ATTITUDES IN SCIENCE

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1.Idealization in science and idealization of science

History of science offers several examples of scientists who more or less consciously applied the method of idealization in their theoretical research. The statements about forms of idealization met in real science expand our knowledge of the way of conduct of real scientists. However, they do not provide us with any hint that would allow to account for the role of this method in science. It means, that equipped with this type of knowledge we are unable to explain why and when scientists decide to make use of the method of idealization. What we can do is to catalogue all situations in which we observe or merely suspect the use of this method. Considered from this perspective the method of idealization appears as one of the many forms of scientific activity. This particular form is neither more significant, nor more peculiar than any other and is designed to fit to specific, well-defined cases. We can identify these cases by searching for statements which describe the behaviour of specific objects such as: ideal gas, frictionless plane or perfect market. The talk about objects of this kind indicates that the method of idealization has been used there. The cautious, detailed analysis reveals the peculiarities of the process of idealizing in every specific case. But real scientists do not restrict their research to idealized objects. Therefore in other cases they make use of different methods. This description of scientists' activities is of little help in the search for the nature of science. What it offers is a picture of science where researchers sometimes apply idealizing procedures and sometimes not.

Quite a different approach has been proposed by Leszek Nowak in his Idealizational Theory of Science (ITS). ITS is not a plain description of the situations that take place in real science. It is the theory of scientific theories conceived of as the products of ideal researchers. Being produced by the idealized counterpart of a real researcher the scientific theory is itself the idealized object and as such cannot directly refer to the real theory. It means that the status of scientific theory in ITS resembles the status of the ideal gas in phenomenological thermostatics. In its main, the most abstract model ITS presents scientific theory as the simple idealizational theory [cf. Kupracz, this volume]. In the subsequent models idealizational theory takes the more complex forms and becomes a better approximation to real scientific theories. This growth of "realitylikeness" of the idealizational theory is accompanied by changes in the concept of the ideal researcher. In the first model the researcher possesses perfect knowledge (a complete knowledge of the essential factors and the influence they exert on the

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determined factor) and ability to conduct a perfect experiment (he can eliminate the influence of all secondary factors). In the next models his knowledge becomes less and less perfect and his experimental abilities gradually diminish too. This process of approaching real science by transformation of the ideal researcher and the idealizational theory into more realistic objects can proceed further and result in new models of ITS.

The idealization of science presented in ITS leads to a uniform, consistent picture of theoretical research and its final product. It seems that this consistency is guaranteed by the assumption that is present in all the hitherto models of ITS. It states that scientific research consists in making discoveries and presenting them in the form of an idealizational theory. Therefore the ideal researcher strives to construct the idealizational theory of the considered factor. However, he is not interested in the existing knowledge about this factor, he wants to propose new theory and present it in as complete form as possible. This aim remains unchanged despite the appearance of further, more realistic counterparts of an ideal researcher. Knowledge, abilities and means at the disposal of the more realistic researcher change but he still tries to build a scientific theory making use of the refined versions of the method of idealization. I think that this ideal of an ideal researcher is based on the image of scientific activity as the enterprise of exceptional, bold individuals who do not content themselves with minor intellectual contributions to existing theoretical knowledge, but who intend and are capable to make new, essential theoretical discoveries. Such description of researchers' activities distinctively deviates from our observations of typical practices met in science. There is no doubt that creation of a new theory belongs to exceptional events in science. The everyday practices of scientists are far more prosaic. Their aims are not far-reaching. What they want to achieve is to amend or to correct the existing theory. Therefore they do not need the method of idealization as the device for inventing theories. At the utmost they use it as the element of the research routine acquired during the process of inculcation to scientific attitude. Below I shall present the more detailed description of the nature of the normal research in science.

