"The Prospects and Promises of “Interglaciations”"

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Essai

THE PROSPECTS AND PROMISES OF "INTERGLACIATIONS"*

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ABSTRACT The term "interglacial" has been subject to numerous changes since its inception. This essay provides a brief historical summary, and suggests that the assignation of "interglacial" status to terrestrial deposits should be reviewed. Past, and the present, interglaciations are particularly important in analysing earlier periods of global change. Proxy-data records of these intervals can provide valuable parameters which should be utilised by modellers to help man-kind face the results of anthropogenic modifications of the environment.

RÉSUMÉ *Les interglaciaires: perspectives et sources de connaissances.* Le terme «interglaciaire» a fait l'objet de nombreux changements depuis sa création. On présente ici un bref historique et on propose que l'attribution du qualificatif «interglaciaire» à des dépôts terrestres soit réétudiée. Les interglaciaires, ceux du passé comme celui que l'on connaît maintenant, sont particulièrement importants pour l'analyse des périodes antérieures de grands changements climatiques. Les données climatiques indirectes livrées par ces intervalles fournissent des paramètres très utiles dont devraient se servir les modélisateurs pour aider l'humanité à faire face aux conséquences provoquées par les changements anthropiques causés à l'environnement.

ZUSAMMENFASSUNG *Die Interglaziale: Aussichten und Voraussagen.* Seit seiner Schöpfung hat der Terminus "Interglazial" zahlreiche Wandlungen durchgemacht. Dieser Essay gibt einen kurzen historischen Abriss und legt nahe, die Bezeichnung "Interglazial" für Erdablagerungen einer Revision zu unterziehen. Vergangene Interglaziale wie auch das gegenwärtige haben eine besondere Bedeutung bei der Analyse früherer Perioden mit umfassendem klimatischem Wachsel. Proxy-Data-Belege von diesen Intervallen können sehr wertvolle Parameter beschaffen, welche von den Modellierern genutzt werden sollten, um der Menschheit zu helfen, den Ergebnissen anthropogener Veränderungen der Umwelt ins Auge zu sehen.

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INTRODUCTION

Probably the most important single planetary effect caused by anthropogenic modification will be a major climate warming trend. Human numbers are expanding at an alarming rate in most of the developing nations (Fig. 1). With the increase in human numbers comes major habitat destruction as forest cover is cleared for fuel and more agricultural space, often to provide plantation foodstuffs to help service large foreign debts. In the developed countries increasing use of fossil fuels as an energy source, and forest clearance for the paper industries adds to the global problem of CO_2 buildup. Atmospheric modellers caution us that summer temperatures could rise from 5 to 9°C in different areas of southern Canada, and that even higher temperature rises could occur during the winter months in northern Canada. The implications of such a man-induced "Super-Interglaciation" are severe. The climate modellers see the present as "the norm", and tend to use the last 200 years (good historical and instrumental records) as the "long-term history" for their climatic scenarios.

Anyone who has worked with the Late Quaternary record will realise that such an assumption is dangerously naive. There is good geological and biological evidence in the very recent past to indicate that climate conditions (and associated physical manifestations) have fluctuated wildly, and extremely rapidly, *without* any assistance from mankind. Accordingly, human modification of the planetary climate can only be superimposed on top of a natural trend of climatic ameliorations and deteriorations from our current "norm". Major climatic warm periods (such as the present) are known as interglaciations. Smaller climatic warming episodes (where the temperatures do not reach present values at a given geographic locality) are interstadials. By far the longest blocks of time belong to the stadials or glacial periods, where temperatures were far below the present figures for practically all high- and mid-latitude regions.

