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"The End of Science": Can We Overcome Cognitive Limitations?

Abstract

"Why is the universe knowable?" DAVIES (1990) wonders. In this paper, I argue that science is not a matter of knowing any universe. Rather, it is a—as history has shown—superior method of guidelines of how to organize experiences yielding predictive power. Historically, two types of models have given rise to the effectiveness of science, narrative and mathematical models. Based on cognitive psychological investigations, I point out that due to the human nature of scientific reasoning both types of models are limited. With the advent of computational devices scientific investigation may now be extended to "externalized deductions", which are not subject to a limited short-term memory and slow performance. To shift this to computational science we have to recognize that models in all three approaches have basically the same function. Although this might not solve the realist's question of how models relate to the world (at a deep philosophical sense), it will guarantee the continued existence of contemporary science beyond the cognitive barrier.

Key words

Philosophy of science, cognition, complexity, models, reality, constructivism, problem solving, artifacts.

EVOLUTIONARY EPISTEMOLOGY has brought forth the idea of science as an evolutionary system (cf. CAMPBELL 1974, OESER 1984, RIEDL 1983). From systems theory of evolution (RIEDL 1977) and the theory of punctuated equilibrium (GOULD/ELDREDGE 1977) we know that evolution does not proceed homogeneously. Rather, periods of stasis are interrupted by dramatic changes. Over the last few centuries we have experienced science as a dynamic enterprise with several revolutions. Will we now face the stasis of science? These arguments are not purely theoretical: In a recent book, John HORGAN explicitly speaks of "The End of Science" (1996). In this paper, I outline the mechanisms of the "evolution of science" by first finding an appropriate perspective on the philosophy of science. Then, after a short review (and rejection) of HORGAN's thesis, I identify three core problems to science. These problems, which are mainly motivated by cognitive psychology, have become serious since science started to deal with complexity. Computer models have been proposed to cope with this latest frontier of science. However, such models have not received acceptance among the scientific community due to the presumingly arbitrary relationship between computational model and "the reality out there" (the reminiscence syndrome). I argue that this must be true for any model, including narrative and mathematical models. The success of models is their predicative power. I conclude that due to cognitive limits of human scientists, model-building is also subject to limitations. By using computational devices, those limitations might be transcended.

Different perspectives on scientific activity

Ralph GOMORY (1995) argues that the choice of appropriate perspectives is significant if we want to make the unknown visible: "[I]n distinguishing the known or the unknown from the unknowable, the level of detail can be decisive" (p88).

This is also true if we look at philosophy of science: to find the "proper" explanation which both explains success and failure of science. Unlike many other papers on the present topic (e.g., LAUDAN 1977, STENT 1978, VAN FRAASSEN 1980, NERSESSIAN 1987, FAUST 1984, GIERE 1993), I will not focus on yet another philosophical treatment. Rather, I will deal with the subject of science in a pragmatic way which aims at the success of predictions. The following list locates this position among all possible views on the philosophy of science. Furthermore, the list summarizes what we potentially can expect from a philosophy of mind. For the rest of the paper, I will, triggered by recent discussions about the end of science, outline why we should concern ourselves with a possible limitation to science at all and what a possible solution might look like.[1]

We must clearly outline what a philosophy of science should do for us:

1. Is it a pure philosophical exercise where arguments of various authors are compared, thus building a discourse which does not necessarily "ground" (HARNAD 1990) in the subject (i.e., scientific activity)? However, the ultimate goal of any scientific inquiry is not to be an end in itself. Rather, it has a constructive character in that it allows us to extend the set of actions which we use in order to predict and perceive our world in an increasingly better way.
2. Is it descriptive in order to explain what has happened to date? Any description may be based on sociological models (cf. KUHN 1962), on a psychological approach (cf. GIERE 1993), or even on a computational philosophy of science (cf. THAGARD 1988).
3. Is it a normative instrument which tells scientists how to do science, such as the research methodology of the logical positivists (SCHILPP 1963) or Karl POPPER's rejection of induction (1934)?
4. Is it generative in that it is capable of predicting what the future of science will be? Can we expect that the principal limits of science can be specified analogously to GÖDEL's Incompleteness Theorem, which poses limits on formal systems (e.g., CASTI 1996a)? Following an entirely positivist view on science, can we even expect the end of science since "all great revolutions are already behind us" as proposed by the recent *The End of Science* book by John HORGAN (1996)?
5. Or will it provide insights and mechanisms which—in the long run—can be automatized and therefore passed over to artificial artifacts which then will carry out scientific reasoning? Such proposals have been around for many decades already, cf. the General Problem Solver of NEWELL and SIMON (1972) and BACON of LANGLEY et al. (1987) More pragmatically, one may think of the usage of computers in mathematics as the first sign of this development. For example, the famed four-color conjecture (APPEL/HAKEN 1977), which demonstrated that problems may no longer be tackled by traditional, human-based methods. It made use of the power of hundreds of hours of computation on supercomputers in order to calculate individual cases rather than to prove the problem in a traditional mathematical way.

The last two items especially may yield the expectation that in future, when the content of scientific theories will have transcended the limitation of the human mind, computers (or other artifacts) may take over the business of exploring Nature.

What can such computers "learn" from human scientific activities, and what does "Nature" refer to? Are there limits to science carried out by humans? If we don't face any such limits, we barely need any artificial extensions. Too much "pleasure" is involved in the process of generating scientific knowledge. But, as with transportation, walking also may provide much pleasure, nevertheless society would not be able to survive without motorized means of transportation. This is a good demonstration of human nature: Although we have been using motor-based vehicles for many decades, we still, and in fact more than ever, enjoy our biological movement, not to mention that our health depends on it. To draw an analogy, in the future scientific reasoning might be done by machines, nevertheless we will still enjoy the intellectual challenge by tackling problems which we can grasp with our (narrow) mind. In the following chapter, I will present these restrictions in more detail, starting from the positivist's fear that the big parts of the scientific pie have already been eaten, leaving only crumbs for contemporary (and future) scientists.

The end of science?

I was recently reminded of the possibility that science might come to an end by the provocative book of John HORGAN (1996) with the self-explaining title *The End of Science*. "The great scientists want, above all, to discover truth about nature", John HORGAN wrote in his 1996 book. And since "researchers already mapped out physical reality", all that is left is to fill in details[2]. To be more concrete, "all" refers to good science, which is capable of producing "surprises", i.e., scientific revolutions as has been introduced by DARWIN, EINSTEIN and WATSON & CRICK. However, "all" neither refers to the (boring?) scientific activities of filling in all the gaps within the map mentioned above, nor to applied science. And it does not refer to what HORGAN calls "ironic science", those efforts of physicists and chaos-complexity-researchers ("chaoplexologists" in HORGAN's terminology, p192) which argue for the existence of high dimensional superstrings and life inside computers.

HORGAN dissociate himself from any relativist view on science brought forth to a large audience by

Thomas KUHN (1962) in the early 60s[3]. He therefore cannot help but think that all present scientific knowledge is the complete framework to describe and cope with reality. Taking a KUHNIAN perspective into account, he might rather—possibly correctly—speak of an end of the *current paradigm*.[4] Indeed, as Melanie MITCHELL (1995) in her response to HORGAN's previously published paper "From Complexity to Perplexity" (1995, p1) pointed out, that "[t]he specter of the "end of science" periodically appears in the scientific and popular literature, often at the end of one scientific era (e.g., NEWTONIAN mechanics), before the beginning of a new one (e.g., quantum mechanics)."

