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Learning to bridge the gap between science-based decision support and the practice of farming: Evolution in paradigms of model-based research and intervention from design to dialogue*

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Abstract. Application of science in agriculture has been primarily to the ongoing improvement of material technologies. But there has also been an expectation that science, in a 'systems' mode, could, and should, contribute to improved planning and decision making by farmers. For over 30 years computerised models of farm economies or production systems have claimed the ability to identify superior alternatives for management action. Over this period, model competence has improved immensely, and farm computer ownership has grown to high levels, but this has not generated conspicuous or sustained enthusiasm among farmers or their advisers.

This paper examines the experience of model-based interventions in farming practice in search of insights to both past failure and future possibilities for models with seemingly impressive capability to be relevant and significant to managers in the challenging task of achieving sustainable farming. The strategy is to 'stand back' far enough to see conceptual and historical 'connections' between research and farming from a vantage point where the difference between a systems view and a philosophical view becomes indistinct. An adaptation of Karl Popper's 'three worlds' model serves as a 'map' of the differences among types of knowledge and among three paradigms for scientific intervention in practice. This aids explanation of the 'gaps' between research and practice when: (*1*) researchers design 'best practice' for practitioners using theoretical models, (*2*) researchers provide practitioners with practice-guiding tools, and (*3*) researchers with theory and models collaborate with practitioners to research 'best practice' in the context of practice. The benefits and challenges of an approach for bridging 'the gap' which uses multiple rationalities and research paradigms are discussed.

Additional keywords: systems, DSS, action research.

Introduction

The fact that agricultural science has been, and continues to be, instrumental in changing farming tends to disguise the 'gap' of the title of this paper. Efficacy of scientific knowledge in changing farming has been overwhelmingly through design of *material technologies* whose use enables efficiencies in farming practices. The title refers instead to the history of our profession's efforts to apply production, economic, and ecological theory or *principles* to farm management. Since the 1960s, this has become the 'systems' theme in agricultural research, using a methodology that relies heavily on complex simulation models with which to *design* superior management practices. This formed the basis for an intervention paradigm that provides recommendations to farmers as to what they 'should' do in order to obtain desirable outcomes. In the 1980s the Decision Support System and the Expert System, which had diffused to agriculture from the field of Management Science, entailed provision of tools for guiding practice as software on the personal computer of the farmer or his adviser. Yet, in spite of over 20 years of effort, such designed decision support has not significantly changed farm management practice. This failure of such an attractive vehicle for delivering scientific knowledge about management to practitioners has occurred at a time when the future of important agricultural systems appears to depend on major changes in the way farming is conducted. The magnitude and importance of 'the gap' has created a dilemma in which the future of a scientific 'systems' theme in agricultural research lies in the balance. This paper addresses this dilemma by looking closely at both sides of the gap: (*a*) the fundamental notion of external support for farmers' decision-making, and (*b*) the nature of 'management' of a family farm.

The 'gap' of the title is a specific case of the gap between scientific theory and real-world practice. Since publicly funded agricultural science was instituted well over a century ago, there have been complaints by practitioners about esoteric research, and researchers have bemoaned the reluctance of practitioners to embrace new 'theoretical

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practices'. But any notion that this gap can eventually be eliminated by better performance on either or both sides misjudges the nature of the difference between good applied research *about* practice and good situated *practice*: the former concerns rational *knowledge* about the world and the latter rational creative *action* either to prevent or to instigate change to the world (Jantsch 1972, p. 9). Existence of 'the gap', in part, reflects the difference between 'knowing that' and 'knowing how' (Ryle 1949, p. 28). But when stakeholders invest in research on better control practices and better designs for change, and when research organisations are accountable for delivering these benefits, there is a high premium for knowing how to bridge this difference. In today's research climate there is ever increasing reason to understand why past attempts to bridge the gap may have failed, and how more effective bridges can be constructed.

As one who was especially concerned with the production of rational knowledge, the philosopher of science Karl Popper acknowledged the unenviable predicament of the 'man of practical action', relative to the scientist, with regard to knowledge gaps. For most scientists, the consequence of an inadequate state of knowledge is mainly the provision of grounds for more research. The consequence for the practitioner of such a state of uncertainty is more weighty: 'For a man of practical action has always to choose between some more or less definite alternatives, since even inaction is a kind of action'; and 'every action presupposes a set of expectations; that is of theories about the world' (Popper 1972, p. 21).

'Theories of action' (Argyris and Schon 1974) inevitably emerge and evolve within communities of farming practice. But in modern agriculture, theories based on scientific research (the sort that most interested Popper) compete with these for the attention of practitioners. This paper about 'gaps' and 'bridges' concerns this rivalry. A major aim is to find a way to answer two questions that Popper (1972) raised following his observations quoted above: 'Which theory shall the man of action choose? Is there such a thing as a rational choice?' (p. 21).

In agriculture, it has long been held that that farmers need an intermediary between applied scientific research about management and farmers' choices and changes. This has been the rationale for most agricultural extension services. Argyris and Schon (1974, p. xviii) articulate this stance: 'There is a profound gap … between applicable and actionable knowledge. The former tells you what is relevant: the latter tells you how to implement it in the world of everyday practice'. *Intervention* in practice is about making applied science knowledge *actionable*.

The tradition in agricultural research by which scientific *intervention* in farming practice is facilitated, almost exclusively by the profession of agricultural extension, has meant that scientists themselves are rarely expected to act as interventionists. But a notable exception to this tradition has been the recent intervention in farming practice using computerised decision aids. This departure from traditional extension mechanisms seems to have begun with the entry to agricultural research in the 1960s of a 'systems' philosophy, which stimulated discipline-based scientists to think more holistically. With the coincidence in agricultural science of 'systems' thinking and computer modelling in various disciplines, there was a tendency for mathematical models, initially produced to simulate physical and physiological processes, to expand to simulate whole plants or animals, fields and herds/flocks, outcomes in response to actions.

By the late 1970s, scientific process models were being used for intervention in farming practice (e.g. Bennett *et al*. 1975; Peacock 1980). By the mid-1980s, constraints to computer-aided intervention practice had been relieved by the emergence of personal computers, expert systems shells, and a plethora of computer programs designed to make applicable scientific knowledge actionable in a wide range of farming practices. By about 1990 there were decision support systems (DSS) judged to be 'commercially viable' or 'ready for commercial release' (Jones 1989). By the late1990s a pervasive state of disillusionment with the DSS as a vehicle for intervention in farming practice prevailed.

It is problematic to refer to a phenomenon of *failure* of DSSs because it is difficult to point to unequivocal evidence. DSS research and intervention efforts have rarely included formal evaluation programs that could provide knowledge of trends in (*a*) number of users of a product, (*b*) mode and degree of use, and (*c*) benefits to users. Without such an investment, only occasionally does such knowledge get beyond the status of rumour. In the absence of such awareness, papers continue to be written whose main thrust is largely for renewed effort in providing more of the same (e.g. Barrett and Nearing 1998; Hodges *et al.* 1998; Hofman and Salomez 2000). The primary recommendation for change is to target advisers rather than farmers in the future, a shift which seems to stem more from lack of success in influencing farmers than from 'market pull' from advisers.

But there is increased overt recognition of the reality of failure of DSSs to influence farm management. The commercial successes forecast over a decade ago by Jones (1989) are still not in sight, and public funding for R&D has dried up. No longer can low usage of such software by farmers be excused by low computer ownership. Recent reports indicated that many more farmers are using computers, but their use of decision support or expert systems remains low (Hoag *et al*. 1999; Parker 1999).

The logical structure of my approach in this paper to elucidate the nature and causes of 'the gap' can be summarised as follows. The system under analysis consists principally of (*a*) research on farm management that utilises models of important aspects of farming, and (*b*) actual management of a family farm. Computer models are logically central because appropriate models represent scientific understanding in ways that make application to specific problems, times, and places potentially feasible. The model-based management intervention is expected to substantially benefit farming practice, and failure to meet such expectation is 'the gap'. The key system variable is the approach by which a beneficial intervention using a model has been or might be attempted. A range of different approaches that have been attempted are reviewed and analysed. The learning that this provides stems largely from the historical succession of attempts in which failure of an approach gave rise to an improved approach which in turn failed, but from which further important learning occurred. Disappointingly low impact on farming practice is offset by the progression in ideas that constitute growth in understanding of the enterprise of 'decision support.' Analysis of concepts and practice of historical intervention efforts reveals three successive paradigms for theory–practice articulation across the gap and a trend that may point the direction for a more promising model-assisted intervention approach in the future.

I am concerned that this analysis call attention to two potential errors in devising research policy during the present period in which the 'agricultural scientific enterprise [is] a system in transition' (Busch and Lacy 1986). Central to this transition in Australia is adaptation to: (*a*) the shift of public and institutional attention from problems of production to problems of ecological sustainability, and (*b*) the drastic reduction of public extension infrastructure as the interface between the community of research practice and communities of farming practice. One error in judgment would be the uncritical extension of the notion of designed decision support from the domain of production efficiency, where a commercial interest of farmers might reasonably be expected (but has not eventuated), to a domain featuring sustainable resource management within which management incentives are, as yet, weak. In spite of the dismal record of DSS intervention in the 'easy' domain, there is worrying evidence in the 'difficult' one of the irrepressible optimism for information technology held by developers and other 'outsiders' that Dreyfus (1994) contended was endemic to this field (e.g. Barrett and Nearing 1998; MODSS, 1998; Vaile 1999; Hofman and Salomez 2000). With this type of error in view, the central aim of this paper is *critical* reflection on important types of 'experiments' in decision support intervention—experiments whose *failures* are not only under-recognised but often misunderstood.

However, a second, contrasting, potential research policy error would be to throw the core idea of models of production to aid planning and decision making out with the 'bath' of the mistaken notion that computer-owning farmers would naturally use decision-aiding software. Recent research indicates that intervention using science-based simulation modelling can be crucially important to effective and efficient change in farm management practice when undertaken in radically different ways.

The first section of the paper advances a conceptual structure for discussing farming as a human activity and for comparing several types of model-based professional intervention in farm management to facilitate what is commonly referred to today as 'best practice'. The next section draws on a largely neglected model in the philosophy of science to map a system of relationships between farm management practice and research conducted to influence it. This framework aids discussion in the remainder of the paper of 3 paradigms of model-based intervention in farm management: (*1*) researchers designing 'best practice' for practitioners, using theoretical models, (*2*) researchers providing practitioners with practice-guiding tools, and (*3*) in collaboration with practitioners, researchers, aided by their models, researching 'best practice' in the context of farm management practice.

A systems view of the farm and intervention in its management

Modelling the farm

From the primary standpoints of agricultural science and most agricultural systems research, the farm is a system of production. Through research in various disciplines, knowledge is generated about biological, chemical, and physical aspects of production. Beginning with chemical fertiliser in the first third of the 19th century, scientific knowledge has enabled the development of material technologies that have displaced or altered many traditional farming practices in much of the world. In many places this scientific knowledge of the production system has also profoundly changed farmers' concepts regarding the way the production system 'works'. But, ironically, the magnitude of this dual revolution in technology and scientific understanding on the part of farmers may have been instrumental in causing a profound misunderstanding in the agricultural science community (and the non-farming community generally) about the way management of a family farm 'works'. A consequence of this misunderstanding is the stereotyping of the farmer as a *technician*, the farming community as a *market* for science-based recommendations for practice that will put the resultant practice on a more 'rational' footing, and farming as a target for legislated regulation of activity. For scientists who aspire to effective intervention in how farmers manage the production system, and even in times when ecological degradation of farmland and associated ecosystems are of widespread concern, this is far too simplistic a construct. To Dillon (1979), whatever physical characteristics it may have, a farm:

> '…can always be seen as a contrived social organization involved in…production and constituting a purposeful system. By their nature as purposeful (i.e. goal setting) systems, farms are subject to the principles of management pertinent to such organisations' (p. 7).

