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Academic Versus Practioner Research in Accounting

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ACADEMIC VERSUS PRACTITIONER RESEARCH IN ACCOUNTING [note: GAP IN PAGES 56-64.]

by
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[....]

Financial Forecast [AICPA, 1980]. This guide describes the changing role of public accountants in reviewing, for a fee, forecasts of management disclosed to external parties. The guidelines given by the AICPA for such purposes stress the primacy of assumptional analysis, i.e., the detection and analysis of assumptions underlying forecasts. The guidelines, however, are extremely vague and the evaluation of the assumption impact entails reporting mainly of a qualitative judgment.

This inspired my AAA research proposal to be that of discovering means of quantifying qualitative judgments on the importance, uncertainty, cross-impact, and interactions of assumptions on forecasts. The approach is primarily downside up since the principal interdisciplinary search for solutions and methods was subsequent to and rooted in the defined accounting problem. However, there is a high degree of problem redefinition that goes along with solving this type of problem.

My search for interdisciplinary solutions has taken me into mathematics, scaling, psychology, and statistics, specialties where I hardly qualified as even being third-rate, especially in eigenvector scaling of nonsymmetric matrices of dominance judgments via the analytic hierarchy process, Saaty [1980]. By sheer bootstrapping I have learned about these specialties and even published papers in other disciplines, Jensen [1981b, 1981c, 1982a, 1982b]. Whether or not I qualify as a rabbit or snail in this regard will depend on the test of time. I have discovered that research deadlines and my own perceived need to publish are rabbitization temptations. I have set my own time deadline for my AAA project, which is good in the sense that it forces me to avoid procrastinating its completion and bad in the sense that I can't be a perfectionist snail. Deadlines often force shortcuts and shortcomings.

Upside Down Research

Much academic research effort, especially in business, is upside down in the sense that the researcher scans interdisciplinary literature, finds an area to zero in on, develops skill in the tools of that area, and then goes searching for an accounting problem for which the tools might fit. Much of the behavioral accounting research has been of this nature. I used to be naively critical of this approach but, having succumbed to it so often myself, I now seek to rationalize its importance in research. For example, at one time I became intrigued with parametric programming and have since looked for accounting problems in which it can be applied, Jensen [1978] and Jensen, Manes, and Park [1981] and Manes, Park and Jensen [1982]. I see similar patterns in many other researchers, including our most respected accounting researchers.

The issue is not whether upside down approaches in research are bad *per se*. Rather, the issues are whether the tools are (i) inappropriately applied to accounting problems, and/or (ii) applied to uninteresting accounting problems. For example, statistical tools have very frequently applied to accounting problems that seriously violate the assumptions of the statistical models, e.g., see Jensen [1979]. In addition, they may be correctly applied in artificial settings that have dubious external validity such as business game behavioral experiments and computer simulation studies. A primary danger in upside down research is the danger of being so strapped by the research tool that neither the novelty nor the applicability of the study is of interest to accountants. On various occasions I have previously made the point (and I'm sure Professor Baxter would agree) that contributions to statistics are best reviewed by statisticians and published in statistical research literature. I have also sympathized with accountants who find little of interest in some of the previously published academic accounting literature. "A major danger is the mechanical use of statistical techniques in overly superficial settings which have little or no relevance to accounting issues," Jensen [1979, p. 189]. *We might paraphrase the thrust of Professor Baxter's comment by saying that "accounting research really consists of a lot of first-rate statistics applied to third-rate accounting problems."*

Do We Invent Something Entirely New?

The end of Professor Baxter's long question contained a "suggestion" in the form of a very open-ended question:

> And the final suggestion: if we do want all this highbrow research, we do look for all the results that seem desirable, oughtn't we to scrap the basis that we have and invent something entirely new?

To which, part of Professor Sorter's response was: "I think, I believe that's gone wrong, we know what we were about but didn't write it down, or didn't communicate very well, what the purpose of accounting was."

The above friendly exchange prompted me to recall the purported distinctions between Models I, O-I, II, and O-II drawn by the insightful, albeit controversial, books by Chris Argyris and Donald Schon. Model I and its organizational O-I counterpart entail single-loop learning as described initially in Argyris and Schon [1974] and subsequently in Argyris and Schon [1978] and Argyris [1980, p. 14]:

> Model I is a model of *single-loop learning*. Single-loop learning is any detection and correction of error that does not require changes in the governing values or variables. This means that the use of Model I seriously limits the actor's learning capacity, especially when the issues are important, ambiguous, and threatening. It is precisely under these conditions where learning is critical that single-loop learning is not likely to be effective.

A helpful analogy here is the single-loop "learning" of a thermostat. The thermostat detects temperature and corrects temperature "error" whenever the temperature falls outside the prescribed tolerance limits. It is single-loop in the sense that it can only learn and control within a given system. It cannot "learn enough" to challenge the basic system and investigate alternative heating and cooling systems.

More to the point of accounting research on fundamental change and accountant interest in such research is the Argyris [1980, p. 14] statement that: "If the information about a particular issue is ambiguous, unclear, inconsistent, and if the problem is a threatening one, then given the governing values of Model I, the actors will hesitate to confront the ambiguity, lest they embarrass others and themselves." Furthermore, in the extension from the individual to groups, the Model I becomes Model O-I as described in Argyris [1980, p. 15]:

> What kinds of learning systems will people create when they are participating in a group and/or acting as agents for families, schools, government bureaus, municipal entities, hospitals, etc.? Briefly, they may be described as O-I learning systems (see Figure 2.2): that

> is, systems that build upon, but conform to, Model I (Argyris & Schon, 1978).
>
> The first feature of organizational learning systems is the primary inhibiting loop just described. People programmed with Model I will create primary inhibiting loops even if the system in which they were embedded were to encourage them to do otherwise. (To date we have found few learning systems that encourage inquiry that goes beyond Model I constraints.)

