on the books to equal the par value of securities issued.”⁸¹ Thus, the excess of par value of a corporation’s shares, over and above its tangible assets and current profits, would be recognized as a legitimate accounting technology—or, in the final analysis, “as elements of the new financial order.”⁸²

With no par stock statutes in place and with judges on their side, managers could more freely move around the value of property on company books so long as investors continued to receive some amount of dividend on a fairly regular basis. In several instances, this involved the creation of the much maligned “secret reserves” as well as the payment of dividends out of capital, rather than earnings.⁸³ Both actions assisted managements in withholding earnings from investors by effectively concealing the amount of profits earned.⁸⁴ Such managerial freedoms, aptly summarized by the Latin phrase *caveat emptor* and the French *laissez faire*, helped make possible the rapid industrialization of a young country.⁸⁵ Nevertheless, many contemporary observers of the

⁸¹ Eric Hake 2001, 427

⁸² Eric Hake 2001, 429

⁸³ Writing in 1937 about the opening decade of the twentieth century, Norman L. McLaren recalled that, “It was a virtue, thirty years ago, to create hidden reserves by the understatement of profits in prosperous years, and brave indeed was the public accountant, if any, who insisted upon a qualified certificate under these circumstances”(430). “The Influence of Federal Taxation Upon Accountancy” *Journal of Accountancy*, Vol. 64, No. 6, December 1937.

⁸⁴ Dale Buckmaster, p. 154. “Income Smoothing in Accounting and Business Literature Prior to 1854.” *The Accounting Historians Journal*, Vol. 19, No. 2 (December 1992): 147-73.

⁸⁵ *Caveat emptor* translates to “let the buyer beware,” while *laissez faire*, which more literally means “allow to do” is better translated as meaning “let alone” or being allowed to act without interference. As to *caveat*

time objected to the lack of transparency and deceptive practices that seemed to vitiate the very protections and stability that incorporation laws were supposed to provide.

2.2. Obsolescence Enters Depreciation

Under such circumstances, accountants struggled to convince company executives to set aside a portion of earned income, on a regular basis, in order to offset a process of decline in the value of capital assets known as depreciation. In what follows, we will highlight moments in the evolution of early twentieth century depreciation accounting, in the hopes of demonstrating how (1) the struggle for professional status, coincided with (2) the entry of R&D into accountancy practice writ large. Certainly, as an object of study, R&D itself was absent from the evolving considerations of depreciation. However, as a source of new and improved technologies, its *indirect* impact grew in significance as the handling of obsolete manufacturing equipment became a widespread concern.

As commonly discussed in the accounting literature, depreciation referred to the inevitable wear and tear on machinery which took place day in and day out. It also referred to the sudden destruction of property due to fire or storms, events that could neither be predicted nor controlled. To managements, however, this was all very academic and hardly worth the reduction of reported income that providing for depreciation would require. When pushed, managers, most prominently those working within regulated industries such as railroads, insisted that simply keeping up with repairs

emptor, Lawrence M. Friedman doubts it was necessary for industrialization; see Friedman (1985/[1973]), 264.

would suffice to maintain equipment in efficient working order.⁸⁶ By charging such repairs to income as needed, there would be no need to provide for depreciation, they argued.⁸⁷ As the eminent accounting historian and theorist Richard P. Brief has shown, this strategy resulted in the underestimation of how much capital was actually consumed in production, with the effect that firms might not have even known they were operating at a loss.⁸⁸

Accountants, still struggling for professional status in the business world of early-1900s America,⁸⁹ lamented the many cases of insolvency that had their origin in the reluctance of managers to adequately provide for depreciation.⁹⁰ At the “present time,” wrote one author in July 1909, “[cotton mills and] other lines of manufacture, particularly machine shops, electric plants, railroad and railway enterprises, public service

⁸⁶ Alfred Knight pointed out that, “Objections are sometimes raised against making charges for depreciation, especially by those who are endeavoring to make a profitable showing and pay large dividends. The plea is commonly offered that if maintenance is charged with all necessary repairs and a plant is kept in good operating condition, its cost value is effectively maintained. However, time is bound to develop the fallacy of this argument, as asset will eventually wear out, and if there is no provision made for this replacement, additional capital must necessarily be expended. Should there be no surplus to draw upon, it becomes necessary to increase the issue of capital stock, borrow fund by means of a bond issue, or devote current earnings to purposes of replacement, the latter course interfering with the payment of dividends and perhaps creating dissatisfaction among the shareholders”(1908, 190).

⁸⁷ “Manufacturers recognized that their equipment suffered from wear and tear and technological obsolescence, but instead of calculation depreciation as a cost, they financed replacements and improvements as needed out of revenues”(Lamoreaux 1985, 53).

⁸⁸ Richard P. Brief. “Nineteenth Century Accounting Error” *J of Acctg Res*, Vol. 3 (Spring 1965): 12-32.

⁸⁹ For a succinct yet excellent overview, see Barbara D. Merino “Development of American Accounting from 1876 to 1976.” *The CPA Journal*, Vol. 46, No. 6 (July 1976): 31-36. Also see Ram S. Sriram & Gloria Vollmers. “A Reexamination of the Development of the Accounting Profession – Critical Events from 1912-1940.” *Accounting Historians Journal*, Vol. 24, No. 2 (December 1997): 65-90.

⁹⁰ Although it is true that, as Lamoreaux explained, “Most nineteenth-century firms, when they calculated costs, simply did not account for depreciation”(1985, 53), this state of affairs persisted into the next century. See Alfred Knight in the January 1908 issue of *Journal of Accountancy*. A year later, in the same journal, Seymour Walton expressed similar frustration thus: “The conscientious auditor may find it his duty to criticize some of the acts of the officers and directors or to call attention to what he knows are important omissions in the neglect to provide sufficient reserve for depreciation or in any other of many essential points, but when he makes his report to those very officers and directors radically criticizing them he is usually certain that it goes no further, but is filed away and promptly forgotten”(Seymour Walton, *Journal of Accountancy* 7, no. 3, January 1909, “Relation of..” p209).

companies, and municipal industries alike, are up against these problems of depreciation in a way never before fully realized. In many of the enterprises a correct solution of these problems is a life or death matter.”⁹¹ Another, writing in the December 1906 issue of the recently-formed *Journal of Accountancy*, concluded that, “Depreciation may properly be considered a branch of the accounting science which, as yet, is not sufficiently appreciated.”⁹²

Managements, as one observer put it, generally “regarded depreciation as a joke, and the average director hardly knew the meaning of the word.”⁹³ Years later, another recalled how, at the time, “depreciation was still very largely an academic question except in the largest and most progressive businesses. One argued constantly that depreciation was a fixed element of expense.”⁹⁴ Even though accountants were hired as specialists, the skills they had to offer failed to win them decision-making authority in the business world. More generally, managers often looked down on accountancy as simple bookkeeping, a merely mechanical exercise of transcribing numbers down on a ledger.⁹⁵ This view was echoed by many university administrators who deemed it an unfit subject for higher

⁹¹ Harvey Stuart Chase “The Cost of Maintenance and Depreciation of Cotton Mills” *Journal of Accountancy* 4, no. 3 (July 1907): 237.

⁹² Max Teichmann, “Depreciation” *Journal of Accountancy* 3 (December 1906), p.101. The *Journal of Accountancy* was launched in 1905, its first issue published that November.

⁹³ Willard Hubbard Lawton, “Depreciation, Intangible Values and Rates” *Journal of Accountancy* 17, no. 5 (May 1914): 325-6.

⁹⁴ Victor H. Stempf “Accounting for Fixed Assets” *NACA Bulletin* (April 15, 1938). One example of a progressive company taking obsolescence seriously early in the century was Hygrade Sylvania Corp. of Salem, Massachusetts, whose assistant treasurer in 1938 wrote that, “From its very beginning in 1901, the Hygrade Sylvania Corp, in the electrical manufacturing field, has found obsolescence to be a large and important function in cost determination, the control of fixed assets, and as an element to be considered in financial policy. ...Obsolescence is certainly a cost of doing business that must be included if the profit and loss statement is to represent actual operating results” (John S. Learoyd Jr., “Obsolescence in the Electric Lamp Industry.” *NACA Bulletin*, April 15, 1938).

⁹⁵ For an example of an impassioned defense of the claim that accounting and bookkeeping were “very sharply” distinct, see Max Teichmann’s “A Sketch of Accountancy” *Journal of Accountancy* 13, no. 6. (June 1912): 420-30. By contrast to the state of affairs in America, accountancy had been viewed as a vital profession in Great Britain since the middle of the nineteenth century.

education.⁹⁶ The disdain towards accountancy endured for many years, long enough for Arthur E. Andersen to express before an audience at the Sixth Annual Meeting of the American Association of University Instructors in Accounting, “Seriously, gentlemen, this popular ‘head bookkeeper, fraud-detecting-servant’ conception of our work is a real detriment to our profession.”⁹⁷ For this and other reasons, American accountants were “little known, little recognized, little wanted.”⁹⁸

However, those adamantly seeking to obtain professional status were not dissuaded from revising and strengthening their theories of depreciation. “This is an age of marvelous development,” exclaimed one author in 1908, “and new inventions are constantly revolutionizing methods. . . . The rapidity with which changes take place in the construction of mechanical devices becomes an important factor when considering the subject of depreciation.”⁹⁹ In addition to daily wear and tear as well as any unpredictable acts of god, the common meaning of depreciation was expanded by a growing number of

⁹⁶ Indeed, as late as 1924, Henry Rand Hatfield wrote of the plight of the “poor accounting professor” within the university: “I am sure that all of us who teach accounting in the university suffer from the implied contempt of our colleagues, who look upon accounting as an intruder, a Saul among the prophets, a pariah whose very presence detracts somewhat from the sanctity of the academic halls. . . . The humanists look down upon us as beings who dabble in the sordid figures of dollars and cents instead of toying with infinities and searching for the elusive soul of things; the scientists and technologists despise us as able only to record rather than to perform deed” (“An Historical Defense of Bookkeeping” *Journal of Accountancy* 37, no. 4 April 1924, p241). See also: David F. Hawkins “The Development of Modern Financial Reporting Practices among American Manufacturing Corporations” *The Business History Review* 37, no. 3 (August 1963): 144. Also see Barbara D. Merino “Development of American Accounting from 1876 to 1976.” *The CPA Journal* 46, no. 6 (July 1976): 31-6.

⁹⁷ Arthur E. Andersen, 1922, “The Need of a Broader Conception of the Work of the Accountant,” *The American Association of University Instructors in Accounting Papers and Proceedings of the Sixth Annual Meeting, Pittsburgh, PA, 1921*, Vol. VI, No. 1 (April 1922): 2. Whether Andersen’s plea was heard by his fellow CPAs in other firms is questionable. Take, for example, an article on “Fraud-Sense” on the cover of the May 1926 issue of the *Haskins & Sells Bulletin* (Vol. 9, No. 5). This article encourages the accountant to hone his intuition with respect to “the possibilities of fraud perpetration and concealment,” a skill that “can be developed” so long as he is “quick-witted and astute” and “willing. . . to learn to recognize the signs when he sees them.”

⁹⁸ Robert H. Montgomery, “What Have We Done and How?” *The American Institute of Accountants, 1887-1937, Fiftieth Anniversary Celebration, 1937* (New York, 1938), p. 80.

⁹⁹ Knight 1908, 190.

authors who emphasized supersession or obsolescence—an occurrence that was hard, if not impossible, to predict and that resulted from the introduction of new, cost-saving inventions.¹⁰⁰

Obsolescence, it was argued, was “a true element of depreciation.”¹⁰¹ It had the effect of cutting short the useful life (and therefore value) of equipment already in use and made the need to provide for depreciation accounting all the more urgent. “In order...that capital may not be impaired,” wrote one author in 1914, “a corporation must provide not only for repairs and eventual replacement, but also for depreciation due to obsolescence and inadequacy. New processes are being discovered and new machinery invented. Plants must be placed upon the scrap heap long before they are completely worn out, because they have been superseded by more efficient machinery.”¹⁰² Here, R&D’s indirect impact on accountancy practice starts to appear.

For modern manufacturing, it was no longer enough to simply continue using outdated equipment up to the point where it was finally scrapped.¹⁰³ Routine repairs performed here and there on machinery did not suffice in protecting the value of capital. As Henry Rand Hatfield famously quipped, “All machinery is on an irresistible march to the junk heap, and its progress, while it may be delayed, cannot be prevented by repairs.”¹⁰⁴ Instead, the newly recommended practice was to keep pace with competition

¹⁰⁰ Wade Kurtz *JofAcctncy* 1919, 83; Paul-Joseph Esquerré *Practical Accounting Problems: Theory, Discussion, and Solutions* (New York: The Ronald Press Company, 1922): 281-282.

¹⁰¹ Harvey Stuart Chase July 1907 *JofAcctncy*, Vol. 4, No. 3, “The Cost of Maintenance and Depreciation of Cotton Mills,” p236

¹⁰² (Carl H. Nay, *JofAcc*, June 1914, 424)

¹⁰³ William M. Cole (1908) *Accounts: Their Construction and Interpretation*. Houghton Mifflin Co, 82-84

¹⁰⁴ Hatfield 1909, 121

by introducing improvements to one's property and thereby lifting up its income-generating or earning ability.

By setting aside a portion of earnings to ensure that property remained state-of-the-art, a company could better keep its capital intact and deter bankruptcy.¹⁰⁵ “[U]nless some provision is made [for obsolescence],” wrote the British émigré and resident Price Waterhouse theorist George O. May, “it will be necessary to write off large sums against operations when units are abandoned on account of obsolescence before being exhausted by use. A failure to provide for obsolescence is therefore open to the objection that it...may result in inability to meet requirements for modernizing in the future which may be essential to the continued success or even to the existence of the business.”¹⁰⁶ Planning for obsolescence was preparing for future modernization and, thus, helping ensure that the firm remained viable. This argument only grew more potent over the years as “Technological improvements in productive machinery [came] much faster than [firms could] assimilate them,” explained a partner at another leading auditing firm, Touche, Niven & Co in 1938.¹⁰⁷ “A substantial portion of investment [in productive machinery] must be recovered before business can afford to scrap the old and put in the new.

¹⁰⁵ “The investor will be given a guarantee that he has never had before, because one of the things which have worried him most has been the fear that the capital of the company would be impaired and his investment be rendered of little value. No system of accounts that can be devised will absolutely prevent impairment, but a well devised system [inclusive of depreciation reserves] will go a great way in that direction, and it will certainly keep the amount of actual impairment under safe control. The removal of such risks will not only attract capital but will lead to the obtaining of capital at a lower rate. When an investor himself must provide against depreciation and impairment out of his interest or dividends, he naturally demands a higher [next] rate of return than if he knows of a certainty that the corporation itself has already made ample provision. This inures to the benefit of the public as well”(Nau, “Uniform Systems of Accounts for Public Utilities” June 1914, 424-5, in an address delivered before the Ohio Gas Association, in Columbia OH, March 25, 1914).

¹⁰⁶ George O. May “The Problem of Depreciation” *Journal of Accountancy*, Vol. 19, No. 1 (January 1915), p4

¹⁰⁷ Victor H. Stempf “Accounting for Fixed Assets” *NACA Bulletin* (April 15, 1938)

Managements with real vision recognize this factor of obsolescence.”¹⁰⁸ Capital had to be maintained, and that now meant keeping up with changes in technology. Put differently, the new or improved techniques and devices that emerged from the nation’s R&D laboratories were making it harder and harder to maintain the overall value of a company’s capital stock.

In making this point, May and other British émigrés were adapting for the American context the professional traditions of their homeland. Constituting an elite circle of accountants, their leadership efforts took place both through their auditing practices and through their influence in public accountancy societies.¹⁰⁹ Their views of on capital impairment and its inverse, capital maintenance, were particularly informed by British traditions. Capital impairment, or failing to maintain the original level of invested capital, both physical and financial, was a clear violation of sound accountancy principles inherited from the British tradition.¹¹⁰

In Great Britain, capital maintenance was bound up in the “widespread attitude among investors that accounting income for a period measured the proper amount of resources to be distributed as dividends.”¹¹¹ In this view, dividends were never to be paid by encroaching upon the value of capital, which at all times must be maintained.¹¹² Since,

¹⁰⁸ Stempf *NACA Bulletin* (April 15, 1938)

¹⁰⁹ This began with the American Association of Public Accountants, formed in 1887, and continued with the renamed American Institute of Accountants (AIA) in 1917, predecessor to today’s American Institute of Certified Public Accountants (AICPA) which took its name in 1957.

¹¹⁰ Dean Ardern and Maxwell Aiken, “An Accounting History of Capital Maintenance: Legal Precedents for Managerial Autonomy in the United Kingdom.” *Accounting Historians Journal*, Vol. 32, No. 1 (June 2005): 23-60; George F. Break “Capital Maintenance and the Concept of Income.” *Journal of Political Economy*, Vol. 62, No. 2 (February 1954): 48-62; and Oscar S. Gellein “Capital Maintenance: A Neglected Notion.” *Accounting Historians Journal*, Vol. 14, No. 2 (Fall 1987): 59-69.

¹¹¹ Dale Buckmaster, “Income Smoothing in Accounting and Business Literature Prior to 1854.” *The Accounting Historians Journal* 19, No. 2 (December 1992): 154.

¹¹² The British “capital maintenance” tradition dates back to 1657 when the East India Company was reincorporated as an permanent enterprise. Originally, explain Bruce G. Carruthers and Wendy N.

in financial reports, capital assets were not to drop below their originally recorded book value, depreciation charges—rather than effecting asset values directly—were reported as charges to an expense account which “was used to even out the differential in charges resulting from costs of renewal and maintenance.”¹¹³ Throughout the nineteenth century, however, US courts denied the importance of depreciation as an expense necessary for maintaining the invested capital upon which businesses were incorporated.

Things began to change in 1909 when the US Supreme Court recognized the legitimacy of providing for wear and tear in regulated industries.¹¹⁴ That same year, the concept of depreciation was accepted for the first time in US tax law by way of the corporate excise tax, which authorized a deduction for depreciation in the computation of taxable income.¹¹⁵ Following ratification of the Sixteenth Amendment to the Constitution, the Revenue Act of 1913 inaugurated the nation’s modern income tax laws and included a provision for deducting from corporate gross income “all losses actually sustained within the year and not compensated by insurance or otherwise, including a reasonable allowance for depreciation by use, wear and tear of property, if any.”¹¹⁶ The following year, the Bureau of Internal Revenue offered for the first time a definition of

Espeland, the company was chartered in 1600 “to operate for only a few voyages. At the end of these, dividends to shareholders liquidated the capital and divided up the profits.” Once placed upon a going-concern basis, however, “dividends could be paid out of profits *only* [while capital] had to be preserved to allow the firm to continue operations” (*American Journal of Sociology*, Vol. 97, No. 1, July 1991, 45).

¹¹³ Gary Previts and Barbara Merino, *History of Accountancy in the US* (1998): 125

¹¹⁴ *Knoxville v. Knoxville Water Company*, 212 U.S. 1 (1909)

¹¹⁵ See George O May’s 1936 article, “Influence of Accounting on the Development of the Economy”, p176-177

¹¹⁶ The Sixteenth Amendment which was approved by Congress in 1909, was ratified by enough states to take its place in the US Constitution on February 3, 1913. The Revenue Act of 1913, also known as the Underwood Tariff or the Underwood-Simmons Act, was signed into law by President Wilson on October 3, 1913. Public Law 63-16, Section II.G(b)

depreciation.¹¹⁷ As Brazell et al point out, however, the term “obsolescence” included in that definition referred not to a process of property becoming obsolete but to the actual act of putting obsolete property out of service.¹¹⁸

Despite these changes in the law, few companies had in place a systematic method of charging off depreciation prior to World War I.¹¹⁹ It would have to await the introduction of wartime income tax laws before depreciation accounting became more commonplace among American manufacturers. Starting with the War Revenue Act of 1917, which contained the nation’s first excess profits tax law and introduced high rates of taxation on war profits, a far greater number of businessmen than ever before began scrutinizing their company balance sheets in search of ways to reduce their taxable net income. The result of these tax-savings efforts was that balance sheets previously full of inflated values or “water” were now being wrung dry.

In order to reduce their tax bill, firms quickly wrote down the value of assets by charging depreciation to income while also explicitly recognizing all debts that were due to be paid for out of income. Having served the War Department as a member of its Board of Appraisers and as its representative on the Price Fixing Committee of the War Industries Board, Robert Hiester Montgomery witnessed first-hand such dramatic

¹¹⁷ Depreciation was defined as “the estimated amount of the loss accrued during the year to which the [tax] return relates, in the value of the property...that arises from exhaustion, wear and tear, or obsolescence out of the uses to which the property is put, and which loss has not been made good by payments for ordinary maintenance and repairs deducted under the heading of expenses of maintenance and operation.” Bureau of Internal Revenue (1914) Article 129, p.69

¹¹⁸ Brazell et al, working paper, May 1989, endnote #21, p.60

¹¹⁹ As William Kemp explained in his *Departmental and Standard Costs* (published by NACA, in NYC, September 1923), “Plant equipment was inventoried occasionally for insurance purposes, and the amount of depreciation charged off annually was governed largely by the amount of profits for the period.(Kemp,8)

“pruning” of financial reports and declared it “a revolution” in practice. Coming from the likes of Montgomery, such statements were not to be taken lightly.

Born in Pennsylvania to a Methodist minister in 1872, Montgomery began work at an early age to help support his ailing father. At 14, he took his first job as an office boy for a public accountant in Philadelphia and learned enough on the job to be made partner in 1896. Three years later, Montgomery and three other members of the firm broke off to found their own partnership, Lybrand, Ross Bros. & Montgomery, in 1898. Passing the CPA examination in 1899 and the New York Bar in 1904, Montgomery practiced both accounting and law as head of his firm’s New York office.¹²⁰ After helping organize the first ever International Congress of Accountants in 1904, Montgomery contributed a piece to the first issue in November 1905 of *The Journal of Accountancy*, a professional journal for public accountants that Montgomery had helped bring into being. That Winter, he began teaching night classes in accounting at New York University, and five years later, offered the first course in accounting ever offered by Columbia University. By 1912, he was appointed instructor in Columbia’s Department of Business while beginning a two-year term as president of the AICPA. In short time, Montgomery was promoted to assistant professor in 1915 and full professor in 1919.

Even with such breadth of experience as an accounting professional, academic, and administrator, Montgomery was taken off-guard by the sudden change of heart that wartime revenue laws had elicited from the nation’s businessmen. “The assets side [of a given balance sheet],” he observed, “was scrutinized by the boss himself before the books

¹²⁰ See Friedman (1985/[1973]), 305 for a discussion of open entry to the bar, which made it possible for men like Montgomery to practice lawfully.

were closed and, if there was the slightest indication of overvaluation, ruthless cuts were made in the book figures. Plant accounts were written down to the lowest possible point by liberal depreciation charges and by reductions in amortization or obsolescence,”¹²¹ with “amortization” referring to the process of allocating the cost of patents and other so-called intangible assets over a period of time. In summary, explained Montgomery, “All possible liabilities were set up in the books.”¹²²

Montgomery, while hopeful in 1919 that “bankers should insist on a continuance of the methods of the last few years,” acknowledged that the country still had a long way to go before financial statements would present as meaningful documents to outside investors, regulators, and creditors. “In the great majority of cases,” he argued, “balance-sheets are still far from being a true picture of financial conditions.”¹²³ Citing his war work with the Price Fixing Committee, Montgomery recalled being “amazed at the tremendous differences which existed” in balance sheets of firms even within the same industry:

Even when the costs of production were somewhat similar, balance-sheet valuations were widely apart. This applied not only to a few industries, but to most industries. We were told by the leaders of various industries that a certain amount of capitalization could be counted upon in relation to a given unit of production or capacity. But few concerns reflected any such uniform or standardized figures on their balance-sheets.¹²⁴

Many firms, he added, did not even demonstrate a practice of accounting for regular depreciation, not to mention depreciation due to obsolescence.

¹²¹ Robert H. Montgomery “Influence of the War on Balance-sheets” *Journal of Accountancy* Vol. 28, No. 1 (July 1919): 3

¹²² Montgomery (July 1919): 3

¹²³ Montgomery (July 1919): 5

¹²⁴ Montgomery (July 1919): 5

Finally reaching President Wilson's desk in February 1919, it was the Revenue Act of 1918 that explicitly included a deduction for depreciation arising not only from wear and tear, but from obsolescence as well. For the first time, obsolescence was viewed, in part, as something distinct from the decreases in property values owing to exhaustion through use. This meant that even when property was kept in repair, it was still to be subjected to a depreciation allowance. All of this signaled a growing awareness as to the impact of industrial research, even if only indirectly through the effect of obsolescence. According to Brazell et al,

The Bureau defined obsolescence as 'the gradual reduction in the value of property due to normal progress of the arts in which the property is used, or to the property becoming inadequate to the growing needs of the trade or business.' ... A reduction in value caused by economic or other factors, which could be anticipated with the same degree of accuracy as wear and tear or corrosion, was referred to as 'normal obsolescence' and was to be reflected in the allowance for depreciation. However, obsolescence (without the 'normal' qualifier) generally referred to a reduction in the useful life brought about by some radical change in circumstances that was not anticipated when the asset was placed in service. Obsolescence was to be evidenced only by a demonstrable and certain shortening of an asset's anticipated useful life.¹²⁵

It was barely a year after the enactment of the Revenue Act of 1918 when Montgomery reminded his audience in the *Journal of Accountancy* that the "attitude of the treasury department in the matter of federal taxation during the years 1909 to 1917 was not helpful.... The agents of the department were constantly disallowing depreciation and amortization charges; allowances for obsolescence were stricken out, and, in general, business men were encouraged to carry their assets on their books at inflated values."¹²⁶ This was the norm in government. As for business, "most managements did not seem to

¹²⁵ Brazell et al, working paper (May 1989): 5

¹²⁶ Montgomery (July 1919): 2

care about public opinion [including the opinions of their own small-scale shareholders],” and so set up reserves for depreciation only when volatility in reported income seemed to demand it.¹²⁷ With reference to the period before WWI, Henry Rand Hatfield recalled that, “any recognition of depreciation was relatively uncommon in the accounts of American corporations, and the relatively few companies that showed depreciation in prosperous years grew fainthearted when business was poor.”¹²⁸

Accountants thus celebrated the emerging change of attitude in government and business alike; “The recognition of depreciation as a concrete, inescapable fact—not merely as a more or less interesting academic fad with accountants—has had a long struggle for existence in this country, but today the manager who still argues that as long as plant is kept in good repair there is no depreciation is in a hopeless minority, and his friends are apt to pity him as a ‘hopeless case!’”¹²⁹ The question that remained to be worked out, however, was how best to treat the cost of incorporating new inventions into existing equipment.

Should all sums relating to work done on property receive treatment as capital expenditures? In other words, did routine maintenance qualify as a capital expenditure chargeable to depreciation, or should it come directly out of income earned through operations? What about repairs that did more than maintain the operation of the

¹²⁷ David F. Hawkins, p.147. “The Development of Modern Financial Reporting Practices Among American Manufacturing Corporations,” *Business History Review* (Autumn 1963): 135-68.

¹²⁸ Henry Rand Hatfield. *Accounting, Its Principle and Problems* (1927): 140.

¹²⁹ May 1917 issue of the *Journal of Accountancy*, by Willard Hubbard Lawton (p.325). The change in attitudes was evident even sooner in the Summer of 1914 when A.P. Richardson, an editor of the *Journal of Accountancy*, declared, “Now, however, a great change has come to pass and the public insists not only that there shall be depreciation but in many cases that the depreciation shall be of a fixed amount or percentage. The expression of this insistence is found in the rulings of sundry intrastate bodies of control” (“Depreciation of Public Utilities” *Journal of Accountancy* Vol. 17, No. 6 (June 1914): 437).

property? If, for example, work done on certain components of equipment resulted in a cheaper or more productive manufacturing process, did this qualify as a capital or a revenue expenditure? And if so, did the capital nature of such efforts extend to R&D, an activity that, into the second decade of the new century, was increasingly recognized by accountants as an important source of cost-saving, production-enhancing inventions?

2.3. Capital Expenditures vs. Income Expenditures

The arrival of research and development on the industrial landscape complicated what was already a treacherously difficult problem, namely: sorting business expenditures either into capital asset accounts or into income expense accounts. In the first American edition of his textbook, *Auditing: A Practical Manual for Auditors* (1905), the renowned British chartered accountant Lawrence Dicksee emphasized the “primary importance” of the “distinction between Capital Expenditure and Revenue Expenditure.”¹³⁰ The following year, John P. Herr of Pennsylvania insisted that, “One of the most difficult things with which the accountant has to deal is the question of capital expenditures.”¹³¹ Leo Greendlinger, writing from New York City in 1911, agreed: “Nothing in accountancy requires more emphatic distinction than the expenditure of money as capital or as

¹³⁰ The first edition of *Auditing* was published in Britain in 1892. Its impact on the accounting profession was profound. In 1902, Dicksee became the first Professor of Accounting appointed to teach at the University of Birmingham, and taught accounting as a lecturer at the London School of Economics that same year. Dicksee was a prolific author of many texts including, *Goodwill and Its Treatment in Accounts*, a text co-authored with Frank Tillyard, a barrister at law at Birmingham, which entered its third edition in 1906. For four years, Dicksee worked simultaneously in university and private practice before resigning from Birmingham in December 1906. He continued to teach at the LSE until his retirement in 1926, and passed away at the age of 67 in 1932.

¹³¹ John P. Herr “The Appreciation of Assets—When Is It Legitimate?” *Journal of Accountancy* Vol. 3, No. 1 (November 1906): 11

revenue.”¹³² By that time, a consensus among accountants held that expenditures that *expanded* a company’s capital base qualified as charges to capital, whereas those that merely *maintained* capital were properly chargeable to revenue.

In 1911, for example, the Chicago-based public accountant Charles Heinrichs Langer insisted that, “Capital Expenditures are not only the original cost of a fixed asset, but also those which add a distinct value to an asset already owned. If this addition is an entirely new thing, the whole cost is chargeable as a capital expenditure, but if it merely the substitution of a dearer for a cheaper thing, only the difference in cost should be so charged.”¹³³ In 1907 and 1908, a question appearing on state C.P.A. examinations asked, “What general principles should be observed in differentiating between capital and revenue expenditure?”¹³⁴ The answer suggested by the *Journal of Accountancy’s* “C.P.A. Question Department,” was a clear echo of the discussion that appeared only a couple years earlier in Dicksee’s *Auditing*:

‘Has the particular expenditure incurred in any individual case been incurred for the sake of improving the earning capacity of the undertaking?’ If the answer to this question is in the affirmative, then, and to that extent, the expenditure in question is capital expenditure.

But if it has only had the effect of putting the earning capacity of the undertaking upon the same footing as that which had previously obtained (and which has since declined by the ordinary process of wear and tear, or the effluxion of time, in respect of which no provision has been made), it must be charged against revenue.

The precise meaning of this latter qualification is that the mere renewal of wasting assets, not otherwise provided for, cannot be called capital expenditure, but that any extension, or the acquiring of fresh assets, is in the nature of capital expenditure.¹³⁵

¹³² Leo Greendlinger *Accounting Theory and Practice* (1911)

¹³³ Charles Heinrichs Langer, *General Accounting* (1911), Chapter 5: 2-3.

¹³⁴ Greendlinger (1908), 78.

¹³⁵ (Dicksee 1905, 82-83).

Greendlinger was thus one of many accountants who believed that, “Not mere renewals but only the extension or acquisition of new assets can be recorded as capital expenditures.”¹³⁶ His concern on this matter involved practices that would later become colloquially known as “cooking the books.”¹³⁷ He provided the following example to illustrate:

A corporation makes repairs and improvements which do not increase the earning capacity of the company, but are actual replacements of assets wasted during operation. Due to business depression the board of directors fear that they will not be able to declare the usual yearly dividend. But withholding the dividend, might cause serious fluctuation of the market quotations of the company’s stock. So they order such repairs and improvements charged to capital, treating it as though it were an acquisition of property and hence an asset. Such procedure leaves the corporation a surplus which may be used for the payment of dividends.¹³⁸

By carefully defining the boundary between capital and income expenditures, many hoped company directors could be disciplined against the temptation to manipulate asset values. Herr offered his own example of how the task of distinguishing between capital expenditures and charges to income, had gone wrong:

As capital expenditures are generally handled at the present item, a manager who has an interest in the profits, or who for some other reason desires to make a temporary good showing, may, by adopting this improper method of capitalizing expense items, and covering up his tracks by looseness or lack of system, show greatly inflated results, cover up shrinkages in the net earnings, losses by bad management, and defalcations, and make it practically impossible for an accountant, unless he combine a very good practical knowledge of values and makes a physical inspection of the fixed assets, to determine whether the charges are for bona fide betterments or not.¹³⁹

¹³⁶ Greendlinger 1911, 355

¹³⁷ “Some corporations juggle their accounts by charging to capital expenditures what properly should go to revenue expenditures, or vice versa, depending on what they intend to conceal.” Greendlinger 1911, 353.

¹³⁸ Greendlinger 1911, 354

¹³⁹ Herr (November 1906), 12

All in all, accountants made constant reference to betterments, improvements, renewals, replacements, and extraordinary repairs in their efforts to articulate a coherent theory of depreciation inclusive of obsolescence. However, even among these more precise categories, little consistency of meaning could be discerned in their use.¹⁴⁰ As accounting historian Dale Buckmaster observed, “The literature indicates that, to say the least, firms were extremely flexible in capitalize/expense decisions for plant and equipment-related costs.”¹⁴¹ Improvements, for example, might refer to the installation of all new equipment or merely to a single component. Accountants also differed as to whether such improvements occurred regularly enough to warrant treating their costs as ordinary expenses, in which case they would be chargeable to income as part of the usual cost of doing business. Infrequent improvements, by contrast, were chargeable to capital—but only if the costs were substantial enough that, if currently expensed, would distort reported income. In this regard, accountants particularly struggled with handling the costs of new component parts that, while not large enough to distort income, occurred regularly enough to be charged thereto.

¹⁴⁰ The problem of a lack of consistent meaning and vocabulary lingered on for many years. “In spite of all that has been written in recent years with regard to depreciation,” wrote a University of Michigan accounting professor in late 1926, “confusion still persists”(31). (H.F. Taggart, “Cheaper Depreciation” *Acctg Review*, Vol. 1, No. 3 (September 1926): 31-44. The following year, Robert H. Montgomery lamented “the misleading and inaccurate terminology in wide use at the present time”(“Reviewed Works: *Accounting-Its Principles and Problems* by Henry Rand Hatfield” *The Acctg Review*, Vol. 2, No. 2 (June 1917): 189-193). (JS Robinson “Some Doubtful Elements of Cost”, pp80-88). In 1943, Earl A. Saliers expressed frustration with the fact that, “The word ‘repairs’ has been used with so many varying meanings that its place in the terminology of accounting is questionable. Sometimes it is used synonymously with ‘maintenance’; sometimes synonymously with ‘renewals’; sometimes in a sense different from either maintenance or renewals. Reference to a number of industrial reports indicates considerable use of the term ‘maintenance and repairs.’ It seems that the use of this expression is not the result of careful reasoning but of the convention of convenience and a tendency to follow established custom”(p259, of Saliers’ “Theory of Repairs, Maintenance, and Betterments” in the *Accounting Review*, Vol. 18, No. 3, July 1943).

¹⁴¹ Dale Buckmaster “Income Smoothing in Accounting and Business Literature Prior to 1954” *Accounting Historians Journal*, Vol. 19, No. 2 (December 1992): 157

Herr, for instance, was quick to emphasize that management decisions to purchase “all new construction and equipment” were, for accountants, “not so difficult to deal with as replacements or what may be called ‘part’ construction.”¹⁴² A couple years later, William A. Lybrand, founding partner of Philadelphia-based public accounting firm, Lybrand, Ross Bros. & Montgomery, made the same observation before an audience at the annual meeting of the American Association of Public Accountants in October 1908. After defending the practice of capitalizing the cost of additions to plant or property, Lybrand argued that the “large class of expenditures consisting not of certain definite additions, but rather of alterations and improvements, is more difficult to deal with.”¹⁴³

Two university instructors explicated this difficulty in their jointly authored work, *Principles of Accounting*. First published in 1916, this text would help launch the careers of William A. Paton and Russel A. Stevenson, with Paton soon to become one of the nation’s preeminent accounting theorists. In the 1918 edition of *Principles*, Paton and Stevenson argued,

[T]here are in many cases practical difficulties in the way of determining just where to draw the line between repairs and replacements, and improvements or betterments. In connection with the upkeep of a complex piece of property, where there is not test of market price available, it may be almost impossible to determine accurately which outlays are actually expenses and which are capital charges. Repairs (so-called) to a factory building, for example, may be so extensive as to actually increase the value of the building, but it may be very hard to determine just what is the amount of the improvement. Arbitrary estimates and rules are necessary in such cases. ... Approximate accuracy is all that can be hoped for in most instances.¹⁴⁴

¹⁴² Herr (November 1906), 11

¹⁴³ Lybrand (December 1908), 116

¹⁴⁴ Paton and Stevenson (1918), 235-6)

Lacking the confidence of knowing whether a given expenditure did or did not enhance the value of capital assets on hand, the best an accountant could do was to approximate the boundary between capital and income. After reviewing the positions taken by Dicksee, Greendlinger, Langer, Lybrand, Herr and others, A.J.B. Schmidt in 1915 summarized “several distinct accounting tests commonly accepted, as to whether an expenditure is chargeable to capital or revenue accounts.” What Paton and Stevenson referred to as “arbitrary estimates and rules,” Schmidt presented as “commonly accepted” tests. These he listed as follows:

Test 1. Does the expenditure increase the capacity or earning power of the plant or undertaking; if so charge to a capital or asset account; if not, charge to revenue.

Test 2. Is the expenditure in the nature of repairs or does it really add to the value of the plant as an income producing good; if the first, it should be charged to revenue, and if the latter it is permissible to capitalize the expenditure.

Test 3. Is the expenditure applicable to the cost of producing the revenue or income for this period or is it in the nature of a prepaid expense, the benefit of will not be reaped in the income until some subsequent period. If the latter, that proportion of the expenditure applicable to such, can be capitalized until such time as it has served its purpose.

Test 4. Is the expenditure in the nature of the renewal or replacement of an asset owned and depreciated in value as an income producing factor of capital or is it an addition to the income producing capital assets. If it falls in the latter category of outlays, it becomes an addition or betterment and is a permissible charge to capital accounts.¹⁴⁵

As Herr observed in 1906, “Considerable differences of opinion exist among accountants concerning this question” of when an expenditure qualified as a charge to capital or to revenue.¹⁴⁶ Test such as those enumerated by Schmidt helped establish practical working guides that accountants could selectively deploy in service of the various

¹⁴⁵ (Schmidt 1915, 289-290) “‘Capital and Income’ – ‘Capital and Revenue’” in *Journal of Accountancy* October 1915

¹⁴⁶ Herr (November 1906), 12

contexts in which they worked. Such tests operated more as rules of thumb, the reasoning behind each one neither perfectly tying into nor entirely nullifying all others.

2.4. Anticipating and Smoothing Income

As we saw in section II of this chapter, the wartime revenue acts authorized depreciation as an expense deductible from, and thereby reducing the amount of, taxable income. The Treasury, responsible for translating Congressional intent behind each revenue act into sets of practicable regulations, helped popularize the use of reserves as a means of accounting for depreciation. In response, one observer in 1918 argued that the “latest regulations of the treasury department make a more consistent set of rulings regarding depreciation and obsolescence than have existed heretofore.”¹⁴⁷ What the regulations authorized, with accountants’ approval, was the leveling effect upon income made possible by accounting for depreciation through the use of reserves.¹⁴⁸ Depreciation was “intended to spread, equitably over the life of the equipment, its original cost” as well as “the cost of such renewals and replacements as are only required at extended intervals, which, if charged directly into expense when made, would result in one period bearing more and another period bearing less than its fair share of the cost.”¹⁴⁹ By setting aside a depreciation reserve out of income, replenishing it annually, and charging the cost of

¹⁴⁷ Henry B. Fernald “Depreciation and Obsolescence as Governed by Federal Income Tax Regulations” *Journal of Accountancy* Vol. 25, No. 4 (April 1918): 241.

¹⁴⁸ Depreciation reserves helped “level the expenses by apportioning to each period its just share of the expense”(Wade Kurtz, February 1919, 82).

¹⁴⁹ Fernald (April 1918): 241-2

obsolescence thereto, firms were in a better position to reduce fluctuations in income from period to period without, at the same time, impairing capital.¹⁵⁰

In Treasury regulations, then, depreciation accounting helped articulate both a boundary and a bridge between capital and income, while at the same time blurring the experiences of the present with expectations for the future. Put differently, while the *method* of depreciation accounting was future oriented, in its anticipation of losses owing to obsolescence, the empirical *fact* of depreciation could only be realized in the present, in the very act of retiring an obsolete item of property. Through the medium of the depreciation reserve, companies could “include in current expenses the proportion of future losses which [were] chargeable against current earnings.”¹⁵¹ This works very well in theory, noted the eminent American accounting theorist Henry Rand Hatfield, but in practice there remains the problem of anticipating a rate of obsolescence—that is, a rate at which new inventions would appear to make existing equipment obsolete. It was this rate that helped determine just how much income managers ought set aside each year to cover the effects of depreciation on capital assets. Any more than was necessary might be seen as managements overstepping their bounds; any less, as a failing of prudent managerial foresight.

Knowing how long (the value of) a given piece of equipment would last before becoming obsolete (and thereby losing its value) “is confessedly vague and indefinite,”

¹⁵⁰ As Earl P. Saliers explained it in 1912, “it frequently happens that some part must be renewed which, if charged to current revenue, would be an uncommon burden. It then becomes proper to arrange that cost of renewal which will last over several years [so that it] may be charged to revenue during those years, instead of making the entire charge to current revenue.” Earl A. Saliers, “Caring for Depreciation” *Journal of Accountancy*, Vol. 13, No. 4 (April 1912), 242

¹⁵¹ “Current expenses,” continued Cowan, “are made to include the present proportion of estimated future losses by the operation of the reserve, since it is created by charging current maintenance and crediting the reserve with the present proportion of such future losses (Cowan 1917, 99-100).”

acknowledged Hatfield, “and implies the ability to calculate the future activities of inventive genius.”¹⁵² Still, he added, the process of invention was, “of course, constantly taking place.”¹⁵³ Nevertheless, Hatfield’s assumption that it was “constantly taking place” should not be read to imply that R&D regularly generated new products and processes in anything even approaching a *predictable* fashion. Thus, even if the activity of R&D was ongoing, individual research projects could result in nothing more than unprofitable dead ends.

If Hatfield had any reservations about anticipating, for depreciation purposes, the timing of potential R&D *output*—i.e., the arrival of new, more efficient, cost-saving equipment—his initial attitudes toward accounting for the expense of *performing* R&D were even more conflicted. In his landmark text *Modern Accounting: Its Principles and Some of Its Problems* (1909), Hatfield, a man known to be “the most significant American writer on accounting of the [early twentieth century] period,”¹⁵⁴ referred to R&D or “experimental expenses” as “now a recognized part of many industrial plants.” He believed such expenses could very well constitute a part of the general expense of a firm, not to be charged to capital. By convention, general expenses referred to quickly-consumed supplies, employee salaries, and any other routine expenses which were incurred and charged within one and the same fiscal period. Sometimes these charges were included in general or administrative overhead, other times directly through the income or profit and loss account. In any case, expenses were understood to be recovered through earnings of the current period. In keeping with this general treatment, the salaries and supplies of

¹⁵² Hatfield 1909, 140

¹⁵³ Hatfield 1909, 140

¹⁵⁴ Reed K. Storey, 1959, p235

R&D employees were suitably treated as elements of general business expenses. But perhaps, wondered Hatfield, another treatment was possible.

He considered a scenario in which a firm has hired a researcher whose efforts as an employee ultimately lead to an invention worth patenting. His salary and expenses, argued Hatfield, “seem to be the cost of the secured invention just as truly as the price paid for the patent right. If this is so,” he wondered, “may not expenses [incurred along the way] be counted as part of the prospective cost even though the goal has not been quite reached?”¹⁵⁵ Without taking a clear position either way, Hatfield used the example of experimental expense to demonstrate “that it is by no means easy to lay down a rule by which to determine whether certain charges are to be treated as expense or whether they are to be held in the Balance Sheet as representing the cost of assets.”¹⁵⁶ Holding experimental expenses in the balance sheet in anticipation of a new discovery—a discovery that could be developed into an asset, such as a patent or a more efficient piece of equipment—involved the deferred expense method. In this method, an amount spent on R&D in the current period was not immediately recovered out of income, but was held, instead, in a sort of limbo, its absorption out of income being postponed to some future period. Only when an experiment finally gave way to an asset (either a profitable product, patent, or cost-saving process) would the deferment come to a close, the expenditures taken out of limbo, and the total amount capitalized by way of the new asset.

¹⁵⁵ Hatfield 1909, p77

¹⁵⁶ Hatfield 1909, 78

Postponing the absorption of R&D over multiple periods thus seemed to offer yet another means of maintaining an even distribution of income from one period to the next. As with the strategy of accounting for obsolescence, the use of deferred charges relied on the anticipation of future income in order to facilitate a process described in post-WWII years as “income smoothing.”¹⁵⁷ Whether through the method of creating “secret reserves,” charging more depreciation in times of business prosperity than in times of depression, or through the vehicle of deferred charges, income smoothing minimized the appearance of “distortions” to reported income and thereby allowed management to make a steady showing, year in and year out, to their investors, creditors, and outside observers.¹⁵⁸ Compare, for example, Hatfield’s early discussion of depreciation with his discussion of deferred charges. With regards to the former, he argued that the “immediate effect of allowing for depreciation is properly to equalize profits during different years. Otherwise the total cost of the machine must appear as an expense of the year when it finally proves unserviceable.”¹⁵⁹ Similarly, with regards to deferred charges, he insisted their use was to “provide for the proper adjustment of net profits as between different fiscal periods...sometimes called Anticipation accounts or Adjustment accounts.”¹⁶⁰

We encounter more income smoothing discourse a few years later in Paton and Stevenson’s *Principles of Accounting* (1916), which insisted that it “occasionally seems advisable” to “treat certain extraordinary expenses as items applying not solely to the

¹⁵⁷ “Modern authors,” explained one accounting historian, “generally take the broad-based, flexible view that income smoothing is management action taken with the intent of reducing the volatility of publicly reported accounting income”(Buckmaster 1992, 147-8).

¹⁵⁸ Buckmaster (December 1992): 166.

¹⁵⁹ (Hatfield 1909, 123)

¹⁶⁰ (Hatfield 1909, 119).

particular period in which the expense takes place, but to several periods.”¹⁶¹ To make their case, Paton and Stevenson used the example of pieces of equipment suddenly becoming obsolete, giving rise to the need for replacing them “with the improved types.”¹⁶²

The management may with reason argue that this is an unusual expense, unlikely to occur again, or at least not oftener than once in several years. Consequently it would seriously distort the...resulting net income figure to charge the entire amount into the expense of a single year. The stockholders must be protected, and it is decidedly to their advantage from the standpoint of dividends that net income be a fairly constant amount. Therefore, it is decided to charge but [a portion] of this expense against the revenue of the current year, and to carry [the remainder] on the asset side of the balance sheet as a “deferred” expense.¹⁶³

In both the first (1916) and second (1917) editions of their textbook, Paton and Stevenson concluded that, “The desire to preserve financial standing, and a reasonably even flow of income is the legitimate excuse for such deferred items.”¹⁶⁴ Clearly, then, an overlapping logic of income smoothing is evident as one of the justifications benefitting deferred expensing no less than in depreciation reserve accounting.¹⁶⁵

Over each edition of their textbook, Paton and Stevenson would begin teasing out important differences in the use of the deferral method that had yet to compel the scrutiny of Hatfield and other writers in the preceding decade. They noted how all deferred expenses were prorated over multiple periods because the benefit of the good or service paid for was consumed gradually over time. One key difference came in the case of mining companies which, through normal operations, required several years of

¹⁶¹ Paton and Stevenson 1916, 98

¹⁶² Paton and Stevenson 1916, 98

¹⁶³ Paton and Stevenson 1916, 98

¹⁶⁴ Paton and Stevenson 1916, 99 and Paton and Stevenson 1917, 207

¹⁶⁵ Earl A. Saliers *Principles of Depreciation* (NY: The Ronald Press Company, 1916) emphasized depreciation as a method for allocating costs to periods of benefit.

“overstripping” the earth *before* valuable ores could be excavated and sold. Until that point was reached, the mine would earn the company no revenues at all, and in the interim, costs of overstripping were carried on balance sheets as deferred expenses. However, unlike the case in which suddenly-obsolete equipment is updated with “improved types,” the deferred mining expenses were more in the nature of assets, specifically, of intangible property. Paton and Stevenson picked up this discussion in a separate chapter on Intangible Assets. In that chapter, Experimental Expenses were included as one example of deferred expenses that, in time, give way to intangible assets. From the 1916 to the 1918 edition, however, the authors doubt about this analogy between mining and R&D began to reveal itself. In the 1916 and 1917 editions, they wrote:

Suppose a firm maintains a department for the purpose of making tests and experiments with a view to developing new processes and methods. Are such expenditures properly chargeable to an intangible asset account, or should they be considered an expense? In so far as such a department is a permanent adjunct of the business, necessary to keep the firm abreast of competitors, such expenditures are clearly expense outlays.

If the experiments can reasonably be expected to result in profitable processes, the costs can be charged to capital outlay and the process itself or the patents representing it can be considered an intangible asset. Such a situation is unusual, however, and the proper analysis in such a case seems to be to charge these expenditures to expense; if the experiments result favorably it will be time enough to revise the analysis.¹⁶⁶

In the 1918 edition, the above passage is removed from the Intangible Assets chapter and transplanted onto the tail end of the discussion on deferred charges. Already in the 1917 edition they added an extra warning: “We must be careful to distinguish between the two

¹⁶⁶ Paton and Stevenson 1916, 174 and Paton and Stevenson 1917, 284

types of deferred expense items discussed in this section.”¹⁶⁷ It wasn’t until 1918, however, that the distinction is more clearly made between deferred expense (or future loss), on the one hand, and deferred assets (or future gain), on the other. In the last edition, the authors introduced a few subtle changes to the passage above, which appear in italics below:

Although the general distinction between an actual deferred asset and expense or loss is clear, it is sometimes hard to draw the line in specific cases. Suppose, for example, that a firm maintains a department for the purpose of making tests and experiments with a view to developing new processes and methods. Are the necessary expenditures properly chargeable to an asset account, or should they be considered an expense? In so far as such a department is a permanent adjunct of the business, and is necessary to keep the firm abreast of competitors, such expenditures are clearly expense outlays—a current cost of operations.

It is often urged, however, that if the experiments can reasonably be expected to result in a profitable process the costs should be carried as a deferred asset in the balance sheet. In general it would seem more reasonable to charge such outlays against current revenue—unless the net revenue figure is thereby seriously distorted.

These changes emphasize (1) that R&D is a particularly difficult case to distinguish between an asset (of capital) and a loss (of capital), (2) that R&D is a necessary expenditure, (3) that R&D is best handled as a current cost of operations, and (4) that the distortion of income can have priority over all other considerations, if need be.

With these emphases, Paton and Stevenson articulated a general rule of current expensing that had two main reasons to recommend it. First was their observation that many firms had begun maintaining R&D departments in the hope that, by doing so, they could stave off losing ground to their competitors. By committing earnings in this way, firms were engaging in an activity that seemed as if it was quickly becoming the new

¹⁶⁷ Paton and Stevenson 1917, 208

normal for their industries. History shows this clearly to have been the case with the chemical and electrical industries, for example, in which maintaining separate R&D laboratories had become a necessary cost of doing business—and therefore, by accounting convention, a current cost of operations. Second, although acknowledging the argument that R&D might “reasonably be expected to result in a profitable process” or product, Paton and Stevenson recommended a “more reasonable” conservatism in predicting the outcome of R&D. Instead of hoping R&D will always or else predictably generate a profitable product, the more prudent alternative is to expense the costs out of current period income, and only resort to capitalization if and when an asset was actually generated.

Nevertheless, Paton and Stevenson did not close the door entirely on deferment: should absorbing R&D costs as incurred “seriously distort” current period income, managements would be wise to postpone the recovery of those particular costs until some future point in time when funds were more readily available for that purpose. However, this single argument in favor of deferral was rooted in the logic of income smoothing, rather than in a logic which equated R&D expenditures with successful R&D outputs.

2.5. Another Turn of the Screw

As with accounting for the effects of depreciation and obsolescence, the difficulty in handling R&D expenditures rested in locating where along the capital-income boundary these expenditures would fall. If R&D expenditures represented the acquisition of assets, then they should fall on the side of capital. If not, they were to be accounted for as expenditures of income. Passed into law only one month after ratification of the

Sixteenth Amendment to the Constitution,¹⁶⁸ the Revenue Act of 1913 effected another turn of the screw. Inaugurating the nation’s modern income tax laws, the Act codified a distinction between (a) ordinary and business expenses, which taxpayers could deduct from the income to reduce taxes owed,¹⁶⁹ and (b) capital expenditures, which were unallowable as deductions from income for tax purposes.¹⁷⁰ In contradistinction to ordinary expenses, capital expenditures were those that produced an asset lasting beyond the current tax period. This has become known more recently as “the asset capitalization rule,” and it developed through the opinions of the courts and of the Treasury, this rule pointed up the difficulty of apportioning R&D expenditures either to capital, income, or some combination thereof.

Following the Revenue Act of 1918, the Treasury in 1919 promulgated Regulation 45 which granted taxpayers either of two alternatives in the treatment of R&D costs. These costs could be (a) deducted, or “written-off,” from income as incurred, or (b) treated as capital assets.¹⁷¹ Not long after, however, the Treasury narrowed down

¹⁶⁸ This amendment allowed Congress to levy an income tax *without* apportioning it among the States or on the basis of US Census data—strategies of revenue collection which the Supreme Court had previously ruled unconstitutional in the case of *Pollock v. Farmer’s Loan & Trust Co.* (1895). In that case, income taxes on rents, dividends, and interest were judged to be forms of direct taxation. Yet, while ratification of the 16th amendment thereafter exempted income taxes from the constitutional requirements attending direct taxation, it did so without defining “income.” This task then fell to Congress or the courts, and failing the efforts to the former, the legal definition of income that evolved over the ensuing years had to be worked out through the judicial system. Most notable here are the cases of *Merchants’ Loan & Trust Co. v. Smetanka* (1921) and *Eisner v. Macomber* (1920).

¹⁶⁹ Revenue Act of 1913, ch. 16, § II(B), 38 Stat. 114, 167, providing that “in computing net income for the purpose of the normal tax there shall be allowed as deductions: First, the necessary expenses actually paid in carrying on any business”

¹⁷⁰ Revenue Act of 1913, ch. 16, § II(B), 38 Stat. 114, 167, providing that “no deduction shall be allowed for any amount paid out for new buildings, permanent improvements, or betterments, made to increase the value of any property.”

¹⁷¹ “A taxpayer who has incurred expenses in his business for designs, drawings, patterns, models, or work of an experimental nature calculated to result in improvement of his facilities or his product, may at his option deduct such expenses from gross income for the taxable year in which they are incurred or treat such articles as a capital asset to the extent of the amount so expended.” This clause reappeared in Regulations 62 and 65, under Article 168.

alternatives available to taxpayers in reaction to two court cases of 1925 that had come before the newly-formed Board of Tax Appeals.¹⁷² Believing that regulations had inadvertently made it possible for companies to “take a second trip to the well,” the option to deduct expenses as incurred was stricken from the regulations of 1926. Nevertheless, the Treasury’s own Bureau of Internal Revenue (today’s Internal Revenue Service) continued allowing the expense option to certain taxpayers who could demonstrate having an established practice of accounting for such costs.

The courts, on the other hand, went their own way. In general, while the Bureau often allowed the deductions as ordinary and necessary expenses, the courts usually treated the expenditures as being capital in nature. In disputes over taxpayers’ rights to treat R&D as a business expense, courts almost always adhered to the asset-capitalization rule and denied the deduction.¹⁷³ Still, courts struggled with such questions as: Did capitalization depend on the taxpayer’s subjective intention or purpose for engaging in R&D in the first place? At what point do R&D activities result in an identifiable asset, the costs of which must be capitalized? If ongoing research projects lasted for several years, how should costs be treated if the project ends up only partially succeeding, or else failing entirely? How about in those cases where an asset is generated on the basis of multiple projects pursued in parallel?¹⁷⁴

¹⁷² See: *Gilliam Mfg. Co. v. Commissioner*, 1 B.T.A. 967, 970 (1925); *Goodell-Pratt Co. v. Commissioner*, 3 B.T.A. 30, 33-34 (1925). The issue in these cases involved expenditures that were deducted from income during the excess profits tax years during the war, only to be later capitalized in the form of patent costs, thereby reducing gain on later sales. Although the Board of Tax Appeals sanctioned this double tax benefit, Treasury officials worried about the observable shift in taxpayers’ accounting practice from expensing to capitalizing R&D expenditures.

¹⁷³ See, e.g., *Claude Neon Lights, Inc. v. Commissioner*, 35 B.T.A. 424 (1937); *Hazeltine Corp. v. Commissioner*, 32 B.T.A. 110 (1935), *affd*, 89 F.2d 513 (3d Cir. 1937); *Clem v. Commissioner*, 10 T.C.M. (CCH) 1248 (1951).

¹⁷⁴ Two years later, the Board of Tax Appeals had the occasion to define a capital expenditure as one which results

In theory, the accounting category of deferred charges could resolve these questions. Two general cases help to demonstrate. In the first case, R&D expenditures are deferred to the future until the point of capitalization, i.e. the transformation into an income-producing asset, or capital. In that moment, deferred R&D expenditures have taken transformed into a new capital asset, and the books of account are adjusted to reflect this new reality. In the second case, R&D expenditures are still deferred, but this time, the project fails to generate a new product or process. Deferred R&D expenditures have been accruing until the point of loss. It is management's prerogative to determine when a project needs to be abandoned and, thus, when the loss is written off out of income.

The problem with this theory, however, was that unlike other deferred charges such as prepaid rent or prepaid insurance, the future value of deferred R&D was entirely speculative and independently unverifiable. Like promoter's profits, deferred R&D expenditures presented a "hopeful and optimistic picture"¹⁷⁵ on corporate balance sheets. In this picture, dormant inside R&D expenditures was a profitable new invention—one that would emerge at some point in the future to transform R&D expenditures into

in the acquisition, improvement or development of a capital asset having a useful life beyond the year of expenditure. In the case of *Homer L. Strong*, the Board reasoned that since costs incurred through experimentation failed to improve upon the machine in question, 'the losses sustained or expenses incurred' were properly deductible. The case did little to clear things up, however, since, as several observers pointed out, taxpayers couldn't know in advance whether an R&D project would successfully result in a new or improved product or process. What's more, the treatment of (capital) losses and (non-capital) expenses was no less unhelpful. *Homer L. Strong v. Commissioner*, 14 B.T.A. 902, 903 (1928). On similar facts, a different court required capitalization of research costs. *Hart-Bartlett-Sturtevant Grain Co. v. Commissioner*, 182 F.2d 153, 156 (8th Cir. 1950). All of this highlighted "a fundamental problem with the general asset-capitalization rule: not only was it highly difficult to apply to research and development, but more significantly it also served to actually discourage important research and experimentation"(Nguyen & Maine 2011, 109).

¹⁷⁵ Dodd (1930), 45.

capitalized earning power. This was an ontology in which R&D expenditures were *essentially*, by their very nature, unrealized capital, capital-in-wait.

Chapter 3

Deferred Charges: A Closer Look

3.1. Making Good on Debts

Importantly, interest in the accounting category of “deferred charges” began developing at a time when the very meaning of “asset” was undergoing a profound shift. The accounting historian Sarah J. Williams has remarkably shown how, beginning in the late nineteenth century, deferred charges began appearing under the Assets heading on balance sheets with increasing frequency. Just prior to this time, the term “assets” appeared in the accounting literature as a synonym for the more conventional term “property.” Both terms represented “things owned, the wherewithal to meet financial obligation.”¹⁷⁶ But by the time General Electric opened the nation’s first industrial research laboratory in 1900, the designation of assets as “what was owned and therefore available to pay debts”¹⁷⁷ was expanded to include the notion of assets as “a storage of services to be received.”¹⁷⁸ From the first, this new definition was amenable to the category of “deferred charges to operations,” for the latter designated current expenditures the benefits or services from which would be realized in the future.

¹⁷⁶ Sarah J. Williams 1995, 1. As Williams wrote more pointedly elsewhere, assets or property were thus “real things, existing things or rights, which were exchangeable for cash”(Sarah J. Williams 2003, 163).

¹⁷⁷ Williams 1995, 2

¹⁷⁸ Charles Ezra Sprague *The Philosophy of Accounts* (New York: The Ronald Press Company, 1907), 41. After Sprague, John B. Canning published his magnum opus, *The Economics of Accountancy: A Critical Analysis of Accounting Theory* (New York: The Ronald Press Company, 1929) in which he emphasized a definition of assets as “future services” which was pivotal to his belief that assets and liabilities should be measured at the capitalized present value of their future cash flows. Both of these writers, however, built upon their own interpretations of the unique economic theories of Irving Fisher, in particular, his notion of assets as service potentialities (1906, 264). Fisher, however, was careful to point out that the future realization of assets, as service potentialities, into actual future economic benefits involved the element of “chance.” See John B. Ryan, “Canning’s Legacy” *International Journal of Critical Accounting*, Vol. 4, No. 4 (2012): 401-432.

To illustrate, consider the example of a shop owner who may choose to pay two years' worth of rent in advance. Of course, this did not mean that the shop owner received the benefit of two years' rental occupancy in advance. Instead, the benefit (or rental service) would be realized over the course of those two years. If, for whatever reason, the property owner violated the terms of the lease agreement and forced out the shop owner before two years were up, the shop owner could sue for unrecovered rental payments in a court of law. This was an extension of the term "asset" that did not stray far from "wherewithal to meet financial obligation." Gradually, however, other expenditures with far less assurance of realization began receiving similar asset-like treatment. Many businesses included large values for goodwill as assets on their balance sheets, while others folded such expenditures as discount on bonds, advertising, seasonal costs, or even company stationary under the deferred charges category. These were seen as highly debatable, if not out-and-out controversial.

By redefining assets as future services, the door had been opened to expanding the category of deferred charges. This expansion took place just as R&D was making its debut on the industrial landscape, although, to be clear, the acceptance of deferred charges did not depend on R&D accounting any more than R&D accounting depended on the acceptance of deferred charges. The two are not causally related, as if one brought the other into existence. In the earliest years of the new century, few companies saw the need for R&D and of those that did, an even smaller proportion could afford it. Thus, the activity of R&D and the use of deferred charges did not co-evolve, but developed largely separate up through the First World War. In those early years, R&D was to many an unproven, unpopular expense; by contrast, the use of deferred charges had much wider appeal among businessmen, and some practicing accountants helped make it so.