2. On product-oriented and action-oriented approaches to science.

It is well known that investigations of science can have as their object either the activities of scientists or scientific products. The contemporary philosophy of science emphasizes the necessity of observing the distinction between science as a unity of actions and science as a unity of products [Popper 1972, 113-114]. According to this view the distinction allows to draw a line between an empirical knowledge of science and the knowledge based on the "hermeneutic" understanding the nature of science [Popper 1972, 160-192]. The empirical science of science is typical of the action-oriented approach and is usually conceived of as a kind of psychological research. The actions studied are usually identified with "scientists' thoughts experienced while practicing science" [Ajdukiewicz 1985, 117]. The understanding science of science is typical of the product-oriented approach. It considers scientists' products as contents of thoughts which are expressed in scientific statements. According to this tradition the nature of these products is neither empirical, nor mental. Being clearly of Fregean origin and reshaped slightly in its modern Popperian version the approach ascribes to scientific products the status of ideal objects which inhabit a separate world (usually numbered as the third one).

The product-oriented approach dominates in the contemporary philosophy of science. Its radical supporters postulate that even actions undertaken in science should be considered and accounted for from a product-oriented perspective.

The adherents of product-oriented approach see its main advantage in the fact that it evades psychologism or sociologism. It is claimed that both these doctrines result from an action-oriented approach to science and their weaknesses have been repeatedly and convincingly demonstrated. The principle of refraining from psychologism or sociologism in considerations about science seems reasonable, but the view that they follow from the action-oriented approach to science is difficult to defend. We can easily imagine such an action-oriented approach that does not entail behaviourism, nor any other doctrine assuming that human actions should be considered from a perspective of the empirical knowledge only. Let us point to the theory of decision as an example. Furthermore, it is difficult to find a product-oriented approach that does not refer to some, be it merely minimal, characteristic of actions which result in the appearance of the considered product. Thus, according to its own accepted criteria, the product-oriented approach is inconsistent. But I insist that this inconsistency is apparent because characteristic of scientist's action need not refer to empirical (be it psychological or sociological) knowledge of human behaviour.

Therefore, it can be assumed that without the danger of inconsistency the investigation of science can take into account both the action and the product aspects of science. I do not mean here a trivial postulate of including into one research programme the reports on scientific actions and independently obtained reports on products of those actions. It can hardly be taken as the recommendation of a productive research programme that allows for complex investigation of science.

The approach I would like to advocate is based on the thesis that considering an action aspect of science does not entail psychologism, just as considering a product aspect does not entail the reference to some understanding process. Therefore, instead of studying actions of real scientists or products populating the third world we should turn our attention to the concept of an attitude of the idealized normal scientist. This idea is based the assumption that to investigate actions in science abstracting from results they bring about is equally impossible as to investigate scientific products abstracting from actions which lead to their appearance.

I am not going to discuss the general notion of an attitude here (It has been done in Klawiter [1991b]). Let me only say that my approach is inspired by the Husserlian concept of *Einstellung* [Klawiter 1990] It is necessary to make one reservation. By an attitude I do not mean agent's intentions nor his dispositions to intentional behaviour, but the state that enables him to execute already mastered skills. The best way to introduce the concept of attitude to considerations about science is to investigate the model situation of the normal scientist.

3. The attitude of a participant in science and its component attitudes.

Many philosophers of science seem to share the view that the investigation in science and the investigation of science follow the same rules. They claim that deviations from these rules met in philosophy of science testify only to the fact that this is underdeveloped science. Even if these rules were not commonly observed in philosophy of science, it is philosopher's duty to aim at their inculcation and compliance. Thus they assume that the differences between science and philosophy of science arise first of all from the dissimilarity of the object of investigation. It means that both disciplines have different domains, although the way of conduct or an attitude of scientists and their inquirers is in principle identical. Seen from this perspective the physicist's mode of conduct does not deviate to a larger extent from that of a philosopher of science, than from the mode of conduct of a sociologist or a historian. Thus the attitude of the investigator of science is acknowledged to have identical nature as the attitude of the one who participates in making science. The possible differences between these attitudes are usually neglected and regarded as resulting from different developmental levels of considered disciplines. According to this view an investigator of science is worse trained to carry research than the one who practices science, although they both proceed along the same standards promoted, finally, by science.

