

15. Biological assessment – the fairy-tale of the magic salve

Hydrobiological Bulletin 17(1983)1, pp. 71-75

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The problem

Some time ago there was a meeting of a small working group on biological assessment and some responses to a proposal for international standardization were discussed. It concerned the assessment of the quality of running waters according to their macro-fauna. One of the responses emphasized the natural difference between the macro-fauna of Dutch streams and that of streams in other countries. It tried to point out in which situations an international system can be helpful by comparing the results with those of a locally used system, both applied in a Dutch lowland stream. Another response expressed the fundamental impossibility to use international standardization concepts. It showed how they lack a conceptual notion of quality, and how the used parameters can never be related to such an undefined basic concept.

A judgement like this seems to be crashing: someone wants to measure something, but fails in saying what he actually is measuring. Some practitioners sighed that nothing could be done with this second response; who endorses it, can cease working. The first response was seen as useful. But after discussion all had to agree, that the second response formed the only fundamental approach.

This example is symptomatic for the ways in which ecological science is used in practice. Experts do not know properly what they are doing. And if they would know, it would become impossible for them to do their job well. It resembles the old Chinese fairy-tale on the magic salve. As long as the box remains closed one can imagine that the salve works. But when the box is opened the fascination is broken. So keep it closed, leave it as it was and do not think too much. We have to promote that the salve works, though we know better. “Pale-face speaks with forked tongue”, Chief Seattle of the Duwamish and Suquamish Indians said in his well-known 1854 message to the ‘White Chief’ in Washington.

I have the feeling that we are constantly forced into situations which oblige us to speak with a forked tongue. Science has to solve the problems of society, and if it proves to be unable to do so; thus we have to pretend it can. It is not a situation to be happy with.

The use of biological criteria for the assessment of water quality

Biological criteria are getting more and more interest for quality assessment. The new 'Indicatief Meerjaren Programma' (Proposed Programme) for sanitation of surface waters for the next five years asks for the establishment of a 'basic quality' which belongs 'by nature' to a certain region. For the assessment of this 'basic quality' biological criteria are needed. Of course we may be pleased to notice the interest, but the way in which it gets formalized reminds of the example mentioned above, in many respects.

What is 'by nature', if we want to speak about a basic quality? It is said that the expression refers to the quality of a water not suffering from pollution by man. It is tried to describe it by using data of the beginning of this century or from sediment studies. In this way the quality in 1900 is considered a kind of reference for the quality of our surface water. It must be admitted that in that year the load of foreign substances was of a completely different order if compared with that of today. Who studies the changes will experience how qualitative differences were caused by excess of organic substances leading to eutrophication and increased production, and by poisonous substances in the water. Studies like these teach about equilibrium and disturbance, deepen our insight into the relationship man-nature. The method helps to establish a holistic interpretation of nature, and hence can be regarded as fundamental.

But what is its concrete significance for our intentions with regard to a sampling station at a given moment? Can our insights in its past influence the future? Do they tell us how to prevent it from deterioration? Do they teach about the objectives for optimal qualities we have to promote? I doubt that. The basic concept, given by the method does not lead to any canvass for direct application, carried out on the spot.

Strictly speaking the ecological situation of 1900 was not more 'natural' than the present one, changed by an 'attack' on nature. The difference between the two situations can be explained primarily by economic conditions which have meanwhile changed enormously. An analysis of the changes can teach us about these relationships, which may be helpful for future strategies. But it is completely insignificant for the question, whether species A or B will stay or come back in waterbody X or Y.

Of course there are species, which rely on very old structures. But how important is that knowledge if someone wants to intervene? If these structures have maintained during the process of deterioration the species will come back as soon as the stress stops; just as the rare orchids, found today along the southern Zeeland highways, now that verges are no longer fertilized. Where the structures of the past have been destroyed, an analysis of the past is unimportant for the future. The closure of the Zuiderzee has caused the disappearance of many characteristic species of brackish water. But if we intend to burst the dikes, we have to look for

other arguments. The diversity of 1900 is not a zero-value for a 'natural' situation and its deviations, but an expression of a past world: ecologically, as well as socially and economically. It is a stage in an evolution which cannot be reversed, not even by ecologists. It must not be our dedication.

Typology and the impact of human charge; opportunities and limitations

Old diversities within a certain region will never be a good basis in a strategy for its future. Such a strategy, which has to guarantee a certain rate of diversity in water types, does not depend on the values of past and present. On the contrary, it depends on the character of social activities in this region. It mainly deals with the relationships between energy and information and with the controversial aspects of internal and external forces. Ultimately (after reasoning the relationships between functions in a system) this will prove to be dependent on the intensity rather than on the sort of the interference.

