Lothar Läsker

Objective Knowledge and the Social Structure of Transformations in its Production

Introduction

In the literature of the philosophy of science, the expressions *transformation of knowledge* and invariance are used without relation to each other or as opposites. In contrast to this, in an analogy to algebraic geometry, I interpret objective knowledge as an invariant of represented situations seen from various perspectives. As genetic epistemology understands it, invariants are the result of transformations of various perspectivistic representations. The organismic formation of traits with signal character, as the perception and processing of stimuli, is bound to the locomotive and temporally-conditioned change of perspective and can be described as a psycho-physiological process. In this model, knowledge conveyed by signs and particularly textual-lingual knowledge can be interpreted as invariants formed from the transformation of knowledge bound to groups or persons. The transformations and their invariants thus form a social structure. Since series of transformations form a group, invariants are independent of specific representations or perspectives. Thus, objective knowledge is valid independently of the particular representations and conditions of its creation. "Valid independent from the particular representations (or perspectives)" easily becomes the objectivist interpretation "valid independently from any representation", which expresses itself in the dominance of the invariants that have become timeless, against which the adequacy of representations is measured. In contrast to this, microsociological analyses reveal the dependence of all knowledge on the particulars of its being gained and insist that there can be nothing other than specific representations. Relativism can be overcome only in broader transformations that take into account the perspectives of all groups affected by the knowledge.

The Social Structure of Cognitive Transformations

Knorr-Cetina (1984: 17) chose this epigraph: "My good man, facts are like cows. If you

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only look hard enough at them, they generally run away" (quoted from Dorothy L. Sayers). I would carry it farther: whoever has something to do with cows doesn't just look at them attentively, he generally also fences them in. And how is it with facts? If we want to work with facts, we have to take care that they don't run away from us. They are fixed by means of methodological rules and definitions, but not eternalized in either Plato's World of Ideas or in Popper's World III. Stuffed cows don't give milk. But we often believe that it is different with facts and with the objective knowledge that it is supposed they permanently justify.

No society functions without knowledge that is taught and learned and whose validity is reliably preserved for certain time periods. But as soon as we consciously begin to try something out on the knowledge regarded as valid, it requires specification, and nothing of the well-taught sentences remains tangible. If we don't stop at mere trying, our new insights precipitate, becoming new sentences which we teach, becoming conscious and also unwitting changes in our way of reading venerable texts.

Just as the economic value of a product cannot be deduced from the analysis of its physical and chemical composition or from its design, the symbol-character of things that have significance for people cannot be explained by using scientific analysis (Marx 1962: 85; Bourdieu 1979: 335). I would like to compare a "reliable piece or body of knowledge" with a structure preserved in a closed series of transformations and stabilized by means of a system of invariants of these transformations.

I will differentiate five phases in the series of transformations whose invariant can be interpreted as objective knowledge. These phases themselves can in turn be analyzed as such series of transformations and thus form a structure of their own and determine themselves, i.e. they can be differentiated from the rest of the whole.

These five phases and the transformations forming them are:

A The laboratory: what scientists do in the laboratory;
B The discourse: what scientists say about their work:
a) to colleagues in the same specialty,
b) to participating observers;
C The literature: how scientists write scientific texts for publication;
D The audience: how a text is received:
a) within the specialized field,
b) in the public realm;
E = A1 The laboratory: how the reception is received (and influences what is done in the laboratory).

I use this model in order to build a bridge between the research on science that analyzes what goes on in the laboratory under the name of research and the philosophy of science that determines its object according to the rules of reason that scientific activity is supposed to fulfill. This will certainly not abolish the distinction, broadly accepted since Reichenbach (1938), between the context of discovery and the context of justification. The intention is, rather, not to sacrifice the insight that research creates the objects treated by scientific knowledge to the demand that scientific knowledge must be objectively valid. Of course this intention can also be achieved through a strict respect for the differentiation of contexts and through a dichotomy between the two areas. In this case, research on science would be concerned with social structure, especially with what can be observed in the laboratory, while the object of the philosophy of science would be what can be logically analyzed, what is true or false, the so-called cognitive area. The connection between these two modes of approach can be found in an overarching theory of society or culture. In the context of these theories, it would remain to be explained why theories can be observed neither in the laboratory nor elsewhere, and why texts are theoretical when they are valid when independent of the local characteristics of the research providing the evidence supporting their results.

