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Solutions in chronostratigraphy: the Paleocene/Eocene boundary debate, and Aubry vs. Hedberg on chronostratigraphic principles

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Abstract

In several recent papers, M.-P. Aubry et al. have argued that "Hedbergian" principles of chronostratigraphy are being violated by the International Commission on Stratigraphy (ICS) when selecting Global Stratotype Sections and Points (GSSPs) for the formal divisions of the geological time scale. The current debate over the definition of the Paleocene/Eocene (P/E) boundary has been a major focus of their arguments. Unfortunately, Aubry et al. have obscured matters by misusing the term "unit stratotype," and by equivocally using the term "stage" for the very different concepts of "synthem" and "global chronostratigraphic stage." The P/E boundary option most repugnant to Aubry et al. (Carbon Isotope Excursion (CIE) = P/E = Thanetian/Ypresian boundary) is perfectly compatible with H.D. Hedberg's views. In contrast, another option preferred by Aubry et al. (recognition of new ~ 1 m.y. duration age/stage between Thanetian and Ypresian) is inconsistent with Hedberg's views. Additional problems with the P/E boundary arguments of Aubry et al. include the fact that a "Ypresian unit stratotype" does not exist, the fact that the base of the Ypresian synthem is not immutable, and the fact that the nannofossil *Tribrachiatus digitalis* is of dubious relevance to the boundary debate.

As for chronostratigraphy in general, Aubry et al. have misrepresented Hedberg's views by: (1) falsely claiming that the content of a stage is what determines its boundaries; (2) misunderstanding the general concept of the boundary stratotype; (3) distorting the "base defines boundary" principle; (4) falsely claiming that traditional (pre-GSSP) chronostratigraphic boundaries cannot be changed; (5) falsely implying that traditional stage unit stratotype boundaries can be adjusted by no more than 300,000 years when defining formal age/stage boundaries with GSSPs; (6) falsely claiming that the definition of a geochronologic/chronostratigraphic boundary should precede its correlation; (7) claiming that traditional unconformable "stage" boundaries may be suitable horizons for GSSPs; (8) distorting the meaning of "arbitrariness" in regard to the definition of geochronologic/chronostratigraphic boundaries; and (9) claiming that GSSPs are inherently unstable in that they are subject to redefinition whenever a more powerful element of correlation is discovered. If taken seriously, the unit stratotype-sanctifying chronostratigraphic philosophy of Aubry et al. would require the creation of perhaps dozens of new Phanerozoic ages/stages of relatively very short duration wherever there was a significant gap between two successive historical stage unit stratotypes. For all of these reasons, the arguments of Aubry et al. have no merit.

Keywords: Unit stratotype; Boundary stratotype; Synthem; Age/stage; Chronostratigraphy; GSSP

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1. Introduction

Aubry et al. (1999, 2000b,c), Aubry (2000a,b), and Aubry and Berggren (2000a,b) have described what they interpret to be significant differences in the chronostratigraphic philosophy of the International Subcommission on Stratigraphic Classification (ISSC) as exemplified by the International Stratigraphic Guide (Hedberg, 1976; Salvador, 1994), vs. the chronostratigraphic philosophy of the International Commission on Stratigraphy (ICS), as exemplified by the guidelines of Cowie et al. (1986) and Remane et al. (1996). Briefly, Aubry et al. have claimed that the ICS guidelines tend to ignore the importance of the unit stratotypes of traditional stages when defining formal geochronologic/chronostratigraphic boundaries, with the result that chronostratigraphy is becoming disconnected from its roots (e.g., Aubry et al., 2000b, p. 208). The current debate on the Paleocene/Eocene boundary has been a major focus of their arguments. Remane (2000a,b) has defended the position of the ICS in two different sets of comments and replies with Aubry et al., but these exchanges have clearly not persuaded his accusers.

Aubry et al. (1999, p. 136) dedicated their major review paper to the memory of Hollis D. Hedberg, and claimed to be upholding his chronostratigraphic philosophy. I share their admiration for Hedberg's accomplishments, and even though I have disagreed with his positions on several theoretical aspects of this subject (Walsh, 2001, in press), I find their own portrayals of Hedberg's views to be seriously flawed. The arguments of Aubry et al. are foundational in nature, and so are relevant to virtually every geochronologic/chronostratigraphic boundary of the Phanerozoic time scale. Because the formal definitions of these boundaries eventually affect the usage of every Earth scientist, the arguments of Aubry et al. must be examined in detail, and if found to be deficient, refuted in detail. This task will require numerous quotations from the papers of Aubry et al., as well as other relevant literature.

The first half of this paper examines the arguments of Aubry et al. in the context of the Paleocene/Eocene boundary debate. The second half of the paper addresses the more general question of whether the chronostratigraphic philosophy of Aubry et al. is consistent with Hedberg's, and if not, whether it can stand on its own merits. In this paper, I use the term "chronostratigraphy" in the relatively narrow sense of Whittaker et al. (1991), wherein this subject denotes the philosophy and procedures involved in formally defining the eras/erathems, periods/systems, epochs/series, and ages/stages of the geological time scale (Walsh, 2001). A similar view of chronostratigraphy was expressed by Aubry and Berggren (2000a, p. 107).

2. Unit stratotypes, synthems, and stages

2.1. Overview

A major source of confusion in many debates about chronostratigraphy stems from the equivocal use of the term "stage" for three very different concepts: (1) the concept of a unit stratotype; (2) the concept of a synthem; and (3) the concept of the set of all existing material strata in a given geographic area (usually the whole world) that were deposited during a given, golden spike-defined Age. A unit stratotype (or "type section") is just a very local stratigraphic section (or perhaps a composite section of two or more local, superposed subsections; Hedberg, 1976, p. 24). In general, a unit stratotype is simply a measured section of outcrop scale with a designated base and top, such as may occur in a particular quarry or on a particular mountainside (Salvador, 1994, Fig. 2A). The maximum geographic extent of a unit stratotype might be as long as several kilometers, perhaps in the case of a thick section of gently dipping strata continuously exposed along a sea cliff.

The concept of a unit stratotype is fundamentally different from the concept of a *synthem*, which is an unconformity-bounded unit of relatively major scope, generally occurring over a large basin, a province, or a subcontinent (Chang, 1975; Salvador, 1994). A synthem generally consists of several superposed and/or laterally interfingering lithostratigraphic units, the latter of course all having their own unit stratotypes. The type sections of each lithostratigraphic unit comprising the synthem will often occur in very different areas (tens or hundreds of kilometers apart), such that a complete section of the entire synthem will rarely if ever be present in any one local area (cf. Willems and Moorkens, 1991, p. 234, for the Ypresian synthem). Thus, a synthem is not in itself a unit stratotype, but is generally a more inclusive amalgamation of various lithostratigraphic units, all occurring above a given unconformity and below another given unconformity.

Finally, a synthem is not in itself a formal chronostratigraphic unit of the geologic time scale, such as a Standard Global *Stage*. This is the even more abstract set of *all of the existing strata in the world* that were formed during a given Age, such a span of time itself being defined by "golden spikes," or Global Stratotype Sections and Points (GSSPs). Unlike a synthem, which can reasonably be viewed as a "natural" stratigraphic entity (Salvador, 1994, p. 45), a stage is merely an abstract class of strata, and so does not exist independently of its conception in the mind of a human being (Walsh, 2001).

The concepts of "unit stratotype," "synthem," and "stage" are obviously distinct, but are all routinely called "stage" by various workers, and such sloppy use of this term can lead to the fallacy of equivocation (allowing a key word to shift its meaning in the course of an argument). Knowingly or unknowingly, this fallacy is invariably invoked by those who erroneously maintain that chronostratigraphic units define geochronologic units, rather than vice versa (Walsh, 1998, 2001, in press). The distinctions between these three concepts will now be discussed and illustrated in detail.

2.2. "Unit stratotype" vs. "stage"

The erroneous use of the term "stage" for the concept of "unit stratotype" is evident in the following passage from Savage and Russell (1983, p. 3):

> stage: A succession of strata or a stratigraphic interval with characterizing fossil aggregate delineated in 'a continuous fossiliferous section exposing also fossiliferous sections of both subjacent and superjacent Stages' (Kleinpell, 1938, p. 103).

