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Making Decisions in the Absence of Clear Facts:
A Historical Perspective

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Abstract

Recently, a claim has been made that executive decisions makers, far from being fact collectors, are actually fact users and integrators. As such, the claim continues, executive decision makers need help in understanding how to interpret facts, as well as guidance in making decisions in the absence of clear facts. This report justifies this claim against the backdrop of the modern history of decision making, from 1956 to the present. The historical excursion serves to amplify and clarify the claim, as well as to develop a theoretical framework for making decisions in the absence of clear facts. Two essential issues are identified that impact upon decision making in the absence of clear facts, as well criteria for decision making effectiveness under such circumstances. Methodological requirements and practical objectives round off the theoretical framework. The historical analysis enables the identification of one particular established methodology as claiming to meet the methodological requirements of the theoretical framework. Since the methodology has yet to be tried on information-poor situations, a further project is proposed that will test its validity.

Introduction – Opening Pandora’s Box

In 2005, Bennis and O’Toole (2005a) published a polemical paper in the *Harvard Business Review* entitled How Business Schools Lost Their Way. The paper described what they saw as significant problems in business schools, ranging from the hiring of appropriate professors to the manner in which students are prepared (or, rather, not prepared) ‘to wrangle with complex, unquantifiable issues – in other words, the stuff of management.’ Their concern with students’ preparedness focused primarily upon the manner in which students are taught decision making. They explicitly stated that, no matter the importance and relevance of the decision support offered by quantitative approaches, decision makers must be trained to deal with:

1. the management of judgments;
2. messy, incomplete and incoherent data;
3. strategic decisions;
4. ethics and morality;
5. the temptation to resort to past behavior or conditioned reflexes when faced with new challenges;
6. the application of rigorous imagination;
7. multidisciplinary;
8. subjective analysis of multifaceted questions of policy and strategy;
9. a mixture of wisdom and experience;
10. the challenge of becoming generalists;
11. the challenge of seeing connections;
12. the indirect and long-term implications of complex decisions;
13. the broader and softer areas of business;
14. interpersonal skills;
15. the teachings of the humanities;
16. pluralism; and,
17. the linking of hard and soft skills and approaches.

In what may be termed an *educational imperative*, the authors provided a neat summary of what is at stake:

Executive decision makers are not fact collectors; they are fact users and integrators. Thus, what they need from educators is help in understanding how to interpret facts and guidance from experienced teachers in *making decisions in the absence of clear facts*. (italics added)

The *Harvard Business Review* received numerous letters to the editor commenting on Bennis and O’Toole’s paper (Leavitt and Frascetti, 2005; Peters, 2005; McGrath, 2005; Fernandes, 2005; Bolton, 2005; Kelly, 2005). Bennis and O’Toole (2005b) responded. None of this correspondence addressed the educational imperative, focusing instead on other issues raised by Bennis and O’Toole.

A couple of years later, Tichy and Bennis (2007) published a paper on decision making, again in the *Harvard Business Review*. This paper explicitly renders synonymous ‘the moment of decision’ with ‘a judgment call’. It is argued that making a decision should be viewed less as an event, and more as a process which allows ‘redo loops’. An exhibit in the paper purports to characterize the distinction between decision making as event (what is termed the ‘traditional’ view) and decision

making as process (the ‘process’ view). According to the exhibit, what constitutes success in the process view is ‘acting and reacting through a judgment process that guides others to a successful outcome’. What constitutes success in the traditional view is ‘making the best decision on the basis of known data’. This latter criterion of success is the *only* allusion, albeit indirect, to the educational imperative in the entire paper. In fact, it is the only allusion to the educational imperative in the entire *Harvard Business Review* since Bennis and O’Toole’s paper of 2005. Moreover, Tichy and Bennis do not conclude that the process view is the way to address the challenge of the educational imperative. The educational imperative has been all but ignored.

Scholarly responses to the educational imperative were actually swift, but they were published elsewhere (Georgiou, 2006, 2008). They provided a direct theoretical and practical answer from the field of systems science (Checkland, 1981). A methodology was detailed not only for making decisions in the absence of clear facts, but for effectively addressing the seventeen points listed above. Given this, one may conclude that the manner in which Bennis and O’Toole’s educational imperative can be addressed has thus been demonstrated.

When Bennis and O’Toole’s paper was published in 2005, however, the *Harvard Business Review* branded it - correctly - as an assertion (Harvard Business Review, 2005). That is to say, the paper was appreciated as an opinion piece. Now, there is nothing wrong with that. Indeed, the paper, although polemical, makes no knowledge claims strictly speaking. At best, the paper may be said to be putting forth some correct opinions. Opinions, however, even where they might be deemed right,

are a fine thing and do all sorts of good so long as they stay in their place; but they will not stay long. They run away from a man’s mind, so they are not worth much until you tether them by working out the reason. [...] Once they are tied down, they become knowledge, and are stable. That is why knowledge is something more valuable than right opinion. What distinguishes one from the other is the tether. (Plato, *The Meno*)

The point is, if one is to put forth an educational imperative because of perceived faults, then one had better provide good reasons as to why decision makers should attend to this imperative. Unsubstantiated claims are the life-blood of the serial fads that insult the intelligence of management thinkers and practitioners (Jackson, 1995). What exactly is a situation that may be said to be constituted by an absence of clear facts? How can it be identified? What skills are required to deal with it, and how can decision makers develop these skills? What other factors, that make further demands on decision makers, impact upon such a situation? What form do decisions take under such conditions? How long have such situations been around? Does the educational imperative point to something new, to something transient, or to something that is of the very essence of decision making? Are human beings neuro-biologically condemned to make decisions in the absence of clear facts? In other words, is it the world that renders the educational imperative relevant, or is it our own biological make-up? If it is the world, can we change it somehow and so alleviate ourselves of this challenge? If it is us, do we dare to genetically ‘upgrade’ ourselves through the latest neuro-biological technologies in the hope that they can help our decision making capabilities? More humbly, are approaches available for supporting and assisting the making of decisions in the absence of clear facts? What is the state of

play of these approaches? How do they help; what do they necessarily leave out; and, what do they teach us about making decisions in the absence of clear facts?

Suddenly, a brief claim from a paper in a management journal unwraps a Pandora's box of hard questions. In this report, we shall concern ourselves with a history of modern decision making in order to justify the relevance of Bennis and O'Toole's educational imperative. This historical research will yield a theoretical framework from which the practice of effective decision making in the absence of clear facts may be realized. A subsequent research project will seek to demonstrate the practice of such highly constrained decision making.

Chapter One – Seven and the Sausage Machine

In 1956, cognitive psychology condemned the human race to make decisions in the absence of clear facts. In that year, renowned Harvard psychology professor George A. Miller (1956) published a seminal paper with the somewhat ominous title The magical number seven, plus or minus two: some limits on our capacity for processing information. The title seems to say it all: human beings are limited in the amount of information they can process at any one time, the limit being around seven items. Decisions requiring the processing of more items, therefore, are neuro-biologically condemned to be decisions made in the absence of clear facts.

This paper is one of the most cited in the cognitive science literature. It therefore merits a closer look to ensure the accuracy of the above understanding. In the first part of the paper, Miller examines the experimental results of various psychologists researching the mind's ability to classify phenomena. Each one of these experiments involved alternative exposures to a single type of stimulus, the objective being to classify the alternatives according to some linear scale. For instance, sound would be classified on a scale of soft-to-loud, tones on a scale of low-to-high, and taste intensity on a scale of salt concentration. Visual stimuli would be classified on a positional scale, or a scale that classified the stimulus' size. Color would be classified on scales of hue and brightness. An experiment involving the placing of a vibrator in the chest region required classifications of intensities, durations and locations, all on linear scales. Another experiment required the categorization of curvature (length of an arc and of a chord), of length and of angle of inclination of lines.

Results from all these experiments averaged 6.5 manageable categories, with a lower limit of 3 (for curvature classifications) and an upper limit of 15 (for positions in an interval). What is notable is that all of the experiments cited concerned tactile faculties such as perception, touch, and hearing, and that all stimuli were sensory. Moreover, all stimuli were unidimensional. That is to say, taste was measured using only salt, the sound projected would be of the same timbre, the variation lying only in its intensity, and so on and so forth. In other words, the type of decision making required in these instances was limited to classifying simple, unitary sensory phenomena.

At this point, Miller tentatively concluded that there appears to be either a conditioned (i.e. learnt) or a neuro-biological limitation that maintains the classificatory abilities of human beings within a range of 3 to 15 categories. In his words, and based on the evidence thus far, 'it seems safe to say that we possess a finite and rather small capacity for making such unidimensional judgments and that this capacity does not vary a great deal from one simple sensory attribute to another.' The key words that delimit the significance of the results are *unidimensional*, and *simple sensory attribute*. In other words, the evidence concerns experiments that could be done on lower primates with a justifiable expectation of certain positive results. That the human range of 3-15 categories is in any way a demeaning limit is an open question, for the experiments cited are a far cry from the multidimensional, simultaneous and complex decision making that underlies the day-to-day activities of human beings.

Miller, of course, recognized that human beings can accurately identify any one of several hundred variables (such as, for example, faces, words, and objects). The question was why the laboratory results differed so much from such experience. He hypothesized that the difference lay in that such variables exhibit a great number of independent attributes, whereas the unidimensional stimuli of the laboratory each differed by just a single attribute (e.g. salt concentration, brightness, and so forth). Objects, such as tables, come in different forms, as do words (think of accents), and, of course, faces. Does interaction with multiattribute variables stimulate a greater cognitive classification ability because of the greater number of independent attributes involved?

Miller begins cautiously, by examining a two-dimensional experiment: the location of a dot in a square. This requires vertical as well as horizontal classification, which are merely two judgments of the same interval type. A previous unidimensional experiment of classifying the location of a dot on a horizontal interval had yielded between 9 and 10 manageable categories. Based upon a novel statistical relation regarding the amount of information required to make a decision between two equally likely alternatives, Miller hypothesized that the addition of a vertical scale for classifying the two dimensional position of a dot in a square would, due to its being of the same interval type, yield between 90 and 91 manageable categories. This hypothesis assumes that (1) either the human brain deals no differently with horizontal or with vertical positioning, or (2) that it can juxtapose the two in a linearly, and proportionally additive fashion that respectively extends the amount of information involved in making the decision, or (3) both. As it happened, the introduction of a vertical scale increased the number of manageable categories to only 24-25. In other words, the experiment showed that the human brain can accurately identify only up to 24-25 positions in the square – a result far short of that expected. Similar results were reported with dual salt-sweet taste intensities, with dual loudness-pitch stimuli, and with dual hue-saturation color experiments. In all these cases, the introduction of a second dimension increased the number of mentally manageable classificatory categories, but by an amount far less than a linearly additive expectation¹.

Following this, Miller searched around for a truly multidimensional experiment that could reflect real human experience with multiattribute variables. He found only one, an auditory study that could set tones according to six different acoustic variables, providing 15,625 possible tones as stimuli. Each of the six dimensions was rated separately by each listener. Under these conditions, 147 categories could be mentally managed. So, an increase in the number of attributes appeared to increase the number of mentally manageable classificatory categories.

Miller concluded with a viable argument based on the data: as the number of independent attributes increases, the mind's capacity for classification increases, but at a diminishing rate. There is, in other words, an asymptotic relationship between the number of inputs and the mind's capacity to classify them, with the latter extending to some limit. Miller does not have a reasonable amount of experimental data that could

¹ The linearly additive expectation referred to here arises from Miller's use of logarithmic computations.

shed some light as to the asymptotic limit of this relationship. Moreover, and in keeping with a question asked earlier in the Introduction, he further notes that it would be of significant interest to discover whether this limit depends upon the attributes or whether it depends upon the mind's inherent capacity to classify them. He does note, however, that, with the introduction of new attributes, the asymptotic increase in mental classification capacity means that classificatory accuracy decreases for any particular attribute. The message seems to be, in his words, that human beings 'can make relatively crude judgments of several things simultaneously.' Miller hypothesizes that this result fits evolutionary theory: organisms that know a little about lots of things, instead of a lot about a small number of things, can respond to the widest range of stimuli in a dynamic environment, rendering them more adaptive and, therefore, giving them relatively higher chances of survival².

In brief, the experiments show that accuracy of judgment per variable decreases as more attributes are introduced. And Miller is at pains to point out that the experiments do not demonstrate that people can judge only one attribute at a time. What they do show is that people are less accurate per attribute if they have to judge more than one attribute simultaneously. As mentioned earlier, finding the limit of this relationship is a significant research question.

While Miller, however, spells out these results about multidimensionality, he is less clear on what the initial unidimensional experiments actually show. He provides three interpretations, all three of which appear immediately after his discussion of the very first experiment to be featured in his paper. Given the context of the paper as a whole, they may be understood in this way.

1. A human being can process, or classify, about seven different unidimensional sensory inputs without risking their confusion. This implies that if more than about seven inputs are presented, the human being begins to confuse them and thus classify them erroneously.
2. Irrespective of the number of unidimensional sensory inputs, a human being can accurately assign them to about seven classes.
3. 'If we know that there were N alternative stimuli, then his judgment enables us to narrow down the particular stimulus to one out of $N/6$ (*sic*).'

Of these, the third is the most cryptic, but consider them in order.

The first interpretation says that, in the context of accurately classifying inputs, there is a maximum number that may be given, beyond which the human being begins to get confused. This interpretation clearly sets a limit on the number of inputs given, if the objective is to obtain an accurate classificatory response.

On this basis, the second interpretation contradicts the first, for it says that, no matter the number of inputs presented, the human being can accurately assign them to about seven classes. Quantity of given input, in other words, in no way influences accuracy of classification. Where the number seven was first attributed to the quantity of the

² The hypothesis is supported by phenomenology and systems science, both of which argue that human beings epistemologically engage with the world with the concepts at hand, and refine these concepts as discontinuities occur (Georgiou, 2007).

given input, it is now attributed to the quantity of available classes. As a result, where, before, classificatory accuracy was a function of the quantity of inputs, now it is a function of the number of classes. More than that, however, classificatory accuracy seems to depend on the given inputs being classifiable into one of only these seven classes. And yet, the second option is silent on whether the inputs are actually chosen so as to be classifiable into only those limited classes.

The third interpretation has been given in Miller's own words. As can be seen, the number 6 features prominently instead of the usual 7. This reflects the result of the first experiment discussed by Miller, immediately after which all three interpretations are given. In the context of the entire paper, this number should probably be 7. In the interest of maintaining a degree of coherence, therefore, the number 7 will be used in the ensuing discussion.

The third option begins by referring to the 'N alternative stimuli'. Given that, at this point in the paper, multidimensional stimuli have yet to be addressed, the 'N' can only refer to the number of inputs presented at any one iteration of the experiment (as opposed to its possibly referring to the number of attributes characteristic of the inputs, as in multidimensional experiments). This is indeed confirmed by the text thus far as whole. Further on from this, this third interpretation refers to the human being's 'judgment'. The only thing discussed thus far that comes anywhere near being understood as a 'judgment' is the classificatory attempt of the mind. Finally, we are referred to 'N/7' ('N/6' in the citation). The only thing to which this can conceivably refer is the number of classes. So we can paraphrase as follows: If we know that there were N inputs, then his classification enables us to narrow down the particular input to one out of N/7 classes. Try this out with a few numbers:

- If we know that there was 1 input, then his classification enables us to narrow down the particular input to one out of 1/7 classes.
- If we know that there were 7 inputs, then his classification enables us to narrow down the particular input to one out of 1 class.
- If we know that there were 21 inputs, then his classification enables us to narrow down the particular input to one out of 3 classes.
- If we know that there were 42 inputs, then his classification enables us to narrow down the particular input to one out of 7 classes.
- If we know that there were 70 inputs, then his classification enables us to narrow down the particular input to one out of 10 classes.

It is obvious that, no matter what numbers are used, the phrase is ambiguous. In general terms, what is meant by the assertion that, once we know the subject classification, we can narrow down the particular input to one out of so many classes? At most we can infer the following: Option 3 is saying that as we increase the number of inputs, the number of classes increases. This, however, contradicts the second option. And by allowing an increase in the number of inputs, Option 3 contradicts the first option.

As a result we are faced with some questions. Does classificatory accuracy depend on the number of inputs (option 1)? Does classificatory accuracy depend on a given number of classes, probably with all inputs classifiable only within these classes (option 2)? Or does classificatory accuracy evolve as the number of inputs changes (option 3)? The third question would only make sense in the context of the multidimensional experiments, but even then the $N/7$ function yields a linear, not an asymptotic, relationship. To undertake an experiment as per the second option would be like purposely holding up a tautology as a mathematical or philosophical proof. Indeed, only the first option comes anywhere near the spirit and content of Miller's argument. As a result, we can conclude only the following: in unidimensional situations, and unrestricted by time, the human being can accurately process only about seven different sensory inputs – a conclusion noted earlier as being far too delimiting to inform the multidimensional, simultaneous and complex decision making of regular human experience.

Overall then, Miller's thesis is as follows. First, the human being can accurately process only about seven unidimensional inputs. This, as noted, is a trivial result in the face of human experience. Second, as the number of simultaneous attributes per input is increased, the human being's classification of these inputs becomes less accurate. This confirms what we know from experience: as the number of attributes we have to deal with increases, our capacity for accurately dealing with them decreases.

Following his excursion to multiattribute variables, Miller then returns to the issue of unidimensional inputs, specifically to see how human beings can (and do) increase the number of unidimensional inputs they can accurately classify at any one time. He notes that there are three techniques available that allow us to get around the 7 ± 2 limit. The first concerns making relative judgments instead of absolute judgments. In this case, for instance, instead of attempting to classify an input accurately on a scale, one merely compares it to any one of the other inputs in order to conclude whether the input in question is greater, smaller, brighter etc etc. Miller does not elaborate on how exactly this technique expands the range of the mind's capacity. The second technique concerns increasing the number of dimensions along which inputs can differ. This refers to the multidimensional experiments already discussed. The third technique concerns arranging the classification task so that the subject can make a sequence of several absolute judgments in a row. Miller now focuses on this alternative.

Early on he notes that this is analogical to mnemonic processes. Music theory, for example, teaches us to remember *every good boy deserves food* for the notes that fall on each of the five lines of the treble staff. In this case, instead of having to remember five letters/notes, we remember one phrase, and thus reduce the amount of input we have to deal with at any one time. Whether this mnemonic is actually useful to music students is not relevant for the present purposes. Here we are merely concerned with the amount of information we have to process at any one time, and there is clearly a reduction in the quantity of this information - a reduction from five letters/notes to one simple and easily remembered phrase. The role of the mnemonic is as an *aide-memoire*. And, indeed, it is with immediate memory that Miller is now concerned.

Miller notes that, thus far, the case of the span of absolute judgment reveals an ability to distinguish about seven categories. Miller then notes that ‘everybody knows’ that the finite span of immediate memory is about seven variables. So, he says, there is nothing more natural than to conclude that both spans are merely different aspects of a single underlying process. This, for Miller, is a fundamentally mistaken conclusion. He argues, with experimental data, that the span of absolute judgment is limited by the amount of information: unidimensional information reduces us to coping with about seven items at any one time, whilst multidimensional information affords an asymptotically increasing rate. Immediate memory is limited by the number of inputs. Thus, in the music example, we have reduced the number of inputs from five individual bits to one chunk – one grouping of items. Miller notes that experiments show that the span of immediate memory depends upon the number of chunks or groupings we make of raw data, not on the quantity of this data itself. In other words, if the span of immediate memory is seven variables (as ‘everybody knows’), these variables can be groupings, each of which can contain any number of individual bits of input of the same type. Miller calls the process of grouping *recoding*. He shows that by recoding inputs, in other words by grouping them, we can increase the amount of inputs we can deal with at any one time. He does not explicitly infer from his experimental data, however, whether the grouping span of immediate memory is around seven. For that, we are referred to his earlier remark about what ‘everybody knows.’

In brief, absolute judgment is limited to seven individual inputs, and immediate memory is limited to seven groupings. In the case of the latter, each group is constituted by an unstipulated number of inputs of the same type. We do not know, in other words, what limit of individual bits in each group can actually be recalled. At best we can say that Miller’s thesis indicates that we can deal with seven units of data at any one time, be they seven individual pieces of data (for the case of absolute judgements) or seven groups of data (for the case of immediate memory). Miller is cautious, and concludes that whether this number is actually seven or not is still an open question.

Indeed, caution is required in drawing conclusions about the mind or about decision making from Miller’s influential paper. The reasons are as follows.

- Only the mind’s classificatory abilities are addressed, and these within a highly delimited range of classification types, resulting in a limited view of the mind’s processing capabilities.
- The thesis mainly concerns classifying unidimensional phenomena. Those multidimensional phenomena that are considered are characterized by up to only six attributes.
- All phenomena considered are simple, sensory phenomena, a significant delimitation in that it reduces the sphere of influence of the thesis to stimulus-response mechanisms.

- The thesis is based on controlled laboratory work. In order to draw any conclusions regarding the mind's capabilities in the flux of the reality of the world, that is, in the flux of real decision making, experiments richer in detail are required³.

Perhaps a more significant reason to be cautious lies in the mathematics upon which Miller relied for his thesis. The statistical relation that Miller used, regarding the amount of information required to make a decision, is based on the information theory developed by Shannon and Weaver (1949). Sperling (1988) notes that it was, for Miller, the only systematic framework available for dealing with information. He adds that, although it is relevant for absolute judgments, it is not so useful in the case of immediate recall. Besides this, however, Miller makes it clear that the statistical computations are useful only for judgments between two equally likely alternatives. What about the case of two or more unequally likely alternatives? Or, more generally, what about the case of existential experience wherein human beings are not often faced with neat 50-50 alternatives? Shannon and Weaver do discuss the mathematical case of multiple, unequally likely alternatives. But their mathematical edifice cannot account for the all-too-human evidence that we sometimes choose the least probable alternative, or even the one that promises the least success. In a word, Shannon and Weaver cannot account for the motivation behind human choices.

Boulding (1956: 153-155), writing in the same year as Miller, takes this issue one step further. On the one hand, he hails 'the development of a mathematical concept of information' as being of equal importance to 'the development of mass and energy in physics', for 'it has opened up the possibility of a new and more quantitative approach to the whole problem of organization.' He continues, however, by asking whether the information concept of Shannon is anything more than a mere statistic, no more insightful than, say, a standard deviation - 'a convenient statistic for use in certain problems in solving the communication of messages over limited channels'. Boulding questions whether the theory can get beyond the pings of a message and explain its more impalpable dimensions, such as its semantic qualities. Since such qualities bear significantly upon a unit of information, is it ever possible to abstract and mathematize what is essential to a message?

It is worth noting that Boulding chose to question the scope of relevance of the mathematics of information theory whilst writing an epistemological thesis. For him, information, communication, judgments, memory, and any approach to understanding the mind, or consciousness, must take into account the epistemological angle, that is, the manner in which we learn, know, and understand. There is something simplistic about the experiments described by Miller, and about information theory as a whole, that Boulding (1956: 28) wants to warn against, and it comes early on in his thesis:

[The mind is not] simply a sausage machine grinding out [responses] from the messages received. It is much more realistic to suppose that between the incoming and outgoing messages lies the great intervening variable of [subjective knowledge]. The outgoing messages are the result of [subjective knowledge], not the result of the incoming messages.

³ An excellent example of psychological laboratory research with greater perceptible relevance to the real world is Milgram's series of experiments in obedience (Milgram, 1974; Blass, 2004).

The incoming messages only modify the outgoing messages as they succeed in modifying [subjective knowledge]⁴.

For Boulding, in other words, two issues crucial to the communication of information - semantics and subjectivity - are ignored by information theory and Miller's thesis. There will be opportunity to return to these issues later. For now, note that, no matter the above caveats, there is one general conclusion that may confidently be drawn from Miller's paper. When faced with making a decision, we can only reasonably rely on limited information, not because the information might not be there but because, even if it is there, we can only deal with a small subset of it. In other words, *given all the facts or not*, we are condemned to make decisions in the absence of clear facts.

This conclusion is all the more disturbing when considering that the experiments to which Miller refers all involve simplistic and only sensory inputs: monosyllabic words, small numbers, dots, lines and so forth. If the human mind has such a limited capacity when faced with making decisions on such simple variables, what chance is there of it having a greater capability in the face of multifarious existence? Short of genetically reprogramming the neuro-biology of human beings, the only viable alternative appears to be the design of decision support systems. But any decision support system is not a substitute for human thinking, it is merely a support. Thus, as Bennis and O'Toole (2005a) say, despite decision support aids, decision makers still need to know how best to interpret facts in order to make decisions that, as Miller indicates, will invariably be based on an absence of clear facts. In what follows, we will explore decision support systems, as well as the manner in which decision makers can interpret facts effectively. But the discussion will leap forward to the multidimensional existential experience of everyday life and thus compound the problem far beyond what Miller envisaged in 1956.

⁴ Readers of Boulding's *The Image* will know that I have substituted 'the image' for 'subjective knowledge'. This stems from Boulding's (1956: 5-6) own definition of 'the image':

What I have been talking about is knowledge. Knowledge, perhaps, is not a good word for this. Perhaps one would rather say my *Image* of the world. Knowledge has an implication of validity, of truth. What I am talking about is what I believe to be true: my subjective knowledge. It is this Image that largely governs my behavior.

Chapter Two – Be Comprehensive, or Be Damned

The last chapter concluded that, when faced with making a decision, we can only reasonably rely on limited information. This is not because the required information might not be there but because, even if it is there, we can only deal with a small subset of it for neuro-biological reasons. In other words, *given all the facts or not*, we are condemned to make decisions in the absence of clear facts.

