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Agricultural Systems 74 (2002) 11–25

AGRICULTURAL SYSTEMS

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Locating agricultural decision support systems in the troubled past and socio-technical complexity of 'models for management'

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Accepted 18 March 2002

Abstract

Although not conspicuous in its literature, agricultural modelling and its applications have inherited much from the field of operational (operations) research. In the late 1940s, techniques for mathematically simulating processes came into agricultural science directly from industry. The decision support system (DSS) concept followed almost 40 years later. It seems that the large differences between *farm* production and its management and *industrial* production and its management account for the failure of agricultural systems scientists to be more attentive students of the experiences in this parent field. In hindsight, the penalty of this is greatest in the matter of the problematic socio-technical relationship between scientific models built to guide practice and actual practice. As a socio-technical innovation, the agricultural DSS has much more in common with DSSs in business and industry than might be expected judging by the domain knowledge content. One implication is that the crisis in the parent field concerning the 'problem of implementation' *could* have served as a cautionary tale for agriculture. Although this opportunity was missed, it is not too late to tap problem-structuring and problem-solving insights from operations research/management science to aid our thinking about our own 'problem of implementation'. This paper attempts this in constructing a framework for thinking about subsequent papers in this Special Issue.

Keywords: Decision support system; Operations research; Model; Socio-technology; History; IT adoption; Simulation

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PII: S0308-521X(02)00020-3

1. Introduction

Important as it is to reflect on the history of the agricultural decision support system (DSS) venture, there is an additional opportunity for increased understanding via the legacy of the DSS endeavour in non-agricultural domains. This opportunity arises from two realities: first, that the history of the DSS and related computerised information systems *outside* of agriculture tends to run ahead of the agricultural by about a decade, and second, that it constitutes a vastly greater collective experience and has a level of economic importance that attracts proportionally more financial and intellectual resources for problem solving. This has resulted in some momentous strides in fundamental ways of thinking about the interface between researchers and managers of diverse systems of human activity. Their timeliness and accessibility make them imperative aids for evaluation of the DSS idea and experience in agriculture. Had we been better students of our roots, earlier, we may have had fewer problems and greater success. But such knowledge, gained retrospectively, might still be useful in understanding past problems and in rethinking how agricultural models in information systems might better serve farm management.

2. Agricultural modelling's legacy

The DSS idea traces historically to the convergence of two radical innovations for management practice—one technological and the other social. The more recent and familiar innovation concerns the technology centred on the electronic computer. The *social* innovation took place much earlier but was no less radical. This was based on the notion that not only could the natural world around us be explained scientifically, but management practices to control the world could be scientifically *designed*. The modelling of human activity to analyse potential for technical efficiency predates the computer by decades and was well developed by the time of the Second World War. Fig. 1 picks up the ancestry of the 'Farming DSS' at the design of innovative wartime systems for British defence operations against the German submarines and aircraft (Lovell, 1988).

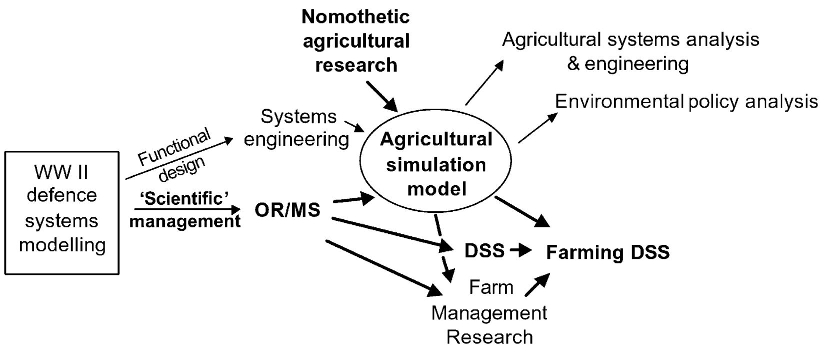

Fig. 1. A family tree of the model-based decision support system for farming.

'Operational research' was the early label for the systems approach that operationalised theory in systems of monitoring, simulation and control that were developed to provide the basis for post-war 'scientific' management. Known as 'operations research' in North America, 'OR' became a significant movement in post-war industry and business as well as in defence. This field later converged with the business management science movement and was often labelled OR/MS, the term I will use in this paper.

