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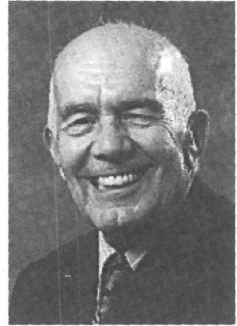
## **1995 ACCOUNTING HALL FAME INDUCTION:**

### **WILLIAM W. COOPER**

#### **CITATION FOR WILLIAM W. COOPER**

By Thomas J. Burns  
Professor Emeritus  
The Ohio State University

He has been a catalyst of change on a world-wide basis for more than 50 years: in his research, with his inspired teaching, as an editor for many periodicals, and as a consultant to both private, governmental and public institutions. A prodigious author, his writings often focused on quantitative and creative approaches to management. Equally important have been his contributions to management education as noted in Ford and Carnegie Foundation reports. Working with others, he has authored 17 books and over 450 articles including ones with Hall of Fame members Robert Trueblood, Eric Kohler and Yuji Ijiri. With his long-time collaborator, the mathematician Abraham Charnes, he was known everywhere as "Mr. Linear Programming," partly because, together, they developed whole new areas of use and research such as "goal programming," "chance constrained programming," and more recently, "data envelopment analysis."



Born in Birmingham, Alabama in 1914, his father was a bookkeeper and later a distributor for Annheuser-Busch. When he was three years old, the family moved to Chicago where his father owned a chain of gasoline stations that he lost in the Great Depression. He continued in high school only until the end of his sophomore year. With his father in ill health and no family revenue, he had to work at whatever he could find. This included everything from professional boxing to spotting pins in bowling alleys and caddying at golf courses.

While hitchhiking to a golf course one day, he met Hall of Fame member Eric Kohler, who thereafter became his life-long mentor. This included a loan of funds which enabled him to start a nondegree track at the University of Chicago. He quickly learned to like the academic atmosphere and soon took the college entrance examinations, intending to become a physical chemist because that seemed to offer the best chance of a job. At about this time, Kohler, then a principal at Arthur Andersen, asked him to look over the mathematics used in a patent infringement suit in which Andersen had been retained by the defendant. He found errors in the mathematics used by the plaintiff's engineers and Andersen hired him full-time in the summer and part-time during the school year. This awakened his interest in accounting, and so he changed his major from chemistry to economics at the University of Chicago, and Kohler helped him to learn accounting. He graduated Phi Beta Kappa in economics in 1938.

Kohler had by then left Andersen and assumed the position of Controller for the Tennessee Valley Authority. Kohler brought him to the TVA to head up work on "procedural auditing" (what would now be called "performance auditing") as well as to advise Kohler on the mathematics of cost allocation and other disputed matters in which the TVA was involved. This included helping Kohler to prepare testimony on these and other matters to be investigated by a Joint House-Senate Investigation Committee. Most of the work was completed by mid-1940 so he left to become a Ph.D. candidate at Columbia University where he had been awarded a doctoral fellowship in the School of Business. After passing his "prelims" in 1942, he again left academia to join the Division of Statistical Standards at the U.S. Bureau of Budget (now the OMB) where, as part of the U.S. war effort, he was placed in charge of coordinating all of the Federal Government's accounting and accounting-related statistics programs. By late 1944, with the war coming to an end, he left to teach at the University of Chicago.

In 1946, he returned to Washington to chair a committee to decide the fate of various war-time programs in financial statistics. He then transferred to Carnegie Institute of Technology (now Carnegie-Mellon University) where he helped found, first, the Graduate School of Industrial Administration and, later, the School of Urban and Public Affairs. There was time out, however, to develop the "end-use" audits that Kohler wanted to institute as Comptroller of the Marshall Plan. In 1976, after 30 years

at CMV, he went to the Harvard Business School to help reorient their doctoral programs while holding the chair in accounting named for Hall of Fame member Arthur Lowes Dickinson. This task completed, in 1980 he went to the University of Texas where he was initially appointed Professor of Management, Accounting and Management Science, and Information Systems, and is now the Foster Parker Professor of Finance and Management (Emeritus) and the Nadya Kozmetsky Scott Centennial Fellow in the IC<sup>2</sup> Institute.