Although Kleinpell (1938) did not exclusively use the term "stage" in this narrow sense, such usage was common among many workers before publication of the International Stratigraphic Guide (Hedberg, 1976). In Europe, of course, such usage inevitably led to a ridiculous multiplication of so-called "stages," whenever a new section containing some biostratigraphic and/or lithostratigraphic peculiarity was described (Arkell, 1933, pp. 11–12; Van Couvering, 1977, pp. 228). Walsh (1998, pp. 162–163; 174) warned against the spread of this European obsession in regard to the recent unnecessary unit stratotype-based definitions of certain new mammalian stages of South America. Unfortunately, the confusion between "unit stratotype" and "stage" has also been perpetuated by the North American Stratigraphic Code (North American Commission on Stratigraphic Nomenclature, 1983, p. 849), which states:

> For such purposes, units of geologic time traditionally have been named to represent the span of time during which a well-described sequence of rock, or a chronostratigraphic unit, was deposited . . . This procedure continues, to the exclusion of other possible approaches, to be standard practice in studies of Phanerozoic rocks.

Although this passage utterly confuses the concepts of "unit stratotype" and "chronostratigraphic unit" (Walsh, 2001, in press), the Code later contradicts itself when it states (Article 78):

> An ideal stratotype for a chronostratigraphic unit is a completely exposed unbroken and continuous sequence of fossiliferous stratified rocks extending from a well-defined lower boundary to the base of the next higher unit [i.e., a unit stratotype]. Unfortunately, few available sequences are sufficiently complete to define stages and units of higher rank, which therefore are best defined by boundary-stratotypes.

But if "few available sequences are sufficiently complete to define stages and units of higher rank," then how can this very procedure of designating unit stratotypes possibly continue to be "standard practice in studies of Phanerozoic rocks"? Such contradictions lend credence to Harland's (1992, p. 1234) claim that "the illogicalities in some guides and codes have repelled the more rigorously minded students from stratigraphy."

To make this crucial point very clear, it is completely unnecessary and in fact counterproductive to designate formal unit stratotype sections for either global or regional geochronologic/chronostratigraphic units. The reasons are very simple and have been thoroughly understood by most workers in this field for at least 35 years (George et al., 1967; Hedberg, 1968, p. 194). Ager (1973, p. 70) put the matter as follows:

> Many countries, especially on the European continent, still favour the [unit] ''stratotype'' concept, whereby a stratigraphical division (normally a stage) is defined by reference to a type section or stratotype, at or near the locality mentioned in the stage name. This objective, though still sought as the panacea for all stratigraphical ills, has caused many of the problems that afflict us today. Thus the Bacchanalian and Machiavellian Stages, though theoretically adjacent in time, will inevitably be defined at their two different type localities. It is extremely unlikely that the top of the Bacchanalian at its type locality will exactly correspond with the base of the Machiavellian in its home ground. There may be an overlap, with resultant arguments between the protagonists of the two stages ... Alternatively, strata will later be discovered that appear to fall into the time gap between the two stages. The resultant pseudoscientific arguments will then concern themselves with the meaningless question as to whether the fauna of the intervening strata pertain more to the stage below or to the stage above.

Thus, the designation of unit stratotype sections as ''stages'' inevitably creates unresolvable gaps and overlaps between consecutive stages, which in turn leads to unproductive arguments about boundaries, or to the definition of useless new ages/stages of very short duration to fill in the gaps. For these obvious reasons, Hedberg (1976, pp. 84–85), Salvador (1994, pp. 88–89), and Remane et al. (1996, p. 78) recommend the use of *boundary* stratotypes to define ages/stages. In short, the use of unit stratotypes in chronostratigraphy has long been intellectually obsolete, but its persistence (e.g., Easton et al., in press) testifies to the exasperating ability of a traditional dogma to overwhelm common sense.

2.3. ''Unit stratotype'' vs. ''synthem''

The differences between ''unit stratotype'' and ''synthem'' were illustrated by Hardenbol and Berggren (1978, Fig. 3), and are further explained in Fig. 1. Three superposed synthems (Bian, Eian, and Gian) are depicted in the form of a projection onto a vertical east–west plane. Each synthem is an unconformity-bounded package of sediments composed of several lithostratigraphic units. For example, the Bian synthem is named after the B Sandstone, but the lowest part of this synthem is composed of the nonmarine, unfossiliferous A Conglomerate. Note, however, that there is no unit stratotype of the Bian synthem. There are unit stratotypes for the A Conglomerate, the B Sandstone, and the C Shale, but no continuous single section exists which delimits the entire stratigraphic and temporal span of the Bian synthem. Thus, the maximum span of time subtended by the Bian synthem is necessarily greater than the sum of the spans of time subtended by the unit stratotypes of its component formations. For the Gian synthem, a single continuous section is available from the base to the top of the synthem in a particular local area. Again, however, the base of this unit stratotype section is necessarily somewhat younger than the oldest part of the Gian synthem, and the top of this unit stratotype section is necessarily somewhat older than the youngest part of the Gian synthem.

2.4. ''Synthem'' vs. ''stage''

The differences between ''synthem'' and ''stage'' were illustrated by Walsh (2001, Fig. 1), and are further explained in Fig. 2. Let us suppose that the Bian, Eian, and Gian synthems are traditional European unconformity-bounded units whose names have been chosen to be used for the formal, golden spike-defined Standard Global Ages/Stages in this part of the geological time scale. Because the bases of each of these synthems are of nonmarine and/or brackish-water facies (and are of uncertain age, and not readily correlatable), two golden spikes (black dots) have been selected to formally define the Bian/Eian and Eian/Gian boundaries. One golden spike is placed in a conformable, fully marine boundary stratotype section in North America, and another golden spike is placed in a similar section in Asia (for concrete examples, see the discussion of certain Devonian Ages/Stages by Remane, in press). Thus, for example, the Eian Stage consists of all existing strata on Earth that were formed during the Eian Age, the latter span of time being defined by the two golden spikes. It will be seen that the Eian *Stage* is a completely different set of rocks than the Eian

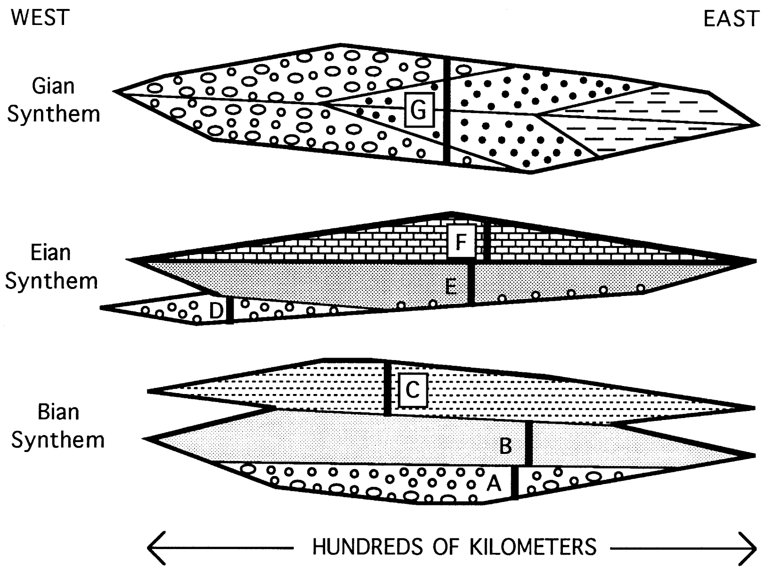

Fig. 1. Diagram showing the fundamental differences between the concepts of "unit stratotype" and "synthem". Three hypothetical superposed synthems are depicted, each composed of several lithostratigraphic units. Each lithostratigraphic unit has its own unit stratotype. There is no unit stratotype section for either the Bian or Eian synthem. There is a unit stratotype for the Gian synthem, but the span of time subtended by this section is necessarily less than the span of time subtended by the entire synthem. See text for discussion.

synthem of Europe. Nevertheless, many workers still insist on calling the Eian *synthem* the Eian *Stage*, thus committing the fallacy of equivocation and leading to endless semantic confusion in discussions of the "Eian problem." Actual examples of such equivocation will be documented below.