Argyris cites repeated examples of O-I learning systems to which one might add inhibiting loops in accounting change. Such inhibiting loops common in the practitioner world may also be found in academic research. Perhaps this is simply repeating to this audience what Homer Kripke [1980, p. 59] already put more directly in an Emanual Saxe Distinguished Lecture:

> To summarize, it seems to me that the FASB is starting down a path which may be largely sterile, and the SEC does not have the will to force basic change. The only persons with a more realistic approach to what the accounting process could be are the academic accountants.

Although I too am defending the role of academic accountants in interdisciplinary research "to force basic change," I perhaps have less faith in academic accountants based upon their track records to date, including the primacy of "normal" science in their research.

Argyris (1980, pp. 17-18) describes change to Model II as follows:

> Our research suggests that in order to begin to change O-I learning systems, individuals must first learn how to double-loop learn themselves. There is no way to bring about the use of Model II simply by changing the environment. Recall that people construct their reality and that they typically construct it with Model I. Hence, they have to be helped to learn Model II before they can take advantage of environmental and societal changes to encourage the development of Model II and O-II learning systems.
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> It is not easy to learn Model II because people who are programmed with Model I: (1) think they have the skills to behave according to Model II but do not; (2) unrealizingly inhibit their own and others' learning (because the only theory-in-use they have is Model I); (3) create O-I learning systems during their learning seminars, thus reinforcing the factors that they are trying to overcome; and (4) become frustrated with the slowness of the learning and the high degree of interdependence that is required.
>
> The governing values or variables of Model II are valid information, free and informed choice, and internal commitment. These are not themselves opposites of Model I governing variables. Similarly, the behavioral strategies required to satisfy these values are not the opposite of Mode lI. For example, Model I emphasizes that individuals are expected to be articulate about their purposes, goals, and so forth, and simultaneously to control others and the environment in order to ensure achievement of their goals. However, in Model II, the unilateral control that usually accompanies advocacy is to win. In Model II, articulateness and advocacy are carried out in ways that encourage others to confront the actor's views and, where necessary, to alter them. The goal is to produce the most complete, valid information possible in such a way as to maximize the participants' internal commitment to their position. Every significant action in Model II is evaluated in terms of the degree to which it helps the participants to generate valid and useful information, including relevant feelings, and to solve problems so that they remain solved without reducing the level of problem solving effectiveness.

Continuing our analogy, whereas Model I entails single-loop thermostat learning and control, Model II entails energy system double-loop learning and control in the sense that alternative systems are investigated.

Argyris makes a strong case that an "action science" should replace traditional "normal science" in building new universes, particularly in the realm of social science. Two purposes of exploring alternative universes are described in Argyris [1980, p. 122] as follows:

> The second major difference is that action science takes as a central concern the building of universes alternative to those presently existing. Because action science values valid information, then it will always limit itself to those alternative universes that do not increase the threats of validity. Action science, therefore, cannot be used to explore any alternative universe.
>
> There are *two purposes of exploring alternative universes.* The first is to discover and create worlds that enhance, more than is at present the case, the production of valid information and its concomitant requirements such as informed choice, personal causality, and ongoing commitment to the detection and correction of error. The second purpose is to create conditions that interrupt the automatic responses that people have acquired through socialization. Automatic responses are interrupted when existing theories-in-use and skills are found to be ineffective. When these factors are experienced as ineffective, a host of defenses organized around the sense of competence and the feelings of self-esteen are mobilized. When these defenses are mobilized, actions are executed that, when examined, lead the actors to become aware of a second layer of defenses that keep them unaware of what would happen if their learned skills were to be interrupted. Recognition of these unawarenesses brings to the surface tacit causal mechanisms that are at the heart of understanding the deeper structure of the present universe. Hence, creating alternative worlds is necessary to get at the operative but tacit structures of individuals and systems of the existing universe. (Emphasis added).

In Jensen [1980] *I reviewed the contention that academic accounting research has had almost no direct impact on accounting practice. This phenomenon is widespread in many, if not most, other professions, especially social science research.* Using leadership research as a point of focus, Argyris [1980, pp. 81-82] uses this as a basis for arguing for action science in place of normal science in leadership research:

> A frequent response to the request for advice on a new or perennial problem is to assert that basic research on the subject is meager and primitive and that little advice can be given that stems directly from the research. For example, a group of executives asked a leading authority for advice that could be derived from his and others' theories about effective leadership. The scholar responded that research on leadership was too primitive and incomplete to derive practical advice. However, he would be glad to offer what he admitted were ambiguous and imprecise generalizations in the form of maxims or guideposts.
>
> There are four aspects of that response that are intriguing. *First, there have been decades of research on leadership, yet, as a recent reviewer suggests, little of it is additive and much that is known does not deal with the complexity in which leaders find themselves in everyday life* (Stogdill, 1974). *Second, the scholar seemed to be unaware of the possibility that the generalizations produced by normal science may not be usable in an on-line model.* Under on-line conditions, human beings use propositions in the form of maxims and guideposts. *Third, there is an assumption that the piling up of knowledge utilizing normal science criteria for rigorous research will lead to useful and directly applicable knowledge.* I believe that this assumption is problematic for reasons already described. Additional reasons will be presented in the next chapter after we are able to document the discontinuity between the form of knowledge produced by normal science and that which is produced by action science. *Fourth, because the social universe is an artifact, every delay in informing citizens about leadership (or whatever other phenomenon) serves to reinforce the present*

> *state of the world.* The researcher who aspires to be descriptive is actually maintaining a normative position of conservatism. (Emphasis added).