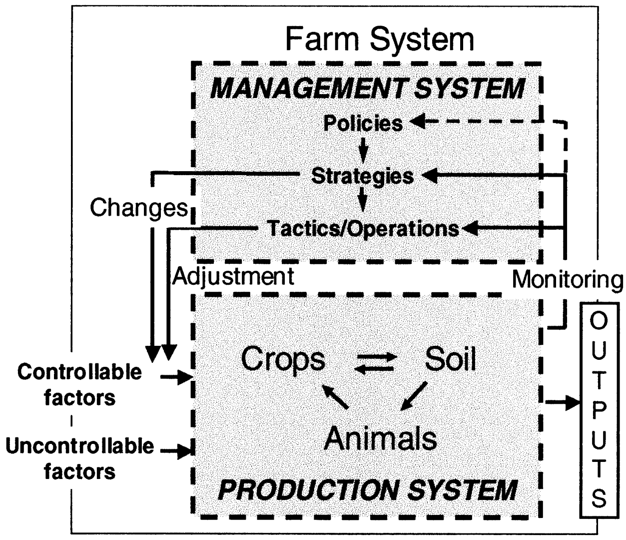

Fig. 1. A simplified model of the farm system, depicting management as normative, instrumental and cybernetic (Adapted from Sorensen and Kristensen (1992).

Figure 1, adapted from Sorensen and Kristensen (1992), depicts the farm as comprising a Production System and a Management System. This representation can be seen as a common core of the more elaborate models of Dillon (1979) and Dent (1994), with the 'Management System' incorporating Dillon's 'Managerial', 'Goals and Values', and 'Psychosocial' subsystems. The fact that Fig. 1 depicts information for decisions flowing into the Management System only from a production subsystem, and not from other important subsystems, e.g. market, institutional, etc., was criticised by Dent (1994). However, this oversimplification serves my focus on expanding the perspective of agricultural production systems research and intervention to include the subjective/social dimension of the Management System.

If it is true that past DSS ventures in agriculture have been primarily the 'packaging'of science-based knowledge about the Production System as software that computer-owning farmers chose not to use, how might appropriate recognition of a Management System in future decision support ventures change process and outcomes?

The Production System is a biophysical and technological domain; the Management System is a human domain—central to which lies a complex mixture of the subjective desires and beliefs of the decision-maker shaped by histories and influences of his/her community. One approach to a more holistic treatment of the farm would be to co-opt appropriate social science expertise to augment existing Production Systems expertise. But as Dillon (1979) cautioned, simply increasing disciplinary comprehensiveness does not ensure more adequate recognition and treatment of the farm as a functioning system in terms of the *interactions* between the technological and the human.

Another approach to gaining more comprehensive 'systems thinking' is for the production-oriented researcher to shift the boundary that delineates the system of relevance, presently the Production System, to include the Management System. But is this professionally feasible? This is not quite the same as, for example, a systems researcher trained in animal science becoming more competent in matters concerning pasture and crop production, soil and climatic processes, etc. Here we are probing the possibilities for synthesis by agricultural scientists of a systems framework for research that includes the reality of the farm as a human system. To counter any alarm prompted by the thought of 'becoming a social scientist', a peremptory conclusion can be advanced. Such achievement by agricultural science-trained systems researchers might be made feasible by the adoption of two complementary strategies. The first achieves more comprehensive 'systems thinking' through new knowledge of social concepts at a 'semi-popular' level. (Opportunity costs in time and attention of busy scientists may be offset by the usefulness of such new insights to their own social worlds and their management). The second strategy is to use a 'systems practice' that, instead of seeking models to better represent the social Management System, *involves* farmers–managers in the research. Their appropriate involvement *represents* the human dimension—their management histories, policies, and processes.

As a starting point it is useful to consider the contents of the Management System of Fig. 1, which depicts two principles of farm management important to Sorensen and Kristensen (1992): the concept of *cybernetic* (see **Glossary**) control of production and the hierarchical organisation of plans. The two are linked by the centrality of overall management *purpose*.

In Fig. 1 the Management System, guided by purposes, controls production. Control is achieved through dynamic interplay between (*a*) knowledge of the state of the Production System attained through ongoing Monitoring, (*b*) management actions on Controllable Factors, and (*c*) adjustment of plans in response to Uncontrollable Factors (Fig. 1). Most fundamentally, even in the most industrialised farming, overall *control* of practice is not primarily a matter of scientific principles: 'In cybernetics, the secret of a control mechanism is hidden in the purpose of the control. Cybernetic explanations are essentially teleological [see **Glossary**] …' (Sorensen and Kristensen 1992, p. 50). Even in small family farms, *purpose* is organised hierarchically in plans and practice as feedback loops at the levels of operations, tactics, and strategies (Fig. 1) (Sorensen and Kristensen (1992, p. 53).

Figure 1 contains one additional, higher, level of organisation—that of *policy* (see **Glossary**). Policy of a farmer, or of a farm family, is analogous to 'policy' of institutions in that it concerns the human predicament of regulation of action in response to multiple, often conflicting, pressures (Jantsch 1975):

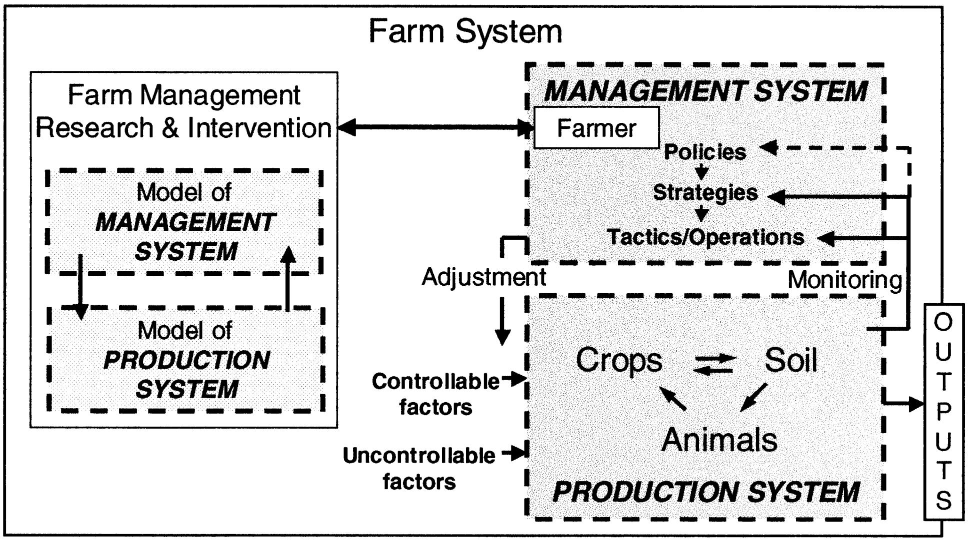

Fig. 2. Farm system expanded to include external intervention in management practice using models.

> 'A policy is a set of principles laid out for the purpose of regulating simultaneously and in a viable mode a multitude of interacting relationships pertaining to many qualities and dimensions of human life—in short, a theme underlying a life' (p. 6).

In a family farm organisation one would not expect these principles to be written down, but they can be expected to implicitly embody the underlying *normative* (see **Glossary**) influence on the farm management system of 'what being a farmer is in this culture'. This can be illustrated by modifying Clancey's (1997) description of an artist in his community of practice, by substituting 'farmer' for 'artist':

> '[The farmer's] intentional, purposive orientation is with respect to the activities in which he is "being a [farmer]". Even when he is alone, his choice about how to spend this time, what tools and results are valued, how to dress in his work, what to do next are constrained by his understanding, his conception, of who he is within the [farming] community. We say, therefore, that his knowledge is inseparable from his identity; that is, *[the farmer's] knowledge is functionally situated* as that of a person who participates in our society in a certain way' (p. 23, my emphasis).

What Jantsch (1972) had to say about the relationship between policy and planning in management of organisations might well have been written to accompany Fig. 1:

> '…the planning process…unfolds in the cybernetic process of rational creative action. To engage this process fully, we have to view it at all three levels, linked by feedback interaction between them: policies (what ought we do?), strategies (what can we do?), and operations or tactics (what will happen if we take a specific course of action?). This vertical integration of planning and action brings human values and norms into play in an important way' (p. 9).

Professional intervention in the farm management system

A feature of agriculture in Western industrialised countries over the past century has been government programs of intervention in farming designed to hasten adjustment to the economic, social, and political impacts of change, either as cushioning of effects or in capitalising on new opportunities. In Australia and the United States this has often been a part of 'farm adjustment' policies and programs (Taylor and Taylor 1952; Makeham and Bird 1969). Throughout most of this period, this incentive for government intervention coincided with a 'progressive' ideology of governance that embodied the expectation that 'management' in general, from government to farms, could be, and should be, more scientific (Maxwell and Randall 1989). Together, these two factors provided a prolonged favourable climate for the science-based intervention in farm management to be researched and trialled in the public sector. This history provides a good example of what Schon (1983) termed 'the Technological Program'.

The dominant discourse of the farm management Technological Program began with the premise that culturally normative practice and cybernetic farm management practices (Fig. 1), based principally on community and individual farming experience, are an inadequate knowledge base for responding sufficiently rapidly to changes in the economic environment or in technology. Scientific analysis and intervention can contribute to a more 'rational' basis for farm management decisions (i.e. in line with new realities) and better outcomes. Figure 2 expands the farm system to include 'Model-based Analysis and Intervention' in the Management System. (Figure 2 resembles the simple representation that Jayaratna (1986) proposed as a generic systems analysis framework.)

The principle of science-based intervention in situated practice has been criticised on moral grounds when the outcome significantly diminishes practitioners' control over their own futures (Habermas interpreted by Bernstein 1985,

Table 1. Comparison of seven types of analysis/intervention

Type of systems analysis/intervention	Characteristics of systems analysis and intervention	Model of production system	Model of management system
	Type 1		
Empirical study of the farm as a business system	Collaborative dev't of farm recording systems; use of average costs to aid planning	Records of production	Records of costs
	Type 2		
Marginal analysis using production economics theory	Recommended action based on whole farm resource optimisation; analysis included marginal returns and opportunity costs	Technical coefficients provided by Production Functions; static input-output transformations	Notionally specified decision problem. Notionally specified conditions for economic model
	Type 3		
Dynamic simulation of production processes	Recommendations based on single factor simulations	Dynamic model of production processes. Notional initial conditions for simulation	
	Type 4		
Decision analysis using dynamic simulation of production processes	Enhanced recommendations based on optimisation of production inputs	Dynamic model of production processes. Notional initial conditions for simulation	Notionally specified decision problem. Notionally specified conditions for economic model
	Type 5		
Decision support system	Interactive decision support system on farmer's computer	Simple model or abstracted output of a complex model of production categories of conditions	Notional decision problem(s)
	Type 6		
Expert system	Interactive expert system software on farmer's computer	Table of action-outcomes	'If…, then…' model of expert manager's procedures
	Type 7		
Cooperative learning using simulation-aided problem analysis and discussion	Facilitation of learning aided by customised simulation by intermediary in response to farmer's felt problems and situation	Dynamic model of production processes Measured initial conditions for simulation	Participating farmer represents management system in setting of problem, customising simulation, and interpreting output

p. 23; Ulrich 1983). But in the case of *decision support*, farmers can choose (and, for the most part, have chosen), to ignore these intervention 'offers'. On the other hand, in principle, there has been no serious barrier to any novel model-based intervention becoming widely used if farmers valued it sufficiently. The most important determinant of outcomes of past decision support efforts in farm management would seem to have been *relevance* to situated management practice and its eventual *significance* in changing practices and outcomes—with both relevance and significance judged by the manager in relation to his/her purpose or concern (Dreyfus 1994).

