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Autor(en): **Mohr, Hans**

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The Significance of the Experiment in the Scientific Approach

Hans Mohr

Reliable Knowledge

The goal of science is reliable knowledge (Ziman 1979). Science is an organized venture of the human mind that aims at reliable knowledge.
Knowledge is reliable if it successfully serves as a basis for action. Reliability becomes obvious in the precision of explanation, in making correct predictions and - in particular - in solving problems. Scientific knowledge is superior to common sense knowledge in being more reliable and more comprehensive.
The prestige of the natural sciences in the public has been based on their power to solve problems on the basis of reliable knowledge. Social sciences have so far not achieved the status of reliable prophet, policy maker, social engineer or therapist (Ziman 1979). Social sciences' continuing failures are, in part at least, the root causes of science's credibility gap under which we suffer at present.
My talk will deliberately be restricted to the natural sciences, physics and biology, which can be considered, from the point of view of epistemology, as a unit.

Logic of Reserarch

'Research' means to obtain reliable knowledge. The logic of research - as I understand it - is concerned with the question of how reliable knowledge is in fact obtained. It seems that a satisfactory answer to this question is beyond the scope of the human mind. As Robert Lindsay (1979) said recently "Man somehow is just sufficiently competent to produce - science and art, but not sufficiently so ever consciously to explain how he does it". My own understanding of what scientific knowledge is and the way it is in fact acquired is based mainly on my own experience as a practicing scientist rather than on the many, largely normative descriptions found in the literature of the philosophy of science.
First of all a sceptical remark: Is reflection about the logics of research really worthwhile for a scientist? What can we expect from this kind of analysis? Can we expect normative insights? Or will our effort lead at best to a post factum reconstruction of the scientific way of thinking? Can we become more conscious, more reliable, and thus better and even more successful scientists? Or is it only that we try to make explicit what every experienced scientist 'knows' anyhow?
How do research scientists go about obtaining knowledge? How should they? Today's scientists tend not to be introspective about these questions. During their apprenticeship, they somehow absorb the necessary pragmatic knowledge and then go about their business. Nevertheless science has advanced, and the fact that a tremendous wealth of genuine knowledge has been accumulated by innocent scientists who did not pay much attention to the theoretical foundations of science is beyond question.
My problem today is to evaluate the significance of the experiment within the process of knowledge acquisition. It is generally stated that observation and experiment are the means of verification of scientific theories. What does this mean in practice?

The Process of Research (fig. 1)

Our understanding of the real world has two roots: perception, sensory impressions, and genetically inherited foreknowledge about the structure of the world. From the point of view of the individual this foreknowledge has the epistemological character of synthet-

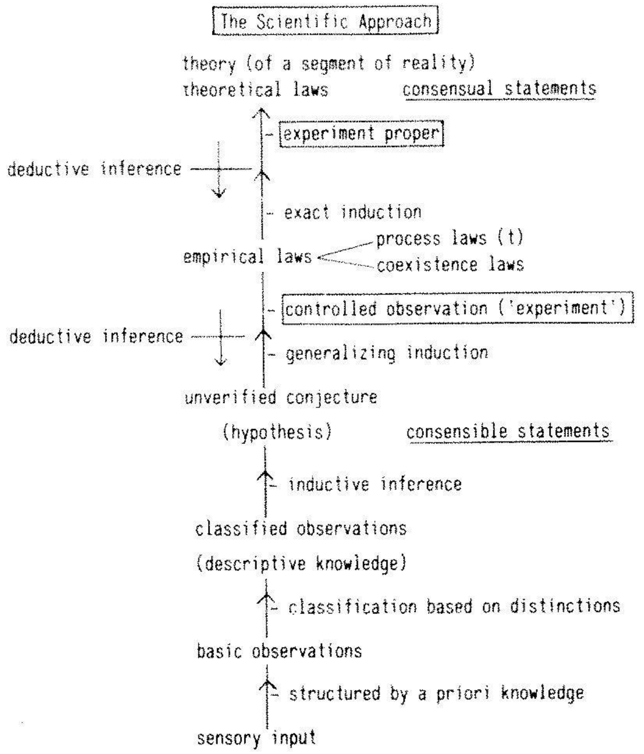

Fig. 1. The main stations of the process of research (to be explained in the present essay).

ic judgements a priori; from the point of view of evolution, however, the same knowledge must be considered as synthetic judgements a posteriori, based on experience. This is, briefly, the major thesis of evolutionary epistemology in which I firmly believe (Mohr 1977). Expressed in other words: There is some truth about the world which we can know independently of experience because we have inherited this truth from our ancestors with our genes. Kant knew precisely that we possess a priori knowledge about the world but he could not explain its origin. Kant had no access to the idea that a process we call "genetic evolution" must have necessarily accumulated experience over time. Kant could not envisage the idea that the seemingly inexplicable a priori knowledge of the individual is actually a posteriori knowledge of the human race about the world, laid down in the peculiar nucleotide sequence of the DNA in our genes.

