knowledge (dis)appearance

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Trefwoorden: Knowledge (Dis)Appearance

CHAPTER 1

IS SCIENCE ACCUMULATIVE?

W.A. Verloren van Themaat

The dispute, whether there are objective criteria for truth, is often conflated with the question, whether science always progresses. Popper in "Objective Knowledge" strongly suggests a general trend of science to progress. In the sequel it is shown, that knowledge can be lost: 1) because it is not recorded; 2) because the documents in which it is stored are lost; 3) because the knowledge of the language of the documents in which it is stored is lost; 4) because the later scientific community does not pay due attention to the documents in which it is stored.

1.1. INTRODUCTION

Loss of unrecorded knowledge is utterly common. Loss of published documents was especially frequent before the invention of the printing technique and has meant a regress especially in some branches of humanities, because either documents constituting the object of these sciences (e.g. in literary history), or records of historical events were lost. Loss of published documents after the invention of the printing technique has been somewhat less important, but has still occurred. Loss of unpublished documents has been very frequent, both before and after the invention of the printing technique. The definitivity of the loss of knowledge in undeciphered documents is somewhat doubtful. Some undeciphered languages may be deciphered. But some undeciphered languages may also remain undeciphered for ever. The loss of knowledge by insufficient attention to the work of an earlier scientist can be repaired, if one still examines his assertions. Loss of knowledge occurs chiefly in humanities. In mathematics, resp. natural sciences there is almost only gain in knowledge by acceptance of proofs, resp. by the reproducibility of the experiments.

1.2. OBJECTIVE KNOWLEDGE AND THE PROGRESS OF KNOWLEDGE

In the discussion between the more absolutistic philosophy of science - which usually does not believe in the possiblity of absolutely attaining truth, except in trivial cases, but believes in the existence of objective criteria for nearness to truth, exemplified by neopostivism, Popper and Lakatos - and the relativistic philosophy of science -which sees all scientific theories as equally subjective projections determined by the cultural backgrounds of the concerned scientists, exemplified by Feyerabend and the relativistic interpretation of Kuhn (1957), to which Kuhn himself does not seem to subscribe - the relativists often reproach the absolutists "Whiggism", that is the belief that science always and in all respects progresses, that the present scientists know everything better than the scientists of the past. The standards of rationality have so often changed in the history of science and philosophy, that the claim of the existence of objective criteria of truth or nearness to truth can in practice mean nothing else than the absolutizing of the working methods of present science or some contemporary philosophy of science.

One of the best expositions of the absolutistic philosophy of science is Popper's "Objective Knowledge" (1972). Now about the question, whether the present scientists know everything better than the scientists of the past, Popper is somewhat ambiguous.

On one hand Popper (1972) states many expressions of his belief in the progress of science. "The fundamental problem of the theory of knowledge is the clarification and investigation of the process by which, it is here claimed, our theories may grow or progress." (p. 35; but pay attention to the word <u>may</u>). "The realistic procedure implies success in the sense that our conjectural theories tend progressively to come nearer to truth." (p. 40). "Epistemology becomes, from an objective point of view, the theory of the growth of knowledge." (p. 142; here again, he does not speak of the possible <u>decay</u> of knowledge). "It explains why we can act upon it (the third world of objective knowledge) and add to it or help its growth." (p. 161; here again he does not speak of the possible <u>destruction</u> of knowledge). At p. 163 he speaks of "Final state of understanding of the third world" (and in the case that the documents containing it are afterwards destroyed?). "For in contrast to most other human activities - art and music in particular - there really is, in science, such a thing as progress." (p. 356).

On the other hand Popper (1982) contains some passages with at least hint at the possibility of destruction of knowledge. On p. 108 he puts the thought experiment of the destruction of all our libraries. On p. 185 he speaks of the reconstruction of a damaged text.

But in general Popper has insufficiently reflected on the material conditions of the preservation of knowledge. A minor point is that he is somewhat too bound to the Western culture with its methods of preserving knowledge. On p. 239 he writes: "Instead of growing better memories and brains, we grow paper, pencils, typewriters, dictaphones, the printing press, and libraries." I do not deny that writing and printing are the most efficient means for the preservation of extensive bodies of knowledge (probably even Einstein himself did not know his complete works by heart. Moreover, a too great emphasis on memorizing by scholars takes away from their time for thinking), but yet, that it is the only possible means for the preservation of knowledge. In some non-Western cultures large texts are stored in the scholars' memories and orally transmitted from teacher to pupil. In India, e.g., the very extensive Vedas were orally transmitted for centuries before they were written down and until the present day are many persons who know the Koran by heart.

Very generally it can be said, that a necessary condition for the preservation of a piece of knowledge is the preservation of at least one exemplar of a document in which it is stored or the existence of at least one person who knows this piece of knowledge by heart. Negatively said: only if all written or printed exemplars of the Koran were destroyed and all persons knowing the Koran by heart were killed the Koran would be destroyed definitively (if the first condition were fulfilled but the second not, it would take relatively little time to restore the Koran with the aid of the memories of some of the persons knowing it by heart).

For the preservation of knowledge in the form of documents it is moreover necessary, that mankind be able to understand the documents. "A book need merely to be able to be understood." (Popper, 1972; p. 116). "Third-world structural units are intelligibles, that is possible (or virtual) objects of our understanding." (Popper, 1972; p. 166). Now there are preserved many documents in undeciphered languages, e.g. Etruscan and the Indus Valley Script. So long as these languages are not deciphered, all information contained in the documents written in them is inaccessible to us.

Finally, a piece of knowledge stored in a document which can be understood may be lost because the later scientific community fails to pay due attention to it.

Summarizing: a piece of knowledge can be lost in four ways:

(1) Because it was only orally transmitted and stored in the memories of some persons, and all persons who knew it are dead;
(2) Because it was stored in documents and all exemplars of the documents in which it was stored are lost;
(3) Because it is stored in documents which are undeciphered and perhaps undecipherable;
(4) Because the later scientific community does not pay due attention to the document in which it is stored.

Some other authors of this book have moreover drawn attention to the cases in which scientific information, though present in the form of publications, e.g. in some libraries, remains unnoticed in the torrent of scientific publications. In the sequel we shall not pay attention to the question in how far scientific information is utilized and consider all knowledge at least accessible in the form of documents and not explicitly refuted by the scientific community.

1.3. LOSS OF UNRECORDED KNOWLEDGE

Loss of unrecorded knowledge is such a common phenomenon, as well in preliterate societies as in literate societies before the invention of the printing technique as after the invention of the printing technique, that it would be useless to multiply the examples. And it is by no means only trivial information which is lost in this way. Modern linguists have complained, that the Romans and Greeks permitted so many languages in their neighbourhood to perish unrecorded.

1.4. LOSS OF DOCUMENTS

Loss of documents is also a very common phenomenon, especially before the invention of the printing technique. Of the works of the pre-Socratic Greek philosophers only fragments have been preserved, and of the earliest ones only very small fragments. Now for the earliest of these philosophers this has only importance for the history of philosophy, not for systematic philosophy, since certainly no modern philosopher would ever think of becoming an adept of Thales, Anaximander, Anaximenes or Parmenides, even if their works had been preserved integrally. But a historian of philosophy in antiquity, when these texts, or larger fragments of them were still extant, could have a far more complete knowledge of pre-Socratic philosophy than we.

But the doctrines of the sophists, with their sceptic epistemology, agnosticism concerning the gods (both to be found in Protagoras) and their moral of the right of the strongest (Callicles) have also a certain systematic philosophical interest, since views, if not identical, at least very similar, are held by many modern philosophers. Here at least for a part of our knowledge it has occurred, what Popper (1972; p. 108) describes:

> "As before, machines and tools are destroyed, and our subjective learning, including our subjective knowledge of machines and tools, and how to use them. But this time, all libraries are destroyed also, so that our capacity to learn from books becomes useless ... For in the second case there will be no re-emergence of our civilization for many millennia."

Of course, the effect of the destruction of (a large part of) the writings of the sophists has not had such a disastrous effect, not even on the development of later scepticism. Since at least fragments of their works have survived, as well as indirect information about their teachings, and may have inspired later congenial thinkers.

Also afterwards many valuable published scientific works were lost. E.g. a team under the leading of Aristotle compiled a list of descriptions of the constitutions of 158 cities, from which only the State of Athens has been refound and the rest has been lost, except some mostly very short fragments. Though from other sources at least something is known about the constitutions of the Greek city-states, an ancient scholar, working when Aristotle's description of the constitutions of 158 cities was still completely extant, must have been far better informed about the constitutions of the ancient Greek city-states than any modern historian.

Of great importance for science is also the loss of literary texts, constituting the object of literary history. E.g. the Greek tragedians composed their tragedies in the form of tetralogies of three tragedies and one satyr play. Now among all Greek tetralogies only one trilogy (the three tragedies), Aischylos' "Oresty", has survived integrally, and of all satyr plays only Euripides' "Cyclops" and the strongly deviant satyr play "Alkestis" by Euripides have been preserved. Moreover, we have good reasons to suppose, that Euripides' "Cyclops" was by no means the best representative of its genre. So an ancient Greek philologist of the tragedies and satyr plays, working when all these tragedies and satyr plays were still extant, was in a considerably more favorable position to approximate the truth than a modern philologist of the same topic.

Perhaps writings exposing dissident views are especially exposed to the danger of being lost. For Antiquity that held especially for the anti-slavery movement and feminism. Finley (1980; p. 120):

> "Aristotle began his analysis of slavery on a polemical note that implied the existence of writings which denied that slavery was a natural institution. He did not name his opponents and they are hard to find in the surviving literature; a case has recently been made out that Euripides was one."

The ancient anti-slavery writings may have been destroyed intentionally by the establishment for their subversiveness, but it should not be forgotten that the abolitionists were a small minority in antiquity. Their writings may as well have disappeared by mere chance together with e.g. a great part of the writings of Aristotle.

Whether the same held for ancient feminism is doubtful. At least one book, though written by a man, even according to modern standards very progressive on the position of woman, has been preserved: Plato's Republic. And the emancipation of woman did not remain mere theory: in the Hellenistic age in some Greek cities women acquired political rights and held public offices (Pomeroy, 1975; p. 126).

Closely similar to the case, that former scientists had access to written or printed documents, to which we do no longer have access, is the case, that they had access to data, to which we no longer have access. Of this too there are examples, and examples of very interesting data too. E.g. Polygnotus, who lived in the 5th century B.C., was according to ancient writers the greatest painter of ancient Greece.

> "Both from his direct influence and from the opinions of the ancient critics we can conclude that he was the greatest painter the Greeks had ever had." (Levi, 1980; p. 114).

But none of his pictures has been preserved. In such a case a modern art historian can do nothing better than citing and compiling the statements about the work of Polygnotus by ancient authors and is not in a position to critically assess their opinions about Polygnotus or to improve them. Levi (1980; p. 148) says about him:

> "Detailed descriptions of his compositions and their shocking effect on the work of his contemporaries, however, make research senseful. He brought great innovations: he did not use the base line in design, rendering tremendous events with few dramatic details, put things into space and suggested rest and comfort of movement in short, a new kind of narrative painter."

Since the invention of the printing technique the chances of preservation of published documents are considerably better. Especially the top works of mainstream science are printed in so many exemplars that their chance of being lost is very small.

The loss of manuscript documents, however, has been a very common phenomenon, both before and after the invention of the printing technique. And some of these manuscripts would have been of great importance for science. The most famous example is Fermat's Last Theorem, about which Fermat wrote in a marginal note opposing a discussion of Pythagorean triples in his copy of Backet's Diophantus:

> "By contrast, it is impossible to separate a cube into two cubes, a fourth power into two fourth powers, or in general any power above the second into two powers of the same degree. I have found a truly marvelous proof of this theorem but this margin is too narrow to contain it."

Since Fermat, who lived in the 17th century, the greatest mathematicians have in vain attempted to prove Fermat's Last Theorem for three centuries. If Fermat did have a proof of his last theorem in the form of a manuscript, which was afterwards lost, this would be a case of loss of a document. If Fermat's proof of his last theorem existed only in his brain, this would be a case of loss of unrecorded knowledge.

It is a generally known fact, that the solution of a puzzle, if one knows that it has been solved by somebody else, requires considerably less intelligence than that of the original solver, unless the solution depends on factual information which the original solver had but the later aspirant-solver not (we do not speak here of such cases as the decipherment of dead languages, which may be desperate even for excellent linguists in spite of the fact that among the people speaking that language all persons able to read and write must have been able to read the texts in their language). For example, in chess reviews one regularly meets rubrics with positions from chess games with the question: "White (or Black) plays and wins." Now these positions are usually derived from the games of masters or grandmasters who succeeded in defeating their colleague masters or grandmasters by brilliant combinations. But considerably weaker chessplayers succeed in refinding these combinations, when they find such positions in chess rubrics with the indication: "White (or Black) plays and wins".

This fact may raise some doubt, whether Fermat truly had a proof of his Last Theorem. Was Fermat so far superior to all later mathematicians, that he was able to find a proof, which until now all later mathematicians have been unable to refind? That Fermat did not possess a proof of his Last Theorem, would be in agreement with the fact, that Fermat seldom gave demonstrations of his results (Benton, Encyclopaedia Britannica 7, page 235). On the other hand, that Fermat had a proof of his Last Theorem but did not publish it, would be in agreement with his general reluctance to publish: a supplement to the Veterum Geometria Promota issued by the mathematician Antoine de la Loubère in 1660 was Fermat's only mathematical work published in his lifetime. So we leave the question, whether Fermat had a proof of his Last Theorem, open.

Another example of lost manuscript documents, which is less illustrious, but has the advantage, that it is certain that the documents in question once did exist, is that of the interlinguistic manuscripts of the language constructor Julius Lott, which after his death were destroyed by his relatives, who did not understand their value. Julius Lott was a language constructor belonging to the naturalistic school, which primarily strives to similarity of the artificial language to the natural languages. In the 1890s he was one of the ablest adherents of this school. His Mundolingue was very similar to Interlingua, now the most widespread and important artificial language after Esperanto. If Lott had foreseen the possibility of the destruction of his interlinguistic manuscripts, he could have bequeathed them to Couturat (1903), the co-author of the standard work "Histoire de la langue universelle", or to Edgar De Wahl, another interlinguist with whom he corresponded. Lott's interlinguistic manuscripts would have been of great value for the history of the planned language movement.

A borderline case between the published and the manuscript documents is often manuscripts meeting with special difficulties in attempts to publication, sometimes for ideological reasons. Dr. H. Schröder has great difficulties in publishing "Déclaration des droits de la femme et de la citoyenne", a document from the French Revolution in 1791 in Paris. If documents exist only as manuscripts, they are exposed to special danger of destruction. On the other hand, so long as these documents exist, even if only as manuscripts, they are not definitively lost for science.

1.5. UNDECIPHERED DOCUMENTS

There are many undeciphered dead languages. On one hand linguists have been very ingenious in devising and applying various methods for the decipherment of dead languages. On the other hand some dead languages have remained very resistent against attempts at decipherment.

Friedrich in "Extinct Languages" (1975; pp. 151-152) distinguishes three cases of decipherment: that of an unknown language in a known script, that of a known language in an unknown script and that of an unknown language in an unknown script. Among these three cases of course the third is the most difficult. In some cases such languages were deciphered with the aid of bilingual texts, of which the most famous example is the decipherment of the Egyptian hieroglyphs by Champollion with the aid of the Rosetta Stone. Yet also without the aid of bilingual texts the decipherment of an unknown language in an unknown script proved to be possible in some cases, as the decipherment of Old Persian by Grotefend (strictly speaking the text on the basis of which Grotefend deciphered Old Persian was trilingual, but since the two other languages were undeciphered themselves, they were of no help). He corretly assumed, that the inscriptions were proclamations of Persian kings and used for his decipherment the lists of Persian kings known from other sources. After the determination of the phonetic value of the Old Persian letters the decipherment of Old Persian was completed by the etymological method, the comparison of the language with closely related languages, in this case Avestic and Sanskrit. This etymological method, however, is only applicable to closely related languages.

In mathematics there are proofs of undecidability, i.e. proofs of the impossibility of a proof of the truth or falsity of a given proposition in a given formalized theory. In linguistics there are no analogous proofs of the undecipherability of a given language on the basis of a given corpus of texts, since for the decipherment of a language there are often used clues outside the texts in that language.

On the possibility that a language will remain undeciphered for ever, Friedrich (1975; p. 151) is somewhat ambiguous:

> "The decipherment of any unknown script or language presupposes the availability of some clue or reference; nothing can be deciphered out of nothing. In those cases where one has absolutely no possibility available to link the unknown to something known, the

> amateur can give free rein to his imagination, but no real or lasting result can be accomplished."

But on pp. 156-157 he says:

> "It is, of course, especially difficult, and in many cases practically impossible, to decipher an unknown script without the aid of a bilingual text containing names. Many a failure to accomplish the decipherment of some writing is directly attributable to the lack of bilingual inscriptions or other references. But the possibility of deciphering a script without billingual texts, too, is demonstrated by the example of Hittite hieroglyphic writing. Thus, it might be more prudent to say when discussing a script which has defied all attempts to decipher it, that science has not yet been fortunate enough to discover a suitable point of departure for the decipherment."

On p. 172, however, he writes:

> "In any case, there is very little reason for hoping that we shall ever be able to reveal the meaning of these tablets of Easter Island."

So long as a dead language is undeciphered, all knowledge contained in its texts, e.g. chronicles about the history of the people which spoke that language, is inaccessible for us. And if for the sake of argument we assume that some dead language is not only undeciphered, but even undecipherable, then all knowledge contained in its texts is as irretrievably lost as if all texts in that language had been destroyed.

An entirely other case of undeciphered documents, far less important for the history of science, is that of the anagram, by which scientists established their priority for some discovery in the early modern time (Meadows, 1974; p. 57). But this method soon got in disuse. If a scientist claimed the priority for a discovery by an anagram and afterwards died before he could reveal the secret of the anagram, in this way a scientific discovery would be lost. But Meadows does not give an example of a scientific discovery lost in this way.

1.6. LOSS OF KNOWLEDGE BY INSUFFICIENT ATTENTION TO THE FINDINGS OF EARLIER SCIENTISTS

Further it is possible, that an earlier scientist found some truth, that the writing in which he communicated that truth has been preserved and can be read, but that later scientists find his communication too phantastic and therefore reject it, (Manquat, 1932; p. 88):

> "Aristotle had discovered a remarkable pecularity in the copulation of the Cephalopodes which was rediscovered only in the 19th century. "Some persons say that of the arms of the male polype which ends by two large cavities, bears a kind of penis comme nerveuse attached until the middle of the arm and that he makes enter it over all its length into the sucking mouth (palleal cavity) of the female.""

A far more important example of an initially not accepted scientific discovery is the heliocentrism of Aristarchus of Samos in the 3rd century B.C. According to Kuhn (1957; p. 42) "... some of these speculative suggestions (non-geocentric cosmologies; he does not explicitly say, whether this also held for Aristarchus' heliocentrism) gave rise to significany minority traditions in antiquity". But in this case the ancients had good reasons to believe, that the earth was at rest. As Kuhn explains (1957; pp. 42-44) the geocentric cosmology is far more in agreement with common sense. Even now, more than 400 centuries after Copernicus, we say in daily life: "The sun rises" and "The sun sets", not: "Our part of the earth turns toward the sun" and "Our part of the earth turns away from the sun". Only for an astronaut, looking back to the earth, it might be natural to say to a fellow-astronaut: "Now America disappears in the nightside of the earth".

Unlike in the case of loss of documents or undicipherable documents (if there are truly undecipherable languages) in this case the loss of scientific knowledge is not unreparable (at least, as far as reproducible phenomena are concerned). But how much remains to be done for the later scientists to establish the correctness of the views of the earlier scientists, depends on the character of the discovery. In the case of the copulation of the polype, which concerned one observational fact, the 19th-century scientists had to do nothing but to observe copulating polypes in order to see that Aristotle was right. But in the case of heliocentrism, which required a major reorganization of the scientific conceptual framework, Copernicus could draw some inspiration from Aristarchus, but Copernicus and his successors had to work hard to refute all arguments against heliocentrism from terrestrial physics and stellar astronomy.

1.7. IN WHICH SCIENCES IS THERE PROGRESS AND IN WHICH SCIENCES IS THERE REGRESS?

Every scientist works within a general conceptual framework (we owe this insight to Fleck and Kuhn), starts from certain empirical data (unless his science is a formal science, mathematics or formal logic) and elaborates them with the aid of mathematics and logic. Since there can hardly be science without argumentation, and this argumentation implicitly uses logic. Moreover, mathematics is used in all sciences, though sometimes only trivial mathematics, e.g. if a historian calculates the age of some person by subtracting the year of his birth from the year of his death.

So in any science these scientists, who work in the most adequate general conceptual framework, who have access to the most relevant data and who dispose of the most powerful mathematical apparatus (in so far as it is relevant for the problems with which they are occupied) have the best chance of coming near the truth.

The existence of different general conceptual frameworks in the history of science and the transitions between them, the so-called scientific revolutions, e.g. from Aristotelian to Newtonian physics, and from Newtonian physics to relativity theory and quantum mechanics, have sometimes been used for the justification of relativistic philosophies of science, as if between general conceptual frameworks, so-called paradigms, no rational choice were possible. Actually the scientific community has shifted to new paradigms on the basis of thoroughly rational arguments. It would lead too far to tell the history of the heliocentric and the relativistic quantum mechanical revolutions in great detail, but briefly they can be summarized as follows:

When Copernicus first launched his heliocentrism, it seemed to bring about a simplification of planetary astronomy, and to cause extra difficulties in terrestrial physics and stellar astronomy, because in a heliocentric universe the fixed stars would show parallax, unless they were very far and therefore had a very large real diameter. The latter argument was refuted by Galilei's telescopic observations, in which the fixed stars did not show a larger diameter than to the naked eye. And in the course of the seventeenth century, especially with Newton's universal gravitation theory, it turned out that also terrestrial phenomena were better accounted for in heliocentrism.

The transition to relativity theory was forced upon the scientific community by persistent anomalies in Newtonian physics, such as the perihelium recession of Mercury and the Michelson-Morley experiment (the latter's influence on Einstein is his initiation of the special relativity theory is doubtful, but it had a decisive influence in bringing the scientific community's acceptation of the special relativity theory). Relativity theory was confirmed by the inflection of a star's light observed during the total solar eclipse in 1919. Quantum mechanics was made necessary by the observation of the entirely new range of the sub-atomic phenomena.

In mathematics there has almost been only progress and no regress at least since the scientific revolution of the 17th century. The only lost piece of mathematical knowledge of which I am conscious is the proof of Fermat's Last Theorem. I have already expressed my doubts, whether Fermat did possess a proof of his last theorem. Moreover, Fermat's Last Theorem is rather unlikely to have importance for the empirical sciences.

So the only way in which in practice scientific knowledge can be lost is, because earlier scientists had access to empirical data, to which later scientists have no longer access. In the human sciences there are indeed many cases, exemplified before, in which documents are lost, either constituting themselves the topic of the science concerned (in the case of lost literary works) or containing information about situations and occurrences, which we cannot supply by independent observation (in the case of lost historical records), or in which earlier scientists disposed of art works, constituting the object of art history, which since have been lost.

In physics, chemistry and the rather young science of molecular biology, however, there is also net gain in the amount of relevant empirical data.

Since the aim of these sciences is not the accumulation of individual facts, but the discovery of the general laws, and we have good reason for assuming that these general laws always remain the same (or, in the case of molecular biology, so long as there is life on earth). The progress in these sciences does not rely on the preservation of all records of physical or chemical observations (indeed, the researchers in these fields, in they are not practising history of science, have very little interest for all but the most recent publications in their field), but, besides on the progress of the mathematical apparatus for the elaboration of their data, on the improvement of the accuracy of measurement and the improvement of experimental techniques, which enabled the scientists to observe matter under the most different conditions. The experiment of Michelson and Morly, which had to be able to registrate minutious effects of the supposed movement of the earth with respect to the light ether, was highly instrumental in bringing about the downfall of classical physics, and the liquefaction of helium, only possible under extreme circumstances, enabled the discovery of many important quantum mechanical effects.

For astronomy matters are somewhat different. There old observations preserve their value. E.g. the accumulating deviations of the astronomic seasons from those according to the Julian calendar, which necessitated the transition to the Gregorian calendar, were due to the difference between the tropical year and the year according to the Julian calendar, small for each year or period of four years (the period, in which one leap year occured according to the Julian calendar), but accumulating over the centuries.

In biology, e.g. Aristotle is known to have composed zoological and botanical works which have been lost. These works may have contained descriptions of plants and animals, which have since become extinct without leaving fossils, and about which we do not have information from other sources either. But this is merely speculation. It lies in the nature of the matter, that we can have only very general information about the content of lost documents!

1.8. CONCLUSION

The belief in the existence of objective criteria for truth is not identical with the belief, that scientists nowadays are better informed than in the past. In sciences, especially mathematics and natural sciences, where all three factors deciding on the nearness to truth, the general conceptual framework, the mathematical apparatus, and the emperical data, favor the present scientists, we should not say for the sake of false modesty, that the physics of Aristotle, of Newton and of Einstein are all three equally good or equally bad. On the other hand, in sciences, especially human sciences, in which earlier scientists had access to data to which we do not longer have access, we should be willing to admit, that they were better informed than we.

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CHAPTER 2

REAPPEARING KNOWLEDGE

M. Hetebrij and A. Nijssen

The lasting of results of social research may be a consequence of dominant political preferences and PR-qualities of the investigator, rather than a consequence of the obvious scientific importance of a research project. As a consequence, an increase is seen in the production of research on fashionable topics. Research projects are judged by an unspecified criterion of 'societal relevance' instead of a specified criterion of scientific value. Suggestions are developed on how to obtain accumulation in social research.

2.1. DISAPPEARING KNOWLEDGE: A PROBLEMDEFINITION

There are, as far as we can see, three ways in which the concept of "disappearing knowledge" is defined. In the first possible definition knowledge is seen as expressed in statements or predictions, which influence reality. The social, or behavioural, changes that follow, result in a situation in which the knowledge has lost its validity (the so-called self-fulfilling or self-denying prophecies). Of course, what really disappears is the situation described in the statement, or the situation described in the cp-clause accompanying the prediction.

The second possible definition of "disappearing knowledge" is a much more superficial one. It roughly states, that so much research is done that scientists are no longer able to digest the results of these projects. As a consequence, the knowledge which is gained during these projects tends to disappear. There are no knowledge-carriers, one might say.

The third possible definition can be placed in the same realm. Knowledge, gained by social research, disappears when, as often happens, it is not in accordance with the interests of the parties that should use the knowledge to inform their societal activities.

In these definitions, scientific knowledge seems to be nothing more than a loosely connected, or unconnected, set of reports on the results of research projects. The existence of such a collection explains the disappearing of knowledge: elements in the collection may disappear without affecting the whole of knowledge. When these elements should form a system - which means: interconnectedness - the disappearing of elements would be impossible. It is this, which has led us to formulate another point of departure. In our view, scientific knowledge - the results of research - may be saved, and not disappear, when an accumulation of this knowledge takes place. The accumulation of scientific knowledge is seen as a, not yet realized, possibility of science.

We suppose that this possibility points to the existence of a certain, yet hidden, structure of science.
We propose to develop this potency of knowledge accumulation by using an instrument based on a theory of this hidden structure, and therefore on an alternative conception of science; a conception which will allow for a system definition of social science as an integrated and functionally differentiated pattern of human activity.

First it will be shown that the concept of 'disappearing knowledge' is not satisfactory and therefore another problem definition will be suggested. This definition will be followed by a short analysis of the current situation concerning the social sciences in the Netherlands. In a third paragraph, an instrument will be developed out of the post-empiricist theory of science which enables the reconstruction of research programmes.

The basic idea is this: Specific, actual research projects may be seen as the outcome of a certain research tradition, which contains elements of a scientific and of a societal paradigm. These elements can be described as the ground for a research programme. In this way, the specific research projects may be reconstructed as part of a, mostly implicit, research programme. A reconstruction which will allow for suggestions for future research based on tacit ('disappearing') knowledge.

2.2. DISAPPEARING KNOWLEDGE: THE SYSTEM MYTH

Current scientific activities, as shown by various inventories, may not be conceptualized as 'paradigms'. There is some consistency between different scientific activities, but not as deep as being a paradigmatic unity. The existence of such loosely connected research activities may be described in two steps:

- intra-scientific influences, the dominance of certain conceptions of science, and
- external influences, for instance the activities of the Dutch department for science policy, the workings of political ideas, etc.

In this chapter these two steps will be elaborated upon.

Dominant Conceptions of Science

It is possible to separate two aspects of a conception of science. A first aspect concerns the most important characteristics of processes. A second aspect concerns the product of social research, its function in the search for the solution of social problems.

Now, an analysis of a recent debate on the methodology of (i.e. conception of) policy research in the Netherlands shows, that this distinction is not customary. What happened is this: The role of science in the finding of solutions for social problems is made the central standard; the organization of research projects is made dependent on this.

In the Dutch situation, a very important part of the total of social research consists of policy research. In projects of this kind, the main goal consists of the use of information for the improvement of policy. The accent is more on the use of research for the solution of policy problems, than on a possible accumulation of knowledge. In those projects, the frame of reference is given by the specific policy activities.

Behind the acceptance of this frame of reference, we find a more general concept of social science. In the mentioned methodology-debate this is shown by the fact that none of its participants (a few prominent acedemic sociologists were among them) questioned the standard of 'utility' (the link from results of research projects with policy problems), and its importance for empirical research. Moreover, 'science' often was seen a synonymous with 'empirical research'.

From its beginning, a rationalist ideal has dominated in the social sciences in the Netherlands. This ideal states that scientific knowledge of the social world may contribute to:

- an understanding of our social world;
- efforts to reorganize this world.

Coupled with the dominant conception of science this leads to the presupposition, that the results of empirical research automatically tend to accumulate, and inform our activities concerning the organization of the social world better and better. Knowledge based on scientific research, it is said, therefore not only helps us to obtain a better understanding of our world; it also allows us to make a better performance in changing this world.

This is formulated as a presupposition, a starting point therefore, and not a desideratum. The accumulation of knowledge was seen as a characteristic of scientific activity, per se. And, until recently, no one felt the need for a structuring of scientific activity to react a goal called 'the growth of knowledge'.

External Influences on Science

As said: 'until recently'. For now the situation has changed and Dutch governmental agencies decided that, since the results of research do not match the expectations (utility), the structuring of scientific activity has to be executed consciously. Unfortunately, the problem they felt - the results of scientific activity do not apply well enough - was not analysed, and a quick solution was found. The results are a kind of researchorganization - labelled as 'Researchprogrammes' - in which research projects are centered around certain hot (and often political) items or specific policy problems. Once the projects are finished, and (political) decisions taken or preferences changed, scientists should move to another item or problem area.

During the development of this policy for the social sciences, the presupposition - inherent accumulation of scientific knowledge - was not really questioned. Another phenomenon should also be mentioned. Our understanding of the social world - let alone our efforts to reorganize this world - is only partially based on scientific knowledge. There is a lot of experience and tradition - the so-called tacit knowledge - in our performance and words.

The dominant empiricist conception of science allows us to forget this tacit dimension, since 'the facts speak for themselves'. However, when the scientists frame of reference is defined, and limited, by some policy-specified criterion of 'utility' or 'relevance', a very specific domain of tacitness creeps in his results. The social experiences and knowledge of government agencies set the stage and define the framework for empirical research.

In conclusion

There is a relation between the disappearing of knowledge and the lack of accumulation in the social sciences. This lack of accumulation has to do with an empiricist and utilitydominated conception of science, which dominates the thinking of scientists as well as the policy of governmental agencies. In that conception, the accumulation of knowledge is taken as unproblematic; a conception, which resulted in the production of scientific knowledge without a clear internal-scientific concept for integration of results.

What we need is a conception of science in opposition to empiricism, in which these concepts can be defined and in which an instrument may be constructed that allows for a systematic attempt to integrate the actually existing social scientific research. In the sequel, this will be the subject of discussion.

2.3. REAPPEARING KNOWLEDGE: A POSSIBILITY

When knowledge disappears because of desintegration, the search for improvements need a concept of science. In our opinion such a concept should offer a scientific structure system, and points by that way at possible actualizations of that system. Some of such actualizations might be improvements of the actual scientific situation, and could be used as descriptions of a desirable and realizable state. In the following description we formulate our concept of science.

A new concept of science

In science three levels of activity can be distinguished: empirical research, reserach traditions and research programs. The most concrete level is that of empirical research. Empirical research is carried out in researchprojects, each of which has its own goal and its own plan. Research traditions form the most general level of scientific activity. On that level fundamental theoretical and philosophical discussions take place. The most important level of this concept of science is the research program, connecting research traditions with empirical research. Each research programme consists of a theoretical and a methodical framework, which lends coherence to various projects of empirical research. Such a framework allows knowledge to accumulate as a result of the development of a succession of theories with an increasing theoretical and empirical content. The theoretical and methodical framework of research programs can be seen as the result of the elaboration of various theoretical notions that have been developed in research traditions.

How should it be possible to conceive the three levels of science and their interaction, producing accumulation of knowledge, as a structure system of science? A specific answer to this question is on this moment still not possible for us. Yet we want to give certain indications of such an answer.

We suppose that the system of three interacting levels is a part of a more comprehensive structure as core of modern society. In that society there is sufficient room for differing opinions, for discussions, for written communication that stimulates activities on the level of research traditions. In such societies there are sufficient instrumental possibilities for the collection of new and challenging data. In such societies there is an institutional possibility for contacts between groups who function on the research traditional and groups who function on the empirical level.

In the history of the philosophy of science many concepts of science have been developed. We conceive such concepts all as attempts to specify the structure system behind the concrete manifestation of science.

When the structure system of science, in our conception, gets fully actualized, we may expect different competing research programmes, even in the social sciences. Each research programme will have its own framework, its own heuristic, and its own series of succeeding theories, more or less growing or degenerating.

Although the social scientific scene does not satisfy a full actualization of the programmatic scientific structure, our science concept allows for the thesis that such an actualization is possible. We suppose even that there is a continuing tendency in the direction of research programmes in the social sciences. This means that it must be possible to conceive the social sciences as a partial actualization of the structure system of science.

When we make a global description of the social sciences as a partial actualization of the scientific structure system, this may get the following form. In the social science there exists a big gap between the level of research traditions and of empirical research. On the research traditional level this means a lot of discussions between theoreticians, often without clear concepts, with a lack of mutual understanding, without generally accepted solutions to conceptual problems, without a growth in theoretical insight. On the level of empirical research in the social sciences we may find many projects of empirical research, without an explicit theoretical framework, without clear indications about datacollection, ways of interpretation, plausible assumptions, etc. On the level of research traditions there exists the threat of drowning in continuous discussions, on the level of empirical research one drowns in data. We may find attempts in the social sciences for the development of research programs, sometimes progressive, mostly degenerative, sometimes explicit, mostly implicit and part of logic-in-use. Sometimes the source of such programs may lie in the growing frame of references, used by different cooperating resarchers. In other cases the dominant source may be found on the research traditional level.

2.4. TOWARDS A PRODUCTIVE DIFFERENTIATION OF KNOWLEDGE

In the first part of this chapter we reinterpreted the problem of disappearing knowledge as a problem of the desintegration of knowledge, the missing of a system wherein each part of knowledge gets its place and relations. Speaking about the social sciences, assuming the described scientific structure system, a specific problem conception appears.

If the scientific structure system is fully actualized we will find a productive form of differentiation in science: a differentiation between discussions on the levels of resarch traditions and empirical research, integrated by research programmatic frameworks. The differentiation is necessary for an adequate reduction of complexity; the relation with research programmatic frameworks compensate for the constraints in each reduction. On the empirical level we find a differentiation between projects within a program, integrated in the same framework. And on the research traditional level we find different fundamental styles, but a common concentration on existing and concurring research programmes. We always find a differentiation on behalf of an effective reduction of too much complexity, in combination with integrative possibilities that compensate for the disadvantages of such reduction.

If the scientific structure system is only partially actualized we may find a differentiated set of activities and (theoretical) products, but a lack of integrative counterforces. It is not astonishing that knowledge on the level of empirical research may disappear: the products do not get a significant place in a research program, remain isolated and may easily be forgotten.

We may now give more depth to the explanation of desintegrated science. That explanation becomes an answer to a new question: why is the actualization of the scientific structure system in the social sciences often partial, and are research programmes often missing?

An answer to this question may again be found in the usual science concept of scientists: their attempt to specificate the scientific structure system. An empiricist science concept means that only attention is given to one layer of science: empirical research. Other layers are not seen as belonging to science. That means the intentional actualization of one part of the scientific structure system, a lack of attention to fundamental theoretical choices, and a lost possibility of consciously integrating the products of empirical resarch projects in one programmatic framework. The accent on a direct practical use of research products, in combination with an empiricist science concept explains why the formulating of research problems and requirements for research products is delegated to policy-instances. Attention for theoretical frameworks is considered as non-scientific: the two other levels of science are left to the free play of societal forces, mostly the field of politics and administration. In that way the partial actualization of the scientific structure system opens possibilities for the actualization of other structure systems in scientific research. Activities in the social science are consequently the product of "co-actualization", wherein the scientific structure system plays only a limited role, interacting with other structure systems.

Improvement of this insufficient functioning of the social sciences is possible. One has to actualize the scientific structure system and its possibilities and tendencies. One way consists of the changing of science concepts, held by researchers, although we find a difficulty here. Discussions about science concepts find their place on the level of research traditions. That kind of discussions are not easy, when partners in the debates are scientist with an empiricist conception of science. These scientists will deny the scientific character of discussions over science concepts, because of their place on research traditional level. Another way to improve the situation of the social sciences may be found in the domain of science studies. It is possible to do research supporting the actualization of the scientific structure system in specific research projects. In these projects a concept of science is used as an instrument for the reconstruction of science.

In such a kind of reconstruction it is possible to identify and to describe beginning research programs. Such programs may be judged by their actualization of the scientific structure system. On the basis of that judgement it is possible to improve the reconstructed program in the direction of a higher actualization level.

We expect reconstruction and improvement of partially actualized research programs to be more convincing than discussions about various science concepts. By reconstruction of research programs, and by placing their elements in a programmatic order, there are possibilities of rediscovering theories. Knowledge that was forgotten because of a low programmatic consciousness of researchers.

CHAPTER 3

APPEARING KNOWLEDGE FOR POLICY FORMATION: SOME BOTTLE-NECKS

J.H.G. Klabbers

For a variety of reasons knowledge, that exists at one place, may disappear when it is transferred to another. For example knowledge that is produced by the social and behavioral sciences may become falsified because individuals or groups change their conduct. It is also possible that preliminary notions from such research may cause people intentionaly to follow the rules that seem inherent in the early results. In the first case knowledge disappears, in the second one knowledge that did not exist formerly, somehow is produced. Knowledge may disappear when too narrow-discipline oriented research produces results that do not fit into the domain of application, or the other way around, knowledge may disappaer when it falls between the scope of adjacent disciplines.

The views, as expressed above, presume that fundamental scientific research generates knowledge as such. In this paper this position is questioned. Therefore we will elaborate on processes of scientific knowledge production and on a particular context of use i.e., policy formation processes. The first aspect deals with some basic characteristics of scientific inquiry, the second with features of policy making and policy makers which form an influential group of potential practitioners or users. The focus of attention will be the utilization of decision- or maybe better policy-support-systems.

3.1. CHARACTERISTICS OF SCIENTIFIC INQUIRY

Macmillan and Garrison (1984) point out that since Kuhn's 'The Structure of Scientific Revolution', philosophy of science has shifted its attention from observation statements, theories and generalizations to 'paradigms' (Kuhn, 1962), 'research programmes' (Lakatos, 1970) or 'research traditions' (Laudan, 1978). Research traditions are based upon:

- **assumptions**, about appropriate methods of defining the domain, entities and processes in the domain of study;
- **methodologies** for investigating the problem (puzzles) and constructing theories of that domain;
- **assertions** about what logic is appropriate in scientific investigation and justification.

It will be apparant that each research tradition develops a more or less specific language and logics. With respect to the social and behavioral sciences familiar research traditions belong to either one of the following major philosophical traditions, i.e., logical empirism, interpretive theories, critical theory and general systems theory.

Logical empirism as in use the social and behavioral sciences views methods and form of the natural sciences as the model for scientific inquiry. The language of positivist empirical research in both sciences is very dominating in general. Experimental designs, survey methods and the whole variety of statistical tools are examples of the positivist approach.

Interpretive theories seek to understand ('verstehen') human and social phenomena instead of trying to reconstruct them in terms of causal relationships. Examples of this tradition are: phenomenology, analytic language analysis and hermeneutics. Interpretive theories seek objectivity and value-free inquiry into the human realm of intersubjective meaning. In this regard they agree with logical empirism which also looks for value-free inquiry.

Critical theorists reject the idea of value free research. They consider logical empirism as an ideology. Criticial inquiry is value-laden and tasks into account the historical-ideological context we live in.

General Systems Theory, or GST, crosses traditional disciplinary boundaries in search for a synthesis of views, emphasizing patterns of similarity. GST is considered a reaction to the narrowly discipline oriented approach mostly on the basis of logical empirism, that focusses its attention to aspects of reality. GST stresses the great variety of reality to which science has to address itself with requisite variety. Therefore GST is problem-oriented, for it deals with future circumstances as a consequence of historic and current decisions and actions of various actors. In developing a systems perspective, methodology is applied as rigorously as possible, but models that are considered to be appropriate-to-the-problem leave room for judgement and intuition (Klabbers, 1982). The frame of reference that GST provides integrates in principle aspects of logical empirism, interpretive inquiry and critical theories when applied to the study of social systems. For instance during the development of an image or model for a social system, interpretive and critical theories play an important role throughout the initial conceptualization

During the subsequent formalization and operationalization a positivist approach is more adequate, while during utilization of the model as a system compensatory to the knowledge, skills and experience of a so-called client system, again interpretive and critical theories are more appropriate, especially in a policy- or action-oriented social environment. As GST is problem-oriented, in the development of a 'model for' there are no separate rooms for either one of the philosophical traditions mentioned above. They all constitute limited views on complex and equivocal reality. Each of them is more or less fruitful, depending on different scientific and social convictions. It is for philosophers of science to quarrel about them.

Each of the philosophical traditions pointed out earlier, generates more or less typical research traditions, research programmes or paradigms. They produce domain specific knowledge. An interesting question is, what is meant with domain specific knowledge.