He has been awarded honorary doctor of science degrees by Ohio State University, Carnegie-Mellon University, and will soon be awarded the degree of *doctor honoris causa* by the University of Alicante in Spain. In 1945, he received an award for the most valuable article on accounting, the first ever awarded by the American Institute of Accountants (now AICPA). A fellow of the Econometric Society, he was the founding president of the Institute of Management Sciences, and he was also president of the Accounting Researchers International Association. He was the Director of Publications for the American Accounting Association. In 1990, he was named an Outstanding Accounting Educator by the same organization. He was Visiting International Lecturer for the AAA, traveling abroad in 1986 to lecture on accounting topics and visit with scholars in Latin America. In 1982, he was co-recipient of the John Von Neumann Theory Medal, jointly awarded by the Operations Research Society of America and the Institute of Management Sciences. In 1988, he received the Distinguished Service to Auditing Award from the Auditing Section of the AAA as well as an award for serving as the founding editor of *Auditing, A Journal of Practice and Theory*. He has also received three McKinsey Foundation Awards for the most valuable article of the year on a management topic, and he has been a consultant to more than 200 institutions including the Marshall Plan, the U.S. General Accounting Office, UNESCO, and others. He believes that one of his greatest contributions was keeping his student, Hall of Fame member Yuji Ijiri, in accounting when he was almost ready to switch disciplines under the influence of Nobel Laureate in Economics, Herbert A. Simon. His wife Ruth, always his teammate, is a lawyer who practiced actively as a member of the bar for 30 years in Pittsburgh. Just back from serving as plenary speaker at the meetings of the European Operations Research Societies' research conference in Israel, he is the fifty-fifth member of the Accounting Hall of Fame: William Wager Cooper.

## RESEARCH AND PRACTICE IN CONTEMPORARY ACCOUNTING

by  
W.W. COOPER<sup>1</sup>

Mr. Chairman, Ladies and Gentlemen, Friends, Colleagues and Former Students: I greatly appreciate your attendance on this occasion when I am to be given the great honor — the very great honor! — of being inducted into the Accounting Hall of Fame. Among my many satisfactions, this will allow me to join the very distinguished company of Eric Kohler, my former teacher, and Yuji Ijiri, my former student — and, hopefully, others of my former students and colleagues will join us in the future.

I would now like to have you join me in some reflections on the current state of affairs with respect to research — especially academic research — and its relevance to the practice of accounting. Before entering into this topic, however, I would like to make some acknowledgments:

First to my parents. Bertrand Russell once remarked that one should exercise great care in choosing one's parents since a good choice of parents is likely to ensure good genes, good character and a good start in life. This I owe to my parents, William Wager and Rae Rossman Cooper — now deceased so they cannot be here on this memorable occasion.

I have already mentioned my teacher, Eric Kohler, and my former student, Yuji Ijiri, and I would also like to add my other present and former students and colleagues to whom I am also indebted. These are persons from whom I have learned much and to whom I owe a great debt as a constant source of new problems and stimulating ideas in exchanges that I have experienced with them over these many years.

I also owe much to the schools and the professional societies — including the American Accounting Association — with which I have been associated, for they have provided me with a rich variety of opportunities and experiences over a period of many years. I should perhaps dwell on this for a moment since (again owing to Eric Kohler) I am probably the only person

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<sup>1</sup>This talk, on the occasion of my being elected to the Accounting Hall of Fame, is dedicated to Ruth F. Cooper, my wife and companion, who has affected all phases of my life, my thoughts and my feelings for more than 50 years.

present who was in attendance at the 1935 founding meeting of the American Accounting Association — the society whose 1995 meeting we are all attending on this occasion here in Orlando, Florida. The then newly founded AAA was intended to have a new (double) orientation toward research and practice with strong interactions between academic research and accounting practice also intended. It is partly from this perspective that I am concerned by what I now perceive as an increasing tendency for the two of them to follow separate paths.

Of course a great deal has happened since 1935. For further perspective, I might add that my graduate and undergraduate education occurred on one side on World War II while much of my research and subsequent experience occurred on the other side of that great dividing event in world history. Only those who have seen both sides can appreciate how different the worlds were on the two sides of the divide provided by that war and the events that terminated and followed it.