Certainly, we may detect specific influences of toxic substances, which differ distinctly from those caused by energetic interaction, but these are particularities in a wide spectrum of possibilities. Diversity as a result of large scale differentiation in a landscape today is overshadowed by the factor of eutrophication. If this pressure is taken away, the old structures will emerge directly, if not destroyed irretrievably. Then there is no hope that they will come back, by whatever policy.

On what places may we expect such valuable developments? It depends on gradient situations of a much larger scale than hydrobiologists are mostly concerned with. But someone who takes these spatial considerations seriously will have no trouble in designating them on a map without any hydrobiological typology. Such a typology proves to be without importance for future strategies. That is, because important facts are known already; pleas for the extension of research are based upon wrong predispositions rather than upon the lack of knowledge.

Someone may object that quantified pronouncements on the chargeability of ecosystems by external dynamics may strengthen our arguments. Not all types of ecosystems behave in the same way if brought under the same stress. Every type has its own resilience mechanisms, and these may vary from place to place. This is certainly true and a typology may give this knowledge. Records from earlier times can be used to register the impact of changing production forces. This is the fundamental objective of typology, just as stressed previously.

But such a view may be shaded. On many occasions biologists are asked to give exact information on how much organic matter, phosphates, biocides, etc. a certain water body is able to endure before its system will

collapse. Such a question is unsolvable. Relationships between quantitative, process-managed alterations within a system, and the conditions under which they occur are of such a complicated nature, that an answer will always remain unsatisfactory. We do not deal with rigid units as the macromolecules are for the chemist, but with living organisms. All specimens therein have their proper identity with an enormous variation in geno- and phenotypic qualities.

Secondly, we mostly do not deal with direct relations between organisms and changing circumstances, but with complex feedback systems in which literally all things take part. Someone who can untie these relationships will have explained nature, and will have made ecology superfluous. It is megalomania or naivety to think that we would be able to answer questions like these. Beside all these considerations, the question remains when we may consider an ecosystem changed or disappeared.

In my opinion the best thing we can do is to start from a general ecosystem concept which has proved to be satisfactory up to the present time. Such concepts cannot express, in terms of micrograms per litre, how far we can go with some kind of poisoning. They make clear, however, which strategies are needed for new developments. In this way they embody a much more offensive position than is ordinarily expected from typological researchers.

As a consequence, such verdicts are, by their more social nature, not easy to handle within actual society. They are disregarded by pragmatists as unusable tools, just as was illustrated in the introduction of this paper. But when a fundamental approach proves not to be feasible in an actual situation, the fault must be sought in the social reality, not in the nature of the basic concept.

The tools of the ecologist; opportunities and restrictions.

Typology is unable to point out the ways of technical handling within a given situation, defined in time and space. This is the conclusion drawn from the previous paragraphs. However, with respect to other purposes, typology can be useful:

- a. It can inform us about the uniqueness of water-bodies, and it allows to compare the actual situation with a desired one. It provides a tool for the distinction of 'good' and 'bad' waters.
- b. It can give a concept on the relationships between diversity (in its widest sense) and external influences, which is of vital importance for future strategies.
- c. It may provide means for the fight against one-sided economism. The concept derives its authority from its empirical base, and hence enables us to compare ecological and economic experience, and to discuss the (ecological) basic concepts of the economic theory.

A direct action force of typology and its researchers seems to be decreased thoroughly by shifting the field of attention, as mentioned above. In our time attention is given to the first of the three possibilities, a. It is worthwhile to give distinguished waters special care, to protect them against certain interventions. To a certain extent they obtain the state of a 'reserve'. This is the expression of the 'separation model', playing an important role in many governmental policy documents about restricting human actions in certain areas. Once again we may speak of a defensive activity, which does not alter conditions, but tries to make the best of it under given circumstances.

If applied in this way, the use of ecological knowledge cannot be regarded as fundamental. The reason is that arbitrary texts are made absolute and choices with respect to time, space and function are disregarded. The procedure is disputable, but it opens the eye towards the more offensive strategies as expressed in the possibilities b. and c.

There is another phenomenon which marks the transition from a. to b. and c. It is the completeness of an analysis, needed for a satisfactory judgement. If we want to study the differences between various parts of the biosphere, it is of importance (in particular if these parts are of a similar structure) to know them as well as possible.

