Health systems research:
a clearer methodology for more effective action

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This same pattern has been followed in the 'birth' of each scientific discipline. It is always difficult to distinguish a new field from those out of which it arises because of the overlap of problems, methods, and concepts. In time the differentiation becomes more complete and practitioners are no longer plagued with the question: 'How does this differ from such and such a field?'

Churchman, Ackoff and Arnoff, on operational research, forty years ago (1957).

Introduction

Though recent years have seen a spate of ever more sophisticated work in the fields of basic, clinical and epidemiological medical research, we have doubts as to how useful some of this is, in terms of knowledge acquired and actual benefits to the population or the sick. The potential offered by research centred explicitly on health systems, on the other hand, seems to us to have been underestimated.

There has, it is true, been an increase over the last decade in the number of studies ostensibly dealing with health systems research (HSR). Unfortunately, judging by their fields of interest, topics studied, working methods and practical implications, these would seem to have done little to clarify what HSR actually is.

It seems to us that the problem with HSR lies in the fact that it still has no reference methodology and is seeking to define itself by borrowing from various existing methodologies.

In 1984, Carl E. Taylor warned against rushing into deciding whether or not a study fell within the field of HSR on the basis of the methods it used: “Health systems research leaves no stone unturned in that it borrows its methods from several disciplines. Given that this field is still evolving, it would be inappropriate to adopt a restrictive approach in specifying the methods to be used. The current practice of borrowing different methods should eventually produce some useful syntheses and adaptations” (Taylor, 1984, p.29). This cautious approach set a fashion. It has made it possible to use the umbrella of HSR to carry out all sorts of studies, ranging from routine collection of information for local management purposes to action research, via purely traditional sociological and epidemiological studies. Perhaps it is now time to move away from a catch-all concept of HSR and offer researchers and research-funding bodies a summary of methodologies which can be used.

Our aim in this paper, then, is to offer criteria for assessing studies in the field of HSR, focusing our attention primarily on methodology. Although our approach is, in part, a normative one in that it seeks to circumscribe a field of knowledge (to say what HSR actually is), its main aim is to draw attention to the need for innovative research in the field of health (to underline that research into the “health sciences” needs to be redirected).

We are less interested in a strict definition of what does nor does not fall within the scope of HSR than in examining the potential contribution various types of research can make to understanding health systems and being able to act upon (and within) such systems.
The paper is in six parts:

1. The first explains the two-fold originality of our strategy: the scientific approach we advocate and the place assigned to HSR in the hierarchy of fields of knowledge. We then bring in the concept of systems analysis and modelling.
2. The second part takes a closer look at the specific nature of HSR, consi-dering these two key concepts through different methodological approaches. We first attempt to trace the dividing line between HSR and scientific management/evaluation. We then examine what operational research and action research can contribute to HSR methodology. After proposing a general methodological approach for use in HSR, we conclude with some considerations on the limitations of HSR and the apparent consensus among HSR researchers.
3. In the third part we illustrate our approach with two examples regarding control of tuberculosis in developing countries.
4. The fourth part looks at four research projects: a notional project taken from Taylor (1984) and three real projects taken from authors interested in various aspects of health systems.
5. These examples require us to draw a dividing line, in the fifth part, between the methodological approach we are proposing and qualitative research in the social sciences.
6. We conclude with a plea for greater integration of research and management into health systems but, at the same time, greater rigour in defining HSR, based on a clearer understanding of available methodologies.

Our professional experience as researchers and teachers will lead us to favour examples taken from the "developing countries", though our approach is intended to be general and readers from the "North" will not feel that our subject matter is "foreign" to them.

The field of HSR

Before getting to grips with our subject, we will say a word or two about the degree of generality aimed at in this paper. It also seems useful to define the field of HSR in terms of its position in the hierarchy of knowledge (or levels of research). Lastly, as we intend to argue that research into health systems needs to adopt a *systems approach* - which may seem a truism, but is actually anything but common practice - we will compare and contrast the analytical approach, favoured in conventional research, and the systems approach.

Degree of generality in our analysis

This paper is about the *health sciences*, and more specifically the study of *health systems*. By health system we mean the interplay of physical, chemical, biological, social, cultural, economic and political elements which influence the health of a population, and constitute reactions to illness. This is an expanded form of the definition given by Cook *et al.* (1992).

The reader should not assume from the level of generality of certain passages that we are talking about science or scientific method in general. Objections would be sure to flood in from individual disciplines if we were so bold as to attempt a universality beyond either our means or our purpose.

The hierarchical level of the object of research

From the most basic particles to the universe itself, little escapes the attention of biomedical research. To fit nature into a hierarchical construct is no easy task and poses a number of epistemological and hermeneutic problems. These have been discussed by Salthe (1985, pp. 163-220), but see also Hull (1974), Nagel (1974) and Rosenberg (1985).
Below are the nine levels usually considered by modern biology[1]:

1. biosphere
2. ecosystem (ensemble formed by the living and non-living elements of a particular environment)

[1] In the remainder of this section we draw upon Bernard Feltz (1996), *Notions de philosophie*, Université catholique de Louvain, Faculty of Science, Louvain-La-Neuve (Belgium), Editions DUC, duplicated, pp. 18-32.

3. community (group of populations living in a given ecosystem)
4. population (group of individuals of a given species)
5. organism
6. organ or tissue (which may form either a single level or two levels)
7. cell
8. organelle
9. biochemistry (subdivisible into macromolecule, monomer and mineral molecule levels).

In fact, this *hierarchisation of nature* not only defines fields of research in which various scientific disciplines have recourse to variable modes of explanation; it also constitutes the paradigm of modern biology, as do the four elements below, given here in order of their historical formulation:

- the predominance of the *mechanistic school* (the absence of any fundamental difference between living and inert matter allows physics and chemistry to serve as a basis for biology) over the vitalist school (living matter is governed by an organising principle which cannot be reduced to the laws of physics and chemistry)
- *cell theory* (since the cell is the smallest element capable of keeping itself alive, controlling its own activity and reproducing through its own means, all living things can be apprehended through the functioning of cells)
- the *evolution* of living things (man emerging from the animal world, itself emerging from the mineral world)
- the concept of the *ecosystem* (the existence of a *de facto* interdependence between the various parts of the living and non-living world, between populations and the inorganic environment).

In this biological paradigm, to recognise hierarchical levels in nature means accounting for a phenomenon at a given level by seeking the key to its explanation at lower levels and most particularly, in this approach, that of the cell: this gives "descending research" linked to a system of "ascending explanation". By its method, then, modern biology sets out to be reductionist, though this *methodological* reductionism of course in no way implies an automatic tendency towards *metaphysical* reductionism, such as would reduce man to a set of physical and chemical phenomena.

This digression via the hierarchisation of nature sheds light on the defi-

nition and specific nature of HSR in three ways:
Firstly, we would say right away that HSR cannot be pinned down to any specific tier of the natural hierarchy, which lends itself more to biology than to the health sciences of value to HSR. In principle, HSR can involve research at all these hierarchical levels, though if one had to be singled out it ought really to be that of the population (or the community).
Secondly, HSR has no use for the special place reserved for the cell in this hierarchy. If, once again, we were forced to choose a particular level of explanation, a sort of HSR-specific "reductionism", we would suggest the population, as a level integrating knowledge and research. But we freely admit we are unhappy with the reductionist nature even of an approach such as this, which is hard to reconcile with the multidimensional nature of HSR.

Thirdly, and most fundamentally, we believe that the rigid frame of natural hierarchisation inherent in the modern biological paradigm can only be a straitjacket for HSR. The push of both health sciences (research tools) and health systems (objects of research) is to "open up" this hierarchical tree to the *human sciences* and let HSR explore the psychological, social, institutional, cultural and anthropological dimensions denied to biomedical research. In terms of methodology, this will require HSR to encompass approaches other than the biomedical approach. This branching out of the hierarchical tree to embrace the human sciences is possible at the population level. Some authors quoted by Salthe (*ibid.*, p. 180) suggest just that, proposing that the content of the "population" level be more sharply defined by subdivision into group, organisation and society levels.

We do not wish to enter into a more complex discussion of scientific philosophy. What has just been said points clearly enough, in our minds, to one of HSR's original features: it is a form of research which, while culling the fruits of biomedical research, must necessarily address the *behavioural, social, institutional and cultural aspects* of the health system and its players. Research broken down along the hierarchical lines of biomedical research cannot be called HSR.

The systems dimension of research

ANALYTICAL AND SYSTEMS APPROACHES

We will consider this point in some detail, as it seems to us to be the source of much misunderstanding. In explaining it we will also suggest a checklist for the various research methodologies which lend themselves to use in HSR.

We will start with what, for want of a better term, is called "conventional research", or perhaps the classical notion of research. This is not to be confused with biomedical research, which is only one possible form of it. Epidemiological and health economy studies can also fall within the scope of conventional research.

We shall take our lead from Ludwig von Bertalanffy (1971, pp. xvii-xviii): "Classical science in its diverse disciplines, be it chemistry, biology, psycho-logy or the social sciences, tried to isolate the elements of the observed universe - chemical compounds and enzymes, cells, elementary sensations, freely competing individuals, what not - expecting that, by putting them together again, conceptually or experimentally, the whole or system - cell, mind, society - would result and be intelligible".

The principles of "classical science" enunciated by Galileo and Descartes, von Bertalanffy continues, have produced enormous progress in many fields. But two conditions have to be met for this *analytical approach* to be applicable:

1. Interaction between the parts which make up the whole must be either non-existent or so weak as to be discountable from a particular research optic: only then can these parts be effectively, logically and mathematically "deconstructed" and "put back together".
2. The relationship describing the behaviour of these parts must be linear, direct, simple, unidimensional: only then is the whole the sum of the parts, does an equation describing the behaviour of the whole have the same form as the equation describing that of the parts, can partial processes be added up to obtain the entire process, etc. (*ibid.*, pp. 16-17).

An analytical approach such as this assumes "the splitting up of reality into ever smaller units and the isolation of individual causal trains" (*ibid.*, p. 44), implying an essentially linear, binary and one-way causality.

Quoting the mathematician Warren Weaver, founder of the theory of information, von Bertalanffy maintains that such an approach is suited only to "non-organised complexity"; this is the domain of probability, statistics and the second law of thermodynamics (*ibid*., p. 33 and *passim*).
Following on from the above, we propose to identify conventional research using the following checklist, which we will use throughout this paper:

- Its contribution to knowledge and action

1. The *objective* of conventional research is to expand scientific *knowledge*, whatever the level of the world around us being studied.
2. Conventional research's *contribution to science* lies in the *universality* of its results, restricted only by the potential for inference, through deduction (based on reasoning) or induction (based on experimentation). The inductive method is generally linked to the experimental approach, even if, as Claude Bernard noted in 1865, "*induction* and *deduction* belong to all the sciences" - a debate we do not wish to enter into as it is not properly part of our subject. The results of conventional research also help to confirm the dominant scientific *paradigm* or, in certain exceptional circumstances, challenge it (Kuhn, 1970).
3. Conventional research's *utility for action* lies in producing information which may serve to identify possible *means of action*, though action itself is not the immediate objective.

- Its methodology

4. The *approach* adopted in conventional research is to single out a very precise subject of investigation and "control" (i.e. keeping constant or under control) all the factors which might come into play within a cause-effect relationship which is the most immediate and linear possible. Conventional research is obliged to sever the link between the studied phenomenon and its environment.
5. The chosen *hypotheses* have *single temporality*: either they deal with the present and concern the *synchronic structure* of the subject of investigation (e.g. the correlation between two biological factors), or they look at the future and concern its *diachronic evolution* (e.g. study of ageing, cellular differentiation, tumour development). In any event, they seek for the most part to establish facts or a series of facts, not to introduce new

elements into the subject of investigation and study their consequences. Inasmuch as knowledge acquired through conventional research is expected to be universal, its validity in the future is also postulated. The environment of the subject of investigation is thus assumed to be stable, or constant. There are, nonetheless, exceptions to this rule, as in the epidemiology of intervention.

> If Archimedes' principle that a body immersed in a fluid experiences an upthrust equal to the weight of fluid it displaces also implies that this will always be the case, it is because, at the level studied by Archimedes, the physical world around the fluid and the solid is stable and predictable enough to ensure that this cause-effect relationship will continue to prove itself today and tomorrow as it always has in the past.
>
> On the other hand, the overt purpose of introducing a tried and tested programme to screen for a disease is to trigger a process which aims gradually to reduce the mortality caused by that particular disease within the general population.

6. The *subject of investigation* is *external* to the conventional research process. We shall not deal here with the familiar epistemological problem of the influence of the research process itself on the subject under investigation. Other than in quantum physics studies of matter, in the vast majority of well-conducted "conventional" studies this problem is not, in fact, so great as to invalidate conclusions at the macroscopic level which concerns us here.
7. The *researcher* is *neutral* vis-à-vis the conventional research process. Here too, we will take this prerequisite of neutrality as given and will not embark on a discussion of the extent to which it is actually the case.

- The subject of the investigation

8. The *type of problem* studied can be both *technical* and *social* and fall within fields as diverse as physics, molecular biology, sociology and the health economy.
9. The *duration of the study* can be extremely *variable*, ranging from an opinion survey to a concerted follow-up over several years.
10. The *research area* is only of interest if it favours the *generalisation* of results, i.e. if it is *representative* of the wider ensembles for which conclusions will be drawn. We are using the term "representative" here in the usual sense of "something which represents well" the whole from which

it comes, "which is typical" of that whole. If the research is of a type involving a probabilistic approach, representativeness will take on the more technical meaning of representativeness of the sample, in the statistical sense of the word, i.e. that characteristic which allows statistical inference to maximise the accuracy of the estimative measurement obtained through the research.

To sum up, the approach adopted by conventional research is fundamentally *analytical*: it slices up that portion of the world it wishes to study into individual objects of investigation and considers their environment to be unchanged, either by exercising maximum control over it or by assuming that "all things are equal".

The limitations of this approach became apparent several decades ago, even in physics. The alternative approach sets out to be resolutely *system-oriented*: it seeks to encompass the subject of investigation and its environment, or rather to define a subject of investigation comprising several components and their interactions.

Literature abounds on the study of systems. Ladrière (1995) and Le Moigne (1995) provide a useful introduction in French. We shall confine ourselves to one of the pioneers of this approach, Ludwig von Bertalanffy, referred to above, as what he set out in his *General System Theory* in 1968 will serve to illustrate our points.[2]

Von Bertalanffy's text (*op. cit.*, pp.xvii-xviii) continues thus: "Now we have learned that for an understanding not only the elements but their interrelations as well are required: say, the interplay of enzymes in a cell, of many mental processes conscious and unconscious, the structure and dynamics of social systems and the like. This requires exploration of the many systems in our observed universe in their own right and specificities" (*ibid.*,p.xviii). "The system problem is essentially the problem of the limitations of analytical procedures in sciences. This used to be expressed by half-metaphysical statements, such as emergent evolution or 'the whole is more than a sum of its parts', but has a clear operational meaning. 'Analytical procedure' means that an entity investigated be resolved into, and hence

[2] French speakers will find an updated and selective bibliography in the "Addendum bibliographique" compiled by Bernard Paulré for the new French edition of: Ludwig von Bertalanffy, Théorie générale des systèmes. Preface by Ervin Laszlo. Paris, Dunod, 1993, pp. 283-294.

can be constituted or reconstituted from, the parts put together, these procedures being understood both in their material and conceptual sense. This is the basic principle of 'classical' science, which can be circumscribed in different ways: resolution into isolable causal trains, seeking for 'atomic' units in the various fields of science, etc." (*ibid.*,p.16).

The systems approach will thus turn to such concepts as information (one possible mode of which is the decision), feedback, control, stability, homeostasis, organisation, differentiation, adaptation, growth, hierarchisation, domination, regulation, teleology, etc. to consider the "whole" rather than chop up the world into ever more scattered subjects of investigation. As von Bertalanffy notes, all these "not so long ago, were considered to be metaphysical notions transcending the boundaries of science" (*ibid.* p.xviii).

A system may thus be defined as "organised complexity" (*ibid.*,p.17) or "a group of interrelating elements" (*ibid.*,p.37). A more complete definition is given by Hall and Fagen (1956): a system is a group of elements in dynamic interaction, organised around a purpose. As de Rosnay stresses (1975,p.101), this purpose of course reflects no plan, but is manifested *a posteriori*: the system is maintaining its structure and adapting to a changing environment.

The systems approach can no longer make do "with two-variable problems, one-way causal trains, one cause and one effect, or with few variables at the most" (von Bertalanffy, *op.cit.*,p.99). Causality becomes circular, calls on the concepts of feedback, both positive and negative, and multiple causality.

These days, no scientific *theory* can afford to neglect the systems dimension. As Paulré (1993) has written, the systems field is tending to merge, in some authors, with that of science itself. But to get back to the health sciences: how can we talk of DNA purely in terms of biochemistry, of a mitochondrion without reference to the cellular system, of an organ isolated from the organism, of a therapeutic test confined to the dose-effect relationship, of a therapeutic strategy without referring to the social environment, of a new diagnostic technique without considering its impact on health care supply and demand? Below, in defending the notion that HSR must necessarily employ a systems approach, we shall not forget that this approach, while vital to HSR, is far from being original or specific to it. Rather, HSR follows the general trend in contemporary science of taking account of the systems dimension in its subjects of investigation.