The answer to Professor Baxter's question might be rephrased in the context of accounting research: "oughtn't we scrap the basis for academic accounting research and invent something entirely new?" In this context it is analogous to the appeal of Professor Argyris [1980, pp. 119-120] for replacing "normal science" with "emancipatory (action) research":

> Normal science research results tend to support the normative position that the status quo should be illuminated. *Emancipatory research that seeks new alternatives is discouraged.* Results are pessimistic about the potential of individuals and organizations for creating double-loop learning and changing in substantial ways. The creation of rare events is highly unlikely and not seen as the basis for a profitable research program. Also, normal science results ignore the inner contradictions of the Model I and O-I world that are not illuminable without interrupting the now of everyday life (in order, for example, to create new states of the world).
>
> Moreover, *many normal science research results are non disconfirmable in the action or local context.* Advice can range from being overly pessimistic about change to being overly optimistic (e.g., organic systems will lead to openness, trust, and double-loop learning; hiring new professionals will change an organization; or shifting leaders around can solve leadership effectiveness issues more effectively than can reeducating individuals).
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> Conventional research also tends to support the present biases and defenses that researchers must have if they are to maintain their present programs. Hence, if a research program assumes that organizations are anti-double-loop learning, then statements will be made to that effect even though no empirical research has been conducted to illustrate the assumption.
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> The advice that social scientists may give, derived from rigorous research to create a world with a better quality of life, is largely limited to a Model I and O-I. Apparently many social scientists are not aware of this limitation. One reason may be that they neither strive to create models of alternative worlds in order to create a dialectic nor do they tend to conduct normative empirical research. Some social scientists go further and assert that alternative worlds do not and cannot exist. This assertion about the potentialities of human nature is made with almost no empirical research evidence. The same social scientists would become quite upset if someone with so little data made equally strong statements about the world as it is.
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> Finally, there is a small group of social scientists interested in new alternatives. Unfortunately, most of these remain at the espoused level of analysis and do not conduct actual empirical research. Hence, the data base that is so urgently needed remains lacking. (Emphasis added).

Consonance Between Academic Accounting Research and Practice

Argyris [1980] is very critical of "distancing" between social scientists and their subjects (executives, practitioners, etc.). On Page 61 he summarizes:

> So far, I have tried to illustrate how the technology of *rigorous research may lead to distancing.* The distancing, in turn, can inhibit the production of valid information and hence constitutes a threat to validity. In this chapter, I should like to step back to explore the almost axiomatic assumption that social science research should describe reality for the purpose of understanding and prediction. Embedded in this requirement, I will suggest, is

> not only an additional threat to validity but also a serious bias against research that may be described as emancipatory (Habermas, 1972). The argument put simply is as follows: If most people hold Model I theories-in-use and if most learning systems are O-I, then the requirement to describe reality will inevitably restrict the researcher's attention to reality as it exists -- the status quo. Moreover, since Models I and O-I assure no learning of the double-loop variety, then it is highly unlikely that descriptive research might by accident lead to generalizations about new (double-loop) states of reality.
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> This argument does not imply that new options are not known. People advocate worlds whose properties are significantly different from Model I and O-I, worlds where, for example, double-loop learning and trust can be enhanced. These options are worth study if social science is to help human beings consider new alternative universes. To date the study of new options has been left by scientists to the politicians, to religious zealots, and to revolutionaries. Indeed, one way to disparage research on double-loop options is to characterize it as falling into one of these domains. (Emphasis added).

He further elaborates on "distancing" on Page 182:

> The dominant aspiration of present social science research is to state as precisely as possible the invariant relationships between, or among, a specified set of variables. Precision is to be achieved primarily through the use of quantitative methods. *In many cases, the type of quantification used tends to distance the researcher and the subjects from the reality to which the propositions are supposed to apply*. To be sure, all concepts are abstractions from, and hence distance individuals from, the context of action; indeed, that is their purpose. However, the distancing that results from the attempt to satisfy the requirements of precision through quantification frequently goes beyond abstracting from reality so that the concepts become disconnected from the context of action. (Emphasis added).

Argyris argues that normal science sacrifices far too much validity for the sake of precision and accuracy. Quantitative methods and experimental controls aimed at precision are referred to by Argyris [1980, p. 124-5] as "technology" not suited for on-line (practical) reality:

> *The difficulty with the technology presently used to gain such precision is that it creates concepts that may not be applicable in the action context, as well as introduces conditions such as unilateral control over subjects and minimal interest in new universes,* all of which increase the likelihood of producing unrecognizable threats to validity. These conditions combine to make it unlikely that knowledge will be additive in the sense that it will eventually yield a significant reduction in the applicability gap. It appears difficult to take rigorous knowledge with high precision and low predictive validity (low, from the actors' view) and combine it to produce low precision and high accuracy. The high precision that may lead to whatever accuracy a proposition contains, requires, if it is to be emulated by an actor under on-line conditions, a set of constraints that act to reduce the accuracy when it is being used in real life. To put it in our terms, the theory-in-use that leads to high precision necessarily leads to conditions that produce unrecognizable errors and low accuracy.
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> Zadek (1972) correctly points out that the relationship between precision and validity is complex. He suggests a "principle of incompatibility" that states that as system complexity increases, our ability to make precise and valid statements is maintained until a threshold is reached. Beyond that threshold, precision and significance are probably mutually exclusive. The closer one looks at the real world, the fuzzier it appears and the fuzzier will be useful solutions. (Emphasis added).

