Hydrobiologia **214**: 201–206, 1990.
J. P. Smith, P. G. Appleby, R. W. Battarbee, J. A. Dearing, R. Flower, E. Y. Haworth, F. Oldfield & P. E. O'Sullivan (eds),
Environmental History and Palaeolimnology.

Are we building enough bridges between paleolimnology and aquatic ecology?*

John P. Smol
Dept. Biology, Queen's University, Kingston, Ontario, K7L 3N6, Canada

Abstract. No.

Few paleolimnologists would argue against the fact that our science has made substantial advances over the last decade. A simple perusal of the development in procedures and applications, conveniently documented at about four to five year intervals in previous symposium volumes (Frey, 1969; Klekowski, 1978; Meriläinen *et al.*, 1983; Löffler, 1986), should convince any sceptic. The papers presented at this symposium further demonstrate that paleolimnology continues to gain momentum.

Yet, despite these advances, it is my opinion that we have been slow, or perhaps reluctant, to use our new and powerful approaches to test a wealth of hypotheses being generated by rapidly expanding ecological theory. The many advantages of having a temporal record providing information on time-scales ranging from seasonal events to thousands (or in some cases millions) of years of community interactions has much to offer aquatic ecology. Nonetheless, I believe that many potential bridges have not been built.

Throughout this commentary, I use the word 'neolimnologist' to designate limnologists working with present-day aquatic systems. Among the practitioners of the synthetic science of 'limnology', I prefer not to differentiate between the more commonly used headings of 'limnologists' and 'paleolimnologists', as both groups of researchers are 'limnologists'. One might argue that this is a minor point of semantics – perhaps it is – but I think it adds to an artificial isolation of two interdependent sub-disciplines.

Are we ready to build these bridges?

I think the answer to this first question is a resounding 'Yes!'. Our techniques have advanced to the point where we can accept challenges posed by neolimnologists and begin evaluating ecological hypotheses that otherwise remain untested.

Part of my mandate for this introductory talk is to review our recent developments. This is a difficult task, for few areas of science have enjoyed such leaps in progress. About a year ago (1988) I was offered a similar indulgence, when I was asked to offer my opinions on paleolimnology's recent advances and future challenges (Smol, in press). To briefly reiterate my comments made then (and I acknowledge these represent my parochial view of ecologically-based paleolimnology, heavily modified by research interests in my laboratory), I believe that our most significant advances can be divided into three interdependent categories.

I would first identify refinements made to sampling techniques, and the resulting temporal resolution we can attain, as primary hurdles that we have cleared. As in most scientific endeavors, technology drives progress, and the continued refinements to coring apparatus (although it is hard to improve on some of the original designs!) and sediment handling protocols (including, but certainly not restricted to, the development of freeze coring, close interval sediment extrusion, tape-peel adhesions, and resin impregnated sedi-

* Opening keynote lecture to the 'Aquatic Ecology and Palaeolimnological Interpretation' session at the Vth International Symposium on Palaeolimnology, held at Ambleside, U.K., Sept. 1, 1989.

ments) have done much to improve our resolution. These developments go hand-in-hand with advances made by scientists working on dating techniques, upon which all ecologically-based paleolimnologists are dependent. Very often, paleolimnologists can select the time scale (e.g. centuries, decades, years, and in some cases seasons) – a distinct advantage over workers restricted to neolimnological protocols.

Secondly, the quantity and quality of information we have learned to tease from the sedimentary record continues to escalate; in fact we have now shown that most aquatic groups leave some sort of morphological or chemical marker. As with sampling procedures, technological advances, such as the application of high pressure liquid chromatography (HPLC) to the study of fossil pigments, have done much to foster the quantity and quality of our primary paleolimnological data. Researchers have repeatedly shown that each additional group of indicators *adds* information, and does not simply confirm information gleaned from other groups. The whole is greater than the sum of the individual parts.

Thirdly, and very importantly, tremendous progress has been made on the ways we are learning to use our indicators – how we have learned to calibrate and quantify their ecological optima and tolerances along environmental gradients. Over a short time, we have progressed from simply using qualitative inferences based primarily on literature citations of ecological distributions, to the use of various indices, to the development of surface sediment calibration sets. With the latter came a variety of new statistical approaches, now culminating in what seems to be our new Rosetta Stone – weighted averaging regression, calibration, ordination, and constrained ordination (e.g. canonical correspondence analysis) as developed by Cajo ter Braak (1987). The power of surface sediment calibrations is hard to over-estimate. Equally important has been the development of error estimates, validation techniques, and quality assurance/quality control protocols, so vital to any science. For several groups of organisms, it is the paleolimnologist, not the neolimnologist, who has gathered the primary ecological data.

