Metodologias Multicritérios de Apoio à Decisão

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Decision science or decision-aid science?

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Abstract: The concepts, models and procedures used in Operational Research and Decision Aid (OR-DA), unlike their counterparts in the physical and natural sciences, can scarcely claim to describe realities which would be independent of the observer and which would exist independently of other human actors. We must admit that in most decision-making contexts, various participants in the process interact with reality – as much through the judgments they bring to bear as through their behavior – and contribute to creating what we would like to describe as an external object. Even in instances in which such interaction is virtually non-existant, the results or 'truths' which the use of our concepts, models and procedures enables us to reach remain contingent upon numerous options (how a problem is formulated, the means by which uncertainty, imprecision and the ill-determination are taken into account, etc.), as well as upon one or more value systems. In order to give meaning to results produced in OR-DA, researchers have followed three main paths. Each of them may be, but does not necessarily have to be, associated with a particular quest: the path of realism and the quest for descriptions for discovering, the axiomatic path and the quest for norms for prescribing and the constructivist path and the quest for working hypotheses for recommending. Each of these paths and quests are presented in turn and submitted to a critical examination. It emerges therefrom that a 'decision science' (the precise meaning of this expression is specified) can only be rooted in the path of realism, which implies accepting postulates and hypotheses which have proved unusable in the practice of OR-DA. The article concludes by showing how by shifting the object of the quest for knowledge it nonetheless appears possible to speak in terms of a decision-aid science. However, within this framework, the validity and viability of the body of knowledge produced remain sources of further questions.

Keywords: Decision theory; Modelling; Multi criteria analysis; Philosophy; Decision aid

1. Introduction

"For which of you, intending to build a tower, sitteth not down first and counteth the cost, whether he hath sufficient to finish it?". [1] For centuries voices have been raised advising us to take the time to reflect, to calculate, to anticipate before reaching a decision and acting on it. Gradually there arose the notion of referring to abstract ideas and hypothetical–deductive reasoning to guide and justify human actions. Even as early as the Pythagoreans, abstract knowledge, the mystery of numbers and the harmony of spheres was thought to confer power over matter. Much later, the science of calculating probabilities began to develop, particularly under the impulse of very concrete issues linked to parlor games and insurance (both of which held an important place in XVIIth century society). See citation 1. According to Condorcet (see Baker, 1982), calculating probabilities should define the instrument through which the contingencies of life and human behavior can enter into the world of mathematical ideals. The field of social mathematics thus created should enable us to change human deliberation and decisions (in particular political decisions expressed through voting) into

[1] Luke 14:28 (St. James version).

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How should we perceive the world, find a place in it, act in it ? ... Westerners, believing they have the best possible conception of the world and the most superior conception of mankind, think that for this reason their vocation is to produce the 'best knowledge' ever known... From hence it results that all other types of knowledge are evaluated with reference to norms and criteria dominant in a society obsessed with the 'rationality' of efficiency, productivity and profit... All the rest is relegated to shadowy corners (primitive mentality, irrationality, magic, mysticism, etc.) (Thuillier (1988)).

Citation 1

rational decisions. For his part, Leibniz thought morality should be submitted to deduction and mathematical calculation. Auguste Comte dreamed of a government guided by science (Citation 1 leads us to question on this kind of objective). Through a scientific analysis of work, Taylor hoped to diminish human misery (see Citation 2). These are merely a few examples, no doubt selected somewhat arbitrarily, among the many others attested in history.

After the second World War, this hope of reasoning out decisions – or will to reason them out more rationally – firmly took root with what is referred to by the general term of Management Science and, more specifically, with Operational Research (OR) and Decision Aid (DA). [2]

At the present time, many organizations, both public and private, have research units or divisions whose job is to prepare decisions in ways thought to be scientific. The number of specialized journals and other works dealing with decision theory or decision science, Operational Research, Management Science, etc. has increased significantly.

[2] The exact meaning attributed to terms like Operational Research and decision aid or science will be explicited later on in the text.

On the subject of the scientific analysis of work credited to W. Taylor, G. Friedman writes: The scientific analysis of work became a universally applicable method because of its precise definition of chronometric measurement which could be applied to any type of operation whatsoever, enabling us to deliit movements rigorously and determine the normal length of any task carried out on the shop-floor or in the office. Thus for each job studied we can define an exemplary method, the best, the only method, 'the one best way'... The goal that Taylor set for himself, in the final analysis, must have been to diminish human suffering. With a purely technical postulate as his starting point, he achieved results of a high moral calibre. He brings up none of the questions that his methods would later raise in the world of work, especially among corporate executives (Gille (1978)).

Citation 2

This work and the activity resulting from it are, in practice, the outcome of an approach to work which frequently diverges from the ideal of objectivity normally attributed to science (in conformity with the meaning of objectivity precised in Citation 3). Indeed in many cases those who claim to shed light objectively on a decision take

In the way I will use the term, objectivism with respect to human knowledge is a view which stresses that items of knowledge, from simple propositions to complex theories, have properties and characteristics that transcend the beliefs and states of awareness of the individuals that devise and contemplate them (Chalmers (1982)).

Citation 3

a stand – consciously or unconsciously – for an a priori position or for a prevailing hypothesis which they then seek to justify. Arguments for making a decision are thus put forward more in the spirit of advocacy than in that of an objective proof (see GRETU, 1980; Amstrong, 1979). We should note that Mitroff (1972) sees this advocacy strategy as a legitimate and perhaps desirable approach to science. What occurs in practice does not prevent a number of Operational Research and Decision Aid theoreticians [3] from assuming – implicitly or explicitly – that only the lack of means (time and money) limits the validity of this decision-aid mode. In so doing, they consider that in all circumstances there is, objectively speaking, an optimal decision (taking into account certain characteristics of the individual or group in charge of it) which any analysis or research should try to approximate as closely as possible.

In spite of this discrepancy between the interpretation of what is done in practice and the theoretical conception of what could or should be done, a certain body of knowledge subject to critical discussion. What gives meaning to this body of knowledge and what are the areas where it teaches and informs us? Does it teach us to know and to approximate the best objective decision or, less ambitiously, to make decisions in favor of what we think is most suitable for us? The aim of this article is to present my own position on this issue and to emphasize how crucial it seems to me by considering the following questions: under what conditions are we justified in recognizing a value in concepts, models, procedures and results, and to what bases should we refer in judging the validity, viability and reliability of knowledge produced in OR-DA. The second question, however, can only be dealt with peripherally in this article.