The joint roles of scientists and practitioners in action science rooted more closely to validity and reality are implicit in the following assumptions listed by Argyris [1980, pp. 181-82]:

1. The function and mode of theorizing for the development of action science and for taking action in everyday life are similar. The function of theory in action science and in practice is to enact an order and to comprehend that order. The scientist and the practitioner use theory to get their arms around the problem, to create some degree of stability, to diagnose the order created under on-line conditions, and to provide the basis for the design and implementation of action.
2. The mode of theorizing is to produce models (such as Models I and O-I) that extract from the action context those patterns required to operate effectively in that context and yet that can be generalized to many different domains. The scientist and the practitioner, therefore, seek those models that comprehend the action context in a way that allows the models to be used under on-line conditions.
3. Public disconfirmability plays a central role in the world of practice and in the world of science. Actors cannot make their intended conse-quences come true nor detect error without placing a high value on disconfirmability. Disconfirmability not only places a responsibility on the actors to behave in ways that encourage testing but also makes it more likely that others will come to trust the actors and their intentions. As trust and credibility increase, the probability that valid information will be produced for the difficult issues also increases.
4. The requirements for producing and using valid information are also consonant. The practitioner is more apt to use models that require or permit: (a) incomplete information; (b) low precision; coupled with (c) high accuracy.
5. The assumptions made by the action scientist and the practitioner about the nature of the universe are also consonant. Both assume that there is an order, that the order is enacted, that the order and the way that it is enacted are discoverable, and that both of these are alterable.
6. The practitioner and the action scientist also assume that some nontrivial concept of causality exists, otherwise public disconfirmability would be a meaningless game. Along with causality is the assumption that elegance is desirable. Elegant explanations are those that comprehend more of the action context with the least number of concepts and untestable assumptions.
7. The assumptions that the universe is enacted, that causality exists, and yet that the order is alterable lead to yet another similarity. The order of the universe acts to constrain what actions are possible. If the order exists, it means that people use skills in ways that lead them to be effective and simultaneously maintain the order. This means that individuals' sense of competence is intimately related to the maintenance of some commonly accepted social order.

Under these conditions, skills used by individuals in everyday life, and the societal norms and values upon which they are based, become tacit. Once tacit, little thought is given to altering them, and much thought may be devoted to preventing them from being altered. But, as we have seen, the resulting overly determined stability acts to make people unaware of double-loop errors and inner contradictions. Therefore, both scientists and practitioners are concerned about generating new alternatives. New alternatives may be *discovered and invented* without changing theories-in-use or learning systems. New alternatives can be *produced* only by acquiring new skills and creating new learning systems. Hence, at the core of action science and of practice is the assumption that individuals are causally responsible for maintaining their world and for changing it.

Professor Argyris advocates a Model II perspective in which researchers are more open minded to creativity and learning new skills to solve problems in a different way. Model II as outlined by Argyris, however, is more a model of spirit of approach than recipe for approach. His concern focuses heavily upon a long-range time perspective for improving leadership, organizations, and society. *Herein lies the core of the problem of extending Model II to accounting research. Neither practitioner nor academic accounting researcher has an environment for Model II.* The environmental impasse centers around:

1. Time pressures for immediate practical application of research or demonstrated linkage with practical problems.
2. Publish or perish time pressures in academia and lack of long-term commitment in

practitioner research discourage longer-term and more fundamental research.

3. Lack of appreciation for the importance of interdisciplinary research coupled with narrow focus of interdisciplinary research, e.g., use of multivariate statistical analysis as a tool to analyze accountant and management opinions is narrowly focused in that the interdisciplinary academic expertise is not being used for creative, frontier research as it is for tabulating already formed opinions.
4. Lack of a basic commitment to and interest in academic research. Practitioners have little interest in academic research literature, and many "so-called" academic researchers are ravenous consultants in sheep's clothing. Other academic accountants drop out of research to teach, write textbooks, and tend their lawns.
5. Lack of understanding of a Model II-type environment.

The important aspect of a Model II perspective is greater concern for validity. Accounting research must ultimately be rooted in the information needs and uses of individuals and society. In this context accounting research becomes social science research. The most basic obstacle in nearly all of social science research is validity. Kerlinger [1973, p. 457] discusses three classes of validity as follows:

> The commonest definition of validity is epitomized by the question: Are we measuring what we think we are measuring? The emphasis in this question is on what is being measured. For example, a teacher has constructed a test to measure understanding of scientific procedures and has included in the test only factual items about scientific procedures. The test is not valid, because while it may reliably measure the pupils' factual knowledge of scientific procedures, it does not measure their understanding of such procedures. In other words, it may measure what it measures quite well, but it does not measure what the teacher intended to measure.
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> Although the commonest definition of validity was given above, it must immediately be emphasized that there is no one validity. A test of scale is valid for the scientific or practical purpose of its user. An educator may be interested in the nature of high school pupils' achievement in mathematics. He would then be interested in what a mathematics achievement or aptitude test measures. He might, for instance, want to know the factors that enter into mathematics test performance and their relative weights in this performance. On the other hand, he may be primarily interested in knowing the pupils who will probably be successful and those who will probably be unsuccessful in high school mathematics. He may have little interest in what a mathematics aptitude test measures. He is interested mainly in successful prediction. Implied by these two uses of tests are different kinds of validity. We now examine an extremely important development in test theory: the analysis and study of different kinds of validity.
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> The most important classification of types of validity is that prepared by a joint committee of the American Psychological Association, the American Educational Research Association, and the National Council on Measurements Used in Education. Three types of validity were discussed: content, criterion-related and construct. Each of these will be examined briefly, though we put the greatest emphasis on construct validity, since it is probably the most important form of validity from the scientific research point of view.

He earlier states [p. 456]:

> The subject of validity is complex, controversial, and peculiarly important in behavioral research. Here perhaps more than anywhere else, the nature of reality is questioned. It is possible to study reliability without inquiring into the meaning of variables. It is not possible to study validity, however, without sooner or later inquiring into the nature and meaning of one's variables.