At a meeting of the Pennsylvania Institute of Certified Public Accountants in 1907, Walter A. Staub, head of the new Pittsburgh branch of Lybrand, Ross Brothers & Montgomery, advocated the use of deferred charges thus: “The fact that operations of a business...usually extend over a long period of years...has made it necessary to take account of assets to be ultimately consumed in operating which have as yet been only partially consumed, or of expenditures made in whole or in part for the benefit of the operations of a future period and which are thus to that extent not chargeable to the operations of the period during which they were made.”¹⁷⁹ Costs and benefits had become dislocated in time, he argued. Financial statements that were once issued on an annual basis were now considered too infrequent to be of use to investors and creditors. By breaking up the annual fiscal year into semi-annual or quarterly periods, many costs incurred in one period now benefitted operations of future period, thus necessitating the use of deferred charges.

Staub was emphatic about gaining wider acceptance among practitioners for the deferred charge category. Others in his firm mirrored his sentiments, if not his zeal. For instance, William Lybrand, founding partner of the firm, discussed the matter in his own terms during a meeting of the American Association of Public Accountants: “In the development of a corporation’s property and in connection with its current operations, there are frequently certain expenditures made of such a character that they are not justly chargeable immediately to the operating costs, nor on the other hand, should they be included with the permanent property or the current assets.”¹⁸⁰ Lybrand believed it was

¹⁷⁹ Staub *Journal of Accountancy* 1909, 401 (meeting held March 18, 1907)

¹⁸⁰ William M. Lybrand, CPA, *JofAccntncy*, December 1908, pp.118-119, presented at annual meeting of the Am Assn of Public Accountants, in Atlantic City, New Jersey, October 23-30, 1908

inappropriate to categorize deferred charges as current assets, for the latter usually designated property that was more readily made liquid.¹⁸¹ That is, property that could be converted into cold hard cash in a very short period of time, thereby making it “available to pay debts.”

That deferred charges could hardly be used to repay debts did not mean that many such charges failed to qualify for asset-like treatment.

Instances of important items of this class are advance payments of royalties and costs of exploration and testing preliminary to the development of properties containing raw materials [ie, mines or oil wells]. ... Advance payments of this character [which also includes unexpired insurance premiums, rentals paid in advance, prepaid discounts, etc.] are usually found grouped under the caption of ‘Deferred Charges to Operations,’ the purpose being to carry them temporarily as assets, and then charge them off to operations of the periods during which the benefits of the expenditures is reaped.¹⁸²

The deferral method allowed for the cost of prepaid insurance, prepaid rent, advance royalty payments, and the like to be stretched over multiple fiscal periods. In each case, contractual obligations guaranteed that regular payments were to be made by the firm in exchange for the stipulated services (of insurance, of rental occupancy, etc.). But not all deferred charges carried the same, legally-enforceable guarantee.¹⁸³

“In addition to expenditures which are without doubt chargeable against future operations,” insisted Lybrand, “there are certain others which are not so clearly defined,

¹⁸¹ See also William Morse Cole, *Accounts: Their Construction and Interpretation, revised ed.* (Boston: Houghton Mifflin, 1915), 22.

¹⁸² William M. Lybrand, CPA, *Journal of Accountancy*, Vol. (December 1908), 118-119. Presented at annual meeting of the Am Assn of Public Accountants, in Atlantic City, New Jersey, October 23-30, 1908

¹⁸³ Two decades later, Charles B. Couchman would emphasize this point in *The Balance Sheet: Its Preparation, Content and Interpretations* (New York: The Journal of Accountancy, Inc., 1924). There, he highlighted “legally enforceable claims for service” as the test against which prepaid expenses were sharply distinct from deferred charges.

but which may with some degree of reason, be included therein.”¹⁸⁴ As examples of more ambiguous items that were nevertheless “not unreasonable to carry over as deferred charges,” he offered the case of seasonal operating expenses and advertising costs. Writing off seasonal operating costs as incurred was “unquestionably the more conservative practice,” he admitted, but by presenting these costs on a balance sheet as deferred charges under Assets, a company could make a better showing to those who might seek to absorb it through a corporate merger or acquisition.¹⁸⁵ As for his second example, Lybrand suggested that “probably no valid objection could be made against [the deferral of advertising], provided it is done in good faith” and so long as “there is a reasonable prospect that the effect of the advertising will continue during the succeeding one or two years.”¹⁸⁶

One of the most adamant exponents for expanding the meaning of “asset” to include deferred charges was the New York City-based, French émigré Paul-Joseph Esquerré who, in 1914, published the widely cited text, *The Applied Theory of Accounts*. Expressing his disagreement with Greendlinger’s more cautious views, Esquerré argued that it was not “necessary that a value ‘may be used for the extinction of a man’s debts’ in order that it may be raised to the dignity of an asset.”¹⁸⁷ In support of his position, Esquerré referred to the growing practice of designating office stationary as assets, and explained matter-of-factly that this was done even though stationary “might have no

¹⁸⁴ Lybrand (December 1908), 118-119.

¹⁸⁵ In his own words, Lybrand defended the deferral of operating costs for the reason that, “in the event of the transfer of the business at any fiscal period, there is an equity existing not shown on the books, which must be taken into consideration” (Lybrand, 1908, 119).

¹⁸⁶ Lybrand (December 1908), 118-119.

¹⁸⁷ Esquerré 1914, 136

value available for the liquidation of any of the debts of the business.”¹⁸⁸ Rather than critique this classification, Esquerré used it as evidence to support his conclusion:

It seems, then, that the definition of assets might be extended to cover their peculiarities. The term ‘assets,’ when this is done, means: 1. For a going concern: That which is owned and invested in the business; that which is earned, although not received, and constitutes a collectible claim; that which has been expended for the benefit of future periods. 2. For a concern about to liquidate: a. If the concern enjoys the benefit of limited liability: That which is owned, invested in the business, and convertible into resources applicable to the liquidation of the liabilities of the concern.¹⁸⁹

This line of thinking encouraged the proliferation of deferred charges representing “items which are not strictly assets (such as expenditure which is being spread over a period) and items which are not really liabilities.”¹⁹⁰

So long as businessmen acted “in good faith,” the category of deferred charges could be appropriately applied to such diverse items as seasonal costs, advertising costs, prepaid rent, discount on bonds, and, apparently, even company stationary. The justifications for deferrals generally broke down along two lines, as exemplified by Lybrand’s discussion of seasonal costs and advertising costs. On the one hand was the practical benefit of moving numbers around so as to present one’s company in the best light. Thus, from Lybrand we learn that a company anticipating an offer of merger or acquisition could defer seasonal costs to make itself more attractive, to sell at a higher price. On the other hand was the theoretical benefit of deferral. Unlike the case of seasonal costs, the rationale for deferring advertising costs was closer to that for prepaid

¹⁸⁸ Esquerré 1914, 136

¹⁸⁹ Esquerré 1914, 136

¹⁹⁰ Sidney Stanley Dawson. *The Accountant’s Compendium* (London: Gee & Co., 1908): 46

rent: matching current costs with the benefits those costs were expected to yield in the future.

Many years later, in another landmark text by Paton published in February 1940 and co-authored with A.C. Littleton, this concern with relating expenditures of one period to income realized in another, became elevated to the status of a formal principle of accountancy.¹⁹¹ Expenses incurred today were to be matched to income earned tomorrow, and in the meantime, the expenses are to be represented as assets: “The factors acquired for production which,” they explained, “have not yet reached the point in the business process where they may be appropriately treated as ‘cost of sales’ or ‘expense’ are called ‘assets,’ and are presented as such in the balance sheet. It should not be overlooked, however, that these ‘assets’ are in fact ‘revenue charges in suspense’ awaiting some future matching with revenue as costs or expenses.”¹⁹²

3.2. The Paton and Littleton Monograph

What was the reason given for this mandatory matching? What led Paton and Littleton to the conclusion that “the essential task of accounting” was the “rational matching of costs incurred with resulting revenues”?¹⁹³ In their view, matching was a means of keeping managements accountable to investors and the public. After first

¹⁹¹ “If a single document can be attributed to have influenced a generation of academics to consider the primacy of the matching principle, an income determination process and earning power, *An Introduction to Corporate Accounting Standards* would have to be the most prominent candidate. Paton was the lead author...”(p313, “William A. Paton (1889-1991): Theorist and Educator” by Gary J. Previts and Thomas R. Robinson, *Twentieth-Century Accounting Thinkers*, John Richard Edwards, ed, London and New York: Routledge, 1994). For a discussion on the separate contributions of Paton and Littleton to this monograph, see Stephen A. Zeff, “*An Introduction to Corporate Accounting Standards*: Detecting Paton’s and Littleton’s Influences.” *Accounting Historians Journal* 45, no. 1 (June 2018): 45-67.

¹⁹² Paton and Littleton (1940), 25

¹⁹³ Paton and Littleton (1940), 59

defining deferred charges as inclusive of “most assets,”¹⁹⁴ Paton and Littleton then explained that without the use of deferred charges, investors would be misled because “services rendered will be incorrectly expressed.”¹⁹⁵ Matching was thus deemed to be correct on the basis that failure to match was “incorrect,” and in this, the Paton of 1940 appears to have transformed some of his earlier thinking from the late 1920s into a full-fledged tautology.¹⁹⁶

In a 1927 essay printed in *The Accounting Review*, the professional journal of accounting professors, Paton had argued that, “The theory that all costs legitimately incurred in the conduct of the enterprise which do not relate to the sales of the current period but may be expected to contribute to the revenue of later periods should be deferred and included in inventories,.. is particularly rational from what might be described as the income statement point of view.”¹⁹⁷ But, we may ask, what about the alternative perspective, that is, the balance sheet point of view? After all, it is the balance sheet where assets are reported, including deferred charges. In his discussion on this point, Paton explained:

¹⁹⁴ Paton and Littleton (1940), 16

¹⁹⁵ Paton and Littleton (1940), 16

¹⁹⁶ It should be pointed out, however, that one of Paton’s colleagues and former student of Littleton at the University of Illinois, Maurice Moonitz, had strong doubt about Paton’s acceptance of the ideas contained in the monograph. Following Littleton’s passing, a collection of anecdotes from his former students and colleagues were compiled by Lorraine Gilbert and published in *The Accounting Historians Notebook* (vol. 23, no. 2; October 2000). Moonitz, who had bitter memories of Littleton, recalled him being a humorless man, while Robert Mautz shrugged off Littleton’s gruffness, recognizing that he “would get so wrapped up in his work that he would become unaware of his surroundings and, at times, lost sight of other individuals’ viewpoints”(33). Littleton’s own son, Scott, had very limited knowledge of his father, depicting him as emotionally withholding and something of a mystery (34). As for the monograph itself, Moonitz was convinced that it “contained mostly Littleton’s view of Accounting, whereas Paton’s view was essentially nonexistent” and that “the Monograph was biased against his view of Accounting.” According to Gilbert, Moonitz suspected that “a tragic accident and death of Paton’s child in 1938” had left Paton “not himself for a long time due to his grief” which “may explain why Paton agreed to the final draft of the monograph”(34).

¹⁹⁷ William A. Paton “Distribution Costs and Inventory Values.” *The Accounting Review* (September 1947), p.251

As far as balance sheet values are concerned, most accountants feel that only deferred items should be included which are capable of realizability in other forms than through future operations; and it must be admitted that many of the distribution and selling costs appear to be somewhat questionable at first sight from this standpoint. A company, for example, lays down a barrage of expensive periodical advertising. How can it be determined when hits will begin to be scored, and how can the number of hits, if any, be discovered? Many accountants feel that the effect of such expenditures upon future business is so difficult to trace that it is more reasonable to charge such costs to revenue in the years in which the costs are incurred than to attempt anything by way of a deferring of such costs.¹⁹⁸

Having acknowledged this common feeling among accountants that not all relationships between expenditures and outcomes could be verified and that deferrals should therefore not be applied in such cases, Paton then went on to show the difference in his own perspective. “On the other hand,” he began,

it must be admitted that, as emphasized above, one class of charges entered into well advisedly is just as recoverable in the total of revenue as any other class of charges, and from this point of view it might be feasible *to take unusual expenditures on faith as we do religion, and allocate them to later operating periods on some more or less arbitrary basis*. ... No doubt accountants lay too much stress upon observable relationships to the neglect of underlying economic considerations. ...

In any event, may it not be urged that attention should be given to the development of methods by which costs which do not have relations to specific sales which the eye or the stop-watch may follow, may be assigned to sales of particular periods? ... [T]he thesis that all costs are on a parity as far as recovery through revenue is concerned and that, therefore, any legitimate cost may be deferred and included in cost inventories provided a reasonable method of working this out may be developed seems to be thoroughly valid.¹⁹⁹

¹⁹⁸ Paton (September 1927), p.252-3. Emphasis added. This was not the first time Paton expressed his frustration with cost accountants unwilling to recognize “that the costs of general professional services...are on substantially the same footing for purposes of accounting as are the so-called direct costs of tangible factors”(“Costs and Profits in Present-Day Accounting,” *NACA Bulletin*, October 1, 1934, p.126). The same sort of discussion appears in Paton’s Dickinson Lecture, in which he argued that “the accountant should not be unduly influenced by apparent physical connections—‘what the eye can see and the hand follow.’ ... The problem is one of reasonable economic association rather than physical incidence”(Recent and Prospective Developments in Accounting Theory. *Business Research Studies*, Number 25. Boston: Harvard University, p.10).

¹⁹⁹ Paton (September 1927), p.252-3. Emphasis added.

Paton did not expand on what he meant by “rationality,” nor did he explain what makes one kind of deferred charged “well advised” and another ill-advised. More importantly, Paton acknowledged that a transformation of certain expenditures into benefits or outcomes was an “unobservable” process, yet he remained confident that “methods by which costs which do not have relations to specific sales” could indeed be developed.

Where did his confidence on this particular matter come from? We do not have to speculate here, for the answer is embedded in the quote immediately above. For Paton, that a measurable relationship between all expenditures (costs) and outcomes (sales) did in fact exist, was simply taken “on faith as we do religion.” That the problem of distinguishing between different kinds of deferred charges was really no problem at all, that all expenditures contributed to income from sales, and that methods for measuring the relationship between any expenditure and its resulting sales income could be developed in due course—these assumptions informed Paton’s collaboration with Littleton in the late 1930s.²⁰⁰

Taken quite seriously by accountants within and without academia, Paton and Littleton’s *Introduction to Corporate Accounting Standards* (1940) popularized matching and cemented its status as a first principle of accountancy.²⁰¹ While the text itself remains seminal for most accountants in the United States to this day,²⁰² for a minority within the

²⁰⁰ Stephen A. Zeff confirms some of this on p.55 of his essay, “An Introduction to Corporate Accounting Standards: Detecting Paton’s and Littleton’s Influences.” *Accounting Historians Journal*, Vol. 45, No. 1 (June 2018): 45-67.

²⁰¹ Stephen A. Zeff “The Evolution of the Conceptual Framework for Business Enterprises in the United States.” *Accounting Historians Journal*, Vol. 26, No. 2 (December 1999); Reed K. Storey “Conditions Necessary for Developing a Conceptual Framework.” *Journal of Accountancy*, Vol. 151, No. 6 (1981); Paul Grady, *Inventory of Generally Accepted Accounting Principles for Business Enterprises*, ARS 7 (New York: AICPA, 1965), p.74; Delmer P. Hylton “On Matching Revenue with Expense” *The Accounting Review* (October 1965): 824.

²⁰² Zeff explains that the Paton and Littleton monograph would become “widely quoted and cited by practitioners and used by accounting academics in their university courses”(Zeff, “The Work of...”2001, 147).

field and for those of us on the outside who take an interest in this history, it can be somewhat jarring to observe the extent to which “matching” became sacred for generations of accountants. The field appears to have grown infatuated with the supposed foresight and unique judgment implicit in the matching process. By this is meant that if matching current costs to future income is correct or true, on the basis that failure to do so is incorrect or false, this requires a commitment to the idea (a) that accountants are capable of discerning which expenditures incurred today *will* give rise to future income, (b) that accountants are capable of recognizing *when* those benefits have arisen at some future point in time, and (c) that accountants are capable of *isolating* out of future aggregate income, that portion which is *specifically* the result of any given prior-period expenditure, such as R&D.²⁰³

Paton and Littleton, however, were not alone in championing the deferred charge method as one of the “phases of the process of matching.”²⁰⁴ One year prior, Stephen Gilman’s *Accounting Concepts of Profits* (1939) laid out an increasingly common understanding of deferred charges, one which he would reiterate in subsequent work.²⁰⁵

A most helpful accounting concept, particularly in relation to profit determination, is the one which considers non-cash assets as being equivalent to deferred charges. ... Expenditures are often made in the hope and expectation of a favorable effect upon any future profits. Often such effects are measurable. On the logical ideal of matching costs and revenues, the expenditures should then be

²⁰³ Deferring and matching costs “involves three stages: (1) ascertaining and recording costs as incurred, appropriately classified; (2) tracing and reclassifying costs in terms of operating activity; (3) assigning costs to revenues. The third stage is crucial from the standpoint of periodic income measurement; it likewise comprehends most of the difficult problems of accounting analysis. Matching costs and revenues requires more than careful procedure and accurate compilation. Recording the inflow of cost is in large measure a matter of close observation and efficient clerical process; recording the outflow of costs as embodied in revenue is essentially a matter of judgment and interpretation”(Paton and Littleton 1940, 69).

²⁰⁴ Paton and Littleton 1940, 16

²⁰⁵ Stephen Gilman “Accounting Principles and the Current Classification” *The Accounting Review*, Vol. 19, No. 2 (April 1944): 109-116.

considered as deductible from the future income for the encouragement of which they were made... While it is common to think of some assets as being deferred charges against future profits, it is not generally recognized that *all assets except cash may be considered deferred charges*.²⁰⁶

Given that capital assets were, by definition, items of property whose value lasted for some considerable duration of time (typically, at least beyond a single year), then to theorists like Gilman, *any* expenditure which generated benefits lasting for more than one period could be represented as a deferred asset.²⁰⁷ We have already seen similar positions taken as early as the opening decades of the new century.

Staub's warning, for example, was limited only to the accuracy of matching current period costs with future period benefits. "Care must be had," he wrote in 1909, "that the amount so carried forward is not greater than the probable future output can absorb, taking into consideration additional expenditures of the same kind which will have to be made in the future."²⁰⁸ For Staub, insufficient "care" bore the blame for a number of corporate scandals that were revealed to have been facilitated by the use of the deferral method. "Abuses of deferred accounts," he explained, "are frequently encountered, and, while they may sometimes be deliberate and intentional, they are more often doubtless unintentional as far as intent to mislead is concerned, though nevertheless really just as misleading."²⁰⁹ Rather than advocate the use of alternatives to the deferred charge method, Staub wrote apologetically that the "treatment of these deferred charges

²⁰⁶ Gilman (1939), 297-8. Emphasis added.

²⁰⁷ In his preface, Gilman acknowledges the "wealth of critical and constructive suggestions" he received from Paton, among others, in crafting *Accounting for Profits*. See Gilman (1939), v.

²⁰⁸ (Staub 1909, 411)

²⁰⁹ Staub 1909, 402

to operating...offers some of the most difficult practical problems with which the accountant has to deal.”²¹⁰

Seymour Walton, editor of the *Journal of Accountancy*'s “Students’ Department,” issued his own warning that deferred charges were not assets in the conventional business sense of the word. However, like Staub, Walton’s enthusiasm for the principled matching of costs with benefits outweighed his concern that the concept of “asset” was being bent in the process:

One of the advantages of the double entry system is that all the elements of a business can be taken into consideration. Instead of being confined to cash receipts and disbursements applying entirely to the period in which the money was received or paid, the effect of any particular item may be distributed to the period which must earn it if a profit, or which will receive the benefit from it, if an expense. These items in a balance sheet are carried as deferred debits or credits.

They are assets and liabilities only in an accounting sense, as being advances made by the current year for account of the succeeding year, or obligations of the current year that will have to be met by the succeeding year.²¹¹ ... A deferred charge to operating is only technically an asset. It will never be realized in cash, but will simply be taken care of by being charged against revenue in the succeeding period.²¹²

That deferred charges were assets “only in an accounting sense” did not prompt Walton to question their inclusion as such in company balance sheets.

By the following decade and into the next, the possibility of applying this technique to R&D had started to be bandied about. In 1916, for example, the prominent cost accountant and consultant Jerome L. Nicholson argued that “when the expense [of an experimental department] results from work on products or processes that are to be

²¹⁰ Staub 1909, 402

²¹¹ Walton May 1914, 384

²¹² Walton May 1914, 386

used at a future time, the correct method is to make a deferred charge of it, which will not be absorbed until the results of the experimental work are in actual operation. In practice, however, such expenses are usually absorbed in current indirect expense, and are not treated as deferred charges unless they are large enough to affect the cost calculations perceptibly.”²¹³ By 1921, William H. Bell of the public accounting firm Haskins & Sells described Deferred Charges as a “classification [that] is intended to cover various expenditures which are applicable to future operations, or which are held in suspense pending determination of their actual status.”²¹⁴ Offering Experimental Expenses as one example of such charges, Bell insisted that the “amount to be shown thus should be the cost to date of experimental work which has not yet reached the point where its value may be determined. It is assumed that if hopes are realized the cost will be capitalized; otherwise it will be written off.”²¹⁵ Gradually, the number of authors encouraging this treatment grew in number.

3.3. “To Avoid Misleading Impressions”

This does not mean, however, that there were no critics of the mainstream. Only three years after publication of the Paton and Littleton monograph, John Arch White, chair of the accounting department turned dean of the University of Texas College of Business Administration, asked: “shouldn’t accounting theory define an asset as something more ‘than a debit balance...according to the rules or principles of accounting?’ The definition appears to be of little use to the accountant in determining

²¹³ Nicholson, *Cost Accounting Theory and Practice*, 2nd ed. (1916), 29

²¹⁴ William H. Bell, *Accountants’ Reports* (New York: The Ronald Press Company, 1921), 44

²¹⁵ Bell (1921), 47

what should be admitted to the asset side of the balance sheet.”²¹⁶ The problem was, there was no definition of “asset” that made it easy to decide whether an expenditure was associated with a capital asset, or whether it was the expenditure itself that qualified as an asset. White also quoted E. B. Wilcox who, in reviewing Paton and Littleton’s treatment of accounting standards, expressed that, “I regret to say I do not feel much better acquainted with those standards than I did before I was introduced to them.”²¹⁷

Around the same time, the eminent George O. May wondered, “If a company does use deferred charges, does it set forth the procedure it uses to gradually amortize them? This is important because substantial overstatements of income may occur through deferment in unprosperous periods of expenses ordinarily chargeable against current operations, possibly followed by writing off such charges in a later year against surplus account.”²¹⁸ And by 1970, Paton himself acknowledged the contributions his 1940 text made to the obsession with matching. “For a long time,” he lamented, “I’ve wished that the Paton and Littleton monograph had never been written, or had gone out of print twenty-five years or so ago. Listening to Bob Sprouse take issue with the ‘matching’ gospel, which the P & L monograph helped to foster, confirmed my dissatisfaction with this publication.”²¹⁹

²¹⁶ John Arch White “The Direction of Accounting Theory” *The Southwestern Social Science Quarterly*, Vol. 24, No. 1, (June 1943): 7. White earned his BBS (1929), MBA (1930), and PhD (1937) all from the University of Texas, where he remained for the rest of his academic career. When the College of Business Administration was reorganized in 1945, White was named the first chair of the Department of Accounting. In 1954, he was appointed associate dean of the same college in 1954, becoming dean by 1958. In his capacity as dean, White oversaw a complete revision of the business curriculum.

²¹⁷ E. B. Wilcox “Comments on ‘An Introduction to Corporate Accounting Standards’” *The Accounting Review* (March 1941)

²¹⁸ George O. May (1946) *Financial Accounting*

²¹⁹ William A. Paton, “Introduction,” in Williard E. Stone, ed., *Foundations of Accounting Theory: Papers Given at the Accounting Theory Symposium, University of Florida, March 1970* (Gainseville: University of Florida Press, 1971), pp. x.

Still, *Introduction to Corporate Accounting Standards* merely formalized a practice that was already in development at the start of the century. We have seen examples of this from Staub in 1907 and Lybrand in 1908. Hatfield offered an example from 1909. That year, while discussing the practice of locating deferred charges under the Assets heading of a balance sheet, Hatfield made the argument that such charges “are not conceived of as representing part of the cost of permanent assets, nor are they charged at once to the expenses of the year. Being clearly expenses they are yet expenses which are *offsets of future earnings, not of past receipts*. While in many cases *not representing any actual asset*, they are correctly treated *as though they were assets*.”²²⁰ Two years later, a similar argument was made by Frank E. Webner, a cost accountant and CPA who, by that time, had amassed twenty years’ experience in the field, working with such firms as the American Audit Company in Cincinnati, Ohio, the American Mechanical Calculator Company, and even Jerome Nicholson’s J. Lee Nicholson & Company.

In his 1911 reference text, *Factory Costs*, Webner used the phrase “Suspense Items,” an older terminology that was often associated with the use of secret reserves by the railroad industry and that by the time of his writing was already being marginalized, if not entirely supplanted, by the category of Deferred Charges.²²¹ A notable difference between Webner’s discussion of Suspense Items and his contemporaries’ discussion of Deferred Charges was Webner’s insistence that, “Such accounts should be kept distinctly

²²⁰ Hatfield (1909), 118. Emphasis added.

²²¹ Thus, in his 1911 discussion, Webner’s discussion of Suspense Items closely resembles what other authors of his time already referred to as Deferred Charges. For example, he insists that the “whole matter is one that should be governed strictly by the facts; i.e., the reservation must be justified and the succeeding periods must be actually benefitted by the expenditure to the same degree as is the current period, or in proportion to the amount charged to such periods.” Frank E. Webner, *Factory Costs: A Work of Reference for Cost Accountants and Factory Managers* (New York: The Ronald Press Company, 1911): 146

separate and apart from other accounts which represent assets, *in order to avoid misleading impressions.*”²²² Others advocating the use of deferred charges made no such claim; what caveats Webner’s contemporaries did see fit to include laid little or no emphasis on the possibility of misleading the readers of financial statements. By 1924, however, Webner himself seems to have abandoned his concern with misleading statements.

In his latest textbook, *Factory Overhead* (1924), Webner’s discussion of “Deferred Assets, Deferred Charges, or Suspense Accounts—as they are variously known” plagiarized from Hatfield’s *Modern Accounting* of 1909, making only the single substitution of the term “overhead” in place of Hatfield’s “expense.”²²³ One can only speculate the reason for Webner’s near-exact plagiarism as acknowledgement of Hatfield’s leading influence upon early twentieth century accountancy. Still, by the time *Factory Overhead* came out in 1924, Hatfield’s views on deferral were growing less optimistic than they had been in 1909.

Over the ensuing years, Hatfield had come to emphasize the uncertainty of outcomes that was particularly manifest in the case of R&D. Eighteen years after publication of his *Modern Accounting* (1909), he recreated word for word that text’s discussion of experimental expenses in his latest work, *Accounting, Its Principle and Problems* (1927). This time around, however, Hatfield dug deeper into the question of whether to treat such expenses as quasi-assets:

²²² Frank E. Webner, *Factory Costs: A Work of Reference for Cost Accountants and Factory Managers* (New York: The Ronald Press Company, 1911): 146. Emphasis added.

²²³ The passage plagiarized from Hatfield appears on page 102 and 103 of Webner (1924) and reads: “Such items hold to some extent an intermediate position for they are not conceived of as representing part of the cost of permanent assets, nor are they charged at once to the overhead of the period. Being clearly overhead they are yet items which are offsets of future earnings, not of past receipts. While in many cases not representing any actual assets they are correctly treated as though they were assets.”

The difficulty is that while the experiments are still being carried on *it is impossible to determine whether the expenditure will fructify into a valuable asset or whether, being unsuccessful, it represents merely an unproductive expense.* There is also difficulty in determining whether the successful invention is to be charged with the expenses of previous unsuccessful experiments, or only of those leading directly to the valuable discovery. With so many uncertainties involved, accountants are generally inclined toward a conservative policy, preferring an understatement to an overstatement of the value of assets.²²⁴

Since the results of an R&D project could not be known in advance, then neither could the market value of those results. Consider, if you will, the following general example:

If a factory was set up to produce whirligigs, one could attempt an estimate of how much production of additional whirligigs would earn you in the market. This estimate could be arrived at through past experience manufacturing whirligigs as well as market research to develop a sense of how consumers may respond to more whirligigs in the future. Of course, there would be uncertainty at play, as there always will be about the future. Whirligigs that had once delighted consumers, could all of a sudden fall out of favor for a number of reasons. Competitors might release new products that make one's whirligigs obsolete or passé. Or perhaps tragedies involving whirligigs moved lawmakers to impose strict regulations to safeguard the public. But these were business risks common to all enterprise ventures. Taken in this light, what made R&D unique was that its project costs may never yield anything of marketable value regardless of market conditions for the anticipated results. This is why signaling to readers of financial statements that R&D was an asset in the traditional sense was so very misleading.

By treating R&D as a general expense, chargeable against income earned in the current period, it made little difference whether or not the results of experimentation

²²⁴ Hatfield (1927), 71-2. Emphasis added.

proved to be profitable. This was because no matter the result, expenses were provided for out of income as they were incurred. If, after multiple periods of experimentation, a decision was made to abandon an R&D project that had lasted for many periods, the company was not left with an enormous “bill,” so to speak. Again, this was because the expenses were already provided for along the way. Although the defeat of a failed project stung, the project could be wrapped up without any I.O.U.’s left on the table. This sort of planning for potential losses was considered a conservative strategy and virtue by early twentieth century accounting theorists. It meant that, as a business owner or manager, you would never promise more than you could deliver. As a result, one’s reputation as well as one’s finances were kept intact, both to the benefit of one’s business and to the community it served.

The more “liberal” alternative of treating the expenses as costs to be absorbed largely by future income, had its financial dangers. Deferral posed no practical problem for accountants if, in some future period, a company found itself in possession of enough cash to absorb the losses resulting from failed R&D projects. The problem only arose if and when a project, whose expenses were spread out (or “allocated”) over an estimated number of future periods, was abandoned at a time when the company was cash-poor. In such circumstances, wherein a company finds itself without the cash on hand to write off deferred charges, losses of abandoned projects would have to be provided for, rather unexpectedly, out of capital. Otherwise, management could decide to postpone recognition of the loss in the hopes of maintaining for a little while longer an appearance of profitability in their financial statements. In so doing, any commitment to a faithful reporting of the firm’s economic well-being, as well as the idea of transparency and accountability to investors, was tossed out the window.

Deferring charges to the income of future operations, therefore, was a gamble that put investors and creditors at risk, a risk which compounded the general business risk investors were accustomed to—and, arguably, reasonably expected to—bear.²²⁵ This concern, however, did not seem to dissuade some firms from pursuing deferment, even if to others this was a most *unconservative* strategy.

3.4. Doubtful, But Still Deferrable

By the mid to late 1920s, the category of deferred charges had been stretched to encompass R&D expenditures without, however, displacing the use of other methods. Although deferral came to be seen as a useful alternative to current expensing, the two methods would coexist over the course of the 1920s with little, if any, discussion about the implications of applying one over the other. This makes it difficult to know just how accountants' views towards deferral, on the one hand, and R&D, on the other, coincided and evolved in the thinking and decision-making of practitioners. If rigorous studies dedicated to the subject did exist, there seems to have been little impetus to make findings public. This remained true even as a growing number of large corporations began opening new R&D facilities, either as in-house divisions or as separately-incorporated legal entities as in the case of Bell Telephone Laboratories, General Motors, U.S. Steel, Standard Oil of New Jersey, and Royal Dutch Shell.²²⁶

²²⁵ Frank H. Knight long ago described this kind of risk in different terms, namely, as the uncertainty of success. Knight contrasted uncertainty, which was endured by entrepreneurs embarking on a new business venture, with probabilistic risk—the latter being susceptible to some degree of control in a way the former specifically was not. See *Risk, Uncertainty, Profit*.

²²⁶ In addition to large firms expanding their own research programs, a large number of small firms began self-identifying as maintaining some form of research practice in response to surveys conducted by the National Research Council, starting in 1920. The extent to which these latter, self-described research organizations should be counted as part of the American industrial research landscape depends upon the

In all, accounting continued to vary across firms. One year after its founding, the National Association of Cost Accountants (NACA) published an article in October 1920 by A.H. Barrett of Boston who observed that “Experimental and Development costs” were “accounted for in several ways.”²²⁷

If incurred for the benefit of the company [rather than on contract for an outside client], they may be immediately charged to general administrative expenses [i.e., to overhead], or to the account of the department [within the business] which is to benefit; or accumulated as a deferred charge item until completion of the work, when they may be disposed of in the surplus account or transferred to a permanent asset account.²²⁸

Despite acknowledging this diversity of practice, Barrett located his discussion of R&D costs under the heading of Deferred Charges, much as the Federal Reserve had done in *Uniform Accounting* (1917) and would do again in *Verification of Financial Statements* (1929).

With this decision, Barrett signaled to his audience the naturalization or legitimization of, if not preference for, R&D deferral. Barrett was not exceptional in this regard. Robert H. Montgomery signaled much the same to his audience in the third edition of *Auditing Theory and Practice* (1922).²²⁹ Unlike Barrett, however, Montgomery qualified “Experimental and

extent to which the work done therein was experimental, or whether it was, to use a distinction made by accountants themselves, merely “routine testing.” Leonard Reich, in the conclusion to *The Making of American Industrial Research*, skirts this issue by arguing that “Even though most of these [small] labs would certainly not meet the criteria of the industrial research definition used here,..a considerable number of American companies were conducting some kind of scientific-technical research by the early 1920s and that they believed support of ‘industrial research’ – however they defined it – was in their best interests”(253). One possibility, based on ___’s study of GE’s publicity efforts as well as the advertisements placed by that company throughout business magazines and newspapers, is that small firms sought to benefit from the glamour of industrial science without actually throwing resources behind it.

²²⁷ AH Barrett (1920), 6-7

²²⁸ Barrett (1920), 6-7

²²⁹ By this time, Montgomery’s reputation had extended far past that of founding co-partner of Lybrand, Ross Bros. & Montgomery; in 1922, he was a practicing attorney, certified public accountant, former president of the American Association of Public Accountant, and professor of accounting at Columbia University.

Development Expenditures” as *Doubtful* Deferred Charges, i.e., “expenditures the benefits from which are doubtful.”²³⁰

Montgomery presented three methods for handling such costs: (1) immediately writing them off so long as they are incurred as part of a continual activity, as in the case of an established R&D department; (2) capitalizing them when extraordinary in amount and incurred for the specific, capital-enhancing purpose of increasing production or reducing the costs thereof; or (3) deferring costs either over a predetermined length of time or over an estimated volume of output. As an indication of when it would be appropriate to use the third method (i.e., deferral), Montgomery gave only the example of “an improved process for refining ore or mining coal [which] should be spread over the life of the mines benefiting from the process.”²³¹ Whether a definitive length of time or output could be established for R&D—that is, as an activity apart from mining—he did not say. The best he could do was to recommend a heuristic analogy without offering his own interpretation as to its meanings or limitations.

Released the same year as Montgomery’s *Auditing Theory and Practice*, Paton’s *Accounting Theory: With Special Reference to the Corporate Enterprise* (1922) argued that “experimental costs” presented a particularly difficult case in which to distinguish a loss from an asset. “Has a particular outlay resulted in something of value from which the benefit will not be exhausted for some time or has the money virtually been thrown away?” he asked.²³² To answer this question, Paton turned to advertising: “A company spends \$100,000 on an advertising campaign, for example. The accountant is called upon

²³⁰ Robert H. Montgomery, *Auditing Theory and Practice* (1922), 574

²³¹ Montgomery (1922), 575

²³² William A. Paton, *Accounting Theory: With Special Reference to the Corporate Enterprise* (1922), 178

to decide whether this value is a loss, a cost of producing current revenue, or an asset balance.”²³³ Here, the point being drawn out was the difficulty facing accountants in the task of valuation, and it is an important point to bear in mind.

Taking a step back for a moment, it is important to recognize that the challenge of representing business activities in quantified terms makes the work of accountancy so necessarily subjective, so dependent on the exercise of one’s best judgment. Certainly, in accounting, as in all areas of inquiry, the Truth does not lay “out there” in some external form against which subjective judgments can be verified or falsified. If matching previously-deferred R&D expenditures to presently-earned income was a practice that seemed to work in the market mania of the 1920s, then the test against experience seemed to prove the viability of deferral. If one were to speculate, it could be said that the experience of the 1920s may help explain some of the change in Paton’s thinking over the course of that decade, from his recognition of the limits of deferral in 1922 to the unqualified faith he would place in it by 1927.

More notable, though, is that neither Paton nor Montgomery—both leaders in the accounting profession—used an example that involved R&D in order to explicate the accounting thereof. Where Montgomery used the case of mining, Paton used advertising. It appears to have been especially difficult, even in theory, to face R&D costs head on and take a definitive position one way or the other. As we have seen, Montgomery was an accountant of vast and diverse experience.²³⁴ The fact that even he struggled to articulate

²³³ Paton (1922), 178

²³⁴ See section XYZ earlier.

a general rationale for deferring R&D should be taken as an indication of just how challenging it was to account for R&D.

The difficulty lay in sorting out whether R&D expenditures were analogous to the purchase of manufacturing assets (or additions to capital), or whether they qualified as ordinary business expenses (or deduction from income). Convention held that asset acquisitions were capital expenditures, whereas income expenditures were those necessary costs of doing business, but in practice, the boundary between capital and income expenditures was fraught with questions of judgment for which there were no easy answers. Locating where R&D expenditures fell across that boundary made reliance on judgment all the more necessary—and all the more susceptible to misuse, intentional or otherwise.

Nevertheless, Paton and Montgomery devoted little space to these issues in their respective textbooks of 1922 and in future journal articles. Arguably, by forgoing the opportunity to investigate the implications of either accounting method, these and other writers of the 1920s helped clear the way for the decoupling of R&D expenditure, from the activity such expenditure helped make possible. More than seventy years later, Walter P. Schuetze, Chief Accountant of the SEC, used the same two cases presented by Montgomery and Paton in 1922 to emphasize the limitations, rather than viability, of the deferral method: “the probable future economic benefit of a successful, direct-response advertising campaign may be many multiples of the cost. The future benefit of a discovery of mineral deposits generally bears no relationship whatsoever to the costs of

finding the deposits. The future benefits of successful research and development also bear little or no relationship to the costs incurred.”²³⁵

Discussing what he called the “cost-*per se*-is-the asset syndrome,”²³⁶ Schuetze concluded that in R&D, advertising, and other cases, “it is the cost itself that is identified as the asset, not the probable future economic benefit.”²³⁷ Only by treating the expenditure itself as a deferred asset could income from and expenditures of R&D be “matched.” If the value of R&D could not be captured by the amounts spent on it—in other words, if expenditures were no measure of R&D’s value to a firm—then such matching was utterly meaningless.

3.5. Simmering Frustrations

Into the 1920s, a frustrated minority of bankers, analysts, and accountants decried the classifications appearing in the balance sheets published by companies and certified by auditors. Speaking before a December 1921 meeting of the Massachusetts Society of Certified Public Accountants, John N. Eaton, Credit Manager of the Merchants National Bank of Boston averred,

We see many so-called *certificates which mean absolutely nothing*—they cannot be taken seriously. Here are a few samples of many which have come to my notice: ... ‘We have examined and audited the books and accounts of.....for the period January 1, 1921, to August 31, 1921, and herewith present the above statement of Assets and Liabilities, subject to such allowances for accrued and deferred items, as may not have been considered.’²³⁸

²³⁵ Walter P. Schuetze “What is an Asset?” *Accounting Horizons*, Vol. 7, No. 3 (September 1993): 69

²³⁶ Schuetze 1993, 68

²³⁷ Schuetze 1993, 69

²³⁸ *The Financial Audit as Viewed by Bankers, Statement Analysis, Suggested Methods for a Credit Index*, publications of The Robert Morris Associates, Condensed, Rearranged, and Commented on by Scovell, Wellington & Company – formerly Clinton H. Scovell & Company, certified public accountants and industrial engineers, Boston, 1922, p.24

As the example given above shows, auditors' opinions were riddled with "subject to" qualifications, the exact meaning of which could be incredibly difficult to discern. The frustrations this caused bankers was not a problem unique to the 1920s, as an address delivered decades earlier by one CPA to an audience of bankers makes clear.²³⁹ Speaking before the Chicago Credit Men's Association in 1906, William H. Roberts listed "Some of the most common violations of accounting principles which tend to cast a rosy glow over an otherwise gloomy statement."²⁴⁰ Third on this list was the "Failure to charge off doubtful accounts receivable or to create a reserve from profits to take care of losses from this source."²⁴¹

Many business men imagine that when they have transferred a doubtful item to suspense account, they have done all that is necessary, failing to understand that unless the suspense account is wiped out by profits, or a reserve created against it from the same source, the balance sheet may be just as misleading as if no transfer had occurred.²⁴²

Dumping expenses on balance sheets in the form of deferred charges relieved business managers from having to show those expenses on profit and loss statements as deductions from income. But deferred charges were, as the name implies, awaiting final disposition, either as capital assets or as losses. If they resulted in a capital asset, the value of that

²³⁹ Although writing many decades later than the period discussed here, Butler University professor Felix P. Kollaritsch explained the task of creditors with respect to accounting figures, so succinctly as to warrant the following quotation from his July 1960 *Accounting Review* essay: "Creditors in general view the information given in the balance sheet concerning assets, liabilities, and equity from a different point of view than that of management or owners. Their interest lies mainly in the security which backs up loans extended. Therefore, a balance sheet of value to creditors must incorporate information that will allow them to judge a debtor's financial position and that will, thereby, meet the creditors' concept of financial position"(Kollaritsch 1960, 482).

²⁴⁰ William H. Roberts "The Credit Man and the Public Accountant" *Journal of Accountancy*, Vol. 1, No. 6 (April 1906): 467.

²⁴¹ Roberts (1906), 467-8

²⁴² Roberts (1906), 468

charge would become capitalized (or, added to the firm's stock of capital assets) by deducting that amount from income. If the deferred charge resulted in a loss, that amount, too, had to be deducted from income. That was how deferred charges were to work in theory, anyway. As it turns out, a number of businessmen had developed the habit of leaving deferred charges on their balance sheets indefinitely. Thirty years later, investigations by the newly-formed SEC revealed that failure to write off losses from deferred charges continued to deceive users of financial statements.²⁴³ If charging these items to capital was misleading, Roberts argued back in 1906, then leaving them in suspense could hardly be considered any less misleading.

Still, if Roberts' address is any indication, bankers were caught between hoping to extend credit only to those who could repay it, on the one hand, and worrying about keeping and attracting clients, on the other. "I am very well aware," he reassured his audience, "that the pressure of competition is so severe that much has to be taken for granted, for fear of offending your trade by 'getting too personal.'"²⁴⁴ Nevertheless, he added,

it would certainly puzzle the hypothetical 'Man from Mars,' to contemplate a business system under which one merchant is expected to invest largely in the business of another with no better knowledge of his condition than is often obtainable. In times of prosperity and expansion like the present, the conservative is apt to be classed a pessimist, but we must not lose sight of the fact that prosperity has never yet failed, by its very existence, to produce its own reversal; extravagance and speculation, followed by disaster and stagnation, make the cycle

²⁴³ In the case of American Gyro Company, for example, the SEC found that the balance sheet item "Development of Invention"—carried as an asset in the amount of \$81,713.30—includes approximately \$40,000 spent for development of various inventions which, on or prior to June 20, 1933, the date of the balance sheet, the registrant [ie, American Gyro Company] admittedly had found to be worthless. Sound accounting principles require that where the results of experimentation, such as has been carried on here, are unsuccessful, the respective development costs should be written off as a loss"(*Securities and Exchange Commission, Decision and Reports*, Vol. 1, 1934, 87).

²⁴⁴ Roberts (1906), 469

of business experience in America, and the climax has recurred with surprising regularity.²⁴⁵

Despite this admonition, clarity as to balance sheet figures continued to elude the business community. Company accountants had little to no influence on their employers; bankers needed to make loans one way or another; potential investors were too limited in number to compel greater publicity; and managements jealously guarded their decision-making authority.²⁴⁶ All of this added up to the barest minimum of safeguards for investors, creditors, and even suppliers. In many cases, one simply could not say with confidence, solely by studying any given balance sheet, what the figures represented, how they were calculated, nor why they were grouped in one form rather than another.

For one thing, when deferred items were used in practice, they were almost always lumped together in precisely the way Walton, Paton, and other accounting theorists expressly rejected, namely: the singular presentation of a “Deferred Charges” category that was anything but the homogenous group of expenditures it was made out to be. Whereas some items in this quasi-asset category were mere expressions of hope that the expenditures would give way to a future benefit, other items were delimited by the terms of contract and so had the force of the law to guarantee the fulfilment of future services. By lumping all these expenditures together into a single asset category, crucial differences as to the likelihood of future benefits were easily blurred. Some bankers decried this practice as concealment and helped lead a movement to introduce uniform auditing

²⁴⁵ Roberts (1906, 469

²⁴⁶ *The Financial Audit as Viewed by Bankers, Statement Analysis, Suggested Methods for a Credit Index*, publications of The Robert Morris Associates, Condensed, Rearranged, and Commented on by Scovell, Wellington & Company – formerly Clinton H. Scovell & Company, certified public accountants and industrial engineers, Boston, 1922, p.24

statements certifying as to the accuracy of the values reported on company financial statements.

One result of this effort was *Uniform Accounting*, which was published in 1917 and revised and reprinted in 1929 as *Verification of Financial Statements*. Under the auspices of the Federal Reserve Board and in partnership with the Federal Trade Commission, the AIA released *Uniform Accounting* with the intention of establishing standardized practices for the auditing of balance-sheets submitted by firms in their applications for bank credit.²⁴⁷ Both pamphlets gave explicit sanction to the use of the deferral method in handling R&D expenses, confining discussion of experimental charges to a section on Deferred Charges to Operations. In neither case was any mention made of absorbing R&D expenditures as incurred.

In the end, although the original and revised pamphlets were widely distributed by the Federal Reserve Board in commercial, banking, and accounting circles, neither was readily adopted.²⁴⁸ The pamphlets' cavalier approach to verification of inventories failed to satisfy the demands of bankers.²⁴⁹ Large numbers of bank failures mounted even after the publication of *Uniform Accounting*, and *Verification*, too, failed to put an end to the more general critiques.²⁵⁰ The same problems arose with respect to the treatment of

²⁴⁷ Although revisions were made in 1929 to the original 1917 pamphlet, discussion of deferred charges went unrevised. Normand & Wooton (2001, 92).

²⁴⁸ Hawkins 1968, 155-6. Also, see Ram S. Sriram and Gloria Vollmers "Reexamination of the Development of the Accounting profession – Critical Events from 1912-1940" *Accounting Historians Journal*, Vol. 24, No. 2 (December 1997): 65-90.

²⁴⁹ "Bankers were concerned by the position taken on inventories and receivables; the standard certificate compounded the problem. Bankers claimed that the short-form opinion, introduced in *Uniform Accounting*, was useless for their purposes. They could not rely on the opinion unless the audit firm was well known or the banker had worked with the firm directly" (Previts & Merino 1998, 232-233).

²⁵⁰ In 1927, a magazine of the American Bankers Association took note of "the large crop of over 3,000 banking failures during the last six years." ("New Vision in Business and Industry" *Trust Companies*, Vol. 45, No. 2 (August 1927): 121).

R&D. *Uniform Accounting* suggested only that, “Whenever possible, documentary proof must be produced in support of the items carried forward, as for example, with...experimental charges the vouchers and particulars of the work done must be looked into, etc.”²⁵¹

Which other forms of “proof” were waved away by the “etc.” one can only wonder. In 1924, AIA and NACA member Frederic W. Kilduff attempted to fill this void by offering his own operational tutorial for auditors facing the task of examining a company’s deferred R&D costs. In *Auditing and Accounting Handbook* (1924), Kilduff first outlined, step-by-step, what kinds of evidence were necessary to validate the use of the deferral method.²⁵² However, it’s not clear whether Kilduff’s text had its intended impact. Theoretically, if R&D costs could be traced and verified by hard evidence, then firms could receive unqualified opinions from their auditors for their evidence-backed use of deferred charges. That auditors were comfortable enough in the 1920s to challenge their clients’ judgment regarding the deferral of R&D seems highly doubtful, however.

While the deferral method allowed firms to match expenditures with their associated future benefits, what seems doubtful is the likelihood that the theoretical advantage of matching was sufficient to induce firms into deferring R&D costs. After all, even with regards to depreciation accounting—which proved so essential to the solvency of many firms—the recommended practice did not become widespread across the American business landscape until passage of federal income tax laws made it desirable to

²⁵¹ *Journal of Accountancy*, 1917, p.418-419

²⁵² 1. Check back from all charges in these accounts to the books of original entry. 2. Obtain and analyze all supporting vouchers for the following data: a. Voucher number. b. Voucher data. c. To whom paid. d. Description of charge. e. Amount of voucher. ... 3. Determine if all the vouchers are proper charges to these accounts. 4. Determine if the basis used in deferring the charge is correct. 5. Verify all calculations used in establishing that portion of the expense to be deferred” (Kilduff 1924, 55).

do so. Other examples of theoretical reasoning going unheard can be cited, including the ongoing rehearsal of *caveat emptor* and the refusal to regularly disclose detailed financial information until compelled to do so by federal Securities Acts of the early 1930s.²⁵³

Providing a rare glimpse into the kinds of corporate reasoning that *were* actually in use, Hasbrouck Haynes of the Haynes Corporation in Chicago discussed why his company carried R&D as a deferred asset. Before an audience at the Second International Cost Conference held in 1921, he explained that the reason for so doing was the same as the reason why “the stock of many new corporations [are] watered through the good will and patent accounts in the hope that subsequent profits will later squeeze it out” and why “reserves for taxes or bad debts [are] set up each month instead of being charged to expense when they actually become due or apparent.”²⁵⁴ Despite the argument sometimes heard “that there is no advantage in deceiving oneself by juggling the accounting figures so as to show a profit..., where there is actually a loss,”²⁵⁵ the motivation behind such accounting treatment was its psychological impact.

During dull periods the monthly profits are not distorted...and the depressing influence of large book losses is thereby avoided. [This advantage] is largely psychological, but is nevertheless an important and desirable one if judiciously handled. Where so used, plants that might otherwise be shut down or sold, as the result of a decision by the Board of Directors, will be kept running. Their organizations [including their R&D organizations] will be retained, and with a proper financial policy, will doubtless come through stronger and healthier than ever.²⁵⁶

²⁵³ Just prior to the passage of the Securities Act of 1933, a hopeful US President Roosevelt expressed to Congress that “this proposal adds to the ancient rule of *caveat emptor* the further doctrine ‘Let the seller beware,’ It puts the burden of telling the whole truth on the seller”(as quoted in Robert L. Knauss, “Disclosure Requirements—Changing Concepts of Liability” *Business Lawyer*, Vol. 24, No. 1 (November 1968), 43)

²⁵⁴ Haynes (1921), 209

²⁵⁵ Haynes (1921), 209

²⁵⁶ Haynes (1921), 210

By making a good showing of profit during times of economic depression, the deferral method helped convince company directors that business operations, including R&D, did not need to be cut from the budget in order to save on costs. Deferral thus helped managements manipulate internal decision making, in addition to the advantage of equalizing income, examined earlier.

At this point, it should be pointed out that this strategy may have been quite useful in keeping R&D organizations alive during the first quarter of the twentieth century. At that time, many boards of directors still viewed R&D as an unnecessary expense, as did other members of the organization. As economic historian Naomi Lamoreaux explains, “even the largest enterprises of the time were reluctant to put too much weight on internal R & D.”²⁵⁷ Initially, R&D pioneers faced a cool reception from many of their new, corporate colleagues. Having risen to the rank of Research Director since first joining Bell Laboratories in 1904, the one-time MIT graduate student-turned-engineering professor, Frank Jewett, recalled the “hard internal sledding of the initial years.”

His many years organizing, observing, and nurturing R&D had made it plain to Jewett that “the first years of every real research organization have been soul-trying years” in which recruits from academia were treated as “intruders” and “impractical theorists.” “The first big job,” therefore, “was to sell the new idea and to acquire that confidence and respect without which nothing could be accomplished.” William S.

²⁵⁷ This passage from Lamoreaux (2010, 13) continues: “Western Union...its managers were not convinced that this was the best strategy for staying on the frontier in a time of rapid technological change.... The position of American Telephone & Telegraph Company at this time was even more extreme. As T.D. Lockwood, head of the company’s patent department, explained: ‘I am fully convinced that it has never, is not now, and never will pay commercially, to keep an establishment of professional inventors, or of men whose chief business it is to invent.’ ... Not until Theodore N. Vail became president of the company in 1907 was this policy reversed... More generally, ... an important function of firms’ early research facilities was to evaluate outside technologies for possible purchase.”

Wheeler, Research Director at McCallum Hosiery Company, agreed: “This is lesson number one for any Research Department—it must be of some service to those from whom it expects to get cooperation. . . . We needed a lot of information and help from every department before we could be of much use to others.”²⁵⁸ The tenuousness of R&D in the opening years of the twentieth century meant that researchers who could demonstrate how they could be of use to their new colleagues, were better able to forge genuinely cooperative relationships. As Wheeler poignantly observed, “New fangled ideas choke when crammed down the throat,”²⁵⁹ and neither R&D nor accounting was any exception.

However, even with the gradual shift in attitude that, by the start of the Cold War, had rendered R&D a valuable end in itself,²⁶⁰ there is no indication that accountancy was headed anywhere near a general repudiation of the deferral method. Thus, even after the middle of the century—when the glamour of R&D had more fully penetrated the American industrial and consumer landscape, when directors incredulous towards the competitive necessity of R&D were far fewer in number, and when R&D was recognized as a necessary, not extraordinary, business expense—there were still those who insisted upon the deferral method.²⁶¹

Kilduff’s presentation was about as far as any accounting writer of the 1920s would go in detailing their views on what a proper use of deferral for R&D would look

²⁵⁸ William S. Wheeler (June 1929) *NACA Bulletin*

²⁵⁹ Wheeler (June 1929) *NACA Bulletin*

²⁶⁰ This shift in the valuation of R&D from a *means* to an end, to an *end itself*, is more fully discussed in Part I of this dissertation.

²⁶¹ In all my research, I have found no other instance in which company decision-makers, like Haynes, offered up such candid admissions as to their companies’ justifications for deferment. When asked for their reasons, executives other than Haynes have only ever pointed to abstract theories, most frequently the matching principle.

like. As for bankers and financial analysts, deferred charges were “clearly different in some sense from cash and receivables,” and although the discussion seems not to have inspired any change in prevailing practice, “the question often arose whether deferred charges would yield anything at all on liquidation.”²⁶² On this question, Haynes was not the only participant at the Second International Cost Conference to share his views. Responding to Haynes, A.S. Merrifield of the Norton Company, a successful and conservatively-run machine tool manufacturer in Massachusetts, spoke unequivocally when he insisted that, “Deferring expense from the present mortgages the future.”

The coming periods will have their own quota of expenses, which may be all that they can take care of. The ‘pay as you go’ policy is the only sound procedure to follow in the long run. Conservatism will not countenance deferred abnormal operating expenses as a valid balance sheet asset. Showing abnormal expenses as a debit profit and loss item, however, will meet any auditor’s or banker’s requirements. ... All current expenses are then taken up in the monthly profit and loss statement [and] management can be said to have adopted a most conservative policy, for values are then more nearly what could be realized in case of liquidation.²⁶³

Presenting at the same conference was Horace G. Crockett of the New York branch of Scovell, Wellington & Company, a cost accounting and industrial engineering firm rooted in Boston. In line with his fellow Massachusettan, Crockett similarly discouraged the use of the deferred charge method. Extending the use of this method into “doubtful” areas would tend to undermine the very purpose of a balance sheet as “a statement of the condition of a business at a given moment.”²⁶⁴ Were we to do otherwise, Crockett asked rhetorically, “what advantage do we gain? We are only postponing that evil day,

²⁶² Heath [1978, p. 34] Norman and Wooton have argued that, “under this reasoning,..deferred charges may be deemed closer to long-term assets than current [assets] and thus classified as such”(Normand & Wooton (2001, 93)

²⁶³ Merrifield 1921, 212-3

²⁶⁴ Crockett 1921, 220

because...nobody can say with assurance that [even the] average of the past will hold for the future.”²⁶⁵

A third Massachusettan at the conference chimed in, adding “it seems to me that this would be both an unsound and a dangerous procedure.”²⁶⁶ The danger, Robert Montgomery explained later, is that if “the business is not successful there will be no future profits to which the deferred items can be charged. Therefore,” he pleaded, “the auditor should use every argument he can muster to induce his client to absorb these expenses as soon as possible.”²⁶⁷ After all,

More than one enterprise has been wrecked by the failure to look preliminary or establishment expenses squarely in the face. The temptation to state the current operations in such a way as to show a profit was too strong; so those concerns have gone along from year to year, the burden increasing instead of diminishing until the inevitable day of reckoning, when it was realized that liabilities cannot be liquidated with capitalized expenses.²⁶⁸

By 1927, Montgomery was even more emphatic about the ongoing problems involving balance sheet valuations. While favorably reviewing Hatfield’s *Accounting, Its Principles and Problems* (1927), he expressed his “hope that there will be more rapid improvement in the designation of assets and liabilities in many published balance sheets.”²⁶⁹

It seems rather ridiculous that the only agreement among accountants is that the two sides of a balance sheet should balance, whereas there are listed among assets items which by no stretch of the imagination could be called assets and there are items listed among liabilities which under no possible construction of the work could be called liabilities.²⁷⁰

²⁶⁵ Crockett 1921, 221

²⁶⁶ 1921, 226

²⁶⁷ Montgomery 1922 *Auditing*, p577

²⁶⁸ Montgomery, *Auditing* (1922), 574-5

²⁶⁹ Montgomery (June 1927), 190

²⁷⁰ Montgomery (June 1927), 190.

Barely a year before the infamous stock market crash of October 1929, another leading accounting theorist, Charles Couchman, echoed Montgomery's concern in an address delivered before the annual meeting of the American Institute of Accountants. Discussing the "Limitations of the Present Balance-Sheet," Couchman first explained that "accountants have attempted as far as possible to apportion the effects of transactions to their proper periods of time."²⁷¹

This apportionment between periods is in many cases the result of exact verification, as in the case of cash and various other current assets, or the result of arithmetical calculations, as in the case of prepaid items which by contract cover definite periods of time.

Other allocations are the result of estimates only, as no enforceable contract governs the allocation. Under this heading comes the estimated loss on receivables, depreciation or obsolescence of fixed assets, and the apportionment of certain types of deferred charges. ...but the proper allocation of values to a balance-sheet of a given date constitutes one of the accountant's chief difficulties.

Accountancy, if it fulfils any function worthy of the place it now occupies in the business world, must base its records and its financial statements upon something that is definite or as nearly definite as may be. Financial transactions, either in the form of accomplished acts or of contracts of a legally binding nature, supply a basis of definiteness and of a fair degree of accuracy upon which the accountant may rear his structure of statements and reports.²⁷²

The emerging practice of treating R&D as a deferred asset thus seems to have taken hold in the 1920s, at a time when concerns were steadily growing among bankers, analysts, and a relatively small contingent of accountants over the veracity of balance sheets.²⁷³

²⁷¹ Couchman (1928), 256

²⁷² Couchman (1928), 256

²⁷³ In a book published the same year as his above-quoted speech before the AIA, Couchman made the following observation of bankers' attitude towards R&D in particular: "Many organizations spend a great deal of money in experimental work, hoping to develop new processes or formulas or to better old ones. ... The account 'experimental expenses' in a balance-sheet indicates costs which may eventually be capitalized or may be written off as losses, depending upon results. It is, therefore, an uncertain asset and is usually given little weight for credit purposes." Couchman *The Balance-Sheet, Its Preparation, Content and Interpretation* (New York: The Journal of Accountancy, Incorporated, 1924), 128-9

Calls were made imploring the last-named group to resolve ambiguities in the conceptual toolkit of their profession. Concealing differences between the various types of deferred items was one way in which the old doctrine of *caveat emptor* came into play.

Take, for example, the exchange in 1899 between the president of the American Sugar Refining Company, Henry O. Havemeyer, and a member of the US Industrial Commission, which was established by Congress in June 1898 to investigate possible restraints on trade. To Havemeyer the commission member posed the question: “You think, then, that when a corporation is chartered by the State, offers stock to the public, and is one in which the public is interested, that the public has no right to know what its earning power is or to subject them to any inspection whatever, that the people may not buy stock blindly?” Havemeyer’s response is telling of the culture of the times: “Yes; that is my theory. Let the buyer beware; that covers the whole business. You cannot wet-nurse people from the time they are born until the day they die. They have got to wade in and get stuck and that is the way men are educated and cultivated.”²⁷⁴

In 1963, having just published a contemporaneous critique of deferred charges in the *Harvard Business Review*, David F. Hawkins presented a different, more historical analysis which identified four “principal reasons why corporate managers were so secretive with regard to their company’s financial affairs.” First, “there was no tradition of publicity [holding over from the nineteenth century to inform practices of the twentieth]; management believed the public had no right to information on these matters; managers felt that by revealing financial information they would unwittingly assist their competitors;

²⁷⁴ *Preliminary Report of the Industrial Commission on Trusts and Industrial Combinations* (Washington DC, 1900), 122.

and, to many, the doctrine of *caveat emptor* seemed as applicable to buyers of securities as to purchasers of horses.”²⁷⁵ The doctrine was deeply-rooted in nineteenth-century American commerce, and was recognized as legitimate by the public and the common law at least through the 1920s.

Although its legitimacy waned into the twentieth century, its impact on investors did not. As one accountant proudly wrote in 1928, “the age-old doctrine of *caveat emptor* is just as applicable to investors today as then.”²⁷⁶ Even when pressure began to mount for increased disclosure of a company’s financial affairs, the deferral of expenses offered one means for preserving managerial discretion against the demands of investors and creditors. Reported income could be manipulated by deferring charges to income at whatever amounts, and for however long, managements so chose.²⁷⁷

3.6. Managerial Judgments

It was in the 1920s when, relatively speaking, a larger number of firms found it useful to set up their own R&D organizations. For smaller firms, this might mean expanding or else hiring for the first time a few university-trained scientists and engineers and equipping them with supplies. For several of the country’s largest firms, such as Eastman Kodak, Bell Laboratories, General Motors, Corning, US Steel, and Du Pont, the growth of R&D in the 1920s meant further disentangling specialized departments and

²⁷⁵ David F. Hawkins “The Development of Modern Financial Reporting Practices among American Manufacturing Corporations” *The Business History Review*, Vol. 37, No. 3 (August 1963): 141.