> To return to the screening example used above, it is quite clearly the overall system within which this action is conducted - and not just the impetus given by the screening process - which accounts for a significant part of the observed effect. The use of mammography to screen for breast cancer in women aged 50 and over helps illustrate this point, as the relative risk of mortality from breast cancer amongst the "ideal" women who follow this screening process is 0.74 compared with those not involved in the screening (Kerlikowske *et al.*, 1995). One might therefore see a binary relation between the screening process and the pathology. However, "real" women will not follow the proposed procedure so well, for various reasons associated precisely with the health *system*: cultural grounds, economic problems, operational difficulties, psychological obstacles, etc. The reduction in mortality actually observed in the target population will be lower than suggested by the randomised control tests. One might therefore talk of the complex system relations which exist between a given pathology, a screening process, the actual capacities of the health service, the socio-economic environment, cultural values, the behaviour of the population, etc. The interaction of these elements has also meant distinguishing the usefulness and the effectiveness of an early detection procedure, the second of these concepts relating to the results actually obtained in a "real situation" (*Does it work?*) as opposed to the experimental context (*Can it work?*) (Haddix *et al.*, 1996, pp. 3-11).
>
> Another example of the difference between usefulness and effectiveness is provided by the use of bednets treated with insecticide to reduce mortality due to malaria. The only randomised control test yet carried out to the point of measuring reduction in mortality *in real conditions* (what the authors call *effectiveness* and which corresponds to phase IV of the randomised test), and not just in *experimental conditions* where every effort is made to overcome the many operational obstacles to the use of bednets (what the authors call *efficacy* and which corresponds to phase III of the randomised test), took place in Gambia (Lengeler and Snow, 1996). While phase III showed an average reduction in mortality of 63% at a cost of $223 per death avoided, reduction in mortality in phase IV plunged to 25% while the cost shot up to $600. This major difference in results is due to the real conditions of the health system (acceptability of the methods, availability of bednets, etc.).

And yet, if we move from the theoretical claims of contemporary science to actual *research* practice, we find that much work is still pursued using the conventional approach, blithely ignoring the system. It cannot be assumed that conventional researchers, conditioned by the discipline of laboratory research and the experimental model, will necessarily take the systems perspective into account. As Feltz (1991, p.25) notes about biology, “A number of theoretical developments of the systems approach are markedly speculative, with all the pejorative connotations that holds for experimental biologists. Does such speculation have an actual impact on research? Can experimental approaches be devised according to systems intuitions? Can analytical approaches be modified to meet systems objections or are we in a situation of relentless opposition and logical incompatibility? Can one trace with accuracy any features which distinguish approaches flying the systems flag from traditional experimental approaches?”

These difficulties, added to the fact that conventional research has a precise and well-known methodology, explain both the continued success and the increasing sophistication of the analytical approach. It might be held, however, that the more sophisticated conventional research becomes, the further the resultant knowledge is removed from the actual reality of the system within which the subject under investigation is anchored. As conventional research becomes more “advanced”, it also becomes more “abstract”. The *richer* the knowledge seems to become in studying a binary relation between two elements of a system, the *poorer* the knowledge seems of the complex relations existing within the system as a whole. We would like to offer two examples of this. Firstly, one might ask whether as our knowledge of the relation between dyslipaemia and atherogenesis increases, research is not making an increasingly slight contribution to the actual management of the health problem of cardiovascular diseases in industrialised societies. Similarly, in randomised control tests on the effectiveness of a medicinal product, the greater the “control” (more specific choice of subjects who might respond to the treatment, elimination of subjects who do not comply, etc.), the further away one moves from the reality of the health system (where subjects do not respond to the same selection criteria, are less regular, etc.). This is not to say that such studies, based on the analytical approach, are meaningless. On the contrary, they can only be meaningful if they are focused on a very specific point - the direct (linear) relation between dyslipaemia and atherogenesis or the effectiveness of the administration of the medicinal product as such - studied in a manner as detached as possible from all outside interference.

However, the further down this path one goes, the further away one moves from the realities of everyday life. In conventional research, the "departure" from reality is deliberate and essential.

Nonetheless, we need to qualify the opposition we have emphasised between the analytical and systems approaches. An absolute systems approach claiming to embrace "the entire system", a sort of "holistic" approach, is an impossibility. Faced with the totality of things, the researcher is powerless. He must at the very least define one or more sub-systems suitable for inclusion in his research approach. Yet this attitude is in itself an analytical approach. Thus the systems approach is always analytical, to some extent. And it can of course incorporate or "appropriate", as possible research tools, the results of research conducted using an analytical approach.

SYSTEMS APPROACH AND MODELLING

The systems approach involves construction of a model. Before proceeding, we need to clear up a frequent misunderstanding rela-ting to the term "model". As understood here, a model is not an "example to be followed", or a formula (or to be more precise a set of structures, relations or procedures) which need only be strictly applied in all circumstances and the results then studied. To take the example which concerns us here, i.e. health systems, their practical situation and organisation clearly vary the world over. A health service model must seek to be independent of these situations. Yet it will not just pop out of some "magic book of systems" or "treasure trove of methodology". It can only result from the comparison of practical examples which have been modelled. The greater the number of practical examples which have tested the model's validity, the richer the latter will become, i.e. capable of taking into account various aspects of real health systems. It may be that in some situations a model will prove not to be detailed enough, or perhaps contain unused "dormant" elements. It is this capacity to reflect the diversity of actual situations and to serve as a tool for analysis and action in different contexts that makes the model so valuable.

So *a model is not prescription but representation*. The different sciences, from the arts to mathematics, linguistics to biology, have made abundant use of the concept of the model. We will content ourselves here with a basic definition: a model is a simplified and hypothetical representation of a process or system. It is "an abstract transcription, controlled by logical and mathematical thought, of an actual empirical situation, the direct study of which would only show approximate relations" (Mouloud, 1995, p. 529). As for the actual modelling process, we would suggest that, among the abundant literature, the reader refer to Phillips *et al.* (1976, pp. 4-11) or to Paulré's bibliography on this subject (*op. cit.*). We will return to this important question of modelling when we consider operational research and action research.

SYSTEMS APPROACH AND SIMILAR IDEAS

Joël de Rosnay (*op. cit.*, p. 119) proposes the following distinctions between the analytical and systems approaches (table 1).

Table 1 : Comparative caracteristics between the analytical and systems approaches

Analytical approach	**Systems approach**
Isolates: focuses on elements	Links: focuses on interactions
Considers the nature of interactions	Considers the effects of interactions
Relies on accuracy of details	Relies on overall perception
Modifies one variable at a time	Modifies groups of variables simultaneously
Independent of duration: the phenomena under consideration are reversible	Integrates duration and reversibility
Validation of facts through experimental proof guided by a theory	Validation of facts by comparing the operation of the model with real life
Precise and detailed models, but hard to use for action (example: econometric models)	Models not rigorous enough to serve as a basis for knowledge, but can be used in decision-making and action (example: Club of Rome models)
Effective where interactions are linear and weak	Effective where interactions are non-linear and strong
Leads to teaching by discipline (juxta-disciplinary)	Leads to multidisciplinary teaching
Leads to fully programmed action	Leads to action by objectives
Knowledge of details, poorly defined goals	Knowledge of goals, hazy details

Other authors prefer to contrast the paradigms of *positivism* (or empiricism) and *constructivism* (Bouchard and Gélinas, 1990). The former refers to logical positivism, which underlies the whole of contemporary epistemology and in which: (i) the fact is independent of the person perceiving it, (ii) investigation techniques must be geared towards systematic observation, objectivity, quantification, internal consistency, repetitiveness, (iii) the world around us can be decomposed, in a process moving from the complex to the simple, into dependent and independent variables, whose relations are to be studied in a context of causality, best understood using the experimental method, and (iv) the researcher must, as far as possible, remain outside the field of research. In constructivism, on the other hand: (i) the interdependence is postulated of subject and object, with human beings constructing their own social reality in a process in which "real facts" are the ways in which individuals define situations, (ii) investigation techniques are geared towards qualitative aspects and participatory observation, (iii) the world around us can be decomposed only into interdependent variables whose relations are to be studied in a complex context, a key element of the analysis, and (iv) the researcher involves himself in the phenomenon being studied and allows his subjectivity to intermesh with what he is analysing.

The social sciences also contrast the *comprehensive* paradigm (or approach) with the *positivist* paradigm (or approach). Unlike the positivist approach, which seeks to *explain* facts and formulate *laws*, the comprehensive approach seeks to "*understand* the dynamic of events through intentionality engaged in the history of the subject's interactions with its environment" (Pourtois and Desmet, 1996, our italics).

All these definitions can sometimes need to be sharpened by reference to the philosophy of science, as, for instance, with the equivalence of approach and paradigm or empiricism and positivism, and the precise definition of sometimes redundant or obscure social science jargon. But that is not our purpose here. The point of mentioning these ideas is to highlight the same attempt to escape the narrow frame of the conventional analytical approach so as better to apprehend "organised complex reality".

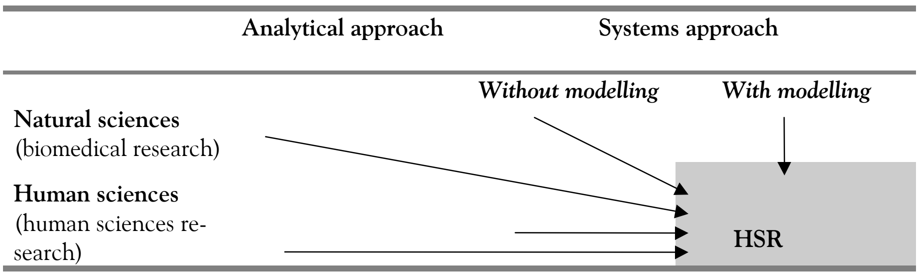
The characteristics of our approach

The rest of this paper deals with the search for a *methodological approach* which will both clarify *health systems* research and make it more productive.

We began by explaining, with reference to HSR, the utility, but chiefly the limitations, of the hierarchisation of nature practised by biomedical research. If the population is to be retained as the special level at which knowledge is integrated, it will only be by extending the hierarchical tree of science to embrace the human sciences, with their psychological, social, cultural, institutional and anthropological components, and taking on board the corresponding methodologies. The vast field of HSR may thus include elements of biomedical research, but must incorporate dimensions of the health system which are the concern of the human sciences.

We then explained that this research could only adopt a *systems approach*, given the limitations of the analytical approach. At the same time, we underlined that a systems approach implied recourse to a *modelling* process.

The specific nature of HSR, as proposed here, is illustrated in the diagram below: HSR is specific in that, in a *systems* approach employing explicit *modelling*, it necessarily integrates dimensions of the health system which are the concern of the *human sciences*. This is not to say that HSR cannot employ scientific approaches proper to the analytical approach or the natural sciences, *provided* it integrates them into an overall approach which meets the description we have just given.



It now remains to review the various *methodologies* which HSR might draw upon, ranging from scientific management to action research, and to attempt to come up with a methodological approach such as will define HSR more clearly and make it more useful.

Our approach differs, therefore, from other attempts which may seem similar at first sight, but appear to us to be both more ambitious in their aims and more limited in their analysis of methodological aspects. One such case is the approach adopted by Battista *et al.* (1989), which aims to facilitate communication among researchers by using a common reference framework to classify all health-related studies from three angles: the focus of interest (states of health or health interventions), the level studied (with regard to states of health: molecule or cell, tissue or organ, individual, community or population; with regard to treatment: technical, practical, programme or policy) and the objective pursued (development, description, explanation or evaluation). Another example is the approach taken by Marconi and Rudzinski (1995) which seeks to improve the typology of studies, with a view to obtaining funds, by classifying them according to their level of analysis (individuals, organisations, systems), their goal (access, cost and quality of the care) and methodological intent (description, data development, testing hypotheses, programme evaluation, dissemination and application of research).

We would also stress that the methodology ideas we will be proposing at the end of the second part of this paper are not the result of armchair cogitations based on compilation of scientific literature, but served as the theoretical basis - at first implicit, even “non-articulated”, then explicit and gradually refined - for organising the health system in the Kasongo health area in Zaire, over a period of many years.

HSR characteristics and methodologies

HSR is scientific research

The first criterion for a better understanding of HSR is self-evident: it is a form of *scientific research*. Like all scientific research, it leads to two results obtained to varying degrees depending on the individual case: an increase in knowledge (about health systems) and identification of means of action (on health systems).

This first criterion - is it scientific research? - often needs to be considered for practical reasons, e.g. a funding decision by a body which supports research. It seems easy enough. However, it is not always easy to distinguish between what does and does not fall within the scope of scientific research. Sometimes the decision can be fairly arbitrary. We will illustrate the difficulties involved in judging just how scientific an approach is in two fields familiar to decision-makers in the field of health: management and evaluation.

"SCIENTIFIC MANAGEMENT"

When people advise adopting a "scientific attitude" in the day-to-day management of a health project, programme or area, they mean departing from administrative routine in order to guide and correct actions on the basis of *objective analysis of observed facts*. In saying this, we are aware that science without theoretical elaboration is not science, and that we might be accused of misusing language, since the "scientific" approach to management does not actually refer to any theory such as would explain anything. But we prefer to avoid unfair accusations. We all speak spontaneously about "scientific research" because we assume it is perfectly natural to link the process (research) to the objective (science), and vice versa. But the link is not automatic. We hold that there may be a "research attitude" without creation of science, and a "scientific attitude" without the research process. It is in this sense that we speak of scientific attitude in management. If the stricter-minded prefer to speak of a "rigorous" approach to management rather than a "scientific" attitude in management we will willingly stand corrected, this being of minor importance.

The crux of the matter is elsewhere. While a "scientific approach" to health service management may involve testing hypotheses when decisions are taken, the *lack of recourse to an explicit model of the system* prevents any generalised use of the results obtained and distinguishes good management from scientific research. There is “research attitude” without creation of science. Thus it is not its intellectual approach which prevents rigorous management being scientific research, so much as the nature of its results: these remain tied to the situation, contribute towards identifying an optimum solution for that situation and cannot be transferred to a different situation.

“SCIENTIFIC EVALUATION”

The purpose of evaluation studies is to determine the extent to which results actually correspond to objectives and to analyse the process underlying the decision-making. The distinguishing factor between an *administrative* evaluation and a *scientific* evaluation is the intellectual process which informs the setting of objectives.

If objectives have been set in an arbitrary, non-methodical (“empirical”) fashion, eschewing all models (i.e. without formulating a hypothesis on the transformation of inputs into outputs in the process to be evaluated), the evaluation has to be restricted to an administrative approach to its subject. For all that it might observe the facts with the greatest rigour, or make use *a posteriori* of explanatory models with a descriptive value, or even succeed in throwing up questions which are relevant in another context, it will nevertheless still be distinguished from scientific *research* by the *absence of test hypotheses*. When the evaluation has to adopt an administrative approach, it produces literature which is useful in this instance and at this point in time, but it does not produce any body of new knowledge which can be used in other places by other parties. This is the case, for example, when the evaluation seeks to check whether a new way of treating malnourished children in maternal and infant health services is reducing the number of cases requiring hospitalisation. The information is of value to the managers directly concerned, but of little use outside that context.

If, on the other hand, objectives have been set on the basis of an *explicit* hypothesis on the transformation of inputs into outputs, i.e. on the basis of a *model* which allowed a scientific approach to be adopted at the planning stage, the evaluation is then able to test this hypothesis and, subject to appropriate rigour in observing the facts, constitutes research by the very way it operates. It is, in short, able to *check whether the basic assumption has in fact been the right approach for obtaining the desired result*. This is the case, to return to our example of malnourished children, if the evaluation compares the expected effectiveness (depending on working hypotheses organised in a predictive model) and the observed effectiveness of the new method of treatment supposed to reduce by half, let us say, the number of cases requiring hospitalisation, according to the model to be tested. In such circumstances, a hypothesis has been put forward beforehand regarding the anticipated effect of the measures taken, and the evaluation (the verification of the hypothesis) forms an integral part of the planning process. The *generalisation* of the results will depend on the type of research concerned: conventional research, operational research or action research.

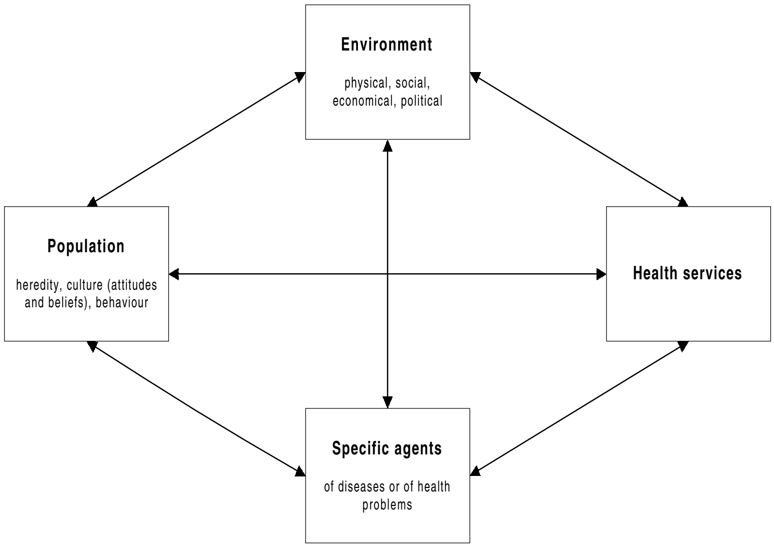
THE GREY AREA BETWEEN RESEARCH AND MANAGEMENT

Thus the management and evaluation of health systems can sometimes include features (testing of a hypothesis, use of a model) which point to their having an element of "scientific research" about them. But there is no research without testing a hypothesis, and no science without reference to a theory. These two requirements may sometimes be met only imperfectly by "scientific" management or evaluation. We have to concede, then, that we find ourselves here in a sort of "grey area" between research and management. And it is better, when dealing with a given dossier, experiment or project, to distinguish its *scientific* character from its *fundable* character as a research process. These are two separate problems. It is easy to imagine that those in charge of a research funding programme would decide not to consider projects dealing with management and evaluation as HSR, despite their "scientific" character.

HSR looks at health systems

Over thirty years ago, in a study on tuberculosis, M. Piot proposed that the health system be defined as the interaction between the characteristics of a population, the environment, specific agents of diseases and the health services (Piot, 1963). This scheme, devised as part of the tuberculosis study, can be extrapolated to study of the health system as a whole. The system can be presented in the form of a representational model (Figure 1).