The controversy between the researchers of science (e.g. Weingart, 1984; Krohn & Küppers, 1989) and the philosophers of science (e.g Mittelstraß, 1989) implies that there is as yet no recognized explanation for how scientists' and technicians' laboratory activity is connected with what is regarded by the scientific and general public as an increase in knowledge. In the polemic with Husserl and later with Popper, Horkeimer & Adorno (1989), and Habermas (1972) have criticized the strict separation between genesis and validity. They are particularly interested in this relationship, because research itself is oriented by a faulty understanding of the nature of research. However, if I correctly understand them, they have not suspected this corelationship on the level of micro-sociological analysis. Rather, Adorno seems to agree with Popper in rejecting the relativisms that arise if the justification for the validity of scientific knowledge remains bound to the reproduction of local characteristics in which it was gained. While Popper (1969: 6—7) emphatically disputes that socio-psychological or ethnographic analyses can contribute anything to the understanding of science, Knorr-Cetina (1984: 22—23) insists that the construction of knowledge is a process of action that can be analyzed and specified, and of which a comprehensive segment can be observed in a laboratory dedicated to natural science.

More recently the controversies trace back to Frege (1973) and especially Husserl, whose strict dichotomy between psychology and logic excluded every possibility of using empirically demonstrable facts belonging to the description of the process of the emergence of a hypothesis to scientifically support the hypothesis (see also Kusch 1989; Dummet 1988). For logicians, it is thus clear that knowledge that can support a theory belongs to a level on which psychological, sociological, and other empirical characteristics of production do not exist. For Adorno, among others, theories, in their accepted meaning, are the result of the adoption of originally exclusively scientific knowledge into the knowledge of cultural public spheres, where they are combined with what can be imagined. Conveyed by this transformation, they become parts of general knowledge, whose validity no longer appears to be bound up with socially determined, culturally specific transformations.

This interlacing of scientific knowledge into the culture, which is as significant as the interlacing of experimental research into technology and its economic efficiency, is the decisive transformation; objective knowledge is formed as its invariant (Tripoczky 1988; Läsker 1988). If the reception of the reception of the results of research specifies the preconditions from which research in the laboratory proceeds, then the cycle is closed and the significance is formed. Activity in the laboratory is then not the entirety of the production of knowledge (Krüger 1990; Latour, 1983).

This way of looking at things differs from the program of the sociology of knowledge in an essential point: it is not enough to merely identify present conscious social structure in the theories, but rather it is necessary to reveal the transformations by means of which the hypotheses and theories initially restricted to expert cultures become scientifically anchored and generally valid knowledge that is technologically, economically, socially, and culturally effective, and which finally becomes the accepted knowledge on the basis of which expert cultures specify the assumptions of new research intentions (Engler 1987).

Transformation and Invariance form a Pair of Terms

When we speak of invariants, it is worth referring to the transformations in relation to which something remains invariant. When we speak of transformations, it is worth pointing to the invariants that produce them. In this sense, objects that we recognize from various perspectives as being alike, despite the difference in the views, are invariant in relation to the standpoints and time from which they are observed. Terms or meanings are invariant in relation to lingual transformations (the change in the verbal designation). To describe invariance in this sense is hardly surprising, if the term is determined through semantic equivalence, as in Logical Constructivism (Kamlah & Lorenzen, 1967: 84). But to say that objects are things identified in a series of changes of perspective initially raises more problems than it solves. For in normal science, as in our everyday life, there are no problems with the existence of objects, and, in everyday language, there is no way to fix a differentiation between objects and things. In the sciences, not only in physics, it is always indispensable to speak of things in the context of methods of proof, criteria of identification, and their depictions when objects can only be known through various representations. This was the great difficulty that Bohr (1958) sought to overcome with his idea of complementarity. This kind of interpretation of scientific objects and terminology formation is a transfer of terminology from algebraic geometry and mathematical physics to matters of philosophy and the philosophy of science. Thus, it is uncertain what it has to offer towards understanding epistemological and theoretical complexes. My intention is to regard, in a less usual frame, the treatment of problems of the theory of science as a transformation in itself, and to ask about the invariants which might become possible solutions.

Stimulus-Perception Transformations

To interpret perception as an invariant is nothing new (Born 1966: 52; Kaila 1979; Klix 1971; Niiniluoto 1987). The psychology of perception, which orients itself along mathematical and natural scientific models, has tried since the 19th century to find the laws of transformation that, in changing situations, produce invariant perceptions from stimuli which, physically measured, vary. It is less common to interpret concrete activities, cooperation and the communication coordinating it, and the change from the respectively varying perspectives of participants in collective enterprises to a group observer perspective as a transformation that produces knowledge as an invariant. This interpretation is incompatible with the still influential doctrine of Empiricism.

The experimental findings that, contrary to the unspoken assumption of the psychophysicists in the 19th century, there is no constant relationship between magnitudes of stimulus and of perception, can be satisfactorily explained. Corresponding findings have strengthened the Empiricist assumptions that recognition can be explained as the reception and treatment of stimuli as a quasi-organismic process; and powerful technical simulations can be made on the basis of these explanations. That social contexts are still bracketed out appears primarily as a problem of method (Klix 1971: 252). From the viewpoint of many experts, there is no doubt that, in an experimental psychology oriented toward mathematics, natural science, and technical science, the methodological keys can be found that will enable an analysis of the sociological conditions of cognitive processes (for criticism of this see: Winograd & Flores, 1989).