Interdisciplinary Research Postscript: "The Snail Discovers a Solution to Yesterday's Problem"

A sad phenomenon that occurs repeatedly in life is bad timing between problems and solutions, e.g., it often occurs that eventual solutions to problems arrive too late for the problem. The workaholic finally retires in comfort only to discover he is too burned out and decrepit to enjoy the good life he can at last afford. His lotus eating progeny and the U.S. Treasury split the fruits of his labor. The airline finds your luggage in Fairbanks on the last day of your Hawaii vacation. The ambulance arrives too late to save a life and hauls the victim to the morgue. You are being fitted with a rubber prosthesis when new medical research reveals surgery wasn't necessary. The waiter finally brings your order at the end of your lunch hour.

In Jensen [1981a, p. 140] I called the time problem the most formidable hurdle between accounting research and practice:

> Years of data gathering, theory development, and innovation in research methods may be needed for results business and the accounting profession want immediately. Research moves like a slow snail in zig-zag lines toward desired objectives. The rabbit may leap faster and farther but be far less successful. It was impossible in 1900 for a round-trip manned venture to the moon, but years of plodding interdisciplinary research in propulsion, flight engineering, biology, computers, etc. eventually produced successful results, results that men leaping off cliffs with flapping arms could not achieve.
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> Like travel in outer-space, accounting research successes will require years of snail'space interdisciplinary research. I stress the interdisciplinary aspect because accountants sometimes fail to see the building-block values of research in the humanities and science. We need on occasion to be reminded that cost accounting had early germination in industrial engineering and economics; computer and data processing systems are rooted in mathematics, engineering, and science; planning and control models such as PERT and linear programming grew out of operations research studies; efficient market studies were initiated by researchers in economics and finance; time-series forecasting was developed by economists and statisticians; subjective forecasting approaches such as Delphi techniques grew out of studies in mathematics and the behavioral sciences; and auditing and tax practice are impacted upon directly by legal research and indirectly by social science research such as political research that uncovers a need for independent audits in government.
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> The need for research in accounting is obvious. Research will play a vital role in the adoption of the accounting profession to changing conditions in modern society. It is less clear, however, who should do this research and how such research should be linked to the standard setting process. *The most formidable problem is probably that of the time lag between the perceived need for action in standard setting and research output*. The wandering research snails can't be found when the FASB, CASB, IRS, SEC, AICPA, NAA, FEI, GAO, etc. need them for burning issues at the moment. For example internal control and auditing research barely began to make progress in punched card computer systems when such systems were rendered obsolete by interactive on-line terminal systems. Snails have a difficult time catching up with moving trains.

Such time lags produce trials and tribulations, but St. Paul states:

> We glory in our tribulations, knowing that tribulation produces patience, and patience experience, and experience hope; and we are not ashamed of hope. St. Paul, *Romans*, 5.

Academic accounting researchers must unashamedly appeal to the practice world for patience.

Recall Professor Baxter's question: "if we really want all this highbrow research, we do look for all the results that seem desirable, oughtn't we to scrap the basis that we have and invent something entirely new?" Even in a Model II "highbrow" environment it is doubtful that our present basis for and practice of accounting would be scrapped all at once; the magnitude of the research problem is much too immense for sudden change. We perhaps will strive for and ultimately reach something entirely new that has met the test of utility in society. More important is the striving for it in a Model II environment. Equally important is that we approach it with better interdisciplinary research. Much of the problem with academic attempts in the past is that they have been entirely "third rate." We may tolerate third-rate auxiliary research skills in our researchers but not the ultimate third-rate research.

Directions for the Future

In Jensen [1981a, pp. 155-66] I propose some directions for the future of academic research. These are repeated below.

Closing Communications and Expectations Gaps

Gaps between academic and practitioner research will continue, but as pointed out previously, such gaps are not as serious as they appear on the surface. Practitioners could perhaps both increase and accelerate research efforts with more rapid and coordinated communications of "discoveries" in industry trenches, e.g., the discoveries of new lease and employment contract provisions. Professors could make greater efforts to assist in translation of their research into terminology more easily comprehended by practitioners.

More serious "expectations gaps" between what is expected from research and what can be delivered when it is needed are unavoidable. Research is a powerful but wandering snail that can be hurried only at extreme cost and within technological restraints. But more can be accomplished to the extent expectations gaps are due to: (1) increasing standards for rigor in academic research that direct attention away from intractable practitioner research problems, and (2) increasing publish or perish pressures that direct attention away from long-term, higher risk research efforts. Less can be done about accelerating research performance, especially research that is interwoven with research in other disciplines and accordingly cannot accelerate at a pace faster than research in those disciplines. For example, major discoveries in integer programming will have tremendous impacts on business and accounting research, but we must await discoveries in the discipline of mathematical programming to provide highly efficient means of dealing with integer restrictions and other nonconvexities.

Increased funding of research, especially research aimed at problems that greatly trouble business and accounting firms, may help to close expectation gaps. Creation of accounting research centers with research as a sole or primary mission may allow researchers the time and resources to conduct full-time research, a luxury that seldom exists for accountants in industry, firms, or universities. Increased utilization of advances in computer and communication technologies may also serve to link researchers in remote locations and to link researchers with data, and library materials, and practicing accountants.

Low Quality Research: Turn Off the Bubble Machine

In 1979/80 *The Accounting Review* accepted only 11.2% of the 260 manuscripts submitted for publication. This was slightly lower than the 12.5% in 1977/78 and 14.2% in 1978/79. Admittedly the refereeing process is imperfect and some innovative studies were rejected while some marginal studies were accepted. However, those of us actively engaged in the refereeing process nearly all agree that many of the papers submitted were atrocious in terms of intent, innovation, method, analysis, and/or writing. Some of the rejected papers are quickly placed by authors in new envelopes and submitted in succession to a sequence of journals until either accepted or until the authors become exhausted in their submission efforts.

