ISSUES IN INTEGRATIVE STUDIES
No. 17, pp 87-120 (1999)

Interdisciplinarity and Transdisciplinarity:
A Constant Challenge To The Sciences

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Abstract: The following text will present an outlook on some new forms of supradisciplinary collaboration where traditional disciplinary bounderies are crossed. It will be shown (a) that the decision as to which form of supradisciplinary collaboration has to be chosen depends strongly on the quality of the given scientific problem; (b) that there does not exist any scientific hierarchy prefering transdisciplinary approaches versus interdisciplinary or multidisciplinary ones and vice versa. The suggestions are empirically based on various observations and experiences with research programs in the fields of Ecology realized in Switzerland and Germany. As the discussion in Europe on supradisciplinary collaboration during the last three decades has specially progressed in the German speaking part of Europe (Germany, Austria, Switzerland), the text mainly discusses important contributions in German literature. After a general introduction we first talk about the expectations (part 2) and the methodological preconceptions (part 3) which are related to these forms of research. In part 4 we clarify the term *Interdisciplinarity*, and in part 5 we offer a casuistry – an argument using general principles of ethics to determine right and wrong in questions of conduct – as a way of discussing how an interdisciplinary approach can be realized. In part 6 and 7 the discussion is then focussed on the nature of transdisciplinary approaches, followed by a conclusion. American readers have to be aware that in Europe the term "science" is used in a less restrictive way and beside natural sciences includes also technical and social sciences as well as humanities.

key words (in alphabetical order): ecology, interdisciplinarity, methodology, multidisciplinarity, philosophy of science, terminology, transdisciplinarity.

I. Introduction

Not only did the participants at the United Nations Conference on Environment and Development in Rio de Janeiro in 1992 open up a new area of activity for the sciences with the theme "sustainability," they also proposed just how the work should be carried out: future research would have to be organized in an interdisciplinary manner (cf. part 35.22 (a) of Agenda 21) (see Internet link: gopher://unephq.unep.org:70/11/un/unced/agenda21). Hence, a discussion which had quietly taken place in academic circles for some time, suddenly became relevant for the policy of science. It was discussed publicly in a positive manner and adopted in research policy: good, creative and innovative research which is worthy of subsidies should be interdisciplinary and in particular, but not exclusively, involve the environmental area.

The actual discussion

Silvio Funtowicz and Jerry Ravetz, two European scientists mainly working in the field of Technology Assessment summarized the actual discussion on the potential of sciences on behalf of the environmental situation most suitably with the following sentences: "We have now reached the point where a narrow scientific tradition is no longer appropriate to our needs. Unless we find a way of enriching our science to include practice, we will fail to create methods of coping with environmental challenges, in all their complexity, variability and uncertainty." (Funtowicz 1991, 151) A similar line of argument is followed by Gibbons and his co-authors in their well known thesis in which the authors claim they have described a new trend in the production of knowledge. Whereas knowledge has been traditionally acquired within the academic framework, which is characterized by homogeneity and mainly organized in a hierarchical manner, the new form can be characterized as follows:

- *The production of knowledge is within the context of application, i.e. the process of obtaining knowledge is characterized by the*

continuous negotiation of interests between the various partners involved (p. 4).
- *The production of knowledge occurs in a transdisciplinary way whereby the result, as a rule, exceeds that of the contributions of the individual sciences. (p. 5).*
- *A notable aspect of the production of knowledge is its heterogenity and organizational diversity (p. 6).*
- *It is socially acceptable and is reflexive (p. 7).*
- *The quality control of the knowledge produced is not only judged by scientists who are active within the same discipline according to internal criteria, (peer reviewers), but also by the criteria of competitiveness of the market, cost effectiveness and social acceptance (p. 8). The first means of producing knowledge is called Mode 1, the latter Mode 2.*

At the moment, a lot of scientists are looking at this development as a rhetorical challenge. It is not enough to submit a well formulated proposal according to disciplinary criteria in order to obtain funding for a project. The project also needs to be presented in an interdisciplinary manner in order that the chances of success are not jeopardized. As well as the existing requirements (i.e. that international or non-scientific partners be involved in a project), an additional hurdle has been created for grant-catching-virtuosi applying for support.

It would be interesting to examine just how these requirements, emerging from considerations of science policy and amenable to interpretation in a rhetorical and opportunist manner, guide the direction of research (see Weingart and in addition Balsiger/Kötter, both 1997). However, we want to concentrate on the following questions:

(a) what expectations, however vaguely formulated, are bound up in these requirements and
(b) whether these can be realized at all by means of organizational support, and if so, in what way?

II. The Expectations Connected With Interdisciplinary Oriented Research

If the discussion on the interdisciplinary concept were to be pursued over a longer period of time, it would be discovered that the driving forces behind it are reasons which stem from experience with the dual functions of science, namely as the source of *theoretisches Orientierungswissen*, as well as knowledge of practical problem solving. As far as the first is concerned, noticeable disquiet has been accumulating for a long time about the fragmentation of the scientific system. The term *theoretisches Orientierungswissen* is hardly to be translated, approximately it could be done with the label "the knowledge of theoretical orientation". In German its meaning expresses a view of the world and humankind based on scientific description and interpretation.

The history of science has displayed an accelerating differentiation of disciplinary units, a process which, despite all the positive accompanying achievements, has at times been considered painful and even dangerous. The German philosopher Jürgen Mittelstrass for example registered with concern the increasing differentiation of the scientific system into 4000 disciplines (Mittelstrass 1996, 7). On the basis of having tracked research activities involving a number of disciplines, he finds no possibility of halting the development. At the same time, he realizes that a compensatory effort is being made involving the reappropriation of the idea of unity, of a scientific system that both opposes the tendency to atomize subject and specialists and that becomes indispensable (Mittelstrass 1989, 102). After all, modern science has always made great demands on orientation — its aim is to impart a unified understanding of the world, free of metaphysical and religious elements. Still, the hope that the development of science would bring mankind closer to this goal has been continually unfulfilled. Each step towards a new discovery has led away from the goal of unified knowledge.

Closely associated with this loss of orientation is the problem among scientists of an increasing inability to communicate. Hence an earlier golden age is envisaged in which scientists spoke a common language and could thus make themselves understood in regard to their work and their aims. Today, on the contrary, we believe that we live in the midst of a confusion of languages in which individual scientists within the scientific community are isolated, regarding themselves as being bound within narrow specialist fields. To many researchers, anything outside the confined area of activity is foreign and they react, as do the majority, to anything strange with suspicion, rejection, and aggression. An appropriate example of this is to be found in the handling of the controversy about Alan Sokal's contribution in *Social Text* (Spring/Summer, 1996. For almost the whole debate on „Sokal's hoax," check the Internet link: http://

www.physics.nyu.edu/faculty/sokal/).