3.2. ASPECTS OF SCIENTIFIC KNOWLEDGE PRODUCTION

It seems reasonable to assume that scientists favor research traditions for their problem solving effetiveness, and so does society. Effectiveness depends on the number and importance of the problems a research tradition can solve, and on the rate at which problems are being solved. Thus we are in good shape, for comparison and choice of a favourable research programme seems straight-forward. What is not so clear yet is, whether we are talking about real life problems, with which people are struggling or with theory driven problems that are advanced by a certain philosophical tradition, because they look interesting and anyhow keep scientists occupied.

By taking problem solving effectiveness into consideration it is not so obvious what is meant by 'the problem'. What is seen as problematic depends on the position a person finds him- of herself in, on the attitudes, beliefs, interest, etc. of that person. Consequently the knowledge that is produced via a certain research tradition to tackle a specific problem may not be regarded as knowledge, for two reasons. Either science is trying to give answers to problems that do not exist as such for a number of people, or the knowledge is presented in a way that does not fit into the assumptions, methodologies and assertions of a person, group of persons or a formal group of scientists. To be able to deal with this ambiguity in the context of knowledge production, the following thesis is proposed:

- Research traditions produce specific scientific data, such as statistics, findings from a study, a metaphor, etc.
- These data, only when constructed by a domain specific theory, become information. Decisions are made or actions are taken on the basis of this information.
- Information confirmed by the results of actual decisions then becomes knowledge. Therefore, knowledge is the condition of knowing something through experience. Knowledge is a reconstruction or rationalisation of past experience. It is a guide for future action.

There are many reasons for supporting this thesis. We focus attention on the following ones. We agree with Vickers (1965) that policy making actually is a process of appreciation. It is the linking of two types of judgements,, i.e., judgement of fact (reality judgement), and judgement about the significance of facts to the appreciator. Judgements about the significance of facts are value judgements. They define what concepts will be used to denote the content of the social system. They depend on the context or frame of reference of the appreciator, which is based on his social theory and social knowledge (Klabbers, 1985). Man is mainly interested in creating a sense of order and meaning in life. Facts must fit into his reference frame, otherwise they run the risk of being rejected.

The difficulty with knowledge-production as presented by this thesis is even more aggravated by the notion that scientific information can only be confirmed scientifically if and only if theory and practice (or application) stay embedded within the same research tradition. This condition generally holds for the natural sciences such as physics. Information is confirmed (repeatedly) by testing it under circumstances that fit into the frame of reference of that particular research tradition. As a consequence, in physics theory and practice obey the same rules of conduct of scientific inquiry. However, what complies with the natural sciences does not apply to the social sciences as well. Regarding the social sciences the community of scientists and of practitioners differ. The community of natural scientists, theorists and practioners, appears to be more closed, more subject to rituals, rules of conduct and pecking order than we are familiar with in the social sciences. For those reasons physics presents itself more as a discipline, with a more explicit reward and punishment system than any of the social and behavioral sciences.

Policy makers are users and practitioners of social-sciences-information usually have few obligations to stay within the specific domains of social science research traditions. It has been pointed out already that 'knowledge is information confirmed by the results of actual decisions'. Therefore it is obvious that scientific knowledge can only be generated within a close community of theorists and practitioners, who oblige to the same assumptions, methodologies and assertions. As soon as this so-called scientific knowledge crosses the borders of that community, it no longer can lay claim to be knowledge. Outside the particular scientific community, scientific knowledge becomes data or information. This applies to knowledge from physics, that enters the realm of the social and behavioral sciences, or that enters society, as well as to knowledge from the social sciences, that is provided to practioners such as teachers, policy makers and therapists, as well.

These data from sciences become information for policy makers when they are arranged by a particular political of social theory. After being tested, preferably repeatedly, this information becomes social knowledge. If a policy maker by his former education is inclined to a particular paradigm, results, generated by that scientific community, that support that paradigm, have a reasonable chance to become information to him. Otherwise they run the risk to be considered of no significance, as they do not reinforce the existing intellectual orientations of a policy maker. It is appropriate now to draw some conclusions.

1. The test of social knowledge, based on data and information from the social sciences, is social or political action.
2. Natural sciences are able to produce knowledge concerning a limited domain, because theorists and practitioners obey the same rules of scientific inquiry.
3. Social and behavioral sciences do not, and maybe cannot, produce scientific knowledge, because practitioners such as policy makers, generally apply other frameworks than social scientists, based on political motives.
4. Even if policy makers, or practitioners in general, apply similar frames of reference, the context of use will hamper a 'controlled' testing of theory.

The consequences of the third conclusion may give rise to further frustrations if one realises that there are some problems with the definition of the policy domain too. Policy makers tend to attack specific problems in the light of a certain political ideology. It is that particular perspective that controls and limits possible explanatory presumptions and the range of policy measures that have been considered regarding drug prevention in the Netherlands, Europe and USA, vary widely and so does the influence of different research traditions in this area. Certain research traditions tend to re-inforce certain policy objectives, and vice versa certain policy objectives tend to re-inforce certain research traditions.

Suppose we stick to Laudan's (1978) conception of science as problem solving. The test of the value of a scientific tradition then is its ability to answer the questions that are crucial in its domain of investigation. Suppose also that in the context of the GST we are interested in developing anticipation models (Niemeyer, 1983) or 'models for'. In such case the domain of investigation and the domain of utilization become interwined. Can both or should both be separated or should we try another approach? Let us try to figure out this bottle-neck by tiptoing through the world of computer-assisted support systems, in particular policy support systems.

3.3. CHARACTERISTICS OF POLICY SUPPORT SYSTEMS

For our present needs it is convenient to represent policy support systems by two types of assumptions: institutional and behavioral. Institutional assumptions refer to the physical properties of the policy formation situation, for instance how many actors are involved, when is what information available to whom, what is the time horizon, what actions may when be taken by whom, what are the outcomes or payoffs, etc. The behavioral assumptions concern the properties of the actors (policy makers), their cognitive schemata, their negotiation skills, political faith, etc. A complete model of policy formation contains both types of assumptions. Putting the behavioral assumptions aside for a moment, policy support systems mainly map out institutional assumptions of the policy formation situation. To be able to estimate outcomes or payoffs of policy actions and to anticipate consequences of the various policies, support systems usually contain information that describes the states of the social system involved. This information may refer to economic, demographic, technological, cultural aspects of the system. It may be represented by a data-base system, that is up-dated regularly. It may be a model that simulates economic, demographic or technological processes. It may also be a knowledge-base system, that adds to the simulation model a cognitive representation of how to make inferences, diagnoses or plans of action. In the last case interactive simulation is supplemented with an artificial intelligence interface. In short, information is provided by a reference system that is being shaped as a data-base- or a knowledge-base system, which in its turn may consists of a representation of physical or material properties, as is the case with simulationmodels, and/or a cognitive representation such as in expert systems.

It will be obvious that developments in the realm of artificial intelligence, computer-assisted design, and interactive simulation of social systems will have an increasing impact on shape and content of individual and social problem solving, to which policy formation also belongs. The nature of steering and self-steering of social systems will be highly influenced by these technological advances (Klabbers, 1985). Although these support systems contain elements that describe parts of social systems, for example macro- or micro-economic processes, and thus reconstruct partially the recent history of a particular social system, they also may contain elements that depict a future situation, as for instance is currently being applied in computer-assisted-design. As more and more elements are included that anticipate on expected or desired future circumstances, support systems become models for various purposes. The basic question then no longer is the validity of the model (model of), but much more its usefulness (model for) to improve expertice and skills in negotiation, policy formation, etc. Criterion for judgement are answers to questions like: "Have our problem solving capacities improved by using such systems?" or to paraphrase this question: "Have we gained knowledge, that we otherwise would not have acquired?". It is clear that answering these questions by policy makers for example is not a purely scientific endeavor, for there are many other factors that also influence policy making. The constituency being just one of them. Therefore (policy) support systems should not be evaluated by the same scientific standards that are used for 'models of'. They do not fit into research programmes that belong to the domain of logical empirism as maybe clear for apparant reasons. However an interesting paradox arises here.

At the level of use, more or less intelligent support systems are meant to improve the way individuals, groups and organizations manage their affairs. This leads to the appropriate question of 'are we doing a better job?' If we include also behavioral assumptions, as mentioned earlier, at least in principle we are able to construct a complete 'model of' a social system, that consists of both institutional and behavioral assumptions, at a higher level of abstraction. At that level, the question is appropriate; "Is this particular model a good representation of the system?"

For our purposes we stick to the use of support systems as 'models for' some more or less well defined purpose, because even if we would succeed in specifying behavioral assumptions satisfactorily, in policy formation situations it is more appropriate to allow the actors to act in whatever way they choose. Such a situation provides a better learning environment as a basis for improvement of self-steering (Klabbers, 1985). Policy makers may also put more thrust into such a setting.

3.4. DEVELOPMENT AND INITIAL USE OF A POLICY SUPPORT SYSTEM: THE CASE OF PERFORM

Recently a project was finished, which aim was to design a policy support system for manpower planning (human resource planning) at Dutch universities (Klabbers et al., 1984). When the project started the Boards of universities and faculties realised that:

1. There exists a lack of knowledge both by the government and the boards of universities with regard to the long term consequences of current manpower policies.

2. It is obscure to university administrators how the various measures of the Minister of Education are interrelated.

3. Manpower- or personnel policies are carried out at several distinct organizational levels, i.e., the Ministery, the Board of the university, the Board of each faculty, departments and units. In this hierarchy top down allocation of financial resources on the basis of short term and many times ad hoc policies has to meet bottom up demands for human resource planning by faculty members to improve competence and expertise. In times of diminishing financial resources it is obvious that both vertical processes in the hierarchy cause many frictions, especially when a comprehensive personnel policy on a long term basis does not exist. Several administrators expressed that they were flying blind.

On the basis of these difficulties in managing universities, support systems were developed that are able to perform the following tasks:

1. To forecast future size and composition of the personnel formation of universities, and faculties, depending on the various hiring, firing and promotion policies.

2. To show how future size and composition of the faculties, with a time horizon of twenty years, depend on:
   - the initial size and composition of faculties;
   - yearly hiring, firing, and promotion rates of the distinct pesonnel categories;

- financial constraints due to budget cuts that force a re-allocation over different faculties.

3. To show the consequences of alternative policies, that resulting from negotiations between the university board on the one hand, and the faculties on the other. Those negotiations can be carried out bilaterally or multilaterally. This implies that the support system should be able to improve hierarchical, multi-level manpower policy formation processes.

Two related support systems have been developed. One is called FORMASY, which is a conversational planning system. The other is PERFORM, which is a multi-level interactive simulation, in which FORMASY has been embedded as will be pointed out later.

FORMASY actually is a set of tools for conversational planning regarding categorial aspects of manpower planning. With this set it is possible to model categories of personnel acording to rank, age, sex, full-time or part-time appointment, etc. Also possible flows of personnel between these categories are defined. After running the model on a computer, distribution of faculty members over ranks and age can be estimated on the basis of hiring, firing and promotion rates. It is possible as well to forecast the (total) number of personnel of faculties, and universities and to estimate the total yearly budget. As FORMASY has a user-friendly interface it can be used by clients without technical skills.

The conversational planning system has been built into the structure of the interactive simulation PERFORM as illustrated in Fig. 1. Although PERFORM is a framework for interactive simulations with actors on different organizational levels, for demonstrating its potential utility a transparant structure has been chosen. It is loaden with data from two different faculties. One with a tradition in research, the other primarily occupied with teaching. Both faculties differ with respect to size and distribution of faculty members over the various ranks. Consequently the budgets for salaries vary considerably. Enrollment of freshmen to the research oriented school is expected to level off and may even decrease. Enrollment of the other faculty reflects the situation of a faculty of law, the research oriented faculty reflects the situation of an actual faculty of chemistry. This version of PERFORM is also loaded with the assignment to cope with decreasing government finding. The frame of PERFORM can be loaded with more faculties, options and policy scenario's. It is also possible to add a third organizational level, that represents the Ministery of Education. What particular structure is chosen depends on the options that are interesting for the client-system.

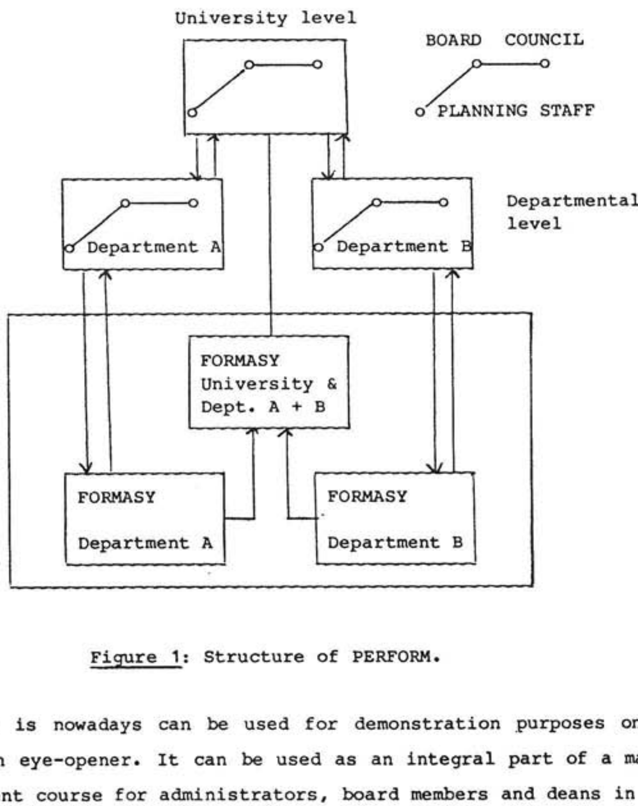

Figure 1: Structure of PERFORM.

PERFORM as it is nowadays can be used for demonstration purposes only to function as an eye-opener. It can be used as an integral part of a management development course for administrators, board members and deans in order to learn to deal with strategic management issues and to learn to negotiate on allocation of human resources. Finally PERFORM can be used as an integral part of policy formation processes i.e., operationally.

During operational use, PERFORM enlightens potential consequences of the policy alternatives that are brought forward during the negotiations. All actors involved are confronted with each others congnitive schemata, implicitely or explictely. They learn to realize that the same data will lead to different information upon the basis of different social theories, ideologies, political faith and/or competetive and conflicting interests. Consequently social knowledge of all actors develops during the subsequent rounds of negotiations and computerruns. It potentially can be improved because of the continuous mutual experiential learning process. The experiential learning situation not only offers an opportunity to learn new concepts, it shows also the way this acquired knowledge can be used while being in one or several positions during a session.

The way collective structures evolve during PERFORM sessions, may give policy makers a better opportunity to cope with long term consequences; of outcomes of negotiations during which short term interests tend to dominate the discussions. Experience with PERFORM during several one-day sessions has learned that representatives of different faculties, with different disciplinary background, tend to demonstrate different styles in handling data and information and thus in applying social theories and social knowledge. From these preliminary observations it is assumed that actors with a background in physics feel more the need to collect data, before they are confident to formulate a policy, than actors with a background in humanities. The former seem to be more data driven instead of theory driven. Especially during rounds of negotiations these different styles produce much confusion and misunderstanding due to different cognitive styles of the actors.

3.5. BOTTLE-NECKS WITH IMPLEMENTING INTELLIGENT SUPPORT SYSTEMS

In linking science production and - utilization, two types of bottlenecks appeared, one related to the investment by the adopting organization, the other to the culture within the organization, especially the policy-making climate.

Regarding initial investments the adopting organizations like the Ministry of Education and all Universities, which were the sponsors of the project, a 'make-or-buy' decision was not necessary anymore, after FORMASY and PERFORM had been developed.

The initial investment that has now to be made is the conversion of both support systems to the host-Computers of the adopting Universities and of the Ministry. To facilitate this conversion process, the initial specifications for the software design were such that the computer programs had to be highly portable. For this reason both FORMASY and PERFORM have been embedded in a Virtual Operating System (VOS), that emulates the UNIX-operating-system. It was estimated that on the average it would take a software engineer three months to complete that conversion. Operational use of both support systems implies that a personnel department has to adopt them, to maintain them and to utilize them for various policy problems that envolve over time. To run both systems the adopting organization has to invest at least in two types of expertise, one in software engineering to maintain them and to develop new versions within the context of the frame-instruments. The second type of expertise that is needed is related to the ability and skills to translate issues of human resources, manpower and personnel into the frameworks of FORMASY and PERFORM and vice versa.

Dependent on the scale and intensity of use, which can vary widely, initial investment in both types of expertise could start on a part-time basis that is, one or two days a week. Together with the decision to make this investment, a department has to be chosen that is responsible for the suppot systems. As a consequence FORMASY and PERFORM have to be included in a well-defined management- and control-structure. It has to be clear who is responsible and entitled to utilize them. At this very moment the policy-making culture and organizational climate come into play.

Currently both FORMASY and PERFORM are being converted to a computer of one university and of the ministry, with the intention to make use of them. Appearantly it is assumed that the necessary investment as mentioned above is worthwhile. However an interesting distinction is being made between the application of FORMASY and PERFORM. Conditions for using FORMASY operationally are rather simple. As soon as it is clear what department is entitled to maintain and use it, the management- and control-structure is well defined. This department defines who will have access to it and what the conditions are for its use. It is resonable to assume that the person who is ultimately entitled to use FORMASY gains in controlling the manpower planning of that university. He is able to outmanoeuvre the respective faculties in bi-lateral negotiations.

Conditions for operational use of PERFORM are 'slightly' different. In the specifications of the design of PERFORM it is foreseen, that each faculty controls its own version of FORMASY, and that on the basis of the skills acquired in using it, faculties engage in multi-lateral negotiations concerning human resource- and manpower planning. This structure implies that more than one management- and control-center is responsible and entitled to use PERFORM and FORMASY, but they may share the technical expertise to maintain them. These multi-lateral negotiations on the basis of PERFORM has been called by one of the members of the board of the university, that is planning to adopt the support systems, a meta-game. He has made clear not to be in favour of such a game, because it does not fit into the existing policy making culture. As Dutch universities increasingly tend become bureaucratic organizations it is obvious that FORMASY fits better into bureaucratic procedures, while PERFORM violates them. FORMASY does not intrude into the existing departementalization, while PERFORM does. We guess, however, that appropriate use of PERFORM does increase the total amount of control as defined by Tannenbaum (1968).

Suppose that FORMASY is being implemented for operational use, while for the time being PERFORM is put into the refridgerator. The planning department that supports the board of the unviersity will be responsible and entitled to use FORMASY. In such a case the university board can take advantage of FORMASY by trying to out-manoeuvre the faculties in bi-lateral negotiations. If that will happen, it may be possible that also faculties will start implementing FORMASY to take advantage of it. It may be possible as well, that faculties lack the resources, which by the way are allocated by the university board, to implement FORMASY and that they reluctantly accept the situation. In that case deans of faculties may put false colors on FORMASY as a support system and on such support systems in general. If faculties decide to implement their own FORMASY systems, and bring their expertise into the negotiations with the university board, gradually a PERFORM-type structure emerges, and attention will become focussed on the quality of the policy formation process.

Thus if implementing PERFORM via the university board is hampered by the top-down bureaucratic procedures, it maybe more suitable to support the faculties in their long term human resource planning bottom-up.

3.6. CONCLUSIONS

It should be realised that support systems like PERFORM are interventions that influence the existing collective structure of a social system. As 'models for' or anticipation models they are aimed at supporting knowledge creation, -diffusion, and -utilization by all relevant actors (Klabbers, 1985).

Operational use of support systems as described here, provide social situations in which science production becomes intertwined with science utilization. Purposes of use are improvement of policy formation processes, of handling available information, as compared with previous by existing situations. If applied appropriately, those support systems can enhance self-steering capacities that are built-in in all social systems, but for various reasons are being suppressed.

Development of intelligent support systems based on General Systems Theory and -Methodology, as anticipation models of social systems, building on progress made in the fields of interactive simulation of social systems (Klabbers, 1983, 1984), artificial intelligence, expert systems, etc., is an emerging research tradition.

Especially multi-actor, multi-level support systems like PERFORM offer the opportunity to couple science production (concepts and data) with utilization (development and testing of social theories and social knowledge), because they create a social situation in which theory and practice are linked together in an experimental and possibly operational setting. It should be stressed that built in algorithms comprising opeations research models should be avoided, as can be observed from the use of several decision support systems.

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CHAPTER 4

BASIC STRUCTURAL PROPERTIES OF KNOWLEDGE AND OBSERVATIONS AND THEIR IMPLICATIONS FOR EXPERT SYSTEMS IN SCIENCE

Maria Nowakowska

Basic structural properties of knowledge in science are analysed, through interrelation of two processes, which represent the changes of two basic components of the discipline: factual and theoretical. Logical mechanisms of interaction between these processes, and also their stochastic version, are shown. In the second part of the chapter, basic problems of networks of observability, in particular joint observability, are considered. The concept of mask, representing the observer's choice of temporal patterns of observation of variables, is introduced and analysed.

4.1. INTRODUCTION

One of the possible ways of deepening the theoretical foundation of expert systems is through understanding of logical structure and dynamics of development of a given domain of knowledge, both by the constructor of the system, and by its user. This will allow to show not only how the information about new facts or new knowledge may affect expert's opinion, but also to better understand how a scientist, in particular an expert, can change the areas of knowledge and its content, by rejection or introduction of new concepts, hypothesis or theory, which better explain the collected facts and hypotheses. In other words, one can show how the expertise changes the state of information in a domain or problem, by design of change, or keeping status quo, if it is considered optimal. Consequently, the automatization of expertise in science or technology (as opposed to expert systems built for medical applications) would have to take into account to a larger extent certain wider characteristics of the domain (e.g. trends, controversial issues, hypotheses tested thus far, models for methods used, etc.). Moreover, among others, the constructors of expert systems must be well acquainted with the process of judgement formation and opinion, as well as with cognitive limitations and biases connected with these processes. In particular, it is important to understand the distortions of judgements connected with the difficulties of discrimination and identification, implying variability of judgements and opinions in time. It is worth stressing that statistical properties of judgements and opinion, were, until now, almost entirely neglected in many important areas of research, such as decision theory, experimental research on subjective probability and, in general, measurement theory.

In recent publications concerning expert systems (see for example Hayes Roth et al. (1983); Nowakowska (1984); Pearl (1984)), while there exists the awareness of the importance of these topics (not only in construction of expert systems, but in evaluation of their efficiency), there is no deeper analysis of the process of expertise formation - the basic cognitive, ethical and social processes involved in it.

Observability is, for the constructor of an expert system another important group of problems. In particular, in scientific expertise, the way of obtaining the database is essential, because of the possibility of many sources of errors.

4.2. BASIC STRUCTURAL PROPERTIES OF KNOWLEDGE IN SCIENCE

We consider the following model of development of structure of knowledge of a given domain or discipline. It is namely represented as a process $\{F(t),T(t)\}$, where the components are interpreted as the factual and theoretical components of the domain. As regards $\{F(t)\}$ it is the process of accumulation of empirical facts. Each fact is represented in form of a statement (description) so that at each time t, F(t) is a set of propositions. The situation is idealised by accepting the postulate that the set F(t) is consistent for each time t. This means that no facts are ever discarded (as false), and that they never contradict themselves (although they may contradict theory). If we denote by CF(t) the set of all logical consequences of conjunction of elements of F(t), than CF(t) also increases and is consistent (i.e., it never contains a pair of statements, one being the negation of the other).

Next, T(t) is the set of theories existing at time t, where the term "theory" means any consistent set of propositions that explain <u>all</u> facts F(t) known at a given t (this means that the concept of theory corresponds to something wider than one normally understands under this term). Thus, a set of propositions H is a theory at time t, if H is consistent, and $CH \supset CF(t)$.

Clearly, if H is a theory, than one can introduce the concept of minimality: H_0 is minimal theory for H, if $H_0 \subset H$ and H_0 is a theory. Thus a minimal theory is a parsimonious set of assumptions that explain all facts.

The interaction between F(t) and T(t) can be explained as follows. Suppose that $T(t) = \{H_1,\dots,H_n\}$ where H_i's are competing theories existing at time t. Suppose that between t and t' some new facts are discovered, so that F(t) increases to $F(t') \supset F(t)$. To describe the possible effect of such a discovery of new facts may have on theories, we shall introduce one more concept, namely the set K of all hypotheses, conjectures, etc., that have been introduced at some time prior to t (and possibly discarded as false, replaced by their modifications, etc.). Generally, K is <u>not</u> a consistent set, since it would typically contain conjectures, together with their negations, etc.

Let now H be some theory in T(t) and denote $F(T') - F(t) = A$, with A being the set of new facts being discovered between t and t'. By assumption, $CH \supset CF(t)$. Various possibilities that may occur may now be summarised as follows

(a) $CH \supset C[F(t) \cup A]$.

This means that H is a theory at time t' also: all new facts are explained by H.

(b) $C[F(t) \cup A]$ is not contained in CH.

This means that H is not a theory at t', since there are some new facts that are left unexplained by it.

Various contingencies may now be summerised as follows.

A It is enough to add some new propositions to H to make it a theory: that is, there exists a set, say N, of propositions, such that

$$C[(H \cup N)] \supset C[F(t) \cup A]$$

and $H \cup N$ is a consistent set. This means that $H \cup N$ is a theory at time t'.

B It is also necessary to remove some parts of H (and add new assertions N). This means that there exists sets $M \subset H$ and N such that

$$C[(H-M) \cup N] \supset C[F(t) \cup A]$$

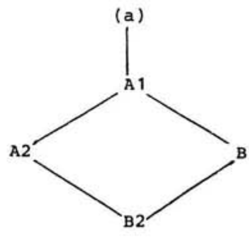
and $(H-M) \cup N$ is consistent.

Contingences A and B describe two ways of "repairing" a theory - one always has to add new hypotheses (postulates, assertions, etc.), and possibly also resign from some other hypotheses. Further classification can now be obtained depending on whether:

1. The set N appearing in A and B can be chosen so that $N \subset K$.
2. No subset of K can serve as a set N satisfying conditions A or B.

In the first case, once can "repair" the theory H by finding some hypotheses that have already been suggested at some time. In the second case, one needs real invention of some entirely new hypotheses.

Thus, the cases are (a), A1, A2, B1 and B2. Their arrangement according to "degree of seriousness" is presented on Fig. 1.



<u>**Figure 1.**</u>

Indeed, (a) requires only presentation of an argument that "old" theory explains the new facts. Next, A1 requires finding some hypotheses, among those that were offered at some time (and possibly discarded) which explain new facts. Situation A2 requires inventing such new hypotheses - since no old hypotheses can explain new facts. On the other hand, in B1 one can again find the necessary new hypotheses among already suggested ones at some other occassions, but one is also forced to remove some of the hypotheses in H as false. This may amount to admitting that one was wrong. Finally, in B2 all of the above are combined: one not only has to resign from some previous assertions but also invent some entirely new hypotheses.

Now, if at t there are some competing hypotheses, then at t' for each of them the situation becomes one of the five kinds (a), A1 - B2, and the state may be categorized according to the most serious of the possibilities. The following tree depicts these consequences:

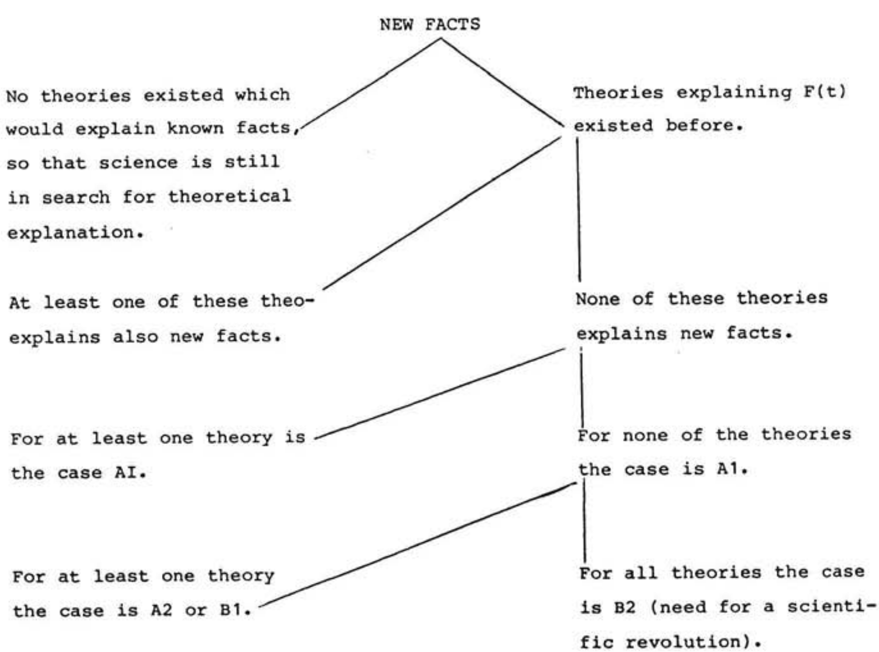


Figure 2.

The above scheme describes the effects of the factual process F(t) on the theoretical process T(t). The interaction in the opposite direction is due mostly to "competition" of the existing theories: to choose one of them, one cannot refer to known facts (since by definition, each of them explains all facts). Thus, new facts are needed and the scientists would usually select the experiments that offer highest chance of deciding in favour of one theory against the others. This affects the stream of new facts that are becoming discovered.

One ought to mention here that there is a third component of knowledge generation, namely the methodological component, that is a set of instruments, techniques, etc. that are used for collecting information, generating hypotheses, inference, etc. in a domain. This component changes typically more slowly than the factual and theoretical ones. The principal changes are of two types: a new method may be invented or brought from another domain; an old method may reach its "ceiling of applicability" or its cognitive limits, and becomes gradually less used or a method may be less used because change of fashion.

At the end of this section it is worth while to mention that the (rather stringent) requirements of the above communication can be easily relaxed. Firstly, as regards theories, they are assumed to be consistent sets of propositions that contain all set F(t). As regard consistency, there does not seem to be any sensible way of relaxing this requirement. However, the condition that a theory, say H, explains all facts known at a given time is not realistic. Most often a theory explaines some facts only, so that (dropping for simplicity index t, i.e., writing F for F(t)) one requires only that

$$CH \cap CF \neq \emptyset \text{ and } CH \cap (CF)^c \neq \emptyset.$$

There are two important possibilities here. Firstly, there may exist p such that $p \in CH$ and $\sim p \in CF$, so that we may say that H makes contra-factual assumption. Secondly, if no such p exists, we have a theory that leaves same facts unexplained.

It is worth stressing that contrafactual assumptions are more frequent than one could expect: for example, any model that treats a discrete variable as continuous, or vice versa makes a contrafactual assumption.

Suppose now that we have a class of theories in the above sense, i.e., class of consistent sets H, such that $CH \cap CF \neq \emptyset$. Let this class be A. We can than distinguish the class A^* of those theories in A which satisfy $CH \cap CF = CF$. The elements of A^* are theories in "complete" sense of explaining all facts. "Incomplete" theories can be ordered as follows: H_1 is "better" than H_2 if $CH_1 \cap CF \supset CH_2 \cap CF$, so that a better theory explains more facts. Elements of A^* are, of course, all equal in the sense of this relation. The sets

$$\bigcap_{H \in A} (CH \cap CF) \text{ and } \bigcup_{H \in A} (CH \cap CF)$$

may be called the core and coverage by set of theories A: the first contains facts that are explained by all theories in A, while the second contains all facts, that explained are by at least one theory. Naturally, if H is such that $CH \cap CF \neq \emptyset$, than for H to be called a theory, it is necessary that the set of facts explained by H, should be "sufficiently large" and retains "important" facts.

The expressions in quotations require introducing fuzzy class of "large" subsets, and "important" facts.

To do such a fuzzyfication one needs to define the appropriate membership functions. The first step consists of introducing a universe W of propositions, and two mappings $a,b : W \to [0,1]$, with $a(p)$, $p \in W$ reflecting the degree to which p is a "reliable" fact about the domain, and $b(p)$ reflecting the degree to which p is "relevant" for the domain. Moreover the functions a and b are timedependent so that the facts can change their relevance in time, and their knowledge may become more or less reliable. The intersection of fuzzy sets corresponding to a and b, i.e. the fuzzy set with membership function $\min(a,b)$ corresponds now to the process $F(t)$.

Such definition of facts $F(t)$ requires explanation of the concepts of consistency and of a consequence. As regards consistency, as already mentioned, no relaxation of stringent requirements for non fuzzy case appears reasonable. Thus, one must require that if some proposition p belongs to set CF in a positive degree, than its negation ~p does not belong to CF at all. This means that

$$\sup_p \min\left[f_{CF}(p),\ f_{CF}(\sim p)\right] = 0$$

where f_{CF} is the membership function in the set CF of all consequences of elements of F. The latter function may be defined in various ways, for instance by postulating the "weakest link" principle: consider all sets of premisses, say $p_1,\ldots,p_n$, that imply p, and then define

$$f_{CF}(p) = \sup \min_i \left\{a(p_i), b(p_i) : p_1,\ldots,p_n \mid \text{such that } p_1 \ldots p_n \to p\right\}.$$

In words, the degree to which p belongs to the set of consequences, is computed as follows: take any set of premises which imply p, and compute the degree to which their conjunction belongs to F. Than take the supremum of such members, over all sets of premises.

At the end, let us return to a non-fuzzy interpretation, with two interacting processes $\{F(t),\ T(t)\}$ as a description of a theory. Suppose that at some time we have k theories, and that a new fact was discovered. This fact can then be classified, with respect to each theory into five categories (a), A1, A2, B1 and B2. Moreover, each fact may be "generated" by some other facts. Such an interpretation may serve as a basis for treatment of the process F(t) as a sum of a branching process. One can namely classify facts into three categories: those which are generated by exactly one fact ("parent"), those which are generated by two or more facts ("hybrids"), and those which are not generated by any facts (to be called "migrant" facts). Consider first only facts of first class, called "normal". Clearly, they form a tree, with all facts, except the top one, having exactly one parent. If p_k $(k = 0,1,\ldots)$ is the probability of k facts being generated by a normal fact, we have here a branching process with generating function for the number of offspring $G(s) = p_0 + p_1 s + p_2 s^2 + \ldots$ Denoting by $Z_n(p)$ the sizes of successible generations of the process originated by a fact p, the generating function of $Z_n(p)$ is the n-th iterate of g(s), i.e., $g_n(s) = g\left[g_{n-1}(s)\right]$, $g_1(s) = g(s)$. Also, the probability that the process will terminate is the smallest root of the equation $x = g(x)$.

Now the whole process F(t) of facts can be regarded as a sum of branching processes of the above kind, each (perhaps) subcritical, i.e., such that $m = \Sigma k p_k < 1$, and each generated by a "migrant" fact, arriving (say) according to a Poisson stream.

This model may be now related to the classification of facts: one can namely conjecture that a fact which belongs to the category (a) with respect to each theory (i.e., does not disprove any of the theories) belongs to a subcritical process with $m < 1$. On the other hand, a fact that belongs to categories A1-B2 with respect to at least one theory, would belong to a supercritical process with $m > 1$. Naturally, the "cascade" of facts, induced by a super-critical process, will not grow indefinitely due to cross-breeding mechanism.

On can apply the same branching process ideas to the process of formulating theories, regarding a new theory as a modification (or "offspring") of an older theory. In fact, when a theory is in a state A1-B2 with respect to a fact it has to be modified, hence replaced by a new theory. Let now $p_0, p_1, \ldots$ be the probability distribution of the number of "offspring" of a theory. We have here $p_0 + p_1 = 1$, so that typically a theory will give rise to one theory only, or become abandoned. Typically, therefore these processes will be subcritical, and the whole process will be supported by "migrant" theories, orioginated according to some Poisson-type stream.

From the above analysis it appears that the process will, in general have a tendency to decrease; there will usually be fewer theories in the next time unit than in the preceding one, and we will observe a "convergence" to a single theory. On the other hand, there will also be periods of high proliferation of theories; this will occur, in general, when the process defined for facts has relatively few elements. There is then a scarcity of new facts, and those which appear are covered by theories. This, intuitively speaking, gives rise to theorizing, hence to an increase.

The assumption that migrant theories appear according to the Poisson process on somewhat higher level suggests a negative association between the processes of facts and theories. Letting X(t) be the number of facts known at t, and by Y(t) the number of theories at t one could conjecture that X(t) and Y(t) are negatively related, so that (pretending they are continuous variables), their derivatives are of the opposite signs: if $X'(t) > 0$, then $Y'(t) < 0$ and conversely.

One of the above implications was justified. The other may be justified by the argument that whenever there are many alternative theories, each new fact has a high chance to "cause trouble" for a number of theories. Thus, as the number of facts grows, the number of theories will tend to decline due to their elimination. One may amplify the above by connecting the rate of elimination of theories with the rate of appearance of fats which lead to situations of type B2 as opposed to "easier" situations A1, A2 and B1.

One of the goals of these considerations was to analyse the effects of "social filtering" of facts, that is, their over- and underestimation. Here the situation is as follows: suppose that two groups of scientists advance two theories. Suppose that a new fact was discovered, and that one group underestimates and the other overestimates this fact. Such a situation may happen, e.g., in a case of competing centers in one domain. Generally underestimation of importance of facts tends to slow down the branching process of fact accumulation, while overestimation has an opposite effect on theories; over- and underestimation will have a tendency to delay or hasten the process of replacement, modification or abandoning a theory.

4.3. BASIC PROBLEMS OF OBSERVABILITY

Problems of observability may be represented as follows. The phenomenon to be observed is assumed to be described in terms of n attributes, say $y_1,\dots,y_n$. To determine the state of the phenomenon at any given time, one needs to know the values of all components.

The constraints on observability may be of three types. Firstly, it is quite common that values of a given variable can be known only up to some precision. In other words, instead of the true value y_1 say, we observe some values y_1^*, where $\varepsilon = y_1^* - y_1$ is the error of observation. Typical assumptions that are made about ε, to be found in any statistic textbook, are that ε has normal distribution, with some mean (zero, if measurements are unbiased), and the standard deviation equals one.

Another kind of restriction on observability is of conditional character: it is typified by a constraint that if y_i assumes a value in some range, then one cannot observe the value of variable y_j (here j may be equal i). In practice such a situation occurs when some values (usually the extreme ones) cannot be observed at all, or prevent observations of some other variable or variables.

Finally, the third type of constraint concerns joint observability; it is typified by the situation where y_i and y_j may each be observed, but not jointly: if one decides to observe y_i (in a given instance of the phenomenon) then one cannot observe y_j. This means, that (in statistical contexts), one may observe only marginal distributions of y_i and y_j, but not their joint distribution.

Of course, all three kinds of constraints may operate at the same time in a given phenomenon. Of most interest is here the third type of constraints. Suppose that one wants to study a random object, such as "epidemic", "inflation", etc. Such objects may be characterized by a (perhaps fuzzy) set of states of a certain system, such as society described in terms of a specific disease, contacts between infectives and susceptibles, etc., or economy, described in terms of supplies of various goods, prices, demands, etc. It will be convenient to refer to the "system" under consideration as carrier.

Thus, a specific carrier may be in a state of an "epidemic", or in a state of "no epidemic"; another specific carrier may be in a state of "inflation", etc. The set of states of the carrier, which are characteristic for an epidemic may be fuzzy or not; we shall return to this point in the subsequent parts of this section.

The main issue now is simply that in describing the state of the carrier at any given time, one has to decide as to which attributes are needed for the description of the state, and which are not. To continue with the example of epidemic, the state description should be sufficiently rich so as to enable one to study a wide class of phenomena covered by "epidemic theory", and yet, sufficiently parsimonious for making it practically usable.

For instance, if we restrict the analysis to one particular disease, say D, the state description might involve:

(a) the specification of the state of health (regarding the desease D) of all members of society. This information has the form of a subset $S' \subset S$, where S is the set of all members of a given society, and S' is the class of all members wo have the disease D;

(b) the specification of all contacts between members of S' and members of S - S', or more generally, between members of S, within some period of time, e.g., last 14 days, etc. This information has the form of a relation $C \subset S \times S$, where sCs' means that s and s' contacted each other in the period of time under consideration;

(c) the location of each member of the society at a given time. This information has the form of a function, say F, which assigns locations to elements of S.

Clearly, such a state description may be too rich for some purposes, and not rich enough for some other purposes. This example illustrates the fact that "completeness" of the state description is a concept relative to the given goal of analysis.

Observability networks

Suppose that the decision about the choice of state description was made. Regardless of the particular form of this description (e.g., specification of the set S', relation $C \subset S \times S$, and function F, as in the case of an epidemic), it may always be assumed that the state description is reducible to the form of a vector $(x_1,\dots,x_n)$ of attribute values. Here

$$N = \{1,2,\dots,n\}$$

is the set of labels of attributes, and for each $i \in N$, we have $x_i \in V_i \cup \{*\}$, where V_i is the set of possible values of the i-the attribute, with * denoting the fact that i-the attribute is "not applicable" in a given situation.

The constraints of observability consist of the fact that not all attribute values may be observed jointly: it may happen that observing a value x_i of i-th attribute in a given instance may make it impossible to observe the value x_j assumed by j-the attribute. Such observability constraints may be related to various causes. Sometimes observations of one variable (attribute) may make it physically impossible to observe some other variable or variables; sometimes the constraints may be due to econmical reasons (e.g., limitations of costs), etc.

Generally, the observability constraints may be characterized by specifying a family, say G, of subsets of N. The interpretation here is simply that if $A \in G$, then it is possible to observe all values of attributes from A, in a given instance (in another instance one may select some other attributes to observe). Naturally, the family G may be time-dependent; for simplicity; however, we assume that G is time-invariant. We assume that G satisfies the following.

HYPOTHESIS 1: If $A \in G$ and $B \subset A$, then $B \in G$,

This hypothesis states that if a certain set of attributes permits joint observability, then the same is true for every subset of this set. To justify this assumption on intuitive ground, it sufficies to rephrase it, by saying that one can always resign from observing some attributes.

DEFINITION 1: We shall say that the attribute $i \in N$ is observable, if there is an A such that $i \in A$ and $A \in G$.

Let N^* denote the class of all observable attributes. Hypothesis 1 allows us to define a natural concept, namely that of maximally observable sets.

DEFINITION 2: A set $A \in G$ will be called maximally observable, if the conditions $A \subset B$ and $A \neq B$ imply $B \neq G$.

Thus, a set is maximally observable if its attributes are observable jointly, but addition of one more attribute to this set makes it not in G. Clearly, the maximally observable sets are not unique; there may exist several of them. Generally, let G^* be the class of all maximal sets. We have then

THEOREM 1: If $A,B \in G^*$, then $A \subset B$ implies $A = B$.

This theorem asserts that maximally observable sets are not comparable with respect to the relation of inclusion. It turns out that the class G^* of maximally observable sets determines uniquely the class G of observable sets; we have namely the following theorem.