²⁷⁶ Book review of Paul-Joseph Esquerré’s *Accounting*, by W.H. Lawton, *Journal of Accountancy*, Vol. 45, No. 3 (March 1928), 228

²⁷⁷ Indeed, it was this precise freedom of maneuver that William S. Kemp of Holtzer-Cabot Electric celebrated in “Development Costs and their Liquidation,” a talk delivered at the First New England Regional Cost Conference in July 1922. (Oct 1922 issue of *NACA Bulletin*)

subsidiary research organizations from their parent firms or other departments therein. In many cases, the role of military contracts with the government spurred this expansion, in small and large firms alike.²⁷⁸

In larger firms, especially, the costs of operations within these new or updated laboratories were written off as they were incurred. This could happen in any of several ways, all of which depended on how a particular firm's accounting system was designed. For example, R&D expenditures could be written off (or paid for, provided for) directly against retained earnings or out of profits earned through manufacturing operations. Alternatively, these expenses could be deducted from a separate reserve out of income previously set aside for this purpose, or they could be written off indirectly as charges to general or selling expenses. These various differences—while certainly important for other historical studies of cost accounting—are not especially relevant here, so long as expenditures were recovered within the same period they were incurred. The crucial feature of the current write-off procedure, is that the recovery of these expenditures does *not* hinge upon hope that future income will be plentiful enough to absorb losses from unsuccessful projects.

Although early twentieth century firms rarely disclosed very much financial information to investors, a report issued to shareholders in 1911 by Westinghouse Electric

²⁷⁸ A similar pattern occurred following the end of World War II, with companies spending massive amounts of capital on lavish R&D facilities that were increasingly isolated from the demands of other departments in their parent firms. The leading study on this topic is easily Scott G. Knowles and Stuart W. Leslie's "'Industrial Versailles': Eero Saarinen's Corporate Campuses for GM, IBM, and AT&T," *Isis*, Vol. 92, No. 1 (2001): 1-33. Others include David Kaiser. "The Postwar Suburbanization of American Physics." *American Quarterly*, Vol. 56, No. 4 (2004): 851-888; Patrick Vitale. "Cradle of the Creative Class: Reinventing the Figure of the Scientist in Cold War Pittsburgh," *Annals of the American Association of Geographers*, Vol. 106, No. 6 (Nov. 2016): 1378-1396; Hyungsub Choi. "The Boundaries of Industrial Research: Making Transistors at RCA, 1948-1960," *Technology and Culture*, Vol. 48, No. 4 (Oct. 2007): 758-782.

and Manufacturing Corporation offers some insight into the accounting treatment given R&D. Following the company's failed attempts to reduce its levels of debt, its subsequent filing for (October 1907) and removal from (December 1908) receivership,²⁷⁹ the firm's lack of transparency, its failure to pay regular dividends, and its disregard for common stockholders, Westinghouse disclosed for the first time details of its company's R&D accounting practice. Setting this Pittsburgh-based firm apart from other industrial giants, the 1911 report has been described as "a model for its time."²⁸⁰

In this once-celebrated report, Westinghouse directors pointed to the cost of R&D, among other items, as justification for their decision to withhold earnings that were otherwise distributed to shareholders.²⁸¹ "It must be borne in mind," reads the report, "that your Company must keep pace in technical skill and inventive ingenuity with its competitors even though their combined capital and manufacturing facilities are greatly in excess of yours. The cost of all new development and redesigning is charged monthly as a part of the current cost."²⁸² By 1911, R&D expenditures at Westinghouse were already considered necessary costs of doing business, and as such, these expenditures were absorbed as incurred, on a monthly basis.

Even though, as a whole, detailed disclosures were considerably lacking from financial reports prior to passage of federal securities regulation in the early 1930s,²⁸³

²⁷⁹ Mary A. O'Sullivan "Living with the U.S. Financial System: The Experiences of General Electric and Westinghouse Electric in the Last Century." *Business History Review*, Vol. 80, No. 4 (Winter 2006): 633-4.

²⁸⁰ Richard P. Brief. "Corporate Financial Reporting at the Turn of the Century," *Journal of Accountancy*, Vol. 163, No. 5 (May 1987), 154.

²⁸¹ For the decade following the company's reorganization in 1908, Westinghouse was "run in a much more financially conservative fashion, and its growth was largely constrained by the internal funds at its disposal"(O'Sullivan 2006, 634).

²⁸² As quoted in Brief (May 1987), 155.

²⁸³ "Prior to 1934, information provided in the annual reports, especially those issued by WHS, was scanty, and its objectivity was questionable. After that year, the standards of disclosure for U.S. public companies improved dramatically with the passage of federal securities regulation and the formation of the Securities

leaders of the Pennsylvania Certified Public Accountants (CPA) society approved of Westinghouse's treatment of R&D as a current expense. At the tail end of 1912, the *Journal of Accountancy* discussed a series of questions which were included in the Pennsylvania CPA examination on the topic of experimental expenses:

Shall the expenditures for equipment, maintenance, salaries and experimental purposes be (all or part) charged up to the year in which they occurred, or shall they be capitalized and then distributed over a term of years? If charged off during the year when incurred, shall they be included in the manufacturing costs or simply as a general expense? If they are to be distributed over a term of years, then how shall the time and amount be determined? Since the same annual outlay for maintenance is going on, is it desirable to defer any of the cost thereof?

... Since the annual cost of [a hypothetical experimental] department will be practically the same, or increasing if anything, and since it is maintained by the management without any assurance of lasting results, it would seem advisable to charge the entire running expenses thereof to Profit and Loss.²⁸⁴

Pennsylvania CPAs were far more likely than the average public accountant to have intimate knowledge of the intricacies of accounting for R&D. For the first several decades of the twentieth century, Philadelphia, Pittsburgh, and their surrounding areas were hotbeds of industrial research activity. From voluntary associations such as the Chemical Foundation, the Textile Alliance, and the American Dyes Institute; to university departments such as those at University of Pittsburgh, Carnegie Tech, and the University of Pennsylvania; to nonprofit organizations such as the Mellon Industrial Research Institute; to corporations such as Westinghouse, U.S. Steel, and Alcoa; Pennsylvania

and Exchange Commission (SEC). From then on, the amount of information available in the annual reports of GE and WHS was substantial, and its quality was relatively high. However, even then, important details, especially on external financing, were often omitted from the statements”(O’Sullivan 2006, 627).

²⁸⁴ Bennet (November 1912), 275-6.

CPAs had more opportunity than most to learn the ins and outs of R&D accounting.
decline

Many of these CPAs, as well as those in the R&D-friendly cities of New England,²⁸⁵ believed that the costs of maintaining an in-house R&D organization were annually recurring and, therefore, unlikely candidates for treatment as capital assets. This regularity, together with the observation that there are no assurances when it comes to the output of experimental activities, was enough for the authors of the Pennsylvania CPA examination to recommend immediate expensing. Similar reasoning is found in “Uniform Cost Accounting Methods,” a report by a committee of cost accountants issued in February 1920 by the American Dyes Institute and which identified R&D as one of seven items that “stand out as the reasons for the difficulty of standardization” in the analysis of costs.²⁸⁶ While the report did not deny a role for the use of deferral over multiple fiscal periods, particularly in the case of new companies still finding their footing, its ultimate recommendation for handling R&D pointed in the opposite direction.²⁸⁷ The committee report observed that “many of the strongest companies will prefer to write this group of charges off currently.”²⁸⁸ Since in “the nature of the dye business, this expense

²⁸⁵ Whereas Pennsylvania was home to all of the R&D-related institutions listed in the previous paragraph, New England was home to the New England Council formed in 1925 as a means of addressing the region’s rapidly declining textile manufacturing base. The promotion of new product-R&D had always been one the Council’s aims, and in 1938 it formed the New Products Committee, led by MIT President Karl Compton. See: Albert N. Leman, “Research for Small Business,” *Forbes* (February 1, 1940), 16-17; David Koistinen’s *Confronting Decline: The Political Economy of Deindustrialization in Twentieth-Century New England* (Gainesville: University Press of Florida, 2013).

²⁸⁶ February 1920, AmDyesInst comm of cos acctnts pubs its report “Unif Cost Acctg Meths” p5

²⁸⁷ Even in this case, the committee limited its approval of deferment by recommending that R&D costs be classified as part of the organization expenses of the firm—not as deferred R&D charges per se.

²⁸⁸ For these well-established companies, the cost of R&D performed with the aim of improving an existing product, was recovered through sales of that product in the same fiscal period. Otherwise, any costs incurred on the development of “products wholly of the future would be written out of current profits, either directly or in some indirect manner”(1920, American Dyes Institute, p22)

will occur year after year, it is advisable to take up the actual expenditure each year as incurred, rather than to pyramid it ahead.”²⁸⁹ The report concluded that “the general opinion of the committee is that Cost of Research and Development should not be capitalized, but instead, handled as a current cost.”²⁹⁰

In reaching its conclusion, the American Dyes Institute cost committee acknowledged that, despite “substantial agreement as to research upon products already in manufacture,” there remained “a difference of opinion as to cost of breaking new ground.”²⁹¹ As to the former, the general consensus appeared to be that R&D work “applying to operations already under way”²⁹² were expensed as incurred on a monthly basis. By contrast, R&D “which is concerned with the working out of new processes and development of new products”²⁹³ could either be expensed along with other operating charges by the end of the period in which costs were incurred, or else deferred over a short number of years.

Evidently, many writers gave their implicit assent to the conceptualization that associated R&D for improvement purposes, with R&D as a recurring expense. Presumably, this is because improvements were understood to be performed on an ongoing basis. Many writers then took the next step in distinguishing recurring and improvement-oriented R&D, on the one hand, from the occasional or “special” projects that sought to develop new products or processes, on the other. The former were routine business expenses properly expensed in the period incurred; the latter were unusual

²⁸⁹ 1920, American Dyes Institute, p22

²⁹⁰ American Dyes Institute “Uniform Cost Accounting Methods”(February 1920): 28

²⁹¹ American Dyes Institute “Uniform Cost Accounting Methods”(February 1920): 28

²⁹² American Dyes Institute “Uniform Cost Accounting Methods”(February 1920): 22

²⁹³ American Dyes Institute “Uniform Cost Accounting Methods”(February 1920): 22

expenditures that were appropriate candidates for deferral *if* outcomes were known in advance to be profitable. We see this play out in Kilduff who, after listing the steps necessary to verify the use of deferral for R&D, contrasted the following conditions: “Unusually large expenditures made for experimental work in connection with the development and perfection of new products, by-products, processes, etc., may properly be deferred as charges to the operations of subsequent periods. Where, however, an experimental department is maintained, such expenditures should not be deferred.”²⁹⁴

The distinction between irregular, new-product R&D, on the one hand, and recurring, improvement-oriented R&D, on the other, was a distinction that built upon another: that between capital expenditures and revenue expenditures. As we saw in Chapter 2, the classification of expenditures into one of these two categories was never a straightforward exercise and involved a whole set of changing assumptions about what a firm owed its investors and creditors, as well as whether certain types of expenses were necessary to remain viable as a “going concern.” By the early 1930s, according to one author, “terms such as ‘non-recurring,’ ‘fortuitous,’ ‘speculative,’ ‘sporadic,’ and ‘non-operating’ were used frequently to describe” changes in a company’s stock of capital.²⁹⁵ Certainly, we see this refinement of terminology distinguishing capital from non-capital expenditures in this present study, although, in our case, this occurs earlier than the 1930s. From the 1920s and evolving in subsequent decades, R&D was increasingly subjected to various capital-revenue classifications. These could be made implicitly or explicitly and can be generally summarized by the following binaries: improvement/new,

²⁹⁴ Kilduff 1924, 55

²⁹⁵ Harold E. Arnett, “Application of the Capital Gains and Losses Concept in Practice.” *The Accounting Review*, Vol. 40, No. 1 (January 1965): 55.

recurring/non-recurring, special/routine, operating/non-operating, material/immaterial, regular/irregular, and ordinary/extraordinary.

We see such refinements at play in the proceedings of June 1926 roundtable discussion held at the Seventh International Cost Congress. At this concluding session of the conference, questions collected from members in the months prior were now raised before those in attendance. Walter Coapman of North East Electric Company in Rochester, New York took over the reins when discussion turned to the question, “To what should cost of experimental work be charged?” In the interim period since the question was first submitted, it had been broken down into a series of related questions which Coapman read from aloud: “When should experimental work be charged as selling expense? When as part of manufacturing burden [or overhead]? When, if ever, capitalized?”²⁹⁶

Coapman himself favored charging to overhead the expense of regularly-performed R&D work which “a progressive management is always doing” for the sake of improving upon products that were already being produced.²⁹⁷ By recovering these expenses through the medium of overhead, Coapman was in effect advocating the absorption of improvement-oriented R&D in the current period. His reasoning was clear: companies in general could be more certain that R&D for improvement purposes would deliver on its promises more than half the time. With such expenditures, he felt confident that “in the long run you are going to win out more times than you lose.”²⁹⁸ This meant

²⁹⁶ 7th Intl Cost Conf, 1926, 263

²⁹⁷ “[F]or instance, in change of design, in which you are trying out something new to see if it will be cheaper than your present design, either in actual machine work or in materials, or something like that.” 7th Intl Cost Conf, 1926, 264

²⁹⁸ 7th Intl Cost Conf, 1926, 264

that the cost of improvement would be more than likely recovered through the income made possible by more efficiently designed and manufactured products.

The same reasoning would not hold true with respect to R&D performed with the aim of developing something new for the firm, something unrelated to its current products or processes. As with improvement-R&D, Coapman also advocated the current absorption of new product R&D. In the latter case, however, he justified current expensing on a different basis:

[I]f a company is developing a new product and does not know what it is going to amount to, it is certainly not a part of selling expense, neither is it a part of manufacturing burden, and it probably is poor policy to set it up and capitalize it. It seems to me that such experimental work is simply a sort of appropriation from profits. There may be experimental work in connection with product that is purely a cost of manufacturing, *but I think experimental work which is really development work for a new product is something entirely different*; it is really an appropriation out of profits rather than part of selling expense or manufacturing burden.²⁹⁹

While costs of improvement would be absorbed indirectly through overhead, costs to develop new products would be absorbed by a direct charge to profit and loss. In both cases, costs were recovered in the current period. Unlike efforts to improve present products, however, any effort to develop something entirely new to the firm carried with it an inescapable uncertainty of outcomes. If efforts to *improve* current products failed, the losses would be absorbed through sales of those products. If efforts to develop a *new* product failed, there would be—by definition—no current products to absorb the cost of failure. Such losses would have to be absorbed not by product line but as a deduction from overall company profits.

²⁹⁹ 7th Intl Cost Conf, 1926, 263. Emphasis added

Following Coapman's discussion on these two forms of R&D, another participant at the Seventh International Cost Conference posed the question, "How about capitalizing experimental expense? Does anybody think it ever should be capitalized?" To this, Coapman offered his own qualified answer in the affirmative: "I think it should be capitalized," he explained, but only "if you are starting out with a new product *in which you have a very definite knowledge that there is a field for it*, and you are going to spend a lot of money and *you know it is going to come back to you*."³⁰⁰ In search of clarification, the chairman of the conference session responded by asking, "If we started out to make a new calculating machine, for example, we would capitalize everything we spent until the time we got the machine on the market?" "That is the idea," replied Coapman, "then spread the preliminary cost over production. ... We estimate how many we are going to make and we add so many cents per unit."

What Coapman described was standard procedure for the amortization of more conventional assets such as patents that were recognized as part of a company's long-lived capital, but, asked another participant, "suppose you do not make it?" That is, what if new-product R&D *did* produce a capital asset, but, for any number of reasons, management decided *not* to manufacture it? "In that case we lose," explained Coapman. "On the other hand, we may make more than we planned and if so there would be a profit. You are obliged to guess most of the time."³⁰¹ Where Coapman differed from many of the other writers we have seen, is the emphasis he places on the uncertainty of

³⁰⁰ 7th Intl Cost Conf, 1926, 264-5. Emphasis added.

³⁰¹ (7th Intl Cost Conf, 1926, 264-5)

outcomes, and it is this emphasis that leads him to discourage the deferral of new product R&D costs.

Compare Coapman's position, for example, with that of Harry G. Specht, Vice President and General Manager of the Eastwood Wire Corporation who, in a talk before the Bridgeport, Connecticut chapter of the NACA, argued that "money spent during a period of depression in developing a new product should be set up as a deferred charge to expense and amortized during a profitable period."³⁰² In reporting on Specht's talk, the *NACA Bulletin* editors added their own brief response, "It might be interesting some time to have this question discussed to see how many of our own members agree with that theory."³⁰³ What this editorial response to the president and general manager of an electric company draws out, is a distinction between top management's perspective and that of the accountant. This was a most tricky area to navigate for those members of a profession then still in its nascency and without the respect of those it served.

3.7. Aspirational Accounting

As more accounting authors began experimenting with deferred charges, discussion of R&D became wrapped up in this trend. Even Robert Montgomery, vigilant as he was about the abuses of the deferral method, only went as far as calling its application to R&D "doubtful." Montgomery contended that, when the benefits of certain expenditures are doubtful, "good accounting practice cannot prescribe specific treatment. The conservative method is to immediately charge off all doubtful items, but if

³⁰² Harry G. Specht talk before the Bridgeport, CT chapter of NACA, "Development of a Cost Reduction Program in a Manufacturing Plant," as reported in the *NACA Bulletin* of December 15, 1930, p.699.

³⁰³ Specht, 699.

there is a question as to what periods will receive the benefit and if the amount involved is substantial, good accounting practice leaves to the accountant the option of charging off such expenditures over a certain period of years or of charging them off currently.”³⁰⁴

Because the “results of experimental and development work cannot be foreseen and, since subsequent benefits are doubtful,” Montgomery concluded that “the treatment is to some extent optional.”³⁰⁵

By the late 1930s, Montgomery would all but give up on the possibility of making deferred charges a less abused, meaningful category. Speaking of discounts on bonds—a deferred charge which, that year alone, engaged his expertise in three matters of litigation—Montgomery wrote that, “Of course, it is not an asset in the ordinary sense of the word. It is a deferred loss. Accounting convention justifies the means by the result and we call it an asset. Rather than attempt to change the ‘asset’ heading on balance-sheets to ‘Assets and deferred items,’ I favor leaving it alone. I no longer have to deal with thoughtful students who are and who should be troubled by what we have handed them.”³⁰⁶

Montgomery’s former colleague at Columbia University, James L. Dohr, did only slightly more to point up the problem of uncertain outcomes. In his 1924 textbook, *Cost Accounting Theory and Practice*, Dohr argued that, “Where the experimental and development work is a continuous process, the costs of each period may be included in the operating expenses of that period.”³⁰⁷ However, he observed that some firms handled

³⁰⁴ Montgomery (1922), 574

³⁰⁵ Montgomery (1922), 575.

³⁰⁶ Robert H. Montgomery. “The Curse of Balancing, or Theory v. Practice,” *Journal of Accountancy*, Vol. 63, No. 4 (April 1937): 281.

³⁰⁷ James L. Dohr, *Cost Accounting Theory and Practice* (New York: The Ronald Press Company, 1924), 372

R&D costs the same as they would an “extraordinary loss...which the owner does not wish to charge to the profit and loss account of a single year.”³⁰⁸ Like Montgomery, Dohr left the difficult problem of deferring or expensing in the hands of management. If “special research or experiment is undertaken for the development of new or improved products, the cost will be carried...to be written off in future periods when the new product is manufactured and marketed.”³⁰⁹

Having forfeited accounting judgments to management, Dohr’s text seems to be a mere description of extant practices. Still, he did more than Montgomery when he insisted that the deferral “procedure should be followed only when it is clear that future period will receive the benefit of the expenditures.”³¹⁰ While Dohr readily accepted the deferral of “prepaid insurance, interest, taxes, discounts, subscriptions, etc.,” he was more hesitant about items “like advertising, research, and experiment expense, patent development, etc., [which] involve more difficulty and should be carried to future periods only when it is clear that those periods will be benefited by the expenditure.”³¹¹ What made the costs of one project a more “clear” candidate for deferral than the costs of another, he did not say.

Like Montgomery and like Dohr, William Kent backed away from recommending one treatment over another in his own (1918) text, *Bookkeeping and Cost Accounting*. A consulting engineer and associate member of the Taylor Society, Kent pointed out that,

³⁰⁸ Dohr (1924), 45.

³⁰⁹ Dohr (1924), 372.

³¹⁰ Dohr (1924), 372.

³¹¹ Dohr (1924), 67.

There are often some costs incurred in connection with a business that are neither factory operating costs nor commercial or selling costs, such, for example, as the...cost of experiments which may or may not prove successful. ...and whether or not any part of them should be charged to factory costs of production is a matter for the management to determine as is also the question whether they should be entirely written off at the end of the year, by charging them to Profit and Loss, or carried in the books as assets, a portion of them being written off each year.³¹²

While the accountant seemed capable of enacting the practice of current expensing, the decision to defer was best left in the hands of managements. This, apparently, was the limit at which the accountant could no longer exercise sound professional judgment. We see this division of responsibility between the accountant (whose purview was limited to discerning current-expense situations) and management (capable of discerning deferral situations) echoed in the work of CPA, accounting academic, and Harvard Business School graduate Norman B. Clark. In the February 1934 issue of the *Bulletin*, Clark argued that where experimental efforts,

are going on *constantly* with about an *equal benefit* to all [product] lines, and no great variation in the *probability of success*, and the work is compulsory, then charging it manufacturing expense is quite justifiable. If they are irregular, and of unequal benefit to all lines and highly problematical as to success, they should be charged as an administrative expense, or an extraneous loss [ie, charged to profit and loss]. If they are in the nature of bidding for business, they may be considered a selling expense.³¹³

By contrast, costs incurred to create new products should be capitalized (1) when they are large in amount, and (2) when “the honest judgement of the management” is that “they will enhance earnings and asset valuations substantially over a period of time.”³¹⁴ The arguments presented by Clark and others seem to suggest that, as R&D found a home in

³¹² *Bookkeeping and Cost Accounting for Factories* (New York: John Wiley & Sons, Inc, 1918), 128.

³¹³ Clark (1934), 709. Emphasis in original.

³¹⁴ Clark (1934), 708.

a growing number of firms, the difference between purposes, scales, and frequency of R&D efforts might have been fleshed out more fully in the 1920s and 1930s. This did not happen. Very little progress was made in teasing out specific criteria for the handling of R&D. Clark himself made this point when he noted,

I was rather surprised when this subject first piqued my interest to discover the paucity of material regarding it in text books. Many are silent upon it and most of the others dismiss it with a brief adjuration to be very conservative. Looking through the files of the publications of our own organization I found only one article on the cost of making an experiment, and a total of three pages in other places. Knowing the vast sums which are constantly being spent upon research, one would expect to find more.³¹⁵

A few opinions were expressed, a handful of articles were written, but for the most part, the profession stepped away from the task and left such complex accounting questions in the hands of managements.

Now, we turn our attention to considering why this trend may have been adopted by certain businesses. We start in the mid-1930s, when a statement of best practices was being compiled for the benefit of the newly-organized Securities and Exchange Commission. First commissioned in 1935 by the Haskins & Sells Foundation and published in 1938 by the American Institute of Accountants (AIA), *A Statement of Accounting Principles* (SAP) described all of “land, buildings, machinery, and equipment” as being “really in the nature of a deferred charge against the future income they will help to produce.”³¹⁶ Widely-distributed at the time, SAP has been aptly described by a leading

³¹⁵ Clark (1934), 701. The only other NACA article Clark found “on the cost of making an experiment” must have been the June 1929 essay in the *Bulletin* by Earl P. Stevenson, Vice President and Director of Research at Arthur D. Little and Company, a commercial R&D consulting and contracting firm located in Boston. Stevenson’s focus, however, was not on accounting but on such interpersonal and organizational issues, such as gaining the acceptance of researchers in firms unacquainted with the demands of research.

³¹⁶ Thomas Henry Sanders, Henry Rand Hatfield and Underhill Moore. *A Statement of Accounting Principles* (New York: AIA, 1938), 58-59.

accounting historian as “a defense of accepted practice”³¹⁷ which Henry Rand Hatfield, one of its three authors, “was so fundamentally in disagreement.”³¹⁸ Hatfield’s reservations aside, SAP “did little, if anything, to narrow the areas of differences.”³¹⁹ As for the deferral of R&D, SAP’s discussion on this particular “area of difference” was cavalier, at best: “often,” it reads, experimental expenses, bond discounts, improvements on leased lands, and other deferred charges “do not constitute parts of regular expenses, and sometimes are abnormal losses which it is not yet convenient to write off.”³²⁰ Convenient, indeed, but for whom and towards what ends, SAP did not say.

Put another way, if the matching of deferred R&D expenditures to future R&D-generated assets was particularly advantageous, then what were these advantages and whom did they serve? Was deferring R&D “convenient” to industrial researchers? What about to creditors or managers? Or was it perhaps investors who were to be advantaged by deferral policies? A few years after SAP’s release, at a conference of auditors held in November 1942, Victor H. Stempf was more suggestive as to whom the convenience of matching would benefit. A partner at the New York public accounting firm of Touche, Niven & Co., Stempf made two moves in his argument that are particularly telling.³²¹

First, he defined the practice of accounting as “essentially” a process of matching, echoing Gilman’s (1939) argument that “all capital expenditures are in effect deferred

³¹⁷ Zeff (1999,91)

³¹⁸ Stephen A. Zeff (2000) *Henry Rand Hatfield: Humanist, Scholar and Accounting Educator* (Stamford, CT: JAI Press).

³¹⁹ Carman G. Blough “Development of Accounting Principles in the United States” *Berkeley Symposium on the Foundations of Financial Accounting, January 13th and 14th, 1967* (Berkeley, CA: University of California, Berkeley Schools of Business Administration, 1967): 7.

³²⁰ Sanders, Hatfield, & Moore (1938), 75.

³²¹ By the time he delivered his talk, Stempf was not only a partner at Touche, Niven & Co, but was also vice president of the AIA, past president of the New York State Society of Certified Public Accountants, past president of the National Association of Cost Accountants, and former vice president of the American Accounting Association.

charges.”³²² Next, he absolved independent CPAs, such as himself, of any responsibility for the “defalcations” and “irregularities” that might arise in the “representations of management.”³²³ With the first move, Stempf indicated the use of the deferral method as being not only available to, but expected of company accountants and managers. With the second move, Stempf suggested that, should matching go wrong—should the deferred charges give way to losses rather than assets—“the judgment” or “integrity of the client” was at fault. After all, the consequences of the deferral method were no less management’s responsibility simply because the profession of accountancy sanctioned its use. This, the “it’s management’s responsibility” defense, continued to be made over the next several decades in the face of increasing controversy and boiling to a head in the late 1960s.

Two decades after Stempf argued that accounting decisions were strictly in the hands of management, public outrage over increasingly misleading financial statements led accounting academic Robert Sterling to critique his fellow accountants and their use of what he called “the ‘management’s statement’ proposition.”³²⁴ To Sterling, who by that time was about to complete his PhD from the University of Florida, there was contained within this proposition “an embryonic tendency to escape responsibility.”³²⁵ Others academics, such as Maurice Moonitz, agreed. Moonitz was accounting professor at University of California, Berkeley and dean of Berkeley’s newly-formed Graduate

³²² Victor H. Stempf “Facilitating the Independent Accountant’s Review of Internal Accounting Control.” *Journal of Accountancy* (January 1943), 49

³²³ Stempf (January 1943), 50

³²⁴ Sterling, *The Accountants Magazine* (November 1963): 593

³²⁵ Sterling (1963): 593

School of Business Administration.³²⁶ To Moonitz, the “first and most important necessary condition for change is for the accounting profession to cease recognizing management as its master with regard to the technical content of accounting.”³²⁷

Still, as the University of Oregon accounting professor Charles Ellicott Johnson pointed out, the “basic assumption that management is responsible for the contents of financial statements has a long tradition and is deeply rooted in the mores of the accounting profession.”³²⁸ At Oregon, Johnson led the University’s Department of Accounting and Business Statistics, before being appointed to dean of the College of Liberal Arts. Part of the reason for the “deeply rooted” habit of deflecting responsibility onto managements, he argued, “is that the rulebook of accounting measurements, titled *Generally Accepted Accounting Principles*, has never been officially codified. This in itself allows management and the auditor a wide latitude in deciding on the principles that are acceptable as a fair representation of corporate performance.”³²⁹ On this point, another writer resigned himself to the observation that, “The rule of ‘generally accepted principles,’ although an absurd piece of circular reasoning, has provided the *modus vivendi* until a better way out of the impasse can be found.”³³⁰ But Johnson added another, “more fundamental” reason:

³²⁶ Several times during his career, Moonitz took leave from Berkeley to refresh his knowledge of public accounting by working at Arthur Andersen & Co. and serving as the AICPA’s director of accounting research in the early 1960s, writing and publishing ARS No.1 and co-authoring the controversial ARS No. 3. American Accounting Association, “The Accounting Hall of Fame: Maurice Moonitz” Retrieved on May 2020 from: <<https://aaahq.org/Accounting-Hall-of-Fame/members/1979/Maurice-Moonitz>>

³²⁷ Maurice Moonitz, *The Australian Accountant* (October 1968), 630

³²⁸ Charles E. Johnson, “Management and Accounting Principles.” *Law and Contemporary Problems*, Vol. 30, No. 4, Uniformity in Financial Accounting (Autumn 1965), 693

³²⁹ Johnson (Autumn 1965), 693

³³⁰ Miles Kennedy, “The Values of Accounting and of Corporations: A Review Article,” *Journal of Accounting Research*, Vol. 1, No. 1 (Spring 1963): 108-109.

A wide variety of alternative measurement methods, allowing for material variations in reported results, exists even in areas covered by official pronouncements [such as the SEC's Accounting Series Release, or the AIA's Accounting Research Bulletins]. Why does the accounting profession tolerate this kind of variety? Accountants in general are reluctant to accept collective judgment that would narrowly prescribe acceptable measurement procedures. They believe such a move would mean giving up their freedom to exercise professional judgment and would reduce their function to that of a clerk.³³¹

The variety of acceptable alternatives not only allowed accountants to refuse responsibility for their clients' potential abuses; that same diversity is what helped make them indispensable as employees, consultants, and auditors. The deferral method, in particular, offered the chance to prove one's predictive prowess. Whereas deferral involved anticipated or forecasted future income, it was the matching principle which helped validate those forecasts, and it did this, in theory, by bequeathing the accountant (how, is unclear) with the discerning judgment necessary to recognize when and to what extent the relevant income has materialized.

"Clearly," wrote accounting academic Robert T. Sprouse in the early 1970s, the "[matching] approach places a premium on judgment ... relying heavily on subjective notions of correctness, applicability, and propriety."³³² The result, of course, was "those unique accounting products that one so frequently finds in today's sheet of balances: deferred charges that are not assets and deferred credits that are not liabilities."³³³ With "maximum latitude in deciding which balances to carry forward,"³³⁴ forecasts of profitability were thereby made to appear to have foundations in theoretically-sound

³³¹ Johnson (Autumn 1965), 699-700.

³³² Robert Sprouse "The Balance Sheet—Embodiment of the Most Fundamental Elements of Accounting Theory" *Foundations of Accounting Theory*, William E. Stone, ed (Gainesville, FL, University of Florida Press, 1971), 56-7.

³³³ Sprouse (1971), 56-7.

³³⁴ Sprouse (1971), 57.

principles. Matching and deferral reinforced the “naturalness” of the other, while at the same time reinforcing the view that the accountant’s judgment was worth paying for. In both cases, therefore, judgment and reputation were at stake: that of the manager no less than the accountant’s.

Chapter 4

Standardizing the Endless Frontier

4.1. “Extremely Hard to Draw the Line”

During the Second World War, the development of accounting standards had necessarily to be set aside as government and industry practitioners found themselves prioritizing speed, quantity, and quality of output over efficiency, cost-savings, and marketability. Almost from the start of the war, the federal government’s newly-formed Office of Scientific Research and Development (OSRD) implemented a surveillance program designed to keep track of the nation’s scientists and engineers, the projects they worked on, the expertise they had to offer, and the resources at their immediate disposal.³³⁵ As the most formidable of head-hunters, the federal government reached deep into the heart of the nation to identify and call upon scientifically-trained men at will. Such surveillance efforts extended, of course, to industry no less than to academia, but for businessmen, patriotism was an insufficient inducement to commit one’s resources in the mobilization for war. For the government, getting their cooperation would require finesse.³³⁶

Disputes within universities over the inclusion of overhead costs as charges to the government; industrialists’ concerns over the rapid deterioration of their plants and machinery under the burden of full-scale war production; and contractors insistence that

³³⁵ The OSRD was established by executive order of the president, issued on June 28, 1941. A full description of President Eisenhower’s expectations for the OSRD is provided in that executive order (no. 8807). Also see Stern (1954). In October 1943, another executive order (no. 9389) gave Bush the authority to manage the OSRD and to appoint advisory committees without the president’s approval.

³³⁶ Ketchum (January 1943): 29-31; Carey (1970): 84-85.

government reimburse costs incurred in the anticipation of, or bidding for, a contract, abounded. These and other issues were addressed through a series of negotiations and revisions to the terms of contracts, made possible by the coordination of civilian administrators in the OSRD, accountants in the Bureau of the Budget and War Production Board, procurement officers in the armed services, and others. Through the success of these efforts, R&D programs were introduced to vast swaths of manufacturers that until that time never experienced this activity first hand. How R&D would fair following the war—whether it would “stick” or else get dismantled during the reconversion process—remained to be seen. Even before the war ended, a deliberate effort to influence which path the nation’s industries would take, had become a top priority for OSRD Director, Vannevar Bush. This effort took form in three major ways: first, by drawing attention to the impact of tax laws on firms’ decisions to pursue R&D; second, by advocating the creation of an independent government agency later known as the National Science Foundation (NSF); and third by reforms to patent laws that would benefit small R&D-intensive businesses as successfully as they had their larger counterparts. In this section, we will focus only on the first of these three.³³⁷ Here, the discussion will be limited to first of these three.

Towards the end of 1944, Bush asked the young Harvard economist John Keith Butters to investigate the regulatory barriers preventing firms from engaging in R&D, and in the Summer of 1945, Butters’ findings were published in the *Harvard Business Review* and

³³⁷ Interested readers can learn more about the controversy origins of the National Science Foundation in Daniel J. Kevles, “The National Science Foundation and the Debate Over Postwar Research Policy, 1942-1945” *Isis*, Vol. 68, No. 1 (March 1977): 4-26.

discussed by the *New York Times*.³³⁸ “At present,” Butters concluded, “the tax law and tax practice in this area come very close to being in direct conflict with each other.”³³⁹ This much was true as early as the 1920s, and innovations in the law during World War II only added to the confusion. With passage of the Revenue Act of 1942, six new categories of so-called “abnormal” income were introduced under Section 721 of the Internal Revenue Code.³⁴⁰ R&D was one of these six categories. By exempting the “abnormal” income earned from R&D, Congress hoped to encourage those companies whose products were valuable to the war effort to maintain their R&D programs. Crafted in times of profound urgency, these changes to the law were confusing at best and, in the decision-making authority they vested in the Commissioner of Internal Revenue, abusive

³³⁸ Having worked for the Treasury Department’s Tax Division in one capacity or another since 1939, Butters returned to Harvard as an assistant professor of financial research before being called upon in early 1945 to serve as special adviser to Vannevar Bush. It was in this latter capacity that Butters completed his survey of businesses and published his findings in the *Harvard Business Review*. Almost instantly, an editorial publicizing his essay appeared in *The New York Times* under the title, “Urges Tax Action to Spur Research: Butters Wants Law Clarified—Finds ‘Muddled’ Status is Hindering New Products.” *The New York Times* (June 26, 1945), 22.

³³⁹ J. Keith Butters “Taxation and New Product Development” *Harvard Business Review*, Vol. 23, No. 2 (Summer 1945): 451-459.

³⁴⁰ Income in the tax code is classified according to the source from which it derives, and with the Revenue Act of 1942, the third of the six categories of abnormal income under Section 721 (a) (2) read as follows: “Income resulting from exploration, discovery, prospecting, research, or development of tangible property, patents, formulae, or processes, or any combination of the foregoing over a period of more than 12 months.” Relief from excess profits taxes would take the form of allocating the abnormal income over the years to which it was economically attributable. However, such relief would also cover any “normal” income in excess of 125% of the preceding four-year average. “Abnormal” income derived from R&D, and “normal” income exceeding 125% of earnings over the 1936-1939 period, thus received the same favorable treatment under the law. See Paul D. Seghers, “What Price Section 721 Relief for Manufacturers?” *Taxes, the Tax Magazine*, Vol. 22, No. 10 (1944): 434-444.

at worst.³⁴¹ Towards the end of the war, Butters summarized the relevant tax laws then in effect,³⁴² and shared his observations of the Bureau's unofficial policy towards R&D costs:

By and large, the Bureau of Internal Revenue in its actual treatment of the handling of research and development expenditures [maintains an implicit policy that] appears to be roughly as follows: If a firm spends approximately the same amount on research and development work year after year and consistently claims these expenditures as deductions from current income, it seldom has substantial amounts of its claims disallowed. On the other hand, if the amount spent on research and development fluctuates wildly from year to year and if the taxpayer does not follow a consistent accounting practice, the Bureau quite naturally tends to be more critical.³⁴³

That tax agents of the Bureau were operating under an unofficial policy had already been brought to Bush's attention. One of the many committees Bush appointed during the war declared in a report dated April 1945 that revisions to the Internal Revenue Code were needed in order to formally codify the options to currently deduct and/or to defer and amortize R&D expenditures.³⁴⁴ In fact, the passage quoted directly above from Butters' *Harvard Business Review* essay seems to have been taken—nearly word for word—out of that April 1945 report.³⁴⁵ Even earlier, in February 1944, J.K. Lasser,

³⁴¹ “The relief provisions, dealing with abnormalities,” complained one contributor to the *Journal of Accountancy*, “are in themselves abnormal in that they represent an attempt to endeavor to make provision for many unforeseeable circumstances, whereas other parts of the tax statute are very specific and contain precise definitions.” See Troy G. Thurston, “Current Problems in Income Tax Procedure” *Journal of Accountancy*, (March 1943), 233.

³⁴² “About all the [Internal Revenue Code] has to say is that: (1) if research and development expenditures constitute ‘ordinary and necessary expenses incurred in carrying on a trade or business,’ they are deductible as such; (2) if they turn out to be worthless, they may be deducted as a loss; and (3) if they result in ‘permanent improvement or betterments,’ they must be capitalized. But the code does not state in which category various research expenditures fall” (Butters 1945, 451).

³⁴³ Butters (1945), 451

³⁴⁴ Report of the Committee on Science and the Public Welfare, reprinted as Appendix 3 of *Science—The Endless Frontier* (1945, 105)

³⁴⁵ See Chapter VI, “Taxation and Research” of the Report of the Committee on Science and the Public Welfare, reprinted as Appendix 3 of *Science—The Endless Frontier* (1945, 104). Butters was not a member of the committee which compiled the report from which he appears to have copied, so presumably, Bush or some other intermediary of like mind shared the committee's findings with Butters. On the basis of timing alone, it is very likely that these efforts were coordinated as part of what Hounshell summarized as Bush's “manipulation” of President Eisenhower. For more details on this episode, see Kevles

editor of the *Journal of Accountancy*'s "Tax Clinic," followed up on a troubling story printed by *Business Week* which reported that businessmen were growing increasingly anxious over the Bureau's handling of R&D expenditures. "Recurring rumors to the contrary," *Business Week* reassured its readers, "the Bureau of Internal Revenue has not tightened up its policy against charging research and development expenses to current income."³⁴⁶

The Bureau has thrown out a lot of deductions for research expense lately and believes that's what touched off speculation about a change in policy. Wartime tax rates have driven companies to claim deduction for expenses that they used to charge to capital. And research on war-production problems has created a flock of borderline cases that haven't been threshed out yet.³⁴⁷

As in times past, the return to a war economy meant a return of the excess profits tax, and with it, a desire on the part of corporate taxpayers to reduce reported income as much as possible. Towards this end, some companies were claiming R&D deductions that Bureau officials greeted with skepticism. The resulting confusion may have meant a reticence on the part of the business community at large to invest in R&D programs. Whether or not this was actually the case, the message throughout these discussions was clear: where ambiguities exist in the income tax law, they only serve to deter R&D. This message Bush reiterated himself both in *Science—The Endless Frontier*, his legendary July 1945 report to the US President, and again in his monograph of 1946, *Endless Horizons*.³⁴⁸

³⁴⁶ As quoted in J.K. Lasser *Journal of Accountancy* (1944), 146.

³⁴⁷ As quoted in Lasser (1944), 146.

³⁴⁸ "One of the most important factors affecting the amount of industrial research is the income-tax law. Government action in respect to this subject will affect the rate of technical progress in industry. Uncertainties as to the attitude of the Bureau of Internal Revenue regarding the deduction of research and development expenses are a deterrent to research expenditure. These uncertainties arise from lack of clarity of the tax law as to the proper treatment of such costs. The Internal Revenue Code should be amended to remove present uncertainties in regard to the deductibility of research and development expenditures as current charges against net income." This passage appears, verbatim, in both *Science—The Endless Frontier* (July 1945, 16) and *Endless Horizons* (1946, 56).

Echoes of the same theme continued to be heard. In a 1947 issue of *Taxes—The Tax Magazine*, one commentator declared, “The time is overdue for clarification and modernization of the law governing deductibility or capitalization of research and development costs.”³⁴⁹ In 1948, at the Seventh Annual NYU Institute on Federal Taxation, law scholar Jack R. Miller underscored the need to address “the perennial question of whether and to what extent taxpayers engaged in research and development activities, just to keep abreast of the time, run a risk of [owing unpaid taxes] by deducting the costs of those activities.”³⁵⁰ And in April 1950, *Business Week* published an amusing yet poignant expression of the arbitrariness of extant practice. Taking the form of a rhetorical Q&A, the segment titled, “What Costs Can You Deduct?” appeared as follows:

Q: When is a business expense not a business expense? A: When the [Bureau of Internal Revenue] won’t let you deduct it from taxable income.

Q: When won’t the bureau permit deductions? A: When it thinks expenses were not incurred mainly to meet legitimate business needs.

Q: How can BIR tell what’s legitimate and what isn’t when it’s so largely a matter of opinion? A: BIR can’t always tell—to the taxpayer’s satisfaction; that’s why so many cases end up in the U.S. Tax Court.

Q: How can the Tax Court tell? A: It uses rules-of-thumb like these: A payment is a legitimate business expense where it is made in exchange for services actually rendered—regardless of the form or manner in which the services were performed. A payment is deductible as a business expense if it is made to a bona fide employee. In borderline cases, the taxpayer usually gets the nod.³⁵¹

At the time, a new round of military mobilization brought renewed urgency to the problem of R&D accounting for tax purposes, and in the early months following US

³⁴⁹ Leon L. Rice, Jr., “Research and Development Costs” *Taxes—The Tax Magazine*, Vol. 25 (June 1947): 41.

³⁵⁰ Miller would go on to become a founding judge of the Federal Circuit Court of Appeals. In a humorous aside, the appointment was made by President Nixon who can be heard on the leaked Nixon Tapes referring to Miller as “a jackass” in a discussion with Henry Kissinger. This arguably lends more credibility to Miller than not.

³⁵¹ *BusinessWeek* (April 1, 1950), 31

entry into the Korean War, another segment in *Business Week* reinforced the Bush-Butters' critique. "There's a gimmick in the tax laws that's curbing industrial research," wrote *Business Week*.³⁵²

Right now, the law says you can't deduct a capital outlay as a business expense. The trouble is that research projects are frequently considered capital items—if research hasn't long been a permanent and regular part of a company's operation. To tax-conscious businessmen, this makes a difference. Business expenses can be deducted from income each year they occur. Capital outlays, on the other hand, have to be written off over a period of years that supposedly corresponds to the useful life of the capital item. ... In research, there's an additional complication. It's extremely hard to draw the line on how to capitalize certain project costs. Not all research leads to a definite end result. ... The result of all this is that many companies that would like to diversify or improve their lines are just standing pat. Research is something they just can't afford.³⁵³

These comments reflect the push by military contractors and the Department of Defense for an officially-sanctioned, unequivocal option to expense R&D as incurred for tax purposes. When US involvement in the Korean War came to an end, Congress enacted the changes Bush and his allies had hoped for all along.

Back in April 1952, in the midst of a "sweeping administrative reorganization of the bureau," the Joint Committee on Internal Revenue Taxation welcomed the testimony of John B. Dunlap, Commissioner of Internal Revenue. At the hearings, Dunlap stated for the record his agency's unofficial policy in terms that had already been expressed earlier by Bush, his committee, Butters, and the business press.³⁵⁴ Dunlap's testimony

³⁵² "Research Stymie," *Businessweek* (October 28, 1950), 44

³⁵³ *Business Week* (October 28, 1950), 44

³⁵⁴ On August 1, 1951, John Bettes Dunlap began work as Commissioner of Internal Revenue at the Bureau of Internal Revenue of the Department of the Treasury. Two years later, he resigned, and in December 1964, Dunlap passed away at the age of 61. His obituary in the *New York Times* is revealing of a most important moment in federal regulatory history: the early 1950s. Said the *Times*: "For several months before his appointment [to Bureau] there had been repeated charges that some revenue collectors had accepted bribes from persons seeking 'breaks' in their tax returns. Revenue collector for several areas had resigned under pressure. And Mr. Dunlap's predecessor, George W. Schoenenman, had told a

made an impression upon Congress and was a decisive influence in the resulting Revenue Act of 1954. The Bureau's informal policy was then formally codified into the Internal Revenue Code. From that point on, tax accounting policy allowed for either the deferral or expensing of R&D cost. This much, advocates for R&D tax accounting had won. The demands of war had shown a spotlight on ambiguities in the tax code. But the war economy achieved something else, as well. As much as war had propagated the concept of R&D as a useful organizational activity, it did so through the introduction of cost accounting systems which many firms were exposed to for the first time in their capacity as contractors.

R&D as an organizational activity had grown up alongside cost control techniques, but until the Second World War, the two had developed largely in parallel. With the arrival of war, the two would intersect in dramatic fashion as R&D contracts made mandatory the keeping of detailed cost accounting systems. As the country began its transition into a peacetime economy, this war-born marriage of strict cost control and R&D was facilitated by men like MIT's Ronald H. Robnett. In the July 1946 issue of the *NACA Bulletin*, an editorial introducing Robnett's "Control of Research and Development Costs," set the scene: "Research bids fair to be of as high an importance in peacetime

Congressional committee that 50 or 60 employees were being dismissed by the agency every year for taking bribes. Then in June, as the reports of scandal mounted, Mr. Schoenenman retired because of illness. As his successor President Truman selected Mr. Dunlap, a tall, easy-going Texan who had risen from clerk in a district office to direct the agency's Special Frauds unit. But the bureau's troubles did not end with his appointment. Instead, there came new disclosures of impropriety during the summer of 1951. High officials in New York and San Francisco were being accused of various misdeeds. Agents elsewhere quit suddenly; a number of others were suspended. And a House committee was looking closely at the bureau's affairs. The situation was so serious that President Truman, after consultation with Mr. Dunlap, on Jan. 2, 1952, ordered a sweeping administrative reorganization of the bureau. According to the plan, which was later approved by Congress, the 64 politically appointed district collectors were replaced with 25 officials with tenure under Civil Service rules and they were forbidden to hold outside employment. Under the new plan the bureau also became the Internal Revenue Service" ("John Dunlap, 61, Ex-Tax Chief, Dies; 1951 Truman Appointee Led Revenue Agency Shake-Up" *New York Times*, December 7, 1964).

industrial operations as it was during the war, and may well be foremost for a while in the activities of many companies as they seek new markets. This brings up the subject of the cost of research projects and whether or not the spirit of effective research is amenable to the controls governing other expenditures.”³⁵⁵ Whereas the editorial left open the question of applying cost controls to R&D, Robnett’s response was unambiguously in the affirmative.

Having earned his MBA from Harvard’s business school in 1934, Robnett joined MIT’s staff that same year as an assistant in the Department of Business and Engineering Administration. While rising through the ranks at MIT,³⁵⁶ Robnett spent 1942 as a visiting lecturer in accounting at Harvard; served as president of the Boston Chapter of NACA from 1944-1945; and was appointed in January 1947 to MIT’s Committee on Educational Survey.³⁵⁷ In 1948, he served on the Advisory Committee on Research and Development Contracts of the Department of the Army, and when MIT President Karl T. Compton was called to Washington to replace Vannevar Bush as Chairman of the Department of Defense Research and Development Board, Robnett was there as Compton’s consultant and aid.³⁵⁸ Finally, at the time his *Bulletin* article was published in 1946, Robnett had taken on an additional role as fiscal officer for MIT’s Division of Industrial Cooperation.

³⁵⁵ Editorial, *NACA Bulletin* (July 1946), 1078

³⁵⁶ Robnett became Assistant Professor of Accounting in 1937, Associate in 1942, and Full Professor in 1947.

³⁵⁷ Later known as the “Lewis Committee” after its Chairman, Warren K. Lew, this group began by examining the educational mission and philosophy of MIT in January 1947 and by 1950, called for greater educational and research emphasis on engineering, science, architecture and planning, and humanities and social sciences.

³⁵⁸ Daniel Kevles, “Cold War and Hot Physics” (1990), 247.

Despite this wide array of experience in the post-war R&D universe, Robnett had never worked as a salaried cost accountant in an industrial firm grappling with the problems of accounting for R&D. What he did have was experience as an accounting educator, institutional policymaker, and government administrator; what he did not have was first-hand knowledge of the possibilities and limitations of R&D accounting. As a result, control over R&D was, for Robnett, not only possible but necessary, and in his *Bulletin* essay, he used a brief vignette as a foil to make his case:

One common attitude toward research expenditures is typified in the reply of a leading industrialist when asked, "How does your company control research costs?" He answered briefly, "We don't!" He then explained that it was company policy to select competent research personnel under a capable director and to supply the group with an adequate budget. Judgment was not passed on immediate and tangible results but on continuing and long-run accomplishments. ... "Thus, you see," said the executive, "one does not apply accounting controls to such a department. Research must be freed from the tight standards that are so useful in operation. One can not establish efficiency variances per unit of output for research. At all costs the research worker must be insulated from the 'penny pinch' executive."³⁵⁹

To Robnett, such views as those expressed directly above were trite and out-of-date. "The fact is," he concluded, "that management has the same obligation to stockholders with respect to funds spent on research as it does towards any of the other manifold activities of the company. Research must pay its way."³⁶⁰ Industrial researchers, rather than being "visionaries incapable of thinking and acting in business terms," could at least be trained to keep an eye on expenses with the aid of research budgets and a properly designed accounting system.

³⁵⁹ Ronald H. Robnett "Control of Research and Development Costs" *NACA Bulletin* July 15, 1946.

³⁶⁰ Robnett (July 15, 1946)

Importantly, even as the quoted executive from the vignette only evaluated overall, “long-run accomplishments,” Robnett himself declined to suggest how to value the a company’s R&D program. He discussed how costs entering into the research process could be properly accounted for, but whether or not the results of that process were to be tied into his proposed accounting scheme, he did not say.³⁶¹ In the same vein, he avoided taking a position on whether R&D costs should ever be capitalized.³⁶² Robnett would not be alone in skirting these two extraordinarily challenging problems of valuation. Had he not passed away in February 1954, at age 48, he may very well have investigated these issues further.

4.2. Losing the “Fight”

Robnett’s was the first but not the last of the articles on R&D accounting to appear in the *Bulletin* in the early postwar years.³⁶³ This is to be expected, given that prior to the war, almost all articles devoted exclusively to this topic appeared in that same, cost accountancy publication. More surprising is the sudden appearance of such articles in the *Journal of Accountancy*, the professional publication of financial accountants that dominated

³⁶¹ “The problem of qualitative appraisal of scientific progress, as compared with the original plan is, of course, another matter.” Robnett July 1946

³⁶² “The questions of whether or when research costs should be capitalized is beyond the scope of this paper.” Robnett 1946)

³⁶³ Norman A. Shephard’s “Industrial Research and the Accountant” (June 1946); Mason Smith’s “Industrial Research and Its Relation to Accounting” (March 1947); Glau L. Smith’s “How We Developed Control of Research and Development Costs” (November 1947); Earl K. Johnson’ (July 1949); J. C. Freeman’s “An Outline of a Product Development Budgetary Control Procedure” (October 1949); W. L. McKinnon’s “Planned Control of Costs in the Research Division of an Oil Company” (June 1950); James A. McFadden, Jr.’s “Cost Accounting for a Research Laboratory” (March 1951); David S. Moffitt’s “Is Own Product Research a Current Fixed Expense?” (June 1951); Hugh Connolly’s “Research Accounting in an Oil Company” (November 1951); W.F. O’Brien, Jr.’s “Budgetary Control in a Research and Dev Company” (March 1952); James D. Wilson’s “Co-operative Control of Research Costs” (June 1952); Allen L. Spurr’s “Cost Accounting in a Research Organization” (November 1952)

the accounting literature into the 1960s.³⁶⁴ Up until that time, the elite financial accountants—those independent, “public” practitioners who audited the books and certified the financial statements of their corporate clients—expressed little interest in the mechanics of R&D and the challenges it posed to accountancy.³⁶⁵ Things began to change at the *Journal*, even if at a snail’s pace, with the arrival of Carman G. Blough, the former (and original) Chief Accountant of the SEC.

Blough had joined the SEC in December 1934 after several years with the Wisconsin Tax Commission (1922-27), the Wisconsin State Board of Public Affairs (1927-1929), and the Accounting Department at the University of North Dakota (1929-1933).³⁶⁶ The SEC having only just taken form, Blough’s career there began as financial analyst and principal staff accountant (1934-5), then as assistant director in the Registration Division (1935), and, finally, as the SEC’s first Chief Accountant, a position he held from December 1935 to June 1938.³⁶⁷ Under the SEC’s third Chairman, William O. Douglas,

³⁶⁴ Into the 1960s, the rise of a so-called “empirical” school of accounting began to gain in popularity, as did the school’s publication, the *Journal of Accounting Research*. Discussion of the empirical school’s brand of positivism would consume more space than is here available. Furthermore, since the focus of empirical accounting researchers was highly theoretical and only very minimally interested in “on the shop floor accounting,” their contributions in the 1960s must be set aside. See Robert J. Fleming, Samuel P. Graci & Joel E. Thompson “The Dawning of the Age of Quantitative/Empirical Methods in Accounting Research: Evidence from the Leading Authors of *The Accounting Review*, 1966-1985” *Accounting Historians Journal*, Vol. 27, No. 1 (June 2000): 43-72; R. Bricker and Gary Previts “The Sociology of Accountancy: A Study of Academic and Practice Community Schisms” *Accounting Horizons*, Vol. 4, No. 1 (1990): 1-14; and T. Dyckman & Stephen Zeff “Two Decades of the *Journal of Accounting Research*,” *Journal of Accounting Research*, Vol. 22, No. 1 (1984): 225-97

³⁶⁵ See Fleischman and Tyson “The History of Management Accounting in the U.S.” in *Handbook of Management Accounting Research*, Christopher S. Chapman, Anthony G. Hopwood, and Michael D. Shields (2007), 1080.

³⁶⁶ Blough only worked directly at the University of North Dakota from 1929 until 1932 when, for the 1932-1933 academic year, he took a leave of absence to pursue additional graduate work at Harvard University. In 1933, he left to North Dakota to take a position as professor and head of the social science department at the Armour Institute of Technology in Chicago, but resigned in 1934 to join the staff of the newly-created SEC.

³⁶⁷ At the SEC, Blough would serve under the Commission’s first three Chairmen: Joseph P. Kennedy, Sr (in office from June 1934 - September 1935); Harvard Law professor James Landis (1934 - 1937); and, finally, the Yale law scholar, legal realist, and future Supreme Court Justice William O. Douglas (August 1937 - April 1939).

heated debates devolved over whether or not government should take the lead in imposing and enforcing mandatory accounting procedures. In these debates, Blough took the position that responsibility for setting accounting standards should remain in the hands of the profession.³⁶⁸ Even as he insisted on this point, however, Blough was fully aware of the failure of accounting leadership to narrow the range of acceptable alternatives. The proliferation of alternative had so plagued corporate financial reporting as to make comparisons between two firms' balance sheets nearly meaningless for investors. Thus, in January 1937, Blough expressed the following frustrations before a meeting of the New York State Society of Certified Public Accountants:

In the course of our work [at the SEC], we have occasion to see a wide variety of procedures followed in the treatment of almost every conceivable kind of an accounting problem. The term "generally accepted accounting principles" has been widely used in accounting literature, particularly by the American Institute of Accountants and the Securities and Exchange Commission; yet I do not know of any satisfactory definition of the term.³⁶⁹

Here was the SEC's Chief Accountant standing before members of the nation's first state society of CPAs. New York was home to the nation's largest and most influential public accounting firms, and it was there that the CPA designation was first established by law. Against this backdrop, here stood the SEC Chief Accountant declaring that "generally

³⁶⁸ See Carman G. Blough "Development of Accounting Principles in the United States" *Berkeley Symposium on the Foundations of Financial Accounting, January 13th and 14th, 1967* (Berkeley, CA: University of California, Berkeley Schools of Business Administration, 1967), 6-7). Along with Douglas, SEC Commissioner Robert E. Healy took the position that, in light of the profession's reluctance to shape its own standards, the SEC ought to step in. Said Healy in a speech delivered to the American Accounting Association in December 1938: "It seems to me, that one great difficulty has been that there has been no body which had the authority to fix and maintain standards [in accounting]. I believe that such a body now exists in the Securities and Exchange Commission" ("The Next Step in Accounting" *The Accounting Review* (March 1938), 5)

³⁶⁹ Carman Blough, "Some Accounting Problems of the Securities and Exchange Commission" Address of Carman G. Blough, Chief Accountant of the Securities and Exchange Commission Before the New York State Society of Certified Public Accountants (January 11, 1937).

accepted accounting principles” was practically meaningless.³⁷⁰ As Blough proceeded to make his case, he shared additional observations from his work as a civil servant examining company balance sheets. The similarity between Blough’s experiences and those of Robert H. Montgomery during the First World War are uncanny. Said Blough:

Almost daily, principles that for years I had thought were definitely accepted among the members of the profession are violated in a registration statement prepared by some accountant in whom I have high confidence. Indeed, an examination of hundreds of statements filed with our Commission almost leads one to the conclusion that aside from the simple rules of double entry bookkeeping, there are very few principles of accounting upon which the accountants of this country are in agreement.³⁷¹

Apparently, only very modest improvements had been made to financial reporting since the time of the Great War. Later in 1937, at the fiftieth annual meeting of the American Institute of Accountants, Blough made a point to clarify, in no uncertain terms, that if the profession did not get its act together, the door would be left open to those in government chomping at the bit to mandate accountancy standards from on high. At the same convention, Robert H. Montgomery delivered a forceful speech as the outgoing president (for the second time) of the AIA, pressing the theme of responsibility upon his audience:

We cannot hope to make progress unless we *fight* for the ideals and standards which have come to us from the founders of the profession. ...
Let’s fight to raise the standards of the Institute in every way.
Let’s fight to suspend or expel any member who is guilty of conduct unworthy of a member.
Let’s fight for a sane Federal income-tax law.
Let’s fight any effort in any state to dilute or weaken the C.P.A. degree.
Let’s fight to strengthen state laws and otherwise assist state societies.

³⁷⁰ Dale L. Flesher, Gary John Previts, and Tonya K. Flesher, “Profiling the New Industrial Professionals: The First CPAs of 1896-97,” *Business and Economic History*, Vol. 25, No. 1 (Fall 1996): 252-266

³⁷¹ Blough (January 11, 1937)

Let's fight for sound business practices. Don't let's wait until unsound practices creep in, are reflected in balance-sheets and embarrass the accountants who are asked to certify to them.

Let's fight any tendency, private or governmental, to break down the independence of the certified public accountant by rules or regulations or business pressure.

Let's fight for honest accounting, clear financial statements and full disclosure of all essential facts.

Let's fight anyone who seeks the assistance of a certified public accountant in the issuance of any kind of misleading statement.

Let's fight anyone who thinks that one certified public accountant will supplant another who has done a good job.

Let's fight for easily understood accounting terms.

Let's fight weasel words.

Let's fight bunk whenever and wherever it appears.³⁷²

This was a call for a revitalized professionalism that demanded more than a mere recitation of generally accepted accounting principles. Such calls for action, as Blough put it, "needled" the profession out of complacency and culminated in late 1938 with the formation of the Committee on Accounting Procedure (CAP).³⁷³ By that time, Blough had taken a position as manager at Arthur Andersen & Co., and he represented the firm in his capacity as member of the CAP. By 1940, he was made partner at Arthur Andersen, but he would not keep his position for long. Answering the call to government service, Blough resigned from the firm and from the CAP in 1942. His return to Washington would coincide with the accelerating mobilization of the nation's industrial forces.

³⁷² As quoted in Lybrand, Ross Bros. & Montgomery, *Fiftieth Anniversary, 1898-1948* (Privately Printed, 1948), 138.

³⁷³ An important step came in April 1938 when "the Commission decided to give the profession a chance to lead the way and issued a statement of its administrative policy in the form of Accounting Series Release No. 4 [which] opened the way by which any recommendations by the organized profession could be given recognition by the Commission as having 'substantial authoritative support' [without the Commission relinquishing its statutory authority to make rules and regulations governing financial reporting]" (Blough 1967, 7)

Appointed to the War Production Board, Blough worked as chief of the Contract Review Branch (1942-1944); deputy director of the Facilities Bureau (1943); and director of the Procurement Policy Division (1943-1944). He also served as coordinating member of all Price Adjustment Boards from 1942 to 1945 and as a member of the Contract Termination Board from 1944 to 1945. One can hardly doubt that, through these experiences, he would become acquainted with many of the accounting challenges involved in the complex and quickly evolving universe of federal R&D contracting. To the extent this holds true, Blough would already have been sensitized to R&D contracting issues when, in 1947, he took on the role of editor for a new, Q&A-style department in the *Journal of Accountancy* called “Comments on Accounting Procedures.”