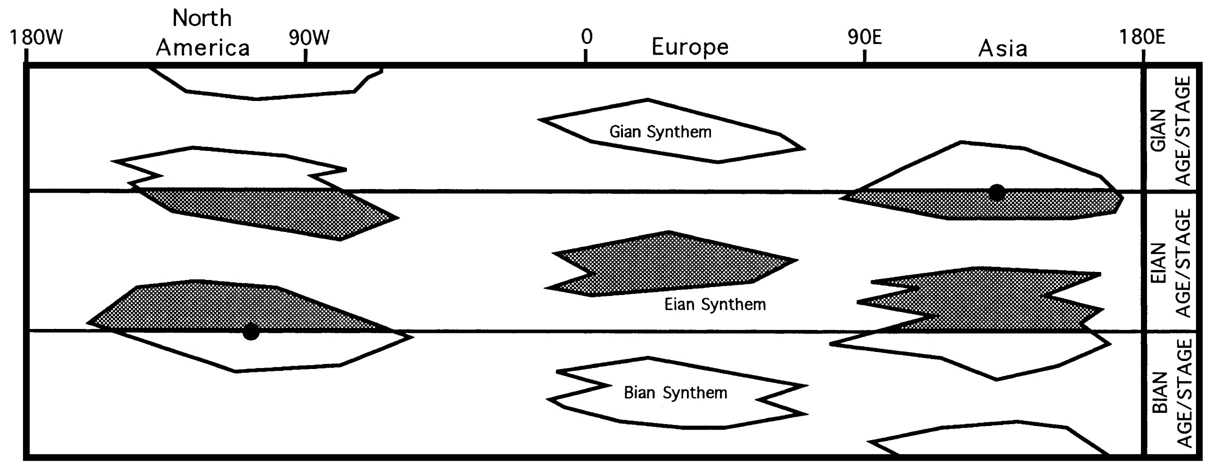

Fig. 2. Diagram showing the fundamental differences between the concepts of "synthem" and "stage". All of the existing strata in the world of an arbitrarily selected age range are depicted as being projected onto a vertical east–west plane (note longitude ticks). The Bian, Eian, and Gian synthems of Europe (previously depicted in Fig. 1) have been chosen to provide the names of the Standard Global Ages/Stages that will be defined for this part of the geological time scale. One golden spike in North America and another golden spike in Asia are placed in conformable, highly correlatable marine sections. These two golden spikes formally define the duration of the Eian Age, which in turn conceptually defines the set of all existing strata on Earth that were formed during it (the Eian Stage). The Eian *synthem* is a completely different set of rocks than the Eian *Stage*. See text for discussion.

3. The Paleocene/Eocene boundary debate

The recent debate over the definition of the Paleocene/Eocene (P/E) boundary and its relationship to the Thanetian and Ypresian "stages" of northwestern Europe has been a major focus of several papers of Aubry et al. Readers requiring detailed information on the subject should consult Aubry et al. (1998a,b, 1999), Aubry (2000a,b), Aubry and Berggren (2000a,b), Remane (2000a,b), International Subcommission on Paleogene Stratigraphy (2000), and Thiry and Aubry (2001). Fig. 3 shows the names of the Paleogene Standard Global Ages/Stages as approved by the International Subcommission on Paleogene Stratigraphy (Jenkins and Luterbacher, 1992), while Fig. 4 illustrates some of the major stratigraphic entities of northwestern Europe that are involved in the P/E boundary debate.

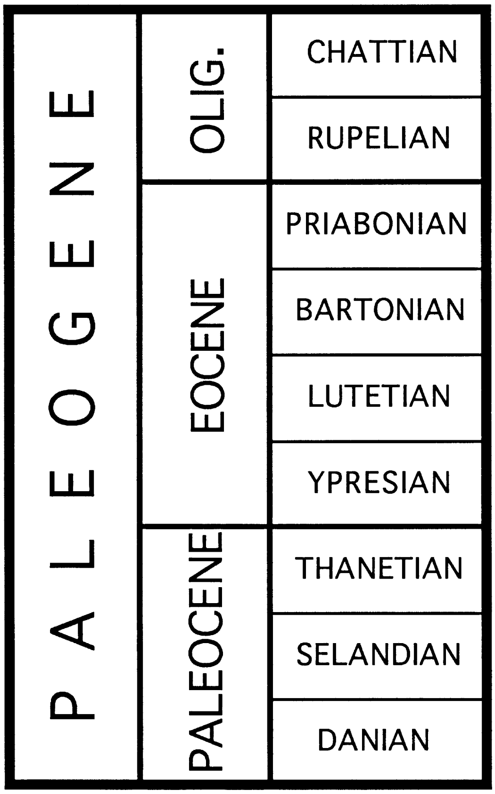

Fig. 3. Names of the Paleogene Standard Global Ages/Stages as approved by the International Subcommission on Paleogene Stratigraphy (Jenkins and Luterbacher, 1992).

As they are generally used in Europe, the Thanetian and Ypresian "stages" are really synthems, being unconformity-bounded units composed of several superposed and mutually intertonguing lithostratigraphic units (e.g., King, 1991, p. 363; Jenkins and Luterbacher, 1992; Aubry et al., 1999, pp. 125–126). The meaning of the term "Thanetian Stage" is variable, being used in both a restricted and an expanded sense (Aubry et al., 1999, p. 121; Aubry, 2000a, pp. 463–464). In the restricted, regional sense, the term "Thanetian Stage" means that particular synthem of northwestern Europe whose upper and lower bounding unconformities correspond to the unconformable boundaries of the Thanet Formation in the London Basin (Fig. 4). This is how the term "Thanetian Stage" has actually been used by most British stratigraphers (Curry, 1981; Siesser et al., 1987; Aubry et al., 1999, p. 119). In the expanded, chronostratigraphic sense, the term "Thanetian Stage" has come to mean all strata in Europe and/or the world that were deposited between the time of deposition of the base of the Thanet Formation, and the time of deposition of the base of the Ypresian synthem (e.g., Berggren et al., 1985, 1995; Siesser et al., 1987, p. 91). The "Ypresian Stage" is based on the Ieper Formation of Belgium, whose lowest recognized lithostratigraphic unit is named the Mont Héribu Member (De Coninck et al., 1983). The term "Ypresian Stage" also has restricted regional and expanded chronostratigraphic meanings analogous to those noted above for the Thanetian.

The P/E boundary has often been equated with the unconformable base of the Ypresian synthem (generally, the base of the Mont Héribu Member of the Ieper Formation and/or the base of the Walton Member of the London Clay), but alternative definitions have also been commonly used. For example, a relatively "old" biochronological placement of the P/E boundary has long been employed by most vertebrate paleontologists at a horizon estimated to be about 1.0 m.y. older than the bases of the Mont Héribu Member/Walton Member (Berggren, 1971, p. 712; Pomerol, 1989; Lucas, 1998; Aubry et al., 1999, p. 126; Aubry, 2000a, Fig. 1). Many marine micropaleontologists have used the NP9/NP10 (calcareous nannofossil) biochron boundary as their working P/E boundary (Berggren et al., 1995, p. 151; Aubry et al., 1998b, p. xii). This biochron boundary is currently estimated to be about 0.6 m.y. older than the base of the Mont