THEOREM 2: <u>We have the following identity:</u>
$G = \{A$ such that there is a $B \in G^*$ with $A \subset B\}$.

We may now describe some of the most important contingencies which may occur.

DEFINITION 3: If $i \in \bigcap_{A \in G^*} A$, we say the that i-th attribute is <u>universally observable</u> and the set $C = \bigcap_{A \in G^*} A$ will be called the <u>core</u> of observability.

Each maximally observable set is therefore representable as a union $C \cup B$, where C is the core, and B may be assumed disjoint with C. Let H be the class of sets obtained in this way, so that

$$G^* = \{C \cup B: B \in H\}.$$

DEFINITION 4: We say that a set of attributes A' with $A' \cap C = \emptyset$ is <u>uniformly constrained</u>, if
(a) A' contains at least two elements;
(b) $C \cup A' \in G$;
(c) $B \in H \Rightarrow A' \subset B$ or $A' \cap B = \emptyset$.

DEFINITION 5: We say that the sets A' and A" constitute a <u>mutual tradeoff</u>, if they are uniformly constrained, and for every $B \in H$

$$[A' \subset B \Rightarrow A'' \cap B = \emptyset \ \& \ A'' \subset B \Rightarrow A' \cap B = \emptyset].$$

To explain the introduced concepts, it is best to use an example. Let a,b,c,... denote the labels of attributes, and suppose that the class G^* of maximally observable sets contains the following sets

$\{a,b,c,d,e\}$; $\{a,b,e,f,g,h\}$; $\{a,b,f,g,h,i\}$; $\{a,b,f,g,h,j\}$

Then a and b belong to every set in G^*; in fact

$$C = \bigcap_{A \in G^*} A = \{a,b\},$$

so that a and b are universally observable, and $C = \{a,b\}$ is the core.

Next, $\{c,d\}$ and $\{f,g,h\}$ appear always together: either all of these elements or none of them belong to very element of G^*. This means that $\{c,d\}$ and $\{f,g,h\}$ are uniformly constrained. Moreover, if c and d belong to the observable set, then f, g and h do not belong to it, and conversely, hence $\{c,d\}$ and $\{f,g,h\}$ constitute an observability trade-off. In practice, it means that if one decides to observe both c and d (or one of them), then one cannot observe any of the attributes f, g and h. The remaining elements, e, i and j do not belong to any constrained set.

An example of observability constraints in the social sciences may be provided by the well known experiment of Asch concerning the resistance to social pressure. There are 8 subjects in the experiment, but only one of them is being tested, while the remaining seven are especially instructed to give deliberately false answers (e.g., to some question about length discrimination, etc.). The person tested replies as the last, and he may or may not "give up" under social pressure, replying as the others, or may follow his conviction. Now, before the experiment starts, the resistance to social pressure may be tested for each of the 8 subjects. However, the decision to observe it for one person makes it impossible to observe it for any of the remaining seven, since they must be instructed in a special way, preventing the measurement at any time later. Thus, the maximal observable sets here are singletons, or sets consisting of one person only.

The consequences of observability constraints are best seen when one considers their affect on collection of statistical data.

Suppose that the values of attributes $(x_1,\dots,x_n)$ characterize the objects from some population. The elements of the population are sampled, and therefore the attribute values may be treated as random variables, say $(X_1,\dots,X_n)$, with some joint probability distribution.

$$F(y_1,\dots,y_n) = P(X_1 < y_1,\dots,X_n < y_n).$$

This probability distribution represents, in a sense, the complete knowledge about the laws governing the attributes, their statistical interrelationships, etc.

Now, observability constraints, as specified by the class G of subsets of N, make it impossible to estimate certain joint distributions. For example, assume for simplicty that x_1 and x_2 are observable separately, but not jointly, so that

$$(\text{for all } A \in G)\text{: } 1 \in A \Rightarrow 2 \notin A.$$

Consequently, for each sampled element we must make the decision whether to record the value of x_1 or x_2 (or perhaps none of them); one cannot observe both of them for the same element of the sample. This means simply that there is no access to the joint distribution of (X_1,X_2), even though we may estimate their marginal distributions.

A similar situation occurs for joint distributions of more than two random variables: the observablity network G, and in particular the class G^* of maximally observable sets, determines for which sets of random variables we may estimate their joint probability distributions.

In subsequent sections, the concept of observability network will be combined with the notion of a mask, which specifies (within the observability constraints) the temporal pattern of collecting information, in case when we deal with a dynamically changing random object.

Further constraints on observability

This section will concern some additional constraints on observability, which operate, so to speak, within the observability networks. Suppose that the observer decided, in a given instance, to observe all attributes with indices in a set A, that is, all attributes x_i with $i \in A$. Naturally, we must have $A \in G$, i.e. all attributes which the experimentor intends to observe must be jointly observable. Let

$$A = \{i_1, i_2, \ldots, i_k\} \subset N \text{ and let } x_A = (x_{i_1}, \ldots, x_{i_k}).$$

We may regard x_A as a realization of a vector-valued random variable X_A. The constraints on observability which we shall consider in this section consist simply of the fact that the experimentor observes not the value of X_A, but some (possibly random) function of X_A, say

$$y = h(x_A, \xi).$$

Here h is some function, not necessarily numerical, and ξ is a random distortion of observation (with ξ and x_A not observable separately).

The above scheme is sufficiently general and rich to cover most cases which may occur. One typical case occurs when x_A represents a numerical value of an attribute, which one tries to measure by means of a certain instrument (e.g. a physical measurement tool, a psychological test, etc.). In this case, ξ is a random error and

$$y = h(x_A, \xi) = x_A + \xi.$$

This simple additive model, according to which the observed value y is the sum of (unobservable) true value of the attribute x_A, and (unobservable) error ξ, serves as a basis for large number of statistical techniques of estimation.

Note that in the above setup x_A may be one-dimensional or multi-dimensional; the random disturbance ξ has as many components as x_A, and addition is interpreted as addition of vectors.

Another fairly typical example is simple the grouping of the data. To illustrate the point, assume that ξ is absent, and that the observations are grouped (i.e. assigned to classes of some classification). In this case we have

$y = h(x_A)$ = label of the class to which x_A belongs.

Typically, instead of learning (say) a person's age, one obtains the information that he belongs to the class labeled "Between 25 and 30" etc. In this case, denoting

(*) $h^{-1}(y) = \{x_A : h(x_A) = y\}$

we know, after observing y, that the attribute value x_A belongs to the above set.

A very common situation here is when h assumes linguistic values, and the set $h^{-1}(y)$ is fuzzy. Returning again to the example with x_A being the person's age, the observer learns, instead of the value x_A, only the value $h(x_A)$ = old, etc. Here h^{-1}(old) is a fuzzy subset of the real axis, with membership function discussed extensively by Zadeh, starting with his earliest papers on fuzzy set theory.

Generally, when the observations are disturbed by the function $h(x_A,\xi)$, the situation may be described as follows. In all cases of practical importance, one is either after some "truth", or one is trying to make a decision, which is to be optimal from the point of view of some criterion. The truth may be identified here with the (unobservable) value x_A of the attribute of attributes, or alternatively, with the probability distribution of x_A. The function $h(x_A,\xi)$ represents then the restrictions conderning the access to the truth: the effects of random deviations from the truth, and the effects of destroying information by grouping, classification, use of fuzzy terms, etc.

If one uses the knowlegde of x_A for making the decision, the situation may be described as follows. Suppose that if x_A were known, then the optimal decision (from the point of view of a given criterion) would also be known. Since x_A is unknown, and only $y = h(x_A,\xi)$ is observed, one cannot, in general expect to do as well as if x_A were known.

Suppose for simplicity that the function h does not involve random component, so that $y = h(x_A)$, and that the sets $h^{-1}(y)$ defined by (*) are not fuzzy. Numerous ways have been suggested for measuring the loss of information due to the use of function h. To present one of them, based on the principle of "regret" from decision theory, assume that the available decisions form a set D, and that with each decision $d \in D$ and attribute value x_A one may associate the "loss" $L(d,x_A)$ which one suffers if one makes the decision d under the "true state" x_A.

In the sequel, we assume that the function L is nonnegative. Moreover, assume that for each x_A there exists the "appropriate" decision $d(x_A)$ with $L(d(x_A), x_A) = 0$. Thus, if xA were known, we would be able to find the decision which yields no loss (hence is optimal, since L is assumed to be nonnegative).

Now, because of the constraints of observability, the decision d has to be based on the observed value $y = h(xA)$. Let us denote this decision by $d(y)$, so that the decision procedure is a function which maps the set of values of h into D. In other words, the decision procedure must be defined on the class of all sets of the form $h^{-1}(y)$, or still differently, must be a function of xA which is constant on sets of partition generated by h.

Now, if we make the decision $d(y)$, while the value of the attribute is x_A (and such that $y = h(x_A)$), then the loss is $L(d(y), x_A)$, and the maximal loss which may occur is

$$R(y) = \sup_{x_A \in h^{-1}(y)} L(d(y), x_A).$$

One may then take as a measure of the loss of information due to the use of function h, the value

$$R = \sup_{y} R(y).$$

The measure above is somewhat pessimistic, in the sense that it assigns the same weights to all $x_A \in h^{-1}(y)$. In reality, one can usually consider the conditional probability distribution of x_A given y, and then take the expected value of $L(d(y), x_A)$, instead of supremum.

4.4. THE CONCEPT OF MASK

Generally, when studying a certain phenomenon, the observer faces the following situation. The phenomenon may be represented by a family of interrelated random variables, say $\{X_w, w \in W\}$, where W is some parameter set. The elements of W are labels of observable random variables, and the values of these random variables represent the results of experiments or observations. Sometimes the observed value is regarded as "distorted", or biased, observation of some underlying "true" value.

The observer's first choice is to define operationally the phenomenon which he studies. This choice depends on the goal of research, and even for the same goal the choice need not be unique. This choice consists of selecting a finite number of elements of V, say $v_1,\dots,v_N$, which he intends to observe, and which (in his opinion) "cover" the phenomenon. To give an example, suppose that the phenomenon in question is generally termed "measles". Various variables which one might wish to observe concern the individual course of illness, dynamics of appearence of symptoms, various facts pertaining to immunity, etc., epidemiological properties, like spread of measles in a given community, and so on. The choice of variables depends on the goal of study, and for the same goal, two researchers are likely to choose different variables. Whether or not the selected variables "cover" the phenomenon, in the sense that they provide information sufficient to attain the goal, is usually impossible to determine a priori with full certainty.

Suppose now that the researcher made his decision, say $v_1,\dots,v_N$ of labels of variables to observe, so that his object of interest are variables $[X_{v_1}(t), \dots, X_{v_N}(t)]$. This set of variables provides then a <u>state description</u> of the phenomenon. As a rule, these variables are interrelated, i.e. constitute a <u>system</u>. To collect information about the interrelations, one needs a sample of data about the values of these variables for a given set of moments. The temporal patterns of a sample point is a mask, as defined by Klir (1972). Thus, assuming for simplicity that time is discrete, a mask is a set of pairs of the form (v_i, k_{ij}), where k_{ij} are some nonnegative integers. The intended interpretation is such that the variable with label v_i is to be observed at each of the times k_{ij}.

For example, if the mask consists of pairs $(v_1,2)$, $(v_1,3)$, $(v_1,5)$, $(v_3,0)$, $(v_4,0)$, $(v_4,1)$ and $(v_4,5)$, then the random variable X_{v_1} is to be observed at times $t = 2$, $t = 3$ and $t = 5$, the random variable X_{v_3} is to be observed at $t = 0$, and the random variable X_{v_4} is to be observed at times $t = 0$, $t = 1$ and $t = 5$. Other random variables are not observed under this mask.

Denote now, for a given mask M

$$t_M = \min_{i,j} k_{ij}$$

and

$$h_M = \max_{i,j}(k_{ij} - t_M).$$

The moment t_M will be called the anchoring time of the mass M, and h_M will be called the horizon of mask M. We shall say that mask M strictly precedes mask M', if

$$t_M + h_M < t_{M'}.$$

This condition means that the collection of data according to mask M' begins after the collection of data according to mask M is terminated. It follows in particular, that if M strictly precedes M', then the sets of pairs in masks M and M' are disjoint.

Formally, a mask is simply a finite subset of the Cartesian product $V \times T$, where T is the time axis. Let F_M be the class of all masks M such that the set of variables $\{X_v(t),\ (v,t) \in M\}$ is jointly observable; the elements of F_M will be called <u>feasible masks</u>. In the sequel, all considerations will be restricted to feasible masks. If M is the mask, let

$$V(M) = \{v \in V:\ (v,t) \in M \text{ for some } t\},$$

so that V(M) is simply the class of all variables which are to be observed under mask M. Furthermore, for $v \in V(M)$ let

$$T_v(M = \{t \in T:\ (v,t) \in M\}$$

be the <u>temporal trace</u> of the variable with label v.

Finally, we introduce the following definition. Two masks, M and M' are said to be <u>temporally equivalent</u>, $M \sim M'$, if there is a t^* such that

$$(v,t) \in M \text{ iff } (v,t + t^*) \in M'.$$

Obviously, the temporal equivalence is an equivalence relation, i.e. it is reflexive, symmetric and transitive, so that the class of all masks may be partitioned into subclasses of temporally equivalent masks. We have the following theorem.

THEOREM 3: If $M \sim M'$,
then $V(M) = V(M')$, and the temporal traces of each variable $v \in V(M)$ are translations of one another by the amount t^*.

We have also

THEOREM 4: If $M \sim M'$, and $t^* > h_M$,
then M strictly precedes M', and if $t^* < -h_M$, then M' strictly precedes M.

To appreciate the variety of situations in which different types of masks are applied, it is best to consider some typical examples.

1. Systems with reset possibility.

Consider a simple system with two components, labeled v_1 and v_2, and a control variable labeled v_3. In the deterministic case, the state of the system at t, i.e. the triplet $[X_{v_1}(t), X_{v_2}(t), X_{v_3}(t)]$, determines uniquely the values $X_{v_1}(t+1)$ and $X_{v_2}(t+1)$, so that

$$X_{v_1}(t+1) = f_1[t, X_{v_1}(t), X_{v_2}(t), X_{v_3}(t)]$$

and

$$X_{v_2}(t+1) = f_2[t, X_{v_1}(t), X_{v_3}(t), X_{v_3}(t)].$$

Typically, the functions f_1 and f_2 are not known, and one of the main issues is to determine such properties of f_1 and f_2 which would enable one to optimally control the system, i.e., find a rule $d = d(t, X_{v_1}, X_{v_2})$ of setting the control variable at time t+1, which optimizes the performance of the system, the latter measured by some known function of the sequence $\{X_{v_1}(t), X_{v_2}(t), t = 1,2,...\}$. The problem stated in such two variables, and more than one control variable.

Obviously, if the unknown functions f_1 and f_2 depend on t, there is not much that can be done. However, often one may assume stationarity, in which case they do not depend on time.

The functions f_1 and f_2 may be determined, at least partially, if one may reset the arguments X_{v_1} and X_{v_2} (inputs) and X_{v_3} (control) at some values of interest, and observe the subsequent values $X_{v_1}(t+s)$ and $X_{v_2}(t+s)$ for a given control sequence. The mask tells us which values to record.

In case of a deterministic system, one such observed pattern may be sufficient to determine the values of the functions f_1 and f_2 for given inputs. In case of non-deterministic systems, one may need a series of "identical" observations, i.e. apply each time a mask which is equivalent to the original one in the sense of time translation. For such observations one needs to be able to "reset" the system at will for some desired set of initial variables.

Often the system which one studies may appear in a number of independent "copies", in which case one may think of each copy as being "reset", but without the possibility of controlling the input variables. For instance, in assessing the long-term effects of some medical treatments, the patients are asked to report to the hospital for a follow-up study. A mask may require that they report for a checkup (and measurement of some variables of interest) at specific times, such as every two weeks during the first 3 months, then one a month for a year, and then every 6 months, etc. Here the masks for each patient are the same up to the choice of the initial moment, so that they are equivalent. In this case, the masks do not have to precede one another, as in the case of one system which must be reset when new observations start.

2. **Stationary processes.**

Resetting the system may be possible in cases of some instrument of device which may be "stopped" and observed, so to say, "from the beginning". Often this is not possible. Imagine, for instance, a system of price changes of some commodities, weather characteristics in several localities, etc. Here we deal with a process (which may or may not be under partial control) which occurs only once, and cannot be repeated. In such a case, a reasonable sampling scheme would call for masks $M_1, M_2, \ldots$, with $M_i \sim M_{i+1}$ fopr each $i = 1,2,\ldots$, and such that t^* is positive. Whether or not each mask M_i ought to be strictly preceding M_{i+1} depends on the statistical procedure employed. In case of strict precedence, the sample points do not overlap. In the opposite case, it may happen that $M_i \cap M_{i+1} \neq \emptyset$, and the sample points may be linked into a chain.

3. **Random masks.**

Often the mask becomes changed in the process of taking the observations, i.e. one may decide to take some additional observations, of new variables, or of the variables observed until a given time. Such a situation may occur, e.g. in case of follow-up medical studies mentioned above. Thus, one may decide to increase the frequency of checkups, or take some additional measurements when observing some abnormal or otherwise interesting results. In this case, the plan calls for amending the mask, depending on the results of observations.

To describe it formally, such a random mask may be characterized inductively by a function K, whose arguments are sets of values observed up to time t (including the empty set), and its values are pairs (V',t') with $V' \subset V$ and $t' > t$, or with value $\emptyset$.

The rule K is to be interpreted as follows. Let $K(\emptyset) = (V',t')$. This means that we are to take the observations of variables $X_v(t')$, $v \in V'$. Given these values, let $K[X_v(t'), v \in V')] = (V'',t'')$. This means that, having observed the values at t', we should now proceed to observe the values $X_v(t'')$, $v \in V''$, and so forth. Eventually, when the value of K at some set of arguments is $\emptyset$, we stop taking the observations. The resulting mask, i.e. the set of values to be observed and the times of taking these observations, is random, and depends on the actually observed values. A typical example of such a mask is the process of sequential testing of hypotheses, where the mask is simply (assuming one observation per unit time) $t = \{1,2,...,N\}$, where N is the random number of observations.

The collection of data observed through a mask or a set of masks is the data base for the study of the phenomenon in question.

The variables observed through a mask are on various levels of generality. Given a mask M and the set V(M) of observed variables, the latter partitions among various generality levels. If V(M) is contained in one level of generality, the mask may be called homogeneous, in the opposite case it may be called heteroneneous (with respect to generality levels). Since masks are sets in $W = V \times T$, one may consider various set-theoretical operations, so that one can speak of intersections, unions, etc. of masks. In particular, if one chooses a subset of a mask on a given level of generality, one obtains a homogeneous mask.

A mask may be called faithful, if the set V(M) contains all variables which are relevant for a given phenomenon, or for a given goal of study. In this sense, faithfulness is a goal-dependent concept.

Given a class of semantically interrelated goals, and a class of faithful masks, one may consider their intersection, and call it the base of faithfulness. In this set, each variable belongs to each goal, hence one may call such a mask invariant with respect to the change of goals. If the estimated value (on the basis of a mask) is close to the true value, we sya that the mask is statistically faithful, or a posteriori faithful, as opposed to the prior faithfulness discussed above.

Still another type of faithfulness may be identified with the robustness of the used method of estimation, i.e. the sensitivity to the deviations from the true value of the parameters of the phenomenon.

The relevance function may change in time, and also as the result of experiments, so that the concept of faithfulness also changes: the mask which was faithful at a given time need not have this property at some other time.

Finally, let us mention that since the masks are used sequentially, they form strings of masks (for the same phenomenon). The class of all finite strings of masks which are admissible for a given phenomenon constitute the language of masks, in the sense to be found in Nowakowska (1984). In fact, we have here a fuzzy language of masks, if one considers grades of admissibility. One may also, using the criteria of admissibility, select subclasses which satisfy to a given degree the criteria of faithfulness and exactness. One could imagine further restrictions of these languages, by ethical or deontic valuations.

It is important to mention that in this case one can carry over some of the ideas of formal action theory (Nowakowska, 1973) especially in its fuzzy version, e.g. concepts of decisive moments, praxiological sets of masks, complete possibility, periodicity, and in particular, the analysis of homonymity and synonymity.

One could hope that the outlined theory of masks and observability may turn out to be fruitful in the foundations of statistics, where the notions of observability and theory of experiments are still not uniformly developed, and there is a lack of unified formal foundations of the theory of observations. Also, the above concepts may prove useful in the theory of mathematical modelling.

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CHAPTER 5

STAGGERING KNOWLEDGE IN A PARALLEL EXPERT SYSTEM

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With an increasing number of rules in a knowledge-based system the process time will slow down. A parallel processing approach would seem appropriate for speeding up performance. This paper aims at selecting the appropriate parallel architecture which will produce a required throughput for the minimum increase in cost. The decision can be made most rationally by simulating alternatives, to see what performance can be expected from them. The results are based on the simulated parallel knowledge-based system at Delft University of Technology: HYDRA.

5.1. PARALLEL KNOWLEDGE-BASED SYSTEMS

A characteristic feature of expert systems is that the inference mechanisms and the knowledge are treated separately. In most expert systems, knowledge is stored in the form of rules. This approach has the advantage that the knowledge is easily expressible and expandable. Rules, being modular, allow augmenting the knowledge incrementally and so to improve the model's verisimilitude.

The most prominent reason for parallelization is increase of speed. Using this technique, a given problem is split into subproblems, which are solved by processes operating almost independently and in parallel. The difficulties lie in achieving independence (Waldinger, 1977) and in handling conditions adequately. These difficulties are worth solving because parallelization can result in surprisingly good performance (Kornfeld, 1982).

Among the early writings in this area are papers by Corkill (1979) and Konolige and Nilsson (1980). But to our best knowledge none of the proposals have been implemented. Corkill generalized NOAH by allocating various high level goals among processes. This is immediate since NOAH assumes independence among goals. The Konolige and Nilsson paper generalizes STRIPS by permitting multiple robots to model each other's planning process. In rule-based expert systems, the rules are loosely coupled. In the next section we point out that we have adopted the architecture of one inference engine parallelized over an adjustable number of rule processors. Remember: the rules themselves are loosely coupled.

An Agenda-Driven Parallel Knowledge-Based System

When building a parallel knowledge-based system the designer must be on the alert not to introduce a bottleneck in the form of a Master Control governing all (mutually parallel) inference engines. A centralized-control approach with such a Master Control is expected to be overloaded by the many tasks concerning jobs (selecting, assigning), control signals (receiving, sending), results (accepting, bookkeeping) and administration (updating, interrogating). Therefore, we have developed a decentralized approach to parallel KBSs (Groen et al., 1985 a).

The chief idea is to parallelize one inference engine into several administrative processes and a number of rule processors, performing only very specific tasks. In the expert system, termed HYDRA (HYpothesis Deduction through Rule Application), this idea is realized. Its fundamentals will be briefly discussed below; an extensive description can be found in Groen et al. (1985 a, 1985 b).

In an agenda-driven parallel knowledge-based system, the following data structures are important: Data Base (DB), Rule Agenda (RA), Rule Base (RB), Question Agenda (QA) and Question Queue (QQ). Five different processes can be distinguished: Data Base Manager (DBM), Rule Agenda Manager (RAM), Rule Processor (RP), Question Agenda Manager (QAM) and User Interface (UI). With the exception of the RPs, the processes appear only singly, the RPs may act with any number of copies allowed by the architecture.

In figure 1 the design of an agenda-driven parallel knowledge-based system with an inference engine consisting of a Rule Agenda Manager (RAM) and a number of simultaneously operating Rule Processors (RPs) is presented. The RPs find their tasks in the Rule Agenda (RA) administrated by the RAM. Whenever a RP can find neither a proof nor a disproof of a rule, nor other rules in the Rule Base (RB) which might be of help, it sends a question to the Question Agenda Manager (QAM). The QAM arranges the Question Agenda (QA) and communicates with the User by means of the User Interface (UI).

The Rule Agenda

The rule agenda is a concept adopted from Lenat (1982). Its structure has been kept simple. For every rule in the rule base, an entry with two fields is created: a status field and a priority field. Each status field may assume one of three values (0, or 1, or 2):

0: meaning that the rule is neither proved nor disproved, and therefore is due for processing;

1: meaning that the rule is being processed;

2: meaning that the rule has been proved or disproved, and therefore its conclusion acquires the status of a fact.

Rationally, we only allow a single rule to be processed by one rule processor at a time. The value of the priority field (a measure of the importance of the rule to be processed) is dependent on the strategy implemented. Of course, an optimal priority function is related to the structure of the domain (Davis, 1982). In the current implementation, however, a plain strategy is employed. Due to parallellization, interruptions of the search process may occur even when acting in a promising part of the search space. This type of interruption is called the staggering effect. In order to prevent staggering special strategies might be developed; although they lead to more complex structures they have proved to be suitable in some domains (Henseler et al., 1986).

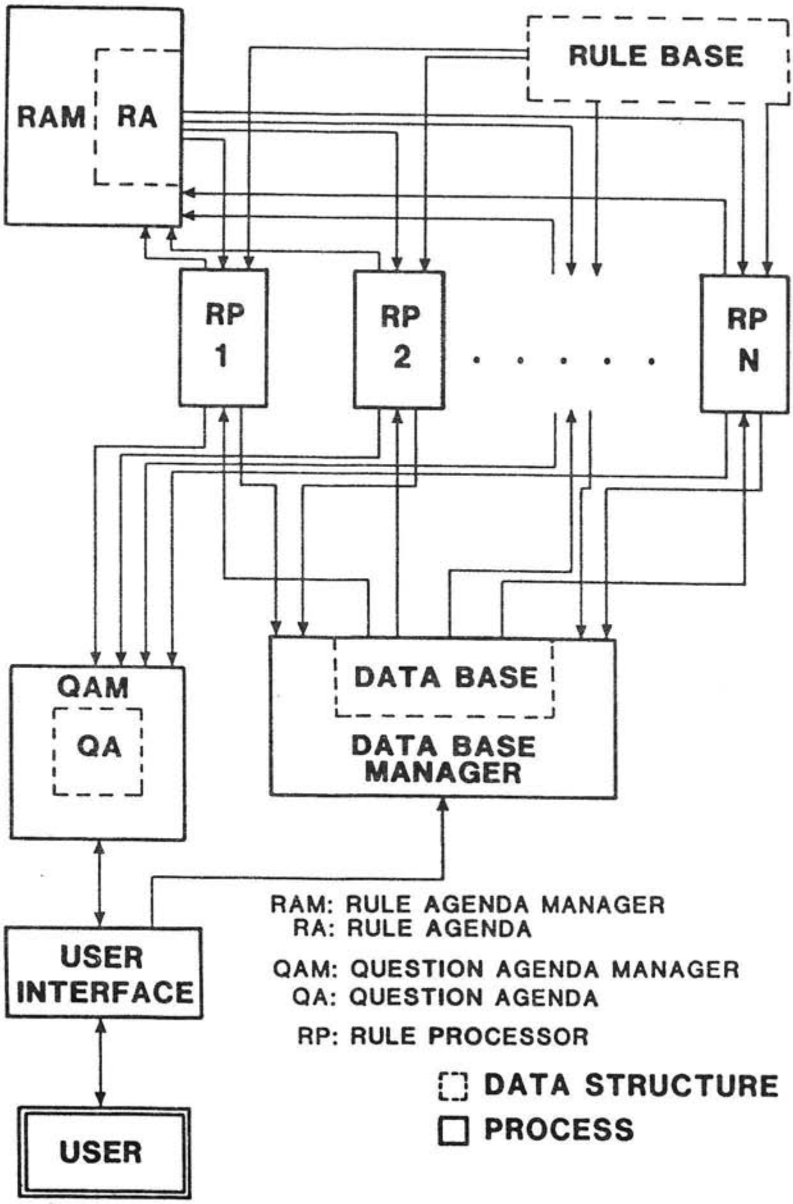

Figure 1: An Agenda-Driven Parallel Knowledge-Based System

The Tasks of a Rule Processor

In the parallel knowledge-based system described above, a rule processor is generally engaged in trying to (dis)prove a rule. As soon as a rule processor is free, it consults the rule agenda for the next rule (with highest priority) to be processed. A rule processor performs its tasks in the following order.

1. Take a rule with the highest priority from the RA.
2. For all clauses inspect whether they are (dis)proved facts.
3. IF definite answer THEN send results to RAM and DBM [rule is proved or disproved], and terminate.
4. IF no definite answer THEN
   [inspect RB for further applicable rules and
   send result to RAM]
   [the rules selected will be included in the RA with their priority values updated], and terminate.
5. IF no further rules are applicable THEN ask the user via the QAM, and terminate.

Depending on the results of such a rule processor one or more of the following actions have to be executed:

- updating the data base;
- updating the rule agenda;
- updating the question agenda.

Accessibility

It will be obvious that access to the RA is exclusive to the RAM and the set of RPs. We allow concurrent reading but exclude all reading while writing. Concurrent reading in the data base is permitted to the RPs. Writing in the data base is the privilege of the DBM receiving its information from the rule processors or the user interface. In relation to the QA the RPs' output is serialized by the QAM.

5.2. HYDRA

Parallel computers and parallel programs dealing with knowledge are still in their infancy; therefore, many researchers resort to simulating a parallel computer for their parallel programs. The methodologies for designing the best, e.g., optimal in time, parallel algorithms are still in discussion. While looking for an optimal algorithm the investigation of the overheads caused by extra search, communication and synchronization must be combined with an analysis of the scheduling strategy (Marsland and Popowich, 1985). Efficient parallel computer programs are shown to be dependent on organizational structures as well as on heuristics guiding the order of the processes.

The absence of an appropriate multiprocessor architecture induced us to simulate the parallel KBS on a uniprocessor system. The program HYDRA is written in Modula-2 (Wirth, 1985) and runs on a PDP 11/45 of the Computing Centre of the Delft University of Technology. Inputs to HYDRA in its present form are:

- the number of RPs;
- the length of QA queue;
- the name of the initial data base to be considered;
- the name of the rule base to be considered.

HYDRA with one RP and with the RAM strategy outlined is, almost, a conventional backward-chaining system with the due acceptance of staggering. Still, even in a backward-chaining system, the order of questions asked depends on its strategy, which may vary all along the path from depth-first to A^* or AO^* (Rich, 1983), hence staggering, though inconvenient, is not at all peculiar to HYDRA.

Relations between processes and data structures

In order to improve the performance of a parallel KBS or even to optimize it, a careful examination of the organization structure with respect to the relations between processes and data structures is required. For this purpose, it is important to know which processes operate on the distinct data structures. Which process is reading (R) or writing (W) in a data structure and how is this action performed: by direct access (D) or by sequentially following the complete data structure (S). In table 1 an overview is presented.

data str. / process	DB	RA	RB	QA	QQ
DBM	(W,D)				
RAM		(W,D)			
RP	(R,D)	(R,S)(W,D)	(R,S)		
QAM				(R,D)	(W,S)
UI				(W,D)	(R,S)

Table 1: How processes make use of the data structures in HYDRA

Legend: (R,*) means reading
(W,*) means writing
(*,D) means direct access to the data structure
(*,S) means sequentially following the data structure

In table 2, the communication and the direction of the information flow between the processes in HYDRA are outlined. Two types of communication are distinguished: direct (X) and indirect (*). When the communication is direct, the receiver will process the information as soon as possible; in case of indirect communication the receiver itself decides on when to process the information delivered.

receiving process / sending process	DBM	RAM	RPi	RPj	QAM	UI
DBM			*	*		
RAM			*	*		
RPi	X	X		*	X	
RPj	X	X	*		X	
QAM						*
UI	X				*	

Table 2: The communiation between processes in HYDRA.

Legend: X means direct communication * means indirect communication

In table 2, it is made clear that HYDRA's decentralized - control approach leads to a structure of loosely coupled processes. Experiments have demonstrated that increasing the number of RPs raises the work load of the RAM (Groen et al. 1985 b). In a parallel environment, the RAM's access time to the RA is in competition with any RP's time. When every RP has to wait 100 msec due to the RAM's activity, the solution is postponed with 100 msec.

5.3. EXPERT DIAGNOSES

Before entering upon a more detailed description of the simulated architecture, we will provide some performance results. Consider, as a small but telling example, a FIFO case with a QAQ length 1 and a single processor, acting on a well-known animal-knowledge base (Winston, 1984) of 17 rules. There are 13 hypotheses and 43 distinct premises. Assume it is to be proved that the animal in question is a penguin.

HYDRA now reaches its goal by successively asking the eight questions

- Is the animal a bird?	(yes)
- Is the animal milk-yielding?	(no)
- Does the animal have hair?	(no)
- Is the animal oviparous?	(yes)
- Can the animal fly well?	(no)
- Is the animal long-necked?	(no)
- Can the animal fly?	(no)
- Can the animal swim?	(yes)

By varying the parameters, we obtain an indication of the efficiency of interrogation under various assumptions. Results are summarized in Table 3.

Th most important remark is that the correct hypothesis always is proved in the end! A star in the table indicates that the question last posed was irrelevant because the final answer was found previously by another branch of the search graph. The difference, etc. in the number of questions needed may be explained by the staggering effect discussed above, when the system is acting on a human interrogee.

# of RPs / QAQ Length	1	2	3	4	5	10	15	20
1	8	7*	7*	7*	7	8*	8	8
2	9*	10*	9*	10*	9*	9*	8	9*
3	9*	8*	11*	11*	9*	9*	11*	10*
4	9*	8*	14*	11	13*	8*	10*	10*
5	9*	9*	15*	11*	12*	9*	10*	10*

Table 3: Number of Questions Asked.

A second example reveals an unexpected but essential consequence of our parallel approach. HYDRA turns out to impose high demands on the consistency of the rule base. While a sequential machine ran the flora-knowledge base, it always found one definite answer, due to implicit sequentialization of the rules within the rule base. However, this ranking being explicit by the accidental fact of a single RP, was not a true characteristic of the system. Hence, HYDRA running on 7 parallel RPs came to a different conclusion; details can be found in Groen (1985 b).

5.4. PARALLEL COMPUTER SIMULATION

On our way to achieve the aim of evaluating a parallel knowledge base computer, three different simulation architectures have been implemented and examined on their merits. The first attempt, a 'Concurrent Process Handler', was to simulate a parallel program by means of concurrent programming techniques (Ben-Ari, 1982). Subsequently, to enable recording running versus idle times, a 'Time Slicer' was developed. This approach revealed severe drawbacks, pointed out below. Finally, a 'Parallel Computer Simulator' was designed and implemented. All three approaches are similar in their mechanisms for mutual exclusions and synchronization, but differ in transfer mechanisms. In all approaches, the processes to be parallelized are put on a (circular) process list in order to provide the scheduler with easy access to the processes.

Concurrent Process Handler

Initially, a Concurrent Process Handler (CPH), determined the order of the processes: RAM, DBM, one of the RPs, etc. The scheduler of the CPH uses a random number generator to choose which process is to be activated next. When a running process calls a procedure for performing some action critical for synchronization, this procedure in its turn will call the scheduler for transfer of control.

The main disadvantage of the CPH approach is the impossibility of including a clock and subsequently of estimating the impacts of parameters, such as the number of rule processors, the number of ports to the data base, the strategy in the RAM, the length of the QA queue, etc. Such an estimate presupposes exact measurings of the run and idle times of the parallel processes implying that a clock device is required. So the Concurrent Process Handler approach was dropped.

Time Slicer

In order to estimate the parameters' impact a time slicer was built (Varkevisser, 1986). It counts slices in multiples of n-hundreds of μs, n to be set by the user. It accumulates delays before individual actions take place and it records them as idle times. Moreover, it provides estimates of the various processes' idle times. As said the processes are placed in a circular list and the scheduler inspects them in a round robin order, imposed by the list. The scheduler is activated by a clock interrupt after a full slice has passed. The transfer of control is by a clock interrupt, thus avoiding one of the CPH drawbacks; this transer mechanism is therefore implicit.

In most experiments, we have assumed the slices to be of the same size for every process; we have experimented with time-slice sizes which may vary with the nature of the processes, but this only complicates matters. In all cases, the time slicer turned out to be accompanied by disadvantages, arising mainly from the fact that the results are dependent on the way in which the processes are ordered in the process list. From laborious experimenting with this order dependency we concluded that the method of a time slicer does not yield reliable simulation results of parallel processes. Therefore, a new method was introduced.

Parallel Computer Simulator

To avoid the disadvantages of the time-slicer approach, another way of scheduling was developed in order to obtain more reliable estimates of the various processes' idle times. The approach, named the Parallel Computer Simulator (PCS), is based on the allocation of an own time space for every process. So far, no shortcomings have been discovered in this approach (Stoop and Varkevisser, 1985). Since the PCS strategy results in a rather complex transfer of control, partly due to the presence of loosely coupled processes as well as independent processes, we shall elucidate its behaviour applied on the parallel processes given in figure 2.

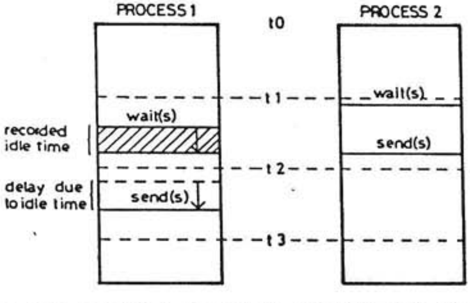

Figure 2: Two Parallel Processes and its Real Simulation

Execution time (run time and idle time together) can be measured in absolute time units (abstime), indicating to what point processes have advanced. The abstime of each process, accordingly called abstime 1, abstime 2, etc., is initialized at zero. The general strategy of scheduling is to select the first non-waiting process with the smallest abstime.

For convenience we have placed some marks in the time space, e.g. $t_0 = 0$. Initially, abstime 1 and abstime 2 are both zero. The scheduler selects process 1 to be activated. Execution continues until wait(s) is called. The call wait(s) is a two-stage procedure. The first stage serves only to announce the "real" wait(s) execution. In the first stage of the wait(s) procedure the scheduler is activated. According to its general strategy, process 2, now with the smallest abstime, is selected. Process 2 is executed until wait(s) is called, the first stage of which activates the scheduler again. The scheduler sees that abstime 2 is still smaller than abstime 1 and therefore (re-)activates process 2. The second part of its wait(s) call is executed and process 2 can perform its critical actions until send(s) is called. The procedure send(s), also built up of two stages, activates the scheduler before executing the "real" send(s) procedure. Since abstime 1 is smaller than abstime 2 the scheduler activates process 1. Process 1 proceeds to execute the "real" wait(s) procedure (the second stage of wait(s)) and finds the semaphore s occupied (in this instance by process 2). Process 1 puts itself in a wait position and activates the scheduler; meanwhile abstime 1 should increase as before (of course, run time and idle time are registered separately). Since only process 2 is non waiting it is activated. It executes the second stage of the send(s) call (the "real" send(s) call) and sets the semaphore s free. With the information available, it is easy to establish the abstime 1 and so the idle time of process 1, and thereafter to change the state of process 1 from waiting to non-waiting. The idle time of process 1 is equal to the difference between abstime 1 and abstime 2; abstime 1 becomes abstime 2.

The rest of process 2 as shown in figure 2 is not coupled with process 1 and, therefore, treated as an independent process; process 2 may finish, then the scheduler is activated. Since process 1 is non-waiting and has evidently the smallest abstime, it is activated and performs its delayed critical action. At the end of the critical action it passes the two-stage send(s) procedure via the intermediate actions of the scheduler as described above and finishes all its actions.

In figure 2, the shaded area indicating the idle time of process 1 is identical in size to that of process 1 in case of real parallelism. Furthermore, it is situated at the same moment of time. The example presented gives insight in the way in which complex cases with n loosely coupled processes are treated by the PCS. Although no formal proof can be provided that the PCS always performs the simulation correctly, it seems to us that the PCS appropriately simulates a parallel computer with n (loosely coupled) processes (Van den Herik, Stoop and Varkevisser, 1986).

Finally, we would like to remind the reader of the potential presence of independently acting processes. To deal with these processes we have also adopted the technique of implicit transfer of control by time slicing in the scheduling mechanisms.

5.5. PERFORMANCE ANALYSIS OF THE PARALLEL COMPUTER SIMULATOR

In order to examine the impact of different processes we ran HYDRA with a variable number of RPs. At every run we let HYDRA deduce the same item in the flora-knowledge base; in the examples below it is the black pine tree. A statistics program was built for analyzing performance of the PCS. Time is measured in multiples of n-hundreds of µs. To eliminate influence of human feed-back in tne results, all answers to questions to be asked by HYDRA (concerning the black pine tree) are stored in a file accessible to the user interface. The statistics program also measures the idle times. In table 4 we list measurements on the processes when HYDRA runs with 1,2,3,4,5,7 and 15 rule processors.

# of RPs	TOTAL	RAM	QAM	DBM	cumul. RP	user interface
1	58975	4931	1881	4400	58049	8810
2	43424	6275	2319	4514	73492	9738
3	39100	7358	2248	4605	83895	10187
4	39076	7148	2181	4585	116325	9825
5	39251	6714	2178	4536	150223	9630
7	42498	6539	2037	4311	225363	9199
15	57225	9403	2050	4281	520271	9288

Table 4: Run and Idle Times with a Different Number of Processors

Table 4 shows that the use of two processors decreases the time it takes for HYDRA to solve the problem substantially, namely from 58975 to 43424. However, the idle times of the rule processors increase, resulting from the

RPs' obliged waiting. The total run time of the RAM shows that with more than one RP it is more often updating the rule agenda.

This tendency is continued in case of three RPs, where the solution time is further reduced to 39100 and the idle time of the processors further increased. Although the solution time is approximately the same with four RPs, the load balancing (not in the table!) over all rule processors is not as good as with three RPs, neither is the idle time as well-balanced as with four RPs (Groen et al., 1985 b).

In case of five RPs, the load balancing for the RPs is improving and the idle times are decreasing. Still, the solution time is slightly increasing even though there are more rule processors to do the job.

Augmenting the number of rule processors worsens the situation; as can be observed in Table 4, the solution time increases slowly. The more rule processors are involved, the more work will have to be performed by the RAM. When as many as fifteen rule processors are in use, the run time of the RAM increases considerably. The reason is that the rule processors do not only search the fruitful branches of the search graph but also enter blind alleys.

The primary goal of the architecture is to parallelize the inference engine. A first result aimed at when studying a model simulating this architecture is to predict the RP utilization for the work-load. The problem is kept constant over all experiments, so that the RP utilization only tells how much the rule processors are being used; this does not provide us with insights on the behaviour of an RP when following a succesful path.