In order to take a stance on the first question and, consequently, to reach a deeper understanding of the concepts involved in OR-DA, the nature of the models produced within this discipline and the nature of the results which OR-DA procedures lead to, it seems appropriate, as we shall see, to analyze the three main paths taken to give meaning to knowledge produced in OR-DA. [4] Each of these paths tends naturally (although not necessarily) to influence, in a certain way, the nature of the goal towards which it draws us, and this affects the answer to the second part of the question: What does this body of knowledge inform us of? It is from this perspective that we shall analyze in turn (see Sections 3–5):

– the path of realism and the quest for a description for discovering;

– the axiomatic path and the quest for norms for prescribing;

– the constructivist path and the quest for working hypotheses for recommending.

At the outset, to avoid any confusion or misunderstanding, it would seem necessary to start first with some remarks clarifying several aspects of our subject (Section 2). In the final section of the article, I shall attempt, first of all, to show that in order to give meaning to the body of knowledge produced in OR-DA in reference to a 'Decision Science', the first of these paths is the one which is and which should be followed, usually in conjunction with the second path. To follow the path of realism is to adopt an ontological position which seems unacceptable to me. [5] Finally, I shall show that il we abandon this path and follow the constructivist path, again in conjunction with the axiomatic path, we shall be going towards a science of decision-aid.

2. Preliminary remarks

Without claiming to give either rigorous or general definitions here, it seems necessary to me, in order to avoid any misunderstanding, to clarify precisely what I mean in the present article by the terms 'OR-DA', 'science', 'viable or reliable knowledge' and, finally, 'problem' (within the field of OR-DA).

2a) Concerning OR, I shall limit myself here to citing two definitions quoted by Van Gigch (1989):

– from Morse and Kimball (1951): "Operations Research is a scientific method of providing executive departments with a quantitative ba-

[3] See especially Kemeny and Snell (1962), Keeney and Raiffa (1976), Roberts (1976), Bourdaire and Charreton (1985), Fischer (1989), Keller (1989) and Von Winterfeldt (1989).

[4] Other points of view could be included within a larger perspective than that given in the title of the present article (see, for example, Déry et al., 1991).

[5] Without envisaging a similar opinion vis-à-vis other scientific fields.

sis for decisions regarding the operations under their control;

– from Miller and Starr (1969): "Operations Research is applied decision theory... (It) requires the use of scientific, mathematical, or logical means to structure and resolve decision problems. Construction of an adequate decision model is crucial".

DA can be defined (see Roy, 1985) as the activity of one who, in ways we call scientific, helps to obtain elements of answers to questions asked by actors involved in a decision-making process, elements helping to clarify this decision in order to provide actors with the most favorable conditions possible for that type of behavior which will increase coherence between the evolution of the process, on the one hand, and the goals and/or systems of values within which these actors operate on the other. Within this framework, DA relies on OR but also on other disciplines and on other approaches. Not every contribution from OR will necessarily be related to DA, insofar as certain purely mathematical studies which bear the OR label are not directly oriented towards decision aid. [6]

The preceding definitions have been reiterated here only to highlight the existence of a project – we could perhaps call it a 'research programme', using Lakatos' (1974) expression, inherent in OR-DA: seeking essentially to give scientific authority to decisions of a purely managerial nature. When I speak of the OR-DA field, I am referring essentially to this project. It is precisely the way in which this goal, ad correlatively certain methods used to attain it, are conceived of, that are under discussion in the present article.

2b) If, in the title of this article, I refer to Science, it is not because I believe that there is a unique, privileged category in relation to which we can situate, without ambiguity, the entire domain of (human) knowledge. As Chalmers wrote in 1982, "philosophers do not have resources that enable them to legislate on the criteria that must be satisfied if an area of knowledge is to be deemed acceptable or 'scientific'". Nonetheless, the qualifier 'scientific' is frequently used in a variety of contexts to express the idea that a body of knowledge (a network of concepts, models, procedures and outcomes) has been established by taking paths of a sufficiently rigorous nature, as well as appropriate to the goal set, to be recognized (at least temporarily) as valid by a given community (see Citation 4). We must be able to submit this goal, as well as the appropriateness of the paths taken to obtain it (especially the nature of the methods used), to critical discussion (see Citation 5).

2c) When we are concerned with either decision-making or decision-aid, to what sort of verification, to what type of critical discussion, can we submit the knowledge it produces? In OR-DA, the terms of a problem are not the same as they are in those disciplines related to the so-called

Scientific knowledge, like language, is intrinsically the common property of a group or else nothing at all. To understand it we shall need to know the special characteristics of the groups that create and use it... There is no standard higher than the assent of the relevant community... There is no neutral algorithm for theory choice, no systematic decision procedure which, properly applied, must lead each individual in the group to the same decision (Kuhn (1970)).

Citation 4

Like myths and cosmologies, science's endeavor is to understand the nature of the world, the way it is organized, and man's place in it... The most important aspect common to Greek thought and to modern science, which contrasts with the religious and mythical form of inquiry, is thus the emphasis on critical discussion and verification (Prigogine and Stengers (1984)).

Citation 5

[6] For the relationship between OR and DA, see Agrell (1983), Hatchuel and Molet (1986) and Roy (1987b).

'hard' sciences. Of course, as with the latter, we can subscribe to the position put forth by Popper and reproduced in Citation 6. Nevertheless, as we shall see, it would seem extremely difficult – whatever path we might take – to seek within Popper's falsification perspective (see Popper, 1968), to test the validity of methods – whether they be destined to discover, approximate or construct the 'best possible decisions' – as well as the reliability of the results they provide (decisions discovered, prescribed or recommended depending on which path we take).

Under these conditions the question becomes on what bases can we say that the knowledge produced within OR-DA is viable, that a method is reliable, that we are dealing with a body of scientific knowledge? As the present article unfolds, I shall base my response to this question on the text by Von Glasersfeld (Citation 7).

2d) In pratical experiences involving decision-making, the way in which *problems* arise is worthy of our attention. The formulation of problems, in the field of management (in relation to which the OR-DA project as we envisage it here is the target) will not be based on a one-to-one relation with a statement referring unambiguously to such categories as those we call 'data', 'unknowns', 'constraints', 'objectives', etc. In practice, in order to cope with a problem effectively, we need a sign of dissatisfaction, a judgment on something to be modified and a stake in which the actor feels involved. Which is to say that the problems we get interested in do not correspond to the Popperian image of a nesting box. Following Smith (1988), I would like to emphasize here the fact that these problems have no 'physical existence' but are, to the contrary, 'conceptual entities', 'constructs' (see also Citation 8).

Taking charge of a problem and, consequently, formulating the problem, cannot be envisaged independently of the relationships between an individual and reality. Furthermore, the perceptions of reality held by an individual, what he says and what he writes on the subject, the questions he brings up about it, etc. constitute a way of interacting with the real situation which may well contribute to changing it. Hence, we often observe that under the very influence of the problem-solving process (in other words, the influence of the role played by certain actors within this process) we find that the formulation of the problem itself is modified. Moreover, this type of modification does not necessarily stop at bringing in additional information or adding complementary formulations to the original one. The change, in some cases, may well be so radical that it is legitimate to query if we are still dealing with the same problem.