A few points should be stressed. The last interglaciation (the Sangamonian of North America, the Ipswichian of Britain and the Eemian of western Europe) had summer and winter temperatures which were warmer than present at some time during the interglaciation. We can establish this from the presence of biological indicators. These might include the giant tortoise, *Geochelone*, in deposits in Illinois (King and Saunders, 1986), fossil insects of the Canadian Arctic islands, in Greenland, and Europe (Coope, 1977), and the presence of the pond tortoise, *Emys orbicularis*, and animals such as *Hippopotamus amphibius* (Stuart, 1977) in deposits as far north as England. Such animal communities suggest that winter temperatures were at least several degrees higher in mid-latitudes, and that summer temperatures could have been 3 to 5°C higher at mid- to high-latitudes. Even the present interglaciation has had higher than present temperatures during the hypsithermal interval. There is evidence of high-latitude early Holocene warming (to above present temperatures) in northwestern Canada and Alaska, and tree stumps of white spruce (*Picea glauca*) on the Tuktoyaktuk Peninsula some 5,000 years old indicate that treeline extended at least 50 km further north in middle Holocene time (Ritchie and Hare, 1971). In Scandinavia Flandrian occurences of water chestnut (*Trapa natans*) and filbert (*Corylus avellana*) are hundreds of kilometres north of the present limit

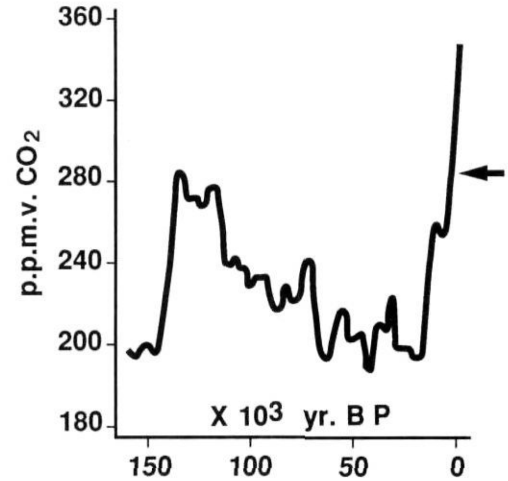

FIGURE 1. CO_2 concentration in Antarctic ice (Vostock core) for the last interglaciation to Holocene cycle. Arrow indicates maximum level of natural CO_2 during the last 160 ka. The rise to 355 ppm above the arrow indicates anthropogenic production of CO_2.
Concentration de CO_2 dans la glace de l'Antarctique (forage de Vostock) à partir du dernier interglaciaire jusqu'à l'Holocène. Flèche = niveau maximal de CO_2 atteint en 160 ka. L'accroissement jusqu'à 335 ppm au-dessus de la flèche montre la production de CO_2 par l'homme.

of these plants (Anderson, 1910). Even if we exclude northwestern Europe where mankind was already dabbling in major deforestation in the middle Holocene, it is certain that the events of the hypsithermal, and of the preceding interglacial, were natural, and not man-influenced warming events. It follows that one way to study potential future climate change, is to review what has happened in the last interglaciation and present interglaciation (Holocene). Obviously there is no way to predict the future, but a better understanding of past changes is not just desirable, it is an essential input to any predictive modelling work. The answers to such a study will lie not only in the instrumental record of man-induced change, and present atmospheric and oceanic circulation patterns, but also in the physical, chemical and biological record provided by proxy-data. Unfortunately there are limitations to such analyses, and these are hinged on the dual questions of recognising an interglacial and the timing of events.

PREAMBLE

Two field geologists mapping a region discover a thick band of organic detritus exposed beneath an undoubted till unit. Having established that the deposit is *in situ* they are now faced with the problem of age determination. As geologists, stratigraphic methods might be favoured, but all that this establishes is that the deposit is beneath till of the last major regional ice advance, and resting on fluvial (or are they fluvio-glacial?) gravels. These in turn rest upon bedrock. The problem of whether the deposit is of interstadial or interglacial, or even preglacial, age, still remains. Radiocarbon dates are of dubious value except in the resolution of the youngest interstadials. There are some promising new techniques of age determination, but given the absence of tephra bands anywhere in the sequence, the geological team is going to be forced to use the identification of biological materials to help resolve their stratigraphic conundrum. Hopefully there will be some biological clues (per-

TABLE I

The time frame of interglacial concepts

1840:	The advent of the glacial theory.
1850:	Discovery of organic beds between tills.
1870's:	Recognition of different horizons of organic detritus between multiple tills.
1880's:	Recognition of leaching, weathering zones and soils as criteria for "interglaciations".
1900's:	Establishment of the fourfold glacial/interglacial sequence in the northern Alps.
1920's:	Creation of the concept of interstadials and interglaciations.
1950's:	Recognition of multiple glacial/interglacial cycles in the deep-sea isotopic record.
1960's:	Controversy concerning biological parameters in the recognition of short-lived climatic excursions which reached present day summer temperatures "interglacial" episodes (insects) versus interstadials (plants).

haps an extinct mammal fauna) which will help to resolve the age of the deposit.