Given the PCS as described in Section 4 it is possible to determine RP utilization, and thereby relate hardware cost to performance. Determination of the performance of alternative configurations is a first step in the design process. Of course, we want to achieve maximum performance improvement at minimum cost. The utilization U of the RPs is therefore computed from the data of Table 4 by the formula

$$U = \frac{1}{N} \sum_{i=1}^{n} \frac{\text{RUN TIME}(i)}{\text{TOTAL TIME}(i)},$$

where N is the number of RPs. The results are displayed in Table 5. The speed-up factor in abstime related to the number of RPs is listed in Table 6.

No. of RPs	UTIL. in %
1	98.4
2	84.6
3	71.5
4	74.4
5	76.5
7	75.6
15	60.6

Table 5: Number of Rule Processors and their Utilization Percentage.

No. of RPs	SPEED-UP
2	1.36
3	1.51
4	1.51
5	1.50
7	1.39
15	1.03

Table 6: Number of Rule Processors and Computational Speed-up.

The utilization percentage of RPs also represents the cost-effectiveness. But if speed-up is a prime requirement then weighting utilization with speed-up yields the relative maxima at N = 2 and N = 5 rule processors.

5.6. CONCLUSIONS

A remarkable result of our experiments is that n simulated parallel rule processors do not only improve the processing speed, but also influences the accuracy of the deductions. The HYDRA concept for parallel machines therefore imposes high demands on the consistency of the rule base as a whole. Moreover, it is shown that additional rule processors lead to an increase of deduction speed up to a certain point. We can conclude that the optimum number of rule processors is to be found at two or five. In chess-tree searching the concept of each processor working at the same depth begins to fail beyond four processors (Hyatt, 1985). Our experiments with HYDRA's QAQ length provides a small but telling indication that storing questions in a queue may stimulate the staggering effect in stead of decreasing it; the latter could be assumed by expecting a clustering of connected questions, which turned out not to be the case at all. When using more than four processors it is shown that, with the present algorithm and the flora-knowledge base, the performance cannot be improved by adding more rule processors. However, the complexity of the search graph, determined by coherence of the rules, may vary between domains, implying that other domains can have an other optimal number of processors.

We may conclude, that simulation of a parallel machine on a sequential machine using the Parallel Computer Simulator provides an excellent testing ground for unstructured models.

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CHAPTER 6

APPLYING THE METASYSTEMS PARADIGM TO IMPROVE OUR KNOWLEDGE ABOUT KNOWLEDGE

John P. van Gigch

The difference between ordinary knowledge and metaknowledge is outlined. Because of its nature and how it is produced, ordinary knowledge is fleeting, whereas metaknowledge is creative and leaves an impact upon scientific discovery and what is known about the world. To be an innovation, knowledge must be the result of paradigmatic rationality which can only take place at higher levels of logic in the matesystem. We first explain the metasystem, several knowledge producing processes and proceed to demonstrate the necessity of the intervention of metasystemic levels to obtain metaknowledge. The question of disappearing knowledge must be qualified. Ordinary knowledge or object level knowledge is bound to disappear, whereas metaknowledge which results from higher level inquiring systems will have more staying power.

6.1. THE METASYSTEM FRAMEWORK

The metasystem framework stems from the metasystem paradigm developed in Kickert (1980), Kickert & J.P. van Gigch (1979, 1984, 1985). Therein the organization is conceptualized as a hierarchy of control systems. A control system consists of a controller (CR) which exercises control over a controlled system (CS) which, in turn, interacts with an environment (E). At the same time, there exists another controller - a metacontroller (MCR) - which exercises control over the control system C made up of CR, CS and E. The addition of a metacontroller (MCR') defines control at the second level of recursion where MCR' exercises control over the control system C' made up of CR', and CS', and E'. It is to be noted that MCR from the first level of recursion becomes CR' at the second. Similarly, the control system C becomes

CS'. Environments E and E' are not necessarily equivalent or isomorphic (see Figure 1), control systems C and C' define two levels of recursion. In each control system, control exercised by the controller (CR) is called control at the object level, whereas control exercised by the metacontroller (MCR) is called control at the metalevel or metacontrol. The same denomination applies to CR' and MCR' and subsequent levels of recursion.

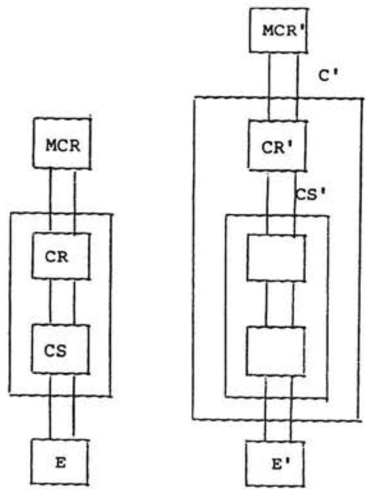

Figure 1: Control systems C and C' with their respective metacontrollers MCR and MCR' define two levels of recursion.

The term "level of recursion" is borrowed from Beer (1979, 1981, 1985) to designate the control system and its metacontroller at each level of logic. Levels of recursion do not, necessarily carry implications concerning authority or ethics, i.e. higher levels of control do not have more authority or are better, in the moreal sense, than lower ones.

The metasystem paradigm embodies some of the methodological requirements which are missing in the science and the systems paradigms. In particular, the metasystem paradigm (van Gigch , 1979b, 1984, 1985; Kickert & van Gigch, 1979) counters some of the criticisms leveled against the other two by incorporating:

1. a hierarchy of problem-solving levels in which higher system levels can judge and rate solutions of lower systems levels;
2. a framework to provide evaluation criteria in metalanguage terms, i.e. a language appropriate to judge lower systems solutions;
3. a guarantor of truth at each systems level, except the very last (or highest).

It is important to note that the characterization of the enterprise according to control systems at several levels of recursion does not necessarily coincide with the traditional model where the form consists of operations, tactical and strategy/policy levels. Difference between these two models should be noted.

6.2. THE INTERVENTION OF HIGHER LEVEL INQUIRING SYSTEMS (IS's)

The use of a hierarchy of inquiring systems can be examplified in several ways.

The first example refers to the hierarchy which consists of the inquiring systems to applied science (lower level IS), pure science (object level IS) and philosphy of science (metalevel IS).

Pure science, at the object level, must decide on what problems to work and it must make choices among alternatives scientific programmes. Pure science is not tied to utilitarian objetives and, to be creative must be, as much as possible, constraint free. Pure science receives its epistemology from the philosophy of science IS which constitutes its metacontroller (MCR). In turn, applied science (at the lower level) receives its metarationality from pure science.

What is provided from higher levels of Logic?

To answer this question one must refer to an inquiring system (see figure 2). Each inquiring system IS is a conversion process which takes evidence from higher and lower levels and converts it into decisions. Each IS is guided by its own epistemology which is made up of the world views of its participants and by rationalities and metarationalities which stem from metalevels of inquiry.

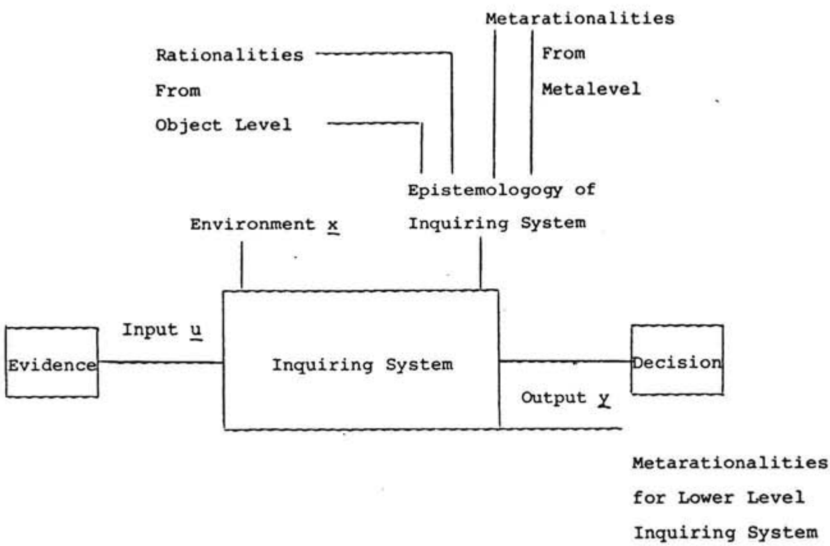

Figure 2.

As has been stated elsewhere (van Gigch, 1985), four rationalities/metarationalities play a role in this conversion process: Structural rationality (Rs), evaluative rationality (Re) substantive rationality (Rc) and procedural rationality (Rp). They provide respectively structure, goal criteria, content and procedural form to the decision outputs. A decision is not logically complete (i.e. it is not "intelligent") unless it is constituted of these four rationalities. For the most part, the structural (Rs) and the evaluative rationality (Re) stem from higher levels of logic and thus provide metarationality to lower levels IS's.

6.3. THE HIERARCHY OF INQUIRING SYSTEMS IN THE PROCESS OF KNOWLEDGE FORMATION

The hierarchy of conceptual levels were higher levels of logic lend metarationality to lower levels, can also be found in the process of knowledge formation as postulated in van Gigch (1982). The hierarchy consists of four levels:

Level I: Or user's level, provides us with FACTS from OBSERVATIONS of the real world. The problem of universe of discourse of level I, consists of making choices among these facts to prepare a DESCRIPTIVE MODEL of the universe of discouse (see Table 1).

Level II: (the observer's level), the descriptive model is elaborated into an EXPLANATORY MODEL. Here, the problem consists of making choises among descriptive models to elaborate HYPOTHESES and the most plausible EXPLANATORY MODELS which accounts for the observed facts.

Level III: (the designer's level), explanatory models become PRESCRIPTIVE MODELS. The designer's universe of discourse or problem is to convert explanatory models into PRESCRIPTIVE MODELS OR THEORIES. The role of an explanatory model is to merely account for a system's behavior ex-post, whereas the role of a prescriptive model is to anticipate its behavior ex-ante.

Level IV: (the epistemologist's level), choice among prescriptive models or theories lead to the elaboration of an EPISTEMOLOGICAL MODEL OR PARADIGM.

Logic Levels	IS (Inquiring Systems)	Universe of Discourse or Problem Addressed	Output
Meta Level	Philosophy of Science	Choice Among Epistemological Models or Alternative Approaches to Acquire Knowledge	Epistemological Model (PARADIGM)
Object Level	Pure Science	Choice Among Explanatory Models	Prescriptive Model (THEORIES)
Lower Level	Applied Science	Choice Among Descriptive Models	Explanatory Model (HYPOTHESES)
Level of Real World	Field of Application	Choice Among Observed Facts	Descriptive Model

Table 1: Hierarchy of Inquiring Systems with Corresponding Universe of Discourse and Outputs.

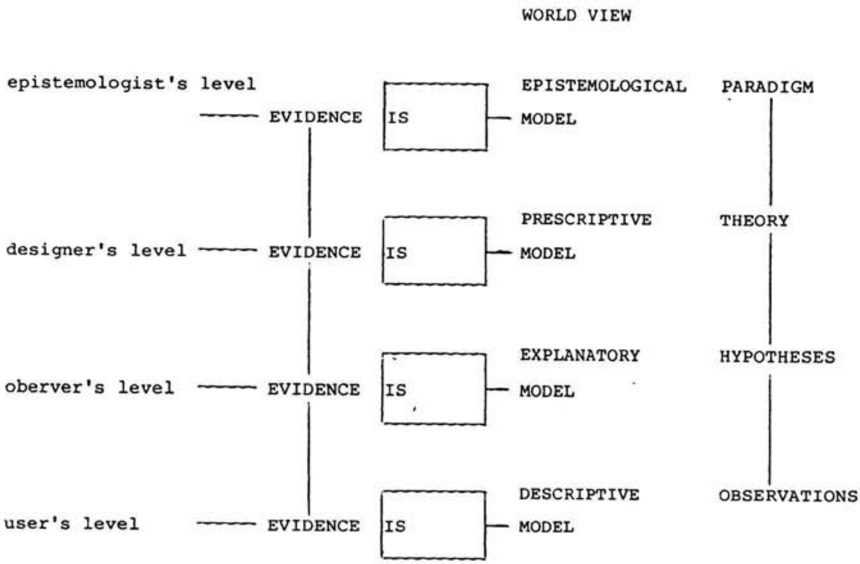

Figure 3: The hierarchy of inquiring systems in the process of theory refutation. Source: Van Gigch (1982).

The crux of the process of new knowledge formation and the progress of thought rests at the interface between the designer's level, level III, and level IV - that of the epistemologist, where the PARADIGM is elaborated.

Seen from the perspective of the designer, the paradigm constitutes metaknowledge. It is at this interface that the dialectic debate, between the epistemologist and the designer on the one hand, and between the paradigm and competing scientific theories on the other, are engaged.

The processes of theory refutation and of paradigm displacement are essential to an understanding of the problem of disappearing knowledge. When a theory resulting from a particular scientific programme is postulated, it will endure until it is successfully refuted and replaced. As explained by Kuhn (1970), when object level knowledge at the designer's level no longer fits the prevailing paradigm (metaknowledge from the metalevel) or is no longer relevant to it, it will be displaced by new knowledge which fits and is relevant to the prevailing world view. It is worthwhile to note that the dialectic debate may work in the other direction: the paradigm itself may lose its credibility and slowly give way to a substitute.

Thus knowledge disappears and is replaced by substitute knowledge as a result of the interplay between prescriptive and epistemological models or between the theories and paradigms. As confirmed by Kuhn (1970), whereas these processes are rather easy to describe, they are in reality frought with complexity and serious conflicts.

Agrell (1983) also described a similar hierarchy from facts to methods to programmes and finally to paradigm. De Zeeuw (1985) makes similar distinctions when referring to the role of "models for" and of "models of" in the formulation of new problems.

6.4. THE LEVELS OF IS IN LEARNING, CONSCIOUSNESS AND INNOVATION

Bateson (1979) provided us with two excellent examples of how the hierarchy of inquiring systems works in the training of dolphins and dogs. He stated that "intelligent" animals are capable of learning at two levels. At the lowest level, they can learn a single task. The capability of a dolphin to learn or to understand that the trainer wants to change the task or the sequence of tasks, lies at the next higher level of learning (learning about learning, or learning to change learning). The same can be found in a dog and, presumably, with other animals such as monkeys and other primates which display higher levels of intelligence.

The trainer or anthropologist who teaches these living beings, must be capable of higher levels of logic than those exhibited by those he(she) trains. Thus we can also apply the notion of levels of inquiring systems to knowledge formation in a learning situation.

Indeed, the hierarchy can be used to model the integrative process by which the human communication channel integrates "unitizes" or "relates" new knowledge with old. There, creative knowledge was found to be an information processing function which takes place at the metasystemic level of inquiry (van Gigch, 1979 a).

The hierarchy of inquiring systems can also be used to model levels of consciousness (Battista, 1985) and of complexity. Indeed, what we mean by intelligence can, in part, be construed as the mind's capacity to generate as well as comprehend conceptualizations at an ever increasing level of abstraction.

Finally, the difference between mere intervention and change in organizational development can be explained in terms of the dialectic debate which takes place between the old organzational logics and those that the intervener seeks to introduce (Agrell and Hatchuel, 1984).

Innovation takes place when, as a result of this debate, the organization makes a move and adopts an innovative stance.

A change is a reconceptualization of the existing organizational logics. This reconceptualization can only occur at a level of logic higher than that of the conflicting logics from which it originates. Indeed, this reconceptualization in the context of the organization is similar to that which we described earlier, in the context of knowledge formation in the scientific community, as the process of paradigm displacement and/or theory refutation. The adoption of an innovation is a form of paradigm displacement which can only occur at a level of logic above that of the existing conceptualizations which they seek to replace.

The adoption of an innovation stems from the dialectic debate between object level knowledge and metaknowledge or between the prevalent paradigm and the competing theories. If a theory is refuted or a paradigm is displaced, they will be substituted by another theory or another paradigm.

6.5. THE NATURE OF METAKNOWLEDGE

The application of the metasystem paradigm to the process of knowledge change has emphasized the need to consider the hierarchical nature of information and decision processes.

Regardless of the level of logic which we choose to consider, a change to knowledge results from the dialectic debate and ensuing reconceptualization between object level knowledge and metaknowledge.

As knowledge is reconceptualized through the interplay of ever higher levels of abstraction, it becomes more complex and hence less liable to modification and change. Therefore, we contend that knowledge pertaining to higher conceptualization levels (metaknowledge) is more difficult to change than knowledge from lower conceptualization levels (object level knowledge).

The best reason which we can give of this increase in complexity is that as we change paradigms to explain a new set of facts, the new paradigm must confront the validity of two propositions:

1. why the old paradigm cannot explain the new fact;
2. why the old paradigm was sufficient to explain the old set of facts.

The new paradigm has a more complex set of propositions to explain than the old one.

This is tantamount to stating that knowledge from higher level IS's or metaknowledge will tend to disappear more slowly or much less than those from lower level IS's. This conclusion places the problem of knowledge disappearance in a different light.

1. We must differentiate between metaknowledge which exists at high levels of conceptualization and abstraction and "object level" knowledge which exists at lower levels IS's. Given its nature, the former will be subject to less change than the latter. We must also notice that knowledge may disappear completely only for certain inquirying systems and still exist for others, as example Newtonian mechanics is still valuable knowledge for an IS whose purpose is to explain the simple collision of two simple bodies on earth, but it is obsolete in regard to an inquiring system which seeks to explain the deep laws of matter.

Apart from its tendency to fade away less, than knowledge from lower level IS's, metaknowledge (i.e. knowledge originating from higher level IS's) has other characteristics.

2. Metaknowledge carries the four types of rationality (thus, it is said to be logically "complete") and thus contributes to the formulation of intelligence which is essential for decision making.

3. In spite of assertions to the contrary, (see below reference to Gödel's Theorem) metaknowledge is paradigmatic, i.e. it provides the epistemological foundations for lower level IS's, and acts as guarantor of truth, i.e. provides the rationality by which lower systems of knowledge can be judged. Taken in the broadest sense, the guarantee of truth includes consideration of the morality of systems, i.e. consonance between the mission of the organization and that of its clients/recipients.

4. Gödel's Theorem implies that some system's propositions cannot be judged (are not "decideable") in terms of the logical system emanating from itself. If Gödel's Theorem is applied literally, it appears problematic to conceive an outside total guarantor for any hierarchy of IS's which produces knowledge. Thus the point under 3 above may require reconsideration.

5. Most organizations lack a metasystemic IS whose epistemology can be used to judge evolutionary changes of lower decision level systems. In other words, these organizations do not embody institutionalized evolutionary systems which can formulate epistemological models or paradigms by which other models or theories can be judged.

6. The role of IRREVERSIBILITY in the ACQUISITION of KNOWLEDGE process. It is important to recall that modern science is rejecting the views and assumptions of classical science concerning the fundamental law of time reversibility. According to the modern view, irreversibility plays an essential role in nature (Prigogine & Stengers, 1979). Reversibility is the denial of the importance of time, whereas irreversibility accounts for the running of time. Tied to the principle of irreversibility is that of randomness, i.e. it is only when a system is random that there can be

a difference between past and future (Prigogine & Stengers, 1979). There is no unique view of the universe. The idealized lawfulness of determinism and reversibility is replaced by a chaotic, stochastic, unruly world where chance and probability play a central role.

The role of disorder was also described by Boudon as the basis of social change. Therefore we oppose the deterministic and reversible, against the stochastic and irreversible. Furthermore, at bifurcation points, one never knows which way the system will go. Increase in entropy, the arrow of time, amplification of fluctuation and irreversibility are all related, and must be taken into account in our brave new world.

As explained by Prigogine, "irreversibility" brings order out of chaos" (Prigogine & Stengers, 1979). We can paraphrase an earlier dictum (van Gigch & Pipino, 1980) according to which the world view went "from the absolute to the probable", and now, "to the fuzzy and irreversible".

6.6. HOW DOES THIS NEW VIEW OF THE WORLD APPLY TO THE PROCESS OF KNOWLEDGE ACQUISITION AND DISAPPEARANCE?

It is only in a deterministic reversible and linearly conceived world that knowledge is accumulated and that we could ever conceive of going back to old forms.

To talk about disappearing knowledge is very much in line with the view that old forms do not help to predict new ones and that the latter are the result of stochastic unanticipated events. In those circumstances, knowledge is like any other complex system, it develops as an avalanche of unanticipated impredicatable stochastic events whose future is impossible to describe. Produced from human intervention, it builds on the foundation of present knowledge, but in the process, old knowledge like old buildings are destroyed and razed. Only some buried remains will stay, to be destroyed and razed. Only some buried remains will stay, to be rediscovered as archeological treasures by another civilization, several thousand years hence. No doubt like old monuments, some milestones from the history of knowledge will remain for all to see in the coming generations. These monuments are the important scientific laws or the art treasures that we collect from the past. Therefore, like important pieces of art or architecture, there is some knowledge from the past which endures.

As an example, in astronomy and celestial mechanics, the dicoveries of Copernicus, Galileo and Newton, can be considered such milestones. While obsolete from a scientific point of view, they do not disappear but are preserved and revered. Furthermore, they will not disappear from the public conscience if they are taught in school to the new generations. Otherwise, they will suffer the fate of those razed buildings we mentioned earlier only to be rediscovered by archeologists of the next millenium.

6.7. CONCLUSION

The metasystem paradigm provides a hierarchical framework in which the process of knowledge formation, disappearance and change can be studied and evaluated. It emphasizes the role of the dialectic debate among information and decision processes, leading to the elaboration of knowledge and knowledge about knowledge (i.e. metaknowledge) at different organizational and eptistemological levels of the hierarchy.

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CHAPTER 7

THE EFFECT OF INTERRUPTIONS ON THE PERFORMANCE OF INDUSTRIAL AND OTHER REPTITIVE OPERATIONS

John Rendel de Jong

Increase in individual skill and knowledge is often attained by attenting training courses. Observing others, hints from colleagues and superiors, and the repeatedly carrying out of the same or similar activities may, however, have an at least as important effect. Also engaging new employees, belongs to the usual means to increasing skill and knowledge, that are available in an organization.

On the other hand, interruptions of the performing of operations, innovations with regard to product design and equipment, transfer of personnel and personnel turnover, are usual causes of the disappearing of specific skill and knowledge.

7.1. INTRODUCTION

It is a condition for effective functioning of most organizations that the effect of increasing or disappeared knowledge and skill on the time that is required for future operations, can be predicted with sufficient reliability: with a view to production control, manpower planning and such likes. That means that time standards should be available and that learning and forgetting - as indicated above - should be taken into account in fixing such standards. In what follows, firstly some remarks will be made regarding the use of time standards. Thereupon the effect of gradual learning on the time required per unit of production (piece, cycle) will discussed. Next the effect of interruptions of production or other operations, i.e. forgetting, on the time required, will be considered.

7.2. LEARNING AND TIME STANDARDS

According to von Wright (1963) important aspects of norms are:

A. Their character: ought, or may, or must, something be done (or no be done)?
B. Their contents: that which ought to, or may, or must, not be, or be done.
C. The conditions of application of the norms.
D. The 'authority(ies)' who prescribe(s) the norms.
E. The 'subject(s)' of the norms: those to whom certain norms are addressed.
F. The occasions on which the contents of certain norms must or may be realized.

Moreover Von Wright mentions as essential in connection with norms:

G. Their promulgation: the way the norms are made known.
H. Sactions: the threat of punishment for disobedience of the norms.

When we analyse time standards as a category of norms in this sense, there appear to be quire a number of possibilities regarding their use and effect (figure 1). Here particularly their quality (Bb infigure 1) is of interest: in how far are effects of gradual learning and forgetting being taken into account? In this respect it is important that time standards often act as selffulfilling prophecies: to the advantage of the organization in question and the persons concerned, or not.

Possible relationships, when time standards are used, are shown in figure 2.

From the point of view of the organization, the sense of using the indicated possibilities depends on the relative benefits and costs. Of the costs may form part the costs connected with determining time standards, performances, and bonuses, and supplying relevant information; against these, the motivation and the performances of the relative employees may be influenced in a positive sense.

'Components' (v. Wright)	Alternatives			
A. Character of time standards	1. Prescribed process duration	2. Prediction of duration	3. Standard time (in conform with a certain performance level)	4. Target (to be attained in future)
B. Contents of time standards	a. Scope of standards:			
	1. Worker	2. Group	3. Department	4. Establishment
	b. Quality of standards:			
	1. Insufficient	2. Dubious	3. Suitable	4. Superfluously accurate
C. Conditions for application	1. Insufficiently regulated	2. Somewhat incomplete and/or unclear	3. Suitably regulated (delays, handicapped workers etc.)	4. Unnecess. complic. regulated
D. 'Authorities' (determin. of time standards)	1. Industrial engineers, man.services officers and such likes	2. Manpower planners, production control specialists etc.	3. Workers (to whose tasks time stand. apply) Relative supervisors	4. Complaints committees and such likes
E. 'Subjects'	1. As D2	2. Accounting department (determin. of perform. and bonuses)	3. As D3	4. As D4
G. 'Promulgation' of time standards	a. Announcement of time standards: 1. Specification of standards 2. Place(s) where time standards can be noted 3. Moment at which time standards are announced 4. To whom time standards are announced			
	b. Announcement of achieved performances: 1. Moment of announcement 2. Scope (as Ba) 3. Period			
	1. Continuously	2. Day	3. 4 weeks/ month	4. Longer
H. 'Sanctions' (praise and censure)	1. Non-financial rewards	2. Paym.-by-res. syst. a. Individ. b. Group	3. Organ. wide bonus schemes schemes	4. Profit sharing

Figure 1: A résumé of possibilities regarding the use of time standards (de Jong, 1984).

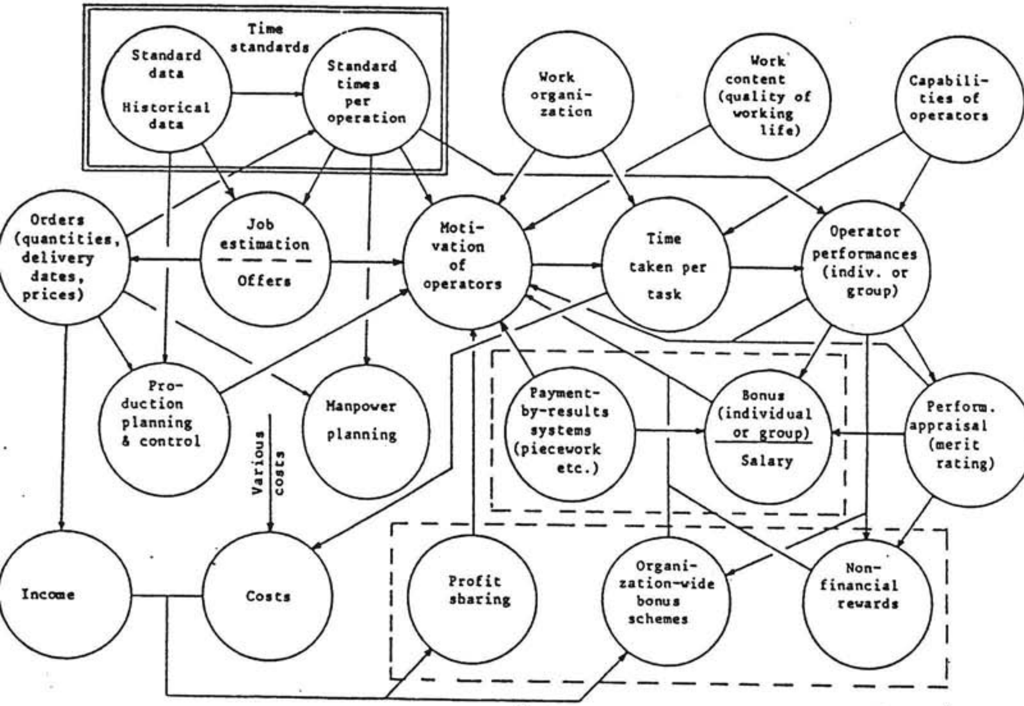


Figure 2: Possible relationships when time standards are used (De Jong, 1984).

7.3. LEARNING CURVES

Figures 3 and 4 show two examples of the effect of gradually increasing specific skill on the time required per production unit.

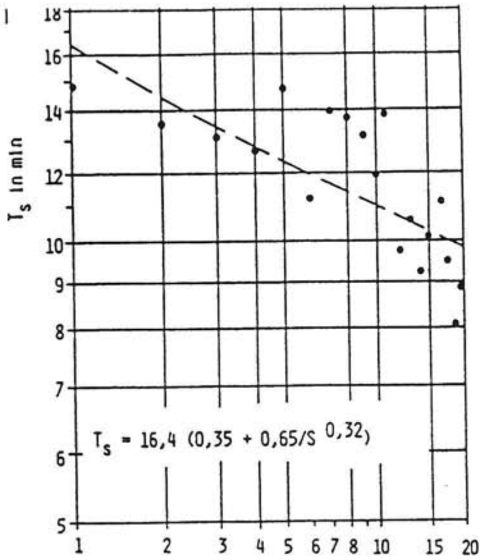


Figure 3: An example of the effect of increasing skill on the time spent per production unit (turning). T_1 = 16.4 min; M = 0.35.

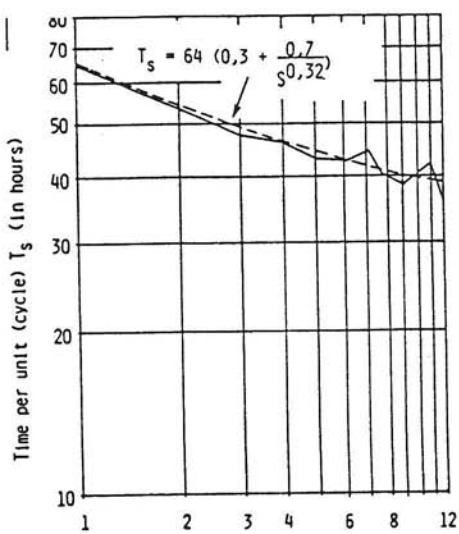


Figure 4: An example of the effect of increasing skill on the time spent per production unit (assembly work in ship building). T_1 = 64 hours; M = 0.3.

As appears from data from various industries, a suitable model for indicating this effect, is the equation (De Jong, 1957):

$$T_s = T_1\{M - (1-M)/s^{0,32}\} \qquad \text{Eqn (1)}$$

where T_1 = time required for the first unit (cycle).
T_s = time required for the s^{th} unit (cycle).
M = factor of incompressibility $0 < M < 1$.
0,32 = exponent of reduction.

For $S \to \infty$, follows from equation (1) that $T_\infty = MT_1$, and that

$$T_s = T_\infty 1 + (1-M)/Ms^{0,32} \qquad \text{Eqn (2)}$$

As appears from film analysis, the fall in T_s is on the one hand caused step by step simplification of the perception and motion pattern, and on the other hand by gradually increasing rapidity of movements (De Jong, 1960).

Relationships regarding the effect of the repetitive performing of the same or similar activities on specific skills and knowledge (and with that on the time required per unit of production or service) are indicated in figure 5.

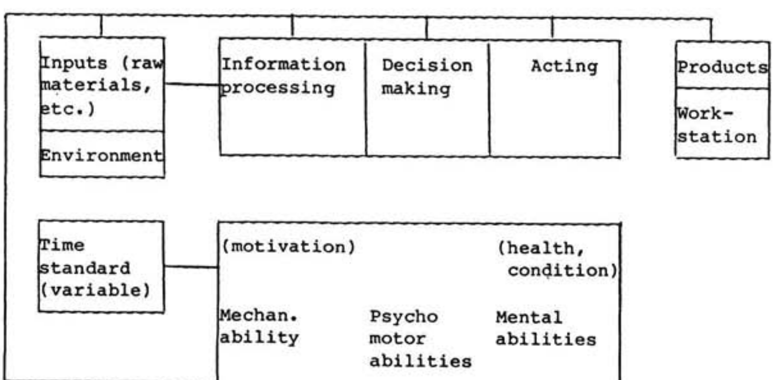


Figure 5: Relationships regarding the effect of the repetitive carrying out of same or similar activities on the specific skills and knowledges in question.

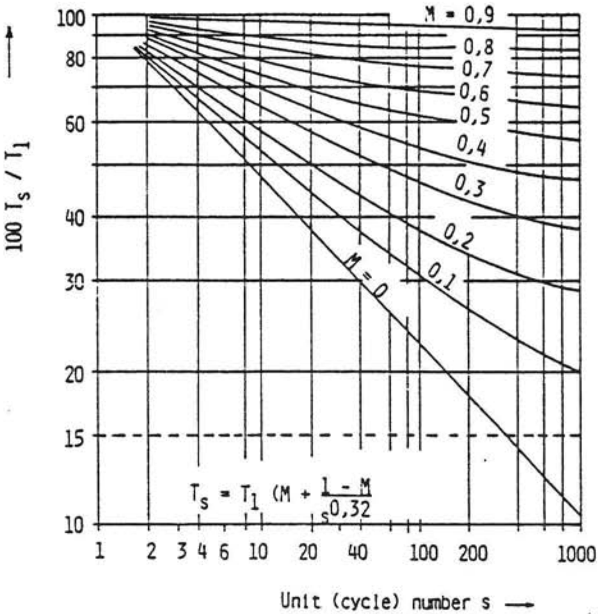


Figure 6: Standard curves, indicating the gradual decrease in time per unit (cycle) T_s, if merely increasing skill and insight play a part. Curves for M = 0 ... 0,9.

Figure 6 shows 'standard curves', based on equation (1), for various values of M. For M mostly values between 0,25 and 0,5 are being found. M appears to be determined above all by cycle time (and, with that, with the diversity of the required decisions and movements within the cycle), and available relevant skill and insight of the worker (which appear to be connected with the mutual diversity of the operations the worker has to carry out successively).

Also other models than equation (1) have been proposed, but the author feels that these as a rule are less satisfactory, and that particularly in case of large values of s, and also in view of their usefulness in practice.

If, and as long as, increasing skill and insight go together with continual organizational and technological improvements, as a model often is usable:

$$T_s = T_1/s^m \qquad \text{Eqn (3)}$$

where m = exponent of reduction.

Figure 7 shows relative curves. As an indicator of the rate of decrease of T_S, factor $\int$ (i.e. 100 (T_S - T_{2s}/T_S = percentage decrease in unit time per doubling of s) is usually preferred to m. For not too small values of s, the following relationsship applies:

$$\int = 100 - 100/2^m = 60 \text{ m} \qquad \text{Eqn (4)}$$

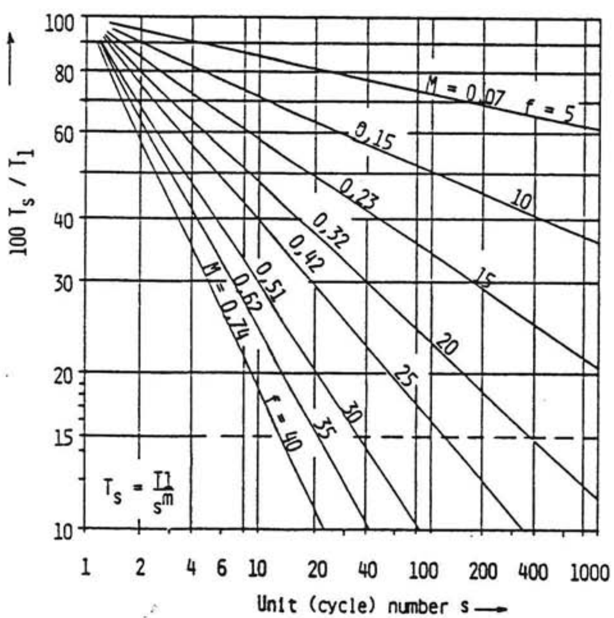


Figure 7: Standard curves, indicating the gradual decrease in time per unit (cycle) T_S, if the combination of organizational and technological improvements and gradually increasing skill cause the fall in T_S.

7.4. THE EFFECT OF INTERRUPTIONS

About the effect of production interruptions on this time that is required per production unit (piece, cycle) when production is restarted, not much has been published. Publications that should be mentioned are those by Cochran (1968) and Lieban (1981). What occurs after a considerable interruption is schematically indicated in figure 8.

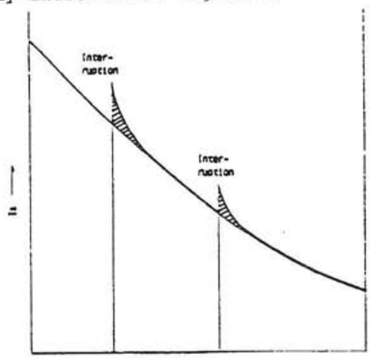
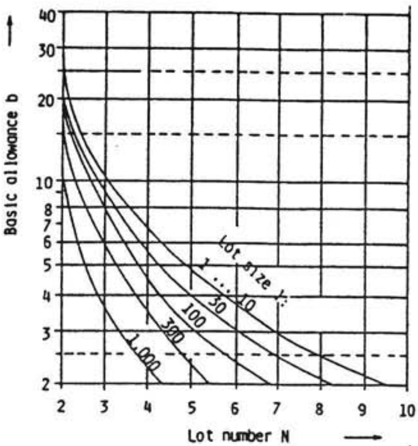


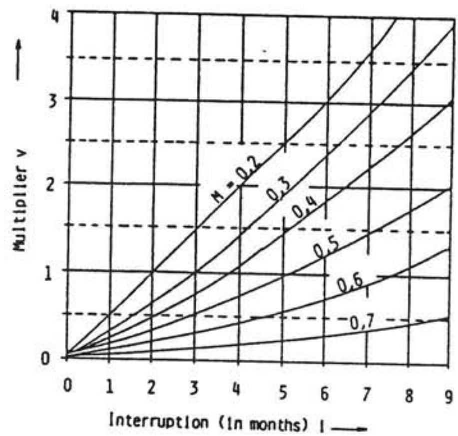
Figure 8: The effect of production interruptions on the time per unit (cycle) T_s.

The effect of interruptions of production on the time per unit, i.e. the effect of 'forgetting', appears to be influenced by the following factors:

a. the duration for the interruptions i
b. the relative factor of incompressibility of M, from equation (1):
   $T_s = T_1\{M + (1-M)/s^{0,32}\}$,
c. lot size 1
d. lot number N.

Available data lead to relationships that are shown in figure 9. For the case of production in lots, it can be calculated by means of the curves of figure 8, which 'allowance' in case of interruptions of production, can be expected to be required <u>above</u> the time per unit (piece; cycle) that would be required, if there was no interruption between the production of successive lots.





9.1: The relationship between lot size, lot number, and 'basic allowance'

9.2: The relationship between the length of production interrupts factor M and the 'multiplier'

Figure 9: Curves for determining allowances because of production interruptions.

This percentage allowance equals:

$$A_r = b.v.,$$ Eqn (4)

where b = basic allowance (to be determined by means of figure 9.1),
v = multiplier (to be determined by means of figure 9.2).

In elucidation, the following example is given. Let lot size 1 = 100, factor of incompressibility M = 0,3 and the duration of the periods between successive lots i = 2 months, then figure 8 gives the following allowances A_r for lot numbers 2, 3, 4 and 5:

- Lot number 2: $b = 16$; $v = 0,7$; $A_r = 16 \times 0,7 = 11,2\%$.
- Lot number 3: $b = 8$; $v = 0,7$; $A_r = 8 \times 0,7 = 5,6\%$.
- Lot number 4: $b = 4,5$; $v = 0,7$; $A_r = 4,5 \times 0,7 = 3,2\%$.
- Lot number 5: $b = 3,1$; $v = 0,7$; $A_r = 3,1 \times 0,7 = 2,2\%$.

In conclusion it should be stated, that it is not claimed, that allowances because of forgetting according to figure 8, are generally applicable.

7.5. SOME CONCLUDING REMARKS

The foregoing concerns for the most part the effect of:

a. the repeatly carrying out of a certain cycle of work, and
b. interruptions of that,

on the time the worker(s) concerned require(s) per work cycle (production unit). Interruptions of the performing of an operation (of the order of months), appear to attain the partial disappearance of the specific skills and knowledges that those concerned have acquired since they started carrying it out. Reliable prediction of this effect, is in many industrial and other organizations of importance in view of job estimation, production control and such likes (figure 2.).

Against the gradual disappearance of specific skills and knowledges during interruptions of the performing of a certain job, there is usually the simultaneous development of other specific skills and knowledges as a result of the performing of other - often more or less similar - jobs. Altogether, mostly the skills and knowledges of workers increase in the course of the years - at least if we look away from the influence of advancing age.

This does all the same not alter the fact that particular skills and knowledges, that are no more in want, for example because of automation, may disappear completely. This applies as well to individuals, as to organizations.

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CHAPTER 8

USES OF KNOWLEDGE: SOME METHODOLOGICAL ALTERNATIVES

Michel Thiollent

Our aim is discussing some problems in the state of the art of scientific knowledge and seek methodological alternatives in the production and spreading of social and technological knowledge. This is done bearing in mind real world applications like communication, education, rural technology diffusion in developing countries, taking Brazil as an empirical reference point.

8.1. GENERAL ASPECTS OF KNOWLEDGE USE

In recent years, internationally a rapid growing of knowledge can be perceived in the amount of published information and kept on files, data bank and centers of several kinds. Some side discussions have been coming up on produced knowledge relevance questioning even the eventual loss of quality, accelerated obsolescence, fragmentation. These are contrasting aspects compared to the earlier years' optimism.

The knowledge production and diffusion systems include unfiled information working in interaction with researchers (knowledge structuring) and with the users (concretization problem resolution). One part of the filed information can have "a revival" by its reinsertion on research and information production, knowledge and didactic material. But it can be seen that there is an accelerated obsolescence process. New information annihilates the former. There is a reduction in the life cycle of ideas and scientific information. Beyond that, a strong repetitivity manifests itself in the knowledge production.

In the words of Gomes (1984), all those "dispersion" aspects of information depend on the social logics in the communication organization and in the generation/circulation of messages and registers. In the words of the author "the social division of work would be one of the basic principles of the social logic".

We can observe that the fantastic accumulation of scientific and technical information doesn't correspond automatically to growth in structured knowledge for the effective solution of problems. Information only makes sense and is efficient and profitable if used for solving real problems.

There is clearly a difference between the effective problems' solution and the symbolic use of scientific information. We understand by "symbolic use" an information use as a sign of cultural differentiation or group recognizing prestige, ostentation fed by elitists and populist discourses. The symbolic use is also found under the massified form particularly in the University channels that are propitious to the "Cultural Consumerism". In all those circumstances, knowledge is not put in the condition of a objective work.

One of the biggest parts of knowledge is subjected to a symbolic valorization. Its detainers acquire prestige positions in the power and communication spheres in a very disproportionated way compared to their real contribution in equationing the relevant problems in the society view point.