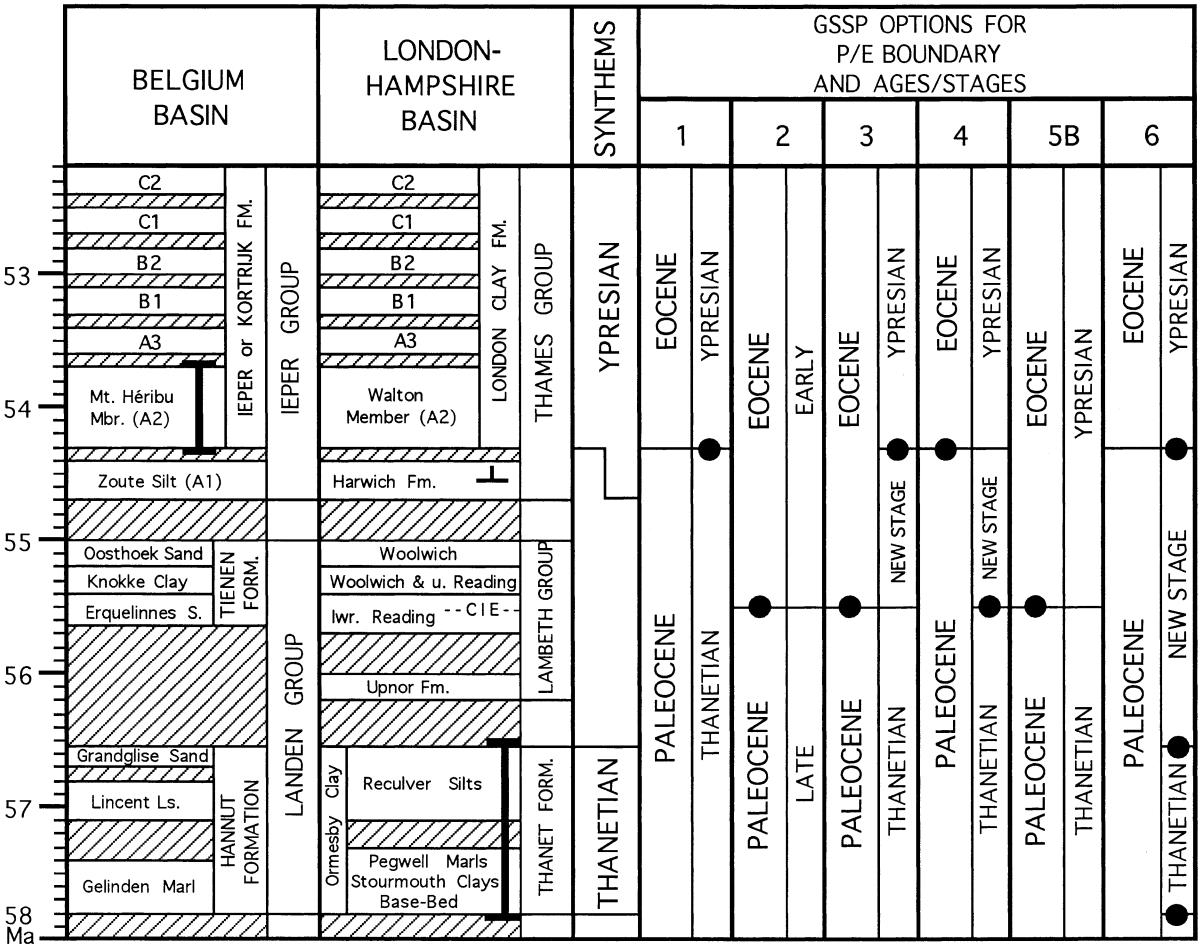

Fig. 4. Temporal interpretation of selected stratigraphic units in northwestern Europe relevant to the Paleocene/Eocene boundary debate. Stratigraphic extent of unit stratotypes of the Thanetian Stage and the Mont Héribu Member of the Ieper Formation indicated by thick vertical bars. Carbon Isotope Excursion (CIE) shown within lower Reading Formation (also present in coeval Argile Plastique in Paris Basin). Approximate level of presumed lowest occurrence of *T. digitalis* in Harwich Formation shown by inverted "T". Different levels for the base of the Ypresian synthem reflect the different opinions of Aubry (2000a) vs. Powell et al. (1996) and Steurbaut (1998). Options 1–4 for placement of the P/E boundary and age/stage boundaries (golden spikes = black dots) are the same as those of Aubry (2000a); additional Options 5B and 6 are discussed in the text. Note that the end of the Ypresian is estimated at 49 Ma (Berggren et al., 1995), so the entire duration of this Age as currently conceived cannot be depicted in this illustration. Simplified and modified from Aubry (2000a, Fig. 2), Steurbaut (1998, Figs. 3 and 12), Siesser et al. (1987, Fig. 3B), Ellison et al. (1994, Fig. 1), and Knox et al. (1994, Figs. 2 and 4). Generalized stratigraphic placement of the Ormesby Clay follows Ellison et al. (1994, Fig. 1), in contrast to the contradictory positions assigned to this unit by Ali and Jolley (1996, Fig. 12) and Neal (1996, Fig. 5) vs. Aubry (2000a, Fig. 2).

Héribu Member/Walton Member (Aubry, 2000a). Yet another alternative placement of the P/E boundary is favored by several marine stratigraphers using sequence-stratigraphic arguments, such as Powell (1992), Powell et al. (1996), and Neal (1996, p. 28). These workers advocate placing the base of the Ypresian synthem (and thus, according to their reasoning, the base of the Eocene) at the base of the Harwich Formation, a horizon estimated to be about 0.3 m.y. older than the bases of the Mont Héribu Member/Walton Member (Aubry, 2000a).

In the past decade, a distinctive negative Carbon Isotope Excursion (CIE) has been documented to occur within the P/E boundary interval (Kennett and

Stott, 1991). It has been identified in the lower Reading Formation in the London–Hampshire Basin, in the coeval Argile Plastique in the Paris Basin (Thiry et al., 1998; Aubry, 2000a), and has also been recognized in terrestrial sections in North America and Europe (Koch et al., 1992, 1995; Stott et al., 1996). Interestingly, the CIE also corresponds to the major change in the mammalian faunas of North America and Europe traditionally equated with the P/E boundary by vertebrate paleontologists (e.g., Gunnell, 1998; Hooker, 1998; Lucas, 1998; Beard and Dawson, 2000), as well as a major extinction of benthonic foraminifera documented in the deep sea record (Thomas, 1992, 1998), and a dramatic global increase in abundance of the dinoflagellate *Apectodinium* (Bujak and Brinkhuis, 1998; Crouch et al., 2000). In view of its global correlatability in marine and terrestrial sections and correspondence with significant marine and terrestrial faunal changes, the CIE has been proposed as a compelling "defining criterion" (see Murphy, 1994; I prefer the less loaded term "guiding criterion") for the P/E boundary. However, the CIE is estimated to be about 1.0 m.y. older than the base of the Mont Héribu Member of the Ieper Formation (Aubry, 2000a). Thus, the major questions in the P/E boundary debate have been:

1. In defining the P/E boundary, should our overriding concern be respect for tradition, which in this case means respect for the views of many (but by no means all) European stratigraphers, who commonly equate this boundary with the base of the Mont Héribu Member of the Ieper Formation and/or Walton Member of the London Clay? If so, but also recognizing that known unconformities are unsuitable horizons for placing GSSPs (Salvador, 1994, p. 90), are we restricted to finding some means of correlation that can approximate the age of the bases of the Mont Héribu Member/Walton Member, and then use this as the guiding criterion with which to drive a golden spike?
2. If we are not so restricted, is it permissible to define the P/E boundary to correspond to the CIE for the sake of better global correlation in marine and nonmarine sequences?
3. If so, should the beginning/base of the Ypresian Standard Global Age/Stage also be extended down to the CIE, or should the base of the Ypresian remain at the bases of the Mont Héribu Member/ Walton Member, and a new, earliest Eocene Age/ Stage about 1 m.y. in duration be inserted between the Thanetian and Ypresian?

Many additional questions concerning the P/E boundary debate are associated with these fundamental problems of chronostratigraphic philosophy, which I will now try to clarify. My purpose is not to advocate a particular solution of the P/E boundary problem, for (with one exception) I have no preference on the matter. Indeed, although some arguments about the P/E boundary continue to smolder (Aubry et al., in press), the main issues in the debate have apparently now been resolved. In the Spring of 2002, a proposal was made by the Working Group on the P/E boundary for the selection of the Dababiya section (Egypt) as the Standard Global Stratotype Section for the P/E boundary (Aubry, 2001) using the CIE as the guiding criterion for the boundary itself. In the Summer of 2002, a vote taken by the International Subcommission of Paleogene Stratigraphy approved the proposed GSSP, which must now also be approved by the International Commission on Stratigraphy and then ratified by the International Union of Geological Sciences (H.-P. Luterbacher, personal communication). Despite these recent developments, the numerous controversial issues that constitute the P/E boundary debate are still highly relevant to this and other boundary debates, and my main purpose is to determine whether or not the arguments made by Aubry et al. have any merit for chronostratigraphy in general.

4. The Ypresian problem

4.1. What, if anything, is the Ypresian unit stratotype?

Aubry et al. have frequently appealed to the historical priority of the "unit stratotype of the Ypresian Stage" (e.g., Aubry, 2000b, p. 684; Aubry et al., 2000b, p. 205). Aubry (2000b, p. 685) stated:

> This implies that the lithostratigraphic horizon that constitutes the base of the Ypresian standard Stage constitutes also (*by principle*) the base of the Eocene Series. Further, following strictly the

principles and the convention that the "base defines stage", the base of the standard stage corresponds to the base of the unit-stratotype. Therefore, the base of the Standard Ypresian Stage corresponds to the base of the Mont Héribu Member [italics in original].

