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**Discussant Comments on “The CPAS/CCM Experiences:
Perspectives for AI/ES Research in Auditing”**

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Having wandered from the halls of academe this past year, it is probably useful to make a few observations from one tainted by the “real world” so readers can appropriately calibrate my comments.

First, this past year has reaffirmed something I have been observing for several years—our financial systems architecture is in a sorry state. Its utility is being questioned more and more, and organization executives are more open to investing in meaningful changes to the financial system than ever before.

Second, layering an auditing decision support system (DSS) on top of the current financial system does not address the underlying architectural problems and therefore is unnecessarily costly and cumbersome.

Third, auditors are sitting on the sidelines of business and information system transformation projects which often result in business and information processes for which they are wholly unprepared to audit.

Fourth, business and information process transformation projects often result in major changes to an organization’s business and information process risks and call into question the utility of traditional control philosophies and techniques.

Fifth, risks and controls are becoming more critical than ever before, and are often more manageable with a different control philosophy that embraces technology as a tool for enhancing control rather than something that only increases an organization’s risks.

Sixth, auditing has a real opportunity to enhance its value by preparing to be much more involved in business and information process transformation.

As I have read the Jenkins Committee report, and early drafts of the Elliott Committee report, I believe my observations are not nearly as heretical as they might have been perceived 10 years ago. These two reports underscore the need the profession has to be involved in serious introspection in order to face the new challenges and opportunities in the market place.

With these observations in mind, I used two papers that have laid out a framework for evaluating artificial intelligence based research:

Denna, E. L., J. V. Hansen, and R. D. Meservy, “Development and Application of Expert Systems in Audit Services,” *IEEE, Knowledge and Data Engineering*, June, 1991.

McCarthy, W. E., E.L. Denna, G. Gal, and S. R. Rockwell, “Expert Systems and AI-based Decision Support in Auditing: Progress and Perspectives,” *Intelligent Systems in Accounting, Finance and Management*, Jan, 1992.

The two primary contributions of these papers are as follows.

The first paper provides a framework for classifying the contribution of a piece of research. As is illustrated in Figure 1, there are three primary areas of contribution: knowledge acquisition, knowledge validation, and knowledge representation. It is rare to find work that provides significant contributions to more than one of these areas primarily because AI work tends to inherently have scope problems. Trying to make a fundamental contribution to more than one of these areas is both dangerous and difficult. Dangerous because closure is made much more challenging, and difficult, because tackling more than one area makes it difficult to specifically identify the contribution and further compounds the closure problem. The paper calls for focus by researchers and lays out a potential agenda for those interested in pursuing AI-based audit research.

The second paper adapts the March (1988) framework for classifying computer science research to the AI-based audit research problem in order to provide some guidance to the question of whether a piece of work is research or development. At the time, the literature seemed to have an avalanche of what was claimed to be AI-based audit research. The authors conclude that, "Building software systems which make marginal improvements with known approaches in established domains is definitely development, while building software systems which make significant improvements with novel approaches in unexplored domains is most certainly research" (p. 62). The intent of the authors was to provide some guidance for distinguishing research and development for publication purposes.

With this as background I would offer the following observations of the paper at hand:

First, I am unclear as to the contribution of the paper in terms of the framework offered by Denna, Hansen, and Meservy (1991) or as to whether the work is better viewed as research or development. This conclusion is the result of not seeing any specific contribution to the domains of knowledge acquisition, knowledge representation, or knowledge validation, nor is there any specific evidence of the work providing "significant improvements with novel approaches in unexplored domains." In making this observation, I am not suggesting the work is neither valuable nor interesting. Frankly, the problem domain and approach are both very interesting and the ideas appear valuable. Nonetheless, the contribution is unclear using the two papers I have cited as evaluative works.

Second, the CPAS/CCM architecture appears to be a function of the traditional financial systems application architecture. A fundamental change in the nature of the financial systems architecture may well make the CPAS/CCM architecture entirely useless or require fundamental changes. This would actually be an interesting follow-up test to perform. A corollary to this observation is the question of how much of extant audit expertise is applicable when an organization undertakes a fundamental transformation of its financial systems or its business processes. It seems this question would need to be resolved before attempting a work as large in scope as the CPAS/CCM project.

Third, I have difficulty concluding whether CCM actually has demonstrated intelligence (what McCarthy, Denna, Gal, and Rockwell [1992] refer to as deep knowledge) or even rudimentary expertise, or whether the application is more appropriately a sophisticated amalgam of mathematical models. If the latter, the work is probably more appropriately viewed as a statistical modeling application, and should thereby be evaluated as such, rather than as AI-based work.

Overall, I think the paper could be significantly improved if the author would clearly specify the contribution to the domains of knowledge acquisition, knowledge representation, or knowledge validation. Without such clarity, readers will be left to wonder whether the piece approaches the purely research hurdle of making "significant improvements with novel approaches in unexplored domains."