According to her and other "chaoplexologists", the specialization in science "has certainly produced great advances, but the problem of complex systems demands approaches that span disciplines". In other words, the current set of paradigms needs to be substituted by another set. Now, will there really soon be a change of paradigm in the traditional KUHNIAN sense?

Certainly we have to take evolutionary constraints into account. This is the line of argumentation which, for example, is followed by Colin MCGINN (1994). Like rats and monkeys which cannot conceive of quantum mechanics, humans may be unable to understand certain aspects which are more sophisticated than our current theories in science. MCGINN primarily addresses the problem of consciousness. He emphasizes that for humans to grasp how subjective experience arises from matter might be like "slugs trying to do FREUDIAN psychoanalysis—they just don't have the conceptual equipment."

These issues make it clear that I am mainly interested in what we can learn from philosophy of science and how we can apply this knowledge to artificial systems in order to transcend the limits of human mind. As mentioned above, due to the ever incomplete aspects of psychology and sociology, any further philosophical treatise will not make further progress. An analogy makes it clear: Since we are not able to build such sophisticated systems like birds, we focus on technical realizations based upon what we have learned about aerodynamics. Our airplanes might have reached a level of enormous complexity (ARTHUR 1993), yet they are not as elegant in their movement as birds. However, planes outperform natural solutions in speed and payload. Likewise, we will construct artifacts that carry out science probably less aesthetically but more efficiently.[5]

Certainly, no theory can ever reach the status of universal applicability. This is also true for any theory that wants to explain the dynamics of scientific activity. Rather, it seems useful to explain science to an extent which will allow us to formalize its key mechanisms and to transfer it to artifacts.

What could the problems be?

The problems which may cause a decay of progress in human science are rooted in its members: the human scientists and their cognitive apparatus. In a nutshell, as human beings in general, and as scientists in particular we all suffer from essentially three problems that limit our cognitive capabilities (RIEGLER 1994):

1. We are used to thinking in *paradigms* in the sense of KUHN (1962)[6]. Indoctrinated at school and university, paradigms speed things up. They enable us to forget about previous steps in our scientific investigation and thus about the need to exhaustively search the entire problem space[7] which is enormously large for scientific investigations. The bad side of this is that this shortcut also limits our way of thinking and problem solving.
2. The limitation of our short-term memory does not allow us to compare more than seven knowledge items at the same time (the well-known *chunks* of MILLER 1956). This even further restricts our capability to entirely step through all corners of nontrivial-sized problem spaces of which scientific issues consist.
3. Faced with the limitations of our thinking and the fact that interesting phenomena are complex by nature, we have to ask: Which items must we choose in order to prune the cognitive search tree[8] effectively? In other words, how shall we solve the problem of *relevance* or the *frame problem* as it is called in artificial intelligence. Daniel DENNETT (1984) illustrates it with the following analogy which will serve as a reference throughout this paper: A robot, R1, as well as its improved descendents, have to learn that its spare battery, its precious energy supply, is locked in a room with a time bomb set to go off soon. To solve this problem the robot has to develop plans in order to foresee effects of its actions. It fails because it does not pay attention to the implications of its planned actions. Taking possible side-effects into account, however, does not help. As the real world is very complex, an exhaustive list of all side-effects would take too long to take any action in real-time. Hence, the robot must know how to

distinguish between relevant and irrelevant side-effects. But even this process of discrimination needs an enormous amount of computation, all the more as each of the possible effects must be assigned with some (quantitative) credit in order to evaluate their usefulness.

All three items are subject to closer investigation in the following sections.

Limiting canalization through paradigms

Science is carried out by human beings whose work is constrained by the current set of scientific methods, the well-known KUHNIAN paradigm. KUHN (1962) describes the relationship between a scientist and his or her paradigm as follows: "Scientists work from models acquired through education and through subsequent exposure to the literature often without quite knowing or needing to know what characteristics have given these models the status of community paradigms." (p46)

Such continuous repetitions of one and the same methodical schema inevitably confine the future scientist's capability of problem-solving. More than 30 years before KUHN, José ORTEGA Y GASSET (1929/1994) described the apparently automatic techniques for problem-solving already quite straight forwardly. He points out that scientists work with available methods like a machine. To achieve a wealth of results it is not even necessary to have a clear concept about their meaning and their foundations. This way, the average savant contributes to the progress of science as he is locked into his lab. ORTEGA compares this situation with that of a bee in its hive and the situation of a donkey in its whim-gin.[9]

Similar to KUHN's notion of paradigm, Paul FEYERABEND (1975) outlined the concept of stereotypical research schemata. He localized their roots in the cognitive development starting in early childhood: "From our very early days we learn to react to situations with the appropriate responses, linguistic or otherwise. The teaching procedures both *shape* the 'appearance', or 'phenomenon', and establish a firm *connection* with words, so that finally the phenomena seem to speak for themselves..." (p72)

FEYERABEND argues that starting in our early childhood we are acquiesced in an education that very clearly outlines both the way we have to view the world and the way we have to act in the world. Alternatives are suppressed or referred to the realm of fantasy. That is how our concept of reality emerges.

The purpose of paradigms, very much like the notion of reality (DIETTRICH 1995), is to secure acquired scientific knowledge and to provide a base for further developments. Historically, the scholastic age is a typical example of where the lack of a true hierarchical organization of concepts and paradigms finally led to its disintegration. Quite obviously, knowledge can only be acquired incrementally step by step without being exposed to the risk of starting from scratch over and over again. Of course, as pointed out by Rupert RIEDL (1977) for the realm of genetics, such hierarchies of interdependent components on the one hand increase the speed of development by magnitudes. On the other hand, they are "burdens" with respect to their canalizing effect since established structures define the boundary conditions for their future evolution. Exactly the same applies to science: In order to achieve progress we have to establish a firm ground of paradigms through education. Each time a new disciplines with a different set of paradigms rises, it has to start from scratch and is thus prone to a weak explanatory performance in terms of details, as the new discipline of complexity research demonstrates.

The psychology of science

Quite clearly, we can find limitations of deductive reasoning, a key component within the scientific method. Human brains are obviously not indefatigable automata capable of storing practically unlimited amounts of temporary information as is demonstrated by the well-studied problem of the Towers of Hanoi (SIMON 1975): The number of subgoals which have to be simultaneously remembered correlates to the number of disks. This means that the subgoals have to be stored in short-term memory which, as already pointed out by the famous work of MILLER (1956), is quite limited. People fail to solve the problem for towers with more than three disks if they are not allowed to use paper and pencil. Therefore, it is not surprising that for systems that consist of a large number of variables we use computer models.

In psychology, an enormous amount of literature deals with the problem solving capacity in human beings. In the following I will present some them which quite clearly show that our cognitive capabilities for problem solving (or *puzzle solving* in a more KUHNIAN terminology) are not only limited but also prone to errors when it comes to investigating complex systems.

