Comments on the EXXON cycle chart for the Cretaceous system

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ABSTRACT

Correlation between the various columns of chronostratigraphy on the EXXON chart has often been assumed without being tested. A considerable number of the tie-lines can be shown to be wrong. As a result one cannot know what many of their '3rd order cycles' refer to in the stratigraphical column. In most examples where such cycles of sea-level change can be dated, somebody else has already recognised them. Most of their '2nd order cycles' are suspect.

Paradoxically, current research shows that changes of sea-level through intervals of a few million years may have been simultaneous in Europe and the USA to an accuracy of ±100,000 years.

Key-Words: EXXON chart, 3rd order cycles, 2nd order cycles, Cretaceous, eustasy, sea-level changes, accuracy in correlation.

RESUMEN

En términos generales se asume la validez de las correlaciones entre las distintas columnas cronoestratigráficas incluidas en la escala de la EXXON.

Sin embargo, es posible demostrar que un número considerable de éstas no

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son correctas. Como consecuencia de esto es difícil saber cuáles de los ciclos de tercer orden existen realmente en el registro estratigráfico.

Por otro lado, en la mayor parte de los casos en los que ha sido posible datar los ciclos eustáticos, éstos han sido reconocidos por otros autores. La mayoría de los ciclos de segundo orden son dudosos.

Paradójicamente, las investigaciones más recientes parecen indicar que para intervalos de tiempo de unos pocos millones de años, los cambios eustáticos podrían ser simultáneos en Europa y Estados Unidos con una precisión de ±100.000 años.

Palabras clave: Curva EXXON, ciclos de 3.^{er} orden, ciclos de 2.^{o} orden, Cretácico, eustatismo, cambios del nivel del mar, correlación detallada.

INTRODUCTION

There is a long tradition in religions and mythology of a world-wide flooding of the land (Dean, 1985). It is now more than 100 years since Eduard Suess put this on a scientific basis. He noticed that there were many regions of the world where Upper Cretaceous sediments spread on to much older rocks. He noted that the widest extent of this transgression of sea on to land was represented by 'Senonian' sediments, but because the transgression seemed to have started during the Cenomanian, he wrote of 'the Cenomanian transgression' (1875, pp. 104-117; 1906, pp. 289-292). Fewer people noticed that Suess also discovered other times in the history of the Earth when sea-levels were exceptionally high, e.g. Middle Devonian, or exceptionally low, e.g. near the end of the Jurassic period. He distinguished these major world-wide changes as 'eustatic movements', but he also wrote, 'A close examination of the stratified series often leads us to suspect the existence of numerous smaller oscillations which are hard to reconcile with eustatic processes' (1906, pp. 544-545).

For many years after Suess' work, ideas on sea-level changes were developed from other systems: for a lucid and learned summary, see the essay by Dott (1992). Local curves for parts of the Cretaceous were produced, e.g. Kahrs (1927) for the south-west margin of the Münster basin in Germany. Under the influence of Stille (1924) sea-levels were always related to tectonics, e.g. Kirkaldy (1939), Arkell (1947), Owen (1971); a eustatic element was not even considered. As recently as 1976, Hughes wrote, 'no world-wide pattern of transgressions is discernible; they are really better regarded as epeirogenic 'immersions' of parts of continental edges and therefore of regional significance or less' (1976, p.66).

The first broader survey was by Matsumoto (1967). This remarkable but obscure paper has still not been superseded. Matsumoto showed that the data demanded that both eustasy and broad tectonic warping must be allowed for if sea-level changes are considered on a world-wide scale. Happily, Matsumoto published a summary in English in 1977.

In the 1970's a team of geologists, led by Peter Vail, working for EXXON in Houston, developed a theory of relationships between cycles of coastal onlap and offlap of sediments which they related to eustatic changes of sea-level. Their cycles or 'sequences' were divided by unconformities – rather like Suess considered that his eustatic lows corresponded to boundaries between systems. From an interpretation of the facies in each 'sequence' and the lateral relationships of these facies, it would be possible to make allowances for a tectonic interference or overprint on the sequence of facies. Their concepts are now usually called 'sequence-stratigraphy'.

The principles of sequence-stratigraphy have been described in English many times, from the original first full account by Vail *et al.* (1977) to a readable summary by Hallam (1992). No attempt is being made here to repeat these discussions of the theory. The valuable review by Cross & Lessenger (1988) sometimes shows more insight into the ideas than are easily extracted from the papers of the EXXON team. Jervey (1988) has provided a full theoretical account of the subject, with beautiful coloured diagrams. Obections to the theory are also numerous. There are the all- embracing views of Mörner (1976, 1981, 1989) who argues that shelf-unconformities cannot be explained in terms of changes in global sea-level because these are affected by deformations of the geoid and differential rotation. Burton *et al.* (1987) and Kendall & Lerche (1988) show that the quantitative relations between sediment accumulation, eustasy and tectonic subsidence can never be calculated exactly. Criticisms of the sedimentological theory have been made by Miall (1991) and of the seismic theory by Neidell (1979), or the resolution of the seismic stratigraphy (Cartwright *et al.*, 1993). The biggest general problem is that the data and origin of the EXXON chart has never been documented, as Miall has pointed out repeatedly (1986, 1991, 1992), and Miall has also argued that the resolution of biostratigraphic correlation will be inadequate to test the eustatic origin of the smaller cycles in the EXXON curves. We are all expected to take the accuracy and reliability of their chart on trust. Altogether there has been a plethora of discussions of the theory. In this paper I try to see how far the EXXON chart works, or does not work, in practice. For if it works, theoretical objections must collapse. If it does not work at all, we should abandon it as a dangerous doctrine. If it works in some parts and not in others, we ought to ask why.