THE HISTORY OF INTERGLACIATIONS

Imagine how simple life was when one could make statements such as the following from Buckland's *Reliquiae Diluvianae* (1823). "In this dipersion of blocks of granite and beds of gravel in North America, we have evidence of a debacle by the diluvian waters in the western hemisphere, analogous to that we have been examining in Europe; and the presence of the bones of elephants, and other animals which are common to the gravel of both continents, shows that the time of its formation was in each case the same."

In the early decades of the nineteenth century the church and science were in conflict over the interpretation of organic remains found in "diluvial gravels". All of the shells, trees, and mammalian remains whether beneath, within, or above "drift" deposits were collectively assigned to the remnants of the Noachian flood. Seventeen years after Buckland published the book cited above, Louis Agassiz (1840) presented his glacial interpretation of drift sequences (Table I).

The problem of ice-free episodes between deposits demonstrably laid down by glaciers has been one which has vexed geologists from the near-beginnings of the glacial theory. In the same decade as the publication of Louis Agissiz' book on the study of glaciers other field geologists were reporting two "drift" layers and the intermediate position of "inter-drift" deposits.

By the mid 1850's "interglacial strata" (deposits between two layers of till) had been reported by Collomb (1847) from the Vosges, by Ramsay (1852) in Wales, by Chambers (1853) in Scotland, and by Morlot (1856) in Switzerland. By the 1870's Geikie had described the stratigraphic relationships of four glacial episodes in the East Anglian drift sequences of England. These deposits had interspersed between them, beds of organic debris which had already been described by Godwin-Austen in 1851. Charles Lyell in his classic text on the "Antiquity of Man" (1863) referred to these deposits as "forest beds", and made mention (p. 216) of plants, vertebrates and insects from the detrital sequences. "The insects, so far as they are known, including several species of *Donacea*, are, like the plants and freshwater shells, of living species. It may be remarked, however, that the Scotch fir has been confined in historical times to the northern parts of the British isles, and the spruce fir is nowhere indigenous in Great Britain. The other plants are such as might now be found in Norfolk, and many of them indicate fenny or marshy ground."

Lyell's observations and comments raise a number of important points. Firstly, he recognises that both extinct and extant species of animals and plants are found below the "Boulder clay of (the) glacial period..." and above the Norwich Crag of East Anglia. Secondly, he states that the modern flora, although similar to that of present day Norfolk, contains Scotch fir (confined to northern Britain) and spruce (extirpated from the British Isles). In this way he first raises, at least indirectly, the problem of floral and faunal composition which must be considered when looking at "interglacial" deposits. Sixty five years were to elapse before part of this problem was re-examined on the eastern side of the North Sea Basin.

QUESTION 1: How do we resolve the climatic implications of plant and animal remains in fossil assemblages when there are no modern equivalents in the same region today?

The answer to this question is not simple. The elements which control the distribution of a plant or animal species are complex and not easily resolved without a great deal of observation. The first complicating factor is that the perceived modern distribution (of the 1980's) for many animal and plant species, is not the same as the recorded range seen perhaps as little as a few decades ago, and certainly not the same as in the pre-European settlement of North America. In order to address the question above one is almost forced to look at recent proxy-data in order to eliminate major human interference of the natural (non-human) habitat. In Europe the time frame would need to be at least an order of magnitude larger to reduce the effects of anthropogenic influence on the natural environment.

There is a danger in using this approach, especially if one wishes to apply the present interglacial flora and fauna to the interpretation of still earlier interglacial deposits from the same region. One has to bear in mind that colonisation potential for plants and animals is not always equal given the same relative warmth during an interglaciation (or even interstadial). The presence of physical barriers, especially temporary ones (major glacial lakes; meltwater rivers, rising sea levels, etc.), as well as local climates created by stagnant ice and/or permafrost, and the method of climatic amelioration (dry, evaporative, or moist humid conditions), coupled with the speed of change at the termination of glaciation create specific sets of conditions. These primary factors in combination with secondary factors (soil maturation, lower plant colonisation) and tertiary factors (predation and competition) set the stage for sometimes subtle, and sometimes more glaring, differences as plant and animal communities migrated with the glacial advances and retreats. Furthermore we have little or no knowledge of past gene pools, or of the ecological tolerences of morphological species, or of the biotypes within such species.