“Stack overflow ”

In the contemporary design of computers, a component called the stack stores temporal information necessary to evaluate mathematical functions. This is similar to the carry when adding large numbers by hand; we also must not drop it in order to obtain the correct result. Since computers are finite implementations of TURING’s infinite machine, the stack is finite, too. This can easily be demonstrated by trying to evaluate an infinitely recursive function, i.e., a function which takes its results as arguments over and over again. Depending on the speed and stack size of the computer, a “stack overflow” error will occur within a few milliseconds, indicating that the stack can no longer memorize all sub-results. The stack in humans, also referred to as short-term memory, does not need to be exposed to infinitely recursive problems in order to show the same behavior.

The example of the mutilated checkerboard (WICKELGREN 1974) is one such case. It asks whether it is possible to arrange 31 domino pieces on a checkerboard on which two diagonally opposite corner squares have been cut off (yielding a 62 squares board). According to the author, it is almost impossible for a naive test person to find a quick solution. Obviously, the number of squares is correct (2 times 31 yields 62) but the human mind is incapable of managing the arrangement of black and red squares on a two-dimensional area. However, the problem becomes “trivial” if one simply counts the number of black and red squares on the mutilated checkerboard which differs by two, whereas on the 31 domino pieces the number of imaged black and red squares is equal. Gestalt psychology argues that we are good at recognizing regularities in pattern, e.g., patterns that consist of black and red areas. But an exact analysis of possible arrangements requires the temporary storage of subresults which transcends the capacity of our short-term memory.

“It ain’t broke so don’t fix it”

In our everyday life, things are used in a particular context, e.g., we use a hammer to drive nails into a wall, matches to light a fire. In fact, things do not seem to exist “outside” their domains of functionality[10]. DUNCKER (1935/45) posed the task to support a candle on a door. The available items were matches and a box filled with tacks. Since the test subjects considered the box as a mere container they failed to empty it and to tack it to the door where it could serve as a support for the candle. In general, our thinking is canalized (or fixed) with respect to the way we have learned to deal with things. Since cognitive development deals with both concrete and abstract entities, we assume that this restriction also applies to abstract concepts which prevail in scientific, especially mathematical reasoning.

The water-jug problem, studied by LUCHINS (1942), provides empirical data for this assumption of “mechanization of thoughts”. He asked test subjects to measure out a specific quantity of water using a set of three jugs with known volume. The first two problems LUCHINS posed could be solved by applying a certain sequence of pouring water from one jug into another. Test subjects had no problems to discover this procedure. Quite the contrary. They got used to it and tried to apply it to further tasks. Like the adage says, “It ain’t broke so don’t fix it”. What the test subjects overlooked was that much simpler procedures would have led to the same result, simply because their inductively working mind was set to the previously successful strategy.

The consequences of these psychological experiments (among others) are clear. During academic education we are subject to courses and seminars in which we acquire a certain way of thinking, a paradigm in the KUHNIAN sense. Recalling the problem of DENNETT’s robot, the advantage of such canalizations is clear: thinking can be abbreviated (and thus accelerated) by dropping computations about implications which are already known. This way, entire branches of our internal search tree can be pruned, thus leaving more time to concentrate on the unknown part.

The general view of human problem solving

KUHN (1962) argued that reasoning within normal science was puzzle-solving, i.e., it is concerned with solving tricky problems. From a general point of view, reasoning is a back-and-forth walk within the problem space, with several decision points. We might find that a particular branch does not yield the desired result, therefore we have to return to a previous decision point and try an alternative branch. Unfortunately, by a priori cutting off parts of the search tree through functional fixedness we are simply blind to those alternative branches and hence unable to find the solution to a particular problem. Rather, as LUCHINS’ *Einstellungseffekt* experiment demonstrates, we prefer to stick to inductive solutions, very much like the turkey in

Bertrand RUSSEL's analogy (after CHALMERS 1982): It started to believe in the charity of its owner—since the latter fed him regularly—before it ended up as Christmas meal.

As we have seen, for certain problems our cognitive limits are quite narrow. In the following, I will first relate these limits to concepts of Evolutionary Epistemology (thus providing some ideas how these limits have been come about). Then I will show that the gap between these limits and the complexity of systems we might consider to be "fancy calculator games", i.e., the computational approach to science, is much bigger than one might assume.

Ratiomorphic apparatus

According to the LORENZIAN Evolutionary Epistemology, human beings feature a system of innate forms of ideations which allows the anticipation of space, time, comparability, causality, finality, and a form of subjective probability or propensity (RIEDL et al. 1992). This ratiomorphic apparatus has to be distinguished from our rational abilities (LORENZ 1973/77, RIEDL et al. 1992) since the former indicates that "...although this ideation is closely analogous to rational behavior in both formal and functional respects, it has nothing to do with conscious reason."

Each of these ideations can be described as *innate hypotheses* (RIEDL 1981/84). These inborn teaching mechanisms are mental adaptations to basic phenomena that enable organisms to cope with them. One of these mechanisms—the ability for detection or discrimination of foreseeable and unforeseeable events—serves as a foundation for all others. This hypothesis of the *apparent truth* (*Hypothese vom anscheinend Wahren*) guides the propensity of a creature to make predictions with different degrees of confidence, ranging from complete uncertainty to firm certainty. Therefore, it produces prejudices in advance or anticipations of phenomena to come. The capability to anticipate is necessary for survival and contributes to the success of every higher organism.

The probability with which an unconditional stimulus follows a conditioned one correlates with the reliability of the response of the organism linking the two. The consequence is that animals and human beings behave as if the confirmation of an expectation makes the same anticipation more certain in the future. This is also the case in science where repeated confirmation of an expectation leads to certainty.

Equipped with this innate set of hypotheses, can we successfully face problems which are by far more complex then those of ancient man? Ross ASHBY in one of his last publications (1973) maintained "...that the scientist who deals with a complex interactive system must be prepared to give up trying to 'understand' it." In order to evaluate this statement let us have a closer look at the concept of complexity.

Complexity in science

In his remarks on constraints on science, Thomas HOMER-DIXON (1995) points out that human cognitive limits are due to the lack of infinite ability to understand and manage the complex, multivariate processes of ecological and social systems. The relationships in some of these systems are simply too numerous and complex to be grasped, much less controlled, by the human intellect.

What is complexity, and how does it relate to the human mind? KOHLEN/POLLAK (1983) characterize the "cognitive enterprise" as follows: "Cognitive science has worked under the general assumption that complex behaviors arise from complex computational processes. Computation lends us a rich vocabulary for describing and explaining cognitive behavior in many disciplines, including linguistics, psychology, and artificial intelligence. It also provides a novel method for evaluating models by comparing the underlying generative capacity of the model." (p253)

They conclude their analysis of complexity with: "[T]he computational complexity class cannot be an intrinsic property of a physical system: it emerges from the interaction of system state dynamics and measurement as established by an observer." (p264)

As pointed out by several authors (GRASSBERGER 1986, WALDROP 1992, HEYLIGHEN/AERTS 1998), complexity is hard to define. Rather than trying yet another definition, I will outline the inherent difficulties in understanding systems which entail a nontrivial amount of interdependent components. Where does this non-triviality start? VON FOERSTER (1985, 1990) provides a useful definition of the potential complexity of algorithms when he distinguishes trivial from non-trivial machines.