These two problems are joined by a third. From the time of the Renaissance, science has always been associated with strong claims, both emancipating and humanistic. Today it would appear that during its development, science has lost this association. Even worse, almost all contemporary forms of exploitation, oppression, war and environmental damage are based on scientific knowledge. In the environmental area especially, the application of science has had problematical consequences which cannot be managed within the framework of science. Recent development is regarded as a consequence of the inner dynamic of modern natural sciences which have developed by means of disciplinary differentiation, which means that the broader relationship is lost. The competence of modern science to solve problems has to be challenged in a very special way. When scientific rationality takes place, problems are created which can't be successfully handled simply within this framework of rationality.

In reaction to this specialization, which is blind to interrelationships (Hubig 1997), there has been and will continue to be within the sciences a need for a scientific global vision requiring continual updating. Thus, what is absolutely essential is a new integral or holistic concept of the environment that permits the transgression of disciplinary boundaries. However, the impetus of this type of holistic philosophy touches upon an unfortunately selected metaphor: A traditional discipline is characterized by a particular subject, as well as by a method which establishes specific description and explanatory tasks for the said subject. The criticism is that the advocates of such disciplines ignore the fact that their subjects which are, so to speak, blocks of a building set will become units of an extensive building, where the blocks are then recognized as whole units. This complex subject could also be treated in another manner, but not in one common to the specialization. If sciences manage to provide a broader perspective, attention will be attracted to a new subject which will then increasingly adopt those contours of the world in which we live.

"Holism"

The word "Holism" is seldom used in daily language. Its character as an unusual word indicates that its purpose is essentially technical and specific to given fields. It is more common to use the word "wholeness," which in the biological context in particular implies the popular expression *that everything is dependent upon everything else. What is meant here is that an organism or a particular part of it must be considered in relationship to the environment in which it is found and its interaction with that environment. This, however, stimulates discourse as to the concept. Quite often, in this discussion, proponents of a holistic approach insist that it is necessary to the development of understanding, while opponents warn that the same approach could lead to irrationality and cultural retrogression.*

Technical application differs slightly in the biological, sociological and scientific-theoretical contexts, as well as in philosophical semantics. Our interest concerns only the biological and sociological usage. The term in the biological sense essentially means the derivation of all phenomena of life from a metabiological principle and thus opposes particularly the mechanistic but also the vitalistic approach. In the sociological context the term indicates that social relationships can only be clarified and explained in the conceptual form of the integral social whole. Accordingly, the individual has scientific significance only as a systemic sub-division of a particular social group.

Karl Popper expressed a crucial objection to holism in an article (1944/45) with the same title as his later book, The Poverty of Historicism. Popper rejected the attitude that sociology, like all sciences, should treat living objects using a holistic instead of an atomistic approach (1963, 17) Noting the fundamental linguistic ambiguity of the term "holism," he claimed that wholeness, in the sense of the whole, cannot be used as a subject for scientific investigation (ibid, 78). His claim involved two crucial points: First, in order to examine a subject, certain aspects need of necessity to be selected, it being impossible to remove an entity from nature in order to observe or describe it, because each description is, in itself, selective (ibid, 77). Second, infinite regression follows necessarily from the assumption in sociology that all social and all personal relationships can be ascertained sociologically because for every established relationship there could be a new one, not yet considered or established (ibid, 81).

The type of ecology based on the so-called "Gaia-hypothesis" can also be considered in terms of Popper's argument against holism. The "Gaia-hypothesis" was originally formulated by James Lovelock and modified later by Lynn Margulis. It argues that the world is a "super-organismic system" and that evolution is not the result of a competitive but rather a cooperative process. If the planet Earth is viewed as a super-organismic *system, then a hypothesis of this type can only be understood as a scientific crutch because of the impossibility of experimental proof. Refutation in the scientific sense is impossible and scientific statements can only provide more or less supportive arguments for the plausibility of the Gaia hypothesis.*

Fifty years later, each of Popper's points and arguments can be used against the understanding of ecology as a science, in as far as ecology assumes that scientific statements have to be based on wholeness, on an examination of all relationships within the entity under study. Favorably considered, the demand for holistic orientation in the sciences can be interpreted as the demand for a systemic point of view, and thereby the adoption of a subject into an environment or a context. However, with this interpretation there remains a certain inaccuracy of language usage for there is an essential difference between the holistic and systemic approach. A system is defined by its limits whereas a holistic entity is not.

Because scientific knowledge is temporal and limited to certain areas, holism can, at best, be considered a utopian scientific aid to orientation, but never a methodological means for obtaining scientific knowledge.

However common the idea of the "scientific subject," it is difficult to demonstrate in a tangible way. Either the subject is outlined briefly and its purpose remains a vague generality, or it is sketched in detail, thus only presenting substitute examples. The origin of these problems lies in the weakness of an epistemological concept, commonly used among scientists who generally pursue a naively depicted form of realism. They cultivate the assumption that scientific subjects exist (whether as a part or as a whole of an entity is irrelevant) and that they only have to be demonstrated correctly. However they ignore the fact that there is no other access to these subjects than through the sciences. Every attempt to characterize them in a non-scientific way leads, of necessity, to fruitless results.

III. Methodological Preconceptions For Interdisciplinary Research

If the expression "subject of science" is considered to be an indispensable term, an active creative meaning for it should be used. Anything can be made the subject of a discipline as long as it can be demonstrated, described and explained by means of the methods of the discipline which are demonstrated in paradigmatic cases. In the philosophy of science it is said that a discipline is characterized by its *"research program"* (Lakatos 1974). A research program of this type does not state *what* the subject of the discipline is but *how* something becomes a subject.

"Research Program"

The colloquial expression "research program" more or less implies a loose association of research projects. There can be a number of reasons for these associations. In most cases they are thematic but they can also be economic or political, particularly in regard to supranational projects (space research) or major research facilities (CERN). The "Intergovernmental Panel on Climate Change " (IPCC), founded in 1988, is an outstanding example of a politically inspired project. A research program of this type is usually limited as far as time is concerned and varies according to the rigidity of the formulated aims. Occasionally methodical parameters can be arranged for individual cases. Examples of research programs of this type are the "Sonderforschungsbereich" in Germany and the "Schwerpunkprogramme" (particularly the „Swiss Priority Programme Environment (SPPE)" within the framework of the Swiss National Science Foundation.