This conclusion was drawn by examining, in quite some detail, the 1956 paper by Miller which, almost single-handedly, gave rise to the field known as cognitive psychology (Hirst, 1988). During the same period, two distinct, but not unrelated, fields were similarly wrestling with the question of effective decision making: *planning theory* and *management science*. Planning theory complained that its own theoretical preoccupation with *too many* facts led, at best, to not informing practice and, at worst, to rendering practice unsuccessful. Management science complained about the same consequences, but from the point of view of its own theoretical preoccupation with *too few* facts.

Neither field was paying much attention to Miller. They were drawing their conclusions from their respective experiences. It is worth beginning with a consideration of planning theory, for its idiosyncratic preoccupation with *too many facts* will prove to be fundamental to the making of decisions in the absence of clear facts.

Just in case there is any doubt as to the relevance of planning theory to decision making, Buchanan and O'Connell (2006) begin their 'brief history of decision making', in the *Harvard Business Review*, by noting that Chester Barnard 'imported the term "decision making" from the lexicon of public administration [i.e. the core realm of planning theory] into the business world.' Consider as well the definition of planning provided by two of the most influential planning theorists, Friedmann and Hudson (1974):

A useful way to look at planning is to consider it as an activity centrally concerned with *the linkage between knowledge and organized action*.

Decision making is, equally, 'centrally concerned' with this link: making a decision involves drawing upon knowledge or information in order to dictate or direct organized action. Planning and decision making are, therefore, synonymous and, as with all synonyms, each serves to enrich the understanding of the other.

Planning on local, regional, national or global scales is, by any account, a Herculean task. The history of planning theory is the history of a field in perpetual struggle against itself and against the world. Planners usually work for the government, or for organizations contracted by the government. Their main goal is infrastructure improvement. It is the very nature of this noble goal that renders the planning task so challenging. For infrastructure affects the social, political and cultural dynamics of a society – to say nothing of the economic dynamics. Even where infrastructure might be universally appreciated as inadequate and in need of serious improvements, the feasibility of implementing perceptibly desirable plans depends upon conflicting social, political, economic and cultural determinations. Financial interests, traditional values, struggles for strategic influence and a host of other pressures make it difficult

to carry out even the best laid plans. Large cross-sections of society are involved, affected, or victimized at any one time. Gains on one level yield losses on another. Pluralism, multiculturalism, democracy, even religion posit innumerable judgments across heterogeneous and often contradictory spectra, rendering the measurement of success or failure intractable, let alone discernible. Planning, of its very nature, is the comprehensive task *par excellence*, the task that needs to take *everything* into account.

Indeed, it was this understanding of its nature and of its needs that led planning theory to define itself, for the better part of the twentieth century, through the idea of comprehensiveness. The story planners were telling themselves (and the world) about their practice was that it aimed for comprehensiveness, that it *was* comprehensive. Comprehensiveness, by definition, involved the stipulation of the goals, the identification of all means to reaching them, the evaluation of the best means, and thus the implementation of the optimum solution. Comprehensiveness, by definition, allowed for all predictable effects to be incorporated into the vision (the plan) prior to its execution. Comprehensiveness, by definition, uncovered what would otherwise have remained unforeseen consequences with possible detrimental effects on people's lives. Indeed, comprehensiveness, by definition, incorporated the views of all the people; it was democracy incarnate. Most of all, comprehensiveness, by definition, was the only defense to the sword of Damocles that hung over the practice of planning: there is no room for mistakes, no toleration of gracious apologies when things do not go *as planned*. Be comprehensive, or be damned.

There was only one problem with this story. There was no evidence that comprehensiveness actually worked – or, at best, there was partial evidence⁵. Even where there was common agreement on goals, the available means were contested. No one optimum solution could incorporate all interests. At some point, there would be losers and winners, because the solution inevitably favored one position more than another. One cannot have a solution that is simultaneously efficient and effective; simultaneously politically amenable, culturally acceptable, socially tolerable, and economically maximal; or simultaneously regionally, nationally, and globally consistent. Cost benefit analysis seems rational, but it ignores a wide range of human interests (Rosenhead and Thunhurst, 1982). Optimization models always provide the optimum solution *for the model*, but an army of extraneous variables are omitted from the modeling process, making for a less-than-optimum real-world implementation. And, to top it all, there was no evidence that comprehensiveness was free of mistakes. Or better, there was no evidence that comprehensiveness contributed to the betterment of human existence. Indeed, from the purely materialist perspective, one could argue that it was the narrow focus of corporate capitalism that elevated human beings from the gutter of the nineteenth century to the suburban homes of the twentieth. Planning theory itself seemed to have contributed not much more than a Haussmannian Paris, designed for military purposes but resulting in the acknowledged world capital of romance. Exemplary comprehensiveness, perhaps?

⁵ Rothblatt (1971) recounts the development of the Appalachian program (later to become President Johnson's 1964 and 1965 Appalachian Bills, and culminating in the Appalachian Regional Development Act of 1965) in order to show how the ideal of comprehensive planning can be approximated.

The perceived failure of the comprehensive planning paradigm led planning theory to advocate a reduction in comprehensiveness and, by implication, the making of decisions in the absence of clear facts - or at least in the presence of fewer facts (which amounts to the same thing). Without delineating more exactly what this reduction of comprehensiveness actually entails, however, the investigation risks remaining on the level of sheer narrative. The general reference that documents the comprehensive planning debate is that of Faludi (1973). There he collects the major papers that defended some version of comprehensive planning, or at least grappled with ways of saving it (Webber, 1963; Meyerson, 1956; Banfield, 1959; Robinson, 1965; Altshuler, 1965; Friedmann, 1965). He also includes the two, and respectively very different, critiques of comprehensive planning that gave rise to the development of new planning paradigms (Lindblom, 1959; Etzioni, 1967). In this and the ensuing chapters, a more detailed understanding will unfold, against which one can not only measure the value of the developments that followed, but the degree to which comprehensiveness was actually substituted by something else. Friedmann and Hudson (1974) offer a good starting point, in this respect.

Friedmann and Hudson (1974) begin by noting that the basic assumption of the comprehensive paradigm 'is that decisions *precede* action⁶.' This implies two things: first, decisions *for* action are to be taken only once all the facts are in; second, all future decisions concerning the resolution of a particular problematic situation are to be taken *in advance*, leaving no room for feedback loops that might inform decision making as the future unfolds, that is, as the consequences of action materialize. Friedmann and Hudson add that, when put into practice, this paradigm rarely accounts for the subtleties that arise when attempting to implement the decisions taken.

To say that decisions *for* action are to be taken only once all the facts are in implies that it is possible to have the facts to guide the proposed action. Thus, the comprehensive paradigm assumes the possibility of perfect information. It is not irrelevant to note that decision theory has developed intricate decision support tools for uncovering the value of *imperfect* information (Clemen, 1996), thus implicitly endorsing the view that the comprehensive paradigm is too simplistic. Consider, however, the time in history when the comprehensive paradigm explicitly emerged: the first half of the twentieth century. This was a slice of history characterized by the consolidation of bureaucracy, and the rise of hierarchical administration, treated in detail by Fayol (1949). Frederick Taylor's (1947) *Scientific Management* was influential especially because it spoke to this context. Henry Ford's success as an entrepreneur depended upon this context (Crainer, 2000). It was a world where hierarchical, trickle-down, centralized decision making was possible – either, as in the case of Taylor, because there was no other viable alternative, or, as in the case of Ford, because the market offered no viable competition to have to think about. Problems were simpler (efficiency was the only key objective), decisions were not subject to time-pressure (the market would wait for the next production run), optimization was possible (exemplified by the division of labor induced by the assembly line). The risk that consequences would somehow come back to haunt the

⁶ Italics in the original.

decisions taken was minimal. The basic assumption that ‘decisions *precede* action’ was, therefore, more than mere assumption: it defined the way the world perceptively worked.

Friedmann and Hudson, however, point to three implementation issues that increasingly challenged this perception. The first is what they call *the problem of knowledge*. This arises from the essential nature of planning problems: no matter what superficial similarities may be deduced between them, they are contextually immersed, non-repetitive and, so, different to one another. For example, railway infrastructure planning might have similar objectives across time and space. Each and every time and place, however, impose social, technical and geographical peculiarities that make of implementation essentially a brand new problem. Historical data are at best partially useful, or at worst irrelevant or nonexistent. The degree to which precedent is absent governs the need to develop knowledge about the problematic situation as it is being tackled (what social scientists like to call *action research*). Consequences, as well as the impact of external circumstances, can be mathematized to an extent through probability, but the employment of such statistical tools is essentially based on subjective hopes or expectations. No matter the approaches to formally employing subjective probabilities (Raiffa, 1968), the curse of subjectivity remains. Indeed, no attempt at making decisions in the absence of clear facts is worth the effort without explicitly confronting and dealing with subjectivity *on its own terms*.

Thus, even where identifying the objectives might not be that difficult, identifying the means of implementation remains problematic. One could argue that the first step should be to collect some information, make analytical predictions, and methodologically evaluate possible effects. That is to say, increase the amount of knowledge *content*. This pursuit of better intelligence, however, for which comprehensiveness argues, is misplaced. The future unfolds in a disjointed series of events of differing impact and magnitude. It does not wait for any fact-finding methodology. Intelligence, comprehensive understanding, and the cumulative growth of knowledge come only after the event, with hindsight. At the moment of impact, one’s degree of knowledge is minimal, and one is faced with having to make decisions in the absence of clear facts. In a world where ‘extreme events’ (Albeverio et al, 2006) increasingly define the norm rather than the exception, one realizes that there is a rift with the knowledge required of the comprehensive paradigm. Where comprehensiveness asks *what* should be included in a model or a plan, reality must, in the absence of information, ask *how* the decision can best be made. There is a fundamental epistemological switch, from a focus on content to a focus on process, when faced with making decisions in the absence of clear facts.

Friedmann and Hudson’s second challenge to implementation is methodological, and it strikes at the very heart of comprehensiveness. It concerns the paradigm’s adherence to a formal stipulation of an objective that integrates all possible preferences and which can consequently enable a trade-off calculation that eventually provides the solution. Trade-offs are possible only in cases where the variables are expressed through a dimension common to them all. The mathematical structure of a linear programming model is exemplary in this respect. In linear programming, the

objective is quantitatively stipulated as a series of related preferences, set against a number of constraints, and an algorithm works its way through the available solution space to find the best (optimum) solution to meeting the objective. The problem with the linear program is the same as that confronted by the comprehensive paradigm's ideal methodology: real-world preferences, as well as constraints, can be reduced to a single dimension (for example, quantitative analysis) in very restricted, and relatively simple, cases. If comprehensiveness is about breaking through the restricted and the simple, its favored methodology stops it dead on its tracks.

The final challenge to implementation posed by Friedmann and Hudson concerns coordination and control, two activities whose effectiveness is inversely proportional to the numerical and characteristic magnitude of decisions and actions. Comprehensiveness, of its very nature, is decision making *en masse*. It is also decision making undertaken through a centralized control-and-coordination authority. This authority need not necessarily be authoritarian. It might well consult various spectra of society, including the potentially affected. The concern is less with this authority's style, and more with its ability to manage the decision making process, and its content, when comprehensiveness is attempted. Friedmann and Hudson draw from the work of Downs (1967) in order to highlight the three 'laws' to which such an authority falls victim: the Law of Imperfect Control, the Law of Diminishing Control, and the Law of Decreasing Organization. These laws operate as a degenerative feedback process that eats away at attempts to implement the ideal of comprehensiveness. Taking each in turn:

- As the planning boundary extends, the effectiveness of centralized control diminishes.
 - In other words, although it is quite reasonable to expect perfectly centralized control over a relatively small area, centralized control becomes proportionally imperfect as the focus is extended.
- As a result, centralized control becomes increasingly weak.
- Weak centralized control results in poorer coordination among the variables.
 - In turn, poorer coordination contributes to an even more imperfect centralized control.

Friedmann and Hudson note that these laws result from 'the fact that each person's mental capacity is limited' when faced with the quantitative and qualitative magnitude inherent in comprehensive planning. This echoes Miller, and indeed the three laws succinctly contextualize Miller's thesis in a decision making environment. No matter the psychological links, however, if the comprehensive ideal, and its inherent preoccupation with *too many facts*, crumbles in the face of real decision making, one must ask why planning theory pursued comprehensiveness so aggressively. Furthermore, how did the failure of comprehensiveness manifest itself?

A clue to the first question lies in the very issue of coordination and control, an issue which, once again, takes us back to 1956. As we have seen, 1956 was the year that Miller published his 'number seven' paper. It was also when Boulding (1956) argued for a wider epistemological appreciation of decision making, one that we briefly

considered toward the end of the previous chapter as a counterpoint to Miller. 1956 also saw the publication of *Introduction to Cybernetics* by Ross Ashby. In this seminal text, Ashby tackles the issue of coordination and control explicitly. Our particular interest is with his Law of Requisite Variety.

In order to grasp the Law of Requisite Variety, consider first an illustration. Skyscrapers are built to withstand a range of windspeeds. Depending on the range considered, a skyscraper will be built in a particular way. Therefore, the variety of windspeeds will govern the variety of measures taken in the construction of the building. One day, a wind blows that is off the scale of the windspeed range initially considered. What has happened is that the variety of windspeed has suddenly changed. The skyscraper suffers relative damage - relative to the amount of wind variation not considered initially. The variety of measures taken in the construction of the building failed to account for the actual variety of windspeeds. The Law of Requisite Variety states that effective coordination and control of a system is proportional to the degree to which the variety of coordination and control accounts for the variety of outcomes of the system, and/or the variety of perturbations to which the system is liable.

Analogically, in planning theory, the wind is the reality being planned for, and the skyscraper is the plan. When, earlier, we discussed the complex nature of this reality, we were actually referring to the nature of its variety: the greater the complexity of reality, the greater its inherent variety. And when we discussed comprehensiveness, we were actually referring to the variety planning theory deemed necessary to incorporate into its plans as a means of coordinating and controlling the variety of reality: the more comprehensive the plan, the more variety it will include to address the inherent variety of reality.

As such, the comprehensive planning paradigm can be appreciated as an attempt to adhere to Ashby's Law of Requisite Variety. If one wishes to coordinate and control complexity, the mechanisms for such coordination and control (in this case, the comprehensive plan) must reflect this complexity. A simple, or simplistic, plan cannot hope to deal with the variety of possible outcomes stemming from implementation, nor with the variety of perturbations to which such implementation might be liable. Thus, where planning perceived itself as having to be comprehensive, with Ashby's Law of Requisite Variety it had the scientific pretext for the design of comprehensive plans.

Irrespective of the weaknesses of the comprehensive paradigm, there was good reason for adhering to it. Cybernetics is the science of coordination and control. Faced with the task of having to take everything into account, cybernetics taught that the plan must be comprehensive, not so much to reflect the objectives and the means, but to be able to coordinate and control their implementation. If there was good reason to pursue comprehensiveness, then, we must address our second question: how did the failure of comprehensiveness actually manifest itself?

Chapter Three – The Cracks in the Masonry

Success or failure of decisions is a reflection of that upon which such decisions are based. The planning task is not only about creating a plan *per se*. It is about creating a plan that serves to support the decisions to be taken. The plan, in other words, is the model, or the decision support tool, upon which all decisions rest and from which they emanate. At the limit, the plan *is* the decision. If anyone wants to question a decision, they only have to examine the plan, the model that supports the decision taken. In order to explicitly highlight the failures of comprehensive decision making, we must look at the decision support systems, the models, the technical details of the plans used to support such decision making.

Comprehensive planning matured within a certain historical context that deeply influenced its development. The information theory Miller used was a mathematical theory. Ashby's (1956) *Cybernetics* was equally based on a quantitative translation of coordination and control. Indeed, the 1950s were a time of great strides in the mathematization of reality. In the late 1940s, linear programming was publicly presented for the first time by its developer, George Dantzig (Dwyer, 1948), leading to widespread applications in industry (Cottle et al, 2007). In 1951, the methods of operations research, that had significantly helped the allies during World War II, were declassified by the US Government (Morse and Kimball, 2003) and quickly embraced by management, resulting in *management science* (Winston and Albright, 2007). In 1953, the problem of queues - the plague of any system constituted by the arrival of inputs requiring processing - found its mathematical translation in the work of Kendall (1953). The mid-1950s saw the mathematical development of multi-stage planning in the face of dynamic, time-varying variables (Bellman, 1952, 1953, 1957). And by the end of the decade, a brand new mathematical methodology for the simulation of feedback relationships had been founded (Forrester, 1958).

Developments such as these served to support decision making through the most objective, and the most rigorous, science of them all: mathematics. Decision making, through its embrace of mathematics, was quickly being granted scientific status: *decision science* (Kleindorfer et al, 1993). Finally, the path had been laid for decisions to be made scientifically, far surpassing the achievements of Frederick Taylor's (1947) *Scientific Management*. Given such developments, the relevance, and application possibilities, of mathematics to decision making were not lost to planners. Comprehensiveness now had at its disposal the most valuable thinking tool of human history. Mathematics not only has generic applicability; it can be verified objectively, and thus serve as a common denominator to the otherwise subjective mess of human interests. All that planners had to do was to incorporate the new mathematical methodologies into their planning, and consensus for implementation was guaranteed. A brave new world was indeed beginning to emerge.

In the beginning of the 1970s, Lee (1973) dissected the technical decision support systems that had been developed for the furtherance of comprehensiveness. His incisive 'requiem' begins with the observation that the models designed by comprehensive planners had proved to be 'fundamentally flawed'. He referred to the models as 'dinosaurs' that had 'collapsed rather than evolved', and added that 'none of the goals held out for [these models had] been achieved.' The models had been

devised as an aid for comprehensive learning about reality. For Lee, however, ‘what was learnt had almost nothing to do with [reality]; the knowledge that was increased was our understanding of model building and its relationship to policy analysis.’ That an understanding of model building had been achieved might be deemed valuable experience, but Lee feared that it was being ignored. His aim was to make the lessons of model building explicit so that future decision makers would not repeatedly fall into the same traps and, consequently, some learning about reality could be achieved.

Lee’s overarching concern is not simply about comprehensive models but about *quantitative* comprehensive models. Decision makers have, understandably, always perceived a particular logic in the mathematical siren song that makes its appeal hard to resist. To begin with, mathematical analysis is scientific analysis through and through. The adoption of mathematical models, therefore, invites the attribution of scientific status to the decisions based upon them. The degree of such attribution is proportional to the degree to which mathematics (1) is adopted as the governing methodology of model building and, (2) provides the criteria for the decisions taken. Therefore, the greater the methodological adoption, and the greater the provision of mathematical decision criteria, the more we can claim scientific status for our decisions, and the less we need rely on ad hoc, intuitive, and otherwise subjective decisions governed by personal interest. The diminution of subjective interjections will enable the provision of the greatest good for the greatest number. Couple that with the incorporation, in the model, of a comprehensive number of variables, and you get a version of modeling practice that comes close to achieving the democratic ideal through the pursuit of rigor and exactitude. Like all siren songs, however, the consequences of attraction can be disastrous. The problems begin with the attempt at comprehensiveness itself. They are compounded by the adoption of the mathematical point of view. Consider each in turn.

We perceive the world as intricately interrelated, and as a system producing all types of side-effects. These relations and consequences stem from the multiple purposes and undertakings that simultaneously act upon the world. The result is a complexity which we find difficult to deal with, let alone understand. There arises the need to somehow integrate it all, so that we can learn about it and more effectively tackle it. The idea of a comprehensive model proclaims the means of doing this. It promises a structure that accounts for the simultaneity of multiple undertakings, and for the tracing of cause-effect paths through innumerable variables consolidated within relational statements. Indeed, the more variables included, the closer we get to the perfect mirror of reality.

This hypercomprehensive justification for the inclusion of a large number of variables in a plan, results in a model whose complexity demands a deciphering effort equal to that of trying to get to grips with reality. There arises a frustrating realization of having to deal with a level of uncertainty in the model that appears to be equal to the real-world uncertainty the model was designed to tackle in the first place. Faced with thousands of variables, can we be certain that the relationships modeled between them correspond to those that exist in the real world? Can we be sure that the explicitly modeled relationships between the variables are not generating implicit, imperceptible, time-sensitive, secondary relationships, or constraints, removed from

the dynamics of reality? Does the introduction of each additional variable refine the model and serve to eliminate uncertainty, or does it 'introduce less that is known than is not known' (Lee, 1973)? To what extent are the results a product of quirks in the model's logic? To what extent have we succeeded in modeling comprehensively microscopic variables when the best information we have is based upon aggregate relationships that include the effects of an unknown number of extraneous variables? What is the methodological basis for the shift from macroscopic understanding to microscopic modeling? And what evidence is there that such microscopic detail yields the adequate richness required for attaining the comprehensive ideal? Lee notes that, as the plan sinks under the weight of an excessive number of variables, the actual level of detail is much too coarse to be of any use to decision making. Moreover, whose interests are reflected in the performance measures incorporated in the model, and whose interests are ignored because of such measures? And *why*? The frustration mounts in that there are no straight answers to the above questions. Indeed, we begin to realize that, in our attempt to make decisions given all the facts, we actually end up making decisions in the absence of clear facts.

What is more, the inclusion of multiple, simultaneous purposes – be it in the service of reflecting the dynamics of reality as closely as possible, or of promoting a democratic utilitarianism, or of achieving economies of scale in the modeling process, or what have you – cripples the ability to differentiate between means and ends, resulting in confusion as to what the objectives might actually be. As any experienced modeler will testify, to demand that a model coinstantaneously serve multiple purposes results, at best, in the discovery that there is no feasible solution and, at worst, that the variables require artificial manipulation to force a solution. In the case of the former, the entire exercise has been futile, and contemplating attempts at rectification is tempered by the risk of analysis-paralysis as the Lernaean Hydra of inadvertent circular relations raises its head. In the case of the latter, the solution reflects an imposition that defies the comprehensive ethos and undermines the reasons for modeling in the first place. Indeed, *Realpolitik* might require that the model be massaged: "What politician will believe it if the model goes against his intuition or his self-interest? [...] Unless it tells him what he already wants to believe, it is hard to imagine what a policymaker might do with this model." (Lee, 1973). Porter (1995: 81) sums it up rather well: 'For purposes of influencing the general public, an argument loses force in proportion as it takes in more terms and comprehends a wider field.'

The problems are compounded when, in pursuance of rigor and exactitude, we apply mathematical bricks and mortar to the comprehensive superstructure - for example, through the quantitative translation of relational statements into equations. It is one thing to do a study and extract some data from it, quite another to find data to inject into a study. A chemist, for example, will extract and collate data from a well-defined experiment in order to assist the understanding of certain chemicals. By contrast, a mathematical modeler working on a plan, whose comprehensive scope is liable to the whims of socio-political fluctuations, is akin to a fireman stoking the furnace of a steam locomotive running at full speed, with the added job requirement that he must quarry his own coal.

The optimism about the relevance of a scientific approach transformed the pursuit of comprehensiveness into the quest for data to feed into an increasingly hungry model. As more data are introduced, more equations have to be introduced or refined. The entire process tends toward resembling less the reality under investigation, and more towards one that attempts to ensure the accuracy of the mathematical theory. This tendency is obvious in at least two fields that comprehensive planning called upon to assist in its own scientization: operations research and economics. We will have an opportunity to consider operations research (i.e. management science) later. Economics, however, is a comparatively easy target. There is a deep gulf, for example, between Galbraith's (1998) intuitively evident economic analyses of human beings' day-to-day realities, and the intricately quantitative analyses published in economic journals. The latter might contribute a mathematically correct interpretation, but it is far removed either from any possibility of application or, worse, from the possibility of ensuring that decision makers understand it in the first place. Lee (1973) makes us wonder whether this is because 'the effort required probably would at least have equaled that of constructing the model itself'.

A comprehensive plan that aspires to be scientific through the wholesale pursuit of quantitative modeling risks becoming a mathematical minefield liable to explode under the feet of its modelers. For the desired scientization of planning stems from an unwarranted assumption that the mathematization of the real world is possible on a level of exactitude and rigor that, in actual fact, only pure mathematics itself enjoys. Pure mathematics is able to minimize and control the degrees of interpretative freedom that can be brought to bear upon its mathematical results. It pursues results purified of any conscious judgments and focuses on discoverable results that are free of any creative intervention (Courant and Robbins, 1996). Once mathematics is applied to the real world, however, such degrees of freedom are necessarily loosened. The very attempt of applied mathematics is but the application of symbols and rules for the understanding of some aspect of the real world. As such, it is but a perspective and, where there is perspective, the barricades to created – as opposed to discoverable – results are forever removed. Any one variable, and its performance, can be interpreted along a spectrum of available dimensions, equal to the variety of human interpretations placed upon such a variable. Evaluative criteria arise from *the field in which* mathematics is applied, not from pure mathematics itself. More ominously, history shows (Porter, 1995) that the use of mathematics in planning serves a politically expedient purpose: trust in numbers emerges in proportion to the distrust in decision makers, and the objectivity attributed to the numbers is used to control or defend against meddling outsiders, constituents, or subordinates. We might add that an emphasis on quantification might actually involve 'less the desire to make a decision "according to the book" than a new kind of response to the instinct of self-preservation' (Jones, 1964). Whichever way you look at it, applied mathematics is more social science than exact science.

And this renders problematical our habit of seeing applied mathematical models as providing solutions. We do not pay much attention to these models for what they are: effective means for generating insights into reality. A mathematical model cannot hope to offer realistic solutions *per se*. It only offers algorithmic solutions to a set of artificial variables that know no existence outside of the limited mathematical

relations in which they are imprisoned. The usefulness of building such models is in the questions they generate about reality, but we must not hope to answer such questions through the mathematical translation of that reality. As Jones (1964) puts it:

[There is nothing] wrong with making models or in having a reasonably consistent methodology for making decisions. Both are useful in much the same way that conventional signs and coordinates are useful on a map. They help the user to discover where he is, in relationship to where he wants to be next. They may even suggest alternatives for getting there, but like the marks on a map, they are not an automatic means for dealing with actual conditions on the ground.