However, in the sense of an originally or subsequently designated unit stratotype section whose base corresponds to what is now the base of the Mont Héribu Member, there *is* no "unit stratotype of the Ypresian Stage." Indeed, the only *formally* designated "Ypresian stratotype" that I am aware of would define an extremely limited stratigraphic scope for the Ypresian that Aubry would certainly reject. According to Willems and Moorkens (1991, p. 232), Dumont (1849) designated no type locality for his "système Yprésien" They further stated:

> The claystone quarry of the "Verenigde Steenbakkerijen van Ieperen" at Sint-Jan, near Ieper was designated by Moorkens (1968, p. 114) as type locality and as stratotype of the Ypresian stage, because it appeared to be one of the only outcrops of the Flanders Clay in the "hills around Ieper", i.e., the type area indicated by Dumont.

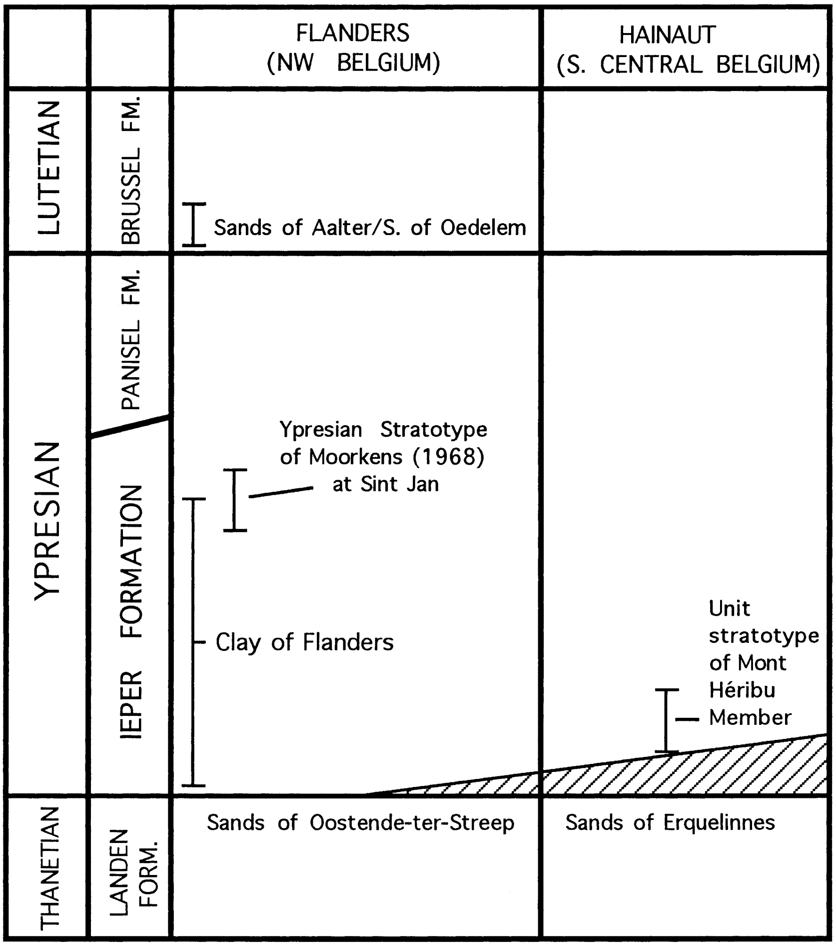

Fig. 5. Diagram showing extremely limited stratigraphic extent of the "Ypresian stratotype" of Moorkens (1968), as well as the interpretation of Willems and Moorkens (1991) that the base of the unit stratotype of the Mont Héribu Member is younger than the base of the Ieper Formation in northwestern Belgium. Note hiatus between Ieper Formation and Landen Formation, increasing in duration to the southeast. Simplified from Willems and Moorkens (1991, plate 1).

According to Willems and Moorkens (1991, p. 232 and plate 1), this stratotype of the Ypresian Stage as designated by Moorkens (1968) corresponds to only the middle part of the Ypresian sensu Dumont (1849), and so would exclude what is now called the Mont Héribu Member (Fig. 5). Indeed, the precise equation of the base of the Ypresian synthem with the base of the Mont Héribu Member is a relatively recent development, as noted by Aubry and Berggen (2000a,b, p. 107), and is by no means universally accepted anyway (see discussion below). Even De Coninck et al. (1983) did not *formally* equate the base of the Ypresian synthem with the base of the Mont Héribu Member; they only defined the Mont Héribu Member to be the base of the Ieper Formation. Therefore, Aubry's (2000a, p. 465) claim that "the P/E boundary is defined by the base of the Ypresian Stage as stratotypified in Belgium, with its base correponding to that of the Mont Héribu Member" is misleading, because there is no formal stratotype for either the Ypresian global chronostratigraphic Stage (in Aubry's sense) or the Ypresian synthem (see also Remane, 2000a, p. 681).

To further clarify this subject, it should be noted again that by definition, a *unit* stratotype is a stratigraphic section that requires both a base and a top to be present in a demonstrably superposed section (Salvador, 1994, pp. 26–27). However, the stratotypes of a synthem are not unit stratotypes; they are boundary stratotypes, where the bounding unconformities of the synthem are well developed (Salvador, 1994, p. 49). Both bounding unconformities of a synthem need not be present in a single superposed unit stratotype section. These distinctions are important to make in the context of the Ypresian synthem. As stated by Willems and Moorkens (1991, p. 234):

> As the Ypresian strata in Belgium are locally more than 200 meters thick and only gently dipping to the North, no outcrop exists, which gives a full continuous stratigraphic section ... Practically all parts of the Ypresian succession can ... be seen in outcrops, but a relatively large number of outcrops must be visited in order to see the whole succession.

Thus, it would seem almost impossible to designate a true unit stratotype for the full stratigraphic extent of the Ypresian synthem, and when Aubry et al. refer to the "Ypresian unit stratotype," they evidently mean the Ieper Formation as a whole, rather than the Ieper Formation in a particular specified unit stratotype section. This usage of the term "stratotype" for a widespread, name-providing body of rock is exemplified by the title of the volume edited by Dupuis et al. (1991), namely, "The Ypresian Stratotype." This phrase obviously does not refer to a specified unit stratotype section of the Ypresian Stage, but rather refers generally to all "Ypresian" strata in the type area of Belgium (i.e., the Ieper Formation and its correlatives). Modifying a proposal of Newell (1972, Fig. 1), I suggest that we use the term "nominal stratotype"[1] when referring only to a name-bearing or name-providing body of rock. As such, the Ieper Formation may appropriately be called the *nominal* stratotype of the Ypresian Stage, but it is simply incorrect to say that the Ieper Formation is the *unit* stratotype of the Ypresian Stage, because the ages of the base and top of the Ieper Formation do not correspond to the beginning and ending of the Ypresian Age/Stage (e.g., Willems and Moorkens, 1991, plate 1).[2]

The above distinctions are important to make, because as shown in Fig. 5 (after Willems and Moorkens, 1991, plate 1), the base of the unit stratotype section of the Mont Héribu Member in the Hainaut area of Belgium is significantly younger than the base

[1] I prefer the phrase "nominal stratotype" over Newell's (1972) "nominate stratotype," because "nominate" is generally construed as a verb. Thus, the latter phrase could easily be misinterpreted to denote a unit or boundary stratotype that has been formally nominated to define one or both boundaries of a standard global geochronologic/chronostratigraphic unit. As illustrated by Newell (1972, Fig. 1), it is of course possible for a specific section to serve as both the unit and nominal stratotype of a given entity (especially in lithostratigraphy). However, such correspondence will seldom occur in the context of standard global chronostratigraphic units, because the base and top of the particular section originally designated as the name-bearing "type section" of a given chronostratigraphic unit will rarely if ever be selected as the formal boundary stratotypes for that unit.

[2] For a similar example from vertebrate biochronology, the Bridger Formation can appropriately be called the nominal stratotype of the Bridgerian North American Land Mammal "Age," but it would be quite incorrect to say that the Bridger Formation is the unit stratotype of the Bridgerian, because the biochronologically defined boundaries of the Bridgerian do not correspond to the ages of the base and top of the Bridger Formation (Robinson et al., in press).

of the Ieper Formation in the Flanders area (as expected, given that the direction of the Ypresian transgression was from the west and north to the east and south; Berggren and Aubry, 1998, p. 28). For the same reason, the base of the unit stratotype of the Mont Héribu Member is probably also somewhat younger than the oldest part of the London Clay. Would Aubry (2000b, p. 685) therefore really insist that the base of the Ypresian Standard Global Age/Stage should correspond to the base of the unit stratotype of the Mont Héribu Member? Obviously not, because then at least some of the strata traditionally and currently assigned to the Ypresian synthem by Aubry et al. would, by definition, not be assignable to the Ypresian Stage.