At the first sight this belief appears reasonable. Though its more detailed analysis leads to conclusion [Klawiter 1991b] that it is dubious and its acceptance requires a separate research and justification. Since we are unable to find evidence supporting the hypothesis that an investigator of science acts along the same lines as participant in science it would be wiser to refrain from accepting it. Leaving this question aside I shall make a more detailed attempt to describe attitudes of those engaged in procedures of "normal" science. I shall call it the attitude of the participant in science. Its description should reveal the nature of an investigative attitude in science and thus demonstrate the limits of its "effective use" outside science (including philosophy of science).

I assume here that what drives the activities of participant in science are his aspirations for changing the *status quo* in a given discipline. If we constrain it to theoretical activities we can say that the participant in science aspires to change some fragment of the theoretical structure existing in a discipline chosen by him as his research area. This thesis seems to be devoid of novelty. But nevertheless it offers the change in the image of science. It manifests clearly when we contrast the proposed approach with the one, so dear to philosophers of science, that says that to practice science means to pursue the truth. There is no doubt that both approaches project different views on the nature of practicing science.

According to the traditional view the pursuit to attain true knowledge (no matter whether it is connected with investigators' actual intentions -as it is usually assumed- or it is just a side-effect of their intended actions) requires the replacement of hitherto existing theories which have turned out to be distant from the truth with theories closer to it. Thus all changes in science should "in the last instance" be accounted for from the perspective of its main aim -pursuing the truth.

According to the proposed view there is no need to speak about the aim of science or, at any rate, reflect on the problem whether science pursues truth or not. The assumption accepted here is much weaker. It states only that there are changes in theoretical structures, that they are results of scientists' activities and that presumably there are regularities of those changes, but they still wait for discovery and investigation. What can be admitted without doubt confines to statement that these changes have been brought about by participants in science. The question of the causes or reasons of those changes has to be suspended. The reserve in answering it stems from the belief that before explaining why participant in science tries, at all costs, to change something (this "something" is usually well defined and carefully selected) in existing theoretical structure, one has to understand *how* he brings it about and *what* are the consequences of his action. What we account for are thus ways of introducing changes into theoretical structures, not the reasons or causes -identified with or referring to investigator's motives. A sharper outline of the discussed approach will be presented below.

Insistence on the view that the essence of scientific conduct consists in the aspiration to change the *status quo* in a given theoretical domain might suggest that it is a defence of some version of an anarchistic (à la Feyerabend) account of science. Such an impression would be totally incorrect. I believe that it would be difficult to find a domain that could so fiercely resist anarchistic practices as science does.

It can even be said that only those are given consent to change the fragment of theoretical structure who before have proved their respect for the old order. The aspiration for a change of the *status quo* in science can easily be adjusted to a conservative attitude. Thus I assume that scientist is allowed to make an effort to replace a fragment of theoretical structure if it occurs evident that he has been effectively trained in dogmatism. It can even be said that the precondition for science to develop is that its participants are so dogmatic that they are unable to notice it. The nature of this specific scientific dogmatism needs a more detailed clarification. We have to analyze a system of elements that compose an attitude of the participant in science. That global attitude contains three component ones. These are the following: the reconstructing, interpreting and developing attitudes. Their description should provide arguments for the thesis that to practice science means to receive credits in scientific dogmatism.

Each of the component attitudes determine a certain stage in the process of practicing science. Let me discuss briefly these particular stages and attitudes connected with them.

4. Reconstructing.

Our analysis will start with a simple imaginary example. Let us imagine a student who decided to become a theoretical physicist and has chosen as his specialty new dynamically developing theory. What has he to do to get acquainted with this fragment of theoretical physics? First, he has to enter the world of scientists' utterances. Our theoretician in spe can read them in manuals, monographs, articles; he can hear them during a lecture, a seminar or in a private conversation with a recognized authority. It is clear that the student cannot get acquainted with everything that has been said or written and what could be in some way related to his subject. The situation forces him to make a primary selection and to eliminate from consideration all these utterances which -from his point of view- are completely useless (or their utility is not worth the work necessary to assimilate them) in his efforts to get acquainted with the intended theory. Therefore he assumes an attitude that allows him to make the a priori judgements about the utterances. He qualifies them as being useful or useless in his cognitive efforts. Let us call the procedure of this selection of utterances the *categorization*. The result of performing the process of categorization is the set of utterances which our potential scientist believes to be worth more detailed study