Historically, different types and combinations of models have successively occupied this interface between farm management theory and actual farm management practice as shown in Table 1. The most historical dichotomy in types is between models of physical production in the Production System and economic models in the Management System. This dichotomy largely corresponds with the dichotomy between matters of resource allocation (the domain of economics), and of crop and animal husbandry (the domain of agricultural science). From the time of the early Farm Management Research (Table 1, Type 1), economists tended to see this as a split between farm management and technology:

> '...economic theorists have provided a promising framework on which a concept of the managerial element in farming can be based. This concept is based on a division between technological skill, and skill in adjusting operations to uncertainty and is associated with the problem of flexibility in the firm' (Case and Williams 1957, p. 362).

But grounds for framing differences among types of interventions on economics *v.* technology have greatly diminished. Firstly, as limits of economic models in this framework became apparent and capabilities of scientific models to represent processes dynamically became evident, the research base servicing the economic managerial framework broadened. In addition, as emphasis in research has moved from production efficiency to ecologically sustainable farming, the conceptual base for the management framework itself has broadened beyond production economics. Secondly, as systems thinking has evolved from emphasis on 'hard' reality to recognition of the importance of 'soft' reality in management (Jarvie 1972), the traditional approaches of economics and agricultural science can be 'lumped' as members of the *same* problematic/limited paradigm.

Economic optimisation as a basis for intervention (Table 1, Type 2) was at its peak in the 1960s (Malcolm 1990), but limitations due to the static nature of the production functions with which the Production System was represented became evident. Late in that decade, dynamic simulation models (Type 3) were being trialled as a replacement for production functions (Dent and Anderson 1971), but two decades later, this experiment by agricultural economists (Type 4) had run its course. Doyle (1990) bemoaned 'the failure of systems concepts and simulation models to have any practical impact on farming'.

A major discontinuity exists between Types 4 and 5, with types 5 and 6 both representing personalised, interactive computer aids to decision-making. Development of DSSs by agricultural modellers began in earnest in the early 1980s and took diverse forms. Although the DSS was touted as a new paradigm for intervention that gave new recognition to the social aspects of decision-making (Keen 1987), in agriculture these model-based products failed to 'do more than nod toward the human element' (Malcolm 2000). The story of the DSS and the Expert System as a paradigm shift in intervention in farm management is the subject of a later section.

Type 7 (Table 1) marks a significant break with Types 5 and 6. Intervention here takes the form of discussions between scientists with models of the Production System and farmers who represent the Management System in their posing of problems, preferred approaches to solutions, and interpretations of model-outputs in terms of desirability as well as management feasibility. This paradigm shift in intervention is the subject of a later section.

All types of interventions in Table 1 claim(ed) provision of a 'rational basis for farm decision and action. But variation among logical paradigms of intervention was not so much a matter of *degree* of rationality but, rather, of *type* of rationality underpinning the models and their implementation as intended intervention in management. The remainder of the paper critically examines three paradigms and their effectiveness in avoiding the infamous 'gap' between the intended intervention and farm management practice. But first, a framework for comparing paradigms is constructed. This takes the form of a 'map' that assists understanding of essential differences between practical management knowledge and knowledge created in management research and in understanding of differences among alternative modes of intervention. The map, based on the 'three worlds' model of the philosopher of science Karl Popper and the typology of research of Oquist (1978), is an aid to researchers 'knowing their way around' an agricultural domain extending beyond the scientific to include the social.

> 'What is characteristic of philosophy is…the aim of knowing one's way around with respect to the subject matters of all the special disciplines. Now the special disciplines know their way around in their subject matters, and each learns to do so in the process of discovering truths about its own subject matter. But each special discipline must also have a sense of how its bailiwick fits into he countryside as a whole. [But]…the specialist must have a sense of how not only his subject matter, but also the methods and principles of his thinking about it fit into the intellectual landscape. It is part of his business to reflect on his own thinking—its aims, its criteria, [and] its pitfalls' (Sellars 1963, pp. 2, 3).

The map elucidates the role of a paradigm which features flexible scientific/economics models in the progressive adaptation of farming to a rapidly changing world but shows why, in the absence of a complementary farming *practice*-based research paradigm, a debilitating 'gap' between management research and management practice is inevitable.

Mapping a system of relationships between farming practice and research conducted to influence it

In making sense of reality, Popper (1972) assumes that the world of physical and biological objects that seems to exist 'out there' (in such configurations as farm production systems) does, in the main, actually exist, in that the objects would still be there in the absence of the mind of the scientist or the farmer *thinking* they are there. He argues that while there is no way to *prove* the existence of this *objective* 'world 1', this *'realist'* assumption concerning the 'nature of reality' (i.e. *ontology* (see **Glossary**)) is one of the more compelling aspects of 'commonsense' (p. 37). But, Popper does not take the position that physics and physiology is *all there is*. He also was impressed by the truth of his own existence as a *person*, of the reality of a very different world of subjective mental states, and of the relationships that result from interactions among persons with mental states:

> 'According to the famous French philosopher Rene Decartes, [as I speak to you] my mind is now acting on my body, which produces physical sounds. These, in turn, are acting on your body, that is on your ears; and then, your body is acting on your mind, making you think. [We may speak about this as] an *interaction* between *physical* and *mental* states. I think that it is just commonsense to accept, at least tentatively, that there exists indeed this interaction between physical states (or processes) and mental states (or processes), or between [what I will call] the worlds 1 and 2. And since things which interact may be said to be real, we may accept reality of these two worlds. Thus I can describe myself as a Cartesian dualist' (Popper 1994, p. 5).

Although this 'dualistic' model of reality consisting of the *objective* physical world (world 1) and the *subjective* world of human consciousness (world 2) makes sense of the

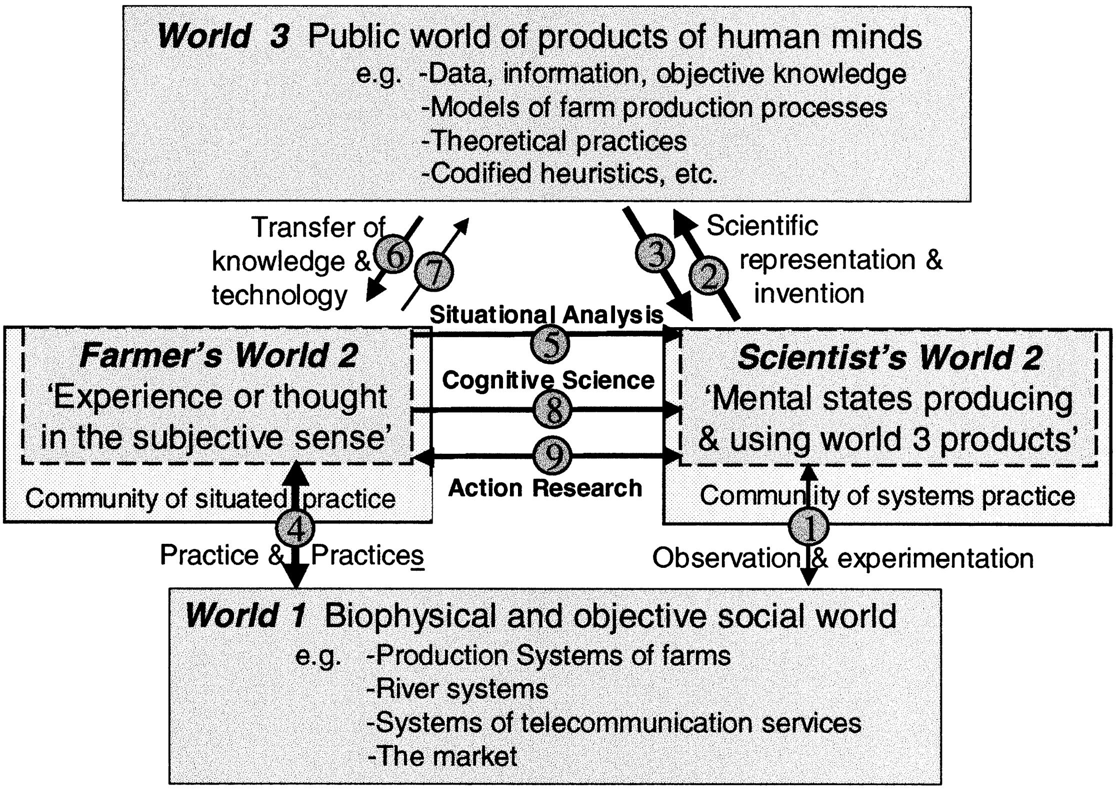

Fig. 3. A 'worlds map' to aid getting around in the domain of systems research and intervention in farming practice. Adapted from Popper's 'three worlds' model but distinguishing in world 2 communities of farmers (left) from communities of systems researchers (right).

primary discontinuity in scientific understanding of the lived-in world, it also leaves a major problem unresolved, as pointed out by Caws (1988):

> '[People] are continually referring to entities—explanations and moral rules and theorems and constitutions and interest rates and genres—that have no existence in the *material* world [Popper's world 1], and attributing to them properties and conditions as if they were perfectly objective and freely accessible to other people [as if they were in world 1], when in fact they are personal (though socially learned) constructions on the part of individual subjects with brains [in Popper's subjective world 2]... [The list includes]...*everything that falls between the subjectivity of the...ego and the objectivity of the natural world*. Their ontological status has always been problematic for philosophy...' (p. 247; my interjections in square brackets and my emphasis).

In Popper's view, all of scientific knowledge lies in this problematic, 'in between' category of mental constructions. His solution was to propose '*the third world*' (Popper 1972, pp. 106, 7). 'By 'world 3' I mean, roughly, the world of the products of our human minds' (Popper 1994, p 5).

> 'Among the inmates of my "third world" are, more especially, theoretical systems; but inmates just as important are [objective/structured] problems and problem situations. And I will argue that the most important inmates of this world are critical arguments, and what may be called—in analogy to the physical state [world 1] or to a state of consciousness [world 2]—*the state of a discussion* or the *state of a critical argument*; and of course the *contents* of journals, books, and libraries' (Popper 1972, p. 107).

Figure 3 is constructed to aid discussion of this 'three worlds' model as it explains differences among the various types of system analyses and interventions of Table 1. Popper emphasises that the utility of this discontinuous model of human reality lies mainly in the *interactions* between the worlds. In discussing the relationship between world 1 and world 3 [(Interactions 1 + 2), Fig. 3], Popper describes world 2 as 'the world inhabited by our own mental states, [whose] main function is to produce world 3 objects (Interaction 2), and to be acted upon by world 3 objects' (Interaction 3) (Popper 1994, p. 7). This facet of world 2 applies well to scientific mental inquiry to describe and explain object states and processes of world 1 *via* Interaction 1. World 3 products that are generated (Interaction 2) include records, data, hypotheses, theories, principles, mathematical models, computer algorithms, practice recommendations, decision support systems, etc. The special contribution of world 3 is that it provides a home for those 'in between' entities described by Caws in the earlier quote in this section, whose awkwardness is due to being ontologically *subjective,* in that they originate as ideas in individual minds, but epistemologically (see **Glossary**) *objective,* since when

Table 2. A typology of research (Oquist 1978)

Descriptive research	Delimits phenomena within typologies of facts and events
Nomothetic research	Attempts to explain and/or predict phenomena with regard to the external relations between a given phenomenon and one or several variables and constants. [Model-making research]
Policy research	The production of knowledge that guides practice, with the modification [in practice] of a given reality occurring *subsequent to* the research process
Action research	The production of knowledge that guides practice, with the modification [in practice] of a given reality occurring *as part of* the research process

made public and open to scrutiny, their destinies are independent of the author/designer (Searle 1995, p. 10). This is Popper's *objective knowledge*—'knowledge without a knowing subject' (Popper 1972, p. 106)—whose production, through design and evolution, is the object of science.

