

Changing Perspectives for Changing Times

My love affair with the Holocene began almost 50 years ago. In those days it was called the 'Post glacial', or the 'Flandrian'. At the time, my Cambridge mentors were much beguiled by the Late glacial. For me though, the main excitement came from trying to reconstruct and better understand the peopled landscapes of the past. Despite many deflections from that concern, it has returned consistently, but in so doing, it has not simply been renewed, but rather reinvented. The passage of time has transformed the priorities for Holocene research in ways that could not have been imagined half a century ago. The idea that learning about past climate variability might inform our view of impending future changes would have seemed far-fetched, and the concept of multi-proxy climate reconstructions based on biological remains, quite out of the question.

In the late 1950's, radiocarbon dating was in its infancy and, for most researchers, dates were extremely difficult to obtain. What will be the 'state of the art' and what will our current 'tool kit', look like in fifty years time? Technical innovations are among the most obvious facilitators of future changes in methodology, but for anyone lacking the relevant expertise in instrument design, they are among the least predictable. In my own experience, the interactions between those posing palaeo-environmental questions and those contributing the technology for resolving them, have been rather like a series of dead ends suddenly and unexpectedly opening out into serendipitous partnerships of great delight and excitement. Since the palaeo-scientists' responsibility begins with the questions posed, we should turn to these.

I think there are some safe assumptions – that the underlying 'future-oriented' research agenda for palaeo-science will not be entirely replaced by something quite new and different; the synergy between model and data communities will increase and become central to most advances in the field; computing power will serve this synergy with ever enhanced capabilities; the speed of data acquisition and assimilation will increase at an accelerating rate; the climatic calibration of environmental proxies and associated uncertainties will become increasingly quantitative; and finally, comparisons between past and present climates on a regional, zonal, hemispheric and global basis will become increasingly secure.

Holocene research has strengths arising from the unique opportunities it offers for secure quantitative proxy calibration with minimum extrapolation, for exploring the antecedents of current conditions, whether of climates or ecosystems, for generating multi-archive/multi-proxy reconstructions with high temporal resolution, for elucidating the long term behaviour of current modes of variability, and for providing the longer term historical context for the detection and attribution of future environmental changes. In my view, two of the most intransigent weaknesses are in the areas of chronology and the reconstruction of the long-term dynamics of terrestrial ecosystems. Resolving many of the crucial future research questions will demand increasingly precise and accurate chronologies, and in some key environmental contexts, they remain elusive. My second point may seem harsh or ill-informed, but I make it nonetheless, for it also signals a major opportunity.

If we consider the synergy between PAGES and CLIVAR, it rests in part on a shared exploitation of the continuum between the palaeo-record and the period of direct observations. Palaeo-climatology, within this context, makes a crucial contribution to dynamics rather than 'history'.

As and when the implications of future global change become more clearly and widely apparent, one corollary must surely be a growing interest in the past *response* of ecosystems to a variety of interacting perturbations, on a range of timescales.

What if the hoped-for broader synergy between 'palaeo' and 'present/future' science fails to develop? Certainly, that threat lies implicit in the gap between ecosystem modelling communities using inputs derived from the short period of direct observations, and the terrestrial palaeo-science community that is often seen as dealing with history rather than process. When we compare the situation with that prevailing at the CLIVAR/PAGES interface, the gap is all the more unfortunate: whereas a global network of standardised and verifiable climate records goes back for a century or so, direct observations of ecosystem structure and function are much less systematic and more recent and patchy over most of the world. Closing this gap between 'palaeo' and 'process' in terrestrial ecosystem studies is an immense challenge, but one that must be met if both communities are to benefit and to make the best possible contribution to our understanding of ecosystem responses to global change.

The time interval from which much of the necessary evidence will be drawn is the Holocene. During its life time, HOLIVAR will have helped to strengthen the basis for future research, both by coordinating and publicising what has been achieved so far, and by helping to create a cohort of young researchers whose skills and opportunities will, in due course, outstrip those of their mentors. That is as things should be.

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