What is wrong with CAE and can it be fixed?

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1 Early CAE promises

"Contemplation of the similarity between the brain and computers promises to shed light on some of the mysterious workings of the mind and also suggest fruitful directions for computer design and programming," announced Dalkey (1958, p. 3) at the ASCE First Conference on Electronic Computation, in 1958. While acknowledging that the above similarity is only an analogy, he did not find it difficult to foresee in 100 years a remarkable era, "not because of the practical benefits which will flow from those remarkable machines, great as those benefits doubtless will be, but because the electronic computer will eliminate our last claim to special status in the universe." (p. 3) The details of realizing these promises that were based on the early work on AI included: a machine that will assist in its own programming (that is, a machine that learns), a machine that will be creative by evolving its programs randomly through mutations (that is, achieving creativity by genetic programing), and the improvement in hardware. Thirty six years later, and in contrast to occasional statements by CAE researchers, the forecast is far from reality. I argue that this contrast is rooted in CAE research methodology.

2 What is wrong with CAE methodology?

The answer to the question in the section title may be as simple as employing the wrong interpretation of the term CAE. Instead of studying *computers* facilities for *aiding engineers*, CAE researchers study *computer-aided engineering*, often in isolation from engineers. This isolation allows researchers to employ whatever interpretation of engineering they wish. Thus, not only could the research issues be irrelevant to engineers but the research strategy employed could easily prevent from addressing the stated issues. For example, the *divide and conquer* strategy that is often employed to decompose large issues into smaller issues, focuses on simple "manageable" problems but whose contribution to solving the original problem is questionable.

CAE researchers explain their activities by claiming that they are working to elevate their research into a science. Therefore, they would like to employ "scientific" and "objective" techniques that are believed to lead to "real" understanding and uncovering of some "truth." This is the foundation of rational thinking. It is clear to CAE researchers that this truth is subsequently operationalized by *others* to form principles for practice. Therefore, CAE researchers need not pay attention to attaining *tangible* results, that is, results evaluated in practice by CAE researchers themselves or by practitioners. CAE researchers may claim that this separation between "science" and what some scholars call "applied research" relieves them from practical testing of their work. The flaw in this argument is that, if we agree that the goal of scientific theories developed in CAE research is to advance engineering, the test of these theories in the course of research must include a demonstration of such advancement. Of course, if we disagree about this goal, the remainder of this paper is immaterial.

What few researchers may do to patch the above flaw is to replace practical testing with testings on benchmark problems. To be relevant to practice, benchmark problems must be representatives of the problems encountered in practice in all their complexity. Unfortunately but clearly, benchmark problems are never complex as practical problems and their relevance to practice is doubtful (e.g., in optimization research (Haftka and Sobieski, 1992)). Replacing practical testing with benchmark testing removes any hope for obtaining even a semi-independent yardstick for measuring research quality or practicality.

Gaining control over the practical relevance of research is the theme of this paper. Therefore, I do not discuss nor advocate for a particular CAE research topic; such topics rise and fall as fashions change. In this position paper, I advocate for an *approach* for doing CAE research that has better chances of impacting practice. In what follows I review three observations that influence the quality and practical relevance of research. The observations are not new, their seeds can be located in very early CAE research projects; however, their impact on research is often ignored. In contrast, I incorporate these observations into a hypothesis about improving CAE research. In the spirit of this workshop, I use examples from Fenves' research: the development of STRESS and the research on decision tables.

3 Observations about research: A CAE perspective

Observation 1 (Rational Research.) *Rational research stemming from a particular paradigm or model of the world can create significant "inertia" that safeguards the research paradigm from any challenge.*

This observation does not eliminate the role of rational research. Occasionally, one can identify problems that are well defined, repetitive and time consuming if done manually. Such tasks are amenable to computational support (Miller, 1963). Such computational support can be developed based on existing or new principles.

Although the development of STRESS (Fenves et al, 1964) was not based on rational analysis, a perspective distant and unfamiliar with some of the details of its development may qualify it as such.[1] STRESS was a computational support tool for structural analysis which was (and still is) a well defined cost element in design and one that engineers do not perceive as a part of the creative act, but rather as a burden. In replacing manual procedures with computational support tools, a purely economical decision could be made: the cost of the procedure replaced must exceed the cost of the new procedure including its associated cost of study and dissemination.

STRESS disseminated quickly into practice; it was challenged and served as a standard for comparison for other programs. Unfamiliar with its development, one may argue that STRESS is an evidence of the value of rational research. In contrast, STRESS succeeded probably due to the involvement in practice and the understanding of available computer technologies that could be used to develop it.