Popper represented world 2 as an undifferentiated domain that mediates between worlds 1 and 3 (Popper 1994, p. 7) and which served his project concerning the 'growth of knowledge' (p. 37) (through Interactions 1, 2 and 3, Fig. 3). However, in Fig. 3, I have chosen to distinguish researchers' world 2 (right) from farmers' world 2 (left). This provides structure for distinguishing differences between (*a*) thinking about farming practice in the practice situation (left) *v.* thinking about what might be *possible* in farming practice as determined by principles concerning the nature of world 1 (right), and (*b*) various approaches to intervention by professionals in farmers' practice. Description of world 2 as 'experience or thought in the subjective sense' (Popper 1972, p. 394) is apt for world 2 farming practice (Fig. 3, left). This is integral to 'commonsense' situated practice by which people live out their lives in families and communities, regulated by individual purpose and community norms and standards. Scientists also engage in situated practice, e.g. when he/she pursues career making, household management, parenting, and activities in the general community. In this world of everyday conscious, subjective, world 2 experience of the objects in world 1, farmers make sense of the experience and act on their beliefs to achieve their ends (Interaction 4). 'Practice' (singular) (Fig. 3) (or, in some literature, *praxis*) refers to the overall conformity of actions with underlying cultural values imbedded in normative Policies (Fig. 1). 'Practices' (plural) refers to various purposeful technical means to ends. In modern farming, some practices (Interaction 4, Fig. 3) were designed in the light of current theory in world 3 and arrived in world 2 practice *via* Interaction 6:

> 'Reform, change, improvement, modification, if any, seem to…proceed from the competitive interaction between our private [subjective] beliefs about the world, and their [objective] third world brothers' (Jarvie 1972, p. xi).

World 3 is occupied largely, but not exclusively, by products of research. Much of this research is justified in terms of its potential to improve the control of world 1 for world 2 purpose—the rationale for a portion of applied research. Dysfunctions have created 'the gap' of the paper title. The problems of traditional research (Ison and Russell 2000; Roling and Jiggins 1998) due to reliance on 'linear' sequence of 'basic research–applied research–engineering–extension' can be seen in Fig. 3 as Interactions 1-2-3-6. This Technological Program (Schon 1983, p. 31) misunderstands the nature of world 2 situated practice. In addition, the differentiation of professional cultures and traditions that is a by-product of this sequential process creates its own internal stresses and dysfunctions as indicated by Passioura (1996):

> 'Science is about discovering how the world works. Engineering is about solving practical problems. The mode of thinking is different. In the profession of agronomy, in which engineering and science are closely intermingled, the tension between the two approaches is often very evident' (p. 690).

Clearly, we need a more adequate framework for thinking about connections between research and practice—an aid to understanding differences in effectiveness of different approaches to intervention in Table 1.

The combination of the 'worlds map' of Fig. 3 and Oquist's typology of research (Table 2) offers such a framework. Farm management practice (Interaction 4, Fig. 3) concerns knowledge in the service of world 2 plans and action in a complex world 1 consisting of the farm and its physical and macro-economic environments. According to Simon (1996, p. 210), scientific research generates two types of knowledge of potential use to practice, one stemming from description of *states* within typologies of facts and events, and the other from description of *processes.* Both provide knowledge of the *structure* of world 1 (Interactions 1 and 2, Fig. 3); both make use of the redundancy in nature to provide simplified description using named categories. These 2 types of world 2/world 3 science practice constitute *descriptive* and *nomothetic* research, respectively, in the research typology of Oquist (1978) (Table 2).

A special feature that Oquist claims for this typology is that each type assumes and builds on the types above it in Table 2 (Oquist 1978, p. 145). For example, early natural science knowledge grew as the result of expansion of the 'collection' of entities described and categorised, as well as the scope and level of detail of description. But, progressively, regularities in observed facts and events could be represented as general rules, often in the form of mathematical models. But this *nomothetic* (rule/model–

making) research was possible only by 'standing on the shoulders' of comprehensive systems of *descriptive* knowledge.

Scientific knowledge of regularities and processes in world 3 (Fig. 3) provides *explanation* of states and their changes as well as a sort of *prediction* of future states. This is accomplished by deductive inference that is symmetrical to the process of explanation. But the connection of scientific *knowledge* to rational management *action* is not straightforward. Although control of nature in the service of human purpose is outside the bounds of science *per se*, science does provide a *foundation* for design of control. World 3 representations of states and processes in world 1 are, in the words of Simon (1966):

> '…the warp and weft of our experience. Pictures, blueprints, most diagrams, and chemical structural formulas are state descriptions. Recipes, differential equations, and equations for chemical reactions are process descriptions. The former characterize the world as sensed. The latter characterize the world as acted upon; they provide the means for producing or generating objects having the desired characteristics' (p. 210).

But world 3 objects also include mental products in the form of *theoretical practices and plans* produced to guide practitioners' world 2 planning and world 1 operations (Interaction 4, Fig. 3). This constitutes Oquist's Policy research (Table 2), the first of the 3 paradigms of analysis and intervention to be evaluated, and the one that is familiar territory for agricultural scientists.

Paradigm 1—Researchers designing 'best practice' for practitioners, using theoretical models

Essential to understanding practice (Interaction 4, Fig. 3) is appreciation of its being situated in both subjective world 2 and objective world 1. This is evident in Oquist's succinct definition:

> 'Practice is policy and action in the context of determinate structures and processes, both those being acted upon and those that condition the outcomes of actions' (Oquist 1978, p. 145).

In terms of Fig. 3, *policy* concerns desires and intentions of situated practitioners and is unambiguously in subjective world 2 (left side Fig. 3); the 'determinant structures and processes' constitute an objective world 1 context. With goals of the practitioner given or assumed, *policy research* attempts to provide objective world 3 guides to practice by utilising theory about the determinant structures and processes in world 1.

As indicated in discussion of the nature of the Management System of Fig. 1, *policy* in situated practice concerns the relationship between stable 'themes' in the life of an individual (or an organisation) and a program of action. The most obvious theme in modern family farming is the economic interests of the family, but this can be expected to be accompanied by other themes, including the identity as a farmer as defined by cultural values and norms in the farmer's community. In a survey of Western Australian farmers, Frost (2000) found that of 14 themes, 3 of the top 6 concerned economics, but highest ranking were 'pride in ownership', 'self-respect', and 'meeting a challenge'. According to Oquist (1978):

> 'Policy consists of the needs and interests, values and norms, ends and objectives, plans and programs, operations and evaluations, and resources related to a given action or potential action' (p. 144).

At one end of this hierarchy of factors that constitute 'policy' lie subjective and social forces that determine purposes or themes for practice; further toward the action end lie elements of *means* both to achieve purposes and to enact themes in a culture. In situated practice (Interaction 4, Fig. 3), policy is structured by *experience* and *imagination* in world 2, the latter, according to Johnson (1987, p. 168), being 'indispensable for our ability to make sense of our experience, to find it meaningful' for future practice.

Although Policy research is concerned with improving outcomes of world 2 situated policies and practice, it is not directly concerned with underlying world 2 values and interests or the practical determination of ends, objectives, or actions. Rather, its domain, in common with that of Descriptive and Nomothetic research, is the representation of world 1 objective reality in world 3—but specifically the '*possible* world 1s' that result from deliberation (and simulation using nomothetic models) of actions to control or change world 1. Whereas Nomothetic research provides objective models of relationships in world 1, Policy research is concerned with interpreting these as guides for world 2 practice by anticipation of meaningful world 2 'what if…?' action questions and provision of objective 'if…then…' answers. The epistemology of Policy research has also been influenced by various forms of Utilitarianism, or Consequentialism.

> 'Utilitarianism is an effort to provide an answer to the practical question "What ought a man to do?" Its answer is that he ought to act so as to produce the best consequences possible. [ ] [This stance] avoids making judgments about what is intrinsically good, finding its content instead in the desires that people…do have. [ ] [But] most preference Utilitarians want to base their judgments, not on the desires that people *actually* have, but rather on *those they would have if they were fully informed and thinking clearly.* It then becomes essential to discover what people would want under these conditions, and, because most people most of the time are less than fully informed and clear in their thoughts, the task is not an easy one' (Encyclopaedia Britannica).

This orientation to discover what the *ideal* practitioners would want/do and to equate this to what *any* practitioner *ought* to want/do, is what gives policy research a *normative* intent.

In agriculture, the archetypal policy research has been conducted by agricultural economists in Farm Management Research. But in agricultural economics a colloquial terminology regarding 'policy' grew out of the relative uniqueness farms as organisations. Research aimed at influencing public management of agriculture is termed '*policy* research'; research concerning 'private policy' of is termed '*farm management*' research (Johnson 1962, p. 13). Both qualify as Policy Research in Oquist's typology (Table 2), which reflects the conventional usage.

The traditional starting point for Farm Management economics is a problem in world 2 'private policy.' But the research itself is not about world 2 but, rather, about market mechanisms in world 1 with notional implications for practice of a farmer. Similarly, most DSS production draws on study of world 1 production systems by applied scientists with the intent of providing 'rational' recommendations concerning 'what farmers ought to do.' In both economics and applied science, research is about world 1 and produces theoretical practices and plans in world 3 (Fig. 3), which are assumed to be superior to those based on 'subjective', 'intuitive', world 2 policies.

On the surface, the methodologies of economists and scientists are very different, but as regards policy research there exists both a logical and an historical link via Karl Popper's foray into social science. However, what makes a look at this compelling is the prevalent influence of Popper on science thinking and practice over the last 5 or 6 decades. An understanding of his views on social science is important to understanding the limits of policy research and to interpreting the history of successive attempts to remedy these.

Popper's philosophy of the scientific study of world 2 situated practice, i.e. social science, was a coherent extension of his philosophy of natural science. Popper recognised that much of real world practice was constructed from individual experience and from heuristics (see **Glossary**) shared within the community of practice. The low regard with which Popper held this 'knowledge of the knowing subject' was due to its use of inductive reasoning, i.e. drawing inferences about the nature of the world from cumulative aggregation of subjective experiences of specific events. One of Popper's early contributions to philosophy of science was his logical refutation of inductive reasoning as a means of acquiring rational knowledge (Popper 1972, p. 1). Unless study of the social world 2 required a qualitatively different approach to the natural science of world 1 (and he argued that it did *not*), his earlier refutation of the logic of induction meant that the ordinary grounds for ordinary practice, i.e. subjective experience, could not qualify as 'rational'. From the stance of Popper's Critical Rationalism, the most *rational* world 2 practice (Interaction 4) would be achieved by adoption of the 'best tested' appropriate hypothesis/theory from world 3 (Interaction 6). But, he made clear that this choice did *not* confer reliability in getting the desired outcome:

> '…we should prefer as a basis for action the best-tested theory. [ ] [But] in spite of the "rationality" of choosing the best-tested theory as a basis for action, this choice is not "rational" in the sense that it is based on *good reasons* for expecting that it will in practice be a successful choice: *there can be no good reasons* in this sense…' (Popper 1972, p. 22, original emphasis).

Popper insisted that the criteria for rationality of action be the congruence of the action with the nature of objective world 1, as represented by theory/hypotheses in world 3. This emphasis on application of *substantive rationality*, i.e. the use of objective knowledge for everyday choice in ordinary practice, had a profound impact on social science, systems thinking, and on professional intervention in situated practice. Only recently in agriculture science have we come to appreciate the penalty attributable to this stricture, aimed at achieving a rigorous form of inference making. Logical rigour has often been bought at the price of low relevance of the knowledge to situated practice as judged by practitioners, resulting in 'the gap' of my title.