Figure 1 : Representational model of the health system



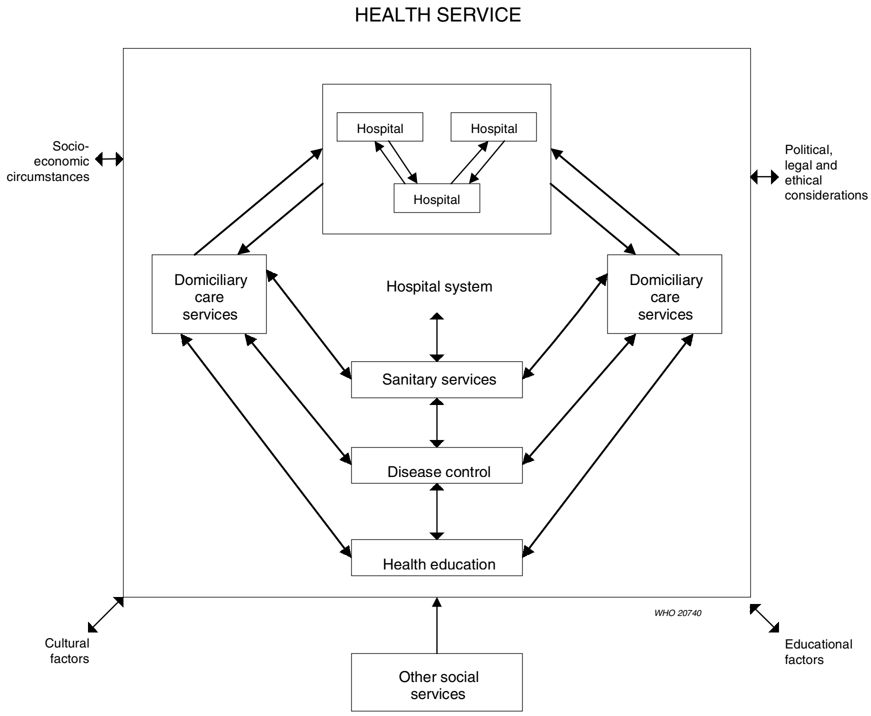
Source : Piot M, 1963

We should point out immediately that this type of system, even when presented in the guise of a universal model as in Figure 1, is not a general-purpose methodological tool for use in each and every HSR study. It is worth looking here at Grundy and Reinke: "*What is to be treated as a system will depend on practical considerations*. For some purposes, activities carried out or devices used in health care are regarded as systems; for others, they are studied as sub-systems of a larger complex ... In ascending order of complexity, the systems studied in health practice research comprise:

(i) the individual components of a health service or some particular aspect of these components;
(ii) the health system service in its entirety;
(iii) the health service and its components in relationship to socio-economic and other factors." (Grundy and Reinke, 1973, pp. 21-22, our italics).

A representational model of the systems mentioned by these authors is given in Figure 2.

Figure 2 : Health systems and sub-systems

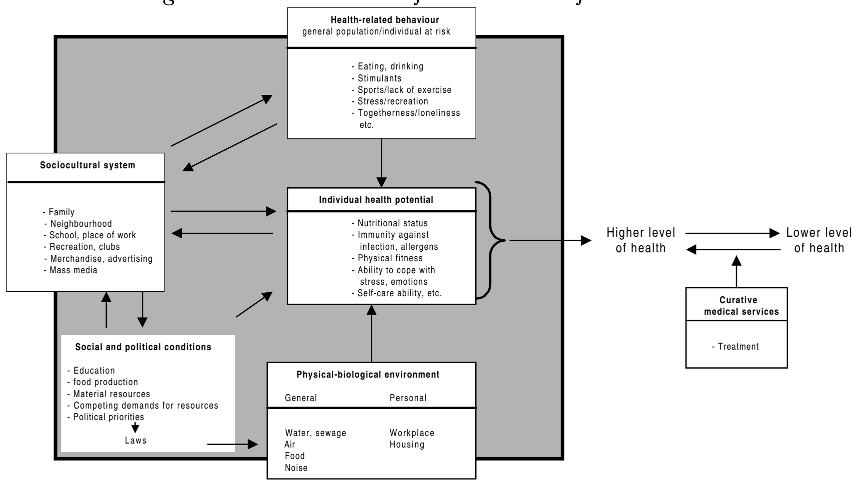


Source : Grundy F and Reinke WA, 1973

The model may seem dated, but while it is true that current models take greater account of health promotion and, for the industrialised countries at least, lay greater emphasis on the sub-system constituted by the environment, as shown in Figures 3 and 4 (Abelin *et al.*, 1987, pp. 32-34), this does not affect our proposition in any fundamental way.

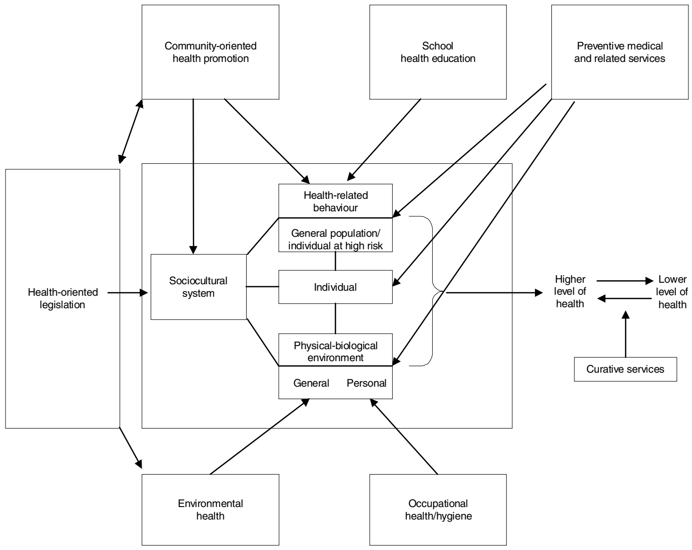
If one accepts the comments made in the first part of this paper, one could also accept that any study on one or more elements of a system counts as HSR, provided it is not confined to results dealing solely with a linear relation (or a sum of linear relations) between two or more elements of the health system, but leads to conclusions which provide a better understanding of the operation or structure of the health system itself (Taylor, 1984, pp. 56-57).

Figure 3 : Factors that influence levels of health



Source : Abelin et al., 1987

Figure 4 : Approaches to health promotion based on factors influencing levels of health



Source : Abelin et al., 1987

This gives our second criterion for assessing the specific nature of HSR: the study *takes account of the system* or a sub-system. That said, it is essential that the system or one of its sub-systems is *really* the subject of the study. All too often, the preamble of a study protocol will refer - obligatory lip-service - to the "health system", only for the study itself to remain focused on an isolated component of the system or consist of a set of conventional research studies with no consideration of systems.

Thus no research which focuses on an isolated component of the system can be called HSR. This would apply to the research we have called conventional. Regardless of its field, conventional research displays features which make its methodology ill-suited to HSR. In particular:

- Conventional research tends to focus on specific aspects of the health system, which are, of necessity, partial (the health system is too complex for experimental study conditions to be created which relate to it as a whole, or even to one of its sub-systems). Yet to await the results of a series of partial research activities before deciding how to change the system would be to risk becoming completely bogged down and allowing the system to develop of its own accord, following the intuition of its players or the force of circumstance, which, it might be said in passing, can also constitute a form of experimentation not devoid of interest. The feared inertia in this scenario stems from the diminishing returns of conventional research or, to be more precise, from the diminishing returns of applying to the health system limited and specific efforts based on the results of conventional research.
- Conventional research looks at one or other component of the system at a given period in time. Given that the system is evolving, the relevance of such research diminishes rapidly. If the system develops, due to external factors or through its own internal dynamic, the results of conventional research can be obsolete before they are even available.

The fact that conventional research is not systems-oriented almost always denies it access to the field of HSR. To qualify as HSR, conventional research would have to be able to study the interrelation between various components of the health system. For instance, it has been suggested (Cook *et al.*, 1992) that study of the introduction of a new technology could be HSR if the social and economic context governing the use of that technology were taken into account; similarly, a prevalence study of a particular health problem may be HSR if the operation of the health system, e.g. the course of therapy followed by the patients, forms part of the study. Yet this is precisely where conventional research begins to overstep itself, because to study the interrelation of several components of a system presupposes that some representation is made of that system, however rudimentary. This representation is a model of the system, of which both operational and action research - approaches better adapted to the dynamic aspects of evolving systems - make explicit use, as we shall see.

Operational research enhances HSR through explicit use of a model

We would now like to consider the contribution made by operational research (OR) to HSR, i.e. examine the extent to which its methodology can be used for HSR.

Several definitions have been given of OR. We will take them from the most general to the most detailed.

- "O.R. [Operations Research] tries to find the best decisions relative to as large a portion of a total organization as is possible ... O.R. is here defined in terms of its important goal: an over-all understanding of optimal solutions to executive-type problems in organisations" (Churchman *et al.,* 1957, pp. 6-7).
- "Operational Research is the science of planning and executing an operation to make the most economical use of the resources available" (Cohen, 1985).
- "[OR is] the systematic study, by observation and experiment, of the working of a system, e.g. health services, with a view to improvement" (Last, 1995).
- "By operations research is meant any formalized quantitative analysis whose purpose is to improve efficiency in a situation where 'efficiency' is clearly defined. Typically, operations research is used to optimize an objective function ... that is defined in quantitative terms" (Grundy and Reinke, *op. cit.*, p. 41).
- "Rather than an arsenal of mathematical methods adapted to optimising processes for the manufacture and distribution of products, operational

research is a set of rational methods and techniques for analysing and summarising organisational phenomena with a view to better decision-making" (Faure, 1975).

- The Operational Research Society of Great Britain gives the following definition:
  "Operational Research is the application of the methods of science to complex problems arising in the direction and management of large systems of men, machines, materials and money in industry, business, government and defence. The distinctive approach is to develop a scientific model of the system, incorporating measurements of factors such as chance and risk, with which to predict and compare the outcomes of alternative decisions, strategies or controls. The purpose is to help management determine its policy and actions scientifically" (quoted by Phillips *et al.*, 1976, pp. 3-4). The authors go on to say (*ibid.*, p. 4) that "the essence of the operations research activity lies in the construction and use of models".
- Hillier and Lieberman (1990) give a terse definition of OR ("a scientific approach to decision making that involves the operations of organizational systems"), only to point out that such a definition is far too broad and could apply to many other fields (p. 4). They explain: "The approach of operations research is that of the scientific method. In particular, the process begins by carefully observing and formulating the problem and then constructing a scientific (typically mathematical) model that attempts to abstract the essence of the real problem. It is then hypothetized that this model is a sufficiently precise representation of the essential features of the situation, so that the conclusions (solutions) obtained from the model are also valid for the real problem. This hypothesis is then modified and verified by suitable experimentation ... However, there is more to it than this. Specifically, operations research is also concerned with the practical management of the organization. Therefore, to be successful it must also provide positive, understandable conclusions to the decision maker(s) when they are needed ... In summary, operations research is concerned with optimal decision making in, and modeling of, deterministic and probabilistic systems that originate from real life" (*ibid.*, p. 5).

A number of concepts emerge from these definitions:

a) the scientific nature of the discipline,
b) quantification (mathematisation) of the subject of research,
c) a practical objective in terms of decision-making,
d) optimisation, efficiency or improvement of the system,
e) the multidisciplinary nature of the methods used.

The methods are in fact very varied: network analyses (PERT - Programme Evaluation and Review Technique; critical path analysis), linear programming, computer simulation, theory of graphs, expectation phenomena, games theory, methods of projection and extrapolation, statistical analysis, cost-effectiveness analyses, etc.

In short, OR is called for when common sense alone is no longer enough to guarantee the proper management of a system. This is typically the case in the following three situations (Faure, 1975; Cullmann, 1978):

1. combinational situations where the number of possible solutions becomes so great as to be hard to count;
2. chance (or stochastic) situations, where decisions have to be taken in a state of uncertainty, i.e. situations where future developments are only a probability;
3. competitive situations, where decisions have to be taken despite variances in the behaviour of other partners in the system (combinational element), which are difficult to predict (chance element).

As Faure, Boss and Le Garff wrote in 1980 (p. 10), "operational research is not a substitute for common sense, but helps make sound judgements by removing difficulties linked to the combinational, chance or competitive structure of real problems. It is in this sense that Pierre Massé was able to state that it was the science of preparing decisions."

The problems most usually examined by OR are stock control, staff recruitment, queuing phenomena, equipment renewal, etc. In the field of public health, the various epidemiological and epidemiometric models for diseases also fall within its scope.

For our purposes, it is important to stress, like Grundy and Reinke (*op. cit.*, pp. 48-49), that "The use of a model of some sort is the basis for each application of an operations research technique". Indeed, some of the above authors incorporate this modelling aspect into their definition of operational research, and highlight the mathematical nature of the model.

OR takes place in a number of stages (Churchman *et al.*, *op. cit.*, pp.12-15):

- formulating the problem
- constructing a mathematical model representing the system being studied
- deriving a solution from the model
- testing the model and the solution
- control system (studying how the model reacts to changes in certain parameters)
- implementing the solution.

Andersen (1963) proposes a similar plan:

- formulating the problem
- collecting data
- analysis and hypothesis formulation, i.e. constructing a model
- deriving solutions from the model
- choosing the optimal solution and forecasting results
- the test run and the control system
- recommending implementation, which may follow on fairly quickly in industry but can run into many obstacles in a public service.

In HSR, the model is all the components, and the relations between them, which must be taken into account if one wishes to analyse a health system and make it work. But the model only has meaning if it is validated by the research. One might say that the model is, therefore, the sum of the hypotheses (to be verified) regarding the main components of a health system and their interrelation. HSR involves comparing what is observed with what was anticipated. What is anticipated is based on the model.

The use of a model is justified firstly in that it provides an opportunity to take all the components of a system into account, but it is also justified for instrumental reasons: this is the only way of making calculations or predictions which can then be validated by observing the actual situation.

"Thus, if a certain amount of guile is employed in producing a model, it is possible to simplify and outline an area of facts. At the same time, however, this transcription helps aggregate the matter being dealt with and avoid excessively unilateral reductions. Models are invented to systematise the points of view in an explanation. A certain constructive rigour can therefore be demanded of them, though this rigour does not translate into rigidity, as the models have to be adaptable. Within the same science, they can proliferate in accordance with the regional properties of the situation to be described, and they remain mobile in order to satisfy the conditions of invention and discovery. Nevertheless, models in the same field of science shun division and gravitate whenever possible towards unifying solutions. Generally speaking, science is fully aware of the instrumental value of models, and does not confuse the implicit truth with the restrictive contents of the representation, thus avoiding the dogmatism which would ensue from confusing the object with its models. The model is a 'controlled fiction'; controlled by the successes or failures of experience, it is subjected to the criterion of consistency guaranteed by logic or explanatory theory. The basis of modelling is a prospective and critical approach to knowledge" (Mouloud, 1995, pp. 540-541).

Different categories of model can be used in HSR, depending on their form and function. This is not the place to expand upon this point, as it has been well summarised by Grundy and Reinke (1973, pp. 32-40), but "As a minimum, any model must: (1) convey information (not always quantitative); (2) illuminate appropriate variables and their relationships to each other, including factors of uncertainty or chance; (3) provide a structure for analysis or stimulation; and (4) be an abstraction of such a character as to allow manipulation without misrepresenting the real situation" (Grundy and Reinke, *op. cit.*, p. 47).

Back in 1963, Andersen (*op. cit.*) stressed the improbability of operational research in the field of public health rapidly reaching the degree of mathematisation it had attained in the military or industrial fields. The author advocated adapting OR to health services research by proposing the following definition: "The name operations research implies that it involves research into some or all aspects of conducting or operating a system, a business, a service, and treating the system as a living organism in its proper environment: thus it distinguishes itself from laboratory research". He also emphasised the need to simplify the representation of the system using a *model* which would allow changes to be introduced, and to validate this model through one or more test runs before any generalisation.

We can now compare operational research with conventional research using the same checklist as before.

- Its contribution to knowledge and action

1. The *objective* of operational research is *optimum decision-making* in a given situation. In contrast to conventional research, the primary purpose is not, therefore, to help expand scientific *knowledge*.
2. Operational research's *contribution to science* consists of knowledge based on *situational results* closely linked to the original situation, in contrast to the results of conventional research, which aim for *universality*.
3. Operational research's *utility for action* is the construction of an essentially mathematical *model*. This means the model can be used in circumstances other than the original ones, subject to validation of the different hypotheses behind the modelling. It has been said that production of a model is not just one of the results of OR; it is a necessary step. *The model is firstly an instrument; this instrument, once validated, becomes a transferable result*. This seems worth underlining, as it is not certain that all researchers recognise this as the chief value of OR.

- Its methodology

4. Operational research has a *systems approach*. In contrast to conventional research, it selects a very specific subject for study but avoids isolating it from its entire environment. OR does not seek to keep this environment constant, or neutralise it or, in the epidemiological sense of the term, "control" it. It aims rather to study the subject in question within the system (or sub-system) to which it belongs, the simplified representation of which is the model.
5. The selected *hypotheses* have *dual* temporality. Some of them are *working hypotheses* or *assumptions* relating to the *synchronic structure* of the object of study. For the sake of brevity, we will call them "static hypotheses". They are proposals relating to the explanation of phenomena currently observable in the system, accepted provisionally pending practical controls. *It is the framework made up of these working hypotheses which forms the model of the system*. Another series of hypotheses, however, relates to the *diachronic development* of the system, i.e. the way it will change in the *future*, depending on whether one or other component is maintained or changed. In this sense we can call these hypotheses *conjectures* or "dynamic hypotheses": they are proposals relating to the foreseeable development of the system over time, and can only be verified by means of a *test run*, i.e. they are validated or refuted by experience, thus perfecting

the model.

6. The *subject of investigation* is *external* to the research process, though the operational research itself may interfere with the phenomena observed, and this must be taken into account, particularly when validating the model (Andersen, 1963).
7. The *researcher* sets out to be *neutral* vis-à-vis the operational research process. As with conventional research, we will take this prerequisite of neutrality as given.

- The subject of the investigation

8. The *type of problem* studied has a predominantly *technical dimension* (stocks, flows, expectation phenomena, algorithms, etc.), linked to the quantitative approach adopted.
9. The *duration of the study* can be *variable*.
10. The *research area*, in the geographical sense or in the statistical sense of a sample, is only of interest if it is *representative* of similar research areas in which reference could be made to the same *model*.

