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George J. Staubus

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AUTOBIOGRAPHICAL

George J. Staubus
UNIVERSITY OF CALIFORNIA, BERKELEY

AN ACCOUNTANT'S EDUCATION

Abstract: This is a review of how various experiences in my career have contributed to my understanding of accounting. I recall the circumstances surrounding several of my efforts towards the development of accounting theories, viz. (1) decision-usefulness theory, (2) activity costing, and (3) market simulation accounting, as well as my excursion into (4) market association research in seeking to validate decision-usefulness theory and (5) a search for the effects of firms' economic environments on the development of enterprise accounting in the 2nd millennium, C.E. I give my impressions of several of the important players in the evolution of accounting thought in the 20th century with whom I was closely associated, such as Vatter, Moonitz, Chambers, and Sterling, as well as other prominent figures in the broad field of accounting. Some of my gains from associations with three institutions—the American Accounting Association, The University of Chicago, and the Financial Accounting Standards Board—are identified. I conclude with a few summary thoughts on what I have learned.

Education is a life-long process—and includes learning that clichés can be useful—but waiting until the end to write about it does not seem to be a sensible plan. So I shall prepare an interim report, not in the belief that my experience is to be recommended, but in the hope that the preparation of such a report will yield a modest increment to that life-long process. I might learn something from this exercise.

Much of my teaching and research can be thought of as pieces of my education. I developed the decision-usefulness theory of accounting because I did not know how to account to investors successfully. I embarked on my lonely and frustrating quest for empirical evidence of useful accounting information in a search for answers to several accounting questions raised by decision-usefulness theory. Activity costing was the product of my concern that I did not know how to produce decision-useful

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cost information. Later on, market simulation accounting grew out of a search for a concise theory of accounting measurement in the context of generally accepted accounting principles. *Economic Influences on the Development of Accounting in Firms* [1996] is the product of a senior academic's search for an understanding of how accounting got to be what it is. All of it has been educational—for me. Other scholars have become interested in several of those quests.

In this report, I shall review the development of my thinking in those five subject areas as well as lessons from my associations with a few great figures in the 20th century history of accounting (Wixon, Vatter, Moonitz, Chambers, Sprouse, Sterling, Gellein, and Carsberg) and with several important institutions: the University of Chicago, the American Accounting Association, and the Financial Accounting Standards Board.

IN THE BEGINING

My "formal" education commenced inauspiciously: in a one-room country school in northern Missouri, where I skipped the third grade for questionable reasons. High school was limited, too: no foreign language, emphasis on vocational agriculture, and poor results in "freshman algebra". When I graduated from high school, I did not have a very good base for higher education and I was too young (barely 16) for university. The fifth (postgraduate) year was more productive, including chemistry, bookkeeping, and a flourishing social life, for a change. Then, after a one-year delay, my university undergraduate years started (by courtesy of the U. S. Navy—during "the war") with two semesters of fairly general required courses, followed by four semesters of business-oriented courses, including considerable accounting (in which I gradually lost interest). In my last (post-navy) undergraduate year, several chance events turned me towards an academic career: an economics of transportation course that turned out to be technical microeconomics because the graduate student instructor was interested in the latter and unprepared to teach transportation; an interesting course in what is now called organization theory from a professor who gave me my first academic appointment, as a paper grader in another course; a public finance course by a stimulating economics professor in which I wrote my first analytical paper (on the incidence of a general sales tax); and a tip by a former navy roommate that led to a full-time teaching appointment (after

one quarter of graduate work). That, in a nutshell, describes my first 16 years of organized education—to the point where I began to learn by teaching.

The University of Buffalo (now State University of New York—Buffalo) was the scene of my first teaching effort. Hired as a full-time, full-charge instructor at the age of 21 armed with a Bachelor's degree, a new level in my education began. I seriously question what my students got from me during my two years at Buffalo, but I can say that I got a good start on my higher education. Rufus Wixon was the new young chairman, straight from William A. Paton's program at Michigan. "Wix" subsequently moved to Wharton for a long career. All of the young instructors took his year-long seminar and learned accounting theory according to Paton. Paton and Littleton's *An Introduction to Corporate Accounting Standards* [1940] was Wix's bible but it was completely new to me. Matching costs and revenues made sense, and costs surely should be attached to the asset to which they were applied. I even understood the difference between expired and unexpired costs—for a while; later I dismissed it [1987]. I don't think I was nearly as clear about revenues as the product of the enterprise and rejected that notion when I got to my dissertation. Indeed, over the next ten years or so I relaxed my firm embrace of Paton and Littleton, relegating it to the status of one of my old friends. Wix also taught us price level adjustments. My biggest disappointment came on the final exam in the second semester: a young lady whom I thought understood less than I did trounced me in that exam. I was discouraged, but found a good solution the following year: "If you can't beat them, join them". Sarah and I have now been married long enough for me to learn a great deal from her, including some accounting.

Meanwhile, I spent three summers in the University of Chicago MBA program with excellent instruction by Cletus Chizek and Kullervo Louhi in the customary advanced practical accounting courses, including some repetition of material I supposedly had covered in my undergraduate courses. Clete was a partner in Crowe, Chizek and Co. in South Bend, Indiana who took the train into Chicago on his teaching days. Kullervo was the best all-around unpublished faculty member I have ever known, who soon moved to Michigan State and quickly became the dean there. Unfortunately, his poor health was to cut his life short.

In retrospect, I see that my education to this point could be characterized as haphazard, much of it not consciously chosen

by me, but with a gradually increasing sense of direction on my part. My interests were beginning to take shape.

UNIVERSITY OF CHICAGO, 1949-1952

Approaching the end of my second year at Buffalo, I visited Chicago in connection with my application for the Ph.D. program. William J. Vatter was the *de facto* leader of the accounting faculty who introduced me to various colleagues, the most memorable of whom was Willard Graham. After listening to my background, which took about ten seconds, Willard exclaimed: "Green as hell, huh?" I acknowledged the accuracy of his observation, and we got along fine thereafter, although I did not see much of him. One relationship involved technical discussions when Willard was chairman of an American Accounting Association committee on price-level adjusted accounting, a subject that I knew well, thanks to Wixon's instruction at Buffalo. Willard's main job during my time in the Ph.D. program was managing the Executive Program; I never had a course from him. Later, he moved to North Carolina and became president of the AAA.

The accounting group on the third floor of Haskell Hall was a friendly bunch. David Green and I, as students and instructors, mixed well with Paul Kircher, Kullervo Louhi and Bill Vatter. I shared an office with Paul, who came from Michigan and went on to UCLA, and took his Accounting Theory course. My main paper for that course was my first publication: "Payments for the Use of Capital and the Matching Process," *The Accounting Review* [1952]. Paton and Littleton and Wixon had taught me that costs (which, from my residual equity point of view, included interest) attach and should be matched with revenues, a view that I stuck to right through my work on capitalization of interest at the Financial Accounting Standards Board (FASB) and in *Activity Costing and Input-Output Accounting* [1971]. Bill Vatter was the intellectual stimulus, Kullervo Louhi the reliable master of all relevant trades whose solutions to the dozens of major "accounting problems" that we taught in the advanced problems courses as well as in managerial accounting were treated as of biblical origin by the rest of us. We gathered in Bill's office every afternoon to share the coffee that he made and for wide-ranging conversations that always included Bill's strong views, whatever the topic. That close knit group—only five since Willard Graham spent most of his time in his downtown office and his campus office two

floors below us—worked as a team and excelled in intragroup communication, a learning opportunity that was never matched at the University of California.

*Vatter and Managerial Accounting***:** The most exciting developments in accounting thought in that setting were encompassed in Bill Vatter's managerial accounting. We saw Bill bang away at his old manual typewriter for hours in the morning, typing directly onto Ditto masters as he composed his thoughts. Then, five minutes before his class time he dashed into the adjacent closet and cranked out enough copies on the Ditto machine to provide one for each student, distributing them *sans* collation. A Prentice-Hall editor got wind of this development and talked Bill into permitting the printing of a "temporary edition" of *Managerial Accounting* [1950]. As it happened, Bill and his wife, Rose, were in our student apartment for dinner one night when I asked him, "Well, how do you like the looks of the book?" "What book?" "Managerial Accounting." "I haven't seen it". So I showed him my copy. It was probably the most influential textbook ever published in accounting—at least since 1494. Other books sold many more copies, of course, but I doubt if any other book in managerial accounting could be considered seminal. It marked the birth of the subject—and the withering of cost accounting as a course title. Over the next few decades, managerial accounting, or management accounting took over. *Managerial Accounting* was dropped by Prentice-Hall and Vatter after 17 printings—presumably a record for a temporary edition—without ever being revised as a permanent, cloth-bound book. Bill could not be bothered to dress it up for the occasion.

The heredity of managerial accounting at Chicago—"management accounting" in many other places—is worth noting. J. O. McKinsey, a prolific author on accounting, management, and managerial accounting subjects in the 1920s, and head of a growing consulting firm, was the University of Chicago accounting professor who introduced some of the ideas; others came through the well-known Chicago economist, J. M. Clark, whose *Studies in the Economics of Overhead Costs* [1923] especially Chapter IX, the title of which included the phrase "Different Costs for Different Purposes," continues to be recognized as an early milestone in managerial accounting. Vatter's book was especially important as "the teacher's book" in the 1950s; it was too difficult to be popular with students. The task of making managerial accounting readily accessible was picked

up by Charles T. Horngren, whose *Cost Accounting: A Managerial Emphasis* [1962], must be familiar to virtually every person teaching in that area in the U.S. today, or who has taught the subject in the last 40 years, as well as most teachers in other English-speaking countries. Chuck, the great communicator, took my desk as a new Ph.D. student and instructor when I left Chicago in 1952, so he had the benefit of five years of intimate association with Vatter until Bill moved to Berkeley in 1957. He has always acknowledged his debt to Bill. Indeed, when I undertook to raise money for a Vatter Room in Berkeley's new business school building in the early 1990s, Chuck, long dedicated to Berkeley's archrival in sports, Stanford, was the largest contributor.