Ideally, a distinction should be made between: (i) whether there have been synchronous changes of sea-level or not, i.e. is eustasy true or false, and (ii) if there have been eustatic changes of sea-level, does sequence-stratigraphy provide a reliable technique to detect, date and measure the changes? To complicate matters further, there are some geologists who recognise simultaneous submersions of widely separated cratons, but still argue that these are controlled by 'tectonic forces', e.g. Sloss (1991), and before him, Stille (1924).

THE COLUMNS OF THE EXXON CHART

The EXXON chart contains 27 columns, most of which are lumped into five groups.

a) Time in millions of years

Since many of their changes of coastal onlap only lasted a few hundred thousand years, the accuracy of this scale is important, particularly because the authors name their sequence-boundaries by these radiometric dates. Their scale for the Jurassic-Cretaceous is taken from dates for stage-boundaries by Harland *et al.* (1982 edition, not the 1990 edition). Haq *et al.* (1988) themselves estimate that the average limits of uncertainty are ± 3.0 m.y. in the Early Cretaceous, and ± 1.75 m.y. in the Late Cretaceous. This is bad enough, but some parts of the Cretaceous are even more uncertain than this. Thus dates for the Jurassic-Cretaceous boundary range from 145.6 Ma, with a possible error of 9 m.y. (Harland *et al.*, 1990) to 130 ± 3 Ma (Kennedy & Odin, 1982), a difference of more than 15 m.y. – equivalent to about three stages of the Lower Cretaceous! Note that this inaccuracy is independent of different biostratigraphic definitions of the boundary between the two systems.

Although Late Cretaceous radiometric dates are much better constrained than Early Cretaceous dates (particularly compared with the pre-Aptian), an uncertainty of 1.75 m.y. is nearly as long as the Coniacian age (2.1 m.y. according to Obradovich (in press); 1 m.y. on the EXXON chart). The EXXON team had to plot their data on some stated scale, and a published radiometric scale is as sensible as any, but one must not conclude that it is as accurate as the implied resolution of the chart. If one uses the dates to name the coastal onlap changes, all the names will have to be changed as the radiometric scale is revised.

An example of the resultant oddities is that the reference date for the 'latest Valanginian' at 119.0 ± 2.0 Ma (Haq *et al.*, 1988, Appendix B) would fall half way through the Hauterivian (121 to 116.5 Ma) on the chart. This results from using 'best-fit data'. A trenchant criticism of such laxity has been made by Odin (1986). The Coniacian stage may be half as long or three times longer than the 1.5 m.y. average of Obradovich (in press) and the EXXON chart, i.e. 0.75 to 3.5 m.y. 'This corresponds to a factor of variation of six to be added to the authors' estimate for the *mean zone* durations in this stage' (Odin, 1986, p.197).

(b) 'STANDARD CHRONOSTRATIGRAPHY'

The classification of the Cretaceous system under erathems, systems, series and stages is in accord with the recommendations of the International Subcommission on Cretaceous stratigraphy (Birkelund *et al.*, 1984). Two alternative stages for the base of the Cretaceous, Berriasian and Ryazanian, are shown. The meanings of the stages are discussed, where necessary, in the next section.

(c) 'BIOCHRONOSTRATIGRAPHY'

Five columns are used here: 'planktonic foram biochronozones', 'nannofossil biochronozones', 'macrofossil biochronozones Great Britain', 'ammonoid biochronozones (tethyan region)' and 'dinoflagellate biohorizons'. Three problems exist here. (1) Are the zonal divisions widely recognisable; in particular, can they be used in regions where the sequence-stratigraphy is going to be worked out? (2) Do the zones in any one column correlate correctly with the zones in other columns? (3) Are the durations of the zones correct? For all criteria, the EXXON team had set themselves an almost impossible task, and there are mistakes that are quantitatively serious in all three.

(i) *Recognition of zones*

For the Middle Albian and part of the Upper Albian the ammonite zones for the tethyan realm are named after species of Hoplitidae, an exclusively old world boreal family. One must suppose that the coastal onlap curve is actually based entirely on boreal evidence at this time.