By the 1880's the work of Geikie (1874), together with Penck in the northern Swiss Alps (1882), and reinforced by mapping in North America, was leading toward relatively fixed ideas of three or four non-glacial episodes which punctuated periods of glacial or drift deposition.

In North America, the decades leading up to the end of the 1800's had seen the recognition of both major ice advances, and retreats, and minor glacial oscillations. These were described as "Stages" (Table II). Leverett's monograph (1899) on the Illinois Glacial Lobe outlined some of the thoughts of the day. Stages 1 to 15 outlined the known stratigraphy from the oldest recognised drift sheet — the Albertan of Dawson, to the oscillations of Wisconsinan drift sheets.

Shortly after the turn of the century, Penck and Brückner's work along the alpine foreland resulted in the classical Günz, Mindel, Riss and Würm terminology, so familiar to all Quaternary geologists (Penck and Brückner, 1909). In the same year the term Nebraskan was introduced (Shimek, 1909) and the North American equivalents became known as the Nebraskan, Kansan, Illinoian and Wisconsinan. The intervening interglaciations in Europe and North America were inferred from the presence of weathering horizons and soil profiles, reinforcing new criteria in the concept of "interglaciations". Unfortunately, the almost total acceptance of their fourfold division of glacial and interglacial periods, blinkered Quaternary statigraphy for decades in many different parts of the world.

Nearly twenty years later in 1928, two north European workers, Jessen and Milthers, pointed out that the interglacial periods which they had studied in Jutland and adjacent northern Germany had vegetational sequences which could be defined by pollen analysis. This keynote paper not only elaborated upon the potential recognition and differentiation of interglacial episodes but also introduced the concept of interstadials.

The view of four major glaciations separated by interglaciations lasted until the 1950's. At that time several developments conspired to throw considerable doubt on the interpretation of the preserved terrestrial record. Foremost among these were the techniques of recovery of long deep-sea cores, the $^{18}O/^{16}O$ paleotemperature reconstruction method developed by Urey, and the publication of generalised paleotemperature curves derived from the pelagic foraminiferan *Globigerinoides sacculifera* from Atlantic and Caribbean cores (Emiliani, 1955, 1958). The terrestrial worker was suddenly faced with the prospect of multiple episodes of glaciation and interglaciation, and with evidence of short-lived partial cycles between the major fluctuations.

The recognition in the deep-sea marine record of perhaps twenty or more climatic deteriorations severe enough to cause glaciations over the last one to two million years created a need to better define the terrestrial concept of an interglaciation. As West (1968) pointed out, the terminology allows three different categories. The first is the stratigraphic "non-glacial" episode, which can range from a minor glacial oscillation to one in which some degree of landscape dissection has taken place. The second allows for the development of soils, and, as such, is indicative of a moderate time interval. The type of soil development may provide some clues about the climate of the non-glacial episode. The third category is one where organic materials are situated between two different till units. Detailed analyses of the fauna and flora may determine whether the organic materials accumulated in an interglacial or an interstadial.

TABLE II

The concept of stages

(as defined by F. Leverett in **The Illinois Glacial Lobe**, U.S.G.S. Monograph XXXVIII (1899), p. 21, 22).

Stages 9-15: Oscillations of *Wisconsin* drift sheets.

Stage 8: the fourth period of recession of deglaciation — the *Peorian* of Leverett, possibly equivalent to the *Toronto Formation* of Chamberlin.

Stage 7: the *Iowan* drift sheet and main loess deposit.

Stage 6: the third inteval of recession or deglaciation — the *Sangamon* of Leverett.

Stage 5: the *Illinoian* drift sheet.

Stage 4: the second period of recession of deglaciation — *Yarmouth* of Leverett.