Consider also that the mathematical modeling of comprehensive plans requires, due the sheer number of variables involved, the organizational and calculative power of computers. The adoption of computing power in applied mathematics distinguishes it even further from mathematics *per se*. Pure mathematics might make use of computers. Its results, however, do not depend on them but on logic. Computer algorithms are no doubt logical, but they have been developed to produce results for well-defined and limited situations, situations far removed from the ambitions of comprehensive plans. The linear accumulation and application of numerous distinct algorithms cannot service the complex interrelational requirements of an integrated plan. A case in point is inventory management, or the management of stocks. Given a long list of assumptions regarding the surrounding environment, inventory dynamics can be mathematically modeled with great accuracy (Fogarty et al, 1991). As each assumption is relaxed in order to reflect the dynamics of the surrounding reality, however, the mathematical model loses its ability to effectively assist in the management of inventory dynamics. The global demand for oil, for example, coupled with the speculation that continuously surrounds it, as well as the volatility of its distribution networks, wreaks havoc in attempts to manage stocks with any degree of mathematical accuracy. Any global supply chain director will tell you that the internationalization of division of labor, the geographical spread of suppliers, assembly plants, and market, as well as the geopolitical forces that impact worldwide distribution render redundant the neat, mathematical supply chain models taught in logistics courses. As Lee (1973) notes:

There is a popular illusion that confronting a computer with one's ideas enforces rigor and discipline, thereby encouraging the researcher to reject or clarify fuzzy ideas. In the very narrow sense that the human must behave exactly like a machine in order to communicate with it, this is true. But in a more useful sense, the effect is the opposite; it is all too easy to become immersed in the trivial details of working with a problem on the computer, rather than think it through rationally. The effort of making the computer understand is then mistaken for intellectual activity and problem solving.

Comprehensive planning cannot use computers as a substitute for reasoning, for planning is not simply calculation. It is difficult to imagine how a wild adventure through the rapids of multiplicity, simultaneity, variability, and complexity can possibly be achieved with only a mathematical canoe. Does the adoption of full-blown mathematical rigor and exactitude facilitate the flexibility required to navigate between the rocks, or does it inadvertently set a collision course? The price of *mathematical* rigor and exactitude is the inability to navigate flexibly through the rapids of real-world decision making. This is not to diminish the value of maintaining a discipline of rigor and exactitude. It is to signal that such a discipline should be

made possible without having to resort singularly to mathematics. Indeed, it is to signal that what is required is a discipline that is not necessarily mathematically based but analogically ruthless.

We are not yet at the point where we can introduce exactly what such a discipline entails. We can, however, provide a neat example of successful planning undertaken without resort to the above complications, albeit in the face of necessary comprehensiveness. At about the same time that Lee was writing, Delhi was faced with a crisis in its urban bus transport system (Tinker, 1982). Not only was the system miserably failing to serve its customers, but projections estimated population growth and infrastructure requirements that far exceeded the government's capabilities to provide. It was said that just to maintain the pitiable level of service, Delhi's bus fleet would need to be doubled. Faced with this situation, comprehensiveness dictated that the planners pursue the renovation or development of infrastructure, most probably through an elaborate study of passengers' origins and destinations, followed by a gravity-type model that would reconfigure the routes. They would need to enhance their model with demographic, economic, social, and even political projections. Such an approach would probably have been, in the main, technically feasible, and perhaps it was even desirable. Like all such comprehensive approaches, however, it had two things working against it: costs and time, the scourges of policy makers whose livelihoods depend on votes. Far from attempting to stoke the fire of an intricate mathematical model, the situation was simply perceived from a totally new viewpoint. Whereas the system was presently destination-oriented, the analysis provided an easily implementable solution from a direction-oriented point of view. Within six months, 'in terms of every measurement of efficiency, the overall system had improved substantially' (Tinker, 1982).

The approach to the Delhi bus system had all the hallmarks of what Lee (1973) considers the keys to effective planning. First, the model was transparent since it was based on the articulation of a different point of view, not on the mystique of computerized mathematical relationships. As such, it was readily understandable by laypeople: the policy makers and their constituents. It may, of course, have been as likely to be wrong as any other model, but its transparent nature allowed for errors to be recognized quickly and hence render further investigation easier.

Second, the model was designed by maintaining a healthy balance between theory, objectivity, and intuition. Too much theory removes us from empirical reality. Too much rigor sends us on a wild-goose chase for numbers. Too much intuition places us at the mercy of caprice. Transport theory distinguished between destination-oriented and direction-oriented approaches. Objectivity was promoted through the maintenance of current efficiency measurements. And, with an eye on theory and methodology, intuition was used when choosing to concentrate less on rebuilding the current infrastructure and more on reconfiguring it.

Third, the approach was based upon solving the problem and not applying some favored methodology. In other words, the real-world purpose of offering better service, including the constraints surrounding policy making, governed the manner in which the solution could be analyzed and implemented. No doubt, the Delhi bus problem was but one in a system of interrelated problems requiring adequate long-

range planning. Instead of constructing grand plans or big models, however, this one problem was evaluated against perceptible long-run consequences, allowing a comprehensive look-out to facilitate implementation without having to solve everything at once.

Fourth, the model was simple in that no large data sets were required, no new criteria needed to be developed, no complex set of relationships had to be designed, and no great communicative effort was required.

In Delhi, the comprehensive requirements of demography, economics, and even infrastructure were not modeled explicitly, but were appreciated intuitively. They served to contextualize the reasoning instead of govern its logic. They were borne in mind instead of specifically modeled. And this attitude toward comprehensiveness facilitated implementation. There was no attempt to take a wide swathe of decisions in advance, no attempt at a wholesale mathematical translation of the variables, no attempt to trade one variable off against another, and no attempt to appear gung-ho scientific. Against the comprehensive ideal, the Delhi case offers an exemplar of efficient and effective planning through humble simplicity.

We must not, however, merely sit back and wonder at the foolish pretence of comprehensiveness. On the one hand, it is true that, by the end of the 1950s, planning theory was well aware that its preoccupation with too many facts led, at best, to not informing practice and, at worst, to rendering practice unsuccessful. Success would entail that decisions could be made once all the facts were in. Practice showed that, despite efforts at comprehensiveness, decisions were invariably made in the absence of clear facts. Comprehensiveness was appreciated for what it was: a desirable normative theory with infeasible empirical demands. Even Meyerson (1956) and Banfield (1959) conceded this - and they are credited with the classic success story of comprehensive planning (Meyerson and Banfield, 1955).

On the other hand, McCleery (1964) makes us wonder whether the perceived failure of comprehensiveness is not somewhat misrepresented. He advocated that comprehensiveness does not demand that decision makers distinguish all the objectives, uncover all the means of attaining them, and investigate optimum solutions. Instead, McCleery holds up comprehensiveness as a 'traditional' theory, even as the basis of 'responsible bureaucracy', meaning that it is a theory of decision making that *abides with political decisions*. It is not a theory that, through its comprehensive aspirations, allows planners to explore new avenues. It is, in fact, more conservative than its name indicates, for it is first and foremost restricted by the dictates of politics. In attempting to explicate the theory of comprehensiveness, the literature has stressed its decision making methodology without emphasizing its political context. In doing so, the theory not only perceptibly requires too many facts, but incorrectly presents itself as a model for democratic decision making. This presentation, for McCleery, is not quite correct:

It is a theory originally designed to apply to situations of limited discretion where the relevant values were ranked and ordered by the political system just as prices were ordered by the market. [It did not] license the impossible task of exploring every conceivable value and operational possibility in the realm of the administrator's

imagination. Instead, it was a mandate to consider the rank order of values as politically defined and the range of operational alternatives available within those narrow limits.

If comprehensiveness, therefore, ultimately results in the making of decisions in the absence of clear facts, it is not merely due to any perceptible inherent glitches within the approach itself. It is primarily because comprehensiveness works with limited facts in the first place: those dictated by political authority higher up in the decision making ladder. That authority sets the parameters of comprehensiveness and the criteria of evaluation. The role of the planner is simply to work within these bounds, bounds that have already defined the workable scope of comprehensiveness.

This insight tempers, and even contextualizes, the perceived failures of comprehensiveness. It does not, however, offer any solution. If comprehensiveness abides with the dictates of political rankings, then how are such rankings undertaken in the first place? Neither comprehensiveness per se, nor its politically charged manipulation, allow us to understand how to make decisions in the absence of clear facts. They merely reinforce that we are doomed to make such decisions. Of course, the political twist of McCleery's description introduces the variable of power, and raises questions regarding its impact on planning and decision making. But let's be clear: no theory of power has yet emerged that effectively allows human beings to deal with it, let alone control it. Even Forester's (1989: 162) thesis on *Planning in the Face of Power* is, understandably, only able to conclude that:

In the face of power, justice and equality are hopes, solidarity is a source of strength, and, however daunting the odds, there is freedom in the struggle.

Centuries of critical social science serve to show merely that power is as ubiquitous as the making of decisions in the absence of clear facts. Theories of emancipation from power forces make interesting reading, but hardly offer practical revelations. Unfortunately, for every decision making approach proposed in the literature, its inability to deal with power is the critic's first port of call, usually followed by throwing the baby out with the bathwater. In doing so, critics usually fail to fill the bath with a workable means of dealing with power. Decision makers can be excused for feeling betrayed by the very thinkers upon whom they depend for guidance. Not only are they told to dismiss a decision making approach; they are left torn apart on the rack of power. Helpless and defenseless, it is no wonder that real-world decision makers are skeptical about the relevance of decision theorists.

McCleery's comment, however, is noteworthy for one reason. It exemplifies the insight that began to take hold by the late 1950s: planning, and especially economic planning, cannot be divorced from politics. 'Political economy' was about to bloom. If by the late 1950s planning theory needed a new understanding that could help it cope with making decisions in the absence of clear facts, such an understanding could not afford to ignore the socio-political dimension of decision making. In 1959, Charles Lindblom offered such an understanding.

Chapter Four – The Muddling David versus the Failing Goliath

Lindblom's response to the weaknesses of comprehensiveness has been criticized 'for being unprincipled, apolitical, or, in a phrase, for admonishing us to "make do"' (Forester, 1989: 32). It has been characterized as 'an ideological reinforcement of the pro-inertia and anti-innovation forces prevalent in all human organizations', and as an approach that 'justifies a policy of "no-effort"' in that it 'is inadequate, having limited validity, and constituting a barrier to the improvement of policy-making' (Dror, 1964). The approach has been labeled as *irrelevant* to situations arising through 'new conditions, lack of precedents, and new knowledge' (Jones, 1964). More specifically, it has been judged as *relevant in restricted situations* where: (1) present policies, and their results, are deemed satisfactory, so that marginal changes to these policies are perceived as providing sufficiently acceptable rates of improvement; (2) the nature of problems changes little in time; and, (3) the means for dealing with problems changes little in time (Dror, 1964). It has also been seen as a bastion of conservatism as well as a philosophy of *laissez-faire* (Heydebrand, 1964). Not unsympathetically, it has been colorfully characterized as a description of how 'we do stagger through history like a drunk putting one disjointed incremental foot after another' (Boulding, 1964). More polemically, however, Faludi (1973: 116-120) introduces Lindblom by writing that 'flaws', 'ambiguities' and 'the habit of mixing *normative* with *factual* statements' are 'characteristics of his thinking'! He charges Lindblom with 'extreme scepticism' and a 'defeatist attitude'. He admonishes Lindblom's 'tendency to represent dominant trends as unqualified facts and limitations on problem-solving as absolute barriers', and adds that Lindblom 'misrepresents theory' and only offers a 'cavalier', 'cursory treatment' of comprehensiveness. He concludes that Lindblom's own alternative 'absurdly magnifies theory'.

So why bother even considering Lindblom? Well, there are at least four reasons. To begin with, he was the first to describe some dynamics of making decisions in the absence of clear facts. His was an empirical observation of real-world decision making, not a normative prescriptive theory aspiring to some scientific status. Second, Lindblom is the Adam Smith of decision theory. Where the latter proposed an invisible hand that controlled and coordinated otherwise independent interests, Lindblom saw that an equally intangible process of mutual adjustment governs the decisions taken by otherwise partisan agendas. Smith eschewed centralized economic controls as a hindrance to growth. Lindblom similarly dismisses comprehensiveness as a fiction, viewing it as an ideology that endangers effective decision making within a democratic context. A third reason is that, very soon after Lindblom put forth his observations, studies began to appear that confirmed them (Pfiffner, 1960). Most importantly, however, Faludi (1973: 116), for all his polemics, concedes that 'there has never been a cogent refutation of Lindblom's thesis from a planner'. For these reasons alone, Lindblom cannot be dismissed out of hand.

In 1959, Lindblom published a paper entitled The Science of Muddling Through – a humorous paradox in terms, underpinned by a certain humility in the face of mainstream pretence to scientific decision theory. This paper is perhaps the most influential in the entire planning theory literature. Twenty years after its original publication, Lindblom (1979) noted that the paper had appeared 'in roughly 40

anthologies'. Forty-four years after its publication, a collection of papers representing 'the central issues in planning theory' (Campbell and Fainstein, 2003: 14) felt it necessary to include it. The paper is, without doubt, a classic.

And yet, the paper said nothing new. Its description of the decision making process as one of marginal adjustments or, as Lindblom put it, *incrementalism*, had first been discussed in quite some depth in *Politics, Economics and Welfare*, a book Lindblom co-authored with Dahl in 1953, that is, six years prior to his classic paper (Dahl and Lindblom, 1953). Although the book received favorable reviews, incrementalism itself did not attract much attention at the time. *Social Forces* (Anderson, 1953) praised the book for its realism, but did not mention incrementalism. *The American Journal of Sociology* (McKee, 1954) praised the authors for eschewing 'the ideological choices of right and left' but, again, there was no reference to incrementalism. Indeed, all reviews focused on the authors' discussion of price systems, hierarchy, polyarchy and bargaining – as can be seen in the reviews from *The Review of Economics and Statistics* (Sutton, 1954) and *The American Political Science Review* (Gross, 1954), both of which, again, did not refer to incrementalism. *American Quarterly* (Taylor, 1954) fleetingly noted the authors' preference for 'gradual change and marginal adjustments' with no direct mention of incrementalism. Neither *The Journal of Political Economy* (Oliver, 1954) nor *Harvard Law Review* (Handlin, 1954) mentioned it by name, though the former described it as 'eclectic liberalism', whilst the latter alluded to it as a 'gradual accommodation' process. *International Affairs* (Streeten, 1954) noted that 'the main interest of the book lies [...] in the suggestion of a new approach than its altogether successful application' but did not attempt to explain it or name it. *The Accounting Review* (Albery, 1954) did not mention incrementalism but did note the authors' "'small group" association and the "planning of personalities" [as] suggested directions of future reform'. *The American Economic Review* (Bye, 1954) did mention incrementalism in passing, but provided no substantial comment on it. Only *Southern Economic Journal* (Smith, 1954) provided a one-sentence definition of incrementalism, but otherwise no substantial discussion.

This general apathy to incrementalism during the 1950s cannot be put down to its presentation in Dahl and Lindblom's book. The decade was simply too immersed in mathematical comprehensiveness to pay much attention. Instead of pushing the point, Lindblom bided his time until a prime proponent of this comprehensiveness appeared against whom he could fence with his incremental foil. His choice of opponent was none other than Jan Tinbergen who in 1969, along with Ragnar Frisch, was the first recipient of The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel. This is the same 'Nobel Prize in Economics' that John Nash shared in 1994. The symbolism is inescapable: the 'muddling' David versus the 'giant' Goliath, indicative not only of the two thinkers' respective methodologies, but of their perceived reputations at the time. In choosing Tinbergen, Lindblom attacked the enemy at its core, for Tinbergen was a well-established and respected figure in the comprehensive school of thought. Instead of inviting pitched battle, however, Lindblom chose a more cautious tactic: a skirmish in a confined space that left the giant little room for maneuver and allowed the snipers of incrementalism free rein. In academic circles, such an engagement is known as a book review.

In December 1958, then, a few months prior to the publication of his classic paper, Lindblom reviewed Tinbergen's (1956) *Economic Policy: Principles and Design*. Friedmann and Hudson (1974) describe the modeling approach of this work as 'ingenious', and note that 'it won the Nobel Prize for its author'. However, any seeming enthusiasm is quickly tempered when they describe Tinbergen as:

elitist, and his model quite unabashedly assumes a top-down process of decisionmaking, including the setting of major sectoral targets. The formal logic of his model was subjected to a detailed critique by a group of like-minded economists in 1964, probably the year of its greatest popularity (Millikan, 1967). While this review was going on, however, a large number of empirical studies of the practice of economic development planning (with an emphasis on the long run) revealed gross deficiencies in the method. The comprehensive modeling of future history (albeit restricted to economic variables) appeared to be no more successful in generating major development action in the arena of national planning than it had been in American city planning practice where comprehensiveness (or the synoptic view of decision making) continued to be an essential article of every planner's faith. (Friedmann and Hudson, 1974)

Of course, at the time of Lindblom's (1958) review, conclusions and judgments such as these were ten years away – which makes the review especially prophetic. Alas, as with all book reviews, Lindblom's is rarely cited. However, the dialectical manner in which Lindblom argued against Tinbergen's main points offers a clear presentation of comprehensiveness and the incremental rebuttal.

Tinbergen's book offers the following norms of comprehensive decision making:

1. The decision maker should pursue an agreed set of consistent objectives (or ends);
2. Aims of the decision should be clearly formulated in advance of choosing among alternative means;
3. The decision should be made by a single unit (rendering the terms *decision maker* and *decision making unit* synonymous);
4. The decision should be based on a comprehensive consideration of variables; and,
5. Coordination should be the explicit function of the decision making unit.

The first three norms indicate that, first and foremost, a decision maker must act according to a certain set of objectives. The aims of the decision will reflect the wish to attain, maintain, or otherwise reinforce these objectives. Therefore, the means for implementing the decision will be in accordance with the objectives, as will the resulting real-world material transformation that takes place due to the decision. So far, the decision maker, the decision, and the means of achieving the decision's desired results, are all subordinate to the set of objectives – a position echoed by McCleery (1964) eight years later, as we saw in the last chapter. Objectives guide and command everything. Objectives rule.

Indeed, the 1950s were very much the decade of objectives. In 1954, Peter Drucker (1954) proposed one of his most famous decision making approaches, 'management by objectives' (MBO) – an approach that was at the core of the 'empowerment' fad thirty years later (Wilkinson, 1998). Against such titans as Tinbergen and Drucker,

Lindblom observed that discussions regarding decision making either assumed the determination of objectives as given or, at the very least, focused on the means of achieving objectives and less on how the objectives were to be decided upon in the first place. Preempting McCleery's (1964) insight on the political dictates on objectives, Lindblom (1958) noted that the choice of objectives 'is often made by pitting power against power rather than by reasoned analysis'. It is, therefore, a process that cannot be squared with the scientific decision making aspiration. Lindblom concluded that, for all its intricate models that claim to evaluate alternative means objectively and comprehensively, a viable theory of the decision making process remains 'only half-constructed if ends are simply taken as given'.

The means of determining objectives was, therefore, Lindblom's first critical point. He differentiated between abstract objectives and their practical realization, and was fond of pitting inflation against employment to illustrate this difference. For example, on the abstract level, low inflation and low unemployment are equally desirable. On the practical level, they are unequally feasible, inconsistent, and hence conflicting objectives. Lowering one raises the other. Neither can they both be pursued consistently, nor can one be pursued in exclusion of the other, without risking the paralysis of an entire economy. Instead, at any one time, more marginal emphasis is put on one to the detriment of the other. Where low inflation and low unemployment are a mix of objectives that, together, constitute what the overall policy is, the concrete decisions of what will be done will depend on how we chip away at this mix in the interest of rendering implementation practically possible.

Lindblom's thesis can benefit from a decision making example which we will borrow from Conklin (2006: 16-18). The marketing department of a car manufacturer decides that the company should offer a car design that emphasizes side-impact safety (say they want to eat into Volvo's market share). The project is approved, given a deadline, a budget, and a senior executive is put in charge. The means of achieving the objective are simple enough: add structural support to the doors to make the car safer from side impact. The designers say that poses no problems. But when the accountants and the financial planners are told about this, they note that the additional structure will double the cost of a door. The public relations people add that the proposed structure will make the doors heavier and harder to open and close, especially on inclined planes, making for unhappy, and possibly injured, customers. The engine designers, furthermore, calculate that the proposed structural support will impact unfavorably upon fuel mileage ratios, an issue which extends to end-users' views on ecology. The engineers also say that adjustments will be required in the suspension and braking systems. Someone asks whether airbags might not be a better alternative. The corporate lawyers remind everybody of the legislation requirements for door strength, and question whether it does not make better corporate sense to abide by these requirements rather than try to exceed them. The sales director warns that such 'drastic' design changes lead only to two results in the market: flop or revolutionary breakthrough. In other words, the risks are higher than one might appreciate. And everybody seems to have an opinion on structural reinforcement, materials, cushioning, window design, hinge placement, and even the door latches. Suddenly, a seemingly simple objective has got us thinking about our entire product range, and about how each of our departments can cope with the suggested design.

Questions abound. At what price can we sell this car? How does side-impact survivability compare to a buyer's other purchasing criteria? What are the trade-offs between safety, performance, appearance, and cost, and how can they be calculated on a common dimension? When is safe *safe*? And as the complexity of what seemed a straightforward decision begins to emerge, the senior executive put in charge of the project feels increasingly unable to control or coordinate it due to insufficient personal knowledge to cope with the issues, and insufficient power to ward off, or even fight against, the different departmental viewpoints, interests and – who knows? – otherwise hidden agendas. The so-called 'senior' executive is outnumbered, outwitted, outflanked (out of sight?).

Perhaps we exaggerate a little about the senior executive, but this is far from being a trivial description. In fact, it points to the complex decisions faced by car manufacturers (not to mention their suppliers) everyday – in other words, a significant portion of the world's industry. Decisions about strengthening car doors might seem simple when first put to the table or when hailed as a success in the market. But look at what happens in between. A multitude of actors gathers round the objective, and knowledge, views, and expertise are exchanged. The actors are not necessarily against the project, but each one has something to lose and/or something to gain from the proposal as it stands. A negotiation ensues between the various actors. A series of decisions emerges: one decision after another, some decisions being based on previous decisions, some decisions intended to slightly alter the course of research and development, car design, marketing strategy and so forth. The one-off decision to offer a side-impact safety car has, in other words, changed into a series of incremental, negotiated decisions between very different actors, with very different knowledge bases and experiences. Each actor chips away at the margins of the proposal, much like a sculptor chipping away on a stone. The final design of the door – assuming the project reaches production – will hardly reflect the design originally envisaged by the marketing department. Instead, the design will reflect the accommodation reached by all parties involved. In other words, far from matching the marketing department's original singular vision, the final design actually reflects the marginal adjustments made to that vision as a result of a necessarily pluralistic decision making process. What is more, the rankings to which McCleery (1964) referred earlier do not come from the top – from the senior executive. They come in from all sides. Maybe that is how McCleery's rankings are decided upon in the first place, prior to be handed down to his 'administrator'. Indeed, his 'administrator' is probably nothing more than the car manufacturer's production floor, and his top level 'political system' is everybody else in the company.

This decision process is easily recognizable. We deal with it everyday, at work with our colleagues and at home with our families. Rarely do we witness bull-in-a-china-shop decisions. Human beings, and their artifacts, are immersed in a social world. Decision making cannot ignore socio-political dynamics without risking major blowback - to borrow an apt term from Chalmers Johnson (2000). Employing only one decision making criterion is unjustifiable and unrealistic. Efficiency alone does not make the world go round, and neither does any other *eff*__. Indeed, is not the pluralistic process described above, bringing in as it does all relevant knowledge sources, more faithful to the ideal of comprehensiveness? When compared to the neat

methodology of comprehensiveness, it undoubtedly appears somewhat chaotic. But centralized comprehensiveness itself is but one small step away from chaos. Just think of what would happen if the senior executive simply went ahead with the project: costs would increase, sub-standard cars would be produced, customers would be lost, market share would decline, law-suits would begin, legislators would subpoena – and all of this because of an otherwise noble objective to offer safer cars to the public!

The nobility of this objective lies in its abstractness. Everybody can identify with the value of side-impact safety cars. As soon as we agree on this value, however, an avalanche of equally important values hits us: the value of financial planning, balance sheets and profit and loss statements; the value of ensuring customers are not inadvertently injured (remember the heavy doors) by our pursuit of the initial value of side-impact safety; the value of favorable fuel mileage ratios, as well as the value of an ecologically-friendly image; the value of suspension and brake systems that actually serve their purpose; the value of ensuring against litigation; the value of minimizing possible market acceptance risks; the value of one structural reinforcement option over another, of one type of material over another, of certain cushioning over another, of one window design over another, of one hinge placement over another, of one door latch over another... the value of one view on safety over another!

All these values reflect a string of objectives that are in conflict with one another. We want a side-impact safety car with doors whose:

1. cost will not price us out of the market (financial objectives);
2. structural reinforcement will not adversely effect the suspension system, the brake system, the fuel performance, and our desire to maintain an eco-friendly image (automobile performance objectives, green objectives);
3. weight will not adversely affect customers' usage of the vehicle nor their perception of our company (public relations objectives);
4. design will minimize the risk of litigation and maximize the probability of success at the market (legal objectives, market objectives); and,
5. peripheral parts will be put together in a way that ensures aesthetic appeal, ergonomic ease, comfort, and security (end-user objectives).