Scientific theories can never be 'justified', or verified. But in spite of this, a hypothesis A can under certain circumstances achieve more than a hypothesis B... The best we can say of a hypothesis is that up to now it has been able to show its worth, and that it has been more successful than other hypotheses although, in principle, it can never be justified, verified, or even shown to be probable (Popper (1968)).

Citation 6

Quite generally, our knowledge is useful, relevant, viable, or however we want to call the positive end of the scale of evaluation, if it stands up to experience and enables us to make predictions and bring about or avoid, as the case may be, certain phenomena (i.e., appearances, events, experiences). If knowledge does not serve that purpose, it becomes questionable, unreliable, useless, and is eventually devaluated as superstition. That is to say, from the pragmatic point of view, we consider ideas, theories, and 'laws of nature' as structures that are constantly exposed to our experiential world (from which we derived them) (Von Glasersfeld (1984)).

Citation 7

Scientific investigation, says Popper, starts with a problem and proceeds by solving it. This characterization does not consider that problems may be wrongly formulated, that one may inquire about properties of things and processes which later views declare to be non-existent. Problems of this kind are not solved, they are dissolved and removed from the domain of legitimate inquiry. Examples are the problem of the absolute velocity of the earth, the problem of the trajectory of electrons in an interference pattern and the important problem of whether incubi are capable of producing offspring or whether they are forced to use the seeds of men for that purpose (Feyerabend (1975)).

Citation 8

As Landry and Audet (1984) [7] have convincingly pointed out, the orthodox model of science and the naturalist attitude connected with it cannot be applied, without difficulty, to what they term the 'administrative science'. The criticisms they put forth concerning this are entirely pertinent for OR-DA: they imply freeing ourselves from what Nietzsche has called the dogma of the immaculate perception. In conclusion, therefore, it is important not to lose sight of the following in the remainder of the present article: that we do not discover a problem as we would a pre-existing object ; the formulation we give to it cannot be generally totally objective, but is expected to evolve throughout the decision-making process (see Roy, 1988).

3. The path of realism [8]: The quest for a description for discovering

I shall begin with a brief explanation of what I understand by following this path of realism (see

Realism typically involves the notion of truth. For the realist, science aims at true descriptions of what the world is really like. A theory that correctly describes some aspects of the world and its mode of behaviour is true... According to realism, as typically construed, the world exists independently of us as knowers, and is the way it is independently of our theoretical knowledge of it. True theories correctly describe that reality. If a theory is true, it is true because the world is the way it is (Chalmers (1982)).

Citation 9

Citation 9) in order to give meaning to the knowledge produced in OR-DA; in so doing, I shall point out briefly [9] how this path has left its mark on OR-DA. Taking this path will usually lead us towards the quest for a description, which should allow us to discover or approach a better decision. I shall then show, through the use of several examples, that this quest comes up against serious difficulties in OR-DA. I shall conclude by pointing out the link between taking this path and the way in which we conceive of managerial problems.

3a) Taking the *path of realism* consists of acknowledging that a certain number of objects, about which we can reason objectively, pre-exists 'out there somewhere' independently of any research work carried out. This is a Platonic concept which is not unrelated to the fact that OR researchers are mathematicians. Indeed, most mathematicians are Platonists as Citation 10 explains (see also Changeux and Connes, 1989).

Faced with a managerial problem involving decision-making, many OR researchers think that a certain number of constraints which delimit a set A of feasible solutions or even potential actions, etc. exist objectively outside time and independently of the different actors involved in this

[7] See also Landry (1991).

[8] which we could also call the Platonic path.

[9] For more details, see Déry et al. (1991).

decision. In the same way there pre-exists in their work and debates, a notion of 'better' and 'worse' which enables us to compare these solutions, actions, etc. It can take the form of an implicit system of preferences, pre-existing in the head of a manager commonly called the 'decision-maker'. There are also costs which represent, in financial terms, the impact of each decision.

The analyst who pursues this type of reasoning will naturally want to describe what exists. He will endeavor to 'stick to' this reality as far as possible, to *simplify*, but not too much, to find correct *approximations*, to avoid *biases*, to track down sources of *imprecision* with reference to this reality which is itself precise. Only our insufficient information and the restricted nature of our computational possibilities will be commonly invoked [10] by those who take this path of realism to explain the imperfections in our knowledge. Be that as it may, what this knowledge teaches us about is clearly defined: a reality which exists independently of the person or people who formulates the problems and/or who produces the knowledge concerning it. Under these conditions, the 'scientific' attitude consists of seeking to describe this reality, of attempting to discover or to approximate – to the extent to which that might be possible – the (or a, if several exist ex aequo) best decision simply because it exists.

If, after all, $e^{\pi i} = -1$ is a fact of the universe, an immutable truth, existing for all time, then surely Euler's discovery of this fact was mere accident... Sooner or later, it would have been discovered... Mathematical Platonism is the view that mathematics exists independently of human beings. It is 'out there somewhere', floating around eternally in an all-pervasive world of Platonic ideas... But there is another view of the matter. This opinion holds that applications of mathematics come about by fiat. We create a variety of mathematical patterns or structures. We are then so delighted with what we have wrought, that we deliberately force various physical and social aspects of the universe into these patterns as best we can (Davis and Hersch (1981)).

Citation 10

3b) What do we observe of *decision-making in practice*?

Not uncommonly, the solution ultimately adopted in the context of a problem does not belong to the set A of feasible solutions taken into account by the OR-DA model. It may be that this solution was not envisaged initially because it was too unorthodox, too troubling, etc. and that only later, during the decision-making process, did it appear to be the 'best' possible outcome, given a complex situation – due to conflicting criteria, for example. It may also be that this solution was clearly excluded because it violated certain constraints, as initially put forth, and that when no solution from A could give satisfaction, the aim of the decision was finally to modify the constraints.

Many experiments carried out to elicit utility functions (in the Von Neumann–Morgenstern and multi-attribute theory sense of the term, see Keeney and Raiffa, 1976) by questioning techniques demonstrate that the results we arrive at depend on how we go about obtaining them (see Fischer, 1979; Delquié and de Neufville, 1988; McCord and Leotsarakos, 1988; de Neufville and Delquié 1988; see also Roy, 1987a).