Stage 3: the *Kansan* drift sheet of the Iowa geologists.

Stage 2: the first interval of deglaciation — the *Aftonian* of Chamberlin.

Stage 1: the oldest recognised drift sheet — the *Albertan* of Dawson, including also the *sub-Aftonian* of Chamberlin.

We now reach the second important question.

QUESTION 2: How does one define an interglaciation, and how does it differ from an interstadial?

The original definition as provided by Jessen and Milthers was that an interstadial was too short (or too cold) to permit the development of temperate deciduous woodland as seen in the Flandrian (Holocene) climatic optimum of the area in question. An interglaciation was defined as an episode where the summer temperature was at least as high as that seen during the Flandrian optimum in the area concerned.

While these definitions allow a mental concept of the terms interglaciation and interstadial, the reliance upon plants and particularly the development of deciduous woodland sequences, does not adequately take into consideration factors involved in plant migration. Similarly, the time-scale involved is also open to question.

This leads us to question 3:

QUESTION 3: Should an interglaciation be defined upon the climatic interpretation provided by one biological group?

The Four Ashes locality (type section for the English Devensian) has a large number of detrital peat lenses contained within a gravel sequence capped by Late Devensian till. The youngest dated organic horizon was 30,000 radiocarbon years old, but others exceeded the dating capability of the Birmingham radiocarbon facility (about 43, – 45,000 years BP). Several lenses at the bottom of the sequence, protected by hollows in the Triassic bedrock, contained faunal and floral elements which indicated the presence of temperate plant growth. Site 44 contained holly (*Ilex*) leaves and fruit stones, alder (*Alnus*) cones, and oak (*Quercus*) pollen, and is believed to represent deposits of the Ipswichian Interglaciation (Morgan, 1973). Sites 9 and 10 (possibly equivalent to the Brörup of continental Europe) possessed spruce and pine (*Picea* and *Pinus*) pollen and contained at least one species of conifer-eating beetle as well as other Coleoptera species found in the boreal forests of northern Scandinavia. Morgan (1973) stated: "...Subsequent to this boreal forest phase there is no further evidence of trees, but it not clear what caused their elimination".

Site 20 at Four Ashes (dated at 43,500 years BP) contained an assemblage of arctic stenotherms. Morgan goes on to state: "If (a) cold period did exist around 50,000 years ago and if it lasted for several thousand years it may have forced the trees completely out of Britain and into southern Europe. Even in southwest France during the 'last part of the Full-Glacial', Oldfield (1964) found no tree pollen, and he postulated that some of the thermophilous trees may have survived this period even further south in Iberia." Morgan then points out that "... open and treeless conditions existed for the rest of the Middle Devensian, despite the marked amelioration which occurred around 40,000 years B.P." Later she remarks that "... temperature is unlikely to be the factor preventing the growth of at least some trees in this area".

Work subsequently undertaken in other British sites suggests that optimum Middle Devensian[1] July temperatures were probably at least 3°C above those suggested by Morgan from the fossil Coleoptera in the Four Ashes sequence. The inference from these insect assemblages is that the short-lived climatic excursion of the Middle Devensian is of interglacial status. The plant assemblages, however, barely responded to this rapid climatic warming, retaining a tundra-like character throughout the Upton Warren "Interstadial". Based upon the plant succession, the Upton Warren Interstadial complex is just that; an interstadial.

This conflict in interpretation raises some questions in the real meaning of the term "Interglaciation"? Should the criterion of an interglaciation be one which is based upon *one* group of organisms (plants), or should it reflect the establishment of temperature conditions as warm as present at that geographic location? Insects happen to be a fairly mobile group, having the capability to move rapidly across large geographic areas. Such mobility is certainly matched by other groups of organisms, although their ability to successfully colonise a region may be dependent upon the growth of plants which act as the food source. This is also true of many, but not all, families of Coleoptera.