The idea that our capacity to structure our sensory input depends on our having, in our brains, adequate models of the world, is not new, of course; however, evolutionary epistemology explains the origin of this capacity.

However, irrespective of the origin of our a priori knowledge the important point is that our sensory input is filtered and organized intellectually by our mind in accordance with some a priori knowledge about the structure and the logic of the real world.

The results are 'basic observations', constructs and relationships between constructs called 'patterns'. The process of research does not start with non-selected, random 'basic observations' but with 'classified observations' which refer to a particular segment of reality in which we are – for some reason – particularly interested. The process of classification is an artifice resorted to in order to simplify the approach to a given theoretical or practical problem by narrowing its broader elements and ramifications on the basis of largely unconscious distinctions. These include motives, feelings and values. Great care must be exercised in the identification and evaluation of these distinctions. Errors in classification of basic observations are a major source for failure in the further scientific approach. If our prejudice leads us to disregard important observations in the process of classification, these are usually lost for ever. Success in science is usually founded on adequate classification.

The next step – generalizing induction – (an example of inductive inference) is the core of the 'art of research'. It leads to a conjecture (or 'hypothesis') which is proposed in an effort to explain the set of classified observations.

The creation of a hypothesis, the consecutive application of deductive inference, inductive reasoning and experimental testing are not only limited by our intellectual abilities and technical skills but also by the paradigms in which we believe. This means that the range of conjectures and the range of experimental answers is usually determined by our scientific prejudice, by the paradigms in which we trust. This is a characteristic, as Thomas S. Kuhn (1970) has pointed out, of 'normal science'. Scientific revolutions, characterized by an exchange of paradigms, are rare, and most scientists are extremely reluctant to admit serious changes within the paradigmatic pattern to which they got used. Scientists tend to be conservative.

In the theory of science there are two ideal postulates which govern the development of an hypothesis:

1. The hypothesis must contain nothing inconsistent with known facts and principles.
2. The hypothesis must not contain postulates which are not subject to verification by empirical test.

Even though these postulates cannot be followed strictly in the scientific practice they serve as guiding principles at least within the realm of 'normal science'.

In particular, it is agreed upon that the statements of a hypothesis must be consensible, i.e. potentially verifiable or falsifiable by the members of a scientific community. Verification or falsification by empirical test is usually performed in – what I call – 'controlled observation'.

We must discriminate between two operations: Deductive inference and controlled observation. 'Deductive inference' means reasoning from general premises to specific derived instances thereof. In testing a hypothesis we usually proceed in the way: "If proposition p (an explicit proposition of the hypothesis) is true, then proposition q (a new derived proposition) is likewise true."

Whether proposition q is in fact true is usually tested in a controlled observation. In this operation the observer must be able to control intellectually and technically all aspects of the investigation and, in particular, exercise command over the variable factors involved. Most so-called 'experiments' in biology are controlled observations to test the validity of some derived proposition.

The actual process of experimentation cannot be described in general terms. The particular interplay between controlled observation, deductive inference and generalizing induction – always threatened by inconsistencies, internal contradictions and simple errors – depends on the particular experimental situation and it depends, of course, on the creativity and ingenuity of the person who does the experiment. For the novice, a general, trivial scheme of the experimental method is hardly helpful. In fact, one can learn how to experiment only in the continued cooperation with a scientific personality, with an experienced and successful experimentator, in the laboratory as well as at the desk.

In any case, successful interplay of inference and controlled observation to validate the propositions derived from the hypothesis stabilizes the original conjecture. A gauge for the fruitfulness of a conjecture is the formulation of empirical laws. Empirical laws – coexistence laws as well as process laws – are reliable general propositions obtained by repeated controlled observation and creative inductive inference. In most fields of biology empirical laws are the best that can be obtained at present. But even in physics empirical laws have been considered as the core of solid science. In fact, empirical laws have remained the reliable basis of large parts of physical and biological technology, including medicine and agriculture. However, empirical laws are still descriptive generalisations, and thus isolated statements not kept together by a unifying theory.