And let's not forget to agree on what we understand by *safe*.

Now, what is obvious here is that none of these objectives will be met equally. Different perspectives, different knowledge bases, different interests are all tugging on a rope that has multiple ends. Where the rope falls will signal marginal losses and marginal gains for each objective. We will achieve a certain aesthetic appeal, a certain degree of security, a certain ergonomic design; we will achieve brake, suspension and fuel performances that operate within certain limits, and we will accept a certain risk of litigation. But, to be accurate, none of these marginal gains or losses is *agreed* upon. They reflect our tolerance. They reside within the levels we can live with: within our limits of accommodation. There is a great deal of difference between a set of objectives (or values, or even criteria) that is agreed upon and one to

which we accommodate ourselves. Agreement implicitly discards wider societal, organizational, and all too human interests and, at the limit, paints the picture of automatons towing the line. In the human world, accommodation is where it's at, as is obvious within the most fundamental decision making unit of society: the family.

In truth, and notwithstanding linguistic habits, we do not reach agreements: we reach accommodations. The first norm of comprehensiveness – namely, what is pursued is an agreed set of consistent objectives - serves as a façade, behind which very tough choices are being made as to which objectives are to be pursued *more* than others. It is neither a question of agreement nor one of mutual exclusion. Marginal adjustments are made on all objectives. Contrary to comprehensiveness, the issue is not whether everyone agrees to the objectives, but whether full representation of views is allowed that can guide their marginal adjustment. There are no practical objectives independent of such adjustments: both inform each other. As such, decisions are based on a mix of objectives and adjustments and, at any one point, there will be marginal gains and marginal losses as the overall policy is pursued.

So, contrary to comprehensiveness, we do not make plans and subsequently sit back and watch them unfold. Decision making is an evolutionary process undertaken through iterative cycles, wherein decisions and actions are taken based on a particular mix of objectives and adjustments, resulting in new opportunities and problems that require further decision making iterations. In this process, neither objectives nor the means to achieving them can be differentiated as easily as comprehensiveness claims: at any one time, objectives for one decision making group are merely means for another. There is a 'constant interplay between ends and means' (Lindblom, 1958) resulting in varying levels of conflict between them as different interests pursue otherwise agreeable abstract objectives.

Such interplay also alludes to the impossibility of adhering to the second norm of comprehensiveness: clearly formulating a decision in advance of choosing among alternative means. The decision to design a side-impact safety car appears to be a clearly formulated decision that invites a subsequent investigation among alternative means. Actually, it is but a clearly worded ideal to which the company can aspire. The actual decision was transformed into a complex of interlocking decisions which, even if clearly formulated, could not be taken *in advance* of choosing among alternative means but *in conjunction* with such choices.

We also saw how, in the beginning, the marketing department appeared to be the single decision making unit – in accordance with the third norm of comprehensiveness. It was, however, quickly reduced to being just another interested participant in the decision making process. It necessarily pursued its objective within a group of interests, all of which could sympathize with its aspiration, but none of which could afford to see this aspiration through uncritically without risking their own individual interests or, even more importantly, those of the organization upon which they depended.

The seemingly downward spiral into which we are tossed by the necessity of negotiation with, and critical feedback from, interested parties is, in essence, a dynamic of knowledge exchange. No one party is privy to comprehensive knowledge.

It needs to consider its own knowledge in conjunction with that of other parties. The coming together of different specialized fields provides the only dynamic that can hope to achieve anywhere near a comprehensive understanding of the situation at hand. The fourth norm of comprehensiveness – that the decision should be based on a comprehensive consideration of variables – is only possible by inviting into the process those parties that have knowledge of the different relevant variables. The marketing department knows little about the engineering of suspension systems; the financial planners are less concerned with market share and more concerned about the value of shares; the public relations people will lap up any idea that ensures greater customer satisfaction, and so on and so forth. Any hope of comprehensiveness does not rest with a single decision making unit: it depends on the involvement of multiple such units.

Lindblom has been unduly criticized for smudging the neatness of comprehensiveness with this messy pluralism. But, in effect, the neatness is a fiction and the reality is messy indeed. On the one hand, if we want to aspire to comprehensiveness, we must necessarily engage with those who hold information to which we are not privy. On the other hand, the moment such engagement takes place, what ensues is less a collation of information in the service of comprehensive knowledge and more a process of negotiation underpinned by political power-play. Lindblom does not put forth incrementalism as a means toward comprehensiveness. But he does bring us down to earth. Whether we attempt comprehensiveness or engage in the realities of incrementalism, we are doomed to make decisions in the absence of clear facts, in the first case due to empirical infeasibility, in the second due to socio-political dynamics. The merits of incrementalism are that it not only reflects the decision making reality, but that its decentralized pluralism, coupled with its evolutionary iterative process, is closer to the democratic ideal held by comprehensiveness itself.

In this light, moreover, we see that, contrary to the fifth norm of comprehensiveness, no one single body coordinates the process. To be sure, some coordinating party might be assigned, but its power of coordination depends more upon the initiative and intentions of the different parties and less upon some supposed ability to coerce coordination (which in any case only results in a state of temporary obedience). Coordination, like Adam Smith's 'invisible hand' hovering above decentralized capitalism, does not dictate. It is an emergent property of the manner in which the related interests ultimately play out, 'a by-product of partisan group activity' (Lindblom, 1958). Whether this pluralistic process results in good or bad decisions, conservative or sweeping changes, is neither here nor there. No one decision theory can guarantee any outcome, and incrementalism does not pretend to offer such guarantees. It merely spells out the existential reality that lies behind the rational veneer that is applied, as Pfiffner (1960) points out, after the fact.

Lindblom's thesis suggests that decision making norms should be forged from rubber rather than from steel (Pfiffner, 1960). In contrast to decisions that target long-term comprehensive objectives based upon equally comprehensive amounts of information, incrementalism recommends short-to-medium term decisions based upon an information base limited in scope and liable to socio-political power-play.

Although it reflects decision making reality, it is hardly good news for normative decision theory whose task it is to help decision makers overcome and improve that reality. What is more, Lindblom did not offer a methodological substitute for the steely rigor presumed by comprehensiveness. Praise of sorts came from Etzioni (1967, 1986), who advocated that a mixture of comprehensiveness and incrementalism was an even more accurate description of decision making reality. If by this he meant that one should never lose sight of possibilities on the horizon, even as one is ‘muddling through’, such looking-out is already implicit in Lindblom’s thesis. And even Etzioni failed to offer a formal methodological substitute that could be adopted as a guide for decision making. In brief, decision theory continued its normative tradition and is today famous for its complex mathematical models geared toward problem solving, whilst their inherent problems are, in the main, conveniently ignored.

Interestingly, Lindblom never claimed that problem solving was key. He subtly pointed to a different requirement: that the mess in which decision making finds itself requires *structuring* (Lindblom, 1958) – a point on which he did not further elaborate. Now, decision making is not commonly associated with a problem *structuring* process. It is usually appreciated more as a problem *solving* process. The idea of structuring a mess, particularly as a means for problem solving, does not appear to be central in planning theory. For instance, one does not find distinct methodologies on structuring situations. Planning theory, however, did theorize on what a mess actually is. This in itself was valuable because it helped define what it was that needed structuring and, in turn, paved the way for structuring to be operationalized. True to its name, operational research was to embrace the theory of messes and operationalize their structuring in a methodological manner, leading to innovative planning and decision making approaches...

But we are getting ahead of our story. Long before all of this, operational research was suffering from its own dilemma: it was failing to inform practice, or was even rendering practice unsuccessful, because it found itself preoccupied with too few facts.

Chapter Five – An Eulerian Spirit

In 1956, Jay Forrester joined the faculty at the Alfred P. Sloan School of Management of the Massachusetts Institute of Technology with ‘the planned purpose of searching for and developing the linkages which might exist between engineering and management education’ (Forrester, 1968a). Within two years, he proposed ‘a major breakthrough for decision makers’ in the *Harvard Business Review* (Forrester, 1958). Three years later he elaborated on this ‘breakthrough’ in his book *Industrial Dynamics* (Forrester, 1961). Ever since then, MIT has been the acknowledged center of Forrester’s methodology, which has come to be known as *system dynamics* (Sterman, 2000).

Forrester is considered a leading thinker in a field that has yet to decide by which name it prefers to be known: management science, operations research, or operational research. All three names are currently in use and, for all intents and purposes, are synonyms. We will abbreviate them to MS/OR. For many outsiders MS/OR is nothing but George Dantzig’s simplex method and the associated mathematical modeling approach known as linear programming. For those inside MS/OR, it is much more. So much more, in fact, that they even lay claim to game theory, which would make economists’ hairs stand on end (was John Nash a management scientist?! No matter such infighting between academic disciplines, the fact is that MS/OR is an interdisciplinary field that draws from mathematics, economics, psychology, engineering, and a broad range of social sciences, in the service of making better decisions. A good review of the field may be found in *The Informed Student Guide to Management Science* (Daellenbach and Flood, 2002).

Forrester, as with many management scientists since Frederick Taylor, saw engineering as holding the key to constructing effective decision support systems. His system dynamics is the application of principles of engineering to management decision making. And like Taylor before him, Forrester was deeply concerned with the state of management decision making, and especially the tools that were available to managers to assist them in their complex task. By the mid-1950s, these tools were formidable indeed. We have already mentioned linear programming and its associated optimization modeling (Dantzig, 1963), cost-benefit analysis (Porter, 1995), decision analysis (Raiffa, 1968; Clemen, 1996), queuing theory (Kendall, 1953), dynamic programming (Bellman, 1952, 1953, 1957), cybernetics (Ashby, 1956), and game theory (von Neumann and Morgenstern, 1944). 1957, moreover, saw the publication of the first management science textbook (Churchman et al, 1957). No matter the power and rigor of these methods, however, Forrester was disappointed.

Forrester (1958) perceived a disturbing compartmentalization in the manner in which management was taught as well as practiced. Managerial issues such as manufacturing, finance, distribution, marketing, sales and so forth were presented, appreciated, and practiced more as separate skills rather than interrelated dependencies. Tools devised for the effective development of one area rarely incorporated the dynamics affecting those areas to which it was related. In other words, specialization within any one department was being pursued with little or no consideration of its effects to, or dependencies upon, other departments. This state of affairs indicated that individual departments were making decisions in the absence of

clear facts. There was a need to integrate and relate the management of diverse activities and departments in order to examine how their otherwise independent behavioral characteristics led to consequences affecting the entire organizational system. Only with such a relational understanding could decision makers move beyond making decisions in the absence of clear facts.

As a management scientist, Forrester was especially disappointed with the trends within his own field, and the opening pages of his *Industrial Dynamics* outline the faults he perceived. MS/OR dealt with problems of operating departments with no consideration of the problems faced by top management, in other words strategy. It barely touched upon the time dimension affecting any individual decision, virtually ignoring time-dependent evolutionary changes that preoccupy managers. It maintained a practice of formal mathematics and logic unrelated to the real motivations of managers or to their need for useful solutions to real problems. It adopted modeling assumptions with questionable justifications, and its models themselves rested upon formal and unrealistic foundations, leading to elegant but otherwise irrelevant solutions. Indeed, the greater scope of managerial problems involved improvement and precluded elegance or optimization, these being misleading goals. As a result, MS/OR's mathematical complexity led to an aesthetically harmonious model-world devoid of practical interest. In brief, any decisions recommended by an MS/OR model were doomed to be decisions in the absence of clear facts.

We can add something else to Forrester's perceptions. The optimization approach, for which MS/OR is famous, implies, by the very term *optimization*, that, given the required information, MS/OR can identify the one best configuration for action to a problematic situation. In actual fact, for any situation amenable to optimization modeling, a number of optimal solutions is available, each offering respectively different configurations for action. The reason is that any one optimal solution depends upon the criterion used to find it. A cost criterion will yield a configuration that minimizes costs. A revenue criterion will yield a configuration that maximizes revenue. These configurations will not necessarily be the same, and most often will be different. By implementing the minimal-cost configuration, one does not guarantee revenue maximization; and by implementing the maximal-revenue configuration, one's costs might increase beyond appreciable levels. Which one is the best choice? Given the very reasonable commercial desire to simultaneously increase revenues *and* minimize costs, this becomes an impossible question. Of course, we can introduce additional constraints in the model by stipulating tolerated cost levels *and* desired revenue levels. Providing such constraints do not impact each other unfeasibly, this will yield yet a third configuration. This time, however, it will not be cost-minimal or revenue-maximal. It will be an optimal solution within certain defined limits, what is known as a local optimal instead of a global optimal. Now we are forced to choose between achieving minimal costs, maximal revenues, or middle-of-the-road costs and revenues. The problem with the third choice is that it begs the question: could we not be decreasing costs, or increasing revenues, a little more? Given this, are we anywhere nearer finding an optimal solution *per se*? Are we even stipulating our objective correctly? Is our mission about minimizing costs and/or minimizing revenues, or is it about ensuring repeat business so that we can live to see

numerous fiscal years? To what degree, in other words, is the optimization approach helping to address the dynamics of reality? And, given that the approach poses more choices and questions than concrete solutions, to what degree does it alleviate the specter of making decisions in the absence of clear facts?

For Forrester, the field of management was constituted by two camps of people who were supposed to be working together for the benefit of decision making as a whole, but who were instead living in separate worlds. Managers were haunted by financial results, a fickle business press, and the particularities of nonlinear and systemic dynamics arising from the interactions between human resources, materials, decisions, and money. Management scientists were haunted by their search for problems to fit the available mathematical tools, by their discarding of admittedly crucial but otherwise un-modelable phenomena, by their reputations based on published journal papers, and by their ignorance of, or inability to model, nonlinear behavior. What management needed was a management science that spoke to its reality, in the same way that natural science speaks of the real natural world. And like natural science, management science needed to develop principles and structures integrative of otherwise different managerial situations, the use of which could assist management in making more effective decisions.

As an illustration of this need, Forrester (1968b) noted that any one decision is not an isolated issue but is one part of a decision making system, whose other part is information. Both, decision and information, are what constitute decision making. As such, decision making itself is not an act exemplified by *the* decision. Instead, decision making is a purposeful system, constituted by the decision and by information, whose aim is to change a present circumstance into a new consequence. For Forrester, the two parts of the system were being treated in isolation, either in their academic presentation or in their practical implementation, resulting in inadequate information systems on the one hand, and poor decision making on the other.

For instance, information systems are concerned with the acquisition, processing, and delivery of information. The purpose of delivery is either to control or enhance a decision. Information acts to control a decision in that it will found or justify the decision, thus helping to avoid capriciousness. Information enhances the decision by being correct, accurate or, in general terms, relevant. Studied or implemented *in isolation*, this otherwise obvious understanding of information is strewn with problems. Ackoff (1967) outlines five such problems, three of which are worth some discussion.

To begin with, Ackoff acknowledges that decision makers make decisions in the absence of clear facts. However, he questions whether this absence is 'the most important informational deficiency from which they suffer'. Is it not the case that, too often, decision makers make decisions in the presence of an overabundance of irrelevant facts? In good faith, information systems designers combat the absence of clear facts by acquiring more information, processing it in ever more intricate ways (usually to squeeze out as much detail as possible), and delivering more of it either in terms of frequency or in terms of volume. This practice rings of comprehensiveness and its associated problems discussed earlier. To those afflictions, we can now

explicitly add information overload. The provision of more information does not necessarily alleviate making decisions in the absence of clear facts. In fact, it usually instigates decision paralysis as decision makers are forced to separate or eliminate irrelevant information for themselves, and otherwise sift, search, filter, and condense information – all of which activities should be done by an effective information system! In the scramble to undertake such activities, decision makers inevitably end up making decisions in the absence of clear facts. We see here the double-edged sword of the issue before us: making decisions in the absence of clear facts occurs due to lack of information as well as information overload.

Although at present we are interested in situations lacking information, it is worth noting that information overload is also the product of a methodological quirk. When designing an information system, the designers will naturally begin by asking the decision maker (the end-user) what information is desired. For textbook decisions, such as calculating an optimal marketing mix based upon financial criteria, the answer is easy, because the routine and well-understood nature of such decisions has rendered them largely operational. They are reducible to algorithmic procedures that provide an answer at the press of a button. Indeed, it is expected that information systems designers will include the required data fields and algorithms in the information system as a matter of course. The question concerns what other information a decision maker might desire for those decisions that are beyond the humdrum of everyday operations. What is obvious is that this is an impossible question. In order to cover all bases, therefore, the only viable answer is ‘everything’. A conscientious information systems designer will provide more than everything, in an attempt to ensure that the decision maker will have information for every conceivable situation. Again, what reaches the decision maker is an overabundance of information that inevitably leads to making decisions in the absence of clear facts.

Finally, Ackoff refers to what might be the greatest culprit, in information systems design, to making decision in the absence of clear facts: the assumption that user-friendliness implies that the system does what it is supposed to do. He writes:

Most MIS [Management Information Systems] designers seek to make their systems as innocuous and unobtrusive as possible to managers lest they become frightened. The designers try to provide managers with very easy access to the system and assure them that they need to know nothing more about it. The designers usually succeed in keeping managers ignorant in this regard. This leaves managers unable to evaluate the MIS as a whole. It often makes them afraid to even try to do so lest they display their ignorance publicly. In failing to evaluate their MIS, managers delegate much of the control of the organization to the system’s designers and operators who may have many virtues, but managerial competence is seldom among them. [...] No MIS should ever be installed unless the managers for whom it is intended are trained to evaluate and hence control it rather than be controlled by it.

One need look no further than one’s computer for an illustration of the problem. Microsoft Excel is one of the most popular analytical spreadsheet software packages available. Used by countless decision makers around the world, its pervasiveness is such that innumerable decisions have become dependent upon it. And yet, it is not without significant computational flaws, especially when it comes to statistical calculations - in other words, the types of calculations that are basic to many quantitative analyses (McCullough and Wilson, 1999, 2002, 2005). Unable to

evaluate 'the ghost in the machine' themselves, decision makers inevitably end up making decisions in the absence of clear facts.

Forrester (1968b) was interested 'with what information should be available at a decision point and the consequences of defects in the information'. More exactly, a decision point, participating in a realm or system that included other decisions, required information concerning the consequences of its influence upon, and its affect from, those other decisions that were being simultaneously undertaken. Traditional MS/OR, with its focus on a singular objective function, assumed a static state of affairs, or at best statistically probable states, with little or no explicit regard of the interactive dynamics to which interrelated decisions are prone. As highlighted earlier by Lindblom, decision makers cannot, should not, and most often do not, take decisions that are independent of each other, because they are immersed in dynamic situations that consist of complex interactions involving continuously changing problems and decision making adjustments (Ackoff, 1979). Notwithstanding the powerful developments of analytical tools, Forrester perceived a need to formalize the kinetic nature of the situations faced by decision makers.

Decision makers needed a decision support tool that could interrelate multiple decision points and information streams. A model of such interrelations would allow decision makers to examine how one decision, or a set of decisions, affected, and could be affected by, the situation as a whole. The model would thus assist in making situational improvements. What would further differentiate Forrester's approach from the MS/OR of the day would be its explicit incorporation of time as an affecting variable.

MS/OR was no stranger to the importance of time. For example, by 1959 the critical path method was well-developed, incorporating two starting times and two finishing times for undertaking any one activity in a project constituted by interrelated activities (Kelley and Walker, 1959; Kelley, 1961). A feature of the method was its ability to identify the critical path of activities on which any holdups would delay the project as a whole. Such incorporation of time, however, treated it as a static variable or, at best, as one which behaved according to a statistical distribution of possibilities. By contrast, Forrester's dynamic treatment of time would make explicit the fluctuating delays, accumulations, amplifications, and flow rates of materials, money, human resources and so forth that impact a decision making situation. His approach was less concerned with time-management (a central concern in critical path analysis) than with demonstrating how time (especially delays) caused knock-on effects that led to counter-intuitive consequences and inadvertent backlogs and bottlenecks. By rendering such consequences explicit, decision makers could learn about problematic situations even as they were making decisions to change them.

For Forrester (1968b), 'industrial dynamics would [help] design policy and its relation to information but might not say how best to implement the individual decisions'. He was concerned 'not with how actually to process the information' but with its contents and how it flowed into decision points. 'Management science,' he wrote (1961), 'does not mean automatic management. It means a platform from which to reach further by the exercise of managerial intelligence and judgment.' His was not an attempt at information system design or decision making automation –

two aspects which MS/OR was explicitly pursuing. The focus was much wider: on the interconnections and effects between various information flows and decision points which, when made explicit, could enrich intelligence and refine judgment. Unconcerned with the design particularities of any one information system, and not aspiring to a taxonomy of decision types, he pursued instead the generic variables relevant to any decision making system, that is, to any situation constituted by decision points as well as information flows. If these variables could be identified and related, they would be applicable to otherwise different decision making situations, resulting in a truly integrative leap that would do justice to the name *management science*.

One must not underestimate what Forrester had in mind. Consider mathematics. As we go through school and college, we are introduced to trigonometry, calculus, infinite series, and imaginary numbers. These are, more often than not, presented and taught as distinct areas in mathematics. Distinct they may be, but unrelated they are not. And yet, the relations between them are rarely taught or presented. As such, the teaching of mathematics is prone to the same compartmentalization suffered by the teaching of management, and perhaps a majority of disciplines suffer from this trend. That a deficient educational system compounds this slide-show approach is not in doubt. Mathematics, however, is especially good at integrating its distinct branches, if only we know where to look. It is not a question of drawing links between aspects that inform each other. Rather, it is a question of identifying an essence that integrates distinct areas in an indivisible manner. For the respectively huge fields of trigonometry, calculus, infinite series, and imaginary numbers, such an integrative base is an equation known as Euler's Identity: $e^{i\pi} = -1$. Simple in form, integrative in structure, rigorously proven, and with applications too numerous to list here, this result may be appreciated on various dimensions - not least of all aesthetics since it relates five of the most important constants in mathematics (zero is implied if you add 1 to each side of the equation), three fundamental mathematical operations (addition, multiplication, and exponentiation), and the basic relation '='. In the same way that the Parthenon symbolizes and founds multiple dimensions of western civilization, Euler's Identity sweeps across centuries of mathematical developments, past, present, and future.

We must not take the analogy too far, but the spirit of Forrester's enterprise is clear. It is a search for the essential structural elements that are commonly present in otherwise different decision making situations. We have already seen that Lindblom himself noted the importance of identifying structural features in problematic situations. If a common structure in otherwise different situations can be identified, their resolution becomes all the more easier. Indeed, without common structures in otherwise different environments, human beings would find it very difficult to make their way in the world – something already hinted at by Miller in his discussion of recoding. Identifying common structures in an otherwise particular situation alleviates somewhat the risk of making decisions in the absence of clear facts.

For Forrester, there are three generic structural elements: levels, flows, and rates. He claimed that no matter the complexity of any particular problematic situation, no matter the wealth of detail involved, and no matter its scope, 'its fundamental nature'

will still be constituted by levels, flows and rates (Forrester, 1961: 67). Moreover, he claimed (1961: 67) that a model of a situation built using only these three elements is:

- able to describe any statement of cause-effect relationships;
- simple in mathematical nature;
- closely synonymous in nomenclature to industrial, economic, and social terminology;
- extendable to large numbers of variables (thousands);
- able to handle continuous interactions (that are unaffected by artificial discontinuities like, say, the limits of computer power); and,
- able to generate discontinuous changes in decisions when these are needed.

Now, that's a tall order from the application of just three concepts – as tall as unifying four huge mathematical fields in a simple equation. Given the variety of decision making situations that face human beings everyday, situations that are far removed from the neatness of mathematics, is not this a somewhat preposterous claim? There is no theory equivalent to a mathematical proof that can justify it. Forrester, however, provides ample empirical examples of the generic value of these three structural elements. Let's consider a simple situation just to get the feel of how levels, flows, and rates work.

A bus company operates a route through the city center. At any one time, it has a stock of buses (a level) in the depot, ready for use, and a certain number of buses in service (another level), that is, operating on the route. The rate at which it makes buses available to the route depends upon the number of people waiting at bus stops along the route, in other words it depends upon the information regarding demand (another level). This decision, and its dependency upon demand, is illustrated in Figure 1.

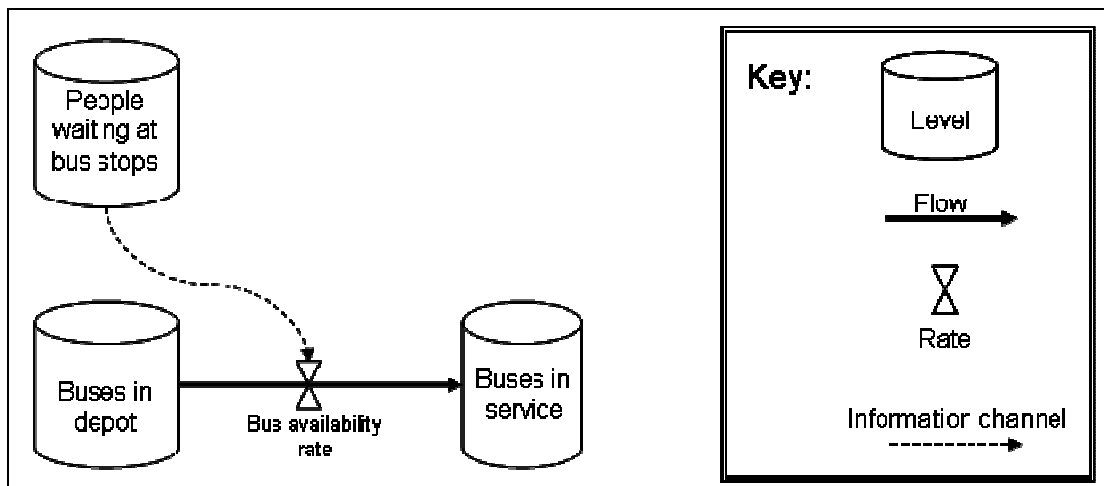


Figure 1: System dynamics model of bus company example, with accompanying key

Now, the rate at which people board buses, that is, depart from the bus stops and head toward their destinations, depends upon the number of buses in service along the route, and the average transit delay (measured by kilometers per hour). Less (more) buses in service, and a greater (lower) average transit delay, lead to a slower (faster)

departure rate of passengers, and more (less) people waiting at bus stops. The average transit delay is measurable periodically and can increase as well as decrease. It is therefore another level. This understanding is added in Figure 2.