Wright (1984) draws attention to the remains of tree birch and juniper in the late-glacial of the English Lake District. He suggests that although the insects indicate temperate conditions (conditions which were, in fact, as warm as the Flandrian maximum), "... the strong diversity of the herbaceous flora implies fairly temperate conditions, even though trees were restricted". Such an accomodation is not as easily explained in the (Middle? Devensian) thermal excursion at Four Ashes; the trees simply *did not arrive*, even though temperature conditions were more than adequate for tree growth seemingly for at least some two thousand years (from about 43,000 to 41,000 yr. BP), and perhaps longer (Morgan, 1973). The factors at play here are many and variable, but distance of vegetation migration and substrate instability might have been important in the apparent inability of trees to grow in central England during this post-Ipswichian temperate episode. Such factors, illustrating the progression from cold cryocratic conditions at the start of an "interglaciation" to the telocratic conditions at its termination, have long been recognised and are described in the literature (Turner and West, 1968).

Differences in interpretation of insect and pollen evidence are not restricted to warming phases, but also apply in certain cases to cooling trends. After 12,000 years BP in Britain (in the time frame approximately contemporaneous with the Alleröd of Europe) *Betula* woodland was expanding under a presumed warming trend, while the insects were signalling the start of a climatic deterioration. Similar observations may have been responsible for differences in interpretation of pollen and insect evidence in the Nelson River site (Nielsen *et al.*, 1986) in northern Manitoba. Perhaps, given the migrational lags alluded to above, we should not look at a single biological indicator, but rather re-evaluate the classical interpretation of interglaciations. Certainly interdisciplinary biological evaluation is the minimum required. If possible, geochemical indicators might be added to help elucidate these rapid changes in past climate.

This last point brings us to a final set of questions:

(a) Should the length of an "interglaciation" be defined?

(b) Should the sole criterion of an interglaciation be that of summer temperatures only, or that summer *and* winter temperatures reached modern values in that geographic area?

(c) What consensus can be achieved concerning interglaciation status?

I leave the answers to the last set of questions in abeyance. Some of the considerations become a matter of semantics, as for example, in the case of the relation of Sangamonian Stage 5 (e to a). Do the deep-sea oxygen isotope records reflect accurately the conditions experienced on the northern land masses? Should the Sangamonian be considered only as Stage 5e? If Stages 5d of 5b were cold enough to cause glaciations should they be part of the Sangamonian? What happens to the definition if short-lived thermal excursions of Stages 5c and 5a approached, or surpassed, modern temperatures in a given geographic location? Given the current knowledge of equinoxial precession and variations in solar radiation, is it now practical or possible to address question (b) above? Can sets of criteria be established to determine the climatic regime at a particular site, bearing in mind that the faunal and floral

1. For example, given the recently revealed uncertainties in radiocarbon dating (plateaus and chronological divergence of calendar versus "Libby" years), is the "Middle" Devensian (Upton Warren Interstadial) possibly equivalent to Stage 5a?

record will vary with different depositional and preservational histories? Can reference sites be located which show a complete sequence through the last interglacial cycle in a number of different geographic locations throughout the world? Can we provide the necessary dating techniques to define segments of the last interglaciation? Still other considerations raise further questions which should be addressed by an international body such as INQUA.

PROSPECTS FOR THE FUTURE

I conclude with an appeal to those who control the Global Change programme, the various governments which will provide the funds, and the ministries which will control the way in which the research is conducted. Global Change is a potential catastrophy facing mankind. It is not the first global threat, pandemics have come and gone, but it is certainly the largest in scope and complexity. Because of the vast numbers of humans, Global Change forces us to carefully consider the planetary implications of living as part of a global village within a fragile bubble in space. Government leaders who all too often have little or no training in, or knowledge of, science have to realise that not all scientific answers come from applied research. There must be strong support and adequate funding for the pursuit of basic knowledge without any applied result in mind. Basic research is time consuming, often (but not always) expensive, and ignored at the detriment of future generations. Much of what has been described above has been resolved by the pursuit of basic knowledge. In doing so, Quaternary scientists have already become familiar with Global Change. Atmospheric physicists and climate modellers who ignore 150 years of accumulated Quaternary data, do so with folly. The results of their computer models cannot, and should not, be regarded as true science without extensive consultations with members of the Quaternary community. Similarly there is a responsibility amongst Quaternary workers to understand, and explain the limitations of their methods, to those responsible for the analysis of Global Change. Only with these two elements combining in close cooperation with other scientists and social workers can we face the harsh realities of the twenty-first century.

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