The process of categorization described here is usually outside the conscious control of the actor. That is, it belongs to the class of learned decisional processes which are performed so to say automatically. It does not mean that it is devoid of all elements of control, though. Its course is determined by rules characteristic of a given scientific community. Those rules decide which utterances are worthy to be accepted as containing scientific statements and which should be rejected as deprived of scientific content. Scientific community carefully observes whether investigators stick to existing rules of categorization and registers the cases of their violation. A system of such rules seems to have a relatively complex form and its structure and functioning resembles rather common law than the statute law of the Continental normative systems.

The process of formation of the rules in question takes every time a different course, that depends upon circumstances and structure of a scientific group. Presumably there does not exist such rule of categorization that would be respected in all scientific communities, regardless of the discipline in which they function. The most general rules of this kind are valid in the restricted class of disciplines.

It would be difficult to make a detailed characteristics of these rules here. I shall formulate a couple of remarks on the nature of their differentiation, though. Let us note that the rules which are mandatory in "the most advanced" natural sciences are much more restrictive than those belonging to the domain of the classical humanities. Their restrictiveness is of a twofold kind. First, the rules in natural sciences determine more accurately the class of utterances which can be numbered among relevant within a given science. This class is relatively small, whereas the class of utterances to be selected (skipped as irrelevant) is large. Second, the sanctions for violating these rules are more severe. The person who does not comply with these rules risks complete indifference of the scientific community working with a given discipline. He will be regarded as an ignorant who does not know the elementary rules of scientific conduct, as someone for whom scientific research is certainly inaccessible. This hidden ostracism practically excludes such a person from science.

The situation of an investigator who practices within a certain discipline of the classical humanities is different. The rules of categorization he is obliged to respect are less rigid. An investigator is allowed to decide more freely which utterances should be numbered among those which have "scientific validity" in a given discipline. For example, in some sections of the classical humanities the following rule of categorization seems to be valid: "If in the epistolary heritage of the investigator X there are utterances which, were they excluded from that heritage, they would be considered to be, by virtue of some other rule, scientifically relevant, then the heritage in question -with the exception of specified cases- can be included among the set of scientifically relevant utterances". The choice whether to include X's letters among statements with potential scientific significance is usually left to the investigator's own decision. In the humanities dominates a conviction that a person who sets to investigate certain conception should familiarize himself with accessible private notes of the author of that conception. It will be difficult to find among the researchers in the humanities anyone who would accept the view that statements of great scholars expressed by them in letters or in private notices are not serious sources of scientific knowledge. For example, it is hard to imagine a renowned psychoanalyst who had learned Freud's conception but completely ignored Freud's letters. What is taken for granted in the humanities is unthinkable in the natural sciences. Nobody expects that a theoretician physicist would, during acquiring knowledge of some theory, diligently investigate the private correspondence of the person(s) considered to be the author(s) of this theory.

The categorization of utterances is only an introductory step in the process of reconstructing. The next step consists in a *codification* of utterances. The utterances previously classified as relevant are now subject to a peculiar "processing". They undergo the process of polishing and refinement, they are evaluated, ordered and harmonized. In effect a system is being made out of them. The results of applying the rules of codification can be most clearly seen in works of a textbook kind. A textbook has normally two principal functions: it presents an updated knowledge from a given domain and serves as the pattern of the model organization of scientific knowledge in this domain. Thus being familiarized with a textbook results in mastering an appropriate knowledge and acquiring the principles of systematizing the utterances within a given domain. Imitating the textbook standards one can learn how to apply the rules of codification to linguistic utterances qualified priorly as containing scientific knowledge. Also that stage of the process of reconstruction proceeds largely beyond the conscious control of the acting investigator. The rules of codification are of equally precedent nature as the rules of categorization.

We can say that everything that is connected with the process of reconstruction of a theoretical knowledge belongs to a scientific communicative routine. Only these persons who are capable of observing this routine have the chance to be included into the given scientific group. But the mere complying to these rules does not guarantee a membership in a scientific community.