With this method, the activities of the perceiving organism and the perspectives changing with its life activity are described from the perspective of an uninvolved observer. What the individual knows as an organism capable of perception is not taken into consideration. It is always regarded as possible to analyze the organism's perception as an objective process, i.e. to compare organisms and their external situations, which are perceived but not created by them, and, through the observer's controlled changing of these situations, to find out which characteristics are relevant to behaviour. The limits of this perspective have been made apparent in so-called Radical Constructivism (Maturana 1982; Schmidt 1987).

Change of perspective has its original or literal meaning in the Objectivist analysis of perception: organisms see things from various directions, from the back and the front, also from above, and nevertheless perceive that it is always one thing. For the observer, the object's existence has nothing to do with the perceptions of the organisms that perceive it. The observer observing the perceiving organisms knows that the thing exists prior to their perceptual activity and that they don't know of the thing until they have formed invariants in the change of perspectives possible (or relevant) to their behaviour. Only if we compare our own perceptions of things with those usual in cultures foreign to us, do obvious aspects become apparent that do not originate in the objective thing alone. The characteristics that are relevant and perceived, and what is identified as a thing, are thus shown to be subjective. In the self-concept of many natural scientists, the illusion was long maintained that it is individuals who, on the strength of their acumen and intellect, perceive. In the "Kritik des Hegelschen Staatsrechts", Marx (1956) wrote: "for example, in science, an 'individual' can complete the general matter. But it doesn't truly become general until it is no longer solely the business of the individual, but of the society. This changes not only the form but also the content."

With the later turn to materialism, the so-called reflection theories rehabilitated the importance of the sensorium as a source of perception. Against the insights of Kant and especially Hegel, this philosophy declared the brain to be an organ of thinking and knowledge was generally interpreted biologically. Within this concept, it was not possible to think about the individual's matter becoming a general matter of society, nor about the transformation of the individual's feeling and consciousness. In perfect agreement with the physical concepts of the 19th century, individuals and their surroundings, given normal development and education, were sufficiently identical to gain the right knowledge, each for himself, from what can be perceived.

For Quine (1969: 87), sense data are subjective. He finds ironical the idea that they can be associated with observation sentences that are meant to be the "intersubjective tribunal of scientific hyotheses".

"The notion of observation as the impartial and objective source of evidence for science is bankrupt." However, Quine (1967: 88) answers the argument of Hanson and others that different people perceive different things in the same situation as follows: "What is regarded as an observation sentence varies with the size of the observing community. But we can always reach an absolute standard by including all or most of the speakers of language."

Of course Quine's (1980: 148) nominalism accounts for the fact that designations cannot be fixed without presuppositions by simply displaying the things. As Neurath (1932: 204) and other Empiricists already knew, stimulations of nerve endings are not organismically processed to knowledge and sentences: that is precisely what resulted in the difficulty of determining what an observation or log book sentence should be. After Quine has discussed various suggestions that language and prior knowledge play a part in whether individuals subjected to the same stimuli agree (or disagree) with sentences, he comes to the following result: "An observationsentence is a sentence that all speakers of a language will judge in the same way if they are subjected to the same accompanying stimuli. Negatively expressed, an observation-sentence is a sentence that, within a language community, is insensitive to differences in past experience."

For the observer who presents the sentences for judgment and who controls the conditions, stimuli nevertheless remain independent of the knowledge that the test persons already possess; or: if the past experiences of the language community determine what the stimulus is, then the position of the observer and his differentiation between observationsentences and other sentences would become radically problematical. If this knowledge determines what is regarded as worthy of designation, i.e. differentiation, then the observer must involve himself in a way that contradicts the ideal of objective knowledge, and we have a form of the feared circularity. To avoid these difficulties, Quine clings to the distinguished status of observationsentences and expects psychology and other cognitive sciences to discover the mechanisms of designation. Assuming the coherence of our lingual knowledge, connections can always be made to sentences that all speakers of a language will agree to if subjected to the same stimuli.

If one becomes aware that coherence is this precondition, then the assumption that observationsentences emerge from observations is already abandoned. When they are so interlaced with the language as to serve as the common basis for mutual understanding of all speakers of a language, they are fixed prior to the observation. The test persons' perception of the situations that Quine imagines could reveal to the observer that what certain words of the sentence presented for judgment refer to is culture-specific. The richer such sentences are, the more they are interlaced in the net of concepts and the more they are secured against possible refutations through confrontations with observations. M. Hesse (1974) made this clear in her attempt to mediate the controversies about the role of the Michelson-Morley experiment in Einstein's thinking. The hope that Quine originally shared with Carnap, namely that observationsentences are a kind of atoms from which all knowledge is put together according to the principles of logic, remains mostly unfulfilled.