The research in accounting that occurred on the pre-World War II side of this divide was largely application (practice) oriented and generally accorded the form of “textbook” presentations. This is one part of what the AAA undertook to change with its monographs and research programs. One should be careful, however, to avoid belittling the preceding record of accomplishments. In a chapter entitled “Early Development of Accounting Standards and Principles,” Carman Blough, the first Chief Accountant of the SEC, notes that these early texts and writings “provided the models that were subsequently used for reforms and standard setting.”<sup>2</sup>

This “practice oriented” research, however, lacked the rigor that is needed to provide a foundation for scientific generalization and testing to determine how far and in what manner extensions might be affected beyond the realm of particular problems and practices. The contemporary research literature now supplies this kind of rigor, but in a way that often seems remote from practice. One way to summarize what is happening is to say that much of this contemporary research is pointed toward “pure science” whereas an “applied science” orientation is what is (or should be) wanted if our research is to interact with practice in a mutually beneficial way.

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<sup>2</sup>This quotation is from the chapter by Carman Blough in *Eric Louis Kohler: Accounting's Man of Principles* (Reston Publishing Co., 1979) W.W. Cooper and Y. Ijiri, eds.

This is not to say that the contemporary research I am describing is bereft of interest in the problems of our profession. It is to say that this research often takes a form that can be described as “theory driven applications” rather than “applications driven theory.” The latter is needed — or at least it is needed in greater abundance — if we are to maintain fruitful contact with the rapidly changing scenarios of contemporary practice and, I might add, it is also needed if we are to serve as a vehicle for attracting contributions from other disciplines for added contributions to accounting. Indeed, an ability to formulate problems as well as solutions in general and rigorous terms is needed to obtain this type of inter-science cross-fertilization. This is what is intended by “applications driven theory”. The application is driven by problems of actual practice with a solution that is also extended and stated with sufficient rigor and generality to be understood by others (including persons in other disciplines).

In principle, at least in some cases, this kind of application driven research is best accomplished in collaboration with persons involved in decisions or policies directed to these problems. In this way we can ensure that the complex and sometimes elusive nature of the problems at issue are identified fully. En route to the desired solutions and generalizations one may also envision situations where team combinations may be used that also involve collaborative efforts with persons from other disciplines — such as statistics, operations research, and computer science.

An example of such “applications driven theory” is provided by the work of Louis Pasteur on the silkworm diseases that were ravaging the French silk industry in the early 19th century. We quote from *Science* (December, 1972) which notes that “at the conclusion of his research, [conducted in collaboration with practitioners] Pasteur found that he had not only solved the problems that were destroying sericulture in France, but he had also pointed out the importance of practical [sic] experimental research on microorganisms for the sciences of microbiology and pathology.” Thus, as indicated, this research had the desired effects in (a) improved practice with (b) impacts on other sciences.

This was not an aberration. In similar activities, Pasteur helped to improve the production of wine, vinegar and beer, and his research on the anthrax that was harming the sheep in France laid the foundation for the modern germ theory of disease and the development of immunology as a whole new sci-

ence. Evidently, it is possible to be basic as well as applied in such research (depending on how it is conducted) and the impact on other disciplines provides one test of whether its achievements are fundamental.

Returning to accounting research and practice, I recall participating (in the early 1950's) in what Hall-of-Famer Robert M. Trueblood referred to as the "Monangahela Project" to study (and test) the possible use of statistics in accounting and auditing. This was done with very concrete applications, first at the National Supply Company (now a part of Armco Steel) and then at Kaufman's Department Store (now a part of the May chain of stores). This work was done on a fee-free basis in collaboration with researchers from the Carnegie Institute of Technology (now Carnegie-Mellon University) and controllers and staff from the companies where research was conducted. The work was then followed by write-ups and reviews some of which were submitted to W. Edwards Deming (from statistics) and Eric Kohler (from accounting) who were asked to appraise the work and its claimed generalizations from the standpoint of the discipline with which they were associated. All of these tests had to be passed in fully satisfactory form before Trueblood would allow his firm to offer these services to clients. Once these tests were passed, an abundance of publications followed and this work soon began to affect practice in accounting. It also affected research in statistics and, in fact, the use of statistics in accounting (now attended to by many others) has recently received recognition in the form of a full-blown National Academy of Sciences report entitled *Statistical Models and Analysis in Auditing* (Washington: 1988).