Popper acknowledged that practice was situated and that theory about practice in world 3 was out of context. His approach to bridging this gap was the methodology of 'situational analysis' depicted in Fig. 3 as Interactions 5 and 2. Popper's own interest was not policy research. He was concerned with extending the methodology of natural science study of world 1 to social science practice. His aim was objective understanding of situated behaviour rather than design of practices that might improve situated practice outcomes. But his approach had important impact on policy research and professional intervention in practice originating in world 2 (Interaction 4) using Interaction 6 (Fig. 3).

Intervention Types 2–4 (Table 1) have in common a methodology that has been integral to modern economics but was borrowed and extrapolated by Popper (Redman 1991):

> 'There are two major theories of rational action at present—decision theory and Popper's theory of situational logic. Decision theory is a prescription for rational action: List the options open to you, estimate the utilities and probabilities of the various outcomes and choose the option that maximizes expected utility. Popper's situation logic is a descriptive theory with methodological implications: if you want to understand X's action, find out what X's goals were and what X perceived his situation to be; X's action will then be seen to be one appropriate to that perceived situation' (Koertge 1974 quoted by Redman 1991, p. 111).

Popper described 'situational logic' as the product of 'logical investigation of economics…which can be applied to all social sciences':

> 'A social science oriented towards objective understanding or situational logic can be developed independently of all subjective or psychological [i.e. world 2] ideas . Its method consists in analyzing the social *situation* of acting men sufficiently to explain the action with the help of the situation, without any further help from psychology. Objective understanding consists in realizing that the action was objectively *appropriate to the situation*. In other words, the situation is analyzed far enough for the elements, which initially appeared to be psychological, … to be transformed into elements of the situation' (Popper 1976, p. 102, 3).

When applied to policy research, a scientist or economist can (*a*) ascertain through analysis using world 3 theory (Interaction 3, Fig. 3) what behaviour/practice would be appropriate to the situation specified using local information gained in Interaction 5 (Fig. 3), (*b*) formulate this as 'what a rational practitioner would do in a situation so characterised', a product in world 3, and (*c*) intervene in practice (Interaction 6) with a proposal for 'rational' action. This 'situational determinism' provides the economic rationality of formal Decision Analysis and provides the basis for the 'objective teleology' of 'hard' systems analysis generally (e.g. Ackoff and Emery 1972, p. 6). Scientific objectivity is maintained by objectifying a subjective *purpose* in situated practice as a given goal. The use of theory is *normative* in its intent to provide guidance on the best way to achieve the given goal.

Situational analysis has provided the logical framework for using simulation models in Production System analysis and intervention (Type 3, Table 1). The simulator is specified for the physical situation, the goal is stated, and the outcomes for alternative actions estimated using the physical and biological relationships in the models. While decision analysis (Types 2 and 4) relies on mathematical optimisation, simulation (Type 3) simply compares a range of sensible alternative actions and relies on ranking of outcomes in terms of the goal values. While the model is often pragmatically specified for a 'typical', rather than an actual, situation there is the potential to mimic a specific situation using local data. Studies using simulations to identify what would be the 'theoretically' superior practice have an implicit normative intent.

The effectiveness of Popper in achieving an approach to social and policy research that satisfied criteria for objective rationality is confirmed by later critics seeking a way to overcome its limitations. In criticising the minimal role in situational analysis of the human decision maker, Simon (1979) observed that:

> 'Behavior is substantively rational when it is appropriate to the achievement of given goals within the limits imposed by given conditions and constraints. Notice that, by this definition, the rationality of behavior depends upon *the actor* in only a single respect—his goals. Given these goals, the rational behavior is determined entirely by the characteristics of the environment in which it takes place.' (p. 67).

Simon went on to point out that classical economic decision analysis rests on two fundamental assumptions about the world 2 environment of a decision. The first is that the economic actor has a particular goal, e.g. maximisation of profit or of subjective expected utility. The second assumption is that the economic actor is rational in the sense that he/she has perfect knowledge of matters relevant to the situation. Together these two assumptions 'freed economics [and Popper's social science] from any dependence on psychology' and attendant 'subjective' economic behaviour, and kept economics as world 3 representations of 'the market' in world 1 (Fig. 3).

Simon's arguments clarify the nature of interventions of Types 2–4 (Table 1). The focus of knowledge–practice relationships is placed entirely on situation-related *outcomes*, at the expense of managers' subjective decision *process*. Given parameters and initial conditions of the system (the 'situation'), theory about the system can be applied in order to ascertain the action that will produce the theoretically best outcome. The role of the manager is simply provision of information for the analysis.

'Normative' intervention intent with regard to theory is quite different from 'normative' with regard to cultural/personal values, but they are related. Normative theory accepts the farmer's culturally normative goals and assumes that the farmer prefers to achieve these goals with high economic and technical efficiency. The theory concerning the situation together with powerful computation provides a means of optimising action in terms of these values. It is a small further step in logic to claim that a person who holds such values 'ought' to carry out the optimal action. Conceptually, the side of practice that can be expressed as 'doing the right thing' is determined by the world 2 *policy* of the practitioner, which depends on value norms of a culture (Oquist 1978); accepting these norms as input, the side that concerns 'doing it right' relies on the power of the theoretical analysis to find a world 3 product which represents optimally efficient means. This formal complementarity of roles for culture and science has, however, often been violated by the phenomenon described in the quote from Encyclopaedia Britannica on p. 558, when scientists infer not just optimum policy *means* but optimum policy *ends* as well.

After more than 5 decades of theoretically normative policy research and interventions, the outcomes of world 3 theoretical practices and recommendations for optimal action have been disappointing. The experience of the Farm Management Research movement epitomises this failure, which is captured succinctly and poetically by Malcolm (1990):

> '…over time emerged an increasingly commonly-held unease, and occasionally conviction, that these were trails which if followed, soon led from the complex and difficult whole-farm pastures of plenty [farm management in world 2] to simpler and easier analyses characterised by incomplete and inappropriate disciplinary balances [in world 3] and *resulting in work which was not really about farm management*' (p. 49, my italics).

Malcolm's paper is a rare example of reflective and professionally self-critical analysis of designed 'decision support' in agriculture. It is reinforced by similar histories in the field of OR/MS (Operations Research/Management Science), a field in which Farm Management Research was a marginal member (Hutton 1965; Agrawal and Heady 1972). From many possible examples, e.g. Ackoff (1973, 1979), Checkland (1981), Schon (1983), one provided by Churchman is particularly helpful in elucidating the nature of the failure. In 1957, Churchman, Ackoff, and Arnoff published 'Introduction to Operations Research'. The aim of OR was unambiguous:

> 'The concern of OR with finding an *optimum* decision, policy, or design [world 3 products of world 2 scientific creativity] is one of

> its essential characteristics. It does not seek merely to define a better solution to a problem than the one in use [in world 2 practice]; it seeks the *best* solution. [It] can be characterized as the application of scientific [world 3] methods, techniques, and tools to [world 2] problems involving the operations of systems so as to provide those in control of the operations with optimum solutions to the problems' (p. 8. original emphasis, my interjections).

By 1964, this same C. West Churchman was trying to understand why there was such a gap between world 3 Management *Science* and world 2 management *practice*. His team was researching the validity of the following 'formula' for effective intervention in control of operations (through Interaction 6, Fig. 3), i.e. 'information + analysis + communication leads to knowledge,' with 'knowledge' defined as 'awareness of the right action':

> 'Unfortunately, we have overwhelming evidence that available information plus analysis does *not* lead to knowledge. The management science team can properly analyse a situation and present [world 3] recommendations to the manager, but no change occurs. The situation is so familiar to those of us who try to practice management science that I hardly need to describe the cases' (Churchman 1964, p. 33).

Research which followed, based on the deduction that success must, therefore, lie in with the remaining term, produced the conclusion that:

> '…the formula [information + analysis + communication] will not do. More precisely, the prescription for "better" communication ends up by being no more than the re-statement of the problem: to find some set of [world 2] activities so that with complete [world 3] information "available", the manager will come to know the correct action [in subjective world 2, as objectively defined in world 3]. It is the attempt to find such a set of activities that makes the problem very difficult because *by now we have exhausted all the obvious possibilities*' (Churchman 1964, p. 34, my emphasis and interjections).

The messages from Malcolm (1990), quoted above, concerning Farm Management Research and Churchman, here, concerning non-agricultural operations research/ management science are similarly pathetic: they were both indicating that if successful designed intervention in management decision making is possible, methods for its achievement must lie in some new direction. For a while this seemed to be the direction of putting the tools for analysis in the hands of the decision maker.

Paradigm 2—Researchers providing practitioners with practice-guiding tools.

Although the Decision Support System and the Expert System had very different ancestry, they tend to be treated as closely related species of the same genus of information technology. Together they represent a paradigm shift in intervention made possible by the personal computing revolution. The dominant paradigm of the scientist communicating results of a computer analysis of a *typical* situation to a practitioner in terms of what he should do in his *unique* situation had largely run its course. The emergence of the personal computer interactive software offered the possibility of the situated practitioner conducting his/her *own* analysis. And for a while it seemed to work. In the words of one of the eminent pioneers of the original DSS movement, Peter Keen, reflecting on the first 10 years:

> 'There had been a huge gap between the logical idealism of optimization science and managers' willingness or ability to adopt it. Too many of the methods of Management Science were useful but not useable. DSS emphasized usability and… respecting the primacy of managerial judgment [rather than the primacy of theory]…This pragmatic attitude accomplished in a few years far more than Management science had been able to stimulate in several decades' (Keen 1987, p. 257).

This account emphasises instrumental progress, but later in the same paper Keen referred to a debt to Herbert Simon for contributions to a complementary *intellectual* progress:

> 'The early progress of DSS depended quite heavily on borrowings from Simon and the Carnegie School's work on decision-making and from cognitive psychology. They provided a useful intellectual base for the era of individual...support' (p. 264).

Simon successfully challenged the dominant philosophical position (i.e. that of Popper), that action in subjective world 2 practice can be *rational* only by importation of 'theoretical practice' from objective world 3. Simon argued that it was rational to entertain multiple rationalities. In addition to substantive rationality of products of science in world 3, a *procedural rationality* is integral to world 2 creative *processes* in communities of practice (Fig. 3):

> 'Economics …is a [world 3] description and explanation of human institutions [in world 1], whose theory [in world 3] is no more likely to remain invariant over time than the theory of bridge design. Decision processes, like all other aspects of economic institutions, exist inside human heads. They are subject to change with every change in what human beings know, and with every change in their means of calculation. For this reason the attempt to predict and prescribe human economic behaviour by deductive inference from a small set of unchallengeable [world 3] premises must fail and has failed.
>
> Economics [in world 3] will progress as we [scientists] deepen our understanding of [world 2] human thought processes; and economics will change as human individuals and human societies use progressively sharpened tools of thought [from world 3] in making their decisions and designing their institutions. A body of theory for *procedural* rationality [of individuals in world 2 situated practice] is consistent with a world in which human beings continue to think and continue to invent; a theory of *substantive* rationality [based on world 3 theory about world 1] is not' (Simon 1979, p. 83. my interjections).