My *Activity Costing and Input-Output Accounting* [1971] might also be recognized as a minor step in the development of the Chicago School of managerial accounting—possibly the most practically significant development of accounting thought in the 20th century.

Prior to *Managerial Accounting*, popular cost accounting textbooks were focused on the calculation of unit product costs in job order, process and standard cost systems. Those cost numbers were visualized as necessary for the preparation of financial statements and, secondarily, for control. Bill painted a different picture in his Chapter 5, "The Nature of Managerial Accounting." "The other view—with which we are here concerned—is that accounting data and procedures are intimately connected with the processes of operation and management of the business enterprise; that *accounting is a part of management*. The data upon which a large part of the fact-finding associated with *managerial decisions* is based are largely accounting data; policies, strategies, and other plans are seldom established without some reference to accounting records" [p. 97, emphasis added]. And [p. 102]: "(T)he decisions of management are always forward-looking in the sense that history is irrelevant except in so far as it may be a basis for forecasting". It follows that ad hoc cost calculations—different costs for different purposes—were a major part of managerial accounting. He gave a great deal of emphasis to management's need for information about parts of a business entity, and he saw the accountant as part of the management team, not as an adversary residing in an ivory tower. This view is well understood now, but it was not before Bill Vatter came along.

I think it was clear to those of us working with Bill in the early 1950s that his greater interest and his strength lay in

managerial accounting. Yet, his interest in and contributions to external reporting were also noteworthy. To many—scholars not working in managerial or cost accounting, such as Maurice Moonitz—his *Fund Theory* [1947] was Bill's most important work, and one that graced the reading lists of most "Accounting Theory" courses in the third quarter of the 20th century. I think I shall never see a more eloquent case for theory than its first paragraph:

> Every science, methodology, or other body of knowledge is oriented to some conceptual structure—a pattern of ideas brought together to form a consistent whole or a frame of reference to which is related the operational content of that field. Without some such integrating structure, procedures are but senseless rituals without reason or substance; progress is but a fortunate combination of circumstances; research is but fumbling in the dark; and the dissemination of knowledge is a cumbersome process, if indeed there is any knowledge to convey [p. 1].

The absence of such a conceptual structure has driven me for half a century.

Bill also introduced the "Direct Method for Preparation of Funds Statements" [1946] which, over succeeding decades (accounting has never been accused of rushing, except by vested interests resisting changes in GAAP), gradually became a popular working paper approach for preparation of funds and cash flow statements. I am sure that today's students and practitioners have no idea how much they owe Bill Vatter for that ingenious, now obvious, practical invention. Another illustration of the man's breadth was his monograph on tax planning for The Controllership Foundation [1951], the first such work to be published, to my knowledge. I had the privilege of working as a research assistant on that project, which certainly gave me an appreciation for the importance of tax planning.

Bill's hobbies were wide ranging, starting with his choice of music as a career. He studied string instruments and French Horn at the Cincinnati Conservatory of Music (while studying other subjects at the University of Cincinnati) but the only instrument I heard him play was the piano. By contrast, he loved to work on his cars, which included a Lincoln and a Packard, among others, at different times. He did not collect them. His travels included driving to Alaska from Chicago and a Fulbright Award to Australia, where he was commissioned to advise on education for careers in accounting. His life has been reviewed

more carefully by Chuck Horngren [1991] and Rodney Rogers [1993].

Bill was firm in his convictions about accounting and never hesitated to disagree with views that seemed wrong. He was not always diplomatic, was sometimes blunt, and could be obstinate, as Maurice Moonitz found him in an AAA committee meeting. He was a modest man with no exalted opinion of his own work. As chairman of the accounting group at Berkeley when Bill reached the mandatory retirement age in 1972, I wanted to have some sort of celebration of his career. Recognizing that Bill was associated with Chicago more than with Berkeley, I asked Chicago's Sidney Davidson to cooperate; he, of course, was quite willing. Then I sought Bill's cooperation when he and his wife, Rose, were at our home one evening. He complained that no one would be interested: "I know what they'll say. What did that old Vatter ever do anyhow?" Sarah and I were incredulous. I pushed on, describing the type of conference that Sidney and I had in mind. Bill's definitive response was: "I won't come. And if you trick me into it I'll walk out". After some more pleading, Rose said: "I think he means it, George." So it never happened. Bill retired, moved to southern California near their daughter and family, taught one semester at The University of Southern California, wrote a substantial review article for *The Accounting Review* [1979], and died September 15, 1990. He received the AAA Outstanding Accounting Educator award in 1984. The William J. Vatter Conference Room in the Haas School of Business, University of California, Berkeley is a modest monument to his memory.

At this point in my life, my education had made a lot of progress. I had been exposed very substantially to the Chicago philosophy: the Great Books and Robert Hutchins and Mortimer Adler approach to liberal education, the value of breadth and of freedom of thought, and the role of an educator as taught by Paul Klapper, a former president of Queens College, in a campus-wide, year-long seminar for future academics. For better or for worse, the University of Chicago was by far the most important of the five institutions of higher education that I attended in shaping my approach to teaching and research, including the fundamental ideas that teaching and research should reinforce each other, that asking the right questions can be productive, and that major contributions must be heretical. Without that training, I don't see how I could have had even the modestly successful academic career I have had.

THE DECISION-USEFULNESS THEORY OF ACCOUNTING

Many episodes in the development of the decision-usefulness theory have been noted elsewhere [Staubus, 2000]. These include its origin in a draft of my dissertation [1954]; rejection of much of it by my dissertation committee; articles in *The Accounting Review* in the 1950s presenting parts of it [1958, 1959]; numerous revisions of the whole; and a lengthy struggle for publication (1956-60). Eventually it was published under the title *A Theory of Accounting to Investors* [1961]; was developed further [1970, 1971, 1972]; was presented in its most complete form as *Making Accounting Decisions* [1977]; and eventually was accepted around the world by English-speaking accounting standards setters and academics (but not by preparers of financial reports). These are the milestones that are matters of fact.

Here I offer a few more speculative observations and some personal opinions. Acceptance of new theories in social sciences typically is a slow process. I once wrote [1975, p. 164] that publishing a new theory in accounting is like dropping a rose petal into the Grand Canyon and waiting for the echo. An early start and good health increase the chances of seeing results in one's lifetime. Like many other ideas in the broad field of economics, decision-usefulness theory may be seen as "no big deal" because it is so obvious. I must say, however, that when I was seeking a conceptual base for a serious study of revenue in 1951, the complete absence of an objective of accounting in the literature struck me as a major handicap—one that I must overcome. Feeling the need for a general objective led me to the idea that financial information should be useful in making decisions. I am not sure I would have reached that point in my thinking in any environment other than that of The University of Chicago.

Accounting did have objectives before 1951, but they were, according to my search, never made explicit. Even the so-called stewardship objective did not appear in the literature until later, and it was never used as a basis for a theory. If anyone had analyzed it or tried to build a theory on the stewardship objective, both the objective and the GAAP tied to it surely would have been found seriously deficient. For example, GAAP measurements were easily "gamed" by managements bent on manipulating earnings—a deficiency that remains substantially intact to this day. Stewardship cannot be evaluated using out-of-date measurements at both the beginning and end of the reporting period.

A study of the history of accounting with an eye to inducing objectives suggests that one of the first reasons for recording aspects of economic events was to aid the human memory; a merchant needed to keep track of who owed him how much, of how much he owed specific creditors, etc. Perhaps one reason for closing the loop with a genuine equation was a businessman's interest in his own wealth and his success in enhancing it over time. When partners were added, the entity's income had to be known before it could be divided. With branches and other delegations of managerial responsibility, accounting's contributions to control must have been valued. Later, the roles of financial statements in bank financing and taxation became important. But by 1951 the uses of financial reports by absentee investors—both creditors and owners—could no longer be neglected if the profession were to serve the public to anywhere near its potential. These various reasons for accounting were occasionally identified, but they had never been consolidated and used as a base for a theory. The decision-usefulness objective encompasses all of them, brings uses by investors to the fore, and builds on the core objective: to provide information useful in making a wide range of decisions.

Once I had come up with that objective, building a coherent theory based on it was an exciting activity to me—one on which I concentrated for several years and returned to from time to time for 25 years, before I felt it was substantially complete. In the 1950s, editors were not easy to convince, so publications came slowly. Roughly speaking, ten years elapsed between the time I hit on the decision-usefulness objective and the publication of *A Theory of Accounting to Investors*. In the meantime, parts of the theory, especially the beginning point—the statement of the objective of accounting—appeared in my dissertation and in two articles in *The Accounting Review* [1958, 1959]. But its influence in the literature was almost nil.