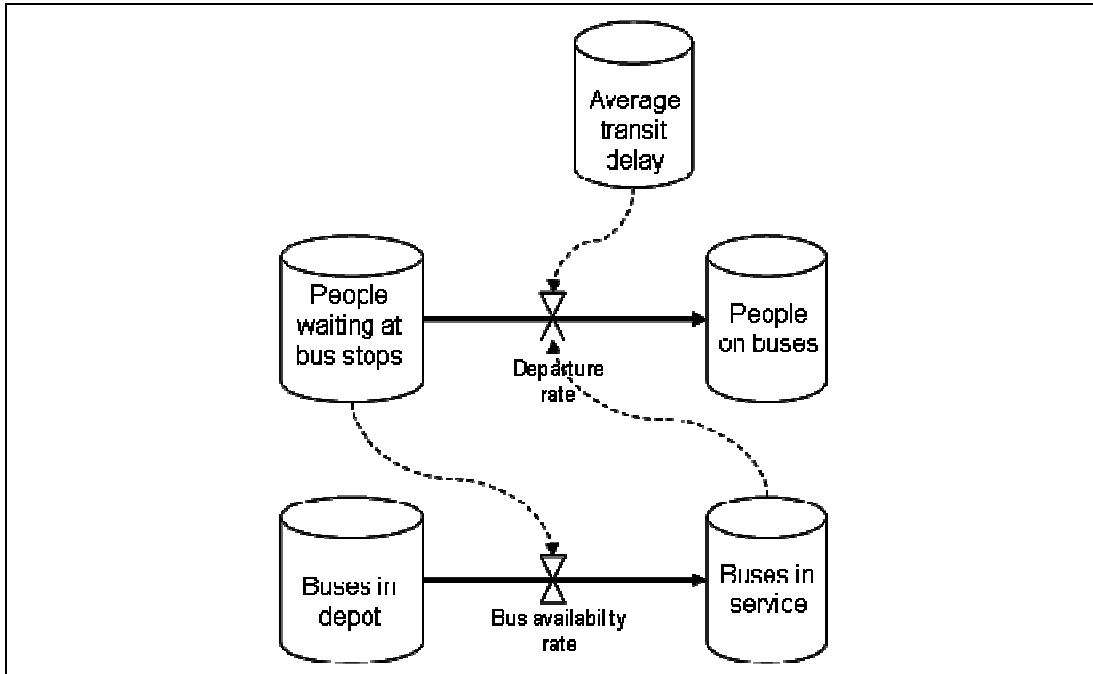


Figure 2: Developed system dynamics model of bus company example

Once on board, the rate at which people arrive at their destinations depends, once again, upon the average transit delay. Once arrived, the passengers leave the stock of people on buses and join another stock: the stock of people that have arrived at their destinations. This final part of the public transport dynamics we are considering is added in Figure 3.

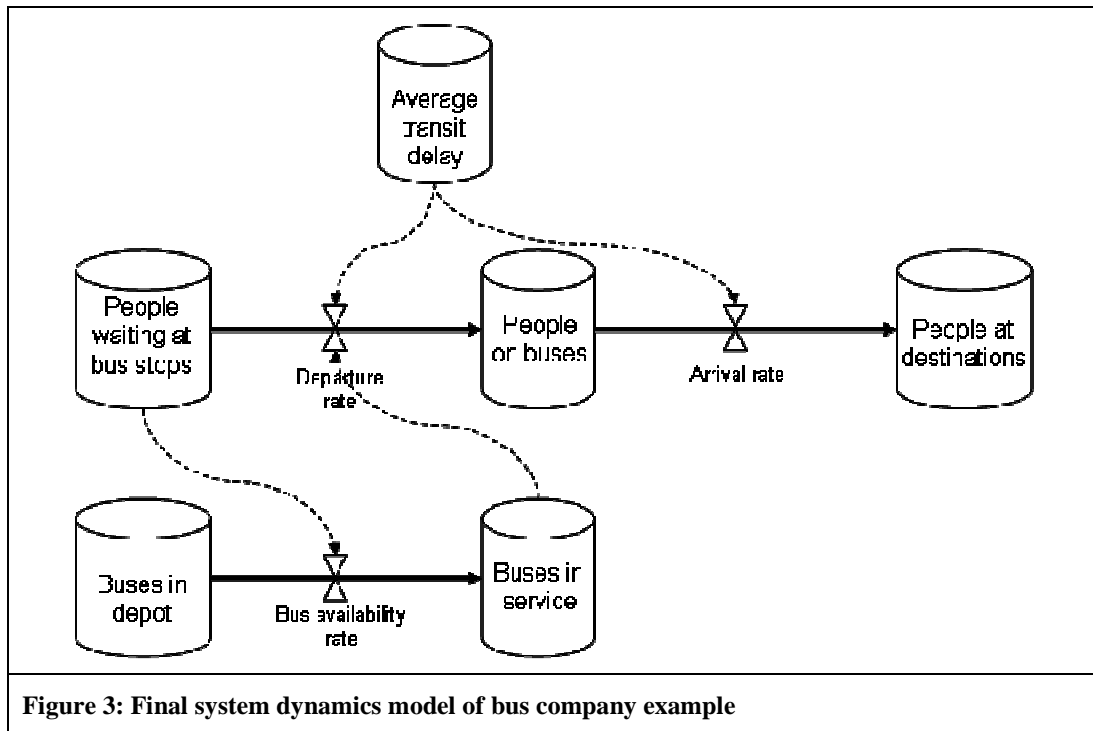


Figure 3: Final system dynamics model of bus company example

Figure 3 shows that the situation in question is constituted by two distinct flows: people on the top and buses on the bottom. Flows are always distinct and are constituted by respectively different variables. However, they may be related through information channels, as the figure shows. Indeed, for Forrester, an information network always forms the underlying relational infrastructure of any system constituted by multiple flows of respectively different entities: it is the thread that weaves the interdependencies between the flows.

The bus company makes buses available depending on the information it receives about demand. More demand leads it to make available more buses, exactly like opening a tap to allow more water to flow. The departure rate ‘tap’ shows that the flow of people from bus stops to buses depends upon the number of buses available and the average transit delay. The arrival rate ‘tap’ show that the number of people arriving at their destinations depends, again, upon the average transit delay.

Forrester (1961: 67-69) notes that decisions are where the rates are, that is, the ‘taps’. In the case of the bus company, this is easily seen. The bus availability rate reflects the company’s decision as to how many buses to make available. Given a certain demand (information), it will release buses at a certain rate (six per hour, for example). This illustrates Forrester’s thesis that decision making is a system constituted by a decision in conjunction with information.

Now decisions might be where the rates are, but the departure rate of passengers is not easily interpretable as a decision to ride a bus. It more obviously shows the dynamics involved in the flow of people from bus stops to buses: the rate of flow depends upon the number of buses available and the average transit delay. Where, one might ask, is the decision? Does this ‘company of people’ *decide* how many of its

constituents will board a bus based upon information collected regarding the number of operating buses and the average transit delay? This seems rather far fetched.

Let's reconsider. At some point, outside of the model, people decide to go to a bus stop to wait to board a bus. This leads to an increasing level of people at bus stops. No balking is accounted for: once at the bus stop, you are there until a bus comes along. Now, you obviously decide to get on a bus *when it arrives at the stop*. And that is the point: your decision is actually dependent on other factors such as the average transit delay and the number of buses operating on the route. More exactly, as a prospective passenger stuck at the bus stop, you actually defer your bus-boarding decision to those decisions taken by the bus company, the innumerable road users, and the traffic management authority (the latter two being responsible for the average transit delay). In a similar way, your decision to arrive at your destination (the arrival rate 'tap') is actually deferred to those decisions that have led to the average transit delay.

So, we can read rates as decisions, but some rates represent decisions somewhat more clearly than others. To be fair, we have already noted that Forrester is concerned neither with how information is actually processed nor with how decisions are implemented. What Forrester's approach does allow us to show is how rates enable various flows to occur depending on states throughout the system. We thus obtain a structure of the dynamics involved in the system and, based upon an understanding of these dynamics, we can decide how to improve the system. In the case of the bus company, improvements can be made based upon reliable information about demand. In the case of bus users, they can improve their choice of public transport by acquiring information on the number of buses operating on the route and the average transit delay.

Given that Forrester's is a methodology about relational arrangements, to focus upon one rate and ask 'where is the decision?' is to miss the point. Consider carefully that the methodology allows you to structure distinct flows and their connecting information channels in order to understand what information influences flow dynamics. It is true that such information leads to decisions that make things happen. But the notion of decision in the form of a 'tap' reflects more a response to particular circumstances than a pro-active will that decides to do something:

We note that we are viewing the decision process from a very particular distance. We are not close enough to be concerned with the mechanisms of human thought. We are not even close enough to see each separate decision as we ordinarily think of decisions. We may not be close enough to care whether one person or a group action creates the decision. On the other hand, we are not so far away as to be unaware of the decision point and its place in the system. This proper distance and perspective is important to our purposes. We are not the psychologist delving into the nature and sources of personality and motivation, nor are we the biophysicist interested in the physical and logical structure of the brain. On the other hand, we are not the stockholder who is so far from the corporation as to be unaware of the internal structure, social pressures, and decision points.

Our viewpoint is more that of the managerial superior of a particular person who is charged with certain responsibilities. The superior is close enough to know how desired goals are established. He is in a position to observe and probably provide the information sources to be used by the subordinate to determine his concept of actual conditions. He

knows in general the guiding policies and the manner in which the subordinate decision maker would respond to various kinds of circumstances. (Forrester, 1961: 96)

Forrester is quite clear: a system dynamics model can locate decision points (as rates), structurally contextualize them (in relation to other decision points, levels and flows), and show the pressures that act upon them. Whether it can show the *social* pressures, as Forrester claims, depends upon how you interpret the term. That the decision to make more buses available is based upon a demand that is, of its very nature, social is correct. But the modeling nature of this social demand is, at root, functional. Demand grows, more buses are made available. There is nothing more social in the model than this. Forrester's claim that system dynamics can model social pressures actually refers to the ability of the approach to model the observable behavioral stimulus-response dynamics of a situation. Any wider social pressures, of the type referred to by Lindblom for instance, emerge after decision makers have studied the model, learnt about the situation, and engage in decisions to improve the situation, in other words, engage in a political-economic negotiation process for which the model can serve as a common reference.

System dynamics, then, is a decision support tool that shows responses based on various stimuli. As a means for offering decision support, its underlying assumption is that effective decisions can be taken based upon a stimulus-response understanding of the world. In other words, the underlying philosophy of system dynamics is one of functional behaviorism.

Now, we must not minimize the advantages here. For this underlying philosophy enables system dynamics to indeed describe cause-effect relationships. These are, moreover, amenable to the differential equations constituting the motor of computerized simulations of system dynamics models. The three concepts of levels, flows and rates are ubiquitous in industrial, economic and even social contexts. In addition, these concepts can be used to reflect thousands of different types of variables (levels of personnel, flows of sales, delays, etc). Given enough computer power, a system dynamics simulation will generate continuous interactions between flows and between levels in any particular flow. And, finally, the methodology's use of rates will account for discontinuous changes in decisions when these are required, simply by opening or closing the 'taps'.

So Forrester's claims are not such a tall order after all, which probably accounts for the popularity of system dynamics as a decision support tool. It is able to describe generic cause-effect relationships using only three types of variables amenable to mathematical equations that serve to motorize computerized simulations which, in turn, allow decision makers to study time-sensitive, dynamic effects of interdependencies. Crucially, Forrester did not abandon the comprehensive ideal. Instead, he refined an understanding of, and approach to, comprehensiveness: system dynamics is able to be comprehensive in scope (that is, to sweep into focus an ever larger number of causes and effects) without being comprehensively detailed (due to its adherence to only three concepts). Years later, in his 'requiem for large-scale models', Lee (1973) would recommend exactly this approach to comprehensiveness, although, for him, system dynamics 'exhibits most of the major flaws of large-scale models'. Notwithstanding this critique, the practical refinement offered by Forrester

indicates that the comprehensive ideal might not necessarily be self-defeating. Indeed his work shows how we can aim for the comprehensive ideal through different means.

Forrester opened the horizons of MS/OR by demonstrating that it is not simply the content of decisions that is important but their relational connections. Of course, von Bertalanffy had begun to outline this importance as far back as 1926 (Hammond, 2003: 109; von Bertalanffy, 1968), but Forrester's work brought it to the forefront of MS/OR. In addition, Forrester was the first to introduce the computational possibilities for modeling decision making relations on computers. This was a big advantage, for it is one thing to talk about such relations and quite another to actually see them simulated on a computer screen. It is also worth noting that, despite a focus on quantification and computation, system dynamics has not ignored the intellectual risks of relying singularly upon computerized analysis. Since Forrester's initial work, the field has evolved beyond computerized modeling, aiming specifically to develop thinking, as opposed to computational, tools by devising approaches to mental modeling and learning (Wolstenholme, 1990). A number of generic problem types (known as 'system archetypes') have also been formally described, providing further assistance for thinking through a problematic situation (Kim and Anderson, 1998; Hallsmith, 2003). Most importantly, perhaps, system dynamics has placed feedback operations in the forefront of decision makers' minds.

Of course, system dynamics has attracted criticism. Its models are data-hungry – as demonstrated even in the simple bus example. It is questionable whether less quantifiable issues, such as 'awareness levels', can be modeled quantitatively, as Forrester (1961, 68a,b) proclaims. Forrester's 'managerial superior' perspective, cited above, might be too far removed from the complexity of reality, and far closer to Tinbergen's norms of comprehensive decision making critiqued by Lindblom earlier. Functional behaviorism is a valid lens from which to analyze the world, but it suffers from the weaknesses of stimulus-response psychology. Jackson (2003: 80) notes that even system dynamics' more qualitative causal loop diagrams are problematic decision support tools. He adds that system archetypes are too simplistic and not that enlightening to decision makers 'who are well aware of them as well as of the myriad of other pragmatic, cultural, ethical and political factors that prevent them acting in the rational way prescribed by system dynamics'.

We can also question the validity of the Eulerian spirit in Forrester's enterprise. The world is a complex place, and its complexity is not merely due to interrelations between levels, flows and rates. Euler's equation unifies diverse fields in a far more rigorous manner than any empirical claim regarding the prevalence of three concepts in otherwise different decision making situations.

Furthermore, a focus on levels, flows and rates is a focus on content, on *what* should be included, on *what* the inputs are. In mapping information channels between otherwise different flows, system dynamics does offer an innovative and insightful modeling technique. Its effectiveness, however, is directly proportional to its ability to access better or more information. Lacking such access, one is hard pressed to find how levels, flows and rates can be of any use. Forrester may have identified how decisions recommended by the disjointed MS/OR models of his day were doomed to

be decisions in the absence of clear facts. But his system dynamics alternative did not directly address *how* to make decisions in the absence of clear facts. Instead, it called for a selection of additional information to be taken into account. Although such selective additional information enabled a more comprehensive scope without requiring comprehensive detail, reliance on access to such information failed to address the challenge of making decisions in the absence of clear facts. The required epistemological switch from content to process, alluded to earlier, had yet to be addressed.

It is strange, therefore, to find Forrester writing that:

The power of system dynamics models does not come from access to better information than the manager has. Their power lies in their ability to use more of the same information and to portray more usefully its implications.

Forrester contends that system dynamics models enable the decision maker to use, with greater effectiveness, whatever limited information is available in a problematic situation, and in addition they help portray the implications of this limited information more usefully. Given that this minimizes the need to gather additional information – a costly, time-consuming process that requires an entire round of decision making prior to tackling the problem or opportunity for which information is sought (Grünig and Kühn, 2005: 181-195) - an implicit claim is simultaneously posited: system dynamics models are not only effective but are also efficient decision support systems. On the other hand, the critique of system dynamics points to its hungry data requirements, to its difficulty in modeling qualitative impacts, to the shadow of comprehensive norms that hangs over the approach, to its stimulus-response tendencies, and to its content orientation. None of these alleviate the dilemma of making decisions in the absence of clear facts. Forrester's claim is questionable.

Behind the sales pitch for system dynamics models, however, Forrester's claim is important in what it implies. Faced with a problematic or pivotal situation, the effectiveness of a decision maker is not demonstrated through access to better or more information. The effectiveness of a decision maker is demonstrated in an ability to use, more resourcefully, whatever limited information is available, and to portray its implications more usefully. This decision maker demonstrates such effectiveness irrespective of whether system dynamics is used or not. For, the decision maker's methodological thinking process itself enables the effectiveness in question. In a world where 'the ability to learn *faster* than competitors may be the only sustainable competitive advantage' (de Geus, 1988), this decision maker may likely be the key to the survival of any organized entity (corporate or otherwise). Such a decision maker is more valuable than any artificial decision support system.

From Forrester, in other words, we get an inkling of what is required of a decision maker in order to effectively make decisions in the absence of clear facts: *effectiveness under such conditions is not measured through access to better or more information, but in an ability to use more resourcefully whatever limited information is available, and in an ability to portray the implications of this information more usefully.*

In this is sowed the seed for making decisions in the absence of clear facts. It would take twenty years for it to bear fruit, and another ten to bring the fruit to market.

Chapter Six – The Wicked World

Developed from engineering principles, applying mathematical formulations, mesmerizing in its use of fast-evolving computer power, and perceived as traceable to the safe objectivity of Newtonian science, Forrester's (1961) *Industrial Dynamics* quickly became a beacon for scientific decision making. Its approach to problems as dynamical systems cemented the term *systems analysis* and, given that Forrester viewed *Industrial Dynamics* as a contribution to management science, *systems analysis* joined the existing triumvirate of synonyms we abbreviated earlier to MS/OR.

A wave of optimism about 'professional' decision making hit the 1960s. For his part, Forrester (1961) chose to include, in his own *Industrial Dynamics*, a thick section on how his approach would enhance management education, and eventually claimed the practice of industrial dynamics to be 'an entire profession' complete with 'internship' (1968b). The greater wave was reinforced by such influential theorists as Chandler (1962), Drucker (1964), and Ansoff (1965), by real-life examples of analytical accountants-turned-decision makers such as Geneen at ITT (Schoenberg, 1985), and by the consolidation of business schools as valid centers for the training of decision makers, most notably through the pursuance of the MBA degree – a degree that supposedly trains for making better decisions. Membership numbers rose in societies such as Operational Research Society of America (ORSA), The Institute of Management Sciences (TIMS) and the British Operational Research Society (ORS), all of whom defined themselves as 'professional' and 'scientific'. Given the numerous advances in decision support techniques that had quickly emerged since the late 1940s, MS/OR increasingly asserted itself, demanding the respect usually afforded to established professions such as the law and medicine. If at one time planning theory had hinted at a brave new world through its comprehensiveness, by the mid-1960s MS/OR appeared to be superseding all expectations. Business schools, learned societies, and the corporation (the defining institutional edifice of the century) all looked to MS/OR as some sort of 'great white hope', as the tangible fruit of that great American philosophy known as *pragmatism*, whose merits (and tragedies) gave rise to 'the organization man', described poignantly in (of all years!) 1956 by William Whyte (1956).

In the beginning of the 1970s, Churchman, a leading surfer on this wave, reflected upon his 'profession' in hardly flattering terms. No matter its professional aspirations cloaked in scientific method, Churchman (1970) highlighted that MS/OR was, at base, 'a branch of applied mathematics called optimization theory'. We can play around with levels, flows and rates all day but, unless an optimal solution is forthcoming, who is to say that these models are any better to any other ones? Optimality, by definition, provides this assurance, and assurances make professions. Churchman, however, was well aware of optimization's weaknesses. On the one hand, he argued, optimization theory relied (somewhat naively) on a centralized decision making locus, and built models that digested information 'from all sources' in an attempt to 'take on the whole system'. On the other hand, a management scientist was not expected to be totally comprehensive if modeling assumptions could at least remain consistent. And if such consistency was to draw charges of

inflexibility, Churchman (1970) countered by writing that ‘OR practitioners realize the importance of maintaining at least the appearance of local [read: Lindblom’s ‘pluralistic’] decision making’.

There is something very disturbing about an aspiring profession when one of its leading thinkers describes it as interested in keeping up appearances. At best, MS/OR, for all its decision making potential, was confused as to how best to steer through the comprehensive-incremental dichotomy. At worst, it was becoming what Albert Camus (1979: 146) once noted of Paris: ‘a marvellous cave, and its men, seeing their own shadows reflected on the far wall, take them for the only reality there is’. As for its ‘professional’ aspirations, the merits and relevance of this goal remain, to this day, ‘a festering sore’ (Kirby, 2006).

Notwithstanding Churchman’s reflections and the interpretations we draw from them, timing is of the essence. The oil crisis of October 1973 was less than three years away. Relative to what lay ahead, the world was still beautiful and blue, calm as a clam, and in many ways predictable. Under such circumstances, any field could afford some navel-gazing within its ranks, enjoy a wry look at its darker side, and invariably plough on in its self-appointed importance. With the oil crisis, MS/OR saw the advent of a new world order where chaos, complexity, uncertainty, and conflict became the key working concepts. But even if they had yet to be properly expressed, the relevance of these concepts was recognized well before October 1973, and not by management science but by management proper. Admittedly, the cry was faint and shrouded within a pedagogic context; but it was published in the *Academy of Management Proceedings*, a scholarly voice of those decision makers who relied on MS/OR decision support tools - and its message was clear.

Some time in the early seventies, studies at California State University and the State University of New York, published by Belasco *et al* (1973) in the *Proceedings*, uncovered certain simultaneous situational characteristics that decision makers commonly faced:

1. the task is ambiguous;
2. the structure through which the task might be accomplished is loosely defined;
3. the standard against which success is to be measured remains unstable;
4. knowledge of the organizational and wider environments remains uncertain;
and
5. the opportunities for collecting more data/information/ facts are constrained.

Today, lists such as these are written on the whiteboards of MBA lectures, as students are told that the contemporary world is much more chaotic, complex, and uncertain than the recent past. It is revealing to see that the pre-October 1973 world was no different. What is clear is that ambiguous problems, loosely defined resolution means, shifty success criteria, incomplete knowledge, and constraints on data gathering created less of an opportunity, and more of a problem, for the application of the management science of the day.

Belasco *et al* designed the five characteristics into what is known as a problem-case instructional approach (Böcker, 1987; Cochran, 2000; Georgiou et al, 2008). Students were immersed in a problematic situation exhibiting the five characteristics and asked to (a) identify the critical issues, (b) decide what methods are appropriate and use them, and (c) ultimately interpret the results of analysis and suggest a plan of implementation. The aim, in other words, was to teach decision making effectiveness in situations characterized by limited information, where time and resources are no longer available to collect more information, yet where a resolution is nevertheless required based upon the information available – in other words, situations that require making decisions in the absence of clear facts.

The studies indicated that substantive decisions were possible under such circumstances. Such indications, however, were vague, and Belasco *et al* provided few details concerning how to make decisions under the stipulated circumstances. More importantly, the authors did not address the quite legitimate question of what form these decisions can take. Thirty years later, Cochran (2000) and Bell and von Lanzener (2000) provided an initial answer to this question in the context of controlled training environments: the objective with the problem cases characterized above is not so much to *solve* them, but to *plan* for the immediate future. Decisions in the absence of clear facts, therefore, come in the form of plans – what may be termed *planning as decision making*.

Still, when faced with making decisions in the absence of clear facts, how is it possible to yield robust and useful plans when, in general, such robustness and usefulness is a function of the availability of information/facts? At the time of the Belasco studies, MS/OR had no answer. If it was paying any attention to the *Proceedings*, it would have noted a set of characteristics constitutive of situations that require making decisions in the absence of clear facts. It would have further noted that such situations lend themselves to decision making in the form of plans of action where, due to item 5 above, the action(s) to be planned is(are) more sophisticated than simply asking how more information can be obtained. If, moreover, opportunities for collecting more data/ information/facts are constrained, MS/OR would have noted that the decision maker – and, equally, the instructor attempting to teach decision making for situations lacking clear facts – faces a non-trivial task. It would take another six years (Ackoff, 1979) before MS/OR eventually took such notes.

Planning theory, however, had a complete diagnosis – though not solution - at hand. A few months before the oil crisis struck, Rittel and Webber (1973) struck out at the Forresters and Tinbergens of this world. They acknowledged that the pursuit of comprehensiveness and the rise of systems analysis had provided a:

growing sensitivity to the waves of repercussions that ripple through systemic networks
[...] We are now sensitized to the waves of repercussions generated by a problem-solving action directed to any one node in the network, and we are no longer surprised to find it inducing problems of greater severity at some other node.

No matter this increasing awareness, however, the authors questioned whether the prevailing optimism of the time was actually able to deal with such repercussions. They noted a paradox in the prevailing faith. On the one hand, comprehensiveness

allowed us to ‘expand the boundaries of the systems we deal with’, and, for its part, systems analysis (and especially Forrester’s approach), expanded the methodological and technical boundaries of applied scientific decision making. On the other hand, however, Rittel and Webber noted that, due to such expansions:

it has become less apparent where problem centers lie, and less apparent *where* and *how* we should intervene even if we do happen to know what aims we seek.