The obselescence question can be also related with the valorization processes: "The attributed value to some determined knowledge varies in function of the moment in which it is available and usable to a determinate end. With the time, an idea depreciates. The symbolic goods are submitted to obsolescence: There is an acute valorization in function of the novelty or the applicability potential of an idea in the symbolic system. In some social circumstances, past ideas are revalorizated may be because there is a lack of imagination on the present or the future (Thiollent, 1979).

Several aspects that interfere in the use conditions of knowledge are made by derivate parts of the cultural valorization mechanisms operating on the society, particularly under the form of "output ideology", differentiation and competition of the individuals and knowledge.

In the knowledge evaluation, quantitative output criteria have a tendence on prevailing (amount of publications) and, the qualitative plane, criteria of "excellence" or "fame". Such criteria can lead to a distorted view in the measure that the quantitative doesn't apprehend the social pertinence of knowledge and the qualitative, when limited to prestige, becomes dependent on badly controlled communication phenomena in which certain persons or groups achieve the divulgation of intellectual production in a essentially symbolic way, far away of compromising with the real world's problems. Many times, excellence criteria are altered by the phenomena which Merton (1973) called "Matthew Effects". It has been observed that fame is relatively separated of a strictly evaluating scientific production. The valorization of the information is influenced by the authors or institutions prestige. According to the "Matthew Effects" communication attributes credit and prestige in a selective way to the production of scientists in terms of the fame they already have. Those who has it, get it more and more. Generally, this principle can shift from the prestige plane to the financial resources plane.

8.2. THE KNOWLEDGE CONTEXT IN BRAZIL

In Brazil, knowledge production and diffusions marked by the same phenomena formerly pointed, but with several aggravations, due to a series of factors: external dependence, elite characteristics leading to a merely symbolic use, lack of application opportunities, bad profiting to the researchers potentialities and the available resources (Julius and Gerritsen, 1969), emptying of college work for economical and political reasons.

Knowledge importation is organized by technology transfer and international cooperation. There is also a more informal disclosure. In the university sphere, this disclosure is manifested through the teachers whose titles are obtained abroad and equally through visiting teachers and by the edition of numerous translated books.

There are multiple influences from the USA, France, Great Britain and Germany. The access to the production of those countries is very unequal and varies between Universities and is also affected by the high cost of the imported publications and by the time needed. There is also the reading capability in foreign languages (Rosenberg and da Cunha, 1983), in which the younger generations are finding ever increasing difficulties.

The knowledge importation, by itself, is not negative. Because it can contribute in the renewal of the approaches and in the outdoing of the rhetorical tradition turned to the past cults, the wisdom of the notable, the exaggerated importance given to the bachelors' degrees, and another forms of dated wisdom and symbolic use. There is, although, a difficult problem in the adaptation of the theoretical reference frames to the surrounding reality.

When these adaption problems remain solutionless, the imported knowledge contributes to the feeding of the symbolic activity, under the form of ostentatious behaviors with elements of erudite knowledge which are rare in the context. At the same time those elements are being diffused, their initial detainers leave them behind, seeking to renew their prominent position acquiring new "rarities".

The relationship with the knowledge production centers of the more advanced countries has other implications. One of them, the most renowned is the "brain drain" of a great number of advanced researches towards the foreign centers. A second implication, is in the fact that the majority of the local scientists, among those who get to reach international level results, are compelled to publish them in North-American or European magazines, offering scientific information to the foreign patrimonies.

Beyond the "brain-drain" and these external result spreading which are not profited upon locally, there is a third side in question pointed up by mathematician U. D'Ambrosio, who says about the scientific cooperation forms in which the researchers and local research centers study "problems worked upon universities or foreign research centers" (D'Ambrosio, 1977).

In Brazil's particular case, we can see several signs of favorable conditions to the knowledge development in the intellectual environment. This is manifested under the form of:

- increase in the researchers number and in the research quality;
- autonomy in some intellectual groups;
- perception in the urgency of social, ecological, technical and educational unsolved problems;
- engagement of a great number of universitary students in the transformation of the agent situation;
- popularity of research methodologies which demand a research commitment, just like the case of the participating research methodologies and the action-research;
- intensity of interest started by the introduction of cybernetics.

In our view point, it is a must to question, at the level of research activity, the ways of increasing the knowledge commitment with societal problems. We need to elaborate practical knowledge strategies.

8.3. SOME METHODOLOGICAL ALTERNATIVES

The development of a kind of knowledge which can be susceptible of offering alternatives in the formerly described context, supposes that research programs are organized and that new theoretical and methodological frames are built in the social sciences and applied fields.

Without exausting the subject, we point some orientations:

1. In the matter of cognitive situations diagnosis methodology, we need to abandone the unilateral emphasis which, frequently, is given to the needs.

In a general way, the diagnosis methodology incorporated to the social research has been conceived in a non participatory way, establishing a dichotomy between the "expert" who elaborates the diagnosis results and the users who must conform to the same. The potentialities are not taken into account and the users self initiative.

Other critiques to the diagnosis concept have been formulated in the peculiar context of rural studies by Scales, Ferro and Carvalho (1984). The authors display that the dominant conception, concerning diagnosis, perverts the reality of small rural producers, who are always considered at needy. The diagnosis always focalizes what lacks: education, resources, technology, etc. The producers and their surrounding environment potentialities are not sensed. There is also the privilege of the producer's perception as isolated individuals, to the triment of their apprehension as grups that pertain to the process of collective production. The authors emphasize that concerning the knowledge production, the traditional model of diagnosing experts profound distoritions: the knowledge process is reduced to a data collection in what the producers are mere informers. We find in the mentioned article a great quantity of other very pertinent observations to criticize the traditional conception of diagnosis, and to develop "the participatory learning perspective" and a way of active collaboration among the producers, technicians and researchers' knowledges. In addition, to the area of rural research, this perspective seems to be suggestive and applicable, with adaptations, in several other areas.

The cognitive situations diagnosis is distorted when is only pointed what lacks comparatively to a situation - judged as ideal - to be imitated from the developed countries, as if the patterns or rationality and efficiency weren't immediately insurmountable.

It is necessary to establish a certain type of very different diagnosis. Instead of underscoring the needs, most important would be the detection of cognitive and human potentialities contained in the situation, in order to favor the new-methods knowledge and know-how appearance that must be usable for a possible change. Instead of "complaining", it is a matter of catalysing the efforts of several groups to act on the socio-cognitive changing situation, the latter refers to the relations with the external production and manners of internal institutional dependency, being an expression of the potentialities, until then restrained.

2. According to what precedes and bearing in mind the utilization of the dispositions of researchers, who are favorable to the social research methodology (of participatory and active type), we may seek for forms of

methodological evolution, taking into account beyond the participatory exigencies, exigencies of a scientific nature that must be propitious to increase the investigation effort and precision and to remove the pre-scientific spirit (Thiollent, 1984) that is frequently associated with participatory resarch or action-research methodology.

As a way of fortifying the scientific tenor, we suggest the establishment of a relationship between the methodology of action-research and the methodology of systems. In a more precise way, in the proposed program of methodological research, it's dealt with the elaboration of a bi-associative structure, relating the reseach exigencies of a participatory type with the cognitive research exigencies, linked to a systemic approach with integration of knowledge derived from the humanistic disciplines (Epistemology, Psychology, Sociology, etc.) and the disciplines entailed to the artificial sciences (design, problem solving, etc.). It seems that this structure is capable of promoting methodological innovations of a participatory, active and systematic character, guaranteeing the scientific exigencies satisfaction and the maintenance of contact with the real problems and the actors and the researchers' (in quest for solutions) involvement.

P. Checkland (1981) has formulated and experimented the principles of action-resarch, adapting them to the resolution of ill-structured problems and to the use of systematic concepts, particularly the one of purposeful systems.

In a discussion about the possibility of a science of autonomy, E. Morin (1983) has observed that the contribution of the systemic approach focalization may lead to a reconciliation and to a scientific reelaboration of social research. According to the author, Sociology has been traditionally divided between:

(a) a scientific Sociology, based on the physical-determinist model, intending to establish the regularities and laws in the relation of the phenomena, from which the actors, the actions, the autonomy of the individuals, etc., would be excluded, and

(b) a pre-scientific Sociology, with a literary or philosophical character, based on the ideas of "actors", "individuality", "finality", "taking of consciousness", "ethical problems", etc. Still according to Morin, this last Sociology, which is frequently considered by participants of the former as being obsolete, has kept nevertheless the essential concepts of autonomy and actors.

The situation of social sciences in Latin American and in Brazil is marked by the dichotomy between the scientifist tendency (based on quantitative techniques) and the humanistic tendency (based, among others, on participatory and active methods). This second tendency - marginalized in countries of greater industrialization - still occupies an important space in the university ambit and among the latin-american groups, particularly among those who are influenced by the church. The scientifist tendency, without any relativism, is, nevertheless, promoted in official programs by means of established conventions with North-American centers of research.

The systematic approach is frequently reduced to a "conventional", "functionalist", "technicalist", "technocratic", complicated and closed to epistemologic discussion. In our point of view, such situation is not inherent to the theoretical and methodological tendency of the systemism, but in the particular circumstances of a partial divulgation.

With sufficient open-spirit, the researchers of each line of methodology (systemic and participatory) could perceive the insufficiency of their approach and the need for redirectioning. The cross-breeding of the approaches centered in systems and actors may generate a methodological language, propitious to the elaboration of new tools of research that would be adapted to a simultaneous apprehension of the problems and global situations, actors action and researchers intervention, with technical implications in matter of practical problems resolution in different fields of technological and social actuation.

3. The prior methodological orientations would be applied particularly to the production, diffusion and utilization of intermediate knowledge.

In terms of knowledge politics, we may consider that beside the development urgency and the basic sciences knowledge diffusion, the need for a politics specifically directed to the intermediate knowledge is revealed. By this expression is understood the practical purpose knowledge, taking place in several areas of actuation among which we detect education, communication, organization, rural development, technology diffusion, etc. It would deal with fortifying the productions and disclosure of knowledge that, in spite of not being much valorized in the cultural-symbolic plane, are of great utility in the real world's problems resolution. Moreover, what we understand by intermediate knowledge is different of the simple common sense. Such knowledge may be considered as vital for the employment of researchers, among which many of them are, nowadays, under-employed in circumstances that generates waste and frustrations in the university community. Nevertheless, it is a knowledge that does not occur immediately in practice. It is necessary to adapt it in a participatory process in which the researchers (and other professionals) and the representative interlocutors take part. The solution of effective problems is found in the collectivity and can only be carried ahead with their member's participation and will be able to be used as a way of sensibilization and awareness. In this perspective, we consider that the action research methodology constitutes a research form, a ratiocination form and a kind of intervention that are suitable to produce and to diffuse the intermediate knowledge, related with the concrete problems found in several considered areas.

In the adjustment between the knowledge and the action, the reduction to the minimal distance existent between the knowledge obtainment and the action planes formulation is intended. By this way, it would be possible to reduce the symbolic uses, that exist on the sphere of conventional knowledge. It is a matter of improving the effective use of knowledge on transforming actions.

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CHAPTER 9

INFORMATION AND KNOWLEDGE IN MODERN SCIENCE: SOME ETHICAL AND SEMANTIC ASPECTS

Roberto Cintra Martins

The idea of entropy emerged in nineteenth century Physics as an indicative of the impossibility to interprete, by the mechanistic conception of the Universe, some phenomena associated to the transformation and transport of energy. According to this idea, physical reality can no more be considered as entirely foreseeable and the behaviour of matter in movement is no more merely described by time-dependent differential equations. The limits to understand the microscopic world are put in evidence; one can only describe the statistical distribution of possible states of macroscopic aggregates of energy and matter. The concept of entropy in Classical Thermodynamics may be taken as a first sign of the radical changes in the way scientists consider the possibilities and limits of the process of knowledge improvement in Physics; these radical changes took place later at the beginning of this century, with Quantum Mechanics.

9.1. ENTROPY AND ENTROPIC DEATH

The second law of thermodynamics, according to which the entropy of isolated systems tends to grow steadily, indicates the expected behaviour of spontaneous physical-chemical phenomena, such as heat transfer, the expansion of "ideal" gases and the change in "free" energy in chemical reactions. Statistical Thermodynamics defines entropy of a system in a specific state as the quantity

$$S = - \sum_{i=1}^{n} P(E_i) \log_2 P(E_i),$$

where E_1, E_2, ..., E_n are events that describe the state and $P(E_i)$ is the probability of occurrence of the event E_i. In the particular case of reversible thermodynamical processes, this definition is coherent with that of Classical Thermodynamics (Sonntag and Van Wylen, 1971).

If we take the classical model of a system of n particles of a thermodynamically isolated "ideal" gas, and apply the second law of thermodynamics, we can foresee a steady change in the material-spatial configuration of the system, until it reaches a final state of maximal entropy, when the probability that a specific particle be found in a specific position in three-dimensional space in a specific point in time is identical to the probability that anyone of the other n-1 particles be found there. The material-spatial structure of the system is then the least organized and the state of maximal entropy is identified with the state of maximal disorganization that the system can reach. The state of the particles is the most indistinguishable, and the discrimination among the particles is the most difficult i.e., of highest cost (Martins and Bartholo, 1983).

According to the considerations above, the second law of Thermodynamics is associated with the tendency of isolated systems to reach maximal disorganization, which has a a corollary the tendency of the maximal disorganization of the Universe, which is also called the "entropic death" of the Universe. Entropy is a quantity radically distinct from almost all other basic physical quantities, such as matter, electrical charge, work, energy, linear and angular momentum, which are all enforced by conservation laws, when they are considered in isolated systems. Entropy, on the contrary, is able to grow and tends to grow in such systems.

9.2. ACQUISITION OF INFORMATION AS AN ANTIENTROPIC PROCESS

The assumption that physical phenomena are not completely foreseeable is essentially associated with the introduction of probability and information as basic concepts of modern Physics. According to Shannon and Weaver, a measure of the quantity of information that exists in any event must be a decreasing function of the probability of occurrence of that event. This requirement is coherent with the anthropological conception of the most unexpected events as carriers of a defiance to Man, as well as of potential information and virtual meaning. The emergence of learning processes in face of these phenomena is linked to the very biological construction of the human being (Shannon and Weaver, 1982).

If we denote by $I(E_i)$ the information associated with the event E_i and consider the requirement that $P(E_i) > P(E_j) \rightarrow I(E_i) < I(E_j)$, the quantity of information given by $I(E_i) = -\log_2 P(E_i)$ measures the minimal number of information units (elemental information, binary information or "bits") required to univocally describe the event E_i. The quantity of information that exists in a system in a specific state is given, according to Shannon and Weaver, by

$$I = - \sum_{i=1}^{n} P(E_i) \log_2 P(E_i),$$

where E_1, E_2, ..., E_n are the events that describe the state. This quantity is formally identical to the measure of entropy in Statistical Thermodynamics and the word "entropy" is often used in Communications Engineering to denote the quantity of information associated with a code or a source of electromagnetic waves or binary messages.

The relation between entropy and information is properly expressed in their antagonism at the semantic level, despite their formal identification. With the classical example of the ideal gas in mind, we can say that the entropy of the system is just the quantity of (binary) information required to know the state of each particle of the gas. Thus, the greater the entropy of the system, the greater will be this required information. To better distinguish between the concepts of entropy and information, Wiener (1966) proposes that the required information, whose measure is equal to that of the entropy, be called the "potential information" existing in the system:

$$I_{pot} = S = - \sum_{i=1}^{n} P(E_i) \log_2 P(E_i).$$

Wiener proposes then the concept of "acquired information" (I_{acq}) which is the part of I_{pot} that becomes progressively absorbed and understood by the human being, through historic-cultural processes of acquisition of knowledge. Potential information is transformed in acquired information through the decrease of the entropy of the system composed by Man as an historical-cultural being and his environment. There are two requirements, in order that such a process could take place:

1) that the system be not isolated and
2) that the entropy outside the system grows,

i.e., that the system "exports" entropy. Acquired information is considered as anti-entropy and the process of acquisition of knowledge is considered as an anti-entropic or organizing process. Acquired information is not enforced by conservation laws; like entropy, I_{acq} can also grow.

The process of acquisition of information starts by the observation and recognition of unexpected phenomena, potentially able to falsify an "explaining" system, based on information acquired in the past. In face of these new surprising events, new hypothesis are placed, which are equally plausible as potential explanations. Through a process of information feedback, controlled by observation and criticism, one hypothesis finally emerges as the explanatory one, and the other are rejected as false. Thus a new explaining sytem is organized, which absorbes the new events as explained events that progressively lose their "novelty" character and tend to turn themselves into "self-evident truths". This new explanation system is then consolidated and previals until new phenomena are recognized, which are inconsistent with it, and the process of acquisition of information start again.

The process of acquisition of information starts with the falsification of a prevailing explaining system, i.e., in a state where $S = I_{pot}$ is maximal and I_{acq} is null. From this initial state, a process takes place by which $S = I_{pot}$ tends to disappear while I_{acq} tends to grow and reach a maximal value, identical to the initial of $S = I_{pot}$. At each phase in this process, the sum $I_{pot} + I_{acq}$ is a constant, unless new unexpected events are observed and recognized. This sum is the appropriate measure of the quantity of information that existed in the specific events, at the moment when they were observed and recognized; it can be taken as the part of all the information of the Universe stored in those events and placed at the access of Man.

The choice of the hypothesis to be accepted to explain the phenomena is not foreseeable from the precedent explaining system. In other words, a theory does not include the propositions from the theory that will displace it. The socio-cultural fixation of the learning process implies the recognition that the prevailing explaining system (or the "scientific paradigms" in Kuhn's (1970) words) conditions the intelligibility of potential information related to observed events. This explains the difficulty to recognize the refutation of a theory by means of its inconsistency with experimental facts, as well as the rapid transformation of "scientific revolutions" in almost self-evident propositions, i.e., the transformation of "novelty" into "confirmation", in the words of von Weizsäcker (1974).

9.3. ON INFORMATION VALUE

The measure of information proposed by Shannon and Weaver (1966) may be applied to any system and will be determined by the minimal number of binary decisions required to know the form of the system. Information measures the quantity of form that exist in the system. According to this conception, by human action the quantity of information tends to grow, as Man acts on the environment with the purpose of conform it to his perception and his use. The production of goods is here considered as the transformation of matter by means of the use of energy and information, by which the informational content of matter increases. The idea is instrumental to the development of a theory of information value, which integrates human action as part of an universal process of production of information and purposes and interpretation of techncal progress as a process of acquisition of information (Pfeiffer, 1974).

The information used in the process of production of goods may increase by means of the learning process that is simultaneous to all production processes. Thus, the increase in the information content of matter does not imply the consumption or destruction of the information applied to it; on the contrary, the information available to reuse in figure work tends to increase (see next figure).

		Before matter is transformed by work	After matter is transformed by work
Available information	to reuse in future work	I_{w_1}	I_{w_2}
	in matter	I_{m_1}	I_{m_2}

In general, we have $I_{w_2} + I_{m_2} > I_{w_1} + I_{m_1}$. Yet we have $I_{w_2} > I_{w_1}$ <u>and</u> $I_{m_2} > I_{m_1}$ if we assume the existence of a learning process parallel to the production process and that the process does not disorganize matter more than it organizes it. Information presents here its auto-evolutionary character and its fundamental role in human action.

9.4. THE ACQUISITION OF INFORMATION AS A META-HUMAN PROCESS

According to von Weizsäcker, the existence of information requires a language, be it spoken or not, recorded in a living or not-living system. Thus, any kind of information requires not only a quantitative and syntactic approach, as proposed by Shannon and Weaver (1982), but also considerations at the semantic level. The semantic character of information, its meaning in specific contexts, presumes the existence of at least two identifiable systems: a sender and a receiver, whose roles are in general interchangeable. The meaning of information is related to the fact that the interchange of information is essential to provide the simultaneous transformation of both those systems. Pragmatic information changes both sender/receivers.

Neural communication developed in the animal world has in the human central neural system the locus "Par excellence" to its most advanced known manifestation. By the advent of the human species, socio-cultural evolution displaces socio-biological evolution. Man carries the learning potential that enables him to place himself in face of the evolution that precedes him and to change the direction of the interactions between living and not-living world, as he projects over the world his conceptions, images and ideas, by the intermediation of culture and technology.

The semantic character of information refers to its meaning in specific contexts. In its environment the human being generates information that provokes an impact of increasing instabilizer effect. Quantitative increase of information without significant change at the semantic level, by which genuine learning experiences are excluded, has leaded to a simultaneous growth in entropy: the process of acquisition of information is here not anti-entropic, but necessarily entropic.

In modern society, the transformation of the natural and social environment by human action, free of constraints that old conceptions of the world had imposed to it, tends to superate social and natural limits, above which social and natural equilibrium may be disrupted. As this process occurs simultaneously to an unprecedented growth in available data bases and information systems, the suspicion increases that the kind of knowledge that could contribute to a peaceful future to Mankind and to the natural environment has been constrained in our times. This kind of knowledge not only disappears, but is constrained not to appear.

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CHAPTER 10

THE IDEA OF A TRANSCULTURAL PLATEAU

Arnold Cornelis

While in traditional epistemology the human understanding was directed passively to knowledge of the external world, the new view is directed towards the human capacity of building active knowledge. The active role of knowledge in my approach stresses its constructivistic character instead of a data based and factual view of knowledge. The important point is not to know facts, but to be able to find them or to reproduce them. So establishing logical relationships can be situated in this perspective of constructivistic knowledge.

10.1. INTRODUCTION

It cannot be deduced from the nature of water how a ship should be build. Of course the ship must remain floating, but the question what is a good ship receives also an answer from the structural qualities and from the intentions for the use of the ship. This comparison is meant to show that verification and falsification of knowledge system depends not only on the points of contact with the external world-say the natural signs, instead of the primitive entities - but also on what is structurally possible in the logic of communication and integration of knowledge.

Let me illustrate this point by an example from linguistics. A person who does not speak French very well may attend to a piece of theatre in the Comédie Fran§aise in Paris. On his side is a French native speaker. The foreigner will complain that he does not understand well what is said on the scene, but in fact the foreigner hears, materially speaking, just as much as the Frenchman does. Only the Frenchman has learned to reconstruct whole sentences from the few words he hears and whole words form a few sounds. So instead of hearing the whole piece, that means instead of databased knowledge, he constructs the whole piece and this ability is precisely what is meant when we talk about a native speaker.

This constructivistic character of knowledge is also present in the subcultures of people who have studied physics, medicine, philosophy or informatics. The idea of a transcultural plateau refers to this ability of knowing what a certain meaning content implies in physics, in medicine, in philosophy and in any subcultural knowledge system.

The development of money as a sign is a beautiful example - from an epistemological point of view - of a meaning carrier totally devoid of meaning contents. There is no natural and no conventional relationship left between meaning content and meaning carrier. This can become rather dramatic when we sell for instance a house we have been living in. This house was a sign in the sense that our memories and our emotions are attached to aspects of its physical appearance. We can tell a story about even a scratch in the wall. But when the house is sold, we receive only money, the house is repainted, the natural signs prove to be disturbing for the new family who moves in. Our money can be used for any new meaning content. In the social communication form of buying and selling the meaning carriers become emptied from their meaning contents.

Nevertheless the meaning carrier which is money has to be provided with new meaning contents in order to function as a sign again in social life. If I want to buy a bread, it is not sufficient to put money on the paying desk. Because the money is a pure meaning carrier I have to add meaning contents from linguistics signs, saying that I want a bread. On the other hand I can explain how good bread is for my organism, but I will not receive the bread as long the money is not there. This example shows that money as a meaning carrier is necessary just as the meaning content. Together, money and message reconstruct the social sign needed for the economic transaction even in a simple case like buying a bread.

If we generalize this result we can see why capitalism and scientific thought are twins in the process of innovating and disappearing knowledge. Because scientific thought is the complementary part of pure meaning contents generated in societies with a surplus of capital. Capital is a meaning carrier in search of new meaning contents to be generated by scientific inquire. Capital is floating meaning carriers. While scientific knowledge is floating meaning contents. Together they express the abstract nature of modern societies. In the history of knowledge we find the surplus of money in Ancient Greece, in Holland during its golden Age together with the development of scientific thought. This illustrate our theory of the abstract signs.

When I started to think about the problem of disappearing knowledge I had in mind the disappearance of whole patterns of culture. So I looked for the way in which knowledge that disappears, say specific cultures all over the world, become transformed in a transcultural plateau which maintains their meaning contents, although the meaning carriers disappear. In this process the role of money and capital as a generalized and universal meaning carrier plays a decisive role. It is because money becomes the universal sign as a meaning carier that all the specific meaning carriers in the world system come to be replaced. So I concluded that I had to go in search for a universal meaning content system and I thought of the idea of a scientization of culture, paralleling the process of its capitalization in terms of money as universal meaning bearers.

When I worked further on the problem I distinguished subcultures not only in the form of styles of life and scientific specializatons, but also in industrial complexes and in political institutions. As a general definition I define now a culture as a knowledge system composed of programs for action and thought with implicitly or explicitly inbuild goals. This definition can be recognized in modern forms of management by objectives, in the organization of defense systems, in curricula of schooling. In short the idea of a culture could be narrowed down until we recognize in it any identifiable self-steering part of the social space of signs. So the question of finding a transcultural plateau meant going in search for a human capacity of communicating over the boundaries of each of these relatively closed subcultures. To find such a transcultural plateau is a matter of survival, not only for human individuals as members of the social system, but also for the social system as such. Because a system of knowledge without logical interaction between its constituent parts can never develop a self-steering capacity.

The idea of a transcultural plateau is affilitated historically with the idea of universal knowledge. It has been said that Goethe, who lived in the eightteenth centry was the last man able to overview the whole of human knowledge. But this is only true for factual knowledge. In our time we have already factual knowledge, because we have data based systems, encyclopedias and enormous quantities of books from where facts can be retrieved. So we have learned to think not in terms of facts, but in terms of programs. The idea of a transcultural plateau refers to the branch of the scientized culture as a whole.

We can illustrate this by a simple example of everyday life. When we read the newspaper we read only the headlines. How is it possible to read a newspaper by reading only the headlines? The answer to this question is that the rest of the newspaper we write it by ourself. We construct and reconstruct all the knowledge further contained because we have read the newspaper of yesterday and the day before and so on. This is an example of our transcultural plateau at work in everyday life. Of course when something new comes up we have to read intensively and to spell out all the details. But the next day our memory is there to regenerate the facts and we can restrict ourself to control whether our interpretation is in accordance with what happens further.

10.2. CONSERVATION OF INFORMATION

Postulating a principle of conservation of information relies on the empirical results of learning by reward. Reward explains why the results of learning are maintained by the learning systems. If we generalize this postulate of learning by rewards, we can use it as a general principle of conservation of information as all learning results wil tend to be maintained as a function of the system of rewards which we call society. However, as the nature of the societal rewards changes, the structure of the knowledge systems must also change. So, although there is a conservation of information, there is also a process of qualitative transformation showing the double spring-off of disappearing knowledge on one side and innovation of knowledge on the other side. We interpret this qualitative change as being an upgrading of knowledge towards a higher level each time when an aspect of the problem of communication between incompatible systems is solved. A transcultural plateau able to reach communication between diverging systems at the start, so transforming them both into a converging direction has a higher quality of information than each of the incompatible systems considered in itself.

The idea of conservation of information relies from a logical point of view on the principle of recusion. So the empirical foundation that has shown its validity in the behavioristic research tradition on learning processes becomes reinforced by logical theory as recusion implies the transformation of the results of learning in the past into new knowledge in the present, with an openness towards new forms of knowledge in the future. Thus, for instance, the organic unity of a human body is transformed in an operational unity of human action.

The idea of conservation of information casts a new light on old theories like metapsychose in the philosophy of Plato and on the idea of archetypes in the psychology of Jung. In our approach, introducing the principle of conservation of information enables to integrate the pathways of human communication in the same perspective of thought. The social theory of learning together with the results of theory of science in combination with recursion theory of human consciousness mold together in a modern view of what ancient philosphy tried to say.

10.3. LOGICAL BACKGROUND OF INNOVATING AND DISAPPEARING KNOWLEDGE

The renewal we propose can be founded on a logical analysis of the dynamics of signs. We assume as given the postulate that all human knowledge is based upon the nature and the use of signs. This postulate differentiates knowledge from organic regulation and abandons the paradigm of evolution when we are talking about knowledge. So we assume that knowledge is encoded in systems of signs, external to human organisms or rather exteriorized in a social space. The social space of signs of differentiated at the same time from the herditary encoding of the organisms and from the physical external world out of our control. The social space of signs is a third world breaking through the dualism of mind and matter, it is the realisui generis of the social system, the object of the social science. The social world of signs has the characteristic of being under control and external at the same time. While consciousness is internal and under control, at least relatively. And while the physical world is external and without control, at least in principle.

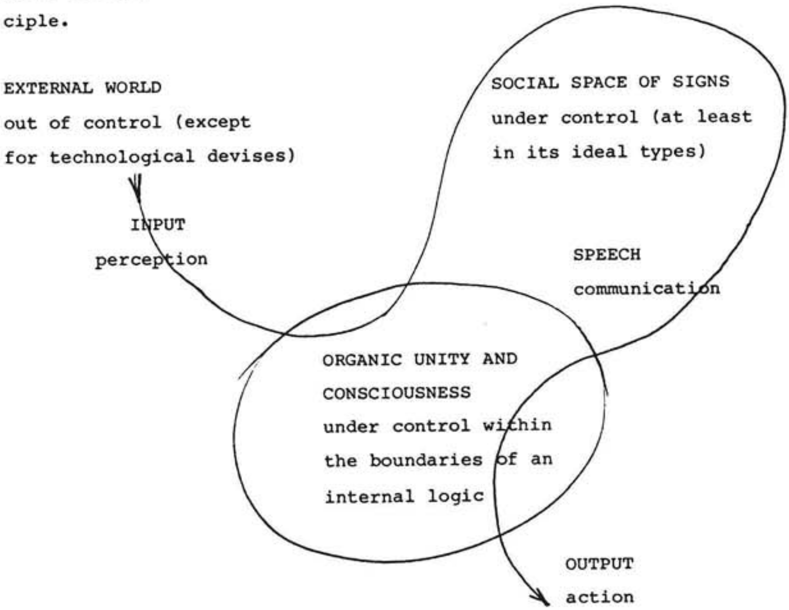


Figure 1: Breaking through the boundaries of dualism between mind and matter we introduce the social space of signs as an exteriorization of knowledge with maintainance of control. Thus the social space of signs represents a transformation of human knowledge by a recursion of the internal logic of control combined with the advantage of exteriorization which makes knowledge independent from the individual life of those who produced it.

10.4. DYNAMICS OF SIGNS

We distinguish three kinds of signs, natural signs, linguistic signs and abstract signs. Natural signs are oldest, not only in human culture, but also in the individual life cycle.

Definition of a natural sign

We define a natural sign as an undifferentiated intermixture of meaning content and meaning carrier. Natural signs are dominated by emotive meanings. Thus, for instance, the meaning of the mother for a child illustrates a natural sign. We assume that all new learning processes start with natural signs. Being in love implies that the meaning content and the meaning carrier cannot be separated. It would be absurd if someone said: 'I am in love, but I do not mind with whom'. The emotional layer build up with the help of natural signs makes possible the construction of a basic knowledge system, as a basic identity or a basic personality. Primitive man as well as civilized man are living in a social space of natural signs by which they have transformed the external world.

The things we are accustomed to and the things we love are natural signs. The physical world as such does not have signs. Human beings create signs and use them. It is very typical for the oldest theories of signs, as we find them for instance in Plato's work, that they can be recognized as talking about natural signs. Thus the idea is that a person cannot have another name than he has because this name expresses the essence of the person. If we look at ancient names, this position is very clear. A name like Prometheus means 'the one who knows in advance', while his brother Epimetheus has a name meaning 'the one who learns afterwards'.

Linguistic signs: transcultural knowledge of translating

The second kind of sign is the linguistic sign as it has been defined by de Saussure. There are two important aspects of renewal here. First de Saussure stresses the distinction between meaning content and meaning carrier. Second, de Saussure defines the relationship between content and carrier as arbitrary. He is right in presenting this renewal, but what has happened that enables him to define the nature of a sign in a new way? Essentially he approaches the problem of defining a sign from the point of view of general linguistics, taking thus a transcultural point of view and moving thus towards a transcultural plateau. General linguistics has the translation from one language in another in mind and is in need of an explanation for the transcultural phenomenon of translation. Translations means a recombination of meaning contents with meaning carriers. Therefore the distinct as well as the arbitrariness of their interrelatedness is logically presupposed. So it is the transcultural plateau that has produced the new epistemological innovations. If we compare with the social space of signs in ancient Greece, we are aware that for thinkers even of the level of abstraction of Plato there was only one language and only one culture. And from this perspective the natural character of signs is readily at hand.

What is the nature of the transcultural knowledge we learn by translating? A same meaning content can become represented by a plurality of meaning carriers. Table, tafel, tisch, appear as indicators that a specific line of decision has been chosen. The English word 'table' implies that we have choosen the strategy of speaking English, following the same line of decision for the other words we will use in speech. The meaning content can be considered hence as a goal setting, permitting a choice among alternative systems of means. So, the transcultural plateau of meaning content has the logical characteristics of a higher logical order compared with the lower logical order of meaning carriers. Meaning contents appear as goals, meaning carriers become means. The transcultural level integrates opposing systems in a logical ordering of programs of choice as alternatives for the same goal.

LOGICAL LEVEL OF MEANING CONTENTS (GOALS AS CONSCIOUSNESS)
Transcultural plateau exemplified by translations: meaning contents become exteriorized along alternative lines of choice into systems of signs are meaning carriers.

LOGICAL LEVEL OF MEANING CARRIERS (SOCIAL SPACE OF SIGNS)
The systems of signs are incompatible and closed as meaning carriers and can be integrated only on the higher logical level of meaning contents.

LOGICAL LEVEL OF EXTERNAL WORLD (PERCEPTIONS AND ACTIONS)
This level of perceptions and actions becomes transformed into the social space of signs as a result of human learning. This transformation process determines the boundaries between what is known and what is unknown about the external world.

Figure 2: The logical ordering as developed in the theory of types by Bertrand Russell permits to understand the transcultural plateau as a higher order integrating of incompatible systems of meaning carriers on the level below. Meaning cntents appear as a systematization of goals by alternative lines of choice for means. The logical ordering integrates the tripartite model of reality (fig. 1) breaking through the tradition of dualism of mind and matter and specifies the social space of signs as a set of alternative means under the control of goals as consciousness.

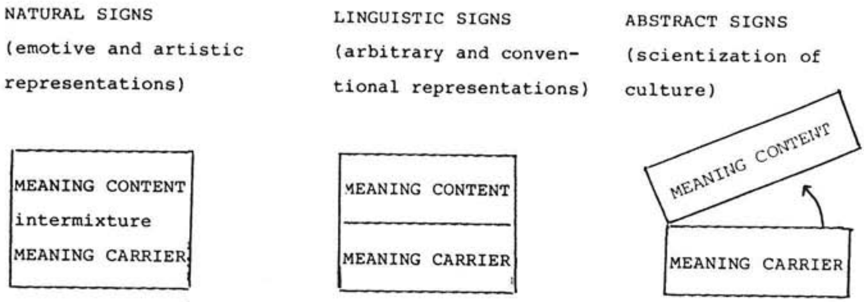


Figure 3: The dynamics of signs can be summearized in three stages of semiotic development. In a first stage signs are natural in the sense that meaning contents and meaning carriers are intermixed. In a second stage apparent in translations from one language into another the relationship between content and carrier becomes arbitrary and conventional making possible the permutation between the two parts of a sign. In a third state we postulate the separation of meaning contents and meaning carriers each organize themselves as identity on one side and structure on the other side. The identity of the learning system goes in search for floating meaning carriers, combining them in accordance with the internal logic of meaning contents. While the structure of the learning system as the social space of signs remains based on meaning carriers going in search for floating meaning contents. The process of innovating and disappearing knowledge is isomorphic with the reproduction DNA molecules in biology. But the difference is precisely in the innovating and disappearing knowledge process instead of mutations in biological encoding. Thus our theory explains the much faster and more systematic changes in epistemological reproduction as compared with the reproduction of biological heredity.

Definition of an abstract sign

The third kind of sign we want to introduce is the abstract sign. An abstract sign has the logical characteristic of abandoning any arbitrary or natural relationship between meaning content and meaning carrier. This process of abstraction is characteristic for scientific thought and more generally for what we call the scientization of culture. Meaning contents and meaning carriers are not longer organized by their interrelationship - either natural or conventional - instead they develop into two different systems, of identity on one side and structure on the other side.

Identity is a part of the learning system organizing itself on a basis of meaning contents and in search for floating meaning carriers. While structure is another part of the learning system, that means its social environment or the social space of signs, in search for meaning contents. So, the abstract sign generates a twin system of identity and structure which both reproduce themselves in a way isomorphic to the reproduction of DNA molecules in biology, but with the new characteristics of innovating and disappearing knowledge.

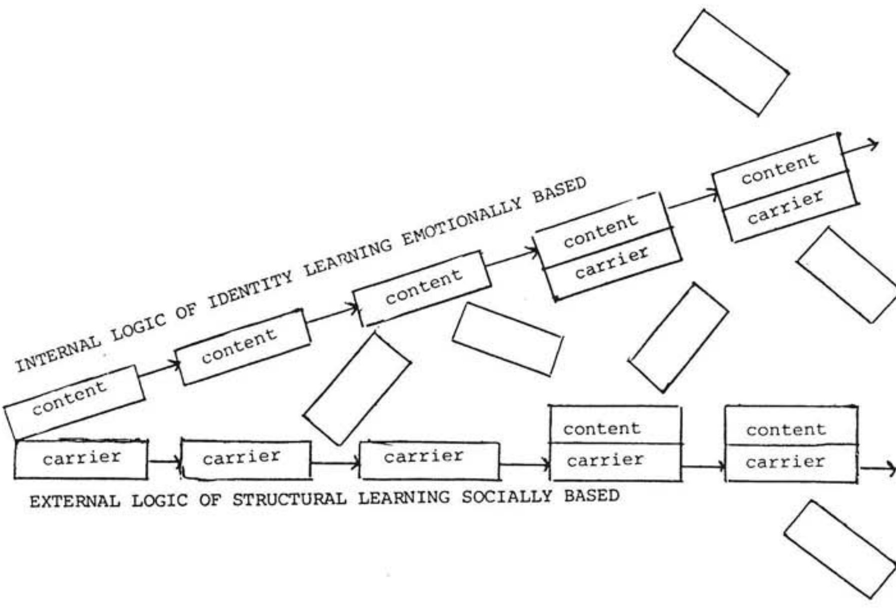


Figure 4: The idea of free floating meaning carriers and meaning contents together with the concept of abstract signs, with a censure between contents and carriers of meaning. The difference, however,

in the case of knowledge reproduction compared with heredity is in the differentiation between identity learning and structural learning. The human capacity of creating continually new meaning contents organizes the human identity on an emotional basis going in search for floating meaning carriers in the human environment or taking them from nature like poets do.

On the other hand the structure of the social space of signs recruits members of the community for working in accordance with the accepted meaning contents. Thus, for instance when someone accepts a job it is implied that the meaning contents (accepted as defining the meaning carriers) will be respected.

10.5. LOGICAL MODEL OF INNOVATING AND DISAPPEARING KNOWLEDGE

In building the identity system of knowledge we search for what we call floating meaning carriers in the social environment. These floating meaning carriers are like bicycles without an owner, standing there ready for use. We can for instance say 'you are my darling' indicating thereby a meaning content 'you are my system. I choose your logic'. In such a case we liberate the meaning carriers from their strict sense and we use them as metaphors. A metaphor is a logical strategy for making a meaning carrier floating in order to use it for a new meaning content.

Although information is conserved, there is a constant innovation and disappearing knowledge by new combinations of meaning carriers and meaning contents. Innovation means to attach a meaning carrier to a new meaning content. Disappearing knowledge means to detach a meaning content from a meaning carrier, making the meaning carrier ready for use in a new content combination. So the reproduction of knowledge in the cultural stage of abstract signs differentiates human identity and social structure as two opposing, but interrelated centers of knowledge organization. The human capacity of creating new meaning contents is therefore strongly reinforced in the semiotic environment of abstract signs characteristic for the scientization of culture in our time. The combination of innovation and disappearing knowledge replaces the notion of mutations in the research traditions of heredity and evolution. Human knowledge reproduces itself in a way that is isomorphic, but different from hereditary and inborn patterns. Human history is not evolutionary, but develops along the lines of innovation and disappearing knowledge.

10.6. IDENTITY STEERING DEVELOPS A TRANSCULTURAL PLATEAU

The interesting point from the perspective of a transcultural plateau is whether either identity or structure is the knowledge system with the greater capacity for variation. As a logical criterion we introduce the question whether the goals of the controlled system can be interpreted in terms of means by the system that controls. Human capacity has the ability to interprete in terms of meaning contents all the meaning carriers of any social structure. The epistemological reason is in the constant production of new meaning contents as the natural capacity of human identiy. While the social structure is restricted in the historical limits of its meaning carriers with their defined meaning contents. Social structure is the social memory of a culture. While the human identity is a transcultural plateau able to intergrate any social structure in the organization of its meaning contents. A human identity living and working ten years in Japan, then years in the United States and ten years in Holland develops a transcultural plateau under the pressure of the problems of communication generated by the mutually exclusive systems of culture. So the basis of an transcultural plateau is in the learning capacity of the human identity.

We can look at human history from the perspective of biology, economy, technology, linguistics, art and each point of view reveals aspects of the human capacity developing itself. If we generalize this perspective of learning, then we arrive at an epistemology of human history. But as history is concerned with the past only, we need to enlarge the scope towards the future. So we can speak about the epistemology of human time, incuding the past as well as the future and of course the idea of a present as the relationship between the past and the future. This perspective of an epistemology of human time enables to specify what is going on in what we call the scientization of culture. The general movements is towards the construction of a transcultural plateau.

Human knowledge is organized along two principles of ordering, tranditionally indicated as space and time. In the theory we develop in this paper space and become transformed in the notion of human identity. So the twin relationship between human identity and social structure can be recognized as an elaboration of the principles of space and time as foundations of organizing human knowledge.