4.2. The boundaries of the Ypresian synthem are not immutable

Aubry et al. (1999, p. 126) argued that it would be unwise to extend the beginning of the Ypresian about 1 m.y. to correspond to the CIE, because the base of the Ypresian synthem "constitutes the base of a well-defined broad synthem in NW Europe." But, just how well defined is it? As illustrated by Aubry (2000a, Fig. 2), the base of the Mont Héribu Member of the Ieper Formation (Belgian Basin) and the base of the Walton Member of the London Clay (London–Hampshire Basin) lie disconformably upon the underlying Zoute Silt and Harwich Formation, respectively. However, this disconformity is portrayed in Aubry's figure as representing a hiatus of only 0.1 m.y., and conceivably could represent even less time than that (see also Aubry, 1995b, p. 251; Ali and Jolley, 1996, Fig. 12). In contrast, the disconformity immediately below the Zoute Silt and Harwich Formation is portrayed as representing 0.3 m.y. (0.4 m.y. according to Ali and Jolley, 1996, p. 129). So, is the disconformity at the base of the Mont Héribu Member and the Walton Member really all that significant? Steurbaut (1998, pp. 146–147) did not think so and assigned the Zoute Silt to the Ypresian synthem on the basis of his belief that this deposit represents the very beginning of the Ypresian transgression (see also Steurbaut et al., 1999). This proposal deserves more attention than given it by Aubry (2000a, p. 469), who accused Steurbaut of ignoring the alleged fact that "a [traditional, pre-GSSP] chronostratigraphic boundary cannot be relocated in order to reflect any aspect of Earth history, not even sea level history." However, in the absence of a formal GSSP, there simply is no valid chronostratigraphic boundary at the base of the Mont Héribu Member (Remane, 2000a; *contra* Aubry, 2000a, p. 466; Aubry, 2000b, p. 684).

The main point here is that, as with a lithostratigraphic boundary, the revision of a *synthem* boundary can be proposed by anyone who has the requisite evidence to do so (Salvador, 1994, p. 51). And, inasmuch as the Ypresian synthem has to my knowledge not been formally defined according to the requirements of Salvador (1994, pp. 49–50), revision of the boundaries of this informal synthem would be that much easier. Indeed, Aubry et al. (1988, p. 734) had previously placed the P/E boundary (and Thanetian/Ypresian boundary) at the base of the "Oldhaven Beds" (later assigned to the Harwich Formation by Ellison et al., 1994), rather than at the base of the Walton Member of the London Clay. Similarly, Powell et al. (1996, p. 179) equated the unconformable base of the Harwich Formation with the base of the Ypresian synthem.[3] Such a definition of the base of the Ypresian synthem would be more consistent with Salvador's (1994, p. 48) recommendation that synthem boundaries should correspond to erosion surfaces "representing a significant hiatus or gap in the section" (it is of course debatable whether a hiatus of only 0.1 m.y. is "significant," but plainly it is not as significant as a hiatus of 0.3–0.4 m.y.). Furthermore, assignment of the Harwich Formation and its correlatives to the Ypresian synthem would be even more reasonable given the existence of an angular unconformity between them and the underlying Woolwich and Reading sequence (Knox, 1990, p. 60; Knox, 1998, p. 94).

I would agree with Aubry's (2000a) criticisms of Steurbaut (1998) only from the standpoint that if there was *already* a formal GSSP definition of the P/E boundary (and Standard Global Thanetian/Ypresian Age/Stage boundary) to correspond to the base of the Mont Héribu Member, then a possibly legitimate downward extension of the base of the Ypresian

[3] Aubry (2000b, p. 684R, second paragraph) equated the "lowermost Ypresian" with the "base of the Harwich Formation," but the latter name appears to have been a mistaken substitution for "Ieper Formation," given the preceding sentences on the same page and Fig. 2 in Aubry (2000a).

synthem would certainly not change the location of the P/E boundary (nor Thanetian/Ypresian Age/Stage boundary). But again, no formal definition of the base of the Ypresian Standard Global Stage currently exists.

It is worth noting that the stratigraphic classification of these complex deposits of the London–Paris–Belgian Basin has changed many times, and will undoubtedly change again in the future. As noted by Berggren and Aubry (1998, pp. 26; 28), some of the strata now comprising the Harwich Formation were previously assigned to the London Clay (e.g., King, 1981; Knox, 1990). Interestingly, however, Hooker (1998, Fig. 20.1) does not recognize the Harwich Formation as defined by Ellison et al. (1994). He continues to assign the Harwich, Oldhaven, and Swanscombe members to the London Clay Formation, shows no hiatus at all between these units and the Walton Member, and so places the base of the Ypresian synthem at the base of the Harwich Member. De Coninck (1999, p. 80; Fig. 3) proposed yet another interpretation, wherein the lower part of the Harwich Formation was hypothesized to be coeval with the upper part of the Woolwich Formation. Even Aubry et al. (1999, Fig. 4) showed the base of the Ypresian extending to the bases of the Zoute Silt and Harwich Formation, although Aubry et al. (2000b, Fig. 1) soon returned the base of the Ypresian to the base of the Mont Héribu Member. There is of course nothing wrong with these revisions; local stratigraphic classification must be free to evolve with new data. My point is that Aubry et al.'s argument for the supposed immutability of traditional "stage" boundaries simply does not apply to synthems, which is what the Thanetian and Ypresian "stages" have historically been viewed as.

4.3. Further reflections on "synthem" and "stage"

Aubry et al. (2000b, p. 205) claimed that a "complete disrespect for earlier scientific studies" would result if the beginning of the Ypresian were extended by about 1 m.y. to the level of the CIE, but this fear only stems from their unwillingness to consistently separate the concepts of synthem and (global chronostratigraphic) stage. Aubry and Berggren (2000a, p. 111) stated:

> Stages are defined not only by their boundaries but also by their stratotypes or by their regional content (see Knox, 1994). Series have no unit stratotypes, and stages—the only concrete unit of chronostratigraphy—should not be modified to meet the need of correlation at the rank of the series.

By claiming that stages are the only concrete unit of chronostratigraphy, Aubry and Berggren are again committing the fallacy of equivocation by confusing stages with synthems. While a *synthem* may be viewed as a concrete, "natural" stratigraphic entity (Salvador, 1994, p. 45), a *stage* is an abstract class of strata and does not differ in kind from a series or system; it differs only in relative *magnitude* (Walsh, 2001, Fig. 1). *Contra* Aubry and Berggren (2000a), and as clearly stated by Hedberg (1977, p. 232): "The concepts for the Stage are the same as for any other chronostratigraphic unit." Aubry and Berggren (2000a) have no excuse for such confusing usage, because the differences between "synthem" and "stage" were clearly understood by Berggren (1971, p. 696) long ago. If stratigraphers would simply use the term "Ypresian *synthem*" when they mean "the European unconformity-bounded unit of traditional scope" and "Ypresian *Stage*" when they mean "all existing strata in the world that were formed during the Ypresian Age" (however, this Ypresian Age will eventually be defined by golden spikes), no confusion would occur, because the unit-term precisely specifies the intended meaning.

Nevertheless, one way to reduce the possibility of semantic confusion would be to use different geographic names for the European synthem and the Standard Global Stage, e.g., "Ieperian synthem" and "Ypresian Stage" (cf. Willems and Moorkens, 1991, p. 232). Alternatively, Aubry et al. have used the unhelpful terms "regional stage," (meaning synthem), "standard stage," and "GSSP stage" (Aubry et al., 1999, pp. 121–122; Aubry, 2000b, p. 685, quoted above). First, the term "regional stage" is unhelpful because it continues to erroneously use the term "stage" for the concept of an unconformity-bounded synthem, rather than for a chronostratigraphic unit. Second, the term "standard stage" is unhelpful for reasons discussed by Remane (2000a,b) in that it begs the question of exactly what is "standard" in the absence of a formally ratified GSSP for a Standard Global Stage. Aubry et al.'s use of the term

"Standard Ypresian Stage" for a commonly used but informal concept of the Ypresian Stage therefore unnecessarily confuses the debate. Ironically, Aubry (2000b, p. 684) accused Remane (2000a) of indulging in a "play on words" aimed at "discrediting any previous concepts of Thanetian and Ypresian Stages and P/E boundary," and suggested that this "serves little purpose other than confuse the reader on the real issues at stake in the issue of the definition of the P/E boundary." On the contrary, it is the deliberately equivocal use of the term "stage" by Aubry et al. that has served to obscure the debate on the P/E boundary.