In the philosophy of science the term "research program" is used in a terminological sense and has an established meaning. This was determined by the Hungarian-British scientific historian and theorist Imre Lakatos (1922–1974). He coined the concept subsequent to the controversy over the ambiguous "paradigm" used by Thomas S. Kuhn (1922–1996). He wanted to convey just how scientific development occurs. What he meant by his "research program" was a form of scientific argumentation. Such a program would consist of a complex of disciplinary (relatively) fixed hypotheses and heuristic arguments from which the scientific content of the discipline is created. Each research program contains a so-called nucleus consisting of methodological principles and fundamental (empirical) assumptions. These can be used to determine just what is an accurate description in a certain discipline, and what facets are in need of explanation or are capable of explanation. In his research program Lakatos used, parallel to this nucleus, prohibited rules („negative heuristic arguments") and permitted rules („positive heuristic arguments"). Negative heuristics imply that the nucleus of the research program cannot be impinged upon. Positive heuristics show how the nucleus is applied in a productive manner. Hence research strategy is rewritten providing research *activities a certain stability, for the obstacle of serious refutation attempts and objection is great. A research program of the type propounded by Lakatos is successful when new empirical facts can be achieved without emphasizing negative heuristics too strongly. For further details see Inhetveen/Kötter (1994) as well as Kötter (1992).*

These thoughts will be explained using a trivial example from everyday life (a simple example was chosen intentionally in order to avoid the immediate occurrence of specialized questions). A mother plays tag with her small daughter; the chase goes around a tree, whereby the woman remains just behind her daughter. After a series of circuits the mother slips on the wet grass and falls. An observer of this scene could ask why the mother couldn't catch her child? In order to provide a possible answer, from a mechanical point of view one could treat the "subject" as "movements of material bodies in space" and by doing so one would demonstrate that the mother obviously displayed insufficient energy for her mass to be accelerated at a speed greater than that of the child. This answer would not be wrong but entirely unreasonable. A satisfactory answer would be that the mother did not want to spoil the child's fun and as a result only pretended not to catch her. However, if the causes of the fall are to be considered, explanations concerning the reasons for the game would be irrelevant. In this case it would be correct to view the mother initially only as a material body subjected to differing forces which in combination upset her equilibrium.

As a consequence it can be seen that in the first case a physical answer is inappropriate not because a human is "*more*" than a subject of physics but because the context of the question does not allow the *human* to be treated *as such a subject*. It is precisely the physical point of view that is required in the context of the second question. Taken further it becomes obvious that it is not necessary to have a complete description of the human as a whole in order to ask relevant questions in a sensible manner and to obtain appropriate answers.

Let us take a further step and assume that the mother was injured as a result of the fall and had to seek medical assistance, with possible health insurance expenses. As far as the insurance company is concerned the following questions need to be clarified: (a) Was the injury entirely the result of the fall? (b) Was the fall itself due possibly to gross negligence, so that there would be no insurance liability? These questions turn the physical events described above into a complex situation. The "material body" is described from a medical point of view as a biotic structure (including the injuries) and its "movements" are legally considered as actions which are subject to social and legal requirements.

This tiny example from daily life was used to demonstrate two things. First, the manner in which the facts are stated is dependent upon the *problem posed*; following the line from the simple question "What are those two doing there?" to the legally accurate description of the action, supported by extensive evidence, we get neither closer to nor further from "reality." Second, it becomes obvious that the complexity of the posed problems cannot be dealt with by "inventing" new and more complex branches of science. At a suitable point in the development, questions will pass from one discipline to another e.g. from physics to medicine or law. In other words, complexity can rarely be controlled by trying to deal with it in toto. It can be reduced in a well measured way for precisely defined problems. This is the crucial point where interdisciplinary research becomes important. At this point, in order to consider the point more closely it is necessary to digress and then recapitulate the general methodological basics of a scientific discipline first.

In a few words, it can be said that all sciences provide abstract information about ideal objects, states and processes. Scientists initially have to deal with specific forms of idealization to make facts available which are not represented in a disciplinary way; therefore characteristics which are, for example, formulated in ordinary language, may be suppressed by following a factually given form of representation, or the expressions are often changed in a contrafactual way. Some points are emphasized while others are ignored.

Using an equation of reaction as a form of representation in chemistry, for example, means that no factual agents but only pure ones are connected; in mechanics the dynamics of bodies are represented as the orbits of solid bodies resulting from corresponding differential equations; in biology morphological and functional types develop, as do geomorphical types in geography, and so on. In order to idealize it is necessary to have a representation to which the process of idealization can be applied. Sometimes these representations are given in day-to-day language but in most cases they are formulated in a terminological mixture using terms of various scientific as well as non-scientific resources. The means of using the techniques together with the art of disciplinary modeling constitutes a great deal of the training of an empirical scientist.

If scientists talk in a specific and ideal manner about disciplinary objects, then specific statements about a discipline are realized (e.g.

physical laws or the chemical equations of reaction). The term "*abstract*" used in this context means that only the idealized character of an object is envisaged. If, for example, the chemical equation of reaction $N_2 + 3H_2 \rightarrow 2NH_3$ is written, the symbol "+" is equivalent to the interconnection of pure agents without supplying any further information of the way these agents are interconnected. This means that the technical realization in particular is of no importance. The sign "→" symbolizes the result of the reaction without giving any information, for example, about the time taken for the reaction.

To give another example, if a behavioral biologist makes disciplinary statements then the observation data are upgraded in such a way that the specific context of observation disappears. This abstraction process is best seen in physics, where natural laws are stated in terms of relations among physical units only, and all substantial peculiarities of the parameters go unstated, as do such technical conditions of the claimed relationship as the way an experiment is carried out. Finally it has to be mentioned that even in the social sciences, the same process is intended as soon as individuals are idealized as role types and their functions represented in an abstract way by means of modeling a social system. Supradisciplinary collaboration commences as soon as the abstract representation of facts is concretized by facts typically treated by other disciplines.

Let us continue with the example of a chemical equation of reaction. In chemistry an equation of reaction is always realized by means of an experiment in the laboratory. If there is an interest in how the reaction is obtained in nature (e.g. either in an organism or in the atmosphere) or in technical engineering (e.g. in a technical plant in the chemical industry), then the conditions of realization have to be outlined by scientists from other disciplines — in the given example, by biologists or engineers. These participating disciplines naturally have always had their own patterns of abstraction which can again be replaced by others. This means that a chemical reaction can be represented by a chemist, the overall technical execution by a chemical engineer, and all these technical processes can finally be represented in the form of a cost-returns ratio by an economist.

Stable supradisciplinary relationships have long been established in the history of science. Good examples of this mutual *concretization* of abstract representation can be found in the relationships between chemistry and physics, biology and chemistry or between economics and engineering. Close cooperation in these fields has already left institutionalized traces.

Institutes for physical chemistry, chemical engineering, biochemistry or business administration and engineering have been founded which cover curricula represented in each field of education. One could argue that these cases are not specific for the development implied when talking about interdisciplinarity because it could be added that the combination of similar experimental methods or mathematical models favours close cooperation strongly.

In a general sense this statement is definitely wrong. Originally physics and chemistry, biology and chemistry, engineering and economics had few common points of contact. Over the years these disciplines have established common areas which have altered the pattern of determining scientific questions as "important" or "interesting." The autonomy of the research programs involved has not, however, changed at all. Hence, chemical thermodynamics is not an alternative to physical thermodynamics; rather, each is an application to a special class of problem. Cases like these are simply the consequences of two definite conditions. First, the disciplines involved must have reached a stage in their development where the level of abstraction in their forms of representation is sufficient to the task of bridging the gaps between their typical facts. Second, a concrete problem must exist which necessitates departure from the abstract representations of one discipline in favor of another discipline.