There were, however, two cases of recognition of the decision-usefulness objective during the decade of my struggles, both published in 1955. One was Ray Chambers' "Blueprint for a Theory of Accounting" in which he explicitly stated the decision-usefulness objective but did not proceed to review users' needs, the focus on residual equity holders, their interest in cash flows to themselves and to the firm, etc—a gap that, in my opinion, was the fundamental reason for the divergence between his conclusions and mine in the measurement area, in which he was deeply interested. (See below for more on our differences.) The other was the AAA's Supplementary Statement No. 8. That

statement gave decision usefulness a central role in *disclosure* but did not apply it to accounting *measurement*. I wish I had inquired regarding how the decision-usefulness objective got into that disclosure statement. To my knowledge, none of the committee members had exhibited any recognition of or interest in that objective prior to publication of the committee report or soon thereafter. I am left with the speculation—perhaps a long shot—that the chairman, Tom Hill from the Massachusetts Institute of Technology, had gotten interested in it from my interview presentation on his campus in 1952. Neither Chambers nor any AAA group built a coherent theory on the basis of that objective in the 1950s.

The real turning point was the publication by the AAA of *A Statement of Basic Accounting Theory* in 1966. It was mailed to all Association members, had the prestige of the Association behind it, was brief and well written, and seems to have been well-timed. That document contributed immensely to the acceptance of the decision-usefulness theory for two reasons, in my judgment. First, it included significant components of the theory—five of the most basic of the 20 that I include in my diagrammatic outline [2000, p. 9]. Secondly, it was widely distributed. All of a sudden, nearly everyone writing on any topic in the general area of financial accounting theory explicitly or implicitly accepted the decision-usefulness objective. Key components of the remainder of the theory quickly became familiar to readers of academic accounting journals.

I have no first-hand information to explain why the AAA committee suddenly adopted the decision-usefulness theory. I should have made enquiries at the time; now it's too late. Looking at the make-up of the committee, I see that none of them had written on the subject, but I find two members that surely had some familiarity with the theory. My colleague, Lawrence Vance, had to have some knowledge of my work after 14 years of association in such matters as curriculum discussions, workshops, and my candidacy for promotion. The committee member most certainly acquainted with decision-usefulness theory was George Sorter, who had reviewed *A Theory of Accounting to Investors* for *The Accounting Review* and who, as a Ph.D. student at Chicago under my chairman (Vatter) might have read my dissertation. Indeed, his own dissertation and his interest in classification of accounting events appear to have been influenced by my detailed classification of accounting events in Chapter IV of both my dissertation [1954, 1980] and *A Theory of Accounting to Investors*. Furthermore, George's powers of per-

suasion and articulation of his thoughts have long been recognized by acquaintances as unsurpassed. Therefore, I speculate that he deserves much credit for the Committee's adoption of decision-usefulness theory. (Unfortunately, the committee report shows no literature search and no references to previous literature.)

After 1966, there seemed to be no resistance *in academic circles* to the decision-usefulness theory. Meanwhile, the Accounting Principles Board (APB) of the American Institute of Certified Public Accountants (AICPA) had foundered in the marketable securities sea and other dangerous waters and a new approach to establishing and changing GAAP was sought. The first step towards a new conceptual basis for establishing accounting standards was an enquiry into accounting objectives by the Study Group on the Objectives of Financial Statements, chaired by Robert Trueblood and assisted by George Sorter as research director. Again, I suspect, George's persuasive powers had their effect, but the voting members of the committee had minds of their own and included men who had been deeply involved in the accounting issues of the time, such as Oscar Gellein of Haskins & Sells, Frank Weston of Arthur Young and The University of Chicago's Sidney Davidson, as well as Trueblood himself. I am confident that they believed in the decision-usefulness objective. Furthermore, I have heard Martin Gans, the study group's administrative director, say, in an interview with Stephen Zeff, that the finance man in the group, Reed Parker of Duff, Anderson & Clark, pushed strongly for the future cash flow orientation—a key step in decision-usefulness theory. By that point in history (1971-1973), with the APB's troubles in everyone's mind, the time was ripe for a progressive-minded body of practitioners to follow the lead of the academics. Stewardship and historical cost were no longer acceptable to those few thought leaders who had been asked to apply themselves to the issue. The result was that the decision-usefulness theory was presented to a much wider readership. The report, like the AAA's, shows no evidence of a literature search, although my Australian study, *Objectives and Concepts of Financial Statements* (with John Kenley, [1972]) was mentioned, along with four other works, in the background section. By that time, the original hard-to-find, hard-to-read works on decision-usefulness theory were indirect sources. The Group's objectives became the starting point for the Financial Accounting Standards Board's "conceptual framework project" which carried through with much of the theory—until it got to the measurement part—but

acceptance across the entire profession has still not been achieved.

After acceptance of *A Theory of Accounting to Investors* for publication in 1959, I turned my attention to a number of more limited topics that fit the D-U paradigm. My serious return to the theory came in 1969-70 at the University of Kansas. Bob Sterling had invited me to fill the Arthur Young Chair as a visitor and to participate in the "Kansas Conference" where my role was to make a case for present value measurements alongside Yuji Ijiri (historical cost), Philip Bell (current replacement cost), and Ray Chambers (exit value)—to my mind, very exalted company. Oscar Gellein was asked to discuss my lengthy paper—an assignment that may have resulted in his subsequent support of my appointment at the FASB and tied into his leadership role in the development and promulgation of the FASB's conceptual framework. David Solomons—the right person for the job—played the role of appraiser of the alternatives. (I am sure I beamed when David said, as he launched into his evaluation of the ideas presented: "I suspect that from here on I'm going to sound a lot like George Staubus"). And from then on, I was more confident of the future of my theory.

As so many authors do, I interpreted my Kansas Conference assignment broadly. My paper included much further development of both the set of measurement methods available to accountants and the criteria for choosing among them. In *A Theory of Accounting to Investors* I ranked the various measurement methods on the relevance criterion, and I pointed out the need for other criteria (reliability, cost), but here I developed the "multiple criteria" approach to choosing a measurement method much more fully. The Kansas paper also developed the role of types of evidence of value and the linkages in the surrogate chain: the economic reasoning that permits net realizable value to be substituted for present value, replacement cost to be substituted for net realizable value, and historical cost to be substituted for replacement cost. The supply adjustment (what difference will it make?) approach to the cost of using a resource, which features so prominently in Activity Costing [1971, p. 17], was also started in that paper. I was pleased with the results of the conference; it gave me confidence that the D-U theory would eventually prevail. The paper was published in two parts: *Abacus* [Staubus, 1970] and the conference volume edited by Sterling [Staubus, 1971a].

I continued to develop decision-usefulness theory in various journal articles until 1975 and in the monograph I did for the

Australian Accountancy Research Foundation in 1972 (with John Kenley). Then I saw that a lot had been added since 1959, so I wrote a completely updated version of decision-usefulness theory, including a substantial collection of reprinted works and study questions, because I felt I (and perhaps others) needed a book that could serve graduate students better. (*Making Accounting Decisions* [1977]).

One of the building blocks of decision-usefulness theory that got a lot of attention in the 1970s was "inflation accounting". I had just barely managed to keep in my dissertation the reporting of gains and losses on net monetary assets or liabilities and the real gain or loss on nonmonetary assets, an innovation that was not widely recognized until the 1970s. Inflation and specific price changes were treated more fully in *A Theory of Accounting to Investors* and they got a lot of attention during the inflationary period in the U.S.—1972-1981—during which time I was deeply involved in those areas at the FASB. If I was "expert" on anything while at the FASB it was accounting for changing prices and I had a great deal of success in convincing the Board on much of the content of Statement 33. (I return to this subject below.)

The increasing acceptance of the decision-usefulness objective in the 1960s and 1970s was gratifying. It became generally accepted among academics, then among standards setters—folks who have reasons to think about that sort of thing. I also observed a few auditors working with standards setting bodies who seemed to accept it. However, the general failure of corporate managers and their sympathizers in the auditing profession to apply the decision-usefulness objective in practice has been disappointing; perhaps I should have foreseen it by trying to put myself in their positions. When I try to think about how I would look at reporting on my own performance, I can see that it is fanciful to expect managers to be objective. And when I recognize that auditors spend much of their work time and social time making friends with client's management personnel, it is clear that they, too, should be expected to lack objectivity. In this context, the few rogues that we see should not surprise us; the surprising phenomenon is the majority of conscientious accountants in preparer and auditor roles. It is easy to stand on principle when dealing with strangers; to do so against the perceived interests of your friends takes courage.

It has long been clear that the myth of auditor independence has been a weak link in the financial reporting chain. The three-party system in which *managers* hire friendly *auditors* to

report to external, unknown *investors* should not be expected to serve the latter well. In present-day corporate America, there is a strong tendency for managers to control the board of directors and the auditors, instead of the shareholders controlling the auditors and the board and thereby, indirectly, the managers. The classic model of the corporation—shareholders over board over management—is hard to find in public companies in America at this time. Corporate governance is in a sad state. I am not optimistic about the near-term futures of corporate governance and corporate reporting in America.

Finally, I have no favorable comments about those journalists and politicians who fail to distinguish between the vast body of dutiful accountants and auditors, the few genuinely rogue accountants and auditors, and the mix of helpful and unhelpful rules that make up generally accepted accounting principles. Let us be specific in handing out praise and criticism of "accounting". Is it the whole body of accountants, the few rogues, accounting standards in general, or the standards that have not been revised in the interests of financial statement users that we are criticizing? And who has stood in the way of such revisions?