Why are bridges so necessary?

Paleolimnologists are dependent on present-day lake processes to interpret paleoecological data and therefore we obviously need the information generated by neolimnologists. My main thesis, however, is that neolimnologists also need our data and the long-term perspective we provide in order to constrain their theories, to add realism to their models, and to test their hypotheses. My friend, Patrick DeDeckker, uses an analogy to make this point: neolimnologists are like people taking photographs of a fast train with the latest photographic equipment. They perhaps take several photographs of the train, but they don't know what the train looked like when it left the station, what trajectory the train followed before reaching cruising speed, and what it will look like on arrival. Neolimnologists tend to only look at a particular portion of time, yet a much longer record of community change is archived in a lake's sediments. I believe that it is this message that we should try to develop and communicate. A few examples might strengthen these arguments.

In 1986, the ecologist Patrick Weatherhead asked the simple but powerful question 'How unusual are unusual events?'. Weatherhead's goal was to document the extent that ecological researchers invoke 'unusual' events to interpret their data. He wondered whether these were really unusual events, or were instead artifacts of the observational approach, or perhaps more precisely, a result that researchers expected to find, given their perspective on the science. He looked at a sample of 380 papers and found that about 11% of ecological studies (averaging about 2.5 years in duration) invoked 'unusual' events to explain the outcome of their observations. These data are especially striking because none of the above studies were published specifically to report the unusual event. Interestingly, almost all of these 'unusual events' were abiotically, and especially climatically, controlled.

It seems that 'unusual events' in ecology are not so 'unusual' after all. Clearly, the temporal perspective and background of ecologists (and other scientists) are dominant operating factors influencing their interpretations.

This latter point is reinforced by another interesting outcome of Weatherhead's analysis. Apparently, the relationship between study duration and the probability of an unusual event is not as clear-cut as one might expect. In general, the frequency of unusual events increased with time in studies lasting up to about 6 years, but for longer studies (up to 15 years), unusual events were less common. Weatherhead observes that the likely reason for this apparent paradox is that we cannot treat these events as statistical phenomena because the criterion used to designate an 'unusual event' was the author's perception that something was unusual (based on the data he or she had at hand). In short, '... we tend to overestimate the importance of some unusual events when we lack the benefit of the perspective provided by a longer study' (Weatherhead, 1986; p. 154).

If we superimpose Weatherhead's finding onto a second study, we see the serious dilemma ecologists presently have with time scales. David Tilman (1989) documented the duration of field studies published in the journal *Ecology* for a randomly selected subset of issues for 1977–1987: a total of 623 observational and experimental studies. Of the observational studies, about 40% were less than one year in duration, and fully 90% were less than 3 years long. The curve for experimental studies is even more skewed towards short-term studies. The 'long-term perspective' is not generally available to most ecologists.

I think there is ample evidence that ecologists increasingly recognize the need for long term data; certainly there is a groundswell of interest and at least moral (if not yet financial!) support for the collection and archiving of long term data (e.g. Likens, 1989). Yet, I feel strongly that many neolimnologists do not fully recognize paleolimnological data as an important source of this information. Many neolimnologists still seem to equate 'long term ecological data collection' exclusively with repeated field sampling, even if this approach is not usually feasible. In my opinion, this epitomizes the lack of sufficient bridges between these interdependent sections of 'limnology'.

Like many people at this symposium, I have recently spent much time as a speaker or discussant at many university, government, and industry sponsored workshops and colloquia, relating to a variety of ecological and environmental problems. The fact that we, as paleolimnologists, are even invited to these meetings should be encouraging, as neolimnologists at least suspect that we have something to offer. However, I have been equally dismayed when, for example, I talk about some of the simplest paleolimnological techniques, such as surface sediment calibrations, a portcullis seems to fall down – once sediments are involved in any way, this all becomes foreign and largely inapplicable territory. Many neolimnologists wrongly believe that our data are somewhere in the realm of fossils, or geology, but certainly not 'limnology' or aquatic ecology.

In general, neolimnologists are unaware of the wealth of excellent ecological data that we can gather and/or that we already hold in our data bases. Even if they are aware of the quantity and quality of these data, many seem oblivious or at least unsure of how they can apply these data to their own research needs. In addition, their perspectives on paleolimnology are frequently rife with misconceptions, not recognizing that many of these at least potential problems have been investigated, quantified, and in many cases, resolved. Yes, we need to build more bridges.