When we are confronted with several criteria that should play a role in guiding decision-making and seek to obtain information concerning the relative importance or weight of the different criteria, it is not usually difficult to achieve that aim. Let us specify that this information (whether expressed in qualitative or quantitative terms) acquires significance only if it is clearly linked, on

[10] We can also consider that objects have an existence independent of the action of those who seek to know them, but the knowledge we acquire of them is dependent on our actions. This is another way of explaining the imperfection of out knowledge, something which is rarely brought to the fore. This explanation corresponds to a constructivist position which does not automatically lead to the quest for a description of concrete reality.

the one hand, to a precise and quantified definition of the criteria and on the other hand, to a well-identified aggregation model (see Roy and Bouyssou, 1993, Ch. 5, Sec. 4). Thus, it is so when this is a weighted-sum model (a model frequently referred to), with any unit changes on a criteria, and, a fortiori, any modification in the definition of that criterion, the weights involved will not remain unchanged. The same is true, moreover, of many other aggregation models. People questioned in order to determine the values to be assigned to weights are not, in general, conscious of the ties which have been mentioned here. It follows that they will reply freely to questions without being disturbed by the fact that the units and even the precise definitions of criteria have been left vague.

Many costs which play critical roles in numerous models refer to virtually unspecifiable realities, at least in the context of certain applications. This is true for stock out costs or procurement costs, for example, in decision-making problems connected to risk management. The same is true for the costs of putting in orders in supply management (see Roy and Letellier, 1989).

These are a few examples chosen among many others, which might also serve to illustrate the same kinds of difficulties.

3c) *Confronted with the above-mentioned difficulties*, we are tempted to say that they arise because the problem has been poorly formulated, notably due to inadequate preliminary analysis. I think that this is not the case, since this objection is valid only if we reply affirmatively to the following question:

In general, when confronted with a situation which requires decision-making, is it desirable and / or possible to set forth the problem *in such a way that resolving the problem will not contribute to changing the very terms used to formulate it?*

It is commonly acknowledged that the above should be true for problems 'correctly set forth'. Whenever the resolution of a problem leads us to query the way in which it was set forth, although whatever (see 2d) gave rise to the problem initially has not been modified, it is a natural reflex to say that the problem in question is *different* from the initial problem, one which had been inaccurately set forth.

This concept of the 'correctly set forth' problem presupposes that the solution is determined solely by its formulation. In other words, the description of the problem should be able to impose the solution. It is thus the way in which a problem is put forth which creates both the existence and content of the solution. The latter, consequently, should never contingent upon the mode of reasoning used.

For whomever would reply 'yes' to the preceding question, it is clear that the path of realism and the whole conceptual universe attached to it are pertinent in giving meaning to knowledge produced in resolving 'correctly set forth' problems. In the field of decision-aid, however, it may be to our advantage not to seek, systematically, to dissociate formulation from investigation. We might indeed want to progress simultaneously on both fronts. This will then lead us to answer 'no' to the question above and to look upon the oft-used notion of the 'correctly set forth' problem as an unsuitable concept.

So the situation is that within OR-DA, it is not a rare occurrence to affirm that a supposedly correctly set forth problem will turn out, in fact, to be inadequately formulated with regard to the reality involved. Moreover, when this does not occur, it seems to me legitimate to ask if the way in which the problem was set forth was the only acceptable way of formulating it (see Weber and Costunoglu, 1990). My entire experience in OR-DA leads me to think that the reply to the question asked at the beginning of c) is, in general, a negative one. In other words, confronted with a given decision-making context, it is, I believe, exceptional to find that there is only one way of 'correctly setting forth' the problem (the formulation of the problem is indeed to a large extent dependent upon how the formulator of the problem looks at and perceive it, as well as on the actions he or she envisages relative to the problematic situation).

Under these conditions, we cannot say that with another formulation (of different data, unknowns, constraints or objectives) and/or another way of coping used by a different actor (see 2d), the solution would have been the same. This is yet another limitation inherent in the path of realism and the quest for a description. Whoever follows the path of realism is naturally inclined to consider that there is only one correct way to set forth a problem and that this correct formulation

itself forms a part of reality. Let us observe that seeking to explain the difficulties alluded to in 3b), with reference to this notion of the inaccurately formulated problem, is a self-referring procedure whose limitations and distorted effects have been ably discussed by Hoftstadter (1979).

4. The axiomatic path and the quest for norms for prescribing

In order to avoid misunderstanding, I think it is necessary to emphasize at the outset that this second path may be combined just as readily with the path of realism as with the constructivist path. I shall begin by explaining what this second path consists of and by drawing attention to the fact that it is often taken in the quest for norms for prescribing. I shall then use a few examples to illustrate this position, examples which will allow us to get a glimpse of the difficulties involved in such a quest. Finally, I shall attempt to bring out certain aspects concerning the scope and limitations of this path, aspects which appear crucial to me when the axiomatic path is associated with a quest for norms for prescribing. I shall use the verb 'prescribe' in its strong meaning, as does Watson in citation 1. in line with the usage common to many anglo-saxon authors.

4a) According to the 1977 edition of the French Robert Dictionary, the term 'axiom' can be understood in two different ways: as an 'undemonstrable but self-evident truth to anyone who understands its meaning (first principle)'; or 'an intellectually self-evident assertion; a hypothesis from which we draw logical consequences with a view to devising a(n) (axiomatic) system'. When the axiomatic path is associated with the quest for norms (see especially Von Neumann and Morgenstern, 1954; Raiffa, 1968), it is rather the first of these two meanings which prevails with reference to a certain conception of rationality. For many authors, however, a certain ambiguity floats between the two meanings. Nonetheless, taking the axiomatic path does not necessarily imply the quest for norms for prescribing. Even il the two are frequently and implicitly associated, the term 'axiom' is also used where there is no question either of an undemonstrable truth or of an ideal rule which it would be rationally compulsory to follow. Axioms are thus seen as frames of reference for working hypotheses (see Sections 5 and 6). This is a different way of taking the axiomatic path, one clearly explained and illustrated in Perny's (1991) doctoral thesis (see also, along these lines, Leontief, 1971, and Vincke, 1991).

In OR-DA, the *axiomatic path* within the context of a problem which aims to combine elements, to aggregate points of view, to take a position in the presence of risks, etc. consists of transcribing, in formal terms, those demands reflecting a form of rationality in order to investigate its logical consequences. The purpose of this path of formal exploration is to learn something concerning the fact that as soon as we accept certain concepts, principles or rules as our starting point, then a given model of representation will arise from these principles, a given procedure will have to be followed and a given result will become truth.

Certainly in theory at least, following this path in order to discover a landscape of consequences in no way puts us under an obligation to conform to it. However, a strong tendency pushes us, de facto, to accept the axioms as such and to look for support to those consequences discovered so as to justify *norms*. In addition, we often attempt to create a situation in which the axioms form such a restrictive system that there is room for only a single mode of representation or a single procedure (up to the value of some scaling parameters). It is implicitly acknowledged that, if each of the axioms taken separately appears sufficiently natural to be imposed as a norm or to

The normative framework of the rational decision making paradigm was widely accepted many decades ago as an ideal to follow ; more recently it has been recognised that there are difficulties in conforming to this ideal. As a result a distinction has been made between normative methods - those that we would ideally like to follow - and prescriptive methods, that is, ways to prescribe how decision makers should approximate to this ideal in practice (Watson (1991)).