Simon argued that it is illusory to describe a decision as 'situationally determined' when it is the *perception* in the mind of the decision maker *of the external situation* that is determinant. Failure to hold the 'perfect knowledge' of the situation assumed by substantive rationality is not the same as being *irrational*. The limits of our mental capabilities, such as memory and computation, are empirical biological facts. Simon's term, '*bounded* rationality,' captured this unavoidable aspect of being human. He argued that our relative success in biological and social evolution is evidence of remarkable skills in using these limited resources in finding solutions that are 'good enough'. Motivated by our aspirations we undertake a subjective *process* that uses our (bounded) rationality to find a solution that we can accept as satisfactory, either by changing world 1 to align with our world 2 aspirations, or by adjusting our aspirations. Since we are incapable of optimising, we pragmatically 'satisfice'.

> 'Behavior is *procedurally* rational when it is the outcome of appropriate deliberation. Its procedural rationality depends upon the process that generated it. [The field of psychology] uses 'rationality' as synonymous with "the peculiar thinking process called reasoning" ' (Simon 1979, p. 68, my emphasis).

The impact of Simon and his colleagues at Carnegie Mellon University in shifting the focus of rational determinism in decision making from the objective *situation* to what happens *inside the subjective decision maker* was extraordinary. They were instrumental in establishing the fields of both cognitive science and artificial intelligence, and in 1978 Simon was awarded the Nobel Prize in Economic Sciences 'for his pioneering research into the decision-making process within economic organisations'.

The effect of a new scientific respectability for rationality of *process* profoundly influenced the development of and acceptance of Decision Support Systems and Expert Systems. It provided a conceptual basis for a new form of intervention that featured world 3 tools to be used by the manager to aid subjective world 2 practice.

> '…[The] DSS provides a computerized [virtual] staff assistant. The manager's judgment selects alternatives and assesses results. "What-if?" Became the cliché of the DSS field' (Keen 1987, p. 257)

Keen contrasts this with the less modest intervention stance of traditional Operations Research/Management Science:

> 'Optimization science in general—normative support…—rests on reducing or even eliminating judgement. It pushes managers towards economic rationality and systematizing of analysis' (p. 257).

Whereas the DSS was a *beneficiary* of the 'personal–procedural' revolution, the expert system was a *direct descendent* of it. Simon's own research program was the empirical study of human decision-making. Broadly, the approach married cognitive psychology and computer science and became known as 'cognitive science'. The evolution of 'expert systems,' or 'knowledge-based systems', was summarised by Winograd and Flores (1986).

> 'Until the mid-1970s artificial intelligence researchers generally believed they could work simultaneously towards two goals: extending the capabilities of computers, and moving towards an understanding of human intelligence. They might emphasize one aspect or the other, choosing to call their work "artificial intelligence" or "cognitive simulation", but that was a short-term strategic decision, aimed towards an ultimate synthesis.
>
> In the last few years, this view has been questioned. There is a tacit acceptance… that the techniques of current AI are not adequate for an understanding of human thought and language. As a result, there is a clear split between the "knowledge engineers," who apply the well-developed technologies of AI to practical problems, and the "mind-modelers", who speculate about the more complex structures that might underlie human thought.
>
> Commercial interest lies in the first direction—in finding profitable applications of a rather limited set of techniques. The greatest interest is in "expert systems"—programs for problem solving in some scientific or technical domain' (p. 126–7).

In traditional policy research, normative intervention in world 2 situated practice was based on the introduction of *substantively rational* practices from world 3. These were designed in keeping with analysis of world 1 mechanisms and constraints and, as such, reflected what Simon termed the *external* environment of decisions and practices. The result of the Simon-led cognitive revolution was recognition of the importance of an *internal* cognitive environment with its own processes and constraints. These were the new objects of descriptive and nomothetic research in cognitive science. But while this marked a significant intellectual landmark in social research, and appeared to offer promise for closing the 'gap' between research and world 2 practices, unfortunately these reforms failed to flow on to the traditional applied science and policy research practice.

In a landmark paper in imposing some theoretical structure to the field of decision support systems, which had been seen as suffering from lack of theory (e.g. Naylor 1982), Stabell (1987) identifies 4 intellectual schools, i.e. Decision Analysis, Decision Calculus, Decision Research, and Implementation Process. But he concedes that these *collectively* represent a minority interest.

> '…"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*' (p. 250, my emphasis).

The problem of the primacy of the 'technological solution' in Stabell's field of Management Science would appear to be small relative to the field of *agricultural* decision support, where nearly *all* the research has been on development of DSS products and very little on decision making or how decisions can be effectively supported. Stabell admonished that:

> 'The point is not that technology is of no importance. Rather, it is an argument for keeping our attention focussed on the central theme: *Better decisions* and *decision support*' (Stabell 1987, p. 250. original emphasis).

Keen (1987) interpreted this problem in terms of balance between social 'decisions', social 'support', and technological 'systems':

> 'The earlier work of such people has Scott Morton, Gerrity, Stabell, and Little began from a discussion of decision-making, moved on to the issue of effective support, and ended up with a system. By contrast, ten years later, books by Bennett and by Sprague and

> Carlson that reflect the mainstream of DSS in 1982 are both entitled "building" DSS and move in the reverse direction: how do we build systems? What does that say about the nature of effective support? *There is little about decisions*' (p. 260, my emphasis).

In agriculture, there seems never to have been a phase where technology did *not* predominate overwhelmingly. The agricultural DSS movement, generally, adopted the DSS technology as a means of 'packaging' research results (e.g. Plant 1997, p. 37), and in doing so, it was not alone.

> '…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).

Having never invested in the side of decision support featuring *decision* in the context of the Management System (Fig. 3), the consequence of market failure of the support technology leaves the field of agricultural DSS without a platform from which to consider diagnosis or reinvention of either technology or intervention approach.

Expert systems have fared no better in world 2 farm planning and decision making than DSSs. In a paper of rare candour in its reflection on what was one of the most substantial efforts, Plant (1997) analysed the failure of CALEX/Cotton and refers to two other failed efforts of similar magnitude which used a similar approach). All were rule-base expert systems providing information for agronomic management as well as for integrated pest management. 'The objective was to develop an integrated program that would provide comprehensive support for crop management decisions (p. 34). Elsewhere, Plant articulated the general appeal of this approach:

> 'From the researcher's point of view, knowledge-based systems are attractive because of their potential to help organize and synthesize knowledge and information of different types. Using knowledge-based systems as the framework, it is possible to focus and apply diverse avenues of research to solve difficult problems. One can link together quantitative data, simulation models, basic research results, and expert opinion into a knowledge-based model of *how difficult decisions ought to be made*' (Plant and Stone 1991, p. 7, my emphasis).

Plant (1997) acknowledged the failure of CALEX/Cotton to persist in use by some 145 registered users. The main learning from the experience was that farmers were much more inclined to use simpler, targeted software tools. This finding was in keeping with earlier experience outside of agriculture. Jones (1989) reported that:

> '…the importance of choosing well-defined, narrow-domain problems quickly became obvious to knowledge engineers in the commercial sector. As a result, many of the most successful industrial applications of expert systems are in very narrow areas of process controls and financial management' (p. 5).

Plant (1997) explains the appeal of 'narrow' over 'comprehensive' domains in terms of match of program complexity to farmers' *computing expertise*. Jones (1989) explanation features differences in *cost–risk consciousness of DSS developers* in business and academic research environments. Both miss what seems to be key to failure—the overriding importance of situation, or 'local context,' in decision-making, including the social context in which the acceptance of 'artificial experts' is determined (e.g. Collins 1990; Berg 1997; Collins and Kusch 1998). Problems and tools that have a very narrow domain are essentially context free. A conclusion that the only successful strategy to achieve relevance to situated practice is restriction to an atomistic problem domain (e.g. the tools now offered by Plant 1997) would be to abandon intervention in farming practice as a *systems* practice. It would be an admission of catastrophic failure of half a century of effort to learn to bridge the gap between theory and practice by making theory valuable to practice outcomes or procedures. And it may be premature.

Paradigm 3—Action research: researchers with theory and models collaborate with practitioners to research 'best practice' in the context of practice

The failure of over 40 years of model-based intervention (as Farm Management decision analysis, simulation models, DSSs and expert systems) to make a difference in the way farm decisions are made is part of a more general failure of such model-based interventions in management practice to live up to expectations, as indicated in the reflections on operations research (OR) by Dando and Bennett (1981):

> '…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.'

The description and interpretation by Schon (1983) of this shift in prevalent 'ways of seeing the world' is particularly insightful (and readable). The dominant way, which he termed the *Technological Program*, showed early signs of decline in the late 1960s (Schon 1983, p.11). Across specialist fields in diverse professions there occurred a crisis of confidence in formal methods for analysing (Interaction 5) world 2/world 1 practice situations (Interaction 4) and in the efficacy of world 3 recommendations and theoretical practices in Interaction 6 (Fig. 3):

> 'When leading professionals write or speak about their own crisis of confidence, they tend to focus on the mismatch of [their] traditional [world 3] patterns of practice and knowledge to features of the [world 2/1] practice situation—complexity, uncertainty, instability, uniqueness, and value conflict of whose importance they are becoming increasingly aware. Why…should leading professionals and educators find these phenomena so disturbing? Surely they are not unaware of the artful [world 2] ways in which some practitioners deal competently with the indeterminancies and value conflicts of practice. It seems, rather, that they are disturbed because they have no satisfactory way of describing or accounting for the artful competence which practitioners sometimes reveal in what they do. They find it unsettling to be unable to make sense of these [world 2]

> processes in terms of the [world 3] model of professional knowledge which they have largely taken for granted. Complexity, instability, and uncertainty are not removed or resolved by applying specialized knowledge to ill-defined tasks. If anything, the effective use of specialized [world 3] knowledge depends on a *prior restructuring* of [world 2/1] situations that are complex and uncertain. An artful practice of the unique [world 2] case appears anomalous when professional competence is modeled in terms of application of established [world 3] techniques to recurrent events' (Schon 1983, p. 18).

Schon describes the shock to makers and users of computerized models. For a 20-year period there was phenomenal growth of Operations Research/Management Science (OR/MS) as a result of success in dealing with complex problems in the military and industry. But these problems were highly structured, i.e. were amenable to analysis by substantive or procedural logic. When models in OR/MS spread to business management where problems were less structured and more dynamic and to fields of social policy where problems were ill-structured/'messy' (Checkland 1981, discussed below), there was a 'widening consensus, even among formal modelers, that the early hopes were greatly inflated'.

> 'Formal modelers have responded to this unpleasant discovery in several different ways. Some have continued to ply their trade in the less demanding [more structured] areas of the field. Some have abandoned their original training in order to address themselves to real-world [ill-structured] problems. Others have decided to treat [world 3] formal models as "probes" or "metaphors" useful [in world 2 practice] only as sources of new perspectives on complex situations [as per Checkland below]. But for the most part, the use of formal models proceeded as though it had a life of its own. Driven by the evolving questions of theory and technique, formal modeling has become increasingly diverted from the real-world problems of [world 2] practice, and…[professionals] who choose to remain on the high ground have continued to use formal models for complex problems [in world 3], quite oblivious to the troubles incurred whenever a serious attempt is made to implement them [in world 2 practice]' (Schon 1983, p. 44).

Schon makes the point that the crisis for modellers in the Technological Program was compounded by the failure of an early attempt at reform. He acknowledges that Simon was right in recognising that the objective rationality of the Technological Program 'fails to account for practical competence in divergent situations' in world 2 but claims that he was wrong in thinking that another *design* approach, albeit with a focus on practitioner cognition, could rectify the problem (p. 47). Schon sees no alternative but to search for an alternative 'epistemology' (see **Glossary**) of practice implicit in the artistic, intuitive processes which some practitioners do bring to situations of uncertainty, instability, uniqueness, and value conflict' (p. 49).