Critics may say that mastering the reconstructive procedures has nothing in common with the true research activity. But this criticism is based on the postulate of an ideal researcher who has the capability to grasp directly the theoretical structures. For such a researcher language is a transparent medium and therefore he does not need to consider the linguistic forms of scientific knowledge. We can say that the model of ideal researcher idealizingly neglects the reconstructive procedures. Since our attention here was focused on normal researcher we had to take into account his reconstructive skills.

The skill of reconstructing does not allow the investigator to "see" the object of his primary interest, i.e. a theory or theoretical structure. Since it is impossible to see a theory by grasping the ordinary linguistic meanings of expressions uttered by scientists our theoretician in spe has to learn new rules. They define the set of procedures which I shall name the interpreting procedures.

5. Interpreting as the process of educing formulae from the reconstructed expressions

We have stated above that a student who gets acquainted with new theory has to learn how to select and systematize utterances of various scientists. It has also been argued [Klawiter 1991b] that even if -by a mysterious coincidence- he managed to understand them fully, he would not see in them a law of science. Since if by understanding an utterance we will mean the act of grasping its sense and denotation, then even the best understanding of scientists' utterances (that is, the most accurate grasping of their sense) would not suffice to see a law of science in them. Law of science is a sentence of the special formal structure (strictly universal, counterfactual conditional with idealizing assumptions in the antecedent) that cannot be found in the texts of real science. No actual utterance or combination of them produces the sentence of this form.

There is a "rational kernel" in the view discussed above. It is the belief that scientists' actual utterances are the material from which laws of science are educed. But this belief is accompanied by another, erroneous one, that laws of science are linguistic entities. It is obvious that sentences of such form do not occur in real science. Therefore it would be reasonable to assume that "objects" educed from scientists' utterances and conceived of as "laws of science" are of nonlinguistic character.

I assume, furthermore, that the relationship between the level of scientific utterances and the level of scientific laws (theories) is not of the kind that could be described with the derivatives of the Fregean notions of sense and reference. The nature of this relationship cannot be expressed in terms of the traditional analytic outfit of the philosophy of science. On the other hand, I doubt if it would be possible to determine this nature at present. One can merely formulate some guesses concerning this problem.

It seems that from a scientist's point of view the relationship between scientific utterances and nonlinguistically understood scientific laws (theories) is of an indicatory rather than signifying nature.

In the process of educing a scientific law from the scientists' utterances the latter are considered as indicating marks not as the meaningful denoting expressions. As being of an indicatory type that relationship would be of a causal nature as opposed to the conventional character of a symbolic relationship. Thus the question of the nature of that relationship would belong to empirical rather than semantic investigations. It does not mean, that logical investigations of an idealized language of science (the one in which laws have the standard form describable in terms of the formal methodology of science) as well as attempts to apply the obtained results to real science are worthless. But the linguistic approach to the nature of laws of science cannot be regarded as the only "scientific" one.

Let us compare two approaches to the question how the scientific law is being educed from the scientists' utterances. According to one approach, the educing consists in extraction from actual utterances met in a given discipline a hidden linguistic structure of a lawlike sentence form. According to the other approach the educing consists in recognizing the nonlinguistic structure indicated by scientists' utterances. This structure represents the basic traits of the "scientific model of the world".

As has been declared above, I assume the latter approach. Since the main problem discussed here are the attitudes of a scientist I shall not deal with the formal characteristics of the law of science conceived of in a nonlinguistic way. Such characteristics has been presented in Klawiter [1991b]. My remarks will be confined to a rough description of the procedure of interpretation.

We start from the assumption that an investigator who aims at acquiring knowledge about a given domain, tries to establish dependencies which hold between objects of this domain. It is natural to assume that dependencies investigated in science are conceptualized as functions of a particular kind. Such a function is defined on abstract counterparts of objects and their relationships. It represents the way in which the configurations of objects (be it individuals, states of affairs, factors) are related to dependencies. In advanced natural sciences this function determines the invariant transformations of dependencies in a given domain. A function of this type will be called a *formula*. It can be shown [Klawiter 191b] that it possesses several essential features attributed to a nonlinguistically conceived scientific law.

