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Palaeolimnology and lake acidification: a summary

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When plans for the realization of the Surface Water Acidification Project (SWAP) were initially discussed, I advocated the inclusion of a palaeolimnological programme. One reason for that was personal: as a palaeobotanist, I wanted to see what palaeolimnology could contribute to solving problems that were, in principle, very similar to questions dealt with in other palaeobotanical research, but on a completely different timescale; years and decades instead of centuries or millennia.

More important, however, were the SWAP problems themselves, which seemed to beg for a palaeolimnological approach. The central problems of SWAP deal with ecophysical conditions in the so-called acidified lakes and mechanisms of their effect on biota. However, before considering such problems, the background should be studied by palaeolimnologists. In short: *did* anything happen, *what* happened, *when* and *why* did it happen? Naive questions indeed, but they demand answers: data about the simple existence and timing of the acidification are fundamental for further work.

The term 'acidification' used throughout is a general term that expresses a group of associated environmental conditions. Changes of pH may be the easiest observable and quantifiable parameter, but its actual physiological significance in relation to the biological phenomena observed may in itself be slight. However, within the general ecological framework it monitors the general status of the basins studied.

It is very simple to define in detail the immediate ecological status: ordinary ecochemical methods are well established and sufficient to confirm that waters are acid and also suffer from other deleterious influences, expressing themselves in fish death. But if we want to see how and when this came about such methods fail us: we have no historical data of this kind. Apart from anecdotal information, which should not be neglected as it is often the only information we possess, very few data are available, and those that may exist were collected for other purposes and provide poor comparative material.

Above all, there is one very important point: the very first, faint traces of a change cannot be pin-pointed in this manner because their manifestations are so indistinct as to be lost in the statistical 'noise'; at that time nobody would have had the foresight to realize what was going on and what could be used to provide us with a database for future reference.

For this purpose we need a monitoring system that is more robust against the 'noise', for example, by integrating it over time. It is then possible to trace the first beginning. Many organisms have this quality; they are, within their restricted field, both faster and more sensitive indicators than ecochemical measurements. Flowering of forest trees, as registered by pollen analysis, is one example: registering climatic deterioration almost instantaneously. However, the complicated physiological pathways of multicellular terrestrial organisms put them at a disadvantage in comparison with small, often unicellular aquatic organisms, the time-lag of whose reactions is very short. If, in addition, such organisms are specifically recognizable also after death and sufficiently resistant against decay and occur in large numbers, we have the ideal index fossils.

In evaluating such organisms it is important to realize that their reactions as observed are not always directly related to the primary ecochemical conditions, but may be mediated via other organisms, which may influence their occurrence. Indicator organisms may be liable to predation by other oganisms that are also dependent on acidification and that may not leave any recognizable traces behind. Such cases may be rather complicated.

Any palaeolimnological investigation that includes necessary and suitable marker oganisms immediately recognizes the onset of ecological change by changes in the numerical relation between groups of taxa with varying ecological niches. The basic reason for the change is usually chemical or physical. By studying ecophysiological characteristics of the organisms in question, they define quantitatively the influence of primary factors as they express themselves in the biota and may produce transfer data for quantifying the immediately observed qualitative manifestations.

This, more or less, was the basis for the palaeolimnological programme of SWAP, or, more correctly, of my understanding of the programme. Here I review the results of the programme that have led to two different types of study. One aims at answering our four basic questions directly. The others are calibration and methodological studies used to create the tools for answering the questions.

The answer to the first two questions is very clear: recently, there has been a change towards acidification in practically all lakes investigated that were situated in areas of high sulphur deposition. Both organisms and physical variables show the same picture. Indeed the only basins studies that did not register the effect are found in areas of low sulphur deposition in northwest Scotland and central Norway.

The timing of change also needed study. The usual dating method in Holocene palaeolimnology is by ^{14}C but this is unsuited to the dating of very recent sediments. Instead dating by ^{210}Pb and by fall-out radioisotopes has been used. These are beyond my field, but the results are consistent.

Diatom data pH calibration is more complicated, especially as organisms react holistically to all external conditions at the same time and reactions are far from being linearly related. Within a certain set of values one specific variable may be decisive, even given a monotonic reaction, but outside those limits other variables influence reactions and may over-ride the effects that we are studying.

As a group, diatoms may be the most versatile and reliable monitoring organisms available. They possess the qualities defined for the ideal index fossil. Also, they are relatively immune against predation (i) because the resistant remains are often preserved even after passage through the gut of a grazer and (ii) because they have an extremely high reproduction rate, indeed continuous reproduction given favourable ecological conditions. They are very sensitive to the quality of the water. In fresh water, pH is the dominating influence and has been studied earlier and very intensively under the SWAP programme. Interesting and penetrating studies have been done in the programme on the ecology of these organisms; for example, their relation to habitat, and in relation to their ecophysiology in acid waters. However the study material of palaeolimnology is thanatocenoses, remains of dead organisms brought together from a variety of habitats. Material from these habitats are inextricably mixed. In the constructions of pH transfer functions these assemblages are compared with average lake-water

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pH. I wonder what these average pH data actually mean. In some bodies of water, pH varies spatially, diurnally and seasonally even in response to short-term meterological conditions; it is far from certain that these variations are the same in basins of different characteristics. All the values from one body of water form a swarm around one central value, but the character of this swarm varies with ecological conditions. What are the relations between the pH of the statistical models and all those varying pH data out in nature? This is an important challenge for diatomists in the future to refine their methods to take such functions into account.