The earliest simulation modelling in agriculture was an offshoot of OR/MS developments in industry. One of the earliest pioneers of agricultural modelling was Professor C.T. deWit of the, then, Wageningen Agricultural University. I once asked Professor deWit how he got into the 'modeling business', and tape-recorded his response.

> In 1948/1949 when I was a student, a new professor named van Wijk came from Shell Laboratories, where he had been an expert on distillation problems—how to get the proper oil fractions out of the distillation columns. You know that's a real operations research problem to get out of them what you want to get out of them—what to do with all the fractions. He was fascinated by the application of quantitative theories, as was I. So, a lot of the origin of my fascination with how to quantify agriculture comes really from the oil industries. In agronomy, nobody could tell me how much a crop could maximally yield if you remove all constraints, and I found that a reasonable question. It cannot have infinite growth—it has to have a limit, and I thought we needed to know the limit. But this question was not even asked, let alone answered, and I was especially fascinated with it (deWit, 1994, recorded interview).

When I referred to the pioneering agricultural simulation study by C.H.M. van Bavel (1953) of supplementary irrigation needs for tobacco production in the eastern states of the US, which I felt impressively utilised an operational research approach, Professor deWit laughed, and said, "Where do you think he trained? We *both* came from van Wijk's laboratory!"

In Fig. 1, the convergence of the interest of agricultural scientists in understanding of production processes in ways that can be expressed mathematically (Fig. 1, 'Nomothetic Research'; defined in Fig. 2) and the methods of OR/MS created the new 'occupation', if not 'discipline', of agricultural modelling (Fig. 1). By the mid 1960s, agricultural simulation models had matured to a stage that made them attractive to agricultural economists in the field of Farm Management Research (Fig. 1) to enhance methods in management 'decision analysis'. In introducing to an agricultural research community a selection of attempts to apply simulation models to management, Dent and Anderson (1971 acknowledged (albeit weakly) a debt to OR/MS beyond the development of models, i.e. their use for aiding planning and decision making.

> This book indicates how the latest management methods (originally developed by military strategists and industrial concerns) can be profitably applied to problems of resource allocation in agriculture (Dent and Anderson, 1971, front flyleaf).

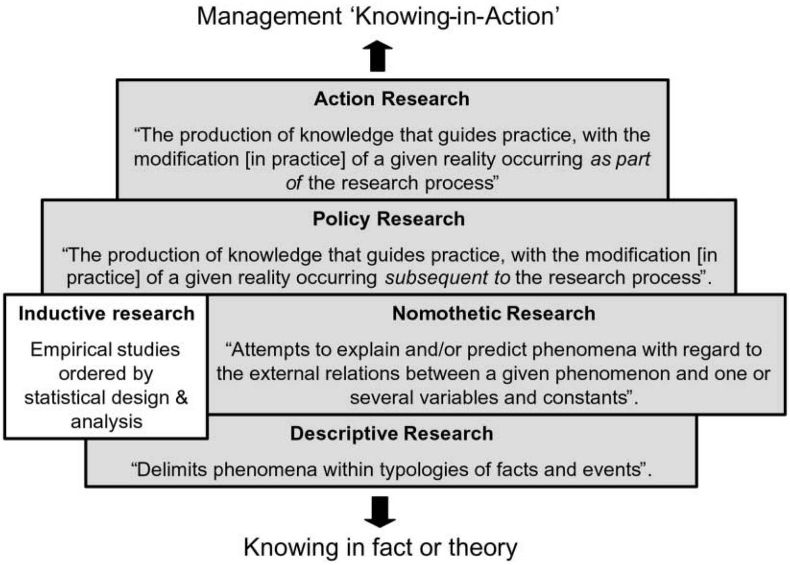

Fig. 2. The relationships (after Oquist, 1978) between different types of research in terms of their contribution to the two kinds of knowledge distinguished by Ryle (1949).

For the agricultural economists, Dent and Anderson, dynamic simulation represented a promising innovation in a long tradition of using static production functions in economic models in Farm Management Research, a field that occasionally acknowledged its own earlier OR/MS methodological legacy (e.g. Hutton, 1965; Agrawal and Heady, 1972).