IV. Interdisciplinarity As A Terminological Problem

Keeping these methodological reflections in mind helps when looking at research, especially at the enormous variability in the different forms of supradisciplinary combinations. There is a large spectrum of various forms of collaboration, ranging from a non-specific to occasional mutual interest in the work of other scientists to a clear and well defined research mandate. So for example, a philosopher may be interested in the way scientists of literature deal with their texts, or a specialist in organic chemistry occasionally may be interested in the kinds of problems his colleagues in non-organic chemistry are faced with, or, again, someone in process engineering may be interested in the state of the art of semi-conductor technology.

On the other side of the spectrum the relationship among disciplines involved can be defined as a sort of service, which is an important issue in scientific practice. A specialist in hydrodynamics requires a mathematician to solve problems in numerical maths which can arise in some special simulations; a biologist analyzing a specific physiological process could require the know-how and the well-developed techniques of analysis used by the biochemist. It may be recognized as a characteristic feature that a scientific discipline, mandated with a special type of service, does not influence the integration of this given question into the research program of the mandating discipline. The serving discipline doesn't even have to know a thing about the context to fulfill the given mandate. On the other hand, the customer is only interested in the result and does not influence the way the engaged discipline treats the given problem theoretically and practically. To put it briefly, one talks about a serving relationship among scientific disciplines as soon as discipline A passes a special topic on to discipline B which formulates it in its own terms, and the judgment of the result is uniquely part of the sovereignty of discipline A.

Let us give an example. If a historian is interested in the composition of the alloy of an ancient coin, he will pass it over to his colleagues from the material sciences, who then will analyze it by following their own disciplinary standards and finally achieve a result. When their work is done it will be within the responsibility of the historian to give a historical meaning to the result of the analysis. Such a strict separation of methodological responsibilities is typical for a serving relationship between disciplines.

Two extreme positions of relationship exist between scientific disciplines. On the one hand a loose relationship, based on academic interests and without any obligations, and on the other, a close relationship based on a serving contract. All forms of disciplinary contact resulting from the orientation of different disciplines towards a common scientific problem can be found between both positions. This is the case when jurists, economists, psychologists and sociologists analyze aspects of the topic "unemployment" within their own particular fields. Various forms of collaboration can be found which range from a simple exchange of information and working data to the realization of a joint research project.

Terminology for supradisciplinary scientific practice

Often the term interdisciplinary is used in a general and unspecific way. But it would be more sophisticated to reserve the term for a specific use. Thus the term supradisciplinary scientific practice is suggested as a collective term for all forms of scientific collaboration where the field of a *single discipline is transgressed. The terms multidisciplinarity, interdisciplinarity and transdisciplinarity are not described in this box for they were defined in the main text. The following list contains specific terms for supradisciplinary scientific practice in alphabetical order. It is not a complete summary of all terms ever used in this scientific debate. It is just a selection of terms used in literature for which a short description is given. Differentiations arising from the adjectival usage of the term "interdisciplinary" are not given.*

Co-disciplinarity: The American scientist Margaret Barron Luszki only used this term once in her study "Interdisciplinary Team Research. Methods and Problems" which was published in 1958. She does not give any further details about the meaning of the term or the manner in which gap-filling collaboration is to be arranged. When looking at the particular section of the text in which the term is used, it can be assumed that she suggests a close relationship to the term interdisciplinarity when she explains: "This kind of interdisciplinary or co-disciplinary research, which is of the gap-filling variety, is the kind of interdisciplinary research that I conceive to be a valuable method of approach at this stage of our development" (Luszki 1958, 119). Because of the casuistry same problem where the term occurs, it is assumed that she means the description of a closer form of scientific collaboration between two scientific disciplines.

Crossdisciplinarity: The term was also used by Margaret Luszki but only in the adjectival form. It is used together with the terms "training," "identification" or "comprehension" (Luszki 1958). A precise definition for this term is also lacking. However, the meaning of the term generally covers the same field as the term supradisciplinary scientific practice. At the famous OECD-Conference held in 1970, on Interdisciplinarity in Universities, organized by the Centre for Educational Research and Innovation, Erich Jantsch, one of the main speakers, did not explicitly refer to Luszki when using the term crossdisciplinarity. In his model, which was hierarchically organized, the term is used to describe a position between multi and pluridisciplinarity on the one hand, and inter and transdisciplinarity on the other (Jantsch 1972). According to Jantsch the term crossdisciplinarity provides a new interpretation of disciplinary concepts and goals in the light of a specific disciplinary goal and forces the disciplines to formulate their well defined controversial positions. This term hasn't been adopted.

Condisciplinarity: This may be the most recent creation in the diversity of terminology. It was introduced in 1990 by Hans Heinrich Schmid, the *president of the University of Zurich, in a public lecture published in a magazine by the University of Zurich, entitled, "unizürich" (Schmid 1990). Schmid was looking for a term that more appropriately described the problem of interdisciplinarity. He felt that the term condisciplinarity described the goal oriented character and scientific performance better than the collective term interdisciplinarity. Schmid explained "What is wanted is true collaboration, a joint effort of various disciplines, scientific problems and methods." This additional dimension led him to define "the outlined goal of supradisciplinary effort with the term condisciplinarity." It is characterized by the situation where an object of research is required, which in turn generates new scientific questions (instead of the question concerning the relationship of a discipline).*

Infradisciplinarity: The term is only known in the adjectival form. It is used prototheoretically and was created by the German philosopher Paul Lorenzen (Lorenzen 1974). He summarized all those prerequisites which are the common basis for all scientific disciplines (e.g. logic, ethics and the philosophy of science). Thus, a condition for the possibility of specialized disciplinary practice is that these prerequisites precede any scientific activity. The term describes a form of cooperation which is not oriented towards integration of any scientific achievement, but which finds comparable kinds of problems in various disciplines.

Intradisciplinarity: This term too was used by Margret Luszki to describe contacts established in a scientific discipline (e.g. psychiatry) (Luszki 1958). Thirty years later the term was used again by the German psychologist, Heinz Heckhausen. It is assumed that Heckhausen didn't know about Luszki's terminological primacy for she was not mentioned in his work. Heckhausen himself created the term after the analysis of projects carried out at the "Center for Interdisciplinary Research" at the University of Bielefeld. The result of his analysis was that, in the majority of projects of this nature, when the criterion of a "theoretical level of integration" is used the process is not interdisciplinary but rather intradisciplinary. This means that within projects of this type, scientific collaboration usually occurs among disciplines with the same theoretical level of integration. Therefore pure interdisciplinarity does not really take place (Heckhausen 1987).

Pluridisciplinarity: According to Erich Jantsch, pluridisciplinarity is the first step of collaboration among various disciplines (Jantsch 1972). The relationship between the disciplines concerned can be enhanced and collaboration is not regulated. The term is no longer used.

It is occasionally mentioned that there is no need for the tentative introduction of the term "interdisciplinarity". It is claimed that a "pragmatic" use of the term is enough, because even the term disciplinarity is not very well defined (see our reference on Lakatos' solution in the box "Research program"; for the German literature see Kocka 1987). This is not the opinion of the authors of this paper. Even if it is claimed that the development of disciplines does not really follow the criteria which are used in the philosophy of science but follows rather the criteria of the general history of culture and facts in the history of institutions (both seem to display a "pragmatic" element), this doesn't mean that forms of collaboration between factually and institutionally determined disciplines cannot be described more accurately. As far as the terminological concept presented in this paper is concerned, it is decisive to emphasize the potential of both theoretical and methodological, as well as the practical and organizational problems related to the form of interdisciplinarity.