Later renamed “Accounting and Auditing Problems,” this and other departments in the *Journal* were part of a reorganization intended “to bring to the readers information about what is going on in the front lines of accounting.”³⁷⁴ With Blough as its editor, “Commentary on Accounting Procedure” made its debut with an inquiry submitted by a reader into “the handling of research, experimentation, and patent development expenses.”³⁷⁵

As an illustration, one of our clients [charges R&D expense] to operations currently on the grounds that such expense cannot be directly attributable to any particular patent, process, or facility. We would like to inquire whether this represents the usual accounting procedure...and whether such procedure has been seriously questioned by the Treasury Department for tax purposes.³⁷⁶

³⁷⁴ Editorial, *Journal of Accountancy* (June 1947): 97

³⁷⁵ Carman Blough, “Comments on Accounting Procedures: Research, Experimental, and Patent Development Expenses” *Journal of Accountancy* (June 1947): 499

³⁷⁶ Blough (June 1947), 499

Blough's response, offered in not one but three different answers, is nearly identical to Robert H. Montgomery's handling of the matter back in 1922. Indeed, the similarity is so close that one wonders if Blough relied on Montgomery's popular textbook to fill in gaps in his own knowledge. After all, to the extent he developed familiarity with R&D through his years in government service, his knowledge of the topic would have been largely limited to contracted-R&D, not own-R&D or firms' experiences implementing R&D programs.³⁷⁷ This question aside, his response is revealing of the continuing diversity that existed in R&D accounting.³⁷⁸

Blough's "*Answer No. 1*" explained that annually recurring R&D was properly expensed, while special or extraordinary projects were to be deferred. "*Answer No. 2*" compared expenditures on R&D to the salaries of employees "engaged in future planning, or advertising related to future sales."³⁷⁹ Because the latter expenses were, by convention, written off in the period incurred, and "because of uncertainty as to the creation of any asset, or the amount of cost which should be attributed to such asset if one is created," Blough warned that "costs of this nature should be deferred only when they clearly relate to assets having a continuing useful life of substantially more than one year."³⁸⁰ Finally, with "*Answer No. 3*," Blough explained that, for newly-developing

³⁷⁷ It would be interesting to learn more about Blough's involvement in the work of Arthur Andersen & Co. We know he represented the firm on the CAP, but surely those efforts did not consume all of his time. Did his experience at Arthur Andersen introduce him to problems of R&D accounting from the business perspective (as opposed to the perspective of SEC regulators)?

³⁷⁸ Montgomery had represented the diversity of practice in accounting for R&D in the 1920s; the NACA research study of 1939 indicated the ongoing diversity of practice in the 1930s; and now Blough was representing the same for the early post-war period.

³⁷⁹ Blough (June 1947), 500

³⁸⁰ Blough (June 1947): 500

companies that had yet to start earning income, any decision to defer R&D charges to the future were allowable so long as financial statements disclosed such treatment.

Blough, like Montgomery before him, associated regularly-incurred R&D costs with routine business expenses, and irregular (“special” or “extraordinary”) costs with capital expenditures. Only two months later, in the August 1947 issue of the *Journal*, he examined the financial statements of several aircraft manufacturers and concluded that “There seems to be considerable difference of opinion as to whether these costs should be capitalized and allocated to future periods, or whether such costs should be immediately charged to income as incurred.”³⁸¹ Despite these early efforts within the institution outlets of the AICPA, Blough’s contributions seem to have gone unnoticed. Over the next two decades, only rarely would one find focused discussion of R&D accounting in the pages of the *Journal*.³⁸²

Similarly slow to build in the post-war years was the interest shown by accounting academics in their own professional journal, *The Accounting Review*. By the end of the 1940s, it almost seemed as if academics might reawaken to the problems of R&D accounting, starting with the January 1949 issue of the *Review*. In “The Mismatching of Costs and Revenues,” John G. Blocker presented the results of a series of informal interviews he conducted with twenty-five executives. Much to Blocker’s disapproval, these interviews revealed a widespread preference for current expensing of R&D. Blocker

³⁸¹ Carman Blough, “Development, Engineering, and Experimentation Costs” *Journal of Accountancy* (August 1947): 146

³⁸² One exception to the rule was Anthony A. Belser, Jr.’s contribution to the April 1954 issue of the *Journal*. In “Five Practical Problems Solved by the Public Accounting Firms,” Belser briefly discusses a case involving “Deferred Development Expense” before concluding “the case illustrates very well the advisability of following the rule of deferring this type of development expenditures only when there is a clear indication that such expenditures have a future value in the sense that they will be productive of future earnings”(Belser 1954, 441-2).

scoffed at his own findings: “It is apparent from the answers obtained that in the majority of companies no attempt is made to match research and development costs against the revenues realized and recognized from these cost expirations. The effect of treating such costs as charges to current operations is to relieve future accounting periods of their proportional share of the cost expirations and to misstate the profit and loss in all periods concerned.”³⁸³ To Blocker, as well as others enamored with the theory of matching (whom I call “matchers,” for short), the proper treatment of R&D was deferral, plain and simple.

Matchers like Blocker seem to have taken for granted that the same sorts of control measures used in other areas of business operations could be used unproblematically to associate particular R&D costs with particular sales income. In manufacturing operations, the measure of labor hours, materials, overhead, and other expenses were typically recorded in accounts set up for each line of product. With this data in hand, managers were better able to set the prices of their products. A generation earlier, confusion over product pricing had triggered a self-destructive (and industry-destructive) practice known as “cutthroat” price competition.³⁸⁴ Trying to lure customers away from their competitors, manufacturers in the same industry pursued policies of price-cutting that spiraled downwards out of control, to the point where products were sold below cost and the very viability of business was placed at risk. Cost accounting

³⁸³ John G. Blocker, *The Accounting Review* “The Mismatching of Costs and Revenues” January 1949

³⁸⁴ Arthur Lowes Dickinson, “The Economic Aspect of Cost Accounts and its Application to the Accounting of Industrial Companies,” *American Economic Review*, Vol. 1, No. 2, Papers and Proceedings of the Twenty-Third Annual Meeting of the American Economics Association (April 1911): 110-123.

systems made it possible for prices to be determined on the basis of costs incurred in the production of finished goods, and with an added margin for profit.

Cost accounting had other benefits, too. By predetermining standards of cost, quantity, or prices for each factor of production, company accountants were able to equip managers with the tools to forecast the volume of sales for future periods. The thinking was that, should production fail to reach forecasted targets, managers would be able to identify weak spots in the manufacturing process simply by comparing the actual contributions of various factors against the predetermined standards.³⁸⁵ This was the old “managing by exception” device developed in the early twentieth century as part of the scientific management movement.³⁸⁶ It meant that, rather than monitoring the production and distribution of a growing number of products and product lines, managers only had to intervene when production or sales volume failed to meet projected forecasts. Matching enthusiasts were unwilling to question the usefulness of this method for R&D accounting purposes.

³⁸⁵ For a phenomenal discussion of the founding of the NACA, the early movement for uniform cost accounting standards, and their relationship to cost control, see Gerald Berk’s “Discursive Cartels: Uniform Cost Accounting Among American Manufacturers Before the New Deal.” *Business and Economic History*, Vol. 26, No. 1 (Fall 1997): 229-51. Berk’s work helps illuminate the shortcoming of H. Thomas Kaplan and Robert S. Johnson’s infamous critique, *Relevance Lost: The Rise and Fall of Management Accounting* (1987). Their text argues unconvincingly that cost accounting became subservient to the demands of financial accountancy in the 1920s.

³⁸⁶ Rosita S. Chen and Sheng-Der Pan “Frederick Winslow Taylor’s Contributions to Accounting,” *The Accounting Historians Journal*, Vol. 7, No. 1 (Spring 1980): 17-35; M. C. Wells, “Taylor’s Contributions to Cost Accounting: A Comment,” *The Accounting Historians Journal*, Vol. 9, No. 2 (Fall 1982): 69-77; M. C. Wells, *American Engineers’ Contributions to Cost Accounting* (New York: Arno Press, 1978); Rosalie C. Hallbauer, “Standard Costing and Scientific Management,” *The Accounting Historians Journal*, Vol. 5, No. 2 (Fall 1978): 37-49; Marc Jay Epstein, *The Effect of Scientific Management on the Development of the Standard Cost System* (PhD thesis, University of Oregon, December 1973); H. Thomas Johnson, “Review of The Effect of Scientific Management on the Development of the Standard Cost System by Marc Jay Epstein,” *The Business History Review*, Vol. 53, No. 4 (Winter 1979): 574; R. Penny Marquette and Richard K. Fleischman, “Government/Business Synergy: Early American Innovations in Budgeting and Cost Accounting,” *The Accounting Historians Journal*, Vol. 19, No. 2 (December 1992):123-145; Richard K. Fleischman “Completing the Triangle: Taylorism and the Paradigms,” *Accounting, Auditing & Accountability Journal*, Vol. 13, No. 5 (November 2000): 597-624.

To those whom the Paton and Littleton monograph functioned as accounting bible, there was no disputing the fact that all “costs attach” to the final product. This was a notion quite at home in the classical political economy of David Ricardo and Karl Marx, for whom the value of labor was *embodied in* the final product. Mocking those who disparaged the truth of his metaphysics, Paton wrote, “Some of us don’t feel safe unless we tie our scheme of cost absorption to the history of specific physical units of property and specific transactions. We hesitate to acquiesce in theories of transmigration of costs through successive units of physical property and successive transactions, even where there are close similarities and relationships which give the case for deferring a somewhat appealing appearance.”³⁸⁷ Thus, whether incurred in R&D, in production, or some other activity of the firm, all costs ultimately “transmigrated” into the final product. This meant that R&D could be analyzed in the same terms as production, with the cost of inputs being accumulated as deferred charges before being disposed of in future periods whenever sales of the new or improved product came rolling in. With this view, important distinctions between R&D and manufacturing were erased, most notably the uncertainty of outcomes. This uncertainty might be mitigated in manufacturing, but as a necessary feature of R&D, attempts to control it undermine the very purpose that R&D was supposed fulfill.

4.3. “A Different and Unique Business”

Given the trends in management accounting over the preceding decades, it’s not difficult to see how an equivalence between R&D and manufacturing could have taken

³⁸⁷ Walter A. Staub, “The Cost Principle” *Accounting Review*, 17, no. 1 Jan. 1942, 17.

hold. Ever since the start of the 1920s and that decade's efficiency craze, on through the challenges of cost savings in the 1930s, and into the 1940s when military contracts required the maintenance of detailed cost systems, pressures for cost control were ever present.³⁸⁸ By the 1950s, with competition at what seemed like all-time highs, cost controls were implemented with renewed vigor and, in business, the cost accountant increasingly took his place as a member of the executive suite, a position that usually came with the title Comptroller, Controller, or, later, Financial Executive. Naturally, many cost accountants celebrated their role in bringing about this elevation in status.³⁸⁹

Back in 1927, a contributor to the *Haskins & Sells Bulletin* had complained that, "The profession today represents little more than a mass of individuals struggling with individual problems, trying to work them out in the light of varying degrees of education, experience, natural ability, and brute strength."³⁹⁰ Thirty years and another World War

³⁸⁸ Writing in the early 1950s, Harvard Business School professor Robert N. Anthony explained that, "The necessity for furnishing information to the government has, in many cases, led to the use of more formal devices for planning and reporting on the progress of all the work in the laboratory, both government and nongovernment, than had been the case prior to the time the first government contract was accepted. ... The second major area in which government contract work affects the control problem is accounting. In nearly all government contract work, even those where the price is a fixed dollar amount, it is necessary to segregate costs incurred on government projects from other costs. Therefore, laboratories with government contracts usually have a mechanism for collecting costs by projects; they tend to use this system for all projects rather than merely for those sponsored by the government"(Anthony 1952, 90). This generalized experience has been confirmed by many writers since that time, including Michael E. Doron's, "I Ask the Profession to Please Stand Still: The Evolution of American Public Accountancy, 1927-1962," *Accounting Historian Journal*, Vol. 28, No. 1 (June 2011), especially pp.125-127.

³⁸⁹ Paul Crossman "The Function of the Cost Accountant in Cost Control," *Accounting Review*, Vol. 28, No. 1 (January 1953): 25-31; J. Hugh Jackson, "Cost Accounting in the Education of Management," *NACA Bulletin* (August 15, 1946): 1183; "A Re-examination of Standard Costs," *NACA Bulletin* (February 1, 1948): 701; William J. Vatter "Accounting Education for Controllershship," *Accounting Review*, Vol. 25, No. 3 (July 1950): 236-50; Fred Couey "A Better Image for the Accountant" *NAA Bulletin*, Vol. 44, No. 11 (July 1963): 51-2; Daniel North Jr. "The Changing Role of the Accountant" *Management Accounting*, Vol. 48, No. 5 (January 1967): 36-7; Editorial "Evolution of a Magazine: Growth of the Official NAA Publication Over a Half-Century Reflects Growth of the Association" *Management Accounting*, Vol. 50, No. 1 (September 1968): 60-61; Robert P. Neuschel, "Making a Permanent Place for Cost Reduction," *NACA Bulletin*, Vol. 37, No. 9 (May 1956): 1068-1079; A.J. Gallanter "Project Costing in a Research Laboratory," *NACA Bulletin*, Vol. 37, No. 1 (September 1955): 30-37.

³⁹⁰ Anonymous, "These Standardized United States" *Haskins and Sells Bulletin*, Vol. 10, No. 3 (March 1927): 17.

later, accountants were nearing the coveted professional status enjoyed by physicians and lawyers. Reflecting on developments in cost accounting since 1920, J. Hugh Jackson, former graduate and professor at Harvard's business school and dean of Stanford's business school, boasted that, "The scientific setting of standards for all the elements of production and distribution costs, and the use of these standards not only in arriving at relative costs but also in determining degrees of efficiency within every part of the business unit, have been among the marvels of modern organization and management."³⁹¹

Prominent though he was, Jackson did not speak for all cost accountants. Many with direct experience handling the problem of how to account for R&D believed that the efficiency methods achieved in manufacturing were not readily translatable to all corners of a business, least of all R&D. It was upon this basis—the distinctive "nature" of R&D, that which made it so hard to classify R&D as either a capital or an income expenditure—that David S. Moffitt of the Connecticut Hard Rubber Company insisted on the method of expensing as incurred. In the June 1951 issue of the *Bulletin*, Moffitt posited that,

This may be difficult for many industrial accountants to understand because, with modern accounting tools, it is very easy to segregate development costs applicable to independent orders, products or sales propositions. There is a strong impulse to believe that every cost which can be separated and distributed to a particular item should be applied to that item. It may very well be that this easy distribution has caused much of the confusion connected with the question of the disposition of research and development costs.³⁹²

³⁹¹ J. Hugh Jackson "A Half-Century of Cost Accounting Progress" *NACA Bulletin* (September 1952). See also his "The Growing Responsibility of the Accountant" *NACA Bulletin* (1945): 839-?

³⁹² David S. Moffitt "Is Own Product Research a Current Fixed Expense?" *NACA Bulletin*, Vol. 32, No. 10 (June 1951): 1253

What made R&D unique, for Moffitt, was the nature of the research process and the capabilities instilled in researchers through years of focused study. Such highly skilled labor, “trained in the intricacies of the particular field in which the company is engaged,”³⁹³ was unlikely to be hired nor fired as readily as other employees. Should researchers leave a company, “they (most probably) will find their way into a competitor’s employment”³⁹⁴ and take with them any competitive advantage they once offered. What’s more, “the results of research and development are unpredictable [which makes it] unreasonable to capitalize the costs of a project value.”³⁹⁵

For Moffitt, all of this—the hard-to-come-by, highly specialized services provided by researchers; the competitive advantage to employers of having them on board; the distinct unease at the thought of potentially losing hard earned know-how to competitors; and the unpredictable results of the R&D process—amounted to a treatment of R&D as a fixed or recurring cost, and as such, one that ought to be currently expensed. As we have seen, accountants early in the century struggled to convince employers that depreciation was a fixed expense that had to be provided for annually and not just in profitable years. Now, Moffitt was among those arguing that R&D had to be similarly treated, that is, as a fixed, regularly-recurring business expense, without which a firm would fall behind and lose to its competition.

Another *Bulletin* essay skeptical of applying cost control techniques to R&D appeared only a few months earlier in the March 1951 issue. In “Cost Accounting for a Research Laboratory,” James McFadden, Jr., controller of the Laboratories Division at

³⁹³ Moffitt (June 1951), 1252

³⁹⁴ Moffitt (June 1951): 1252

³⁹⁵ Moffitt (June 1951): 1253

Radio Corporation of America (RCA), explained that “modern research...is a different and unique business, different because many of the ‘tried and true’ principles of ordinary cost and financial management have no place in the industrial research laboratory.”³⁹⁶ This meant that, “the accountant must adopt a philosophy and an accounting technique which may appear to be drastically different from the ordinary. ... This is quite natural, for with an unknown task or problem and a very indefinite ‘end product,’ it can be readily understood that any application of standards or an adaptation of process unit costing is rendered impracticable, if not impossible.”³⁹⁷

McFadden emphasizes this point again: “Individual research tasks embody requirements the costs of which are not only impossible to estimate in advance but which may be extremely difficult to control during the term of the project.”³⁹⁸ And again: “The nature of research makes it impossible in most cases to determine and appropriate a sum which will guarantee any definite or even any positive result.”³⁹⁹ Norman A. Shephard, Chemical Director of the American Cyanamid Co. in New York, agreed. Speaking of “the scientist-accountant feud”⁴⁰⁰ in the June 1946 issue of the *Bulletin*, Shephard argued:

Until the laboratory or the design engineers have carried their exploratory work to a certain stage, the equipment costs and capital expenditure for the plan can only be guessed at. And then there is the ‘induction period,’ or the ‘shirt losing’ stage, as Kettering expresses it, during which the bugs must be gotten out of the process or the product, and then the market and sales development period during which the new product is introduced to and tested by prospective customers. How long will this be and what will be the cost?⁴⁰¹

³⁹⁶ James McFadden, Jr. “Cost Accounting for a Research Laboratory” *NACA Bulletin*, Vol. 32, No. 7 (March 1951), 823

³⁹⁷ McFadden, Jr. (March 1951), 825

³⁹⁸ McFadden, Jr. (March 1951), 830

³⁹⁹ McFadden, Jr. (March 1951), 831

⁴⁰⁰ Norman A. Shepard, “Industrial Research and the Accountant” *NACA Bulletin*, Vol. 27, No. 19 (June 1, 1946): 957

⁴⁰¹ Shepard (June 1946): 963

This, the inability to estimate with certainty the time and resources to be consumed in a prospective project, was just one difficulty in attempts at cost control. Another was the “great deal of bad feeling...engendered in charging back research expense to the manufacturing and/or sales or other departments which may benefit from the work.”⁴⁰² This latter, interpersonal and organizational problem was a result of efforts to trace R&D costs through the firm so as to identify the route by which, in Paton and Littleton’s phrase, costs “attached” to products and ultimately congealed in product sales. Whenever R&D personnel left their own departments to attend to problems elsewhere in the firm, an accounting question arose as to which department ought to bear the cost of the work done: the R&D organization, or the department hosting the researcher? In the latter case, a researcher attending to the problems of pilot production, for example, would have his time charged to the production department.

By absorbing a portion of R&D costs in this way, the hosting departments were left with higher overall costs and, thus, weaker performance reports indicating an inefficient use of resources.⁴⁰³ The “great deal of bad feeling” that sometimes resulted in a “scientist-accountant feud” could be mitigated, however, by redesigning cost systems so that managers were only held responsible for the performance of their own departments or divisions. In fact, even before the introduction of R&D as a separate sphere of activity, the functional separation of accountability had been an important, even if not widely

⁴⁰² Shepard (June 1946): 966

⁴⁰³ As one cost accountant wrote in a letter to the editor of the *NACA Bulletin*, “Why should I be charged with that? This is an increasingly common complaint heard from budget and cost-minded foremen and department heads. Cases in point are numerous.” See: F. J. Wagner “Progressive Costs,” *NACA Bulletin*, Vol. 32, No. 6 (February 1951): 689.

practiced, aim of cost control since at least the 1920s.⁴⁰⁴ On the other side of the Great Depression, one cost accountancy writer echoed the views of many when he observed how “a cardinal rule of management” required that “responsibility for results must be accompanied by authority to accomplish the aims desired.”⁴⁰⁵ Without negating this tradition, Shephard recommended that rather than attempt to tie R&D into costs of sales or production, research expenses were best handled by charging them off through general overhead in the period incurred.

Even those company accountants who believed some aspects of the R&D process *did* lend themselves to the use of traditional cost control standards, were nevertheless quick to point out the limits of such efforts. James D. Willson offers one example. Willson

⁴⁰⁴ This need was rediscovered after World War II and systems designed to address it were referred to variably as responsibility accounting, profit center accounting, and activity-based costing, among other labels. Shu S. Liao, for example, wrote in July 1973 that “In the past 30 years, the rapid expansion of the size of business firms has resulted in a reappraisal of the roles of the top level and lower levels of management. ... During this period, responsibility accounting has evolved as the accounting correlate of motivation theory. The essence of responsibility accounting is the accumulation of costs and revenues according to areas of responsibility.” See “Responsibility Centers” *Management Accounting* (July 1973). Contrary to the implication of Liao’s award-winning article, the concept had been around a lot earlier than the 1950s. Like so many aspects of post-war accountancy, however, early precedents had been all but forgotten by authors who were unwittingly drinking old wine from new bottles. In the words of Gloria Lucey Vollmers, “In forgetting accounting history, researchers are forced to reinvent the wheel.” See “Accounting for Distribution Costs in the Dennison Manufacturing Company During the 1920s and 1930s” *The Accounting Historians Journal*, Vol. 20, No. 2 (December 1993): 83. Vollmers makes a similar point in her “Accounting for Idle Capacity: Its Place in the Historical Cost Literature and Conjecture About its Disappearance,” in which she demonstrates “the sophistication and innovation of early writers” that was often overlooked by later writers on the same topics (*The Accounting Historians Journal*, Vol. 23, No. 1 (June 1996): 26). See also: Walter F. Titus “Management’s Responsibility for the Control of Waste” *NACA Bulletin*, Vol. 16, No. 16 (April 15, 1935): 902-8; Homer N. Sweet “Industrial Accounting as an Aid to Management” *NACA Official Publications*, Vol. 1, No. 4 (June 1920): 3-10; C. H. Smith “Distribution of Defective and Spoiled Material” *NACA Official Publications*, Vol. 1, No. 6 (July 1920): 3-16; Gould L. Harris “Overhead Distribution, Compilation and Presentation” *NACA Official Publications*, Vol. 1, No. 4 (May 1920): 3-18; John M. Scanlon “Costs as an Aid to Management” *NACA Official Publications*, Vol. 3, No. 2 (October 1, 1921): 3-12; Charles Van Zandt “Normal Burden Rates: Some Problems in Their Application” *NACA Official Publications*, Vol. 3, No. 19 (July 1, 1922): 3-9.

⁴⁰⁵ Walter B. McFarland “The Economics of Business Costs” *The Accounting Review*, Vol. 25 (1940): 202 . See also: Lewis R. Crum “The Role of Cost Accounting in Cost Control” *The Accounting Review*, Vol. 28 (1953); Clement L. Stanford “Cost Minimization and Control as a Function of Cost Accounting” *The Accounting Review*, Vol. 23 (1945); William L. Ferrara “Responsibility Accounting and the Contribution Approach” *NAA Bulletin* (December 1963).

worked as the controller of the Plaskon Division at Libbey-Owens-Ford Glass Company in Ohio, and in June 1952, he published an essay in the *Bulletin* responding to the question, “what can be done to assure that research is actually effective?”⁴⁰⁶ He began by pointing out that, “the relationship between expenditure and results can be quite remote and not apparent over a short period of time. Moreover, the projects may be completely dissimilar to other past operations.”⁴⁰⁷ For Willson, this did not negate the fact that “the necessity for certain controls must be accepted” if a business was to “live within its income.”⁴⁰⁸ He believed that unit performance standards—such as a set percentage of net sales resulting from an improved or new product—could “be of assistance in evaluating the quantitative aspects of some phases of the work.”⁴⁰⁹ Such arguments no doubt contributed to the belief that R&D and manufacturing were more-or-less equivalent, so far as cost accounting was concerned. Still, Wilson did make sure to warn against reading into such standards “a substitute for the watchful eye and necessary guidance of the research supervisors.”⁴¹⁰ So long as accountants saw to the “proper application or

⁴⁰⁶ Libbey-Owens-Ford was a producer of flat glass used by the automotive and building products industries. The company was founded in 1930 with the merger of Libbey-Owens’s sheet glass operations and the Edward Ford Plate Glass Company, both of which were located in Toledo, Ohio. After a failed attempt by Henry Ford to acquire Libbey-Owens, the carmaker awarded a contract to Libbey-Owens in 1928 to supply the Ford Motor Company with windshields for the Model A, introduced that year. This made Libbey-Owens the first company to produce automotive laminated safety glass on the basis of its R&D operations. Following the merger with Edward Ford’s Plate Glass Company (no relation to Henry Ford), Libbey-Owens-Ford produced glass for the new Empire State Building in Manhattan, New York. By 1940, the company owned 70% of shares in The Plaskon Company, Inc., the largest manufacturer of urea formaldehyde plastic molding compounds in the US, and in April 1943, Libbey-Owens-Ford marked the completion of its takeover with the creation of its new Plaskon Division. Willson worked in this division prior to joining the Norris-Phermador Corporation in Los Angeles, taking a position as the company’s Vice President of Finance in February 1958.

⁴⁰⁷ James D. Willson “Co-operative Control of Research Costs” *NACA Bulletin*, Vol. 33, No. 10 (June 1952), 1199

⁴⁰⁸ Willson (June 1952), 1199

⁴⁰⁹ Willson (June 1952), 1205

⁴¹⁰ Willson (June 1952), 1205

interpretation of accounting information,”⁴¹¹ standard costing offered a workable means of controlling R&D expenditures.

Other contributors to the *Bulletin* were far more aggressive in debunking the idea that R&D was susceptible to the same degree of control as manufacturing operations. Reporting the results of a survey of 200 publications and interviews with 60 executives in some of the nation’s largest research-intensive businesses, James Brian Quinn cut to the chase with a dramatic opening line: “Anyone who thinks that he can control research and development expenditures by means of a budget is sadly deluded.”⁴¹² Budgets were crucial management tools, he went on to explain, but only for *planning* R&D programs—not for controlling them with the aim of maximizing output. Contrary to Willson, Quinn believed that unit cost standards could not be effectively established for R&D because, in his view, technological knowledge was the output of R&D and such outputs could not be prepackaged into discrete, homogenous units.

Analysis of variances from the budget, as ordinarily practiced in accounting, requires definition of the units to be produced and development of a predetermined cost standard for each unit. Unfortunately, units of knowledge output cannot be defined before the knowledge is obtained, nor can the cost of producing an increment of new knowledge be predicted accurately enough to allow the use of the prediction as a cost standard.⁴¹³

Certainly for those intimately familiar with R&D, it was evident that knowledge or “know-how” was a frequent outcome of R&D projects, and Quinn, having held several

⁴¹¹ Willson (June 1952), 1201

⁴¹² James Brian Quinn “A Study of the Usefulness of Research and Development Budgets,” *NACA Bulletin*, Vol. 40, No. 1 (September 1958): 79

⁴¹³ Quinn (September 1958), 84

accounting and administrative positions in the business world before settling in academia, made a career out of this fact.

Born in 1928 in Memphis, Tennessee, Quinn earned a bachelor's in engineering from Yale (1949), an MBA from Harvard (1951), and a PhD in economics from Columbia (1958).⁴¹⁴ At some point around the early 1950s, most likely after finishing his MBA, he found work in New Jersey for the Allen B. DuMont Laboratories, Inc., an R&D-intensive manufacturer of oscilloscopes and television equipment that competed heavily in the commercial television and broadcasting markets of the late 1940s and 1950s. Importantly, Quinn's graduate studies occurred at a time in America's postwar development when R&D—still a novelty to many businesses—was *valued as a source of new, marketable products*. Indeed, a growing number of businesses in the 1950s competed on the basis of the kinds, quality, and range of products they could bring to market.⁴¹⁵ “As products reach the customer there is the ‘moment of truth,’” wrote James J. Eberl, Assistant Vice-President for Research at the Scott Paper Company in Philadelphia. Research oriented around the demands of the consumer was, for Eberl, “the core of

⁴¹⁴ With his PhD behind him, Quinn went on to become a figurehead for the new area of study sometimes referred to as “knowledge management.” Soon, this research area would become quite trendy among American business schools, and much of this development is owing to Quinn's tireless efforts as educator and dean at the Amos Tuck School of Business Administration at Dartmouth College. Quinn worked at Dartmouth from 1957 until his retirement in 1993. In the earliest years of his career, Quinn's research was supported by the McKinsey Foundation for Management Research, Inc. (sometime prior to 1963), the Ford Foundation (1963-1964), the Alfred P. Sloan Foundation (1967-1968).

⁴¹⁵ The increased focus on new product development was reflected in the literature, as well. See, for example, James A. McFadden, Jr., “Organizing New Product Development,” *NACA Bulletin*, Vol. 37, No. 7 (March 1956): 832-840; James C. Stephenson, “Quality Control to Minimize Cost Variances,” *NACA Bulletin*, Vol. 38, No. 2 (October 1956): 264-275; Russel S. Meinholz, “Separating the Product Testing Costs of a Steel Mill Research Laboratory,” *NACA Bulletin*, Vol. 37, No. 11 (July 1956): 1364-1370; Robert C. Perry, “Forging the Tools of Cost Control,” *NACA Bulletin*, Vol. 36, No. 5 (January 1955): 661-671; A.L. Hunt & H. J. Walker, “Standards to Meet Product Changes,” *NACA Bulletin*, Vol. 32, No. 6 (February 1951): 658-662.

ethical conduct.”⁴¹⁶ Quoting print off of his company’s own institutional (i.e., inwardly-directed) advertising, Eberl added the following flourish: “Poised above the product hangs / The hand of the American Housewife / This is the moment of truth in the market place / Will she or won’t she? / What influences her / Scott has found—that her loyalty must be earned with honest value.”⁴¹⁷ The narrowing of R&D projects down to new or improved products had become the name of the game.

What Quinn’s interviews and survey data showed was that, to those within industry already familiar with and engaging in R&D, the knowledge to be gained was useful and valuable, even if it could not be directly quantified nor its value demonstrated on the basis of product sales. Historiographically speaking, this means it is crucial to place Quinn’s emphasis on knowledge in perspective. To do otherwise risks the misleading conclusion that R&D was and is a knowledge production process and that it should therefore be analyzed in those terms. Sociologists of (scientific) knowledge, for example, may understand R&D as such. So too would scholars engaged in a strict history of ideas, or any others, for that matter, whose analytical toolkit is intended for conceptual or discursive developments over time. This interpretation would be a mistake, and for the simple yet overlooked reason that R&D may produce know-how as well as tangible products. The point bears repeating: firms would not engage in R&D if it’s only “product” was abstract knowledge, and to treat R&D as a “knowledge production” process is to deny this reality.

⁴¹⁶ James J. Eberl, “Ethics and Integrity in Industrial Research and Development,” *Research Management*, Vol. 7, No. 3 (May 1964): 181.

⁴¹⁷ Eberl (May 1964): 181.

There is no *a priori* need to confer upon tangible results a lesser status than intangible concepts, just as there is no *a priori* reason to analytically privilege, say, marketable products over and above the know-how gained through R&D activity. A historical study of R&D, rather than a study of “scientific research,” makes this much plain. Thus, by emphasizing knowledge, Quinn was, in effect, attempting a re-balancing of expectations about R&D, which in the 1950s were heavily lop-sided towards the view that the essence of R&D was the production of new products.⁴¹⁸ It is through the lens of this problematic, indeterminate situation—and not the lens of “knowledge production” theories—that the remainder of Quinn’s argument should be read:

No man, no matter how clairvoyant, would be willing to predict the amount and nature of a research program’s knowledge output. He could not. If he were able to predict this output, there would be no need for doing the research in the first place. As a consequence, there is no reasonable way to define in advance a unit of research output.

Since units of research output cannot be predetermined, there is no way to forecast cost standards for these units. If standard costs for units of output cannot be established, the budget cannot serve as a basis for either saying whether the

⁴¹⁸ By the end of the 1970s, Quinn had expanded the scope of his earlier work, shifting his focus from the management of R&D in the private sphere, to “innovation,” a concept which he used to emphasize the resolution of technological problems in the public sphere: “The crucial science and technology issues of the 1970’s-’80’s are no longer the enhancement and allocation of national R&D efforts. Instead they are: (1) To recognize that growing populations, affluence, and social complexities have created a new set of national goals and demands...that are every bit as valid as the individual and industrial demands of the past. ... (2) To organize science and technological efforts in new ways to solve large scale problems we have never faced before. (3) To provide incentives which stimulate the invention, risk taking, and investment needs to solve these problems. (4) To break down institutional barriers to needed technological solutions and design new institutions more responsive to future demands. (5) To regenerate positive attitudes towards science, technology, and the possibility and essentiality of progress. ... These can be done if we are willing to learn from past experiences. ... People genuinely want better natural environments, improved disease prevention, clean, flexible energy sources at reasonable prices, safer, more humanized, and less alienating work environments, and more effective use of nonrenewable resources”(Quinn 1977, 11). The IT boom in the 1990s confirmed, in Quinn’s view, that Americans were living in a “knowledge economy,” and by 2000, he urged businesses to consider “strategically outsourcing innovation” since “no internal R&D group can possible predict, evaluate or cover all possible designs or competitive positions. To prosper in this environment—even survive—companies need to systematically tap the capabilities of external knowledge leaders”(Quinn 2000, 14). See his “Outsourcing Innovation: The New Engine of Growth,” *Sloan Management Review*, Vol. 41, No. 1 (Summer 2000): 13-28.

operation is producing what it should be producing or indicating whether it is producing its output efficiently. Consequently, the budget is not a useful device for controlling costs in the research and development operation.⁴¹⁹

With manufacturing operations, managers kept an eye on how far actual production varied from predetermined standards. The extent to which a production process was or was not efficient, could be measured by the degree to which the performance of that process varied from standards. Here, the epistemic influence of early-twentieth century scientific management remains palpable: with a given set of standards representing an ideal guide to action, efficiency becomes knowable as the approximation to that ideal.⁴²⁰ However, Quinn's interviews revealed a parting of ways with this line of thinking.

When R&D activities *did* consume resources significantly above or below budgeted amounts, executives explained that such variances were not interpreted as signs of waste, failure, or inefficiency. Instead, they simply alerted managers to the fact that unplanned changes had taken place, and that meetings should be held so that operating and research executives "can ask penetrating questions" of one other in order to learn the meaning of those changes.⁴²¹ Quinn stressed that, "*Not a single* operating or research manager interviewed found the research budget useful as a standard against which to measure the efficiency or effectiveness of the research program. They said that, if a research director so desires, he can 'hit his budget on the nose every time.'"⁴²² Even

⁴¹⁹ Quinn (September 1958), 84

⁴²⁰ Walter B. McFarland, "The Basic Theory of Standard Costs," *Accounting Review*, Vol. 12, No. 2 (June 1939): 151-158

⁴²¹ "Significant variances," wrote Quinn, "advise management to raise questions about the activity on projects which have deviated from their plans. Under- or over-expenditure on such projects may indicate a change in emphasis within the program"(Quinn September 1958), 87

⁴²² Quinn (September 1958), 84. Emphasis in original.

Wilson conceded as much when he wrote, “Necessary though budgetary control is, performance measured by it cannot and should not be the sole criterion of research effectiveness. It is entirely possible to waste funds or use them inefficiently but yet remain within the limits imposed on expenditures.”⁴²³ Others made the same point: budgets were not to be used as blunt tools for reducing a company’s overall expenses, but as means for keeping track of expenses as projects progressed and, if need be, deciding how to redistribute funds among research projects.⁴²⁴

For Quinn, R&D had to be dealt with on its own terms since individual creativity could not be forecast: “Research output, i.e., the exploitable technology created by research, depends upon the idea output of individuals and bears no relationship to any cost input factor.”⁴²⁵ Even then, it was difficult to tell whether the ideas were solely that of a single individual, of a team, the result of some experience outside the lab, or just plain luck. McFadden certainly agreed.⁴²⁶ As some of the executives interviewed by Quinn

⁴²³ Willson (June 1952), 1205

⁴²⁴ According to Darrell H. Voorhies of the Department of Organization at the Standard Oil Company of California, “Cost control...is not a mechanism for reducing research expenditures. It is considered as a basic form of assurance that appropriated funds, which might otherwise be diverted, are spent for productive research and development, or are saved, particularly in cases of nonresearch service costs. The effect of such savings, in the long run, is to make more money available for technical activities.” - “The Coordination of Motive, Men and Money in Industrial Research,” (San Francisco, CA: Standard Oil Company of California, 1946), 62.

⁴²⁵ Quinn (September 1958), 85

⁴²⁶ “1. The salary paid to a particular researcher is in no manner indicative of his productive (creative) ability. This is particularly true of the younger members of the technical staff who have not had time or opportunity to apply their intellectual efforts. ... 2. Hours to be spent in the laboratory, upon which the employment contract of the researcher is predicated, do not take into consideration the many hours of reference reading and paper research, which is invariably done on the individual’s own time, by all persons who are engaged in the work of technological research. 3. ...The element of cooperation and teamwork among research personnel has been a deciding factor in the progress made in all scientific fields, and it is only by collective intellectual effort that many research problems are solved. The studies made on one project may be instrumental in arriving at the solution to the task presented by other projects. This may eliminate the necessity of further cost in terms of additional time and effort on projects which have contributed nothing to absorption of the dollars of the direct cost of the studies concerned. Even if an attempt is made to apportion these costs beyond the project on which they were incurred, it is doubtful if any sensible evaluation could be made of the amount of benefit derived by each product assisted. 4. It is impossible to place a dollar sign on genius or to time the birth of inventions. The solution to any research

explained, “‘There is always a question as to whether credit for research accomplishments should be given to the scientist or to God.’ ‘Pure luck’ or ‘chance’ is often a major factor in determining the speed of success of research. Therefore,..the analyst is always left with the question, ‘Was the research accomplished with efficiency or simply by luck?’”⁴²⁷

Throughout the 1950s and into the opening years of the next decade, such warnings against attempts to forecast and control for the output of R&D were commonplace. During this period, even those matchers who believed that “the need for strong cost control is the greatest it has ever been” qualified such remarks by identifying that, “Of course, cost control is not a ‘cure all,’ for a great number of the costs are beyond the immediate direct control of management or control is effected by means other than through costs.”⁴²⁸ For them, R&D costs were fixed costs of doing business, incurred as a result of management policies and not susceptible to the same efficiency drives used to control the manufacture and distribution of products. We have seen several examples of this from the 1950s. An essay appearing in the *Bulletin* by Fred. W. Shaffer offers an example from the early 1960s.

The child of a working class, religious family in the mining and farming regions of central Pennsylvania, Shaffer left home to earn a bachelor’s degree in history from Duke University before spending a year at the Yale Divinity School studying initially to become a cleric. After serving two years in the military, Shaffer switched gears, enrolling in the Wharton School of the University of Pennsylvania where he graduated in 1960 near the

problem and even the vital spark of invention, may come to the scientist when he is in the barber’s chair, on the golf course, at the breakfast table, or at any other time or place.”(McFadden, *NACA Bulletin*, March 1951)

⁴²⁷ Quinn (September 1958), 87

⁴²⁸ Lewis R. Crum “The Role of Cost Accounting in Cost Control” *The Accounting Review*, 28, no. 3 (July 1953): 363.

top of his class. When his essay for the *Bulletin* made it to print in November 1962, he was only a couple years' into his new job as assistant accounting supervisor at Rohm & Haas, a specialty chemical company in Texas.⁴²⁹ In his essay, "Research Budgeting—Different But Useful," Shaffer argued that, in the factory, "Standards normally result from repetitive past experience, whereas research is by definition nonrepetitive activity."⁴³⁰ The reasons for this were many, but they all boiled down to the uncertainty inherent to the R&D process. "The best approach to a particular research problem cannot be known in advance," he explained, "it is, therefore, difficult to set standards to measure the efficiency of the research operation."⁴³¹

Research results are often unpredictable, as conclusions may be unexpected and encountered problems impossible to solve or, if solvable, only at prohibitive cost. The time required to complete a project is often easier to predict than the outcome of the experiments. This unpredictable nature of research makes the laboratory an area of continual change, new projects initiated or old ones stopped as new information is learned either through research or from outside sources. It should not be forgotten also that many significant developments result purely from accidents. ... For these reasons, research performance can hardly be measured on the basis of predicted results, which naturally complicates the control problem because cost control mechanisms rely on comparison between actual and forecast output in measuring efficiency.⁴³²

Towards the end of the decade, however, warnings such as these against the use of mechanical standards became harder to find. In their place appeared expressions of fear

⁴²⁹ In the years that followed, Shaffer would take on greater responsibility in the company's Philadelphia office, first as controller (1972), then as vice president and finally chief financial officer (1978). In 2009, Rohm & Haas was acquired by Dow Chemical, and merged with DuPont in 2017 to form DowDuPont. Shaffer himself passed away on New Year's Day 2018.

⁴³⁰ Fred W. Shaffer "Research Budgeting—Different But Useful" *NAA Bulletin*, Vol. 44, No. 3 (November 1962): 39

⁴³¹ Shaffer (November 1962): 39

⁴³² Shaffer (November 1962): 39

that companies weren't getting their dollar's worth out of R&D.⁴³³ In November 1968, a *Forbes* editorial quoted executives of several R&D-intensive firms, all of which shared similar anxieties. Lee L. Davenport, president of one of the nation's largest labs at General Telephone & Electronics, argued that "The bloom is coming off the rose. There are many companies who view the results of R&D over the past 20 years with some disappointment." From Du Pont, vice president George E. Holbrook was quoted as saying, "We've been on a research binge in this country. A lot of people got into it without studying it in any depth." And from RCA, executive vice president George H. Brown is quoted as saying, "There are some 15,000 industrial research laboratories in the U.S., and the nation is spending about \$24 billion [two-thirds of which is federal money] in them to develop some new technical product annually. However, this is less than one new product per laboratory per year. Of those 10,000 new products, some 80% die in infancy. And most of the survivors represent not substantive innovation, but merely style modification."⁴³⁴ However, none of these sentiments necessarily implied that firms ought to apply the same cost control techniques to R&D as they were to manufacturing. Instead, these were expressions of fact: more money spent on R&D did not guarantee more profitable products or increased sales income.

⁴³³ In the second half of the 1960s, concerns over inflation gave way to national policies intended to increase the price of money through the manipulation of interest rates. The following decade, some blamed this for the decline in long-term investments in R&D. It was said that higher interest rates made common stocks less attractive to investors, placing new businesses in the position of relying more on debt, rather than equity, financing. Straddled with debt, new businesses would suddenly become much riskier investments, causing venture capitalists to withdraw from their role as sources of early-stage financing. See James Brian Quinn, "New Approaches for New Public Needs," *Research Management*, Vol. 20, No. 6 (November 1977): 12. Since the theoretical relationship between interest rates and business decision-making has been a perennial source of controversy, we can be sure that interest rates played *some* role in *some* of the anxiety that emerged in the late 1960s over R&D budgets, but we cannot be sure to what extent or how the two are related.

⁴³⁴ Editorial, "Research & Development" *Forbes* (November 15, 1968): 36.

The same editorial also suggested that “the new breed of technologically oriented top management” was responding to escalating R&D costs by learning more about their own programs and increasing their communication with R&D departments. As *Forbes* saw it, Robert A. Charpie, president of Bell & Howell Co. and the former director of technology at Union Carbide, was “a man who is typical of the new breed.” “We in top management have to do our job better,” Charpie explained for the magazine. “There’s no substitute for hard-headed involvement by the chief executive. You have to have the guts and the patience to see it through. But you also have to have the courage to quit when the time comes or modify your plan, if it’s not living up to advance notices.”⁴³⁵ To *Forbes*, this meant that, “Quite clearly, business has come a long way from the days when money lavished on R&D was a self-justifying good. Getting the goose to lay those golden eggs requires far more than dollars. More and more chief executives are realizing that they must give firmer guidance to R&D—and *learn* about the innovation process.”⁴³⁶

Joining this discussion was Harold D. Sasaki, whose May 1969 article in *Management Accounting* (formerly the *NACA Bulletin*) reiterated many of the same warnings that Quinn, Moffitt, McFadden Jr, Shaffer, and others had expressed a decade earlier. By its “very nature,” argued Sasaki, R&D activities “cause many problems relative to planning and control at the organizational level.”⁴³⁷

The fact that the result of most research activity (the product) is not known with any reasonable degree of accuracy, in advance, is an important feature which makes R&D so different from production and other routine operations. In fact, all that is possible in most cases are judgements, concerning the probabilities of

⁴³⁵ *Forbes* (November 15, 1968): 39.

⁴³⁶ *Forbes* (November 15, 1968): 39.

⁴³⁷ Harold D. Sasaki “Planning and Controlling Research and Development Costs” *Management Accounting*, Vol. 50, No. 9 (May 1969): 44.

possible outcomes. A company's major input to R&D is money (wages and salaries account for about 75%); however, the output of the R&D effort may be intangible and difficult to measure. ... Thus, the progress of a research venture is not a function of time; it cannot be measured in terms of so many units produced per hours.⁴³⁸

Again, it was “the very nature of R&D” that made it “exceedingly difficult to measure a project's ultimate financial potential accurately.”⁴³⁹ If R&D productivity was to be enhanced, it had to be done by improvements in planning, budgeting, and communication that helped integrate researchers' on-the-job experiences with company needs—not by increasing or decreasing project funding on the basis of changing sales volume.

In the same issue of *Management Accounting*, Howard W. Dicke drew out the implications of Sasaki's observation that researchers' salaries accounted for the largest chunk of any R&D budget. Dicke, a midwesterner and MBA graduate of the University of Washington, was controller at Concrete Pipe Machinery Co. of Sioux City Iowa as well as a member of the Cedar Rapids NACA Chapter. Researchers, explained Dicke, possessed a “spirit of logical inquiry [which] is not confined to technical problems but extends to all rules, regulations, practices and events which affects them.”⁴⁴⁰ This meant that, if the rigorous cost control techniques used in manufacturing were transplanted onto R&D, one could expect that researchers “will not cooperate to make these practices effective unless they are convinced they are worthwhile.”⁴⁴¹ Researchers could be forced

⁴³⁸ Sasaki (May 1969): 44.

⁴³⁹ Sasaki (May 1969): 41

⁴⁴⁰ Howard W. Dicke “Management Accounting for Research and Development Projects” *Management Accounting*, Vol. 50, No. 9 (May 1969): 40. Dicke does not suggest that the “spirit of logical inquiry” is an in-born feature of the researcher, but the result of his having been “better educated, which tends to spawn criticalness”(ibid).

⁴⁴¹ Dicke (May 1969): 40

to oblige, of course, but the fact that “the value obtained from research rests upon effective utilization of highly trained and very costly manpower,”⁴⁴² made it undesirable to push cost saving procedures too aggressively upon the R&D program. On these premises, Dicke reached the same conclusion as Sasaki in arguing that,

The nature of Research and Development work is such that it is neither possible nor desirable to supervise and control activities as closely as production operations. The composition of costs for research and development work is quite different from that of ordinary manufacturing operations. ... The application of budgetary cost control to Research and Development emphasizes advance cost planning and compensation of current expenses with budgets to aid management in keeping actual expenses within authorized limits. In this respect, the control of research and development costs is more like the control of advertising and administrative expenses. ... Rather stringent administrative and accounting controls are not particularly applicable to Research and Development personnel since, by virtue of the type of work, a certain amount of freedom and latitude must be allowed.⁴⁴³

Treating R&D costs in the same manner as administrative expenses implied that current expensing was no less appropriate for R&D than it was for administration. As we have already seen, this lesson was learned by cost accountants early on. They were, after all, the in-house professionals responsible for designing, maintaining, and improving cost systems for their employers. In this process, cost accountants bumped up against the needs of R&D personnel; may or may not have entered into a “scientist-accountant feud”; and emerged on the other side with systems and practices designed to accommodate all parties as best they could. This was a cooperative approach. While postwar research managers like Charpie were only beginning to discover there was more to R&D management than a budget forecast, the need for cooperation and mutual

⁴⁴² Dicke (May 1969): 40

⁴⁴³ Dicke (May 1969): 40

recognition would have been old news to those who pioneered the nation's first R&D organizations.

Research managers at DuPont, General Electric, Eastman Kodak, Corning Glass, Westinghouse, and Standard Oil, for example, long ago recognized the necessity for accountants and scientists to learn to accommodate one another's on-the-job needs. Charles Edward Kenneth Mees offers a straightforward example from that earlier period. Mees had been recruited to Rochester, New York from England early in the century to lead R&D activities at Eastman Kodak, and for many years, his success at cultivating a culture conducive to R&D had helped make him and his company famous. Through many speeches and writings on the subject, Mees quickly became well-known as an R&D pioneer and expert. Ahead of its time, *The Organization of Industrial Scientific Research* (1920), the first textbook on R&D management, was reprinted in 1960.⁴⁴⁴ In it, he explained that, "An estimate of the cost of a new investigation is exceedingly difficult to prepare and can be based only on careful and accurate costs which have been kept previously for work of a similar kind."⁴⁴⁵

A satisfactory cost accounting system is extremely valuable in a laboratory, both for the control of current expenditures and for the preparation of the budget. The cost accounting methods employed for the control of manufacturing processes are not suitable for the control of experimental work, and it is undoubtedly better to develop an accounting system in the laboratory itself rather than to obtain the costs by general cost accounting methods used in the Works. A laboratory cost accounting system...if properly directed it can relieve those in charge of the laboratory from some of the burden of supervision.⁴⁴⁶

⁴⁴⁴ Hounshell

⁴⁴⁵ Charles E.K. Mees, *The Organization of Industrial Scientific Research* (1920), 127

⁴⁴⁶ Mees (1920), 127

Mees' text suggests that, wherever R&D personnel and cost accountants approached the problems of the other with an earnest attempt at cooperation, the otherwise unwieldy problems of accounting for R&D could be made tractable.

By contrast to the cost accountants, financial accountants were outsiders to the emerging R&D process, and remained as such throughout the ensuing decades. The functions of financial accountants were in large part limited to auditing the books of their clients and sometimes, for an additional fee, providing them with a little tax help. Over the years, they had replaced direct inspection of a company's resources with statistical sampling methods that made the complexity of global forms of business more manageable. Financial accountants may have possessed all of the prestige,⁴⁴⁷ but without the experiences confronting cost accountants in their day-to-day jobs, the auditor elites never developed comparable expertise in the area of R&D accounting. One might naively expect that, with all their experience auditing the books of various firms, financial accountants would be able to offer a unique perspective based on a study of differences in R&D programs across firms. Through such comparative study, this group could have identified best practices in R&D accounting and accordingly refined the "professional judgment" they prized above all else. For three-quarters of a century, this did not happen. Despite all their exhortations about principles and judgment, despite pleas heard from some accounting academics to "engage in a bit of soul searching regarding our basic assumptions,"⁴⁴⁸ financial accountants as a group failed not only to appreciate the "spirit

⁴⁴⁷ Alan J. Richardson "Professional Dominance: The Relationship Between Financial Accounting and Managerial Accounting, 1926-1986" *Accounting Historians Journal*, Vol. 29, No. 2 (December 2002): 91-121.

⁴⁴⁸ V. E. Odmark "Current Challenges to the Accounting Profession," *Accounting Review*, Vol. 32, No. 5 (April 1960): 275

of logical inquiry” and “criticalness” that animated R&D; they failed to integrate that same critical spirit throughout their own work, as well.

Chapter 5

Uniformity vs. Flexibility

5.1. Ignorance is Bliss?

While the growing cost accounting literature was rife with nuance, disagreement, case studies, and recommendations relating to R&D, financial accountancy of the 1950s and early 1960s was too preoccupied with other matters immediately under its purview to notice. Of all the cost accounting contributions overlooked in the 1950s, the NACA's Research Series No. 29, *Accounting for Research and Development Costs*, was by far the most blatantly neglected. Building on the earlier study completed by NACA research staff in 1939, as well as the essays that appeared in the *Bulletin* since that time, the NACA Committee on Research set out in the mid-1950s to bring together "a comprehensive discussion of accounting as a tool for research management [that was otherwise] lacking."⁴⁴⁹

As late as 1939, nearly forty years after the nation's first industrial research lab went into operation, the prospects for clarity on these issues were not looking good. In the opening line to the NACA staff study of 1939, the ongoing confusion around R&D accounting was made plain: "Probably there is no group of expenditures about which there is less agreement as to its disposition in the accounts than the group usually designated as research and development costs."⁴⁵⁰ On a questionnaire sent out to 106

⁴⁴⁹ NACA, Accounting Research Series No. 29, "Accounting for Research and Development Costs." *NACA Bulletin*, Section 3 (June 1955), 1375.

⁴⁵⁰ R.P. Marple. "Present-day Practice in Accounting for Research and Development Costs" *NACA Bulletin* (March 1, 1939), 889

companies, respondents were to indicate how they treated the costs of R&D with respect to new products, improvement on products, new manufacturing methods, new and special machinery, and pure research. Even the much-discussed distinction between new-product and product-improvement R&D proved to be “but a minor variation in practice.”⁴⁵¹ Most companies charged off *both* these costs, as incurred. The assistant to the president of IBM in Endicott, NY insisted on this point in an earlier *Bulletin* essay of 1935: “If we start to develop a machine and our laboratory spends \$10,000 on it before discovering that it is no good, or our patent department finds that someone else has beaten us to it, what do we do? Do we capitalize that \$10,000? No! Do you charge it to the cost of developing another machine which comes through the laboratory successfully? No! We charge it off.”⁴⁵²

“The impression one gets from these figures and from comments on the questionnaire,” wrote R.P. Marple, the report’s author, “is that there exists a very great reluctance to capitalize research and development expenses.”⁴⁵³ In Research Series No. 29, the NACA Committee on Research observed that, of the companies participating in the 1955 study, the vast majority charged costs against income of the current period in which they are incurred. The earlier NACA study of 1939, the Committee noted, had “also found that research and development costs were usually expensed.” Apparently, very little had changed in the time between the 1939 study and study completed in 1955. Most companies expensed R&D as incurred, but a fraction continued to defer the same costs.

⁴⁵¹ Marple (March 1, 1939), 886

⁴⁵² Walter F. Titus “Control and Distribution of Selling Costs” *NACA Bulletin* (September 1935)

⁴⁵³ Marple (March 1, 1939), 889

To make sense of some of their latest findings, the Committee recalled the remarks of Harvard Business School professor Robert N. Anthony who, in a presentation before the Fifth Annual Conference on Industrial Research at Columbia University, warned “against the uncritical application of formulas and yardsticks for evaluating research.”⁴⁵⁴ These included the popular research-as-a-percentage-of-sales measure, as well as the less commonly use percentage-of-invested-capital and percentage-of-net-profit. Not only did such yardsticks not speak to the quality of the work done in a given R&D organization; they were built on the assumption “that there is some relationship between research spending and sales,” capital, or profit.⁴⁵⁵ Anthony and his colleagues had just completed several studies of industrial research commissioned by the Office of Naval Research and the Bureau of Labor Statistics, and the experience had made clear to Anthony that generally accepted accounting principles developed by the profession’s elites were nearly useless for R&D accounting.⁴⁵⁶ In a study completed in 1952, Anthony had observed that,

There are so many differences in the accounting definitions used by various laboratories that it is impossible to compare dollar figures for two separate laboratories unless care has been taken to make appropriate accounting adjustments. There is no uniform set of accounting rules adhered to by all, or a large number of, research organizations. ... The generally accepted accounting principles are few in number, and they are stated in such broad terms that they usually do not provide a basis for answering the questions that arise about research costs. Although some of them apply to the definition of research costs in

⁴⁵⁴ NACA (June 1955), 1435

⁴⁵⁵ Robert N. Anthony *Management Controls in Industrial Research Organizations* (Cambridge, MA: Harvard University Press, 1952): 295

⁴⁵⁶ Although Anthony’s work in the late 1940s and 1950s focused on R&D and its relation to cost control, by the late 1950s his sights were set elsewhere. See his “Management Accounting: A Personal History” *Journal of Management Accounting Research*, Vol. 15 (2003): 249-253, and “Reminiscences About Management Accounting” *Journal of Management Accounting Research*, Vol. 1 (Fall 1989): 1-20.

total, none affects the detailed breakdown of costs for use in the internal administration of research organizations.⁴⁵⁷

By 1954, and indeed throughout the rest of that decade and on into the next, Anthony would become increasingly frustrated at the way accounting was being used to manage R&D.⁴⁵⁸ At the Fifth Annual Conference on Industrial Research, Anthony presented a paper from which the NACA report of 1955 quoted at length:

In my opinion, no yardsticks available are sufficiently accurate to be useful as a means of measuring the efficiency with which research spending was done, although some are extremely useful as a guide to research planning, which is another matter. In particular, I take a rather dim view of the widely publicized formulas that purport to measure the profitability of research by comparing research costs with some measure of profit earned from the sales of new products. These profit formulas may yield impressive figures that will help sell management on the desirability of increasing the research budget and if they do, fine. But to attempt to use them as a means of measuring how well the research was done is, so far as I am concerned, like attempting to evaluate a Rembrandt painting with a spectroscope.⁴⁵⁹

The NACA Committee confirmed Anthony's findings with their own: "This study shows that, where possible, estimates of expected benefits from specific research projects are made in dollar terms. At the same time, it brought out the limitations of available measurement methods and the need to use them with judgment. In this discussion, it was often stated that judgment of competent research management may be more reliable than financial figures."⁴⁶⁰ The same financial considerations used in the planning of R&D were inappropriate tools for its valuation, and this message reappeared throughout the report.

⁴⁵⁷ Anthony (1952), 91-2

⁴⁵⁸ We see Anthony's frustration build up to the point that, in the May/June 1963 issue of the *Harvard Business Review*, he defiantly insists that with the AICPA's ongoing failures to issue useful accounting principles, the time had come for the SEC to use its legal authority and mandate those principles from above. See his controversial and widely-cited "Showdown on Accounting Principles"(1963).

⁴⁵⁹ Robert Anthony "Some Key Qs for the Research Administrator," Paper presented at 5th annual conference on industrial research, Columbia U, 1954

⁴⁶⁰ NACA (June 1955), 1435

In an earlier chapter, the Committee highlighted one area in which theory and practice were at an impasse. Although it “is usually possible to measure the costs of research efforts [for budgetary purposes,] it is often very difficult to assign research costs to products and to measure the results obtained from research in terms of sales or profit.”⁴⁶¹ One of the “principal reasons for this,” the report stated, was “that the outcome of experimental work always involves a degree of uncertainty.”⁴⁶² Both the outcome itself (i.e., whether a project would succeed or fail) as well as the timing of that outcome were mired in uncertainty. The diversity of accounting treatments used in practice reflected the absence of any effective “generally accepted accounting principle” for handling the necessary uncertainty of R&D. Without any such shared understanding to guide them, accountants engaged in practices that, to a matching purist, violated the theoretical equivalence between the costs of a product, on the one hand, and the profit earned from its sale, on the other. The report explained that, even when the costs of R&D *can* be charged directly to a specific product or process account, as in the case of improvement-oriented R&D, “this practice does not always measure the incidence of benefits from research.”⁴⁶³

For example, in one company a series of experiments directed towards improving manufacturing processes for a specific product was only partially successful. However, incidental knowledge was gained which led to creation of a new and entirely different line of products several years later. Under such conditions, the product charged is not the one which receives the future benefit and customary cost assignment procedures do not match [R&D] costs with corresponding product income.⁴⁶⁴

⁴⁶¹ NACA (June 1955), 1379.

⁴⁶² NACA (June 1955), 1379. As we have already seen, McFadden’s studies would confirm these findings.

⁴⁶³ NACA (June 1955), 1428

⁴⁶⁴ NACA (June 1955), 1428

Hardly the panacea its promoters made it out to be, matching deferred R&D costs to future income proved to be as arbitrary in practice as it was pure in theory. By contrast, the reasons given for current expensing were developed through the test of experience. Thus, according to the report, the “reasons usually given for viewing research and development costs as a current expense” could be summarized as follows:

1. In most companies research is a continuing operation....
2. Benefit received from research often cannot be measured and related to sales income of any specific period. ...
3. Research involves risk of failure and sometimes the success or failure of a project may not become apparent for several years. In some types of research, successful projects comprise a very small percentage of the total number of projects undertaken. To defer research and development project costs until the outcome is definitely known introduces an asset of uncertain value into the balance sheet. ...
4. The useful life of knowledge gained by research cannot be predicted with sufficient accuracy to serve as the basis for amortizing [i.e., gradually writing off] costs over a series of future periods.⁴⁶⁵

These results were printed in a June 1955 issue of the *NACA Bulletin* and seemed to confirm the experiences of many cost accounting writers of the time. But, again, financial accountants did not acknowledge the report nor the issues that were at stake. A less charitable reading of events would suggest that, had the elites of financial accountancy paid the report any mind, authorities in the profession might have taken more seriously the problems caused by the deferral of enormous R&D charges in the late 1950s and throughout the 1960s. A fairer reading, however, would reiterate an observation made in 1969 by George R. Catlett, senior technical partner at Arthur Andersen and member of the APB. Catlett noted that the APB, no less than the CAP before it, “has been so busy ‘putting out fires’ and dealing with a large and ever-increasing backlog of current

⁴⁶⁵ NACA (June 1955), 1415-9

problems that it has never established an adequate basis upon which to build.”⁴⁶⁶ John Queenan, APB member and senior partner at Haskins & Sells, agreed; the APB, he explained “always ended up fighting brush fires.”⁴⁶⁷ In the same vein, perhaps the APB was busy “putting out fires” when, in 1962, it waved away a plea from the banking community to clarify the accounting for deferred R&D expenses.

5.2. The Fiero Letter

According to the minutes of the APB’s June 14-15, 1962 meeting, the plea was first made on February 26th of that year when Charles E. Fiero, Vice President of the Chase Manhattan Bank in New York, wrote a letter to J. Kenneth Boyles, Vice President of the National State Bank in Elizabeth, New Jersey. In this letter between bankers, Fiero “raised the question of the proper showing of unamortized engineering and development costs in contractors’ balance sheets.”⁴⁶⁸ The relevant portion of that letter reads as follows:

We urgently recommend the whole subject of reporting unamortized engineering and development expenses be reviewed. In this connection we specifically would like...any such amounts (whether included in “Contracts in Process” or similar accounts) be broken out and shown under a separate caption on the balance sheet. Where the amounts are material we propose that the caption be footnoted to disclose that portion which is attributable to contracts on hand and that portion attributable to contracts yet to be written. Where the latter is material we further recommend that some explanation be provided to enable the reader to assess potential exposure.

As you know, generally accepted accounting procedure permits a company which has incurred substantial engineering and development expenses to amortize the amounts over a period of years against the related contracts. Where the amounts

⁴⁶⁶ Catlett 1969, 62

⁴⁶⁷ Editorial “Accountants Turn Tougher” *Business Week* (October 18, 1969): 125.

⁴⁶⁸ Minutes of the meeting of the AICPA’s APB (June 14-15, 1962), 4-5

are to be written off against contracts on hand, the amortization schedule is readily established. Where the expenses are to be written off over anticipated production runs and against business not yet on the books, most companies attempt to estimate production experience and set a schedule of amortization accordingly.