Citation 11

constitute an acceptable working hypothesis, then the procedure or mode of representation in question should be accepted. This is to some extent validated by the axiomatic system.

When the axiomatic path is taken in this spirit, it contributes to conferring meaning on certain concepts, certain procedures or even on the optimal character of a solution. These concepts, procedures and results can, in turn, participate in the development of norms, as well as of prescriptions, within the framework of some models. The prescription established in this way refers to an ideal which it claims only to approximate. It is the very existence of this ideal created by the axiomatic system which then confers all its validity and authority upon the prescription conceived of on such bases.

4b) In OR-DA, *axiomatic system have been developed*, designed to demonstrate, for example, that within a more or less clearly delimited context:

– addition was the only valid means of combining in order to aggregate elements of diverse types;

– a majority vote was the only equitable system for designating a representative;

– the cardinality of the symmetric difference between two binary relations R and R' (defined on the same set) was the only appropriate distance for appraising the greater or lesser quality of the approximation of R by R';

– the calculation of expected value was the only correct course of action for guiding behavior in face of risk.

Of course, within a different context justifying other demands, the system of axioms would not be the same. Other modes of aggregation, other means of voting, other distances, other courses of action for taking risk into account may then become legitimate. The link thus established between a system of axioms ad a procedure or a mode of representation necessarily arising from it constitutes, without any doubt, useful knowledge. The examples presented above give us a glimpse, however, of the difficulties involved in giving authority to norms on an axiomatic basis and in establishing a prescription as an approximation of an ideal characterized by an axiomatic system.

4c) In order to *evaluate the range* of this axiomatic path, whether to prescribe or (in the weaker sense) to recommend decisions, we must

> *To 'accept' something - an experience or an object - 'as such' consists in taking it for what it appears, or is purported, to be, and proceeding on that basis. There is all the difference in the world between (1) accepting something as being what it appears to be and proceeding on that basis ; and (2) merely exploring the consequences of the assumption that something is what it appears to be (Stolzenberg (1984)).*
>
> *Citation 12*

draw attention to and clarify certain points. I think it is useful at the outset to remind ourselves that for some the final goal of scientific endeavor is the construction of axiom-based deductive systems. This is an objective, however, which gives rise to controversy. Coumet (1975) emphasizes that Popper has changed his mind on this point: "The admiration he brought to the 'marvellous deductive systems' in mathematics and physics had given him too much confidence in this ideal which had dominated the theory of knowledge from Euclide to Einstein.... He has come to acknowledge that this confidence itself originated in a prejudice: the remains of the domination of Newtonian ideas and, to go back even further, Platonic and Euclidian concepts ; what is much more decisive than the 'deductive development', upon which scientific rationality resides, is the 'critical procedure' in its different forms, through which 'our errors instruct us'".

The axiomatic path leads us with ease to believe that with axioms we possess the means of gaining access to truth clearly validated by what the axioms seem to convey. This is precisely the danger in accepting things 'as such' which Stolzenberg denounces (see Citation 12). An axiom is not necessarily the incontestable formulation of rationality that it appears to be. It is interesting to suppose it so for the purpose of exploring the possible consequences, but it is only in relation to the consequences thus brought into the open, within the framework of an axiomatic system, that we will understand what the axiom considered really is. The pitfall then consists of

taking it rather for what it seems to be or for what it claims to be and to proceed following this basic datum.

I shall conclude by offering three remarks meant to illustrate the points discussed above and, more generally, to make plain the extent and limitations of the axiomatic path in OR-DA.

1) It is not always easy to establish a link between a formal statement expressed within an abstract context and reality related to decision-making or behavior. This can result from the rather esoteric nature of the axioms. Nonetheless, we should observe that a statement will seem much more intelligible, attractive and gratifying when it is in harmony with the surrounding culture and will thus tend to be accepted without any critical discussion. Thus it was that for centuries scientists believed that the postulate of parallel lines could one day be deduced from other Euclidean postulates. Because of this, it was non-productive of serious knowledge to try and deny the postulate of parallel lines (Gauss hid his work on non-Euclidean geometry). In the same way, in OR-DA certain formal properties which seem natural in guiding decisions, such as the transitivity of indifference, or indeed strict preference or even independence in relation to irrelevant alternatives, can be productively challenged.

2) Even if each of a system's axioms taken separately seems undeniable (and should be accepted as a norm), we nonetheless have no basis for accepting these axioms collectively. Indeed we know of many cases in which this type of system-building has produced surprising results: Allais' theory of utility and paradox, Arrow's impossibility theorem reinterpreted within a multi-criteria decision-aid context, etc. We can thus be right in rejecting one axiom in consideration of others that we believe to be accepted.

3)In order for a procedure or a mode of representation to be well defined, up to the values of some scaling parameters, by a system of axioms, it is often necessary for the latter to admit of a statement which uses the path of realism. In this way Von Neumann–Morgenstern's utility theory and, more generally, multi-attribute utility theory presuppose the existence somewhere (in the mind of a decision-maker) of a complete preorder in the set of probabilistic distributions; everything else is formulated with reference to this reality.

5. The path of constructivism [11] and the quest for working hypotheses for recommending

The third path and the quest associated with it take on all their meaning only if we acknowledge (see (2d)), as we progress gradually towards the elements necessary to solving a problem, that some initial data may cease to be pertinent, others may appear, new questions may be substituted for the original ones (even if the initial problem has not fundamentally changed). This means that we would accept a negative answer to the question raised in the beginning of (3c). It is, consequently, within this framework that I shall present this path and the quest for working hypotheses to which it leads. I shall then illustrate this with several examples. Finally, I shall discuss the scope and limitations of this third path in comparison with the two presented earlier. This will, in turn, lead me to approach the issue of the validity and viability of knowledge produced in OR-DA.

5a) Taking the *path of constructivism* consists of considering concepts, models [12], procedures and results to be *keys* capable (or not) of opening certain locks likely (or not) to be appropriate for organizing a situation or causing it to develop. The concepts, models, procedures and results are here seen as suitable tools for developing convictions and allowing them to evolve, as well as for communicating with reference to the bases of these convictions. The goal is not to discover an existing truth, external to the actors involved in the process, but to construct a 'set of keys' which will open doors for the actors and allow them to proceed, to progress in accordance with their objectives and systems of value.

It is within this perspective that we can most suitably submit to critical discussion, not only the set of keys itself, but also the manner in which it is used. In the same way that in order to open a series of locks we can have recourse to numerous

[11] Piaget's view (1967) that 'intelligence organizes the world by organizing itself' is central to understanding this path.