Any form of unspecified collaboration will be called *multidisciplinary* and the term *interdisciplinary* will only be used for those forms of supradisciplinary collaboration where various disciplines, keeping their own autonomy (i.e. without becoming a serving discipline), solve a given problem which cannot be solved by one discipline alone, in a joint way. As soon as a given problem raises from outside of the scientific context and it has to be solved in the form of a joint collaboration between scientists and practitioners, the terminological suggestion is to use the term *transdisciplinarity*. But there is a special danger which has to be taken in consideration. Transdisciplinary projects should not be loaded down with tasks which do not belong to the scientific context. In no way can the implementation of suggested solutions into practice be carried out by science as a *substitute* for practice. If this occurs there is a definite danger of science drifting into ideology.

H. Zandvoort (1995) also focused on the methodological collaboration outlined, in a paper of note in which he concentrated primarily on ecological research. In his opinion, scientific disciplines in an interdisciplinary research project-group are related in a so-called "interactive model." There is no strict hierarchical relationship between the participating disciplines in a model of this type but merely a "guide-supply-relationship." This means that in the "guide-mode," a discipline formulates a task, which is adopted and dealt with by another discipline which is claimed to be in the "supply-mode."

> The cooperative development of research programmes comes about in the following way. Some of the research programmes do not define their own primary problems. Instead, they aim at solving problems arising in and defined by other research programmes. The latter programmes may not themselves have the (efficient) means to solve those problems. The programmes generating the problems I have called 'guide programmes,' because they act as guides for the programmes that aim at solving those problems. The latter programmes I have called 'supply programmes,' because, when successful, they satisfy the needs of the other programme. (Zandvoort 1995, 53).

This means, in particular, that abstract descriptions which arise from the perspective of a discipline that is actually in the "guide-mode" have to be concretized in such a way that new tasks are created for disciplines in the "supply-mode." It is then decisive from our point of view that both the direction in which the abstraction is to be put into concrete form as well as the other disciplines can only be gained by means of the initial nature of the problem, as has been shown by the above example from daily life. A serving relationship differs from a guide-supply-relationship, for in the latter the discipline is not and does not remain solely responsible for the treatment of a given problem but merely takes over the additional task of distributing complementary research projects (recall our example of the ancient coin from above). Even if one discipline is especially recommended as the first partner in the treatment of a problem, such as climatology and the carbondioxide problem, that problem will be developed by consulting further disciplines. Finally, there must be a guarantee enabling the guide-supply-relationship to change during the execution of the project, which will never be the case in a serving relationship.

This is then the keystone of the problem of interdisciplinarity. An interdisciplinary research combination only attains a unique position amongst all disciplinary research if it is based on a well-defined *problem* which interrelates various research programs, as has already been explained. A problem needs a solution which forces various scientific disciplines to collaborate in a specific way. In contrast, a thematic topic only needs to be treated and the various forms of treatment only need to be considered. *A priori,* there is no need for interrelationship between these forms. Therefore, the decisive step to successful interdisciplinary collaboration is carried out through the formulation of a *structural* *description* of the given problem. It can be seen in a description of this nature:

(a) that the given problem cannot be handled by one discipline alone, and
(b) what expectations each discipline has in regard to the contributions of other disciplines towards solving the given problem (i.e. which guide-supply-relationships exist between the participating disciplines).

In order that common ground is found for *structural descriptions* of this type, the above mentioned expectations have to be adopted and accepted in a natural way. This means in particular that a common language for describing the presentation of the problem and its results needs to be agreed upon, and that explanations must be given for the specific heuristics which in any discipline are needed to distinguish genuinely scientific problems.

The fact that interdisciplinary research is only successful if it is based on and driven by a concrete problem is greatly underestimated in research practice. The reason that a lot of so-called interdisciplinary projects end up unsatisfactorily, that scientists are disappointed and turn away from projects like this or start to deal with interdisciplinarity only in an opportunistic manner, as was indicated at the beginning, is due to the fact that most of these projects begin research without an established goal. One believes that a scientific problem has been labeled, when at the very most only the outline of a theme has been formulated. The most important difference between problem-oriented research (which leads to interdisciplinarity) and theme-oriented research (which leads to multidisciplinarity) will be demonstrated by means of a fictitious case in the following part.

Problem-oriented and theme-oriented research

The term problem-oriented research was used by the Belgian philosopher of science, Pierre de Bie in 1970. However, de Bie's usage focused on a type of practice which will in the following text be described as transdisciplinary (see de Bie 1973, 9). Our terminology will help differentiate between the various forms of scientific disciplines. The essential differences are a close form of collaboration defined by the relationship of a problem and an open form which is guided by a theme.

By formulating a theme the conceptual frame is set, inside of which scientists dealing with the theme are free in the formulation of their own concrete scientific problems. All contributions then will be taken as elements of a set, which is bounded by the theme, but they don't need to show any closer relations with each other. One can work on themes, but one does not solve them.

A problem needs a solution, and the posing of a problem delivers the expectations and criteria that a good solution has to meet. Every step in a problem oriented research regarding its content or its organizational framework has to contribute to the solution of the problem.

V. A Fictitious Case Study On "Ozone and Traffic"

Let it be assumed that a research funding organization was motivated by public discussion and decided to undertake a priority program on "Ozone and Traffic." Usually when an announcement is made about a project of this sort, a list of constraints have to be fulfilled by the proposers. This list presents the thematic framework and outlines the direction in which the research programs are to develop. The thematic framework and direction of development define the group of scientific disciplines to be involved. It is assumed in the following example that the proposals submitted can be split up into thematically structured groups.

Group A: *Ozone in the troposphere*; this group should encompass projects coverving the field of atmospheric chemistry and involving toolmakers who might address questions about the kinds of instruments that are needed to measure ozone concentrations accurately.

Group B: *The toxicity of ozone*; the projects involved here cover the fields of environmental medicine (e.g. What are the effects of ozone exposure to risk groups?) or botany (e.g. What is the ozone damage to foliate plants?).

Group C: *NOx-, VOC reduction in vehicles*; these projects should cover the field of vehicle technology, chemical engineering (e.g. What are the needs to develop efficient catalytic converters?) or traffic engineering.

Group D: *Automobile and society*; the projects here should cover the fields of sociology or economics which are concerned with the problem of how society and mobility behavior are interrelated.

The proposals are then submitted to a committee for evaluation. Proposals are usually accepted or rejected using disciplinary criteria. In large projects of this nature one has to consider that various forms of collaboration will be presented. For instance, it could be imagined that environmental medicine would be very interested in the development of a simple instrument for measuring ozone. Conversely engineers would be interested in considering the needs and wishes of the users of their product. It is even possible to imagine collaboration between sociologists and traffic engineers. But even if the funding agency demanded collaboration it would still remain superficial and coincidental, as long as the participating projects have to prove their importance and significance within their own scientific disciplines. *Interdisciplinarity* cannot be decreed.