In both instances the unamortized amounts are set up on the balance sheet and included in the caption “Contracts in Process.” Seldom are the amount broken out and hence the reader has no way of knowing what exposure there is to sudden cancellation of orders or failure of anticipated order to develop. The absence of disclosure here assumes special significance for, while many contracts provide for termination claims, engineering and development expenses are always difficult to allocated and hence justify.⁴⁶⁹

These bankers were troubled by the practice of grouping own-R&D with contracted-R&D and presenting them as a single category on balance sheets. Doing so meant that the costs of own-R&D incurred in *anticipation* of winning government contracts was treated in the same manner as R&D costs incurred on contracts *already in hand*. This problem was relatively new to bankers since, as far back as the opening decades of the twentieth century, most loans were made to industrial concerns on a short-term basis. By the 1960s, however, major banks were now providing long-term or “cash-flow lending.” This brought the role of the banker closer to that of investors,⁴⁷⁰ and subjected bankers’ funds to the ambiguities of R&D accounting. According to the eminent accounting historians Gary Previts and Barbara Merino, this change from short-term to long-term lending led to a situation in which, “Bankers wanted to predict future cash flows [but] maintained that this was impossible given the inconsistencies in GAAP.”⁴⁷¹

⁴⁶⁹ As quoted in Carman G. Blough “Accounting and Auditing Problems” *Journal of Accountancy*, Vol. 114, No. 2 (August 1962): 69-70.

⁴⁷⁰ By 1972, two authors writing in the *Financial Analysts Journal* observed that, “The division between lenders and equity holders has less reality today than in previous periods, although the equity holder, bearing the residual risk, desires more detailed information for investment appraisal than the lender”(Norby and Stone 1972, 40).

⁴⁷¹ Previts and Merino (1998), 337

By February 1966, a prominent banker could be quoted in the *New York Times* as describing “generally accepted accounting principles” as a mere turn of phrase, one that had “almost reached the status of a cliché.” Reporting on a speech delivered before the annual meeting of the American Bankers Association, the *Times* quoted J. Howard Laeri, vice chairman of the First National City Bank of New York, as saying: “The accounting profession cannot say precisely—or perhaps even approximately—what those ‘generally accepted principles’ are. These are nice words, but they are not especially informative, and they do nothing to help us interpret disclosure. ... You can easily guess what kind of confusion this produces in trying to judge an important aspect of a company’s worthiness for term loans.” Reaction to this piece took the form of a defensive, self-righteous editorial in the *Journal of Accountancy* that waved away the *Times*’ warning that patience among bankers “is starting to wear thin.”⁴⁷²

Generally accepted accounting principles had long held that contracts of any sort—whether for R&D or otherwise—were to be included in a company’s balance sheet, much like a newly-purchased piece of equipment. Both had definite values, the former set by the terms of the contract, the latter by the price paid in the transaction. The problem raised by Fiero’s letter was whether the *anticipation* of contracts was sufficient grounds to warrant the inclusion of the associated R&D costs along with other balance sheet assets. Experience had proved this practice deficient, as the remainder of the banker’s letter makes plain:

In recent years, cancellation of orders and sharp cutbacks in original production schedules have resulted in substantial “write-offs” of unamortized engineering and

⁴⁷² H. Erich Heinemann “National City Aide Criticizes C.P.A’s” *The New York Times* (February 2, 1966): 43 & 45; Editorial “A Banker Looks at Auditors” *Journal of Accountancy*, Vol. 121, No. 3 (March 1966): 31-2.

development expenses. Some of these “write-offs” have occurred with no mention of such possibility in the report [i.e., in a company’s audited financial statement]. We naturally hope that the accounting fraternity will take measures to tighten its procedures to insure that this does not happen again.

The problem of sudden cancellations arose as a result of government’s power to enter or exit contracts at its convenience. When competitions for contracts were brought to an abrupt close, firms that had been incurring R&D costs in the hopes of placing a winning bid would discontinue their efforts and write off the accumulated deferred costs out of income. The same could happen when a contract already in hand was rescinded by a sudden cancellation; firms could seek renegotiations or appeal to the government to recover charges already incurred in pursuit of the terminated contract, but there was no guarantee that this would work. Anywhere from a few hundred thousand to several million dollars could be at stake, and with these amounts written off, the resulting losses were unwelcome surprises to investors and creditors, to say the least. Recognizing these difficulties, Fiero’s letter suggested how accountants could help mitigate some of the damage:

[W]e recognize that *the accountant is faced with some very difficult problems in this area. In large measure he must rely heavily upon management’s appraisal of these expenses and the status of the related backlog of firm or anticipated orders. Judgment becomes a vital consideration. So long as this is the case, however, we feel it is especially important for the accountant to segregate the unamortized amounts for all to see and question. This at least would take some of the onus off the profession and notify all concerned with the companies’ affairs of an area which could require further explanation. Where the amounts are large, qualifications of the accountant’s certificate might even be justified.*⁴⁷³

⁴⁷³ Carmen G. Blough “Accounting and Auditing Problems” *Journal of Accountancy* (August 1962), 69-70. Emphasis added.

Fiero had hit the nail on the head. Auditors were hardly equipped to verify the future value of R&D expenditures, and this already-hazardous task was compounded by the vicissitudes of defense contracting. Both factors compelled accountants to “rely heavily upon management’s appraisal.” Whether sudden losses arose as a result of a failed bid or contract termination, Fiero pleaded, the amounts yet to be written off (i.e., “the unamortized amounts”) should be disclosed separately from the amounts recoverable from government (i.e., the amounts “written off against contracts on hand”). Distinguishing the two, he concluded, would better protect investors and bankers, such as himself.

A month later, copies of the letter were distributed at a joint meeting of the credit-granters’ organization known as the Robert Morris Associates and the AICPA’s committee on relations with banks. At this meeting of March 29, 1962, John W. Queenan, AICPA President and managing partner at Haskins & Sells, together with Richard C. Lytle, director of the AICPA’s technical services division, “agreed that a reply to the letter should come from the Institute [ie, the AICPA] without going through the research and other procedural mechanics involved in the usual accounting pronouncements.”⁴⁷⁴ Founded in September 1959, the APB’s *raison d’être* was its bold new research orientation, which took institutional form in an expanded Accounting Research Division with the intention of reducing the more troublesome differences in practice. Studies were commissioned and research efforts were monitored and assisted by technical advisory committees appointed for each research study. Once projects reached completion, final reports were submitted to APB members for their consideration. Now,

⁴⁷⁴ Minutes of the meeting of the AICPA’s APB, June 14-15, 1962, pp.4-5

in the aftermath of two-recently completed research studies, Accounting Research Study (ARS) Numbers 1 and 3,⁴⁷⁵ Queenan and Lytle were recommending bypassing the APB's research process in handling the questions raised by the Fiero letter.

That Queenan, in particular, made this recommendation comes as something of a surprise, given the remarks he made less than six months earlier at the annual meeting of the AICPA, held in Chicago in November 1961. Commending the APB and its embodiment of the profession's newfound commitment to research, Queenan proudly declared that, "It is this approach of building from a common foundation that distinguishes the present coordinated research program from our former piecemeal approach to individual problems."⁴⁷⁶ The former approach referred to that associated with the APB's predecessor, the Committee on Accounting Procedure. Having been "needled" into existence in the late 1930s, CAP members had early considered using research as a means to clarify such indispensable concepts as "asset," "liability," or "income." It was decided, however, that such a project would take many years' time to complete, and so the idea was shut down before it had a chance to begin.

As Queenan reminded his audience that November in Chicago, the resulting piecemeal approach had done little to strengthen the resolve of practitioners tasked with applying extant principles to challenging situations. "[M]ost of us encounter frequently the problem of convincing clients or others of the basic reasoning or authority for the particular application or the principle itself. In such circumstances," he explained, "it is

⁴⁷⁵ Accounting Research Study No. 1, *The Basic Postulates of Accounting*, by Maurice Moonitz; and Accounting Research Study No. 3, *A Tentative Set of Broad Accounting Principles for Business Enterprises*, by Robert Sprouse and Maurice Moonitz.

⁴⁷⁶ John W. Queenan "The CPA and the Business Public" *Haskins & Sells Selected Papers* (1961): 16

not uncommon for the CPA to encounter criticism or difficulty because the client or other interested parties have their own ideas concerning the proper principle or application.”⁴⁷⁷ The best remedy for the crippling circumstances that CPAs often found themselves in, was “a comprehensive, coordinated, and well-reasoned statement of accounting principles.”⁴⁷⁸ By Spring 1962, however, Queenan’s focus had shifted dramatically. Earlier, his concerns laid with the CPA whose judgment was being undermined by clients insisting on their own interpretation of accounting principles. Six months later, his new priority was placating business leaders outraged over the principles proposed in the newly-issued ARS No. 3—a report described as “a lightning rod that many viewed as highly controversial and objectionable even before it was published.”⁴⁷⁹

No opinion was ever issued by the APB on the basis of ARS No. 3, nor on the study upon which it built, ARS No. 1. Together, these two studies had elicited an “almost universally strong negative reaction.”⁴⁸⁰ At the outset, the purpose of ARS No. 1 was to propose a set of accounting postulates, while ARS No. 3 was to translate those postulates into principles. Both had been assigned in the hope of establishing the “common foundation” that Queenan and other leaders of the profession had so desperately fought for. But when the projects were submitted for consideration, the “fundamental limitations of the [postulates-and-principles] method” were exposed.⁴⁸¹ This approach relied heavily

⁴⁷⁷ Queenan (1961): 15

⁴⁷⁸ Queenan (1961): 16

⁴⁷⁹ Robert J. Swieringa “Robert T. Sprouse and Fundamental Concepts of Financial Accounting” *Accounting Horizons*, Vol. 25, No. 1 (March 2011): 210.

⁴⁸⁰ Myron J. Gordon “Postulates, Principles and Research in Accounting” *The Accounting Review*, Vol. 39, No. 2 (April 1964): 252

⁴⁸¹ Myron Jules Gordon of the University of Rochester summarizes this method as follows: “First, a set of propositions called the postulates of accounting have an objective existence and validity. To discover these postulates one need only commission a competent scholar to look at the economic and political environment... Second, once these postulates are ascertained, the principles and rules that result in the

on deductive logic and theory to arrive at a set of principles governing what accounting “ought” to be. In the eyes of the APB, the authors of these studies had so fully severed ends from ends, what ought to be from what is, that the resulting treatises were little more than thought experiments. The Board’s reaction, combined with outrage expressed from the business community, effectively shut down the effort to establish a common basis of concepts upon which to build coherent guidelines for the practicing public accountant. Seen in this light, Queenan and Lytle’s decision to respond to the Fiero letter “without going through the research...involved in the usual accounting pronouncements” starts to make sense.

Prior to receiving the results of these two studies, members of the newly-formed APB had felt confident that “independent,” “thoroughgoing,” “carefully reasoned,” and “fully documented”⁴⁸² research would recast—rather than depart from—much of the profession’s collective wisdom, developed over many years of practice against the test of experience. What they received, instead, was, a set of “theoretical hypotheses untried in practice or tried and discarded as impractical, ” argued APB member Carman Blough.⁴⁸³ Fellow Board members Oscar S. Gellein, John H. Zebley, Jr., and Leonard Spacek agreed. The set of principles articulated in ARS No. 3, noted Spacek, “represents a series of personal opinions that are not supported in any significant manner as to *how* or *why* the so-called principles ‘meet the needs of all interested groups,’ or as to *why* the conclusions

uniform and correct practice of accounting may be deduced by deductive argument”(Gordon (April 1964): 253).

⁴⁸² As quoted in Stephen A. Zeff *Forging Accounting Principles* (1972): 176

⁴⁸³ “Comments of Carman Blough” in Accounting Research Study No. 3, *A Tentative Set of Broad Accounting Principles for Business Enterprises*, pp. 60-63

represent a sound and coordinated framework of accounting theory.”⁴⁸⁴ As a result of its experience with these two studies, suggests Stephen Zeff, the APB became “disillusioned with, or at least skeptical toward, the potential that fundamental or ‘theoretical’ research might have for solving accounting problems.”⁴⁸⁵ Thereafter, the APB would pursue the sort of “piecemeal approach to individual problems” that characterized the earlier efforts of the CAP and that Queenan himself had once disparaged.

As the APB withdrew into the same ineffective habits of the past, Lytle wrote to APB Chairman Weldon Powell on May 10, 1962, inquiring as to whether the Board might offer not an official opinion, but an interpretation of a CAP pronouncement dating back to 1953. This pronouncement, Statement No. 8, was found in the Inventory Pricing chapter of Accounting Research Bulletin No. 43, and it merely emphasized that companies ought to value their inventories, inclusive of contracts, on a consistent basis across successive financial statements.⁴⁸⁶ If managements decided changes had to be made in the way inventories were valued, then Statement No. 8 required that such changes be disclosed publicly. Rather than risk issuing a transformative opinion, Lytle’s recommendation to Powell was unlikely to disturb management clients. ARS No. 3 had embarrassed the Board, and to conservatives like Powell, rocking the boat with corporate clients was absolutely to be avoided if the APB was to succeed.⁴⁸⁷

⁴⁸⁴ “Comments of Leonard Spacek” in Accounting Research Study No. 3, *A Tentative Set of Broad Accounting Principles for Business Enterprises*, pp. 77-79.

⁴⁸⁵ Stephen A. Zeff *Forging Accounting Principles* (1972): 177

⁴⁸⁶ Accounting Research Bulletin No. 43: *Restatement and Revision of Accounting Research Bulletins*, was issued in June 1953. Chapter 4 of this Bulletin, “Inventory Pricing,” defines “inventory” as “the aggregate of those items of tangible personal property which (1) are held for sale in the ordinary course of business, (2) are in process of production for such sale, or (3) are to be currently consumed in the production of goods or services to be available for sale”(1953, 27).

⁴⁸⁷ Weldon Powell “Putting Uniformity in Financial Accounting in Perspective” *Law & Contemporary Problems*, Vol. 30 (Fall 1965): 674-689.

Powell, a senior partner at Haskins & Sells' Executive Office in New York, was Queenan's longtime friend, colleague, and former instructor at the University of Illinois, Urbana-Champaign, and the two men shared a belief that reforms in accounting had to come about gradually and only with the full assent of the business community.⁴⁸⁸ Having considered Lytle's recommendation, Powell drafted a response in letter form and read it aloud at the APB's June 14th meeting. According to minutes from that meeting, Powell argued that "engineering and development costs not incurred in connection with firm contracts ordinarily would not be shown as part of the inventory of contracts in process but would instead be shown as deferred charges."⁴⁸⁹ There is no indication in those meeting minutes that even a single objection was raised to Powell's stated position.

Left unspoken was the possibility that own-R&D—or, in Pentagon lingo, "Independent R&D" or IR&D—might be better accounted for as a current expense. Instead, meeting minutes noted that the "consensus appeared to be that the literature on the subject is reasonably clear and that it should not be necessary for the Board to take action on this specific application of the related principles."⁴⁹⁰ At the same meeting, Carman Blough was asked to share the Board's decision publicly in his *Journal of Accountancy*, and in the August 1962 issue, a long passage from Powell's letter is reprinted in Blough's column, as below:

⁴⁸⁸ In addition to Powell and Queenan, other major players in early- and mid-twentieth century accountancy who hailed from the same University of Illinois department include Leonard Savoie, Arthur Wyatt, Andrew Barr, Paul Grady, Vernon K. Zimmerman, Robert K. Mautz, Maurice Moonitz, Orace Johnson, A.C. Littleton, and Hiram Scovill. The reputation of this accounting department made its faculty and graduates obvious candidates for recruitment by headhunters at the Big Eight firms. For more on the significance of this institution, see Norton M. Bedford's *A History of Accountancy at the University of Illinois at Urbana-Champaign* (Urbana: Center for International Education and Research in Accounting, 1997).

⁴⁸⁹ Minutes of the meeting of the AICPA's APB, June 14-15, 1962, pp.4-5

⁴⁹⁰ Minutes of the meeting of the AICPA's APB, June 14-15, 1962, pp.4-5

There is no specific pronouncement by the American Institute of Certified Public Accountants that deals with this matter. However, the general pronouncements of the Institute that concern reporting are applicable to it where the financial statements are accompanied by the opinion of a member of the Institute.

The fourth standard of reporting listed under generally accepted auditing standards calls for reasonably adequate informative disclosures. Article 2, Paragraph 2.02 of the Code of Professional Ethics calls for disclosure of material facts necessary to make the financial statements not misleading.

It seems to me that where accumulated research and development costs not related to firm's contracts are carried forward in the balance sheet in amounts that are material in relation to the financial position and operating results, consideration should be given to the classification of the unamortized balances and to the extent of explanatory disclosure concerning them.

Ordinarily, I think such balances would be classified as deferred charges rather than inventories and would be subject of footnote or other comment if the salient facts were not apparent from the face of the balance sheet. However, the treatment appropriate in a given case would be determined through the exercise of judgment in the light of the facts in that case.⁴⁹¹

The APB's response was simply this: there is no established convention or principle to deal with this specific issue, nor does there need to be. Have faith in the best judgment of the ethical accountant to discern, in each particular case, the proper classification of own-R&D expenditures and their presentation in clients' financial statements. This was not the first nor last time the APB would absolve itself of responsibility when problems were brought to its doorstep.

5.3. Uniformity vs. Flexibility

Similar responses were directed at those, both within the profession and without, who sought to reduce the range of accounting alternatives available to managements.

⁴⁹¹ As quoted in Carmen G. Blough "Accounting and Auditing Problems" *J of Accountancy* (August 1962): 69-70

This issue had been raised on many occasions in the past, most notably in the aftermath of the stock market crash of 1929.⁴⁹² After the war, the uniformity-vs-diversity debate reached a “veritable explosion”⁴⁹³ in the early 1960s as one more in a line of widely-reported survey revealed a stunning loss of investor confidence towards auditor-certified financial reports. Earlier surveys from the late 1940s had revealed much the same,⁴⁹⁴ compelling Maurice H. Stans, partner at Alexander Grant & Company, to pose the following questions of his peers:

What is wrong with accounting as the public sees it? Could it be the fact that there is still no broad authoritative code of accounting principles? ... Could it be that there is still a great range of accounting of accounting practice and that many identical transactions are treated in different ways by different companies? Could it be that the chief criterion of accounting procedures is their general acceptance, which is a passive concept in which the acceptance is by the creators of the practices and not by the reader?⁴⁹⁵

⁴⁹² Alfred I. Bernheim, *The Security Markets: Findings and Recommendations of a Special Staff of the Twentieth Century Fund* (New York, NY: Twentieth Century Fund, 1935); Howard C. Greer “To What Extent Can the Practice of Accounting Be Reduced to Rules and Standards?” *Journal of Accountancy* (1938): 213-223; Paul L. Morrison “The Interest of the Investor in Accounting Principles,” *The Accounting Review*, Vol. 12, No. 1 (March 1937): 37; A. C. Littleton, “The Relation of Function to Principles,” *The Accounting Review*, Vol. 13, No. 3 (September 1938): 233-241; Maurice C. Kaplan and Daniel M. Reaugh, “Accounting, Reports to Stockholders, and the SEC.” *The Accounting Review*, Vol. 14, No. 3 (September 1939): 203-236; Carman G. Blough, “Accounting Principles Interpreted in Light of Recent Developments.” *Journal of Business*, Vol. 12, No. 3 (June 1939): 265-279.

⁴⁹³ Stephen A. Zeff “The Work of the Special Committee on Research Program” *Accounting Historians Journal*, Vol. 28, No. 2 (December 2001): 141-186

⁴⁹⁴ Elmo Roper, *A Report on What Information People Want About Policies and Financial Conditions of Corporations*, Vol. I (The Controllership Foundation, Inc., 1948); Thomas H. Sanders, *Company Annual Reports to Stockholders, Employees, and the Public* (Cambridge, MA: Harvard University, 1949); Charles T. Horngren, “Disclosure” *The Accounting Review* (October 1957): 598-604; Charles P. Rockwood’s “The Changing Image of the Profession,” *Journal of Accountancy*, October 1960; Anonymous “Pitfalls for the Unwary” *Barron’s*, Vol. 42, No. 5 (December 24, 1962): 3, 8, 10, & 13; Corliss D. Anderson, *What the Financial Analyst Wants to Know* (New York: Financial Analysts Federation, 1962); Samuel W. Bryant “How’s the Annual Report Coming?” *Fortune* (January 1964): 104-7 & 172-4; William J. Baumol, *The Stock Market and Economic Efficiency* (New York: Fordham University Press, 1965); Corliss D. Anderson, “The Financial Analyst’s Needs” *Financial Analysts Journal* (March-April 1969): 98-109. For other discussions on this point, see: Weldon Powell “The Development of Accounting Principles” *Journal of Accountancy*, Vol. 118, No. 3 (September 1964): 37-43; Harold Q. Langenderfer “A Problem of Communication” *J of Accountancy*, Vol. 123, No. 1 (January 1967): 33-40; Editorial “Uniformity and Flexibility” *J of Accountancy* (April 1961): 33-34

⁴⁹⁵ Maurice H. Stans “Weaknesses in Present Accounting Which Inhibit Understanding of Free Enterprise” *Journal of Accountancy* (December 1949): 468-469

Evidently, unrestrained diversity, or “flexibility,” in accounting practice was misleading investors, and for Stans, the onus fell on the profession to right this wrong. George O. May of Price Waterhouse, stronghold of the old guard, saw things very differently. In an essay titled, “The Choice Before Us,” May’s challenge to Stans belied an elitism still pervasive among the profession’s leaders. We trust physicians to prescribe treatments when we fall ill, he wrote. We trust engineers to build bridges over which we pass. In neither case do we, as laymen, challenge their authority. Why, then, should accountants “insist or concede that income determination should be such that every reader should be able to understand exactly what was in the mind of the accountant in deciding its content and structure? Accountants do not render adequate service by accepting as a goal a simple ‘factual’ report which the users must interpret for themselves in the light of their appraisal of the economics conditions and on the basis of their lesser knowledge of the events which the accounts reflect.”⁴⁹⁶ Failures, if there were any, were on the part of uneducated investors, not accountants.

Despite this admonition, investors continued to look upon certified financial statements with suspicion. Corporate scandals made possible by changes in accounting methods were exposed publicly in 1962. “Generally accepted accounting principles” provided them no reliable basis for comparing one financial statement to another, neither with respect to the performance of a single firm over time, nor across firms within the same period. Along with investors, a growing number of bankers, brokers, and financial analysts were convinced that accountants could not be trusted, that they were free to

⁴⁹⁶ George O. May “The Choice Before Us” *Journal of Accountancy* (March 1950), 208. To this, Stans replied: “I believe, with Mr. May, that accounting should be utilitarian; but I think it can serve the great numbers of people, rather than the few” (“To the Editor” *Journal of Accountancy*, March 1950, 211).

apply all sorts of techniques in calculating figures most flattering to the managements they served. Those who hadn't already known as much learned sooner or later, for example, that earnings-per-share figures could be wildly inflated by a change in accounting policy.⁴⁹⁷ These sentiments drew from and fed into articles printed by the business press, culminating in such tropes as a “credibility gap” which began to feature prominently towards the end of the decade.⁴⁹⁸ This gap separated what investors expected of auditors and what auditors—like May—expected of themselves. On one side were the consumers of financial information; on the other, were the preparers of such information, usually a company controller or head accountant. Of the two, accounting leadership had long aligned itself with the latter, and in the 1960s, the actions of the APB proved no different.⁴⁹⁹

On several occasions, the Board acquiesced to preparers' increasingly vociferous and public complaints, starting with the APB's handling of the new Investment Tax Credit. This event lives on in accounting history infamy. Introduced as part of the

⁴⁹⁷ Samuel R. Hepworth, “Smoothing Periodic Income” *The Accounting Review*, Vol. 28, No. 1 (January 1953): 32-39; Committee on Accounting Concepts and Standards, “Accounting and Reporting Standards for Corporate Financial Statements,” American Accounting Association (1957); Carman G. Blough “Challenges to the Accounting Profession in the United States,” *Journal of Accountancy*, Vol. 108, No. 5 (December 1959): 37-42.

⁴⁹⁸ Lee J. Seidler & William Benjes, “The Credibility Gap in Interim Financial Statements,” *Financial Analysts Journal*, Vol. ??, No. ?? (September-October 1967): 109-115; Editorial, “What Are Earnings? The Growing Credibility Gap,” *Forbes* (May 15, 1967): 28-34, 39-42, & 44; Editorial, “How to Keep From Being Taken,” *Forbes* (May 15, 1967): 222-226 & 231. Apparently, US Senator William Fulbright found the notion of a “credibility gap” useful for other purposes. Speaking on the floor of the Senate on December 13, 1967, Fulbright used the phrase to account for the causes of student unrest across the nation's campuses. See Congressional Record, Proceedings and Debates of the 90th Congress, First Session (Wednesday, December 13, 1967), 36182.

⁴⁹⁹ See for example, Weldon Powell “Putting Uniformity in Financial Accounting into Perspective” *Law & Contemporary Problems*, Vol. 30 (Fall 1965): 674-689. In this essay, Powell leans heavily on the argument that the responsibility for financial statements rests with management and that a range of choices must be guaranteed to the them in order to accommodate differences in “management attitude” by which he meant: “One management may be imaginative, another dull; one bold, another cautious; one conservative, another optimistic”(681) and so on.

Revenue Act passed in October 1962, the provisions of the tax credit were intended to enhance US balance of payments by subsidizing domestic capital spending.⁵⁰⁰ According to John L. Carey, the AICPA's lifelong administrator and champion, "Since there was no precedent, no established practice for dealing with a tax credit of this nature, the situation offered an ideal opportunity to establish one practice, rather than alternatives which would obstruct efforts to compare the results of one company with those of another. But the Board members were unable to agree. The argument became an angry one, and personal tensions developed."⁵⁰¹ By the end, three of the nation's top accounting firms who were represented on the APB refused to comply with the Board's opinion and this, combined with pressure from the Financial Executives Institute and the SEC, led the Board to rescind its opinion. According to Carey, by buckling under pressure, the "prestige and authority of the Board had been badly damaged in its first effort to advance the cause of comparability. ... The accounting profession and the [AICPA] became targets for a barrage of public criticism unprecedented in the profession's history."⁵⁰² For the first time, accountancy was making news, and for the notoriously private world of the leading accounting firms, such negative publicity had come as a rude awakening. *Business*

⁵⁰⁰ The Investment Tax Credit appeared as an element of the Revenue Act of 1962, enacted in October of that year. The general justification for this subsidy was to effect more favorable balance of payments. The reasoning was that by stimulating investment in productive assets, American producers would be able to compete more effectively against their counterparts in Europe. In its details, the investment credit would reduce income tax equal to a percentage of the cost of depreciable assets acquired and placed in service after 1961. The credit could be accounted for in one of three ways: 1) as a subsidy, it could be accounted for as a contribution to capital stock; 2) alternatively, the credit could be used to reduce taxes otherwise applicable to the income of the year in which the credit arose; or 3) it could be accounted for as a reduction in the cost otherwise chargeable in a greater amount to future accounting periods. Wide disagreements arose between members of the APB on which of the three methods to espouse.

⁵⁰¹ Carey 1970, 98-99

⁵⁰² Carey (1970), 103-4

Week, Forbes, the New York Times, the Wall Street Journal, Barron's, Fortune, and other outlets all broadcast this and other failings of the APB to wide audiences.⁵⁰³

At the end of 1962, for example, *Barron's* drew its readers' attention to the "many acknowledged alternatives by which earnings can be legally exaggerated or minimized,"⁵⁰⁴ and cited the option to expense or defer research costs as one of several examples.⁵⁰⁵ The same article presented the views of Leonard Spacek, managing partner at Arthur Andersen & Co. who for years had been a vocal critic of his profession's inability to agree upon the most basic of concepts, such as when an expenditure qualified as a capital asset and when it did not. "In our firm," he recalled for *Barron's*, "we were faced with questions as to whether we were right in certifying financial statements based on such wide variations in accounting principles. And the more we inquired why these differences existed, the more we were convinced that we could not justify to the various segments of the public the differences in results that were reported."⁵⁰⁶ Decades later, in an interview conducted by his former colleagues, Spacek revealed just how frustrating it

⁵⁰³ In other cases that lost it credibility, the APB's efforts were successfully delayed or derailed by industry groups. In one example, the fire and casualty insurance industry embarked on what Carey describes as an "all-out pressure campaign" to prevent an audit guide requiring better information disclosure to investors. In another case, opposition to the APB's Opinion on Accounting for Income Taxes was led by the Financial Executives Institute, which organized a massive letter-writing campaign targeting the AICPA, and by Price Waterhouse, which released a pamphlet widely quoted by the press. As with the Investment Tax Credit fiasco, the APB caved in to pressure and issued a revised Opinion dropping the controversial issue of the tax credit altogether. Other proposals by the APB dealing with the use of convertible securities and warrants to dilute reported earnings per share figures, were also resisted by outside groups, including investment bankers. (Carey 1970 133-134) As a final example of the APB's perceived failures, from 1968 on through 1970, as the conglomerate merger movement becomes intense, pressure mounts on the APB to act decisively from the SEC, Congress, the Administration, the FTC, and financial analysts. Joining them was the Financial Executives Institute, which, "flooded the nation's press with criticisms of the APB's draft pronouncements"(Zeff "How the Accounting Profession Got Where It Is Today, Part I" 2003, 196).

⁵⁰⁴ Steven S. Andreder "Pitfalls for the Unwary" *Barron's*, Vol. 42, No. 5 (December 24, 1962): 8.

⁵⁰⁵ "...considerable latitude exists in handling research and development costs. ... The roster of discretionary accounting practices available to management is extensive"(Andreder, 8).

⁵⁰⁶ "Andreder, 3.

had been when, in the 1950s, Arthur Andersen & Co. clients demanded their books to be audited against the most favorable of principles available.

Arthur Andersen himself, a former professor, individualist, and a most forceful man, demanded that all employees learn to speak with one voice, a policy built on his “one-firm” concept and his admonition to “think straight—talk straight.” As the firm’s own institutional history explains, “The firm adopted early in its history the policy that all partners would follow a common approach in dealing with any particular accounting problem and in their utterances outside the firm. As far as the public is concerned, it was essential to have one official viewpoint—to speak with one voice—on important matters involving accounting principles.”⁵⁰⁷ Away from the public eye, partners held nothing back from each other whenever disagreements in opinion arose, true to the think straight-talk straight culture of the firm.

Heated arguments over principles were quite typical among the partners, including between Andersen and Spacek, but at the end of the day, rigorous deliberations could not resolve the fact that clients were free to choose among a wide range of accounting alternatives.⁵⁰⁸ The firm did famously drop several clients over disagreements in accounting practice, but there were limits to how many it could afford to lose. At other times, the firm would go so far as to embrace, rather than grudgingly accept, flexibility. One instance, documented by the aforementioned interview of former Arthur Andersen

⁵⁰⁷ Arthur Andersen & Co. *The First Sixty Years: 1913-1973* (Arthur Andersen & Co., 1974): 80

⁵⁰⁸ *The First Sixty Years: 1913-1973* (1974): 81. Spacek and Andersen “got along so well,” explained Spacek, “because he liked the idea of bucking the trend” (Interview with Spacek, 231). Starting in 1941, Spacek began inscribing the results of these argument into a working document called the Subject File. This grew to become a meticulously indexed collection of cases, principles, histories, and experiences, a copy of which was updated and maintained by each one of Arthur Andersen’s domestic and international offices.

employees, involves a company of limited means that was brought to Spacek's in the late 1950s.

Science Research Associates was a local, educational publishing business founded in 1939 by a twenty-eight year old University of Chicago graduate sociology student, Lyle M. Spencer. Two decades later, Spencer sought to take his company public and capitalize on growing interest from investors in companies like his own. At the advice of his council, the soon-to-be FCC Chairman Newton Minow, Spencer sought the help of Arthur Andersen & Co. and handed over his balance sheet. Minow described what happened next:

Leonard takes a pencil, like an artist, a magic wand. "Well," he said, "you're expensing out all your research and development. We're going to capitalize that. We're going to do a few other thing." I looked at it and, my God, there was a balance sheet with assets, with the net worth, it was a totally different business! I said, Leonard, "Can you give us a certified statement?" He said, "Yeah, we'll audit it, we'll go in and check everything out and we'll be sure that this is all valid. I'm just talking about the principles." So Arthur Andersen audits the business, they come back and it's a very respectable looking statement. ... A year or two went by, three years went by, IBM bought the business, Lyle had more IBM stock than the Watson family.⁵⁰⁹

By changing Spencer's method of accounting for R&D, Science Research Associates was made to appear far more capitalized than it had been. The resulting flood of interest from investors made possible the creation of the Spencer Foundation in 1962 and drew the attention of IBM, which acquired the company two years later. Listening to Minow recall the story, Spacek felt compelled to clarify his reasons for treating R&D as an asset. To Minow's account, he quickly added the following details:

⁵⁰⁹ Interview with Spacek (1989): 148

I must say that this research work that Lyle was doing, he had it well organized. He had a good group of people on it. He had good advisors and a good board, and this was an education, probably the forerunner of what we now call a cassette. ... His research was such that we could put it up by divisions of what each product was. Therefore, we had a right to capitalize it because we had a productive unit. It wasn't like doing original research where you didn't know whether you were going to get anything out of it. He knew exactly what he was going to get and his record of sales were just terrific. So, that's the way it came about. ... And to the extent, I don't know if—but the point that was there, and that's the reason I made it a point, his projects were all like building a building. After he got through, he had the product, here was the cost of building it—quite definitely an asset.⁵¹⁰

Science Research Associates, Spacek pointed out, was more in the business of producing educational devices than in testing out new theories or concepts. What's more, the vast wealth the sequence of events launched by the change in accounting method, was used to endow the Spencer Foundation with funds to support education research. The company was examined closely by Arthur Andersen auditors, and showed no signs of deception. What harm was there in capitalizing the development costs of products that were only a hair's breath away from being full-fledged assets? Have trust in my judgment and in the judgment of all Arthur Andersen men, Spacek seemed to be saying.

Still, the confidence he had in his own auditors did not extend to those at other firms. Despite the double-standard, the desire for limiting alternatives was real and became a cause célèbre of Arthur Andersen & Co. Internal debates over accounting principles with the other partners began to feel unproductive so long as the interpretation of “generally accepted accounting principles” remained in the hands of corporate managements. Having reached an impasse, Spacek began speaking publicly against the

⁵¹⁰ Interview with Spacek (1989): 150-152

AICPA, an organization he viewed as a high society, elite club of New Yorkers who were perfectly content to drag their feet on reforming accountancy in the public interest.

In a public address that would live in infamy, Spacek in 1957 proposed an idea for a federal accounting court which would help resolve the conceptual conflicts and disjointed priorities of professional practice. The AICPA leadership was stunned. One of their own had gone rogue, and by doing so publicly, he risked shattering the profession's fourth wall. Accountants, they believed, had to be seen as disinterested, impartial, unbiased in the public sphere. This was how the elites of the profession interpreted the Congressional mandate, introduced in the Securities Acts of the 1930s, that certified accountants act independently and in the public interest. Clearly, they believed, Spacek had to be reined in before he caused further harm to the public image of the profession as a unified community of staid, unbiased, and discrete gentlemen.

“They called me down for meetings,” Spacek recalled in the interview years later. “[Y]ou’re talking all the time about accounting as though all accountants should express the same opinion,” he remembers an AICPA representative explaining to him. “[T]hat isn’t the way the accounting profession is worked, is organized. The accounting profession is made up of people all having their own views and they’re all professional people and they all should be able to express themselves individually.” Spacek’s takeaway from this meeting hits at the core of the uniformity-vs-flexibility debate: “if that was that,” he concluded, “then everything everybody expresses automatically becomes generally accepted accounting principles.”⁵¹¹ Or, as another Arthur Andersen partner put it a few years later, this “philosophy...is based on a self-perpetuating circle of logic that

⁵¹¹ Interview with Spacek, 238

accounting principles are accepted because they are sound and they are sound because they are accepted. This results in ‘the tail wagging the dog,’ since many accounting practices are justified merely on the basis of acceptance (custom), which is given more weight than the fairness of the financial presentations that result from their application.”⁵¹²

Unapologetic and showing every sign of his intention to continue his public campaign, Spacek then became the target of an investigation by an ad hoc committee appointed by the AICPA for that purpose. When the committee completed its report and the issue came before the AICPA membership, the majority of members refused to vote Spacek out.⁵¹³ If the national organization would not expel Spacek, perhaps the Illinois Society of Certified Public Accountants would, but there, too, the AICPA lost its battle to silence the man whom the business media would come to call “the maverick” of accountancy.

Spacek continued his crusade at the AICPA’s Seventy-Third Annual meeting in Philadelphia, where he and two other leading accounting figures presented their views in a debate on uniformity vs. flexibility. Held in September 1960, the event was covered in dramatic fashion by *Fortune* magazine,⁵¹⁴ which concluded that, “What now bothers many members of the profession, and what occasioned the debate, is the fact that the limits of

⁵¹² George R. Catlett “Factors That Influence Accounting Principles” *Journal of Accountancy*, Vol. 110, No. 4 (October 1960): 48.

⁵¹³ Spacek recalls that academics voting in his favor was a large reason why the AICPA was unable to expel him from the association. He did not disclose the names of these academics. All we know is that they must have been credentialed as CPAs, since this was a requirement of membership to the AICPA.

⁵¹⁴ “One afternoon during the convention, about a thousand of the most distinguished C.P.A.’s in the U.S. gathered in the Rose Garden Room of the Bellevue-Stratford Hotel, to hear what was billed as a debate on the principles of their profession. An uninitiated visitor wandering into the room might have thought at first that they were arguing over some minor, tedious technicalities, and so might have been puzzled by the passions that were manifestly stirred up” T.A. Wise “The Auditors Have Arrived, Part II” *Fortune* (December 1960): 144.

permissibility stretch pretty far these days.”⁵¹⁵ Representing the pro-diversity (or “flexibility”) camp was Maurice E. Peloubet who argued that the “impossibility of presenting the accounts of different companies in the same industry on a completely comparable basis arises from the fact that physical and financial conditions and management policies are different. The accountant, public or private, cannot influence management decisions in any other way or to any greater extent than is warranted by his role as adviser.”⁵¹⁶ Naturally, Spacek represented the minority view in asking,

If uniformity in generally accepted accounting principles is basically bad, is it unreasonable to ask why not one of our professional leaders who holds this view has come forth (1) with a specific case where uniformity would result in damage to the reported financial condition of a business entity and to the economic rights of the various segments of the business community interested therein, or (2) with a case where the alternative principle now followed would produce rightful advantages for all of the various segments involved? ... They arguments [made against greater uniformity] were only that flexibility was good, per se, and that the elimination of flexibility was bad, per se.⁵¹⁷

Spacek and a third speaker, Michigan State University’s Charles J. Gaa, demanded that accountants restrict managements to a narrower range of accounting methods. By contrast, Peloubet represented the dominant position of the accounting elite by insisting that accountants ought not dictate orders to their employers and clients. Blough, a fourth speaker on this topic, took a middle road: “While I strongly subscribe to the idea that there should be a much greater narrowing of these areas of difference than has been possible to date, it seems to me it is, of necessity, a matter of evolution. ... That involves a great deal of education, debate, and interchange of ideas.”⁵¹⁸ In response, prominent

⁵¹⁵ Wise (December 1960), 144.

⁵¹⁶ Maurice E. Peloubet “Is Further Uniformity Desirable or Possible?” *Journal of Accountancy* (April 1961): 39

⁵¹⁷ Leonard Spacek “Are Accounting Principles Generally Accepted?” *Journal of Accountancy* (April 1961): 43.

⁵¹⁸ Carmen G. Blough “Principles and Procedures” *Journal of Accountancy* (April 1961): 52

academics like Robert Anthony argued that “The nature of this evolutionary process is left vague.”⁵¹⁹ Alluding to Blough and those who adopted his hands-off approach to standard-setting, Anthony explained that,

Sometimes these say that the process is analogous to the common law. But when it is pointed out that the foundation of the common law is precedent, that a precedent is binding, and that such a process would therefore sharply reduce management’s freedom of choice, they say, ‘No, that isn’t what we mean at all.’ What they really want is *laissez faire* which is the antithesis of the litigation, adjudication, and enforced judgements and rulings that are the essence of the common law process. *Laissez faire* is not the same thing as flexibility.⁵²⁰

What those in the flexibility camp “really want,” Anthony suggested, was the freedom to abdicate responsibility for their decisions.

Appearing in the *Harvard Business Review*, Anthony’s scathing essay was widely read and further embarrassed the elites of the profession. In the very next issue of the *Harvard Business Review*, AICPA executive vice president Leonard M. Savoie sought to put an end to the foolish views espoused by Anthony, Spacek, Gaa, and a growing number of others aggressively pushing for reform. “There are those who imply that we are in a crisis situation,” he wrote dismissively, “and that immediate action must be taken or dire (but often unspecified) results will occur.”⁵²¹ Savoie would have none of this. “In traveling the road toward comparability in accounting, there is greater danger from proceeding too fast than too slow. If we go too fast, either because we are enamored with the ideal or obsessed with fear of regulation, the result is likely to be the same—ill-considered changes

⁵¹⁹ Robert N. Anthony, “Showdown on Accounting Principles,” *Harvard Business Review* (May/June 1963): 101.

⁵²⁰ Anthony (May/June 1963): 101.

⁵²¹ Leonard M. Savoie “Accounting Improvement: How Fast, How Far?” *Harvard Business Review*, Vol. 41, No. 4 (July/August 1963): 145.

that will cause immediate, widespread, and unfair distortion in financial results, with little or no long-term gain in real usefulness and comparability.”⁵²²

Greater uniformity, in itself, was a threat to clients’ financial reporting practices, and accountants, Savoie suggested, would do well to identify more closely with the managers whose books they audited.⁵²³ Those taking the opposite view were miring the profession in “a false crisis precipitated by false prophets.”⁵²⁴ Returning the accusations lobbed against AICPA leadership, Savoie suggested these dissidents were negligent in their duties and relishing the opportunity to unburden themselves from the responsibilities required of professional men. “Can it be,” he asked, “that they seek the freedom from responsibility that results from unquestioned compliance with regulation?”⁵²⁵ Calls for a standard-setting body independent of the AICPA smacked of the kind of authoritarian control that was so inimical to the American free enterprise system, and the example of R&D, he believed, made this much clear:

Would it be better if there were someone who could legislate that all research must be expensed immediately, or that all research must be deferred over a period of, say, three years? Would this improve financial reporting? It might well provide the appearance of uniformity and comparability, but would it better report each specific company’s results? ... It would probably assure that the presentation in financial accounts of management objectives would be wrong more often than right.⁵²⁶

⁵²² Savoie (July/August 1963), 145.

⁵²³ This is suggested by the simile in the following statement: “accounting, like management, is a complex art which requires carefully considered exercise of judgment...”(Savoie 1963,145).

⁵²⁴ Savoie (July/August 1963), 146. Continuing on the same page, Savoie declared, “it is now apparent that the real intention of these critics is to remove from business management the right to participate in development of accounting principles and place this important function in the hands of an external regulatory body, either within the accounting profession, or, failing that, within the government.”

⁵²⁵ Savoie (July/August 1963), 146

⁵²⁶ Spacek (1963), 148

George R. Catlett was unimpressed. Catlett was an accounting graduate of the University of Illinois, a partner at Arthur Andersen since 1952, and chairman of his firm's committee on accounting principles and auditing procedures. In a few short years, he would succeed Spacek as the firm's representative on the APB, and would serve as president of the Illinois Society of Certified Public Accountants. As Spacek's right hand and collaborative speech writer,⁵²⁷ Catlett responded to Savoie's essay with a letter to the editor that represented the Arthur Andersen point of view.

“Savoie takes a somewhat curious and rather superficial approach to the problem of responsibility for improvement in accounting principles,” wrote Catlett. Indeed, Savoie seemed to suggest that “assumption by any group of the responsibility for achieving improvement represents some undesirable form of ‘regulation’ [and that the] responsibility for improvement belongs to everyone in general and no one in particular.”⁵²⁸ Here, Catlett was getting at the heart of the issue, in clearer terms than even Robert Anthony.⁵²⁹ Those seeking to preserve the status quo insisted that “evolution” would lead the way to greater uniformity, but that accountants must have no part in the evolution of their own principles. Again, “evolution” was held responsible for improvements to accounting practice, not the practitioners themselves. The following year, Catlett reiterated this point: “reliance on evolution is frequently used as an excuse

⁵²⁷ Stephen A. Zeff “George Roudebush Catlett, 1917-2013” (June 5, 2014) American Accounting Association <<http://commons.aaahq.org/posts/d96e42d8e6>>

⁵²⁸ George R. Catlett, letter to editor, *Harvard Business Review* (November-December 1963): 39 & 200

⁵²⁹ In fact, Catlett had offered his own definition of evolution in a few years before, indicating his disapproval rested not with a reference to evolution as a process but strictly with the mystical, undefined way it was put to use by his interlocutors. See his “Relation of Acceptance to Accounting Principles” *Journal of Accountancy*, Vol. 109, No. 3 (March 1960), 36.

for not taking any significant action on a professional basis. ... To assume that unsound practices will be eliminated automatically is to ignore history.”⁵³⁰

Too often, the diversity of accounting alternatives made it possible for auditors’ clients (i.e., corporate managers, controllers, financial executives) to present company finances in the most flattering light. The “crux of the problem,” exclaimed Thomas G. Higgins, partner at Arthur Young & Co., is that “when we independent accountants report that financial statements are presented in conformity with ‘generally accepted accounting principles,’ we cannot be sure what we mean, because the expression ‘generally accepted accounting principles’ has never been satisfactorily defined. For the same reason, those who issue the financial statements on which we report, and those who use them, do not know what we mean either.”⁵³¹

This debate, however, led nowhere. The deadlock between the two opposing camps continued and was sustained by an unwillingness of accounting leadership to include investors in their definition of the public.⁵³² To them, the “public” in “independent certified public accountant” referred only to the clients they served, the managers that hired them. At the same time, the “independence” they defended was increasingly defined as independence from responsibility to third parties, including investors, rather than independence from the executives whose books they audited. Despite the heated exchanges of the early- to mid-1960s, the uniformity-flexibility debate

⁵³⁰ George R. Catlett “Controversy Over Uniformity of Accounting Principles” *Journal of Accountancy*, Vol. 118, No. 6 (December 1964): 39.

⁵³¹ Thomas G. Higgins “The Accounting Principles Board and Uniformity in Financial Reporting,” *Proceedings of the Twenty-Fifth Annual Institute on Accounting, May 17, 1963* (Ohio State University, 1963): 67-68.

⁵³² “A void in leadership exists now,” wrote Catlett. “The procrastination and indecision which have been characteristic of this controversy over uniformity must be eliminated”(December 1964, 43).

remained unsettled and the APB would continue for the next several years in largely the same way as it had before.⁵³³

5.4. A Slow Burn

The APB's failed handling of the issues raised by the Fiero letter was thus only one instance of a larger trend, as accounting leadership waived away broader concerns over how the tools of their trade were being put to use.⁵³⁴ With respect to R&D alternatives, their only effort to address the issue occurred in early 1964 when the interim director of accounting research, Paul Grady, assigned a new study on the subject to two partners with the firm Haskins & Sells.

One year later, Thomas D. Flynn, partner at Arthur Young & Co. and president of the AICPA, referenced this study in a speech delivered before a January 1965 meeting of the New York State Society of Security Analysts. In an effort to quell critics in the financial analyst community, Flynn acknowledged the following: "As you are well aware, there has been criticism of corporate financial statements in recent years on the ground that they embody such a diversity of accounting principles make it difficult, if not impossible, to compare one company to another." Nevertheless, said Flynn, "Much of

⁵³³ Robert Bloom has written that, "Looking back, there appears to have been a code of silence among most of the accounting firms to accept the status quo, perhaps because any changes would prove far less appealing and considerably more costly to implement than existing arrangements" (*Accounting, Business & Financial History*, November 2007, 433-434).

⁵³⁴ These are two separate issues: urging for a reduction of accounting alternatives did not necessarily imply an endorsement of the current write-off treatment of R&D, as the case of Harold Q. Langenderfer makes clear. While a passionate advocate of greater uniformity, Langenderfer described R&D and depreciation as two examples in which "diversity is justified by differing circumstances." He wrote: "should the lack of conclusive evidence as to the nature of the benefits to be received be grounds for a free choice by management between current writeoff and capitalization? When management made the decision to expend funds for [R&D], it presumably made a judgment that future benefits probably would accrue to the firm. Until there is evidence that management is wrong, it seems appropriate to assume that [R&D] costs will provide benefits to the firm beyond the current period" (1967, 38).

this criticism is more vociferous than justified.”⁵³⁵ Insisting that criticisms of the APB were misplaced, Flynn used the examples of pension costs, business combinations, foreign operations, the allocation of federal income taxes, and research and development as areas in which the APB was busily hammering out new opinions. This was a rather obvious overstatement of the APB’s success. As far as the project on R&D was concerned, the two partners from Haskins & Sells had barely begun to explore this complex issue, and it would take nearly a decade before a final report of the study was presented to APB members and published.

It’s not clear what led Grady in early 1964 to place R&D on the agenda for the AICPA’s research division. As director of that division, the choice was in his hands, yet one struggles to find any indication in his written work that he took a particular interest in R&D. Grady is most prominently associated with his voluminous *Inventory of Generally Accepted Accounting Principles for Business Enterprises*, published by the AICPA in March 1965 as Accounting Research Study No. 7. With *Inventory*, Grady catalogued practices then in use by accountants and shrugged off any controversy around conflicting alternatives—including in the handling of R&D—as just a basic fact of life.⁵³⁶ All that his writing makes clear is that Grady, along with Leonard Savoie, Weldon Powell, Philip Defliese, and other

⁵³⁵ Thomas D. Flynn “Accounting Principles and Financial Analysts” *Financial Analysts Journal* (March/April 1965): 17.

⁵³⁶ More than half of *Inventory’s* 469 pages were mere reproductions of pronouncements on accounting terminology and principles. That *Inventory* only reinforced the status quo is a critique been made repeatedly in historical studies of the evolution of accounting principles, but it was Leonard Spacek who, in his capacity as APB member, appended *Inventory* with the following contemporaneous dissent: “This study, if it was not to be limited to a description of what exists today, should have given (1) more emphasis to the need for the elimination of the existing double standards created by alternative accounting practices and (2) less emphasis to the need for continuing the present undesirable state of affairs. . . . if this study is used as authoritative support for the resistance of progress and as an excuse for perpetuating present accounting practices, some of which are not “sound” even though they are ‘generally accepted,’ then it will serve a detrimental purpose”(Grady 1965, 445).

Big Eight partners, was very much a member of the gradualist school that favored flexibility and prioritized managements' needs over those of investors.⁵³⁷ The impetus may have come from public testimony made by representatives of the SEC speaking before Congress in early 1964, but without further knowledge of the decision-making internal to AICPA at the time, this conjecture is nearly impossible to prove.⁵³⁸

Regardless of the reasons that finally drove the AICPA to launch an inquiry into R&D, years would roll by with the study left unfinished. From early 1964 to 1973,

⁵³⁷ Grady had become partner within six months of joining Price Waterhouse, a bastion of the flexibility school, after twenty years of working for Arthur Andersen that came to an abrupt end when he was passed over as successor to Andersen. Grady had been described as the heir apparent to Arthur Andersen, which makes his departure from the firm especially conspicuous. While it's not clear what happened exactly to cause the falling out between Grady and Andersen, an interview with Spacek decades later (*supra*) suggests that he may have been partly responsible for the final decision. Not incidentally, that decision involved naming Spacek as successor. Still, in the same interview, Spacek gave no indication that there remained bad blood between the himself and Grady.

⁵³⁸ In February 1964, West Virginia Congressman Harley O'Staggers of West Virginia asked the SEC Chairman William L. Cary for a statement detailing those areas of accounting where alternative practices had been shown to produce materially different results under generally accepted accounting principles. The SEC's chief accountant, Andrew Barr (1956-1972), had accompanied Cary to Congress that day, and the task fell on him to submit the statement requested by O'Staggers during the hearing. Barr's memorandum was later entered into the Congressional record, and in it, R&D was identified as one of eight key areas in which alternatives had made it acceptable for firms to select among dramatically-different figures when crafting their financial statements. Because the timing of the Congressional hearings and the assignment of the study seem to coincide, this suggests that the AICPA was alert to O'Staggers inquiry and sought to preempt any further questioning about accounting alternatives by immediately organizing the R&D study. However, this hypothetical sequence of events is undermined by a small detail in a table included in a manuscript by accounting historian Stephen Zeff, which suggests that Grady assigned the R&D study in January 1964—a month before the Congressional testimony made by SEC representatives. There are no citations about this specific timeline in Zeff's manuscript, *Forging Accounting Principles in 5 Countries* (1972, 223), and Zeff himself has never made R&D an object of his own study. Further complicating the issue is a statement made decades later by one of the two Haskins & Sells partners assigned to the project, Maurice S. Newman. In 1988, Newman wrote a short retrospective piece in which he claims the R&D project was originally begun by the CAP, predecessor to the APB. Apparently, that project never led anywhere, received minimal exposure, and was ultimately abandoned before being picked up by the AICPA. With no citations or details to validate Newman's claim, it's hard to accept it at face value, especially since the CAP was shut down in 1958 and the R&D project was not assigned until 1964. Committee on Interstate and Foreign Commerce during hearings on the proposed Securities Acts Amendments of 1964. See "Part 2: Investor Protection" *Hearing before a Subcommittee of the Committee on Interstate and Foreign Commerce, House of Representatives, 88th Cong., 1st and 2nd Sess.* Dec. 3-5, 1963; Jan. 21-24, 1964; and Feb. 4, 5, 18, & 19, 1964. Both before and after these hearings, Barr delivered public speeches in which he made passing reference to the problem of R&D accounting alternatives. See, for example, "Financial Statements—How Reliable?" address before NYU business school alumni on April 16, 1963, and "The SEC and the Accounting Profession—Yesterday and Today" address before the Banking, Corporation & Business Law Section of the NY State Bar Association on Jan. 26, 1967.

progress on the R&D study was a slow burn. Reasons that might explain this prolonged effort are hard to find, although some can be inferred. Catlett's comments from 1969 suggest one reason for the delay: "With no agreement as to the objectives and concepts,..the effectiveness of the research has been considerably impaired" and had "flown off in all directions."⁵³⁹ At the time, Catlett was speaking of ten studies completed to date as well as those still "in process," including the R&D study. If such projects were to successfully narrow areas of difference and advance the profession, then it was time for the profession to quit "putting out fires" as they arose and define its core concepts. Catlett listed these in question form:

What is the general objective of financial statements and what characteristics would make the most useful and result in the fairest presentation of the facts? What is the purpose of the balance sheet? What kinds of assets should be recognized and, in general, how should they be valued? What types of liabilities should be shown? What is the purpose of the income statement? What represents income realization? When do costs become expenses to be charged off?⁵⁴⁰

Without answers to these questions, the two Haskins & Sells partners assigned to the R&D project, Oscar S. Gellein and Maurice S. Newman, were facing the same impossible task of starting from the ground up. When, in early 1965, Flynn spoke before New York financial analysts, he did what he could to convince his audience that the task was more manageable.

Struggling to downplay the APB's failures, Flynn offered a most awkward defense of deferring R&D cost—even as analysts' and investors' experiences were already pointing up the limits of this approach. "Generally speaking," he began, "substantially all

⁵³⁹ George R. Catlett "Better Objectives Needed to Improve Accounting Principles" *Journal of Accountancy*, Vol. 128, No. 4 (October 1969), 53.

⁵⁴⁰ Catlett (October 1969), 53.

listed companies charge off general research and development costs as incurred because of the difficulty of finding rational bases for allocating such costs to future periods.”⁵⁴¹

If one company in an industry charges off high expenditures to develop, say, color television, and another company in the same industry undertakes no general research at all, can we, without qualitative evaluation, compare their earnings per share? Modern accounting attempts to match costs with revenues. This necessarily involves assumptions that such costs will in fact produce revenues, and when they will be received. Such assumptions as to the future will rarely prove precise in the light of subsequent events. But this approach yields far more useful earnings information over a period of years than any other yet discovered.⁵⁴²

For Flynn, deferring R&D costs may rest on shaky assumptions, but it made earnings information more useful—although how, he did not say. We can safely assume, however, that for Flynn, the usefulness of matching or deferral was rooted in its ability to smooth income across periods. Delmer P. Hylton, professor of accounting at Wake Forest College, recognized this full well. Later that same year, Hylton argued that “Procedures that artificially level (manipulate?) income of course may make it easier to predict future income statements. But how useful is the income that these ‘forced’ statements indicate?”⁵⁴³ In contrast to matching and deferral, “an intelligent attempt to portray those economic events that occurred in a given accounting period” would be “devoid of predictions of the amount of future revenues.”⁵⁴⁴ “Perhaps,” Hylton concluded, “an answer to this question (from the accounting profession) is in order.”⁵⁴⁵

Based on the published work of accounting academics at this time, it seems that Hylton was one of only a few exceptional cases. Many, as we have seen, embraced the

⁵⁴¹ Flynn (March/April 1965), 21

⁵⁴² Flynn (March/April 1965), 21

⁵⁴³ Delmer P. Hylton *The Accounting Review* (October 1965), 828

⁵⁴⁴ Hylton (October 1965): 828

⁵⁴⁵ Hylton (October 1965): 828

matching principle from start to finish, while others adopted a more ambivalent stance, especially in regard to its application for R&D. Warren Kantor exemplified this latter, more conflicted view in a 1966 essay reviewing the policies in use by the eight largest accounting firms: “The majority of the policies of these eight accounting firms will allow companies to expense or capitalize research and development costs under the same circumstances in any given firm.”⁵⁴⁶ Though he, too, felt torn between his faith in the matching principle and the troubles it caused R&D accounting, Kantor nevertheless spoke for many when he made the plain observations that,

The accounting profession has not issued an official bulletin concerning the handling of research and development costs in financial statements.⁵⁴⁷

There is no uniformity in the accounting profession for the handling by industrial companies of research and development costs. ... The American Institute of Certified Public Accountants has expressed no specific opinion on this problem of research and development costs; when to expense them or when to capitalize them. The American Association of Accountants has not given an opinion on the subject.⁵⁴⁸

The following year, the same observation was made by Orace Johnson, another accounting academic like Kantor who was sympathetic to matching: “The accounting profession,” he wrote bluntly, “has not reached an agreement on the handling of R & D.”⁵⁴⁹ For Johnson, the challenge facing the profession was to identify guidelines by which accountants could “set up an accurate amortization schedule for each R & D project.”⁵⁵⁰ Johnson explained his position as follows: “Although there is uncertainty

⁵⁴⁶ Warren Kantor “Research and Development Costs: Should They Be Expensed or Capitalized?” *Economics & Business Bulletin of the School of Biz Admin*, Vol. 18, No. 3 (1966): 16

⁵⁴⁷ Kantor (1966): 8

⁵⁴⁸ Kantor (1966): 14

⁵⁴⁹ Orace Johnson, “A Consequential Approach to Accounting for R & D,” *Journal of Accounting Research*, Vol. 5, No. 2 (Autumn 1967): 164.

⁵⁵⁰ Johnson (Autumn 1967): 165.

about the individual project results, the average return may be known with greater certainty. The uncertainty problem could be handled by applying to all R & D spending the average expected life pattern.”⁵⁵¹ This justification for deferral relied on the use of long-run averages to smooth over differences between the theory of deferring R&D and the actual effects of applying this theory in practice.

This sort of reasoning gnawed at another academic, Robert R. Sterling who, in a July 1968 essay in the *Accounting Review*, decried the “widespread use” of what he called “the it-doesn’t-make-a-difference argument.”⁵⁵²

This argument is applied in many areas but most often to the difficult capital versus expense decisions. For example, in deciding how to treat research and development expenditures, it is often said that ‘it-doesn’t-make-a-difference because the amount of amortization will tend to equal the amount of the expenditure after a suitable time lapse.’ ... For this to be true, it requires a *constant* expenditure each year, and that each year’s expenditure yield a *constant* benefit. In turn, this requires *constant* prices of the factors that go into research and development. If the prices were to go up and the total expenditure remains constant, then *ceteris paribus* the quantity of benefits would decline. Thus, the it-doesn’t-make-any-difference argument is quite true if everything is constant.⁵⁵³

Writing towards the end of the decade, Sterling’s was not a voice heard in the dark, for by that time, critique of the profession had starting reaching fever pitch.

Many members of the old guard who, for many years, had succeeded in squelching the profession’s ability to reform its own standards, were now retired or passed away. Accountants were increasingly challenging the old guard’s caricature of greater uniformity as “a procrustean bed” that would be used to erase real differences between circumstances, as an authoritarian “cookbook” of step-by-step rules for the practitioner,

⁵⁵¹ Johnson (Autumn 1967): 165.

⁵⁵² Robert R. Sterling “The Going Concern: An Examination” *Accounting Review* (July 1968): 490

⁵⁵³ Sterling (July 1968): 490

and as a “straightjacket” on professional judgment. Those who identified with the “flexibility” camp treasured these arguments for their sensationalism, but in time, their efficacy wore off as more practitioners and academics joined ranks with Spacek, Catlett, and the “uniformity” camp.⁵⁵⁴ As Charles E. Johnson of the University of Oregon expressed it in 1966, it was time to challenge “the inability or unwillingness of the profession to narrow the definition of generally accepted accounting principles.”⁵⁵⁵

A few stragglers, such as Leonard Savoie, continued to maintain that disclosure—rather than common understanding of core concepts such as asset or liability—would prevent further abuses of accounting methods. The misuse of R&D deferrals, so widely reported in the latter 1960s, would have been prevented were a standard in place requiring greater disclosure. “We should have had the accounting aspects of R&D settled three or four years ago,” he complained to a reporter from *Forbes* magazine. Apparently, however, Savoie lacked appreciation of the sheer scale of the problem, for it would still take another three years before Gellein and Newman of Haskins & Sells would finish analyzing the diversity of business practices and arrive at a set of recommends to guide R&D accounting.

As a first step in this process, back in 1965, Gellein and Newman enlisted the help of Elmo Roper & Associates to design, distribute, and aggregate the responses to an R&D questionnaire. By September 1965, work on the Elmo Roper survey had been completed, and the results would appear in an appendix to Gellein and Newman’s final report. This final report, however, took nearly a decade to complete, and in the meantime, the

⁵⁵⁴ A few academics taking a stand against the old guard were discussed in Chapter 3.

⁵⁵⁵ Charles E. Johnson “Management’s Role in External Accounting Measurements” in Robert K. Jaedicke, Yuji Ijiri, and Oswald Nielson (eds), *Research in Accounting Measurement* (AAA, 1966): 97

surprise write-offs described in Fiero's letter continued unabated. The military procurement market had been in full swing ever since the launching of Soviet satellites in the late 1950s, and R&D was rapidly becoming a key element of stock market analysis, speculation, and trading. Renewed R&D spending by the federal government following US entry into the Vietnam War only made the need to address R&D's role in the Cold War economy all the more urgent. But to do so, the very meanings of "capital" or "asset" had to be re-examined and updated. That this pressing need could have slipped past the profession's elite standard-setters for an entire decade is, from the perspective of hindsight, almost too incredible to believe. And yet it did.