The next and final step of the scientific approach – exact induction – leads to a theory and thus to the formulation of a coherent framework of theoretical laws which permit an explanation of the empirical laws. It is the experimental test of the theoretical laws which has been considered by the philosophers of science as well as by the theoretical physicists as the decisive point where the experiment comes into play in the scientific approach.

The major difference compared to the previously discussed controlled observations leading to empirical laws is that single crucial experiments determine whether a theoretical law and sometimes even a whole theory can be maintained or must be abandoned, or at least seriously modified.

Even though Popper's (1959) view about falsification is far too rigorous compared with the reality of the scientific approach, it is obvious that a theory with a high claim becomes more vulnerable against incompatible observations. An example is the quantum theory of light (Walls 1979). The quantum theory of light, beginning with Planck and Einstein, played a central part in the development of quantum theory during this century. As the quantum theory was developed a sophisticated theory for the interaction of photons and electrons evolved, namely quantum electrodynamics. Amongst the derived propositions ('predictions') of quantum electrodynamics was that the emission of light from an atom would experience a small shift away from the resonance line of the atom. The experimental observation of

this shift, known as the Lamb shift, came as a major triumph for the quantum theory of light. A negative outcome would have very seriously affected the foundations of quantum electrodynamics.
However, the decision whether a theoretical law is true or not can also be made in an uncontrolled investigation. You are all aware of the importance of observations of stellar light during a total solar eclipse to test Einstein's General Theory of Relativity. According to this theory, a ray of light from an object beyond, passing close to a massive body, is slightly bent away from it. The eclipse of 21 September 1922 furnished an almost ideal opportunity since measurements could be made in the desert climate of Northern Australia for almost 6 min. A deflection of 1. ″72 ± 0. ″11 was obtained which is in excellent agreement with the theory (Olivier 1968).

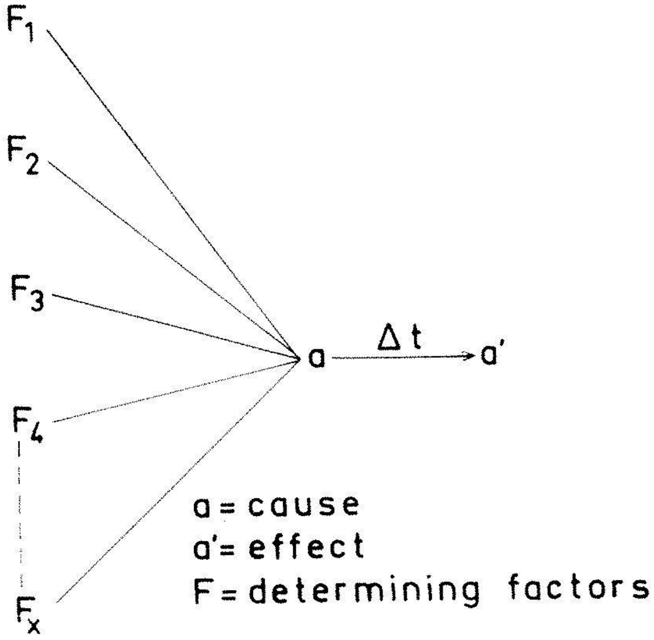

Fig. 2. Formulation of principle of causality to indicate use of this principle in 'causal' research in biology (after Mohr 1977).

Experimentation in Biology

Non-trivial experiments in Biology are difficult to perform. Why? Biological systems are by their very nature always complex, and useful experiments require the maintenance of the complexity, the intact system, whose properties we want to measure, e.g. a human body, a cell, a mitochondrion, a multienzyme complex. The limits of reductionism are obvious to the modern experimentator. With an isolated mitochondrial preparation you cannot expect to find the laws which govern the energy status of the cell. With a culture of human fibroblasts as an experimental material you cannot expect to discover the coexistence laws which govern the mechanical stability of the human body. With a plant cell suspension culture you cannot study the process laws which govern embryogenesis. In theoretically advanced physiology the tendency of the researcher is directed towards a conservation of a largely undisturbed system complexity. In vivo experiments are preferred, mostly performed as black-box-experiments in which an output of the system – the effect – is measured as a function of some input – called the variable factor while the boundary conditions are strictly defined and controlled. In modern physiology input and output are usually measured in physical, chemical or physico-chemical terms. Black-box-experiments in biology require a high degree of descriptive knowledge about the system in question and a clear notion of what *causality* means. The common sense view of causality "An identical cause produces an identical effect" no longer suffices and is in fact misleading. Sheer ignorance of the logical structure of 'causal' research in physiology has created much confusion, in particular in genetics and molecular biology.