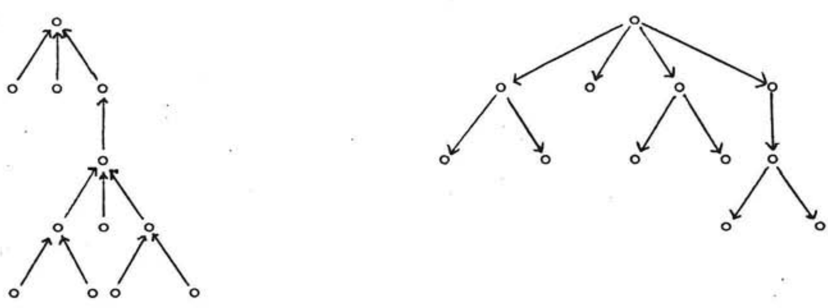
The line of development of human culture in the process of scientization starts with naturalism as an epistemology in which space and time become objectified in the study of the physical world. Next comes the epistemology of structure, beginning at the edge of our century, as an epistemology of the social space of signs. For the future we expect the advent of an epistemology of identity which is an elaboration of the epistemology of time.

The foundation of the epistemology of identity is based not on meaning carriers, like in the epistemology of structures but on meaning contents as a principle of organization. Why is the epistemology of time so late in the line of development of culture? The main reason is that time cannot be seen in contrast to space and to the structures of meaning carriers representative of signs. However, meaning contents are just as indispensable for signs, as we have seen. Moreover meaning contents have a larger range of variability making them suitable for the construction of a transcultural plateau.

Time functions as a logical foundation of the meaning contents in the processes of communication. Time is implied in the notion of an event, in telling a story, in explaining the causal succession in processes in the external world, in forecasting the future and in prescribing what has to be done. Time summarizes the steering capacity of human consciousness either by explaining why happened what could not be avoided or by giving reasons for what is or was choosen to be.

As for what cannot be avoided the idea of a transcultural plateau was easily reached in scientific thought. The general character of theoretical physics runs through all cultural boundaries. As soon as choices have to be made the transcultural plateau can only be reached by taking into account the set of alternatives by which socially anchored knowledge is attained. We have related this aspect to the knowledge reached by translations and interpretations. However, the more serious problems are those caused by opposing interest dividing knowledge systems and making their integration impossible in the present state of affairs of politics, economics and of the financial system in the world. The introduction of an epistemology of time permits to elaborate further what is avoidable and unavoidable. There are aspects of reality that may seem to be avoidable in the short run, but not on the long run. The scientization of culture consists in enlarging the scope of time making clear what is inavoidable in steering intentions on a transcultural plateau of human thought.

TRANSCULTURAL PLATEAU (innovation and disappearing knowledge management)



IDENTITY LEARNING (INDUCTIVE)
developing the next higher level of alternatives as innovative knowledge

SOCIAL STRUCTURE (DEDUCTIVE)
present and past knowledge as candidates for disappearing by integration or obsoleteness

Figure 5: Innovation comes from identity learning using a higher order learning and transforming thus existing and past knowledge into alternatives that can be replaced by new knowledge. The idea of a transcultural plateau is to be located in the superior level of the identity learning ready to transform the social structure from above.

10.7. THE TRANSCULTURAL PLATEAU: STRUCTURAL FALSIFIERS AND IDENTITY FALSIFIERS

There are two reasons why identity learning arrives at a transcultural plateau. One reason is that identity learning has to master and to play a role in more than one structural branch of the social deductive tree of applied knowledge. For instance a woman with a family and children is in the branch of education. But she may at the same time have a job. So she is also in the branch of labour structure. Maybe she even continues studying or doing research. Each of the branches in the social structure represents different subcultures which are mutually exclusive. The working situation ignores family life. The organization of studies ignore the working situation. But the identity of the woman is obliged, in order to continue the kind of existence she is engaged in, to combine these subcultures in one transcultural plateau. Her epistemological position makes her innovative. Children can receive part of their primary education in the working situation and adult schooling becomes organized. At the same time older models becomes absolete.

The second strategy for arriving at a transcultural plateau consists in enlarging the scope of time. Strictly speaking the epistemology of time is a discovery of falsification. The reason why knowledge disappears is that knowledge becomes false. The most dramatic situation is of course that knowledge disappears because the person representing and bearing the knowledge dies. In a pessimistic mood Thomas Kuhn has suggested that old paradigms of scientific thought disappear in this way. But I doubt whether biological interpretation is a good description of what actually happens. Of course death is a falisfier in time in the field of economics. To loose a war or a political position is another form of falsification in time. Now, what is the role of scientific thought and more specifically what is the role fo cybernetic research and systems theory in this process of falsification? I suggest that this role is to anticipate by avoiding such disasters. Therefore a transcultural plateau has to be designed as preventing real falsification.

10.8. THE TRANSCULTURAL PLATEAU AND SOCIAL POLICY

The renewal in thinking that is implicit in our approach to the problems of disappearing knowledge emerges most clearly if we apply it to the relationship between scientific thought and social policy. The view that scientific research is unable to formulate value statements is one of the most important examples of knowledge that has to disappear. Instead the innovative approach is indicated by the idea of a transcultural plateau.

I have tried to show that there is nothing special about the problem of values, which is in fact the problem of making choices in a way that can be logically and epistemologically justified. The general strategy of scientific thought, already formulated by Karl Popper is to formulate knowledge in such a way that it can be, in principle, falsified. We can notice that Popper introduced the idea of an epistemology of time without mentioning it as such. Because a hypothesis remains valied as long as it has not been falsified. But here we introduced a new element in the discussion by asking 'what kind of falsifiers do we admit?'

In Popper's Logic of Scientific Discover I see one type of falsifier, that is those phenomena produced in the external world. I want to add two other types of falsifiers: structural falsifiers and identity falsifiers. This will make clear why I have introduced a three partite model of reality. As reality is build up from three categories of existence, the external world, identity learning and the social space of signs called structure, we can expect also three kind of falsifiers corresponding to each of them. Structure is a social transformation of space, while identity elaborates time.

What is a structural falsifier? In the social space of signs such a structural falsifier defines what is possible and impossible in individual action and in social policy. As soon as we have this notion of a structural falsifier in our mind we can go in search for empirical manifestations of it. Is it structurally possible to install rockets in the most densely populated part of Europe? Look whether structural falsificators are coming up. Is it structurally possible to economize in developing countries in order to diminish inflation? Is it structurally possible to continue the pollution of the sea? The idea of structural falsification can guide social policy.

Along the same line of thought we search for identity falsifiers. It is in the logic of scientific thinking that we shall not do certain things that may be structurally possible - which is already a restriction compared to what is physically possible - precisely because we bear in mind the situations of the future. The identity falsifiers take into account the dimension of time, not only be looking at the past, but also by making the future possible. The identity falsifiers contribute in a scientific way not only to guide towards specific innovations, but also to make certain models of action disappear. The problem of disappearing knowledge becomes thus a problem of scientific inquiry.

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CHAPTER 11

DISCOVERY OF THE DISAPPEARING KNOWLEDGE BY THE "USOMID"* METHODOLOGY

Matjaz Mulej

Not only Yugoslavia, but actually all underdeveloped countries (industrial latecomers, former colonies, now economic neocolonies) seems to be in trouble due to their lack of knowledge (Stanovnik, 1984, 1985). More or less the same is true for the less industrialized members of OECD (OECD, 1984). On the other hand, the USA study transition from the scientific leadership to technological leadership (NSF/ISTI, 1984) and are afraid of their own economic situation and prospectives. So do many other countries all around the world.

We study a facet of this problem. I will deal only with situations which I face daily. Their common denominator might be twofold:

- the rate of growth of productivity (effectiveness and efficiency) lags far behind the rate of growth of the number of persons finishing their education (of any level up to Ph.D.);
- still, nobody (to best of my knowledge) has ever got in trouble for never proposing anything new.

11.1. FORM DIALECTICAL SYSTEMS THEORY TO USOMID - AGAINST THE RIGHT OF IRRESPONSIBILITY

Over a decade ago I started to interlink creativity and systems theory from a methodological viewpoint which led to establishment of the Dialectical Systems Theory (Mulej, 1979). It was to assure methodologically that the demand of the General Systems theory for wholism becomes possible at an in-

* USOMID is the Slovene abbreviation for: Oriented, Wholistic, Co-opeative and Organized Invention and Innovention Activity of the Masses.

terdisciplinary level. Soon, it was clear that the formulation was not simple enough for the DST to be applied, which led to USOMID (Mulej et al. 1981, 1983, 1984, 1985).

The five components of DST are:

1. the law of the hierarchy of succession;
2. the ten rules to provide innovative starting points of persons involved;
3. the ten rules to provide wholism throughout the entire process from definition of starting points till the final result;
4. the methods of modelling creative work processes to make the creative work and co-operation easier;
5. the law of entropy (as the natural tendency toward distruction), a permanent source of the need for creativity.

I will not elaborate upon them, this was done in several conferences (EMCSR, Vienna, Austria, 1976, 1978, 1980, 1982, 1984; Amsterdam, 1979, 1983; Raleigh, NC., USA, 1984; Hartford, Co., USA, 1984; etc.) and is available in proceedings.

The ten components of USOMID are:

1. education for creative co-operation with short courses (of which 80 were given from June 1981 until March 1985) and practical experience;
2. development of "being considered" as the basic value guiding people toward creativity, and so a precondition for their motivation for creative co-operation;
3. charting the processes which take place in the organization, in co-operation with workers who do them and know most about them, including also the processes of creative work, in framework(!) programs, compiled into a "programmoteque";
4. possibly, a computer-aided programoteque, deeper in quantitative insight into the basic process (supply-production-marketing);
5. the USOMID/SREDIM procedure as the method of creative co-operation;
6. deliberate seeking for possible changes as the basic orientation of such a creative co-operation;

7. application of "management by objectives" which include support and constant care for creative co-operation as a whole and its strategies for innovation with objectives of every (middle) manager and foreman;
8. proceeding from the general insight toward details in order to fight unavoidable one-sidedness by interlinking the specialists' viewpoints into a (dialectical) system of all essential viewpoints;
9. studying the process and their environment, not the organizational structure and the hierarchy of subordination, and following the links of their steps with the "causes tree" (based on Arthur Spinanger's idea of the elimination chart);
10. the USOMID circle as the organizational possibility for a permanent creative co-operation; they work in the regular and extended staffing.

The result can be called a sort of a transition from the level of the basic science to industrial technology; the (dialectical) systems thinking enters the heads and hearts with no single word of philosophy, system science, etc. This proved to be very useful as a tool to reduce the disappearing of (previously unused) knowledge.

Namely, such a disappearing takes place permanently due the general application of the so-called Scientific Management (Taylor, 1957). It causes the classification of people into the "thinking ones" and the "working ones" and gives them both the right of irresponsibility which is incorporated into their position in the hierarchy of subordination. "The working ones" are not supposed to think, but to obey blindly, the "thinking ones" cannot be controlled because they do the creative work. Besides, the thinking ones command and do not have much time left for acquisition of new knowledge as well as for creativity, and the working ones are alienated and need no new knowledge or creativity, or at least do not feel such a need.

This situation, obviously, need a dramatic change of mentality, social and industrial relations. Therefore the starting points have to be influenced, e.g., like by USOMID:

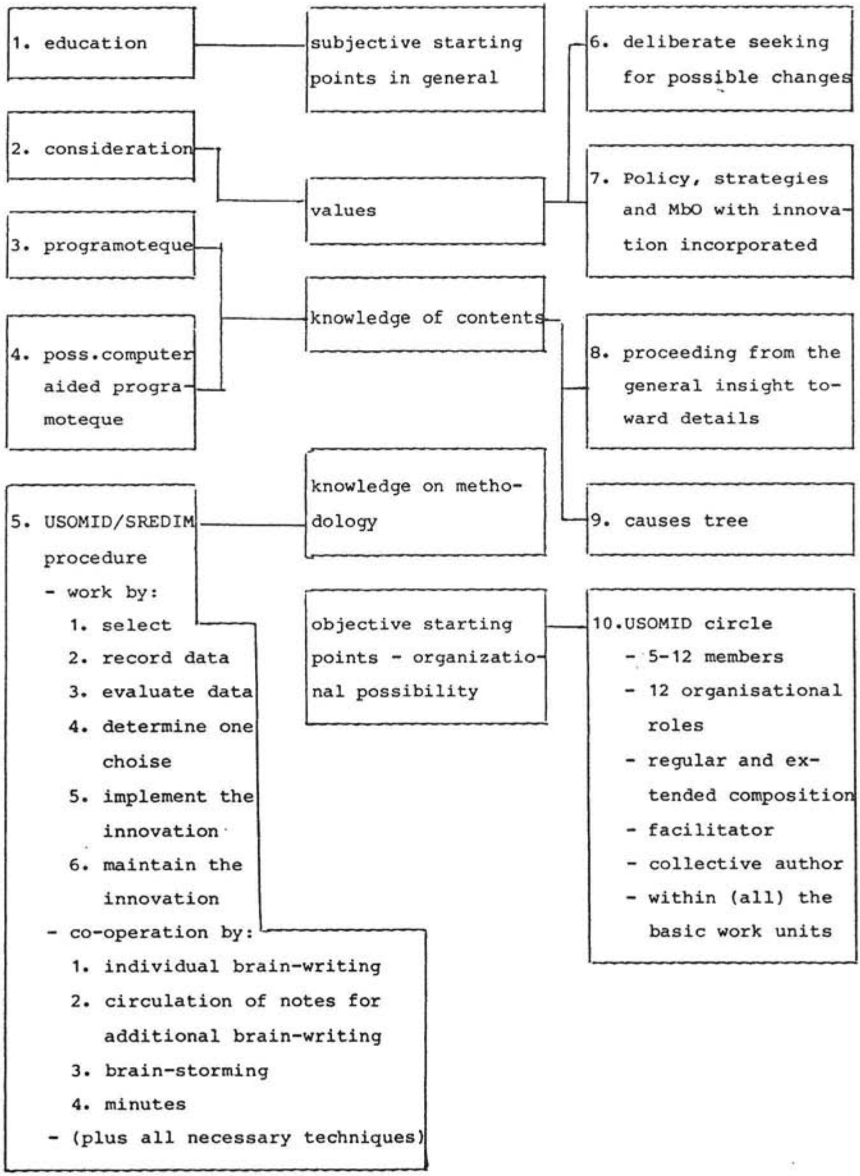


Figure 1: Influence of the (dialectical) systems thinking over the subjective and objective starting points through the methodology for creative co-operation of the many USOMID.

11.2. SOME EXAMPLES FROM THE EXPERIENCE WITH USOMID

In one of the first organizations wherein we applied USOMID (though still in its rudimentary form), we were contracted to help them to solve the problems of job description and wages. They had a very poor insight into their process of work and co-operation in it, due to the Tayloristic stressing the hierarchy of subordination and work units rather then their common process. So we found an engineer who signed the statement that he has no defined task to perform. We also found a lady a few months before retirement, who was the only person to do foreign exchange bookkeeping. She has never been asked what is her opinion about anything. When we approached her with such requests and questions about her own work, she kept telling us about the procedures and contents of her work for three days. Without USOMID her knowledge would disappear when she retires.

In another factory we were contracted to help to solve the organizational problems which came along with the growth within years without reorganization. A part of their production process was performed by a group of poorly educated, but well experienced workers with a lot of routine. They were soon able to co-operate with us, we charted their process together and they provided a lot of information. Their production manager, when he saw the results, did not believe it: "They cannot think", he maintained.

After these two examples of one type of our experience, let me give an example of a positive and productive attitude and behaviour of the manager who is not Tayloristic.

In a factory with which we still work, managers believed in their workers' capabilities before our co-operation. One of their projects was the transition from quality control to quality assurement by co-operation. They formed quality teams and soon found a lack of capability of co-operation. This brought us in. We gave a series of USOMID courses, and in one division we worked the model out with all details. Again, we could dig out a lot of knowledge by such a co-operation.

A wholesale warehouse moved from many obsolete small facilities into a large and modern building with no additional training of their workers. The results lagged far behind the expectations, though their new manager used to design such warehouses for eight years, including this one. He was far from satisfied, but he had no new ideas anymore himself, neither did he fiund any solution in literature or in consultancies, nor has any of his workers ever been an inventor. He still had no other way but relaying on them. Due to the previous Tayloristic management they new nothing but their own jobs in a very narrow sense of the world. They were on constant fight with the commercial department about the issue who has caused the mistakes which occurred daily. They blocked the computerization of their work. They used to go on strike for a few hours almost weekly. But the manager was not only a college guy, he started as a plain worker and studied part-time. So he understood the situation and feelings better. So he started looking for a solution. We hapened to meet a few months later, when he already had his first results, but saw the limits. He started applying USOMID in combination with his own experience and ideas. They charted their organizational environment, their work processes, found out the interdependencies with the commercial department and with the computer, they got those guys involved in a co-operation, the manager himself supplied the information from professional literature and experience, etc. After two years, the results were surprisingly: the best economic result we have ever had with USOMID, clear mutual relations, no more strikes. They have a permanent panel to speak out and to be actors in an action, they are creative inventors.

I could give several other examples from the three years of a broader application of USOMID in organizations. But in essence this would be repetitive.

11.3. TAYLORISM INSTEAD OF A (DIALECTICAL) SYSTEMIC CO-OPERATION - THE MAIN CAUSE OF DISAPPEARING OF KNOWLEDGE IN BUSINESS ORGANIZATIONS

I believe that there is no reason for the limitation to the business organization, since my first good experience was in my tennis club when we prepared our first Davis cup matches sixteen years ago. But, officially, USOMID so far was applied in business organizations only.

Taylorism was created with a very good intention, nearly a century ago, to bring more science into engineering in order to diminish the selfwill of the owners in relation to the work load of the workers. This happened, but not the relations which Taylor wanted. The real relations which surfaces, divided the companies body into the thinking part and the working part, the owners left aside more and more.

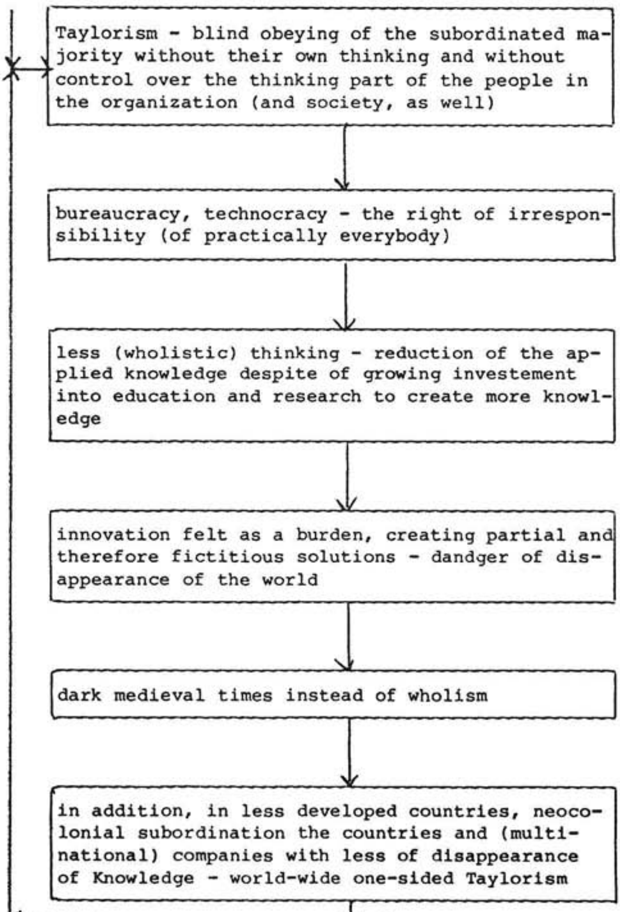
This last element has a lot to do with socialism. Though Taylorism in such a version has nothing to do with the basic paradigm of socialism or self-management. Lenin's practical troubles incorporated the "scientific management" into socialism (Merkle, 1980). Lenin dismissed the owners after his October Revolution and found Taylorism the best managerial technology in his circumstances with no owners and no knowledge with the workers, and only few experts. So, he had to install them, Taylorism backed him best of all. In those circumstances I would not blame him, nor the American and Westeuropean development in the same direction. Besides, the mass production of standardized products demanded and still demands a lot of order (Reich, 1984), but it also kills room for creativity and knowledge but the repetitive routine, except for the managing elite.

Since they need to be understood by their coworkers, and since competitiveness has depended more and more on innovation and expertise, now-a-days the number of teaching staff at universities and other institutions of higher education in the world (and in Yugoslavia) is equal to the number of the primary school teachers in Taylor's times!! (Pecujlic, 1980). Technology is very different, too, and changes much more rapidly, so it demands much more knowledge and even fresh knowledge, i.e. much more innovation then ever before. Two of the consequences are very important from the viewpoint selected for this chapter:

- Taylorism makes us all so narrow specialists that we hardly can be creative enough any longer unless in co-operation, but the vas majority has never been educated or trained for co-operation, only for work;

- Taylorism (in its living, not written author's model) causes imposing of the novelties upon the workers, who tend to be against the novelties, unless they are their (co-)authors themselves, trusted by their management and feeling that they are considered.

Both consequences cause the following chain:



<u>Figure 2</u>: The permanent blind alley due to the disappearance of the (permanently created, but not applied) knowledge caused by Taylorism.

The famous eight rules, learned in the best-run companies (Peters, Watermann, Jr., 1984) help much more against the industrial relations which cause the disappearance of knowledge:

1. A bias for action: a preference for doing something - anything rather than sending a question through cycles and cycles of analyses and committee reports.
2. Staying close to customer - learning his preferences and catering to them.
3. Autonomy and enterpreneurship - breaking the corporation into small companies and encouraging them to think independently and competitevely.
4. Productivity through people - creating in all employees the awareness that their best efforts are essential and that they will share in the rewards of the company's success.
5. Hands-on, value driven - insisting that executives keep in touch with the firm's essential business.
6. Stick to knitting - remaining with the business the company knows best.
7. Simple form, lean staff - few administrative layers, few people at the upper levels.
8. Simultanious loose-tight properties - fostering a climate where there is deciation to the central values of the company combined with tolerance for all employees who accept those values.

From the viewpoint of management, the key to stamp out Taylorism may be best expressed so (Blanchard, Johnson, 1984):

. "Take one minute out of your day to look into the faces of the people you manage, and realise that they are most important resources of wisdom. Recognize the importance of these sources of wisdom! People who work with you as their manager will look at you as one of their sources of wisdom".

We found in our experience that, at least in an underdeveloped country with a short history of industrialization and democracy, it is true what The New York Stock Exchange has well documented in 1982: the key is the attitude of the managers, they need a reeducation from Taylorism toward co-operation for mutual creativity. The workers hardly do any harm, they come along. This is especially hard with those who came to their (middle management) position with the following combination:

1. the soap rule : when coworkers can no longer stand a person, they get him out by squeezing;

2. Peter's rule : he got over the ceiling of his capabilities; now he needs co-workers to do the job for him;

3. Parkinson's law : the more subordinates he has, the bigger boss he is;

4. double jacket rule: there is not enough work to be done, so one has a jacket on one-self, the other one in the closet in the office; after arriving there, one puts the jacket from the closet over the chair ("I will be back in a second!") and off we go.

Figure 3: The combination of rules to produce non-creative (middle) management with the right of (well-paid) irresponsibility.

11.4. INCORPORATION OF INNOVATION BY CREATIVE CO-OPERATION OF MANY AS OUR WAY FROM THE TAYLORISTIC RIGHT OF IRRESPONSIBILITY AND THE DISAPPEARANCE OF KNOWLEDGE

In our most recent research (Mulej et al., 1985) we created the following answer how to fight Taylorism and routinism, which both kill creativity and cause the huge discrepancy between the potential knowledge and its actual application for creativity:

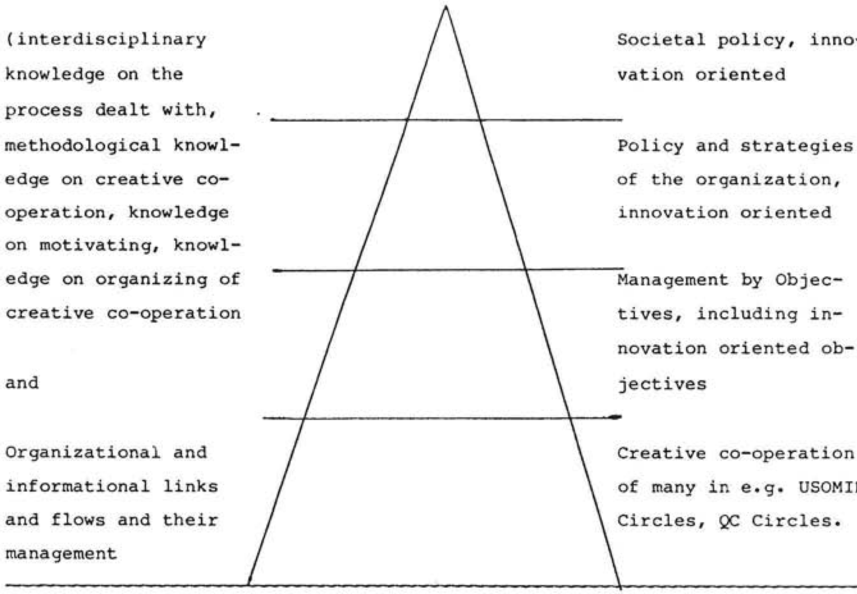


Figure 4: Innovation policy and practice.

No one of the six elements may miss. In the study we elaborated all of them.

On the level of the societal policy, things have moved ahead in the recent period in Yugoslavia (CK ZKS, 1984 and 1985; Crveni barjak, 1984; Sabor SRH, 1984; Yugoslave Federal Assembly, 1985), but less in the financial motivation acts. Also, some other important problems are still open, which create disappearance of knowledge, e.g.:

- the need and possibility for mutual support of R & D departments and the oriented and organized creative co-operation is still too rarely perceived, as well as their mutual interdependence, which is extremely strong, especially under self-management;
- innovation activity of many, is still bound to the Suggestion System only;
- innovation activity is still perceived more as an unimportant technical issue, rather than as an important economic, psychological, sociological and political one;
- etc.

All these relations have to be changed if we want to stop the further disappearing of knowledge and to foster its application, e.g. by:

- more creative co-operation, which could diminish their right of irresponsibility, routinism and active use of less than half of their actual capabilities;
- more agreed orientation of creative efforts, so to concentrate upon the really important parts of the processes on the basis of consideration of people as possible and valuable co-authors, deliberately seeking for possible changes, in line with the policy, strategies and MbO which are all innovation oriented, wherein the programoteque demonstrates the necessary points of concentration for innovation and offers the (most general, not all) basis for creative co-operation as the source of information about the processes which are not elaborated in the technological documentation;
- more wholism (= systemic, systematic, realistic and dialectical thinking), due to the application of USOMID.

11.5. CONCLUSION

Social and industrial relations oblige an employee to behave on the basis of division of labor (= Taylorism). Thinking from the viewpoint of management is neither important nor desired. So, creativity is not expected and alienation grows. The lack of a dialectical wholism causes a neocolonial subordination to those who develop, prosses and apply knowledge and to not let it disappear. That is what we are trying to fight against with USOMID.

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CHAPTER 12

THE DISTINCTION BETWEEN 'HARD' AND 'SOFT' SCIENCES AS A QUALITY THREAT IN PHYSIOTHERAPY TREATMENTS

Arno Goudsmit

We may distinguish between 'hard' and 'soft' sciences, and accordingly between 'hard' and 'soft' techniques and skills in health care. In this paper 'soft' techniques denote procedures from behavioral and social sciences, in which a humanist, non-technical, approach prevails. They are characterized by respect for the patient's integrity and autonomy. Treatment is first of all 'facilitation', not 'manipulation' (cf. Rogers, 1951). In 'hard' techniques, on the other hand, the patient is considered to be controllable by correct application of procedures. Both hard and soft techniques are of use when treating people.

12.1. INTRODUCTION

Generally, soft techniques are about coping with the patient's motivation, his confidence in the therapist, his anxieties and defensive tension (a muscular tension that occurs in anticipation of painful manipulations by the therapist), the therapist's use of tact (cf. Reik, 1948) and his ability to join the patient (cf. Minuchin, 1974). The vital importance of them is generally recognized, as most medical and paramedical curricula contain psychology and social skills courses.

This recognition however is rather formal and official. Something curious is the matter with these soft skills in physiotherapists' practice (among other health care disciplines). The position of soft techniques within physiotherapeutic practice and their relationship with hard, medico-technical, procedures, is far from clear and is even hardly studied (cf. Caney, 1982). This may be due to a language barrier that succesfully separates the domains of bodily and mental phenomena, diseases and treatment. Now that economic recession urges everyone to account for their activities, physiotherapists cannot account for the use of soft techniques as they can for hard techniques. And unaccountability leads to can celling, at least to a decrease of financial support. This paper attempts to show a way to account for soft techniques in physiotherapeutic practice.

12.2. THE ACCOUNTING PHYSIOTHERAPIST

In interviews physiotherapists either apologize for using soft techniques, or for not using them. Their apologies show they are not able to look upon these activities as compatible with their regular tasks, however necessary they may find them. When they do apply soft techniques, they often apologize for not doing their proper job. When they do not apply them, on the other hand, their apologies concern their not being able or entitled to apply them (e.g. due to their position in the health care network). Apologizing seems to be an unavoidable component in physiotherapists' accounts of their use of soft techniques, whether they use them or not. What is the matter? Are soft social science skills and techniques to be looked upon as mere hobby and unproper job aggrandizement for the physiotherapist? Are they elements of physiotherepeutic treatments that are necessary, but of unexplicable importance? A few cases, as they appeared during interviews, may illustrate the point.

> A physiotherapist emphasizes that he prefers to deal with people without having to treat them as little children. He wants them to be motivated for practicing the homework bodily exercises, but knows few ways to motivate them, apart from arguing. Using psychology or psychological skills does not seem to him part of his profession. He prefers to converse about hockey matches with his patients during treatment.

> Another physiotherapist is so much convinced of the predominant role of human relationships during therapy, and of the function of the patient's life perspective, that he does not refrain from guiding a handicapped patient into the local library, in order to have him expand his 'life space'. Though very much convinced of

the use of these activities, he is not able to account for them towards his doubtful colleagues.

A general hospital physiotherapist contends to have a good relationship with a patient, while it appears that he avoids crucial problems. After ten minutes of digressions on the pleasantness and openess of the contact with a terminal lung cancer patient, it appears that he avoids to get informed about whether or not his patient does have knowledge of the terminal state of his lung cancer, asking neither his team colleages nor the patient himself. It does not impede him from doing his job in the way he wants to.

As a contrast, a physiotherapist proclaims at the start of the interview to prefer clear and effective techniques in treatments, whereas at the end of the interview he deems the patient's personal trust in the therapist 'the only thing that really works, whatever technique you apply'.

Finally, what to think of the physiotherapist who considers all physical diseases 'physic', keeping utterly vague about what he means by it?

These are examples of the diversity of weights physiotherapists attribute to applying soft techniques. These therapists have in common their inability to account for combining them with their formal tasks unusually formulated in terms of hard techniques). One may consider the physiotherapist's soft techniques as his set of tacit skills (cf. Polanyi, 1958), important, but hard to make explicit even by the experienced user.

12.3. THE ROLE OF ECONOMIC FACTORS

Because of the recent increase of economic pressures on the health care system, there is a tendency to attach more and more value to those elements that are concrete and controllable. This trend is in schools of physiotherapy, and in physiotherapist's practices. In curricula more and more emphasis is laid on proper techniques and adequate medical knowledge, whereas psychology courses get a subordinate, disconnected status (resulting in teachers' defensive justifications as "psychology is also important"). In physiotherapists' practices the personal contact between patient and therapist becomes restricted to the necessary minimum, e.g. as patients are treated simultaneously and application instruments are used much more than before. Therapists less and less allow themselves to spend time and energy to those activities that cannot be defined as a hard technique, and therefore not be specified on the declaration list for the patient's insurance company.

Furthermore, due to economic pressures, health care sponsors are less and less accepting to financially support activities (both therapy and research) that cannot be defined in terms of concrete procedures and controllable results. This partially explains the dominant place procedural technology receives in health care, both in medicine, e.g. developments in medical expert systems (Engelhardt, Spicker & Towers, 1979) and in psychology, e.g. the emphasis on outcome reserach in psychotherapist (Smith, Glass & Miller, 1980). Those soft skills and procedures that do not fit in well with this language will have to be reformulated in order to accomodate, or will be cut short financially or even extinguish. Accountability seems to be of major concern; activities loose support, if they cannot be accounted for in terms of costs and effects. Soft techniques happen not to have these features.

12.4. THE LANGUAGE BARRIER AND THE NEED FOR ACCOMODATION

Apart from economic factors, soft skills are undervalued because of the language in which they are expressed and the unclear rules according to which they are applied. Soft humanistic approaches, as they are formulated now and cultivated in social sciences, are incompatible to the language of concrete technical procedures. Accomodating the formulation of the soft techniques to the prevalent language, would seem to improve their accessibility to those medically trained, as well as their applicability. A considerable linguistic and cultural barrier is often felt to using psychotherapeutic or social work skills. What is more, there is a technical barrier as well. Soft skills cannot be applied like hard skills: performed technically and 'coolly' they estrange the patient rather than comfort him. Also measuring their effectiveness (as is tried extensively in psychotherapy research) has been seriously frustrated by the lack of valid criteria.

Not accomodating leads to alienation from the mainstream health care. This is happening with movements that introduced a new holistic medical view of man (e.g. in a.o. bio-energetics, acupuncture or anthroposophy). The same is true for various countermovements in physiotherapy (e.g. Colton, 1983). In the Netherlands, Veltman (1977) and others elaborated a branche of physiotherapy called "haptonomy'. Its ideas are based on phenomenological existentialism, and stress the importance of interpersonal encounter during treatment.

However valuable these ideas may be, they are not comprehensible to the majority of physiotherapists. The language in which they are expressed is highly incompatible to the medico-technical language. More generally, one may contend that there is a large treasure of useful knowledge (e.g. Merleau-Ponty, 1942; Buytendijk, 1950), that is ignored and neglected, as it is concealed behind a web of phenomenological phrases. Here too, reformulation seems to facilitate application.

The accomodation of the formulation of soft skills to medico-technical language is not to be the mere appending of one corpus of knowledge to the other, as is the case very often in psychology curricula for medical disciplines. Rather, some interface is needed for the connection to be made. Only then soft skills may become accessible to the medical technician, and become accountable in terms of clear procedures. What is needed is a translating instrument, that leaves both hard and soft techniques intact.

12.5. TWO SYSTEM MODELS

Autopoiesis theory, as has been developed by a.o. Maturana (1978) and Varela (1979) might be useful as such a translating instrument for two reasons. First, it distinguishes between open and closed systems; second, both observer and observed system being living organisms, they can be modeled by the same kind of closed system. This allows us to discuss the structural coupling between two systems, one of which is observing and describing the other system according to the open or closed system model. The reader who is not acquainted to this theory is referred to Varela (1979) and Zeleny (1981). In the following pages first two systems views will be sketched, that can be used for describing living systems (organisms), especially in regard to their dysfunctioning. Next, switching between these two models will be set out as a way to connect hard and soft techniques, and making the latter instrumental to the former.

Illness and treatment according to the open system model

The open system is seen as a set of relations between input evens and output events. The system can be controlled ('instructed', as Maturana calls it) by manipulating its inputs, so that the outputs change. This may be common knowledge since Ashby (1956).

According to the open system model, illness can be defined as a deviation from criterion conditions due to a deviation of the system's inputs. Thus a bacterial infection may be looked upon as an input, that results in various symptoms (outputs). Therapy consists of changing the system's inputs (e.g. by means of antibiotics) and thus influencing the outputs by manipulation of inputs. Since these notions are too well known, they will not be elaborated upon here.

Illness and treatment according to the closed system model

The closed system, on the otherhand, is defined as a set of relations between components, such that state transitions of the system are 'state determined' (Maturana, 1975). A perturbation of these relations will result in the system's compensating for the perturbation (with certain limits). Beyond those limits, the perturbation will destroy the system, or change it into a system that has a different organization. The closed system strives to maintenance of its organization. Thus, perturbing events are dealt with in a way determined by the present state of the system. To an external observer, however, it may look as if the system behaves as an open system, showing an output in response to the input (perturbation). A closed system, however, functioning autonomously, cannot be instructed and influence as an open system. To ask how to influence it, is to ask firs how to perceive the system as an open system. The role of the observer is vital in treating the system. Switching between (open and closed) system conceptualizations will be discussed in section 2.3.

According to the closed system model, the system continously undergoes perturbations that have to be compensated. Maturana (1970) introduces the term 'description' to denote the closed system's way to restore its equilibrium, and thus to compensate the perturbations that work upon it. We will stick here to this use of the terms 'description' and 'describe'. Describing the environment is seens as a cognitive activity, performed both by the system, and by its external observer. Each can be seen as continously generating descriptions of his environment that are maximally 'viable' (cf. von Glasersfeld, 1981), i.e. that give the best opportunities for dealing with the environment (in terms of actions, theoretical models, or whatever). As the observer's theoretical constructs (such as his open and/or closed system models) are seen as his descriptions of his environment, and, therefore, as his compensatory behavior, the notion of a therapist's intervention optimum becomes possible, as will be discussed below.

$$x_1, x_2, \dots x_i, x_{i+1}, x_{i+2}, \dots x_j, \dots x_n, \dots \qquad (1)$$

(1) Sequence of compensatory behaviors of system X: x_i, etc. compensate for perturbations in X, triggered by environmental events and by previous compensations x_{i-1}.

Each time the system compensates perturbations, new perturbations are produced, in their turn in need of compensation (1). If the deviations to be compensated are decreasing in size, the system is to be considered as constructing adequate, or viable, descriptions of its environment. The occurring processes then may be called 'health', but also 'spontaneous recovery'. In this way, self-healing processes in an organism are to be seen as signs of health. On the other hand, if theorganism's compensatory activities result in amplification or perseveration of deviations, the system's compensations are to be considered as inadequately describing its environment. The organism can no longer be seen as self-healing. Thus a sequence may arise that cosists of compensations for the effects of previous compensations, and that finally leads to total desintegration, or to a loop of self-compensating compensations (which are self-describing descriptions) (2).

$$x_1, \ldots x_n, \ldots x_p, x_q, x_r, x_p, x_q, x_r, x_p, \ldots \qquad (2)$$

(2) Sequence of dysfunctional compensatory behaviors of system X: after x_{p-1} system X is trapped in a loop of compensations triggered by previous compensations.

Since closed systems are by definition not open to instruction or input manipulations, interactions between two closed systems have been described in terms of a process dubbed 'structural coupling' (Maturana, 1978). Here each of both systems (organisms) develops a domain of behaviors, which are compensations for perturbations triggered by the other system's behavior. Interactions between systems consist of perturbing events and compensating actions, not of mutual informational inputs and outputs (though an external observer may prefer to conceptualize them this way).

$$\begin{matrix} a_1, a_2, \ldots a_i \\ b_1, b_2, \ldots b_i \end{matrix} \qquad (3)$$

(3) Sequence of compensatory behaviors of A and B when interacting: a_1 perturbs B, B compensates with b_2, etc. Also b_1 perturbs A, A compensates with a_2, etc. (a_2 and b_2 also compensate for other perturbations, external to the interaction).

Now speaking in terms of structural coupling between closed, perturbation compensating systems, can one conceptualize one system A's behavior directing another system B's behavior instead of merely triggering compensatory behaviors in it? I propose one can, namely in respect of the quality of their structural coupling. A may behave in a way that leads to minimal perturbation in B. The A's behavior becomes complementary to B's compensations of some perturbing evens (other than the minimal perturbations due to A's behavior itself) in its (B's) environment, and the effects of A's behavior become to B indistinguishable from its own compensations. To an external observer they appear as functionally equivalent to B's compensations. Now A's behavior may be called as functionally equivalent to B's compensations. Now A's behavior may be called 'joining with B', or A can be said to develop 'empathy' to B. As a result of A's joining, B will start to depend partially on A for compensating of perturbations (Goudsmit, 1984).

Since we conceptualized illness according to the closed system model, as an inadequacy of compensating its environmental perturbations, which results in a chain of self-compensating compensations, treatment has to be intended at a replacement of his chain by more adequate compensative ways. While system A compensates some of system B's perturbations, A effectively overtakes part of B's compensatory chain. Suppose B's sequence b_{i+1} to b_n would occur if A and B stop interacting after i^{th} step. Now A's behavior may be called functionally equivalent or 'joining with B' (indicated with apostrophes as in a'_n), if A's behavior sequence a'_{i+1} to a'_n triggers in B the same sequence b_{i+1} to b_n.

$$\begin{array}{l} a_1, a_2, \ldots a_i \\ b_1, b_2, \ldots b_i, b_{i+1}, b_{i+2}, \ldots b_n, \ldots \end{array} \quad (4a)$$

$$\begin{array}{l} a_1, a_2, \ldots a_i, a'_{i+1}, a'_{i+2}, \ldots a'_j, \ldots a'_n, \ldots \\ b_1, b_2, \ldots b_i, b_{i+1}, b_{i+2}, \ldots b_j, \ldots b_n, \ldots \end{array} \quad (4b)$$

(4) Behavior sequences of A and B. A's behavior sequence a'_{i+1} to a'_n (4b) triggers in B the same sequence b_{i+2} to b_n as would occur without B's interactions with A (4a).

Now that A effecitvely overtakes b_{i+1} to b_n, this has two implications. First, A partially extinguishes B's generating the sequence b_{i+1} to b_n (the effects of which are equivalent to the effects of A's behaviors). That is, A's performance keeps intact B's organization, including its inadequate ways of compensating perturbations. B's gain, however, consists of the relief A's performance implies. Thus B, treated by A as a closed system, is facilitated to find new ways of perturbation compensating behaviors b_i'' to b_n'', that are different from those that led to current, inadequate compensations. this is of interest when b_{i+1} to b_n contains loops like in (2). Second, B's sequence of compensating behaviors does not (or only minimally) change as a result of A's behaviors, though A helps triggering them.

12.6. SWITCHING BETWEEN OPEN AND CLOSED MODELS: THE INTERVENTION OPTIMUM FOR TREATMENT

What woud happen if A were to alternate his functionally equivalent (joining) behaviors with behaviors that are not functionally equivalent to B's compensations? Then B's behavior would again consist of compensations for A's behaviors, as in the case in (3). Suppose in (5) A switches at time j to a non-joining behavior a_j. B generates b_{j+1} leading to b_{j+2} etc. These may be behaviors that would have otherwise (without joining episode by A) been much more difficult to trigger by A. Without joining episode, B's behaviors b_{j+1} and further were evokable without dealing with B's compensations for earlier behaviors of A.

$$\begin{array}{l} \ldots a_i,\ a'_{i+1},\ a'_{i+2},\ \ldots a'_j \\ \ldots b_i,\ b_{i+1},\ b_{i+2},\ \ldots b_j,\ b_{j+1},\ b_{j+2},\ \ldots b_n,\ \ldots \end{array} \qquad (5)$$

Just like the external observer, it is possible for A to construct a model of B both as a closed system and as an open system. Treated as a closed system, the quality of the structural coupling can be influenced by A in generating more or less functionally equivalent behaviors. Treated as an open system, A may devise behaviors a_q in order to trigger certain behaviors in B, thus constructing I/O relationships between his own and B's behaviors. Now whenever B's behavior appears to be in a direction not desired by A, the latter may switch back to generating functionally equivalent behaviors, before deviating from them again.