I would suggest that the main, unmentioned source of confusion in the whole issue stems precisely from the stubborn refusal of some workers to accept the adoption of "stage" as a unit term of the global chronostratigraphic hierarchy (Hedberg, 1976; Salvador, 1994). Although it had long been used in this sense (e.g., Schenck and Muller, 1941), many stratigraphers were opposed to such usage, believing that this term should be restricted to regional lithostratigraphic/allostratigraphic concepts (e.g., Van Couvering, 1977; see Hedberg, 1977, p. 232), or to regional biostratigraphic concepts (e.g., Ludvigsen and Westrop, 1985a,b; see also Aubry et al., 1999: Appendix A). This unfortunate multiple usage of the term "stage" is also why I am sympathetic to the suggestion of Aubry et al. (1999, p. 132) that subepochs/subseries (e.g., "Early/Lower Eocene Subepoch/Subseries") be formalized as the standard lower-level global geochronologic/chronostratigraphic units for at least part of the Cenozoic time scale. If we would all just use "Early/Lower Eocene" (as most stratigraphers around the world now do anyway), then the European traditionalists could have back the name "Ypresian" to use as they see fit (use of subepochs and subseries is explicitly permitted by Salvador, 1994, Table 3). This alternative is discussed in more detail below.

4.4. How relevant is the FAD of *Tribrachiatus digitalis*?

As noted, Aubry et al. believe in the importance of priority in chronostratigraphy, and so have urged that the unconformable bases of the Mont Héribu Member of the Ieper Formation and/or Walton Member of the London Clay be regarded as the P/E and Thanetian/Ypresian boundary. Although these horizons were not previously correlatable with deep sea marine biochronologies, by means of paleomagnetic, lithostratigraphic, and sequence stratigraphic correlation, Aubry (1995b, 2000a, p. 470; 2000b, pp. 684–685) proposed that the First Appearance Datum, or FAD (read: time of evolution) of the nannofossil *T. digitalis* can be used to approximate the age of the base of the Mont Héribu Member, and thus would be a suitable guiding criterion for the P/E boundary GSSP. Certain controversies concerning the taxonomy of *Tribrachiatus* and preservational problems with *T. digitalis* have been discussed by Aubry et al. (2000a), Monechi et al. (2000), von Salis et al. (2000), and Aubry (2000b, p. 685), but my purpose here is to evaluate whether the hypothetical selection of this taxon as the guiding criterion for the P/E boundary would be consistent with Aubry's own arguments.

T. digitalis is not known from the Mont Héribu Member or Walton Member, but it is apparently[4] known in the London Basin from the upper part of the Harwich Formation (Aubry, 2000b, p. 684; International Subcommission on Paleogene Stratigraphy, 2000, p. 42), which disconformably underlies the Walton Member (Fig. 4). However, given that the biochron of *T. digitalis* is estimated by Aubry (2000b, p. 684) to be 0.2 m.y. in duration, then contrary to her assertion that the FAD of *T. digitalis* approximates the age of the bases of the Walton Member and Mont Héribu Member, her own figure (Aubry, 2000a, Fig. 2), implies that the *Last* Appearance Datum, or LAD (read: "time of extinction") of this species would be a

[4] However, Aubry and Berggren (2000a, p. 110) inconsistently stated that "locating the Thanetian/Ypresian boundary at a level representing the FAD of *T. digitalis* would have the desirable result that the base of the Ypresian Stage would be located at a horizon which falls in the stratigraphic gap between the Harwich and Walton formations (Ellison et al., 1994; see also Knox, 1994), thus preventing any unwelcome violation of regional stratigraphic practices." But if just one in situ fossil of *T. digitalis* is known from the Harwich Formation (as stated by Aubry, 2000b, p. 684), then the FAD of this species *cannot possibly* have occurred during the hiatus between the Harwich Formation and the overlying Walton Member of the London Clay. These contradictory statements by Aubry et al. raise further doubts about the relevance of *T. digitalis* to the P/E boundary debate. Does it or does it not occur in the Harwich Formation?

better estimate of the age of these horizons (assuming that the lowest occurrence [LO] of this taxon in the Harwich Formation closely approximates its FAD). Furthermore, given Aubry's own preferred use of the FAD of *T. digitalis* as the guiding criterion for the Paleocene/Eocene (and Thanetian/Ypresian) GSSP, at least the upper part of the Harwich Formation (and by correlation, at least the upper part of the Zoute Silt of Belgium) would necessarily become Ypresian in age. It is therefore difficult to understand why Aubry (2000a, p. 469) was so opposed to Steurbaut's (1998) assignment of some of these same units to the Ypresian. Hasn't she understood the logical consequences of her own preferred definition of the Thanetian/Ypresian boundary using the FAD of *T. digitalis*?

More contradictions are evident when we remember Aubry's (2000a, p. 469) claim that "a [traditional, pre-GSSP] chronostratigraphic boundary cannot be relocated in order to reflect any aspect of Earth history." But if this claim is taken seriously, then Aubry herself is violating it simply by proposing the use of the FAD of *T. digitalis* as the guiding criterion of the P/E boundary. It would not matter whether the FAD of this species was only about 0.2 m.y. older than the base of the Walton Member/Mont Héribu Member, because "relocated" means exactly that—the beginning of the Ypresian Age using the FAD of *T. digitalis* would simply *not* be the same age as the base of the Walton Member/Mont Héribu Member. Wouldn't the use of the FAD of *T. digitalis* also violate Aubry's (2000a, pp. 461, 467) claims that "chronostratigraphy must remain ... independent of any aspect of Earth history, either paleobiologic, tectonic, or climatic," and that "A P/E boundary defined on paleobiologic criteria is simply irrelevant to chronostratigraphy?" Surely, the FAD of *T. digitalis* is a paleobiologic event!

5. GSSP options for the P/E boundary and associated ages/stages

Four options for defining the P/E boundary and its associated Standard Global Ages/Stages were presented by Aubry (2000a, Fig. 2), which are shown in Fig. 4. These will now be analyzed in detail, along with another option (Option 5B) that was discussed by Aubry et al. (1999), as well as a new option (Option 6) that to my knowledge has not been previously proposed. Although it now appears that Option 5B may soon be approved by the ICS and IUGS, a review of these options will be useful for illuminating general principles relevant to all stage and series boundary disputes.

5.1. Option 1

Option 1 of Aubry (2000a) first involves defining the Thanetian/Ypresian boundary to correspond to either the base of the Mont Héribu Member of the Ieper Formation, or to the base of the Walton Member of the London Clay, or to the FAD of *T. digitalis* (these suboptions of Option 1 have all been proposed at one time or another by Aubry et al.; see Aubry et al., 1999, p. 123; Aubry, 2000a, p. 470; Aubry et al., 2000b, p. 208). Secondly, the P/E boundary would then be equated with the Thanetian/Ypresian boundary (Fig. 4). Aubry (2000b, p. 685) claimed that Option 1 is required by Hedbergian principles:

> Application of Hedbergian principles would read: the P/E boundary is defined by the base of the Ypresian Stage [meaning synthem], based on the principle that the base of the stage defines the base of a series, and is correlatable on the basis of the FAD of *T. digitalis*.

I have already discussed certain problems concerning the use of *T. digitalis*, but now wish to demonstrate that Option 1 actually contradicts Aubry's own argument regarding the alleged importance of unit stratotypes. Aubry (2000a, p. 470) stated:

> [Defining the P/E boundary to correspond with the CIE] would have the significant inconvenience of lowering the base of the Ypresian Stage (following ICS rules) to a level significantly older (>1 m.y.) than the base of the Mont Héribu Member. Indeed, if such a GSSP is selected the redefined Ypresian Stage will have little to do with the standard Ypresian Stage, its unit stratotype, its long history of documentation and its correlation with marine deposits throughout Europe.

Let us now apply these remarks to the "Thanetian" as generally used by Aubry et al. Siesser et al. (1987, p. 90) stated:

> Curry (1981) noted that "Thanetian" (English spelling) has almost always been used (in Britain) in the restricted sense of Dollfus (1880). As no formal stratotype was proposed by Renevier or other early workers, Curry (1981) eventually designated the cliffs at Pegwell Bay and Herne Bay–Reculver to be co-stratotypes for the stage (this definition excluded the overlying Woolwich Bottom Bed and Oldhaven Beds).