A *trivial machine* is a machine whose operations are not influenced by previous operations. It can be described by an operator (or function) p which maps any input variable x to an output variable y according to a transition table: $p(x) \rightarrow y$. For such machines the *problem of identification*, i.e., deducing the structure of the machine from its behavior, can be solved, since they are analytically determinable, independent from previous operations, and predictable.

On the contrary, *non-trivial machines*, i.e., TURING-like devices, consist of a memory holding an internal state z and two operators:

1. The "effect" function p_z realizes the state dependent mapping: $p_z(x) \rightarrow y$
2. The "state" function p_x performs the state transition within the non-trivial machine: $p_x(z) \rightarrow z'$

The important issue here is that the identification problem is not longer solvable even with very small non-trivial machines. Consider a machine with two states, four inputs, and four outputs. The number of possible models that potentially implements such a relatively simple system is: $4^4 \cdot 4^4 = 2^{16}$. A similar machine with three instead of two internal states requires 2^{24} models. And if the number of internal states, in- and outputs is not known to the experimenter, there are some 10^{155} possible models of that machine. And this number is *transcomputable* in the following sense: Hans BREMERMANN (1962) claimed that "[n]o data processing system, whether artificial or living, can process more than $2 \cdot 10^{47}$ bits per second per gram of its mass".[11]

Even if we consider the entire Earth in its over 4 billion years of existence as a computer, no more than 10^{93} bits could have been processed, the so-called *BREMERMANN's limit*.

These dimensions make it clear that one should not underestimate the complexity of systems with even simple structures. In artificial life, BRAITENBERG's (1984) famous vehicles perfectly illustrate this phenomenon that complex and hard-to-analyze behavior can be generated by simple rules. It also confirms the view that biological cognitive apparatus are not necessarily more complex than artificial ones.

Using the concept of BRAITENBERG bricks in a more abstract way, we may claim that the perceived world consists of numerous such entities which mutually interact without knowing the internal organization of each other. Let's think of a society where living and non-living entities form a web of interdependencies. Such a web must be maintained and controlled in one way or the other. Among others, POPPER (1961) advocated the idea of piecemeal social engineering, namely the idea to utilize science as a tool for political reform. The following example shows that such a program piecemeal engineering is hopelessly inadequate.

Complex Problem Solving—An Example

Years before "SimCity" became a popular game, Diettrich DÖRNER used simulation to scientifically investigate the problem of social and economic engineering. DÖRNER et al. (1983) created "Lohhausen", a computational simulation of a small city. Its economic situation is determined by the city-owned clock company, by a bank, shops, practices of physicians, and so on. 24 female and 24 male test subjects have to take the office of the city's mayor for a total of 120 (simulated) months. Since the clock company is publicly owned, the mayor is able to massively influence the economy of the city. Due to a large variety of parameters, like the freedom to arbitrarily set the level of tax, the test subjects had more freedom than in a real situations (DÖRNER 1989, FUNKE 1986). To measure the effectiveness of the virtual mayor, a set of parameters was defined, such as the "satisfaction" (i.e., the weighted sum of single aspects of living comfort) and size of the population, the financial situation of city, company productivity (in terms of sales and back orders), the income of the bank, the average standard of living, the number of unemployed and homeless people, the use of energy, etc.

In summary, Lohhausen pointed out several weak points of human problem solvers who face complex systems. It's interesting to note that these "flaws" are similar to those of the robots in DENNETT's illustration of the frame problem. The test subjects were likely to fail because they did not carefully analyze the current situation. Rather, they referred to a kind of "intuitive" interpretation of the state. They also tended to neglect side-effects and future long-term impacts. The test subjects thus treated the complex net of interdependencies among variables as simple linear accumulation of facts. Even worse, the virtual mayors tended to focus on a single core variable which then became the starting point for a long chain of causal connections. Such strategies reduce cognitive efforts and allow the outline of a clearly defined goal which is inevitably linked to the improvement of that core variable. They provide the illusion that the system is controllable and make it easy to forget feedback mechanisms.

Lohhausen was not only a prototype for a new type of experiment within cognitive psychology. It was also a pleading against the analytic method of traditional analytic science. The investigation of highly interconnected components of a complex system—and sciences are increasingly face such systems—by selecting a few variables is insufficient, but this is all what human problem solvers can do.

Many scientists, especially positivists, may reject the significance of such simulated worlds. Rather, they emphasize that our scientific knowledge comes exclusively from Nature, which a fancy simulation program will never be able to represent. This perspective is true to the extent that indeed the relationship between a simulation and the "natural" phenomenon with which it is associated remains unclear. However, the crucial point is: What is the "nature" of Nature? How can one claim that there is a fundamental gap between the qualities of a simulation and the qualities of Nature. In other words, where does the knowledge in (natural) sciences come from?

Where does scientific Information and knowledge come from?

In his otherwise quite comprehensive treatise on science, Atlee JACKSON (1995, 1996) pointed out that there are solely three different approaches to scientific information:

- Physical observations
- Mathematical models
- Computational explorations

By proposing this list, JACKSON seems to confuse apples with pears. Humberto MATURANA (1978) very clearly outlines the steps of the traditional scientific methods. He distinguishes four cyclic steps:

1. Observation of a phenomenon that, henceforth, is taken as a problem to be explained.
2. Proposition of an explanatory hypothesis in the form of a deterministic system that can generate a phenomenon isomorphic with the one observed (or internal model, as will be outlined in the next section).
3. Proposition of a computed state or process in the system specified by the hypothesis as a predicted phenomenon to be observed.
4. Observation of the predicted phenomenon.

Hence, physical observations refer to the process of gathering data in order to build up an internal model. They are not a model themselves and thus are not a source of information. Observations without a model do not make sense. Rather, they are necessary for a model to fit the "facts".

In addition, JACKSON missed another source of information: Scientific literature. As already pointed out in the previous section, only if we are able to "atomize" a chapter of scientific discovery into a single "fact", can we build up a hierarchical knowledge system. This is in fact the great strength of the scientific method: It first requires one to investigate the observed phenomena and then to make the results available to others. In this sense I speak of "atomization", of condensing the results of often several years of research into chunks upon which further research can be carried out without the necessity to repeat the previous experiments.

Furthermore, JACKSON's use of language is misleading for several reasons

- It suggests that only physical models are observations, i.e., they have an exclusive option on discovering "reality".
- Only through a formal mathematical approach we can establish scientific models.
- Computation may be another source but it plays the role of a scout who explores the unknown before civilization, i.e., mathematics and physics, dare moving in to this area.

JACKSON makes this fundamental distinction explicit when he notes that these source are fundamentally different. For the following reason this distinction is more of an obstacle than helpful. Computer models are just as good as mathematical models. Any formal logical-mathematical model can be fully mapped onto a computational system. This equivalency is basically what TURING showed in 1936. Both the mathematical and the computational approach are capable of serving as a model. The only difference is that they use different notations and therefore different deductive mechanisms.

Despite this fundamental equivalence, computational models are not fully accepted as information sources. Critics of the computational philosophy of science movement disqualify such models as fancy calculators (GLYMOUR 1993). HORGAN (1995, 1996) even calls such approaches "ironic science" which has no practical use. Either mathematical and computational models both are valid instruments for science or neither of them. It all depends on what we expect the role of a model to be.