Peter Checkland conducted a search for such an epistemology. Beginning with the tools and approaches of his fields of industrial science and operational research, his search led to his development of Soft Systems Methodology (SSM) (Checkland 1981; Checkland and Scholes 1990). Checkland's search *process* (and the account of it in Checkland 1981, Part 2), from science, through 'hard' systems, and the less mapped, 'soft' territory beyond, serves as a paradigmatic example of a procedure for reform of traditional intervention methodology conducted in Schon's Technological Program. As background, Checkland simplified the complex world using four categories in a typology of systems—natural, designed physical, designed abstract, and human activity. (These map remarkably neatly onto Fig. 3, with 'natural systems ' and 'designed physical systems' in world 1, 'designed abstract systems' as world 3, and 'human activity systems' as world 2.) Checkland (1981) discussed Popper's three worlds model wistfully. There was no question of the *desirability* of producing world 3 theories in the *systems* community of practice using *holistic* rather than reductionist methodologies (p. 101), but he questioned the *feasibility* of such structure in human activity systems. The identification of the goal in situational analysis or decision analysis is often problematic in real world 2 situations. Agreeing on what is the problem is often a problem:

> '... in problems in human activity systems, *history* always changes the agenda! The contents of such systems are so multifarious, and the influences to which they are subject so numerous that the passage of time always modifies the perception of the problem. Such perceptions of problems are always subjective, and they change with time. [ ] Such problems, though "recognisable", cannot be "defined" [and so don't qualify for world 3]' (Checkland 1981, p. 154).

Checkland distinguished 'soft' systems thinking from 'hard' systems thinking primarily on the basis of the differences in explicit recognition of subjective *purpose* in world 2 human activity and the need for methodology that (*a*) did not require this to be objectified, and (*b*) recognised and utilized human *experience* in practice as a source of guides to practice to augment theory:

> 'After [my] experience as a manager in an innovating industry—man-made fibres—I was interested to see to what extent "hard" systems [world 3] thinking could be applied both to the kind of fuzzy problems which managers face and to social problems which are even less well defined. [ ] If the work started from the well-established methods of goal-directed systems analysis and consisted of trying to use them in the ill-defined problems, then it would be possible both to cling onto the known as far as possible and to mark out the areas in which the known had failed. [ ] The course followed was not a *theoretical* pathway but the result of a particular set of *experiences* in actual problem situations. [ ] The research work...was carried out as an...*action research* program...' (p. 150, my emphasis).

Of the 3 'Interactions' in Fig. 3 between the entities of Community of Science Practice and Community of Social Practice, 'Action Research' is the one that features a genuine two-way communication. It is the only one whose aim is not to construct a world 3 objective 'problem' and to design a world 3 science-based 'solution.' Instead, it aims primarily to aid *learning* in world 2 social practice and secondarily to produce world 3 products to aid further such learning. 'Participation' was seen by Checkland as central to achieving these aims:

> 'The concept of action research arises in the behavioural sciences and is obviously applicable to an examination of human activity systems carried out through the process of attempting to solve problems. This core is the idea that the researcher does not remain an observer outside the subject of investigation but becomes a participant in the relevant human group. *The researcher becomes a participant in the action*, and *the process of change itself becomes the subject of research*. In action research the roles of researcher and subject are obviously not fixed: the roles of the subject and the practitioner are sometimes switched: the subjects become researchers...and researchers become men of action' (Checkland 1981, p. 152, my emphasis).

This is research 'to guide practice' that relies on experience in the action rather than on theory about the practice situation, as in policy research (Table 2). In the succession of research types of Table 2, *Action* Research marks the radical departure from the scientific tradition of *descriptive* and *nomothetic* research anchored in Popper's Critical Rationalism and from a *policy* research tradition based on normative theory and nomothetic tools (Oquist 1978). The trend to include such 'human experience' in formal methodology for research inquiry has been significantly reinforced by recent conceptual developments in both cognitive science (Johnson and Erneling 1997) and in anthropology which have resulted in a new convergence of these fields in the interrelated concepts of 'situated cognition' (e.g. Clancey 1997), 'situated action' (e.g. Suchman 1987), and 'cognition in action' (Lave 1988). Out of the disappointments in artificial intelligence and rule-based expert systems has emerged a new *ecological* approach to knowledge and action. This movement co-opts the biological concept of interaction between organism and environment (with *culture* as part of this environment), to achieve a more dynamic, and, apparently, realistic treatment of 'practice'. From this point of view, some of the problems encountered by scientists in trying to bridge the 'gap' may be due to misunderstanding about the nature of farming and of how change in practice normally comes about.

The key implication of this epistemology of practice is that modern farming practice is not mainly the collective application of technical and business *practices*, for which scientific and other outside intervention/support is required. Rather family farming is most helpfully seen as *situated social practice*. This aspect of agents being situated in *communities of social practice*, both in farming and in research, is depicted in Fig. 3 and is an addition to Popper's 'three worlds' model. This perspective can accommodate ongoing entry of technological innovations to farming. It does, however, indicate that effectiveness in decision support may require an altered view of our profession's traditional view of managers as clients for technology. This situated view emphasises both the commitment and agency (see **Glossary**) of a farmer to manage the complexity and uncertainty of his/her own 'small world' (Savage 1972) that is shared, in part, with family and community of practice. This 'small world' ('relevant world' of Agnew *et al.* 1997; 'inhabited world' of Clancey 1997; 'appreciated world' of Vickers cited by Jantsch 1975) exists as a dynamic, subjective subset of the wider and more comprehensive physical world and of all-possible actions, events, and outcomes (the 'grand world' of Savage 1972). A 'small world' is constituted and continuously reconstituted by the agent's making meaning from elements of the wider world in relation to a structure of conscious and tacit *purpose* as Oquist's 'policy' (see **Glossary**), which, like the world, is complex and dynamic. Lave and Wenger (1991) provide a potted version of the nature of situated social practice according to this theory:

> 'A theory of social practice emphasizes the relational interdependence of the agent and world, activity, meaning, cognition, learning, and knowing. It emphasizes the inherently socially negotiated character of meaning and the interested, concerned character of the thought and action of persons-in-activity. This ["small"] world is socially constituted; objective forms and systems of activity, on the one hand, and agents' subjective and intersubjective understanding of them, on the other, mutually constitute both the world and its experienced forms. Knowledge of the socially constituted world is socially mediated and open-ended' (p. 50).

The *social perspective* of situated cognition/action implies a farming practice being shaped by values and commitment underlying 'Policies' in the Management System in Fig. 1. To again paraphrase Clancey (1997, p. 24), the *social* view emphasises an individual's *intentional, purposeful* orientation with respect to the activities in which he is *being* a farmer. Further, from the standpoint of participation in a community of practice, a practice is 'a complex choreography of role, involving a sense of place, and a social identity, which conceptually regulates behaviour' (Clancy 1997, p. 24).

Terry Winograd, a pioneer in artificial intelligence applications (e.g. Winograd 1973), highlighted an incongruence between intelligent decision computer programs and decision practice that is situated in the world:

> '[Simon's] bounded rationality approach does not assume that a decision maker can evaluate all alternatives, but it takes for granted a well-defined problem space in which they are located. It is not clear for what observer this space of alternatives exists. In describing the behaviour of a manager we (as observers) can formalise the situation by describing it as a set of alternatives with associated properties. In doing so we impose our own pre-understanding to create distinct alternatives out of the full situation. In order to write a computer program we are forced to do this kind of analysis *a priori*' (Winograd and Flores 1986, p. 146).

For Winograd and Flores (1986), the paradigm shift that corrects the incongruence and offers new prospects for intervention is the notion of using computers to aid managers in the situated practice of thinking about potentially relevant new possibilities in the wider world—to facilitate *discovery learning*:

> '...the most essential responsibilities of managers...can be characterised as participation in "conversations for possibilities" that opens new backgrounds for the conversations for action. The key aspect of conversations for possibilities is the asking of the questions "What is it possible to do?" and "What will be the domain of actions in which we will engage?" This requires a continuing reinterpretation of past activity...—interpretations that carry a pre-orientation to new possibilities for the future' (p. 151).

The idea that it might be possible to reinvent a Farm Management Research featuring intervention in world 2/1 practice using simulation models to facilitate such discovery learning about possibilities and actions aligns with recent research outcomes using this intervention paradigm (McCown *et al*. 1998). A number of significant changes in the climate for agricultural research contribute an urgency to such a 'reinvention' project: (*1*) a pragmatic change in the philosophical climate that condones, even encourages, experimenting with mixing methods from 'contending' paradigms in research on management (e.g. Mingers 1997), and (*2*) a research funding climate that rewards professional practice that genuinely delivers value to practitioners (Gibbons *et al.* 1994)

General discussion

My analysis of the nature of the 'gap' and interpretation of the different historical efforts in bridging it have relied heavily Popper's 'three worlds' model. This is risky on two counts. To many in the earthy domain of agricultural science, both the three worlds construct and some of the associated philosophical concepts and language may be unfamiliar, and the entire treatment may seem esoteric. On the other hand, those for whom a philosophical perspective on theory-practice matters is important may disapprove of such an out-of-fashion philosophical framework. My commitment to this route has been motivated by the belief that as a profession challenged by unprecedented changes in its environment, we have an unprecedented need to see our 'specialties' in a perspective that can make sense of the changes so that we can respond in ways that are more 'rational' than they might be otherwise. In the sense of the metaphor of Sellars (1963), introduced on p. 555 above, I believe that Fig. 3 does aid the 'knowing our way around' the decision support countryside beyond the bailiwick of our specialities and in discovering pitfalls generally overlooked. But Sellars himself was conscious of the perils of too much philosophising:

> 'It is…the "eye on the whole" which distinguishes the philosophical enterprise. [ ] To the extent that a specialist is…concerned to reflect on how his work as a specialist joins up with other intellectual pursuits…he is said…to be philosophically minded. And indeed, one can "have one's eye on the whole" without staring at it all the time. The latter would be a fruitless enterprise' (p. 3).

Those inclined to be critical of the three worlds model on philosophical grounds are in the excellent company of such eminent critics as Habermas (1984), Ulrich (1983), and Ryle (1949). Significantly, however, these authors did not dismiss this framework lightly, and the limitations that they emphasised have been reduced, I believe, by the introduction of multiple world 2 'communities' in the adaptation of the concept in Fig. 3. But if I am wrong, I have the safety net provided by Ryle, who after calling the multiple worlds model which underlies Fig. 3 a 'philosopher's myth' made the following concessions:

> 'A myth is, of course, not a fairy story. It is the presentation of facts belonging to one category in the idioms appropriate to another. To explode a myth is accordingly not to deny the facts but to reallocate them. [ ] It would... not be true to say that the two-world's myth [of Decartes, and which Popper made a three-worlds myth] did no theoretical good. Myths often do a lot of good, while they are still new' (Ryle 1949, p. 10,24).

The importance of 'newness' presumably has less to do with age of the concept itself than to the new gain in sense made with it by its new holders, relative to the concept replaced. If Fig. 3 can help us clarify old problems and new possibilities regarding science-based decision support for farmers, it need do no more.

In the midst of a revolution in communications technology said to be comparable to the invention of mechanical printing, it seems obvious to many that Information Systems (personal computing/telecommunications) will become increasingly important, and that it is only a matter of time until the computers that are becoming clerical tools for many farmers will also become integral to their planning decision making. But, by my interpretation of the history and logic of model-based research and intervention, the trend does not support this intuition of technological determinism. Instead, the trend that is described in this paper is in quite the opposite direction. Computer models were initially part of a 'hard' concept of using theory about the external world of managers to determine, with little involvement of the manager, what a rational manager 'ought to do'. Cognitive science brought a marginally 'softer' notion of recognition of the realities of a manager's *internal* world and of the demonstrably successful 'artful' ways that good managers cope with the external world. But neither the Expert System, which captured expert managers' procedural 'rules' for action, or the Decision Support System with imbedded substantive models to augment subjective, situated, judgment making proved meaningful in the situated practice of farm management. More recently, action research has shown that models that simulate important aspects of farming can come to be valued by farmers (Hochman *et al.* 2000; Lloyd 2000). This mode of intervention is *situated* in two key respects: (*1*) models are specified for local conditions using local data, and (*2*) and models are used in discussions between farmers and professionals to facilitate exploration of possibilities for change. Thus, the trend has been from the 'hard' systems approach that used models to design best practice to a 'soft' systems approach in which models aid dialogue about farmers' felt problems and possibilities for improved practice in actual management situations.