To avoid misunderstandings stemming from the fact that the expression "scientific law" is usually applied to certain linguistic objects, I shall be using here the term "formula". So instead of saying that a researcher is interested in determining a scientific law I shall say that he aims at determining a formula. Having found the searched formula the investigator can recognize a new relationship in the domain of his research.

Let us return now to our potential physicist who learns a new theory. In the light of the above remarks learning a scientific law does not consist in constructing a statement of a definite form and acquiring the ability to apply it properly, but in acquiring the ability to educe a formula from a set of reconstructed utterances. It means that a beginner who is learning an already formulated theory does not want to discover formulae on his own. His brings out the formulae that someone else has discovered earlier. As I have mentioned, formula is an abstract object that represents the class of dependecies which hold in a given domain. Scientific utterances do not directly refer to formulae. Apart from their standard semantic function they play a special role, they function as indicators of formulae.

We can now turn to a procedure of interpretation in science. It consists of two stages. To the first stage belongs the activity of bringing out, on the basis of an indicatory function of scientific utterances, a formula which represents a dependency that is believed to hold in a given domain. A procedure of such a kind can be called the *eduction of a formula.*

It is evident that a scientist who makes research in a given domain will not content himself with educing a single formula. To be able to understand a theory he has to educe a system of formulae. To be more precise, the system of formulae is a specific structure that cannot be represented by a linguistically defined theory [Klawiter 1991b]. Therefore, the term "theoretical formation" will be more appropriate for a structure of formulae. We can say that having educed formulae an investigator aims at establishing the principle that organizes them in a system or, in other words, he aims at *educing the theoretical formation.* It is the second stage of the procedure of interpretation in science.

In practice his efforts bring only a partial success. So he does not succeed in educing the complete theoretical formation but educes merely a fragment of it. One can idealizingly assume, though, that an investigator is capable of bringing out the existing theoretical formation, i.e. the whole system of all already established formulae.

Summing up the above consideration we can say that the procedure of interpretation consists of two subprocedures: the educing of formulae and the educing of the theoretical formation.

Interpreting, just like reconstructing, takes place according to certain rules imposed upon members of a given scientific group. Whoever wants to learn a given theory, that is wants to educe the hitherto existing form of a theoretical formation, has to master the rules of interpreting. These rules are equally unconscious as are the rules of reconstructing. They belong to investigator's "tacit knowledge" and their mastering can be compared to the acquisition of a certain practical skill. It has to be stressed that in the process of mastering those rules an investigator has no chance to manifest his criticism or his own inventiveness. An investigator is not allowed to question, nor to change the rules. A striving for freedom in acquisition or application of these rules is threatened with exclusion from a scientific group. As we have said, reconstructing and interpreting belong to investigator's tacit knowledge. Mastering it results from an arduous training in which independence or intellectual criticism are not allowed to enter.

If the above view is accurate then it can be said, that understanding a scientific theory does not require any special intellectual dispositions which would enable a scientist to make "insights" into such theory. It requires a humble imitating the way of conduct of renowned researchers and thus on mastering a practical skill that consists in an unconscious application of the rules of reconstructing and interpreting valid in a given scientific community.

It has already been mentioned that to perceive a theory (or, as I suggest to call it, to educe a theoretical formation) is by no means an investigator's main task. First of all he intends to change the theoretical status quo. It means that he wants to change a fragment of the theoretical formation by introducing new formula into existing structure of formulae.

6. Developing

When philosophers of science discuss the problem of actual theoretical research they link this activity with cognitive acts which are supposed to produce new knowledge. What they disregard in their debate is the role of the procedures of reconstructing or interpreting. Even if someone notices them, he will not take a risk to include them among practices of a true scientific research. A true scientific research is supposed to be a conscious, creative activity subjected to systematical control and improvement, while reconstructing and interpreting demand unconditional subordination to anonymous authority and routine. Nevertheless, the view stating that theoretical research is confined to creative cognitive actions seems to be unjustified.