[12] Here, even more than with the other two paths, the term 'model' designates a schema which, for a given field of questions, is accepted as the representation of a class of phenomena, more or less adroitly detached from their context by an observer, in order to serve as a support for *investigation* and/or *communication*.

sets of keys and handle them in many ways, there is not only one set of tools suitable for clarifying a decision nor is there a single 'best' way to make use of them.

The selection and development of keys, namely the concepts, models, procedures and even results (possibly deduced from an axiomatic system) should be – if we want to be able to submit them to critical discussion – clearly connected to (indeed logically deduced from) one or several working hypotheses. The quest for such working hypotheses should be guided by the judgment brought to bear on their suitability, their appropriateness for progressing towards certain convictions able to provide the basis for what we shall call a *recommendation*. In this paper the term 'recommendation' has a weaker meaning than 'prescription' (see (4a)): the recommendation can be developed without seeking to approximate any (real or formal) entity considered to pre-exist, with regard to the study undertaken. The contents of the recommendation may be only the fruit of a conviction constructed in the course of a process necessitating multiple interactions, bringing into play a variety of actors involved in a complex managerial environment.

Thus, according to this path, we are forced to recognize that the knowledge we acquire in order to shed light on a decision cannot be wholly independent of the course chosen to arrive at this knowledge: OR-DA concepts, models, procedures and results appear as tools destined to accompany processes which converge only in that they lead to recommendations. [13] This recommendation cannot, generally, be regarded as the sole possible solution but as one we would like to be especially well-founded. All the same, as with all other solutions, a prescription remains contingent upon the course followed in order to obtain it. Even if the convictions we arrive at seem to be 'cast in concrete', this path always leads us to doubt that it is the 'only possible concrete' (see Citation 13). Within a decision-making context, the quest for working hypotheses upon which to base a recommendation is oriented towards the production of knowledge concerning how to act (contributing to a decision-making process), as much as towards the content of the recommendation, which is not based on the claim of discovering a reality.

> *We are fascinated by unity. Only unity appears rational to us... We construct realities which are rationalities. We construct one reality, amongst all possible realities, which is rational amongst other possible alternatives, just as we put down concrete on the ground. It is not the only possible concrete, it is not the only possible covering (Serres (1982)).*
>
> *Citation 13*

5b) The examples which follow *illustrate this quest for working hypotheses* which should enable us to select and/or develop keys leading to a recommendation.

1) There are numerous inventory management models based on the concepts of stock out costs and procurement costs: these costs are presented as if they enabled us to describe an objective reality and could, therefore, be calculated with suitable accuracy. Nonetheless, we often observe that in replenishment policy (notably for maintenance goods) such costs are quite artificial. To calculate them, we must develop many rather arbitrary hypotheses. Stock out costs and procurement costs are, more often than not, 'constructed' and the way in which they are constructed in general leaves a large margin for lack of determination. When we must choose the 'right' supply policy, working hypotheses are necessary to justify one, or possibly several, numerical values for such costs. These costs may also appear to be inappropriate keys or, in any case, keys to be used only with caution and as late as possible (see Roy and Letellier, 1989).

2) To effect comparisons and establish preferences, we are induced to conceptualize indices and criteria; these are often presented as descriptive of a reality which is designated in general terms: price rises, average traffic, rate of penetration, etc. The path of realism consists of postulating the existence of a real figure which measures the quantities thus designated and of attempting

[13] This idea of 'leading to' obviously does not imply that we are dealing with a linear series of a deductive type.

Let us observe that the notion of 'true value' of parameters characterizing a natural phenomenon (for example, necessary conditions of movement for the three body problem) relate only to representations of the phenomenon, and not to its reality. Stating that a parameter's value is found within a certain interval of precision in no way means that a value, which would be the 'true value' of this parameter, exists in this interval - because reducing the interval of precision through progress in our knowledge could necessitate a transformation in the very definition of the quantity the parameter in question seeks to apprehend and the splintering of its meaning into distinct meanings which only approximately cover the same ground (Bonitzer (1988)).

Citation 14

to construct an index or a criterion which most closely and/or without bias approximates this figure. The constructivist path does not seek to root this figure in objective reality. This path acknowledges that what we are concerned with is of such a high degree of complexity that there is no single dimension which allows us to apprehend it clearly and that, consequently, there are no 'true values' enabling us to give meaning to the notions of approximation and bias (see Citation 14).

In this context, in other words, there is an irreducible lack of determination margin to quantify what we are talking about (see Riveline, 1986; Roy, 1989). The key here is the index or criterion which has been constructed and accepted as the basis of reasoning and of comparison. It may be useful to assign it a discrimination threshold which should not be confused with a simple margin of imprecision (see Bouyssou and Roy, 1987).

3) As soon as several criteria come into play in order to make a decision, the question arises of how to characterize the specific role which falls to each of them. As such, it is usually a matter of the importance or weight given to each criterion. It is not uncommon for a decision-maker to affirm spontaneously that, in his or her view, such-and-such a criterion is 'more important than' another and that it would not trouble him or her in the least to give one the value of 0.6 and the other 0.4. The OR-DA literature abounds with articles describing procedures for 'estimating' these coefficients of importance or weights k_j, $j = 1, \ldots, n$ when n criteria are at stake. In this context, the concept of estimation is not usually an appropriate key. It has meaning only if somewhere there are well defined quantities k_j that we want to approximate as precisely as possible. It appears that this would only be true in exceptional cases for reasons that we will mention only briefly (for more details, see Mousseau, 1993).

The k_j coefficients appear in the majority of aggregation models and procedures (weighted sum model, hierarchical analysis, multi-attribute utility theory, ELECTRE and PROMETHEE methods, etc.). These models and procedures underscore the fact that the meaning of k_j-coefficients may be closely linked to the precise definition of criteria (notably those units chosen), as well as to the mode of aggregation (compensatory or non-compensatory). True values that can be estimated, therefore, exist only if the decision-maker clearly takes into account the finely honed definition of criteria in a well-defined aggregation model. We may consider that this is not usually the case (see 3b).

Under these conditions, the values envisaged for the k_j coefficients appear to be values that 'fit', values with which the decision-maker is willing to work. The k_j coefficients can, therefore, be looked upon as the keys which allow us to differentiate the role played by each criterion in the aggregation model selected. The meaning of these coefficients (and consequently of their values) is closely linked to the hypotheses which underlie this aggregation model.

5c) The recommendation developed may be formulated either as a remedy for the dissatisfaction from which the problem originated (see 2d), or as and temporary, intermediary acquisition to serve as a basis for discussion and communication. In both instances the recommendation's *impact will be limited* by the difficulties encountered in validating it: what confers a value on it and proves its viability ? Before suggesting several elements of a response to this question, I think it

is useful to emphasize (following on from considerations taken up in 3c) and 4c)) that it is not easier to provide answers to this question with one or the other of the two paths discussed above (even if the question is asked in rather different terms).

