In this Issue

Albin and Weinberg's 'Work Complexity in Structured Job Designs'

Professors Albin and Weinberg are culminating their studies in system structures and industrial complexity, attempting to measure 'job content' towards better job design and restructuring, better job satisfaction and higher productivity.

As a model they employ the traditional 'black box' model from finite automata theory. One underlying assumption is that, under some circumstances, a worker can be interchanged with a machine. This is of course true for an increasing number of simple and more complex tasks. Advances of robotics show that workers *can* be replaced advantageously by machines. Workers do not have to toil anymore in dangerous environments, physically demanding tasks, or boring, repetitive activities. They can be replaced by machines that are cheaper, do not take time off or call in sick, do not bargain for cost-of-living adjustments or produce defects and scrap. Industrial robots are changing the workplace through creating new types of jobs, demanding new skills, making the retraining process almost mandatory.

The new generation of Japanese robots is characterized by parallel computing, features of artificial intelligence (rudimentary problem solving and decision making) and less by the mimicking of inefficient and often clumsy humanoid work patterns (e.g., the human hand is eminently unsuited for tightening screws as it cannot turn 360°). Robots are replacing *skilled* workers and the whole notion of 'skill' will have to be redefined within a few years.

Albin and Weinberg make a distinction between *complexity* and *complication* of a job. So-called 'job enrichment' or 'job enlargement' very often implies more complicated (quantitatively, sequentially) rather than more complex (qualitatively, in parallel) tasks. Such job redesign schemes, ostensibly instituted to produce 'meaningful' or 'significant' increases in job content often produce only superficial improvement and may impose types of 'complication' which can detract from job satisfaction and reduce productivity by turning 'three boring, lousy jobs into one larger, more-boring, lousy job'.

The authors suggest substitution of worker pacing for machine pacing of production lines – through placing an intricate network of buffers and other inventories along the line so that the worker is given scope to control the intensity and pace of work over significant time intervals. More modern Japanese industrial experience dictates clearly: 'no buffers!' Buffers transform the efficient and lean 'just-in-time' systems into the 'just-in-case' of American industrial folklore. Buffers hide a system's unevenness, deficiency and overproduction of scrap. An unbuffered 'just-in-time' line increases workers' responsibility, reduces defects and scrap rate, increases quality and lowers costs, involves workers more directly and more responsibly in the overall production process. All troubles, quirks and misalignments are quickly identified and removed, they are not 'buffered over'. The authors have not directly addressed these questions of productivity, efficiency, quality and responsibility in this article.

They do however recognize the role and the need for more decision-making responsibility and larger autonomy of workers on the line. Such are the sources of satisfaction and affiliation.

A battery of machines can be run as a segment of a long line (maxi-line), or as a short line with combined tasks (midi-line), or as a small group with flexible tasks (mini-line). The authors consider the mini-line as approaching the autonomous group concept. In an autonomous group, 'autonomy' and 'decision responsibility' are institutionalized, to a large degree, as norms. Albin and Weinberg use this foursome classification as examples of progression from predominantly routine tasks to structured task combinations and adaptive decisions, providing the job content concept and practical job design task with quantitative meaning.

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Drake, Miller and Schon's 'Reflection-In-Action'

In evaluating community-level nutrition programs, the authors repeatedly discovered how difficult it is to establish, with reliability, that a positive change in nutritional status had taken place in the target population. Classical statistical experimental design, the so-called model of rigorous experiment, has obviously failed in the nutrition field.

The authors conclude that each experimental situation is *unique, unstable,* and *unpredictable*: it is virtually impossible to predefine the experimental conditions to account for a number of confounding and intervening changes.

The so-called 'contextual inquiry', recognizing the uniqueness of each experimental situation, 'on-the-spot experiment', incorporating responses to sources of indeterminacy discovered in the course of inquiry, or 'rapid information feedback', focusing on local information systems and on-line use of data for redesign, all of these are alternatives to the 'rigorous' statistical model and are summarily referred to as *Reflection-In-Action (R-I-A).*

R-I-A labels the comprehensive process by which researchers, inquirers and interveners respond to the detection of surprising outcomes by surfacing, criticizing, restructuring and testing the context-specific frames, theories and strategies which they have brought to the situation: a new model of field research which answers best the constraints of 'rigorous' experiment.

R-I-A, as a model of intervention and experiment, requires revision of the prevailing view that research and practice should be separated and separately handled. Actually they cannot be, they mutually support one another. Research-in-action, as action research in management, has been and remains a principal methodological concern of *HSM*. The context of experimental research *cannot* be considered distinct from the context of intervention. Intervention-oriented research can be carried out effectively only through actual intervention in particular communities. Practice should be carried out by practitioner-researchers and research-practitioners.

The issues of proper social experimentation and intervention become relevant and significant for other social sciences as well. The classical example of failed, context-free, long-term efforts of Margaret Mead presents both warning and motivation to change. Mead's loud recommendations for nutritional intervention (to solve the problem of World hunger, each American should eat one hamburger less) should be delegated to the pre-scientific era of social research.