Observation 2 (Engineering Practice.) *Without submitting theories to test in challenging engineering practice, no failure can occur thus installing a sense of apparent confirmation (restating observation 1). Nevertheless, engineering practice gradually evolves and finally challenges theories in ways not predicted before. Without the immediate practical utilization of theories, it may be hard to relate failures to their proper causes.*

In contrast to STRESS that was challenged by immediate practical use, the products of another research of the 60s — decision tables for standard processing — did not experience such testing. In 1966, Fenves outlined the use of decision tables for standard processing, its potential and scope: it was suitable to long chains of logical connectives, but,

> the translation of engineering decisions into rigorous decision tables is not always a straightforward mechanical process. [...] Many of the currently used (1966) criteria are not directly amenable to a rigorous formulation, but depend to a great extent in implied information and judgment. With the increased use of computers in design tasks, engineering decision-making processes will have to be reevaluated in this light. (p. 490)

Such recent reevaluation reveals that significant effort was spent on decision tables processing over close to three decades without much practical impact (Fenves et al, 1994). In contrast to structural analysis discussed before, design standards are used by almost all participants in the design process, including some that work in synthesis. Therefore, standard processing does not represent a well defined cost element of the design process.

Over a period of 3 decades much effort was spent on auxiliary research issues, such as "what type of logic is required to represent and process standards," that emerged from the divide-and-conquer rational research strategy employed. Software tools that emerged from this research, such as SASE (Fenves et al, 1987), never sustained real practical testing due to various technical and social reasons and the failures of these tools to impact practice were not used to drive subsequent research. The

[1]The details about STRESS and the research on decision tables is based on my interpretation of discussions with Steven Fenves and of a recent paper by Fenves, Garrett, and Hakim (1994). To the effect that these sources were distorted to fit the theme of the paper, I am to be blamed.

social dimension of research seems to have played a larger role in shaping the research on decision tables than did engineering practice.

Historical reviews of science by Kuhn (1962), Feyerabend (1975), and others reveal that the role of the social processes in research is primary. Therefore, no research could be "objective." Furthermore, the influence of social processes extends beyond shaping theories to include the infrastructure of research. To illustrate, consider the processes that influenced the implementation of computer arithmetic (MacKenzie, 1993) that is taken for granted as an "objective" tool for carrying out various kinds of research. Therefore,

Observation 3 (Social Dimension.) *Engineering theories rest on significant social construction and manipulation. They do not have the objective status suggested by rational research.*

While the development of STRESS was not influenced significantly by social aspects of practice, the work on standard processing had to take the social aspect into account. Yet, in the course of this research, there were no observational studies of practitioners using standards or hardly any participation of practitioners in determining their needs for standard processing software. Therefore, it was not clear that manual practice could be replaced by software or what the nature of such software should be if it is expected to support diverse needs of multiple users.

So far I have discussed engineering practice in general, but acknowledging that the complexity of contemporary design practice increases continually, requiring the collaboration of interdisciplinary teams located in several places, the study of, or the provision of, computational support for such design processes is a significant challenge requiring prompt attention to social issues.

4 Contextualized research

The three observations can be operationalized into different research approaches, each of which constitutes a *hypothesis* about how research can be performed to yield practical impact. One hypothesis rests on the following lessons:

(1) Avoid purely rational research by embedding research in engineering practice.
(2) Use research results immediately after they are developed in as many practical situations as possible.
(3) Encourage the expression of, and legitimize different viewpoints about, all facets of research; in particular, encourage the participation of the users of the research end products. Pay attention to and study the social dimension influencing engineering practice.

It is clear that these lessons lead to a *holistic*, rather than a *divide-and-conquer* research strategy because one cannot divide a problem into smaller chunks, conduct research, test the results on small problems, and claim success. Rather, a researcher must maintain a complete working solution that can be used in the context of real design practice. A holistic strategy does not abolish the need for developing specific expertise. It merely suggests that different issues are central to the conduct of research:

(1) Contextualized research, also called *participatory research or participatory action research (PAR)*, requires the involvement of researchers from diverse disciplines since a working support system for a complete engineering problem must be maintained; and practitioners because if they are expected to use a CAE research tool in their work its usability must be monitored and improved continually with their participation. Such participation is mandatory for addressing the needs of both researchers and practitioners.

Contextualized research involves the management of research tasks and coordination of researchers activities — two missions that characterize the nature of contemporary design activities. This observation is significant because CAE researchers who devise tools for other designers are now in a position of designing, that is, designing and executing complex research projects. Therefore, many researchers might be able to use the tools that they or their peers are developing. Such usage can provide prompt feedback to tool developers and fulfill a moral obligation to use the tools researchers claim are beneficial to other designers.