It is important that we clarify the distinction we intend to make here between working (static) hypotheses and conjectures (dynamic hypotheses). It can happen that studies labelled as OR never develop beyond the stage of working hypotheses and are not actually tested in daily practice by trying out appropriate conjectures. Beech (1995) gives us an example of this in his study on what could be achieved by isolating genes through DNA testing in the development of pregnancies where there is a risk of the child being born with cystic fibrosis. The study is clearly within the field of operational research, making use of representational and mathematical modelling, but only really provides information for the decision-maker. There is nothing to say that its actual application in a target population would verify the conclusions of this preliminary study, a point acknowledged by the author: "OR [Operational Research] analysts recognize that their science is about aiding decision-making. Hence, rather than attempting prescriptive solutions, OR analysis increases the volume of information available to decision-makers, provides systems to support the decision-making process, and increases the level of understanding about a problem area. 'Management' must then take whatever action they think is necessary" (*ibid*., p. S91).

In principle, OR leads to the validation of hypotheses *in the world* *around us*. However, some researchers allow their OR activity to end before the proposed model has been put to the test: "In all cases, the final decision rests with those in control of the operations, not with the operations researchers. The team can only recommend solutions or the basis on which solutions can be selected. It can, however, assist in implementing the solution once the decision is made" (Churchman e*t al*., 1957, p. 8). When they speak of "testing the model", these authors simply envisage comparing the anticipated results with what they actually find, or with retrospective data, and - only if really necessary - with the result of a *trial run* or pretest (*ibid*., p. 14).

The fact that OR can stop short like this is perhaps where it parts company from HSR, or rather is what labels it as a possible tool for HSR. To become HSR, OR ought really to be carried out right through to the actual validation stage, to a *test run*. This presupposes that one moves on from a static hypothesis about the current state of the system to the statement and verification of dynamic hypotheses involving observation of the system's development following introduction of the proposed change.

To return for a moment to conventional research, it is when it tries to exceed its limits and take account of the interrelation between the objects of the research that it runs into the system itself. The best it can then do is attain the level of static hypotheses, i.e. take account of this systems dimension, without any possibility of conducting a test on the system itself, for lack of an explicit model.

To explain more clearly what we mean by static and dynamic hypotheses, we will consider the specific example of coverage of prenatal activities in a developing country:

> Let us assume that in a given health area we observe an average coverage rate of between 40% and 50% of the target population for prenatal care (PC) activities organised by health centres. If we wish to increase this coverage rate, we need to examine the factors explaining why pregnant women do not make greater use of PC, so that we can then try to exert an influence on these obstacles.
>
> a. A first possible level of research is *conventional research*. We would first identify the elements relating to prenatal coverage. One hypothesis to be tested, for example, would be the geographical accessibility of the care. We might formulate the hypothesis that the

less accessible a centre is, the less it is frequented, assuming a distance of 5 km from a health centre as the indicator of accessibility (one could of course adopt a shorter or longer distance, or several critical distance thresholds). This hypothesis might have a simple mathematical formula: the average coverage of the area is the weighted average of the coverage of sub-area A (covering locations situated less than 5 km from a health centre) and the coverage of sub-area B (covering locations situated more than 5 km from a health centre):

Coverage of A (<5 km) = (coverage rate of A) . (proportion of target population living in A)
Coverage of B (>5 km) = (coverage rate of B) . (proportion of target population living in B)
Average coverage for the area = Coverage of A + Coverage of B
(1)

N.B. These equations can also be expressed in terms of probabilities: the coverage rate then becomes the *probability of being covered in one of the given sub-areas* and the proportion of the population becomes the *probability of belonging to this given sub-area.*
We will call this hypothesis on geographical accessibility a *static hypothesis*: to check it out in practice there is no need either to introduce change into the health system or to observe its spontaneous development; the data of the situation are simply studied.
Assuming now that the data collection produces the following results:

population of sub-area A (<5 km): 30% of the total population
population of sub-area B (>5 km): 70% of the total population
coverage of sub-area A: 80% of the target population
coverage of sub-area B: 30% of the target population
this gives:

Coverage of sub-area A (< 5km)	= (0.3).(0.8)	= 0.24 = 24%
Coverage of sub-area B (> 5km)	= (0.7).(0.3)	= 0.21 = 21%
Average coverage for the area (A+B)	= 0.24+0.21	= 0.45 = 45%

The hypothesis can be considered to have been verified: there is a relation between distance, regarded as the indicator of accessibility, and the number of people using PC.

But is this relation causal? In other words - and this is what the decision-makers need to know - if we made PC more accessible by increasing the proportion of the population living in an area with the characteristics of sub-area A (< 5km), would we see an increase in prenatal coverage for the population concerned and, consequently, in the average coverage for the area? Conventional research provides no answer to this question, which is why we are interested in other types of research.

N.B. The same logic could apply to any other hypothetical factor influencing the use of PC, e.g. cultural, linguistic, socio-economic differences, etc.

b. A second possible level of research is *operational research*. Here, equation **(1)** becomes more than a mathematical formula: it becomes the mathematical model to be tested by our research.

$$p_C = (p_{CA} \cdot p_{LA}) + (p_{CB} \cdot p_{LB}) \quad \textbf{(2)}$$

where

p_C = probability of being covered by PC

p_{CA} = probability of being covered by PC in the sub-area located within 5 km

p_{LA} = probability of living in sub-area A

p_{CB} = probability of being covered by PC in the sub-area located beyond 5 km

p_{LB} = probability of living in sub-area B

This model will allow us to test the following hypothesis: if insufficient geographical accessibility, measured by the indicator of distance to be covered, is the cause of low prenatal coverage, improved accessibility will result in an increase in coverage. We will call this a *dynamic hypothesis*, signifying that we will have to introduce a specific change into the health system to effectively improve the geographical accessibility and compare the effect achieved with the anticipated effect on prenatal coverage.

The mathematical model in equation **(2)** allows us to carry out the following operational projection: by raising the proportion of the population living less than 5 km from a health centre from 30% to, say, 60%, one might expect the following development in prenatal coverage:

$p_C = [(0.6).(0.8)] + [(0.4).(0.3)] = 0.48 + 0.12 = 0.60 = 60\%$

We must stress that the 60% objective has no sense if it is arbitrary, i.e. if it is set without reference to what can actually be done to attain that rate of coverage or if those who lay it down have no real idea how to make PC more accessible in practice.

At this stage of operational research, we have determined the mathematical model and its various parameters. We now need to carry out a *test run*, i.e. test the hypothesis, thereby testing the value of the model. The test will consist of checking the dynamic hypothesis by realising the condition which will ensure easy accessibility for 60% of the population (e.g. by opening new health centres or using "advanced strategies" to bring the services closer to the population). If, having realised this condition, a prenatal coverage rate of 60% is achieved, the dynamic hypothesis can be considered to have been verified.
Thus operational research enables us to answer the question left open by conventional research (is poor geographical accessibility the cause of mediocre coverage?) and to apply the means of tackling our operational problem: improving prenatal coverage on the basis of the test model (aim of the research).

The *results* of such operational research cannot be applied generally: they are only meaningful in these specific circumstances, including the 5 km limit adopted in our argument. The *model*, on the other hand, as expressed in equation **(2),** can be transferred to other situations.

Critical readers will ask what has happened to the system in this operational research, what has happened to interdisciplinarity, and in what way the proposed analysis has taken anything other than a "conventional" scientific approach. The answer is that the system is here confined to the interactions between health cover and accessibility of services; but this *is* a subsystem of the health system. The approach is "conventional" inasmuch as it turns first to an algebraic analysis of the interaction between these two elements of the subsystem, but becomes systems-oriented when it comes to test the model in the real world. Lastly, interdisciplinarity is indeed very slight here, but it must or could have played a part in the analysis which produced the hypothesis of a relationship between coverage and accessibility. That said, we must point out that, for didactic purposes, the above example was chosen for its great simplicity compared with the high degree of sophistication possible with OR methods.

c. Let us now assume that the operational research we have just described failed to verify the model, i.e. the "objective" change made to the system (improved geographical accessibility - or any other similar objective measure) did not lead to an improvement in prenatal coverage. We then need to take into account other factors, which could be described as "subjective", "cultural", or "behavioural" and can range from the measure of trust between the population and the health staff to popular beliefs regarding the supposed effects of PC, or changes in attitude of the population in small urban centres, etc. To what extent do these factors influence the use of PC?

The quantitative methods of conventional and operational research cannot answer these questions, except, perhaps, through their ability to provide indicators of change. It is necessary for the researcher to involve himself in the action in order to identify the factors

> blocking improvements in PC coverage and the means of influencing these. We are, however, touching here upon a *third level of research*, that of action research, which will be examined below.

In concluding this section, we would point out that OR can become very sophisticated and cease to be practicable in terms of its usefulness for health policy. It is indeed possible that the amount of information required to operate the model is so great or of such a complex nature that the approach becomes difficult to manage and drifts away from decision-making applications. Such OR can, however, still be useful from the point of view of gaining a better theoretical knowledge of the system studied. Some epidemiometric models of diseases can be laid open to this criticism.

Thus far, we have moved forward in three stages towards a stricter definition of HSR: it is *scientific* research, relating to *systems*, and involving a *model.* We would now like to underline how much we believe can be gained from having health systems research draw upon on action research.

Action research enhances HSR by placing the researcher in the real world of complex social and human systems

We have just placed OR within the framework of HSR. But OR also has its limits: it has a substantial mathematical component and is thus only suited to problems where the parameters can be quantified with a sufficient degree of accuracy. The importance of human factors, which are not directly quantifiable, argues in favour of the idea that HSR can be conducted in a different manner, through recourse to action research (AR). The latter will set out to:

- identify the non-quantifiable variables by immersing the researcher in the subject of the research;
- test these variables through action.

It is not enough merely to immerse the researcher in the subject of the research; cultural anthropology uses the same tactic, but remains a non-interventionist method of observation. AR will combine the two aspects, immersion and action.

By working from a *model* and incorporating the *qualitative* factors, AR

offers HSR a research method in which consideration of human values and behaviour - by nature difficult to quantify - plays a key role.
Whilst OR originated in military and industrial research, AR came from the social sciences, which were faced with problems hitherto unresolved by traditional methods. Lewin (1946) was the first to use the term in his approach to the problem of minorities in the United States. It must be stressed, though, that, contrary to Europe, social sciences research in the United States has always been geared towards action, in line with the democratic ideal at the heart of the North American cultural and institutional venture. Since then, action research has sparked off much debate regarding its definition, field of application, methods, scientific criteria, similarities and differences vis-à-vis conventional research, and its ability to represent a new paradigm in human sciences (ULB, 1981; INSERM, 1985; Mayer and Ouellet, 1991).

We would opt for the definition given by Mayer and Ouellet (*op. cit.*, p. 108), which was based on the terms used by Gauthier (1984): AR is "a form of research which makes the player a researcher and which focuses the action on research issues. It differs from basic research, which is not founded on action, and applied research, which continues to regard the players merely as objects of the research and not as active participants." Goyette and Lessard-Hébert (1987), who are quoted by the same authors (Mayer and Ouellet, *op. cit.*, p. 110), emphasise two important aspects of AR: its goal of educating those involved, which involves the acquisition of knowledge, and its circular logic, which means that information feedback during the research process can modify both the research and the action.

Taking the human sciences as the point of departure, Rapoport (1960) conferred a formal legitimacy on AR by stressing its "working-through" approach which involves: i) players seeking a new action paradigm; ii) researchers, who are not necessarily different from the players and who are expected to provide provisional work tools to satisfy the need for change; and iii) an approach which gives form and finality to experiences thanks to a model which, once verified and corrected, can be transferred to analogous situations (Resweber, 1995).

Aside from its purely mathematical aspect, everything which has been said about the model in operational research also applies in action research. While mathematical models may seem ideal, they are often inapplicable in public health. It is worth recalling von Bertalanffy's comment (1971, p. 23): "The advantages of mathematical models - unambiguity, possibility of strict deduction, verifiability by observed data - are well known. This does not mean that models formulated in ordinary language are to be despised or refused. A *verbal model* is better than no model at all, or a model which, because it can be formulated mathematically, is forcibly imposed upon and falsifies reality."

The model used in AR will this time be more qualitative than quantitative and, given the researcher's involvement in the action and the participants' status of researcher-players, will take the fullest account of the specifically human aspects affecting the players' behaviour in the system.

Authors such as Susman and Evered (1978) from the field of administrative sciences emphasise one characteristic of AR perfectly suited to HSR: "to develop the self-help competencies of people facing problems". AR is an "enabling science". Below, we will adopt the position of these authors on two important points in the discussion on AR: the cyclical nature of the approach and the specific characteristics of AR compared with conventional research.

AR follows an iterative process well known to planners: diagnosis of the situation (identifying a problem), planning the action (examining alternative solutions to the problem), opting for a particular action (selecting one of the solutions examined), evaluation (studying the process and consequences of applying the solution), formative conclusion (identifying the general results). In a process of this type, the researcher is involved in the "client system", i.e. the system as both object and initiator of the research.

When it comes to its specific nature, there are, for the authors we mentioned, six characteristics which differentiate AR from what they call "positivist science":

- AR is *future-oriented*, and has links with the planning process
- AR is *collaborative*, involving interdependence between the researcher and the system studied
- AR involves *development of the system*, producing both a link between the system and its environment and procedures aimed at solving problems
- AR engenders a *theory based on action*, to be revised in the light of the evaluation and its consequences
- AR is *agnostic*, as both the theories resulting from the action and the

recommendations for action are themselves the products of the previous action and have to be re-examined when confronted with a new research situation
- AR is *situational*, in that it is not based on relations previously observed between an action and its result, but reformulates the action to be taken and the expected results according to the new situation to be analysed.

In AR, the role of the researcher, both within and outside a group which itself is the object and subject of the research, is to perform a transfer of knowledge enabling the group to clarify its method of operation and its relation to the rest of the social system, in a "procedure for reactivating a situation on the basis of its components" (Pirson, 1981). The researcher's role may, furthermore, fit into a fairly well mapped out methodology, particularly in the form of focus groups (Simard, 1989; Krueger, 1994).

The question of whether AR needs to justify not meeting the criteria of positivist science or, on the contrary, needs to affirm its scientific nature on other bases has been considered, *inter alia*, by Susman and Evered (*op. cit.*), who deal with it at some length. They conclude: "Action research constitutes a kind of science with a different epistemology that produces a different kind of knowledge, a knowledge which is contingent on the particular situation, and which develops the capacity of members of the organization to solve their own problems."

Curiously, Mayer and Ouellet (*op. cit.*), in raising the problem AR's particular scientific nature, are well aware that the latter is linked to the potential for generalising research results, but fail to examine the point in detail.

Whilst not excluding conventional research from the AR approach, Resweber (1995) contrasts "what is known as pure or basic theoretical science" with action research by stressing the different position held by theory in the two approaches.

1. The theoretical moment involved in AR is entirely subordinate to the desired change and is the obligatory prerequisite for action. However, "the theory, extracted as it were from the reality it legitimises, generally remains hidden".
2. The theory is the very driving force of the action, the normative profile for the new requirements to which the action must conform.

3. The theory derives from the change itself, which it then sanctions after the event.

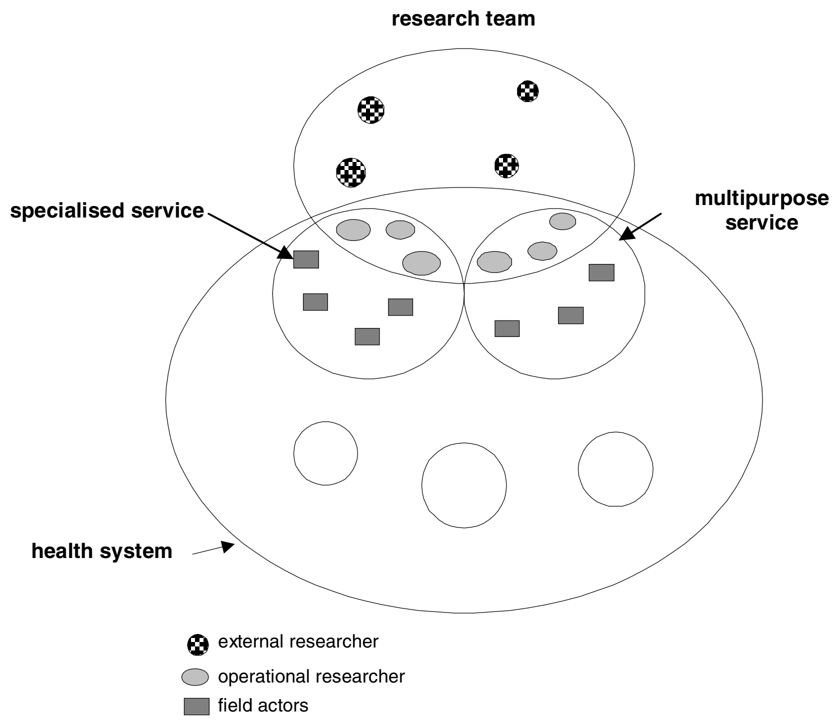
We will see that these views can be given shape in the HSR model which we shall set out below.

In response to those who might object that these three links between theory and practice are no strangers to basic research, Resweber points out that the context and aims of the two types of research are not the same. In basic research, the context is first of all an established and recognised fact, which is then put forward as a reference value, "in the form of a law, book or formula" (*ibid*., p. 12). The aims are formal, explanatory and abstract, which explains why the practical consequences of basic research are usually indirect and mediate. In action research, on the other hand, the framework is that of human activities or work undertaken in an indefinite future and its aims are "directly and explicitly oriented towards the expected transformation, the anticipated change, the crisis to be provoked or resolved, or towards the future values to be promoted" (*ibid*., p. 13). "Pure research is linear and segmented: it records definitive conclusions, even if these are 'falsifiable'. Action research, on the other hand, is dialectic and circular: it proposes provisional interpretations, which may be reworked following exposure to practical situations. The two have different goals. The scientist constructs general laws, whilst the field researcher expounds regulatory processes whose models are generally transferable to other fronts" (p. 16).