It is, of course, a two-way street. Neolimnologists desperately need long-term data. Paleolimnologists, however, can also greatly benefit from the wealth of ecological theory currently accumulating. Much of this theory, in my opinion, is still poorly developed and tested. By embracing this theory, we can further develop our science and integrate it into a broader ecological framework. We have much to gain from these interactions.

The future opportunities for these interactions are especially bright, as paleolimnologists are presently faced with tremendous research challenges. Below, I identify a few that I am more familiar with, but I appreciate that there are many more – several of which have been discussed at this symposium.

Paleolimnological approaches have been widely used to study the long-term effects of damage to ecosystems. I begin with acidification: partly because it is a subject with which I am familiar, and partly because it has important legacies that we can exploit. Many of the new techniques and protocols, as well as the sizeable data bases that this research is generating, can now be transferred to other research endeavours. In addition, there is still much we can learn from ongoing and newly proposed acidification studies. For example, lakes can be randomly chosen from a region. Pre-industrial age pH levels can then be inferred, and compared to the present lakewater pH. The resulting 'change-in-pH', or for that matter change in other variables (such as dissolved organic carbon or selected metals), can be mapped and population based estimates of lakewater changes can be generated. The study of lake recovery (if in fact it occurs) can also be documented by fine-interval sediment analyses. In addition, paleolimnological studies provide one of the only avenues for model validation.

Long-Term Monitoring Programs (LTMPs) continue to gain popularity with many lake managers, as well as other scientists. The significant recognition that organisms are important adjuncts to 'snap-shot' water chemistry measurements is equally gaining momentum. All of the indicators we use in paleolimnology should be prime candidates for inclusion in LTMPs, not only because we already hold a wealth of high quality ecological data on species distributions that could easily be applied to LTMPs, but also because these organisms respond quickly (in a predictable and quantifiable manner) to even subtle environmental changes. Equally, paleolimnological studies should be included in all LTMPs, for without these data the background variability (which any new environmental changes must be measured against) cannot be assessed. It is impossible to extrapolate reliable ecological models without first determining if organisms are responding to an unusual phenomenon (e.g. a culturally induced environmental change) or the result of a long-term trend.

Lake trophic dynamics continue to be a main focus in neolimnological studies, and one to which paleolimnologists can make an important contribution. Quantification of trophic variables (e.g. nutrient concentrations) to species distributions is under way in a number of paleolimnology laboratories, often applying similar calibration techniques to those developed in acidification studies. Once calibration is available, paleolimnologists should be able to quantify past lake trophic status, from which a plethora of hypotheses could be tested and developed (e.g. the terrestrial/aquatic linkage, anthropogenic effects on lake systems, etc.). In addition, these studies will be vital to help evaluate the rapidly developing theory associated with topics such as cascading lake trophic dynamics (e.g. Carpenter, 1988). More process-oriented paleolimnological techniques (e.g. biogenic silica, chemical accumulation rates, etc.) may be especially applicable to these types of studies.

Just as acidification was a major environmental focus in the 1980's, global climate change appears to be rapidly becoming the major concern of the 1990's and beyond. Virtually every technique paleolimnologists use can be applied to both hindcasting climatic change (absolutely vital for model validation) and studying the limnological effects of past climatic changes. Although palynology will provide much important proxy data, paleolimnological approaches have much to offer, and in fact, have many advantages over more traditional techniques. Saline lake systems and high polar regions will likely become important research foci for these studies.

What obstacles do we face?

Given these research opportunities and the recognized need for long-term data in ecological studies, what obstacles do we face in building bridges?

Aquatic ecology is presently rich in ecological theory, whereas paleolimnology is, in general, theory-poor. We will always be plagued by critics who see this as a major drawback to making paleolimnology an 'exciting' science. This, however, did not bother nor hinder such eminent scholars as E.S. Deevey (1984). Also, because we are an historical science, we cannot undertake 'true experimentation'. I do not see these as serious problems. To use my favourite example of acidification and its ensuing problems, paleolimnology did not provide many new elegant theories about the acidification process and its effects (in some ways, I am thankful for this, given the large numbers of untested theories already in the literature). Nor did paleolimnology (technically, at least) 'prove' anything in the acid rain debate – but it certainly presented a very powerful case! When it came to even the most basic questions, such as: 'Have lakes acidified and, if so, when, and by how much and how fast?', it was mainly paleolimnology that provided these answers. Paleolimnology provided data that were vital to falsify many theoretical proposals.