The research issues that emerge from contextualized research subsume questions of ordinary research to include the questions dealing with the organization and context. Some examples of questions are:

(1) How are relationships between engineers and researchers set up to avoid organizational hurdles?
The conflict between researchers and engineers is clear: too much attention to usability and practical necessities may defer progress in researching the technical area of interest to researchers whereas an inattention to usability may preclude the effective use of tools in practice. Thus, the mechanics of participatory research projects must be studied with regard to their influence on the outcome of different projects originated in different contexts.

(2) How can information generated in a multidisciplinary research project that lead to practical impact be communicated to and shared by other projects or practices?
In rational research, results are perceived as "objective general truth," therefore, they can be transferred to other contexts. In contextualized research it is unclear what is strictly the product of the context and what is general than the specific situation and can be transferred (even with modifications) to other situations. Often it is the research process, rather than the result that is transferable.

(2) Research questions evolve to reflect an increased understanding of practical needs and technological capabilities. This understanding may lead to focussing on specialized research topics that need further study and can be facilitated through tight evolutionary development of CAE tools and their immediate test by practitioners.

Clearly, many specialized issues must be addressed when developing CAE systems including: software design, management of collaboration, history keeping, information distribution, communication, data (quantitative and textual) analysis and management, and user interfaces. Nevertheless, the details of studies must reflect particular needs that arise in the course of conducting a holistic research project.

(3) Projects teams could be assembled based on the expertise of researchers and their temporal make-up evolved reflecting the development of the issues addressed in the project.

The ramifications of this item may be difficult for researchers to accept. For example, if a research project is initiated by several researchers from different disciplines, and the research questions lead to a path that is clearly outside the scope of expertise of some of these researchers, it becomes necessary to change the research team. The management of such affairs is clearly not within the scope of present research, but important to contextualized research.

In elaborating the above issues I only presented a sample of issues related to knowledge accumulation, goodness criteria, values, ethics, training, implementation, and methodology of research.

5 A brief example

One project that follows the three lessons and struggles[2] with the research questions involved is n-dim (Levy et al, 1993). n-dim is developed by a group that includes researchers from diverse disciplines such as engineering, computer science, philosophy, and sociology.[3] n-dim is a philosophy and a theory and not just the software tool that makes some aspects of the theory persistent. One theoretical aspect of n-dim addresses the issue of building CAE tools that make practical impact through participatory research (Reich et al, 1992). As such, n-dim must be developed with users and tested in actual practice. Such testings would reveal critical problems or impediments that require further research and development. Such testings have been initiated with some of the sponsors of the project that became participants in the research by making a strong resource commitments. It is clear that the participation of different sponsors may lead to the development of different tools based on the core of n-dim and built using the facilities that n-dim provides. This variability strains the development of n-dim because it must be designed to withstand several practical testings. After several practical testings, the core of n-dim that allowed addressing the concerns of most practitioners participated in these testings could provide a partial answer to what can be transferred from one contextualized research to another.

Within the n-dim project, there are no specific research topics that are defined *a priori*. Rather, specific research issues are uncovered, formulated, and addressed in response to subjecting the tool to practice, even if presently limited to few of the group members themselves. This does not mean that there are no contributions that the project makes in specific research areas; in contrast, such contributions are easier to accomplish than the overall goal of n-dim and include developments in object-oriented programming (Levy and Dutoit, 1993) and progress towards understanding the nature of engineering modeling (Subrahmanian et al, 1993). Of course, some research topics are selected based on researchers' interest rather than being purely need-driven; nevertheless, in order to be part of research, their practical relevance to the technical issue must be demonstrated eventually.

[2] I say "struggles" because the different motivation of researchers and practitioners involved in part of this research elevates the organizational issues to a primary status.

[3] The n-dim group includes (in alphabetical order): R. F. Coyne, A. Dutoit, E. Gardner, S. N. Levy, S. L. Konda, I. A. Monarch, Y. Reich, E. Subrahmanian, M. Thomas, and A. W. Westerberg.

In relation to the last lesson, the *n*-dim research group composition encourages the incorporation of interdisciplinary perspectives. If this practice is accepted and practiced, it becomes easier to handle the evolution of research questions with respect to researchers expertise and interests.

Briefly, *n*-dim provides the following answers to research issues: (1) the issue of *knowledge accumulation* is addressed by the creation of repositories of engineering information, and the development of core mechanisms that are used for the management of these repositories; (2) the issues of *methodology, training, and implementation* are addressed by the exercise of participatory research; (3) more specifically, the issues of *values and ethics* are addressed by negotiating research questions with whoever might be influenced by a particular research project; and (4) the issue of *goodness criteria* is addressed by noticing an improvement in engineering practice as perceived by *all* project participants.

6 Temporary closure

This position develops a hypothesis that can be tested in practice: *contextualized or participatory research can lead to CAE research that improves design practice.* I believe that this hypothesis will receive significant supporting evidence in future projects that employ it.

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