Resweber offers two criteria as basis for the objectivity of the knowledge produced when researchers and players come together: consensus and effectiveness. *Consensus* relates to the manner of understanding and conducting joint work and to the objectives of more effective action and the required changes in roles - in short a "project". By selecting, clarifying and reformulating the intentions of the players, the consensus guarantees that the project is consistent with the topics examined and the action to be undertaken. Effectiveness has two dimensions: *symbolic effectiveness* depends firstly on the *reliability* of the resultant knowledge: this should be able to concur with the social and scientific order, i.e. "move" in the area of social communication as well as in the scientific community. But symbolic effectiveness also depends on the transferability of the resultant knowledge. "The better a paradigm [we would say *model*] performs, the more able it is to shed light on practices other than those from which it was derived". The second dimension to effectiveness is *pragmatic effectiveness*: "the models transform the practices for which they are required" (pp. 41-42).

We still need to specify the position of the researcher in AR. Not every player in the system is necessarily a researcher: some may be objects of the study, others both players and researchers and others again researchers only (but involved in the action). Figure 5 illustrates the various possible roles taken by researchers in AR.

Figure 5 : Role of researchers in operational research and action-research. The case of a health system and two of its sub-systems



Source : Mercenier P and Prévost M, 1983

Let us end by stressing the fundamentally "humanist" (some would say "democratic") nature of AR: it is the players in the system who "hold" the solution to the action research problem which has been posed, and the set of hypotheses forming the model proposed by the researchers will only be usable once it has been understood, accepted and recognised by the group. This specific feature of AR may have political dimensions, and it has been emphasised that the "liberation" aspect of AR, conceived as participatory research, may sometimes turn physically against its players in political regimes reluctant to tolerate free speech (Smith *et al.*, 1993).

To sum up, AR, in its various forms, always offers an original synthesis of the following four characteristics: it is research applied to action, carried out *with* the players and not just on them; it is involved research, rejecting the positivist gambit of neutral and external observation of phenomena; it is imbricated research, presupposing that players are also potential theoreticians in a socio-political field which cannot be deemed constant; it is engaged research, which may take the form of practical experimentation or social and political intervention (Paillé, 1996, pp. 193-195). Without cutting itself off from quantitative approaches, AR assumes the full mantle of qualitative research and bids farewell to "research without a researcher" in the social and human sciences (*ibid.*, p. 59; George *et al.*, 1996).

We can now compare action research with operational research and conventional research using the same checklist as before:

- Its contribution to knowledge and action

1. The *objective* of action research, as of OR, is *optimum decision-making* in a given situation. In contrast to conventional research, the primary purpose is not therefore to help expand decontextualised general scientific *knowledge*. Nonetheless, a type of knowledge is obtained which Susman and Evered (*op. cit.*, p. 599) liken to principles of action or guides for dealing with different situations and producing techniques which they call *practices*, making AR, as has already been noted, an *enabling science*.
2. As with OR, action research's *contribution to science* is knowledge based on *situational results* closely linked to the original situation, in contrast to the results of conventional research, which aim for *universality*.
3. Action research's *utility for action* is the creation of a model. Unlike OR, this model is only incidentally mathematical, or not even mathematical at all. Since it makes considerable use of human factors, we will call it a *behavioural model*. As in OR, production of a model is not just a result of AR, but a necessary step; the model is first of all a research instrument.

- Its methodology

4. Action research takes a *systems approach*. Readers are referred to what we said about OR.
5. The selected *hypotheses* have dual temporality. Readers are referred to

what we said about static and dynamic hypotheses in respect of OR. Human behaviour is the product of history. Where this behaviour is a key element of the system being studied - as is the case with health systems - change cannot be introduced into the system from the platform of analytical-type research which ignores history, as conventional research typically does.

6. The *subject of investigation* is *internal* to the research process, the "client system" described by Susman and Evered (*op. cit.,* p. 588). Obviously, the very existence of action research can, more than in other instances, interfere with the phenomena observed. However, it is not specific local results which are of interest for research, but generalisation of the model used for the system.
7. The *researcher is not neutral* vis-à-vis the action research process. On the contrary, he is *involved* in the action. The researcher's subjectivity may affect the model as much as the results. Hence the importance of the objectivisation processes evoked above, founded on consensus and effectiveness. Let us remember though, that situational results are unimportant in terms of their reproducibility; it is the generalisation of the model which counts.

- The subject of the investigation

8. The *type of problem* studied has a predominantly *social, institutional and behavioural dimension*. It concerns staff attitudes, patient behaviour, population reactions, health/clerical worker motivation, etc. This social and behavioural dimension is of course not confined to *individual* actions or behaviour. It is just as concerned with the *collective* dimension entailed by the interplay of individuals within institutions or other structures.

    For instance, the system of health care payment (per consultation, per episode, capitation) will, through structures and institutions, condition the right of access to care and the distribution of the cost of care within the population (Abel-Smith, 1994, pp. 149-202): the resulting social and behavioural dimensions go beyond the sphere of particular individual behaviour and may give rise to action research in which the institutional and collective dimension predominates.

9. The *duration of the study* will generally be *long-term*, since any system in which human behaviour plays an important role will in most cases (i.e.

except in cases of disaster) only evolve gradually.

10. The *research area*, in the geographical sense or in the statistical sense of a sample, does not need to be representative, but rather *conducive* to action research. The purpose is in fact to show that, providing certain conditions exist, change is possible.

We try to summarise the main differences between conventional research, operational research and action research in Table 2.

Table 2 : The different types of research on which HSR may draw

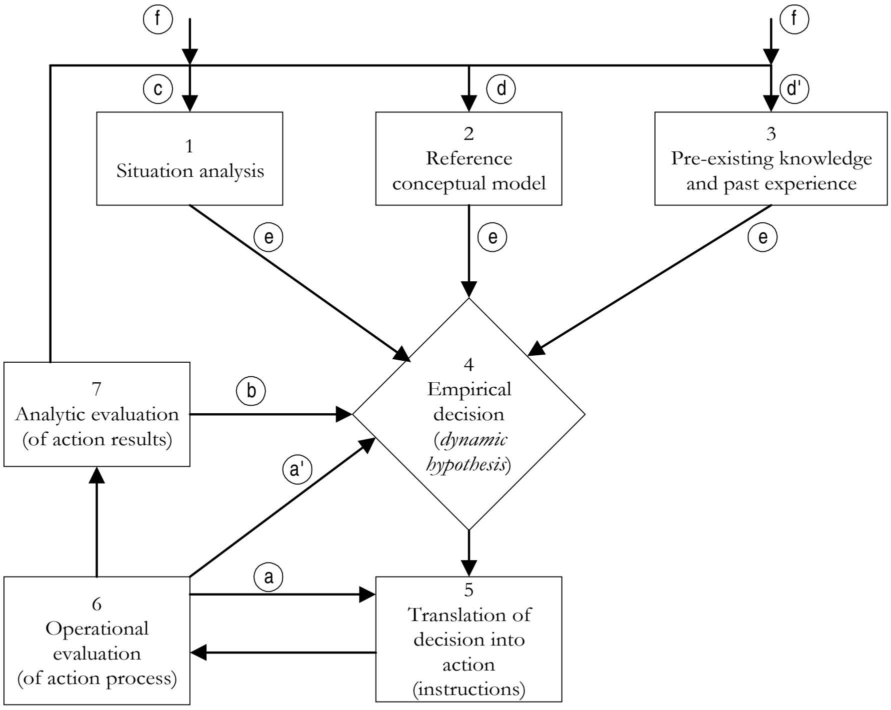
CRITERIA	CONVENTIONAL RESEARCH	OPERATIONAL RESEARCH	ACTION RESEARCH
Objective	Helps expand scientific *knowledge*	*Optimum decision-making* in a given situation	
Contribution to science	In terms of knowledge, the results aim for *universality*. They also help confirm (or challenge) the dominant paradigm	In terms of knowledge, results are *situational*	
Utility for action	Information for *action*	Creation of a *mathematical model* which it may be possible to generalise	Creation of a *behavioural model* which it may be possible to generalise
Nature of the approach	The approach is non-systems-oriented and *decontextualised*	The approach is *systems-oriented*	
Type of hypothesis / Time factor	*Static* hypotheses / No conjecture as to the future	*Dynamic* hypotheses / "Observation of the present plus interpretation of the present from knowledge of the past, conceptualization of more desirable futures" (Susman and Evered)	
Position of the subject of the investigation	The subject of the investigation is *external* to the research process		The subject of the investigation is *internal* to the research process
Position of the researcher	The researcher is *neutral vis-à-vis* the action		The researcher is *involved* in the action
Type of problem studied	May be *technical* or *social*	Has a predominantly *technical dimension*	Has a predominantly *social and behavioural dimension*
Duration of the research	*Variable*		*Long term*
Nature of the research area	The research area must be *representative*		The research area must be *conducive* to action research

Proposal for an HSR methodology

What sets HSR apart is not so much creation of new knowledge as reference to a system requiring management.

Building on what we have discovered in our tour of research methodologies, we shall now propose a methodological reference scheme for HSR based on the following requirements: scientific approach, systems approach, explicit use of a model, involvement in the action. We shall be developing and modifying the scheme already proposed by Mercenier (1992) and Nitayarumphong and Mercenier (1992). As with Susman and Evered, this is an adaptation of the planning cycle (Figure 6).

Figure 6 : A basic methodological scheme for health systems research



Source : Nitayarumphong S and Mercenier P, 1992

Two things must first be made clear. Firstly, we are not seeking to show that action research is the ideal methodology for HSR. Secondly, in proposing a reference scheme we shall not be attempting to standardise HSR. While recognising the signal interest of HSR for such complex human systems as health systems, we are more interested in trying to capitalise on the advantages of various types of research within a consistent overall approach to HSR.

CONCEPTUAL AND CHRONOLOGICAL ELEMENTS OF THE SCHEME

In this methodological scheme, analysis of the situation [1] corresponds to standard practice in any "realistic" or "incrementalist" planning: the situation is not analysed exhaustively, as in "synoptic rational" approaches which set out to plan everything from a platform of complete knowledge of the initial situation; instead, only those aspects are analysed which are relevant to the proposed planning, research or action (for a recent and concise summary of the various theoretical conceptions of planning, see Walt, 1994, or Green, 1992). Of course, in view of the foregoing, analysis of the situation will include an inventory of interrelations between players.

The existence of a conceptual reference model [2] is for us the first key element of the approach. Every player has a certain perception of the system in which he operates: his actions, reactions and initiatives are in fact based on an *implicit model* of the system of which he is part. If one were to talk in terms of attitudes and behaviour, one could say that most of the time this model is "non-articulated". A good manager will thus develop an overall representation of the system he manages, even if he is not always capable of explaining why a particular procedure remains appropriate to the situation or a particular measure has fallen into disuse. The difference between good management ("the scientific approach" to management) and HSR, other than the *obligatory formulation of a hypothesis*, lies precisely in the *explanation of the model* of the system. The model will vary according to the system or sub-system studied: it may, for example, be a model of a health district, of relations between the private and public sectors, of the operation of hospital services, etc.

We could endorse here what Churchman *et al.* said back in 1957 regarding operational research as systems-oriented research: "To assert that O.R. is concerned with as much of the whole system as it can encompass does not mean it necessarily starts with the system as a whole. Most O.R. projects begin with familiar problems of restricted scope. But in the course of the research the scope is enlarged as much as circumstances permit" (p. 8).

We would also emphasise that HSR can get under way without first having *completed* its reference model. If the HSR tends towards OR, the model will tend to be more complete and established from the outset, this being a prerequisite for its mathematisation. But if it tends more towards AR, the research can begin with a much more *incomplete* model. To try to impose a highly elaborate and relatively fixed model on HSR from the outset can prove counterproductive: the risk with ill-conceived modelling is rigidity and paralysis. If a model is "too well built", the research itself may be doomed merely to shift the problem: blockages in the day-to-day functioning of the system, which HSR was supposed to analyse and resolve, are transformed into paralysis in the very fabric of the system - the result, perversely, of applying a model which sets out to predict everything, too much even, in advance, including the material management and follow-up of the research process. The model eventually proves too inflexible to cope with the real situation (proliferation of executive committees, consultation groups, follow-up cells, on-going monitoring schemes, etc.). Furthermore, if one waits to have a very elaborate model before starting HSR, one may find oneself, in practice, facing an extremely inhibiting situation in which one ends up doing no research at all, or opting for a form of decision-making which might well be empirical and reasoned, but which cannot be classed as scientific research.

One might, at first sight, place knowledge of contextual elements and previous experience [3] in the analysis of the situation [1]. We prefer to distinguish between the two, at least in part, because what we understand by this is information on *the actual conditions in which the conceptual reference model is used and adapted in a given situation*. This model must necessarily be simplified if it is to become a decision-making tool. Thus there will rarely be scope for describing it in a manner which is precise and comprehensive enough to be understood outside the context in which it is conceived. If this were nonetheless the case, box [3] would be redundant. Generally, though, it will be required, as one can rarely ignore the environment of the system, or of the model which re-presents it. And, as we shall see, the research process will have a different effect on our knowledge of the context and our analysis of the situation. Churchman said that the man of science needs to find a way of thinking about a system's environment which is richer and more subtle than mere research of frontiers. He will achieve this by noticing that when we say something is "outside" the system we mean that the system has but little influence on the characteristics or behaviour of that something. In fact, the environment comprises things and people which constitute "data" from the point of view of the system ... The environment is not only something which escapes the influence of the system; it is also something which in part determines the system's performance (Churchman, 1974, *op. cit.*, p. 40).

The second key element of the approach is the empirical decision [4]. All action calls for a decision (and not to decide is also a sort of decision). The peculiarity of the methodological approach we are proposing is to consider this decision as *introducing a change into the system, from which certain effects are anticipated*. It is supposed to be the best possible decision, i.e. it will have been taken in the light of the advantages and disadvantages it involves, objectivised as far as possible. We have proposed that this type of decision be called a *dynamic hypothesis*. The hypothesis will be all the more relevant, the more correctly the situation has been analysed, the more knowledge and experience are acquired and the more light the model of the system (made *explicit* in the research approach) sheds on the variables, constraints and dynamics in play.

The next stage is to move on to *action* [5]. This takes the form of a number of activities. It will follow precise instructions guaranteeing consistency between the decision taken and the process to be undertaken in order to obtain the desired change. The dynamic hypothesis will be validated or invalidated by checking whether the effects of the action correspond to what was expected when the decision was taken. This operational phase requires the researcher to be involved in the action itself. An understanding of what has to be done (decision) and the grounds for doing it (action) are, in the approach proposed here, essential elements for effective application of the decision (testing the hypothesis).

The next element is *evaluation of the process* for implementing the action [6]: are the activities being carried out as they should be in order for the decision to be effectively applied? Without this verification, the relation will never be established between the decision taken and the observed results (it will not be possible to base the test of the dynamic hypothesis on observation of the relevant facts).

The final element is *evaluation of results* [7]: do the observed results correspond to what was expected when the decision was taken? It is this analysis which will determine the validity of the dynamic hypothesis. In fact, given the conceptual framework employed here, incorporating the contributions of action research, we should speak of the *veracity* of the hypothesis - as in the social sciences - rather than its *validity* in the sense used in the basic sciences or in conventional research in general. But analysis of results has a second function, which is to check the extent to which the elements of the empirical decision-making were originally assessed satisfactorily. Just because the results are "positive", it cannot be concluded that the hypothesis was valid/truthful: we still need to know why the decision produced the expected result; other conducive elements may also have helped produce the results by pointing in the same direction as the elements which led to the decision being taken. Similarly, if the results are "negative" we should be able to say why: which adverse elements, not considered when the decision was taken, have prevented us achieving the expected results? In plain terms, one might say it is a question of trying to work out what good or bad luck accompanied the decision-making, of reaching a verdict on whether the initial decision was really *the best*.

FEEDBACK

A methodological scheme such as this involves a whole series of feedback flows. The first feedback (a) occurs during the evaluation of the process and involves correcting an action process which has started badly or diverges from the decision taken. A variant of this first feedback (a') may be to challenge the empirical decision itself if evaluation of the process shows it to be inapplicable.

The second feedback (b) derives from evaluation of the results, giving an opportunity to correct the decision if it proves to be unsatisfactory not on account of a shortcoming in the implementation process, but because the results prove not to be those anticipated. This feedback is essential for *action*.

Evaluation of results also gives rise to the third feedback (c), which helps extend the initial analysis of the situation and takes account of any changes.

However, what provides this approach with an element of scientific *research*, and justifies specific investment in this type of research, is the feedback from evaluation of the results to the original model (d) and to the contextual elements from which it was derived and to which it remains linked (d'): this double feedback makes for better understanding of the system through refinement of the conceptual model and more precise measurement of the contextual elements which determine its reproducibility. *It is this conceptual model which permits generalisation of the results* of the research and which allows the same players in an open-ended situation, or other players operating in different circumstances, to analyse, adapt and test the model in a new set of specific circumstances (e).

We must stress the importance of feedback from evaluation of the results to the contextual elements needed to understand the conceptual model (d'). Retroactive correction of the initial context - and not just correction of the conceptual model itself - is necessary for the model to be transferable to other circumstances. Let us illustrate the importance of this distinction between feedbacks d and d': the *health district model*, eminently adaptable to specific local situations, may also display broad similarities from one country to another as long as we are dealing with *rural* districts. If, however, we take this supposedly well known and well established model as the basis for organising an *urban* health system, the odds are that the original conceptual model will be inapplicable. One might say that its conditions for transferability are located in a separate "toolbox", which it is essential to explore and inventory. This "toolbox", containing the contextual knowledge and experience which made the model usable in the initial circumstances, should enable us to adapt the rural district model to the new urban circumstances.

In this scheme, the role of conventional research is to contribute to analysis of the situation and help provide new knowledge (f), for which we still need to find the place in the model, or knowledge of its context.

Some may be astonished, concerned even, by the importance we attach to the understanding and motivation of the researcher, as if there were not enough subjective elements already in AR, on which our approach draws. We believe these two elements are indeed necessary for testing the hypothesis. While the action must adjust constantly to its objectives, the researcher must be capable of rapidly interpreting the immediate feedback from the implementation of the decision: thus he must at all times *understand* what is at stake in the research under way. And if HSR, like AR, is to "*release human potential*" (Susman and Evered, *op. cit*.,p. 600), the researcher must *believe* that this human potential can truly be released. His *motivation* is therefore essential to the performance of the research. We saw earlier, in respect of AR, that these subjective elements did not prevent generalisation.