If observationsentences lose their distinguished status, the conventional interpretation of discoveries proves to be very incomplete or even refuted. "*X*" *perceived (discovered) that Y*" (Robert Koch discovered the tuberculosis bacillus) is no longer to be interpreted: "*Y exists, and X discovered that Y exists and reports this to others, so that these, through neutral examination, are able to convince themselves that Y exists (the assertion that Y exists is true).*" Rather, it is the socio-cultural conditions under which what X (Robert Koch) did and represented to the public that are to be regarded as a discovery. It must thus be explained how he could assume that a look through his microscope would convince every impartial and reasonable and seeing person that his theory is true (or how we can assume this and also that he correctly assumed this).

Born (1966) described his memory of the shock he was given by the insight that everything claimed by scientists (and also by people in everyday life) refers to agreements that have been made only slightly consciously. Whether it is colors or things we deal with every day, the names that designate them refer to agreements between all members of the language community, who understand such words as *green, tree, sky, evening star* as names for characteristics or things. This need not, as Kant assumed, be decided prior to all experience. People may have agreed upon what they regard as the same and what they regard and perceive as different, and this sameness need not be identical (nor become identical through agreement) with that which each feels or perceives for himself. This is all the more true for what is regarded as the same and different in the scientific disciplines and for what technical instruments open up to the senses according to methodological instructions. Language fixes what is the same; what I feel, I cannot say; or what I say, insofar as it is language, refers not to my feelings but to what my feelings are in agreement with the feelings of everyone else who can understand me. This is an expression of the fact that there can be no private languages.

The conviction that science could transform all ideas locked into activity into lingual and conscious knowledge may be a calamitous illusion. It is important to recall this, because the assertions of existence formulated in individual scientific disciplines have their sense and are to be decided, experimentally or theoretically, in the context of these disciplines. The sense of these assertions and their decision cannot be reconstructed in everyday language, in which it is a question of this or that thing, this or that ability, this or that being, but not of the preconditions of the sense of our conviction of the reality of these things.

In the sciences, too, it is disputed whether there are atoms, molecules, or genes; but here it is a question of the respective effects observed and requiring explanation, a question of the witches, and not of what there is. The question is to be decided in each individual case, precisely as Evans-Pritchard (1937: 82—83) described it for the Zande: "There is no elaborate and consistent representation of witchcraft that will account in detail for its workings, nor of nature which expounds its conformity to sequences and functional interrelations. The Zande actualize these beliefs rather than intellectualize them, and their tenets are expressed in socially controlled behaviour rather than in doctrines. Hence the difficulty of discussing the subject of witchcraft with Azande, for their ideas are imprisoned in action and cannot be cited to explain and justify action."

Transformation and Invariants in Physics

Classical (Newtonian) physics prefers to describe the movement of mass-points within a Cartesian coordinate system. Formally (i.e. mathematically), the three dimensions of (Euclidian) space and time are treated as coordinates and the transformations in relation to which the laws are invariant are defined. Soon, after the successes of mechanics, the Euclidian space thus constructed, the universal time-scale, the Galileo transformation and the constancy of mass were silently accepted as unquestionably valid assumptions. They appear to owe nothing to the transformations and the invariance of the equations created along with them. In essence, this was the assumption of an absolute coordinate system, in relation to which the inertial systems could be determined before the transformations that created the systems, but an assumption which cannot itself be experimentally determined in the context of the Newtonian laws. Rather, in this context, all systems in which the law of motion is valid (the inertial systems) are of equal value before every transformation. It was not noticed that the unquestioned assumption of such an absolute system, in which the determination of this equivalence is possible, is problematic — until the isotropic propagation of light was explained in the framework of classical mechanics. Although it is quite clear that this coordinate system was assumed in order to bring the equations describing the propagation of light waves into a defined mathematical relationship to the laws of motion of mechanics, it won its explanatory value through the ontological status of the ether resting in it (Einstein, Infeld, 1956).

In the epistemological self-understanding of classical mechanics, it was regarded as possible to identify invariants of mathematical transformations and interpretations of experimental experience, and thus of theoretical knowledge and the structure of concrete reality. With H. Hertz' (1984) considerations as to which of the various possible images should be chosen to depict mechanics, a new level was introduced into thought in the natural sciences, one not identical with the immediate observations and measurable variables nor with that which is observed and measured. This understanding gained an essential importance in the quantum theory: symbols and a symbolic algebra stand for situations and observables. Observations consist of measuring an observable, whereby the result of measuring is a number. In quantum mechanics, observables must be repeatedly measured in order to form a middle value afterward. The middle value is defined, and the most important link between the symbolic algebra and the physical facts is found in the assumption that this middle value can really be found in the real acts of measuring. The abstract symbols are represented in systems of numbers and canonical transformations convert one depiction of observables into another depiction of the same observables. The observables are thus defined as invariants. "The important things in the world appear as the invariants (...) of these transformations."(Dirac 1981: VII).