Four ammonite ones have been fitted, with some difficulty, into the tethyan Coniacian. These zones are from the work of Kennedy (1984) on the Coniacian ammonites of France. Most of this research was done in northern Aquitaine and the lowest of the four zones has not been found in France outside Aquitaine; whilst the top zone is not clearly demarcated in Aquitaine. Three of the four genera of the index species are also known in the western interior of the United

States, but it has been found necessary to use different index species there (Kennedy & Cobban, 1991). Such fine local divisions on the EXXON chart give a spurious appearance of precision.

The ammonite index used for their Upper Maastrichtian is *Pachydiscus neubergicus*. There is one specimen of *P. neubergicus* (von Hauer) known from the base of the Upper Maastrichtian (on the belemnite scale) in Denmark; and in Australia there are chronological subspecies in the Upper Maastrichtian (Henderson & McNamara, 1984), but *P. neubergicus* is typically a Lower Maastrichtian species, probably passing into *P. gollevillensis* (d'Orbigny) in the Upper Maastrichtian (Kennedy, 1986). Does this mean that the Upper Maastrichtian parts of the EXXON curves are actually Lower Maastrichtian?

(ii) *Correlation between columns*

It is seldom realised by geologists at large how insecure most zonal schemes are, and how few are the regions in which any one scheme has been successfully tested. Papers with titles such as 'Standard of Cretaceous System' (Muller & Schenck, 1943) do our science a disservice. A few years ago a small group of biostratigraphers was asked to produce *the* standard scale of the Cretaceous system on Earth, along the lines of the EXXON chart, for the use of sedimentologists, tectonicians and similar geologists. That such a standard did not exist; that possible scales needed disclaimers of application and accuracy; that almost no reliable correlations existed between different scales; all these limitations were met with amazement. An example of the current state of knowledge is shown by comparison of expert opinion on different groups of fossils from one unit of a few metres at Tercis in south-west France which, prior to discussion between the 'experts', ranged from Lower Santonian to Lower Maastrichtian (Hancock *et al.*, 1993)! Hence, only a few examples of the problems can be taken from the EXXON chart because most of the tie-lines between these columns have yet to be proved.

Every wide-ranging stratigrapher working on the Mesozoic meets the difficulty of correlations between boreal and tethyan realms. One of the times of particular difficulty lies around the Jurassic-Cretaceous boundary, however that is defined. The EXXON chart shows the tethyan Berriasian extending down into the Jurassic, and starts the Cretaceous with the base of the Ryazanian stage, i.e. gives the boreal definition priority over a tethyan definition. Whilst it is probably correct to place the tethyan *jacobi-grandis* Zone well below the boreal *Runctonia runctoni* Zone, it must largely be a matter of faith (see Hoedemaeker, 1987). The current fluidity of understanding and knowledge of Lower Cretaceous ammonite zonations can be seen by comparing three schemes which have been published in the last few years (Hoedemaeker & Bulot, 1990; Hancock, 1991; Hoedemaeker *et al.*, 1993). It is not a criticism of Peter Vail and his colleagues but an emphasis on the difficulties with which they are faced. Thus for the tethyan Aptian stage there are 11 ammonoid zones on the EXXON chart. In Hoedemaeker *et al.* (1993) there are eight zones; only two index species are common to the two charts!

Whereas some of the 'tethyan' ammonite zones are actually boreal, the planktic foraminiferal zones are tethyan, most of them 'tropical', rather than 'temperate' – where there might be an interfingering correlation with a boreal area (see Caron, 1985, fig. 5). Thus the extinction level of *Globotruncanita calcarata,* here placed at the summit of the Campanian, is known to be several million years older than the start of the Maastrichtian as defined on the chart by the base of the Zone of *Belemnella lanceolata* ('macrofossil zones Great Britain') and the base of the Zone of *Acanthoscaphites tridens* (ammonoid zones, tethyan region, although *A. tridens* has not yet been recorded from the tethys) (Salaj & Wiedmann, 1989; Schönfeld & Burnett, 1991; Burnett *et al.*, 1992). Interestingly, this has recently been confirmed by eustatic changes of sea-level (Hancock *et al.*, 1992)!

(iii) *Duration of the zones*

It is common praçtice, in the absence of other criteria, to assume that zones were of equal duration, particularly if they are based on the same group of organisms (e.g. Hallam *et al.*, 1986). The EXXON chart follows this principle within each stage, having chosen the isotopic dates for each stage boundary. Unfortunately, this assumption – which I have used myself – can be wildly false: for example, the number of rhythms in chalk-marl facies in the Cenomanian indicate that the Zone of *Acanthoceras rhotomagense* was some three times as long as the overlying Zone of *A. jukesbrownei* (Gale, 1990).