Chapter 6

Capital Gains and a “Permissiveness that is Shocking”

6.1. A Scientific-Technological Elite

In his televised farewell address to the nation, the outgoing US President Dwight D. Eisenhower warned his countrymen of “grave implications” attending a “permanent armaments industry of vast proportions”—a feature of American life that Eisenhower himself helped build up.⁵⁵⁶ It was then January 1961, and Eisenhower had grown impatient with what seemed to him increasingly gratuitous wheeling and dealing between procurement officers of the armed services, private sector defense contractors, and members of Congress.⁵⁵⁷ Perceived Soviet aggression towards the American way had made necessary what Eisenhower famously referred to as the military-industrial complex

⁵⁵⁶ The single best source from which to learn the ideas, concerns, experiences, and ambitions behind the “military-industrial complex” concept, as well as the interpretations and applications of this concept in the years since Eisenhower’s farewell address, is James Ledbetter’s *Unwarranted Influence: Dwight D. Eisenhower and the Military-Industrial Complex* (New Haven: Yale University Press, 2011). Ledbetter details how Eisenhower’s thinking with respect to the military domination of the US economy, evolved over the course of the 1950s to the point that, “By 1959, Eisenhower had begun to see military contractors as self-interested, malign actors in the budget process. In a June meeting with legislative leaders about defense appropriations, he questioned an additional \$85 million that had been put into the ATLAS program, an early intercontinental ballistic missile. ... According to the meeting notes, ‘The President protested the political pressures that the munitions industry brings to bear on the Congress, and especially the resort to full-page advertisements such as that by Boeing...’”(2011, 96-7). Also, see: Dolores E. Janiewski “Eisenhower’s Paradoxical Relationship with the ‘Military-Industrial Complex,’” *Presidential Studies Quarterly*, Vol. 41, No. 4 (December 2011): 667-692.

⁵⁵⁷ Firms that could afford it had evolved to demonstrate skilled lobbyists, advertisers, and promotional efforts aimed at the procurement arms of the military. Such advertising appeared in trade publications, such as *Aviation Week* or *Air Force* magazine, which hocked not only aerospace technologies but the raw materials these technologies were made of. Among other reasons, it was the gratuitousness of these advertisements combined with contractors’ lobbying individual Congressmen, that led Eisenhower to warn of the overreaching arms of the military-industrial complex. For more on this, see: Karen S. Miller, *The Voice of Business: Hill & Knowlton and Postwar Public Relations* (Chapel Hill, NC & London: University of North Carolina Press, 1999); as well as ch.4 of Ledbetter’s *Unwarranted Influence* (2011).

(MIC). About a decade later, this concept would become popularized by a wave of literature protesting the lives lost and profits made by US involvement in Vietnam.

For the president, though, it was not war profiteering alone but that combined with certain features of post-war R&D which he held “largely responsible for the sweeping changes in our industrial-military posture.” Defense contractors quickly became skilled at lobbying members of Congress to authorize special budget items, while members of Congress, in turn, were engaging more boldly in pork-barrel politics. Eisenhower feared that, if this rising trend was not brought under some restraint, “public policy could itself become the captive of a scientific-technological elite.” Rather than heed this warning, neither the incoming administration of John F. Kennedy nor those on Wall Street paid the aging general much mind. The former vice president of Convair spoke for many when he said, “I believe President Eisenhower to be an honorable, well-intentioned and amiable man.... I also believe him to be mortal, fallible and culpable, [that] he has listened to the drums of a bygone day—out of tempo with the space age.”⁵⁵⁸

For his part, Kennedy had run his election campaign on an interventionist slogan of “getting the country moving again” while, at the same time, sounding the alarm about a science-based “missile gap” between the US and the Soviet Union.⁵⁵⁹ This gap would

⁵⁵⁸ “Lamphier Details Views on Defense Needs” *Aviation Week* (April 11, 1960): 127.

⁵⁵⁹ The ominous threat of a “missile gap” was a recycled campaign strategy, one that Kennedy had earlier used to help win re-election in Congress. Once he made it to the White House, Kennedy’s defense secretary Robert McNamara conducted an investigation that demonstrated there never was any such missile gap—a fact that the Eisenhower administration had itself urged upon deaf ears. The myth about a Soviet lead over American military development, as well as the proposal to bridge that gap by a massive military spending program, was enflamed by H. Rowan Gaither’s RAND Corp report, “Deterrence & Survival in the Nuclear Age.” According to Ledbetter, “there could be no hiding the fact that the billions in increased military spending called for by the panel would benefit many of the very people making the recommendations. Two of the report’s principal directors were Robert C. Sprague, who headed his own business of military electronics, and William C. Foster of the Olin-Mathieson Chemical Company, a producer of gunpowder and ammunition”(2011, 91).

have to be closed by heightened defense spending, and to this proposal, though not to Kennedy's social reform programs, Congress was more than happy to oblige.⁵⁶⁰ By that time, a fresh new cohort of investors, brokers, and fund managers had arrived on Wall Street, bringing with them a taste for stocks that promised to ride the wave of Kennedy's "space age" militarism. As stock exchange historian Robert Sobel has shown, by the late 1950s this new kind of investor had begun displacing the veterans of Wall Street for whom the memory of 1929 and its aftermath had lost none of its potency.

In the early 1930s, investors had been left in a daze as they bore witness to a nationwide economic collapse that unfolded in the wake of October 24, 1929, a day of high-volume, panicked selling that became bitterly known as "Black Thursday." What had gone wrong to bring the "Roaring Twenties" to such an abrupt end, and what form would a revived investment environment have to take? Many theories were posited, including that of "technological unemployment," a term used against corporations whose R&D investments generated the labor-saving operating equipment that put many out of work. Others, such as Graham and Dodd's theory of "intrinsic value," sought to distinguish investment more clearly from speculation. In their seminal text, *Security Analysis* (1934), the two Columbia University business school professors, Benjamin Graham and

⁵⁶⁰ Editorial, "Kennedy Gets His Package—Almost." *Businessweek*, August 26, 1961

David Dodd, argued that sound investment practice required detailed study of a firm's "fundamentals," rather than extrapolations made on the basis of reported earnings.^{561 562}

Recall that not until the Security Acts of the early 1930s were firms required by law to disclose certain accounting data. Until that point, most managements operated under the principle of *caveat emptor*, sharing very little or no information about the inner workings of their firms. Even when accounting data were reported, professional investors knew they were not to be taken at face value. This was crucial to Graham and Dodd's theory, as it meant that insider knowledge (or some approximation of it) was far more reliable for valuing a firm than were reported figures. In short: the firm itself, rather than its stock price, was the desired object of analysis. Later known as "fundamental analysis," this practice informed the investment decisions of the older generation that would lead the bull market of 1949. These investors retained a conservative preference for income stocks, those of well-established companies which paid regular dividends and offered their owners greater long-term financial security. Unimpressed by the potential capital gains to be made when stock prices were on the rise, they looked askance at stocks whose prices

⁵⁶¹ This meant investigating the actual resources that a given firm put to use in its daily operations, such as the assets it had on hand, how well those assets were maintained and put to use, the regularity of dividends paid out, the load of debt carried on the books, and the average of past earnings over time. For Graham and Dodd, the net assets and earning power of a firm—i.e., its "intrinsic value"—were represented in the share price more often than not. This meant that the task of the security analyst was to identify those instances when prices were materially lower than intrinsic value, for such anomalies indicated an investment opportunity. As Bruce Greenwald and Paul Johnson explain it, "The concept is simple—understand the fundamentals of companies, identify those that are undervalued, and stick with them through thick and thin. Don't follow the pack"(2016, 29).

⁵⁶² Standing before a meeting of the American Academy of Political and Social Science in the Summer of 1941, MIT President Karl T. Compton recalled how, "Ten years earlier when the depression had upset confidence in the very foundation of our economic structure, we were told by some that unrestrained enterprise and technological progress had overbuilt production way beyond our capacity to consume the goods produced. We technologists wondered if we had any right to be alive, let alone to do our job, as we heard and read the theories of those who believed that higher standards of living are assured by curbing production, not stimulating it; by making production more expensive rather than cheaper; by distributing wealth by laws without much thought of how this wealth can be created"("Technological and Scientific Resources" *Annals of the American Academy of Political and Social Science*, Vol. 218 (1941): 66.

rose faster than underlying business operations seemed to warrant. To them, this was speculative gambling, not investing, and it was something to be avoided. “Still,” reported *Barron’s* in the late 1940s, “depression psychology persists, and nowhere more markedly than in financial centers. Undoubtedly, thinking is influenced by the memory of the great depression which followed the long boom of the ‘twenties.”⁵⁶³

By the subsequent bull market of 1954, this older generation began brushing up against a growing number of capital-gains minded investors, those for whom dividend income paled in comparison to the fortunes that could be reaped through stock price appreciation. For the Graham and Dodd generation, the analysis of a company’s average performance functioned as an approximate limit on what one could expect of that company’s future. For the younger generation, however, the future was limitless. As Sobel suggests, 1954 marked the year when “social and structural changes began that would become the hallmarks of the bull era, among which were optimism, unbounded faith in the future, security in the fundamental virtue of America, and an unbridled belief in progress.”⁵⁶⁴ Brokers began speaking of “a new kind of customer appearing in the offices, people who were more gamblers than investors, and didn’t know how to handle them. Old-timers recognized some as being similar to the clients of the 1920s, people who wanted action more than security.”⁵⁶⁵ Americans who before had kept their money in savings accounts and bonds, were now entering the market for the first time, and with

⁵⁶³ *Barron’s*, “The Trader Gives His View of the Market” (August 8, 1947): 2

⁵⁶⁴ Sobel 1975, 224

⁵⁶⁵ Sobel 1975, 224

each passing year, the number of Americans owning shares of publicly-traded corporations grew ever higher.⁵⁶⁶

By 1962, New York Stock Exchange president Keith Funston was able to declare that 1 in every 6 adults owned shares of stock; a decade, earlier that number had been 1 in 16. Colloquially known as “the Big Board,” the NYSE had just completed its latest shareholder census, revealing that between 1952 and 1962, more than 10.5 million Americans had become shareholders.⁵⁶⁷ For these new investors, as well as the community of brokers, analysts, specialists, and investment bankers on Wall Street, “dividend return, once the *sine quo non* of a ‘prudent’ investment, was forgotten.”⁵⁶⁸ As one investment banker observed in 1959, “several million individuals have been initiated to the exciting exhilaration of easily-made capital gain or, the mental depression of ill-afforded capital loss.”⁵⁶⁹ Bonds and savings accounts, by contrast, promised no such excitement.

⁵⁶⁶ Charles Merrill (who died in 1956 after having professionalized brokerage and brought wall street to main street) “emerged in the late 1940s and early 1950s as the most influential Wall Streeter since World War I. Just as the elder Morgan had helped refashion investment banking and became the spokesman for the industry, so Merrill did the same for commission brokerage, with an even greater impact on the nation”(Sobel 1975, 173). “By 1950 Merrill Lynch was the largest brokerage by far”(179). Merrill, Lynch, Pierce, Fenner & Smith mailed free investment advice around the country in order to attract and educate middle class Americans on the basics of investing. In 1960, for example, the brokerage firm mailed a pamphlet titled, “How to Invest: The Who, Why, What, Where, How, and When of Buying Stocks and Bonds,” which contains such statements as this: “There was a time when the word *capitalist* was reserved for people of great wealth. But no more. Today the country is full of capitalists, of people—many of them in the middle income brackets—who have a nest egg somewhere, a stake in the future. And the reason is clear. ... Spendable income has been more abundant and more widespread in recent years than at any time in the nation’s history. Once hundreds of investors with millions of dollars were able to supply all the funds that American industry needed for expansion. Today industry looks also to the millions of people with hundreds of dollars to meet the ever-increasing need for capital,” and this: “But don’t be panicked into selling without good reason. If you invested with care and for the long term, you should not let every change in the price of your stocks be a signal for gaiety or gloom. Keep your perspective, and remember that it is the nature of the market to fluctuate.”

⁵⁶⁷ Specifically, there had been 6,490,000 shareholders in 1952; 8,830,000 in 1956; 12,490,000 in 1959; and 17,010,000 in 1962. (Editorial, “U.S. Shareholders Reach New High” *The Commercial and Financial Chronicle*, June 28, 1962, 44).

⁵⁶⁸ Robert Sobel 1975, 241

⁵⁶⁹ (Casper M. Bower, Nov 1959 *The Analysts* 7, 15(5):34).

The gradual shift in the culture and norms governing investment habits had almost fully run its course by the end of 1957. As the market began to rally once more in mid-1958, it would be led by the new generation and its “pie-in-the-sky ‘cheap-at-any-price’ attitude.”⁵⁷⁰ The tone of the ensuing bull market of 1958-1959 set a course that would be followed for many years to come. Again, we turn to Sobel to paint us a vivid picture of the times:

Wall Street brokerages busily sold the vision of a scientifically oriented future to eager buyers. At night customer’s men would pour over physics texts so as to understand the terminology of newly glamorous industries. People who only a few years before had been concerned with railroads and automobiles, utilities and grocery chains, now spoke knowingly of transistors, klystron tubes, space travel, and the like. Such firms as Texas Instruments, Beckman, Ampex, Thiokol, Varian, and Hoffman, hardly seasoned corporations, were selling for well over fifty times earnings by mid-1959. Companies like these were in the vanguard of the space-age bull market. And new ones joined them—usually with prefixes like ‘electro’ and suffixes like ‘tron’ tacked on. Astron, Dutron, Transitron and many more were taken up by investors and speculators eager for action and glamor, excited at the possibilities of participating in romantic ventures.⁵⁷¹

Sobel had written this passage in 1975, but already in 1960, a similar, more

contemporaneous set of observations appeared in the pages of *Research Management*:

Years ago the investment analyst talked principally about price earnings ratios, consumer disposable income, gross national product, and margins before and after taxes. These factors are just as important as they ever were, but the analyst today is just as much concerned with magneto-hydrodynamics, thermionics, and moletronics. The analyst of today and tomorrow has to have an appreciation of technological developments and, in fact, of innovations of all kinds if he is to properly serve his clients.⁵⁷²

⁵⁷⁰ Wilfred May, “Observations...” January 3, 1964, *Commercial and Financial Chronicle*, p24)

⁵⁷¹ Robert Sobel 1975, 236

⁵⁷² O. C. Roehl “The Investment Analyst’s Evaluation of Industrial Research Capabilities” *Research Management* Vol. 3, No. 3 (Autumn 1960): 144.

And so it was that, with unbridled optimism, the postwar generation traded in shares of the latest “glamour” industry: the R&D-intensive space-age group. The concept of glamour in the stock market was an old one. In the 1920s, Goodrich, RCA (Radio Corporation of America), General Electric, R.K.O. (Radio-Keith-Orpheum Corporation), Paramount, Famous-Players-Lasky, Westinghouse, American Telephone & Telegraph, and International Harvester were considered glamour stocks, a status which garnered these and other stocks higher price-earnings ratios than the typical issue.⁵⁷³ The most glamorous of all businesses in the 1920s were in the radio, automobile, and motion pictures industries,⁵⁷⁴ but by the start of the 1960s, the glamour had long gone elsewhere. Aerospace firms such as Aerojet-General, Douglas Aircraft, General Dynamics, Lockheed, Marquadt, Glenn L. Martin, Thiokol, Reaction Motors, and others saw enormous gains in the month and half following the launch in October 1957 of Soviet Sputnik satellites. October 1957. This event, along with the viability of developing thermonuclear warheads,⁵⁷⁵ changed federal spending dramatically.⁵⁷⁶

6.2. Space Racing

In May of 1957, Defense Secretary Charles E. Wilson had taken his cue from the President Eisenhower, by announcing a defense economy directive that would impose a \$38 billion ceiling on procurement and a policy that would “stretch-out” defense orders

⁵⁷³ Robert Sobel, *The Great Bull Market: Wall Street in the 1920s* (New York & London: W.W. Norton & Company, 1968): 38-39 & 121.

⁵⁷⁴ Robert Sobel, *The Age of Giant Corporations: A Microeconomic History of American Business, Second Edition* (Westport, CT & London: Greenwood Press, 1984): 25-51.

⁵⁷⁵ Gainor “The Atlas and the Air Force”

⁵⁷⁶ Sobel “In 1957 Eisenhower reported a budgetary surplus of \$1.6 billion. The budget showed a deficit in 1958 of \$2.8 billion, and in 1959, the deficit was \$12.4 billion, the largest the nation ever had in peacetime.

over a number of years. For the next five months, every aerospace firm was faced with one or more stretch-outs or cancellations, but in the wake of Sputnik that October, Secretary Wilson's budget ceilings were lifted and new contracts were entered into. Although share prices of defense contractors had taken a dip since the end of the Korean War, new expectations of Department of Defense contracts had brought them to life again in 1958.⁵⁷⁷

The government threw itself into a series of multi-billion dollar crash programs with the expectation of accelerating development of U.S. satellites and intercontinental ballistic missiles (ICBMs). The race was on, and in May 1961, investors were given another dose of space-age enthusiasm when President Kennedy committed the nation to sending a man to the Moon by the end of the decade. The following year, NASA threw glitter onto glamour as it released drawings of the three-man Apollo spacecraft to the public and announced its decision to build the Saturn C-5 rocket as the lunar launch vehicle. As expected, multibillion dollar contracts were involved. Meanwhile, with Convair's Atlas ICBMs and Glenn L. Martin Company's Titan ICBMs already in place, R&D was begun on a new generation of ICBMs, with Boeing taking the lead on the Minuteman and Lockheed on the Polaris fleet ballistic missile. As economists Merton Peck and Frederic Scherer observed in 1962, contracts on these and other projects propelled the defense industry to new heights:

⁵⁷⁷ "Even with a sharp decline in margins, earnings have more than quadrupled from the first year of the Korean War. ... The aircraft group was increasingly profitable for the half dozen years from the outbreak of the Korean War (June of 1950) until the mid-1950's. Practically every company had at least one major contract involving long production runs. ... The net earnings gain, however, was held down by Korean War excess profits taxes, with some of the companies in the 70% bracket. Thus, with the elimination of EPT in 1954, net earnings showed a sharp jump"(Hartman L. Butler, Jr. "Aerospace Fundamentals and Industry Analysis" *Financial Analysts Journal*, Vol. 22 (January 1966): 56.

The extent to which large weapons firms are the large firms in the economy generally can be determined by comparing the membership lists of the 100 largest defense contractors with the membership of a list of the 100 largest industrial firms in the economy as measured by their sales. The appearance of the same names on the two lists indicates either that their extensive weapons business alone makes some firms large enough so that they are counted among the largest industrial corporations, or that corporations, large from their commercial business, have entered into weapons making.⁵⁷⁸

Peck and Scherer's observation could hardly have surprised anyone on Wall Street.

Corporate net earnings were no longer diminished by the excess profits tax of the Korean War years, and with the space race well underway, the defense industry was booming. This led one investment analyst to argue that increased contracting, changes in stock prices, and the investing public's enthusiasm for space age technologies had all become deeply enmeshed.⁵⁷⁹ Citing a Department of Commerce study that seemed to indicate as much, he concluded that "defense spending has become woven into the warp and woof of our economy."⁵⁸⁰

To keep up with changes in Pentagon priorities, defense manufacturers moved quickly as a group to reorient their operations away from aircraft towards missiles. The complex electrical components needed to make missiles, satellites, and spacecrafts viable, landed many firms in the newly-renamed "aerospace" industry among the ranks of electronics companies.⁵⁸¹ Thus, although not entirely distinguishable from the aerospace

⁵⁷⁸ Merton J. Peck and Frederic M. Scherer, *The Weapons Acquisition Process: An Economic Analysis* (Boston, 1962): 118

⁵⁷⁹ Casper M. Bower "A New Economic Barometer Needed To Measure Impact of Peacetime Defense Expenditures," p35. The bull markets of the late 1950s impacted more than just aerospace and electronics. Those markets lifted share prices of other, more established firms as well, such as National Cash Register, Pitney Bowes, Sperry Rand, Addressograph, IBM, Bristol-Myers, American Home Products, Pfizer, Eastman-Kodak, Rohm & Haas, Corning Glass, and even the utilities.

⁵⁸⁰ Bower, 35

⁵⁸¹ "The change in the title of the industry from 'aircraft' to 'aerospace' has undoubtedly improved its investment standing. This goes well beyond a euphemism as the new dimensions of space is added and design capabilities and production vehicles are no longer limited by the atmospheric envelope"(Hartman L.

contingent, electronics firms constituted another glamour industry of the late 1950s and 1960s.⁵⁸² By September 1963, the *New York Times* could report that “within industry, parallel with the growth of the Space Age, the largest amount of dollars for research and development is in the aircraft and missile fields. Electrical equipment and communications companies account for the next biggest slice of the R & D industry pie.”⁵⁸³ The electronics industry that emerged out of World War II represented a collection of older electrical equipment manufacturers that, beginning in the 1950s, were rapidly joined by a large number of newly-incorporated electronics firms. This hodge-podge industry was home to the likes of Texas Instruments, IBM, Polaroid, RCA, Litton Industries, Syntex, Beckman Instruments, Motorola, AirTechnology Corporation, Hoffman Electronics, Ampex, Honeywell, Sanders Associates, Magnavox, and Fairchild Camera and Instruments, to name a few.

Electronics companies without any track record at all watched as their share prices floated ever higher by eager investors looking to be among the earliest to spot a bargain. Many young companies that today we would call “start-ups” sought to go public with their stock and filed the necessary paperwork with the SEC in order to do so. In the fiscal year ending June 1962, the number of registered statements on file with the SEC reached 2,307, representing a rise of 26% over fiscal 1961. According to the SEC, this number “far exceeded that for any previous year in the Commission’s history.”⁵⁸⁴ These new issues, reported an SEC *Special Study*, “represented in many instances young, untried,

Butler, Jr. “Aerospace Fundamentals and Industry Analysis” *Financial Analysts Journal*, Vol. 22 (January 1966): 55-56.

⁵⁸² Frances Stone *Financial Analysts Journal* (January-February 1963)

⁵⁸³ *New York Times* (September 1963)

⁵⁸⁴ S.E.C. *28th Annual Report of the Securities and Exchange Commission, Fiscal Year Ended June 30, 1962* (Washington DC: Government Printing Office, 1963): 4.

small businesses frequently with scientific-sounding names ending in -namics, -onics, or -mation. Among these were: Digitronics, Hedtronics, Pacotronics, Microsonics, Nucleonics, Techmation, Pneumodynamics. There is little doubt that some of the many first-time investors who eagerly bid for shares in these companies had little understanding of what these companies manufactured.”⁵⁸⁵ Electronics firms featured prominently in the exploding new-issues markets, “accounting for more than half of the issues and almost half of the dollar amount” being traded.⁵⁸⁶ “Clearly,” observed Neil H. Jacoby in the *Commercial and Financial Chronicle*, “the electronics industry has been a consistent favorite of American investors during recent years, as the spectacular gains in the market prices of its equity securities testify.”⁵⁸⁷ This much was made clear by Lawrence H. Taylor, Chairman of the newly-organized National Stock Exchange in New York City.⁵⁸⁸ In early 1962, he shared a primer on “going public” that laid particular emphasis on the needs of small and medium firms:

If a company is in a rapidly growing field like missiles or electronics which have caught the public fancy and where these companies do not have to fight entrenched competition of large companies predominating the market, the stocks once on the market can in time sell at prices as high as 50 or 60 times earnings or sometimes at very attractive prices with no earnings being shown at all, the price being based largely on promise. ...

The companies selling primarily on future prospects are speculative by their very nature yet they have strong appeal especially in publicized industries of the future. The romance of possibly investing a founder’s dollar in a General Motors of

⁵⁸⁵ Committee on Interstate and Foreign Commerce, *Report of the Special Study of Securities Markets, Part 1*. 88th Cong., 1st Sess. (Washington DC: Government Printing Office, 1963): 824

⁵⁸⁶ Committee on Interstate and Foreign Commerce, *Report of the Special Study of Securities Markets, Part 1*. 88th Cong., 1st Sess. (Washington DC: Government Printing Office, 1963): 486

⁵⁸⁷ Neil H. Jacoby, Dean of the Grad School of Biz Admin, UCLA, “The Broad Economic Features of American Electronics Industry,” *Commercial & Financial Chronicle*, 193(6050), April 27, 1961, 27)

⁵⁸⁸ The National Stock Exchange was granted registration as a national securities exchange on August 16, 1960; began operations on March 7, 1962; and closed up shop on January 31, 1975.

tomorrow is a powerful motivation force and untold fortunes have been lost as well as made this way.⁵⁸⁹

As one might predict, many of these companies had very little capital to work with, and this usually meant that it was in their best interest to make a good showing in their financial statements so as to attract as much equity capital as possible. An especially effective way of achieving this more flattering earnings picture was to defer R&D costs against the future, although it was not uncommon to have these costs included as an undifferentiated element of “organization” or “development” expense—i.e., the deferred costs incurred in organizing and running a developing company that had yet to earn the regular profits characteristic of companies in the going concern stage. Whether as separate line items on financial statements or as elements of organizational expenses, the desired effect of deferring R&D costs came in the form of enhanced reported profits or else reduced reported losses.

In January 1962, at the annual meeting of the Controllers Institute of America, SEC Chief Accountant Andrew Barr delivered a speech that anticipated a memorandum that he would be asked to submit to Congress two years hence. As we saw earlier, in 1964, Barr identified R&D as one of eight areas of practice in which alternative accounting methods produced considerably different values. Back in 1962, and before an audience of financial executives, Barr shared two examples that illustrated just how, “Accounting for research and development [has become] a troublesome matter especially in new companies.”⁵⁹⁰ One balance sheet that recently came before his desk had listed

⁵⁸⁹ (*The Commercial and Financial Chronicle*, Vol. 195, No. 6130, “When to ‘Go Public’ for the Acquisition of Capital” February 1, 1962, p.33)

⁵⁹⁰ Andrew Barr, “Business Combinations and Other Financial Reporting Problems” January 18, 1962

\$250,000 worth of deferred charges, a deficit of \$350,000, and a net equity (or shareholder capital) of only \$20,000. That \$250,000 of R&D expenditure was not an expense chargeable to current income, but a charge held in suspense, an asset laying in wait. Examining the footnotes of this financial statement, Barr gleaned a reason for the deferral policy:

A note disclosed that at one time amortization of research and development was based on an expectation of selling 75 units but the accounting had been changed to a four year basis. The text of the prospectus stated that 13 units had been sold, none recently, and there were no orders at the balance sheet date. As you might expect, the accountant's certificate discussed the situation and the opinion was subject to the ultimate recovery of the deferred charges.⁵⁹¹

By deferring R&D, the managements of the young company were able to report a less-foreboding deficit to their shareholders. After summarizing another such case that had come to his attention just a month earlier, Barr concluded this portion of his speech with a series of questions posed to his audience: "These examples pose the basic question of when is a going concern not a going concern, or, if not that drastic, when do you give up hope on a new idea? Or, in cases like the Cuban situation, when do you take your losses?"⁵⁹² Finding answers to such questions was extremely important not only for R&D accounting or companies in their early developmental stage, but for accounting in general. Millions could rest on how such questions were answered. Compare, for example, a failing R&D project with a failing division of a company. Researchers struggling to work out a solution to a company problem may report that they simply do not see a way forward on the project. If management is convinced a dead-end has been

⁵⁹¹ Barr (January 18, 1962)

⁵⁹² Barr (January 18, 1962)

reached, what happens to the R&D costs that have been accumulating in the meantime? Similarly, if a recently-acquired subsidiary has never once earned any revenue or introduced any savings for its parent no matter how many structural changes are introduced, at what point does divestiture start to make sense? In either case, the question is: when do you cut your losses, and how are those losses absorbed?

6.3. Bargains at Any Price

Clearly, these were significant problems, not only for R&D managers but also for controllers or head accountants, those who prepared the financial statements by which investors and the public could evaluate the condition of the firm for themselves. The typical 1960s shareholder, however, was ignorant of these difficulties and cared only about the company's bottom line, i.e., earnings per share. This left a lot of room for "romance," "magic," and "glamour" to be built into the new, "pie-in-the-sky" stock market. To analysts, this meant that a company's past performance and current earnings were no longer seen as meaningful indications of future earning ability. Other indicators would have to be found, and for this purpose, the amount a company spent on R&D seemed as promising a candidate as any. In another *Research Management* essay written three years later, Irwin Goldman observed with some frustration that, "there is the feeling that the act of performing research will carry with it a magic growth producing effect on the corporation; presumably, the more research, the more corporate growth."⁵⁹³

Published at the same time as Goldman's piece was an article in the *Financial Analysts*

⁵⁹³ Irwin Goldman, "Govt Contract Res & Dev: Effects on Planning and Control of Corp Res Program" *Research Management*, Vol. 6, No. 1 (January 1963): 56.

Journal that took on a more celebratory tone. Written by Frances Stone, analyst at Merrill Lynch Pierce Fenner & Smith with a PhD in economics from Columbia University, the article argued,

There appears to be no doubt that research and development expenditures have significance for the buying and selling of securities. With R & D altering every single facet of a company's operations, the record would *a priori* alter the market valuation. The changes vary by degree. Some corporations have been completely revamped from finished products to raw materials; other only to minor degrees. . . . The one conclusion that can be drawn from the past is that the developments will add to some company sales and income, and subtract from another company, and as certainly, the result will appear in the price of the stock.⁵⁹⁴

That earnings growth had become dependent on R&D, to one extent or another, was accepted as a new fact of life. Less obvious, however, was how information about a firm's R&D program could be used as the basis for forecasting future stock prices. In developing their forecasts, financial analysts projected earnings into the future on the basis of arbitrary assumptions regarding R&D and its relationship to earnings. Once the sum of all future earnings were "discovered," all the analyst had to do was discount a firm's prospective earning power to arrive at the present value of the firm in question. There were no strict rules about what that discount rate could be. It might be set using traditional rules of thumb, such as the cost of capital or interest rates. Alternatively, analysts might use what Quinn referred to as "probability estimates of the likelihood that the income figures will actually materialize" from the exploitation of R&D products.

Regardless of the rate chosen, if the resulting discounted present value indicated that a stock was currently undervalued, this signaled an ideal investment opportunity. An undervalued stock was one to buy up now, before anyone else discovered the same

⁵⁹⁴ Frances Stone, *Financial Analysts Journal* (January-February 1963): 23

bargain. As soon as others made the same discovery, it was only a matter of time before their investment decisions began pushing the price of stock upward to the point where it was no longer a bargain. For those who got in early, the difference between the price they purchased the stock for and the price it rose to reflected the capital gains to be made once the stock was sold. For those who arrived late to the scene, just before the share price began to slow down or plateau, their capital gains prospects were rather dim. Of course, because future values were unverifiable in the present, the current market price of *any* stock could be made to appear a bargain. If, for example, the future revealed that shares of a given company would reach \$100 per share, then buying stock of that company at a current price of \$60 was a steal. Should the price begin nearing the original \$100 estimate, adjustments to earlier assumptions may very well generate new forecasts exceeding, say \$150 or \$200 per share. For glamorous companies, especially, such upward adjustments were routine practice to the fortune-tellers of Wall Street. This, one columnist at the *Commercial and Financial Chronicle* referred to derisively as the “‘cheap-at-any-price’ attitude.”⁵⁹⁵

These practices were not all about making quick profits or building up reputations, although these motivations were certainly present. Many analysts delighted in the work of crafting scientific-like charts on the basis of admittedly arbitrary assumptions. They spent a great deal of effort adjusting, updating, rethinking, and debating their techniques. So much so, in fact, that they became known as the technicians of Wall Street. With their charts, graphs, projects, statistics, and formulas, analysts’ decisions to recommend or buy shares appeared to their readers to be grounded on the

⁵⁹⁵ Wilfred May, “Observations...” January 3, 1964, *Commercial and Financial Chronicle*, p24)

basis of cold hard facts. Such techniques were reminiscent of scientific practice, even if the objects subjected to these techniques were the products of individual and collective fantasy and not empirically-observed phenomenon. In a tongue-in-cheek way, this much was recognized by the investment community itself. For one thing, a running joke on Wall Street held that investors were not only discounting the future but the hereafter as well.⁵⁹⁶ Similarly, an editorial in *Forbes* made clear that the discounting process was an expression of investors' collective foresight—and if it had to be taken on faith, then so be it.

[Some investors] think the market is more dependable when it reflects than when it discounts. They are a little suspicious of the discounting process, and reluctant to admit that the behavior of the mass, in the investment world, represents any dependable intuitive wisdom. Be that as it may, it seems to me that the only way to explain the better behavior of stocks...is to recognize that, right or wrong, those who have been buying equities of late are convinced that business is about to get better. In other words, the stock market is discounting business improvement in advance rather than waiting for tangible and statistical evidence of recovery. What we are doing, therefore, in the words of the poet, is believing where we cannot prove.⁵⁹⁷

A shrinking minority were worried by such statements. Even Jacoby, dean of the UCLA business school, inserted a brief pause in an otherwise-enthusiastic essay to recommend restraint: “There are ominous signs,” he cautioned, “that investors are becoming indiscriminate in their eagerness to buy anything with the magic ‘electronic’ on the stock certificate. ... In the case of many electronic companies, investors have certainly discounted growth in earning power and in yields (in capital gains as well as in current

⁵⁹⁶ M. Richard Sussman, “Discounting the Hereafter” *The Commercial and Financial Chronicle*, Vol. 193, No. 6050, (April 27, 1961): 13); Alan Abelson “Up and Down Wall Street” *Barron’s*, Vol. 46, No. 16 (April 18, 1966):23; Sobel 1975, 235

⁵⁹⁷ *Forbes*, Vol. 87, No. 3 (Feb 1, 1961) “Believing Where We Cannot Prove” by L. O. Hopper. 38)

dividends) rather far into the future.”⁵⁹⁸ Speaking before a meeting of the American Finance Association in December 1961, the 67-year old Benjamin Graham offered more strident criticism:

If the recent picture had been one of the stock market’s advancing in step with the national product, and in close proportion with it also, then the observer might conclude—somewhat to his amazement—that not only has the economy been reformed but human nature as well. But here the facts part company with the hypothesis. The stock market level has not been governed primarily by the level of business, but rather by the development of new investment theories and attitudes and by a typical growth of speculative interest and activity. ... Yet I have a feeling that the financial world has become too complacent about the future, too confident of the invulnerability of common stocks as a whole to a drastic change in their fortunes. ... My own inward picture of the present stock market is that of an institution cut adrift from old standards of value without having found dependable new standards.⁵⁹⁹

One year later, Burton Kolb, associate professor of finance at the University of Colorado business school, set out to demonstrate a method by which Graham and Dodd’s fundamental analysis could be reconciled with the new R&D climate. His “Research Development and Common Stock Values,” opens with the following observations:

The importance of research and development activities in the analysis of common stock values is well recognized, particularly with regard to research-oriented companies in the chemical, drug, electronics, aircraft and missile groups. Almost every annual report of investment analysis dealing with firms in these fields places heavy emphasis on the R and D program. Market prices have reacted (and often overreacted) to news, rumors, hopes and aspirations of research efforts. Yet with all this recognition, no systematic, logical methods of interpreting the significance of R and D outlays have emerged.⁶⁰⁰

⁵⁹⁸ Neil H. Jacoby, Dean of the Grad School of Biz Admin, UCLA, “The Broad Economic Features of American Electronics Industry,” *Commercial & Financial Chronicle*, 193(6050), April 27, 1961, 29)

⁵⁹⁹ “Are We Too Confident About Invulnerability of Stocks?” *C&FC*, Feb 1, 1962, p32

⁶⁰⁰ Burton Kolb “Research Development and Common Stock Values,” *Financial Analysts Journal* (September-October 1962): 9.

Rather than speculate as to intangible values, fundamental analysis emphasized evaluation of the tangible assets that a firm had on hand, as well as how effectively these resources were put to use. To Kolb, R&D had a place in this analysis because R&D spending was not unlike investments in tangible assets. By contrast, the technicians' forecasts lacked such substantial foundations:

Rather than sharpening the focus upon investment values [defined as Graham and Dodd's "intrinsic value"], recognition of the importance of R and D has obscured it. This is reflected by a decline in confidence in current and historical earnings as a guide to the future earning power component of investment value; faulty recognition of past and present earnings resulting from the accounting and analytical treatment of R and D outlays; and considerable departure from the basic concepts underlying the determination of appropriate earnings multipliers (or, reciprocally, capitalization rates).⁶⁰¹

Kolb's corrective to such outlandish discounting practices was to combine a firm's earnings with a proportion of R&D expenditures incurred in the same period. This, he believed, better captured a company's financial condition; offered a basis upon which one could derive more reasonable forecasts; and, therefore, avoided the questionable assumptions made by discount-happy technicians. This was all well and good, unless one questioned the assumptions built into Kolb's method.

In particular, Kolb believed that new-product R&D ought to be treated as investments of capital, with the same "definiteness of results" as any other investment in plant or equipment. This was true, he explained, because managements simply would not commit funds to such projects unless their outcomes were known in advance.⁶⁰² However,

⁶⁰¹ Kolb (September-October 1962): 9.

⁶⁰² "It might be argued that the results of pioneering R and D are so indefinite that it is unreasonable to consider such outlays to represent the reinvestment of earnings. ... [Because that new-product R&D is] not in any way related to the production of current revenues, it then would follow that these funds had been wasted. Clearly, this is not the case. Management has weighed the expected future revenues and the related

this begged the question: *could* managements know the outcomes of R&D projects in advance? Others following Kolb's tack made similarly problematic moves in their reasoning, including Allan R. Drebin, Cornell University accounting professor who's *Accounting Review* essay of July 1966 took the following position: "It seems clear...that if a firm spends money for research which is expected to benefit future periods, it has acquired an asset, not an expense. Spending money on research to benefit future periods is analogous to acquiring a tangible asset, like a building, which is also expected to benefit future periods."⁶⁰³ Thus, according to Drebin, R&D is capital in the same way that buildings are capital: both generate returns on their investment.

Another example from the same year as Drebin's *Accounting Review* essay is one by William J. Priest, Jr. printed in the *Financial Analysts Journal*. In "Evaluating Research and Development Expenditures," Priest began with an observation which, by that time, was already taken as granted: "it appears that there is a close relationship over the long run between the amount a firm spends on R and D and the total number of important inventions it produces." This association had made it "crucial," he wrote, "for the analyst to develop a rational, systematic method of appraising R and D." To meet this need, Priest developed a "projected income statement...to demonstrate the effect of R and D on earnings."⁶⁰⁴

Given two companies in the same industry and applying the model to both, it should be possible to determine whether or not earnings will rise or fall at an increasing or decreasing rate from that of the past for both companies. In this

risks...with the same degree of wisdom as in determining any other commitment of company funds"(Kolb 1962, 12).

⁶⁰³ Allan R. Drebin *Accounting Review* (July 1966): 414

⁶⁰⁴ William W. Priest, Jr. "Evaluating Research and Development Expenditures" *Financial Analysts Journal* (July-August 1966): 43-44

manner, a comparison of the effectiveness of the research programs of the respective firms can be made.⁶⁰⁵

Common to Priest's "rational, systematic method" and Kolb's "systematic, logical method" was the belief that R&D affected future earnings the same way as "plant and equipment outlays." Yet, as we have already seen in the previous chapter, R&D does not "work," "succeed," or "come to fruition" merely because it cost money. "The product of research activity," explained the National Industrial Conference Board in 1963, "is not known with any reasonable degree of certainty in advance."⁶⁰⁶ By contrast, the results of putting plant and equipment to work could be known in advance with a "reasonable degree of certainty." Continued the Conference Board,

All that can be available, in effect, are judgments regarding the probabilities of various outcomes. In this respect, research shares certain features with lotteries, gambling and insurance. When a business firm or a Government agency decides to spend money on an R. & D. project, all that it is really buying at the time is a chance that an event will occur within the time limits specified by the amount of money spent. The event itself may be a discovery or development of new knowledge, a product or a process, or a series of discoveries or developments. The sponsor also buys a chance that regardless of the outcome of the main objectives of the project, some other new and entirely unforeseen fruit may be yielded by the research process.⁶⁰⁷

While a piece of capital equipment "succeeds" at being an asset the moment it's purchased and put into use, R&D does not succeed in the same way. The large part of R&D outlays represent researchers' salaries, and while their work may be more or less productive depending on the environment and individual researchers' needs, the final output of their efforts simply cannot be predicted in advance. If this were not true, there

⁶⁰⁵ Priest, Jr. (July-August 1966): 43-44

⁶⁰⁶ As quoted in Richard Rutter, "Research Costs to Be Reviewed" *New York Times* (September 22, 1963): 10.

⁶⁰⁷ As quoted in Richard Rutter, "Research Costs to Be Reviewed" *New York Times* (September 22, 1963): 10.

would be no need for R&D in the first place, an observation made repeatedly throughout the century—even if only to fall on deaf ears.

To be fair to Priest, his treatment of R&D as a sort of capital outlay was deployed for heuristic purposes, with acknowledgments made to the differences that distinguished outlays for capital assets from outlays for R&D. And to be fair to Kolb, it does seem that both the earnings multipliers and the discount rates commonly used by the technicians were indeed based on arbitrary, even mystical, assumptions. Yet, given all the difficulties of relating R&D to a firm's growth in the context of such accounting gray-areas as defense contracting, high-tech start-ups, and conglomerates, reliance on such subjective judgments was widely tolerated by the investment community. These gray-areas weren't the only obstacles confronting anyone attempting to apply Graham and Dodd-style analysis to R&D-intensive firms. About the functioning of R&D itself, many questions remained unanswered which might otherwise make such analysis more tractable.

6.4. A Dearth of Data

Along with expenditure totals, analysts desperately sought other statistics of relevance to R&D. For example, how many engineers and scientists did a firm employ? How many of them held PhDs? How many contracts, and of what size, did a given firm win? Were top executives demonstrating enthusiasm for what their R&D programs were up to? More importantly, did top and middle level managements give indications that they understood how to balance and encourage researcher productivity? The information needed to answer these and similar questions was very difficult to come by. The overwhelming majority of companies simply did not disclose the level of detail that analysts hungered for. David Green Jr. and Harry W. Feick recognized this dearth of

information earlier than most. In a 1960 essay published in the University of Chicago's *Journal of Business*, Green made the following observation:

As things now stand, it appears that part of the investing public is attempting to measure assets compared with expense for research and development, even though the data available to them are meager. At least, this is a possible explanation of the remarkable price-earnings ratios on traded stocks of companies with research and development expenditures that are large in relation to sales, reported earnings, and reported stockholder equity.

The data were indeed meager to analysts, yet for Green, this was only a passing observation, however astute. Feick, however, sought to bring clarity where there had only been mystification. Research and development, lamented Feick, was subject to “a state of faddism which is unfortunate” and which relied on suspect “formulas of percent expenditure in R&D in relation to annual sales, and ratio of patents to R&D employee establishment”—all of which “provide only a partial answer to what stockholders, new purchasers of securities, and corporate management can expect from the firm of five, ten or twenty years hence in concrete R&D profitable output.”⁶⁰⁸ Analysts, Feick argued, should help “bring perspective back” so that managements might be inclined towards “more skillful and astute use of the unusual talents contained within the R&D complex.”⁶⁰⁹

Rather than treating R&D as manufacturing, Feick left the uncertainty of R&D intact by focusing on the conditions that tended to make an R&D organization function more smoothly within its parent corporation. In an essay in *Financial Analysts Journal*, Feick shared observations he made on the basis of 34 in-depth interviews involving a range of

⁶⁰⁸ Harry W. Feick “Digging Deeper Into Research and Development” *Financial Analysts Journal*, Vol. 18, No. 5 (September-October 1962): 79.

⁶⁰⁹ Feick (September-October 1962): 81.

R&D personnel, from lab technicians all the way up to executives. No organizational form of R&D that had caught Feick's attention was the crash program. These were "generally very expensive (unless Uncle Sam is picking up the tab), and is indicative that management in the past lacked perception of the future needs that should have been undertaken years before. Further, this type of 'catching up' is not conducive to holding key researcher employees."⁶¹⁰ Crash programs were, for Feick, a red flag that usually pointed to an unbalanced R&D program led by short-sighted management.

Feick was especially mindful of the relationship between the ability to retain "key researcher employees," on the one hand, and the quality and effectiveness of an R&D organization, on the other. Crash programs were one way of wearing out researchers. Another, he suggested, was committing them too heavily to product improvement R&D at the expense of "striking for the innovation, the new and the unique."⁶¹¹ If researchers were unmotivated by their work or felt under-appreciated, they could and did leave to find new work. Richard S. Morse of the National Research Corporation had made the same point a decade earlier. "These men," explained Morse, "cannot be indiscriminately hired and fired; they constitute a permanent obligation of the company."⁶¹² This reasoning led Feick to recommend that analysts start asking, "how active is the turnover in scientific personnel at the R&D installation?"⁶¹³ After all, he explained, "It is axiomatic that *people* make effective R&D, not the annual expenditure."⁶¹⁴

⁶¹⁰ Feick (September-October 1962): 80

⁶¹¹ Feick (September-October 1962): 80

⁶¹² Richard S. Morse "New Developments on the Research Front" *Analysts Journal* (May 1953): 117

⁶¹³ Feick (September-October 1962): 81.

⁶¹⁴ Feick (September-October 1962): 81. Emphasis in original.

Unfortunately, the majority of companies did not report how they spent their R&D funds, whether certain projects were successful, how many researchers they employed, what their turnover rates were, how researchers' divided their time across projects, and other such disclosures that might tip off analysts to the firms with more promising R&D programs. While the outcomes of these programs were not predictable, investors could at least feel more confident knowing that management policies were not actively undermining researcher morale. The lack of insider knowledge made this extremely difficult to evaluate. Even with a narrow focus on annual R&D expenditures, analysts often struggled to find the information they were looking for.⁶¹⁵ R&D totals did not appear as a line item on financial statements, but were aggregated under other labels, such as deferred charges, general expenses, administrative overhead, or contracts-in-process. For space-age investors, all of this made the job of predicting future earnings especially difficult. The same did not hold true for institutional investors, though.

After limping along in the 1930s, institutional investors, of which mutual funds are a large part, had resurfaced in the post-war recent years, more powerful than ever. They went to work buying and selling on huge volume, picking up stocks in large blocks at a time and igniting speculation among smaller investors as to which stocks they would gobble up next—perhaps shares of Xerox or Polaroid, Control Data or Fairchild Camera, Litton Industries, or maybe Texas Instruments? Unlike the institutional investors, the “small guy” lacked the resources to identify which of the latest glamour stocks were worth buying and which were void of any intrinsic value.

⁶¹⁵ Samuel W. Bryant “How’s the Annual Report Coming?” *Fortune* (January 1964): 104-7 & 172-4; Abraham J. Briloff “Old Myths and New Realities in Accountancy” *The Accounting Review*, Vol. 41, No. 3 (July 1966): 484-95.

Take the case of O. C. Roehl as a heuristic. Vice president of a Boston-based mutual fund, Roehl wrote an article in *Research Management* emphasizing that “many companies are dependent on [Government-funded] research appropriations for their very livelihood. For many companies the continuance of Government support is the vital factor—and this fact is most important in any investment appraisal of such a company.”⁶¹⁶ Certainly, news that a firm had won a contract from the Department of Defense, the Atomic Energy Commission, or NASA might signal a profitable investment opportunity, especially if the firm in question already had several contracts under its belt and was in the running of several more. But Roehl went further than this in his recommendations, warning investors against any company that showed “disturbing evidence it has not kept up with the rapid advances in missile and electronic technology.”⁶¹⁷ But how was such evidence to be garnered?

From his lofty perch, Roehl suggested that such information could be readily gleaned from “company managements, trade and engineering publications, government reports, and special studies that may be prepared by competent independent research and engineering firms.”⁶¹⁸ Yet, without the considerable connections and resources of fund managers, it’s questionable how effectively most investors could pursue these and other recommendations by Roehl, almost all of which required close acquaintance with R&D managers, top executives, and considerable freedom to witness experimental activities first hand.⁶¹⁹ Even financial data required by the SEC of all publicly-traded firms and

⁶¹⁶ O. C. Roehl (Autumn 1960): 129-130.

⁶¹⁷ O. C. Roehl (Autumn 1960): 131.

⁶¹⁸ O. C. Roehl (Autumn 1960): 135-6.

⁶¹⁹ “Having done what work he can do on his own, the analyst also discusses the research activities of the specific company with the company’s management, with the company’s competitors and customers, and with consultants and specialists in the particular field. The analyst’s visits with management are naturally

open for public inspection remained out of reach to those with limited means; “Though the information disclosed in 10-K reports is available to the public,” noted one author, “it is available in only four major cities and is not easily accessible to many investors.”⁶²⁰ In short, few investors had the same level of access as the institutions. Without their resources, they could not pursue the in-depth study necessary to assess the strength of a firm’s R&D organization and likely relied on extremely rough estimates based on the word-on-the-street and whatever accounting figures they could find. So long as prices were on the rise, however, there could be no losers.

6.5. End of the Soaring Sixties

This was the investment paradox of the space-age: to those deciding which stocks to buy or sell, a poor earnings history no longer disqualified a firm from the running, and prices, discounted from the hereafter, always promised to go higher. Some executives, such as Mundy I. Peale, president of Republic Aviation, used this paradox to great advantage in the public sphere. R&D costs, Peale declared, were “the true capital of the Space Age.”⁶²¹

Whatever a company pays for admission to the aerospace competence club, there are sound reasons for believing it will be worth the price. Federal expenditures for research and development in fiscal year 1964 are expected to be more than \$13 billion. More than a third of this staggering sum will be spent by NASA. The Department of Defense and the Atomic Energy Commission will spend as much or more. The entire budget for missiles and space will be more than \$18 billion.

most important and helpful. While he will have some general ideas as to the company’s research work, generally only after a visit with management will he have enough information to enable him to make an investment decision.” (Roehl 1960): 135-6.

⁶²⁰ Surendra S. Singhvi “Corporate Management’s Inclination to Disclose Financial Information” *Financial Analysts Journal* (July-August 1972): 67.

⁶²¹ Mundy I. Peale “Research and Development” *Financial Analysts Journal*, Vol. 18, No. 5 (September-October 1962): 45.

Some of this money will come to Republic and help to pay for our large and growing staff. We currently have 51 contracts in various areas of Government research, compared with 11 in the year 1959.⁶²²

Without saying as much, Peale indicated that the hard-earned return on that capital was taxpayer money, and for added flourish, he exclaimed, “We have more in our knowledge account than we do in our bank account.”⁶²³ As one might expect, Peale was overselling his company; in poor shape at the time, Republic’s stock was gradually purchased over the early 1960s by Sherman Fairchild, founder and owner of Fairchild Aircraft. By 1965, Fairchild had acquired enough stock to complete his purchase of Republic Aviation and fold it into what eventually became known as Fairchild Industries. A similar fate befell Douglas Aircraft when, following a series of disappointments in the commercial jet market, the company found its stock quietly being bought up by James S. McDonnell, Jr., founder and chairman of McDonnell Aircraft Corporation.⁶²⁴ In 1967, McDonnell completed its takeover and the two firms merged to become McDonnell Douglas.

These two examples were not atypical of the times. Mergers and acquisitions, including several of the infamous conglomerate variety, were all the rage in the 1960s. They were a quick and easy way of reporting dramatic growth in earnings without any growth having occurred at all. In the latter half of the decade, especially, this particular method of earnings manipulation built to a crescendo. By 1968, the number of conglomerate mergers reached an all-time high of 4,462, or 50% more than the previous

⁶²² Peale (September-October 1962): 46.

⁶²³ Peale (September-October 1962): 46.

⁶²⁴ In the mid 1960s, McDonnell Aircraft was in a strong financial position owing to large sales order from the Navy and the Air Force for its F-4 fighter planes. Already in August 1964, *Fortune* was reporting that the “company’s backlog has soared to more than \$1.3 billion, and there is every reason to believe that McDonnell will stay busy right out into 1970”(1964, 211).

year.⁶²⁵ What makes the Douglas Aircraft case especially interesting is why—despite winning a prime contract to the tune of \$1.5 billion in 1965 for work on the Air Force’s Manned Orbiting Laboratory—the company nevertheless lacked the capital necessary to remain independently viable. In December 1966, an essay in *Fortune* magazine set the scene:

For decades under Douglas Sr., the company followed conservative accounting practice of writing off development costs of new planes, such as the DC-8 [commercial jet], as they were incurred; that is, such costs were shown as expenses charged against current receipts. In 1962, as one of the changes introduced by Douglas Jr., this system was abandoned. Since then the company has postponed writing off development costs of new planes until the burden could be eased by income from sales of the plane itself. While the DC-9 was being developed, the \$100 million in such costs was shown as an asset under the heading of ‘deferred charges’ on the company’s balance sheet.

The plan then was to amortize the \$100 million at a rate of about \$200,000 per plane over 500 planes. Douglas began this procedure last spring, with delivery of the twenty-first DC-9—that is, at the very moment when it had planned [next] to begin a manufacturing profit that would have made the amortization easier. The object of this kind of ‘managed earnings’ accounting, as it is sometimes called, is to avoid a big cut in earnings during the development phases of a new plane, and an excessive jump in earning after the plane begins to make money.

As it worked out with the DC-9, the procedure was initially successful. If development costs had not been deferred, Douglas would have shown a loss not only for the first three quarters of 1966 but also for all of 1965. Now, however, with the unexpected crisis in the company’s fortunes, the deferred costs become, to say the least, extraordinarily inconvenient. On top of the \$400,000 that Douglas is losing at present in current costs on each DC-9 produced, it must add the \$200,000 item for amortization. That means losses probably will continue well into 1967 and perhaps longer.⁶²⁶

Far more impetuous than his father and typical of the new breed of MBA-carrying managers, Douglas Jr. committed his company to what proved to be an unsustainable

⁶²⁵ Editorial “1968 Mergers Set Record” *Management Accounting*, Vol. 50, No. 7 (March 1969): 4

⁶²⁶ John Mecklin, “Douglas Aircraft’s Stormy Flight Path” *Fortune*, Vol. 74, No. 7 (December 1966): 256 & 258

method of financing.⁶²⁷ Entitled, inexperienced, personally aggressive, and dismissive of those who disagreed with him, Douglas Jr. was resented by nearly everyone in his company. From the executive suite on down the line, many either left on their own volition or were summarily fired in what *Fortune* later described as “a bloodbath that Douglas people still discuss in subdued voices.”⁶²⁸ Under Junior, project costs were continuously deferred on the hope that income from sales would be regular enough to amortize (or charge off) deferred charges. Today’s earnings would amortize yesterday’s deferred charges; tomorrow’s earnings would amortize today’s deferred charges; and so on and so forth.

⁶²⁷ Business historian Alfred DuPont Chandler is one among several who have made a similar point about the post-war MBA generation. Prior to WWII, the number of MBAs were a relatively few in number, but by the 1960s, business school curricula had been transformed by new “sciences” of management that seemed to equip graduates with objective tools that could be applied to any place, to solve any problem. Having reviewed the evolution of curricula at Harvard and Columbia business schools in the postwar period, Susan Ariel Aaronson concludes that such schools “generally did not train students to make operational decisions”(1992, 161). Instead, argues John Brooks, these schools were busy in the 1960s “trying to enshrine business as a profession, and often taught that management ability was an absolute quality, not limited by the type of business being managed”(1973, 156). According to Chandler, business schools “encouraged the hubris of American managers by teaching that those who learned the general principle of management need not be intimately concerned with its product-specific content”(1992, 37-8). Students flocking to business schools to earn an MBA “were being told by academic practitioners of management science that management was a general skill,” and when the conglomerate merger wave began to pick up steam in the 1960s, the result was that “many—but certainly not all—had come to believe that if they were successful managers in their own industries, they could be just as successful in others”(1992, 26). Plucked straight out of their MBA programs and appointed to management positions, these young executives “consider[ed] themselves to be professional managers” and, as one *Fortune* reporter observed in 1965, “seem unworried about whether they have any special training in the prospective company’s area of activity. They feel competent to apply their talents to the benefit of any corporation—from steel to brassieres,” as one of them puts it”(Freedgood 1965, 153). Taking a long look at “some of the changes in managerial patterns that are under way,” William J. Vatter in 1961 argued that, “On balance, we would expect an M.B.A to have more usable skills; he should adjust to the demands of a job more readily, progress faster, and ultimately achieve greater things than the holder of a bachelor’s degree. This is only partially verifiable in practice. There are plenty of exceptions. ... it is a fair question as to whether maturity is attained any better in a classroom than from work experience”(Vatter 1961, 22).

⁶²⁸ John Mecklin, “Douglas Aircraft’s Stormy Flight Path” *Fortune* 74, no. 7 (December 1966): 170. Those who remained called him “Junior” when he was out of the room.

Throughout the 1960s, countless numbers of companies made similar changes to their R&D accounting policies whenever financial pressures started piling up.⁶²⁹ Many of these were members of the glamour industries. Take Control Data Corporation, for instance. Formed in 1957, the handful of engineers who founded Control Data had become millionaires in less than three years' time. In 1964, however, one of these founders developed a craving for rapid growth, and—along with so many other young executives of the go-go years—began buying up smaller firms through the exchange of company stock. Facilitating this effort was a change in accounting policy that put an end to the expensing of R&D and committed the firm to deferral. According to an article in *Barron's*, Control Data “switched to a deferral method at a time when profits would have been badly damaged” by expensing.⁶³⁰ Together with reductions in depreciation charges made possible by yet other changes in accounting policy, Control Data was able to report earnings \$3.4 million higher than they would have been otherwise. In that same article, the *Barron's* writer explained:

To be sure, all companies are required to spell out the accounting techniques and changes they utilize in their prospectuses and stockholder reports. Unfortunately, investors and most analysts work almost exclusively with earnings as they are reported. The obsession is with reported earnings, not what lies behind them. ... The accounting comparability problem is not unique to the computer industry. Yet, with the possible exceptions of commercial aircraft manufacturers, it does seem that in no other industry is the degree of variance so extreme. One of the principal reasons for this disparity is that the tremendous growth the electronic data processing industry is experiencing has put a huge strain on its finances and as equally big premium on flexibility.⁶³¹

⁶²⁹ Eunice M. Filter “Accounting Practices of Major Computer Companies” *Financial Analysts Journal* (May-June 1971): 14-52

⁶³⁰ Barton M. Biggs “Numbers Game: Computer Earnings Lean Heavily on the Accountant’s Art” *Barron's*, Vol. 47, No. 3 (July 24, 1967): 8.

⁶³¹ Biggs (July 24, 1967): 3.

That the glamour industries appeared to have caught the deferral bug more than others may well be true, as the case of Computer Applications, Memorex, Computer Sciences, and University Computing suggest.

Falling short of revenue expectations, Computer Applications began deferring its R&D costs in 1966 and by 1969, the firm was forced to write off enough of these costs to incur a \$10.1 million loss. Even though the company was unable to absorb these costs, it nevertheless continued its deferral policy and carried another \$6.4 million of deferred R&D on the books that same year.⁶³² As for Memorex Corporation, the company found itself owing more than \$200 million in debt by early 1973, enough to lead its management to begin “considering discontinuing its deferral accounting policies, which [had] been criticized by some conservative financial analysts.”⁶³³ With the company’s net worth reported at \$32.2 million, the \$35 million write-off of deferred R&D would have thrown the company into the red. Said the *Wall Street Journal*, “The company, in retrospect, appears to have expanded at a rate faster than could be sustained. In March 1972, for example, it announced it was entering the main-frame computer market even though it didn’t have a commitment for financing to make and market the computers”⁶³⁴ By charging current R&D costs to the future, Memorex’s deferral policy certainly would have failed to focus management’s attention on the realities before it.

Around the same time, management at Computer Sciences decided to write off \$6 million that had been poured into its Computicket program over the years, while at

⁶³² “The Missing Dimension” *Forbes* (May 15, 1970); “Thinking Big: Profits in Computer Software Are Not What They Seem” *Barron’s*, Vol. 47, No. 41 (October 9, 1967): 8

⁶³³ “Memorex Says Its Facing Big Writeoffs” *Wall Street Journal* (July 9, 1973): 5

⁶³⁴ Richard R. Leger, “Memorex Corp. Says It’s Facing Big Writeoffs,” *Wall Street Journal* (July 9, 1973): 5.

University Computing Corporation, write-offs of deferred R&D helped plummet the company \$175 million into the red.⁶³⁵ Many others could be added to this list, including but not limited to U.S. Industries, Anelex Corporation, Collin Radio, RCA, Ampex, Boise Cascade, I.T.T., Singer Co., Xerox, Allis-Chalmers, Scientific Data Systems, and Combustion Engineering.⁶³⁶ Even Control Data, which had apparently switched back to expensing, became desperate once more to make a good showing in its annual report and deferred \$28 million in R&D costs towards that end.⁶³⁷

That deferred R&D could make for an effective earnings-management device was generally ignored by much of the financial press throughout the 1960s, but when times got tough, as they did during the recessions of 1962 and 1970, the issue received relatively more coverage than normal. Thus, in April 1970, we encounter Heinz H. Biel's *Forbes* essay, "Beware!" in which he made the relationship between deferred R&D and earnings management explicit:

[N]owadays almost every annual report has to be scrutinized with meticulous care to discover what a company has actually earned. 'Generally accepted accounting principles' have given way to a permissiveness that is shocking.... Some corporations develop the habit of overstating their operating profits by capitalizing R&D expenditures, 'good will' on acquisitions, and even promotional expenses. This tends to make their company's stock move up in the market and may facilitate the raising of new money. But eventually the day of reckoning comes, and everything that should have been expensed when incurred is being charged against surplus at one full sweep.⁶³⁸

⁶³⁵ "Thinking Big: Profits in Computer Software Are Not What They Seem" *Barron's*, Vol. 47, No. 41 (October 9, 1967): 12; "The Heady Dreams at University Computing" *Business Week* (May 1, 1971): 55

⁶³⁶ In 1971 alone, Celanese wrote off \$82,200,000; I.T.T. wrote off \$70 million; RCA wrote off \$490 million; and Boise Cascade wrote off \$78 million.