They noted an ‘arrogant confidence’ in systems analysts who:

pronounced themselves ready to take on anyone’s perceived problem, diagnostically to discover its hidden character, and then, having exposed its true nature, skillfully to excise its root causes. Two decades of experience have worn the self-assurances thin. These analysts are coming to realize how valid their model really is, for they themselves have been caught by the very same diagnostic difficulties that troubled their clients.

For Rittel and Webber, a major culprit of this state of affairs was reliance on the promise of rationality:

The difficulties attached to rationality are tenacious, and we have so far been unable to get untangled from their web. This is partly because the classical paradigm of science and engineering – the paradigm that has underlain modern professionalism – is not applicable to the problems of open societal systems. One reason the publics have been attacking the social professions, we believe, is that the cognitive and occupational styles of the professions – mimicking the cognitive style of science and the occupational style of engineering – have just not worked on a wide array of social problems. The lay customers are complaining because planners and other professionals have not succeeded in solving the problems they claimed they could solve. We shall want to suggest that the social professions were misled somewhere along the line into assuming they could be applied scientists – that they could solve problems in the ways scientists can solve their sorts of problems. The error has been a serious one.

The authors, being planning theorists, published in the important planning journal *Policy Sciences*, and wrote about ‘open societal systems’, ‘the publics’, ‘the social professions’, and ‘social problems’. Their writing was set in the language of planning theory. We have already noted, however, the synonymous relationship between planning and decision making. With reference to the above citation we can note the following: decision making is a social enterprise irreducible to the classical paradigm of science and engineering, as Lindblom is in pains to show us; its consequences extend to any number of publics, being felt beyond the immediate desires of any one decision maker; and, decision making participates in, and is liable to the impact of, open systems constituted by extraneous variables. In sum, the above is less specifically about planning, and more about decision making in general.

To counter any accusations of undertaking a carpet-bombing campaign against the entire methodological edifice of decision making, Rittel and Webber added a surgical strike:

By now we are beginning to realize that one of the most intractable problems is that of defining problems (of knowing what distinguishes an observed condition from a desired condition) and of locating problems (finding where in the complex causal networks the trouble really lies).

In other words, the authors were not merely dismissive of the prevailing leading methodologies. They questioned the very cornerstone upon which such methodologies were built: the assumption that the problem to be tackled is

identifiable, knowable, recognizable, and definable. Contrary to natural scientists, they claimed, this assumption just does not hold for decision making in the social and administrative fields. Indeed, the greater problem for decision making – immersed, as it is, in a heterogeneous environment – is to define the problem to be tackled in the first place! Even if system dynamics can claim to enable a more exploratory approach - for instance, by offering a visual simulation of a situation instead of forcing a problem definition - its formulation of situations into levels, flows and rates simply transfers the *problematique* of problem definition into the problem of identifying and defining these variables, all the while assuming their generic relevance.

Where Belasco *et al* claimed that decision makers face increasingly ambiguous tasks, Rittel and Webber sourced such ambiguity in the enigma of locating and defining the problem to be tackled. They noted that the ‘systems-approach “of the first generation”’ (i.e. the prevailing methodologies of the time) offered means for analytically weighing alternatives toward some solution, but no guidance for either identifying problems or stipulating goals. These latter two – standing, as they do, at each end of the analytical task - were either assumed as given, or were explicitly left out of decision making methodologies. Due to its rational aspirations, decision making had restricted itself to analysis: *given* a discrepancy between a recognizable problematic situation and a stipulated goal, its self-appointed task had been to find the means to minimize such discrepancy. This assumes that decision makers, and human beings in general, are quite adept at recognizing problems and identifying goals; help is only required for measuring discrepancies between them. A neat division thus prevailed, one that undoubtedly had helped decision making define itself ever more rationally, precisely and professionally.

For Rittel and Webber, however, the world was not so neatly divisible. For them, the systems approach of the first generation had been too ‘docile’ in dealing with the ‘wicked’, ‘malignant’, ‘vicious’, ‘tricky’, and ‘aggressive’ world of decision making. They called for a ‘second generation’ decision making paradigm. Such a paradigm would guide the elaboration of new decision making methodologies. These new methodologies would not be based on the same assumptions as the prevailing approaches. Indeed, they would be based on the absence of these assumptions. The new methodologies would not, for example, assume the existence of objective, logical, or even conventionally agreed upon, criteria that could indicate whether *the*, or *a*, solution had been found. They would not even presume to claim that all possible solutions to a problematic situation have been identified and considered. They would not attempt to prove that the proposed solution to, or proposed structure of, a problematic situation is correct, true, wrong, false, good, or bad. They would not ensure that the level at which the problem is being tackled is effective. Nor would they hypothesize an exhaustive formulation that could be said to contain all the information a decision maker needs for tackling a problem. The reason was simple enough: such criteria, claims, proofs, assurances and hypotheses were impossible, indefensible, unreliable, intractable, or downright questionable. In assuming them, decision makers were already being led to make decisions in the absence of clear facts. Furthermore, although the first generation methods targeted uncertainty, they artificially manipulated it into ‘the clear uncertainty of probability or game theory’ (Churchman, 1970), reducing the entire challenge posited by uncertainty to one of

learning the calculus of risk management. Rittel and Webber wanted methodologies that could deal with the ‘wicked’ uncertainty of the world where, at best, a range of possible events (contrary to explicit consequences) is sensed, and no probability distribution is forthcoming.

This is a tall order for decision making methods. At least Forrester’s claims about system dynamics were based on what it should be able to do. With each empirical test of his approach, each claim could be ticked off the list. He had emphasized, however, that any model built as a decision support tool must be defensible, that is, the boundaries of the model, its variables, and the modeled interactions must be justifiable (Forrester, 1961: 117-119). Any aspect included in a model must be traceable to some knowledge of the situation being tackled, and not to mere assumptions about how the situation is believed to be. The more we rely on assumptions, the greater the risk of making decisions in the absence of clear facts. This risk is minimized in proportion to the degree to which we focus upon what we actually know, no matter how minimal such knowledge might be.

We might add that, when, over thirty years later, Bennis and O’Toole (2005a) assert that decision makers must be trained to deal with the management of judgments, messy, incomplete and incoherent data, the application of rigorous imagination, multidisciplinary, subjective analysis of multifaceted questions of policy and strategy, and other related issues listed in the Introduction, what are they doing but confirming the contemporary relevance (not to say urgency) of Rittel and Webber’s requirements? But does this mean that we have yet to move on? Are we still, today, stuck with Rittel and Webber’s diagnosis without a cure in sight? Rittel and Webber offered only a vague vision of what was required:

a model of planning as an argumentative approach process in the course of which an image of the problem and of the solution emerges gradually among the participants, as a product of incessant judgment, subjected to critical argument.

Bennis and O’Toole do no more than add a list of desiderata, implying that we have yet to answer challenges set more than thirty years ago. Have there really been no answers since 1973, or have Bennis and O’Toole missed something in the intervening years?

In 2006, Buchanan and O’Connell (2006) wrote ‘a brief history of decision making’ for the *Harvard Business Review*. There are nine sentences, in this eight-page paper, that superficially allude to the weaknesses of probability, to the threat of *groupthink* (Janis, 1982), to the Janus face of technology, and to the failings of intuition. The rest constitutes an upbeat presentation that tells the following story: decision making is about the application, through technology, of probability theory, to assist executive groups who credit the majority of their successes to intuition. Despite the questionable relevance of mathematical probability to instinct-driven decision makers, or perhaps because of it, the paper is revealing of the cave in which the bulk of decision making theory and practice continues to find comfort, and by which it allows itself to be defined. Have the decision making sciences stagnated? Or have they failed to communicate their developments to interested publics, such as those of the *Harvard Business Review*? Or, perhaps, the interest of these publics is overestimated, and they are best fed on conventional myths. No mention of

wickedness here. No systemic thinking. No alarm bells about resorting to antiquated or conditioned propositions when faced with new challenges - even Bennis and O'Toole warn about this. Indeed, no mention of Bennis and O'Toole's *making decisions in the absence of clear facts*. Readers might be excused for wondering what the difference is between learned journals and airport books.

Belasco *et al* and Rittel and Webber offer a glimpse at the pre-October 1973 world of decision making. It makes for sober reading. For if that world was as complex, uncertain, and even chaotic as they make it out to be, then these terms understate our world today. Today's optimists consider such concepts as already tamed by our technological capabilities and advanced probabilistic methods. Rittel and Webber would call them 'arrogant'. For the pessimists, the terms point to insurmountable forces, leaving us no choice but to resort to our intuition. In the ocean of decision making, the optimistic fish are fish food, whilst the pessimistic fish are obsolete. All fish, however, get to appreciate the relevance of water when they are fished. When having to make decisions in the absence of clear facts, are decision makers to be left hanging on the fishhook of methodological platitudes? Or will the methodological reel lower them into a genuinely navigable stream? What exactly *is* the 'second generation' methodological paradigm? How can it be operationalized? And can it be defined in more positive terms instead of the assumptions it rejects? The answers are to be found neither in planning theory, nor in management proper, and much less in the *Harvard Business Review*. Paradoxically, the answers lie with the main advocate of rational analysis, the culprit that attracted the wrath of Rittel and Webber: MS/OR.

Chapter Seven – Another Nice Mess

Aside from going by four different aliases, MS/OR is geographically divided. On the one side, there is the American School of *operations* research. In 1969, Adelson and Norman (1969) described its focus as lying within decision analysis (Clemen, 1996) and its game theoretic variants (Dixit and Skeath, 2004). To see whether this has changed or developed in any way, one can do no better than examine the vision of one of this school's leading contemporary advocates.

In 2006, upon assuming his post as editor of *Operations Research*, Simchi-Levi (2006) described this journal as the 'flagship' not only of its publisher, the American MS/OR society known as the Institute for Operations Research and the Management Sciences (INFORMS), but of the 'profession' as a whole. He viewed his editorship as 'an opportunity to reflect on changes in the profession and society that should influence the journal', and proposed 'to identify what works well, what needs to be improved, and what requires significant change in direction and emphasis.' He acknowledged that, in the past, 'the focus was mostly on the development of quantitative methods to solve operational and managerial problems'. He perceived that 'the emphasis of current research has shifted toward solving more relevant problems', and that this entailed 'expanding the scope and coverage of the journal so that it reflects and possibly influences the evolution of the profession'. He stated his mission statement as follows:

To serve the entire operations research community, including practitioners, researchers, educators, and students.

He added that *Operations Research* 'has always emphasized the publication of papers that are of interest to more than a small portion of society'. He also acknowledged, however, that '[a]t present, there seems to be some perception that *Operations Research* is too focused on technical contributions and that some areas of interest to the community are not covered by the journal'. He thus stipulated his objective as being 'to broaden the journal content, and consequently the field, by publishing material that covers the entire spectrum of problems of interest to the community and by identifying new and emerging areas'.

So far, one could be excused for thinking that MS/OR was about to grab the bull - unleashed through the Rittels and Lindbloms of this world - by the horns. But there was a linguistic twist to all of this. The terms *society* and *community* were not being used as synonyms, and the focus was most definitely on *community*. The established strengths of the *community* were listed as being 'Decision Analysis, Optimization, Stochastic Models, and Manufacturing, Service, and Supply Chain Operations.' The community's emerging strengths were described as lying in 'Financial Engineering, Revenue Management, and the application of operations research to other sciences, through Computing and Information Technologies'. 'Marketing Science' was offered as an example of these 'other sciences', along with the claim of its having 'a long tradition in the [use of] rigorous mathematical models to improve decision making.'

Here is the crux of the matter. Simchi-Levi's *community* refers strictly to those writers who choose to submit articles to *Operations Research*, or, at best, to this journal's readers. This community's interests are represented in one of its chief

advocates, the editor of its flagship journal, when he writes: ‘I would like to see *Operations Research* attracting and publishing *high quality managerial or technical papers* that are based on rigorous mathematical models’. Here, then, is a clear indication of the interests of this community. Simchi-Levi divides these interests into two distinguishable, but not unrelated, sets. First there is the *Science of Operations*. This:

refers not only to contribution to theory and the development of new methods, but also to analytical frameworks, quantitative relationships, and mathematical models, some of which may provide only insights into various problems not necessarily specific numerical solutions. Two good examples in this category include the celebrated Little’s Law from queueing theory, and the more recent literature on supply contracts illustrating the impact of risk sharing between suppliers and buyers.

Then there is the *Engineering of Operations*. This:

focuses on solving specific operational problems and hence requires real data and demands the development of computationally tractable algorithms.

Simchi-Levi sums up the future content of the journal as follows:

Thus, the journal is interested in papers that focus on one or more of the following dimensions:

- Define new problem domains for the field [Read: new *mathematically* amenable problem domains];
- Introduce innovative concepts and mathematical formulations of problems [Read: innovative *mathematical* concepts];
- Provide new insights into operational problems [Read: new *mathematical* insights];
- Develop new methodologies to approach known and new problems [Read: new *mathematical* methodologies, and known and new *mathematically* amenable problems];
- Apply operations research methods in creative ways to interesting application areas [Read: *mathematically* creative ways to interesting *mathematically* amenable applications areas].

Concerning whatever ‘changes’ in ‘society’ might ‘influence the journal’, or the journal’s own aspirations to publish ‘papers that are of interest to more than a small portion of society’, neither these, nor *society* itself, are mentioned further. In brief, nothing has changed for the American School since 1969. If anything, its members seem to be ever more entrenched inside their ‘marvellous cave’. John Mingers (2006) said as much when he wrote how he:

read the article by David Simchi-Levi on the new mission for *Operations Research* with interest and then increasing disappointment.

Setting aside the failure to address societal needs or interests, Mingers explicitly added that, based upon Simchi-Levi’s editorial, the *community* itself was being ill-served:

the journal does not serve the whole of the O.R. community. It misses out completely on what has become a major part of MS/OR, at least outside of the United States, and that is what is known as soft O.R., soft systems or problem structuring methods (PSMs).

Mingers described this ‘major part’ as having emerged from:

the failures in practice of mathematical modeling to be able to deal with the messy, multi-dimensional, often unquantifiable nature of complex problematic situations.

He described them as:

rigorous, structured and above all successful in bringing about improvements to such “wicked” problems,

and added that:

[e]verywhere else in the world they are accepted as a genuine, if not crucial, part of MS/OR, both as practiced and as taught in O.R. programs.

Mingers could only conclude, rightly, that:

Operations Research is restricted to only publishing papers that are based on mathematical modeling of some form. As such, it cannot claim to represent the whole operations research community and should perhaps be re-titled “*Mathematical Operations Research*” to make clear its actual focus of interest.

Neither Simchi-Levi nor his editorial board seem to have paid attention to Mingers. In 2008, when taking the ‘opportunity to reflect on many of the initiatives implemented by the board in the last two years’, Simchi-Levi’s (2008) editorial continues to reflect the hypnotizing shadows on the cave walls of the *community*. For a bunch of people aspiring to be professionals, this ostrich-like behavior is strange. Forget society if you must, but Mingers is a distinguished management scientist of international renown. Brushing aside a distinguished professional, for whom it claims to speak, as well as a ‘major part’ of the field, for which it claims representation, is not what one expects of a ‘flagship’ journal.

But what is this ‘soft OR’? What are these ‘soft systems’ or ‘problem structuring methods’ to which Mingers refers? If they really do address wicked problems, as he says, then they must be of some use to making decisions in the absence of clear facts. And the American School *does* know about them. In 1996, a full ten years prior to Simchi-Levi assuming the *Operations Research* editorial post, Rosenhead (1996) presented them in another INFORMS journal, *Interfaces*. What is more, they not only claim to address such challenges as laid out by the Belascos and Rittels of the world (*American* writers publishing in *American* journals); they trace their lineage directly to challenges laid out by a leading *American* management scientist in his critique of *American* management science. Before we go any further, therefore, it is worth providing some additional context that can shed some light on Mingers’ response to Simchi-Levi.

The American management scientist in question is Russell Ackoff – educator, consultant, and planner *extraordinaire* (Kirby, 2003), and Emeritus Anheuser-Busch Professor of the Wharton School at the University of Pennsylvania, which institution founded, in his honor, the Ackoff Collaboratory for Advancement of the Systems Approach (ACASA). In 1979, Ackoff published a paper across the pond, in the British School’s own *Journal of the Operational Research Society*. The paper bore the somewhat macabre title The Future of Operational Research is Past. His journal choice was deliberate. ‘American Operations Research is dead,’ he began, ‘even though it has yet to be buried.’ He hoped that the British and, by extension, the Europeans, would be more open to a ‘renaissance’ than the hypnotized Americans

ever could be. The paper may be appreciated as an awakening, by MS/OR, to the wickedness of the decision making world, and to a realization that a radical transformation for the field was in order. It had taken six years for MS/OR to digest the implications of Rittel and Webber's diagnosis. In Ackoff (1979), MS/OR offered a postscript of no small significance.

Much like Rittel and Webber, Ackoff perceived MS/OR as a first generation systems approach relentlessly pursuing an 'analytic problem-solving paradigm'. The consequences of this pursuit could be seen in the technical practice of MS/OR as well as in its ethical (or rather, unethical) approach to decision making. On the technical side, Ackoff perceived an MS/OR obsessed by mathematical models and algorithms. Although this obsession had led to ever more astounding technical modeling results, the price had been unawareness of, or indifference to, or even incapability to deal with, decision making situations in dynamic, turbulent environments. In other words, a large dose of *ceteris paribus* was evident in the practice of MS/OR. Now, a chemist, and to a lesser degree a theoretical physicist, might bracket out a number of variables in order to concentrate on a specific causal relationship. Economists do so as well, although the interdependencies and unpredictability of global market players render the usefulness of such a practice questionable. For Ackoff, this practice had led MS/OR to search for problems where its techniques may be applied, instead of living up to its original promise: to take on the whole system. What is worse, MS/OR was tending to distort problematic situations so that favored techniques could be applied to them, instead of dealing with such situations on their own terms. Ackoff summarized MS/OR as either devoid 'of any substantive knowledge or understanding of organizations, institutions or their management' or, at best, as insensitive to their needs and reduced to 'mathematical masturbation':

In the first two decades of OR, its nature was dictated by the nature of the problematic situations it faced. Now the nature of the situations it faces is dictated by the techniques it has at its command. The nature of the problems facing managers has changed significantly over the last three decades, but OR has not. It has not been responsive to the changing needs of management brought about, to a large extent, by radical changes taking place in the environment in which it is practised. While managers were turning outward, OR was turning inward - inbreeding and introverting. It now appears to have attained the limit of introversion: a catatonic state.

These are strong words.

Imagine MS/OR personified as a patient at the General Hospital. The doctor walks in and offers the following diagnosis:

MS/OR

Profession: search and distort agent.

Physical state: dying.

Psychological state: psychosis (loss of contact with reality), hallucinations (false perceptions), delusions (false beliefs), flattened affect (restricted range of emotions), cognitive deficits (impaired reasoning and problem solving), and occupational and social dysfunction.

Behavioral symptoms: The patient's psychological state is reflected in catatonic (extreme) behavior, ranging from maintaining a rigid posture and resisting efforts to be

moved, to engaging in purposeless and unstimulated motor activity, usually in the form of masturbation.

Overall diagnosis: schizophrenia compounded by behaviorally catatonic masturbation.

No wonder the Americans would not listen. It is a miracle the British ever published the paper.

The myth that MS/OR aspired to serve the whole system stemmed from its roots in World War II. Back then, interdisciplinary teams of scientists were set to work on military problems whose solution, by default, would favor not only the allied war machine but save the entire world from tyranny. Under war conditions, a centralized decision making body is effective and, in aiding its decisions, MS/OR was aiding the free world. However, when this affiliation to centralized interests was transferred to the relatively peaceful, but more complex, world of commerce and development, MS/OR increasingly retreated from the whole system. Ackoff described this system as constituted by:

three fundamental interrelated organizational problems: how to design and manage systems so that they can effectively serve their own purposes, the purposes of their parts, and those of the larger systems of which they are part. These are the *self-control*, the *humanization* and the *environmentalization* problems, respectively.

He added that:

OR has been and is almost exclusively concerned with organizational self-control. It has virtually ignored the other two types of problem and the relationship between them and self-control. Furthermore; it employs a Machine-Age approach to the self-control problem. Its method is analytic and its models are predominantly of closed mechanical systems, not of open purposeful systems. This is clearly revealed when one considers OR's use of two concepts: *optimization* and *objectivity*.

OR had retreated to treating one part of a tripartite system. It had ignored Forrester's advice on the importance of analyzing relationships, except in the case of closed mechanical systems. Even this particular case was tarnished by the employment of an antiquated, or at least very limiting, approach. Moreover, Ackoff added, any aspirations to interdisciplinarity had faded away with the pursuit of professionalizing the discipline, the desire for registering qualified practitioners, and the attempts at accrediting academic programs. As for optimization and objectivity, adherence to them was leading to some highly dubious ethical practices.

Ackoff, himself a seasoned consultant, was exasperated by the failures of optimization. Time after time, he dissected optimization models - that had been accepted by decision makers - only to find that data had been massaged in order to fit the algorithms, especially because neither the quantity nor the quality of the data required by such algorithms was available (not to speak of the unreliability and inaccuracy of the data used). Costs of researching and building such models had not been factored into the savings they proclaimed to offer. Adequate tests of the models' possible implementations were hard to come by, reducing implementation procedures to one-shot operations based on faith and minimizing the opportunities for learning. Coupled with the models' inability to conform to socio-political as well as environmental changes, Ackoff could only conclude that if an optimization model was actually solving a problem, it was not the problem for which it had been

commissioned, or if it was, circumstances had changed so much by the time the optimization was proclaimed, that the model was 'still-born'.

Ackoff also questioned the utility of the instrumental ideology behind optimization modeling. Such an ideology assumes that ends (such as maximizing profit or minimizing costs) are valuable in themselves and means are to be employed in strict pursuit of these ends. In the real-world of systemic interrelationships, however, there is no neat dividing line between the two. For Ackoff, MS/OR falsely believed that optimizing all the quantities of life could lead to optimum quality of life. Economic growth is a limiting objective, and not merely from the ethical point of view.

Be that as it may, economics had permeated the tripartite system of self-control, humanization, and environmentalization in which decision making played itself out. Whenever the three parts entered into conflict, MS/OR treated it with a gloss of objectivity through the application of game theory in the interest of securing self-control. Although 'such conflicts are frequently conceptualized by managers and the researchers who serve them as games to be won,' Ackoff viewed 'such a formulation [as] irresponsible, unprofessional, and unethical':

It seems to me that it is the responsibility of managers and their researchers to try to dissolve or resolve such conflicts and serve all of an organization's stakeholders in a way that reflects the relative importance of the organization to them, not their relative importance to the organization. This cannot be done without involving them or their representatives in the organization's decision making. To fail to take all stakeholders into account, as OR usually does, is to devalue those who are not considered or involved in the decision process but who are affected by it. Their exclusion is a value judgment, one that appears to me to be immoral. Science has a moral responsibility to all those who can be affected by its output, not merely to those who sponsor it.

We find here an echo of Rittel and Webber's call for a second generation decision making paradigm based upon an 'argumentative approach process.' Ackoff himself explicitly called for a new 'synthesizing planning paradigm' that involved 'designing a desirable future and inventing ways of bringing it about'. It is important to note that, in both cases, the call for a new paradigm was being made not only on ethical grounds, but because of the technical failings of prevalent approaches, as well as the ostrich-like behavior to which the 'profession' had become prone.

Ackoff's call for a new *planning* paradigm did not preclude decision making. It was not based on some supposedly neat division between planning (including planning theorists) and decision making (including management scientists). In keeping with our earlier discussion, he saw the two as synonymous. He intended, moreover, to expand the entire idea of what decision making entails. This stemmed from his observations, in both the private and public sectors, of what managers actually do:

Managers are not confronted with problems that are independent of each other, but with dynamic situations that consist of complex systems of changing problems that interact with each other. I call such situations *messes*. Problems are abstractions extracted from messes by analysis; they are to messes as atoms are to tables and chairs. We experience messes, tables, and chairs; not problems and atoms.

Messes are 'systems of problems'. As such, messes are liable to the structural dynamics of systems. Systems are constituted by interrelating parts. If we optimize each of the parts, this does not necessarily lead to optimal performance of the system.

Indeed, one part's intended optimization might conflict with that of another's as each part effectively competes for more resources. This leads to detrimental consequences for the system as whole. The performance of a system 'depends more on how the solutions to its parts *interact* than on how they act independently of each other'. Without focusing on such interaction, the system risks collapsing, leading to the demise of its parts. Game theorists would call this a *lose-lose* situation.

Of course, such an insight was what motivated Forrester to develop his industrial dynamics methodology. Whereas, however, Forrester presented his approach as a complement to the existing MS/OR techniques, Ackoff was calling for an overhaul of MS/OR – and with good reason. Ackoff perceived a striking dichotomy between MS/OR practice and decision making practice. MS/OR, steeped in an atomic, analytical paradigm, focused upon problems, not messes. 'Managers,' on the other hand, 'do not solve problems; they manage messes'. As such, MS/OR, if it was solving any problems at all, was not solving the real-world decision making dilemma of managing messes.