What is the very nature of a model in general?

John HOLLAND et al. (1986) and Brian ARTHUR (1994) outline the importance of models as temporary internal constructs. They are constructs in that we build them inside our minds on the basis of experience. They are temporary since they are exposed to continuous modifications. This pragmatic model concept can be outlined (and extended) as follows:

1. In order to cope with an (apparently) complex problem we create a model. Such a model may for example consist of schemata (in the psychological sense), i.e., if–then rules. This is the root of scientific abstraction: we subsume a certain contextual configuration in the if part of such a schema and associate it with an expectation or action on the right side, the then part. It is important to note that in general neither guidelines are given of how to choose the appropriate level of abstraction nor what expectations or actions to associate with a particular if.
2. We have seen that the human mind is subject to several serious restrictions, such as the problem of correct deductions in large systems, e.g., when ruling a city as the example of Lohhausen has shown. We are simply unable to concurrently focus on more than one chain of inference. Fortunately, one feature of our internal models is that it allows for simple deductions as compared to its model, the "real world"
3. As a next step we act upon the result of these deductions.
4. If our actions are successful and our expectations associated with the then part are fulfilled we are likely to keep our mental model and think of it as a "representation of the world". Otherwise, we may modify the set of rules, add new rules in order to cover new contexts, or delete obsolete rules or those which have been proven false (in the sense of Popper).

In other words: "[W]e use simple models to fill the gaps in our understanding ... This type of [inductive] behavior... enables us to deal with complication: we construct plausible, simpler models that we can cope with." (ARTHUR 1994, p407)

This characterization of models not only resembles the notion of scientific hypothesis, it also clearly states that any act of thinking is based on such models. Some of them might be quite simple, others more sophisticated with regard to the number of schemata involved. As a consequence, not only scientific knowledge is formulated this way, but also our "knowledge about the world". Ultimately, this leads to the picture that when comparing a mathematical or computational model with Nature, we in fact compare two models with each other: the mathematical/computational one with our Nature model we have been constructing all our life. The roots of the latter can be found in our childhood. Since this period is no longer accessible by introspective reflection, we tend to assign an objective ontology to our well-developed model of Nature (cf. VON GLASERSFELD 1987).

Due to this relativist (or constructivist) position models are what Erwin SCHRÖDINGER (1961/64) originally assigned to metaphysics: scaffolds for our thinking, and, consequently, scaffolds of the scientific building.

Models as scaffolds of thinking

From a psychological point of view, there is no difference between scientific and nonscientific thinking. "Scientific Thinking... depends on the same general cognitive process which underlie nonscientific thinking" (FREEDMAN 1997, p3) Therefore, one should expect that our mind in general works like the scientific method commands.

Indeed, SJÖLANDER (1995) proposes an alternative perspective on thinking. In his view, mind actually generates hypotheses in order to make sense of perception. As long as the internal hypothesis is able to let perceptions fit in, we will keep that hypothesis rather than thinking of alternatives[12]. Despite the simple structure of such internal models, they sufficiently abstract from the perceived "real world" in the sense that they allow for successful anticipations. Thus, phrases in oral speech like "I want to draw your attention to..." are obviously referring to the fact that we need to build a "good" internal model if we want to understand another person. In other words, we need the opportunity to build (implicit) anticipations about what is to come[13]. SJÖLANDER illustrates this with an example from biology: A dog hunting a hare "...does not need a full picture of a recognizable hare all the time to conduct a successful hunt. It is able to proceed anyway, guided by glimpses of parts of the hare, by movements in vegetation, by sounds, by smell, etc. If the hare disappears behind a bush or in a ditch the dog can predict the future location of the hare by anticipating where it is going to turn up next time, basing this prediction on the direction and the speed the hare had when seen last." (p2)

The need of internal models upon which we can draw conclusions (the "innere Probierbühne" with the words of SJÖLANDER) becomes even more clear if we investigate the "world" of people who have a reduced spectrum of perception, e.g., blind people. Oliver SACKS (1995) describes the case of man, Virgil, who had been blind since early childhood. At the age of fifty his eye sight was restored. Contrary to the general expectation, this was no help for Virgil since the way he has been living as a blind person was incompatible with the way normal sighted people perceive and organize their world view. With effort and practice, he was able to interpret some of the visual data in terms of the world as he had known it through his other senses, but he has immense difficulty in learning these interpretations. For instance, visually he cannot tell his dog from his cat. For him, due to the lack of visual impressions, the temporal aspect of his world had priority. He recognized things by feeling their surface in a particular order. He didn't get lost in his own apartment because he knew that after entering there was furniture in a particular sequence which he perceived in a temporal order. To put it differently, he was living in world of anticipation. A particular cupboard was followed by a table, so once he reached the cupboard he anticipated reaching the table with the next step.

Having this relativist but nevertheless powerful concept of models in mind we may now turn to a final view on the relationship between models and "reality".

Models and "reality"

HORGAN (1995) quotes Jack COWAN, according to whom "chaoplexologists" suffer from the reminiscence syndrome: "They say, 'Look, isn't this reminiscent of a biological or physical phenomenon!' They jump in right away as if it's a decent model for the phenomenon, and usually of course it's just got some accidental features that make it look like something." (p74)

This syndrome resembles the old philosophical conundrum of how to know that a model of a natural system and the system itself bear any relation to each other. How can a deductive operating system, such as mathematics, allow for building bridges and flying to the moon?[14]

First, it is useless to speak of "the system itself" because we cannot make statements about that system outside the framework of science without violating the scientific imperatives. But describing the system with the methods of science is exactly what we want to do. We thus cannot anticipate the result of our inquiry (cf. VON GLASERSFELD 1987).

Second, what we actually do by building a model is to install a second source of information, namely the model itself. Originally, we wanted to investigate the observed system but due to its complexity and/or hidden features we are neither able to sufficiently explain the historical behavior nor to anticipate the future behavior. Thus we build a simplified analogy which we hope exhibits similar or identical behavior. In order to gain maximum security we apply our set of scientific methods. Of course, this is only relative security, as POPPER already pointed out several decades ago: he argued against the idea that the inductive principle of verification could ever lead to secure knowledge. He was, however, not aware that his falsification imperative cannot yield a secure knowledge either. One can never be sure whether he or she actually included all explanatory components that show that a theory is definitely wrong (cf. the example in LAKATOS 1970). DENNETT's example, well-known in the artificial intelligence community, demonstrates that any effort to determine all relevant factors is a non-practical enterprise. We need not even to refer to GÖDEL's Incompleteness Theorem to find scientific reasoning restricted within the vast complexity of combinatorics. It is appropriate to state that from an epistemological point of view such a situation is highly unsatisfying. On the contrary, we—like the robot in DENNETT's example—cannot spend almost endless time on building science by taking all possible (borderline) cases into consideration. Fortunately, from a pragmatic perspective, the scientific method—mainly based on the reproducibility of experiments—enables to build sufficiently reliable models and artifacts.

Before I investigate the limits of internal models, I first want to provide arguments as to why narrative descriptions in natural language can be considered as models, in order to underline the basic claim of fundamental equivalence of all sources of scientific knowledge.