To the extent that in OR-DA we are frequently led to doubt (see 3b)) the very existence of what the path of realism leads us to describe, what means do we have at our disposal for validating a prescription? In particular, I have demonstrated elsewhere (Roy, 1987a) the illusory and even dangerous elements that could arise from trying to base this validation on a hypothesis of existence. With regard to the axiomatic path, I have explained why (in 4c)) it could only with difficulty claim to provide the foundation for a 'true' decision: the body of knowledge produced by this path does not of necessity spring from norms of action. The constructivist path, even if it is no more certain, at least has the advantage of claiming neither to provide the foundation for nor to approximate a truth.

What are the means at our disposal in OR-DA for validating the knowledge produced by the constructivist path? This is a subject which goes well beyond the scope of this article: I shall limit myself here to several observations and questions.

1) The mere fact that a recommendation developed using certain concepts, models, procedures and/or results has been accepted and has given satisfaction in no way constitutes a validation of these concepts, models, procedures and/or results. Likewise, a rejection of the recommendation cannot be regarded as a falsification (in Popper's sense of the term) of these concepts, models, procedures and/or results. To recognize their scientific value, it would seem to me that two minimal conditions must be met:

(i) that there is a sufficiently large research community which is interested in them and which sees – in these concepts, models, procedures and results – instruments appropriate to OR-DA projects;

(ii) that there is a fabric of decision-making situations, within the framework of which we can attest to their productive nature, enabling us either 'to make predictions' or 'to bring about or avoid, as the case may be, certain phenomena (i.e. appearances, events, experiences)' which relate to decisions.

These conditions might appear 'lax' and, consequently, insufficient for the kind of validation we would look for, when we are not referring to predictions.

2) In order to reduce this insufficiency we should, naturally, seek to reinforce the second of the two conditions mentioned above, furnishing at least some elements of an answer to the two following, closely interrelated questions:

– *In order to give a convincing account of their productive nature in terms of appearances, events and experiences related to decisions, what must we be able to observe?*

– *What protocol and demands must the account satisfy so that the validity and viability of the concepts, models, procedures or results in question will be recognized?*

Let us emphasize that within the path of realism, there is one correct answer to these sorts of questions: that we can give an account stating that, under experimental conditions, in line with a well-defined protocol, certain events of and qualitative or quantitative nature are produced in conformity with the *predictions* that the body of knowledge in question allows us to make. It is in this way that numerous experiments have enabled us to witness consistently the gaps between the real behavior of decision-makers put in risk situations (described in probabilistic terms) and the predictions of such behavior given by the experimental mathematical utility model in the Von Neumann–Morgenstern sense. This model's inability to explain the decision-making behavior it claims to describe constitutes a form of falsification which is not without interest for OR-DA. For all that, however, neither the concept of utility function nor the procedure for calculating expected value loses all its interest or viability as keys which can develop convictions and cause them to evolve, as well as communicate with respect to the foundations of these convictions (see Bouyssou, 1990).

The example given above underlines a major difficulty for replying to the questions posed previously. Within OR-DA, trying to base an account of validity and viability on the fact that predictions are verified would be too restrictive because the concepts, models, procedures and results whose validity we are attempting to evaluate are bound to influence future decisions and not give an account of what occurs outside their influence.

It is a metaphysical doctrine that from the same antecedents follow the same consequents. No one can gainsay this. But it is not of much use in a world like this, in which the same antecedents never again concur, and nothing ever happens twice... The physical axiom which has a somewhat similar aspect is that 'from like antecedents follow like consequents'. But here we have passed from sameness to likeness, from absolute accuracy to a more or less rough approximation. There are certain classes of phenomena... in which a small error in the data only introduced a small error in the result... The course of events in these cases is stable. There are other classes of phenomena which are more complicated, and in which cases of instability may occur, the number of such cases increasing, in an extremely rapid manner, as the number of variables increases (Maxwell (1873)) [1].

[1] *Quoted in Ekeland (1988).*

Citation 15

3) In OR-DA, much more than in the natural sciences, all experimentation will encounter instability (see Citation 15). This comes from the multiplicity of variables or factors which are impossible to control completely: the not always perceptible discrepancies in the human and organizatonal context, in the purpose of the decision, in the way questions are formulated or certain aspects of the consequences apprehended, etc. can be the cause of fundamental differences. Because of this, trying to base an account of value and viability essentially on the *repetition* and *reproduction* of experiments seems to me to lead to an impasse (which does not mean that we must give up all experimentation).

4) The final, and undoubtedly not the least of our difficulties in going beyond the necessary minimal conditions laid out in (i) above, derives from the interactions, which are often not negligible, between a finely honed and detailed observation of a decision-making process and the evolution of that process. Frequently, the mere fact of being interested in a decision, talking about it, reviewing the arguments, modelling and performing calculations interacts above all with the final result, but also with the entire decision-making context. Everything leads us to believe that in many cases (see 5b)) it is the very questions, techniques of observation, instruments for measuring, etc. which give concrete form and consistency to qualitative and quantitative aspects that we would have hoped to apprehend without in any way disturbing the system within which they are supposed to be resident.

5) Validation must always rest on the consensus of a community which has set rules for itself to this end. We should note that the rules that physicists set for themselves (following the path of realism), which were satisfactory over a long period of time, are today the subject of controversy (see, for example, D'Espagnat, 1983; Ortoli and Pharabod, 1984; Prigogine, 1990), primarily due to the type of difficulties cited above in 3) and 4). Overcoming such difficulties, as well as those mentioned in 2), and suggesting sufficient validation rules suitable for OR-DA hardly seems feasible as long as we want to proceed by transposing or transplanting rules which have succeeded in physics. We should avoid having to invent a reality for the needs of the cause. Be that as it may, it is important not to forget that the difficulties in validation which we have presented here are not inherent in desiring to follow the constructivist path in order to give meaning to knowledge; they are inherent, first and foremost, in the very object of the knowledge in question.

6. Why 'decision-aid science' rather than 'decision science'?

Throughout this article I have acknowledged that the OR-DA project (see 2a)) was capable of producing a certain kind of knowledge which is not devoid of interest. This is an assertion which might lead to controversy amongst those who, having adopted a determinist position (in line with the well-known text by Laplace reproduced in Citation 16), leave no room for free arbitration. For anyone thinking along these lines, the very notion of decision, as well as that of deci-

> *We ought then to regard the present state of the universe as the effect of its anterior state and as the cause of the one which is to follow. Given for one instant an intelligence which could comprehend all the forces by which nature is animated and the respective situations of the beings who compose it - an intelligence sufficiently vast to submit these data to analysis - it would embrace in the same formula the movements of the greatest bodies of the universe and those of the lightest atom ; for it, nothing would be uncertain and the future, as the past, would be present to its eyes (Laplace (1951)).*
>
> *Citation 16*

sion-aid, loses all interest. It is clear that I do not subscribe to such a position, but to elaborate any further on thispoint would fall outside the scope of the present article.