For Ackoff, the key lay in MS/OR's obsession with *solving* things. 'Effective management of messes,' he wrote, 'requires a particular type of *planning*, not problem solving'. Whereas MS/OR analysis focused on taking things apart, decision making required a synthesizing practice of '*design* (or redesign) of organized systems so as to reduce or eliminate messes'. Ackoff's wording is crucial here: *mess reduction* is as equally valid and useful as *mess elimination*. In other words, under a 'synthesizing planning paradigm', optimality would no longer govern the practice of MS/OR. Furthermore, since systems are defined by their structure and invite structural, that is, relational analysis, *designing* systems implied *structuring* messes. Optimization would be substituted by structurization as the key guiding principle of the new MS/OR practice. Systemic improvement, contrary to some limiting economic growth, would be the objective of *planning as decision making*.

In addition, if decision makers could not manage or even understand the messes confronting them, sheer synthetic practice or structural analysis was not enough. Ackoff contended that the practice of system design required *learning* about the unstructured reality even as structural attempts were being made to improve it. Decision making, in other words, had to be infused with a degree of action research – a requirement already alluded to earlier by Friedmann and Hudson (1974). Therefore, whatever new decision making methodologies would emerge from the new 'synthesizing planning paradigm' for the resolution of messes, they would have to facilitate learning and effective adaptation in rapidly changing, dynamic environments. As Forrester has indicated, in situations lacking clear facts, learning translates to more resourceful use of whatever limited information is available, and to the useful portrayal of the implications of this information. The still-born products of optimization would thus be countered with formal learning products, ones that could squeeze more juice out from the available orange, and enable effective navigation through an otherwise unstructured, messy, and wicked reality. These products would not solve, but *resolve* over and over again in response to, and attempts to influence, changing circumstances.

The relevance of learning to decision making was emphasized ten years later by de Geus (1988) when, in stressing that ‘the ability to learn *faster* than competitors may be the only sustainable competitive advantage’, he sowed the seeds of organizational learning and knowledge management. In other words, not only can decision making benefit from learning, not only are the two intertwined, but accelerated, sustainable learning approaches might be the only remaining key to organizational survival. Forget marketing mixes, discount offers, merger opportunities, new product launches, and all the usual managerial foci. The tendency toward a catatonic state is inversely proportional to the pursuit of learning, or equally, a sustainable state is proportional to sustainable learning.

Of course, when faced with an absence of clear facts, the issue is not so much *sustainable* learning. The term implies a temporal development whereby more facts continuously unfold. Making decisions in the absence of clear facts requires *knowledge management* of whatever facts are at hand – an activity, moreover, which subsequently contributes to sustainable learning. The better we can manage knowledge from the facts at hand, the more confidence we will have for making decisions based upon those facts.

We have, therefore, two issues that impact upon our ability to make decisions in the absence of clear facts: knowledge management, and systemic planning – the latter stemming from Ackoff’s call for a *synthesizing* planning paradigm that accounts for systemic interactions, as well as Forrester’s teachings of the importance of relational dynamics.

Now, it is quite obvious that these two issues are far removed from those stipulated by the ‘flagship’ journal of MS/OR – especially since they have arisen from equally distant issues such as the exploration and design of desirable futures, the reduction of elimination of messes, action research, structuring, and ethics. A new MS/OR paradigm is indeed required if the conclusions we are drawing from Ackoff ever hope to be translated into decision making methodologies. We need not attempt an overhaul of MS/OR. Indeed, given the usefulness, albeit limited, of traditional management science, it would be unwise to throw the baby out with the bath water. More to the point, what is required is an alternative, complementary paradigm that can resuscitate MS/OR from its dying throes. Mingers has already indicated that Ackoff’s ‘renaissance’ is already in motion in the form of ‘soft O.R., soft systems or problem structuring methods (PSMs)’.

Be that as it may, we still have some unanswered questions regarding our own conclusions above. In particular, what are we to make of this *knowledge management* issue? And must we merely take Ackoff at his word that a ‘synthesizing planning paradigm’, in other words, one based upon systemicity, is needed? Rittel and Webber already noted that, although our exposure to systemic network models has increased our awareness of real-world systemic repercussions, it has also exacerbated the difficulty of identifying problem causes within modeled or real-life networks. They added that this difficulty is compounded by a felt inability, when faced with the complexity of network relationships, to effectively locate where intervention is required and how such intervention should proceed. Perhaps such difficulties can be overcome by investing in training and/or appropriate technology. Such investment,

however, will not be forthcoming based upon the contentions of a single thinker, no matter his status in his field. We must search the real world for evidence of the need of a systemic approach.

Chapter Eight – The Paradoxical Challenge

In the previous chapter we noted two issues that impact upon our ability to make decisions in the absence of clear facts: knowledge management, and systemic planning. We concluded by asking, (a) what we are to make of knowledge management, and (b) for evidence, from the real world, that Ackoff's call for a systemic approach is actually required. In answering these two questions, we will end up perceiving the paradoxical demand that must be met when making decisions in the absence of clear facts.

First, then, what are we to make of *knowledge management*? Knowledge management views knowledge as intellectual capital, a resource to be tapped for the furtherance of organizational activity. It is highly dependent on technology, especially searchable databases, which have led knowledge management to be 'equated to mining for data' (Scarborough and Swan, 2003). But if *knowledge management* is synonymous with *data mining*, this synonymy is problematic for the case of making decisions *in the absence of clear facts*. Data mining is a technologically-dependent activity that attempts to automatically uncover imperceptible correlations, patterns, anomalies and clusters within large volumes of data (Alavi and Tiwana, 2003). This focus on large volumes of data clearly renders it irrelevant to those situations characterized by an absence of clear facts. Indeed, data mining is another first generation systems approach and, by extension, so is knowledge management. We require the development of intellectual capital through other means.

There is, moreover, a wider problem with knowledge management. In a world requiring the versatile use and portrayal of limited data, or information, with a view to construct knowledge, enable learning, and inform action, knowledge management sells itself on the promise of offering practicable 'ways of disseminating and leveraging knowledge in order to enhance organizational performance' (Easterby-Smith and Lyles, 2003: 3). However, an investigation into whether the field has lived up to its promise paints a disturbing picture (Kawalek, 2004):

[W]hen investigating the conceptual literature on knowledge management it seems that it is burgeoning with viewpoints that overlap, and commonly contradict each other... the literature has not provided *methodological* guidance for *doing* knowledge management (i.e. managing knowledge), without which knowledge management is fated to remain ill-defined, open to misinterpretation and sometimes abuse by unscrupulous practitioners... there are quite significant differences between the writers on knowledge management, and following each will lead to quite different approaches to knowledge management practice... While the knowledge management literature presents many insightful points, definitions and analyses, none inspire confidence that successful management of knowledge will result (or is even possible) as a result of a process of selecting from these insights.

Personified as an executive, this is equivalent to being called in by the boss, only to be told that 'after a thirty-year career, we have found you to be contradictory, incoherent, unpredictable and untrustworthy.' Not good news.

For our purposes, an effective decision maker is one who can do knowledge management resourcefully in the absence of clear facts. The field of knowledge management is not only silent in the face of this absence; it fails to live up to the minimal of expectations even when data are available. We shall have to look

elsewhere for the knowledge management needs of decisions in the absence of clear facts.

A glimpse of how such needs may be addressed is afforded by reminding ourselves of the importance of semantics and pluralism to the making of decisions in the absence of clear facts. Bennis and O'Toole (2005a) also point to the relevance of the management of judgments, ethics and morality, rigorous imagination, subjective analysis, wisdom and experience, and interpersonal skills. We can also reasonably expect that problematic situations lacking clear facts allow for relatively few outright structural assumptions about them. Perhaps no prioritization is available among seemingly important factors, or perhaps key aspects appear to be equally necessary, though treatable on respectively different dimensions. In brief, an open-ended nature characterizes such situations. This, in turn, invites variations of interpretations about what is going on.

If on the one hand, however, an imperfectly known situation opens the doors to wide interpretations, ambiguity, on the other, constrains the degrees of freedom allowed in interpretation. Here, then, lies the essential need. Care is required so as not to introduce assumptions which do not fall within the framework of the situation as given – remember Forrester's warning: any conclusions must be traceable to knowledge (no matter how minimal) of the situation being tackled, and not to mere assumptions about what the situation is believed to be. A certain degree of mental discipline, or interpretative rigor, is called for when conceptually framing the situation, avoiding any suggestions or conclusions which are not clearly within the bounds of what is given. The risks of not adhering to this are tantamount to resolving an irrelevant, imaginary, nonexistent, or wrong problem.

The effectiveness of decisions in the absence of clear facts and, by extension, decision making ability, is proportional to the deduction of significant information, from the facts available, which respects the degrees of allowable interpretative freedom relevant to the situation. Significant information, in turn, may be understood not only as information which is interpretatively sound, but as information which effectively serves the interests of the management of uncertainty inherent in the situation, and thus ultimately renders the decision maker tangibly better informed, better equipped, and more confident to deal with the situation.

It is here, with the focus upon developing interpretation skills, that we begin to appreciate the non-trivial, even daunting, task faced by decision makers in situations lacking clear facts. When was the last time MBAs were offered a course on interpretation skills; or a course in rigorous imagination, or even in subjective analysis, or wisdom and experience, or pluralism? For Bennis and O'Toole, these are the very courses required for the complete training of decision makers. Perhaps it is true that formal education has changed little since the 19th Century (Banathy, 2001). What is clear, however, is that these are the very skills required for making decisions in the absence of clear facts.

Thus far, therefore, our knowledge management requirement can be described in the following terms. Given an absence of clear facts, and therefore sparse knowledge of a problematic situation, what is required is a way to extract information from the

available facts; a way that stands in contradistinction to the traditional, volume dependent, data mining techniques; a way that is informed through a methodological discipline that can offer interpretatively sound conclusions. Contrary to traditional knowledge management, as exemplified in its data mining methodologies, the effectiveness criterion for knowledge management will not lie in the manner in which it resourcefully searches through volumes of data, but in the manner in which it resourcefully interprets limited facts.

Interpretation, however, must also have a purpose. Ever since Lindblom, everyone consulted in the discussion thus far has stressed the importance of structuring the problematic situation. In effect, they have indicated that we not only need to extract whatever information we can; we must structure such information in some way that will guide us toward problem resolution. Where the search activities of data mining identify otherwise imperceptible aspects in reams of data, knowledge management in the absence of clear facts must offer a means of structuring the extracted information in a manner that enables rigorous problem definition. After all, as Rittel and Webber point out: the greatest problem confronted in having to make decisions in the absence of clear facts is to define the problem about which decisions must be made. Unless such definition is forthcoming, there is little point to any knowledge management methodology.

Knowledge management for decisions in the absence of clear facts, therefore, must address the following: the production of interpretatively sound knowledge from whatever limited or limiting sources are available, and the rigorous application of this knowledge in the service of problem definition. It must, in other words, answer the following two questions. First, given an absence of clear facts, how is it possible to discriminately extract interpretatively sound information from the facts that are available? Second, if such information can indeed be extracted, how can it be discriminately structured in a way which enables rigorous problem definition?

Notice that *information extraction* must be undertaken in a manner that offers *interpretatively sound* information, and that *problem structuring* requires a manner that enables *rigorous* problem definition. Information contributes to the development of knowledge. It addresses epistemological needs. As such, its extraction must be governed by an epistemological criterion. Structure, on the other hand, provides the basis upon which a solution is designed. Much like an architectural structure, in order to ensure a solution to the real, existent, relevant and correct problem, rigor is required in building the problem's definitional structure. Both activities, moreover, must be undertaken *discriminately*, that is, in a manner that yields interpretive and structural conclusions that are defensible and justifiable against the facts at hand.

In summary, effective knowledge management for decisions in the absence of clear facts is achievable through the resourceful use of limited facts. Such resourcefulness is achievable through a means for discriminately extracting interpretatively sound information from the facts that are available, as well as a means for discriminately structuring the extracted information in a way that enables rigorous problem definition. Figure 4 summarizes the effectiveness criterion and methodological requirements of knowledge management, one of two issues that impact upon our ability for making decisions in the absence of clear facts.

Making Decisions in the Absence of Clear Facts		
Impacting Issue	Effectiveness Criterion	Methodological Requirement
Knowledge Management	Resourceful use of limited facts	A means for discriminately extracting interpretatively sound information from the facts that are available
		A means for discriminately structuring the extracted information in a way that enables rigorous problem definition

Figure 4: The effectiveness criterion and methodological requirements of knowledge management for decision making in the absence of clear facts

So much for knowledge management. What’s the big deal about systemicity? Well, to begin with, the first direct theoretical and practical responses to Bennis and O’Toole’s educational imperative came from the field of systems science (Georgiou, 2006, 2008). Second, we have seen that the scientific pretext for the design of comprehensive plans is traceable to Ashby’s Law of Requisite Variety for controlling systems. Indeed, the very pursuit of comprehensive implies a perception of interrelated issues. Third, with Forrester we saw the emergence of mathematically formulated systems simulated on computers, enabling an awareness of the impact of systemic dynamics. Forrester’s approach allowed MS/OR to be equated with *systems* analysis. He made us aware of decision making as a system, constituted on the one hand by decisions, and on the other by information. Furthermore, he highlighted how any decision point participates in a realm or system constituted by other decisions. In other words, relational dynamics and the interdependency of decisions came to the fore. Forrester was also the first to indicate the link between structural design and systemic improvement. Ultimately he demonstrated how a systemic approach enables a comprehensive scope without necessarily requiring comprehensive detail. System dynamics has also laid claim to generic, identifiable, systemic structures in the real world known as *system archetypes*. More broadly, scholars and consultants engaged in systemic problem solving have created a score of systems methodologies to assist decision makers (Jackson, 2003). And let’s not forget that Forrester sowed the first seed regarding decision making *effectiveness* in the absence of clear facts. Beyond Forrester, we saw how MS/OR aspires to ‘take on the whole system’. We found Rittel and Webber acknowledging the awareness of network dynamics afforded by the systems approach, irrespective of their having tempered such enthusiasm with new challenges. Rittel and Webber also warned us about the dangers of treating problematic situations as closed systems instead of open systems liable to extraneous variables. And when they called for a new decision making paradigm, they explicitly

called for a second generation *systems* approach. Mingers then threw in the idea of *soft systems*, equating it with *soft OR* and *problem structuring methods (PSMs)*, and claiming it to be a 'major part' of MS/OR – at least outside the United States. When the Wharton School decided to honor Ackoff with a research center, its title proclaimed the *Advancement of the Systems Approach*. When Ackoff described organizational problems, he painted the picture of a tripartite system of self-control, humanization and environmentalization. He echoed Rittel and Webber's warning about closed systems, and added that MS/OR was ill-equipped for meeting the systems challenge. It has been talk of systems that has also made us aware of the fuzzy line dividing means and ends, of the weaknesses of optimization and of the questionable consequences arising from the pursuit of objectivity. And of course, without talk of systems, we would never have been made aware about messes and their relevance to decision makers, nor indeed about learning and knowledge management. Indeed, without talk of systems we would never have been led to consider an alternative paradigm for decision making. We would never have been led to coin the phrase *planning as decision making*. And the idea of systemic planning would never have occurred. The very term *systemicity* would more likely have us recalling an album by The Police (*Synchronicity*) than considering it as something relevant to decision making.

Systems thinking has permeated our discussion. No doubt, we have yet to fully grasp this idea, its methodological implications, and its application to decision making. We do appreciate, however, that systems thinking will be central to making decisions in the absence of clear facts. Let's be clear about this. Forrester introduced systems thinking in order to alleviate the traditional, disjointed MS/OR approach that was leading to decision making in the absence of clear facts. Rittel and Webber then contended that, although such disjointedness had indeed been alleviated, decisions were still being made in the absence of clear facts because, within a complex network of interrelated issues, it had become increasingly difficult to identify problem centers and the manner in which effective intervention was possible. Nevertheless, they perceived the utility of a systems approach in dealing with an absence of clear facts, and called for a second generation systems paradigm. Ackoff then threw in the messes decision makers have to deal with. A mess is an unstructured reality constituted by interacting, changing problems about which we can, at best, only have partial information. Its interacting nature compounds the necessity for a systems approach, but explicitly adds the requirement to be able to deal with an absence of clear facts even as a systemic attempt toward resolution is being made.

Systemicity, then, plays a key role in making decisions in the absence of clear facts. But our appreciation need not rest on the claims of people like Ackoff, nor on any suspicions that the discussion thus far has been cleverly constructed to ensure the inclusion of the systems idea. We need only look at the world around us for evidence of the importance of systems thinking to decision making. For there is an unquestionable, not to say insatiable, demand for decisions to address the holistic or systemic nature of problematic situations.

The Inquiry into the 1997 Southall rail disaster in the United Kingdom found that 'it would be wrong to concentrate on the failings of the driver when there is compelling

evidence of serious *systemic* failings within Great Western [Trains]⁷ – failings further attributed to the rail industry as a whole by one of the companies prosecuted for the October 2000 Hatfield crash.⁸

In an interview given by the then UK Liberal Democrats' leader Charles Kennedy to the BBC's Peter Sissons on 4 June 2001, Kennedy called for a 'holistic approach to government which is longer-term and I think more far-seeing than the short-term which has tended to plague successive British administrations.'⁹

In his 2002 annual review Nick Land, Chairman of Ernst & Young, concluded that:

The root cause of corporate collapse and scandals in companies like Enron and WorldCom was not audit failure. They came about because of *systemic* failure in the US around corporate governance and transparency, accounting standards and regulation, and, perhaps most importantly, as a result of greed.¹⁰

In 2005, New York Federal Reserve President Timothy Geithner expressed his concern over a developing paradox: whilst increased complexity of financial systems reduces the individual vulnerability of firms, it compounds uncertainty as to how the financial system as a whole might function in the context of a *systemic* shock from hedge funds and other unregulated institutions.¹¹

Three years later, in the midst of the most severe credit crunch in living memory, White House Press Secretary, Dana Perino reminded the press of the Bush administration's awareness of 'the *systemic* risk posed by Fannie Mae and Freddie Mac because of the very large role they play in housing markets, and because of their business practices'¹². Merrill Lynch, the global financial services firm, added that 'any solution to the credit crisis needs to take the approach that it is a *systemic* problem', qualifying a potentially systemic solution as one that would 'facilitate

⁷ As reported on the BBC Internet site on 21 December 1999 in a report entitled Rail Managers Rebuked Over Catalogue of Errors at the following URL: http://news.bbc.co.uk/hi/english/uk/newsid_573000/573740.stm

⁸ As reported by the British newspaper *The Daily Telegraph* on 8 October 2005 in an article entitled Companies fined £13.5m for Hatfield crash at the following URL:
<http://www.telegraph.co.uk/news/main.jhtml?xml=/news/2005/10/08/nhatfield08.xml>

⁹ As reported on the BBC Internet site on 4 June 2001 at the following URL:
http://news.bbc.co.uk/vote2001/hi/english/programmes/specials/election_call/newsid_1369000/1369845.stm. It is also worth mentioning that a holistic aspiration to government (known as *joined-up government*) guided, at least in theory, Prime Minister Tony Blair's New Labour government in the United Kingdom during his administration (Pollitt, 2003).

¹⁰ Ernst & Young's chairman's review of the year 2002, as reported on the firm's internet website on 23 October 2005 at the following URL: http://www.ey.com/global/content.nsf/UK/UK_Annual_Review_2002_-_Chairmans_review

¹¹ As reported by Reuters on 18 October 2005, in an article entitled Fed's Geithner: Market changes have altered risk at the following URL: http://today.reuters.com/investing/financeArticle.aspx?type=bondsNews&storyID=2005-10-18T200837Z_01_N18235770_RTRIDST_0_ECONOMY-FED-GEITHNER-UPDATE-1.XML

¹² As reported by MarketWatch (of The Wall Street Journal Digital Network) on 8 September 2008 in an article entitled Press Briefing by Dana Perino at the following URL: <http://www.marketwatch.com/news/story/press-briefing-dana-perino/story.aspx?guid=%7B26BC94D3-0AE7-4F8A-B183-5A7D028A5250%7D&dist=hppr>

consolidation’¹³. One commentator added that “‘Systemic solution” is econ-geek talk for the mother of all bailouts’, leaving the rest of us to draw our own conclusions as to whether economists really know the difference¹⁴.

In 2008, as the new school year was beginning, Dennis Cuddy, Senior Associate with the U.S. Department of Education during the Reagan administration, wrote: ‘Americans wonder how... a downward slide in education has occurred over so many years, and the answer is that the problem is *systemic*’¹⁵. He may as well have been writing about any country’s public education system.

During any one particular 24-hour period, a search for the word *systemic* on the Google News Internet site can yield upwards of 6000 results! Many of them refer to systemic problems or systemic crises calling for systemic approaches and systemic solutions. Even if you grant a degree of duplication between the results, there is no doubt about it: addressing systemicity is *dans l’aire du temps*, or as von Bertalanffy writes:

If someone were to analyze current notions and fashionable catchwords, he would find ‘systems’ high on the list.

Except that he wrote this in 1967 (von Bertalanffy, 1968: 3), which goes to show that systemicity is not just a contemporary phenomenon. It is perennial in the fullest sense of the term: it not only constantly occurs, but it also generates long-term repercussions. On the one hand, it is perceived as a problem with complex symptoms whilst, on the other, it is perceived as a solution to the complex reality of the world. It is, at once, problem and prize. As such, for all the talk in the news (as well as in MBA classrooms), systemicity continues to confound. We are hard-pressed to define systemic problems. We are hard-pressed to construct systemic plans. We are hard-pressed to find systemic solutions. We are hard pressed to find someone who can do either or all of these activities, not to mention teach them. Yet the world cries out, and not because *systems* is a notion or a fashionable buzzword. From global warming to supply chain management, from experiential learning theory to computerized system dynamics simulations, from graph theory to social networks, from Jungian psychology to gestalt psychology, systems are, if not evident, then at least felt everywhere - including, and especially, in decision making. And, as we have seen, decision making in the absence of clear facts is not immune.

¹³ As reported by the CNN Internet site on 8 September 2008 in a report entitled [UPDATE: GSE Plan Limits Risk For E.U. Lenders, But Write-Backs Unlikely](http://money.cnn.com/news/newsfeeds/articles/djhighlights/200809081257DOWJONESDJONLINE000473.htm) at the following URL: <http://money.cnn.com/news/newsfeeds/articles/djhighlights/200809081257DOWJONESDJONLINE000473.htm>; see also [Debate shifts to systemic solution](http://www.ft.com), by Krishna Guha, Michael Mackenzie and Saskia Scholtes as reported by the Financial Times newspaper’s Internet site www.ft.com on 12 September 2008 at the following URL: <http://www.ft.com/cms/s/0/e25fab04-8063-11dd-99a9-000077b07658.html>

¹⁴ As reported on 19 September 2008 by USNews & World Report in an article by James Pethokoukis entitled [The Colossal Bailout of 2009](http://www.usnews.com/blogs/capital-commerce/2008/09/19/the-colossal-bailout-of-2009.html) at the following URL: <http://www.usnews.com/blogs/capital-commerce/2008/09/19/the-colossal-bailout-of-2009.html>

¹⁵ As reported on 8 September 2008 by NewsWithViews.com in an article by Dennis L. Cuddy entitled [Educational problems & solutions](http://www.newswithviews.com/Cuddy/dennis137.htm) at the following URL: <http://www.newswithviews.com/Cuddy/dennis137.htm>

Immune it might not be, but making decisions in the absence of clear facts poses a severe challenge to the necessity for systemic solutions. In order to appreciate the nature of this challenge, let's reconsider a little. Earlier, Forrester taught us that the effectiveness of a decision maker is not demonstrated through access to better or more information: the effectiveness of a decision maker is demonstrated in an ability to use, more resourcefully, whatever limited information is available, and to portray its implications more usefully. We could conclude back then that, since the acquisition of more information can be time-consuming, costly, and compounded by delays in completing the meta-level decision process which addresses procurement in the first place (Grünig and Kühn, 2005: 181-195), a decision maker whose sheer thinking process enables the effectiveness in question may well be in high demand.

As an example, consider the following. Your boss calls you in, gives you a single sheet of typed paper, and says: 'I need to make a decision based on these facts. What do you suggest?' You consider the available details on the sheet of paper, and reply: 'I'll need \$30,000, a team of three people, and six months to get you the information we need to make the right decision.' Your boss then calls in a colleague of yours, hands over the same sheet of paper, and asks the same question. Your colleague takes a moment to study the available details, and then replies: 'I'll have your decision on your desk by Friday.' Whose thinking process won them the assignment, not to mention the confidence of their boss?

The boss chose upon the promise of effective knowledge management. We have seen, however, that making decisions in the absence of clear facts must practice, and be informed by, systemic *planning as decision making*. It is not enough to discriminately extract interpretatively sound information from the facts that are available. It is not enough to discriminately structure the extracted information in a way that enables rigorous problem definition. For although such extracting and structuring activities help us move toward a decision, the decision itself must be systemic, and for this one requires a systemic plan whereby *the* decision is seen as a *set of interrelated* decisions. Decision making in the absence of clear facts, therefore, must answer the following question: given a discriminate extraction of interpretatively sound information from the available facts, and given a discriminate organization of the extracted information in a way that enables rigorous problem definition, how is it possible to discriminately plan a systemic approach toward resolution? This activity, like those of knowledge management, must be undertaken *discriminately*, that is, in a manner that yields a systemic plan that is defensible and justifiable against the facts at hand.

Effective knowledge management for decisions in the absence of clear facts is, therefore, achievable not only through the resourceful use of limited facts, but through the incorporation of systemicity. This latter is achievable through a means for discriminately planning a systemic approach toward resolution. Figure 5 summarizes the results of our understanding of the two issues that impact upon our ability for making decisions in the absence of clear facts.