"EMPIRICAL" RESEARCH

I use this heading to cover research on the association of financial accounting variables and securities values. In the course of my normative work in the 1950s I began to realize that I should try to find ways of providing empirical evidence on the merits of methods that my reasoning indicated "ought to be used" if one sought to provide financial statement information that could be used by investors to make better decisions than they could without it. Because I placed a great deal of emphasis on the relationship between cash flows from the enterprise to its investors and net cash flows to the enterprise, I felt that I could not be sure that depreciation accounting (and amortization of intangibles) improved the usefulness of a net operating flow measure. A major concern was that I could find no connection between (1) the amortization of historical cost according to a semi-arbitrary formula and (2) an economic cost to owners. Neither could I believe that, under the stable price level assumption, limited-life assets retained their original market-established values. That posed an extremely important accounting question: Is net income after deducting depreciation per GAAP superior to net income with no deduction for depreciation as a measure of

operating success for use in making investment decisions? That is, is depreciation accounting, as practiced, helpful? I did not know, nor did decision-usefulness theory tell me; it did not help me choose between two defective measures: income with an expense missing, or income with a poorly measured expense.

I had had one undergraduate course in statistics, but it did not cover the coefficient of correlation, r-squared, and least-squares methodology. I was, to say the least, statistically challenged. Unaware that I did not know how to carry out such a research project, in the summer of 1959 I hired research assistants—one of whom (Clarence Houghton) later became partner in charge of the Northern California practice of Deloitte, Haskins & Sells and, since his early retirement, has served with distinction as an adjunct professor at Berkeley—and we started collecting data on a random sample of companies to ascertain which of those variables—earnings or "cash flow"—was the more closely associated with the discounted values of common stocks. I was interested in *ex post* discounted values—assuming various holding periods from one to 12 years—of the dividend streams and terminal market values as a surrogate for value, as opposed to current market price, which, I reasoned, reflected current fads and misperceptions. At that time, allowing for varying risks among the stocks in the sample was not feasible as I, like other accountants, was unaware of beta as a measure of risk.

The work expanded to the testing of other accounting hypotheses—LIFO vs. FIFO, other versions of cash flow, tax allocation, nonrecurring items, as well as dividends and book value as indicators of investment value—and it went very slowly. The desk calculators of the time, later punched cards and then an IBM 701, were time-consuming for a novice quantitative researcher. Chicago's "CRISP" data were not available. Development of an alternative to the seriously limited least squares methodology (the median deviation percentage), a one-year leave of absence, and the learning of enough statistics to muddle through took additional time. Nevertheless, the seven years of work yielded some understanding of empirical research; several fairly convincing results, and several inconclusive results, including that pertaining to the original depreciation accounting issue; a few journal articles [Staubus, 1965, 1967a, 1968a, 1968b]; and some ability to follow the subsequent market association tests made by much better qualified researchers commencing in the late 1960s and continuing by the thousands since then. The totality of that effort is summarized in an un-

published report: "Testing Accounting Hypotheses" [Staubus, 1966].

I might add that I also learned that an insecure academic should not attempt research for which he/she is not qualified—without a qualified partner. If only I had had some training in research methods—even if far short of the great skills used so effectively by "the Chicago Boys" of the late 1960s, including the king of empirical research and all-round accounting scholar, William H. Beaver. To some of my friends, the most incomprehensible aspect of my struggle was that my office mate in the early stages was Tom Dyckman, who was teaching statistics as well as accounting—obviously exceptionally well-qualified for such work. As I look back on it, my failure to enlist his aid must have been due to a combination of my being too ignorant to understand fully how badly I needed help and Tom's lack of confidence in me as a possible partner. I guess I proved that I was a loner; I've only collaborated on two occasions. Later, when I was at the FASB, I did engage Tom to do a research study on the market response to the oil and gas exposure draft.

ACTIVITY COSTING

During the many years that I was working on what turned out to be *A Theory of Accounting to Investors*, I felt that the complete accounting academic should, over the course of a career, work on accounting to investors, accounting to managers, and accounting to government, thereby covering the major users of enterprise financial information. Two of the three is the best I can do, I'm afraid. My early acquaintance with managerial accounting *a la* Vatter and my ongoing work on developing the curriculum at Berkeley gave me some feel for cost accounting but no satisfaction with its state. In the late 1960s, I was spending a majority of my time teaching and writing on managerial accounting topics, and at some point I settled down to work on a major study of cost accounting—an example of a research project growing out of teaching. The immediate impetus was the raging argument over direct costing vs. full costing and especially a pair of articles by Horngren and Sorter in *The Accounting Review* [Horngren and Sorter, 1961; Sorter and Horngren, 1962]. I entered the fray with "Direct, Relevant, or Absorption Costing?" [1963] to which *The Accounting Review* editor appended Sorter and Horngren's "A Reply to a Postscript". It was the type of measurement issue that I should be able to answer. As in the case of my serious look at financial accounting for my

dissertation, I found no solid foundation on which to work. That's when I came up with the idea that management decisions tended to focus on *doing things,* on choosing from among alternative courses of *action*—in short, on *activities*. I proposed that *products* should not be treated as objects of costing but that *producing* commodities and/or services was commonly the focus of decisions so should be the objects of costing. Managers need to know the cost of making a product, of acquiring a resource, of using resources—in general, the costs of performing activities.

My interest in microeconomics affected my thinking a great deal, as did the importance of a future orientation as emphasized by Vatter. Cost must be thought of as an economic sacrifice resulting from the decision to do something. It followed that (1) the objects of costing must be activities, (2) cost data must be as relevant to the future (as current) as possible, (3) the cost of using a resource must include all required inputs, such as holding costs (including cost of capital) between acquisition and use, (4) the resources being used were not necessarily direct materials, direct labor and overhead; each significant input should be identified and costed separately to facilitate managers' decisions regarding mix of inputs, (5) decisions are made regarding activity increments of various sizes (calling for a range of cost concepts from marginal costs to life cycle costs, i.e., tailor-made incremental costing), (6) cost data must be free of aberrations that are not predictive (standard costs). This reasoning led to such developments as supply adjustment analysis to determine whether the cost consequences of using a resource would be its replacement (replacement cost), reduction in its sale (net realizable value), or less use in another enterprise activity (value in alternative use). Variable rate costing was emphasized—an approach that is getting a lot of attention these days by folks interested in "peak-load pricing" of electricity, bridges and roads, and airline seats. All of this called for a real-time integrated system that was kept up-to-the minute by cost accountants so as to provide managers with current information on the cost of using any internal resource, such as machine time, or of replacing products sold to the outside world. If all of this could be done, the managerial accountant would indeed make himself/herself a valuable member of Vatter's management team.

Activity Costing and Input-Output Accounting was completed in early 1970 before I left the University of Kansas, and was published by Richard D. Irwin with the date January, 1971.

Robert N. Anthony at Harvard had some responsibility for advising the publisher on accounting books at that time, and he strongly recommended its publication. As in the case of decision-usefulness theory, activity costing received little attention in the early years following its publication. In the 1980s and 90s, "activity-based costing" has become popular. As I see it, ABC typically incorporates the emphasis on activities in that it treats service activities in a manufacturing setting as providing fairly specific resources to "producing" activities—a more careful approach to accounting for overhead—but the other features of activity costing have not become popular. That may well be defensible from a cost-benefit viewpoint. I wonder what the future holds.

MARKET SIMULATION ACCOUNTING

My work on market simulation accounting [1985, 1986] marked an about face in my theory approach. In my previous efforts in financial reporting theory and managerial accounting I had adhered strictly to a normative approach; I loved telling readers what accountants ought to do. I think it is fair to say that normative work by an individual scholar reflects considerable conceit; by applying my reasoning powers I can ascertain what financial information would contribute most to achievement of users' objectives. Descriptive research, such as market association tests, may involve less dependence on imagination; perhaps that is a strength.

I became interested in describing accounting practices in 1983. I noticed that the literature of accounting included no major work that could be identified as a descriptive theory of financial accounting. For example, the FASB's conceptual framework was not intended to be descriptive. Indeed, the Board intended to establish a set of long-range goals—a target for standards setters to aim at over years of work. In other words, it—the Board's version of decision-usefulness theory—was/is a normative theory. But no concise statement of a theory of GAAP was available. According to Vatter's above-quoted paragraph, every science, methodology, or body of knowledge must have a conceptual structure; I sought to find it, not to establish it.

My analysis of measurement methods applied in GAAP employed seven qualities of accounting measurements—essentially the criteria for evaluating alternative accounting methods from Chapter III of *Making Accounting Decisions* but omitting the

relevance criterion—emphasis on simulated market prices takes care of that. My observations uncovered three additional criteria that seemed important to the profession: flexibility and control of income measurement by management; stability of income statement numbers over time; and conservatism. I found that stability of income numbers and conservatism appeared to be weighed most heavily and that reliability, economy, comparability, and understandability were valued less highly; flexibility and control of income measurement by management fell in between.