I can only speculate as to reasons for some of the lamentable rejected papers. Many seem to be from struggling faculty poorly trained in research and writing who are unwillingly caught up in the publish or perish syndrome. A few may be from adequately trained faculty who in an effort to compile a long resume elect a shotgun approach with a high volume of short and sloppy efforts. Another tactic is to milk a reasonably good study with multiple papers that stretch the study for more than it's worth. A few papers may be from the unimaginative who simply follow the recipe of a study found in another (e.g., psychology) discipline and fail to demonstrate utility of the study to accountants. Still others try to hide the absence of originality in a camouflage of length and/or mathematical notation.

In some instances we are repeatedly deluged with bad papers from authors that never give up. Editors report receiving ten or more papers in succession from an author who has never had or rarely has an acceptance. At the other extreme there are some very capable academic and practitioner researchers who are diverted from research because of personal or other professional interests, e.g., textbook writing, teaching, consulting, and business interests.

Research having both high potential and high risk is often avoided. Reasons may include the low expected reward relative to other opportunities and/or the low expected reward of a major single long-term project relative to a higher count of shorter low risk research endeavors. Research may also be avoided if it is difficult to find required funding. Some researchers indicate that they avoid long-term, high risk research because uncertainties and capriciousness of the journal refereeing process increase the risks of insufficient recognition for their efforts.

Hopefully, in the future capable scholars will be better enticed into taking on long-term and higher risk research endeavors. Constraints arising from the lack of funding, publish or perish syndrome, and uncertainties of publication should be relaxed and that research motivation should be increased. A point for discussion in this conference is how to widely communicate innovative ideas without having to be delayed and/or blocked by the journal refereeing process, e.g., by publication of nonrefereed abstracts.

The Myth of Volume Publishing by Academic Accountants: Turning Rocks into Snails

One of my colleagues, Jim Hasselback, obtained some rather surprising results on a study of publication records of 2,910 accounting educators in over 300 journals for the 1970-77 period. He also performed an analysis by individual universities having doctoral programs. The average number of articles published over the entire eight year period was 1.92 or 0.24 per year. In only one university did the faculty average more than one article per year. In other words, an accounting professor is about average if he or she publishes one article every four years. A large proportion of the articles published in that period did not report research findings and did not purport to be research studies. Many could hardly be considered major articles or widely cited articles. In Hasselback [1979, p. 7] it is concluded that:

> As can be seen from the information presented, individuals in Accounting are not publishing to the extent believed. Many individuals talk of "one major article per year." The fact is the average is less than two for an eight--year period -- and that includes many articles that could hardly be called major.

What this suggests is that too many accounting faculty are research rocks rather than snails. More educators should be expected to take on research intended for publication, and they should be encouraged to work toward "major" efforts, i.e., turning rocks into snails but not research rabbits.

Research for the Standard Selling Process

To date, research of greatest interest in standard setting has been research aimed at detecting and measuring the impact of selected standards on disclosures (e.g. financial statements, ratios, etc.), on security prices, managerial decisions (e.g. shifting from leases to bond financing), and other economic impacts. Little, if any, attention has been directed toward higher order impacts on society and public policy. Major deficiencies along these lines are noted by May and Sundem [1976, p. 752]:

> Accounting policy makers must specify the set of accepted accounting practices, which

> depends on the collective choice rule and social decision system they use (which were discussed in the previous section). It also depends on the accounting alternatives available; development and refinement of these alternatives is an important area for research. Non-accounting public policy is also an important input. An often neglected area of research is how accounting policy fits into an overall public policy framework.

They also note the lack of research relative to nonuser impacts [p. 753]:

> Direct effects of financial reports on individual actions may be assessed by examining the decisions of users of the data. But secondary effects, some due possibly to the presence of information or decision alternatives that would not exist in the absence of financial reports, may affect the action of nonusers as well.

Probably too little attention has also been devoted to costs and confusion in industry over meeting standards. An excellent example is the unforeseen implications of the Foreign Corrupt Practices Act of 1977. In a recent study, Mautz [1980] found much confusion over the meaning of the act, definitions of terms, and economical and feasible ways of accomplishing the act's expressed purposes.

In addition to unbalanced research considerations relative to impacts of standards on users, nonusers, firms, and society, too little research attention has been devoted to forecasted impacts of alternative standards prior to choice of a particular alternative. Prior to implementation of a given standard, alternatives are evaluated heavily on the basis of a priori reasoning and public testimony. It may be possible beforehand to also test hypothesized relationships, e.g., in behavioral experiments and systematic futurism research.

There is no evidence that behavioral research has been important in standard setting even though the impact on behavior is the crux of most issues in standard setting. A major problem lies in the artificiality and inconclusiveness in most behavioral experiments to date. The resulting lack of generalizability is not unique to behavioral accounting research. Winkler [1973, p. 267] notes:

> The need for more realistic experiments is particularly pressing in view of the rapid increase in the practical application of modern inferential and decision-making models. If the results of artificial experiments do not carry over into realistic experiments, the implications with respect to the implementation of such models would be very significant. A potential example is the development of "information processing systems," discussed earlier in this section.

The point is that if greater attention is paid to developing more realistic behavioral impact research, these studies may prove extremely valuable in the standard setting process. Dynamic longitudinal studies of behavior over time in varieties of contexts are needed. The importance and difficulties of studying how decision makers arrive at their evaluation and choice processes is noted by Birnberg [1980, p. 350]:

> What is being suggested is that we, as accounting researchers, may begin better to understand accounting/auditing decision making if we examine it in an evolutionary state. Unfortunately, such a series of studies is likely to be formidable. They may require tolerance from promotion committees and journal referees as authors work on the puzzle one small step at a time.