Models in natural language

In a nutshell, natural language may serve as a basis for internal models in the above sense, since

- language is constructed by humans;
- one can carry out deductions from statements without being "grounded" (in the sense of HARNAD 1990);
- the correspondence to the "real" world is arbitrary (from a general (i.e., population) point of view; for individuals, it has communal character).

A theory merely formulated in everyday language may also serve as a model for science. In contrast to a formal mathematical or computational model it has neither clearly defined entities nor clear rules. Referring to VARELA (1990, p95), where the author compares the crystal-clear world of chess with the world of a car-driver, a scientific model built in natural language is potentially more complex than a formal model: states and rules are ambiguous and thus cannot be easily handled by the human mind. (Cf. the psychological findings on the performance of humans for Tower of Hanoi). In addition, the distinction between natural language models and mathematical models mirrors the superiority of the scientific method over an everyday explanatory approach since it makes use of crystal-clear and therefore more "debuggable" (in the sense of falsifiable) structures.

A prominent problem in philosophy addresses the issue of genuine no-go areas (STEWART 1997): One can propose scientific questions which are not solvable. Examples are time travel, the intention to go north of the North Pole while staying on the surface of the Earth, speaking about the time before Big Bang (which originated time), and perhaps the current search for a General Unified Theory. At first glance, these are questions about something that obviously does not exist. But within the framework I outlined so far such questions are examples of the very nature of language as a model. Again, no statement in natural language actually describes something. Rather, it is a model to which we seek correspondence in the set of phenomena we perceive. As has already been acknowledged by many linguists (e.g., LENNEBERG et al. 1967), language is a very powerful mechanism in that it can create patterns of arbitrary length and recursivity. Therefore, any natural language model (as well as questions that arises from such models) can be arbitrarily long and recursive. The only constraints arise in the process of synchronization within a community, e.g., a scientific community where a certain set of questions is simply ignored.

The arbitrary correspondence to a "real" world is also the place where the "symbol grounding" problem (HARNAD 1990) is located. It arises from the fact that formal computations (according to the Physical Symbol System Hypothesis of NEWELL/SIMON 1972) are the manipulation of symbols devoid of meaning. In his paper, HARNAD asks: "How can the semantic interpretation of a formal symbol system be made intrinsic to the system, rather than just parasitic on the meanings in our heads? … The problem is analogous to trying to learn Chinese from a Chinese/Chinese dictionary alone." (p335)

From a realist point of view it would be desirable for symbols to indeed have a semantic content. It is true that the realist position distinguishes between computational tokens, which may be meaningless symbols, and the representation per se.[15] But as FRANKLIN (1995) notes, things do not come labeled. This constructivist statement is indeed the crucial point: Symbols receive their meaning through projection of an observer, through his or her interpretation.

This instrumentalist point of view emphasizes the notion of a knowledge that fits observations, or, as VON GLASERSFELD (1990) puts it, "It is knowledge that human reason derives from experience. It does not represent a picture of the real world but provides structure and organization to experience". Searching the correspondence between an internal model and the world which is experienced as the "outside world" is like the relationship between a key and a lock. Many keys open a lock. VON GLASERSFELD (1984) speaks of the crucial distinction between *match* and *fit*: The fact that we can open a lock with a key does not tell us anything about the structure of the lock. It merely shows that the key is viable. In the same sense we can interpret physical observations.

Where do these interpretations originate? In the above argumentative framework, the notion of reality and knowledge are subject to relativism. But how can an individual get to know these ideas of an absolute truth? In accordance with Ernst VON GLASERSFELD (1982, p629), the process can be outlined as follows: First, the active individual organizes his or her sensorimotor experiences by way of building action schemata. Only those schemata are maintained which yield an equilibrium or help to defend it against perturbations. Second, these operational structures are abstracted from the sensorimotor "content" which originally gave rise to their creation. Consequently, they are ascribed to things and thus "externalized". Continuously viable ascriptions yield a belief in their independent existence and henceforth a belief in an objective truth. In other words, the individual established an internal model upon which he or she can carry out deductions "in an atmosphere of security" since such deductions strictly follow a logical–mathematical calculus.

Limitations of model-building are the limits of human sciences

Whatever approach we choose—the natural language model, the formal-mathematical or the computational model—we end up with a simplification in our mind. We draw deductions and conclusions upon this abstraction. Then we seek to fit (in the sense of VON GLASERSFELD) the results with the "outer world". In the case of natural language models, these deductions are traditional views of discourse, which require rhetoric abilities. In the case of mathematical models, we find the

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classical tools of strictly defined logical rules. Finally, in computational models, we externalize deductions in the sense that we compute them in artifacts rather than in our own brains. Is this already a first sign of future developments where more and more parts of scientific reasoning will be shifted to automata? Gain for speed may only be one advantage of this "takeover". The other advantage is the possibility to overcome the shortcomings of deduction (as shown in the case of Lohhausen and the Towers of Hanoi).

Fortunately, to give an outlook of the computational science as anticipated in this paper, making use of models can be formulated algorithmically (cf. HOLLAND et al. 1996 and RIEGLER 1997 for examples). Since the pragmatic perspective of science also does not provide mapping-rules between a model and the experienced reality, such scientific machines may gain true intellectual independence. This means that in contrast to artificial intelligence programs whose input is fed by humans and whose computational output is interpreted by humans, scientifically reasoning devices will develop their own interpretation of perceived data.

Conclusion

The recent *End of Science* affair triggered by John HORGAN reminds us that we have to seriously think about the possibility that the *progress* in *human* science will decay and finally arrive at a *cognitive* barrier. In contrast to HORGAN's romantic view of science, according to which we have to seek for *The Truth*, the matter of science is not the reality. Rather, it consists of fairly sophisticated scaffolds which both permit predictions and create meanings.

In their analysis of the limits to scientific knowledge, philosophers tend to forget that science is carried out by human beings who are anything but infallible machines. Hence, it pays to look at the cognitive limits rather than at the theoretical limits of disciplines such as the applicability of GÖDEL's Theorem to physics and to the philosophy of mind. Like it is impossible to build infinitely high scaffolds, we cannot manage infinitely large cognitive scaffolds. The conclusion of an end of *human* science thus neither repeats previous we-already-know-everything arguments nor forgets the merits of what we have achieved so far. And, fortunately, it gives hope that a possible *trans*-science, carried out by computational devices, will at least preserve the powerful feature of predicting.

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Notes

1 Since philosophy of science can potentially be an endless discourse of arguments referring recursively to each other, I will apply OCCAM's Razor in order to not get lost in a "jungle" of arguments in favor of concentrating on the essential issues. However, when it becomes necessary, I will refer to more details, such as findings from psychology.

2 Horgan earned many critics, among whom are ANGIER (1996), CASTI (1996a, 1996b), HAYES (1996), MITCHELL (1995), SILBER (1996), and STEWART (1997)

3 His main argument is the apparent paradoxical situation in which he fancies such perspectives, i.e., the self-applicability of a meta-science. "Is falsificationism falsifiable?", he asked Karl POPPER in one of the numerous interviews which make up his book.