The best that can be expected of such a thematic arrangement is that it might lead to the recognition of problems that need to be solved using an interdisciplinary approach. For, even if every single result of our large thematic project is of top quality, the lot of them taken together represent only simple points on a given theme. The consequences of this kind of theme-oriented research will become drastically clear if we assume, contra-factually for the sake of the example, that medical practitioners in the field of environment had come to the conclusion that ozone levels in our area did not constitute a health hazard: Participants in the other partial projects might have been surprised at these results, but the results would have had very little effect upon their own work.

The reason that the topic "Ozone and Traffic" can even be used as a research theme at all indicates that the possibility of potential risk and its causes are felt. This means that researchers themselves are aware that a certain problem exists. If this aspect is considered carefully, it can be seen how theme-oriented research turns into problem-oriented research. The first step is to establish just what kind of problems the population relates to ozone. The second step is to check if the problems are of such a serious nature that further research on the topic is of value.

To continue the example, suppose that environmental medicine leads the way by posing questions in a guide mode, as Zandvoort would say. Perhaps environmental medicine as the result of experimental investigation discovers that a certain percentage of the population, when participating in strenuous physical activity, suffers from objectively measurable and subjectively sensed inflammation of the respiratory system when ozone levels rise above a certain level. An abstract scientific problem can be identified here. In order to concretize this abstract problem, two steps have to be taken. First, it has to be proven that so-called ozone-summer-smog conditions really do reach the experimentally measured limits with certain frequency. Second, it should be shown in a plausible way that increased physical exertion does occur in a normal living and working environment. If both are present, then a concrete health problem has been confirmed, which means it is worthwhile looking for strategies to overcome it.

In general a fact becomes a scientific problem when it either opposes recent judgment or explanation, or when it prevents or hinders, directly or indirectly by the limitations of the choice of skills, the pursuit of any specific target. Scientific disciplines are occupied with the theoretical part of this adjunction so it is their duty either to explain or describe a phenomenon in a way that places it within a class of well-known phenomena. If theoretical achievement of this sort is available, the practical aspects of a problem can be more easily handled. Because of causal explanations, for example, the emergence of a phenomenon can be prevented or its effects compensated. Functional explanations may possibly permit a skillful adaptation to the given problematical situation.

When considering the ozone case, this means that firstly the mechanism of causality, the reason for ozone-summer-smog, has to be elucidated. Chemists of the atmosphere are primarily responsible for this problem. It is then in the responsibility of those technical disciplines that have developed those machines, equipment or vehicles which emit NOx and VOC to draw up a concretization of the given problem. As soon as scientific and technical causality has been explained, a socioeconomic interpretation must be given. The use of machines, cars, and so on is based on a specific relationship between ends and means. These relationships must be explained in all detail in order to have an idea for the development of a strategy to reduce these harmful substances. Finally, the social sciences have the problem of determining the amounts of those substances which the population is willing to tolerate.

Assuming that all these relationships have been explained scientifically, the final result would be related to problem-induced expectations: It is known that a concrete health problem exists, and that the problem is a consequence of the toxic effects of ozone which become dangerous under given conditions. This information forms a good platform for political negotiation. In this form of disciplinary collaboration, each contribution by a participating discipline is developed from the originally formulated problem and each is defined by the scientific questions of the other participating disciplines, in content and depth. In addition to this it is also possible in a sense to get some feedback. Results which have been realized from a scientific discipline in supply-mode lead to modification and supplementation of the given problem, which was originally the base from which the scientific discipline in the guide mode started. This is the reason the specific case is labeled interdisciplinary in the strongest sense of the word.

However it also becomes clear that reference to the problem cannot be formulated too narrowly. As has been shown above, the sciences primarily give abstract descriptions and general explanations. The special case is only of interest in so far as it helps develop a model for a structure in a more concrete sense. Accordingly, the primary interest of problem solving is the development of specific types of solutions for complete classes of problem rather than with the individual solution. At best, it may be the task of scientifically based advice or technology to use scientific results for concrete cases in practice.

VI. Transdisciplinarity As A Terminological Problem

The example given above shows how problem-oriented research starts, and the specific qualities of problems which often arise, particularly in the field of environmental research. These problems are generated in daily life and are given form through the application of disciplinary knowledge. Scientific disciplines no longer seem entirely responsible for the recording or handling of problems of this sort. Research programs can no longer be simply described or evaluated scientifically. They are of a supradisciplinary character and to be solved they need interdisciplinary collaboration which is occasionally called transdisciplinary research.

In various publications on this topic, the already mentioned German philosopher, Jürgen Mittelstrass, repeatedly uses this specific word which means that interdisciplinarity in its correct sense is "in reality, transdisciplinarity" (1995, 52). This occurs as soon as "research leaves its disciplinary boundaries and begins to define and solve problems independently." (ibid, 52) In addition, Mittelstrass stresses that transdisciplinarity as a principle of research links the disciplinary organized sciences with the scientific future and at the same time with the living world ("Lebenswelt"), which has an internal form which is scientific, i.e. a form which is determined by the progress of science. In this sense the transdisciplinary future of science will also be the future of our "Lebenswelt" (1995, 53).

These explanations are not suitable. We suggest that any problem of the type under discussion should be treated in a completely new context to be established alongside science. Granting that it was Mittelstrass' intention to focus on problems which could not be treated by the scientific competence of a single discipline, we feel that it is overstatement to claim the progress of our living world is governed only by the progress of transdisciplinary research. An overview of various approaches can be found in Balsiger, Defila et al (1996) and especially in Balsiger (1996).

No doubt, it is specific to transdisciplinarity that the scientific problem which has to be solved is not only transgressing the boundaries of scientific disciplines but also the area of science in general. This arises when :

- the problem is generated in an extra-scientific field (the economy, politics, the living world),
- a solution to the problem is urgently required in this field,
- public opinion considers these fields relevant and
- it is brought to science in an institutional way (research tasks, financing of project)

As well, it is necessary to talk about transdisciplinarity as soon as science realizes that specific developments may also lead to socially relevant problems of which the public is not yet aware. As a result, in this case, science may have to adopt the special task of informing the public. It must be emphasized that the suggested terminology focuses in a rather specific way on a relationship between science and society which is itself linked to specific prerequisites. The term *transdisciplinarity* is therefore unsuitable for characterizing in general terms the mutuality between society and science. Against this opinion the German sociologist Peter Weingart (1997, 589–597, esp. 593) has stated that it helps both to reproduce the questionable dichotomy of internal/external and to misappropriate mutuality. But there is a misunderstanding. Our suggested differentiation begins when the internal/external dichotomy is clarified — when the situation concerning the level of expectations and the level of tasks has been explicated and, in the usual case, given shape in an institutionalized form (e.g. the announcement of research programs by funding institutions).