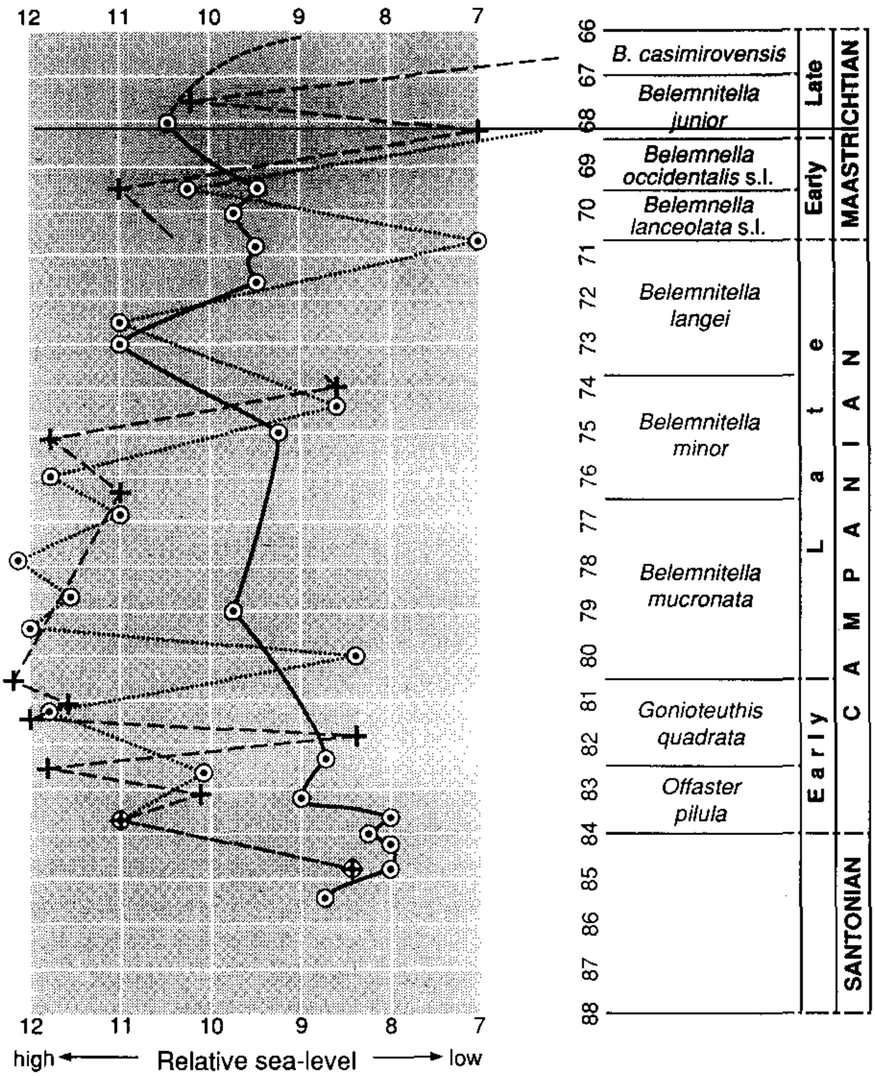
In the Turonian the Zone of *Collignoniceras woollgari* is given four times the amount of time allowed to the Zone of *Subprionocyclus neptuni,* apparently to fit in the four subzonal divisions recognised for the *woollgari* Zone in Touraine.

For the Campanian equal time is given to the Zones of *Offaster pilula, Gonioteuthis quadrata* and *Belemnitella mucronata,* although it has been known for some time that the lower two combined lasted only about 3 m.y. compared with 8 m.y. for the *mucronata* Zone (Ernst *et al.*, 1979), a disparity now increased (Hancock, 1991; Kennedy *et al.*, 1992). The highest macrofossil zones listed for the Campanian are actually well below the top of the stage in the sense used. Thus, above the Zone of *Belemnitella mucronata* s.s. there are the Zones of *B. minor* and *B. langei,* although it is true that these younger zones are not always recognised. But since the work of Blaszkiewicz (1980) it has been known that there are one, possibly two, ammonite zones above that of *Bostrychoceras polyplocum* and below that of *A. tridens*. This is the gap of about 2 m.y. between the extinction level of *Globotruncanita calcarata* and the appearance of *Belemnella lanceolata* referred to earlier.

All these present weaknesses in the bio-chronostratigraphy are open to future improvement, but it does mean that, for the time being, in those parts of the coastal onlap curve, where these inaccuracies exist, one cannot know what changes in sea-level are being postulated. There are sufficient of these problems to make it very difficult, and sometimes impossible, to reconcile their observations with those of other geologists. An example of such problems is illustrated in fig. 1.

Fig. 1.—Sea-level changes during the Campanian-Maastrichtian. The continuous line represents my own view of the times of the principal peaks and troughs of sea-level during the latest Cretaceous. For the basis and construction of this graph, see Hancock (1990). The dotted line represents the EXXON 'curve', taking only their peaks and troughs; the horizontal axis is from EXXON's radiometric scale; the vertical axis assumes that the height of their peak at 72.5 Ma on their scale equals my *langei* Zone peak, and that the depth of the late Turonian trough is the same as mine. The dashed line represents the EXXON 'curve', taking their cephalopod biostratigraphic scale. Since they have no representation for the belemnite Zone of *Belemnitella langei* or the ammonite Zones of *Didymoceras donezianum* and *Nostoceras hyatti*, there is a gap in the curve corresponding to this part of the biostratigraphic scale. There is no simple right or wrong answer to each point on these graphs. They have been plotted not just to show that there are disagreements, but the impossibility of comparing results unless one is working on identical stratigraphic scales.