In contrast to the practice of quantum mechanics, classical physics assumed that, in principle, a single experiment was adequate; deviating measurements were interpreted as a scattering around the true value, which an ideal act of measuring would deliver; the invariants existed independently of the transformations. In these interpretations, acts of measurement have no influence upon what is measured, and the production of knowledge belongs to a different context than the validity of knowledge.

We find transformations and their invariants not only in physics but also in other natural sciences. The following interpretation of a much-discussed example from the recent history of molecular biology that I would like to oppose to the analytical interpretation (e.g. Giere 1985) may seem violent: Watson and Crick followed a method of Pauling's and built wire, metal, and cardboard models of molecules, taking into consideration Chargaff's pairing rules, Pauling's discovery of the alpha helix, the discovery of the molecular building blocks of DNA, and Franklin' and Wilkins' X-ray structure analyses. These models of wire, cardboard deliver the instruments for the definition of theoretical models. The theoretical model is an idealized nucleic acid with idealized sub-units, themselves formed of idealized individual atoms in a very specific spatial arrangement. Watson's hypothesis was that the actual DNA structure closely resembled that of the theoretical model. Watson and Crick assumed a correspondence between the biologically active, chemically prepared, and physically examined DNA structure and their wire-and-cardboard model, without mentioning the mediation of the theoretical model. The correspondence between the conclusions arrived at through the model and the actual observations and measurements supported a kind of realism.

But the model, which imitated the spatial arrangement of the atomic building blocks of DNA, was conversely built to fit all available knowledge of biological function, of the nature of bonding, and of the X-ray structure analysis. In hindsight, the actual achievement consisted in interpreting the X-ray-photographs, the pairing rules of the four nucleotides, and the knowledge of the role of DNA in cell division, inheritance, and virus replication as varying depictions of one and the same structure.

Watson (1969) and Crick kept changing their model until theoretical predictions and measured values corresponded. In a first phase, the effects of already known but more or less isolated data dominate. In the second phase the reverse effect dominates, in which measured data are interpreted as confirmation of the theory, if they satisfactorily correspond with the calculated values. Then the construction of the model becomes the discovery of the structure: the structure appears to owe nothing to the transformations — here realized as the one-to-one correspondence of the parameters of the various representations by which the model of the one molecule is formed as an invariant. In this case, the representations were not only methodologically different, they were also gained in various, competing laboratories and defended as respectively original achievements. The one-to-one correspondence of the parameters thus presupposes interdisciplinary communication.

In empirical analyses, convictions that are undisputed can be distinguished from views and opinions that still require this foundation. And the evidence refers to mutually accepted models which can easily be interpreted as invariants of the transformations of various representations. Vice versa, the truth value of models is disputed if they cannot be transformed into other theoretical-methodological systems. From this one can conclude that these models belong to different phases, which are differentiated by the different status of the invariants (Schulze 1990).

Transformation of Ability into Knowledge

When programs of evidence in the analytical theory of science seem to be achieved, they are plagued by circular argumentations that, by their own standards, invalidate the entire effort. We can also only speak of transformations of representations of the same object if we know what is represented. We can only speak of the invariant that is represented if various representations and the rules governing their conversion into one another are known.

The analyses in which Piaget (1975) examined the genesis of volumes, permanent object, and concepts of space and time showed a way to explain such circular argumentations, so that they are no longer disturbing. In the terminology chosen here, the situation in which transformations can orient themselves toward the invariants would be the expression of the balance between various representations. The formed invariant predisposes investigators to confirm it and appears to exist prior to the transformations. It is, then, contained in every representation — and the earlier history is forgotten. Here, we could go into more detail on the Constructivist program of evidence, which refers to pre-lingual knowledge that shows itself in concrete acts (Janich 1969; 1989).

Piaget (1973) speaks of reflecting abstraction, which can also be set in relation to invariant-formation or the individualization of the formed invariant as a prototype, pattern, or symbolic representation. Reflecting abstraction adds something to the things: they are represented from a previously unusual perspective. In this, it is not initially clear that it is the same thing. Not until after the movement from exploring activities and attempted representations to the interpretation of various representations as the representation of one thing, do the invariants, fixed in thought and individualized in signs, attain primacy over the transformations. Such movements are characteristic of research.

Genetic epistemology offers the chance to understand the relationship between activities as practical-concrete transformations, the permanent objects that are their invariants, and the signs that represent them. It appears that Piaget (1962; 1972) unconsciously assumed a specific social structure to be a more or less universal and constant set of conditions in the development of intelligence. But activities only become transformations if their starting conditions are comparable to their results. Beyond that, from the individual view of the person, no activity is the same as any other, not even if, as in science, it has precise descriptions and if an experiment is carried out under strict rules for the purpose of confirmation. To say that an activity is a repetition (of an experiment), it is not only necessary that there be numerous, not always conscious corrections to allow the real process to approach a stipulated ideal. It also requires the common ideology of those who regard deviations as inessential and correspondence as essential (Collins 1985: 33). The invariants are thought of as ideals, and, in thought, they are therefore always primary and not results of the transformations.