This type of behavioral research might be extended to the standard setting process itself, i.e., behavioral study of the evolutionary states of decision processes of those entrusted with the responsibility of setting standards.

It appears that behavioral accounting research is accelerating in academic research. Journal editors reveal a rapidly rising proportion of submission of papers in these areas. Of the 260 new manuscripts submitted to the Accounting Review in 1979/80, 26% were classified by the Editor as behavioral accounting, 11% were market studies, 8% were time series and forecasting, 4% in taxation, and 3% in history, leaving 48% unclassified, Zeff [1980, p.2]. Hopefully, amidst the acceleration of behavioral accounting research, there will be more studies and ways of improving rather than merely describing performance. Winkler [1973, p. 268] notes:

> In general, experiments concerning human behavior in realistic inferential and decision-making situations could have important implications for the determinations of inputs for formal models, the training and utilization of experts, the roles of humans and computers, the gathering and summarizing of information, and many other important questions. The ultimate practical question with regard to studies of human behavior in inferential and decision-making situations is this: How does a highly motivated, experienced individual in an operational setting in his area of expertise, given appropriate feedback regarding past predictions and decisions, perform inferential and decision-making tasks, and can his performance be improved upon in any manner?

It should also be noted that academic research has especially neglected specific problems at industry and company levels in accounting and auditing, e.g., problems in accounting for real estate, railroads, airlines, municipalities, banks, farms, etc. One is more apt to find these issues mentioned in practitioner journals. More research, including doctoral thesis research, should be directed in such areas.

Applications of Interdisciplinary Innovation

In my viewpoint, there is too little systematic and in-depth monitoring of research in science, engineering, law, humanities, etc. in an effort to distill that which has potential application in accounting. Increased efforts in this regard improve the chances for discovery of important theory and applications in accounting. The major drawback is that there are so few accounting researchers relative to the vast explosion in the literature of multiple disciplines.

1. Is accounting attracting research efforts from specialists in other disciplines?

2. How much time is both available to accounting researchers and utilized in scanning interdisciplinary research, as evidenced in part by hours spent in libraries and interdisciplinary conferences?

3. How can interdisciplinary research scanning be systematized, coordinated, communicated, and stimulated on a continuing basis?

Interdisciplinary Futurism Research in Accounting

The major dilemmas faced by standard setting bodies are in: (i) detecting accounting and auditing alternatives, and (ii) forecasting the impacts of these alternatives on behavior of investors, management, government officials, securities markets, employees, consumers, courts, economic development, etc. Major inputs in both regards are invited. For instance, with respect to the FASB, Mosso [1979, pp. 1-2] writes:

> The FASB has demonstrated a willingness to listen and to respond constructively that is unmatched by any government agency that I have been associated with or have observed. Many are skeptical on that point because they do not see their suggestions adopted by the FASB, but I can assure you that no comment or suggestion is ignored by the Board. Even those that are rejected have a tempering influence and often cause subtle changes in adopted accounting standards.

But as mentioned previously, the volumes of written and oral testimony need a fact distilling machine of one type or another. That machine ideally entails research components. However, when scientific research snails are too slow, as is usually the case, the distilling process is reduced to mainly expert judgment relative to impact conjectures, hypotheses, and theories expounded in mountains of testimony and written comments regarding controversial issues.

The obvious question is whether there is a happier medium where research tools and expert judgment can be joined together to make both discovery of alternatives and impact forecasts more effective and/or more efficient. One place to look is futurism research, which over the years has generated novel approaches such as Delphi technique, cross-impact analysis, scenario generation, expert panels, brainstorming, gaming, historical analogy, decision trees, probabilistic forecasting, etc. for the purpose of exploiting human judgment in detecting futurism alternatives and forecasting futurism outcomes, e.g., Holloway [1978, p. 20].

By way of illustration, consider the tasks of standard setting bodies such as the FASB, SEC, AlCPA, etc. and then consider basic missions of futures research described by Enzer [1972, pp. 30-31] as follows:

> It is within an environment of dynamic uncertainty that futures research is attempting to provide some needed guidance. Futures research is an interdisciplinary activity concerned with the development and application of systematic techniques for obtaining and evaluating judgments on the nature and desirability of alternative futures. The basic mission of futures research is to broaden our time horizons and enable us not only to anticipate long-term change per se, but also to see how, by controlling such changes, we can increase the range of our alternatives and select alternatives likely to produce a better society in both the near and longer time periods.
>
> It is important that such an activity be interdisciplinary, because changes in one aspect of society can often affect key factors in other aspects, and because in our highly specialized organizations such expectations often go undetected, thus reducing our time horizons.
>
> It is important that such an activity be systematic, because we may inadvertently be preoccupied by topical or pet ideas or concepts which may make it difficult for us to consider the many facets of the complex issues confronting us. This may lead to our overlooking important trends, developments, or casual relationships, thus reducing our time horizons.
>
> It is important that such an activity include judgmental data, because much of the insight we require for anticipating future considerations cannot be obtained by any other means. This does not mean that we should use our judgment where other more rigorous techniques are available, but we should recognize that judgment is often our only source of information about the future and we should use it appropriately to avoid reducing our time horizons.
>
> It is important that such an activity seek alternative futures, because the only valid reason to be concerned about the future is its relevance to actions or decisions we may presently be considering. The greater our alternatives, the more purposeful and confident our actions become, and the more sensitive we become to the problems and opportunities they are likely to present. With such understanding and sensitivities, we broaden our time horizons.
>
> Futures research is concerned with the forecasting rather than predicting the future. This does not deny that some things may be regarded as inevitable, but if all aspects of an issue were inevitable, and nothing could be done to alter the future, the analysis of an outcome would become a point-less intellectual exercise. What is important is to identify what alternatives are possible, how likely each is, and what controls exist to increase or decrease their likelihoods of occurrence.