Causality in Biology

Causal research in biology (fig. 2, 3) should actually be called (Mohr 1977) 'factor research' since we are not able to study the relationship between cause and event (effect) but only the relationship between variable factors and events. This is far more than a semantic problem. Formulation of the principle of causality requires the notion of determination and a time directedness. The principle can be formulated as an if-then proposition (fig. 2): If x distinct factors (F_1, F_2 ... F_x) determine the state a and if a' follows from a with time, then the general proposition is that if the state a (determined by x factors) is given (cause), the state a'

(effect) will always follow. A formulation of the principle of causality as a negation is: There are no indeterminate events.
In 'causal' research we cannot do more than to vary one, two, or (rarely) more factors in an experiment and to record the effect. Those of the x factors that we vary in the experiment are called variable factors or briefly 'variables'.
In an attempt to illustrate the formal model of the principle of causality we consider the case of a single factor analysis in genetics. To characterize living systems, the scientist uses traits (characteristics, characters). These terms designate such properties or abilities of living systems that can be measured. The sum of the traits is called the phenotype.
As an example, we chose the trait 'anthocyanin'. The red plant pigment anthocyanin can easily be detected and be measured quantitatively. Since the formation of anthocyanin is a luxury function of the cell, i.e., not required for the existence of a cell per se but

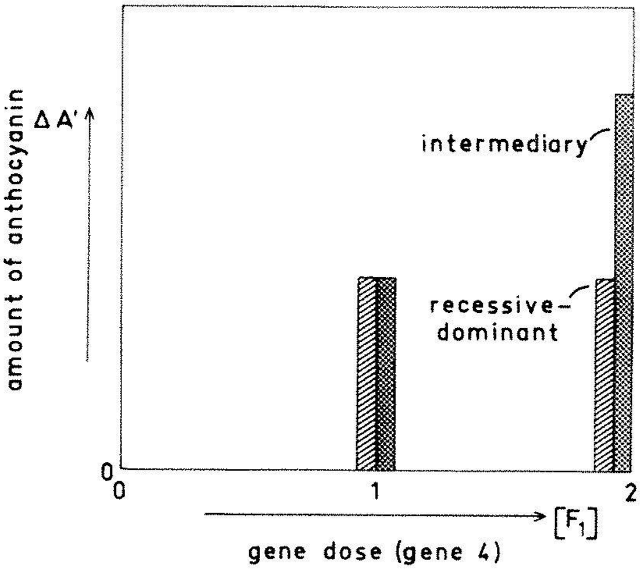

Fig. 4. Quantitative relationships between gene dose (factor dose) and amount of trait (effect) in a diploid system producing anthocyanin. Intermediary inheritance: amount of anthocyanin is proportional to gene dose. Recessive-dominant inheritance: gene dose 1 (of $gene_4$ in fig. 3) saturates system. With increasing gene dose other factors limit production of anthocyanin (after Mohr 1977).