It may be that I have devoted too much space to the naive empiricist understanding of knowledge. However, it is always latent in our culture. Many of the problems long and heatedly discussed in philosophy and the theory of science have resulted from the fact that expressions of the language that form a theoretical or, in a stricter sense, logical structure refer through their definitions to facts. In experimental research, expressions designate something that has its meaning in a context of practical activity. Such contexts of practical activity are closer to everyday situations and thus closer to everyday understanding than scientists demonstrate to the general public.

Propositions, which logicians employ as the equivalent of facts, are regarded by the experimenter as empty formulas whose contents are literally worked out in experimental experience and depend on what he can see, smell, hear, hold in his hands, and imagine. Thus, for chemists, atoms had hooks and eyes, and physicists, despite the insight that elementary particles are theoretical constructions, can imagine them existing in space and time. Propositions can only have the meaning valued by the experimentally experienced scientist for those who can understand them, because the horizon of their experience is comparable to his. This meaning does not completely transfer to the language uncoupled from the speaker; i.e. the person deciding whether the judgment is true takes into consideration who made it. "Not words have meaning, but the speaker or listener who means something by them." (Polanyi 1958: 252). Whether the listener trusts the speaker and understands him as the latter intends has many reasons, of which only a few have to do with logic. Thus, part of the formation of meaning is that the person asserting a matter convinces people in his surroundings of the truth of his assertion. The resources he can resort to in this are, however, different from the methods recognized as legitimate in science.

Definition places concepts into a theoretical context. The practical knowledge of the experimenter places the words that designate the concepts into a context of action differing from the theoretical context. In the everyday process of research, the two need not correspond nor be consciously differentiated. Definitions, which place terms in a theoretical structure, and expressions, which refer to concretely perceptible effects, belong to different levels of discourse.

In unreflected explanations in the language of physics, the validity of the laws (that are discovered) is one of the resources that can support explanations, rather than one of the phenomena that require explanation. Thus, it is accepted as an explanation that the motion equation is invariant, because it expresses a law of nature. Because constructions do not have the status that makes further evidence neither possible nor necessary, it is also rejected that in mathematical construction we are careful to see that the important equations are invariant in relation to the transformations, and call them laws for that reason.

From the perspective of scientists working experimentally, it is the interpretation that he intends to achieve which first makes the invariants of formal transformations into natural laws. As Born (1958) formulated, the problem begins with the interpretation of the empirically gathered data. If this substantialization has occurred, the illusion arises that general validity exists prior to the transformations. If, from varying perspectives, scientists came to judgments corresponding to each other, this is the proof of the objectivity of the method used in researching their actually existing thing.

Ethnomethodology

Knorr-Cetina (1984: 22—23) wants to gain a concept of knowledge "that sees the natural scientific result not only as historically-socially embedded, but also as concretely constructed in the laboratory." This program of explaining knowledge as a synthesis of the construction of knowledge in the laboratory and of the knowledge of the culture in which the laboratory is embedded, is intended to uncover the transformation of various representations of the same things and also the formation of invariants. But this proves to be an extraordinarily complicated venture, because completely different methods are needed to analyze the various phases and environments involved. Analyzing the transformations and constructions of knowledge in the natural scientific laboratory appears to demand fundamentally different methods and a fundamentally different program than analyzing whether and why theoretical justifications of scientific constructions are convincing in a particular culture. As is well-known, the dispute begins where the assumption is made that the analysis of the natural scientists' work in the laboratory, making use of ethnographic methods, can contribute anything to the understanding of the production of knowledge. If it remains unclear where the difference lies between the goal of the ethnographic analysis and the goal of those observed in the laboratory, then the dispute cannot be made productive.

If a physicist says, "I am observing the atom", the ethnomethodologist, as observer, can observe how the physicist observes but not what the physicist observes. When the physicist speaks with his colleague, who has made comparable observations and plans further observations, then the two of them can make themselves understood to each other about something about which they say is what they observe. In the communication, they construct an object or an object-structure of which they would then say that this object formed the thing they were observing. Something similar repeats itself at the level of the theory: they operate with symbols, write equations, calculate, discuss solutions. They dispute about the interpretation of terms that they have, it appeared, initially introduced for exclusively mathematical reasons, and they are in agreement that, as they say, the symbols and symbolic operations deal with a reality that is symbolically reproduced.