An alternative interpretation of this trend away from reliance on the technical rationality of traditional modeling is that it is simply the inevitable correction that accompanied disillusionment with the Technological Program (Schon 1983) and the ideology of 'progressive' government (Maxwell and Randall 1989). By this view, the successive shifts in concepts toward 'softer' modes of intervention are stages in painful realisation by scientists that formal models, although sometimes valuable in *research* practice, simply are not appropriate to the physically and socially situated practice of farming. By this interpretation, what is needed in the future is a form of engagement between communities of farming practice and other communities, including communities of systems practice, where *communicative* rationality (Habermas 1984, p. 10) is not distorted by scientists claiming a disproportionate influence due to special knowledge embodied in scientific models for aiding farmers' management.

The history of 'the gap' between scientific models and practice provides some empirical justification for the latter stance: practitioners have rarely judged models to be relevant to their practice, and modellers' shortfalls in achieving relevance to world 2/1 practice have often been attributed to over-concern with rigour in describing world 1 mechanism. But change has been taking place. The growing inclusion of 'action research' and 'farmer participation' in discussion of agricultural research reflects a new level of recognition that *effective* systems research is *meaningful* to farmers in their situated practice. The nature and importance of this change is such that juxtaposition of the criterion of *relevance* to rigour becomes secondary to its juxtaposition to s*ignificance,* as the two elements that constitute *meaning* for practitioners of theory for their practice (Dreyfus 1994, p. 261).

Among research types in Table 2, Descriptive and Nomothetic research have no meaning other than in making objective reality in world 1 intelligible. In contrast, Policy research, as 'the production of knowledge that guides practice,' is meaningful in its intent to be significant to the efforts of purposeful practitioners. The structuring of world 2 *scientific* activity (right side of Fig. 3) by the values of *relevance* and *significance* is evident in formal Decision Analysis. Possible solutions to a problem are identified as relevant, and analysis singles out one of these as the optimal, or most significant, option. But *meaning* in the 'real world' only comes from application of such 'theoretical practices' to situated practice. The problem this constitutes is captured by Dreyfus 1991:

> 'The phenomenal account of how scientific facts are arrived at by leaving out [world 2] significance shows why once we have stripped away all meaningful context to get the elements of [world 3] theory, theory cannot give back [world 2] meaning. Science cannot reconstruct what has been left out in arriving at theory; it cannot explain significance [to situated practice]. For this reason, even though natural science can explain the *causal basis* of the referential whole [i.e. the system], " 'nature' can never make *worldliness* [i.e. situated practice] intelligible" ' (p. 121, my interjections).

Designed (world 3) decision problems are treated according to objective weightings of relevance and significance of context-free objects and states assigned at the time of design. By definition, an individual's world 2 is inhabited by entities that are relevant and significant to situated practice, but relevance and significance of objects and states are variables that are dynamically influenced by practice context (Dreyfus 1994, Ch. 8). In action research, dialog about management, including potential contribution of models, becomes part of this practice context. In contrast to designed decision support this makes it possible to address *open-structured* problems where what is relevant or potentially relevant is initially unclear (Dreyfus 1996, p. 257). This provides the opportunity for farmers and scientific professionals to 'negotiate' relevance with regard to possible intervention, and particularly, the relevance to actual farming problems and actions of simulations of alternative actions and their consequences. In a recent program of action research, initially skeptical farmers came to value the contribution of such situated simulations to discussions about management plans and decisions (Hochman *et al.* 2000).

A shift in systems research from a *design* paradigm to one featuring *dialogue* in order to construct relevance to practice seems to be a necessary condition for bridging 'the gap' of the title of this paper. But farmers' expectations for discussions about farm management go beyond models mimicking *relevant* things to models making a difference to the Management System (Fig. 2) that is *significant.* Such awe-inspiring rules of engagement provide special incentive for having models that are demonstrably up to the job. But, as Dreyfus (1991) pointed out (above quote), relevance and significance to situated practice has been sacrificed in model making in order to abstract transparent, general structure. Simulations of actual situations can at best achieve realistic scenarios and certain types of highlights of situated reality. The potential for relevance and significance to world 2 situated management practice lies in the relative value of such impoverished descriptions when they constitute the *richest description available* to the uncertain manager facing turbulent or novel change. The potential for an intervention to be seen as relevant and significant depends fundamentally on the reliability with which models of system structure, specified for local conditions, can simulate likely consequences determined by (*a*) uncertain events in the world 1 environment of practices, or by (*b*) implementations of innovations in world 2 practice policies that go beyond the manager's experience. Fortunately, important advances have continued to be made in describing and modeling biophysical processes in agricultural production systems, providing new capability for simulating the both the occurrence of problems and the consequences of various actions to alleviate them (e.g. Keating *et al.* 1999).

Since the 1960s, there has existed a community of scientists who believed that appropriate simulation models had the potential to be important to farm management, but their confidence has been severely tested by the long wait for confirmation. News that the perceived potential can be achieved but only when models are used in a paradigm that differs radically from normal science practice can be expected to be met with some consternation and resistance—and will, mistakenly, be labeled '*extension*'. The *research* paradigm that features theoretical design of practices has had strong appeal to modelers and their institutions: it is a natural application of core scientific capabilities that serves as a kind of 'value adding'; its application to the human activity of decision making has not required special social science expertise; and rewards have not depended upon being relevant and significant in farming practice but, rather, on logical relevance and theoretical significance to a general objective problem. Gap-bridging research paradigm shifts undertaken by individual scientists might be expected to be difficult and rare. After proclaiming the arrival of a new multi-paradigmatic era in Management Science, Brocklesby (1997) examined the individual and professional difficulties of moving from a hard to a soft systems paradigm of management intervention. He found the problems of a shift in this direction considerable, but not insurmountable; the shift of individuals in the opposite direction seems to be precluded by problems of cost-efficient acquisition of the requisite skills.

In conclusion, even if the case for a new paradigm of model-based farm management intervention proved to be sound and convincing, its achievement in Australia would seem to be determined largely by two dynamic forces. One is the influence of research policy and funding bodies on the shape of research. Recent trends in funding by agricultural R&D Corporations for 'systems' research has been away from designed decision support and towards on-farm participatory research. The second force is rooted in the emergence of natural resources issues as the most pressing in farm management. Such problems generally create additional challenges to management research in its struggle to be relevant and significant to practice. They characteristically have temporal and spatial aspects that severely limit the efficacy of experimental research methodology and make systems analysis using simulation an attractive alternative. But evaluation of simulated consequences of alternative actions is complicated by plurality of stakeholder interests and values.

In a brave new world of systems practice, instead of primarily using their models to design 'best practice' or decision support systems for managers, scientists may be invited by farmers and other land use stakeholders to bring their simulators of aspects of farming and off-farm impacts into projects to help explore multiple consequences of possible farming actions/strategies and discuss the plural significances. Such a cooperative learning project represents a new mode of systems research that offers new prospects for bridging gaps between theory and practice.

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Appendix I. Glossary of knowledge-action terms

Action

Purposeful, willed, behavior.
'Action is intentional modification of a given reality' (Oquist 1978, p. 144).

Agency

The capacity to translate human intention into action that causes the desired event. 'Typically when I reason from my desires and beliefs as to what I should do, there is a gap between the causes of my decision in the form of beliefs and desires and the actual decision, and there is another gap between the decision and the performance of the action. The reason for these gaps is that the intentionalistic causes of behaviour are not sufficient to determine the behavior' (Searle 1998, p. 107). In other words, 'If wishes were horses, all beggars would ride' (Simon 1996, p. 12). *Agency* is the missing ingredient.

Cybernetic systems

Cybernetics is the science of communications and automatic control systems in both machines and living things. Cybernetic systems feature control governed by comparison of existing and desired states and 'feedback' of information on existing states between actions.

Epistemology

The aspect of philosophy concerning the theory of knowledge, especially with regard to its methods, validity, and scope. Epistemology is the investigation of what distinguishes justified belief from opinion (New Oxford Dictionary). *An* epistemology is a theory of knowledge—a view and a justification that can be criticised and thereby can be regarded as world 3 objective knowledge rather than world 2 subjective beliefs. (Blaikie 1993, p. 6).

Heuristic

Pertaining to *procedure* for learning and problem solving in world 2 social practice. A heuristic procedure reflects the ability to discover/invent procedures, beginning with interpretation of the structure of the problem, to obtain a reasonable solution. A 'heuristic' can be communicated as a 'rule of thumb.' Development of methods in the field of artificial intelligence began by formalizing in world 3 successful world 2 heuristic procedures. (After Silver *et al.* 1980, p. 153)

Ontology

The field of study of 'being', or 'reality'. Ontology refers to the claims or assumptions about what exists, what it looks like, what units make it up, and how these units interact (Blaikie 1993, p. 6).

Normative

This term is used to refer to quite different, but related things. Fig. 3 helps clarify this.

'Normative' in world 2 situated practice: 'pertaining to value norms or normative values.' Any purposeful activity is value-oriented. Laszlo (1996) emphasises that although it has been fashionable to reduce values to nothing more than 'likes or dislikes' in world 2, any 'systems view' recognises that deep values correspond to world 1 realities. As an individual, you must keep yourself 'running' against the inevitability of physical decay of all things, repair yourself, and it may be important to reproduce yourself. These are values common to all natural systems, and 'no system is free to reject these values for very long'. Laszlo also emphasises that *cultures* have values—whole hierarchies of them, but that a value *norm* does not exist separately from the variable expressions of the value in the behaviour of individuals in the culture. Hence, normative values are not described, they are postulated by 'outsiders' and (variably) felt by insiders as determinants of what they *ought* to do. (See Laszlo 1996, pp. 78–88).

Normative (in world 3): Research to produce knowledge for guiding action, i.e. policy research, can be normative or descriptive. Descriptive, or positive, research describes world 2 social practice in order to identify procedures for modifying world 1 which excel in achieving stated world 2 goals. In contrast, normative research uses theory about world 1 to identify what a practitioner with a stated world 2 goal ought to do to achieve the outcome that achieves the goal. There is a clear premium in normative policy research for identifying an optimal action. Whereas cultural normativism largely concerns goals, or ends, theoretical normativism is solely about efficient means to achieve given ends. This literal distinction has often been blurred in practice as indicated in the quote from Encyclopaedia Britannica on p. 558 above.

Policy (p. 552)

The intelligent structure that regulates desire, will, intent, etc., resulting in the consistency of action that characterises 'practice'. This structure includes 'Needs and interests, values and norms, ends and objectives, plans and programs, operations and evaluations, and resources related to a given action or potential action' (Oquist 1978, p. 145).

Practice

'Policy and action in the context of determinate structures and processes' (Oquist, 1978, p. 145).

Situated

Clancey (1997, p. 23) sets out three distinct meanings in relation to describing complex systems, two of which are relevant here: (*1*) *functional* in a culture (a choreographed activity, conceived as a social process of identity construction and enactment) and (*2*) *behavioural* in relation to specific time and place in world 1.

Teleological

Explanation of system function in terms of 'purpose'. Not admissible in scientific explanation of the given world of nature, but essential to analysis of man-made systems.

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