The fact that the body of knowledge produced can be a matter of concern for a science (in the sense put forth in 2b)) has been only indirectly discussed in the preceding sections of this paper. What allows us to judge the validity and viability of this body of knowledge has been the source of many questions. Are the methods used to produce this body of knowledge really appropriate to the specificity of problems involving decision-making, within the framework of the goal defined and, above all, does this goal actually stand up under critical scrutiny? The answer will differ, as we shall see, according to whether or not we consider that the object of the body of knowledge in question is decision-making per se or only decision-aid.

In order to speak in terms of a 'decision science', – in the sense I shall give to this expression below – we must be able to accept the first of two possibilities mentioned in the preceding paragraph. In the first instance, I shall explain why, in my opinion, decision science does not stand up to critical discussion. I shall then show that this is not the case with the second possibility, and I shall endeavor to explain (succinctly) my conception of a decision-aid science's goals and methods, envisaged in relation to the constructivist and axiomatic paths.

6a) I will designate with the term *decision science* [14] a science whose purpose would be the search for objective truths in decision-making and, more particularly, the knowledge – if not precise, then at least approximate – of the best decision within a given context, through the use of models presented as simplifications of reality. Such a science could not, in my opinion, exist without a first and fundamental postulate which I have termed the postulate of the optimum. I feel it is necessary to begin here by reviewing this postulate.

Postulate of the optimum. *In situations likely to involve decision-making, there will be at least one optimal decision, namely one for which it is possible (on the condition that we have adequate time and resources) to establish objectively that a clearly better decision does not exist. It should be possible to do this while remaining neutral in terms of the decision-making process itself.*

Objectivity, which goes hand in glove with neutrality, is based here on the following two hypotheses:

Hypothesis 1. *A criterion giving meaning to the concept of optimum can be defined independently of any opinion, conviction, value or human prejudice (see Citation 1).*

Hypothesis 2. *The optimal decision can be discovered or approximated and recognized as such independently of the models and procedures used to arrive at it.*

These two hypotheses stand alone only if we also accept the following postulate (see Roy, 1985):

Postulate of reality of the first order. [15] *The principal aspects of reality (an individual's prefer-*

[14] We will observe that it is impossible to confuse a 'decision science' thus defined with a science whose purpose is focused on describing and studying how actors decide, even if this science is to develop models which account for actors' behavior and predict some of their decisions (see, for example, Bell et al., 1988). We should observe that this science of *decision-making behaviour* covers a sphere whose concerns are rather distinct from those encountered in OR-DA. This does not mean that the body of knowledge it produces is not of interest to OR-DA.

[15] With reference to Watzlawick's (1984) terminology.

> *The utopian political expectations that have been removed from any context of divine revelation and attributed to science are relatively young... Whoever succeeds in comprehending nature's intrinsic order, in its existence independent of human opinions, convictions, prejudices, hopes, values, and so on, has external truth on his side. The scientist takes the place of the seeker after God ; the objective truth takes the place of superstition (Watzlawick (1984)).*
>
> *Citation 17*

ences, the borderline between possible and impossible, the consequences of an action) on which decision aid is based relate to objects of knowledge that can be seen as both given (existing outside any modelling thereof) and sufficiently stable (in the face of duration, diversity of actors, discourse held, observations made) to legitimate reference to the exact state or the precise value (which can be of either a certain or a stochastic nature) of those specific characteristics judged significant of one aspect of reality.

It emerges from the preceding discussion that a 'decision science', in light of the very object of its quest for knowledge, can only be anchored in the path of realism (the quest for descriptions and objective truths) although it must at the same time, in order to validate the knowledge it produces, find support in a quest for norms rooted in the axiomatic path. If, as I would claim, the postulates and hypotheses above are inherent in the goal of such a science and in the paths they must take to give meaning to the knowledge they produce, then the possibility of such a 'science' must be rejected. Indeed, the arguments developed in Sections 3–5 have shown why this conjunction of the first two paths is impracticable in OR-DA and have put forth many examples invalidating the postulates and hypotheses above. Is accepting these postulates and hypotheses not – as Citation 17 here suggests (see also Serres, 1989) – tantamount to wanting to make science play the role of a divinity and creating a new superstition out of objectivity?

6b) Let us now envisage another kind of 'science' whose object is not to know or to approximate the best possible decision but to develop a corpus of conditions and means on which we can base our decisions in favor of what we believe to be most suitable. By thus shifting the object of the quest for knowledge, it seems to me that we can conceive of a decision-aid science.

In my understanding of it, the *science of decision aid* would seek to develop a network of concepts, models, procedures and results able to form a structured and coherent body of knowledge which can act – in conjunction with the corpus of hypotheses – as keys, so as to guide decision-making and to communicate to its subject in conformity with objectives and values. Rigorous concepts, well formalized models, precise calculation procedures (notably optimization procedures) and axiomatic results should be at the heart of such a science. Through them, we should be able to claim to enlighten and scientifically accompany decision-making processes notably:

– by making the objective stand out more clearly from the less objective;

– by separating robust from fragile conclusions;

– by dissipating certain forms of misunderstanding in communication;

– by avoiding the pitfall of illusory reasoning;

– by emphasizing, once they are understood, incontrovertible results.

Thus conceived of, decision-aid science escapes those fundamental criticisms set forth above with reference to a 'decision science'. Indeed, when we consider the shift in the object of the quest for knowledge, it is possible to take into account:

(i) those aspects of reality which give meaning, value and order to facts (and thus free us from Hypothesis 1);

(ii) the influence exerted upon this reality by observing it, organizing it, provoking within it certain forms of debate or even having certain tools placed there (and thus freeing us from Hypothesis 2).

Such a science cannot claim to root its body of knowledge in the knowledge of objects entirely external to its own workings. Consequently, this

science cannot establish validity for its body of knowledge on the basis of its greater or lesser aptitude for reflecting, even through simplifications, what is claimed to be reality. This science would have no need, therefore, of the two postulates put forth in 5a). This does not signify that it should give up all description: descriptions of physical, financial, institutional, socio-economic, psychological, etc. systems (produced [16] in other disciplines) will obviously have a role to play within decision-aid science. It is nonetheless true that the path of realism can only play a rather marginal role in producing and validating knowledge in decision-aid science. The latter must, in my opinion, be developed essentially within the constructivist path, taken in conjunction with (and observing certain precautions) the axiomatic path.

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[16] This is particularly true of descriptions reached within what we have termed, in Section 6 (see footnote 14), the science of decision-making behavior. Note that these descriptions often bear on the result (the decision ultimately made) of an interaction governed by a rigorous protocol implemented within a very specific context. Thus it is shown that a certain type of model may or may not give an account of what such an interaction produces.

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