⁶³⁷ Editorial, "How to Keep From Being Taken" *Forbes* (May 15, 1970): 226

⁶³⁸ Heinz H. Biel "Beware!" *Forbes*, Vol. 105, No. 7 (April 1, 1970): 77

And in the December 1962 issue of *Barron's*, we encounter an essay warning investors to keep an eye out for the practice and pointing to several firms that, to the author, were particularly guilty:

[C]onsiderable latitude exists in handling research and development costs. Companies can either write off such expenses as incurred, or defer them as did Chock Full O'Nuts—on the ground that they are more properly deductible when the work in progress bears fruit. Unfortunately, there is no assurance that research efforts will adequately repay development costs, a melancholy circumstance demonstrated, for example, by General Dynamics and Lockheed. Paramount, for its part, carries on the books a net balance (after tax credits) of \$4.9 million in development expenses incurred during 12 years of work on a color television tube. The amount, it says, will be amortized against future revenues from the tube, once it becomes commercially available. However, in other areas the movie maker has adopted a new accounting approach. It has begun to write off as they are incurred all development costs connected with the pay-TV system of its subsidiary, International Telemeter Co. Paramount also is amortizing over a 10-year period costs that had been deferred, the unamortized part of which at the 1961 year-end aggregated \$4.2 million. The effect of this change was to reduce 1961 earnings by \$1.1 million, or 68 cents per share. ... The roster of discretionary accounting practices available to management is extensive.⁶³⁹

Yes, even Chock-Full-of-Nuts and Paramount Pictures were “guilty” of deferring R&D.

Less surprising, perhaps, is the mention of General Dynamics (GD), parent company to Convair,⁶⁴⁰ or Lockheed, a favorite contractor of the military.

Back in 1958, GD management was feeling confident about the company's future. Sales to military buyers had left GD awash in earnings, and management believed that continuing modifications to its model 600 and 880 jets would allow the company to claim a sizeable chunk of the burgeoning commercial aircraft market for itself. By that time,

⁶³⁹ Steven S. Anreder, “Pitfalls for the Unwary,” *Barron's*, Vol. 42, No. 5 (December 25, 1962): p.3 & 8

⁶⁴⁰ General Dynamics acquired Convair in March 1953. The latter was itself the product of merger, one that took place in 1943 when Vultee Aircraft Company and Consolidated Aircraft Company combined to form Consolidated Vultee. Later known as Convair, this company was responsible for the Atlas missile, the F-102 and the F-106 interceptors, the B-36 strategic bomber, and the B-58 Hustler supersonic intercontinental nuclear bomber.

\$50 million of deferred R&D costs had already been written off, and in September, shareholders were reassured that although such write-offs were ongoing, they had already “passed their peak.” Meanwhile, as deferred costs from prior periods continued to be written off, the company made no change in its accounting policy. Another \$24,012,000 of R&D costs were deferred that year alone, making it possible for the company to report net earnings of \$36,729,113. In Spring of 1959, the company reported that income from sales had fallen below expectations; by mid-year, another \$18 million was written off; and by Fall, executives were delivering reassuring speeches to security analysts around the country.

At a meeting of the New York Society of Security Analysts, GD executives suggested that write-offs for its jet program were now (for the second time!) reaching their peak, and so long as sales picked up, the impact of such write-offs would ease up in 1960. The company, explained the GD board chairman, had overestimated the immediate market for sales on the medium-range airliners. Even so, all signs seemed to indicate that the 1960s would be banner years for the company. Halfway into 1960, however, the same executive would have to admit that the market for the 600 and 880 “had not come up to expectations.” By the end of 1961, GD had written off some \$425 million deferred R&D costs accumulated by its commercial jet program. This erased all profits earned that year from \$2 billion in sales and left the company \$27 million in the red. Unexpected problems had arisen in the development of the model 990, the latest iteration of the 600. The 990 was mired in ongoing engineering trouble and when it failed to achieve the expected speed of about 620 miles per hour, commercial customers were not pleased. Orders for the 990 were either scaled back or cancelled altogether, while orders for the 880 barely limped along.

By January 1962, the *Wall Street Journal* was reporting that the company had suffered “whopping losses despite its large sales of missiles, bombers, and Polaris-firing submarines,” while *Fortune* published a two-part exposé on “How a Great Corporation Got Out of Control.”⁶⁴¹ Said the *Fortune* writer,

Over the past two years General Dynamics has incurred the biggest product loss ever sustained by any company anywhere. ... As long ago as September, 1960, General Dynamics was announcing it had written off ‘all anticipated future [jet] losses,’ only to have fresh losses make a mockery of its cost projections. ... As a consequence, General Dynamics is being run under the corporate equivalent of martial law. An executive committee of seven directors took charge of the crisis last July; banks, alarmed because G.D.’s earned surplus had been nearly wiped out, moved in the next month with an armful of circumscriptions. ... What could have gone so spectacularly wrong?⁶⁴²

As the exposé made clear, executives had ignored engineering reports made as far back as 1957, showing how even if estimated sales of the company’s jets were to come to fruition, GD would nevertheless fail to recoup the amounts spent on R&D and supplies. More than that, there would be losses. In subsequent years, others in the company urged cancellation of the jet program, and these pleas, too, went unheard. Instead, as the *Fortune* writer suggests, the company “doubled down.” As a senior vice president put it in 1962, “It’s a grave question in my mind as to whether General Dynamics had the right to risk this kind of money belonging to the stockholders for the potential profit you could get out of it. All management has to take a certain risk for big gains. But I don’t think it’s right to risk so much for so small again.”⁶⁴³ The case of General Dynamics is just one instance in

⁶⁴¹ “General Dynamics Names R. Lewis Chief Executive,” *Wall Street Journal* (January 26, 1962); Richard Austin Smith “How a Great Corporation Got Out of Control, The Story of General Dynamics: Part I” *Fortune*, 65, no. 1 (January 1962); Richard Austin Smith “How a Great Corporation Got Out of Control, The Story of General Dynamics: Part II” *Fortune*, 65, No. 2 (February 1962).

⁶⁴² Smith (January 1962): 65-66.

⁶⁴³ Smith (February 1962): 187.

which freedom to defer R&D imparts a misguided sense of confidence about future prospects.

Like General Dynamics, Lockheed had its own share of financial and technical troubles at the start of the decade involving, among other things, moves into the commercial aircraft field with its Electra passenger jet. These challenges had made deferral an especially attractive option at a time when the company was struggling, but by the mid-1960s, Lockheed was in a much stronger position and it seemed as if the tide had turned. By the end of the decade, though, Lockheed would suffer a spectacular fall from glory that few could have seen coming. The role of deferred R&D in that fall makes this case particularly worth considering in some detail.

6.6. The Case of Lockheed Aircraft

In the 1950s and early 1960s, Lockheed thrived on billions of dollars' worth of R&D and production contracts for its Polaris strategic rocket, C-130 Hercules turboprop troop carrier, F-104 jet interceptor, Agena space vehicle, and the C-141 Starlifter military jet transport. By August 1964, however, *Fortune* was reporting that the company's future prospects in the defense contracting area were "highly iffy" at best, and that "already Lockheed's engineering load has begun to lighten," with nearly 10,000 R&D personnel dropped from payroll.⁶⁴⁴ The following year, things seemed like they were picking up when, in late 1965, Lockheed signed a \$1.9 billion Air Force contract for the C-5A Galaxy cargo-carrier, and in 1966, it received an R&D contract from the Army that led

⁶⁴⁴ Charles J. V. Murphy, "The Defense Industry is Facing Trouble" *Fortune*, Vol. 70, No. 2 (August 1964): 206

to an \$875 million production contract for the Cheyenne helicopter. Neither aircraft did the company much good, however, and in 1968, Lockheed began work on a new commercial jet that executives hoped would make the company less reliant on government purchase orders and the hazards of contract renegotiation settlements.

Known as the L-1011 Tristar, this new passenger jet would be equipped with an RB.211, a state-of-the-art engine to be designed and built by Rolls Royce. At the time, this was the largest aerospace export order ever received by a British firm, and when the news broke that Rolls Royce had won its bid to Lockheed, the British government rejoiced at the potential impact this deal would have on the nation's otherwise-dismal balance of payments.⁶⁴⁵ With fanfare on both sides of the Atlantic, Lockheed investors may easily have overlooked details in the 1968 annual report which hinted that management may have been risking more than it could handle. In the auditor's statement on the annual report, the firm of Arthur Young & Co. wrote: "In our opinion, subject to the realization of the works-in-process inventories and accounts receivable described in Note 2, the statements mentioned [in the financial statements prepared by the company] present fairly...except for the change in accounting for administrative and general expenses and independent research and development costs described in Note 1."

Note 2 involved the recovery of costs associated with the C-5A program, the estimates of which were under dispute at the Pentagon. Management reassured investors

⁶⁴⁵ Between 1960 and 1964 alone, the balance of payments deficit ran to about £1.15 billion. The deficit continued to expand in subsequent years, culminating in the devaluation of the pound sterling in 1967. Lockheed awarding the engine contract to Rolls Royce led Queen Elizabeth to knight David Patrick Huddie, the man who brokered the deal, for "services to export." Huddie was managing director of the AeroEngine Division, having joined Rolls Royce as far back as 1939. In 1967, Huddie and his family left England to take up residence in New York, where Huddie could concentrate full-time on selling the RB.211 to Lockheed executives.

the dispute would be settled in the company's favor; so long as its cost estimates were accepted by the Pentagon, the company expected to suffer no losses on the C-5A. In the aerospace industry, losses on contracts occurred frequently enough to be reported and discussed by the financial press. By contrast, the implications in Note 1 may have been harder for investors to decipher. Indeed, the average investor of the 1960s was wont to read the company's bottom line, i.e., its earnings-per-share figure, and little else. This group of investors was especially unlikely to catch the disclosure in Note 1 of an important change in the way the company accounted for R&D.

It seems that, at least in the years immediately prior to 1968, Lockheed expensed as incurred R&D costs not specifically provided for by contracts already in hand. Such R&D was known by the Pentagon as IR&D, or independent R&D, and for many years, considerable controversy attended the question of whether or not government should reimburse IR&D costs. Difficult as it was for contracting officers to distinguish R&D performed for a contract from IR&D, companies sometimes got away with charging IR&D costs to the government. Whether IR&D costs were considered "allowable" (i.e., reimbursable) was so controversial that many firms would not commit themselves to disclosing these figures publicly. In an article published two years later, *Forbes* reported that 60% of firms surveyed refused to disclose the amounts spent on R&D; were they to do otherwise, suggested *Forbes*, defense contractors could very well hurt their chances to offload these costs on government.⁶⁴⁶ As of 1968, possibly because of greater tolerance for the practice in the Pentagon, Lockheed no longer charged IR&D to income as incurred, but to contracts in progress. The change marked a complete abandonment of current

⁶⁴⁶ "The Missing Dimension" *Forbes* (May 15, 1970)

expensing in favor of deferring all R&D costs, whether incurred on contract or not.⁶⁴⁷ According to *Forbes*, this policy change alone “increased earnings by \$22 million, out of a total of \$44 million” reported in 1968. “Without the change,” explained a writer at *Fortune*, “Lockheed’s profits would have been the company’s lowest since 1960, when it suffered a loss of \$42,900,000 because of write-offs associated with the turboprop [commercial passenger plane] Electra.”⁶⁴⁸ If the company couldn’t collect its outstanding claims to the government—amounting to \$140 million for the C5-A Galaxy, alone—this could very well place Lockheed on the brink of ruin.

An unexpected turn of events came in March 1969 when one of the company’s Cheyenne helicopters crashed and killed a test pilot, prompting the Army to cancel all orders for the Cheyenne that May. At the time of the crash, Lockheed’s books were already showing short-term debt of \$135 million. The company had been expecting to earn \$177 million in profit from the Galaxy, but by mid-1969, Houghton was estimating a \$13 million loss. On the basis of its own estimates, the Air Force put that figure at \$285 million. By the time the 1969 report was issued, the company was showing \$19.5 million

⁶⁴⁷ Interestingly, both President A. Carl Kotchian and board Chairman Daniel Haughton started out as accountants before rising up the ranks to become chief executives at Lockheed. (See, “For Lockheed, Everything’s Coming Up Unk-Unks” *Fortune* (August 1969): 81) Prior to joining the company, Kotchian had worked at Price, Waterhouse from 1936 to 1940. In 1976, Kotchian testified before the Senate that he had successfully bribed Japanese government officials and businessmen to purchase 21 of his company’s Tristar aircraft—a confession that led to the conviction in Japan of Prime Minister Kakuei Tanaka and other high-ranking public officials. Kotchian also testified to bribing a Dutch official in the early 1960s with a \$1.1 million bribe; the official later was later identified as Prince Bernhard, inspector general of the Dutch military and husband of Queen Juliana; as a result, Prince Bernhard stepped down from his role in the military. As for Kotchian’s own fate, the board pointed to \$38 million in “questionable payments” in ousting him from the company in March 1976. A year later, the *New York Times* quoted Kotchian as saying “Lockheed has become the scapegoat for 300 companies that the S.E.C. said were doing the same thing, and Haughton and I are the scapegoats for the scapegoat.... Some call it gratuities. Some call them questionable payments. Some call it extortion. Some call it grease. Some call it bribery. I look at these payments as necessary to sell a product. I never felt I was doing anything wrong”(July 3, 1977, “Kotchian Calls Himself the Scapegoat”)

⁶⁴⁸ “For Lockheed, Everything’s Coming Up Unk-Unks” *Fortune* (August 1969): 78

in losses. From the same report, investors learned that provisions for future losses on contracts had been set aside in a reserve totaling \$150 million. Anyone who paid the footnotes any mind would have further learned that, in the company's own estimate, ultimate losses on contracts could amount to \$500 million before taxes. To those who overlooked the warnings signs in 1968, the company's 1969 annual report must have come as a shock. In the 1969 report, said *Forbes*, "the full magnitude of the disaster started to unfold. The report took on a funeral aspect."⁶⁴⁹ Wrote the company's auditors: "the company is faced with contingencies of extraordinary magnitude arising from disputes with, and claims against, the U.S. Government as well as uncertainty as to its commercial Tristar program. These items are material to both the financial position and the results of the operations of the company, and their resolution may significantly affect its future."⁶⁵⁰

With development of the Tristar underway and the possibility of winning an appeal on the cancelled Cheyenne contract, Lockheed may have believed it could still keep its head above water without changing any of its strategy. In August 1969, *Fortune* reported the following:

If the army's cancellation for "default of contract" stands up, Lockheed will have to return at least part of \$54 million in progress payments, and could suffer additional out-of-pocket losses of perhaps as much as \$68 million from unreimbursed costs. The company contends, however, that the Cheyenne met 90 percent of the performance specifications and therefore the contract was not in default. ...says [board chairman Daniel] Houghton: "If they wanted to terminate, they should have terminated for convenience, not default. My goodness! You don't make a quantum jump in technology without encountering problems."⁶⁵¹

⁶⁴⁹ "How to Keep From Being Taken" *Forbes* (May 15, 1970): 225

⁶⁵⁰ As quoted in "How to Keep From Being Taken" *Forbes* (May 15, 1970): 225

⁶⁵¹ "For Lockheed, Everything's Coming Up Unk-Unks" *Fortune* (August 1969): 78

The chairman's point was a valid one: encountering unexpected problems is part and parcel of R&D. However, Lockheed executives refused to admit that the very same contingency and uncertainty inherent to R&D made the anticipation of related profits an especially hazardous gamble. Perhaps Lockheed's executives believed the U.S. government would come to their rescue, should the billions poured into R&D not pan out as hoped. In fact, this is exactly what happened.

In March 1970, Houghton pressed Deputy Defense Secretary David Packard for help. Lockheed, he insisted, would either need interim financing to the tune of \$600 million in federal loan guarantees, or else its disputed contracts on the Cheyenne and the Galaxy, as well as the motor of the SRAM and a host of ships for the Navy would need to be settled immediately. More than \$750 million was at stake in these four contracts, but ongoing disputes over cost estimates postponed the reaching of an agreement between the Department of Defense and Lockheed for nearly another year.⁶⁵² Having already reported a \$10 million profit for 1970, Lockheed was forced to revise this number following a contract settlement begrudgingly agreed to by Houghton in February 1971. As a result of

⁶⁵² Appointed to the post of Deputy Defense Secretary by President Nixon, David Packard left Hewlett-Packard, the company he co-founded and led as CEO, in order to join the Pentagon and help mend the fractured agency in the wake of Robert McNamara's strong-armed policies. When McNamara himself left Ford Motor Company to serve the Kennedy Administration as Secretary of Defense, he brought with him the centralized cost-control techniques that served him well at Ford. These techniques entered into the redesign of R&D and procurement contracts and promised to save the nation money that, in McNamara's view, was otherwise wasted by letting the Service Chiefs dictate which weapons they needed to solve tactical problems. Not long after entering office, Packard's critiques of the state of affairs left by McNamara and his Whiz Kids were published in *Fortune*: "We are designing and building weapons that are too complex, and therefore too costly. We further compound the problem by trying to produce hardware before it is fully developed," Packard is quoted as saying. "Meanwhile," continued *Fortune*, "the Defense Department is already introducing a number of promising techniques to discourage the practices Packard has identified. Among them: tying the start of production to the achievement of research and development 'milestones,' relying more on prototypes of new equipment instead of paper plans, and requiring the services to save money on one project if they add to the costs of another"(Editorial, "It Is Time to Audit the Defense Department" *Fortune* (August 1, 1969): 62).

this settlement, Lockheed's \$10 million profit for 1970 was wiped out and in its place stood a \$86.3 million loss.

The punches, as they say, kept on coming. Having finalized the disappointing settlement with the military, Houghton flew to England the very next day to check on Rolls Royce's progress on the RB.211 engine. Having the engine perfected and on schedule was critical to Lockheed, for without the engine, there would be no Tristar, and on the Tristar rested Lockheed's hopes of returning to the commercial aircraft business after nearly a decade spent catering to the military. Upon arrival, Houghton received shattering news. He had been aware of the enormous cost overruns and possible delays on the RB.211, but expected that Parliament would bail out Rolls Royce as it had multiple times in the past. A new prime minister had entered office, however, and having bailed out the company once since taking office, he now decided to put his foot down. British taxpayers would not be burdened by yet another aid package for the ailing company. Already the Tristar project had absorbed more than \$1.7 billion of American and British resources. Lockheed itself had spent \$990 million, its subcontractors another \$350 million, and Rolls Royce and its subcontractors \$400 million.⁶⁵³ Unable to draw any more cash from Parliament, Rolls Royce almost immediately thrown into receivership—just one week before Houghton's arrival, in fact. Apparently, the same earnings management evidenced by Lockheed had been company policy at Rolls Royce for the last decade.

⁶⁵³ *Fortune* (June 1971): 68

The trouble at Rolls Royce began in the late 1950s when customers for its products were in short supply. A passage from an essay in *Fortune* describes what happened in the ensuing years:

By 1961, though, the company's problems were multiplying dangerously. It had been passed over for two major government contracts.... To avoid showing the first loss in its history—of nearly \$5 million for the year—Pearson [head of the company] and his financial staff decided on a critical change in accounting policy. They deferred part of the development costs of new engines such as the Spey, with the costs to be amortized later as the engines were delivered and paid for. The change permitted a book profit of \$7 million for 1961. In 1962, an even worse year, the new accounting system permitted a book profit of almost \$5 million. The asset column of the company's books at the end of 1967 showed some \$20 million carried as deferred costs, and they have climbed appreciably since then.⁶⁵⁴

By May 1969, when the above passage was written, an increasing number of companies were starting to reveal the painful effects of deferral policies that had failed to live up to expectations. That same *Fortune* essay offered the following, perceptive observation:

While the change in accounting has worked well so far, it has also added a distinct new risk to the company's operations. Unexpected costs, canceled orders, strikes, and the like could make eventual amortization payments extremely inconvenient, to say the least. Something like that happened to Douglas Aircraft in 1966-67 with much larger deferred write-offs on the DC-9, and eventually the company had to merge with McDonnell to survive.⁶⁵⁵

Extremely inconvenient, indeed. Like Rolls Royce, Lockheed was now headed for bankruptcy. Unlike Rolls Royce, government would step in to clean up the mess.

In the Summer of 1971, US Vice President Spiro Agnew stepped into the halls of Congress to cast a tie-breaking vote authorizing \$250 million in federal loans to help save the aerospace company. This is where our Lockheed case study comes to an end, but a

⁶⁵⁴ *Fortune*, March 1969, 127

⁶⁵⁵ *Fortune*, March 1969, 127

word of warning: the point to be made by this exercise is *not* that deferred R&D triggered a series of events leading to what Senator William Proxmire called an act of “corporate welfare.”⁶⁵⁶ Instead, what Lockheed’s experience demonstrates—no less than any of the other cases we’ve seen—is that R&D deferral policies were usually, if not always, adopted as a means of manipulating earnings in times of financial hardship. This is a far cry from the abstract universe of dominant accounting theory in which firms use deferrals only in the pursuit of truth, only to satisfy the Platonic ideal that is the matching principle. That deferrals could be used to accommodate some of the worst of management practices, simply did not register for many theorists.

A few who took the time to study the issue were frustrated by how the application of their beloved matching principle to R&D seemed to be misleading investors in the 1960s. Warren Kantor, for example, quoted Paton as saying, “The revenues of a particular period should be charged with the costs which are embodied in or associated with the product represented by such revenues,” before proceeding ever so delicately to suggest its limitations:

The primary purpose of such phrasing is clear on their face; their effectuation in practice is, as usual, attended by many difficulties. They connote that that there must be a determination of revenues properly allocable to a period, and that having done so, there must be a corresponding allocation to the same period of all costs which properly attach to the earning of the revenues with which the period has been credited. Neither the revenue side nor the cost side is always easy of measurement; but it is asserted, as an important rule, that both sides must be governed by the same conditions and measurement. There can be little dispute about accepted these ideas as desirable basic concepts. The problem comes when

⁶⁵⁶ “While the Lockheed bailout was the largest and best-known instance of direct government intervention to save a military contractor, subsequent journalistic investigations turned up numerous, previously unrevealed (and now largely forgotten) episodes in which the military [next] acted to save companies that supplied it” (Ledbetter 2011, 180-181). Lockheed (C-5A Cargo Plane & Army’s Cheyenne helicopter), Grumman (Navy’s F-14 fighter plane), and Litton (Navy’s DD963 and LHA Surface Ships) all had either large govt bailouts or subsidized loans

one tries to follow the principle and put it into practice. ... It's time that the profession tried to set up these guidelines [for how to apply matching to R&D] so that accounting statements will present more fairly the position of its clients.⁶⁵⁷

Matching, to Kantor, qualified as “good accounting theory.” To him, it represented an ideal that's very hard to achieve in practice but still worth striving for. This suggests that the matching principle may be useful for heuristic purposes—*not* as a tool to justify unseemly accounting practices. This is true as far as it goes, but unfortunately, it does not go far enough. As the next section will show, many of those who urged the FASB to adopt an accounting standard for R&D inclusive of a deferral option, did so by pointing to the matching principle as a self-evident truth. Rather than clarifying accountants' and managements' thinking with respect to R&D accounting, the matching principle simply validated the sort of financial chicanery that brought the 1960s to such a dramatic and pathetic end.

⁶⁵⁷ William Kantor “Research and Development Costs: Should They Be Expensed or Capitalized?” *Economics & Business Bulletin of the School of Business Administration*, Vol. 18, No. 3 (March 1966): 15-9.

Chapter 7

A Stand(ard) Against the “Measure of Our Ignorance”

7.1. A Failure of Standard Setters

Though alluded to over the years, the possibility of establishing criteria for R&D inspired very little discussion among accounting leadership until the early 1970s. It was then that, for the first time, a Federally-recognized, private sector organization of standard-setters zeroed in on R&D accounting alternatives. After decades of deadlock and inaction among accounting leadership, first in the Committee on Accounting Procedure (CAP, 1939-1959) and then in its successor, the Accounting Principles Board (APB, 1959-1973), establishing a clear standard for R&D accounting was finally placed on the profession’s agenda with the creation of the Financial Accounting Standard Board (FASB, 1973-present).

Unlike its predecessors, the FASB found its home in Stamford, Connecticut, physically removed from the professional organizations and public accounting firms that dominated professional practice in nearby New York City. Through a revision made in May 1973 to its Code of Professional Ethics, the AICPA recognized the FASB as the authoritative accounting standard setter for the profession.⁶⁵⁸ This adjustment was a

⁶⁵⁸ The problem of enforcing accounting standards became the center a debate at one of a series of conferences organized in the late 1960s, a time when as financial reporting scandals involving the auditing, financial and business sectors were rapidly growing out of control. Convened in Seaview, New Jersey under the auspices of the AICPA, Financial Analysts Federation, the Financial Executives Institute (FEI), and the Robert Morris Associates, these conferences came to be known as the “Seaview Symposia.” The first Seaview Symposium on Financial Reporting was held in 1968, its proceedings subsequently published as *Corporate Financial Reporting: Conflicts and Challenges* (1969). Proceedings from the Seaview Symposium of fall 1970 bore the name, *Corporate Financial Reporting: Ethical and Other Problems*. In an address delivered before the 40th annual meeting of the New York State Society of CPAs in June 1973, the chairman of the New York

necessary step, of course, but it would leave the FASB as toothless as its predecessors unless legal weight could be brought to bear on those accountants who deviated from prescribed practices. Recognizing as much, the SEC issued Accounting Series Release No. 150 in December, stating that FASB pronouncements would be considered “substantial authoritative support” for accounting practices and that any departures therefrom would be seen as misleading the users of financial statements. Many, including the SEC Chief Accountant himself, John S. Burton, were so disillusioned by the failures of the CAP and APB that they could hardly refrain from expressing their doubts about the new organization. Since the founding of the CAP in 1939, the profession had grown more jaded with each failed attempt to reduce the range of accounting alternatives that resulted in like situations receiving unlike treatment.⁶⁵⁹

Although in many ways the Great Depression had ripped the spine out from the profession,⁶⁶⁰ the creation of the CAP had managed to inspire some confidence that accountancy could get its act together and effect real progress. Carman Blough, for instance, shared the profession’s hope in the newly-formed CAP and that Committee’s power to issue decisive, substantive opinions. “It is believed,” observed Blough, “that

Stock Exchange, John J. Needham, referred to the proceedings of the 1970 Seaview Symposium as having “indicated a feeling on the part of some participants that an institutional vehicle of some sort was needed to address this problem” (“Current Financial Reporting Issues” *The CPA Journal*, Vol. 43, No. 8, August 1973, 659).

⁶⁵⁹ Frustrations had built over time, as “the persistent unwillingness of the CAP to make difficult choices on controversial topics” came to the surface (Zeff 2011, “The Work of the...” *Accounting Historians Journal*, 148)

⁶⁶⁰ Editorial “A Nervous Profession” *The Accounting Review*, Vol. 9, No. 4 (December 1934): 334; Editorial “Standards Must Come” *The Accounting Review*, Vol. 9, No. 4 (December 1934): 334-6; Carman G. Blough “Some Accounting Problems of the Securities and Exchange Commission” Address Before the New York State Society of Certified Public Accountants (January 11, 1937); William W. Wertz “Some Current Problems in Accounting” *The Accounting Review*, Vol. 14, No. 2 (June 1939): 117-126; D.R. Scott “Responsibilities of Accountants in a Changing Economy” *Accounting Review*, Vol. 14, No. 1 (December 1939): 396-401; Diane Robert “Changing Legitimacy Narratives About Professional Ethics and Independence in the 1930’s *Journal of Accountancy*,” *Accounting Historians Journal*, Vol. 37, No. 2 (December 2010): 95-122.

these expressions will help materially in the development of standards for the profession on various matters now seriously in controversy among its members. ... It is hoped and anticipated that the results of the work of this committee will do much to meet the criticisms that have been directed at the profession for lack of uniformity.”⁶⁶¹ Twenty years later, the same hopes, and the same disappointments, followed the creation of the APB.⁶⁶²

For more than a decade, public accountants divided over the “procrustean bed” of uniformity and the “Gresham’s law” of flexibility, but this debate did not happen in a vacuum. The Board’s botched handling of the Investment Tax Credit marked the first of several controversial opinions which, when subject to challenge by the industries effected (such as the insurance, banking, real estate, etc.), were either modified, ignored, or else made moot. Where industry groups did not succeed in pressuring the APB directly, they engaged in massive public relations campaigns challenging its authority. At other times, they headed to Washington and lobbied Congress, or else freely flaunted APB opinions without consequence, being under no obligation to adopt the recommended changes in procedure. The APB’s failure to resist outside pressure smacked of corruption, and the business press had no qualms emphasizing the profession’s bias towards the client-managements they served. That accountants ought to defer to the judgment of their clients was unproblematic to AICPA leadership; nevertheless, the publicity was most

⁶⁶¹ Blough 1939, 278

⁶⁶² In a retrospective published by Arthur Anderson & Company, the firm noted that, “In spite of the considerable optimism over the Accounting Principles Board, it soon became apparent that the Board was subject to the same weaknesses as were inherent in its predecessor”(*The First Sixty Years, 1913-1973*, 1974, 116). See also Robert K. Mautz “Accounting Principles – How Can They Be Made More Authoritative?” *The CPA Journal*, Vol. 43, No. 3 (March 1973): 185-192.

unwelcome, and over the course of the 1960s, the air of scandal around accountancy not only failed to dissipate but became cloudier, more involved and undeniable.

With larger numbers of Americans entering the stock market for the first time, those companies hungering for equity capital became fixated on improving their quarterly earnings reports. For less-conservatively run firms, the range of accounting alternatives offered managers the tools to show growth in earnings where there was none. Eventually, this trend resulted in corporate scandals and investor lawsuits that targeted managers as well as—to their surprise—the auditors who attested to the fairness and accuracy of their clients’ representations. Among the more infamous cases were those involving the Yale Express, Westec Corporation, Continental Vending, National Student Marketing, Four Seasons Nursing Homes, King Resources, Black Watch Farms, American Express, Mill Factors, R. Hoe, Penn Central, Performance Systems, Revenue Properties and Commonwealth United—all of which brought embarrassment to the accounting elites.⁶⁶³

By October 1966, with more than fifty lawsuits pending against Big Eight accounting firms, *Forbes* sought out the opinion of Leonard Spacek on Westec’s widely-reported “now-you-see-‘em-now-you-don’t”⁶⁶⁴ earnings manipulation. Spacek, though, was reluctant to point the finger at Ernst & Ernst, the Westec auditors. “We accountants,” he told the magazine, “should look at this situation and say: ‘There but for the Grace of God go I.’ All Ernst & Ernst did was to allow the use of a wide variety of accounting principles available to them.” When asked whether he believed the publicity

⁶⁶³ For a defense of accountants written by the general counsel of Peat, Marwick, Mitchell, see Victor M. Earle, III, “Accountants on Trial in a Theatre of the Absurd,” *Fortune*, Vol. 85, No. 5 (May 1972): 227-8.

⁶⁶⁴ “An Accountant Speaks Up,” *Forbes* (October 1, 1966), 22.

over the Westec scandal would bring about the kinds of sweeping reforms that Arthur Andersen & Co. had long advocated, Spacek shook his head: “No, not unless the public demands it, as they did of the auto companies over the safety issue. . . . We can holler all we want that the brakes aren’t any good, but unless there is an accident and someone insists we fix them there isn’t much we, as an individual firm, can do.”⁶⁶⁵ The point was not lost on *Forbes* editors, nor its readership.⁶⁶⁶

Opening the next issue of *Forbes* was an editorial titled, “Unaccountable CPAs,” which took the accounting leadership to task. *Forbes* editors had taken it upon themselves to use their platform and do what Spacek had suggested: to respond to the car crash that was Westec and Yale Express before it by insisting, as a voice of the public, that the profession’s “brakes”—i.e., its “generally accepted accounting principles”—get fixed:

It’s past time certified public accountants were called to account for practices that are so loose that they can be used to conceal rather than reveal a company’s true financial picture. The owners of public companies and the analysts who recommend purchase or sale of their securities used to think they could rely on the honesty of financial statements certified by reputable outside auditing firms. But in some very spectacular situations, it has turned out that such certification was not of the value or meaning or importance that the public thought. All these certifications usually bear the phrase: ‘according to generally accepted accounting principles,’ a phrase which is now coming to be generally accepted as damned meaningless.⁶⁶⁷

⁶⁶⁵ “An Accountant Speaks Up,” *Forbes* (October 1, 1966), 22.

⁶⁶⁶ Two letters to the editor responding to the October 1st essay were printed in the next issue of *Forbes*, one from a reader in California, the other from Ohio. “In the field of financial accounting Mr. Spacek stands out like a shining beacon of light on a foggy night,” said the first. The “shareholder deserves a better fate than that of being treated like a third-class citizen. It is high time that the accounting profession, financial analysts and shareholder groups shake off their apathy and provide some meaningful support for Leonard Spacek.” These same sentiments came through in the second letter: “More power to accountant Spacek and his principles of accounting. Few things will undermine public confidence more than losses caused by loosely prepared financial statements which may be misleading. Neglect of these principles is an invitation to misinformation.”

⁶⁶⁷ Editorial, “Unaccountable CPAs” *Forbes*, Vol. 98, No. 8 (October 16, 1966), 15.

AICPA leadership was incensed by the editorial. In the November 1st issue of *Forbes* appeared a response from John L. Carey, AICPA executive director, who questioned the editorial's call for "accounting standards that will be standard." "If this means strait-jacket rigidity," wrote Carey, "the result would be undesirable because business, composed of widely diverse units, would be forced into a Procrustean bed. If it means making like things look alike and unlike things different, the entire accounting profession agrees fully with your point, and the Accounting Principles Board of the AICPA is currently pressing forward at an accelerated pace toward the very goal you suggest."⁶⁶⁸

By then, Paul Grady had already shared his thoughts on the *Forbes* editorial in a speech at the University of Illinois on October 20th. Barely masking his contempt for Spacek, Grady declared that, "it should not be surprising," he insisted, "that impatient, irresponsible, and sometimes misguided critics of the APB and of the accounting profession have found it easy to make spectacular headlines by proposing uniformity in accounting, to a degree not compatible with the 'facts of life' in our economy, together with authoritarian measures in the development and enforcement thereof."⁶⁶⁹ At the time, Grady's views were still dominant among the AICPA leadership, but as we saw towards the end of Chapter 5, the number of vocal dissenters was already on the rise. "Now the ranks of skeptics are swelling," reported *Forbes* in May 1967. As a result of a sudden "barrage of public criticism" and "thinly disguised threats [of] unilateral action" by the SEC, the APB "has finally swung into high gear with a batch of new opinions during the past six months, after years of dragging its heels." Sprinkled throughout the

⁶⁶⁸ John L. Carey, Letter to the Editor, *Forbes* (November 1, 1966)

⁶⁶⁹ As quoted in Elliott L. Slocum & Teresa T. King's "Paul F. Grady and the Debate on the Authority of the APB" *The Accounting Historians Notebook*, Vol. 6, No. 2 (Fall 1993), 8

essay were quotes that help to reveal the tremendous disorder and disagreement surrounding the profession at the time:

“We have the gun to our back today.” - Joseph P. Cummings, partner at Peat, Marwick & Co.

“There can never be one set of inflexible rules in the name of uniformity. Companies that appear on the surface to be the same may actually operate much differently.” – John W. Queenan, managing partner at Haskins & Sells.

“If you look at the opinion of the Accounting Principles Board, you will rarely ever see the investor’s point of view established. ... My brethren say, just tell the investors what the facts are and let them be on their guard. I say ‘booby trap.’ We cannot just present statements to investors and say, ‘You ought to be able to find the booby traps.’” – Leonard Spacek, chairman of Arthur Andersen & Co.

“A single accounting treatment applied to differing facts and circumstances can produce a lack of comparability just as surely as diverse accounting treatments applied to similar facts and circumstances.” – Herman W. Bevis, head partner at Price Waterhouse & Co.

“Watchdogs can’t afford to be accommodating.” – Jacob S. Seidman, senior partner at Seidman & Seidman.

“You don’t have to be in our end of the securities business very long before you realize that what companies call their earnings can be almost anything they want them to be.” – unnamed head of a leading Wall Street investment firm.

“Give me the books of almost any company and within a year’s time I can double the earnings.” – unnamed partner at one of the Big Eight auditing firms.⁶⁷⁰

“In short,” added *Forbes*, “today’s highly flexible accounting practices have grown up over the years to accommodate the needs of managements.”⁶⁷¹ A year later, the only thing that had changed was an explosion of new shareholder lawsuits.⁶⁷² In the June 15, 1968 issue

⁶⁷⁰ “What Are Earnings? The Growing Credibility Gap” *Forbes* (May 15, 1967), 28-39.

⁶⁷¹ “What Are Earnings? The Growing Credibility Gap” *Forbes* (May 15, 1967), 29.

⁶⁷² In the Summer of 1968, Arthur M. Louis observed that “the board has made very little headway toward formulating a consistent set of accounting principles whose meaning and application are unmistakable. Companies still have considerable latitude in deciding how they will account for almost every item on the balance sheet or income statement.” (Arthur M. Louis, “The Accountants are Changing the Rules” *Fortune*, June 15, 1968, p177)

of *Fortune* magazine, Arthur M. Louis reported that one law firm in New York was then handling forty malpractice suits and estimated that the majority were filed within the past year or so. Several of the Big Eight firms were defendants in more than one case, and as a result, companies offering liability insurance to these firms had begun “raising their rates, or even refusing to renew.”⁶⁷³ “And this,” observed Jacob Seidman, “is in the face of prosperity. If we were to undergo a broad economic decline, in which the public experienced great losses, this would snap back on the accountants. What happened in the last economic depression, in which bankers were driven from the temple, could happen to us, and with some justification.”⁶⁷⁴ In less than two years’ time, Seidman’s warning proved to be prophetic.

Despite increasing public outrage over business failures and abuses of the kind discussed in Chapter 6, and despite blistering media coverage that eventually spilled over into mainstream publications such as *Newsweek* and *Time* magazine, the AICPA continued kowtowing to those managements who had spent the decade latching onto every earnings-boosting accounting method available to them. One of the most common of these methods was the option to defer rather than expense R&D.⁶⁷⁵ To be clear, not all firms with R&D programs used this option. Firms such as Du Pont, IBM, Eastman Kodak, Corning Glass, and Union Carbide—all of which had already accumulated

⁶⁷³ Arthur M. Louis, “The Accountants are Changing the Rules” *Fortune* (June 15, 1968), 177.

⁶⁷⁴ Arthur M. Louis, “The Accountants are Changing the Rules” *Fortune* (June 15, 1968), 177.

⁶⁷⁵ Along with deferring or capitalizing R&D, the other most common method to inflate earnings was to switch from an accelerated to a straight-line basis for charging depreciation in order to offset undesirable effects on earnings. See “What are Earnings? The Growing Credibility Gap” *Forbes* (May 15, 1967), 39. Other methods include postponing the reporting of losses to the most advantageous fiscal year and, especially in the latter half of the 1960s, use of the “pooling of interest” method of accounting for mergers and acquisitions, among others. See: Allen H. Seed, III “The Rational Abuse of Accounting Information,” *Management Accounting*, Vol. 51, No. 7 (January 1970), 9-10.

decades of experience with company-sponsored R&D—were disinclined to join their more glamorous counterparts in using the deferral method.⁶⁷⁶ In fact, if an essay published in Winter 1969 by Harvard professor David F. Hawkins is any indication, “Few companies permit divisional managers to capitalize research and development costs, principally because this ‘tends to let them off the hook.’ If they were permitted to capitalize research and development the managers might be less inclined not to terminate projects with poor prospects of success because their costs could be kept out of the income statement.”⁶⁷⁷ Were managers allowed to defer R&D at whim, Hawkins explained, they could get away with postponing those costs indefinitely so as to keep losses from failed projects out of the income statement. And yet, added Hawkins, “in the case of external reporting, it is often those companies who are in trouble and whose research projects are the most dubious that try to capitalize these costs.”⁶⁷⁸

Shortly after Hawkins’ essay went to print, the economy fell into a recession large enough to bring investors’ space-age enthusiasm to a frustrated and most unglamorous end. Firms that had once benefitted from deferring R&D and other legal methods of earnings manipulation, were now feeling the consequences of those decisions. Many of these methods, *Forbes* wrote solemnly in May 1970, “amounted to nothing more than borrowing from future earnings.”⁶⁷⁹ As a result, many financial statements had become “outright deceptive. Only a minority are truly frank and honest.”⁶⁸⁰ And this, added *Forbes*, “despite some 15 ‘Opinions’ handed down by the [APB] in the last eight years,

⁶⁷⁶ “How to Keep From Being Taken,” *Forbes* (May 15, 1970), 231

⁶⁷⁷ David F. Hawkins, “Behavioral Implications of Generally Accepted Accounting Principles,” *California Management Review*, Vol. 12, No. 2 (Winter 1969), 20

⁶⁷⁸ Hawkins (Winter 1969), 20

⁶⁷⁹ “How to Keep From Being Taken,” *Forbes* (May 15, 1970),

⁶⁸⁰ “How to Keep From Being Taken,” *Forbes* (May 15, 1970).

and despite rulings promulgated by the Securities & Exchange Commission, and despite so-called full disclosure by management. The annual report has fallen to the lowest ebb of confidence it has reached in many years.”⁶⁸¹ With stock prices collapsing and investors abandoning the market, managers who were already facing rapprochement began writing off massive losses that shook the investment community down to its toes.⁶⁸² This action itself became known as “taking the big bath,” and the business press devoted countless articles to dissecting this phenomenon. Were accountants to be “driven from the temple,” as Seidman had warned before the market collapse?

By August 1970, *Fortune* featured an editorial titled, “It’s Time to Call the Auditors to Account,”⁶⁸³ and in mid-November, almost as if responding to this call, three of the Big Eight accounting firms threw down the gauntlet.⁶⁸⁴ Harvey E. Kapnick, chairman of Arthur Andersen & Co., Robert Trueblood, chairman of the policy group at Touche, Ross & Co., and Ralph E. Kent, managing partner at Arthur Young & Co., each sent separate letters to the new AICPA President, Marshall S. Armstrong, in which they threatened to withdraw their support for the APB and to reject any further pronouncements issued by the defunct organization. Armstrong’s response to these letters

⁶⁸¹ “How to Keep From Being Taken,” *Forbes* (May 15, 1970). Also see: Samuel W. Baker, “The Credibility Gap in Financial Communications” *Management Accounting*, Vol. 51, No. 3 (September 1969).

⁶⁸² For an insightful discussion of the sudden write-offs, see Alfred M. King, *Management Accounting* (March 1975)

⁶⁸³ “Much of the business and financial community, including corporate managements, boards of directors, security analysts, and the press, has until early this year been swept along in the general go-go mood, hypnotized by p/e’s [ie, price/earnings] and earnings per share to the neglect of less dazzling corporate data. ... Nevertheless, it is clear that the auditors have failed to assert their all-important independence, and to insist that company figures not be used to obfuscate the true picture”(Editorial, “It’s Time to Call Auditors to Account,” *Fortune*, Vol. 82, No. 2, August 1970, 98).

⁶⁸⁴ Of course, an essay in *Fortune* did not suddenly compel these three firms to act. While Kapnick, Trueblood, and Kent had been critical of the APB’s performance in general, the straw that broke the camel’s back seems to have been the APB’s botched handling of the two latest opinions released by the board: Accounting Principles Board Opinion No. 16, *Accounting for Business Combinations* (i.e., mergers) and Accounting Principles Board Opinion No. 17, *Accounting for Intangible Assets* (exclusive of R&D).

was decisive, marking the start of a series of events culminating in the demise of the APB and the birth of the Financial Accounting Standards Board.

7.2. As the FASB Takes Shape

Acting quickly, Armstrong called for a meeting of the AICPA Board of Directors in January 1971, followed by a special Conference on Accounting Principles where a resolution was adopted urging the AICPA president to appoint two independent study groups: first, to articulate, as unequivocally as possible, the *purpose* of financial statements, and second, to recommend reforms to the *process* by which accounting principles were established. Led by Robert Trueblood, senior partner at Touche, Ross & Co., the Study on Objectives of Financial Statements group was known colloquially as the Trueblood Committee, while the Study on Establishment of Accounting Principles group was known as the Wheat Committee, led by former SEC Commissioner (1964-69) and practicing attorney Francis M. Wheat.⁶⁸⁵ It was on the basis of the Wheat Committee's report, released March 1972, that the proposal for the seven-member FASB took shape.⁶⁸⁶

⁶⁸⁵ Following the release of the Wheat report, an editorial in *Business Week* warned that it was "certain to raise the hackles of businessmen who still think that an accountant, like J.P. Morgan's lawyer, is not supposed to tell the client what to do but how to do what he wants to do. They will see no reason to strengthen the rule-making function of the profession." Nevertheless, added *Business Week*, the "fact remains that businessmen need impartial accountants who are trusted by the public just as badly as accountants need them. The Wheat report deserves the backing from business as well as from the accounting profession." (Editorial, "A Dose of Self-Discipline," *Business Week*, April 1, 1972, 80). The same issue of *Business Week* features a discussion of Wheat, the man, in an essay called, "The Man Behind the CPA Study."

⁶⁸⁶ The novel organization of the Board, it was hoped, would help regain lost confidence. Unlike the CAP and APB, the FASB was composed of salaried members who left their former institutions and affiliations to work on setting accounting standards on a full-time basis. In the past, members of the nation's leading accounting firms had to split their time between the demands of research and standard setting, on the one hand, and the needs of their firms' clients, on the other. To avoid "the inordinate delays" in completing research studies that had once been considered vital to the standard-setting process, the Wheat committee insisted that, "Full-time research should, we believe, be the normal pattern for the future." Furthermore, procedures that the APB had adopted late in life, such as holding public hearings on opinions it had under consideration, would be expanded greatly by the new Board. Accounting standards, rather than

The proposal was quickly met with the approval of the AICPA Board of Directors, the Financial Executives Institute, the Financial Analysts Federation, the National Association of (Cost) Accountants, the American Accounting Association, and the Big Eight auditing firms. On October 10, 1972, these groups agreed that Armstrong was the appropriate man to serve as the FASB's first Chairman. His position would take effect on November 1, 1972, and within seven months' time, the last of the appointments were made and the APB finally put to rest. However promising each of these little victories may have been, the condemnation against accountancy in the business press was unrelenting. All around them, the economy was mired in a slump from which it seemed unable to escape.

Just prior to the public release of the Wheat report, *Businessweek* reminded its readers of the import of the moment.⁶⁸⁷ The following month, the same magazine featured another essay decrying the complacency of the accounting profession in allowing clients to select methods most favorable to their bottom line. Reported *Businessweek*: “‘The client can make that choice,’ says [Ernst & Ernst’s Dick] Baker. ‘Anyone who tells you different isn’t leveling with you.’ Adds one Big Eight partner ruefully: ‘We...have a couple of real tough guys as clients.’ One thoughtful CPA looks at the role of the independent certified public accountant this way: ‘...I think there’s simply got to be a

“principles,” would be determined through a swift, deliberative process involving a wider array of interests than before. (Wheat Report, p31)

⁶⁸⁷ Prestigious accounting firms, said *Businessweek*, had found themselves “embroiled in nasty lawsuits recently, coming in the wake of corporate bankruptcies, brokerage house failures, and precipitous declines in the price of high-flying stocks.” Over the years, the APB had demonstrated a clear “inability to anticipate or respond soon enough to change.” Having devolved into “an arena for industry squabbling,” the APB and its members had “invariably run into stiff protests from the companies they serve.” Said APB member David Norr before a hearing held by the Wheat committee, “I think there is too much of a feeling that management should pick the principles it wishes.” As a result, when opinions did emerge, they were “often late and too watered-down to do much good.” See: “A Report to Change the Accounting Profession,” *Businessweek* (March 25, 1972), 86-7.

mild adversary relationship there.”⁶⁸⁸ At *Forbes*, one contributor wrote a scathing, across-the-board indictment of the business and financial community. Not only managers and accountants, but reporters and stockholders were held complicit as well. Reminded of the frauds perpetuated in the 1920s, the author argued, “Today we have our occasional outstanding fraud, but much more frightening is the general acceptance of what is politely called ‘managed earnings.’ Its effect on investors is nearly as bad as the case of outright fraud. This ‘management’ is simply false reporting.” The investor, he insisted, “should not be required to be his own auditor.”⁶⁸⁹

All of the earnings-manipulation of the 1960s and early 1970s had done much to undermine the credibility of accountants and their ability to self-govern, yet few, if any, believed that politically-appointed government regulators could effect better results. Whether the new Board would succeed where the APB and CAP had failed, remained an open question. “There is no way to guess whether the proposed new ‘Financial Accounting Standards Board’ will calm the heavy weather the accounting profession has been experiencing recently, but it seems better designed for its role than its predecessor,” reported the *Wall Street Journal*.⁶⁹⁰ And in the pages of *The CPA Journal*, Walter P. Stern, president of the Financial Analysts Federation, suggested that the “new arrangement will work if corporate management, government agencies, and others will accept it as the authoritative body on accounting standards. . . . But if management runs to Congress

⁶⁸⁸ “Accounting: A Crisis Over Fuller Disclosure,” *Business Week* (April 22, 1972): 60.

⁶⁸⁹ Gerald M. Loeb “Now You See ‘Em, Now You Don’t,” *Forbes*, Vol. 109, No. 10 (March 15, 1972): 280.

⁶⁹⁰ Editorial, “Accounting Principles,” *Wall Street Journal* (May 23, 1972): 20.

when a ruling reducing reported earnings, then the effort will be to no avail and accounting is likely to become politicized.”⁶⁹¹

A few months later, Leonard Savoie—the former executive vice president of the AICPA and, until recently, an unrelenting advocate of “flexibility” and a vocal critic of reform—expressed his concerns over the future of the FASB. At a convention of the American Mining Congress in September 1972, Savoie urged the following upon his audience: “let us ask ourselves some searching questions, in the hope that we may perhaps protect the new board from some of the hazards which confronted and brought down the APB. ... In addressing the [AICPA], Frank Wheat urged business statesmanship in dealing with FASB. We can all urge statesmanship in dealing with FASB, but we must question whether this will stop an industry or a company from lobbying against an FASB proposal which would require the industry or company to report lower profits.”⁶⁹² Others, like Reginald H. Jones, chairman of the board at General Electric, took a more optimistic view, urging businesses to rally behind the new Board and lend it their support.⁶⁹³

At the charter dinner hosted by the FASB in late March 1973, Jones spoke to an audience of more than 1,200 representatives from finance, accounting, business, and government; “Let’s not lose sight of the public and professional momentum that has

⁶⁹¹ Walter P. Stern, “Will the New Financial Accounting Standards Board Work?” *The CPA Journal* (June 1972): 446.

⁶⁹² Leonard Savoie “The Goal of the Financial Accounting Standards Board,” *The CPA Journal*, Vol. 43, No. 2 (February 1973), 95-6.

⁶⁹³ Apparently, the GE executive had to make this case whenever outrage about a FASB pronouncement threatened to tear apart the private sector’s last chance at self-regulating accountancy practice. According to an article in *Business Week* from 1976, “Jones continually has to remind his fellow chief executives of his warning when the FASB was launched.” See: “Focus on the Balance Sheet,” *Business week* 2435 (June 7, 1976): 56.

brought us this far,” said Jones. Managements must recognize the fundamental importance of financial statements that “are understandable, complete, unimpaired, and sound. In short, they must have integrity written between the lines.”⁶⁹⁴ In the same speech, Jones quoted from an editorial in *The Wall Street Journal* that echoed his view. Although the “new board’s future is by no means a certain one,” wrote the *Journal*, it nevertheless “deserves the heartiest good wishes in its task of settings rules that would restore credibility to financial reporting.” This was because “No amount of institutional strengthening will do the job...unless the corporate community generally recognizes the importance of its long-term interest in the credibility of reports. Or to put it more bluntly,” added the *Journal*, “there has to be some support in the corporate community for the FASB when it wants to hold out against corporate pressures for accounting rules that would show maximum immediate profits. Such pressures, and the lack of support against them, are what wrecked the APB.”⁶⁹⁵ It was against this mixed backdrop of optimism and anxiety that the FASB officially got to work in its new headquarters in Connecticut on July 1, 1973.

7.3. An R&D Agenda

With Marshall A. Armstrong at the helm, the original Board members included Donald J. Kirk, Robert T. Sprouse, Walter Schuetze, Arthur Litke, John Queenan, and Robert Mays. Even before the APB closed up shop for good, R&D was already placed on the new Board’s agenda, along with six other subjects. In the process of culling seven

⁶⁹⁴ Editorial, “Unity is Vital for Financial Accounting Standards Board,” *Journal of Accountancy* (May 1973): 10.

⁶⁹⁵ Editorial, “More Than Accounting,” *The Wall Street Journal* (March 7, 1973): 16.

subjects from a list of thirty, the Board passed from the antecedent conditions of inquiry—i.e., the sense of dissonance effected by the abuse of deferred charges in the 1960s—into the first stage: the institution of a problem.⁶⁹⁶ At the time, Robert Sprouse had just completed a term as president of the American Accounting Association and left his post at Stanford University to join the new Board, and it was Sprouse who Armstrong appointed as director of the R&D project.⁶⁹⁷ To assist him in the early stages of the work, Armstrong appointed an R&D Task Force in June 1973, composed of sixteen persons from industry, government, public accounting, cost accounting, finance, and academia.⁶⁹⁸

⁶⁹⁶ Over the course of three days in April 1973, FASB members culled from a list of thirty potential subjects, seven items to be placed on its initial technical agenda, R&D among them. The remaining six topics include: accounting for foreign currency; reporting by diversified companies; criteria for determining ‘materiality’; accounting for leases; accruing for future losses associated with catastrophic events, foreign operations, and self-insurance; and broad qualitative standards for financial reporting, which was understood to be a long-term project intended to provide general guidelines for establishing and interpreting standards. The following month, a brief statement from the Board describing these seven areas mentioned only that, “The accounting treatment of certain costs, such as research and development, start-up and relocation costs, is a broad area in which companies have adopted a variety of policies. The Board plans to consider this matter in depth with the objective of narrowing the differences in existing accounting practices and providing guidance for the appropriate accounting.” (“FASB Speedily Releases Deliberations After Parley,” *Journal of Accountancy*, Vol. 135, No. 5, May 1973, p9).

⁶⁹⁷ Sprouse has appeared several times throughout this story. It was “Bob” Sprouse whose arguments had recently convinced Paton of the shortcomings inherent to matching and deferral, which the Paton and Littleton monograph had drilled into the minds of accounting students since its publication in 1940. See Chapter 3, page __. Later, we saw Sprouse co-authoring with his mentor, Maurice Moonitz, the second study commissioned—and summarily rejected—by the APB in 1962. See Chapter 5, page __. Finally, we saw Sprouse present his famous “doesn’t-make-any-difference” argument in Chapter 5, page __. Now, he makes his final appearance embarking on what would turn out to be a twelve and a half year term serving on the FASB.

⁶⁹⁸ Representatives on the R&D Task Force from public accounting firms included Oscar Gellein, at Haskins & Sells; John W. March, of Arthur Andersen; E. Palmer Tang, of Touche Ross & Co.; and Raymond C. Lauver, of Price Waterhouse. From government came John A. Grady, Director of the Bureau of Accounts at the Interstate Commerce Commission, while from the financial sector came T. Lincoln Morrison, Jr., assistant vice president at First National Bank of Boston, and David Noor, of First Manhattan Corp., a major investment banking house in New York. Representatives industry included Robert E. Leech, of A.M. Pullen & Co.; Martin S. Gerstel, financial vice president at Alza Corporation; F. William Gridely, assistant comptroller at Chrysler; Robert A. Morgan, controller at Caterpillar Tractor Co.; Dudley E. Browne, consultant to and former controller of Lockheed Aircraft Co.; Joseph L. Stebick, assistant controller at Robert Shaw Controls Co.; and Robert W. Farrell, executive vice president and director of research at Bache & Co. Of these, Morgan, Bedford, and Browne sat on the R&D subcommittee of NAA’s Management Accounting Practices (MAP) Committee; chaired by Browne, the MAP subcommittee had been formed in late 1971 to address what the APB had failed to do: namely, the problems of deferred R&D charges which, having been deferred over the 1960s, were now coming home to roost. On the R&D Task Force, Bedford also represented the academic point of view, as a professor in the College of Commerce and

Six months later, at the tail end of December 1973, the Board issued its first Discussion Memorandum (DM), *An Analysis of Issues Related to Accounting for Research and Development and Similar Costs*.

Following the submission of position papers responding to the DM and a public hearing on the subject, the FASB ultimately decided to narrow the scope of the project and take up the question of “similar costs” in future standards.⁶⁹⁹ Thus, of the twelve issues that originally delimited the scope of the DM, only the first seven related to R&D. Posed in question form, for the public’s consideration, these issues were: What activities are encompassed by R&D? What elements of costs should be identified with R&D? What is the appropriate accounting treatment at the time R&D costs are incurred? What is the appropriate subsequent disposition of any costs not initially expensed? And finally, What special disclosures, if any, should be provided about R&D activities? Here, for the first time and after many generations, accounting leadership would help advance the profession out of the indeterminacy surrounding R&D accounting. With these seven questions, accountancy was passing from the first stage of inquiry into the second: the determination of a problem-solution.

By that time, the Gellein and Newman study had finally been published in February 1973 as ARS No. 14—one of the very last studies ever published by the APB,

Business Administration at the University of Illinois, Urbana-Champaign; Orace Johnson of the College of Administrative Science Ohio State University represented the second appointment from academia.

⁶⁹⁹ Included in “similar costs” were: costs of relocation and rearrangement of facilities; start-up costs, such as new plant or new retail outlet; the cost of marketing research; promotion costs, such as advertising a new product or service; the cost of developing computer programs; and the cost of training personnel, such as training flight crews for a new aircraft. These similar costs were believed to have three things in common with R&D: “(1) They are incurred with the expectation that the primary economic benefits, if any, will be derived in future periods, rather than during the period in which the costs are incurred. (2) Any future benefits to be derived from these costs exist presently in intangible form. (3) These costs are incurred in the process of developing or improving future benefits, in contrast to costs that may be incurred in utilizing existing benefits or in acquiring existing benefits from others”(FASB, DM 1973, 2).

during a time of its transition when its activities overlapped with those of the FASB. Gellein and Newman's *Accounting for Research and Development Expenditures* offered the new Board a wealth of material on which to build, starting with the discussion memorandum. Grappling with the problem that R&D expenditures did sometimes generate tangible products and/or intangible knowledge, ARS No. 14 recommended immediate expensing *except* in cases where all eight of Gellein and Newman's criteria for deferral were met.⁷⁰⁰ Similarly, in Chapter 1 of the DM, the FASB considered, on the one hand, that "unlike production and construction activities, any future benefits to be derived from [R&D] costs exist presently in intangible form."⁷⁰¹ On the other hand, even as "intangible form" was said to "connote the same set of attributes that the term 'intangible asset' ordinarily implies," the term "intangible form" was not intended to "prejudge the accounting treatment that is appropriate for [R&D] costs."⁷⁰² This amounted to saying that, in the act of thinking about R&D costs, the latter's affinity with intangible assets may very well suggest itself, but in the act of accounting, the two need not be treated the same. Clearly, the difference between the two terms hinged upon what the FASB understood by "asset," but this was hardly straightforward, even given the discussion taken up in Appendix C.

In that appendix, the FASB explained that the pronouncements of its predecessors had failed to "provide effective guidance in developing" the kind and quality of principles

⁷⁰⁰ The eight criteria were: 1) Substantial assurance that the products or processes can be produced. 2) Substantial assurance of the marketability of the products or processes. 3) Substantial assurance with respect to the revenue flow anticipated both with respect to the total amount and timing thereof. 4) Reasonable probability that the products or processes will be profitable. 5) Substantial evidence to the effect that the project or product would not have been undertaken except to fill existing orders. 6) Substantial evidence indicating that the failure to consider intangible costs as a product as opposed to a period cost would result in a material mismatching of costs with revenues. 7) Substantial evidence that the failure to defer research and development costs would produce a less tenable position than deferral. (p23)

⁷⁰¹ DM, p6

⁷⁰² DM, p3

required of the AICPA's own Code of Professional Ethics. Looking to other sources, the Board took note of the prevailing definition that equated assets with "future benefits that accrue to a specific accounting entity." Unhappy with this definition, the FASB suggested supplementing it with another: the notion of severability as defined by theorist Raymond J. Chambers.⁷⁰³ If the FASB were to adopt Chambers' concept of severability, R&D expenditures simply would not qualify as assets—and neither would many other deferred charges.

By March 15, 1974, the date of the public hearing, the FASB had received sixty position papers together with outlines of oral testimonies to be presented at the hearing. Altogether, these amounted to over 400 pages, and even after March 15th, the FASB continued to receive another fifteen position papers in the mail, bringing the total count to more than 500 pages containing seventy-four letters in all. Of all the Big Eight firms, most insisted in upholding managements' freedom to decide when R&D expenditures were and were not deferrable, or what the DM had called "selective capitalization."⁷⁰⁴

⁷⁰³ To Chambers, "severability" referred to "any means which, at any given time of action, may be converted to other means by exchange or by the processes of production, or which may be alienated by way of gift." As quoted in Appendix C, DM, p95

⁷⁰⁴ The Big Eight firms to advocate selective capitalization included: Ernst & Ernst, Price Waterhouse, Coopers & Lybrand, Haskins & Sells, and Touche Ross. J.K. Lasser & Co. and Main Lafrentz & Co., two medium-sized accounting firms in New York City, advocated the same. Others favoring selective capitalization included the members of the glamour industries, Martin Marietta, Datran, Textron, and TRW Inc.; the petroleum companies Exxon Corporation and Shell Oil; the public utilities Peoples Gas Company, Consumer Power Company, Commonwealth Edison, and the Cincinnati Gas & Electric Company; and the professional associations the Civil Aeronautics Board, the Federation of Government Accountants Association, the New York State Society of CPAs, the Accounting Standards Division of the AICPA, and former SEC Chief Accountant Andrew Barr. These were joined by the Evans Products Company, Inland Steel, Aetna Life & Casualty Atlantic Richfield Company, Transamerica Corporation, the American Gas Association, the Financial Executives Institute, the Machinery and Allied Product Institute, and the Management Accounting Practices Committee of the National Association of Accountants.

Only Arthur Andersen & Co. and Peat, Marwick, Mitchell advocated expensing as incurred.

Among those who assented to the current expense view were representatives of Masonite Corporation, Merck & Co., Eli Lilly and Company, G.D. Searle & Co., American Cyanamid, General Mills, Minnesota Mining & Manufacturing (3M), Texas Instruments, International Harvester, E.I. du Pont de Nemours & Company, General Electric, Reliance Electric Company, and Marcor.⁷⁰⁵ American Telephone & Telegraph (AT&T) was largely in accord with this group, although it believed that R&D intended to improve manufacturing processes, rather than produce products for sale, might reasonably be deferred. Altogether, as this list suggests, many companies that cautioned against deferral and capitalization had long been leaders in R&D. Letters submitted in response to the DM by these companies and by Arthur Andersen & Co. and Peat, Marwick, Mitchell, reflect their years of experience handling R&D costs:

“Deferral of these costs in balance sheets is, in effect, an attempt to anticipate future economic resources and to admit assets to the balance sheet that are incapable of satisfying any claims held by creditors and in which any viable equity on the part of owners is highly questionable.” – George R. Catlett of Arthur Andersen & Co.

“On a theoretical basis, deferral of certain [R&D] costs may be sound and has been accepted in practice. However, it is widely recognized that extremely subjective judgments are unavoidable in determining amounts of costs to be deferred and of the future environment. ... In our view, the need to maintain the credibility of financial reporting in the eyes of users takes precedence over theoretical considerations involved.” – Peat, Marwick, Mitchell & Co.