Preferably we would want to have a complete inventory of all elements at our disposal. This is impossible, but we can make rather strong conditions with respect to the completeness of a recording, the outrageousness of a determination and the time-range in which observations have taken place. Strongly pushed specialization is a condition for a well-based evaluation, as well as for the draft of a balance of gains and losses in nature, which are important for the conservationist. In cases b. and c. such a specialization is not needed, it may even be unwanted under certain circumstances. If the relationship between environmental variables and the occurrence of a certain species is known, the confirmation of presence or absence of this species will be sufficient.

If we try to find out these relationships, we can restrict ourselves to a selected number of species, needed to build up a classification of our findings. In diversity measurements the same principle will hold; it is not important whether and how such values fluctuate around an equilibrium; we just want to know whether a community is rich, poor, or somewhat in between. And for statements on the chargeability of ecosystems, in favour of future models, an elaborated typology is not needed. That is particularly the case because only few parameters are concerned, viz. those related to internal and external flows of energy.

Four basic properties

The use of four basic properties (internal and external energy, as well as information) and a subdivision of each in three characteristics (oligo, meso and poly) enables a rough division into about twenty classes²³. A division of this kind is more shaded than even the most optimistic expectation of the future will permit. For this reason a further refinement is senseless for our practice.

This conclusion leads to the apparently paradoxical statement that we know too little (and will always know too little) for basic answers to daily questions, whereas for the formulation of a fundamental theory on quality as an ecological phenomenon we know more than enough since long. For future strategies theoretical sub-structuring is of immensely greater importance than enlargement of empirical experience. Such a theoretical sub-structuring has to express the universality of the concept; it has to outreach reductions in space, time and function; and, hence, look outside its own specialism.

The same considerations apply more strongly for the third possibility of action. The relationship between nutrient richness, pollution, diversity and the presence of specific organisms, put together in a model answering the question 'what is water quality?', can hardly be changed by new experiences. And the question: which parameters someone has to use for justified pronouncements, cannot be clarified by more research. It is of more importance to direct one's attention to spatial universality, i.e. to compare the experiences of the various specialists working with different organisms, in other scales of time and space. Are conclusions, based on the distribution of planktonic algae, within a eutrophication gradient identical with conclusions based on the richness of flora and fauna during the formation of a landscape in 2000 years of time? Such approaches help to make a distinction between arbitrary, scale-bound phenomena and the more fundamental, universal ones. They strengthen the concept, matching it with economic theories.

But I want to go further. Ecological over- and underdevelopment can be related to economic over- and underdevelopment. It leads to new ideas about what is known as 'human needs', as a compromise between what is consciously wanted and what is possible by nature. It expresses and gives opportunity to formulate changes of production relationships in a new way. Economists appear to appreciate this approach. In current ecological strategies (like the 'General Ecological Model' (GEM)) such approaches are lacking, which makes them useless for economists and planners.

²³Literally 81 classes, but the connection between informative and energetic properties reduces this number in practice.

Future strategies, using this kind of interpretation, seem to be more fruitful (though for the ecologist mainly aggravating towards problems concerning segregation and integration) than looking for remnants of past economic structures. The same way of arguing applies to the use of technical means to stretch the death struggle of nature; the former themselves are new threats to nature as part of a 'spiral of misery'.

The fairy-tale of the magic salve

The presumed approach has empirical backgrounds and is linked up with a consistent, holistic ecosystem concept, which makes it fundamental. That cannot be said for methods asked for by daily practice, in which judgements with respect to time, space and functions are neglected, leading to a would-be objectivity. But a fundamental approach, as described here, conflicts with actual social situations – not at least by the dominance of external energy flows. It is for that reason that this fundamental approach is not so much appreciated; it is not useful in actual situations, as is described in traditional biological assessments or in the evaluation of the GEM-procedure.

The picture of the fairy-tale of the magic salve unmistakably enters our mind, just as the story of the Indian chief, hearing white men speak with forked tongues. The law on the sanitation of surface waters puts questions to hydrobiologists which they are unable to answer in a sensible way. And only by fooling ourselves, we can maintain.

A fundamental approach of the problems appears to be unusable, and therefore practice is satisfied with something that has only pragmatic value but that is lacking every basic concept. In this way it is possible to do research. Still we do not know all plankton communities occurring in the Dutch surface waters, and certainly our methods are to be improved or refined. But what does they yield? The problem is solved already, and the remaining questions are in principle unsolvable. They are brought about by subjective values and these cannot be avoided by hundreds of years of research.

By collaborating with this kind of research we pretend to overcome the questions. We even develop methods for them. And we assume that this approach is 'scientific'. But it appears to be a fallacy.

Literature

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