Although Fig. 1 indicates that the agricultural simulation model stemmed from OR/MS, this is not conspicuous in the early literature concerning models in agricultural science. While Spedding (1980) acknowledged that "There is no great difficulty in stretching the term [operational research] and its definitions to fit application to agriculture"...his preferred term, 'systems approach', has prevailed in agriculture. Spedding argued that "...this term...can embrace any of the meanings attached to operational research but...puts emphasis on the approach itself rather than on its area of application." The development of a field of agricultural systems research concerned with management, largely in isolation from the mainstream OR/MS movement, had a serious downside that has become apparent only with the passing of time. The under-recognition of the *similarities* of agricultural systems research to OR/MS has resulted in agricultural researchers overlooking the earlier and richer OR/MS experience as a way to reduce mistakes and inefficiency in our research.

3. A further legacy: difficulties in using models for guiding management

According to Rosenhead (1989), the OR/MS tradition is identifiable as far back as the 1930s, and

> ...operational research [became] an example, perhaps the exemplar, of 'rational comprehensive planning'. This is a prescription for planning, policy formation or decision-making consisting of five stages:
>
> 1. Identify objectives, with weights.
> 2. Identify alternative courses of action.
> 3. Predict consequence of actions in terms of objectives.
> 4. Evaluate the consequences on a common scale of value.
> 5. Select the alternative whose net benefit is highest. (Rosenhead, 1989, p. 3)

The relationship between OR/MS and the scientific research enterprise is shown by Fig. 2. Operational research sits within *policy research*, as defined by Oquist (1978)—"The production of knowledge that guides practice..." Oquist claimed for those elements shaded in Fig. 2 a relationship in which "each successive type [higher in Fig. 2] is based upon and assumes the prior types [lower in Fig. 2]". At the 'foundation', *descriptive* and *nomothetic* research contributes to making the complex natural world and the objective social world of economics intelligible. Policy research uses descriptive, nomothetic, or statistical models of the material system and external environment in designing *optimal* action, given a management *policy* (summarised as a goal or objective). Such research on *efficient means to implement management polices* enables the 'rational comprehensive planning' process of OR/MS described by Rosenhead (earlier). It overlays much of Spedding's 'systems approach', and constitutes much of the so-called 'hard' systems *genre* to which Checkland (1978) attributed the power to deal with 'the *logic* of situations' in management.

But Checkland's main point was that being able to deal *only* with the 'logic of the situation' was the root cause of the problem of "the divergence between textbook OR/MS and what practitioners actually do" (Checkland, 1983). This 'gap' between theory and practice symbolised the chronic failure of information systems produced for managers to make a difference in management practice (Howard, 1963; McArthur, 1980; Longworth and Menz, 1980). The point that is crucial to the objective of this paper is that not only does the model-based agricultural DSS have its roots in early OR/MS but our present problem of *non-adoption* by farmers of available model-based DSSs has an historical precedent in this earlier field as well. Knowledge of the diagnosis and response to the earlier problem in OR/MS of non-use of scientific models in management practice may be important to our response to our current crisis.

Although clear in hindsight to later commentators (e.g. Lilien, 1987; Rosenhead, 1989), at the time, there was no unanimity that the 'gap' between science and management qualified as a 'crisis'. As Checkland saw it,

> One man's 'crisis' is another's 'business as usual', hence I would not wish to waste effort in discussing whether or not the situation in O.R. constitutes a 'crisis', a 'paradigm shift', a 'revolution', a 'debate', a 'schism', or 'unruffled normality'. All these labels would no doubt have their defenders. What would

> probably be agreed is that for some years, especially in Great Britain and the USA, there has been a marked readiness to discuss the nature of O.R. and how it is changing or how it should change. [ ] . . .operational researchers. . .have been asking themselves what it is they are doing, what is the scope of O.R., its present limits and its future (Checkland, 1983, p. 662).

Checkland then referred to sobering findings from a comprehensive review of papers in the Journal of the Operational Research Society, summarised by the authors as follows:

> . . .it seems clear that in the 15 years from 1963 to 1978, the OR community has shifted from a widespread feeling of certainty about its role and optimism about the future to a state in which significant sections are experiencing and expressing considerable uncertainty and pessimism. Furthermore, there seems to have been an increasing divergence of views expressed not only about OR itself, but about the nature of science and society in general. Sometimes the protagonists appear to see the world in quite different ways (Dando and Bennett, 1981).