Then both for A and for an external observer, A's generation of behaviors that are functionally equivalent to B's behaviors, amounts to A's treatment of B as a closed system, whereas A's generation of other behavior amounts to treating B as an open system. Thus A's alternations between functional equivalents and other behaviors amount to switches between two models of B. When A intends to change B's way of functioning, i.e., B's way of compensating perturbations, A's alternations may be composed into an optimal mixture, consisting of interventions that are based on both models.

12.7. THE USE OF MODEL SWITCHES

One may wonder which of both system models, the open or the closed, is most appropriate to use as metaperspective from which one is to look at the act itself of switching between the models. It seems, as if both can be used. Switching between system models, as an observer may do, is to be classified as constructive behavior, i.e. as switching between the different descriptions of the environment. Here the closed system model seems the appropriate metaperspective to behold the observer's actions. On the other hand, switching between models may offer a useful mixture of joining and non-joining behaviors that may be used as an input to the observed system and steer it in a desired direction. Now, switching between system models can be considered as a manipulate procedure, compatible to other hard techniques. Here, the open system model seems to offer an appropriate metaperspective. And switching between metaperspectives is like switching between system models!

Formulated as in the above paragraphs, switching between open and closed system models, is to be seen as valuable tool for chaning a living system's inadequate compensations, i.e. its illness. As a switching technique, it is comparable and compatible to other instruments and procedures the medical technician disposes of. Switching between models may become part of the clinician's repertoire of dealing with patients, now treating them according to the open model, than to be the closed model. Paradoxically, however, if the therapist is to be use this technique, he is first to place his familiar open system model between brackets, and to look at the patient in a way that does not seem to contribute to the solution of the problems he is facing. Switching between models cannot be used as an explicit technique, as long as the therapist does not consider his view of his patient's dysfunctioning as his own construct, exchangeable to more viable others, if opportune. This makes accounting for the use of soft techniques so hard when only using the open system model.

Now this discussion of switching between models allows us to classify a wide variety of professional activities as a mixture of both models. Both anaesthesia by a surgial team, and a dentist's kindly asking his patient to open his mouth, are examples of the use of soft, 'joining', techniques during medico-technical procedures. In both cases there is an increase of the accessibility of the treated and the effectiveness of the interventions. Likewise, in psychotherapy dealing with the patient's resistance often receives priority to the contents of the problems mentioned (Greenson, 1967).

As to physiotherapy, the therapist for instance may find it useful not to attack the patient's painful spot immediately at the strength, necessary for treatment, but to prepare such an intervention by first having the patient accomodate to his touchings of the spot's environment. In fact this means, the therapist is also accomodating his procedure to the patient. The smaller the defensive tension the patient shows, the more effective will be his manipulations with e.g. the patient's elbow joint. If he starts too quickly to manipulate the elbow, the patient may be too intolerant of the pain, his fear of pain and ensuing defensive tension too high (in its turn leading to more pain and tension). When this happens, the physiotherapist may switch back to joining in order to minimize the triggering of perturbations in the patient. It is this use of soft techniques that usually happens without much ado and without a felt needed for explanation.

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CHAPTER 13

THE GHOST OF THE MACHINE: QUALITY OF PSYCHOTHERAPY

Karel A. Soudijn and Gerard de Zeeuw

Many people are trying to define and make transparant what kind of quality psychotherapy has, and whether it has such quality in specific situations (Garfield & Bergin, 1978). There is a remarkable element in this latter type of involvement: results are evasive. Psychotherapeutic methods sometimes seem to work, but effects are seldomly experimentally repeateable (cf. Cullington, Butler, Hibbert & Gelder, 1984; Wojciechowski, 1984). And quite frequently - in fact too frequently for any type of rationale - positive results seem to appear, when they do, not on the target criteria, but an unexpected variables.

13.1. KNOWLEDGE SINK

To increase the chance of any psychotherapeutic method being positively evaluated, in a stable sense and on some fixed set of criteria, it must be vaguely worded. Research on the quality of psychotherapy seems to be a knowledge sink: anything that appears useful at any time will disappear the next time around. A striking example is given by Truax and Carkhuff (1967). They showed that three characteristics of a therapist were necessary and sufficient for 'good' psychotherapy: accurate empathy, nonposessive warmth, and genuineness. Translated into prescriptions, new therapists should behave in such a way that they score high on these three factors. But a psychotherapist who focusses too much on these characteristics becomes highly vulnerable: such a therapist is unable to handle certain kinds of behavior of clients. In order not to get entangled into unwanted processes, psychotherapists have to know how and when to deviate from the research findings of Truax and Carkhuff: sometimes it seems necessary to show no empathy at all, to be cold, and to behave unreal.

One can envisage many different explanations for the psychotherapeutic knowledge sink. One explanation is that research into psychotherapy will never, on a more specific level, reveal consistency, and hence no recognizable quality as it seemingly will in ever in rituals, like those of the church. Another explanation is that such quality and consistency can only be revealed by research when the latter is done differently, compared to what happened hitherto.

In this chapter, we will assume the second explanation. From this, two main questions arise:

(1) what aspect of research is 'quality' of doing away with results;
(2) what can be used to replace this aspect, with remedial effects?

To answer these questions, first various ideas will be explored that deal with the question of what quality in psychotherapy may be.

Usually any psychotherapeutic method that breaks away from what previously was done, will be positively evaluated, on all obvious criteria. As soon as a psychotherapeutic method is taken as a standard, effects of its use become negative on those criteria. For instance, in its revolutionary phase psychoanalysis scored high on criteria of quality. When the practice of psychoanalysis became an established one, the new and at that time revolutionary Rogerian client-centered psychoterapy had seemingly more quality: practitioners had more success with it; Rogerian therapy could be applied to more and different groups of clients, in less time, with less training for the professional, and with lower costs. Later on, in the 1950s, the at that time new behavior therapy claimed a consistently higher rate of success than all forms of 'verbal therapies'. Behavior therapist claimed a success rate of 80 or 90 percent (independent of criteria), against a much lower rate of 40 to 50 percent of the more established verbal therapies. Over the years, the claims of behavior therapists lowered steadily (cf. Wolpe, 1973). As soon as behavior therapy itself became an established practice, it appeared as good or as bad as verbal therapies like psychoanalysis and Rogerian therapy - the effects seem to be, on the average, slightly better then the effects of a waiting list (Sloane, Staples, Cristol,Yorkston & Whipple, 1975). More successful, nowadays, is a break-away form of therapy (Shapiro & Shapiro, 1982). But also in this case, it can be expected that its higher success rate will be wiped out as time goes by.

It should not surprise us that there are literally thousands of identifiable psychotherapeutic methods, each with its own enthousiastic, but recent adherents: breaking away from the orthodox view seems to be a road to quality enchancement. Good therapy must be a mild form of anarchy. Even so, a criterion like 'breaking away' (from what?) is not easy to handle, in any obvious way.

Second, an important constraint on psychotherapeutic improvement is identified in the ideology of research - the assumption that a psychotherapeutic activity can exist as an 'object', that can be studied in terms of input-output equations. Research on the effectiveness of psychotherapy is often carried out with the idea of establishing in absolute terms 'the' effects of a treatment. Earlier, we cited the claims of revolutionary behavior therapists that success rate can be transformed into a percentage. Different reserachers use different units of measurement, however. In recent meta-analyses those different units are pooled into an overall score: Smith, Glass and Miller (1980), for instance, calculated in this way the average effect of the average psychotherapeutic treatment. According to their meta-analytic calculations, the quality of psychotherapy is equal to .85 standard units 'effect size'. Here, it is not important what precisely the meaning is of this number; important is the supposition behind such a calculation: the quality of psychotherapy seems to be something that can be measured in standard terms, independent of the reviewer, and the practice of psychotherapy is something like an object to be measured.

Not every researcher is interested in establishing 'the' effect of psychotherapy, or of a certain branch of it. There also is research on 'what really matters' in psychotherapy, or 'what works'. Jerome Frank and his co-workers, for instance, have been involved in a 25-year research project at Johns Hopkins University to lay hands on what they call the 'effective ingredients of successful psychotherapy' (Frank, Hoehn-Saric, Liberman & Stone, 1978). Also in this approach psychotherapy is supposed to have object-like characteristics: each ingredient of its own will have - to a certain degree-quality, independent of its users. A 'good' treatment is the 'right' combination of all the elements that obtain a certain 'goodness'-

score: therapy is like baking a cake, combining prescribed quantities of the right ingredients (this approach can also be seen in the earlier example of Truax and Carkhuff).

Reading the reports of Frank and his co-workers, it is surprising to find out how the effective ingredients are formulated. In his summary of 25 years of research, Frank concludes that 'the best substantiated findings are the following':

(1) The major determinants lie in the patient. Qualities that enable a person to utilize any form of interpersonal help also make him a good prospect for any form of psychotherapy.

(2) Next most important are properties of the therapist such as therapeutic intuitiveness, persuasiveness, enthousiasm, and flexibility.

(3) Also relevant are the nature and modifiability of the stresses in the patient's life situation.

(And as a fourth finding, Frank ads that no therapeutic school appears to be clearly superior to any other school.)

In Frank's formulation of effective ingredients, characteristics of patients and therepists are formulated in terms of preconditions of an interaction; nowhere is specified how precisely such an interaction has to proceed: the client must be able to establish a relationship; the professional must be able to react in a flexible way, etc. Where is Frank's formulation, one can ask, are the cake-like ingredients? A 'warm' therapist in the Truax and Carkhuff sense can perhaps raise the emotional temperature of the client - but what is the effect of a therapist who shows flexibility? And what happens when a flexible therapist meets someone who is able to establish rewarding relationships?

In standard forms of research one proceeds as if psychotherapy can be compared to a given set of procedures, applied to a given set of clients with rather strictly defined problems. In trying to establish the effectiveness of (a certain kind of) psychotherapy, researchers want to specify the 'goodness of fit' between a set of procedures and a set of personal problems. The implicit assumption is that problems suitable to psychotherapeutic treatment, can be formulated in a canonical form: all information that may help to solve it, or is allowed to help, is included in the formulation of the problem (De Zeeuw, 1985). Frank's summary of ingredients, however, shows quite a different picture of psychotherapy: a going together or two (or more) people who establish a relationshp in which their interactions do not proceed in a completely prescribed form: in their interactions, the form of the problem may change. Or, stated otherwise, even if client and therapist agree at a certain moment 'what the problem is', this problem formulation does not contain all the information about the way how client and therapist should proceed in order to 'solve' it.

A therapist with a high deree of flexibility (a 'good' characteristic in Frank's sense) is not a preprogrammed machine to start an interaction with. A relationship, in the true sense of the word, will not proceed in a completely predictable way: therapists and clients will 'talk back' to each other, and - in that way - change the conditions under which a psychotherapeutic treatment proceeds. One step further: a client who is capable of interaction (also a 'good' characteristic in Frank's sense), will not completely be controlled by the therapist: such a client will also interact (and change) between sessions, outside the consulting room. In many of these senses, a client is more than an object with fixed properties during treatment.

These interactional properties can explain, in a way, why the quality of psychotherapy cannot be established in an absolute sense. Therapy will not show a fixed degree of goodness over time, because those who are involved may constantly change their ideas about what is important, or what should be called the 'real' problem to be solved, or what is possible or not, etc.

This non-canonical form of psychotherapy can be illustrated with reference to Carl Gustav Jung, the dissident crown-prince of Sigmund Freud. There is an interesting difference in the way Jung and Freud made use of dream analysis in the treatment of their patients (cf. Jung, 1985). Freud, according to Jung, was involved in 'why-questions': why occurred this dream to this patient? Here, the dream materials and the free associations of the patient are given, and the psychoanalyst has specific rules for interpretation; Freud tried to find the meaning of the dream in the sense of finding its origin. Jung, according to his own view, was not involved in why-questions, but in 'what-for' questions. His interpretations focussed on possible meanings of the dream for the future life of his patient. Jung tried to find out what 'extra' information a dream could provide: possibilities, or dangers, for future developments in the life of the patient. Here, the dream does not have one final solution; in interpreting, the therapist is amplifying the perspectives of the patient. A dream interpretation is of 'good' quality, one can say, if the patient is able to use that interpretation in giving different meaning to his own life. Criteria for judging the quality of that meaning however, are not fixed.

In this perspective, psychotheraphy is not so much an 'object' with given properties, but more like a social contract (cf. Hubben, 1984). The contract specifies a temporary restriction in the way people involved will behave. Also, the contract specifies which forms of interaction are suitable. According to Jung, a patient involved in Jungian therapy will soon produce 'jungian' dreams, while a patient involved in Freudian therapy will produce more 'Freudian' dreams. And, one could add, a patient involved in orthodox behavior therapy will produce no dreams at all. The contrast itself offers a framework for developing local criteria for judging quality, but these in itself do now show when the contract should be changed, or abandonned; local criteria should be amplified by more global criteria if one wants to make judgements about the contract.

It looks as if standard research in psychotherapy has been predominantly focussed on these latter, more global criteria. Many researchers are trying to judge under what conditions a certain kind of contract (i.e., the psychoanalytical, Rogerian, or behavorial contract) is 'good', or 'suitable'. Unfortunately, there is a complication here. If we define psychotherapy in terms of contracts, it must be concluded that most of these are self-referential: by contract, participants are allowed to change the contract in the process of psychotherapy. Research that tries to specify under what conditions what kinds of therapeutic contracts will have what kind of quality, will be necessity get stuck in vagueness.

Focussing an interaction, psychotherapy is a reciprocal process. An intervention is not good because the patient is in a certain condition; an intervention now is a procedure meant to give patients certain possibilities of developing their behavior, their way of life, their self-image, or their criteria. The quality of an intervention is not determined by the conditions under which it is implied, but more in terms of next events. Which events become possible by this intervention? What are the possible (negative and positive) side-effects? In what way future interactions will be enchanced, or obstructed by a certain intervention? Questions like these become relevant if one takes the interactional aspect of psychotherapy serious (Soudijn, 1982).

In this view, research on psychotherapy will not tell us what is 'good' to do (in an absolute sense); the question now is, how information about the behavior of people can be translated into suggestions for developing interaction, in for overcoming certain obstacles in interaction. A psychotherapeutic intervention can be seen as having meaning, or quality, in a certain context, but the intervention itself will also change the context, in which therapist and client are interacting. This process of changing the context has largely been neglected by research into psychotherapy.

Let us conclude with a story that shows how absurd behavior can be if one does loose sight on the flexibility of context. Levi (1984), an italian chemist, tells a story about the boiling of linseed oil. In a German prescription book, published about 1942, Levi found the advice to introduce into the oil, toward the end of the boiling, two slices of onion. Levi could not understand why one had to do this: the prescription was given without any comment on the purpose of the additive. Some time later, Levi found an old partitioner who could reveal to him the origin of the prescription. When thermometers had not yet come into use, he told Levi, 'one judged the temperature of the batch by observing the smoke, or spitting into it, or, ore efficiently, immersing a slice of onion in the oil on the point of a skewer; when the onion began to fry, the boiling was finished' (Levi, 1984; p. 148). levi continues with the remark: 'Evidently, with the passing of the years, what had been a crude measuring operation has lost its significance and was transformed into a mysterious and magical practice'.

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CHAPTER 14

PROBLEMS OF DISAPPEARING KNOWLEDGE IN THE TEACHING MANAGEMENT

W.H. Weekes

Disappearing knowledge in the teaching of management covers two problem areas. In the first problem area, basic fundamental and innovative ideas seem to have been dissipated when the innovator's model is "corrected" to correspond more with the interpreter's view of the world than the innovator's. This seems to have happened to the work of the innovators of the Scientific Management movement and to the work of the Human Relations school of management. It also seems to have happened to Keyne's innovations in economics, to the application of compound interest formulae in financial decision making, and to the direction taken by accounting theoretist at the expense of management accounting. This first class of problem seems to have arisen out of the second class of problem, which is concerned with the practical organization of fundamental inquiry. Full time management educators with virtually no senior management experience tend to revert to the reductionist rigor of the alleged scientific method supported by mathematics, rather than to intuitive reasoning based on historical observation and hand on experience. These two problem areas, combined have had profound effects upon the economies of the Western world.

14.1. INTRODUCTION

This paper will take the view that the rapid disappearance of knowledge in the teaching of management in Western economies has resulted in an ageing industrial infrastruture and a decline in relative economic efficiency. For instance, we shall discuss later the fundamental message in Keynesian economics: that an appropriately low rate of interest is the critical variable in maintaining a rowth rate of new investment sufficient to maintain full employment. This concept seems to have disappeared from economic, if it even ever was there. The situation in most of the Western economies is now that the interst rate is so high that business are not prepared to borrow for long terms of twenty or thirty years.

> "Short term borrowing has become much more important, as have bounds or loans maturing in less than ten years - which are playing the role of 'long-term' debt.
>
> These shifts in financing have inevitably increased the importance of current cash flow, with interest costs growing as a cost of doing business. Fewer and fewer businesses can finance capital investment needs from current retained earnings. To maintain a stock price in excess of the corporate assets (that is, book value) and to maintain access to the most favorable vehicle for borrowing, current earnings assume predominant importance. Rapid payback of investment and the possibility of near-term rather than long-term growth become increasingly important in decision making. The largest firms thereby gradually lose their international competitiveness for lack of capital investment, while entrepreneurial small businesses find scant opportunity for financing growth at interst costs that allow them to expand to participate in international markets" (Hubbard, 1984).

This decline in international competitiveness which Hubbard says in a result of high interest rates, has also been attributed to the preponderance of business school trained lawyers and accountants in top management positions who prize analytical detachment and mathematical elegance over the insight and intuition that comes from hands on experience (Hayes & Abernathy, 1980). Moreover, Hayes & Garvin (1982) specifically blame the mathematical formulae used in normative finance theory for biassing capital expenditure decisions in favor of projects with high returns in the short term. Unfortunately, projects that are necessary for long term survival, such as the development of new and existing markets, new products and new process technology, usually require high investment in the short term and don't usually make profits until a considerable time into the future. Such projects would appear unfavorable when mathematically evaluated. If they are rejected, firms would not spend the money on long term development that is required to survive in the long term in the international market place. This is what has happened. Western economies are losing out to better managed economies in the East with lower interest rates, and managerial cultures which recognize the importance of long term investment in product and process technology (Weekes, 1985 a).

A further manifestation of the disappearance of managerial knowledge in the West has been the way that organizations have been structured:

> "Business performance in the United States has deteriorated badly, at least compared to that of Japan, and sometimes to other countries - and in many cases absolutely, in terms of productivity and quality standards. We no longer make the best or most reliable products and we seldom make them for less, especially in internationally competitive industries" (Peters & Waterman, 1982, p. 41).

The problem, say Peters & Waterman, is that not only do the business schools over emphasize quantitative methods, but over emphasis on the rational, analytic approach tends to stress cost reduction at the expense of revenue enhancement, promotes a cautious, inflexible and heartless philosphy and ignores the importance of values and people. The rational approach is said to have originated in the Scientific Management movement. My view is that the original thrust of the Scientific Management movement was synthesis based rather than rational and analytical; but that this disappeared under the weight of the reductionist paradigms already in place in the minds of its later interpreters.

Moreover, the questions of values, informality, flexibility and the importance of people in organizations were all highlighted by writers such as Follett (1941), Mayo (1948) and Barnard (1938). In some way, however, the messages communicated by these scholars seem to have disappeared in the ever increasing crowd of quantiatively inspired analytical and rational techniques taught in Western business schools. Strangely, there are no business schools in Japan (although there are departments in some Universities) and yet the Japanese organizational culture seems to reflect much of what these three scholars espoused forty years ago.

Finally, there seem to have been an obsession, in recent years, with theories of financial accounting in business schools. Professional bodies have attempted to establish financial accounting standards, which in turn have been inflicted upon reluctant clients by practising public accountants and auditors. So much time has been incurred in resolving conflicts with clients over the application of these "standards" that the fee revenue of public accountants has mushroomed. But the whole question of the feasibility of establishing overall accounting standards has been questioned by Anthony (1983) as we shall see later.

Meanwhile the discipline of management accounting and the design of information systems for specific industries seems to have linguished. The emphasis in information systems design has been shifted from a study of the physical activity flows within the organization and a matching of information with physical activities; to a pre-occupation with the technical specifications of various mismatching pieces of individually manufactured computer software and hardware, and how you can get them to somehow fit together. There have been few advances in the design of classifications of accounts, and the matching of budgetary procedures with standard costs, in any of the text books on management accounting for over twenty years. The proliferation of texts on the subject all seem to be copies of each other - some perhaps with chapters arranged in a difference sequence - but all basically the same. The technical aspects of the arrangement of the actual accounts in the ledgers - still a fundamental necessity in systems design - which was a current topic years ago, seems to have disappeared from the knowledge base. The result is that more and more out of data and inefficient systems are being computerized. Their antiquity is being preserved within the shrine of high computer technology.

The next sections of this paper will discuss some of the fundamental concepts of management education that have been referred to, and suggest that these concepts have been misinterpreted and have virtually disappeared. The final sections of the paper will consider how this has happened and what might be done about it.

14.2. SCIENTIFIC MANAGEMENT

F.W. Taylor (1911) is considered to be the father of the Scientific Management movement. Taylor articulated that there was 'one best way' to perform physical tasks. His method was to make detailed (scientific) analysis of the components of each task, and then to synthesize these components into an improved way of working which would combine less effort with higher output. Rather than make people work harder, Taylor's idea was to make them work smarter. He showed how tasks could be designed so that you received more money for doing less work, but produced more. Higher productivity meant lower costs. Lower costs meant that the worker could be paid more, as not all of the cost reductions were passed on to the consumer. However, the effect overall was to lower the production costs of all goods while at the same time raising wages. Thus the worker had cheaper goods with higher wages and was much better off.

Taylor also studied organization structure and recommended the concept of functional formanship. This applied the principles of specialization to supervision and ensured that the work was properly planned and prepared for the worker. These principles have in fact been adopted today but in a different guide. There are nowadays a number of specialists - methods engineers, production planners, cost accountants, purchasing officers, quality controllers and so forth each providing specialist services to ensure that maximum output is achieved consistent with a pre-determined standard of quality of with minimum effort.

In introducing his ideas, Taylor encountered resistance from mangement, unions and workers, who misinterpreted his concepts. He stressed the need for a 'mental revolution' in industry so that management, unions and workers would work together to achieve common aims in their common interest.

Although Taylor used a stop watch to analyse work, he did not develop the detailed 'time and motion' techniques that are in use today. The detailed study of motions was pioneered by Frank Gilbreth (1911) and his wife, Lillian. Gilbreth was probably the ultimate reductionist, breaking down motions into seventeen Therbligs - fundamental parts of motions that could not be broken down any further. Using these basic components, any task could be described and designed by a synthesis of the fundamental components of motion. But the idea was still to work smarter, not harder, and for more money.

Moreover, the Gilbreths were pioneers in industrial psychology and ergnomics - particularly in the design of the workplace, the importance of color and lighting in the environment, and in the minimization of fatigue (Lillian Gilbreth, 1914).

Yet the work of the Scientific Management movement has been denigrated in recent years. Factory work has been criticised for being boring, impersonal and dehumanising. Attempts have been made to correct this deficiency through the use of autonomous work groups but this has had only limited, if any, success in the West. On the other hand, the sytem seems to work better in Japan, where the workers possess different cultural values and seem to retain own identify even when performing repetitive work.

It would seem to me that it is not the ideas originated by the Scientific Management movement that are at fault, but rather the way in which they have been applied and interpreted by people who have converted the original ideas to their own use. I believe that both, Taylor and the Gilbreths, used both analysis and synthesis in designing jobs and organizations, but that many of their interpreters have concentrated on analysis at the expense of synthesis. The totality of the original concepts of the Scientific Management movement, in other words, disappeared.

14.3. ORGANIZATION

One of the major contributors to organization theory during the Scientific Management era was Harrington Emerson (1913). It would seem, however, that Emerson was a reductionist rather than a synthesist. The criticism of American organization structures that was made earlier is largely a reflection of Emerson's concept of line and staff, based on the organization of the German army, and is in direct contrast to Taylor's functinal formanship. This bureaucratic kind of organization structure was reinforced by Fayol's (1930) Principles of Administration, and Weber's (1958) praise of bureaucracy.

Moreover, these concepts could also have reflected some of the forces at work in the dehumanising applications of Scientific Management.

In this rush to reductionism in organization design, the work of Mary Parker Follett (1941) appears to have gone relatively unnoticed. Writing in the 1920's, her theme was that human relationships are at their best when differences are solved through conference and co-operation; that is, when the parties at interest evoke each other's latent ideas based upon the facts of the situation; come to see each other's viewpoints and understand each other better; and integrate these viewpoints and become united in their pursuit of a common goal (1941, p. 14). Her view was that functionally relating was the continuing process of self creating coherence - the intertwining and the interpenetrating of the best ideas of both the leader and the led in continuously creating new situations. In these conditions, the real leader will have the insight not only to meet the next situation, but to make the next situation. The organization that will allow men to create the next situation will be based on functional unity rather than arbitrary authority (1941, pp. 27-28).

It has always seemed to me that Follett's ideas laid the foundation for the subsequent work of Elton Mayo (1953) and Chester Barnard (1930). The Hawthorne experiments from 1927-1932 made Mayo famous, in that they were the medium through which is concept of the worker as a social man rather than a ratinal man motivated by personal economic needs, received recognition. Mayo spotlighted that a true concern for workers paid dividends, and that group processes and group rewards were important, as did Follett.

Barnard wrote from his practical experience as the chief executive of the New Jersey Bell Telephone Company. He recognized the importance of informal groups within organizations as distinct from the formal organization structure. It was therefore necessary to ensure that the goals and objectives of the organization and the aims and needs of the workforce were kept in balance. Moreover, management must utilize these informal groups, even if at times they may appear to be working at cross purposes to the overall organizational objectives.

It is interesting that the concepts of Follett, Mayo and Barnard seem to be similar to the characteristics of successful companies that are described by Peters & waterman, referred to earlier:

> "... the good news comes from treating people decently and asking them to shine, and from producing things that work. Scale efficiencies give way to small units with turned-on people. Precisely planned R & D efforts aimed at big bang products are replaced by armies of dedicated champions. A numbing focus on cost gives way to an enhancing focus on quality. Hierarchy and three piece suits give way to first names, shirtsleeves, hoopla and project based flexibility. Working according to fat rule books is replaced by everyone's contributing.
>
> Each management's job becomes more fun. Instead of brain games in the sterile ivory tower, it's shaping values through coaching and evangelism in the field - with the worker in support of the cherished product" (Peters & Waterman, 1982: XXV).

Earlier in this paper, we made the point that the industrial infrastructure of the Western world was becoming uncompetitive. Yet here are Peters & Waterman saying that there are a number of excellent companies in the West, who operate very similarly to the successful ones in the East. It would therefore seem that the successful companies are those where the fundamental systemic knowledge concepts of organization structure - such as those proposed by Follett, Mayo and Barnard - have not disappeared. On the other hand, it could be said that these companies are in a minority, and that is the reason for the declining competitiveness of our industrial infrastructure. We shall examine why this is so, later in this paper.

14.4. ECONOMICS

It is generally conceded that John Maynard Keynes was one of the greatest thinkers of the twentieth century. He developed a new schema of economics which many say was responsible for the stability and sustained properity with full employment, of the Western economies for almost thirty years after World War II. Yet recent publications (Hession, 1984; Mggridge, 1973; Skidelsky, 1983) would indicate that, although Keynesian concepts have made a tremendous impact on the world, his basic ideas have been grossly misinterpreted and have virtually disappeared from the economic discipline and from Keynesian economics.

My interpretation of Keynes' work is that the critical variable in the economic sytem is the rate of interest (Weekes, 1985 b). Yet Keynesian economics is today synonomous with deficit financing, and we have a situation in the Western world where deficit financing is combined with high interest rates. This is the antithesis of Keynes' thinking!

Keynes' idea was that the driving force in any economic system was the intelligence, determination and executive skill of the entrepreneur, who created employment by taking the risk on new employment opportunities. What was needed was an interest rate low enough to induce entrepreneurs to invest in new projects - where the D.C.F. was positive at ruling rates of interest. Provided that the interest rate was sufficient to generate the appropriate rate of new investment to maintain full employment, no other fiscal action by the government should be necessary. Such action would only be needed if the lowest interest rate did not generate the desired rate of growth in new investment.

Perhaps the only two major economies in the world to have needed Keynes are Japan and Switzerland, both of whom have low interest rates, relatively low inflation and full employment. Keynes' basic concepts seem to have disappeared into the black hole of traditional (monetarist) economics which has been influential in sustaining high interest rates in most Western economies, which also have high rates of unemployment.

Hession (1984, pp. 267-369) says tht it is now increasingly recognized that misconstruction of Keynes' General Theory of Employment, Interest and Money has occurred, mainly due to the highly mathematical approaches of the so-called Neo-Keynesians. As with innovations, the innovator's view of the world is at variance with that of his audience. The innovator's model is thus likely to be 'corrected' to correspond more with the interpreter's view of the world than the innovator's. Economist such as Hicks, Samuelson, Hanson, Klein and Modigliani have misconstructed Keynes' work in terms of static equilibrium rather than flow equilibrium and this has resulted in the so-called Keynesian revolution going off half cocked.

14.5. ACCOUNTING

Robert Anthony (1983) has come to the conclusion that all accounting should be management accounting and that the most significant accounting statement is the Profit & Loss Statement rather than the Balance Sheet.

Yet, as mentioned earlier, professional accounting societies have invested heavily in research, in the last decade, into the establishment of accounting standards, the outcome of such investment being reflected in increased fee revenues arising from the extra work generated in attempting to implement such standards.

Anthony, however, who has been a member of the Financial Accounting Standards Board in the U.S.A., felt that the problem of obtaining unanimity on accounting standards was so great that not much progress was being achieved. This, he said was because the members of the Board all seemed to have different implicit premises about the nature and behavior of accounting entities.

Anthony likens organizations to systems - a system being any set of elements that are related to one another. Information about the system is of two kinds - flow information and state information:

> "Flow information describes the inputs to the system, the work done with these inputs and the outputs of the system during a period of time. State information describes the status of the system as of a moment in time" (Anthony, 1983, pp. 25-26).

Hence Anthony concludes that the measurement of flow information is the more important activity and that state information is impossible unless the value of assets could be measured in a meaningful way. The value of a business entity is the present value of the stream of future earnings that it will generate. But there is no way that accountants can estimate the numbers in this earnings stream. Hence it is virtually impossible for accountants to accurately calculate the value of an economic entity.

This has been well known by practitioners of management. Yet is it not generally recognized by teachers of accountancy, nor by auditors, nor by the draughtsmen of Companies legislation, who place the onus on auditors to certify that the statements of accounts reflect a 'true and fair view of the state of affairs of the company' (or similar). This is impossible to quantify, for, according to Anthony, this derives partly from the skill of its management, its position in the market, the products that it is developing, and other nonquantitative considerations (1983, p. 40).

Apart from Anthony's view, however, the skills of the management accountant seem to have disappeared from the curricula of management schools. being swamped by the theories of traditional financial accounting, which Anthony now challenges. Management accounting was not developed by accountants, but by the engineers of the Scientific Management movement such as Taylor and Emerson, mentioned earlier. It should thus rightfully appear among the techniques of Production and Operations Management, but it does not. It is treated rather sparsely in the current texts, which make only fleeting reference to Operations Management and tend to concentrate on the very aspects of reductionist approaches to cost control that are criticised by Peters & Walterman. In fact the lack of consideration of such fundamentals as classifications of accounts and standard journal entries detracts from the usefulness of current texts. I have endeavoured to rectify this (Weekes, 1984) but it is too early, yet, to discover what effect this may have on the profession. Management accounting is inextricably linked to budgeting, which is linked to the production planning and work measurement functions of Operations Management. This basic fact of existence has disappeared from most texts on both Management Accounting and Operations Management.

14.6. FINANCE

Concepts of normative finance theory that are now promulgated in buisness schools are of fairly recent origin. Yet the basic theories were developed, and their limitations delineated, by such scholars as Fisher (1907, 1930) and Grant & Ireson (1930). But the basic limitations of the use of compound interst in what is now known as capital investment theory seem to have disappeared from most of the texts on normative finance theory.

As far back as 1907, Irving Fisher (1907, 1930) used compound interest formulae to select which project from a number of options would be most profitable at a particular rate of interest. However, he stressed that different projects could be more attractive than others at different rates of interest, depending on the timing of the cash flows. Later, writers on engineering economy studies, such as Grant (1930), described the method as a way of comparing the cash flows from competing capital projects on an equivalent basis.

The concept fell into the hands of finance theorists following the work of Joel Dean (1951, 1954) and Ezra Solomon (1955) but what disappeared from their reasoning was the original concept of equivalence and the problems of distortion associated with problems of different cash flows and different interest rates (Weekes, 1978).

The result has been confusion, although the controversy surrounding the topic has proved a fruitful subject for publication in academic finance journals. One of the few writers to highlight the still unresolved areas in capital investment theory has been Allen (1983, pp. 196, 219, 217) who asserts that there is still no general theory of investment for the corporation under coditions of uncertainty. The tax system and the existence of inflation serve to further complicate matters, while the investment and financing decisions, which are considered to be separate in theory, are in practice unlikely to be separable. Moreover, the use of compound interest formulae assumes that the internal reinvestment rate of the project's cash flows is equal to the discount rate employed, which is supposed to be the firm's marginal 'cost of capital'. But if there are any projects under consideration at the same point of time with different D.C.F. rates they will have to be assumed automatically to have different reinvestment rates. This is unrealistic, as the firm could have only one reinvestment rate at any point in time.

Once more it would seem that our over emphasis on mathematical techniques has not only swamped the practical realities of capital investment theory developed by earlier writers, but has resulted in a theory which can be falsified. Unfortunately, it is precisely this kind of reasoning that has been referred to earlier by Hayes & Abernathy and Haynes & Garvin, and which could have been a major reason for the decline in the industrial infrastructure and international competitiveness of the Western nations.

14.7. CAUSES

It would seem that the disappearance of fundamental knowledge about management has been caused by the two factors that are the theme of this conference. The first is that original innovative ideas become distored in the hands of their interpreters, who try to fit the new paradigm of the innovator into the rigid framework of the traditional paradigm that is held in the interpreters' heads.

This leads to the second factor, which is probably the cause of the first. This seems to be that with the growth in size of tertiary instutions since World War II, and the virtual accessibility of tertiary education to all, the whole education process has become bureaucratized. This process has tended to emphasise linear and reductionist thinking as this kind of approach is more easily taught in talk and chalk situations.

Up until at least the middle of the ninetheenth century, most of the great scientific discoveries were made by amateur scientists on a trial and error basis with practical experimentation. People like Boyle, Galvani, Volta, Lavoisier and Darwin were amateurs, that is, they were not working in Universities and were either wealthy enough to be self supporting or were supported by patronage. Darwin was supported by the Wedgewood pottery fortune on both his mother's and his wife's side of the family. Keynes never obtained a Ph.D. and was never a full professor. He made his reputation initially after resigning from the British Treasury during the Versailles conference and writing a best seller about it (1919). While Keynes was lecturing at Cambridge - virtually on a part time basis - after the First World War, A.C. Pigou was Professor of Economics.

Einstein was a patents office clerk when he developed, from the earlier work of Michelson-Morley on the speed of light, his revolutionary ideas on the equivalence of matter and energy and his famous equation $E = MC^2$. Moreover, Einstein was not a particularly outstanding student at the Zurich Polytechnic. His mathematics were not especially good. But he thought visually rather than varbally, and was dogged (Caler, 1982, p. 140).

These people were all working in the field of theoretical experiment rather than in the field of the alleged rational scientific method, which goes as follows (Canont, 1951):

1. Recognition of a problem and formulation of an objective.
2. Collection of 'relevant' information.
3. Formulation of a working hypothesis.
4. Deductions drawn from hypothesis.
5. Deductions tested by actual trial, and
6. Hypothesis either supported or rejected.

The idea is that from a system of variable, one variable is isolated and tested while the others are assumed to be constant. Mathematical techniques are employed to test the hypothesis concerning the behavior of the variable under study.

This approach is the essence of reductionism. It is narrow, linear and non systemic. Yet it seems to dominate the institutionalized tertiary education system in the Western world. But this approach was not the approach of the scholars that we have discussed in this paper, whose insights, based on observation, have helped to shape much present day thinkig.

Hession (1984, p. 367) particularly emphasises the degree of insight and intuition in Keynes' thinking, based on his hands on experience as a government employee in the finance industry. He tended to rely on observation and intuition, preferring to see the truth obscurely and imperfectly rather than to maintain error - even if reached with logic, clearness and consistency, but through an hypothesis that is inappropriate to the facts.

The other basic thinkers to whom we have referred earlier, also were not working within a University environment. Taylor was a mechanical engineer and worked as an employee in industry until starting his own consulting business. Frank Gilbreth was a bricklayer who established a construction business and then became an independent consultant with his wife. She eventually obtained a Ph.D. after giving birth to twelve children, but worked as a consultant until her husband's death, when she became a university professor. Emerson was a professional manager. Follett was a phychologist and social worker. Barnard was a telephone company executive who never completed an undergraudate degree. Mayo was University professor at Harvard but had a varied career after being born in South Australia and studying medicine at Edinburgh.

The point is that none of these basic contributions to management thinking were developed from within the confines to the alleged scientific method in bureaucratic tertiary institutions. Rather were they developed by intuition; and in the case of the industrial engineers by trial and error in practice in the field.

14.8. CONCLUSION

The foregoing discussion thus leads one to the view that one of the problems of disappearing knowledge in the teaching of management is the reliance upon the steps in the alleged scientific method, a culture long established in universities and which is rooted in reductionism. It is not so much that the inquiry to discipline oriented as that it is too narrow. The narrowness of the inquiry then tends to restrict it to a single discipline.

The problem is further expanded by the fact that there are very few academics in business schools that have had any hands on practical managerial experience at a senior level. This was commented upon by Simon (1967) who said that an academic who had previously had a successful business career was very rare. Copeland and Weston (1980, viii) made the same point about finance theories. Prior to the Second World War, many lecturing posts in business schools were occupied on a part time basis by skilled practitioners - such as Taylor, Follett, Keynes and barnard - working full time in industry and commerce. The increase in educational facilities after the Second World War resulted in a demand for full time academic staff whose total experience is academic. In fact, there could be a case to be argued that the start of the decline in the productivity of Western industrial nations coincided with the growth of business schools and the provision of equal full time tertiary education opportunities for all.

The problem is that academically trained lecturing staff are trained in analysis rather than synthesis. They tend to see an organization from a vantage point outside the organization whereas a manager sees the organization from a vantage point within it. The great thinkers referred to earlier were all synthesists. Even Taylor described how time study was a process of both analysis and synthesis. Synthesis requires the balancing of both long and short term goals and a balancing of the different functions within the organization so that the organization is maintained in a kind of antopoietic tension - the flow equilibrium of Bertalanffy's open system (Davidson, 1983).

The result of the analytical approach of the Western business schools has been a proliferation of 'by the numbers', rational, strategic and financial planners. This is in contrast to Japan, where practical insight is the key to planning and where analysis is not so much rejected out of hand but used more to stimulate the creative process, test ideas that emerge and work out their strategic implications (Ohmae, 1982, pp. 2-5).

What is needed in our management teaching is an input of synthesis, based on observation, and the intuition that comes from hands on experience. There should be a greater input of lectures from experienced practitioners. Research should be more practitioner oriented, rather than theoretical and hypothetical. Teaching should be conducted in a more experiental group mode where experience and insight can be tapped. More part time study rather than full time study should be encouraged so that theoretical concepts can be related to real life situations. More project work should be encouraged where students must solve problems and implement solutions in their own organizational settings, akin to Revans' (1982) concepts of Action Learning (Muster & Weekes, 1983). Problem solving methodologies should be developed through kinds of inquiry that are systemic and unrigorous, based on reflection-in-action about the typical ill-structured and unformed problems that are encountered in real life; compared with the well formed problems that are essential for the 'rigorous' inquiry of the allenged rational scientific method. We need a rebirth of interest in the ancient topics of craft, artistry and myth; a recognition of problematic situations out of which problems must be constructed in situations of complexity, uncertainty and uniqueness, and a recognition of problematic situations out of which problems must be constructed in situations of complexity, uncertainty and uniqueness, and a recognition that there is a kind of knowing that stems from experience rather than from prior intellectual operation. Rigorous analytical techniques may be replaced by an epistemology of practice that places technical problem solving within a broader context of reflective inquiry and shows how reflection-in-action may be rigorous in its own right (Schon, 1983).

I see that the development of such an epistemology should be one of the major objectives of the General Systems Research movement. Some progress has been made by such activities as Action Learning and by off campus management courses such as those conducted at Deakin University (Weekes, 1985 c). The problem of course, is to make knowledge which has disappeared, re-appear. Perhaps this can only happen by studying those cultures where it hasn't disappeared; such as, for management practice, Japan. The study of Japanese organizations is now a boom industry as once upon a time was the study of American industry, when the U.S. was the leader of the world.

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CHAPTER 15

KNOWLEDGE ONITORING IN INDIVIDUALS

Miro Valach

Knowledge appearance and disappearance is traced during its residency in an individual caused by its acquisition, usage and changes in its representation. Knowledge acquisition is the first filter a knowledge has to pass. Consciousness, awareness, and other functions of nervous system structure and restructure it in order to be able to look at it, comprehend it and sort it out so that it can be incorporated and reincorporated into the knowledge pool of the individual. When a knowledge is used it is subjected to transformations in representation that adds a dynamic dimension to its content. Cognitive forms of knowledge may be required to change from one symbolic representation to another. Non-symbolic representations are often attached as companions in form of sensations, and feelings. Transformations from one form of representation to another are necessary. Example of such forms is visual, accustic, symbolic, sensory, and sensational representations. There are also translations from one language to another language, natural and/or artificial, that need to be considered. Transformation of knowledge into skills is a dramatic example of a change in the knowledge form and its content. Manipulation of knowledge frequently adds or removes something from the content of the knowledge. There is a trade-off between how much is gained and how much is lost by a transformation. An important aspect of knowledge manipulation is the purpose of the manipulation which provides means for the assessment of gains.

15.1. ABOUT THE DEFINITION OF KNOWLEDGE

There is no single definition of knowledge. As in all such cases, a definition has to be accompanied by a point of view from which the definition is created or by a purpose the definition is to serve.