Research which crosses disciplinary boundaries is in a general sense neither new nor special. In engineering the pool of problems has been taken from extra scientific fields. In a similar manner, the history of chemistry, pharmacy and economics has also been characterized by this phenomenon. Faculties of natural science and engineering cannot be imagined without research per order of industry. Therefore the question is asked here whether “transdisciplinary research” doesn’t simply mean what was earlier called “applied research”? It is a fact that the points of contact cannot be overlooked but there is nevertheless one point which justifies another differentiation. All cases of applied research known as typical are cases of the production of knowledge based on the division of labor. Specific tasks arising from economic, organizational or personal problems are granted to research institutions by private or public clients. The results obtained from this type of research are then integrated into the clients’ own existing operational and decision making framework, i.e. the clients have the power of use of the knowledge produced in research. In the framework of UNESCO, the already mentioned Pierre de Bie tried to characterize *applied science* as the application and utilization of acquired knowledge in regard to its concrete and practical usage (de Bie 1973, 28). This formulation seems a little too narrow in regard to the constraint of the utilization of knowledge already gained but still makes the point concerning concrete and practical usage. It is the implementation into a well-defined process which is based on the division of labor that characterizes research as applied, even if it is scientifically creative and original.

However, if the use of the word “transdisciplinarity” draws attention to the extra-scientific origin of the problem, focus is placed on an entirely different and rather specific quality of this type of problem. It doesn’t arise simply as the result of a well-defined particular interest, but from the presence of various threatening interests. In these cases the interest holders are not individually defined. The problems involved concern the whole population of a specific region, or a whole society, or even the whole of mankind. In other words, all these problems concern public goods and the way of dealing with them. Furthermore these problems are not, as is normally the case in applied science, formulated in scientific terminology or even in a language close to it. Transdisciplinarity doesn’t deal with the suggestions of an Institute of Economics, nor is it concerned with the efficient management of a hospital or with the development by a faculty of engineering of a high performing pump for an industrial partner. What transdisciplinarity does mean is that problems affecting undefined numbers of people are tackled with the intention of establishing the manner and degree of the problem’s influence. Consequently, all knowledge gained from transdisciplinary research maintains the character of a public good, a characteristic of science. Unlike applied science, knowledge will not become the property of a public or private customer.

In a way, with this characterization of both interdisciplinary and transdisciplinary research, "pure" types of research have been presented. Increasingly research projects represent a form of hybrid composed of transdisciplinary, disciplinary, and applied research (the latter is especially important when dealing with industrial partners). A very clear example of this is the "Swiss Priority Programme Environment (SPPE)". This program first started with the thematic pre-requisites for all the so-called integrated projects (e.g. the topics waste, bio-diversity or soil). These pre-requisites were explicitly expected to instigate a transdisciplinary research process guided by the topics suggested. What generally happened was that some partial projects of an umbrella project began to collaborate in a more or less transdisciplinary manner and by doing so provided the opportunity for the addition of other partial projects which were mainly disciplinary oriented or even involved in applied science.

VII. The Nature Of A Transdisciplinary Research Process and Its Consequences

Both the universality and complexity of the problems within a transdisciplinary research process present a burden for the relationship between the scientific world and the population. This stems from the very different criteria used by the two partners in regard to the perception and judgment of problems. In the everyday world, events or processes in the environment become problematical as soon as they begin to disrupt the common relationship between purposes and skills (technical problem), or as soon as they give rise to questions or doubts about the pursuit of specific purposes (orientation problem). In the sciences, however, events or processes only become problematical when they become a challenge to the power of explanation and description of actual theories. The presentation of an experience of daily life cannot necessarily nor in every case lead to the formation of a scientifically relevant problem. On the other hand, not everything that seems important and of some scientific relevance is of importance to everyday life. The current discussion on biodiversity may be seen as typical; the transition from the level of scientific description to the level of perception in everyday life is so vague and speculative that no serious problem of public awareness has arisen. There are three reasons for this difference in point of view:

(1) Some environmental problems which are within themselves only of little relevance (they could be handled by means of well-known technical skills and with reasonable cost), become big problems because they are allocated symbolic value in daily life. These cases become substitutes for a whole class of problems (e.g. as soon as ozone-summer-smog is taken as a theme, people frequently mean traffic, which in general causes air pollution). When dealing with the public, it is necessary for scientists to develop a certain hermeneutic skill if they want to deal with problems which the public regard as theirs. In order that they have worthwhile feedback from the public, it is necessary that both sides develop a commensurate attitude towards the problem.

(2) In daily life and in the sciences, problems are defined according to different time scales. A problem in everyday life which appears urgent and has a tight time scale could be considered on the broader scale of science as insignificant. On the other hand, in daily life developments are often not seen as problems but do turn out to be of importance in the long term. There is a further difficulty here and that is that a problem like bio-diversity which science recognizes as a problem, is not treated as such in everyday life.

(3) In everyday life causal processes, even if they are of the same kind, are distinguished qualitatively according to the personal and institutional holders of the causal relationship. This is so because in everyday life the question of causation is closely related to the question of responsibility in its organizational, legal, and moral senses. This kind of distinction is not made in the natural sciences. Here the claim to general statements is realized by distancing as widely as possible from any specific context and in particular by speaking about causal relationships in an abstract way (hence they are invariant in regard to the material or to the carrier of the examined processes). As has been seen, this occurs at different levels; biologists describe the interaction of species, chemists the conversion of materials, and physicists mass and energy flows. In each case however, the description is free of its relationship to cultural facts which doesn't exclude the possibility that the description itself only makes sense in a specific cultural context, e.g. in that of scientific practice.

In fact social scientists do grasp at the connection between reason and responsibility and are in principle in the position to describe this interrelationship of actions which influence the environment more accurately. This would however assume that causality could be settled in complete detail during the process. This cannot normally be done in the field of social sciences. In other words this means that an environmental problem as found in everyday life cannot be tackled by the natural sciences or the social sciences alone. Each discipline is forced because of methodological reasons to ignore specific aspects of an environmental problem stemming from everyday life or only to refer to them schematically. This shows once more just why a new "holistic" environmental science cannot be established. Some steps of abstraction have to be carried out by scientists in order to accomplish descriptions or explanations. Where several scientific fields are involved, this means, figuratively speaking, that an object is to be presented and regarded from various perspectives at the same time. Although this led to some very interesting results in cubism, it would be difficult to transfer this to science.

Some consequences of judgment and form of organization result from these contextual specialties of transdisciplinary research.

(1) Firstly, it must be emphasized yet again that no *a priori* judgment on different forms of supradisciplinary practice in science exists (as was intended by Gibbons or Mittelstrass). In other words, a transdisciplinary research process is not "higher" or "better" interdisciplinarity. Fundamentally the same argument can be used concerning the relationship between disciplinary research and any form of interdisciplinary research. We argue that it is the posed problem alone which defines the needed form of disciplinary or supradisciplinary scientific practice. It is unfortunate to inflate disciplinary problems into interdisciplinary ones as well as it is unfortunate to try to limit a problem to disciplinary boundaries which the problem obviously transgresses.