The problem of the 'gap' did indeed become a crisis to a profession whose epistemology of practice was the scientific design of optimal action. But this happened over two decades of effort to understand and solve what became formalised as 'the implementation problem' (Huysmans, 1970; Shultz and Slevin, 1975).

In 1960, Russell Ackoff, the leader of the OR/MS group at Case Institute of Technology in the US published a paper with the title "Unsuccessful case studies and why".

> Since there are relatively few operations-research groups that have performed a relatively large number of OR studies, it struck me that it might be helpful if one from such a group would review the experience of that group in breadth. The OR Group at Case has been involved in 48 projects since its foundation in 1951, all but a few in industry.

Ackoff reported that of the numerous disappointing outcomes of attempts to introduce scientific models into practice, he could find no failures that he felt were *technical*. Rather, failures were *practical* and resulted in "partial or complete failure to implement [research] findings." Although he provides a synthesis of four reasons in terms of the management setting and practice "why results of OR studies have not been put into operations," his five recommendations are for changed *research* practice. His final paragraph touches on the root issue that twenty years later was seen as integral to the 'crisis' in the profession.

> We must continually remind ourselves that the ultimate objective of operations research is the improvement of operations [i.e. practice]. This improvement cannot come without *implementation* of findings. Unless the researcher is involved in and concerned with implementation we shall succeed only in amassing technical

> successes and practical failures. We must avoid operations of which can be said that the surgery was successful but the patient died. The surgeon cannot survive many such deaths (Ackoff, 1960, p. 263. My interjection and emphasis).

OR/MS featured researchers and consultants using models to generate plans and recommendations *for* managers in their practice. Some years later, Ackoff (1979) articulated the implementation problem as one due to ignoring that "the principal benefit of planning comes from *engaging in it*. In planning, *process* is the most important product. [ ]...no one can plan effectively for someone else; *it is better to plan for oneself, no matter how badly, than to be planned for by others, no matter how well*." Increasingly the focus of concern in the intervention in management practice using models shifted to the social process of implementation. The challenge was to overcome the dual deficiencies of (1) lack of 'involvement' and 'concern' of researchers beyond the technical and (2) the marginalisation of clients in model-aided planning conducted for them.

4. The DSS as a solution to the 'problem of implementation'

Models in OR/MS were used by specialist systems analysts/planners to provide guidance to decision makers. In a paper often cited as the first that advanced the DSS idea, Little (1970) proposed circumventing the human intermediary by developing an interface for the manager.

> The big problem with management science models is that managers *practically never use them*. There have been a few applications of course, but the practice is a pallid picture of the promise. Much of the difficulty lies in implementation, and an especially critical aspect of this *is the meeting of the manager and the model*. I believe that communication across this interface today is almost nil and that the situation stands as a major impediment to successful use of models by managers. [ ] A model of his operation can assist him but probably will not unless it meets certain requirements. A model *that is to be used by a manager* should be simple, robust, easy to control, adaptive, as complete as possible, and easy to communicate with (Little, 1970, p. B-466, my emphasis).

By removing the scientist intermediary and giving the manager direct access to the computer technology, Little's idea simplified the social structure of OR/MS. It is important that we note that this historical proposal was put forward as a *solution* to the 'problem of implementation' of models in OR/MS. Yet our reason for probing this history is to make sense of a 'problem of implementation' of the DSS *itself*. To add to the irony, one of the promising findings reported in this Special Issue (Carberry et al., 2002), is that success of model-based decision support can *improve* when operation of the model is shifted from the farmer to a specialist intermediary, the very arrangement Little aimed to escape. It looks like we may be going in circles. My ultimate challenge in this paper is to dispel this conundrum by looking at decision support from a different perspective. It takes no more than the right change in perspective to make 'going in circles' a spiral progression.

Compared to our present crisis in agricultural DSS, the magnitude of the crisis 20–30 years ago in OR/MS was enormous, not because there was a more serious gap between the scientific approach to management and actual management, but because of the greater size of the OR/MS venture. In spite of this prominence and its occurrence prior to the start of the DSS era, there is little evidence that our profession was aware of this failure of model-based decision support and the possible relevance to prospects for our own efforts. But it may be that it is only the experience of our own disappointments that now produces the incentives to be 'retrospective' and to use the earlier events to help make sense of our own experiences and to invent more successful ways to bridge the gap between management science and management practice in agriculture.