These rather mundane results were not terribly satisfying; I was hoping for a central objective of GAAP comparable to the decision-usefulness objective in normative theory. Then another look at the measurement methods in use showed a commonality: some version of a past or present market price is nearly always the starting point for each method. Those market prices, however, typically were adjusted, tinkered with, or revised to accomplish—what? Two considerations became apparent. One, to make the market price a more relevant measure of current market value. Two, to temper the result by the above-mentioned criteria prized by the profession, especially stability of the series of income numbers. For example, amortization typically produces a stable charge to expense, and, under GAAP's stable price level assumption, tends to produce a balance sheet number for the used asset that is closer to its current market value (than unamortized historical cost). Similarly, the discounting of future cash flows, providing for uncollectibles, and the practice of combining costs of various resources going into a product—such simulation techniques produce liability or asset values closer to what both economic reasoning and observations suggest would be market value in a complete and perfect market. So I concluded that the central objective of GAAP measurement was to simulate the price for an asset or liability that would prevail in a regime of complete and perfect markets. That requires recognition of the *specific setting* in which the asset or liability exists. All observable market prices are defective as measures of entity wealth because no observable price reflects the exchange of money at the precise measurement date for the rights/obligations associated with the net asset item in its existing unique setting. I visualized the accountant as attempting to simulate the setting-specific market value of a wealth item, and that requires using whatever techniques can be applied with acceptable reliability. I recognized that the observable *anomalies* are so material that the market simulation theory of accounting measurement is hard to swallow. Also, the MSA view fails to

account for jointness just as do other views of accounting measurement. I would say, however, that the rough trend in standards setting is consistent with the market simulation hypothesis, despite the roadblocks thrown up by those who place a high value on stability of the series of reported income numbers, conservatism—forms of secret reserves available for smoothing?—and flexibility and control over income measurement.

In sum, accountants have little choice but to record newly-acquired assets at their (historical) cost (current market value); they could not reasonably ignore the cash disbursement or incurrence of liability, or record a gain or loss on acquisition. They could conceivably continue over time to report historical cost, but they do not, except in minority cases such as land and certain investments in financial assets—practices explained by the preferences of preparers of financial statements. Instead, they amortize it or write it down, or less frequently, write it up, in the interest of more relevant, even though often less reliable, measurement. Nominal amounts of financial assets and liabilities are discounted, amortized, and/or adjusted for estimates of uncollectibility. All of those changes in measurements are intended to improve the item's measurement as an estimate of current fair value. They attempt, without articulating it and in crude ways, to simulate market values, and standards setters are staggering towards that intuitive objective without seeing the target.

ECONOMIC INFLUENCES ON THE DEVELOPMENT OF ACCOUNTING IN FIRMS

This book [Staubus, 1996] continued to reflect the descriptive interest that produced market simulation accounting. How did accounting get to be what it is? My economics orientation led me to study the economic environment in which accounting developed in past centuries. In studying the nature of the firm, I learned a lot from Ronald Coase, Oliver Williamson and many other economists. The result was to emphasize bounded rationality, self-interest and opportunistic behavior, firm uniqueness, externalities, information losses in transmission, and indivisibilities (and economies of scale in acquisition of resources) as the most fundamental, "tier I" influences. Then came cost of information, asset uniqueness, performance evaluation and incentive plans, and (especially) conflicts of interests. Finally, size of firm, vertical integration, diversification, and form of organization were found to be important direct influences.

Consider bounded rationality, perhaps the greatest influence on accounting. If the ancient merchant had been able to remember all of his debtors and creditors, he might not have gone to the trouble of keeping records of them. Similarly, there can be little doubt about the influence of self-interest and opportunistic behavior on the development of such control devices as double-entry bookkeeping. Conflicts of interests call for auditing, and so on. Some of these influences, such as indivisibilities in acquisition of plant assets (e.g., buying a ten-years supply of machine services in one transaction), represented serious challenges to accounting—solved in practice by allocation to time periods. Others, such as the great size of some enterprises, were primarily positive. This study significantly improved my understanding of why accounting is what it is. I also note that recent history has shown the importance of clear recognition of just how critical asset uniqueness—consider intellectual property—and conflicts of interests, as manifested in the financial scandal *du jour*, are as challenges to accounting. That investigation gave me a much better understanding of extant accounting.

Thinking about the historical development of accounting in relation to the decision-usefulness objective is appropriate at this point. Accounting has always had objectives, or reasons for being. Helping a merchant manage his rights and obligations, controlling the assets and operations of a branch, and dividing profits among partners were good reasons for accounting. Recognition and articulation of a decision-usefulness objective may not have been helpful. We can see now that achievement of the above-mentioned objectives involved using accounting information for making decisions, but it was not necessary to call the reasoning of managers using that information "decision-making". Investing in a joint merchandising venture may seldom have been based on accounting information since such an investment typically would have been made before there was any record of that venture, although the promoter's records of previous ventures might have been relevant. According to my limited reading of history, the type of bank lending activities that seem to have become common in the 19th century in Europe and North America might have been the first common situations in which it would have been natural to speak of using accounting information as a basis for making financial decisions. The widespread development of share ownership in the 20th century extended that use greatly, of course. It is not surprising that the language of using accounting information in making financial decisions did not develop in those centuries in which the use of

joint stock companies with numerous absentee owners was uncommon. Also, there is no reason to criticize pre-20th century accountants for not thinking of the processes of using accounting information in running the enterprise as decision making. In my opinion, the decision-usefulness objective could have been identified, and used productively, by the rapidly developing accounting profession much earlier in the 20th century than it was but its lack of identification and articulation before nineteen hundred need not be lamented. Indeed, its contribution to date in a world of antipathetic preparers may not be detectable by financial statement users.

MY GREATEST INTELLECTUAL DEBTS

The positive influences of two senior colleagues and teachers—Rufus Wixon at Buffalo and Bill Vatter at Chicago—have been noted above. They contributed immensely to the development of my thinking on accounting issues. The third such influence was Maurice Moonitz. Perry Mason was the senior professor in the accounting group when I arrived at Berkeley in 1952, but he left two years later to join the American Institute of Accountants staff in New York. That left Maurice and Lawrence Vance as the senior full-time professors in accounting. Maury had the greater interest in financial accounting theory. The two-volume textbook in use at the intermediate/advanced level at Berkeley at that time was by Moonitz and Charles Staehling [1952], who had just retired. The unique feature of that book was the "valuation experiment" in which the authors and students worked through two periods' financial events for a firm with only monetary assets and liabilities subject to present value measurement. It was difficult for the students, but I took to it like a duck to water. It fit nicely with the cash flow orientation of the decision-usefulness theory that I was working on at the same time. Present values of future cash flows were recognized as the most relevant to investors' future cash flow-oriented decisions of all measurement methods in Chapter III of *A Theory of Accounting to Investors*. (Now, I would state my preferences slightly differently, in the context of market simulation accounting [Staubus, 1986]).

More generally, Moonitz set a high standard of reasoning and was more devoted to research than most of my colleagues in the 1950s. His AICPA monographs, *The Basic Postulates of Accounting* [1961] and *A Tentative Set of Broad Accounting Principles for Business Enterprises* [1962] (with Bob Sprouse) prob-

ably are his best-known works. Like Vatter, he is a musician, continuing to play violin parts in quartets to the age of 90. (Come to think of it, Perry Mason was also a musician, but when I became group chairman, that requirement was dropped.) Coincidentally, Moonitz, like Vatter, grew up in Cincinnati, they attended the same high school (Vatter five years ahead of Moonitz), and both started their higher education at the University of Cincinnati. His was a consistent voice for economics-based theory and has been a good friend to this day, even though he was slow to climb aboard the decision-usefulness train.

Aside from colleagues, the two accounting writers that I admired most in my formative intellectual years were Paton and Canning; I know I am in good company as admirers of their works. W. A. Paton's greatest contribution was his *Accounting Theory* [1922] which included certain principles that he temporarily ignored under the influence of 1930s economic conditions and A. C. Littleton. He subsequently returned to those more progressive views. Canning's *Economics of Accountancy* [1929], which was his dissertation at the University of Chicago, emphasized the lack of logic in accounting measurements in practice and the logic in present value measurements. For example, I was impressed by his cynical version of the profession's definition of financial position: "That which the balance sheet is intended to reflect" [p.180].

A little later in my career I came to admire the works of Raymond J. Chambers and Robert R. Sterling as progressive thinkers who embraced the decision-usefulness objective. They were the most brilliant creative writers of accounting literature in the second half of the 20th century, in my opinion. I remember, shamefacedly, my belated discovery of Ray in the summer of 1958 when I read his "Asset Revaluations and Stock Dividends" article in the *Journal of Accountancy*. I thought it was the most exciting accounting article I had ever read and I wrote him a fan letter telling him so (December 29, 1958), including recognition that our thinking was similar. That was the start of a rewarding (for me) professional relationship that included working together at Kansas in 1970, his visits to Berkeley and the FASB, and my visits to Sydney, among other contacts. Later, I discovered that I had overlooked several earlier and more fundamental works by Ray, especially his thought-provoking blueprint articles [1955, 1957]. My failure to read them earlier reveals, I'm afraid, a weakness that has cost me a lot of time and possibly ill will over the years: not keeping up to date on published litera-

ture (and pre-publication drafts) of colleagues whose work deserved recognition and could have been helpful to me. This observation is relevant to my disappointment in the level of intragroup communication at Berkeley and my tendency to work as a loner.

I studied Ray's and Bob's works carefully once I caught on to their consistently high quality. Bob's works that impressed me most were his devastating journal articles. Ray's major monuments that serve as bookends for hundreds of other good pieces were his 1966 and 1995 books. I learned the most from *Accounting, Evaluation, and Economic Behavior* [1966] because of its contributions to areas of special interest to me, but I think my greatest admiration is for his thesaurus [1995], which shows a lifetime of scholarship that is unsurpassed in the accounting field as far as I know. I wrote him another fan letter when I discovered it.