Fig. 1.—Cambios en el nivel del mar durante el Campaniense-Maastrichiense. La línea continua representa mi interpretación del momento en el que tienen lugar los principales máximos y mínimos del nivel del mar durante el Cretácico terminal. Para una descripción más detallada del método utilizado en su construcción, ver Hancock (1990). La línea de puntos representa la "curva" de la EXXON, y en la que únicamente se representan los máximos y mínimos. El eje horizontal corresponde a la escala radiométrica de la EXXON. El eje vertical se representa asumiendo que el valor del máximo que se observa a los 72,5 Ma en su escala, es equivalente al máximo que aparece en mi Zona *langei*, y que el valor del mínimo que se observa en el Turoniense superior es el mismo que el que aparece en el mío. La línea discontinua representa la "curva" de la EXXON, dibujada con base en su escala biostratigráfica de cefalópodos. El tramo sin datos que se observa en esta curva se debe a que estos autores no han representado en su escala bioestratigráfica la Zona de belemnite *Belemnittella langei* ni las zonas de ammonites *Didymoceras donezianum* y *Nostoceras hyatti*. Es difícil determinar cuáles son los errores y los aciertos de cada una de las curvas que se representan en estos gráficos. Estas se han representado no sólo para demostrar la existencia de discrepancias notorias, sino también para poner de manifiesto la imposibilidad de comparar los resultados, a no ser que se trabaje con escalas estratigráficas equivalentes.



12 11 10 9 8 7
12 11 10 9 8 7
high ← Relative sea-level → low
66 67 68 69 70 71 72 73 74 75 76 77 78 79 80 81 82 83 84 85 86 87 88
B. casimirovensis
Belemnitella junior
Belemnella occidentalis s.l.
Belemnella lanceolata s.l.
Belemnitella langei
Belemnitella minor
Belemnitella mucronata
Gonioteuthis quadrata
Offaster pilula
Late
Early
Late
Early
MAASTRICHTIAN
CAMPANIAN
SANTONIAN

(d) 'SEQUENCE CHRONOSTRATIGRAPHY' AND 'EUSTATIC CURVES'

The EXXON chart agrees with some long established trends and widely agreed changes in sea-level, e.g. Suess noted that there were extreme lows very early in the Cretaceous. That there was a prominent peak sometime close to the start of the Turonian was already recognised in southern England by Hume (1894) and in the south-west part of the Paris basin by Cayeux (1897); in the USA this was shown by Kauffman (1969) and has been maintained by him since (Kauffman & Caldwell, in press). A very strong and relatively brief fall in sea-level early in the Late Turonian has been known since the 19th century (Hébert, 1876), and shown on many later charts.

Since their first comprehensive paper on global cycles of sea-level changes (Vail *et al.*, 1977), the team have claimed that their charts show cycles of three orders of magnitude. It is convenient to discuss the chart in these terms, even though their classification may be an illusion: it is notable that neither Kauffman & Caldwell (in press) in North America nor myself in Europe have used any but the shortest cycles.

(i) *3rd order cycles*

The finest cycles of coastal onlap on the chart, i.e. the finest resolution used in the construction of the chart, lasted $^1/_2$ m.y. These are called '3rd order cycles'. Since Cretaceous ammonite zones (or subzones) ranged from around 0.4 m,y. to perhaps 2 m.y. (Kennedy & Cobban, 1976; Hancock, 1991), the 3rd order cycles will have averaged very approximately one to two ammonite zones. Therefore, where ammonite correlations can be applied, it will be possible to test the reality of 3rd order cycles.

Valanginian.—At the base of the Valanginian there is a eustatic trough recognised by both the EXXON team and by Hoedemaeker (1987), but his next low is at the base of the Zone of *Himantoceras trinodosum* where EXXON has a eustatic peak. Somehow, Hoedemaeker, with his well nigh encyclopedic knowledge of the bottom four stages of the Cretaceous in south-west Europe, has not noticed the most marked coastal offlap on the EXXON curve in the Zone of *Thurmanniceras pertransiens*. This may be because they are actually getting their data from different faunal realms.

Aptian.—It so happens that the sequence-stratigraphy of the Aptian and most of the Albian of Kent in south-east England, where there is a precise biostratigraphic control, has been studied in detail by Hesselbo *et al.* (1990). Their diagram (fig. 8) comparing their sequence-boundaries with those of Haq *et al.*

(1988) shows good agreement. However, when this succession is interpreted in terms of sea-level changes, difficulties appear. Their boundary LG2 corresponds to sequence-boundary 109.5 Ma of Haq *et al.* It is shown at outcrop by the Sandgate Beds resting unconformably on the Hythe Beds. According to the EXXON eustatic curve, sea-level was falling at the sequence-boundary; it is shown as reaching a eustatic low in the Zone of *Parahoplites nutfieldiensis.* Sediments of this zone are transgressive across the stable London platform, joining the sea of southern England with that of the North Sea region. It seems difficult to reconcile this with a fall in sea-level.