According to a characterization of ethnomethodology made by Barnes (1982), its analysis should not bother with what the observed participants believe they are observing. If an ethnologist analyzes the pottery artisanship of the culture that interests him, he will, after all, not learn anything that will put him in a position to produce pots himself. He doesn't want to become a potter. He doesn't want to learn anything that would be published in a handbook of the potters' guild. He wants to learn something about the potters because, in the culture he is analyzing, they are particularly interesting people. If the ethnographer learns the practice of, for example, potting, or, as Evans-Pritchard (1937) so plastically described, of the oracle and the medicine men, then he learns this in order to understand what the practices mean in the culture of the Zande (or another culture). Must he learn this? If he does learn it, does it help him to understand its significance? If the research on science adopting the ethnographic method is not interested in science because of its importance for our culture, but rather because it is itself a culture, then conscientious consideration is necessary as to what the result of such analyses could be. If objective knowledge (as separate from knowledge that can only claim local validity dependent upon unique conditions) has been realized for the entire society and its culture, such analyses cannot discover the production of objective knowledge.

In analyses of the histories of discoveries, the point for the ethnologists cannot be to decide who in fact discovered what through which coincidences and fortunate ideas, but rather to gain an understanding of why an event is regarded as a discovery in the local culture and not in another. It is interesting, how and by which methods words are charged with meaning. Why are scientists interested in something that is hardly worth mentioning in other times and cultures? How are things made visible in order to provide a referent for words that designate theoretical concepts?

For the unreflected self-concept of the natural scientists and philosophers of science, who, like Popper, have well understood how to depict this self-concept, the ethnographers' method simply doesn't take for its object that which characterizes science or that which allows activity in the laboratory to become science. But it and other suggestions to determine the specifically scientific in a manner itself scientific or meta-scientific remain unsatisfactory because they do not function practically. Life and work in laboratories and institutes is no different from life and work in other areas of the society, as the ethnomethodological studies have clearly shown. He who wants to know what physics is will not experience this by observing physicists at work. He has to study physics and prove himself as a physicist. This understanding cannot be replaced by the philosophy of science nor by research on science. But that wasn't the goal anyway. What questions must then be raised, with which goal, in order to explain science, rather than being questions of the sciences?

The analyses that make us conscious of the manner in which scientists, laboratory assistants, and technicians in fact do research cannot be judged according to the goals valid in the analyzed sciences and in the various fields of analytic philosophy of science. If the goals are other than these, especially if the goal is to understand science as part of our entire culture, then one must clarify what one must know of this science. What must one know of what the scientists in the respective field know, in order to approach this goal?

No one disputes that hypotheses cannot be evidenced by the representation of the respective unique situations of their formulation - not because they are in contradiction to these situations, but because they have no relationship to them. Even if we have no doubts that the discovery of the structure of DNA occurred precisely as Watson tells us and that Lwoff's remembrances of the discovery of lysogenesis are correct, they contribute nothing to the development of biochemistry. In his foreword to "The Double Helix", Sir L. Bragg nevertheless speaks of the valuable contribution to the history of the natural sciences made by Watson's notes: the same could be said of the essay Lwoff wrote from memory. So why shouldn't one read not only the publications in "Nature" or the "Annales de l'Institut Pasteur" but also the amusing stories Watson, Lwoff, and others tell us? Is there any kind of relationship at all between the two kinds of texts?

In a textbook "Klassische und molekulare Genetik" we read: "The structure of DNA was clarified in 1953 by Watson and Crick. The fascination of this discovery and the problematic of human weakness in such situations was later portrayed by Watson himself ("The Double Helix", in German from Rowohlt 1971). This book provides an exciting backstage view of great science." (Bresch, Hausmann 1972: 141)

The stage curtain separates the public representation of finished science from what scientists do behind the curtain before they show in front of the curtain what they have done.

The differentiation between public science and what happens backstage puts the question as to whether and which relationships exist in a context that raises further questions: in the theater, the curtain is used to hide and to create illusions. Nonetheless the stage set is part of the theater. It is no fraud, and the director does not expect the viewer to let himself be deceived without knowing he is being deceived. But children and the naive are disappointed when they see the person who just died in agony beaming on the ramp for applause. What did they really expect?

Not only is the difference between the respectively unique situations of research and the claim to universal validity unclear, the relationships between the practical activities of scientists in the laboratory and the theories of which it is later said that they explain what was invented and discovered in the laboratory are also mostly unclear.

The essential question arises: is it a characteristic of science that the results researchers and groups of researchers arrive at in laboratories and institutes merely spread into society? Are no transformations — enriching comparisons with knowledge from other areas of science, with poetry, or with other art forms — possible? Are there only popularizations that nevertheless correctly reconstruct the representations in the language of other cultures of experts and of the general public? The answer will depend upon the role in the consideration and negotiation of future research orientation played by the reception in the institutes and laboratories of the ideas that sciences lead to in inexpert public realms, of the insights, ideas, hopes, and fears tied to scientific inventions and discoveries. This touches the relationships between micro- and macro-sociology. If it is assumed that the result of research is as it was achieved in the laboratory, and is then already finished, then the analysis of the production of knowledge belongs entirely within the field of micro-sociology (Knorr-Cetina 1990). The Umweltschleifen (environmental loops) in the self-organization model of Krohn and Küppers (1982) serve the continuability of research and have no part in the results' formation of meaning, which is internally formed as an independent value of research activity.