As the case stories in this Special Issue convey, DSS development work in the 1980s and early 1990s was an exciting adventure. Optimism of modellers seemed to grow in proportion to advances in personal computing technology. The earlier diffusion of the simulation model from OR/MS into agriculture had created a new research occupation in agriculture that served to add 'prediction' value to the 'explanation' value of nomothetic research (Fig. 2) in various agricultural disciplines and also served to integrate nomothetic research from different disciplines. In the 1970s, modelling had become accepted in agricultural research as having an 'occupational role', i.e. doing work with value and sufficiently standardised so as to be recognisable by other members of the community (Moore, 1970, p 52). In Moore's classic book on the nature of 'professionalism', he further distinguishes a professional (from non-professional) occupation in terms of service provision to a clientele '*utilising systematically accumulated, generally applicable, knowledge*'. The transition of agricultural modelling from applied research to provision of DSS can be seen as a move from a research role into a *professional* role that uses scientific models with farmer clients. This notion is obviously potentially problematic for those rewarded for *research* achievements. But any similarity of this to 'extension' was minimised by the nature of the then prevailing view of the *professional* stance, which tended to minimise conflicts between research and service provision roles.

> [Contrary to the idea of] dispensing a series of unique services in treating a series of unique problems...what we are suggesting...is that there are sufficient uniformities in problems and in devices for solving them to qualify the solvers of problems as professional. [ ] [The]...essential point...is that professionals apply very general principles, *standardized* knowledge, to concrete problems requiring solution or palliative measures. Yet it remains true that the client's problems and the professional's knowledge may have a rather small overlap (Moore, 1970, p. 54, 55, original emphasis).

The optimism surrounding the notion that the DSS offered an opportunity to make the scientifically powerful simulation model useful to farm management existed in an intellectual, and even, popular, atmosphere of expectation that computers, and especially personal computers, were about to revolutionise *practice* in diverse fields of management in modern societies. Adding to the seeming inevitability of this technology's social determinism at the time was the primacy of the 'information processing' theory of human cognition.

Although the DSS movement had pragmatic origins in OR and management science, a few DSS workers acknowledged the strong philosophical influence of "the Carnegie School" (e.g. Keen, 1987; Stabell, 1987). This influence stemmed from a confluence of remarkable innovations by a group at the then Carnegie Institute of Technology in Pittsburgh, led by Herbert Simon and Allen Newell. They argued that a key aspect of the 'gap' between theory and practice was that, as humans, managers are incapable of mentally *optimising* practice, since optimising assumes perfect knowledge of the states and relationships in the environment. In real life, management behaviour amounts to a search for outcomes that are *satisfactory* rather than theoretically *best*. Although inevitably failing to satisfy the criteria for *substantive* rationality based on what is theoretically 'true' in the biophysical world, this approach results in behaviour that can be shown to be *procedurally* rational (Simon, 1979). This so-called 'behavioural' approach utilises research to understand the mental processes of decision-makers managers in order to build computer programs that can mimic and improve on these internal *heuristic* processes. These workers linked this approach to the radical notion that human mental process can be considered analogous to symbolic information processing of digital computers—an idea that revolutionised cognitive psychology and gave rise to the field of cognitive science and artificial intelligence and to their derivatives, expert systems and knowledge-based systems.

A message implicit in this 'cognitive revolution' that did not go unnoticed by DSS developers was that modern management (including that of farms) was becoming so complex that without computerised decision support systems future managers will be unable to cope. This interpretation of the workplace was reinforced by burgeoning and widely publicised studies in cognitive psychology that placed emphasis on human cognitive limitations—the manager-as-cognitive-cripple thesis (Stabell, 1987)—i.e. having severe biases in decision making due to multiple perceptual 'insensitivities', 'misconceptions', and 'illusions' (Tversky and Kahneman, 1974).

The pure DSS *idea* is elegant—easy-to-use software on a computer readily accessible to a manager to provide interactive assistance in the manager's decision process. There existed a plausible theory for both the need and mechanism for decision support. But review of 30 years of DSS R&D shows that the reality has been more chaotic than elegant, with one of the continuing challenges on the theoretical front to identify a commonly agreed *definition of* 'Decision Support System' (e.g. Sprague, 1980). One of those who was there from the early beginnings, Peter Keen, explained

> ...that the intuitive validity of the mission of DSS attracted individuals from a wide range of backgrounds who saw it as a way of extending the practical application of tools, methods and objectives they believed in (Keen, 1987, p. 255).