Making Decisions in the Absence of Clear Facts		
Impacting Issue	Effectiveness Criterion	Methodological Requirement
Knowledge Management	Resourceful use of limited facts	A means for discriminately extracting interpretatively sound information from the facts that are available
		A means for discriminately structuring the extracted information in a way that enables rigorous problem definition
Systemic Planning	Incorporating systemicity	A means for discriminately planning a systemic approach toward resolution

Figure 5: The effectiveness criteria and methodological requirements of the two issues that impact upon decision making in the absence of clear facts

It is now possible to perceive the severe challenge posed by making decisions in the absence of clear facts. On the one hand, effectiveness is measured by resourceful use of limited, partial, incomplete information. On the other hand, effectiveness is measured by incorporating systemicity, by providing systemic results, by implementing wholes rather than disjointed parts. A decision maker who can effectively do knowledge management in the absence of clear facts is not enough. Neither is a decision maker who can simply plan or solve systemically. Making decisions in the absence of clear facts poses the following paradoxical demand: it asks for useful and practical *systemic* results in the face of *partial* information or, equally, for implementable *wholes* in the face of informational *incompleteness*. In increasingly complex networks of interrelated issues, where neither the relationships nor the issues can be understood completely, decision making effectiveness is proportional to our ability to deal with this paradoxical challenge.

In summary, then, we have identified two activities required for making decisions in the absence of clear facts: knowledge management and systemic planning. Effective knowledge management will be proportional to the resourceful use of limited facts, whilst effective systemic planning will be proportional to the incorporation of systemicity in decision making. Any methodology that purports to support and guide decision making in the absence of clear facts must provide a means for doing knowledge management by (a) discriminately extracting interpretatively sound information from the facts that are available, and (b) discriminately structuring the extracted information in a way that enables rigorous problem definition. In other words, the methodology should produce knowledge and structurally apply it in the service of defining the problem at hand. In addition, the methodology must provide

for a systemic approach toward resolution. The product of this approach should be a systemic action plan of interrelated decisions that maps out how the problematic situation will be tackled systemically.

Earlier, Mingers indicated that ‘soft O.R., soft systems or problem structuring methods (PSMs) [are] able to deal with the messy, multi-dimensional, often unquantifiable nature of complex problematic situations’, adding that they are ‘rigorous, structured and above all successful in bringing about improvements to such “wicked” problems.’ It is time to consider whether Mingers’ recommendation can answer our requirements.

Chapter Nine – Structuring the Theoretical Foundation

Ackoff's (1979) diagnosis of MS/OR, published in the British *Journal of the Operational Research Society*, did not fall on deaf ears. Indeed, in the same year of its publication, and in terms reminiscent of Lindblom and Rittel and Webber, two British management scientists were expressing their concern that MS/OR was all but ignoring the complexities of real world decision making (Eden and Sims, 1979). Two years later, the *Journal of the Operational Research Society* published a paper proclaiming that the 'O.R. community' was in the throes of a 'Kuhnian crisis', and it predicted that it would most probably produce a formal, alternative decision making framework along the lines of Ackoff's 'synthesizing planning paradigm' (Dando and Bennett, 1981). During the 1980s, this 'Kuhnian crisis' was played out largely as a war between ideologies concerning the nature and practice of MS/OR. Even during its early stages, it led some sympathizers to proclaim that, although it made for 'enjoyable debate', it was 'pointless', 'futile', and 'irrelevant' to practitioners who faced real problems in the real world (Eden and Graham, 1983). Perhaps this was a premature conclusion, since the 'enjoyable debate' did eventually give rise to methodological insights and, indeed, entirely new methodologies. Aspects of the debate were collected by Flood and Jackson (1991) and Keys (1995). We shall follow the debate only indirectly, however, choosing to concentrate instead on what will prove to be relevant to decision making in the absence of clear facts. Indeed, as stated in the last chapter, our aim is to uncover the relevance, to the issue at hand, of Mingers' (2006) 'major part of MS/OR' which, in keeping with the field's traditional terminological indecision, he has labeled as 'soft O.R., soft systems or problem structuring methods (PSMs)'. We shall refer, in the main, to problem structuring methods (PSMs), since through an understanding of this particular label the ideas of soft OR and soft systems will become clear.

The idea of methods that structure problems takes us back to Lindblom who, as we saw, first alluded to the importance of structuring in managing messes, but who offered no formal methodology for undertaking it. That was in 1958. Twenty years later, two British management scientists, began to formally consider the idea of 'problem structuring'. They published their findings in three papers, two in the *Journal of the Operational Research Society* (Pidd and Woolley, 1980a; Woolley and Pidd, 1981), and one in *Interfaces* (Pidd and Woolley, 1980b), the most open-minded journal of the American school. Although they did not cite Lindblom, they pointed to the 'methodological issues' raised by Ackoff (1979) as a prime motive for their research, and they clearly considered structuring to be an important issue worthy of consideration in the methodological debates underway at the time. In particular, they hoped that their findings would 'inject some empirical evidence' that could help shape the future practice of MS/OR.

In conjunction with their empirical research, Pidd and Woolley searched the literature for some guidance on problem structuring. They were disappointed with what they found. Structuring approaches varied from the simplistic 'checklist', to an as-yet ill-defined 'people'-oriented perspective that hoped to formally operationalize Lindblomian pluralism and serve as an aid in negotiation processes. In between, they found plenty of evidence supporting the traditional MS/OR quantitative approach

toward model building, not dissimilar to the one we saw presented by Buchanan and O'Connell (2006). They also found evidence of another quantitative approach, one that did not search for the outright maximization or minimization of a particular variable, but instead emphasized an exploratory process across multiple variables – an approach, for example, of the type offered by Forrester. Pidd and Woolley concluded that none of these approaches provided effective means toward problem structuring. They found the literature overly obsessed with problem solving, capable in tackling well-defined, algorithmic 'puzzles', but inept at addressing the challenges laid down by the likes of Lindblom, Rittel and Webber, and Ackoff.

Pidd and Woolley's empirical research found that, contrary to any formal approaches recommended by the literature, real-world decision makers seemed to practice an iterative exploratory process, indeed one that resembled Lindblomian pluralism where intangible variables and social processes were as important as tangible, quantifiable variables. The authors emphasized the informal nature of this process by noting that it did not even refer to the 'literature on creativity', a field which tends to offer much less rigorous approaches to decision making than MS/OR. They observed that some form of structuring was evident throughout interventions, with greater emphasis in the early stages of tackling problems. Perhaps most importantly, however they noted that learning was being achieved through problem exploration. As such, a process, that would have probably been labeled as 'problem solving' by the decision makers, was in some significant way akin to action research.

Pidd and Woolley ventured to suggest that any future, formal problem structuring methodologies should offer processes that would facilitate sufficient understanding of 'the symptoms and dissonances' of messes, thus translating ill-defined and ambiguously conceptualized problematic situations into effective intervention strategies. These processes would involve 'the consideration of possible modes of implementation' and necessitate a certain 'technical competence' for guiding intervention. Most importantly, however, problem structuring methodologies were to facilitate 'the set of inherent decisions' which would guide any subsequent intervention.

We find here an equation between problem structuring and decision making. It points to *problem structuring as decision support system*. Problem structuring was to be conceived as a decision making approach and, moreover, it was to be appreciated as at least equally valuable to, or even more relevant than, the likes of decision analysis, game theory or any other established method. Undoubtedly, the term *structuring* does not lend itself particularly well to our quotidian conception of *decision making*. Where the latter connotes action and leadership, and is often peddled as such, the former seemingly alludes to passive irresolution, and is ignored. Neither sophistry nor nescience, however, make for effective decision making and, during the 1980s, a handful of British management scientists took it upon themselves to demonstrate the power of *structuring as decision making*.

It is worth noting that the management scientists in question did not suddenly take up the structuring challenge in the 1980s. Indeed, some of their work can be traced back to the late 1960s (Gupta and Rosenhead, 1968; Friend and Jessop, 1969). By the mid-1980s, six fully-developed problem structuring methods were evident in the literature:

1. Soft Systems Methodology (Checkland, 1981);
2. Strategic Options Development and Analysis (Eden, 1982, 1985, 1988; Eden and Sims, 1981);
3. Strategic Choice Approach (Friend and Hickling, 1987);
4. Robustness Analysis (Rosenhead 1980a,b);
5. Metagames (Howard, 1986; Howard, 1987); and,
6. Hypergames (Bennett and Huxham, 1982; Bryant, 1983; Bennett, 1985).

Given the scattered presentation of these methods across books and technical journals, it was not at all obvious that a structuring movement within MS/OR had actually been taking place for some time. The Kuhnian crisis of the 1980s offered an opportunity to consolidate and refine twenty years of theory development and practical experience in the use of these methods as aids to structuring as decision making. It was not a question of innovation but of presentation, one that could convince MS/OR academics, practitioners, and their publics, that a methodological answer was available that could tangibly and positively contribute toward the 'renaissance' called for by Ackoff. By the end of the decade, the presentation was published in a book entitled *Rational Analysis for a Problematic World: Problem Structuring Methods for Complexity, Uncertainty and Conflict* (Rosenhead, 1989).

This book, currently in its second edition (Rosenhead and Mingers, 2001), is recognized as an outstanding prescription, not only for the rejuvenation, but for the relevance of MS/OR to real world problematic situations. It not only presents the theory and practical usage of each of the methods; it also provides an overarching paradigm that serves to unite them under the umbrella term *problem structuring methods*. This paradigm spells out those situational characteristics and methodological requirements for which the methods have been designed. For instance, the methods are applicable to situations:

- concerning multiple, simultaneous and equally necessary objectives, measurable in respectively different dimensions;
- constituted by multiple variable-types that preclude overall optimization;
- characterized by a lack of explicit information on what needs to be done;
- that preclude a technical or algorithmic solution because they are full of human interests with associated opinions and judgments whose viability must be taken into account;
- that undoubtedly require a systematic approach, but for which the scientific methodology of quantitative methods is found wanting;
- constituted by actors who are not necessarily hierarchically related, and not necessarily in agreement with each other, and whose decisions impact in various degrees and in different aspects.

It is worth noting that even this brief outline points to issues that have been raised in our discussion of making decisions in the absence of clear facts. To talk of multiple

objectives measurable in respectively different dimensions recalls our Lindblomian example of side-impact safety cars. To note the prevalence of multiple types of variables recalls Friedmann and Hudson's, as well as Lee's, critiques of quantitative comprehensive modeling. To acknowledge a lack of explicit information on what needs to be done is to point directly to our own concerns for making decisions in the absence of clear facts. To incorporate human interests is to reflect our own requirement that subjectivity be tackled on its own terms. To demand a systematic approach unbounded by the constraints of quantitative methods speaks directly to our need for a discipline that is not necessarily mathematically based, but that is analogically ruthless in rigor and exactitude. And to account for non-hierarchical human relationships and decisions of varying impact is to acknowledge the need for a modeling approach based upon relational dynamics.

The methods are designed to answer particular methodological requirements that arise from these situations. For instance, they facilitate the exploration and design of interrelated solutions across different dimensions without treating each of them as mutually exclusive. The methods tame our infatuation with, and combat our weakness in, data-dependency, aiming to attain greater integration between quantitative and qualitative data within socio-political, cultural and economic processes. They offer sophisticated processes and modeling tools in order to account for the dimensional richness of the issues involved. They do so, however, in a manner that is transparent enough for laypersons to engage with the processes and models. Participation, therefore, is key, in that the methods promote the treatment of people as actively interested subjects with a stake in the decisions to be taken. Uncertainty is viewed as a constraint that cannot be abolished through a mathematically accurate model. Instead, uncertainty is tackled by maintaining options open for future resolution according to the element of uncertainty perceptible in decisions to be taken. Ultimately, the methods incorporate complexity and interdependence in a manner that allows at least a fair shot at the ideal of truly systemic planning.

On this latter point, it is worth noting that the essential difficulty with complexity is not in its resolution. For complexity is not irresolvable. Complexity is irresolvable only when accompanied by disorder. Hence, the road toward resolving complexity does not lie with approaches focused upon problem solving. The road lies with approaches that can, first and foremost, transform the disorder into some order. This implies the imposition of structure. *Ergo*: the need for problem structuring.

The tools incorporated by the methods reflect not only a need to tackle the above situational characteristics and methodological requirements, but also a need for process facilitation. We have already noted why making decisions in the absence of clear facts requires a fundamental switch in focus, from content to process. Problem structuring methods operationalize this switch by offering tools that have the following characteristics:

- complexity is represented through diagrammatic, instead of mathematical, means;
- solution spaces, instead of single solutions, are designed so as to aid holistic decision making;

- alternatives are considered and compared in a discrete manner as possibilities, instead of forcing the design of probabilistic calculations; and,
- scenarios, instead of mathematical forecasts, are developed as means for learning about the consequences of decisions.

We can appreciate, now, why Mingers refers to ‘soft OR’ and ‘soft systems’ in the same breath as ‘problem structuring methods’. The *softness* arises because, with problem structuring methods, MS/OR is freed from the rigidity of its mathematical cage to engage in cross-disciplinary work. Systems are no longer simply there, in the world; the manner in which human beings perceive them is equally relevant. Indeed, problem structuring methods are soft in that they address the suppleness, subtleness, and subjectiveness that constitute the very substance of real-world decision making.

Figure 6 summarizes the situational characteristics and methodological requirements that we have identified as relevant to making decisions in the absence of clear facts. A comparison with the situational targets and methodological compositions of problem structuring methods reveals non-trivial similarities. We have already seen Mingers (2006) claiming that problem structuring methods are ‘able to deal with the messy, multi-dimensional, often unquantifiable nature of complex problematic situations’ adding that they are ‘rigorous, structured and above all successful in bringing about improvements to such “wicked” problems’. We have already noted how situations lacking in clear facts have a messy and multi-dimensional nature, compounded by unquantifiable complexity. And we have seen how their wickedness requires a rigorous *structuring as decision making* approach for facilitating improvements. Given all this, surely there is something in these problem structuring methods that can assist decision making in the absence of clear facts.

We have, however, identified three *essential* methodological requirements for making decisions in the absence of clear facts. In particular, if problem structuring methods are to prove useful to making decisions in the absence of clear facts, they must provide means for discriminately:

- extracting interpretatively sound information from the facts that are available;
- structuring the extracted information in a way that enables rigorous problem definition; and,
- planning a systemic approach toward resolution.

Our question, then, is very specific: which of the problem structuring methods, or any combination thereof, address these three essential methodological requirements? The literature points to one particular problem structuring method, one that was born within the systems science movement, one which has profoundly influenced systems thinking, and the one to which any talk of ‘soft systems’ most directly refers: Soft Systems Methodology, or SSM. Relevance to decision making in the absence of clear facts is, moreover, offered on the general as well as specific levels.

Situational Characteristics of, and Methodological Requirements for, Making Decisions in the Absence of Clear Facts								
Chapt.	1	2	3	4	5	6	7	8
The Situation	concerns multiple, unequally likely alternatives, the choice of which does not necessarily follow, or even enable, probabilistic judgment	is contextually-immersed, with particularities that render it non-repetitive, different from other seemingly similar situations, and essentially a brand new problem that does not fit into pre-established algorithmic approaches; historical data are at best partially useful, or at worst irrelevant or nonexistent	is being analyzed through models where the effort required to interpret them at least equals that expended for their construction, implying a need for greater transparency in process and content	cannot presume the formulation of a decision in advance of choosing among alternative means, but in conjunction with such choices	is compartmentalized and suffers from methodological specialization	exhibits any or all of the following characteristics: (a) the task is ambiguous; (b) the structure through which the task might be accomplished is loosely defined; (c) the standard against which success is to be measured remains unstable; (d) knowledge of the organizational and wider environments remains uncertain; (e) the opportunities for collecting more data/information/facts are constrained; (f) it is not apparent where problem centers lie, and less apparent where and how intervention can be undertaken, even if objectives happen to be known; (g) the most intractable problem is that of locating, identifying and defining the problem	is a dynamic mess, consisting of complex systems of changing problems that interact with each other	is unstructured, open to interpretation
	accounts for the semantic, as opposed to objectively quantifiable, qualities of available information	demands the development of knowledge about the problematic situation as it is being tackled (a simultaneous task of iterative exploration-resolution known as action research)	not necessarily mathematically based, but analogically ruthless in rigor and exactitude, whilst maintaining a healthy balance between theory, objectivity, and intuition	provides means for determining and managing conflicting, yet equally relevant, objectives, and facilitates problem solving by focusing upon problem structuring	accounts for relational dynamics, by modeling dynamic situations consisting of complex interactions involving continuously changing problems and decision making adjustments	based upon a planning as decision making paradigm that guides the identification and stipulation of problems as well as goals, whilst dealing with the 'wicked' uncertainty of the world where, at best, a range of possible events (contrary to explicit consequences) is sensed, and no probability distribution is forthcoming	is based on a synthesizing planning paradigm, one that promotes the exploration and design of a desirable (not necessarily optimal) future, and enables exploration of the ways it might be brought about	should develop decision making ability through a mental discipline of interpretative rigor that: (a) produces knowledge from the facts available; (b) applies this knowledge in the service of problem definition
The Methodology	is informed by the manner in which human beings learn, know, and understand	confronts and deals with subjectivity on its own terms	transparently articulates different perspectives, and produces of readily understandable models that allow for errors to be recognized quickly and hence render further investigation easier	enables group decision making processes, facilitating an exchange of knowledge, perspectives, and expertise, thereby furnishing models as useful coordinating devices for negotiation processes that can promote satisfactory degrees of accommodation between interested parties, and thereby promote effective action	can be applied equally to operational as well as strategic problems, helping to design policy and its relation to available information, and amenable to evaluation not only by technical designers but by decision makers themselves	does not (a) assume the existence of objective, logical, or even conventionally agreed upon, criteria that could indicate whether the, or a, solution has been found; (b) presume to claim that all possible solutions to a problematic situation have been identified and considered; (c) attempt to prove that the proposed solution to, or proposed structure of, a problematic situation is correct, true, wrong, false, good, or bad; (d) ensure that the level at which the problem is being tackled is effective; (e) hypothesize an exhaustive formulation that could be said to contain all the information a decision maker needs for tackling a problem.	can handle dynamic situations consisting of complex systems of changing problems that interact with each other, i.e. messes, through a particular type of planning, governed by structuring instead of optimizing	decisions, and other conclusions, must be traceable to knowledge (no matter how minimal) of the situation being tackled, and not to mere assumptions about what the situation is believed to be
	accounts for decision making as an output resulting from the subjective transformation of information inputs, making this transformation process a crucial variable to be tackled	operationalizes a fundamental switch in focus, from decision making content to decision making process	evaluates particular problems against perceptible (though uncalculated) long-run consequences, thus providing a comprehensive look-out without having to solve everything at once	facilitates an evolutionary, iterative decision making process, wherein decisions and actions are taken based on a particular mix of objectives and adjustments, resulting in new opportunities and problems that require further decision making iterations	focuses upon improvement, instead of outright optimization, by interrelating multiple decision points and available information streams, allowing decision makers to examine how one decision, or a set of decisions, affects, and could be affected by, the perceptibly whole situation	offers a model of planning as an argumentative approach process, in the course of which an image of the problem, and of the solution, emerges gradually among the participants, as a product of incessant judgment, subjected to critical argument	can help manage the interaction between the parts constituting a mess	effectiveness is measured in proportion to the deduction of significant information, from the facts available, which respects the degrees of interpretative freedom allowed by such facts
			alleviates the need for large data sets, enables the development of new criteria, accounts for the socio-political dimension, facilitates the design of complex sets of relationships, and provides effective communicative means	facilitates short-to-medium term decisions based upon an information base limited in scope and liable to socio-political power-play, whilst maintaining a look-out of possibilities on the horizon	applies generically applicable structural concepts that can enrich intelligence and refine judgment, and provide models to be used as a common reference for studying, learning, and negotiating about problematic situations	offers models whose boundaries, variables, and designed interactions are not based on assumptions about how the situation is believed to be, but are defensible and justifiable against traceable knowledge of the situation being tackled, no matter how minimal such knowledge might be	promotes a synthesizing practice of design (or redesign) of organized systems so as to reduce or eliminate messes, whilst enabling action research, organizational learning and effective adaptation in rapidly changing, dynamic environments	views as significant information that: (a) is interpretatively sound; (b) effectively serves the interests of the management of uncertainty inherent in the situation; (c) ultimately renders the decision maker tangibly better informed, better equipped, and more confident to deal with the situation
					enables more resourceful use of whatever limited information is available, as well as a useful portrayal of its implications; identifies feedback loops and assists in their resolution; and, might offer a comprehensive scope, but need not require models to be comprehensively detailed			produces a systemic action plan of interrelated decisions that maps out how the problematic situation will be tackled systemically, based upon a means for: (a) discriminately extracting interpretatively sound information from the facts that are available; (b) discriminately structuring the extracted information in a way that enables rigorous problem definition; (c) discriminately planning a systemic approach toward resolution

Figure 6: Summary of situational characteristics of, and methodological requirements for, making decisions in the absence of clear facts

On the general level, Sinn (1998) notes that ‘Soft Systems Methodology (SSM) is a problem-solving framework designed specifically for situations in which the nature of the problem is difficult to define’. An understanding of SSM as designed ‘specifically’ for such situations renders the methodology relevant to the making of decisions in the absence of clear facts. In such cases, moreover, the limited information available will inhibit assumptions about the real world. Since, as Sinn goes on to explain, SSM does not demand that such assumptions be made, we find that, at least on the general level, SSM is worthy of consideration.

On the specific level, the evidence is in three parts, each referring to one of our methodological requirements. First, Brocklesby (1995) finds that SSM ‘can be exploited to produce information superior to that obtained through using conventional methods’. This clearly addresses the first of our three general methodological requirements, even if Brocklesby does not elaborate on what is meant by ‘superior information’. We have criteria for identifying ‘significant’ information. We shall be looking, therefore, at the extent to which SSM’s information extraction can meet our criteria.

Second, for Checkland (1999: A43; 2000) and Rose and Haynes (1999), SSM facilitates decision making effectiveness because, *qua* methodology, it is flexible to use but simultaneously provides a ‘rigorous approach to the subjective’. What we require is a rigorous approach to problem definition. Since, however, our ‘problem definition’ depends on a discriminate structuring of interpretatively sound information, a ‘rigorous approach to the subjective’ is part and parcel of our second methodological requirement.

Third, for Bolton and Gold (1994) the seemingly paradoxical mix of rigor-inflexibility, referred to above, facilitates systemic planning. ‘Soft Systems Methodology,’ they claim, ‘offers a rigour and discipline which automatically forces systemic thinking over and above received “textbook” wisdom or entrenched custom and practice’. This ‘automatic forcing’ of systemic thinking addresses the third of our methodological requirements for discriminately planning a systemic approach toward resolution.

We have, therefore, expert testimonies, stemming from direct applications of SSM, that respectively speak to our three methodological exigencies for making decisions in the absence of clear facts. This matching between the testimonies and our methodological requirements amplifies our theoretical basis for making decisions in the absence of clear facts. It further presents us with three practical objectives: (1) knowledge production/extraction, (2) knowledge application to rigorous problem definition, and (3) systemic planning as decision making. A summary of this theoretical foundation is given in Figure 7.

Making Decisions in the Absence of Clear Facts				
Impacting Issue	Effectiveness Criterion	Methodological Requirement	Soft Systems Methodology (SSM)...	Practical Objective
Knowledge Management	Resourceful use of limited facts	A means for discriminately extracting interpretatively sound information from the facts that are available	... 'can be exploited to produce information superior to that obtained through using conventional methods'	Knowledge production / extraction
		A means for discriminately structuring the extracted information in a way that enables rigorous problem definition	... 'is a rigorous approach to the subjective'	Knowledge application to rigorous problem definition
Systemic Planning	Incorporating systemicity	A means for discriminately planning a systemic approach toward resolution	... 'offers a rigour and discipline which automatically forces systemic thinking over and above received "textbook" wisdom or entrenched custom and practice'	Systemic planning as decision making

Figure 7: Theoretical foundation for making decisions in the absence of clear facts

It is worth pausing for a moment to appreciate how far we have come, not only temporally but especially conceptually. We began with Miller’s memory-based experiments in order to get an idea of cognitive limitations operating on decision making. Moving away from the laboratory and into the real world, we perceived a fixation with quantitatively-based comprehensive models. Their dependence on having all the facts rendered them practically cumbersome, not to say unsuccessful. This same conclusion was being felt in management science, but this time from the point of view of working with limited facts. Management science developed a means for providing comprehensive understanding without comprehensive detail. Although a significant step forward, it gave rise to new challenges, namely the difficulty of identifying problems and relevant interventions in complex networks. A consensus began to emerge as to what was required for dealing with complex, pluralistic situations. This pointed toward a synthesizing, as opposed to purely analytic, decision making paradigm. It led us to identify two essential issues that impact upon making decisions in the absence of clear facts: knowledge management, and systemic planning. The lessons from our historical excursion enabled us to identify effectiveness criteria and methodological requirements for these two issues. Developments in management science, furthermore, enabled us to identify one particular methodology, SSM, that appears to speak to our objectives.

We should also recall the main reason why we embarked on this journey in the first place. Bennis and O’Toole (2005a) challenged us to provide decision making methodologies that could ‘help in understanding how to interpret facts’ and in making decisions in the absence of clear facts. In Figure 7, we have not only provided a theoretical foundation for such methodologies, but identified one particular methodology that the literature claims might answer the need for effective decision making in the absence of clear facts. In the Introduction, we also uncovered seventeen skills which Bennis and O’Toole consider important for the training of decision makers, and especially for training in making decisions in the absence of clear facts.

Our historical excursion has given us an opportunity to consider many of these skills, and we have appreciated their relevance to real-world decision making. It remains to be seen to what extent SSM can provide the required training. Indeed, our next task will be to operationalize the theoretical basis in Figure 7, so that effective decision making in the absence of clear facts can be rendered a reality.

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