Nonetheless, several obstacles remain. I think most of these center on the misconceptions and communication problems that I alluded to earlier. I think communication is the only approach that will overcome these barriers. We must continue to attend neolimnological and ecological workshops and symposia and explain how our approaches are applicable to the research interests of neolimnologists. Publication in a broad spectrum of journals, in terms that are useful and applicable to neolimnologists, is also vital. Large advances have occurred in neolimnology, large advances have occurred in paleolimnology – it is time we start talking to each other. Although paleolimnology makes its way more and more into the 'psyche' of neolimnologists, I strongly feel that we, as paleolimnologists, must take the initiative in showing how important, applicable, and necessary our approaches are. A good start might be to begin framing our research projects more in tune with current ecological thinking. We should keep astride of these new developments, and use our procedures to test these theories.

Finally, cross-disciplinary research initiatives might be especially helpful in forming conduits of communication and interest between neo- and paleolimnologists. For example, data obtained from sediment trap studies bridge the usual boundaries of neo- and paleolimnological research. Moreover, these data provide the opportunity to fine-tune the temporal scale and quality of our calibration data, as well as provide important information on taphonomic processes.

Conclusion

Paleolimnologists share with neolimnologists many of the same problems and complexities. I believe we should foster a new activism, one in which paleolimnological research becomes more integrated with theoretical and applied limnology. We are at an important juncture in time, especially with the almost insatiable appetite ecologists now have for long-term data. We should seize the opportunity while it is before us. Paleolimnology can accommodate the scrutiny of neolimnologists and other scientists, and paleolimnology can benefit from the infusion of fresh ideas and criticisms that would inevitably come from these synergisms.

Ecologically-based paleolimnology is still a young science, but even a cursory look at its development documents the tremendous advances that we have made. A scan of the papers presented at this symposium is further evidence of our progress. I can't wait to see what we will accomplish in the next four years.

Acknowledgements

I would very much like to thank Dr. E.Y. Haworth and the Paleolimnology V organizing committee for inviting me to present this lecture, and for providing such a stimulating and enjoyable symposium. The quality of this manuscript was greatly improved by the comments provided by the 18 scientists in my lab, as well as Drs. Livingstone, Birks, Schelske, Stoermer, Binford, DeDeckker, Engstrom, Haworth, Charles, Brenner, Leavitt, and Siver.

The day I presented this paper was 10 years to the day that I arrived at Queen's University and began to work on my doctoral research with Prof.

S. R. (Ted) Brown. I would like to dedicate this paper to him.

References

Carpenter, S. R. (ed.), 1988. Complex lake interactions. Springer-Verlag, N.Y., 283 pp.

Deevey, E. S. Jr., 1984. Stress, strain, and stability of lacustrine ecosystems. In E. Y. Haworth & J. W. G. Lund (eds), Lake sediments and environmental history. University of Minnesota Press, Minneapolis: 203–229.

Frey, D. G. (ed.), 1969. Symposium on paleolimnology. Mitt. int. Ver. Limnol. 12: 1–448.

Klekowski, R. A., 1978. Second international symposium on paleolimnology. Pol. Arch. Hydrobiol. 25: 1–498.

Likens, G. E. (ed.), 1989. Long term studies in ecology: Approaches and alternatives. Springer-Verlag, N.Y., 214 pp.

Löffler, H. (ed.), 1986. Paleolimnology IV. Hydrobiologia 143: 1–431.

Meriläinen, J., P. Huttunen & R. W. Batterbee (eds), 1983. Paleolimnology. Hydrobiologia 103: 1–318.

Smol, J. P., (in press). Paleolimnology – recent advances and future challenges. In R. De Bernardi, G. Giussani & L. Barbanti (eds), Scientific Perspectives in Theoretical and Applied Limnology. Mem. Ist. Ital. Idrobiol. 47 (in press).

ter Braak, C. J. F., 1987. Unimodal models to relate species to environments. Thesis, Wageningen.

Tilman, D., 1989. Ecological experimentation: Strengths and conceptual problems. In G. E. Likens (ed.), Long term studies in ecology: approaches and alternatives. Springer-Verlag, N. Y.: 136–157.

Weatherhead, P. J., 1986. How unusual are unusual events? Am. Nat. 128: 150–154.