Keen (1987) went on to suggest that the diversity of definitions of the 'Decision Support System' could be logically ordered on a spectrum that at one end emphasises the nature of the 'Decisions' and method of 'Support' and on the other end emphasises 'System' technology. This construct makes explicit the *socio-technological* nature of the DSS.

Nearly all DSS research in agriculture can be found at the 'system' *technology* end of Keen's spectrum. With few exceptions, agricultural modellers indeed have been little interested in *theory* of decision support, but, together with those in the broad business systems field, saw the DSS as 'a way of extending the practical application of tools, methods and objectives they believed in', as per Keen (1987), earlier. Although entry at this 'end' is legitimate and non-problematic when a DSS proves to be *useful and used*, deficiencies show up when there are disappointments and a subsequent need to understand them. Attempts to critically reflect on individual experience in order to make sense of it, such as the six case stories in this issue, seem rare, or at least are rarely made public. This is undoubtedly attributable to lack of rewards for 'retrospective' investment of scarce research attention. But even when it is attempted, it is especially challenging. One challenge is to understand the consequences of the *assumptions* made in technical 'System' development about the social 'Decisions' and 'Support' processes at the other end of the spectrum. More often than not this requires conscious effort in identifying what these assumptions in fact *were*. This is the starting point for understanding past problems and for more effective use of information systems for intervention in management.

Keen's 'spectrum' is greatly enhanced by overlaying the typology of Stabell (1987) of several 'schools' of DSS activity (Fig. 3).

Stabell distinguished four alternative perspectives on management concerning "the role of computer-based systems as a management tool and how to realise this vision in practice": decision analysis (DA), decision calculus (DC), decision research (DR, but denoted in Fig. 3 as Expert Systems, the most prominent form of the DR 'school' in agriculture), and implementation process (IP). But he acknowledged that most DSS activity lay outside all of these.

> ...it is apparent that the 'builders' of DSS *technology* might be considered the largest school in terms of number of actors involved and committed resources. For this school,...the road to better decisions is through better technological solutions. [ ] The point is not that technology is of no importance. Rather, it is an argument for keeping our attention on the central theme: *Better decisions*

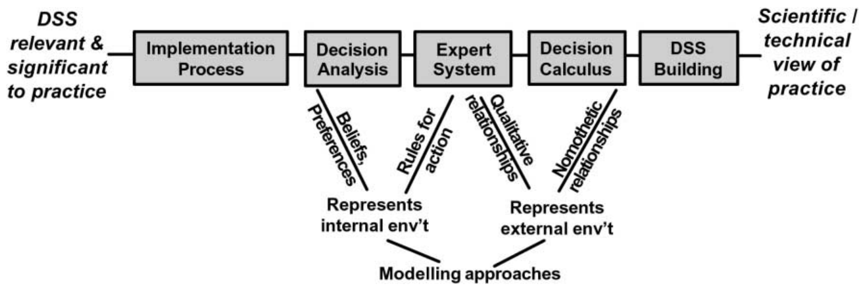

Fig. 3. Schools of DSS thinking (Stabell, 1987) on the social-technical spectrum of Keen (1987).

> *and decision support*. A viable DSS-school should provide a perspective on how we might apply any technology, existing and future, as a means to achieve these ends (Stabell, 1987, p. 250, original emphasis).

The supporting 'tree' of Fig. 3 explicates an aspect of Stabell's classes that enables meaningful positioning on Keen's technical-social/practical spectrum. Decision making, as a personal behaviour (B), is generally conceptualised as a process influenced by an *external* environment (E) and personal attributes of the decision maker (P)-an *internal* environment. Kurt Lewin, a pioneer of industrial psychology, formalised this as

$B = f(P, E)$ (Vroom, 1960).