But there are other problems. Individuals with too little experience in analytic endeavors tend to overemphasize assertions supported by *quantitative* evidence. Their inadequate critical level of expertise in experimental design and in the interpretation of quantitative analysis, leads them to emphasize these 'hard' aspects at the detriment of true value and originality of the idea. Funding bureaucracies, especially in social, decision and management science fields, are saturated by such oversimplifying practices, explaining perhaps the prevalent insignificance of funded research. One should not base decisions on improperly or incompletely developed 'hard' data at the expense of field wisdom.

Obviously, if a funding agency accepts R-I-A, then it cannot require complete specification of the details of an intervention in advance. Funding agencies concentrate on detailed projections of needs for supplies, materials, staff time, services rendered, telephone calls, etc., and very little on the actual, significant aspects of research. Evaluations must focus on actual (not proposed) outcome indicators and the task of attributing observed outcomes to the intervention cannot be avoided. This is of course hard and unlikely to be favored by bureaucracies.

Drake, Miller and Schon are very much aware of the challenge which R-I-A presents to funding bureaucracies and bureaucrats. Yet, they already report some evidence of useful, funded application of R-I-A in Sri Lanka.

Green, Bean, and Snavely's 'Idea management'

With the advancement of the high-technology era and the reorientation of business interests toward research and development of new ideas, the problem of managing idea flows within R&D labs is becoming pertinent again. The trio of researchers from the University of Cincinnati, Green, Bean, and Snavely, have tackled the enormous task of reviewing the voluminous literature on human information processing and distilled the management task into four basic stages of idea flow: idea generation, idea capturing, idea retention, and idea retrieval.

The authors adopt a *descriptive* model of human information processing to identify activities that may be important (or even critical) to effective idea management in formally organized R&D labs. Ideas do get generated, they are often creative and innovative, but R&D companies suffer real losses of ideas and money in the transition stage to the formal project selection.

The experience with Japanese successes should have brought the lesson home. Most of the Japanese new products and methodological innovations have been originally conceived in the U.S., then abandoned, discarded or put in the safe. The Japanese put them, vigorously and uncompromisingly, into business practice. Ideas do get generated; they are not followed through, they are not implemented, they are not applied. America is becoming a country of 'lost ideas'. There is this little demon sitting at strategic places throughout the system, and it thrives on losing the ideas.

The 'Lost idea' problem is not a problem of scientific creativity or innovation. It is the problem of ossified, short-term oriented, uninsightful, poor management. Managers do not 'notice' good ideas the way they used to, somebody sighed recently. Organizations of people know less than their individual members.

Thus it is the idea retention, the memory of it, its availability to be recalled in proper form when needed, which is more important, at least at this stage, than idea generation. It is hoped that the emerging telecommunications/computer technology hybrids will offer opportunities for novel approaches to the design and development of networks for idea storage and exchanges. Artificial intelligence efforts for semantic networks (linking) of ideas, concepts, objects, etc. are especially promising. But these are only budding areas, not yet tended by sufficient numbers of qualified researchers.

After capturing and retention of ideas in organizations, the ways of their proper, accurate retrieval become crucial. 'Examining the database' is an example of the type of activity commonly associated with the process of retrieval. One has to be able to trace the associations and linkages among and within idea groups, to reconstruct exact replicas of past ideas if needed, and to reconstruct associative sets of ideas reliably. Suppressions of ideas or their false reconstructions should be minimized; perhaps through building necessary redundancy in retrieval processes.

In summary, there is a large number of important critical issues beyond the problem of idea generation. Green, Bean, and Snavely succeed in drawing our attention to these issues and show how complicated the development of adequate decision support systems is going to be. The idea management support system is still to be built, even in its most general outlines. The authors, perhaps wisely, refrain from designing such a system at this stage. But their paper is likely to become a required reading for potential designers of such systems for industrial R&D labs.

Our OR/MS or operational sciences professions are failing by paying unjustified attention to mathematical models of queueing, inventory control and dual theories of nonlinear programming. They continue formulating problems so that they can be solved using the tools they know best. Where their writings should be filled with Decision Support Systems, Microcomputers, Productivity management, Robotics, CAD and CAM, Artificial intelligence, Communication networks, and decision graphics, they insist on restating forty-years-old models in Sobolev spaces. As the authors quote in one of their footnotes, 'Give a small boy a hammer, and he will find that everything he encounters needs pounding.'

Yuan's 'China's economic planning'

China's economic system and its development are very much in the forefront of international studies. Is China going to be able to transform itself from a poorly developed, backward country into an economically significant partner through a period of 'readjustment'? Is restructuring, consolidation and gradual improvements sufficient policy towards lasting, long-term rejuvenation of national economy? Yuan provides *HSM* readers with a thoughtful analysis based on first-hand experience and participation roles in the process of 'readjustment'.