It is common, I believe, for a person greatly devoted to his/her work to cast an especially critical eye on those works closest to his/her own. So it is with my views of Ray Chambers' work. Overall, I think he was the greatest. I also think he would have had more influence if he had not gotten stuck on a single-value theory. As decision-usefulness theory became more accepted, the superior relevance, to cash flow-oriented decisions, of current measurements over old measurements became widely recognized. Furthermore, many agreed that *properly chosen* exit prices were likely to relate more closely to the future course of the asset than entry prices—that they typically would be more relevant and so, given equal reliability and measurement cost, preferable. That brings us to the divergence of views. Some of us did not believe exit prices typically were as reliable and economical as entry prices. And of course prices in an unlikely course of action, such as current liquidation of assets generally thought of as integral to ongoing operations, simply did not seem to us to rank high on relevance to users' cash-flow oriented decisions. Ray and a few others liked to think of the decision facing the owner/manager of an asset who was faced with the decision to sell or hold that particular asset. The majority of users of external reports, in my opinion, were more concerned with the decision facing external, noncontrolling owners and creditors faced with the choice of whether or not to hold an investment in the company. They are the users of financial statements on which I focused. To such decision-makers, I reasoned, the cash flow potential of assets in their most likely future course (typically not current liquidation) was the more relevant

measure. In most nonmonetary asset cases, such a value-in-use or likely course measure had to be rejected on reliability and cost grounds. That steered us back to the less relevant measure—replacement (current) cost—as our choice in many nonmonetary asset cases. Personally, I referred to the "gap chart," which I first used in my original dissent [Staubus, 1967b] to Chambers' views, as illustrative of the economic relationship between cost and selling price that showed the logical surrogacy of current cost for net realizable value when the latter failed the reliability test—and the relevance test if the exit price under consideration was related to an unlikely future course for the asset. This reasoning can be adapted to liabilities as well.

Another way of looking at our difference is to note that Ray was concerned with the asset owner's sell or hold decision and he recognized the need for valuation of the sell side. I looked at the present or prospective external investor's choice of whether or not to hold an ownership or creditor interest in the enterprise and saw the opportunity for enterprise financial statements to inform the hold side of that choice. Those external investors rarely have a decision to make about either selling or holding a company's specific asset or about liquidating the enterprise. To me, the liquidation values of assets owned are quite relevant to assessing the enterprise's *liquidity*, an important concern to which cash flow statements and net "quick assets" are relevant, whereas balance sheets should be relevant to assessing the enterprise's ongoing wealth under the most likely future course.

In fairness to Ray, I must address his greatest concern about my approach: lack of additivity. He felt that the amount of one asset measured at present value, another at replacement cost, a third at net realizable value and a fourth at historical cost produced a sum comparable to the addition of US dollars, pounds sterling, Australian dollars, and Swiss Francs. My view was, and is, that none of those accounting measurement methods is perfect. All reflect *a market's estimate of an asset's value*, at some time, somewhere, so are additive. Differences in estimation techniques do not destroy additivity, although the addition of dollars of different sizes (from different dates) is highly questionable, so the omission of historical cost from the tool kit is a great leap forward in logic.

In my opinion, Ray could have had more impact on accounting thinking if he had been more in tune with finance theory where concepts such as enterprise continuity, present value of cash flows, and best use of assets are more imbedded in contemporary thinking. In the practitioner community, I believe

that Ray's underestimation of the reliability weakness in current exit values and his unwillingness to address the problem (foreseen by many) that acquisition of nonmonetary assets typically would require reporting a loss were viewed as demerits of his proposal. Indeed, that is a good, although not overwhelming, reason for objecting to the current treatment of home-grown intangibles in GAAP. Nevertheless, Ray Chambers was a powerful force for progress and discipline in accounting thought.

THE AMERICAN ACCOUNTING ASSOCIATION

Now I turn to two institutions with which I have had rewarding experiences. The AAA is the only professional organization in which I have maintained membership throughout my career. The first annual meeting that I attended was at the University of Michigan in 1949. In those days, universities were hosts and their faculties made arrangements for the logistics and the program. Attendance, of course, was much smaller than it is now, perhaps partly because employers were not as liberal with expense reimbursements and travel was more difficult. More importantly, there were fewer accounting teachers then.

Among the several committees on which I served, I learned the most from the other members of the Committee on Concepts and Standards for External Financial Reports during the 1973-76 period, despite my frustration at its inability to settle on decision-usefulness theory as the most helpful theory for accountants to embrace. We presented three "basic theoretical approaches" to accounting: "(1) classical ('true income' and inductive) models; (2) decision-usefulness; and (3) information economics" [AAA, 1977, p. 5]. A 21st century reader may be surprised at such indecisiveness; only decision-usefulness has provided significant guidance since 1977. Kermit Larson and Larry Revsine served effectively as successive chairs. Jim Boatsman, Joel Demski, Jack Kennelly, Bob Sterling, Jerry Weygandt and Steve Zeff all contributed significantly as members.

My association with the AAA has been rewarding to me in two other respects. I was selected to serve as the outgoing Distinguished International Lecturer in 1982 to visit Japan, Korea, China, Hong Kong, Thailand, Singapore, and Indonesia. Sarah and I added periods of teaching at the Beijing Institute of Foreign Trade. Two anecdotes have stayed with me. When we visited the railroad office in Beijing to arrange for train transportation, a clerk looked in a file for information on us, which

he did find. I never knew how the railroad office got it; we had never made plans to travel by rail. Second, when I arrived at the Shanghai University of Finance and Economics to arrange for my lecture there as agreed by the AAA office the chairman asked me to give five three-hour sessions on Activity Costing. When I questioned whether I would be able to prepare satisfactory materials in time, he said: "That shouldn't be hard; you wrote the book". So I did the best I could: preparing hand-written transparencies each evening; meeting with the interpreter in the morning; and delivering the lecture in the afternoon. The assigned interpreter was having a difficult time with the sequential translation, with frequent second-guessing by members of the audience. After two such sessions, we were able to get one of the latter—a Touche, Ross & Co. staff member from Canada whose native dialect was Shanghaineze—to take over the interpretation duty. Needless to say, I did not see much of Shanghai.

My other satisfying association with the AAA is the reception that its flagship journal has given to my writings. I have had more material (pages) published in *The Accounting Review* than any other living author (but not as much as an early editor, the late A. C. Littleton). Naturally, I consider *The Accounting Review* to have been the best of the accounting journals, even if I find little of interest in it these days.

As an aside here, Berkeley's involvement in the AAA, and AICPA, is noteworthy. AAA presidents who were students or professors at Berkeley include Henry Rand Hatfield, Perry Mason, Lawrence Vance, Robert Sprouse, Maurice Moonitz, Thomas Dyckman, Gerry Mueller, and Michael Diamond. Furthermore, 14—that's right, 14—AAA presidents have served as visiting professors at Berkeley, several of them in summer sessions in Henry Rand Hatfield's era. Four alumni or ex-faculty (Michael N. Chetkovich, John F. Forbes, N. Loyall McLaren, and Louis H. Penney) have headed up the AICPA or its predecessor, the American Institute of Accountants. We have a long history of professional involvement.

THE FINANCIAL ACCOUNTING STANDARDS BOARD EXPERIENCE

My association with the FASB was truly a learning experience although I never did learn why I was chosen to serve as Director of Research and Technical Activities. My speculation is that the two board members who knew my work, Bob Sprouse (vice chairman) and Oscar Gellein, who has been widely recog-

nized as one of the most valuable members the Board has ever had, were most influential in the Board's choice. Bob had been my next-door colleague at Berkeley for seven years and Oscar had served as discussant of my paper at the 1970 Kansas Conference. As a conscientious discussant—and every task that came Oscar's way was done conscientiously—he was thoroughly familiar with decision-usefulness thinking. Bob was, I believe, a late 1960s convert; his monograph with Maurice Moonitz in 1963 did not adopt decision-usefulness theory (much to my dismay). In any event, I was invited to interview in November 1975, and served part-time on a traveling basis until July 1, 1976 when I started full time.

This may be an appropriate point to note that I was following a well-established Berkeley tradition when I went into the business of establishing accounting principles, or standards. In the 1930s, Berkeley's Henry Rand Hatfield, the first full-time professor of accounting in the U. S., had worked with T. H. Sanders and Underhill Moore in writing *A Statement of Accounting Principles* [1938], commissioned by the Haskins & Sells Foundation and published by the American Institute of Accountants. The intention of the Foundation was to establish "a body of principles which will become useful in unifying thought and which by its acceptance will serve to standardize accounting practices" [p. xi]. Perry Mason moved from Berkeley to the AICPA office of accounting research in 1954, followed by Maurice Moonitz as Director of Accounting Research in 1961. Reed Storey (Ph.D., 1958) served in that same office, succeeding Maurice Moonitz as Director of Research, then moved to the FASB staff when it was established. Bob Sprouse, long-time FASB vice-chairman, was a Berkeleyan for seven years and board member Gerhard Mueller came out of our Ph.D. program (1962).

The "conceptual framework project" was a prominent activity during the time I was there, having been selected by the Board as one of the original seven projects it would undertake. The "Trueblood Committee," more properly the Study Group on the Objectives of Financial Statements, had submitted its report in October 1973, and the Board used that report as its starting point. Sprouse, Gellein and staffer Reed Storey were actively in charge of the project. It would be reasonable to speculate that I had some influence on the development of the conceptual framework during the period I was in Stamford, but that would be wrong. Any influence I had came earlier and worked through others. The fact of the matter was that the trio running the

project were so active and so well-qualified and had such a good start that there was nothing for me to do in that area. There was one exception. I commissioned David Solomons, who was recognized as the primary author of the "Wheat Commission's" original design for the FASB, to write a report that served as a starting point for SFAC No. 2, "Qualitative Characteristics of Financial Information" (1980). The Board was happy with his work. My only complaint was that it should have been called "Criteria for Making Accounting Choices" but the Board was already committed to qualitative characteristics.