If metaphors in which organisms are presented as machines, societies as organisms, the base-triplets as an alphabet, etc. are merely personal aids to understanding of which no trace remains in the finished piece of science, if political campaigns against the production of poison gas have no effect in the chemists' laboratory, if environmental protectionists are merely troublemakers, if doubts about the wisdom of genetic engineering merely express the ignorance of the layman, then the scientific result remains what it is at the moment of its publication. Possible and desirable and above all feared retroactive effects of public concern on the evaluation of controversial topics of biological research increasingly influence public discussion (Herbig, Hohlfeld 1990). If the general public's reception does not enrich the meaning, then the reception of this reception in the laboratory can only induce satisfaction or annoyance, but not itself be constructive.

Participant and Observer

Just as logic can only be applied to sentences, knowledge exists only in relation to people who know or have known.

Popper chose "Objective Knowledge" (1974) as the title of his book: it is his conviction that objective knowledge is without a knowing subject. In the three worlds model serving to illustrate this thesis, it is the autonomy of World III, where books contain knowledge that is independent of who receives it or whether it is ever received, that is valid. In his understanding, the sociological or ethnographic analysis of laboratory activities can have nothing to do with whatever is a part of World III, the world of objective knowledge. On the other hand, knowledge of the theories and methods provide essential information for understanding laboratory practice.

Knowledge without any knowing subject means: equally valid for every subject. Husserl (1984) also took this step from equally valid for every subject to valid independently of any subject when he developed his idea that logical sentences were in themselves true and that this has nothing to do with the things to which they apply nor with the acts of thinking. As a matter of fact, the psychology oriented toward the organum of the natural sciences, which wanted to understand the knowledge and thinking achievements of individuals, proved unsuitable to explain the genesis of discursive thinking, and thus the socio-cultural preconditions for the validity of logic. Precisely these preconditions — among which logical ones can be cultivated, i.e. conclusions applicable to all contents and compulsory for every subject, quite separate from any concrete content — are distinguished as being generally human, i.e. rational. Popper expands the claim of subject-independent validity (which Husserl claimed only for logic) to all kinds of sentences, whereby rationality is measured by the ability to enrich the stock of knowledge through clever conjectures or to purify it through refutations.

For the understanding of science, it is decisive that the results of observation cannot be discovered or reconstructed alone in the observation — whether by oneself or by others — of the observation. By observing the observation, the meta-observer learns no part of what the observed observer learns. Without observing himself, the observer cannot become conscious that he is observing and what he is observing; and without any relation to another observer, the observation by others, self-observation does not occur. Thus the insight that observation is not the source of knowledge that it appears to be, does not allow us to conclude that it has become completely senseless to distinguish between the observation of an object, on the one hand, and self-observation or being observed by another, on the other, nor to grant each of these levels of observation a respectively independent but transformable and thus objectifiable knowledge. We can't suspect anyone of wanting to claim that ethnomethodological analyses in the research laboratory also contain the specialized knowledge gained there through the medium of the observed scientist. The problem is rather whether and how the specialized knowledge and the methods followed by the observed scientist can explain their social behavior. If the norms postulated by Merton and Popper's ethic of refutation cannot be found in actual scientific operations, it could be possible to characterize the structure of projects, objects, or problems as invariants of the social transformations of various perspectives, particularly those in which the participant perspective is transformed into the observer perspective.

Scientists who see the sense of their efforts in contributing to the development of science with new knowledge are of course convinced that their understanding, as apparent in their publications and lectures, captures the essence of the topic. They judge the apparent chaos of their own work in the same way as they have learned to discover order in the apparent confusion of the laboratory. What was essential shows itself in the later result. Even if they don't dispute the great difference between work according to the textbook and the actual activities and their conditions, the textbook representation is not invalidated. Only certain illusions have been dispersed. If we then say that the representations of research as can be found in textbooks and popular writings are constructions from the viewpoint of the results and thus do not correspond with reality, this judgment demands commentary. It is a judgment directed against the absolutization of the perspective from which these representations are given and does not yet say anything about the perspective's limited validity, or anything against this perspective. This view of science must first be examined, and in this it must avoid evaluating the tie to a perspective as a fault in comparison to a postulated unbiased description. If no one correct perspective can be distinguished, all that remains is the orientation toward the comparison of the various perspectives the society provides room for.

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Lothar Läsker
Am Friedrichshein 9
D-1055 Berlin
Germany