By its very nature, technology assessment is concerned primarily with identifying alternatives and making broad assessments of their implications. This basically involves exploratory forecasting analyses, and while there is no unique procedure for conducting an exploratory analysis, there appears to be a sequence of steps that has general applicability. The steps in this sequence attempt to:

> Define the issues and current status,
> Identify possible futures and their likelihoods of occurrence,
> Identify possible actions and their suspected impact,
> Evaluate alternatives and select possible desirable courses of action.

Although the current techniques of futures research are superior to those our forebears possessed, they do not enable us to forecast over longer time periods than they did. They do enable us to see further into change than they did, but not into time. This distinction between forecasting into change and forecasting into time represents a serious dichotomy. Clearly any forecaster can accurately predict tomorrow if it is certain to be the same as today. It is the prospect of change which introduces uncertainty into forecasts and it is the fact of change which reduces the accuracy of these forecasts as the time period increases.

Against this background of analytic needs and analytic procedures, the role and importance of cross-impact analysis can be shown.

Olaf Helmer and Theodore J. Gordon originally conceived of the concept of the cross-impact method in designing the game Future. The technique was subsequently extended and tested experimentally using a computer to aid in the synthesis of the event interactions. The first operational analyses using the cross-impact method were made in studies designed to forecast issues and opportunities for the State of Connecticut and to identify developments of importance to the future of education. Since then, other analyses using this technique have been reported.

Futurism research and assumptional analysis in accounting are topics of my present research project by the American Accounting Association. Hopefully, more academic and practitioner researchers will also explore the exciting possibilities of new avenues of research in these directions.

Forecasting Performance: Reviews and Consulting

At least two objectives need to be distinguished, i.e., (1) the objective of forecasting impacts of accounting standards versus (2) the objective of forecasting impacts of management decisions and external events on firm performance. Futurism research might play an important role relative to both objectives, but there are probably more obstacles in the latter case. May [1980, p 56] notes:

> Disclosures relating to future transactions and events are considered to be more competitively disadvantageous than disclosures related to past events. Financial disclosures of past transactions and events can, at worst, disclose already existing conditions. Disclosures of future events, however, might expose and thereby sacrifice a present competitive advantage or might provide competitors with information that could lead them to new competitive advantages.

There are accelerating pressures for management, accountants, and auditors to assume greater responsibilities in reporting both (i) widescope information for improved investor forecasting of enterprise performance; and (ii) expert (notably management) forecasts of enterprise performance. Accountants and auditors are being pushed and pulled into uncharted territories of futurism assessment. New roles must be played in providing qualitative assessments of linkages between past and future performance, aiding management in identifying assumptions and future business environmental conditions, assisting in finding "most probable" outcomes, and reviewing (perhaps eventually auditing) forecasts reported to the investing public.

It is with some trepidation that accountants push into new territories of aiding, reviewing, and perhaps auditing forecasts and evaluating future projections. The track record of both objective forecasting techniques (time series projections, regression analysis, econometric models, etc.) and published management forecasts have not been very encouraging. The economic, social, and political environment has become more unstable, which in turn causes greater nonstationarities in world business. Countless variates impact upon business performance, many of which are qualitative and interact in a complex manner.

Objective forecasting tools are inadequate in the face of severe nonstationarities and interactive complexity. It becomes necessary to resort to more subjective forms of analysis that rely on human abilities and intuitions. This, in turn, greatly complicates the role of an accountant seeking to review information provided to investors.

Future research is soon to have a major role in accounting and auditing. Pressures have risen for both increased and improved management forecasts in annual reports. Managers are likely to seek increased help from accountants in future gazing and reporting. External auditor association with management forecasts, long verboten, is now encouraged and may soon be required. The S.E.C. historically prohibited forecasts in prospectuses but now allows forecasts under broad standards and disclosure requirements and a safe harbor rule (Releases 33-5992, 34-15305, 34-6084, 34-15944, 35-21115, and 39-532). The A.I.C.P.A. issued a series of guide-lines (MAS Guideline Series No. 3) including Guideline No. 8 which reads as follows:

> A financial forecasting system should provide adequate documentation of both the forecast and the forecasting process.
>
> Documentation makes possible management review and approval of a forecast. It facilitates comparison of the forecast with actual financial results, and it provides the discipline necessary for reliable forecasting.
>
> Documentation involves recording the underlying assumptions as well as summarizing the supporting evidence for the assumptions. Documentation should provide the ability to trace forecasted financial results through intermediate calculations back to the basic underlying assumptions.
>
> Adequate documentation makes it possible for persons experienced and qualified in forecasting to reconstruct the forecast. Documentation should cover the system, as well as individual forecasts, and should provide an organized record of both that can be maintained and made available for subsequent use.

The February 28, 1980 "Exposure Draft: Proposed Guide to Review of a Financial Forecast" outlines auditor responsibilities in reviewing financial forecasts. Particular emphasis is placed on investigation and reporting of underlying assumptions.

Investigation of underlying assumptions behind a management forecast in many instances entails in-depth probing of future scenarios envisioned by management and subjective probability linkages of future events. Although isolated applications of the Delphi technique appear in accounting literature, little mention is made of more recent extensions of this and other techniques in future research.

Research Problem Formation in Accounting

Locke once stated that "a problem well put is half solved." Few attempts have been made to study the critical conception and embryo phases of accounting research, where creativity, innovation, experience and enthusiasm intersect at the crucial early stages. Researchers interested in the pursuit of methods for studying problem awareness, problem definition, hypothesis formation, etc. are referred to Lyles and Mitroff [1980].

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