To this point, our focus has been on financial accounting, but one also needs to accord recognition to pre-World War II accomplishments in managerial and cost accounting. Here we may single out developments like standard costing, flexible budgets and differential and variable costing as major contributions to accounting and management practice. Standard costing, which has its roots in the work of F.W. Taylor and his associates, is the only one of these developments that exemplifies attempts at scientific rigor and generalization. The other developments are, of course, important to the topics we are addressing but, for the present, we simply note that it was only standard costing that made its way from internal management use — when it was argued that inventory should be reported at standard-cost values in financial statements with accompanying dis-



closures of "efficiency" and "inefficiency" gains and losses.

Financial statement disclosures in terms of efficiency gains and losses now seem to have fallen out of favor, both in practice and research. In some ways this is unfortunate, since efficiency evaluations are important topics for both public-policy and investor evaluations of management performance. In another way, however, the lack of contemporary research on these topics brings us to yet another facet which we explore in this postwar "pure science" emphasis in research.

If I may be permitted a personal experience, I can report that DEA (Data Envelopment Analysis) represents yet another attempt to develop better methods for evaluating managerial performance with special reference to activities using multiple inputs to produce multiple outputs with no easily identified "bottom-line". Now a part of the literature of operations research and economics, DEA is being used to evaluate activities of police forces, schools, hospitals, etc. in many different parts of the world.

Why was DEA not developed as a part of the accounting literature? At a very early stage, David Sherman and I submitted an article to one of our peer reviewed journals in accounting only to have it summarily rejected by the editor who refused even to send it to referees because, as I subsequently discovered, it had no "theory". What the editor had in mind was what I shall refer to as "substantive theory" as distinguished from the "methodological theory" on which DEA is based. The former i.e., "substantive theory", is exemplified by what is wanted in the search for regularities (or laws) based on ideas like the behavior of "homo economicus" in economics. The latter, i.e., "methodological theory," is exemplified by the kind of research and theory that underlies the statistical tools used in much of the "pure science" oriented research we are considering.

At this point, it seems fair to recall accounting's origin as a methodologically oriented discipline in Pacioli's book on mathematics and, for years, double-entry accounting was known as the "Italian method" of bookkeeping. The opportunities for methodologically oriented research are also far from exhausted in both financial and managerial accounting. Witness, for instance, the "triple-entry system" of Yuji Ijiri which can now provide integrated explanations of the income statement changes together with balance sheet changes in a manner that extends the classical "double-entry" use of income statements to "explain" changes in successive balance sheets. Recent activity in

areas such as "ABC costing" has also attracted research attention with needs now becoming apparent for more rigorous treatment of topics like complexity, flexibility, etc. — which are increasingly being encountered in dealing with accounting problems in high-tech industries.

Having addressed topics like financial and managerial accounting, I would be remiss if I did not also mention auditing in these all too brief comments. Here, too, I am struck by the fact that a major development like "performance auditing" has developed both in governmental and internal auditing with very little attention in the research literature that I am considering.

I should add, however, that I have no doubt that the abilities of contemporary researchers are up to supplying what is required to develop more fruitful and mutually beneficial interactions (a) with practice and (b) with other sciences. Some kind of shifting about and experimentation with present institutional arrangements might help to bring this about. The accounting profession has shown itself capable of responding to challenges in the past and the challenges here are certainly not beyond its capabilities. Here, however, the challenge is not so much a threat but rather a series of opportunities for improvements in both research and practice.

This drifting apart between research and practice has roots, I think, in the preparation provided by our current Ph.D. programs. This usually takes a form in which students are taught to draw their research problems from the literature in which they intend to publish. This is fine for theory development with "theory driven applications," perhaps, resulting at a later stage. It falls short of what is required, however, in research which is to take the form of "applications driven theory."

A few years ago, the "Bedford Committee" issued a report which has led to important changes in education for practice — at least in some institutions of higher learning. A similar committee might now begin to examine changes in the way we provide preparation for research. The trick in accomplishing what is required, I think, will involve effective arrangements to bring this type of education and research into focus on what is going on and needed to help improve professional practice without losing the rigor that is needed to maintain contact and influence what is going on in the sciences. The results could be large rewards for practice, for research and for science and society generally.