The potential field of HSR

Though the methodological approach we have proposed for HSR draws largely on AR, it must be remembered that no form of research need be excluded from the field of HSR as long as it really is *scientific research* and relates to the health *system* (or one of its sub-systems). However, the respective contributions made by conventional research, operational research and action research do need to be made clear. This will avoid much confusion, including in reference works, where, for example, HSR can be presented:

- as an "umbrella" for a *multidisciplinary approach* covering operational research, research into health services, research into personnel management, political analysis, economic analysis, applied research and research into decision-making (Varkevisser *et al*., 1991, p. 15);
- but "participatory in nature" (*ibid*., p. 24), which should imply the *specific approach of action research*;
- yet never mentioning the use of an *explicit model* of the system under study;
- despite the fact that this work abounds with figurative models;
- and one is led to believe (though it is not made clear) that HSR is the opposite of basic research and falls more within the field of *applied research* (*ibid*., p. 15), a contrast which to our minds seems rather sterile.

If we had to sum up, we would propose the following classification of the types of research which can be used in HSR, though not necessarily in order of usefulness:

1. Conventional research, *provided* it considers the interrelations between

the object being studied and the rest of the health *system* (or a sub-system); yet by meeting this condition, "conventional" research ceases to be conventional and becomes systems-oriented, though without being able to go so far as testing a hypothesis on the system's dynamic. There will be few cases in which conventional research can be described as HSR.
2. Operational research, which combines the use of a (mathematical) *model* with a systems approach
3. Action research, which, on top of the systems approach and the use of a (behavioural) model, adds the *involvement of the researcher and the players*, or at least some of these, on the principle that their practical experience of the problems is greater than that of the researchers but will gain from being made explicit in a conceptual model.

The sole aim of the methodological approach we have proposed is to capitalise on the specific contributions of these various types of research.

Obstacles to change in HSR

Various obstacles can hamper the introduction of change into a health system.

a. *Cultural obstacles*. When a population's value system is involved, it is only accessible to HSR over a very long period, if at all. Thus, in the field of family planning, it makes a difference whether the problem to be solved relates to aspects contingent on sexual behaviour for which solutions exist and which are sensitive to HSR (e.g. use of contraceptives) or whether it relates to a deep and culturally determined aspiration to have a large number of children - which will limit HSR in the short term.

b. *Material obstacles*. Where a solution requires more material resources than are available, HSR will be ineffective. For example, the professional commitment of health staff vis-à-vis the population they serve depends on the solutions found to various material problems governing the living conditions - and in some cases the very survival - of the health workers. To have to live or work in persistently poor material conditions can divert them from their responsibilities. If these problems can be managed by bringing various mechanisms into play to enable the population to ensure that health staff enjoy acceptable social status, then there is an opening for action research. But if the question of status is one that the population is unable to satisfy (much better salary offered elsewhere in a programme funded by an international organisation, educational facilities for the children of staff with higher aspirations), any HSR at this level will soon know its limitations.

c. *Structural obstacles*. When the very structure of the system is involved, the solution might be beyond HSR. For example, HSR might be able to illustrate that the system of payment per consultation clashes with an overall, ongoing and integrated method of paying patients' costs, but it will be unable to change this situation: it is the method of payment itself which would need to be changed and a decision of this sort concerns the very structure of the health system - the interests at stake, the power structure and the underlying ideology.

In all three situations, though, we need to distinguish between obstacles to action and obstacles to research. The fact that action may not come to fruition is also information. It might be possible to conduct HSR without this leading to change; it will succeed only in singling out the causes of the blockage, and this, in itself, is also a research result.

Results and completion of HSR

When does HSR end, and with what result? We can distinguish three situations.

a. The hypotheses have been tested, the model validated and the conditions of the model's validity (contextual elements) made explicit. For instance, it can be shown that the performance of ancillary health staff can be improved under certain conditions (Kasongo Project Team, 1976). The hypothesis is that ancillary staff can be just as effective as more highly trained staff in a large proportion of basic care activities, but only if (contextual elements) they are given the right technical tools and the action (decision) is conceived as staff promotion; this means weighing up the contextual elements as to which activities are to be delegated, what training is to be provided and what supervision is required.

b. It has not been possible to test the hypotheses, though the limits to which change is possible and the obstacles defining those limits have been identified. Such identification of limits and obstacles is also a research result. This situation of apparent "failure" can be highly instructive and signal a possible "completion" of HSR.

c. But there may also be no precise point at which HSR "ends". More than a "finished" result (in the sense of a *circumscribed, complete and final result*), the aim of HSR, especially in the form of AR, is to expand the players' knowledge of and empowerment in their own system. A result such as this is harder to show and demonstrate than a result in conventional research. In this sense, when does action research end? Does it even have a predictable end, or does the empowerment process not continue for some time after the research has formally ended?

Apparent consensus on HSR

To conclude this second section, it is worth reviewing the points on which researchers describing their work as HSR tend to agree (Grundy and Reinke, 1973; Taylor, 1984; CEC, 1992). In the light of the foregoing, we intend to qualify these points of apparent consensus.

a. HSR includes, but is more than, study of the biomedical aspects of diseases and health determinants, and study of health services. It has as its object the constituent components of *health systems*.
We would like to add that HSR also goes beyond the study of system components *analysed in isolation.* HSR can only exist when the study takes in system components other than those forming a binary and linear relationship. It is the *interrelation* of these components which characterises a system and is thus the focus of HSR.

b. Each component of a health system, including its biomedical aspects, can give rise to research. Here too, we must insist on the following detail. This research will only be HSR if the component is not examined in isolation but *in the wider context of the system*. In other words, it must be possible to extend the conclusions of the data analysis to an understanding of the *interrelations* governing the health system.

c. Given the complexity of the subject studied, HSR can make use of *various methods*: sociological analysis, statistics, epidemiology, operational research, etc. Often, the approach will be *multidisciplinary*.
We would like to warn here against the ill-considered use of a multidisciplinary approach.

For one thing, it is precisely the lack of a separate methodology, and the difficulty of relating it to existing methodologies, which in our view explains the lack of esteem in which HSR is still held. It is dismissed as a minor science, soft and ill-defined. A certain "methodological ecumenism" should not mask a degree of laxity in recourse to the various existing disciplines.

In addition, the multidisciplinary approach is too often taken to be desirable in itself, to confer an automatic advantage on studies which proceed under its banner or put it into practice. It is true that a multidisciplinary team may enhance the approach to a new type of problem with a plurality of ideas and solutions beyond the scope of a specialist from an isolated discipline, as was well demonstrated in the genesis of operational research (Churchman *et al.*, 1957, pp. 9-11). Yet very often the multidisciplinary approach is no more than the inevitable consequence of increasing specialisation amongst researchers, who no longer dare embark on anything outside their own increasingly narrow field. Multidisciplinarity is less a virtue than a necessity given the fragmentation of knowledge and research.

In short, while the multidisciplinarity of the research team may be important, what really counts is a multidisciplinary conception of research and a multidisciplinary approach to the subject of that research.

d. The consensus among researchers is that HSR has the status of *scientific research*, insofar as it guarantees that the research methods can be transposed to another context (ability to identify the crucial elements of a problem), the results can be reproduced (the same results can be obtained when applying the same solutions in similar situations), and the conclusions can be developed into general principles or theories (understanding of the causal relationship between measures taken and results obtained) (Taylor, 1984, pp.1-2).

We would, however, qualify the statement about the reproducibility of results. Given that HSR is a form of operational research or action research, it is not the results which can be reproduced (on the contrary, they are very closely linked to the specific situation studied) but the *models* which have been used for the research.

e. For most authors, HSR has *essentially practical objectives*, allowing answers to be found to the day-to-day concerns of decision-makers and managers.

We feel this opinion has been insufficiently discussed. Without denying the immediate practical interest of HSR, we have argued that it also possesses a *theoretical dimension*. All the more so in that one of its specific characteristics is the use of a *model*. While the practical conclusions of HSR may only be valid in the context of the local situation, the model can be theorised and reproduced.

In addition, the practice of automatically attributing a practical dimension to HSR can come across as rather too normative, or even dogmatic, an approach. One might just as easily reverse this argument and state that in order to understand the structure and operation of a health system, preference should be given to research which specifically targets the dynamics of the system *qua* system, rather than to accumulating partial studies on each of its components. Such an approach may not find instant practical application.

f. For most authors, the whole point of HSR is to *change* the health system, a notion bordering on the one we have just discussed.
We will qualify this statement in two ways: firstly regarding the status of change in HSR (means or end) and then regarding the very need to make changes to the system in HSR (compulsory or optional). With regard to the first of these two points, one might indeed consider change to be the ultimate goal of HSR, in which case we will need to come up with the means of *changing the system in order to improve it*. But change might also be considered simply as one of the means used by HSR: it is often the case that in order to *gain a better knowledge of a system*, a change is made and studies are then carried out into the complex effects of that change. As for the need for change, we have seen that action research is the very form of HSR which requires a *change to be made* to the system. It is, however, just as easy to imagine HSR which refrains from making any changes at all to the system, in order to observe and understand the *spontaneous development* of the system without external intervention. We have argued that this form of HSR requires the use of a model, whether or not changes are made to the system.

In conclusion, we feel that this apparent consensus fails to lift us above the “confusion” between HSR, action research, operational research and operational analysis which Reinke seemed to regard as inevitable (Reinke, 1988, p.33).

It also falls short of the ambitions which Grundy and Reinke (*op. cit.*, p. 18) set for “health practice research” back in 1973. Although they restricted this to health *services* or the supply of health *care,* they attributed the following characteristics to the research:

1) a systems orientation,
2) a multidisciplinary approach,
3) the use of the scientific approach conceived in terms of models, objec tives and feedback,
4) the objectivization of the decision-making process.

Ultimately, point 2) is the only one on which there seems to be real consensus. All too often, in our opinion, consensus on point 1) exists in word only. The reality of research protocols shows that the systems aspect is all too often lost from sight. There is, finally, no consensus among researchers involved in HSR on points 3) and 4). Yet it is precisely these characteristics which seem to us most worth emphasising if one is seeking a better understanding of the specific nature of HSR.

Example: contribution of the various types of research in the treatment of tuberculosis

Continuity of care in the treatment of tuberculosis

In the provision of long-term treatment for the chronically ill, the problem arises of the treatment being prematurely abandoned, especially if the patient is supposed to continue the course of treatment even after all the symptoms have disappeared. This problem can prompt various types of research.

a. *Conventional research* will formulate a number of hypotheses as to the possible factors behind the abandonment of the treatment. It could consist of a survey of non-compliant patients' reasons for abandoning the treatment (the treatment centre's opening hours were unsuitable, the medicines produced side-effects, the patient had to make a journey, etc.). It might also, in a case control study, try to compare the frequency of the factor which might explain the abandonment in subjects who followed the treatment regularly and in those who stopped (probability of abandonment as a function of accessibility, professional activity, etc.). Such studies fit the mould of traditional epidemiology - descriptive in the first case and causal in the second. The hypotheses put forward are, however, *static hypotheses*: they entail no action on the health system within which treatment is being abandoned. As for the degree of causality they are able to reveal, this, as we know, varies widely depending on the type of study, and it is interesting to note that the arguments for a cause-effect relation in studies of causal epidemiology (Hill, 1965) belong more to the realm of the philosophy of science than to epidemiology itself.

b. *Operational research*, as in the example of prenatal care, will test the hypothesis of a causal link between a possible factor of abandonment and actual abandonment of treatment by putting forward a *dynamic hypothesis* on how a change in the health system might encourage people to continue treatment. This change will be suggested by the representational model of the health system, which will establish the relations between the various possible factors explaining the decision to abandon treatment within the system in question. We will not enter here into the detailed arguments already presented regarding prenatal care.

Both conventional research and operational research do, however, come up against certain problems inherent in the nature of some of the possible factors behind abandonment of treatment, and these cannot be directly influenced, or even fully understood, without the researcher becoming involved. For example, conventional research of the case-control type might have shown that women are more likely to complete the treatment than men (or vice versa), or that it depends on level of education, level of income, etc. Or else the surveys will have shown that some patients abandon treatment because they feel better and see no further need to continue; others, on the other hand, stop because they can detect no improvement. Cases like this show the limitations of both conventional research and operational research.

An illustration of these limitations can be found in Varkevisser *et al.* (*op. cit.*, pp. 52-55), who use a causal model which aims to be exhaustive by including all the "objective" reasons for failing to complete a course of tuberculosis treatment. These range from the patient's sex to queue length via perceptions of the disease in the community. A causal model of this type can be useful when analysing the situation, provided the systematic nature of this method does not drift into simplistic cataloguing of single cause-effect relationships or obscure the multiple interactions which might exist between the various factors identified. The limitations of this "objective" approach do not, however, lie in the use of a causal model. The authors are well aware that the usefulness of identifying a potential factor of abandonment depends on the information available on it, its responsiveness to corrective action and its classification as a priority problem, but they are obliged to conclude: "The dissection of the diagram into different parts and selection of one part for research is not advised if insufficient insight exists into the nature, relative weight, and interrelations of the various factors contributing to the problem. You would risk concentrating on marginal factors and coming up with marginal solutions ... An *exploratory study* would then be indicated, limited rather in the number of informants than the number of factors included in the study" (*ibid.*, p. 55).

In our opinion, the way out of this impasse is to conduct not an *exploratory study* but an *explanatory study*, which falls squarely within the field of HSR of the action-research type. There are reasons for all phenomena, even if they are inaccessible to a certain type of "positivist' research: if patients abandon a course of treatment, it is because *they* have a "good reason" for doing so. Or rather, they are bound up in a web of diverse causes which dictate whether they continue or discontinue the treatment, and rather than trying to find out why patients fail to complete their treatment, it would sometimes be better to ascertain why they continue with it.

This brings us back to the fundamental problem raised in this paper: if HSR is making no progress it is because, faced with complex situations, researchers are clinging to a "conventional" approach to research and shying away from using other methodologies.

c. *Action research*, in a situation of this type, could come up with two hypotheses: i) completion or abandonment of tuberculosis treatment is the result of a complex set of interactive behavioural factors which are inaccessible to comparative studies and may even escape identification under a traditional "rational" approach; ii) there is no absolute cultural obstacle to following a course of treatment: even if the patient sometimes employs baffling strategies, "subverting" for personal or social "gain" a therapy which in a positivist approach to the illness appears quite straightforward, the basic fact is that anyone who is ill with tuberculosis wants to get better. These hypotheses, *which need to be verified*, together with other "objectivisable" elements already known about the health system, make up the *model* used by action research.

To check these hypotheses (which amounts to checking the validity of the model), the researcher needs to engage in ongoing dialogue with patients who have abandoned treatment (chase up patients not regularly following treatment) or, even better, those still undergoing treatment (prevent abandonment) so as to identify the behavioural elements of the patient, health staff, family, etc. which influence the decision on whether or not to continue treatment. It is not a question of simply observing - without acting - qualitative socio-cultural variables. It is about working from these variables to introduce change into the way tuberculosis sufferers are treated: the change (or lack of it) in the regularity of patients' attendance will validate (or refute) the representation of the health system in which these variables function (in other words, its model).
So this type of action research has a dual function: a) to verify the two ini-

tial hypotheses in a dynamic way by involving the researcher in the action; b) to verify, and if necessary amend, the explanatory model which makes it possible to reproduce this study in other situations.

Integrating tuberculosis treatment into primary health care

We can examine the possible roles of conventional research, OR and AR in integrating tuberculosis treatment into standard activities, according to the level of development of the primary health care (PHC) structure.

A programme to control tuberculosis is technically demanding and socially enriching for health care agents, from both a diagnostic and a therapeutic point of view. In other words, the ability to diagnose and treat tuberculosis may be perceived by the staff as a satisfying activity which nonetheless demands compliance with strict technical standards. It is no easy matter, then, to incorporate such a programme into primary health care: the problems to be resolved will vary according to the situation and require different types of research.

Depending on the strength or weakness of the PHC structure concerned, integration of tuberculosis control measures will have very different consequences. By a strong PHC structure we mean effective medical units: the technical level is good, and good throughout. By a weak PHC structure we mean poorly-performing medical units: the technical level is poor or mediocre, throughout. An intermediate structure can be imagined which performs relatively well, but is still too uneven. These three situations are shown schematically in Table 3.

A strong PHC structure will have no difficulty adding tuberculosis control to its range of activities, and if research is required its primary purpose will be to maximise results. Conventional research, of the cost-effectiveness or epidemiological type for instance, can easily be used in this context.

A weak PHC structure may well be overburdened by the technical demands of tuberculosis control, and integration of such control would not only be likely to produce poor results but could also demotivate staff. The aim of any research required here would be to identify means of improving this situation. Typically, action research will be tailor-made for such circumstances, where what counts is to release the existing human potential.

An intermediate-type PHC structure is likely to react in a highly variable manner to an attempt to integrate tuberculosis control. Local factors governing the applicability of control strategies, and inappropriate aspects of strategies devised at national level, are bound to give rise to specific research. Situations such as these might best be served by operational research.

Table 3 : Preferred method of research depending on the level of performance of primary health care in the case of integrating tuberculosis control

Level of development of the primary health care (PHC) structure	**Consequences of the process of integrating tuberculosis control**	**Specific objectives to be achieved by the integration process**	**Preferred method of research**
Level 1: weak PHC structure (unsatisfactory level of performance)	Major interaction between PHC and integration : the integration process can positively influence the PHC structure (opportunity to improve overall performance) but may also destabilise it (programme too demanding for staff, who are unable to cope with the demands of monitoring and supervising it)	Optimise the operation of the two components (PHC and tuberculosis control) within the integrated system	Action research
Level 2: PHC structure imperfectly established (level of performance relatively solid but still heterogeneous)	Variable interaction between PHC and integration : the integration process runs little risk of destabilising the PHC structure but needs to be adapted in places	Adapt the strategies for controlling tuberculosis to different situations in the existing PHC structure or define factors for the applicability of tuberculosis control strategies within the PHC	Operational research
Level 3: Strong (good level of performance) and homogeneous PHC structure	Slight interaction between PHC and integration: the integration process is easily absorbed by the PHC structure	Define an optimum strategy (technically the best) for controlling tuberculosis	Conventional research

Critical review of approaches which can be analysed as HSR

Action research on AIDS with women from Kinshasa: more action than research

The CONAISSIDA project ran from 1985 to 1990 in Kinshasa (Schoepf, 1993), with support from a range of sources including the Rockefeller Foundation, OXFAM/UK and UNICEF. It was presented as a transdisciplinary medical anthropology project, falling into a category of action research labelled "intervention ethnography" or, later on, "performative ethnography", a term taken from J. Fabian (1990).