4 But this, of course, does not sound as dramatic as the title he actually chose.

5 Relating Pierre TEILHARD DE CHARDIN's (1966) concept of "Noosphere" to the present World Wide Web is certainly of historical and philosophical interest in that it demonstrates that the idea of a global net is certainly not a product of the most recent decades. Nevertheless, a mere discussion of the possibility of such a net does not create the net. But now since it is existent we can prove earlier predictions of former thinkers.

6 As already pointed out by several authors before me (most prominently by MASTERMAN 1978), KUHN did not provide a strict definition of a paradigm. I do not think that such a definition is possible, since it would require exhaustively including psychological and sociological aspects of individuals. I therefore would like to define a paradigm as the *implicitly* known set of standard procedures of how to perceive and investigate a problem. Since perception is selective, problems may stay invisible.

7 By "problem space" I refer to the *n*-dimensional abstract space set up by the *n* variables that characterize a problem. Most likely, not all these variables are visible within a current paradigm. Therefore, the current paradigm is a subspace (with lower dimensionality) of the entire problem space. Problem solving is moving in the problem space by varying one or more variables concurrently.

8 The notion of a search tree refers to the graph in the *n*-dimensional search space whose knots are the decision points.

9 Wolfgang STEGMÜLLER (1971) finds even harder words for this dogmatism. He writes that we should feel sorry for the average scientist since he or she is a uncritical, narrow-minded dogmatist who wants to educated students in the same way.

10 This psychological finding resembles the philosophy of Martin HEIDEGGER. See DREYFUS (1991) for an overview.

11 BREMERMANN calculated this number by evaluating the maximum possible energy content within a gram of mass.

12 Cf. also the example of the mermaid by von GLASERSFELD 1983, p54: Somebody changes the subjective interpretation of an expression only if some context forces him or her to do so.

13 In my functional model of a cognitive apparatus (1997) I take advantage of this "constructivist-anticipatory" principle: Behavior of cognitive creatures is controlled by schemata which, once invoked, ask for sensory or internal data *only* when they need them. In other words, the algorithm neglects environmental events *except* for the demands of the current action pattern. The algorithm leads to a significant decrease in performance costs since the simulation algorithm need not provide the full environmental information to the agent at every time step. This is in contrast to the information-processing paradigm that defines the cognitive system as a bottleneck. The essential features must be selected among the wealth of "information" is provided by the "outside" in order to decrease the enormous amount of complexity.

14 For the relationship between mathematics and physics in particular see, for example, WIGNER (1960).

15 The hope of the artificial intelligence community is therefore that a formal model containing meaningless computational tokens need not necessarily imply a meaningless representation of the system.

References

Angier, N. (1996) The Job Is Finished. The New York Times Book Review, June 30 1996: 11–12.

Appel, K./Haken, W. (1977) The solution of the four-color-map problem. Scientific American October: 108–121.

Arthur, W. B. (1993) Why Do Things Become More Complex? Scientific American May 1993: 92.

Arthur, W. B. (1994) Inductive Reasoning and Bounded Rationality. American Economic Review 84: 406–411.

Ashby, W. R. (1973) Some peculiarities of complex systems. Cybernetic Medicine 9: 1–7.

Braitenberg, V. (1984) Vehicles: experiments in synthetic psychology. MIT Press: Cambridge.

Bremermann, H. J. (1962) Optimization through evolution and recombination. In: Yovits, M. C. et al. (eds) Self-organizing systems. Spartan Books: Washington.

Cambell, D. T. (1974) Evolutionary epistemology. In: Schilpp, P. A. (ed) The Philosophy of Karl Popper. Open Court: La Salle.

Casti, J. L. (1996a) Confronting Science's Logical Limits. Scientific American, October 96: 78–81.

Casti, J. L. (1996b) Lighter than air. Nature 382: 769–770.

Davies, P. C. W. (1990) Why is the universe knowable? In: Mickens, R. E. (ed) Mathematics and Science. World Scientific Press: Singapore.

Dennett, D. C. (1984) Cognitive Wheels: The Frame Problem of AI. In: C. Hookway (ed) Minds, Machines, and Evolution: Philosophical Studies. Cambridge University Press: London.

Diettrich, O. (1995) A Constructivist Approach to the Problem of Induction. Evolution & Cognition 1 (2): 11–29.

Dörner, D. (1989) Die Logik des Mißlingens. Rowohlt: Reinbeck bei Hamburg.

Dörner, D. et al. (ed) (1983) Lohhausen: Vom Umgang mit Unbestimmtheit und Komplexität. Hans Huber: Bern.

Dreyfus, H. L. (1991) Being-in-the-World. A Commentary on Division I of Heidegger's Being and Time. MIT Press: Cambridge.

Duncker, K. (1935) Zur Psychologie des produktiven Denkens. Berlin: Springer. Translated: (1945) On Problem Solving. Psychological Monographs 58 (270), 1–112.

Faust, D. (1984) The Limits of Scientific Reasoning. University of Minnesota Press: Minneapolis.

Feyerabend, P. K. (1975) Against method: Outline of an anarchistic theory of knowledge. NLB: London.

Foerster, H. v. (1985) Entdecken oder Erfinden. In: Mohler, A. (ed) Einführung in den Konstruktivismus. Oldenbourg: München.

Foerster, H. v. (1990) Kausalität, Unordnung, Selbstorganisation. In: Kratky, K. W./Wallner, F. (eds) Grundprinzipien der Selbstorganisation. Wiss. Buchgesellschaft: Darmstadt.

Fraassen, B. C. v. (1980) The Scientific Image. Oxford University Press: Oxford.

Franklin, S. (1985) Artificial Minds. MIT Press: Cambridge.

Freedman, E. G. (1997) Understanding scientific discourse: A strong programme for the cognitive psychology of science. Theory and Review in Psychology. http://www.gemstate.net/susan/Eric.htm

Funke, J. (1986) Komplexes Problemlösen. Springer-Verlag: Berlin, Heidelberg.

Giere, R. N. (1993) Cognitive Models of Science. University of Minnesota Press: Minneapolis..

Glasersfeld, E. v. (1982) An Interpretation of Piaget's Constructivism. Revue Internationale de Philosophie 36 (4): 612–635.

Glasersfeld, E. von (1983) Learning as a constructive activity. In Bergeron, J. C./N. Herscovics, N. (eds) Proceedings of the Fifth Annual Meeting of the North American Chapter of the International Group for the Psychology of Mathematics Education. University of Montreal: Montreal, pp. 41–69.

Glasersfeld, E. v. (1984) An Introduction to Radical Constructivism. In: Watzlawick, P. (ed) The Invented Reality. W. W. Norton: New York.

Glasersfeld, E. v. (1987) Wissen, Sprache und Wirklichkeit: Arbeiten zum radikalen Konstruktivismus. Braunschweig: Vieweg.

Glasersfeld, E. v. (1990) An Exposition of Constructivism: Why Some Like It Radical. In: Davis, R. B./Maher, C. A./ Noddings, N. (eds) Constructivist Views On The Teaching and Learning of Mathematics JRME Monograph 4. National Council of Teachers of Mathematics: Reston.

Glymour, C. (1993) Invasion of the Mind Snatchers. In: Giere, R. N. (ed) Cognitive Models of Science. University of Minnesota Press: Minneapolis.

Gould, S. J./Eldridge, N. (1977) Punctuated equilibria: the tempo and mode of evolution reconsidered. Paleobiology 3: 115–151.