Models in OR/MS, agricultural simulation models, and DSSs based on the latter all relate to decisions essentially by contributing to answering "what if I decided to do...?" (B). The models are abstractions of scientific knowledge concerning the external biophysical environment (implicit in E). This is Stabell's Decision Calculus school, and of the three modelling approaches imbedded in Fig. 3, it contains the simplest treatment of the *internal* decision environment (P). Decisions are seen to have technical consequences determined by the external environment and are given a *practical* orientation only insofar as a practical *goal*, for example, profit maximisation, and a rational attitude of the manager are assumed. Knowledge of the external environment (E) is made relevant to given decisions via inferences about decision *consequences*, given parameters and initial conditions of the external environment. Stabell notes that the reference discipline of the decision calculus approach to DSS development is OR/MS; the title of the paper by the OR researcher cited earlier (Little, 1970) that marked the origin of the DSS era was "Models and managers: the concept of a decision calculus".

In contrast to the decision calculus, the most 'social' of the three modelling approaches, is DA (Fig. 3). Here, the focus is on the 'internal' decision environment of a particular decision-maker. On the one hand, DA requires as inputs 'internal' information in the form of the decision maker's personal strength of beliefs about relevant aspects of the external world and his/her preferences for various risky prospects (Anderson et al., 1977). On the other hand, DA utilises 'external' statistical decision theory. This theory is largely 'analytic a priori' (Halter and Jack, 1961) and rests on transparent mathematical logic rather than empirical relationships, as in Decision Calculus.

The Expert System lies between the other two in the technical-social/practical spectrum in Fig. 3. As discussed earlier, this 'behavioural' DR school acknowledged internal drivers of behaviour but focused on cognitive impediments, for example, ignorance as to what the external environment *allowed* as a potential for action, limited information processing power, biases, etc. DR features capturing the ways successful managers cope with matching the goal, or task, with the realities of the external environment by either modifying the action or modifying the goal. These indigenous action rules, or *heuristics,* the products of trial and error learning and shared within the decision-maker's 'community of practice', were seen to provide the grounds for computer models of decision making. In time, this line of research pragmatically led to Expert Systems and Knowledge-Based Systems, in which the 'model' became the structured knowledge elicited from the 'internal' environment of an expert. Expert Systems began to be developed in agriculture following the release in the 1980s of the expert system software 'shell', a by-product of pioneering work in expert system software for medical diagnosis.

5. The DSS for farmers

Decision support systems to aid farmers ('Farming DSS' in Fig. 1) had multiple origins as offshoots of the main DSS movement. Instances of all three of Stabell's DSS model types can be found in agriculture. Decision analysis was the first to arrive (Fig. 1), introduced from OR/MS by agricultural economists in Farm Management Research along with other OR mathematical tools (Agrawal and Heady, 1972). Agricultural economists were also the leaders in the early application of the agricultural simulation model as a decision calculus to farm management decision making (Dent and Anderson, 1971). That the Farm Management economists were first, is unsurprising, since unlike agricultural modellers at the time, they were already in the business of attempting to use complex formal models to aid farm planning and decision making.

The preponderance of effort in agricultural DSS has been in 'Building' (Fig. 3), and building initiatives have generally represented an application of an existing simulation model to calculate the consequences of a decision, i.e. a decision calculus. The starting point for the agricultural modeller has been at the end of the spectrum that utilised his scientific knowledge, flexibly imbedded in simulation models of the *external environment* (Fig. 3, right). Simulation, and the derived decision calculus, unlike the other two modelling approaches in Fig. 3, do not formally 'solve problems', i.e. mathematically optimise. The underlying philosophy seems fairly put by Plant and Stone (1991):

> ...simulations of reality...may be very informative and useful in making a decision. [ ] [Although] providing information doesn't solve problems,...it can make a problem's solution trivial (Plant and Stone, 1991, p. 11).

DSS technology adapts these tools and/or their output to provide guides for superior technical practice. Several different strategies for making complex scientific simulation models useable for non-scientists have been adopted, and three are demonstrated in the decision support cases that follow in this Issue.

The use of decision analysis (Fig. 3) in agriculture occurred largely in Farm Management Research prior to the DSS era and will not be considered further in this paper.

Decision research is grounded in the notion that managers are incapable of fulfilling the 'rational manager' assumption made when designing 'best' practices based on knowledge of the 'external' environment. The cognitive scientist, Herbert Simon, won the Nobel Prize for economics in 1978 for introduction of the notion of ‘bounded rationality’ as a constraint in the ‘internal’ environment. Because of bounded rationality, in pursuing their goals (part of the internal environment), managers can only *satisfice*. But procedures for achieving ‘satisfactory’ outcomes that sometimes approach theoretical optima can be learned by experience. This gave rise to engineering of Expert and Knowledge-Based Systems aimed at extracting and utilising *heuristics*, or ‘internal’ rules emerging from expert practice, to imbed in computer models to aid the practice of others (Fig. 3).