In a preliminary phase more than 30 researchers set up a network of information providers in a bid to obtain a better understanding of "the constraints originating in psychodynamics and cultural expectations of sexual partners, and to create awareness of how socioeconomic conditions contribute to the creation of risky situations" (Schoepf, *op. cit.*, p. 1403). The organisers then worked in teams in a problem-solving and learning-by-doing approach leading up to the training of the trainers who prepared the community-based workshops proper. This phase served to devise the type of workshop ("risk-reduction") used for the action research with the Kinshasa women.

An initial group of fifteen women prostitutes took part in four one-morning workshops from late October to early November 1987. Techniques such as role-playing, case studies and simulations were employed to dispel misinformation about AIDS, raise awareness of risks, develop the ability to refuse unprotected sexual relations, promote use of condoms, etc. The approach was as participatory as possible, with conventional didactic features kept to a minimum.

Follow-up evaluation sessions were held after three months and eight months, followed by a third evaluation in February/March 1990. Each time the objective was to gauge the changes brought about and the effects observed: regular use of condoms, acceptance of condoms by clients, knowledge of AIDS, status vis-à-vis entourage, identification of sources of misinformation (particularly student clients, themselves misinformed but enjoy-ing very high status), assessment of the educational value of the workshops by the participants themselves, etc.

A second group of 60 "churchwomen" (so called because they held their meetings in a Baptist church) asked for similar courses after they heard of the workshops for the prostitutes. The two series of workshops were held simultaneously. Naturally, the churchwomen raised different points: acceptance of condoms by their partner, sterilisation of equipment in the medical units they attended, protection of older children, discussion of use of condoms within couples, etc. The same evaluations were conducted to assess changes in behaviour and take account of the new issues raised by the participants.

Finally, requests for similar workshops were received from other women's groups, colleges, officials, etc. But several aid agencies refused to fund this "empowerment method" which they considered "excessively academic"(?) and no doubt politically unfeasible.

This article stirs considerable sympathy but leaves the reader with a sense of frustration. It took over 30 researchers working for five years with 15 prostitutes and the 60 churchwomen to produce results which, though interesting and in many ways positive, nevertheless varied over the period and were often mixed and, ultimately, uncertain. But the most frustrating aspect of all is that the project cannot serve as an example because there is no theorisation with a view to reproducibility. Readers in other countries with similar problems in the field will say "There are surely interesting ideas in this Kinshasa project. But how can I apply them in my work here?"

The article is packed with information and ideas which could have formed the basis for a model, to be tested against experience in the risk reduction workshops, and *in this way* be of use to other researchers elsewhere. Without such a model, the CONAISSIDA project is no more than a tale from Kinshasa, with no possibility of generalisation.

Points raised in the article which could have formed the framework for a test model are quoted at length below. Though only scattered through the text, they show that the researchers had material to hand which would have enabled them to analyse the situation, build a model, make decisions on an empirical basis, test them in the field, correct the model and define contextual validation tools which would have permitted highly stimulating theorisation of their approach.

- One working hypothesis was that increased information would be necessary but not sufficient to change complexly motivated sexual behavior (*ibid.*, p. 1402).
- A second hypothesis was that poverty and gender inequality are related to the spread of HIV (p. 1402).
- We proposed to use action-research, an empowerment methodology, to enhance the capacity of existing community groups and networks to undertake risk assessment and generate social support for behavioral change (p. 1402).
- We hypothesized that the method could be adapted readily to integrate HIV/AIDS prevention in programs of community development organizations, informal voluntary associations, [etc.] (p. 1402).
- While AIDS education is indeed difficult, the chief difficulty may reside in the teaching methods, rather than in cognitive deficiencies of the learners or their belief systems (p. 1403).
- The dialogic approach challenges teachers to find ways to render complex concepts accessible by starting with what people already know (p. 1403).
- Because the group is actively engaged, much technical information can be conveyed and misinformation corrected without exhausting participants' attention and causing them to "switch off" (p. 1403).
- Participants are empowered to make their own situational risk assessment and to decide upon appropriate actions to take as individuals and as a group (p. 1404).
- Instead of focusing on behavior that cannot be altered under present circumstances, experiential training helps people to discover what they can do to make their situation somewhat better in the short-term (p. 1404).
- Creation of a favorable learning climate is crucial ... Adults require that education relate to their experience, challenge their powers of observation and reasoning, allow them to participate in shaping their curriculum, and foster rather than attack their sense of personal worth and dignity (p. 1404).
- Since communication systems are social systems, reception of information and the capacity to act on information received are differentially distributed in hierarchical systems. Consequently, differences in social

power must be considered in designing prevention strategies (p. 1407).

- The stage of the epidemic is an important factor in behavior change ... Change would have occurred without the workshops [but] participatory learning can bring about earlier change, especially among marginal social groups, before mounting death toll heightens general awareness (p. 1407).
- Because the terrain includes multiple, often competing discourses, including racism, moralism, male chauvinism and denial, the changing representations of AIDS are a necessary starting point for behavioral change (p. 1409).
- Because STD treatment is laden with cultural meanings, stigma and misinformation, health workers need special training to provide sensitive, confidential, effective care (p. 1409).
- AIDS prevention based on social and self-empowerment techniques ... could strengthen PHC delivery systems as well as other special (vertical) programs (p. 1409).
- It is unrealistic to expect sustainable programs in the current fiscal crisis, and "health-on-the-cheap" will not stop AIDS (p. 1409).
- The problem of relative social power - and powerlessness - has been neglected in prevention policy (p. 1409).

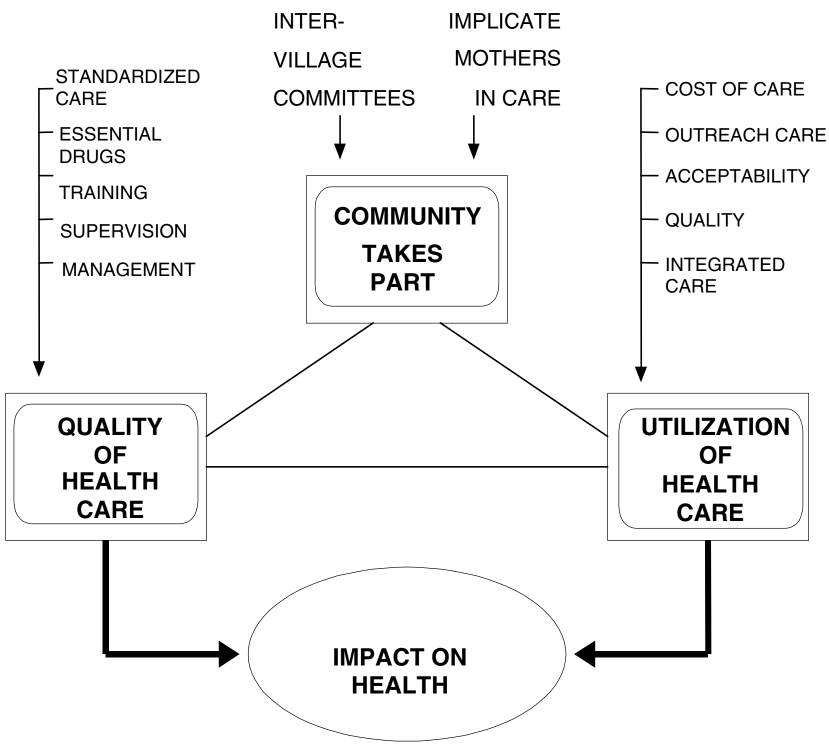
This project is thus a case of researchers engaged in non-conventional research which makes fairly clear use of AR methodology. The authors seem to judge the short to medium term results fairly positively, though we would tend to find them limited. But this is not the main criticism which could be levelled at this study. The regrettable thing is that failure to organise the resulting knowledge to produce a model usable elsewhere makes the situational results of the research seem inconclusive. Certainly the project provided the researchers with "an intensive fieldwork apprenticeship for research assistants" and an "opportunity to gather data on the polysemic cultural construction, social context, changing response to and impact of AIDS" (*ibid.*, p. 1409). But the *lack of any possibility of generalisation* - not of the qualitative or quantitative results of the research but of the approach itself - with the aid of a *model* casts doubt over the validity of the author's conclusion, based on five references to his own work, that "action-research interventions are widely applicable and can be adapted to integrate HIV/AIDS prevention in programs of existing community and workplace organizations throughout Central, East and Southern Africa". We do not believe any such general conclusion can be drawn from the study as conducted and as presented in this article.

Research on the health system in Burkina Faso: limitations of the experimental model in HSR

An HSR project in Burkina Faso provides an example of how the conventional experimental approach is ill-suited to action research. The study's methodology has been described (Diesfeld, 1992; Sauerborn, 1992), but the final results have yet to be published.

The study is presented as action research. It seeks to measure the impact on the health of the population of a package of new measures, by comparing an intervention zone of around 100 000 inhabitants (where the package was introduced) with a control zone of identical population size (where the health services were left to operate as normal). The basic hypothesis is: "To what extent does the execution of a service package influence the state of health of a population in a given zone?" Use is made of a representational model (Figure 7), though without any feedback mechanisms, in which involvement of the community, improved standards of care and greater use of care services have an impact on health.

Figure 7 : Intervention model on health system in Burkina Faso



Source : Sauerborn et al., 1992

The intervention had to last three years and was made up of three components: community action, reorganisation of the district health services (including medical supplies) and the training of health staff, community health workers, village health committees and "key mothers".

The effect in both the intervention zone and the control zone was measured in terms of community participation, the use, effectiveness and acceptability of services, and costs. This involved relatively sophisticated means such as specific mortality rate by cause and age, verbal autopsies, the nutritional status of children, morbidity (means of measurement unspecified), the financial and indirect costs of care, price elasticity of demand for care, etc.

The researchers were from outside both the area and the project: they were students from the University of Ouagadougou and various persons from the national Ministry of Health's central or regional services or from aid agencies (ORSTOM, University of Heidelberg, GTZ).

Some of the many problems posed by this type of research are very clear to the authors. They are thus very wary about involving the researchers in the action, and see it as a last resort liable to spoil the purity of the experimental model: "In action research, a clear distinction has to be made between the intervention as such and the research, observing the intervention, measuring its input, output and outcome without interfering with the intervention itself. The problem of the researcher influencing the object and the subjects of his research cannot be overcome. The mere fact that something happens, where before a rather stagnant routine was going on, makes a change for the provider as well as for the user of services. Even the 'control zone' is not free from that bias. The control zone might become 'infected' and the observation of the non-intervention is still a certain intervention" (Sauerborn, *op. cit.*, pp. 43-44). We tried to show above that involvement of the researcher in the action is one of the defining characteristics of action research and that the reference methodology no longer *has to* be that of conventional research. The legitimacy of involving the researcher is part and parcel of action research and the protocol need pay no deference to conventional research methodology. Similarly, the research area does not necessarily have to be *representative*: if the objective is to demonstrate an effect, the area should be *conducive* to action research.

Secondly, it is not clear where the study stands on the use of a model. In the same sentence, the term *model* seems first of all to mean "an example to be followed" and then to signify a "research hypothesis": "The research objective has been defined as the evaluation of an innovative model of the Ministry of Health for reorganizing the basic rural health services before generalisation on a national level, or more precisely: testing the hypothesis that a model can be applied operationally, which should lead to a quantitative and qualitative improvement of services which are then better utilized (output) and which ultimately lead to a selective improved level of health of the population in the catchment area (impact)" (*ibid.*, p. 46). It seems to us that the model can be argued to have two roles in action research: it forms part both of the research methods (instrumental role of the model) and of the scientific results of the research (transferability of the model, *corrected by the action research process itself,* to other situations). In both cases, however, the model remains a conceptual representation of the system and not a specific experience to be replicated.

Thirdly, our most basic criticism of this study is that within an action research approach it seeks at all costs to preserve the experimental paradigm of conventional research, in the form of a random controlled test. Let us make it quite clear: HSR can take the form of a comparison between an intervention zone and a control zone. But is this the most useful form? Are these the most relevant terms in which to formulate the research hypothesis?

In reply, we would first of all ask if it was even worth formulating the basic hypothesis, which we would express as: "improved provision of services is preferable, in terms of population health, to the status quo". Is there not already enough documentary evidence elsewhere to be able to answer "yes, it is better to improve health care than do nothing"?

And if anyone doubts this, will they obtain the answer by comparing two zones in accordance with the conventional research paradigm? Given that we are dealing with a *system*, a criss-crossing of multiple elements acting and reacting with each other, should the research method adopt an approach whose basic rationale is to isolate the subject under investigation as far as possible from its context? And does the study in question not show that, to meet this objection, it has to include in its analysis a large number (too many to handle?) of components of action, measurable outputs and means of measurement?

But we can go further than that. Supposing the study in question shows that the intervention is in fact ineffective: there is *no* difference between the intervention zone and the control zone. What deductions can be made by the researcher on the one hand and the political decision-maker on the other? The researcher, if he takes a critical approach to his own work, could always claim that he had not "controlled" all the variables, that elements "beyond the reach" of the study (i.e. outside the model used) had intervened, that complicating factors had obscured the comparability of the two zones, etc. In short, he will conclude that his protocol was either insufficiently rigorous or underdeveloped, or completely inappropriate for the problem addressed. As for the political decision-maker, will he conclude from the lack of difference between the two research zones that nothing at all need be done? That there is really no need to improve services? Of course not.

Suffice it to say that the problem has been ill-defined if a system is studied using a research method inspired by conventional research. Conventional research neglects the history of the system, its internal dynamics and its potential; in short, it fails to position the subject under investigation within an evolving socio-historical context. Conventional research is obliged to freeze the system at one or more specific points in time in order to conduct an abstract analysis of it. The health system in Burkina Faso, as it exists at present, is the result of historical development, financial constraints, power struggles and social tensions. Is the problem one of knowing whether it *should be* improved - and to objectify in advance the measurable impact of some measure or other - or of knowing *how* to improve it and thus introducing an active change based on a model, where action alone will allow judgement to be passed on its relevance?

There is one final element which we feel must be stressed. In their concern to distinguish "intervention" from "research", the authors of this study entrusted the research side to parties with as little connection as possible to the field of action (university students, ministerial departments, foreign research institutes, etc.). But does this not effectively deny the very nature of action research, which binds indissolubly action *and* research, and constitute an act of self-delusion about the *mere* role of observers played by such parties? Let us assume the study shows it would in fact be better to make changes to the health system than to preserve the status quo. How is one to generalise the conclusions of such research? The changes will have been *made and observed* by parties as far removed from the action as possible. The very model for this "action research" is therefore bound to contain this methodological dimension. If the model is to be generalised to the rest of the country, will the intervention of the GTZ, the University of Heidelberg, ORSTOM, etc. also be required in all the other districts, given that they played a full input role (as players) in the basic model and were not mere observers? Is the transferability of the model not compromised by inclusion in a research method which draws too heavily on conventional research?

We fear that this type of research, which sets out to appear rigorous in a desperate attempt to achieve experimental objectivity, only ends up discrediting the whole HSR approach and rekindling the spurious debate between *hard* and *soft* research, to the detriment of HSR. Why could these researchers not accept the fact that they were carrying out action research and why did they not shake off the "complexes" of the Western scientific paradigm - the experimental approach - while maintaining a control zone if they so desired? Behind this research protocol looms the intimidating presence of the reference model imposed by conventional research, even in innovative approaches and in matters which are *a priori* quite relevant.[3]

It has been claimed (Delruelle-Vosswinkel, 1981) that action research cannot represent a new paradigm for the social sciences, that the difference between conventional research and action research is more one of degree than of kind and that a slant towards practical application is not a characteristic peculiar to action research alone. It seems to us, however, that the study we have just looked at clearly illustrates the difference in kind between action research and conventional research. Increasing the number of conventional research studies *within the framework* of a given health system is not enough to turn it into research *into* that system.

This project subsequently abandoned the term "action research" in favour of "applied research". [4] But that has no fundamental bearing on whether or not it is HSR. The fact is that, in practice, the "control zone" could not be maintained but was "contaminated" by changes which should have been reserved for the intervention zone. For want of a research methodology explicitly open to change in the system and not based on the experimental model consisting of comparison between an intervention zone and a control zone, the researchers, by their own admission, are now reduced to falling back on multivariate analyses if they are to continue to

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[3] We must emphasise that we are criticising a *research protocol*, as *published* in an *article* at a *particular moment* (1992), not a project in the field and even less so the authors or players. We are using this article for purely didactic purposes.
[4] Applied research project for improvement of health care. Summary of results and prospects. Discussion paper for the inter-phase conference on 4-7 February 1997. Nouna, Tougan, Ouagadougou, Heidelberg. Duplicated, undated, provisional unpublished document.

analyse the situation - a development unforeseen in the original protocol. This escape route into refinement of the statistical approach is, in our view, a vivid illustration of conventional research ill-suited to its subject and running into a dead end.

Meanwhile, the project has produced several very interesting findings (all of them the fruit of the conventional research approach, however), some of which have been published (Sauerborn *et al.*, 1995; Sauerborn *et al.*, 1996): health survey, recording of vital events, verbal autopsy, survey of households, observation of care, and case control studies on maternal mortality. Three studies are proposed as focus groups on strategies for adaptation to the economic costs of illnesses, the perception of acute respiratory infections and maternity risks.

Like the AR on the women of Kinshasa, the project offered various opportunities to cast the research in a different form, with sufficient material to formulate hypotheses for testing within an open-ended conceptual model, as proposed here, and yet this methodological approach was never chosen. Nevertheless, some members of the team are aware of the inevitable systems dimension of such a field, judging from their thinking on the subject, published, significantly, as separate, general ideas which are certainly of interest but of no direct application to the research just described (Reerink and Sauerborn, 1996). This general line of thought could well provide the basis for modelling the health system covered by their research in Burkina Faso.