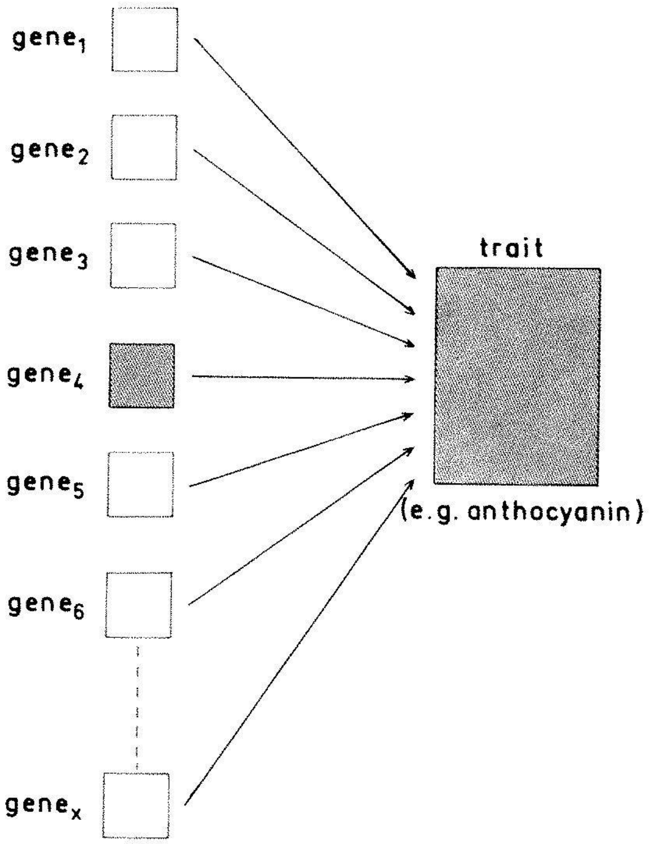

Fig. 3. This sketch is supposed to illustrate formal gene-trait relationship and 'logic' of single-factor analysis. 'Trait' is used in sense of classical genetics. E.g., red coloration of petals by anthocyanin is considered a trait (after Mohr 1977).

only relevant for the reproductive fitness of the whole organism, this trait has been used extensively in genetics. Under experimental conditions it is irrelevant for the well-being of a plant whether anthocyanin is being synthesized or not. Comparing figure 3 with the model (fig. 2) it becomes obvious that those x genes that contribute to the trait anthocyanin must be identified with the cause a whereas the trait proper (the appearance of anthocyanin) must be identified with the effect a'. Now we choose one of these x genes as a variable, say $gene_4$. This particular $gene_4$ is equivalent to the variable factor F_1 in the general model. Without $gene_4$ the trait will not develop at all even though all other factors are available. The law-like relationship between the amount ("dose") of $gene_4$ and the amount of the trait ("effect") has been known since the early days of formal genetics (fig. 4). In the case of intermediary inheritance in a diploid system, $gene_4$ is the rate-limiting factor whereas with recessive-dominant inheritance the gene dose 1 is already saturating. This implies that some other factor(s) of the x factors become limiting with regard to the quantitative expression of the trait anthocyanin as soon as the gene dose 1 of $gene_4$ is available. Most of classical genetics is based on this model. It is obvious that a statement such as '$gene_4$ causes anthocyanin synthesis' is not an appropriate description of the actual situation. Anthocyanin synthesis is caused by the total set of x genes. However, the statement 'the lack of appearance of the trait anthocyanin is caused by the lack of $gene_4$' is logically correct and should thus be preferred.

The Process of Research in Reality

In my brief sketch of the scientific approach I did not want to give the impression that this process is a systematic, well organized, and deliberate procedure. The opposite is true (Goldstein 1978). The scientific enterprise is a complex and erratic one. It includes a large number of people with different interests, purposes, skills, and depth of understanding. Not all experiments succeed. Sometimes they are just badly done, and the scientific consensus, recognizing this, disregards them. Sometimes, although they are carried out well, the results are confusing and raise more questions than they answer. But from time to time, out of this welter of uncoordinated activity, results emerge that we recognize as important and reliable. Knowledge is reliable (as I said) if it will serve successfully as a basis for action. The natural sciences achieve reliability by a complex social process whereby the scientific statements must be, first, consensible (that is, potentially affirmable) and, ultimately, consensual (that is, in fact affirmed by most qualified scientists). The consensus principle is the essence of science. Science is neither subjective nor objective but 'intersubjective'. The pitfall that the individual researcher who makes an observation or does an experiment is part of the operation and may affect it by his presence, his particular expectations, his preconceptions, by the state of his mind, has been overcome in science.

Science and Authority

A final point: It is often stated that modern sciences started with the appeal to experimental test. This is not really true. The basis of science is disciplined learning by experience, experience as the final court of appeal. Whether experience is obtained in an uncontrolled investigation such as the observation of an eclipse, in a controlled investigation such as the observation of plant growth on a klinostat or in an experiment proper such as the input – output causal investigation is a secondary matter.

The decisive point which marks the rise of modern science is the rejection of authority other than experience, the rejection of the idea that the properties of the real world, including man himself, could be deduced from a priori philosophical principles alone.

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Address of the Author:

Prof. Dr. Hans Mohr
Biologisches Institut
Albert-Ludwigs-Universität
D-7800 Freiburg im Breisgau