Gomory, R. E. (1995) The Known, the Unknown and the Unknowable. Scientific American, June 95: 88.

Grassberger, P. (1986) Towards a quantative theory of self-generated complexity. International Journal of Theoretical Physics, 25(9): 907–938.

Harnad, S. (1990) The symbol-grounding problem. Physica D 42 (1–3): 335–346.

Hartwell, A. (1995) Scientific Ideas and Education in the 21st Century. Inst. for International Research: Washington D. C.

Hayes, B. (1996) The End of Science Writing? American Scientist 84 (5): 495-496.

Heylighen, F./Aerts, D. (eds) (1998) The Evolution of Complexity. Kluwer: Dordrecht.

Holland, J. H./Holyoak, K. J./Nisbett, R. E./Thagard, P. R. (1986) Induction: Processes of Inference, Learning and Discovery. MIT Press: Cambridge, London.

Homer-Dixon, T. (1995) The Ingenuity Gap: Can Poor Countries Adapt to Resource Scarcity? Population and Development Review 21 (3): 587–612.

Horgan, J. (1995) From Complexity to Perplexity. Scientific American 272: 74–79.

Horgan, J. (1996) The End of Science. Facing the Limits of Knowledge in the Twilight of the Scientific Age. Addison-Wesley: Reading.

Jackson, E. A. (1995) No Provable Limits to 'Scientific Knowledge'. Complexity 1 (2): 14–17.

Jackson, E. A. (1996) The Second Metamorphosis of Science: A Second View. Working Paper 96-05-039, Santa Fe Institute: New Mexico.

Kolen, J. F./Pollack, J. B. (1983) The Observers' Paradox: Apparent Computational Complexity in Physical Systems. The Journal of Experimental and Theoretical Artificial Intelligence 7: 253-277.

Kuhn, T. S. (1962) The Structure of Scientific Revolutions. University of Chicago Press: Chicago.

Lakatos, I. (1970) Falsification and the Methodology of Scientific Research Programmes. In: Lakatos, I./Musgrave, A. (ed) Criticism and the Growth of Knowledge. Cambridge University Press: London.

Langley, P./Simon, H. A./Bradshaw, G. L./Zytkow, J. M. (1987) Scientific Discovery: Computational Explorations of the Creative Processes. MIT Press.

Laudan, L. (1977) Progress and its Problems: Towards a Theory of Scientific Growth. Routledge & Kegan Paul: London.

Lenneberg, E. H./Chomsky, N./Marx, O. (1967) Biological foundations of language. Wiley: New York.

Lorenz, K. (1973) Die Rückseite des Spiegels. Versuch einer Naturgeschichte menschlichen Erkennens. Piper: München, Zürich. Translated: (1977) Behind the Mirror. Harcourt Brace Jovanovich: New York.

Luchins, A. S. (1942) Mechanization in Problem Solving. In: Psychological Monographs 54/248.

Masterman, M. (1970) The Nature of a Paradigm. In: Lakatos, I./Musgrave, A. (ed) Criticism and the Growth of Knowledge. Cambridge University Press: London.

Maturana, H. R. (1978) Biology of Language. In: Miller, G. A./ Lenneberg, E. (eds) Psychology and Biology of Language and Thought. Academic Press: New York.

McGinn, C. (1994) The Problem of Philosophy. Philosophical Studies 76: 133–156.

Miller, G. A. (1956) The magical number seven, plus or minus two: Some limits on our capacity for processing information. Psychological Review 63: 81–97.

Mitchell, M. (1995) Complexity and the Future of Science. Announced for publication in Scientific American. http://www.santafe.edu/~mm/sciam-essay.ps

Nersessian, N. J. (ed) (1987) The Process of Science. Martinus Nijhoff: Dordrecht, Boston, Lancaster.

Newell, A./Simon, H. (1972) Human Problem Solving. Prentice-Hall: Englewood Cliffs.

Oeser, E. (1984) The evolution of scientific method. In: Wuketits, F. M. (ed) Concepts and approaches in evolutionary epistemology. Reidel: Dordrecht.

Ortega y Gasset, J. (1994) The revolt of the masses. W. W. Norton: New York. Spanish original: (1929) La rebelión de las masas. Revista de Occidente: Madrid.

Popper, K. (1934) The Logic of Scientific Discovery. Springer: Berlin.

Popper, K. R. (1961) The Poverty of Historicism. Routledge & Kegan Paul: London.

Riedl, R. (1977) A systems-analytical approach to macro-evolutionary phenomena. Quart. Rev. Biol. 52: 351–370.

Riedl, R. (1981) Biologie der Erkenntnis. Die stammesgeschichtlichen Grundlagen der Vernunft. Parey: Hamburg, Berlin. Translated: (1984) Biology of Knowledge. The Evolutionary Basis of Reason. John Wiley & Sons: Chichester.

Riedl, R. (1983) Die Spaltung des Weltbildes. Parey: Hamburg, Berlin.

Riedl, R./Ackermann, G./Huber, L. (1992) A ratiomorphic problem solving strategy. Evolution & Cognition 2: 23–61 (old series).

Riegler, A. (1994) Constructivist Artificial Life. PhD Thesis at the Vienna University of Technology.

Riegler, A. (1997) Ein kybernetisch-konstruktivistisches Modell der Kognition. In: Müller, A./Müller, K. H./Stadler, F. (eds) Konstruktivismus und Kognitionswissenschaft. Kulturelle Wurzeln und Ergebnisse. Wien, New York: Springer.

Schilpp, P. A. (1963) The philosophy of Rudolf Carnap. Open Court: La Salle.

Schrödinger, E. (1961) Meine Weltansicht. Zsolnay: Hamburg. Translated: (1964) My view of the world. Univ. Press: Cambridge.

Silber, K. (1996) Goodbye, Einstein. Reason 28 (5): 61.

Simon, H. A. (1975) The functional equivalent of problem-solving skills. Cognitive Psychology 7: 268–288.

Sjölander, S. (1995) Some cognitive breakthroughs in the evolution of cognition and consciousness, and their impact on the biology of language. Evolution and Cognition 1 (1): 2–11.

Stegmüller, W. (1971) Hauptströmungen der Gegenwartsphilosophie, Band II. Kröner: Stuttgart.

Stent, G. (1978) Paradoxes of Progress. W. H. Freeman: San Francisco.

Stewart, I. (1997) Crashing the barriers. New Scientist, March 97: 40–43.

Teilhard de Chardin, P. (1966) The Vision of the Past. Harper & Row: New York.

Thagard, P. (1988) Computational Philosophy of Science. MIT Press: Cambridge.

Varela, F. (1990) Kognitionswissenschaft – Kognitionstechnik: Eine Skizze aktueller Perspektiven. Suhrkamp: Frankfurt/M.

Waldrop, M. M. (1992) Complexity: the emerging science at the edge of chaos. Simon & Schuster: New York.

Wicklegren, W. A. (1974) Single-trace fragility theory of memory dynamics. Memory and Cognition 2: 775–780.

Wigner, E. P. (1960) The Unreasonable Effectiveness of Mathematics in the Natural Sciences. Communications on Pure and Applied Mathematics 13: 1–14.