Another point is that the pH tolerance of a species need not be constant: it may be modifed by other influences especially nutrients. If the long-term 'acidification' of a lake is interrupted when the area has been opened by cultivation: is that only a pH effect, or does it mean that the changed nutrient status of the lake permits the diatoms to survive at lower pH, and some more demanding ones to immigrate into water that they would not tolerate at lower nutrient levels? Developing models to differentiate between such factors could also be a desirable future goal for diatomists.

Mutatis mutandis, I have a similar feeling with regard to some of the other groups studied, although to a smaller extent. I must admit that my practical knowledge of these groups is more superficial than with regard to diatoms. Chrysophytes seem to have many of the same qualities.

Certainly our knowledge of the ecology of diatoms has been greatly advanced by these studies, which have also given material for quantitative answers to SWAP problems.

It must also be stressed that by using catchment models as well as diatoms it has been possible to produce some tentative and not too cheerful predictions for the development of lakes under given conditions of sulphur deposition in the future.

The studies have not only given a convincing indication that the investigated lakes in contaminated areas have recently become more acid, but also that this acidification on the whole follows a pattern to be expected according to hypotheses (cf. below). The acidification is faintly foreshadowed since the middle of the 19th century, becoming strongly manifest during the post-war period.

The next question is whether the development thus observed and dated represents a unique event or (as maintained in discussions) it has occurred previously and for the same reasons. The answer is that even if some developments more or less resembling the recent one have been registered or postulated, the present event is unique and thus is confirmed not only by detailed studies in the SWAP programme but also by other work in the U.S.A., Finland, Sweden and the U.K.

Several studies in SWAP specifically address the last of our questions: why did acidification occur? Three hypotheses have been put forward.

1. Long-term acidification.
2. Effect of changes in land-use.
3. Effect of industrial pollution.

The hypotheses are not mutually exclusive and the effects may have been synergistic. Due to a vigorous defense of the land-use hypothesis from its originator, the land-use hypothesis was considered of central importance in 1984 when the SWAP study was designed and several sub-projects have dealt with this question.

Most of the studies gave a clear answer in favour of the acid deposition hypothesis, but some of the data may be interpreted in several ways.

In all cases the biological effects of land-use change according to hypothesis 2 would be very difficult to separate from those of atmospheric pollution. It is not easy to find deposits that register in such a way or under such circumstances that one of the competing hypotheses can be falsified but in the palaeolimnological programme two lines have been pursued that have given clear answers.

The reaction of diatoms and other biota to acidification is not influenced by the way in which this acidity has arisen, only on its existence. It is therefore necessary to find an indicator, the occurrence of which cannot be caused by one or the other influence. Spherical carbonaceous particles satisfy this condition. They are produced by modern, high-temperature combustion of fossil fuel; they are dispersed in the same way as the supposed atmospheric contaminants and they are very resistant. In cores, they are found from the mid-19th century and after 1940 the curve rises suddenly. The parallelism between acidification and the occurrence of these particles is a strong argument in favour of the atmospheric pollution hypothesis.

Another approach has been to find lakes in which land-use influence can be ruled out and to compare them with adjacent lakes that have been under influence of land-use. Several studies of such paired lakes have been done. An example is the 'perched' lake project, i.e. lakes situated in such a position (on the top plateau of hills) that they themselves constitute a major part of the catchment area, the rest of which is covered by, at most, a very scanty vegetation, and where no local agricultural or other human influence can have occurred during the period in question. Such lakes give a direct, almost unadulterated representation of precipitation. They present the same picture as the other lakes with a clear acidification after *ca.* 1850.

The 'perched' lakes and other similar projects in SWAP and reported in the literature thus falsify the land-use hypothesis. The carbonaceous particles and other indicators of atmospheric pollution give positive evidence for the atmospheric pollution hypothesis. The matter might therefore be considered settled, but even if acidification is registered under circumstances when land-use effects are excluded, I feel that it is not possible on the basis of the present material to say that land-use changes have not under certain circumstances also played a (probably minor) role in the acidification process. The influence of pollutant scavenging by coniferous forests is a case in point.

Comparisons have also been made with changes in lakes following the abandonment of cultivation in earlier epochs and with the natural immigration of spruce 2000–3000 years ago. Strict historical parallels do not exist but none of these studies so far have shown any effect comparable to today's acidification.

In the heated discussions of hypothesis 2 against hypothesis 3, number 1 has been less prominent. As compared to the effect of atmospheric pollution the effect of long-term changes is certainly very subordinate. Some long cores indicate such effect, others show a clear acidification after deglaciation and in the early part of the post-glacial period. But in any case over the last 5000 years the effect is so small that it could not conceivably produce the violent changes apparent in recent sediments.

Returning now to my original four problems: has the palaeolimnological sub-programme fulfilled its purpose and given answers to the questions? I feel that we may confidently say yes to that, and even more importantly, to the question of whether it was worth the effort; it was.

I should stress that no attempt has been intended nor made and no palaeolimnological data have been obtained that indicate the source of the atmospheric contamination: whether locally produced or coming into Scandinavia from the industrial areas to the south or the west, or both. This could be a worthwhile project for the future.

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Finally, I want to point out very forcefully the role of taxonomy and taxonomists in this investigation. Where would we have been if it had not been possible to call upon and draw upon the expertise of a large body of taxonomic specialists in diatoms, chrysophytes, Cladocera and chironomids. Who would have done the monitoring? In today's university debate so-called progressive scientists have blown their own trumpets, with almost indecent volume, denigrating taxonomy and taxonomical expertise and, above all, the natural history museums. The SWAP palaeolimnology story shows what would have happened if they had had their way and if there had not existed museum material on which the taxonomists could support their identifications. I sincerely hope that it will also be possible in the future to localize the material upon which this study is based in the museums and compliment those taxonomists who in the beginning of these studies managed to align their diatom taxonomy to achieve the results obtained.