In relating Fig. 3 to Fig. 2, whereas the decision calculus school tended to behave as though effective policy research and intervention in farming practice would follow as long as nomothetic models were good enough, the expert system ‘school’ displayed a much more elaborate conceptual framework. This framework encompassed a rationale based in the nature of management behaviour, heuristic models of behaviour, and qualitative alternatives for environmental models where nomothetic models did not exist.

Plant and Stone (1991) argued that this technology was needed because:

- Farm management is becoming much more intensive and demanding in terms of time and expertise. External experts are increasingly needed, but “the shortage of experts in agriculture and the seriousness of the problems makes the development of some form of computer-assisted management tool imperative” (p. 8).
- Farmers manage farms holistically. Expert Systems can be more comprehensive because they don’t need to side-step aspects of farm management where our knowledge is too ‘incomplete and uncertain’ for constructing simulation models (p. 2).
- Expert Systems can support a decision maker by mimicking an expert person’s reasoning, knowledge and experience, including the use of information systems and models, to solve complex problems. Provision of support can be seen as provision of heuristics for farmers for new situations.

> . . .successful farmers will manage from a systems perspective, but in the face of all the complexity and uncertainty implied in farming, it is clearly unreasonable, if not impossible, for a farmer to acquire and apply all this knowledge each time a pest management decision is made. Instead, farmers develop heuristics, rules of thumb, that help them make difficult decisions. These rules of thumb take the place of a systems-level understanding of all the complex interactions affecting crops and livestock. Over time, heuristics are tested using a kind of natural selection.[ ]. . .as agriculture changes and new technologies emerge, heuristics must change as well. It is not practical to require farmers to learn purely from experience. Research must provide not only new technology but also new heuristics (Plant and Stone, 1991, p. 9).

Advocates of Expert Systems in agriculture generally recognised the established place of existing simulation models and their adaptation in decision calculi for DSS but called attention to their limitations, especially in the light of the new theory concerning the 'internal' environment of decision making. According to Plant and Stone (1991), agricultural simulation models as a sole basis of decision support are deficient in three ways that are overcome by either substituting or integrating them with an Expert System. First, they can only deal with aspects of the farm for which models exist, and then often inadequately due to model constraints. Second, they can merely provide certain types of information for a manager to mentally process; the analysis does not 'solve' a problem by outputting an *optimal* action. Third, the consequence of this failure to internally reduce information to that relevant to superior action results in a tendency 'to overwhelm their users with complexity and information'.

It is ironical that 'Expert Systems', the school with the best-developed theory, experienced the greatest reversal of prospects of any of Stabell's DSS 'schools of thought' as a result of the agricultural DSS learning experience in combination with a paradigm shift in theory (McCown, 2002).

The element of Fig. 3, Stabell's schools of DSS, that remains to be discussed is Implementation Process. The aim of IP is to understand and overcome the so-called 'problem of implementation'. The central concern is with system use and usefulness. As Stabell (1987) indicates, this school has several variants in the main DSS movement, all concerned with *process* for DSS development that is of a 'participatory' nature.

> A central notion is that implementation is not an issue to be dealt with *after* a system has been designed. Successful implementation requires that it be considered right from the start of the system development process. Guidelines for successful implementation suggest that it is important to start simple, get started quickly, and gradually improve and extend the system as experience is gained through the interactions between user, system and builder. (Stabell, 1987, p. 247).

System Implementation research adds a new dimension to the 'social' aspect of DSS development. Differences with respect to 'social' between the three analytic approaches (decision calculi, expert systems, and decision analysis) represent differences in formal treatment of the *internal* decision environment, or personal factors (P). The IP 'school' (Fig. 3) shifts the focus of the treatment of the social dimensions of the DSS from the way the manager's internal environment is *modelled* to the relationships between managers and scientist/DSS developers—the latter a part of the *external* social environment. This change in focus and its implications for understanding problems and future prospects of the DSS is a major focus in the concluding paper in this Special Issue (McCown, 2002).

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