Cenomanian.—Although most of the EXXON chart is not backed by recorded observations, for the Cenomanian stage we have now been given a sequence-stratigraphic interpretation of an actual area, namely for Sarthe on the south-west flank of the Paris basin (Haq *et al.*, 1988, fig. 12). The stage is shown as containing three sequence-boundaries, that is strong offlap shortly after a eustatic peak, at: 95.5 Ma (middle of the Lower Cenomanian, in the upper part of Amédro's Zone of *Mantelliceras cantianum*); 94 Ma (in the Middle Cenomanian at the junction of the Zones of *Acanthoceras rhotomagense* and *A. jukes-brownei*); and 93 Ma high in the Upper Cenomanian, a little below the top of the Zone of *Metoicoceras geslinianum*) (radiometric dates by EXXON). Each sequence-boundary is said to be immediately preceded by highstand deposits with eustatic highs a little earlier than the above dates. Immediately following each date there is believed to be a rapid and distinct fall in sea-level, of which that at the base of the *jukesbrownei* Zone is said to be the strongest.

The person with the most knowledge of the Cenomanian of the Sarthe is Pierre Juignet (1974, 1977) and, as it happens, Juignet has published his own analysis of the transgressive-regressive history (1980). He recognises 'deux épisodes transgressifs atteignant leur phase paroxysmale au milieu de Cénomanien inférieur et à la fin du Cénomanien moyen'. The first of these, corresponding to the Marnes de Ballon, agrees with Haq *et al.*; the second in the *jukesbrownei* Zone, near the top of the Craie de Théligny, is where Haq *et al.* have their strongest offlap. He does not separate a third 'phase' in the Late Cenomanian, but like Haq *et al.*, recognises 'une tendance régressive' at the end of the Cenomanian. Juignet's regressive phases are late in the Early Cenomanian and in the Late Cenomanian. The second probably corresponds with the eustatic fall immediately after the 93 Ma sequence-boundary of Haq *et al.*; the earlier regressive phase is placed by Juignet through the Sables et Grès de Lamnay, which embrace the top part of the '*cantianum* Zone' but largely belong to the Zone of *Mantelliceras dixoni.* This is a longer regressive phase than that immediately following the 95.5 Ma sequence-boundary of Haq *et al.*, but the two began at about the same time.

There is some agreement here between the two, but also a major disagreement about what was happening late in the Middle Cenomanian. Further comparison is difficult because the two authors use a different number of phases or cycles. In fact, Juignet recognises considerably more oscillations but did not try to map them out in the 1980 paper.

If this basin-marginal succession is traced northwards from Sarthe into the less marginal facies of the Pays de Caux in Normandy and on the north French coast, as Juignet himself has done (1980, figs. 3, 4 & 5), one can give more precise dates for the main changes. Thus the biggest regressive trough is marked by hard-ground Rouen No. 1, at the base of the Craie de Rouen, at the bottom of the Zone of *Acanthoceras rhotomagense*; this corresponds to the basal phosphatic fauna of the Craie de Théligny in Sarthe and the sub-Totternhoe Stone erosion in southern England (Hancock, 1990). In the coastal section at Cap d'Antifer there are no hardgrounds above Rouen Nos. 1 and 2 until Antifer No. 1 at the base of the Zone of *Metoicoceras geslinianum* (Juignet, 1974, fig. 27), thus confirming Juignet's eustatic high near the top of the *jukesbrownei* Zone (also known in England).

Further confusion has resulted from a mis-dating of the top Cenomanian – lowest Turonian formations in Fig. 12 of Haq et al. Very reasonably, they have placed a eustatic low immediately above the base of the Sables de Bousse, but this is at the base of the *M. geslinianum* Zone, not the base of the Zone of *Neocardioceras juddii* as they have shown it. Their 94 Ma sequence-boundary lies at the base of the Plenus Marls in southern England.

These inaccuracies by Haq et al. (assuming that Juignet and I are correct do not disprove EXXON theory, but they are certainly worrying. If each part of their general curve is based on several areas, there should be an automatic check from a comparison between them to prevent mistakes. It also confirms that it is better to use relatively open sea successions rather than basin-marginal successions, where vagaries of topography or tectonics, sometimes difficult to allow for, have a more marked effect.

Campanian.—Anyone familiar with the general geology of the Campanian in the northern hemisphere would probably be puzzled by the EXXON sea-level changes for this stage. It shows five eustatic peaks, which on their biozone scale for Great Britain are: (i) just above the base of the stage; (ii) at the junction of the Zones of *Offaster pilula* and *Gonioteuthis quadrata*; (iii) in the middle of the Zone of *G. quadrata*; (iv) just below the top of the Zone of *G. quadrata*; (v) a little below the middle of the Zone of *Belemnitella mucronata*. Their two highest sea-level peaks are nos. iii and iv, both in the *quadrata* Zone.