For example, a student has defined B as an inductively defined set and has proved P a property of elements of B. Suppose he needs later to decide whether or not an element x is a member of B. There are several ways he can do it. He may show that x can be reached by the induction and thus prove that x is element of B. He may also show that x does not have property P and therefore it is not an element of B.

What do we want to call the knowledge of the student in this example? He has the knowledge of logic. He has a new knowledge, set B he created. He has the knowledge of a property P of the universe of B. He has the knowledge of proving the property. He also has the knowledge of how to use the property to make a decision about x not being member of B.

There are two unseparable concepts of knowledge:

1. **Static concept of knowledge.**
   The static concept deals with the cognitive aspect of knowledge. In dealing with it we will use terms as static knolwedge, information content, structure and organization.

2. **Dynamic concept of knowledge.**
   The dynamic concept deals with the knowledge utilization that is closely related to some kind of process. In dealing with the dynamic aspect we will use terms as dynamic knowledge, skill, mental skill and mental process.

In the context of computer science the static knowledge may be compared to the data and dynamic knowledge to algorithmic processes. However, we wish to show that the data may only be one form a static knowledge in an individual, and an algorithm may only be one part of the dynamic knowledge of an individual.

An example of static knowledge of a student may be his knowledge of induction rules of the set B and the property P. He may recall it, think about it, but he does not change the content of it in any way. His dynamic knowledge is represented by his ability to prove the property P (to generate a process of creating the proof) and his ability to utilize P in his decision making process. Part of the process is the application of static knowledge.

Static and dynamic aspects of knowledge can be theoretically handled as separate aspects. Practically, they are difficult to separate. They act as two faces of the same fenomena. A static knowledge is not usable unless it can be accesses. To make it accessable, one needs a process (algorithm) that represents the dynamic aspect of the utilization, even as simple as location of the particular element in the organization. Storage and retrieval not necessarily need to change the content, i.e. something be added or lost.

There is a point of view from which the total static and dynamic knowledge in a system is referred to as intelligence of the system. In consequence of this point of view intelligence of a system has two sides, static and dynamic, represented by the system's static and dynamic knowledge. In some systems the static knowledge may be predominant, in other systems the main portion of the intelligence may rest in its dynamics.

15.2. ACQUISTION OF KNOWLEDGE

Knowledge enters a system through the sensors. The representation of the knowledge is therefore first formed by the sensory system.

Next, the knowledge is met with the resident knowledge of the system that creates a context for the interpretation of the incoming knowlege. Without this interpretation the external knowledge cannot be accepted. The individual would not know what to do with it. The process of relating new knowledge with the resident knowledge may be an objective one, but it also can be, and in most cases is, a subjective process.

Subjectivity enters in form of the interpretation of the incoming knowledge in terms familiar to the system. There is a confidence measure an individual uses for evaluation and making (flagging) the knowledge. Confidence level reanges from being completely confident on one hand to complete distrust on the other hand. The confidence level is a subjective one. As a subjective measure it cannot be transmitted with the knowledge, because the evaluation is the receiver's reaction to it under his control. In living individuals knowledge may have an emotional component that may influence (voluntarily or involuntarily) the level of confidence.

15.3. BALLAST

When a symbolic (verbal) knowledge is transformed into a grpahic form, a two dimensional plane is added to the knowledge representation and elements of knowledge are mapped on the plane. The plane is a ballast added to the previous form of the knowledge. Ballast plays an important organizing role in the representation of knowledge. Let us assume we wish to find a minimal cut of a graph consisting of several points and edges, the connectivity of which is represented by a table. Tabular form does not contain any organizing feature that would be much helpful to our senses to manipulate relations represented in the table. Therefore, we choose to represent the graph by using a plane. A plane contains two sets of ordered points called coordinates. We can shuffle the points around until they are conveniently placed from left to right and from top to bottom. The ordering feature of the plane is used to emphasize relationships among the points of the graph. Gradual improvements of the planary representation by rearranging the points helps to solve our problem to find a minimal cut. Plannary representation enables us to use terms left, right, up and down as an additional tool for solving our problem.

Features of the ballast, like the notion of left and right points, are instrumental in solving problems. The notion of possible two-dimensional ordering was not possible in the tabular representation. Left, right, up and down arrangement of points provided new means for finding the solution. Otherwise, the plane was an unnecessary ballast. Yet the ballast become a catalyser of the solution process.

15.4. USE OF THE BALLAST FOR INCREASING EFFICIENCY

The use of a ballast is not restricted to graphics. Behind almost any transformation of knowledge representation there is a change in the knowledge organizing ballast. A motivation for transformation of knowledge from one form of representation to another is in changing, adding, or removing a convenient or inconvenient ballast. A transformation may offer new way of facilitating a particular decision making process. Thus, an improved efficiency may result by relying on different ballast found in the new representation.

Organization of a static knowledge can be regarded as a part of the ballast. Organization is an intended ballast without which usage of the knowledge is not possible. However, there are also cases in which an unintended ballast contributes to the knowledge organization. Such ballast can be smell, emotional interference, intensity of light, or other perceptions or associations present at the time of knowledge manipulation such as, for example, storing, restructuring, reformating, and re-representing. While such instances are unintentional for the purpose of organization, living mind may take note of it and may convert it into an organizing element. For example, a sudden intensive light may be remembered in connection with an episode for a long time and later used as a retrieval guide. A discomfort during a performance may be another example of an unintentional ballast that may later used as a retrieval clue for the whole event.

15.5. BALLAST AND SKILL LEARNING

There is one transformation of knowledge into a process that should not escape our attention. It is the skill learning. There is a special role ballast plays in the triggering of the next segment in the chain of subsequent mini-events of the performance. The ballast is picked up during the learning process, in form of clues for the triggering the next segment of the performance. The clues are subconsciously selected from the external or internal environment of the learner. They become part of the learned skill and they have to be present when the skill is performed, no matter whether or not they are logically related to the performance.

There is no specific rule for picking up a clue in learning situations. Performers know well that they need to concentrate on their performance. The concentration means they will shut off all undesirable interferences. By concentrating they reconstruct exact environment they were in during their practizing. The reconstruction is important because, besides others, it creates conditions for bringing up all ballast that was present during the practice and served as triggering clues for the smooth continuation.

For example, at a certain point a pianist associates a segment of a piece he is playing with a landscape around his house. The next segment he is going to play may become conditioned by the presence of this image. If, during the performance of the same piece the immage of the landscape is not present he might loose the continuation of his performance.

What distinguishes one individual from another is the sensitivity to the ballast in recalling stored information, ballast that was present at the time of storing. The secret of concentrating is, among others, the ability to recreate crucial circumstances under which an information has been both organized for the purpose of storing and stored. This ability creates the individuality of an individual in handling the knowledge at the knowledge entrance point and at the time of its retrieval. The ballast that accompanies the storing and retrieving mechanism is not under the conscious control of an individual. It means that an individual cannot consiously determine which ballast should or should not be present and which clues should or should not be used when a storing or retrieving takes place.

15.6. PURPOSE OF KNOWLEDGE

There are three purposes of interest for which a knowledge may be acquired:

1. Storing of the knowledge for the purpose of using it later. This leads to two problems, knowledge representation and knowledge organization for the purpose of storing it. Both representation and organization cannot be separated from the interpretation of signals that carry the incoming knowledge.

An individual has a faculty to understand what is coming in. More objectively, there are degrees of understanding, levels of understanding, ways of understanding and points of view of understanding. Briefly, to understand something is an individual affair. From an individual point of view, understanding is reflected in the way how the individual treats the information from the beginning and later during its use. Classroom experience of teachers shows very clearly that there is a considerable difference between the intended meaning of an explanation and the individual acceptance of that explanation by the students.

Ideally, the same knowledge is the same for all. From an individual point of view, knowledge is an individual possession with individual usage, individual understanding, individual relating to the rest of the individual's knowledge and individual internal representation.

2. Knowledge reconstruction in order to improve its organization representation and utilization.

Our consciousness creates a feeling that the things around and inside us are stable. It is the role of the consciousness and it plays this role very well. However, the stability is in fact only in the interpretation of what in reality is an on going change. Without going into more details, we can say that the mechanism of consciousness focusses on what is stable and filters out all that would erode the smoothness of our perceptions. Along the same line, we perceive our own knowledge as a stable body, which in reality it is not at all stable, not in its content, nor in its form.

There is an on going process in an individual called thinking on re-thinking which leads to a reconstruction of a knowledge. Though we have the subjective feeling of stability, knowledge in an individual should be regarded as a dynamic concept that changes not only its content but also its representation. Just by reusing certain knowledge we make it more readily accessable, more so at one time and less so at another time.

Functions that cannot be clearly recalled are reconstructed from the rest of the knowledge. Sometimes, an individual is aware of this reconstruction, but many times not. Thus the content of the knowledge changes, sometimes slightly, sometimes more substantially.

Besides subconscious changes in knowledge frequently there is a conscious transformation. When, for example, reading ab ook, one imagines the content. This is a transformation of words and symbols into a visual domain. The transformation is a drastic one because it adds substantial portion of information that comes from the imagination rather than from the exact content. Imagination allows different types of representation of the same knowledge, representation that is each time accompanied by different ballast.

In an individual, knowledge lives, so to speak, in constant change, affecting its internal organization, representation, accessibility, interpretation, and content.

3. Knowledge representation in order to find most useful form in which to keep the knowledge for certain purpose.

In a problem solving environment we remodel the whole situation in order to find a better representation under wich the problem may be solved. We create graphic image of relations otherwise given in words, we express in equations descriptions of a situation. We use binary, octal, decimal representation of numbers in order to make it more suitable for further processing. The content remains the same, the representation is each time different. Yet, the representation has profound influence on how the knowledge may be further processed.

There are different rules for multiplying decimal numbers than binary numbers. There are different rules for traversing a graphically represented tree structure than traversing it by using recursion. The search for proper representation of knowledge is pursued by individuals each in its own way. Some people have better performance in certain areas of thinking simply because they have better internal representation of knowledge for the particular purpose.

For example, residual classes number system (modual arithmetic) leads to the fatest known addition and multiplication. It is very difficult to make divisions in the same system. Yet, amounts represented by residual classes system are the same as amounts represented by decimal number system. The difference is in how the amounts are represented by their digits.

15.7. KNOWLEDGE TRANSFORMATION

Transformation of knowledge from one representation into another is again an on going process in an individual and is sometimes control by consciousness only in a small way. When I think how to get from here to New York, each time I think about it I imagine it in a different way. At one time I see myself going to the airport by taxi, at another time by my car. But, even if I think of the same way, by my car, the details are always different.

In general we need to find such representation of the problem we need to solve on daily bases, that will bring us into a comfortable internal environment in which we are able to bring up the solution. Since this environment constantly changes, so does the knowledge. When we come back to the same problem, we in the meantime have established new relations, sometimes seemingly unrelated to the problem. Yet, when we rethink the problem, the changed internal environment will contribute to a new, may be better solution.

The point here is that in an individual, no two transformations are the same. Even when a sentence is repeated twice one after the other there is a slightly change in meaning. Second time the sentence is interpreted already in the context of knowing it. It is as if an individual cannot go back to transform or handle the same knowledge twice the same way. Not only the knowledge changes but the way it is manipulated changes even when our consciousness says you have made the same transformation as before.

15.8. CONCLUSIONS

A non living individual differs from a living individual in one outstanding point. Knowledge is a non living system, such as a computer, tape, book, library enjoyes a great deal of stability that does not easily changes its content. Management of knowledge is relatively simple because, besides the given information and its content there are no on going unintended changes.

On the other hand, in living individuals knowledge is a dynamic entity that changes in many ways. As the time processes, knowledge is re-organized, re-interpreted, re-represented, and subjectively re-evaluated. Changes in content and its representation take place as both intended and unintended changes. Management of the changes is an individual one. Problems of maintenance, supply, and interchange of knowledge in individuals needs to be, therefore, approached with considerably more attention to their individuality than in case of non-living knowledge systems. Knowledge is a private and subjective property that carriers not only its objective content but also subjective characteristics of the individual in its many faceted dimensions.

CHAPTER 16

THE SCIENTIFIC-TECHNOLOGICAL DISSOLUTION OF THE MEANING OF LIFE

Roberto S. Bartholo Jr.

From an antropological and philosophical point-of-view men and women are beings who need to direct their lives through an idea of themselves and of the world. This leads to the possibility of a specifically human way of exercising power, independent from any direct physical coercion, as not all individuals conduct their lives according to their own minds, but rather their lives are directed under the "spirtual guardianship" of the controllers of the social system of representations concering the meaning of life.

"Domination" must be distinguished from simple "power". "Power means the possibility of imposing one's own will in a social relation, even though against every type of resistance. "Dominant" means the opportunity of meeting with obedience for a command with certain content.

The most important features of every system of "spirtual guardianship" are:

1. a theoretical explanation of the world;
2. a system of meaningful propositions aiming at shaping human action in the world;
3. human powerlessness in face of cosmos.

Every hierocratical system of ideas contains practical imperatives on what must be done and what must be avoided, and it indicates how it must be done or avoided. The hierocratical administration of the "Hope Principle" transforms itself into a solacing achievement, the root of the human need for salvationist doctrines. The history of Western Civilization expresses a continuous process of secularization.

The industrial civilizing process builds a network of human interactions mediated by the "objective impositions" of science as technology. Personal experiences of powerlessness are reduced to problems capable of a technological/scientific solution. The ways of realization of the personal happiness of autonomous individuals, unengaged with the normative system imposed of industrial civilization, are denied. Within a society, which has in science as technology a source of power, the struggle for the monopolization of "scientific truth" becomes decisive for the legitimacy of domination. "Truth" and "rationality" become operatinal concepts, so that every practical and scientific piece of knowledge has to attach itself to the frame of reference of prescribed "ultimate values", predetermined by the "new clergy".

Whereas for Kant the Enlightenment meant the individual's escape from a situation of "spirtual guardianship" based on his own incapacity of using his own Reason without needing somebody else's guidance, the Enlightenment of the industrial civilization turns to be an instrument of annihilation of the individual ethical autonomy through schooling. Within the Wester cultural tradition, Humanism and Englightenment altered the ideal of cultural acquistion as an ethical self-construction of existence through religion. Science and art start being regarded as autonomous ways of cultural acquisition "... with the objective of achieving a timeless ideality of the person and of creating an internal freedom for an existencial form which knows how to rise above the mere demands of activities externally put forward by the world"[1]. This is the core of W. von Humboldt's idea of University of the institution that makes possible the dialectical supersession of popular religiousness. This is expressed in Goethe's "Zahme Xenien":

> "Those who posses science and art
> also posses religion.
> The one who possesses neither
> he has religion."

For the Enlightened humanist ideal the individual self-education is a precondition for ethical responsibility. Cultural acquistion is identified with the self-construction of an individuality internally directed, so that the Humboldtian University is conceived as an institution of ethical self-formation. This ethical self-formation should make possible a process understood as meaning "living in the highest sense of the word" and expressing "... the ultimate task of our existence: to provide the concept of humanity in our own selves with a content as wide as possible; this task resolves itself through the association of our "self" with the world for a more general, active and free interaction"[2].

Science as technology of the quotidian world can not serve as a vehicle for an ethical self-formation of the individual. A dialectical supersession of the usual scientific-professionalizing education and an ethical confrontation of the individual with practical imperatives of modern technology are needed. H.A. Steger expresses this as an actualization of Goethe's poem:

> "Those who possess the capacity for an ethical
> [confrontation with modern industrial civilization]
> also possess science.
> The one who does not possess this capacity
> he has science."

16.1. NOTES

1. Quoted from H. Schelsky, Der mensch in der wissenschaftlichen Zivilisation. In: Auf der Such nach Wirklichkeit, München, 1979, p. 474.

2. Quoted from W. v. Humboldt. Theorie der Bildung des Menschen, in Gesammelte Schriften, Berlin, 1968, vol. I, in H. Schelsky in Einsamkeit und Freiheit. Reinbek bei Hamburg, 1963, p. 81.

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CHAPTER 17

MECHANICAL TREES

Ranulph Glanville

The salient question is how do we know about knowledge: what is necessary for it to have a reality to our perceptions, our minds? For, even should knowledge have the sort of objective, external existence "out there" as is often claimed for the Laws of Physics, this existence is neither apparent nor useful to us without our knowing.

17.1. INTRODUCTION

Knowledge is a particularly difficult subject to discuss because, in dealing with it one is obliged to use the concept itself. Virtually the whole of the tradition of thought, and the acceptable mechanisms of thinking and argumentation, within which we (in the west) have been brought up not only denies the legitimacy of a formulation, but abhors it. Yet it seems to need to be, just as the cell needs to contain its own complete self-description in order to reproduce (be). So it seems pointless to argue that because our tradition denies its possibility it is not: we whould change our tradition (Knowledge of Disappearing Problems). This point is not being argued here, but needs to be kept in mind.

So knowledge requires, as its pre-conditions, a knower and an act, knowing. Whether or not it exists out there, and, if so, the requirements it may have there, we can never know, for all our knowing depends on our knowing: or rather, all that we know depends on each of us coming to know it. And, before it is known, it is unknown, and who knows whether or not it exists. (The earlier point of the difficulty of self-reference should now be transparant.) What, then, can be said about knowledge? It depends on a knower and a knowing, which transform the unknown to be known. Or, rather more accurately, I should say knowing constructs the known and the knower. (This has nothing to do with notions such as correctness, verifiability, etc., which are the conventional technology of knowledge.) But that does not yet clarify that knowledge is. Knowledge is, I assert, not that which is known, not in the mind, but an external representation o the act of knowing. As such, it may have very little direct connection with the subject of which it is purported to be knowledge other than the formal (there is surely to need to argue Wittgenstein (1966) here in their own terms).

Activity (knowing) is central. The acting helps bring out both the knower and the known. But it is hard to conceive an action without the actor. Thus, there is a secondary difficulty: how does the knower know that there is a knower to know the known. And the answer is that the knower and the known must be (aspects of) the same, in this primitive act of knowing, the self-consciousness of self-knowing. So that, when I assert

I know this

I am, necessarily, asserting

I know I

and thus

I know I know this.

And, by means of the minimal courtesy of reciprocity (since we can know nothing of anything without knowing, and must therefore assume that that which is known is in a position to be known, which condition, as above, is satisfied by the object of knowing being also its own subject), we assume

This knows this.

And therefore

I know I know this know this.

So, behind the assumption that there can be knowing, there lies a profound and essentially novel philosophical assertion about the nature of the self, of existence through knowing which is self-referential, being the form of self-knowing. (There have been a number of formulations, in quite different fields, of positions similar to this. They include Pask's Topics and P-individuals (1976, 1978), Varela's Self-referential Logic (1975), von Foerster's Eigen Objects (1976), Spencer Brown's Logic of Distinctions (1969) and Varela and my extension of this into Self-distinctions and Mobius Strips (Glanville, 1979, Glanville and Varela, 1980).)

Notice how formally like the conditions for problems, as argued above, this turns out to be!

17.2. DIS(APPEARANCE) KNOWLEDGE

Knowledge, then, if it is anything at all, is an exteriorization of the act of knowing, a representation that may have nothing very much to do with the act (except by being associated with it), an artifice of the "out there". And, in this sense, I have made it "disappear", for I have replaced it by knowing. This way of making knowlege disappear is no problem at al: unless you are hooked on nouns rather than verbs, on "out there" rather than constructing, on representation rather than process.

Sadly, this is a problem: most of us seem to prefer the tokens of knowledge to knowing. This is a weakness that is pervasive in many fields, but nowhere more so than in the (meta) fields of education and training, of teaching and learning. It is apparent, through the personal experience of all of us who went to school, that a great deal of work has gone into the knowledge side of things, with which goes a particular philosphy that I find unacceptable, in light of what I have said above.

The philosophy is the mechanicism that, in its most extreme form, is embodied in behaviourism. This can be seen clearly in our attitude to text books (and even more sophisticated embidiments, such as are commonly used in Computer Aided Learning, and in the approach to teaching and learning. At its most extreme worst, this is the ancient pumping in of knowledge to the blank brain of the learner (teaching and learning by rote, also known as the battering ram technique). And, although training methods are now changing, most thoughts of training are centred on a modification of this notion. As Michael Robinson (1979) has pointed out, even the arrangement of desks, the provision of uniform, and the domain authority of the teacher is set up to reflect and support this attitude.

The difficulty with this attitude is that there is no way of knowing that there is any similarity in what the learner and the teacher understand. Indeed, the method is intended to remove this question from the field of possibilities, since all that can be done to test understandings is repetition, essentially in the form of the knowledge (e.g., text-book).

Thus, the test is of the response, not of the connection between the stimulus and the response: the external, not the internal. But knowledge does not necessarily bear any direct relation to knowing, as I have emphasised. What is necessary is to give sensible weighting to knowing (and to the study of this). This, however, brings out another contrast: "learning" by rote reflects a causalistic approach, yet knowing has to be made individually by each participant, and the check for understanding can, for similar reasons, only be carried out by me (as, e.g., the teacher) seeing if I can construct an understanding of your (the learner's) understanding of me, which seems to me to be similar to my original understanding.

Such a mechanism, first explicitly proposed by R.D. Laing (1966, 1970) and formalized by Gordon Pask (1976, 1978), is called a conversation. It is not necessary to go into it here, only to indicate that it exists and can be used to permit us to examine knowings. For it is knowing that matters, as the great liberal educationalists, such as Pestalozzi, Froebel and Montessori understood, and that what childred do, and like to do is to make their own knowing, on their own account, and that this, above all else, is to be valued.

C.A.L. is caught in this contrast. While it permits of more structured routes through some subject area (although dipping into books is pretty flexible, too), it concerns itself little with knowing, even in the most sophisticated notations such as Pask's (1976) Entailment Meshes. The computer is as the textbook, without the teacher (except in programmed learning, which is rote learning by another name). In order for C.A.L. to develop, we will have to study the mechanism of knowing, instead of the organization of representations of knowing (knowledge), so that the computer can converse with the learner, as good teachers do. And, of course, finding this mechanism will be the crock of gold to Artificial Intelligence, for computers will be able to learn for themselves, in their own way. To understand knowing is the challenge.

17.3. KNOWING

This challenge is going to be difficult to meet. To start with, there is the difference between that which is represented and that which is the representation: it may be that the only way to get over this is either to assume that there is no difference or that we deal only with representations. But I have already shown the short-comings of such approaches. It is a difficulty that is all too familiar, but often only consciously remembered when dealing with the inarticulate, such as animals and babies.

However, some work has been done that appears promising and relevant. It is allied to the concept of "Constructivism". Although the idea has been around for a long time, and has particularly suffused the work of Piaget (1955, 1972) and Kelly (1955), and has been publicly expounded many times by von Glasersfeld (1974, 1977, 1985) and von Foerster (1973), it is perhaps best summarized in George Spencer Brown's terse command:

Draw a distinction.

This command is at the root of his "The Laws of form" (1969), a calculus, which, amongst other things, gives a relational logic for the constructions made by such distinction drawing. It should be noted that, in the drawing of a distinction, both that which is distinguished and that which it is distinguished from become apparent. (This is similar to the

I know I know this knows this

earlier, for it is the distinction that provides the symmetry of process that allows the difference of identity.)

There are some difficulties with Spencer Brown's work (see Varela (1975), Glanville (1979), Glanville and Varela (1980), and some believe that "all" Spencer Brown has done is to produce "another" modal logic. But even were that to be the case, the point is being missed. The point is that comprehension of the world is constructed (YOU draw a distinction), and this is why it is relevant here, for it is not about representation or about an existing world out there, but it is about creating the it and the I, and hence about knowing rather than knowledge.

Nevertheless, having a logical calculus is not the same as having a psychological understanding, so while Spencer Brown provides an inspiring statement and set of manipulations, he does not provide the tool needed. To the best of my knowledge, there are (only) two attempts to find such a tool. They are Pask's Lp (1985) and my own Model Dimensions (Glanville, 1980 a). Initially, Lp is an attempt to find a set of rules for the legitimate generation of Entailment Meshes, while Model Dimensions is a method, akin to that of dimensioning in Physics, to check the manipulation of arguments. Thus, both suffer from the fault that they deal in, as it were, knowledge, rather than knowing. But what makes them interesting and possibly relevant, here, is that they do not presume an a priori set of items awaiting distinction, and they do demonstrate relationships established by a distinction drawer between that which has already been distinguished (but may be changed while being re-distinguished), and that which is being distinguished now. In fact, both are used in computational procedures that at least begin to show some elementary semblance of intelligence, not because the machine has been programmed to respond as a facsimile of the programmer's intelligence, but through the development of strategies and distinctions by the machines themselves (i.e., the programmer installs a very simple program, which runs, if anything, as a meta-program, and is not full of value statements, algorithms, etc.). So both concern themselves with the drawing of distinctions that make values and, in so far as they are concerned with knowledge, they are concerned with its growth rather than its unravelling (Glanville, 1985).

It is this growing, above all, that makes both Lp and Model Dimensions candidates here for an understanding of knowing. For both are contentless, allowing content to be developed. And, in the same reciprocal manner that makes "this knows this" follow from "I know I", theprocess of knowing may be reflected in the relationships demonstrated in the field of knowledge. I.e., it is reasonable (and probably the only possibility) to consider that the process demonstrated in knowledge relationships reflects knowing. For, the distinction drawn distinguishes both the distinguisher and the distinguished (and which is which?). It is certainly the case that both would aspire to this, for they are seen as reflecting the mechanism by which concepts (in Pask's (191975) terms, topics, in mine (Glanville, 1975, 1978) Objects (of attention)) exist and relate.

(As a sideline, it is worth reminding ourselves that we, in developing both our personality and our (intelligent) world view, potentially spend 24 mobile hours a day for many years, in company with a "responsive" environment, equipped with sensory and motor devices. Computers are hardly given a fair chance, in comparison! (Glanville, 1980 b).

17.4. INTERLUDE

Who knows, anyhow, how we think, and how we know? It is surely a matter of concern that this HOW should be examined and answered, for the WHAT depends upon it, and not vice versa (just as we have enough problems with natural intelligence without going overboard about artificial intelligence, for all of the strength of Boden's (1977) argument about spin-off). Some few things we do seem to know: that we handle our knowing in chunks rather than in discrete atomic units (see Simon's (1966) experiments on chess, or mine on calculating (Glanville, 1985): that we handle a limited and more or less regular number of such chunks at a time (Miller's (1956) "Magical Number 7 +/-2"): that we develop world views that aré essentially habituations and allow us not to work out from whatever first principles what to do when confronted by the red bus, by frameworks of interconnections that form personal heterarchies (Kelly's Personal Construct Theory, my own work with architecture students (Glanville, 1980 c): and that, when, for instance, we see a crack in a wall, we have no way of knowing whether it was there all along and we failed to notice it, or whether it has just appeared (even asking others does not help us with this, it only shifts responsibility). But, who knows how we think, what our knowing is?

17.5. CONCLUSION

We need to concern ourselves with knowing, rather than knowledge. Knowledge is an impediment ot this study, and, indeed, leads to a type of mental constipation (as described in various ways by Lakatos (1969), Popper (1969) and Kuhn (1970)). When Knowledge disappears, we may look at knowing. We may also be able to completely change the construction of our perceptions, slowly as in psychiatric therapy, or quickly as in the conversion of Saul to St. Paul on the road to Damascus. And it is, possibly, only like this that we will be able to deal with the complexities of the modern world, and the design of modern life (Jones, 1985). I try to spend my time redesigning my universe (my standard biographical blurb gives my hobbies as "studying the horizontal and redesigning the universe"). Putting it another way (Anderson, 1985)

> You know,
> They are growing mechnical trees:
> They go to their full height,
> And then, they chop themselves down.

There is no problem of disappearing knowledge: Disappearing knowledge is good.

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CHAPTER 18

THE TOO-HASTY APPLICATION OF INADEQUATELY CONCEPTUALIZED "KNOWLEDGE"

H.D. Kimmel

Underlying the theme of this book on (Dis)appearing Knowledge is the assumption that the fundamental knowledge gained in scientific research somehow may vanish or cease to exist when an effort is made to use the knowledge in the "real" world. On the assumption that knowledge actually can disappear when it is applied, authors have attempted to identify the conditions under which this disappearance might be more or less likely to occur or to discover what might be done to prevent or retard its disappearance.

18.1. INTRODUCTION

In this chapter, the assumption that kowledge disappears is unequivocally rejected. Indeed, it is assumed that scientific knowledge cannot ever even have the kind of substantive existence necessary for it to disappear. Scientific knowledge is necessarily always tentative and provisional. It is constantly in need of additional empirical testing and it must be revised repeatedly and adjusted in accord with the outcomes of these evaluations. Knowledge is anything but static; knowledge is by nature dynamic. Unlike "true love", knowledge does indeed "bend with the remover to remove" and "alter where it alteration finds". And kncwledge is constructed in a manner which encourages its up-dating and revision and which is designed to facilitate its empirical falsification.

In the discipline of psychology, the two most common causes of what is misperceived to be the disappearance of knowledge when it is applied are:

1. application is attempted too hastily, before the attainment of understanding of the basis process;
2. knowledge is formulated uncritically and at too molar a level.

It should be obvious that these two factors are not independent, since the motivation that results in premature efforts to apply scientific knowledge may also fuel its overgeneralized and dogmatic representation. The very thing that fosters too early application, usually the perceived promise of a "breakthrough" in dealing with some significant societal ill, also tends to discourage skepticism and normal scientific caution, resulting in a formulation of knowledge in an overly simplistic (easier to apply) and dogmatic (impervious to negative instances) fashion.

The main thesis of this chapter, thus, is that knowledge only seems to "disappear", in the sense that its application often does not have the anticipated effect, when what has really happened is that the knowledge was either inaccurately or incompletely conceptualized or was falsely expected to have applied utility due to overgeneralization of the basic research findings. Two specific examples of this problem are described, the "unfulfilled promise" of biofeedback and the "token learning" that results from token reinforcement. The first example shows how great expectations for alleviation of illness may encourage excessive haste and inadequate skepticism. The second is an example of the effect of parochial theoretical dogmatism and simplistic overgeneralization, exacerbated by the influence of an authority figure. The first example illustrates the influence of extra-scientific social factors, while the second must be attributed to factors <u>within</u> the scientific system itself.

18.2. THE UNFULFILLED PROMISE OF BIOFEEDBACK

Some 25 years ago, a few investigators began to question the belief that "involuntary" behavior can only be modified by Pavlovian conditioning (Kimmel, 1967). After several studies showed that responses of the automatic nervous system and electrical rhythms of the brain could be modied by informative, rewarding/punishing feedback, an entirely new area of possible application appeared to open up. From a conservative perspective, it was not unreasonable to hpe that some human psychosomatic illnesses might be treated successfully with techniques based upon these laboratory results.

But, some misinformed and excessibely zealous individuals imagined that they saw in biofeedback the seeds of a mind-expanding nirvana, a way to enrich and enhance human experience beyond even the hallucinations produced by LSD and other psychotomimetic drugs. Others foolishly suggested that biofeedback could free people from enslavement to their biological existences and elevate the "mind" to total control of the "body".

Biofeedback's effectiveness as a treatment of migraine, epilepsy, hypertension, asthma, Reynaud's syndrome, and other illnesses has turned out to be all but nil (Yates, 1980). Its greatest therapeutic successes have been in its least exotic applications, e.g., motor retraining following stroke. And, of course, no one's mind has actually been expanded by biofeedback, although some people's bank accounts have grown due to "therapeutic" biofeedback-exploitation of others or due to the sale of popular biofeedback books and biofeedback electronic equipment for the home.

The unfulfilled promise of biofeedback is a good example of alleged "knowledge-disappearance" which is reality is knowledge that is inadequately or insufficiently worked out and rushed into the marketplace disguised as something it really isn't. The social pressure to promote biofeedback as a cure-all for both physical and spiritual ailments reached such a pitch at one time that speakers at a plenary session of the Biofeedback Society meetings who urged that a more cautious approach be taken and that more basic research be done were actually shouted down by the audience.

The popular "slick" press contributed to this pressure for application, because articles on biofeedback were sure-thing attractions when they could be written uncritically for a literate-but-technically-uninformed audience that hungered to be told what wonders were in store for them in the space-age future. Letters to the editors of these magzines, pointing out the technical-factual shortcomings of such journalistic pieces, went unpublished and unanswered (Kimmel, 1985).

The result has been disappointment and disillustionment, as well as a tendency to view biofeedback with suspicion. The need for substantial additional basic research on the underlying mechanisms is greater now than it was at an earlier stage in its history, but the negative aura resulting from premature application and consequent failure has tended to make research of this type less socially attractive to younger behavioral scientists. It remains to be seen whether the errors of the past can be totally overcome and a new beginning established.

18.3. TOKEN LEARNING RESULTS FROM TOKEN REINFORCEMENT

B.F. Skinner's operational simplication of the meaning of "reinforcement" was intended to rid the concept of unnecessary theoretical baggage so that its empirical laws could be better elaborated. To Skinner, "reinforcement" referred to the increase in probability of response (read "frequently" of response) resulting from the response-contingent administration of the "reinforcing" event (Skinner, 1950). Behavioral scientists were not to "reason why" (i.e., ask "how" reinforcement works) but to "do or die" in determining empirically the influence of different reinforcement schedules on responding. What makes "reinforcement" reinforcement is that it works, that the behavior increases in frequency. That is all that reinforcement means according to Skinner's "empirical law of effect".

Skinner's anti-theoretical posture was partly principle and partly pose. At the same time that he assumed the role of "Bricoleur" (Levi-Strauss, 1969) in his assertion that theories of learning are not necessary (Skinner, 1950), he was playing the opposite role of "engineer" to the lay public, telling them that the "technology" of behavior already was developed to an extent that warranted its broad application to solving a wide range of social problems (Skinner, 1953).

But, it was one thing for Skinner to engage in metatheoretical polemics with his learning-theory opponents and a completely different thing to imbue relatively naive behavior technicians with an uncritical mission to "use" reinforcement to change everything from the non-verbal behavior of autistic children to the toilet habits of the mentally retarded. As it turns out, the latter appears to yield more readily than the former to response-contingent reward.

The energetic proselytizing by Skinner's representatives in public education, hospitals, penal institutions, etc., has resulted in an enormous amount of work being done in the field of behavior modification. Literally tens of thousands of applications of the empirical reinforcement principle have been undertaken, some with substantial success (Allyon & Azrin, 1968) and others with very little (Kuypers, Becker and O'Leary, 1968). Over the past 10-20 years of zealous application of reinforcement it has become clearer and clearer that the reasons for the successes are no better known than the reasons for the failures. The problem, of course, is that what was presumed to be "knowledge" (i.e., the wide-ranging effectiveness of reinforcement) was really only a simplistic, easily packaged, homily rather than a sufficiently worked out understanding of basic relationships. When Breland & Breland (1961) reported on the tendency for operantly conditoned pigs to revert ot burying the tokens they received for performing certain behaviors rather than converting these "secondary reinforcers" into food, this "misbehavior" was treated as an oddity rather than taken as an indication of insufficient comprehension of the reinforcement mechanism. This was not an example of the disappearance of knowledge as much as it was an example of the absence of genuine knowledge.

In recent years several writers have presented evidence of the potentially negative influence of using "extrinsic" rewards to establish and/or maintin behaviors that might otherwise be "intrinsically" rewarding (Deci, 1971, 1972; Levine & Fasnacht, 1974). In general, these studies have shown that the removal of the extrinsic reward may have the effect of rapidly stopping the behavior upon which the reward was contingent. Indeed, whether primary reinforcement (e.g., candy) or secondary reinforcement (e.g., tokens that can be "spent" on other primary and secondary reinforcers) is involved, there appears to be a tendency for the intrinsic value of performing certain behaviors (e.g., playing the piona) to diminish as a consequence of being given extrinsic reinforcement for doing it (Levine & Fasnacht, 1974). These authors suggest that "natural" reinforcers might be expected to not have these effects.

The "empirical law of effect" is more than a theoretically neutral "operational definition" (MacKintosh, 1983). Skinner quite explicitly has argued that it doesn't matter how reinforcement works in the sense of mechanism. What maaters is that it works. But it doesn't work well or often enought to justify stubborn refusal to examine its underlying mechanisms.

Arguing that it doesn't matter what the mechanism underlying observerd reinforcement effects may be, of course, implies that all instances of responsecontigent "reinforcement" that result in increased response frequency are examples of a common process ("reinforcement" must be written in quotes here because the use of the term already implies that an increase in response frequency has been observed). That is, the mechanism underlying reinforcement can only be considered "unimportant" if it really doesn't matter, i.e., if all instances of reinforcement are equivalent. Thus, if response-contingent administration of M & M's to children when they say, "Please, may I?" "Would it be alright if?" etc., is equivalent to response-contingent administration of Noyes food pellets to rats when they press a lever in a Skinner Box, then the equivalence must reside in the fact that both situations involve training living organisms what must be done to obtain a desired food substance. Yet, when the M & M's are withdrawn, the children are likely to explore additional verbal mitigators besides those that were originally reinforced; removing the Noyes pellets, on the other hand, results in a burst of lever-presses but no "analogous" responses. In this case, reinforcing a "class" of responses may have a different effect from reinforcing a single response.

Training a rat to enter a goal-box in a maxe, using food as a reinforcement, likewise, is rather different from similar training with a brief light as reinforcement. Both may prove to be effective, in the sense that an increase in frequency of goal entries may occur, but removal of the light results only in extinction while removal of the food results in aggressive behavior as well as eventual extinction. It is obvious that the notion of "strength" of reinforcement (food vs. light) might be relevant in this situation, but such a concept is antithetical to Skinner's domatic argument against analysis of reinforcement mechanisms.

The possibility that there is more than one kind of reinforcement has even been considered recently by Skinner (1979). Nothing that it is often not possible to reduce the delay of reinforcement that naturally is necessary in real-life behavior modification situations, for example the increase in longevity that might be expected to occur following cessation of smoking, Skinner points out that a variety of "contrived" intermediate reinforcement contingencies may be used to bridge the temporal gap between the behavior-change and its natural reinforcing consequence. After all, if an individual is, say, 40 years old and stops smoking, whatever increase in life-span that this might bring about will not occur until 30-40 years later. This "gain" can hardly be expected to act as a response-contingent reinforcer in maintaining the behavior of not smoking. Thus, symbolic and other contrived reinforcers must be used to maintain the desired behavior. But contrived reinforcers do not work as well as natural ones, and this may be one reason for what looks disappearance of knowledge when the use of contrived reinforcement to stop smoking ends in failure.

Skinner exemplifies the meaning of "contrived reinforcement" by relating an anecdote from his personal life.

> He was playing with his daughter, holding her on his knee. He notices that it was beginning to get dark, so he turned on the light in the room. His daughter smiled when the light came on, apparently in response to the change. Skinner decided to try to "use" the turning on of the light to reinforce some simple movement. He turned the light off again (a compliation that makes this example less than ideal) and, when the infant raised her hand slightly, he turned the light on. The he again turned off the light and waited for the baby to lift her hand again, so he could "reinforce" this movement with the light.

In this way, Skinner gradually modifed his daughter's behavior, so that she would lift her hand as soon as the light was turned off.

Skinner's point in this example is that there is no <u>natural</u> connection between the moving of the hand and the increase in illumination following it in this situation and, apparently, reinforcing it. Certain behaviors are naturally followed by reinforcers, such as eating and drinking, or entering a cool swimming pool on a hot day. Indeed, there is some confusion regarding the exact meaning of reinforcement in these cases, since the swimmer doesn't keep coming out of the swimming pool so that the behavior of entering it can be repeated. Skinner, himself, displays this confusion regrding the meaning of "reinforcing" in the first volume of his autobiography, <u>Particulars of my life</u> (Skinner, 1976). He reports that he learned to masturbate when he was about eleven years old. He found that "several rhythmic strokes" had "a highly reinforcing effect" (1976, p. 64). One cannot help wondering whether Skinner meant "pleasurable" here when he said "reinforcing". The "empirical law of effect" is an explicit rejection of lwas of effect based upon the pleasantness or unpleasantness (or some other affective data) associated with the reinforcer.

The meaning of natural reinforcement is also conveyed by Skinner in an anecdote. He gives his daughter a rattle and she accidently shakes it. Shaking the rattle automatically produces sound, and the child shakes the rattle again, and again. Here the sound produced by shaking the rattle is assumed by Skinner to be a naturally reinforcing consequence of the shaking movement, in contrast with the contrived contingency involved when he turned on the light each time his daughter lifted her hand. The question of why natural reinforcement works better or lasts longer than contrived reinforcement is not addressed by Skinner, although it would appear that even to discuss the distinction marks a change from early Skinner to more recent Skinner. He denies, however, that this constitutes a change.

One of the reasons why so much candy has been given to so many children, with so little in the way of long-lasting behaviorial changes to show for it, is that Skinner has actually been quite successful in convincing his disciples that it is not necessary to analyze the mechanisms of reinforcement beyond describing its molar empirical relationships. The so-called technology of behavior has been grossly oversold, so that attention is focussed only on success and failure has been swept under the rug.

The fact of the matter is that the empirical law of effect is a convenient device for avoiding the difficult task of analyzing the many instances in which behavior-strengthening might have been expected but did not occur. What exactly are the necessary conditions for achieving the outcome that the empirical law of effect calls "reinforcement?". As soon as Skinner recognized that it would not be easy to discover what these conditions are, he gave up even trying and urged others to join him. But, even Skinner's own "disciples" have come to recognize the inadequacy of his simplistic anti-analytical position. For example, Premack elaborated the principle by proposing that reinforcing outcomes depend upon the relative reflex strengths of the modified and modifying reflexes (1965). Although Premack's principle was a significant step in a genuinely theoretical direction, it was still inadequate for all but the simplest reinforcement situations. More comprehensive and more theoretical analysis remained to be proposed (e.g., Catania, 1966, Herrnstein, 1970), and the applied implications of these recent advances have still not been realized or appreciated by the "front-line" behavior modifers who still think that Skinner's empirical law of effect is a sufficient behavior-modifying tool.

The ultimate behavior modification, as Skinner himself has observed (1979), is the change in the behavior of the scientist when his data do not agree with this theory or when his methods fail to produce their intended and expected results. Negative feedback from real-life application of a scientific principle should eventually result in modification of the principle, so that it better fits the discrepant empirical data. This, of course, is the kind of change that has been going on in some of the basic operant conditioning laboratories (op. cit. Catania, 1966; Herrnstein, 1970). Eventhough Skinner, himself, may be unable or unwilling to admit the need for change, the nature of the scientific enterprise eventually overcomes dogmatism and the "knowledge" that results can be expected to be less likely to "disappear".

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