Those with an active interest in the development of accounting standards in the mid-1970s remember the intense argument that went on for several years. The opposition to decision-usefulness theory was well organized and active. Robert K. Mautz was the accounting guru partner in Ernst & Ernst who led the opposition. He was a member of the Board's Conceptual Framework Task Force, so had full access to all of the materials being gathered and developed. Then, as now, the Ernst firm had a special relationship with the Financial Executives Institute which welcomed Mautz and allies as speakers against the FASB at many FEI meetings. They also wrote papers stating their position. As I saw it, FASB opponents believed, quite rightly, that acceptance of the decision-usefulness objective would lead to acceptance of current values when they could be measured with acceptable reliability and cost. That would mean the loss of a great tool for managing earnings—the ability to convert noncurrent values to realized gains or losses as needed. They did not give high priority to informing investors, and who can blame them? We all give high priority to our own interests. Managers' interests in how to report on their own performance will always conflict with investors' interests in information for decisions and the Ernst firm sided with their client managements, not with financial statement users. That's why a regulatory body is needed. But many accountants do recognize the concept of social responsibility.

Mautz and company fought tooth and nail against the conceptual framework and did succeed in delaying it somewhat. But the case for decision usefulness eventually carried the day through most of the conceptual framework. Even at the end, the acceptance of alternative measurement methods to fit varying cost and reliability circumstances fits the theory, in my opinion. However, we progressives must not feel too satisfied. In practice, the forces of resistance to informing investors continue to win battles, as in the cases of valuation of marketable securities,

treatment of the costs (to stockholders) of stock options, and accounting for costs of pensions and other post-employment benefits. As I write, the U.S. Congress is holding hearings on the financial reporting at Enron, which may serve to shine a bit more light on the difficulty in getting managements to inform investors.

When I arrived in Stamford, the Board was busily engaged in several other contentious projects, notably those pertaining to exploration and development costs in the extractive industries, the restructuring of troubled debt, and leasing. The public hearing on the restructuring of debt was held in New York at the end of July 1976. Banker after banker—Walter Wriston of Citicorp, David Rockefeller of Chase Manhattan, Norborne Berkeley of Chemical Bank—told the Board how hard it would be for important borrowers to get loans if bankers knew that those loans would have to be marked to market if they ran into trouble. They cited the nearest they could find to heart-rending potential cases—friendly foreign governments, municipalities (New York City was at that time very publicly broke), and fledgling minority businesses—to convince the Board of the potential adverse *economic consequences* of a tough standard on the restructuring of debt. Personally, I was more concerned about the economic consequences of not requiring lenders to bite the bullet when loans went bad. In the end, the Board compromised—and lenders have continued to take great risks, often being bailed out by taxpayers. Think of the financial disasters associated with Mexico, Indonesia, Argentina, savings and loans, etc.

The economic consequences argument, i.e., the concern about adverse effects on managers' and/or investors' decisions of a significant change in a financial reporting standard, was brought up in opposition to changes in the reporting of leases and in other projects on which the Board worked during my tenure. The granddaddy of them all, in terms of the efforts expended by opponents to change, was the oil and gas project. The "successful efforts" method, to which most of the board members were leaning, required taking a hit to the income statement for the costs of a dry hole. The "full cost" method was more liberal in capitalizing and amortizing the costs of all drilling, thus typically yielding a smoother series of income numbers. The "Ad Hoc Committee for Full Cost Accounting" was chaired by the late J. Stanford Smith, the chairman of International Paper Co. which had a large oil and gas subsidiary. We saw him as an enemy of the Board. I remember a dinner party at our home that happened to include as guests Don Kirk (Board mem-

ber) and a young lady neighbor of ours who worked for International Paper. We were amused when she told us the story of how John S. Smith became J. Stanford Smith. She reported that John Smith held a contest among his friends to find the most impressive alternative name. The winning entry was J. Stanford Smith. My staff also was able to get a copy of Smith's employment contract showing the dependence of his earnings on the company's reported earnings. All of this helped Board personnel understand "where J. Stanford Smith was coming from".

The Board's exposure draft (ED) "Financial Accounting and Reporting by Oil and Gas Producing Companies" favoring successful efforts accounting was issued in July 1977. I commissioned two research studies to detect possible adverse effects of successful efforts accounting on producers' ability to raise capital. A market response study by Tom Dyckman was unable to find any lasting, statistically significant effect of the exposure draft on the prices of companies using full cost accounting—those that would have to switch to more conservative accounting if the ED were to become GAAP. Horace Brock interviewed officers of 27 successful efforts companies in the Southwest and found that none of them felt that successful efforts accounting had hindered their capital-raising activities. But the ad hoc committee for full cost accounting continued its resistance, working the Washington beat intensely. It also hired Dan Collins of the University of Iowa to do a market response study. Surprise!? He found that the ED did hurt the prices of full cost companies' securities. The conflict between Dan's and Tom's results surely did not boost the stock of market response research, nor did Dan's sway the Board nor Tom's influence the SEC. As many remember, the Board went ahead with its standard favoring successful efforts [1977]; the SEC issued a statement [1978] canceling the GAAP status of the Board's standard; and the Board issued a revised standard [1979] rescinding its requirement for successful efforts accounting. So much for the power of an independent, private sector standards-setting body!

Economic consequences got special attention at the FASB during the oil and gas imbroglio. The issue had risen to such a prominent level in intercourse pertaining to major projects, and was so heavily weighted as an argument by many respondents, that I felt more light should be shone on it. I was familiar with the reasoning supporting consideration of economic consequences because it (under the name "effects via other parties") was a criterion an evaluator with a special interest might use in choosing an accounting method as discussed in Chapter III of

my *Making Accounting Decisions* [1977]. Prem Prakash at Pittsburgh and Al Rapport at Northwestern, among others, were writing about it [1976]. I discussed the issue with Al, then got Board Chairman Marshall Armstrong's approval to issue a call for papers and hold a conference on the subject. I sent letters to many organizations whose members might be interested and had notices published in several journals inviting one and all to submit research papers that might shed some light on the issue of possible economic consequences of financial reporting. It is interesting that, despite their frequent citing of possible economic consequences as a basis for objecting to proposed changes in accounting standards, when the opportunity came for them to show something specific about possible adverse economic consequences, not one of the Financial Executives Institute member firms submitted a paper. In fact, the Institute's president, Charles Hornbostel, was exceptionally vociferous in complaining about my call for papers and plan for a conference.

In due course, I received 22 papers—mostly from academics, none from the most vocal complainers—and my committee of six judges (research directors of the FASB's six sponsoring organizations) picked five for presentation at our conference and one more for publication in the related book [FASB, 1978]. The one-day conference, which I chaired, was held on March 23, 1978. As I had anticipated, no one was able to show that financial reporting standards adversely affected the economy through either managers' or investors' decisions. In my opinion, that effort substantially weakened the economic consequences argument, although it certainly did not stop it from being dredged up by those opposed to a position taken by the Board. The points became clear to many: (1) economic consequences of financial reporting can be favorable or unfavorable for the economy; (2) the economic consequences of financial reports that include irrelevant or unreliable measurements or that misrepresent risks being taken by the entity can be unfavorable, whereas financial reports with the opposite characteristics are most likely to have favorable economic consequences. It would be a rare case in which the Board should pull its punches because of possible adverse economic consequences.

When Marshall S. Armstrong, the Board's highly respected first chairman, retired, Donald J. Kirk succeeded him. Marshall permitted Don to reorganize to suit his own style before the official handover, at which point Don brought in a more managerial type of person—Michael Alexander—to replace me as Director of Research and Technical Activities. I was switched to

full-time work on the "inflation accounting" project, where I might have been more valuable and which I certainly found more satisfying. During my term as director, each of the major projects was under the supervision of a board member—an arrangement that probably served best at the beginning because it gave those board members an opportunity to learn about the various phases in the development of a project. Also, much of a board member's time when the organization got into a steady state was spent on matters pertaining to public hearings, responses and deliberations on exposure drafts and final statements, matters requiring little board member time in the organization's youth. That arrangement meant that the Research and Technical Activities director did not provide much direction to the staff; most of them worked more closely with a board member. Later, when Jim Leisenring and Tim Lucas served as Research and Technical Activities directors, the job became more of a chief operating officer position. In my term, however, technical advisor was a better job description; I did a great deal of reading and commenting on project managers' drafts, especially on a couple of projects that were not supervised directly by a board member. One of those was capitalization of interest cost, which was issued as Statement No. 34. That topic had been of great interest to me since my first published paper, in 1952. I was pleased to see it become part of GAAP.

One minor duty of the Director of Research and Technical Activities was to interact with other research directors of North American accounting organizations in a six-member organization known as the Council of Accounting Research Directors (CARD). We met every six months to exchange ideas and keep each other up to date. I especially recall attending one of those meetings in Toronto. Two or three other members were based in New York and it so happened that we all traveled to Toronto on the same flight. We chatted in the waiting room, then boarded together. On the plane, however, we parted. One of the privileges of the FASB research director was first-class travel. I found my seat in the first-class section but the others went on to economy class. My embarrassment was heightened by my awareness that the members of their organizations were funding the FASB, so I was traveling first-class at their expense. From that date forward, I forewent my first-class privilege. Another lesson in the education of an accountant!