Whatever number of peaks one believes can be recognised in Campanian successions, there is one high in the Campanian which stands out in both north-west Europe and the U.S.A. (Hancock, 1993). This spread the Chalk over Ireland and is low in the Zone of *Belemnitella langei* as this zone has been used in the U.K. (Hancock, 1990). Ironically, it is this great eustatic peak which has enabled critical correlations to be made between the USA and Europe (Hancock & Kauffman, 1989; Hancock *et al.*, 1992). Within the limits of experimental error it agrees with both ammonite and strontium isotope intercontinental correlation of the Campanian-Maastrichtian boundary (McArthur *et al.*, 1992). Where is this great eustatic peak on the EXXON curve? Even their fifth peak is much too early; whilst their highest peak, No. iii, does not seem to correspond to a peak on anyone's curves for northern Europe, with the just possible exception of the western slope of the Urals in Arctic Russia (Naidin *et al.*, 1980). Where on Earth did the EXXON team collect their data?

Conclusions on 3rd order cycles.—I find myself in the odd position of believing in eustasy but finding many mistakes in the EXXON curves wherever biostratigraphy allows one to test them. Some of the sources of mistakes and errors have been indicated earlier in this paper; still others on the accuracy of the results have been discussed by Miall (1991, pp. 503-504). It worries me that many of their successes are where peaks and troughs have previously been recorded, but that many of those that are purely their own are difficult to substantiate. This sounds unfair, so can their detailed techniques be used by other geologists? Lots of geologists think they can, but all too often they seem to be just fitting their own records into the EXXON chart; the potential errors in the Late Cretaceous, and even more in the Early Cretaceous, make this easy. An example is shown by the work of Ernst and Küchler (1992, fig. 1) where they have ignored their own, approximately correct, radiometric dates and used EXXON's false apportionment between Early and Late Campanian time! In south-east France, Ferry (1990) has interpreted the meaning of limestones in limestone-clay alternations in exactly the opposite sense of the EXXON team, and ammonite stratigraphy is not adequate to see which is correct.

However, there is one criticism by Miall (1992) which is partly misplaced. He has pointed out that synthetic sections constructed from tables of random numbers allow a minimum of 77% successful correlation with the EXXON chart. This experiment makes no allowances for relative heights of individual peaks and troughs: they are not all the same. He partly recognises this by referring to the possibility of a second order eustatic changes as shown by writers such as Hallam (1984) and Weimer (1986).

(ii) *Cycles longer than 4 $^1/_2$ m.y.*

In their original work (Vail *et al.*, 1977, fig. 2) their 'second order cycles' ranged from 10-80 m.y. In the current chart their second order cycles are subdivided into 'supercycles' and 'supercycle sets'. The lower and upper boundaries of the Cretaceous system do not coincide with 'supercycle' boundaries, but there are approximately seven supercycles (4 $^1/_2$ to 12 m.y.) and nearly two supercycle sets (31 to 39 $^1/_2$ m.y.) in the Cretaceous system.

These cycles do not seem real to me. The resultant sea-level curves do not fit with well known published evidence. The nomenclature is derived from the classification of 'sequences' on the North American Craton by Sloss (1988); for anyone outside North America, it smacks of mumbo-jumbo; paradoxically, Sloss himself believes that the sequences reflect a tectonic history rather than eustatic changes of sea-level (Sloss, 1991).

There may or may not be long cycles of sea-level change but the EXXON curve allows too little change over a long time: there are too many returns to the same level. Such simple oscillations make it easier to see cycles in the pattern. Take their 'supercycle set Upper Zuni A', which ran from 107.4 Ma, the middle of the Zone of *Hypacanthoplites milletioides* in the Lower Albian, to 67.8 Ma, somewhere in the Zone of *Pachydiscus neubergicus*, placed in the Upper Maastrichtian but actually Lower Maastrichtian. This supercycle set contains four supercycles which can be tabulated thus:

Upper Zuni A		somewhere within Upper Maastrichtian
	UZA 4	Earlier half of broad low in long term curve: minimum sea-level, height 215 m, maximum 245 m. In short term curve the minimum is about 110 m at top boundary of supercycle, maximum 245 m in the middle of the Zone of *G. quadrata*.
		within the lower half of the Zone of *G. quadrata*
	UZA 3	Complete high-low-high in long term curve: minimum sea-level height 225 m, maxima at 240 and c.250 m. In short term curve the lowest minimum is c.125 m (low in the Upper Turonian); a lesser minimum of 180 m is placed in the middle of the Upper Santonian.
		little below top of Zone of *C. woollgari*
	UZA 2	Broad high in long term curve; minimum c.235 m at start of cycle. In short term curve and long term curve the maximum is 260 m, low in e Zone of *C. woollgari;* this is the highest sea-level in the whole of the EXXON chart for the Mesozoic and Cenozoic. The lowest minimum in the short term curve is 180 m near the top of the Middle Cenomanian.
		little below the top of Zone of *M. inflatum*
	UZA 1	Second half of broad low in long term curve; minimum sea-level height c.160 m at start of cycle, maximum of 245 m at the end of the cycle. On the short term curve there are five minima and five maxima, the last two in the Upper Albian being at 230 m, low in the Zone of *Euhoplites lautus*; and 245 m, high in the Zone of *M. inflatum*.
		lower part of the Zone of *L. tardefurcata*