Major problem is that the current economic system is not Chinese by origin or design, but organized according to the Stalin-era version of Soviet strictly centralized and overcentralized planning structure. Thus, decentralization and more reliance on economic forces are obvious and desirable directions of China's valiant efforts for curing the sick economy.

Too much iron and steel, neglected light industry and agriculture, sparse and shoddy consumer goods and services, prices that reflect nothing, enterprise losses and profits that do not mean anything, slow and unreliable investment decision making – these are some of the legacies of the past.

Yuan is convinced that developing computer-based planning support systems should certainly become one of the ways to improve China's economic planning. He suggests four major tasks: (1) improving economic statistics, (2) building economic models, (3) analyzing economic plans, and (4) evaluating economic policies. He then elaborates in some detail on each of these tasks. He complains, for example, of the lack of specialists in econometrics, computers, management sciences, and operations research. It is interesting to note that Chinese research in mathematical theory of fuzzy sets (!) is one of the liveliest in the world. Why?

Five-year economic plans are extremely unrealistic and inflexible, shifting or rolling planning is unheard of. Yuan goes into some detail in proposing a national economic *planning-support system* and its implementation. He remains skeptical about anything more than verbal acceptance of his and similar proposals.

It should be interesting to add that current approaches to readjustment and price reform are conceptually similar to those attempted in 1967 in Czechoslovakia and carried out during the seventies in Hungary. Preparing an economy-wide price reform is, however, hindered by a severe lack of data, inadequate input-output tables and a doubtful approach to artifical input-output prices. The ways of reducing discrepancies between domestic and world market prices are not even discussed yet.

The goals are far, the problems enormous, contradictions seemingly insurmountable. We should remember that until now *no* centrally planned economy has been able to design a system of economic management which would internally generate prices usable as a reliable basis for economic decisions. Centrally planned economies, in trading among themselves, cannot use their internal prices, but are using so-called world (understand capitalist) prices.

As J.V. Skolka recently observed: "China faces the problem, unresolved anywhere until now, of designing a system of economic management which would combine planning and market and would also produce endogenously, in daily economic life, prices which would meet both the requirements of economic theory and the practical needs of economic policy making." The question remains, for Yu-Fei Yuan and other sincere and concerned researchers and scientists: are the computers, econometric models, quantitative analyses, and similar and related tools going to be sufficient for resolving this, never-before-solved problem of inherently conflicting demands and assumptions?

Barish and Ehrenfeld's 'Estimating utilities'

This is the second and the last in Barish-Ehrenfeld two-paper series on estimating utilities through the classical approach of so-called 'revealed preferences'. In their philosophy they rely on an older and mostly exhausted concept of utility or expected utility in decision making.

The readers are invited to read the excellent article by Blair (*HSM 3* [4] [1982] 279–288) showing quite convincingly why formal utility theory of decision making does not satisfy the criteria of falsifiability, refutability and testability of a good science. For example, one claim which can never be refuted is that *whatever* decision a person made, it can be demonstrated that a utility function (of some identifiable sort) has been maximized. Another problem, which Barish and Ehrenfeld are aware of, is that utilities cannot be measured through questionnaires and artificially created gambling situations. Such artificial scenarios absolve the decision maker of responsibilities, bearing of consequences, and personal riskiness of real-life situations.

Another approach is to observe a decision maker's past decisions and assume that some utilities can be fitted to these past data and the utility function thus 'captured'. The problem with this approach is that it assumes a virtually context-free utility structure. Because human utilities are strongly dependent on given circumstances and context, 'capturing' them within one context is not transferable to another context; capturing them over a number of different contexts becomes meaningless to any of the many possible future contexts. Because human preferences are dependent on the means (alternatives of choice) currently available, they will necessarily differ and change over different sets of alternatives. Yet, this is what decision making is all about. One does not capture the utilities but the intricate, dynamic relationship between means and ends (preferences and alternatives) and their mutual co-determination.

Barish and Ehrenfeld assume a simple, additive aggregation of attribute measurements in order to arrive at the overall safety of the product or system measure. They rely on the idea that safety or risk, in some sense, can be evaluated and measured by a single number or index. These indexes are then aggregated into another single number or index. The multidimensional nature of risk and safety concepts, representable by irreducible arrays or vectors of perceptions and measurement is not currently studied.

Given these conditions and assumptions, the authors develop a model for *one* environmental condition and *one* use or purpose. Another model is the so-called aggregated model where different uses and environmental conditions are not explicitly considered but fully aggregated into a unidimensional situation.

Some numerical examples are given for *two* levels of ratings and *two* actual states of nature (or safety). Probability distributions are assumed to be known.

The authors conclude: "The model can also be used to assist in predicting actual performance using the evaluator's ratings and to study how good these predictions can be expected to be. They can, of course, be studied when more than two rating levels are used, giving rise to multivariate distributions."