A more time-consuming duty was recruiting, which was much more difficult than I had anticipated. In one case I rejected an applicant who later became an effective board mem-

ber, fortunately after I had left the organization. I'll never know if he would have made a good project manager. It's safe to say that the sets of personal qualities required for the two jobs are far from identical. To a large extent, qualified people were partners or heading for partnerships in accounting firms, enjoying similar success in industry, or full professors at universities. By academic standards, FASB personnel were well paid, but the salaries were only competitive with those in other fields, and many applicants did not see much upside potential. Temporary positions—as industry fellows, practice fellows, or academic fellows—could be filled more easily, and we were fortunate to attract some good people for those roles. The qualities needed for senior staff—technical knowledge, writing ability, and interest in evaluating accounting alternatives—were not so easy to find in people willing to join that peculiar, risky organization, the FASB. I can think of senior staff members who were nearly worthless to the FASB who went on to great successes elsewhere. But we also had some outstanding people, such as Jules Cassel, whom I believe is the longest serving staff person at the FASB, having started well before I came and continuing as I write.

My most successful recruit was Bryan Carsberg, who came in 1978 and became my partner on inflation accounting. I first met Bryan when I was at the London Business School in 1967 and Bryan was a graduate student and lecturer at the London School of Economics and Political Science. Will Baxter invited me over to the LSE to lecture on divisional performance measurement. I structured my lecture to start with a possible but poor measure of performance, point out its weaknesses, then move on to a better measure, etc. Each time I would pause for questions before suggesting the preferable measure, Bryan would raise his hand and say something like "but I would have thought (what I was about to suggest as the preferable measure) would work better". Naturally, I thought that young man was very smart.

As chairman of the accounting group at Berkeley in the early 1970s, I invited Bryan to spend a term with us as a visitor, which reinforced my judgment. At the FASB, we worked together on the specific technical content of the inflation accounting statement until the time came to draft the exposure draft. Bryan asked if I would like for him to have a go at it; I agreed. He worked on it two evenings at home, writing in longhand and bringing the work to me in the morning. I was astounded. Not only did he seem to have never needed to cross out any of his

first choices of words, but I could not find any editorial suggestions to make either. In one week he joined the ranks of such top technical writers as Reed Storey and Paul Pacter. And his understanding of the technicalities of the subject, once again, agreed precisely with mine. Bryan stayed with the Board for three years during which period he was promoted to assistant director. That experience may have contributed to his later successes in leading major regulatory bodies of Her Majesty's Government (Director-General of Telecommunications (Oftel) and Director-General of Fair Trading) as well as the British and international accounting standards-setting bodies. I am sure that the skills he polished at the FASB have served him well in all of those positions. Will Baxter and the LSE should be proud of Sir Bryan.

Of all the projects in which I was involved at the FASB, I surely made the greatest contribution to Statement No. 33, "Financial Reporting and Changing Prices". including the title, which I chose as the best I could think of to cover both specific price changes and changes in the (price of the) measuring unit—the U.S. dollar. I drafted an exposure draft on general price level adjustments that was issued in March 1979, but which the Board did not expect to finalize; it followed through with an approach that was started earlier but was already deemed unacceptable: accounting for inflation without updating measurements of specific net asset items. The technical features of that ED did, however, carry through to Statement 33, including my definitions of monetary and nonmonetary assets and liabilities, which were stated carefully so were difficult to understand. That has been characteristic of much of the writing in FASB work: we/they have not always struck the balance between precision and readability that many readers would have preferred. The Board has insisted on "getting it right technically". Subsequently, the staff has developed ways of helping readers comprehend technical standards by providing supplementary educational materials.

From the above comments, I am sure the reader can see that my FASB experience contributed heavily to my education. I added much to my technical competence, although I would not claim to have reached the level of such genuine experts as Oscar Gellein or Tim Lucas. I acquired the habit of arguing two or more sides of issues, of seeking counterarguments, of presenting as whole a picture as I could. Because the Board was determined to work within the logic of GAAP rather than overturn the whole structure, reasoning by analogy within GAAP became

important. High quality writing was admired and I sought to improve my own, and I gained some experience speaking to groups that I did not know as well as students and fellow-academics. I learned to think about the background for a person's—especially a critic's—point of view and to recognize that no one was completely objective, not even my admired academic friends, or myself. All of the Board's constituents had special interests; advocacy of those interests is not an evil act, but it does impede the performance of a body established to work in the interests of the general public. The Board's mission was to represent the latter; in my opinion, Board members have consistently sought to do so.

My admiration for Board members I have known, while I was there and subsequently, has been quite general, although not universal. Marshall Armstrong was the type of steady, calm leader that the first Board needed. Don Kirk, a younger, vigorous man, provided a stronger style of leadership and added an emphasis on business-like efficiency, including individual work plans and monthly reports, in the operations of an organization of some 100 people. Bob Sprouse served as vice-chairman for nearly all of his 13 years on the Board and consistently offered balanced, well-reasoned views; he had served with distinction as president of the American Accounting Association and was the ideal choice for the first academic position on the Board. Oscar Gellein, who came from Haskins and Sells, was greatly admired by all who knew him, as nearly as I could tell. His reasoning often made the difference in the conclusion reached by the Board. In an environment in which nearly every professional felt pushed by the work load, Oscar once told me, "I like this job; my only complaint is that there is not enough to do". Just a little later, Ralph Walters and David Mosso became very effective members. I enjoyed reading the transcripts of David's well-crafted, highly literate speeches. In general, the Board Members made the transition from their special interest positions to representatives of the public interest quite effectively. I never felt that any of them exhibited special interest bias. Unfortunately, the Board was subject to a modest degree of "regulatory capture," but far less than what one sees in such U.S. regulatory bodies as the SEC and the Federal Communications Commission, or the auditing profession.

My views of the FASB have changed a bit over the years. As one board member explained to me when I inquired about the change in his position on an issue after moving from a corporate financial officer position to the Board: "Where you stand de-

pends on where you sit". I was, naturally, loyal to the cause when I was a part of it but in later years I have been disappointed in two aspects: speed and independence. When the chronological length of projects is counted in decades, as in the cases of pensions and other post-employment benefits, consolidations, and financial instruments, I cannot be proud of it. And the repeated cases of yielding to pressure, including by delays, have been discouraging. It surely is difficult to make a case that the Board's standards involving restructuring of debt, pensions, other post-employment benefits, marketable securities, and stock options are consistent with the conceptual framework. The trend of board decisions surely has been in the direction of providing more useful information to investors, and I think the Board deserves to have reached the greatest age of any U.S. accounting standards setting organization, but still the record is not one deserving strong accolades. On balance, however, it is a force for good in financial reporting.

I am pleased to see that recent legislation has provided for financing the Board from fees paid by public companies to the SEC, thus eliminating the threat that unhappy corporate managements might withhold contributions to the privately financed Financial Accounting Foundation that has funded the Board in the past. I am convinced that that threat has influenced Board members' judgments on controversial issues. Members of Financial Executives International have been the most frequent and politically strongest opponents of proposed changes in financial accounting standards; their agents in the auditing profession have also been poor supporters when the chips were down. None of the sponsoring organizations, however, have good records of support, including the generally apathetic AAA. I would not object to the Board's supersession by the International Accounting Standards Board. One way of meeting the widespread concern that detailed "cookbook" standards are too easily "loopholed" is to adopt the "general principles" approach that many critics of the Board and the IASB prefer and to establish an advance opinion service that would, for a fee, provide an ex ante opinion on a company's proposed reporting. The auditor would then be required to call attention to any deviation from that opinion. Should the SEC do this?

CONCLUSIONS

Academic accountants have shown a much lower level of interest in accounting theory in the last quarter century than

they did in the previous two quarters. That applies to both their research and their teaching. I have no good explanation of why this has been so. I think we all know that such an explanation would have to explain the reward systems in university faculties. I am willing, however, to confess that my interest has not waned. If that marks me as antiquated, so be it.

What have I learned in the course of my education? These points I find enduring:

- That accounting has the honor of serving as a major provider of information, and information lubricates markets. Those who enjoy the fruits of markets should recognize the value of accounting. Governmental regulatory activities that positively influence the flow of objective information are consistent with belief in the value of free markets in other goods.
- Education makes information accessible. Accounting education empowers information providers and users.
- Accounting is a branch of economics. The measurement of wealth—positive and negative, inflows and outflows—is the heart of accounting, including the use of accounting in controlling the handling of wealth.
- That teachers can add value and interest to their financial accounting courses by emphasizing decision-usefulness theory in addition to generally accepted accounting principles.
- That measurement methods that simulate the market price of the asset or liability item in a regime of complete and perfect markets provide information relevant to the decisions of interested parties who are future cash flow-oriented.
- That the prospective reliability of those measurements must be weighed heavily in choosing among them and in making the cut-off between those items that are measurable with useful reliability and those that are not, especially intangible investments.
- That no accounting system using only one measurement method can serve users well.
- That accounting measurements always will be fraught with imperfections and that the portion of net asset items that is not measurable with acceptable reliability is increasing.
- That cost finding can be taught by starting with ad hoc situations calling for predictions of economic sacrifices

required to perform an identified activity. Then, *common* cost information needs can provide the basis for developing cost accounting *systems*.

- That many academic accountants can improve their teaching and research by using each to help the other in a symbiotic relationship. That requires that the teaching and research focus on the same subject matter.
- That a career in academic accounting can be rewarding.

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