The maximum heights in these 'supercycles' should give a pattern of the relative areal extents of sedimentation at these times. Of course, one must investigate the evidence with some care because former sediments may have been removed by erosion, thus destroying the evidence, although experience suggests that it is difficult to remove all evidence of a great spread of sediment. A good example of this problem is illustrated by the eustatic high which lies low in the *C. woollgari* Zone, where I happen to agree with the EXXON team. There are few areas in northern Europe where one can see overlapping *woollgari* Zone sediments because they were usually eroded away, along with earlier zones, in basin-marginal districts immediately after, during the subsequent *S. neptuni* Zone eustatic low. But onlap by the lower *woollgari* Zone can be seen on the south-west flank of the Massif Central to the east of Périgueux in France, where it rests directly on Callovian-Oxfordian limestones (Platel, 1979).

However, in a broader sense the relative heights during the Middle and Late Albian cannot have been of the same order as those given for the Campanian. This would give an equal extent for the Zone of *Mortoniceras inflatum* and *Gonioteuthis quadrata*. Where is such a pattern of distribution seen? The slipper-clay Gault of England and Germany would have extended over Ireland and Sweden. According to Haq *et al.* (1988, pp. 107-108) most of their Cretaceous evidence came from France, plus Belgium and the Netherlands for the Campanian-Maastrichtian; in the U.S.A. they used Colorado, Utah and central Texas. These regions do not show an equal spread of Upper Albian and Lower Campanian, although you might conclude this in central Texas or the Pays d'Auge in France if you ignored the facies involved (Ménillet & Monciardini, 1991). Against this EXXON view is the far greater spread and/or deeper water facies of Upper Campanian sediments across Europe from Ireland to Bulgaria; along the Atlantic seaboard of the U.S.A., through the Gulf Coast from Alabama, through Mississippi, Arkansas and into north-east Texas; in the western interior of the U.S.A. and Canada in spite of late Cretaceous uplift in the region; in southern Argentina; probably in Western Australia. In Nigeria the maximum associated transgression peaked slightly later in the Early Maastrichtian (Reyment, 1980).

The first graphs that I produced of Cretaceous sea-levels (Hancock, 1975) show an overall rise through the Albian and Late Cretaceous. Such graphs have now been refined but the trend has not changed: the levels during the Albian were never as high as those during the Late Campanian. What seems to be an obvious mistake makes one suspicious of other '2nd order cycles'.

The EXXON team are good geologists but since we know only the general basis of their method and not the details (except for the Cenomanian of the

Sarthe, see above), it is necessary to make some guesses, hopefully inspired. There are localities which show very high Cretaceous sediments resting directly on mid-Cretaceous, resting directly on pre-Cretaceous rocks. In the Cotentin peninsula in Normandy there are Upper Maastrichtian tuffeau facies resting directly on Cenomanian or Albian, which in turn is underlain by pre-Mesozoic rocks. Note that the raw evidence does not accord with either EXXON or myself, but it is certainly closer to the EXXON view. The real point is that in the investigation of eustasy by the study of marginal facies it is necessary to consider a very wide body of evidence. This is the major reason why Erle Kauffman and I have argued in favour of using basin-centre or open sea successions for the evidence, and not allow yourself to depend on the vagaries of local shorelines (Hancock & Kauffman, 1979).

CONCLUSIONS

It is perhaps inevitable that discussion of the EXXON chart should seem to be a catalogue of complaints. It did not need this chart to persuade many geologists to accept eustasy as a major control on Cretaceous facies. Indeed, it is possible that by assuming too fine a resolution of successions from seismic stratigraphy, they have damaged their own theory. The main virtue of the work by Peter Vail and his colleagues, who are more modest about their results than many of the followers, is that it has forced us to examine our results in much greater detail: compare the curves published in *Cretaceous Research*, volume 1, with the EXXON chart. Are we yet in a position to recognise simultaneous changes of sea-level in different continents at the finest biostratigraphic resolution? The sort of simultaneous changes that Hancock & Kauffman (1979) indicated, were only to sub-stage resolution at best.

New work with Bill Cobban of the U.S. Geological Survey suggests that some peaks and troughs of sea-level were coincident between England and the western interior of the USA down to a zone or fraction of a zone: a resolution of the order of less than 100,000 years. These are changes over time intervals which would be called 3rd order cycles by the EXXON team. As their biostratigraphy is improved, it should be possible, in spite of Miall's worries, to test other 3rd order cycles. Sequence-stratigraphy or similar techniques, can date relatively fine changes of sea-level, but the best dates are still given by facies- successions in the open sea, far from a shore-line.

At time-scales longer than about 12 m.y. plate movements are commonly strong enough to give a false picture, i.e. all EXXON's second order cycles are suspect. Eustatic changes over this time interval are not always strong enough or fast enough to prevent major plate movements dominating the regional picture.

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