⁷⁰⁵ Joining them were David Norr of First Manhattan Corporation; the New York Stock Exchange; the New England Chapter of the credit-granters association Robert Morris Associates; the Financial Analysts Federation; the National Electrical Manufacturing Association; C. Merrick Payne, a consultant with but not claiming to represent McKinsey and Company; John A. Grady, Director of the Bureau of Accounts at the Interstate Commerce Commission; and others.

“Future benefits, if they exist, are both uncertain and unquantifiable. ... Capitalizing [R&D] costs would not or could not represent hard assets which can be converted to liquid form, such as cash. In fact, capitalized R&D costs would represent only a hope for successful new products or services.” – W.W. Brown, assistant comptroller at AT&T

“[B]usiness has the responsibility for never overstating its assets, so when there is doubt (and there must be doubt about the future profitability of any [R&D] activity) the costs should be expensed.” – E.D. Greiner, vice president of administration at Masonite Corp.

“In our ever changing industries, with their high degree of obsolescence, deferrals may result in unusual write-offs in subsequent periods of costs previously expected to be of substantial future benefit. We believe that any potential benefit derived from deferring such costs is far outweighed by the unfavorable effect that subsequent write-offs can have on the credibility of corporate financial statements and management.” – C.H. Calder, controller at American Cyanamid

“Future success is unpredictable regardless of the costs incurred.” – R.D. Ebbott, assistant treasurer at 3M

“To capitalize [R&D] and show relatively high earnings in one period, and possibly depressed earnings in another period as heavy write-offs might occur, is unfair to both present and future investors, and would be misleading to both. In a highly competitive business such as ours, with rapidly changing product technology, as well as changing markets, we feel very strongly it is only prudent and fair to our stockholders to expense [R&D costs] as they occur.” – E.O. Vetter, executive vice president at Texas Instruments

“Any criteria for deferring all or part of R & D expenditures...would foster an area of suspicion that management can ‘manipulate’ profits. ... There may be many ‘false starts’ or changes in objectives or possibly even an abandonment of the entire program. This, along with the uncertainties as to the extent of future benefits, also dictates the write-off of such costs as incurred.” – Roger J. Crise, vice president and comptroller at International Harvester

“[I]t would be difficult for independent accountants to judge whether deferral criteria had been met. They generally would not be familiar enough with the technology involved to bring judgment to bear, and it would not be possible, for security reasons, to bring in an outside technical expert—even if one existed. ... Expensing would indicate more clearly whether R&D could be financed out of current cash flow. If not, the need to explain the viability of a company’s future prospects would be demonstrated.” – W.E. Buxbaum, comptroller at Du Pont

“The financial problems involving deferring expenses that have come to light in recent years have all been due to the need to write off substantial deferring expenses that have come to light in recent years have all been due to the need to

write off substantial amounts of deferred R & D and similar costs that turned out not to have the value originally thought. To the best of my knowledge, none of the corporations that expensed such items currently developed any such problems.” – E.M. Robinson, first assistant treasurer at Du Pont

“It is the great uncertainty of the ultimate benefits to be enjoyed, if any, which dictates that [R&D] costs should be charged to operations as incurred. We believe it would be extremely difficult to develop explicit criteria for capitalization under specific circumstances and the application of such criteria would probably result in even greater inconsistency in financial reporting for this cost than exists presently.” – M.H. Mayo, vice president and comptroller at General Electric

Those opposing such views drew upon a range of arguments to defend management’s prerogative to defer R&D. Many, especially the handful of academics who responded to the DM, referenced the need to satisfy theory.⁷⁰⁶ Some referenced the matching principle as if it were a self-evident Truth, while others insisted that R&D was indistinguishable from capital as traditionally theorized; Solomon Fabricant, a famed macroeconomist, was among those who adopted the latter view. Still others warned of the disincentive that mandatory expensing would impose upon managers; removing their option to defer R&D was the same as asking them to accept blame for lower earnings reports. Some expressed fears that, without doing everything we can as a nation to encourage R&D spending, competitor nations such as Japan or Germany would continue their growth while the American standard of living crumbled into oblivion. Small technology businesses, especially, needed help. Many seemed to beg for leniency towards start-ups, which relied on the deferral method to draw in equity capital from shareholders otherwise deterred by yearly losses. It was hard enough to get a new business off the ground, the argument went, so why deny them an accounting method that turns red ink into black?

⁷⁰⁶ The one exception was professor _____ at university of _____ who advocated for current expensing.

All of these positions, for and against mandatory expensing, were echoed at the day-long public hearing in March. Thirteen statements were delivered before the Board that day.⁷⁰⁷ Following the presentation by one Winford H. Guin, manager of accounting classifications at the Bell System Companies (AT&T), FASB member John Queenan asked, “Are there any particular categories of research and development that you would say should definitely not be capitalized?” Guin’s response is representative of those who testified against a standard requiring the deferral option:

No. I suppose I am just a proponent of current expensing. ... I believe the accountant should have, and management should have latitude where they really see future benefit, but I honestly believe it is difficult to try to come up with guidelines and say okay, this is pretty strong evidence that we do have it. ... We can’t, even with hindsight, see that we could assign applied research and development, much less basic. That has been our many years of experience. We just can’t do it, even with hindsight. ... I can’t see, that you can go back with hindsight and say that this scientist’s work could have been capitalized and expensed over exactly three years—he could have invented it in 1925 and we use it today as something great. But at the time, it didn’t have any particular meaning, and we couldn’t see any.⁷⁰⁸

Guin didn’t rule out deferral entirely, though, since it might come in handy “if for nothing else than to stay in business”—an argument akin to the defense of technology start-ups. Yet, as a representative of the National Electrical Manufacturers Association

⁷⁰⁷ In order of appearance, these thirteen statements were made by: Dudley E. Browne, representing the NAA’s MAP Committee; Richard W. Stahl, a member of the Controller’s Office of the City of New York; Solomon Fabricant, of NYU; Winford H. Guinn, manager of accounting classifications at the Bell System Companies (AT&T); Thomas B. Fauls, full-time financial and accounting consultant to the National Electrical Manufacturers Association; C. Meyrick Payne, consultant for McKinsey & Co. representing the investor point of view; Arthur Wyatt and William Hall, representing Arthur Andersen & Co.; Frances Stone, T.R. Lilley, and Frank Block of the Financial Analysts Federation; John A. Grady, of the ICC; Robert C. Thompson and Robert Koons, of Shell Oil Co.; Kenneth W. Stringer, partner at Haskins & Sells; Robert K. Mautz, partner at Ernst & Ernst; and Richard W. Russell, partner at Coopers & Lybrand.

⁷⁰⁸ FASB, Public Hearing (March 15, 1974), 54-54.

explained, deferral implied matching, and in the case of R&D, “matching is impossible of achievement.”⁷⁰⁹

Sending incurred costs into the future search for elusive revenue will not have an acceptable level of accomplishment. This uncertainty of future benefits is made impractical by the very nature of the research and activity itself. Research goes down many paths before one of those paths is fruitful, and at the time of going down those paths, no one knows which one will be fruitful. ... Accountants do not have, nor their advisers, E.S.P.—extrasensory perception—or perhaps I should say E.A.P.—extra-accounting perception. ... Pursuit of conceptual accounting purity cannot justify putting information-dependent financial statement users in jeopardy.⁷¹⁰

There were no crystal balls to guide accountants and managers, and despite exclamations to the contrary, the matching principle did not provide certainty where there could be none. This was as true for start-ups no less than going concerns.⁷¹¹

Another presentation that stood among from the rest was that of Solomon Fabricant. Fabricant was an economics professor at NYU whose predilections towards capital investment theory made him extremely receptive to colleagues at NYU’s Vincent C. Ross Institute of Accounting Precision. For years, long after the FASB issued its standard on R&D, the handful of professors and visiting scholars who populated this research center advocated tirelessly for treating R&D as capital. After going on about how the efficient operations of capital markets relied upon the disclosure of R&D as capital, Fabricant proceeded to excuse misrepresentations of R&D on the basis of an analogy between depreciation and obsolescence charges, on the one hand, and the money

⁷⁰⁹ Thomas B. Fauls, FASB Public Hearing (March 15, 1974), 58.

⁷¹⁰ Fauls, 58-59.

⁷¹¹ “When early losses occur, so be it. Let them be displayed. Gertrude Stein, were she alive here, would say, a loss is a loss is a loss. We see no reason why a loss cannot be expected, experienced and disclosed by a company newly formed. To refuse to show a loss because of concurrent revenue is to deny the very reason for the loss. Investors in new companies must expect a bearable loss for a bearable period of time. If they can’t, they should not invest.” Thomas B. Fauls, FASB Public Hearing (March 15, 1974), 63

spent on R&D, on the other.⁷¹² Both, in his view, were capital investments in that both represented “investment of [money] capital in plant and equipment.” “It is exactly parallel to that,” he added.⁷¹³

Such tortured reasoning was out of step with reforms that, after decades of struggle, were finally underway in the accounting profession. Fabricant seemed to miss that these reforms were intended to make financial statement figures more readily understandable and less misleading than they had been in the past. The economists’ efficient markets hypothesis, however, assumed away such misleading statements, as Frances Stone, head of the Financial Analysts Federation pointed out. The “problem indeed with the theory,” she explained in her own testimony before the Board, “is that it assumes that everybody knows the information at the same time and understands it. And this I think is a real fallacy. Although the information may be available, not everyone, even our professional analysts, fully understand what they are reading, and consequently, we do not have the highly efficient market the theory says we do.”⁷¹⁴ Given the uncertainty of R&D and the inability to match costs to benefits, she concluded that “expensing of all [R&D] costs leaves less for interpretation.”

Fabricant denied the validity of these claims. Struggling to understand the difference between what he believed as an macroeconomist and what accountants were

⁷¹² “In a related vein, it has been argued that capitalizing research and development would make for less precision in the accounts. Here I simply quote from a well-known economist’s statement made many years ago: “The possibility of making accurate estimates of a theoretically untenable item is not an argument for substituting it for a tenable item that can be estimated roughly.” This surely is something with which accountants must agree, at any rate when they accept estimates of equipment depreciation and obsolescence estimates that can never be precise.” Solomon Fabricant, FASB Public Hearing (March 15, 1974), 35.

⁷¹³ Solomon Fabricant, FASB Public Hearing (March 15, 1974), 100.

⁷¹⁴ Frances Stone, FASB Public Hearing (March 15, 1974), 111.

now asking him to address, Fabricant proved himself unable to set aside Economics' theories of capital and value—concepts which were infamously at odds with the vicissitudes of everyday accounting.⁷¹⁵ With each question separately posed to him by FASB members Donald Kirk, Robert Sprouse, Walter Schueze, and John Queenan, Fabricant's performance deteriorated by the minute. With their questions largely unanswered, the FASB was unconvinced by Fabricant's line of reasoning.

At the end of a long day, the FASB concluded its public hearing and embarked on a two and a half month long process of deliberations which culminated in the Board's first Exposure Draft (ED), *Proposed Statement of Financial Accounting Standards: Accounting for Research and Development Costs*. Released on June 5th for public comment, the ED included in Appendix B a discussion of the meaning of asset. As in the DM, the Board recognizes that no general agreement existed in this area but that “two criteria that have been suggested are *measurability* and *exchangeability*.”⁷¹⁶ However, said the Board, at the time of their incurrence, R&D costs failed to satisfy either of these criteria. No mention was made of Raymond Chambers' concept of severability. Integrating severability into a definition of assets was either too bold or else too early a step for the FASB to take. At the time, the FASB's larger conceptual project, in which such definitions were to be worked out, was

⁷¹⁵ This is not to imply that there was one single, unchanging understanding of capital and value within the field of economics. However, only rarely have practicing economists debated the meaning of these terms, the so-called “Cambridge capital controversies” representing perhaps the most important of such episodes, with Harvard and MIT economists (located in the Cambridge, MA area) pitted against British economists (largely based at the University of Cambridge). See: Mark Blaug “Kuhn versus Lakatos, or Parradigms Versus Research Programmes in the History of Economics” *History of Political Economy*, Vol. 7, No. 4 (1974): 399-433. For an interesting historical discussion of the concepts of capital, income, and wealth, see: Jonathan Levy, “Capital as Process and the History of Capitalism,” *Business History Review*, Vol. 91 (August 2017): 483-510.

⁷¹⁶ FASB, Exposure Draft (June 5, 1974) *Proposed Statement of Financial Accounting Standards: Accounting for Research and Development Costs*

still underway, and the potential implications of severability for other areas of accounting had yet to be fully reckoned with.⁷¹⁷

The Board received hundreds of letters between June 13 and August 5 in response to the ED. Finally, in October 1974, the FASB released the first accounting standard ever issued by the profession's leadership on the treatment of R&D. Known as Statement of Financial Accounting Standards No. 2, or SFAS 2, *Accounting for Research and Development Costs* required the current expensing of all non-contracted, company-financed R&D expenditures. For generations, while the majority of businesses expensed R&D as incurred, there remained a steady contingent of managers who insisted on deferral so as to smooth the peaks and valleys of earnings from one period to the next, obscure the impact of losses upon a company's bottom line, and attract stockholder capital and bank financing.

The final nail in the deferral coffin was the uncertainty of outcomes, that which marks R&D activities as experimental inquiry. This very lesson was lost on those economists, accounting theorists, administrators, and regulators who, following the victories of World War II and the role of R&D therein, saw in R&D an investment like any other. While SFAS 2 had buttoned up the debate on R&D accounting, it was the work of mainstream macroeconomists—those designing models with clear implications for national policy—that kept the notion of R&D capital alive.

⁷¹⁷ This is most likely the reason for postponing a decision on whether or not to include severability among the criteria for assets. Less likely is that the FASB succumbed to pressure from representatives from Shell Oil who pleaded against Chambers' definition, both in their position paper responding to the DM and in their oral presentation at the public hearing. Their concern was not with the potential impact on R&D accounting, but on accounting for the intangible asset "goodwill." Where the Board failed was in explicating their justification on this point. Instead, the question of severability was merely ignored in the ED and the final standard.

7.4. Discovering the “Measure of Our Ignorance”

During the mid-1950s, a major research program underway at the National Bureau of Economic Research (NBER) in New York generated what appeared to be an especially confounding anomaly for the practice of econometrics. Before the war, NBER economist Simon Kuznets, under the guidance of Wesley Clair Mitchell, had helped expand the toolbox of national income accounting practice by developing the first-ever measures of Gross National Product.⁷¹⁸ After the war, Moses Abramovitz, John W. Kendrick, and Solomon Fabricant, among others, experimented with data from Kuznets’ national income accounts in the hope of developing an empirical system for documenting trends in national productivity over time. It was here that the anomalous “residual” arose: growth in the nation’s output far exceeded growth in inputs, and try as they might, manipulating the data through various statistical and inferential techniques could not close this gap.

“Despite the greater increase in capital than in labor or inputs,” Kendrick observed, “output per unit of capital has generally risen over the long period.” In the broadest sense possible, Kendrick identified “the almost universal gains in productive efficiency” with the introduction of innovations and concluded: “Innovation has therefore been capital-saving as well as labor-saving, on the whole.”⁷¹⁹ Abramovitz, in discussing the increase in national income as a result of such productivity, presented the facts as he saw them: “To account for the quadrupling of net national product per capita [ie,

⁷¹⁸ Carol S. Carson, “The History of the United States National Income and Product Accounts: The Development of an Analytical Tool,” *Review of Income and Wealth*, Vol. 21, No. 2 (June 1975): 153-181

⁷¹⁹ John W. Kendrick, *Productivity Trends: Capital and Labor* (New York: NBER, 1956), 15-16.

national income], the productivity of a representative unit of all resources must have increased some 250 per cent.”⁷²⁰

Abramovitz, like Kendrick, was shocked by what he found: productivity increased across all factors. Previously, it was generally assumed that productivity increases occurred either through capital or labor—but not both. These latest findings made that assumption obsolete. An increase in productivity was thus felt to be affecting each input into the economy not by increasing the *amount* of that resource, but by increasing its *effect*. As a quality shared by all inputs, productivity made each input “do more work,” so to speak. “This seems to imply,” continued Abramovitz, “that almost the entire increase in net product per capita is associated with the rise in productivity.”⁷²¹ With these observations, the NBER group was left facing a massive change in productivity that they simply could not account for. Outside of the NBER, in the economics department at MIT, the problem of the residual would be taken up by Robert Solow, a young economist whose research interests had only recently shifted away from empirical econometrics towards more abstract macroeconomic modeling. In his hands, a new method of isolating the residual was born out of a union of national income accounting practice and production function analysis.

Using a model of economic growth he had developed a year earlier,⁷²² Solow’s innovation in 1957 was to provide an economic interpretation to the calculations of input

⁷²⁰ Moses Abramovitz “Resource and Output Trends in the United States Since 1870,” *American Economic Review*, Vol. 46, No. 2, Papers and Proceedings of the Sixty-Eighth Annual Meeting of the American Economics Association (May 1956), 11

⁷²¹ Abramovitz (May 1956), 11

⁷²² Robert M. Solow, “A Contribution to the Theory of Economic Growth,” *The Quarterly Journal of Economics*, Vol. 70, No. 1 (February 1956): 65-94. For a history of the state of growth theory at the time of Solow’s writing, see Verena Halmayer “From Exploratory Modeling to Technical Expertise: Solow’s Growth Model as a Multipurpose Design,” *History of Political Economy*, Vol. 46, Annual Supplement (2014):

and output indexes coming out of the NBER. He achieved this by turning to total factor productivity, or TFP, which represented the productivity affecting the traditional inputs of production, labor, L , and capital, K . In “Technical Change and the Aggregate Production Function,” which the Nobel Prize committee described in 1987 as having “laid the foundations for what was later to develop into ‘growth accounting,’” Solow reconceived TFP as a scale factor in the Cobb-Douglas production function.⁷²³ This was an equation developed about thirty years earlier by an economist and mathematician and for purposes other than explaining national growth.⁷²⁴ Reaching for the Cobb-Douglas production function, $Y = (L, K)$ in unspecified form, Solow introduced TFP as A , a scale factor that would multiply the effects of K and L inputs proportionately, $Y = A(L,K)$. For Solow’s contemporaries and for the rising generation of macroeconomic model-builders, this was a revelation; and for the generations of students yet to come, it would appear as standard textbook fare.⁷²⁵

229-250; Verena Halmayer and Kevin D. Hoover “Solow’s Harrod: Transforming Macroeconomic Dynamics into Model of Long-Run Growth.” *European Journal of the History of Economic Thought*, Vol... (2015): 1-36; and Tiago Mata and Francisco Louçã “The Solow Residual as a Black Box: Attempts at Integrating Business Cycle and Growth Theories” *History of Political Economy*, Vol. 41, Annual Supplement (2009): 334-355.

⁷²³ Robert M. Solow, “Technical Change and the Aggregate Production Function,” *The Review of Economics and Statistics*, Vol. 39, No. 3 (August 1957): 312-320. See: The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel, 1987, Robert M. Solow “Press Release”(October 21, 1987) <www.nobelprize.org/prizes/economic-sciences/1987/press-release>.

⁷²⁴ The Cobb-Douglas production function was presented by Paul Douglas at the 1928 annual meeting of the American Economics Association. Later, Douglas could recall that the equation he developed with mathematician Charles Cobb was received with unanimous displeasure by his audience at the meeting, although this is not entirely accurate. Regardless, with the exception of agricultural economists such as Gerhard Tinter at Iowa State College and later Earl Heady who put it to use in their own work, the production function would have to wait until the postwar years to gain favor within mainstream economics.

⁷²⁵ “Ever since Solow’s celebrated article of 1957, estimation of aggregate Cobb-Douglas production functions for purposes of measuring the sources of economic growth and drawing inferences about the nature of technical progress has become a widespread practice in economic research. ...most economists are delighted with puzzle-solving activity of an empirical kind even if it is virtually tantamount to ‘measurement without theory.’ [Alfred] Marshall used to say that ‘explanation is predication written backwards.’ Many economists forget that prediction is not necessarily explanation written forwards. It is only too easy to engage in empirical works that fail utterly to discriminate between competing

Although the new method would later help earn its inventor the Swedish Bank's 1987 Nobel Prize, it did little to clarify what captivated the attention of the NBER group: How could economists account for what sat behind the mysterious residual? All anyone knew for certain was that it represented aggregate productivity, that is, productivity of a kind that scales up the influence of all other inputs into the economy. Whatever was responsible for productivity effected the rest of the economy in equal proportions, they believed. For Solow, "technical change" was a convenient even if vague catch-all explaining the residual, one that no doubt awaited further refinement; but for many of his contemporaries as well as for the generation of economists following in his footsteps, the indeterminacy of "technical change" would soon be resolved by what they believed to be a more precise category: R&D.

Whether identified as total factor productivity, qualitatively described as technical change, or quantified as the scale factor, A , in a production function, Solow's work "only made the stylized facts starker and more troublesome." As far as the NBER group of the late 1950s was concerned, the residual was still unaccounted for. "This result is surprising in the lopsided importance which it appears to give to productivity increase, and it should be, in a sense, sobering, if not discouraging, to students of economic growth," urged Abramovitz. "Since we know little about the causes of productivity increase, the indicated importance of this element may be taken to be some sort of measure of our ignorance about the causes of economic growth in the United States and some sort of indication of where we need to concentrate our attention."

explanations"(Mark Blaug "Kuhn versus Lakatos, or Paradigms Versus Research Programmes in the History of Economics" *History of Political Economy*, Vol. 7, No. 4 (1974): 426).

Certainly, as empirically-oriented researchers, the group at NBER had their own ideas about what constituted the residual, including changing composition of the workforce, investments in education, the reallocation of resources from lower to higher productivity activities, economies of scale, and of course, advances in technology. Kendrick, for one, had explicitly listed “research and development” as one of a long list of factors potentially excluded from his original analysis, and Abramovitz, too, referred to the omission of research from the factor inputs in the national accounts. When Brookings Institution economist Edward Denison conducted his own in-depth studies on the topic, he too mentioned the contributions of research activities in explaining growth. But none of these authors even so much as hinted that R&D ought to take a leading role as *explanans*, over and above all else. It was, instead, one of many factors that were likely at play, operating altogether.

Like Denison, Theodore Schultz, chair of the University of Chicago’s Economics Department and the only agricultural economist ever to win the Swedish Bank’s Nobel Prize, was driven to examine what he felt simply had to be a more complex set of relationships at play beneath the residual. Ever since his days at Iowa State College, Schultz was resolute in his advocacy for a particular, experimental type of empirical research fostered by the state agricultural research stations. Immediately after the first reports came out from the NBER that identified the inexplicable change in growth rates, Schultz made this statement:

We would like to know: To the extent that growth in output does not come from additional conventional inputs, *where does it come from?* The facts are strong and stubborn in supporting the inference that much, and probably most, of the growth in output cannot be explained by increases in the size of the labor force and in the stock of reproducible inputs. We have cited Fabricant and Abramovitz who found *four-fifths* of the remarkable economic growth of the United States of the last eight

decades unexplained by additional inputs. Fabricant explained it by an appeal to “improvements in national efficiency.” But what is that? The question remains: Where does all of this unexplained increase in output come from? Is the four-fifths beyond economics? ...

More recent theorizing about economic development, in the main, has been tied either to savings and the formation of conventional capital (items), or to that of disguised unemployment. ... [Often neglected by economic theorists,] there is the improvement of the *quality* of people as productive agents.

Schultz had a profound influence on Zvi Griliches, a graduate student at Chicago who would go on to become one of the leaders in R&D economic theory.

As the heir to Schultz’ program, Griliches was unimpressed with Solow’s treatment of the residual. Writing in 1963, he argued: “This formulation of the problem and the direction in which the resulting research has evolved are, in my opinion, not very helpful to the understanding of growth. ... Moreover, it does not further our understanding of growth to label the unexplained residual changes in output as ‘technical change.’”⁷²⁶ Griliches recommended another tack, in which “changes in output are attributable to changes in the quantities and *qualities* of inputs, and to economies of scale, rather than to ‘technical change.’” He believed that the “residual measures of productivity growth [ought to be] viewed not as measures of technical change but rather as the result of errors in the measurement procedure.”⁷²⁷ By the late 1960s, Griliches had become renowned for his econometric analyses of R&D, even as Solow-style growth theory came to rule the roost.⁷²⁸

⁷²⁶ Zvi Griliches, “The Sources of Measured Productivity Growth: United States Agriculture, 1940-60.” *Journal of Political Economy*, Vol. 71, No. 4 (August 1963): 331

⁷²⁷ *Ibid*, 332.

⁷²⁸ For a good retrospective of his own work, see: Zvi Griliches, “Hedonic Price Indexes and the Measurement of Capital and Productivity: Some Historical Reflections,” 185-206

Unlike Solow, Griliches was committed to searching out and analyzing more “insightful”⁷²⁹ databases from which to explain the nation’s growth trajectory. Echoing the kind of empiricism conducted at agricultural experiment stations, which had so influenced his earliest work and continued to inspire the work of his mentor, Theodore Schultz, Griliches was relentless in his pursuit of data through which he could develop, test, refine, and, yes, reject his own theories.⁷³⁰ Throughout his career, he did not fail to recognize the limitations of accounting for the economic consequences of R&D spending.⁷³¹ Griliches was not alone in criticizing Solow. Taking a dramatically different approach than either Griliches or Solow, a small handful of economists employed at the RAND Corp in the 1950s began work on a study of R&D that, in time, would help open up the heterodox field of evolutionary economics.

7.5. RAND Economics

⁷²⁹ “Notes on the Role of Education in Production Functions and Growth Accounting” in *Studies in Income and Wealth*, NBER, Vol. 35 (New York: Columbia University Press, 1970).

⁷³⁰ Although Griliches did not find insurmountable the challenge of econometrically capturing R&D, he did take it seriously. He had successes. He made mistakes. He reached dead ends. And he learned from them all, improving and refining ways of approaching long-standing problems. This he did explicitly and with a humility that is only confirmed by the stories of the man shared by his many, adoring former-students. In the discussion that followed Griliches’ “Research Expenditures and Growth Accounting,” presented in 1973 at the Conference of the International Economics Association in St Anton, Austria, Professor Patinkin “ended with the comment that he felt the tone in the paper was one he welcomed—it was one of humility with reference to econometric studies which was all too often missing from the field”(Science and Technology in Economic Growth, Proceedings, Macmillan, 85). See the “Tree of Zvi,” online.

⁷³¹ “[M]ost private R & D is treated as a current expense (an intermediate good) and does not appear explicitly in the value of output. Moreover, we have no explicit income stream (return) to associate it with. Thus, any accounting scheme based on factor shares is very difficult to implement... The basic difficulty of treating research as an investment is that it is largely an “internal” investment, without an explicit intermediate market for its product. When we talk of “tangible” investment, meaning equipment and construction, these can be valued by what was spent on purchasing them. ... But research, like advertising and other costs of change, is largely internal to the firm and does not show up in its output accounts. More importantly, ...it might not even show up in the income account of a firm.” Griliches 1973, 68-69

RAND, a name inspired by *Research AND Development*, was incorporated as an independent, multidisciplinary research organization at the end of World War II. Of the many developments generated in RAND's history, the best known by far is systems analysis. Indeed, systems analysis is what put RAND on the map. Less well-known is the organization's R&D Project, an initiative of the still-nascent Economics department that developed a critique against the overreaching arms of systems analysis. This project began in 1955 with the intention of assisting the Air Force manage R&D more effectively, but even before this project, dissent was already voiced against the systems analytical thinking that dominated RAND's other departments.⁷³² Starting with the critiques made by Armen Alchian, the first economist hired by the organization, a small but persistent contingent of researchers voiced their dissent against the application of systems analysis in the R&D decision making process.

After thoroughly reviewing the practices in place for designing intercontinental ballistic missiles and speaking with whatever military men were willing to cooperate, Alchian insisted that the use of such a large number of variables within a series of simultaneous equations could only mean too little was actually *known*, in *concrete* terms. This made the resulting conclusions not only meaningless but also, in light of the Soviet threat, dangerous.⁷³³ Leave systems analysis for procurement decisions, Alchian pleaded, but keep it far removed from research and development;

⁷³² In a RAND economics paper of November 1959 titled, "The Economics of Parallel R and D Efforts: A Sequential-Decision Analysis," economist Richard R. Nelson observed in a footnote that: "The idea of a parallel development strategy has a long history at RAND. Burton Klein, William Meckling, Emmanuel Mesthene, Leland Johnson, Thomas Marschak, Armen Alchian, William Capron and others all have contributed to its evolution"(p.1, f.2).

⁷³³ The internal response within RAND was dismissive, accompanied by a decision that "the method would work on more tightly bounded problems wherein the specification problem could be kept under control." David A. Hounshell (2000) "The Medium is the Message" p.259

For the Research and Development decision, part of the required information pertains to the feasibility of designing a producible weapon which will have specified performance characteristics. But this is not enough, a second kind of essential information is the type of performance that is wanted. The kind of performance that is wanted depends upon the kind of war to be fought and the enemies' capabilities. If both these kinds of information, that is, 1) the future state of the enemies' capabilities and intentions, and 2) the design and production feasibility of new weapons, were known with certainty, we could concentrate our research and development effort on the optimal weapon. But, since we suffer from predictive myopia in both eyes, we either can guess and then design what we hope will be the optimal, or, a good weapon—or, we can truthfully admit we don't know and obtain insurance by designing several alternative weapons, one for each possible contingency.⁷³⁴

When deciding which R&D plans to initiate, prolong, or bring to a halt, argued Alchian, let the uncertain nature of R&D be its own selection process. Take the decision-making process out of the hands of military procurement officers and allow the chips to fall where they may, with only the unpredictable dead-ends and discoveries to determine where R&D project would lead. Having demonstrated such a straightforward nod to the evolutionary theory of natural selection,⁷³⁵ it is no surprise, then, that when tasked with recruiting new men to staff his department, Alchian hired like-minded economists, primarily from Chicago, Yale, and Harvard, who were open to experimenting with evolutionary thinking in their own work.

⁷³⁴ The passage continued as follows: “The Research and Development effort is intended to create designs of new weapons which will form our confirmed and broad set of weapons available for procurement. It must be recognized that R and D is directed toward providing a set of available choices rather than toward providing the one weapon that *ex post* best collates with the realized state of the world ten years hence. To assume that our foresight is adequate for this purpose is the error of not knowing how blind we really are. R and D not only advances us technically—it is also our only assurance of flexibility and wide range of choice in the future. An intelligent R and D program must satisfy *both* objectives.” Armen A. Alchian “The Chef, Gourmet and Gourmand” US Air Force Project RAND Research Memorandum, No. 798-PR. (March 24 1952): 6-7

⁷³⁵ In 1950, Alchian submitted a paper to Milton Friedman, editor of the *Journal of Political Economy*; “Uncertainty, Evolution and Economic Theory,” was one of the earliest attempts to rethink the research and development process in an evolutionary framework.

First under Charles Hitch and then under Burton H. Klein, the R&D Project would come to include Reuben A. Kessel, Kenneth J. Arrow, Richard R. Nelson, and Sidney G. Winter. It was Nelson and Winter who, during and after their time at RAND, published dozens of articles over the period of more than a decade, culminating in their 1982 magnum opus, *An Evolutionary Theory of Economic Change*.⁷³⁶ Along the way, a slowly expanding number of economists followed their lead, designing models of economic development that, like Nelson and Winter's, incorporated Michael Polanyi's (1958, 1966) concept of tacit knowledge into a Darwinian-esque natural selection framework.⁷³⁷ In their models, R&D expenditures were no guarantee of growth. Instead of "correctly maximizing profits," firms had varying degrees of success and failure depending on their experiences in "gradually groping, innovating or evolving towards more profitable ways of doing things."⁷³⁸ However, interest in this approach would soon wane in the 1980s when another wave of R&D aggregate production function theories sought to revive interest in Solow-style analysis. By that time, data on R&D that had first been pooled together somewhat haphazardly by the National Science Foundation in the late 1950s, had long since become a regularly-issued trove of statistics. These and other figures made it simple enough to harvest data on R&D spending and prove one's pet theory of R&D capital.⁷³⁹

⁷³⁶ Richard R. Nelson and Sidney G. Winter, *An Evolutionary Theory of Economic Change*. Cambridge, MA: Harvard University Press, 1982

⁷³⁷ Tacit knowledge is not without its problems, though space does not allow for their discussion in the present paper.

⁷³⁸ Richard R. Nelson and Sidney G. Winter, "Evolutionary Theorizing in Economics" *Journal of Economic Perspectives*, Vol. 16, No. 2 (Spring 2002): 23-46.

⁷³⁹ The NSF's Office of Special Studies published its first survey of R&D statistics, albeit in preliminary form, in 1956; this report was based on data on R&D expenditures for the years 1953-1954. As the nascent Foundation began to find its footing, it would increasingly act as a centralized repository for such data that had previously been collected elsewhere, throughout various agencies including the Bureau of Labor Statistics, the Research and Development Board of the Department of Defense, and the National Academy

While Solow's macro growth model left "technical change" undertheorized, growth theorists were as susceptible to the glamour of R&D as any other group. This, combined with the statistics available from the NSF and other sources, resulted in a slew of papers in which the candidates for A , the scale factor in Solow's aggregate production function, had been narrowed down to indexes of R&D. In order to experiment with various macro-level relationships, these studies relied on a range of dubious assumptions: first, many of the studies assumed that more money spent on R&D necessarily translated into more patents; second, that firm or industry level spending could be inferred from aggregates reported by the NSF; third, that the values given to R&D in financial statements were comparable across firms; and fourth, that R&D was most appropriately understood as an investment in capital.⁷⁴⁰

of Sciences-National Research Council. Independently of government, impressive data was collected by Douglas Greenwald and Dexter Keezer of the Department of Economics at the McGraw-Hill publishing company; for more on this work, see footnote 87, *infra*.

⁷⁴⁰ Daniel Hamberg, "Invention in the Industrial Research Laboratory," *Journal of Political Economy*, Vol. 71, No. 2 (April 1963): 95-115; Daniel Hamberg, "Size of Firm, Oligopoly, and Research" *Canadian Journal of Economics and Political Science* (February 1964): 62-75; Marshall Hahn, "The Determinants of Investment Variation in Research and Development" *IEEE Transactions on Engineering Management*, EM-11 (March 1964): 8-15; Dennis Mueller "The Firm Decision Process: An Econometric Investigation" *Quarterly Journal of Economics* (February 1967); Dennis Mueller "Patents, Research and Development and the Measurement of Inventive Activity" *Journal of Industrial Economics* (November 1966); Edwin Mansfield and R. Brandenburg "The Allocation, Characteristics, and Outcome of the Firm's Research and Development Portfolio" *Journal of Business* (October 1966); A. Phillips "Patents, Potential Competition, and Technical Progress" *The American Economic Review*, Vol. 56, No. 2 (May 1966): 301-310; Daniel Hamberg (1966) *R & D: Essays on the Economics of Research and Development*, Random House; Frederick M. Scherer "Research and Development Resource Allocation Under Rivalry" *Quarterly Journal of Economics* (August 1967); National Science Foundation "Decision Making on Research and Development in the Business Firm" *Review of Data on R and D* (February 1964); Henry G. Grabowski "The Determinants of Industrial Research and Development: A Study of the Chemical, Drug, and Petroleum Industries" *Journal of Political Economy*, Vol. 76, No. 2 (March-April 1968): 292-306; Edwin Mansfield (1968) *Industrial Research and Technological Innovation: An Econometric Analysis*, W.W. Norton & Co.; William S. Comanor and F.M. Scherer "Patent Statistics as a Measure of Technical Change" *Journal of Political Economy*, Vol. 77 (May-June 1969): 392-398; L. G. Telser "Comment" *The American Economic Review*, Vol. 59, No. 2 (May 1969): 121-123; Dennis C. Mueller and J.E. Tilton "Research and Development Costs as a Barrier to Entry" *Canadian Journal of Economics*, Vol. 2 (November 1969): 570-579; T.M. Kelly "The Influences of Firm Size and Market Structure on the Research Efforts of Large Multiple Product Firms" PhD Dissertation, Oklahoma State University (1970); Henry G. Grabowski and Dennis C. Mueller "Managerial and Stockholder Welfare Models of Firm Expenditures" *The Review of Economics and Statistics*, Vol. 104, No. 1 (February 1972): 9-24; M.N. Baily "Research and Development

When Paul Romer's "Increasing Returns and Long-Run Growth" (1986) launched the new endogenous growth literature,⁷⁴¹ it displaced evolutionary growth theory with a productivity model that drew heavily from Kenneth Arrow's (1962) articulation of knowledge spillovers.⁷⁴² Understood as one class of "positive externalities," Romer's theory of R&D-knowledge spillovers enjoyed widespread support by policy-makers as a means "to justify such interventions as the intellectual property system, government support of R&D, R&D tax incentives, and the encouragement of research partnerships of various kinds."⁷⁴³ And this, despite the fact that by the mid-1970s, accounting standard-setters had rejected the treatment of R&D expenditures as capital.

Costs and Returns: The US Pharmaceutical Industry" *Journal of Political Economy*, Vol. 80 (January-February 1972): 70-85; W.N. Leonard "Research and Development, Product Differentiation, and Market Performance" Mimeo, Hofstra University (1972); Henry G. Grabowski and Nevins D. Baxter "Rivalry in Industrial Research and Development: An Empirical Study" *The Journal of Industrial Economics*, Vol. 21, No. 3 (July 1973): 209-235; B. Branch "Research and Development Activity and Profitability: A Distributed Lag Analysis" *Journal of Political Economy*, Vol. 82 (September-October 1974): 999-1011; J. Spitzer "The Rates of Return to Capital, Advertising, and Research and Development: Their Sensitivity to the Structure of the Distributed Time Lag" PhD Dissertation, Duke University (1974); D. Schwartzman (1975) *The Expected Return from Pharmaceutical Research*, American Enterprise Institute; R.J. Stonebraker "Corporate Profits and the Risk of Entry" *Review of Economics and Statistics*, Vol. 58 (February 1976): 33-39; Edwin Mansfield, J. Rapoport, A. Romeo, S. Wagner, and G. Beardsley "Social and Private Rates of Returns from Industrial Innovation" *Quarterly Journal of Economics*, Vol. 91 (May 1977): 221-240; Henry G. Grabowski and Dennis C. Mueller "Industrial Research and Development, Intangible Capital Stocks, and Firm Profit Rates" *The Bell Journal of Economics*, Vol. 9, No. 2 (Autumn 1978): 328-343.

⁷⁴¹ Paul M. Romer. "Increasing Returns and Long-Run Growth," *Journal of Political Economy* 94, no. 5 (1986): 1002-37; David Warsh (2006) *Knowledge and the Wealth of Nations: A Story of Economic Discovery*. London: Norton.

⁷⁴² In "Economic Welfare and the Allocation of Resources to Inventive Activity," Arrow began by identifying information as the main output of scientific research, and argued that firms, technical change, and knowledge are understood in terms of information processing. In this framework, R&D spending tended to be non-optimal owing to the indivisible, non-excludable, and uncertain nature of information. Because of these three essential features, any investment in R&D that generates inventions also generates positive externalities of the information about those inventions. These positive externalities, or spillovers, benefit competitors who then used that information to produce lower-cost imitations of an original invention. See: Paul Nightingale 2003, 150.

⁷⁴³ Hall 2002, 36. Building on this same rationale is a book by Gregory Tassej called, *The Economics of R&D Policy*. Tassej opens a chapter on the "Rationales for Public-Sector R&D Policies" by stating rather flatly: "The key rationale, in fact the only rationale, for a public R&D policy is the existence of a private market's failure to invest in an 'optimal' amount of R&D" (Tassej 1997, 81)

By explaining the residual in terms of R&D, aggregate production function theorists contributed to a process by which the latter was all but drained of the very advantages that made it so meaningful—and yet so untenable as capital. What R&D offered was a focused means of identifying, articulating, and experimentally grappling with the specific uncertainties that marked what Dewey called “indeterminate situations”—and this, *without* any guarantees of success. Picking up the views that prevailed in the 1950s and 1960s, in which R&D was seen as manna from heaven, macroeconomists responded to and helped enact a new, ontological significance of R&D, one characterized by a pervasive sense of *certainty* in the relationship between cause and effect, ends and means, process and outcome. Lost was the important factor of *uncertainty* that attended R&D as an experimental realm of activity, one defined no less by necessary failure than by success. What Abramovitz had once called the “measure of our ignorance” was now capitalized in the form of R&D.

Chapter 8

Experimental Outcomes

8.1. Learning and Teaching

Today, the notion that R&D is valuable, that it has value, is taken as given. Mobilization of the nation's industries and researchers had produced a range of technologies that helped bring World War II to a triumphant end, and in so doing, demonstrated the value of well-funded industrial research. The war experience seemed to prove that the more one spent on research, the more brilliant technologies and patentable products would result. Yet, as David Hounshell points out, "Seldom have the lessons of war been more fundamentally misunderstood."⁷⁴⁴ Everyone, he adds, "including those who should have known better," saw in research a magic bullet for growth. Hounshell rightly criticizes the absence of "engineering and manufacturing know-how" from accounts of research as a source of new products. To the missing production engineers I would add the missing cost accountants. These individuals experimented with various ways of integrating R&D into a company's cost classifications, budgeting procedures, and systems of accounts and—like the production engineers and researchers, both—soon learned the necessity of cooperation, of becoming acquainted, even in broad strokes, with the demands particular to each other's work.⁷⁴⁵

⁷⁴⁴ David Hounshell, his chapter from *Engines of Innovation* (1996), 41

⁷⁴⁵ Such mutual understanding could not be achieved if R&D labs were located apart from the rest of a company, as they were in the 1950s, or otherwise kept isolated from the needs and know-how of the other arms of a business. The results often proved disastrous. An essay by James B. Quinn in 1963 includes the following example. At one consumer products company, the research director had "shielded his researchers from 'commercial pressures' so effectively that researchers did not understand the company's technological

That cost accountants confronted and worked through the challenges that arose with the introduction of a new professional and organizational activity comes through clearly in the pages of the *NACA Bulletin* in which accountants swapped what Steven Shapin in a different context referred to as “‘war stories’ among congenial colleagues.”⁷⁴⁶ Articles in the *Bulletin* were often a combination of description and reflection on the efforts made to handle old and new problems alike. The problem of accounting for R&D was no exception. As advances were made in other areas of cost accounting, such as the development of budgetary controls or the allocation of overhead, writers shared their experiences adapting these tools to R&D. These essays inspired NACA staff to conduct a survey of industry R&D accounting practices, and on the eve of war, the report of 1939 declared that, even if an entirely logical classification scheme for R&D could be devised, “there would still exist the problem of deciding which expenditures should be capitalized or deferred and which should be written off currently.”⁷⁴⁷

An important feature of business experience during World War II and into the Cold War was the requirement made of contractors to keep detailed cost accounting records. Many firms simply did not maintain such levels of detail of their cost flows, and with these systems in place following the war, greater interest arose in experimenting with cost controls in other areas of business. Thus, during the early post-war years, questions were raised over the usefulness of applying to R&D the same sorts of cost controls

needs and could not direct themselves toward problems of commercial significance. Operating groups did not hear about significant technical results until those in research were ‘sure they would work.’ Consequently, no significant research results moved to operations for three years.” James Brian Quinn and James A. Mueller, “Transferring Research Results to Operations,” *Harvard Business Review* 41, no. 1 (January-February 1963): 52.

⁷⁴⁶ Steven Shapin, *The Scientific Life* (2009), 131

⁷⁴⁷ NACA Research and Technical Service Department, “Present-Day Practices in Accounting for Research and Development Costs,” *NACA Bulletin*, Vol. 20, No. 13 (March 1939): 889.

traditionally limited to manufacturing. This post-war debate among cost accountants brought out the inadequacy of an analogy between R&D and production, as did the second research study on R&D completed by NACA in 1955. Both the 1939 and the 1955 study revealed a strong preference among firms for currently expensing R&D as incurred; by contrast, efforts to successfully use the deferral or capitalization method were undermined by the fact that “the outcome of experimental work always involves a degree of uncertainty.”⁷⁴⁸ Despite the findings of these two reports and their conclusion that R&D continued to be an elusive object of accounting, and despite the debate demonstrating over the applicability of conventional cost controls in this area, many continued to misunderstand the lessons of the war.

Among those ascribing to the R&D production model, which related the inputs of expenditure to the outputs of sales income, were accounting theorist advocating the logic of the matching principle, many members of the public accountancy elites, and a new generation of investors, brokers, and analysts of the financial sector. By the late 1950s, prices in the stock market had begun reflecting the country’s enthusiasm for President Kennedy’s space-age brand of militarism. Suddenly, the conservatism of past generations was out, and the magic of the Go-Go Years was in. Looking back on those years, Lee Davenport, head of R&D at General Telephone and Electronics, recalled how, “In the fifties and sixties, R&D was God as far as the world of Wall Street was concerned. If you had an R&D program, even if they didn’t understand it, you were a progressive

⁷⁴⁸ NACA Research Series No. 29, “Accounting for Research and Development Costs,” *NACA Bulletin*, Section III (June 1955), 1378

company.”⁷⁴⁹ For many investors and analysts of the 1960s, a firm engaged in R&D could never be overvalued and was, *ipso facto*, a promising source of capital gains.

Not everyone on Wall Street was so smitten. Having lived through the Great Depression that emerged in the wake of the 1929 market collapse, more seasoned observers of the market warned against valuation practices which “discounted the hereafter” built on flimsy forecasts in which the R&D mystique played a leading part. A brief recession in 1962 brought an even briefer pause in the escalating frenzy over companies in the glamour industries. These were companies in the still-forming electronics industry and in the recently-branded aerospace industry, most of which relied on military contracts the way you or I depend on air to breathe. The moment of reflection soon passed, and by the middle of the decade, right around the time the US entered the Vietnam War, stock speculation had picked where it left off.

Companies that received R&D contracts, companies that sponsored their own R&D programs, and companies that had “high-tech” sounding names all saw their stock prices skyrocket, and for a host of reasons—such as increasing inflation in the latter half of the 1960s; efforts to fend off hostile takeovers from acquisition-hungry conglomerates; pressures on managers for earnings growth from stockholders and company incentive schemes; and an influx of unseasoned MBA-graduates into managerial roles, among other factors—a growing number of R&D-intensive firms switched accounting methods from expensing to deferral. To the auditor elites of the accounting profession and, later, to the new generation of macroeconomists, the significance of practical problems faced by firms were pushed to the distant periphery in service of advancing theoretical precepts.

⁷⁴⁹ As quoted in Buder, 97

Although the epidemic of earnings manipulation in the 1960s ultimately came to the surface by the early 1970s and fully exposed the failures of accounting leadership to the public, calls for reform by thoughtful practitioners, scholars, and, yes, even a few in government, had been ongoing for more than a decade.

Despite the years of struggle to reform accountancy culminating in the creation of a new standard setting organization and that organization's rejection of R&D as capital, those little acquainted with the problems of valuing, controlling, and organizing R&D have succeeded in translating theories of R&D capital into actual policies that reward R&D spending with what I view to be grossly negligent tax breaks. When distanced from the failure and duplication of effort that necessarily attends the experimental process, R&D came to be seen by policymakers as another magic bullet, one that could be reliably expected to yield a stream of marketable products and more efficient production processes to benefit the nation at large. Taught to generation after generation of students, the toolkit of macroeconomic growth theory shifted R&D away from a practical means of achieving a desired end-in-view, and towards a more abstract understanding that rendered it a *telos*, a pure end in and of itself.

For many years, the matching principle and its twin, deferred charges, were to accounting leadership what production functions were to macroeconomists: *a priori* Truths against which reality would be tested. In other words, experience was presumed to be effectively guided by theory. Unlike in R&D macroeconomics, the events of the 1960s dissolved this fantasy: accounting theory had been used to justify abuses that had measurable impacts on the public. Macroeconomics, by contrast, only comes into contact with experience at the level of national policy—a much harder venue for adjudicating consequences. In both cases, accounting and economics, it was only by denying the

uncertainty of R&D that a causal model relating R&D expenditures to growth of income became possible. As the RAND economist Burton Klein once said, “the uncertainties of the future cannot be resolved by pretending that they are certain.”⁷⁵⁰

This lesson should resonate across disciplinary boundaries and institutions with every single researcher, scholar, experimenter, thinker. But old habits are, by definition hard to break, and in the face of an indeterminate situation, many can and do shrug off the challenge that *indeterminacy*, the challenge that *uncertainty* poses to our most routine and/or preciously-regarded habits. Any form of analysis, wrote Dewey, that “in its quest for certainty ignores the reality of the uncertain in the ongoing processes of nature ... is marked at some point by a division of its subject-matter into the truly real and the merely apparent, a subject and an object, a physical and a mental, an ideal and an actual, that have nothing to do with one another, save in some mode which is so mysterious as to create an insoluble problem.”⁷⁵¹ Truly Real vs. Merely Apparent. Subject vs. Object. Physical vs. Mental. Ideal vs. Actual. These are the inheritance of Western civilization dating back to antiquity. It is time we disposed of them.

Even in the histories in the 1980s that were purportedly about R&D rather than “pure research” or “pure science,” we find ongoing commitments to the idea that only a certain type of individual with certain types of credentials possesses the ability to perform research that is recognizable as “knowledge-producing.” In economic history, essays and books continue being published by such well-regarded figures as David C. Mowery, Nathan Rosenberg, and Joel Mokyr, among others, that perpetuate the same problematic

⁷⁵⁰ Burton Klein, “A Radical Proposal for R. and D,” *Fortune*, 57, no. 5 (May 1958): 112.

⁷⁵¹ John Dewey, *The Quest for Certainty: A Study of the Relation of Knowledge and Action* (Gifford Lectures, Univ of Edinburgh, 1929): ???

“science vs. engineering” and “knowledge-producing vs. knowledge-using” dualisms. Those with science degrees hold no monopoly over the ability to recognize and responding the uncertainty of a problem or felt need in a thoughtful and controlled way. Anyone can do this, with or without any degree at all, so long as the habit of inquiry is learned and rehearsed over time.

It is up to educators to see to it that this habit of thought/practice is instilled in every class, through every activity, and in all areas of study, from the humanities on through the arts, the social sciences, and the laboratory sciences. And it is up to scholars to demonstrate this habit of thought in their work by clarifying the indeterminate situation that motivated their inquiry in the first place. This much has been remarkably demonstrated in an essay that recently appeared in the *Journal of Business Ethics*, describing what Dewey would describe as the authors’ own ends-in-view: “we wanted researchers to think about everything they do as ethics-laden by considering the purpose and consequences of their actions. This includes how variables are defined, decisions about which analytic strategy to use, what counts as observation or measurement, and importantly the kind of people who do quantitative research and the purpose of it.”⁷⁵² Incorporating such inquiry into the classroom and into scholarship would go a long way to encouraging research that is responsive to problems in the present in light of past experience, including not only successes but failures, uses, abuses, and reforms, as well.

8.2. The Actions of Knowing and Doing

⁷⁵² Michael J. Zyphur and Dean C. Pierides, “Making Quantitative Research Work: From Positivist Dogma to Actual Social Scientific Inquiry,” *Journal of Business Ethics* (May 2019): 2.

By adopting a pragmatist genealogical approach, I reinforce a call for “an alternative framework for analysis” made by accounting historian Barbara Merino. In 1993, Merino recommended pragmatism as a means of studying the development of accounting and she did this much by drawing together Foucauldian genealogy and Deweyian inquiry. Merino thus celebrates the genealogical method and the pragmatist’s “demands [for] an active human agent” and its rejection of “determinism in any form.”⁷⁵³ Others outside of STS, such as Colin Koopman, echo this message. Within STS, Mario Biagioli clarified some of the barriers that slowed down the pragmatist orientation that is now starting to pass.

Since its beginnings in the 1970s and 1980s, scholars that we now identify as the founders of STS, set out to flatten dualisms of the kind listed in the previous section. Over the past decade or so, we have witnessed increasing references to and use of classical pragmatic thinkers William James and, to a lesser extent, Dewey. The path getting STS to this point, however, has been circuitous, prolonged, and still underappreciated. With each wave of internal debate, scholars have bumped up against the problem of how to better articulate the gap between nature and society. Shifts within STS have been marked by the introduction of new conceptual devices designed to bridge this gap: the social construction of technology, co-production, boundary objects, mangles of practice, tacit and explicit knowledge, social epistemology, Mode-1 and Mode-2 science, sociotechnical imaginaries, embodiment, and so on. Some of these devices may have been more

⁷⁵³ Barbara D. Merino, “An Analysis of the Development of Accounting Knowledge: A Pragmatic Approach,” *Accounting Organization and Society* 18, no. 1/2 (1993): 163.

successful than others, yet all of them contribute to what Mario Biagioli called STS's "peculiar metaphysics of presence."

Biagioli's point here is crucial to understanding the promise for STS that lays in a combination of classical pragmatism and historical genealogy. For this reason, consider the following discussion by Biagioli:

Science studies intellectual agenda still focuses on the age-old question of how one gets from many competing claims down to one. ... In its most radical expressions, science studies has turned upside down received views on the production of scientific knowledge, which presented nature itself as the ultimate arbiter of knowledge. But while placing society and not nature in the arbiter's seat, these radical revisionists moves have maintained a causal epistemological framework—one that is ultimately rooted in the nature-society dichotomy. ... Although the field is vocal about the need to question the separation between nature and society, it usually ends up reinforcing such a dichotomy by casting its analyses as attempts to 'bridge' it rather than to do away with conceptualization of the problem that would then cast bridges as the natural solution. ... Science studies has reproduced, *mutatis mutanda*, the traditional distinctions between the 'context of discovery' and the 'context of justification,' and has dealt almost exclusively with the latter.⁷⁵⁴

Though this critique is now nearly twenty years old, STS as a field is still struggling with the same issues. Some, however, have demonstrated concerted efforts to move away from this gap-bridging activity. With respect to pragmatism, Latour is one of the more outspoken of the group.⁷⁵⁵ Other scholars were named in Chapter 1. It is no coincidence that across their work, increasing reference has been and is being made to classical

⁷⁵⁴ Mario Biagioli 2001, 274-5.

⁷⁵⁵ "If by 'epistemology' we name the discipline that tries to understand how we manage to bridge the gap between representations and reality, the only conclusion to be drawn is that this discipline has no subject matter whatsoever, because we *never* bridge such a gap—not, mind you, because we don't know anything objectively, but because *there is never such a gap*. The gap is an artifact due to the wrong positioning of the knowledge acquisition pathway. We imagine a bridge over an abyss, when the whole activity consists of a drift through a chain of experience where there are many successive event-like termini and many substitutions of heterogeneous media. In other words, scientific activity raises no especially puzzling epistemological questions"(Latour 2007, 95).

pragmatism. For pragmatism, there is no need to build a bridge between subject and object, nature and society, mind and matters, knower and known, etc., and for the simple reason that there is no gap. We are continuous with nature, and knowing is an action that we perform on and with objects, both tangible and otherwise.

8.3. A Guide to Future Inquiry

The original set of questions motivating the present inquiry were, “What is R&D? For whom, and for what purpose?” Importantly, though, a pragmatist genealogy can only be successful to the extent that the antecedent condition of inquiry—the initial sense of doubt or uncertainty that permeates a situation and qualifies it as indeterminate—is transformed into a tractable problem, one possible of solution. To achieve this necessary condition therefore required a further narrowing of scope. Towards this end, the present inquiry has focused on the processes by which R&D became problematized by practitioners of accountancy or, more specifically, the institutions, techniques, politics, and individuals whose theoretical and practical habits shaped the conditions in which different kinds of R&D accounting became possible.

In addition to establishing a more tractable and meaningful inquiry, focusing on accountancy has had the advantage of pointing up a site of valuation often overlooked by STS scholars. Nevertheless, in taking this route, I have necessarily foregone many others, all of which suggest themselves for further inquiry. Many events, individuals, problems, and institutions have been necessarily minimized or else entirely overlooked. Among the areas of study left out is the postwar (re)birth of venture capital markets that began with what is often identified as the nation’s first venture capital firm, American Research and Development Corporation. On a related note, another site of valuation left unexamined

is the bond market and, more generally, the whole range of securities aside from common stocks. Some work in this area has already been completed by Mary O’Sullivan and Paul Miranti.⁷⁵⁶

Also sidelined is the rise of empirical accounting research, a positivist methodology that began to emerge in the US in the early 1960s and soon came to dominate academic accounting theory and education, both domestically and abroad.⁷⁵⁷ This area was briefly alluded to in the quote found in Chapter 7 from Frances Stone of the Financial Analysts Federation. In her presentation for the FASB at its public hearing on R&D, Stone critiqued the efficient markets hypothesis which undergirds much empirical accounting research. The only other instance in which this body of so-called “empirical” research appears is in Chapter 1 where developments of postmodern accounting theory were discussed. The challenge posed by this body of theory began outside the US in the 1980s and 1990s. By that time, the scientism of empirical accounting research had spread globally and, with the ascendancy of “agency theory” and the “shareholder value” trope, helped naturalize the worst of managerial decision-making.⁷⁵⁸

Additionally, while Chapter 2 presented a brief history of modern corporation law, this discussion could be drawn out forward into time so that a fuller picture of how

⁷⁵⁶ Mary A. O’Sullivan, “Living with the U.S. Financial System: The Experience of General Electric and Westinghouse Electric in the Last Century,” *Business History Review*, 80, no. 4 (Winter 2006): 621-655; Paul J. Miranti, “Innovation’s Golden Triangle: Finance, Regulation, and Science at the Bell System, 1877-1940” *Business History Review* 2016 (Summer 2016): 277-299.

⁷⁵⁷ Wai Fong Chua “Radical Developments in Accounting Thought,” *The Accounting Review* 61, No. 4 (October 1968): 601-32; Delfina Gomes, “The Interplay of Conceptions of Accounting and Schools of Thought in Accounting History,” *Accounting History*, 13, no. 4 (2008): 479-509.

⁷⁵⁸ Rather than develop means by which abuses could be prevented, this body of research spit out a whole range of strategies all predicated on the assumption that, with the proper collection of incentives and reward schemas, managers’ perfectly rational abuses of power could be channeled in such ways as to benefit company stockholders.

the evolution in state incorporation statutes has influenced the meaning and use of “capital.” In the 1950s, for example, the American Bar Association worked hard to encourage a federal incorporation law which would help institute more uniformity across states. A better understanding of what victories the ABA won in this effort and where it failed, could help elucidate some of the problems of competition among states that result in watered down restrictions on what corporate managements are free to do, in the famous words of Louis Brandeis, with “other people’s money.”⁷⁵⁹

A crucial discussion absent from this dissertation has been the highly fraught development of R&D contracts. These contracts were modeled on procurement contracts during World War II and went through many changes not only during that war but in the subsequent wars that followed. These changes introduced new freedoms and new constraints, especially when President Kennedy took office and appointed Robert McNamara the new US Secretary of Defense. McNamara, and the other “Whiz Kids” recruited into the Pentagon from the Ford Motor Company, revolutionized R&D contracting on the basis of assumptions built into systems analysis. Understanding both these elements, the evolution of contracts and the expansion of systems analysis, is crucial to a broader historical study of R&D. It will require in-depth study of declassified archival documents that can be supplemented by the many histories written about science in the military. Some of the best of these histories are those written by graduate students in

⁷⁵⁹ Brandeis used this phrase in reference to bankers, not corporate managers, but the effect is the same. In a series of articles published in *Harper's Weekly* following the successful presidential campaign of Woodrow Wilson, Brandeis argued that businessmen were colluding with the larger banking houses to create trusts that had become so large as to hurt industrial efficiency and stifle competition. These articles were compiled and printed in book form in 1914 with the title, *Other People's Money and How the Bankers Use It*. A couple years later, Wilson appointed Brandeis to the US Supreme Court. Though the appointment was bitterly contested in Washington, Brandeis served as an associate justice on that court from 1916 to 1939.

military and naval academies, almost all of which have been overlooked by scholars in traditional humanities departments.

In the latter chapters, many references are made to the business press. Left out, however, is the story of how magazines such as *Forbes*, *Fortune*, *Businessweek*, and *The Wall Street Journal* developed their own journalistic cultures and areas of expertise, on the one hand, and their patterns of financing and the means by which they underwent explosive growth, on the other. *Businessweek*, for example, hired economic experts such as Douglas Greenwald, Dexter Keezer, and Leonard Silk who translated new economic models into digestible bits for the readers. Silk, for instance, was a Duke University-trained economist who became editor at *Businessweek* and drew upon his many (unnamed) articles in that journal when writing *The Research Revolution*. *Businessweek*'s publisher, McGraw-Hill, released this widely-read text in 1960 with an introduction by the famed Harvard Business School economist Wassily W. Leontieff. That the publisher was quite taken by what Silk called “the research revolution” is evident from the company history written by Robert Burlingame and titled, *Endless Frontiers: The Story of McGraw-Hill* (1959), a clear allusion to Vannevar Bush's *Science—The Endless Frontier*.⁷⁶⁰

Other areas not addressed by this dissertation and which offer additional avenues for research include the unwritten history of the US Tax Court, where many corporations following World War II found themselves defending their methods of R&D accounting

⁷⁶⁰ Starting in 1958, McGraw-Hill also conducted a survey of American industrial research and development as part of the McGraw-Hill questionnaire on business investment plans. The data from these surveys was used by many economists to supplement or replace the less useful, aggregate-level data reported by the NSF. See: Douglas Greenwald, “The Annual McGraw-Hill Research and Development Survey,” in *Methodology of Statistics on Research and Development Costs and Manpower*, ed. _____ (Washington DC: National Science Foundation, 1959), 53-4; Dexter M. Keezer, “The Outlook for Expenditures on Research and Development During the Next Decade,” *American Economic Review* (May 1960): 365-6

against the claims made by Internal Revenue agents; changes as well as continuities in American business school curricula in general and in MBA and accountancy programs in particular following the publications in 1959 of *Higher Education for Business* (1959), a report by Robert Gordon and James Howell commissioned by the Ford Foundation, and *The Education of American Businessmen*, a similar report by Frank Pierson commissioned by the Carnegie Foundation; and finally, the formation and evolution of the Industrial Research Institute, the Directors of Industrial Research, The Research Corporation, the Arthur D. Little & Co., and other venues where R&D was performed on contract for client corporations.

The lessons to be learned from the history of accounting for R&D extend to economists and policymakers no less than to educators and scholars other areas of practice. While growth theorists continue to this day to persuade policymakers that R&D is a capital investment just like any other, the consequences of this line of thinking were forcibly brought to the attention of accounting leadership decades ago, by the end of the Go-Go Years. Their decision to require current expensing, to deny the capitalization of R&D, has the distinct advantage of leaving intact the uncertainty of R&D and all the problems such uncertainty necessarily engenders. These problems are interpersonal and organizational, no less than technical, concerning how to account for an activity that resisted traditional control measures, that did not guarantee specific results, and that required a learned tolerance for dead-ends, false-starts, and doubt.

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