The argumentation defended here leads to conclusion that to become a scientist means to be properly trained in following the rules of reconstructing and interpreting. The person who does not apply these rules will not be recognized by members of scientific community as the qualified researcher and, in consequence, his possible innovative ideas have no chance to be considered as candidates for original scientific proposals. The situation can be accounted for in the following way: the person who has not mastered the rules of reconstructing and interpreting is not able to educe the present state of a theoretical formation. Since he does not "perceive" a theoretical formation he cannot point to an isolated fragment of it and try to convince his colleagues that some formula in that fragment needs correction or replacement. His colleagues will consider only such proposals which, in their opinion, were formulated by people who properly "see" the existing theoretical structure.

It can be said, that only those persons who have been properly trained in dogmatism, that is who have learned how to reconstruct and interpret, may develop science (modify a theoretical formation). Practicing science can thus be viewed as a twofold activity: it is the essence of science that it can be creatively practiced but only by those who have manifested high conformity, that is, who have adapted themselves to the rules of reconstruction and interpreting. What is commonly referred to as practicing science is just a part of that activity and it consists of the procedures of changing a theoretical formation. I shall call the activities of such a kind *developing of a theoretical formation* .

Let us describe in rough terms what such developing consists in. Let us imagine that our exemplary potential theoretician has learnt everything he could accomplish within a given group. That means that he has mastered a practical skill of educing a given theoretical formation (I assume a simplifying condition that he is able to reconstruct the whole formation not only its fragment). "Having seen" it, our investigator makes a decision that will work creatively just within its frameworks. He thus analyzes critically its fragments because he searches for its weak points, that is, formulae which in his view are susceptible to correction. The search in question is a part of investigator's strategy that is supposed to strengthen his position within a scientific community.

When he has detected formulae which he believes to be susceptible to correction, he selects one for "improvement". He looks for such formula whose possible correction will be highly estimated by his scientific community. Finding it a scientist performs, or rather tries to perform, its modification. Since formula is a function of special kind, its modification will consist in finding such function that satisfies certain criteria to a higher degree. The criteria are used to estimate the cognitive value of a formula. If all conditions are satisfied then that new function is "added" to the hitherto system of formulae. Such inclusion of a new formula into existing theoretical formation contributes to its change. This change will be called the development of a theoretical formation.

It is just this process of developing a theoretical formation that is traditionally identified with the true scientific research. Searching for new formulae the investigators reveal features which are so highly estimated by a lay public: criticism and inventiveness.

In our picture of scientist's conduct his criticism consists in "testing" the formulae belonging to a theoretical formation to find these formulae which are susceptible to modification. And inventiveness manifests in the ability to modify the found "weak" formula. But those procedures can be performed only by a person who is able to educe the hitherto existing theoretical formation, that is the person who can reconstruct and interpret according to standards observed by members of scientific community. Furthermore, such conduct should be in accordance with criteria of evaluation of formulae and with principles of developing a theoretical formation. They are also imposed on researcher by scientific community. Only if all these conditions are jointly satisfied a person has a chance to be acknowledged as a creative scientist.

The attitude of a participant in science consists of: reconstructing, interpreting and developing. It has to be stressed that the "genuine" scientific research must be preceded by a period of inculcation in candidates for scientists certain skills. These reconstructing and interpreting skills result in the predisposition to scientific research. Thus the nature of scientific conduct is composed of two factors: predisposition to scientific research and the research proper. It is impossible to practice science without the appropriate predisposition (without skills of reconstructing and interpreting), whereas predisposition alone does not suffice to become a scientist.

We shall say that a given person is predisposed to scientific research (or, in short: has scientific predisposition) if, following the rules standing in a scientific community, he performs categorization and codification of scientific utterances as well as educing formulae and then a theoretical formation.

If we assume that to tasks of the philosophy of science belongs also research of the scientific attitudes we can say that such research should cover the problematic of scientific predispositions and the problematic of practicing science. Succumbing to some terminological fashion we can say that philosophy of science should comprise the hermeneutics of science, i.e. the study of scientific predispositions and the heuristics of science, i.e. the study of practicing science. The former would investigate dogmatic processes in science, whereas the latter -innovative processes. I have tried to argue that the nature of scientific conduct is composed of processes of both kinds.

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