(2) Secondly, where collaboration based on an internal development of scientific problems becomes necessary, where research programs reach out towards other scientific disciplines, initial problems in understanding and organizational detail may arise, such as the question of how to institutionalize these different forms of research. Experience has shown that it is quite easy to overcome such difficulties. Thus, interdisciplinarity should not be talked about as a challenge here.

(3) The situation is quite different when talking about transdisciplinary tasks. What is important here is to incorporate problems in a dialogue in order to prepare them for a scientific approach. This requires special rhetorical and hermeneutical skills on the part of the scientists. The first requirement is the ability to communicate the potential of their disciplines for solving problems to the uninitiated in their fields or even

to non-scientists. It is not enough to define fundamental terms which are specific for the discipline or branch, or to paraphrase. An explanation has to be given about the way in which the specific problems of the discipline will be formulated and modeled. Few scientists are able to do this. The second requirement is even more difficult. Scientists usually have something they want to say and they make adequate use of this skill. What they lack is the ability to listen, above all when the person they are talking to is not a colleague from the same department or branch. It is precisely this ability that is required for suprascientific dialogue, which certainly cannot take place if the participants only answer those questions they assume to be the problem of someone else. Here is a small anecdote to exemplify what is meant: during a coffee break at a conference it was observed how a famous climatologist was asked his opinion on climatic changes and its future by a layman. Instead of using this opportunity to learn what the layman felt about the topic and to hear what people accept about the situation or consider to be a problem, he nervously moved from one foot to the other and jumped at the first opportunity to offer a mini lecture on the subject of "Research on climate change — what is the position and what has to be done." In our experience this reaction is typical for most scientists: They are apt to regard their own relevant questions as the only relevant questions. Assuming that their questions are everyone's questions, they remain oblivious to input from others, especially lay people but also practitioners of other disciplines.

Special conditions are required for the planning period of transdisciplinary research projects. The formulation of a structural description of the problem, which is to be the fundamental basis of the project, cannot only be written in collaboration with scientists from different disciplines but must also take into consideration the opinions of partners outside of sciences. Such a collaboration between scientists and non-scientists requires a much greater utilization of time and humanpower resources than is normally the case with disciplinary projects. This fact has to be taken into consideration by any institution funding research. These institutions have to appreciate that preparatory periods need financial support and this must be regarded as venture capital, because *ex ante* it cannot be assumed that a concrete problem can be posed from a given theme. Nor can it be certain that a problem, initially vaguely posed, will be clearly transformed into a problem with a degree of concreteness or that there is promise of the project being realized successfully.

(4) Finally, the problem of assessment and evaluation of transdisciplinary projects has to be mentioned here. Questions of assessment and evaluation are of importance at two main points during a project run. Firstly, at the moment of approval and secondly when the final report is made. All supradisciplinary projects have the same problem: only the peer reviewing system for disciplinary research projects is known. Consequently supradisciplinary projects are splintered into the disciplinary contributions of their partial contributions. These projects are then evaluated in a separate process using the standards of the specific discipline (taking into consideration the actual state of the research, the originality of the question, and utilization of proven methodical skills etc.). There is no doubt that this method does not acknowledge the function of a partial project and its contribution to the whole. The normal peer reviewer can be characterized by the standard comment that he only assesses what is covered by his own disciplinary competence and this leads to a dilemma. On the one hand it cannot *a priori* be expected that a top performance by a disciplinary contribution will stand out in importance from the whole project. On the other, the strong aspects of a very original contribution could lead to disciplinary methods used as standards to provide answers to questions from fields outside the specific discipline. This dilemma can only be overcome if the interdisciplinary structure of research is reflected by the reviewers or experts. It is essential that the reviewers form a definite opinion as to the aim of the whole project, prior to making rational judgments on the position and value of partial projects. This is valid for approval and the final evaluation.

Transdisciplinary research projects do have a special problem when scientists take on a non-scientific problem. The question arises as to the awareness of the non-scientific partner of his position in the posed problem, as well as the form of anticipated participation. This question alone could occasionally become a task for a preparatory research activity. It must be added that a transdisciplinary research project may not present only one solution for a given problem which in principle is feasible. The proposal needs to be suitably formulated to be understood by the non-scientific partners, because the economic and political aspects are of importance (this is where the famous “problem of feasibility” becomes relevant). The last point at least cannot be handled within the framework of a “classical” process of evaluation.

All forms of supradisciplinary research have a fundament problem of assessment in common: reliable assessment criteria are not available. In a contribution, published in the journal of the Federal Institute of Technology in Zurich, the philosopher Gertrude Hirsch (1996) enumerates a list of (believed) criteria which on closer observation merely turn out to be desirable characteristics of transdisciplinary projects. In the framework of disciplinary research there are at least clear ideas concerning the quality of a research project. The originality of a question or problem can be ascertained by checking to see if it provokes additional problems. If the problem is in line with current research it will be documented by relevant literature and when comparisons are made with other related research projects it can be decided whether or not the financial aspects and personnel are being used efficiently. The basic thematic similarity of research projects within one specific discipline or subdiscipline, together with the research tradition of a discipline and the homogeneous expectations regarding future continuity are belayed by criteria of evaluating research projects based on experience

However, supradisciplinary projects lack an objective common ground which could become the basis for comparison between projects like these. There is no common research program for inter- or transdisciplinary projects. Projects of this nature need to be regarded as single units unto themselves, and common organizational questions about them need to be disregarded. The point is thus made: projects of this type either land with precision—if the expectations are fully met by the results—or they crash. In order to avoid a gap between expectations and results, it may be necessary to forego the otherwise common anonymous final report which ends a disciplinary project and to use instead an open and continuous monitoring process. This enables close coordination between the project leader and the scientists involved, as well as with the external peer reviewer. The “Swiss Priority Programme Environment (SPPE)“ took this into account when establishing workshops, held annually, where scientists, together with the peer reviewer report on the actual state of the project. In this case a criterion for the evaluation of a supradisciplinary project would become the level of agreement between the two participating interest groups and this would not be the worst criterion to use in the discursive business that science in end effect is.

VIII. Conclusion

To draw a more precise map of the scientific landscape, the concept of transdisciplinary research should be reserved for a special kind of interdisciplinary research, which is oriented to the solving of problems growing from our treatment of public goods. Concerning this prerequisite it is obvious that transdisciplinary research is in no way “better” or more fashionable than disciplinary research. The key point are the problems which determine the path one should follow to come to a solution. On the other hand there is no doubt that in our days problems are accumulating which preferably are tackled by a transdisciplinary approach. Science has to adapt this pressure in a positive and open-minded way which in particular means that one has to worry more strongly about the methodological, organizational and educational problems connected with transdisciplinary research.

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This work emerged from the project “Transdisciplinary research processes” which both authors carried out within the framework of the “Swiss Priority Programme Environment (SPPE)“ of the Swiss National Research Foundation. In addition to this the work was made possible by a habilitation grant from the Swiss National Research Foundation for Philipp W. Balsiger.

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