Health coverage plan in Zimbabwe: example of the grey area between "scientific management" (called "action research") and HSR

Health infrastructure planning in the Murewa district in Zimbabwe provides a final real-life example of HSR as a misnomer - this time a rigorous planning scheme presented as action research (Criel *et al.*, 1996).

This project, supported by Medicus Mundi Belgium, financed by the European Union and with scientific monitoring by the Tropical Medicine Institute in Antwerp, tackled a fairly classic case of district health coverage, for which a rigorously thought-out solution was found which could be classed as what we earlier called "scientific management" of the health system. It could also have been HSR but was not given sufficient means. The article is misleading since it is entitled “action research”, claims to use a model and supposedly follows a “systems approach”, though this is not always very clear from the text.

In short, in response to the usual problem of insufficient and irregular use of rural health infrastructure, overcrowding of the district hospital by primary health care cases and the sudden need to put health centres in charge of a vertical programme based on a mobile team previously funded by outside sources, the Murewa district authorities opted for a “rational” coverage plan giving the entire population easy access to health centres, which entailed moving some of these and setting up a new health centre in the urban centre of the district.

We would criticise the presentation of this project on the following grounds:

1. It is not action research: there is no sign of constant active participation (beyond information and consultation) by clearly identified players in the system.
2. There is insufficient explicit reference to a model: the only mention of a model is when the authors argue, in passing, that it is difficult to solve the overcrowding at the district hospital without knowing which tasks are to be attributed to the hospital (to dispense or support primary health care). This is of course true, but if it was central to the approach in the field, it should have been at the heart of the reasoning and of the article. Only a very experienced reader could deduce the model used from what we are told.
3. No hypothesis is tested: the article explains how the new coverage plan was decided but not whether it is operational nor how the model and action were changed in response to the decision. It would be understandable if the testing of the hypothesis were not yet described in the article (if the action were still in progress) but readers should at least be given details enabling them to assess the content and context of the hypothesis or hypotheses. However, the central section of the text is unclear. For example, it mentions phasing in the coverage plan (p. 702) without explaining clearly how this was decided, why, by whom and to what end (in other words, which hypothesis was under examination). The same questions must be asked of the staff’s motivation to set up an

urban health centre (p. 706).

4. It fails to capitalise on the systems approach: the reader guesses that various components forming a system have been taken into account, and worked into a model, but the danger is that the failure to explain the system or the model clearly could leave the reader with the impression that the systems approach has been confused with the need to combine measures in various fields, as is often the case with management of a complex issue. Taking account of the history of the health infrastructure since independence and of its interrelation with the road system is not enough to warrant talk of a systems approach. If such information is given without explaining the system analysed and the model tested, readers will see it as no more than the usual components of the sort of situational analysis found in any well-conceived plan.

Under the guise of action research and using jargon very similar to that proposed here - even to the extent of referring to some of the authors we have quoted - this article seems to have missed an opportunity to show how to move from a "non-system" (or, rather, since there is always a system behind such a situation, from a non-operational system developed on the basis of unconnected criteria which vary in time and space) to a system consciously organised around an explicit model, and to system-oriented decision-making. Readers who fail to grasp this dimension could be left with the impression that this is merely a "scientific" approach to rigorous, methodical management of a district - which is by no means negligible and could certainly be just as inspiring as the articles on the women of Kinshasa or the health system in Burkina Faso, but can in no way be called action research and, as portrayed, fails to display sufficiently clear characteristics of *research* into a health system. In practice, we are in the inevitable grey area between HSR and rigorous health system management.

Notional project

A notional example quoted by Taylor (*op. cit.*, pp. 8-9 and 38-41) asked whether, in a developing country, the use of oral rehydration sachets to combat infantile diarrhoea was preferable to using sugar- and salt-based solutions prepared directly by the mother. It was decided to conduct research in three zones of 50 000 inhabitants each which were basically comparable in terms of the variables influencing diarrhoea and dehydration. In the first zone, families received five sachets of oral rehydration salts before the period of exacerbated diarrhoea and a nursing assistant renewed this supply by visiting the home every three months, when he/she would collect data on bouts of diarrhoea, use of the sachets and mortality. The same home visits were carried out in the second zone, the difference being that the health worker checked for the presence of sugar and salt and demonstrated how to use them in the event of diarrhoea. The third zone served as the control zone. A local group of researchers from a school of public health was put in charge of developing a monitoring and information system, controlling data quality and conducting the final analysis. The main criteria for assessing the outcome of each case were infant mortality, the length of the bout of diarrhoea and the decision on whether or not to treat it through rehydration, with the reasons for doing so being given in each instance (*ibid.*, p. 40).

In our opinion, this research falls much more within the scope of conventional research than health systems research. Studies are carried out - quite legitimately - into two methods of dealing with a given health problem. The methodology is strict and the results could probably be generalised, at least within the country concerned. But what account is taken of the health *system*? It is not enough just to mobilise a large number of players *from* this system (school of public health, health centres and their pharmacies, district nurses, mothers, etc.) for this to become research *into* the health system.

As for the model applied, it is a reductionist model (as it must be) of experimental research: it isolates as far as possible the "means of rehydration" input, and the three outputs: "decision to rehydrate, morbidity, mortality". It is interesting to note that the author had, in passing, identified the following additional parameters to be measured: the safety of the method, its acceptability, its cost, and the relations between the other variables - e.g. was the advantage of the slightly lesser risk of sodium overdoses from sachets of electrolytes not cancelled out by the danger of stocks running out, particularly in the poorest households where the problem posed by diarrhoea was precisely at its most acute? (*ibid.*, p. 39). In fact, the author was here proposing a list of questions which could have led to the formulation of a research model encompassing a group of health system components. His approach could have produced interesting operational research or action research. But he seems to have been “held back” by his own notional characters, presumably the epidemiologists from the school of public health, and in the course of the investigative procedure he forgot about the health system!

HSR and qualitative research

Readers will surely have been struck by the similarities between the approach proposed here and qualitative research in social sciences. This vast subject has been back in favour for the last twenty years, sometimes unreservedly staking its claim as applied research, hitherto under-rated (cf. special edition of *Social Science and Medicine* Vol. 41, No 12, 1995). Often the debate is narrowed down to:

- the *validity criteria* specific to qualitative research. Either validity criteria other than those for quantitative research are sought or the correlations between the validity criteria for quantitative and qualitative research are sought, for example by proposing credibility in place of internal validity (Yach, 1992; Mucchielli, 1996);
- the *participatory nature* of this research, and possibly its political implications (Smith, *op. cit.*; Cornwall and Jewkes, 1995).

While not denying the importance of these issues, we only wish to discuss a few specific points here.

The danger of indicators

If comparable, reproducible results are desired, it is always better for research to be quantitative. Quantitative results always hold an advantage over - or, in any event, are more persuasive than - qualitative research results.

That said, qualitative approaches are sometimes the only possible solution for complex systems. It is true that the advantages of quantitative research are lost, but to deny all scientific value to anything unquantifiable in effect means excluding a large proportion of human activity from the rational systems approach.

So while necessary, but nonetheless limited in its methods (comparability, reproducibility), qualitative research sometimes succumbs to the temptation of *indicators*. As their name suggests, though, however sensitive, specific and relevant they are in theory, indicators never give more than an *indication* (approximate figures) for elements in the system which are not directly quantifiable. The danger is that an indicator could be confused with real measurement. If the indicator is understood as an attempt to make non-quantifiable components comparable, it creates the illusion of quantification and many people could take it purely and simply as a real measurement. By their very make-up, however, indicators always include a subjective element, all too often forgotten. In a value system where quantification is considered intrinsically better, use of indicators carries the inherent risk of a complete misunderstanding of their significance.

To take one example from primary health care strategy, which by nature lends itself badly to a purely quantitative approach, let us consider integration of a vertical programme into routine activities. Operational research could very well attempt to quantify all aspects of this integration. But OR is limited to human labour, human resources. The researcher might therefore wish to quantify this aspect as two measurable parameters: (i) staff skills, which would decide the staff's tasks in the system and, ultimately, the expected output (measurable parameter); (ii) staff salaries, which would affect staff motivation and provide a defined input (measurable parameter). In a model such as this, human resources will therefore be condensed into two measurable quantities. Such a model could conceivably be applied in situations where there is a clearly-defined relationship between input and output, relatively independent of human factors, for example in a factory - and even then not always! - or in a team of surgeons using advanced technology, or for a laboratory service involving standardised examinations calling for specific skills, or even for vertical programmes with clearly defined output. But a quantitative approach like this becomes misleading when the relational aspects outweigh the technical.

In the case of primary health care, two aspects in particular make this approach hazardous. First, the variability of the return on human resources: contrary to the OR researcher's model, skilled staff are sometimes unproductive if motivation is lacking and, conversely, staff who, at first sight, are relatively unskilled can offer high productivity unexpected by the model. In other words, there is no directly measurable correlation between skill levels and expected output. An AR (qualitative) approach would be more appropriate for evaluating this aspect than an OR (quantitative) approach.

Second, integration of activities: the more integrated the activities, the more synergies are sought, making it difficult to single out a simple, direct relationship between inputs and outputs. For example, communication (empathy) between doctor and patient is supposed to foster a certain investment in preventive medicine, but it is difficult to measure this effect in terms of input/output. What does recourse to indicators mean in a situation such as this? Not that the researchers (or system managers) are less aware that the results of qualitative research are less comparable than those of quantitative research, but that there are non-measurable positive effects of which only indications can be obtained. Successful integration of health care activities means that the staff accept this integration and benefit from it psychologically, in the form of job satisfaction and feeling more able to perform their tasks. When staff say "This is my job and I am pleased that specialists [vertical programmes] are helping me to do it", that is a sign of successful integration. But this aspect of primary health care is impossible to measure, and so inevitably more open to discussion. Indicators are the only means of approaching it. To return to our example of integration of tuberculosis treatment, an indicator of regular patient attendance for treatment will give an "indication" (but not a measurement!) of successful integration of tuberculosis control. The same indicator will also signal the availability of health care staff willing to integrate activities under conditions good enough to ensure the continuity of health care. But regularity of patients will never be a measure of the integration or success of the tuberculosis control programme. This is what we mean by the danger of indicators in situations not lending themselves to quantification.

Lack of modelling

We also wish to stress one key difference between a certain type of qualitative research, which considers that there is always something to learn from particular cases, and our approach to HSR: the obligation, in our view, to proceed via a model. As we see it, much qualitative research remains at the level of edifying stories which give food for thought but without necessarily providing all the keys. Disdained for too long, but nowadays perhaps sometimes over-rated in certain circles, qualitative research lacks the means for adequate generalisation. It is not enough to affirm that "one can learn from particular cases" (Criel *et al.*, *op. cit.*, p. 708). In so saying, these authors refer to Barker who, in a stimulating article with which we fully agree, also states, without saying how, that "One can learn from the particular, as well as from the generalisation" (Barker, 1995). It must be realised that, without *any* possibility of generalisation, it is indeed difficult to use the particular. Without recourse to a *transferable model*, how is it possible to learn from the qualitative and local and to advance beyond the anecdote ?

When Rifkin (1995, p. 1653) mentions the difficulties faced by managers in using qualitative research "to translate findings into broad solutions for common problems" yet goes no further than that, is it not precisely the lack of reference to a *model* which is being stressed, subconsciously, as the weakness of certain qualitative research, which we would readily call flesh with no backbone?

To put the quotation from Barker in context: "Much of the applied research which would be useful to the health manager is context-specific, and does not seek to test generalisation nor to produce hypotheses of general application. For example, one might imagine that the problem confronting a manager is represented by the question 'Why do sick people avoid going to hospital X for treatment, even when they live close to it?' Management problems do not come conveniently attached to a hypothesis to be tested; an inductive approach may well be the appropriate one. The answer to this question will not of itself offer new theories, although it may well provide a story of great interest to other health service managers. One can learn from the particular, as well as from the generalisation" (*op. cit.*, p. 1659). We would not hesitate to say that if we have an inductive approach, we are dealing with scientific management of services. But if we wish to carry out research, and thus attempt a degree of generalisation, we cannot see how to open up qualitative research to generalisation without reference to a model.

We see this as a dividing line between some forms of qualitative research and the approach proposed here: our approach absolutely requires modelling. It is not from the particular anecdote that lessons are learnt, but from local attempts to retest the model behind the anecdote. And even then, this model and its context, i.e. the conditions governing its validity, must also be explicit.

In this light, one might analyse the terrible failure of community health workers (who in many countries have all too often constituted the thrust of a misunderstood primary health care policy) as the uncritical replication of qualitative approaches applicable to a limited time and place and which failed to define their context adequately. A *model has been copied* (in the sense of following an example or a general formula) without taking the trouble to *verify the model* (in the sense of a set of hypotheses derived from a particular context, the validity of which has still to be tested elsewhere).

In essence, well before the resurgence of qualitative approaches in the human sciences, von Bertalanffy foresaw the role of models in such situations: "A general theory of systems would be a useful tool providing, on the one hand, models that can be used in, and transferred to, different fields, and safeguarding, on the other hand, from vague analogies which often have marred the progress in these fields" (von Bertalanffy, *op. cit.*, p. 33).

Our purpose here is to prevent HSR, when it cannot take the form of quantifiable results, from becoming the ultimately unenlightening "vague analogy" feared by von Bertalanffy.

In OR the model clearly lies at the heart of the research. In AR, this is already less clear-cut. In the vaguer forms of qualitative research, reference to a model can disappear altogether, as we demonstrated in our case studies. The unique feature of our approach, with regard to qualitative HSR, is the uncompromising demand for reference to a model, as a precondition for generalisation.

HSR and politics

We acknowledge that certain kinds of HSR, when it takes the form of AR and emphasises participation and empowerment, are close to the limits of what researchers can allow themselves in terms of engagement and to the limit of what research results may express in the political field.

The fact that long-repressed human potential, held back by circumstances, can suddenly be released by participatory HSR is something else the researcher must be ready to assume responsibility for: the research is politically charged.

But we would warn against politicisation of participatory HSR: its ability to release human potential should not lead to a sort of militancy which, in the final analysis, would rob the research of its impact and credibility.

Conclusions

Other authors, reflecting on the course of epidemiology over the past century, have expressed their desire for this discipline to enter a new era and embrace both the molecule and the health system (Susser and Susser, 1996; Pearce, 1996). It is from a similar viewpoint that we would state the case for a new dynamism in the field of public health which would give an explicitly systems dimension to research.

Our paper reflects a normative approach which seeks to show the interest or relevance for health systems research of conventional research, operational research and action research. Admittedly, even after trying to specify what contribution these three types of research might make to HSR, we have to concede that there remains a "grey area" between what exactly is or is not HSR, though the debate about where exactly the boundary lies within this grey area between HSR and the rest of research only needs to be settled where funding or specific institutions exist for one or other type of research.

What is more important, it seems to us, is to know what type of research is useful in the "health sciences" today. The last fifty years have seen a certain type of research, which we may categorise as conventional, enjoy remarkable success. "Scientific" research and "conventional" research have become synonymous in the minds of many, as if other types of research could not also be scientific.

One might well wonder, however, about the benefits this vast body of ever more advanced research actually brings in terms of health policies, improvements to health systems and, finally, improvements in the state of health of the population and individuals. This research is perpetuated, and continues to attract funding, because its methodology is well known and its subject easily defined. But is it not also facing a "law of diminishing returns" which, as in economics, should prompt us to think about changing the combination of production factors or indeed changing markets? The closer the researcher comes to the social and human sciences, the more quickly he comes up against the shortcomings of conventional research. And if he continues to use it, he becomes more and more aware of its limitations and seeks to overcome them. Once health sciences research goes beyond the measurable, whether at cellular or population level, it very soon comes up against the limits of conventional research. So should more attention not be paid to the virtually untapped reserves of health systems research? We would say the answer is yes, provided more care is taken in choosing which studies are to bear this name.

The vagueness which seems to beset health systems research derives from the lack of a specific methodology and an easily definable subject. This problem can be overcome if, as proposed here, more detailed thought is given to the potential contribution which conventional research, operational research and action research could make to health systems research. It is also important that research which casts off the shackles of the positivist approach, and the experimental model in particular, should not lay itself open to the reproach of drifting aimlessly in the seas of *soft* research. If action research is to be a fruitful addition to health systems research, it has to be given a strict methodology. This, for us, entails the use and specification of a *model*, both as a research tool and as the transferable product of research, transcending local circumstances.

HSR also provides an opportunity for collaboration between conventional researchers and system managers. It offers a form of integration between two worlds ultimately perceived as different, with practitioners increasingly viewing conventional researchers as theorists of little help in the scientific management of the health system and researchers disdaining practitioners as incapable of theorising. As Barker says of action research: "It is probably the case that health service managers get on with problem solving in their own way, but do not see this activity as related to formal research, which is often something other people do ... What is needed is a research process which takes seriously a problem-solving approach to health services management, and which helps the manager to see that there is not only a continuum between more formal research processes and action research, but also that there is a continuum in the opposite direction between action research and other decision-making tools" (*op. cit.*, p. 1664).

In so saying, we are well aware that, funded by countries in the northern hemisphere, conventional medical research in all its forms has focused mainly on pathologies which affect the so-called affluent societies. The developing countries certainly still have a lot to gain from conventional research, from basic research right through to therapeutic trials on new molecules. Right now, however, in both North and South, a vast field of research lies fallow - research into health systems and their performance. We can expect great things of a vaccine against malaria or a better treatment for cancer, but there is no doubt that we can expect just as much from the improved operation of health systems faced the world over with problems of choice, costs, results and the management of change.

Finally, since most research is carried out in affluent countries, a *de facto* balance is struck there between research and health systems. New knowledge is integrated if not spontaneously (cf. the debate on evidence-based medicine) at least more easily into the health systems of industrialised countries. Health systems in developing countries do not generate the bulk of research work. It is more difficult for these health systems to integrate research carried out elsewhere. HSR, building a methodological bridge between research and practice, between theorists and fieldworkers, provides health systems in developing countries in particular with a means of evolving towards a situation which will allow easier integration of research.

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