If Aubry et al. are to be taken at their word concerning the alleged importance of the unit stratotypes of stages, then the top of a unit stratotype must be given the same weight as the base, because a unit stratotype does indeed require both a base and a top for its delimitation. However, in recent time scales, the top of Curry's (1981) formally designated Thanetian composite unit stratotype has been ignored by Berggren et al. (1985, 1995), who have extended the top of the Thanetian Stage all the way to the base of the Ypresian synthem (Siesser et al., 1987, p. 91). Therefore, this unit stratotype-ignoring, expanded usage of the "Thanetian" commits exactly a "gross violation of the historical concept of [the] Thanetian" that was so feared by Aubry (2000a, p. 469).

It must be emphasized that although I do not believe Option 1 to be the best available solution to the P/E boundary problem, I have no objection to it in its most general form. My points are threefold: first, as noted above regarding the Thanetian, Option 1 contradicts the avowed unit stratotype-sanctifying philosophy of Aubry et al.; second, contrary to the claims of Aubry (2000a,b), Option 1 is by no means required by Hedbergian principles; and third, that in its primitive form of defining the P/E boundary to simply *be* the unconformable base of the Mont Héribu Member/Walton Member, Option 1 is actually antithetical to Hedbergian principles (see Section 7.7).

5.2. Option 2

Option 2 of Aubry (2000a) involves defining the P/E boundary to correspond to the CIE, removing the European stage names "Thanetian" and "Ypresian" from the formal global chronostratigraphic framework, and recognizing the Late/Upper Paleocene and Early/Lower Eocene Subepochs/Subseries in their places (Fig. 4). As Aubry et al. (1999, p. 128) pointed out, most non-European stratigraphers do not speak of "Thanetian" or "Ypresian" strata in their correlations anyway; they use the terms "late Paleocene" and "early Eocene" instead. The latter terms form the basic common language for stratigraphers around the world working in this part of the time scale. Use of subseries/subepochs for the other subdivisions of the Paleocene and Eocene would have the added benefit that such terms are self-defining in the sense of relative age within a given epoch. That is, the term "Late Paleocene" immediately gives someone who is not familiar with European Paleogene stage nomenclature a good idea as to the age of the rocks or events being discussed. In contrast, the term "Thanetian" carries no such self-reference for the uninitiated. The arguments of Aubry et al. (1999, pp. 128–132) for Option 2 were quite compelling in my view, so it was disappointing that this option was immediately abandoned in all subsequent papers of Aubry et al. I urge that this simple nomenclatural alternative be considered in other unresolved Phanerozoic stage decisions.

A modification of Option 2 would simply involve a change of unit terms, such that we would refer to the Late/Upper Paleocene Age/Stage and Early/Lower Eocene Age/Stage, rather than using Subepoch and Subseries. This would have the benefit of maintaining the current hierarchy of geochronologic/chronostratigraphic unit terms, although it would again require that we use the term "synthem" when referring to the traditional Thanetian and Ypresian unconformity-bounded units of Europe. This arrangement also has a conceptual precedent in that the epochs of the Devonian Period are simply called "Early Devonian," "Middle Devonian," and "Late Devonian," rather than being labeled with different geographic names (Ziegler and Klapper, 1982; Harland et al., 1990, pp. 40–41). Of all the options discussed herein, this is the one that I personally would have preferred. To my knowledge, the only problem with Option 2 is that it would violate the vote of the Subcommission on Paleogene Stratigraphy in 1989 selecting "Thanetian" and "Ypresian" to be the names of the contiguous Standard Global Ages/Stages on either side of the

Paleocene/Eocene boundary (Jenkins and Luterbacher, 1992; see discussion below).

5.3. Option 3

Option 3 of Aubry (2000a) is a variant of Option 2 in that the P/E boundary would still be defined using the CIE as the guiding criterion. However, a new earliest Eocene age/stage (of about 1.0 m.y. duration) would then be incorporated into the global chronostratigraphic framework immediately preceding the Ypresian, rather than extending the beginning of the Ypresian to the CIE. Aubry (2000a, p. 470) stated:

> I reiterate that whatever decision is taken regarding the location of the P/E boundary, the concept of the Ypresian Stage should be preserved in its current form (with the slight adjustment proposed above). Preserving the central role that stages have played in chronostratigraphy until now would thus require introducing a new stage whose base would be defined based on the CIE and whose top would correspond to the base of the Ypresian [synthem]. Aubry et al. (1999) reluctantly considered the introduction of a new stage because it multiplies the chronostratigraphic subdivisions, the GSSP's and the risks of miscarriage.

The reluctance of Aubry et al. (1999) to consider the introduction of a new stage of relatively short duration was well founded in my view; however, Aubry et al. (2000c, p. 214) subsequently recommended this very solution. Unfortunately, Option 3 would conflict with the vote of the Subcommission on Paleogene Stratigraphy in 1989 that the Ypresian should be the oldest Eocene Age/Stage. This vote has been cited several times by Aubry et al. in support of Option 1 (e.g., Aubry et al., 1999, p. 123; Aubry and Berggren, 2000a, p. 109). Regarding this vote, Aubry (2000a, p. 463) stated:

> The Ypresian Stage has been the object of a detailed monograph (Dupuis et al., 1991; with substantial additional documentation by Steurbaut, 1998) and is the internationally accepted (vote of the Subcommission on Paleogene Stratigraphy, IGC Washington D.C., 1989) lowest Eocene Stage, whose base is the Mont Héribu Member as proposed by De Coninck et al. (1983).

A similar claim was made by Aubry and Berggren (2000b, p. 14). Although these statements imply that the 1989 vote of the Subcommission decreed that the base of the Ypresian Standard Global Stage must correspond to the base of the Mont Héribu Member, such is not the case. How *could* the Subcommission have done so, before International Geological Correlation Project 308 and the P/E Boundary Working Group of the ICS were even established? (in 1990; see Aubry et al., 1998a,b, p. xii). Rather, the 1989 vote was on a purely nomenclatural matter. It merely decided the *name* ("Ypresian") that the oldest age/stage of the Eocene should be called, once a formal, golden spike-based definition for the P/E boundary had been agreed on (Jenkins and Luterbacher, 1992, Fig. 1). Aubry et al. (1999, p. 126) were well aware of this crucial distinction, but the statements of Aubry (2000a, p. 463) and Aubry and Berggren (2000b, p. 14) have obscured the nature of the 1989 vote.

A much more important problem with "Option 3" involves the possibility that it would create a trend to multiply Standard Global Stages unnecessarily. Although Aubry et al. (2000c, p. 214) claimed Option 3 to be in agreement with Hedbergian principles, in Figs. 13 and 14 of Hedberg (1976) and Salvador (1994), respectively, no examples of this practice were discussed or illustrated, because they were implicitly thought to be unnecessary. Boundary stratotypes were depicted as being located in a compromise way such that the introduction of new ages/stages of relatively very short duration were not needed (see, for example, Van Hinte's, 1968, p. 314 discussion of this point regarding the temporal gap between the unit stratotypes of the Campanian and Maastrichtian stages). If Aubry et al. should succeed in their proposal for a very short duration age/stage to be formally inserted between the Thanetian and Ypresian, then other Phanerozoic stage boundary disputes could eventually give rise to dozens of virtually useless new ages/stages, all about 1 m.y. or less in duration. Do they really want this?

Although Aubry et al. are naturally very interested in the minute details of European Paleogene stratigraphy, they tend to overemphasize its importance to other stratigraphers around the world, most of whom (if I am a representative judge) just want reasonably broad, stable, correlatable global temporal pigeonholes to assign their local rocks to (Chlupác et al., 1981; Menning et al., 2001; Walsh, 2001). Finer age resolutions can then be expressed in numerical terms (e.g., 53.4 ± 0.7 Ma), in magnetochronologic terms (e.g., C22n), in provincial biochronologic terms (e.g., late Wasatchian), or in terms of "standard" pelagic marine biochrons (e.g., P7), without any need for new golden spike-defined ages/stages of only ~ 1 m.y. duration. The vast majority of Paleocene–Eocene rocks around the world will not be assignable with any confidence to a golden spike-defined interval of only 1 m.y. (cf. Drooger, 1974, p. 175; Remane, 2000a, p. 681), and so formally naming such an age/stage would violate Salvador's (1994, p. 78) statement that the age/stage "is the smallest unit in the standard chronostratigraphic hierarchy that can be recognized at a global scale." Indeed, recent geomagnetic polarity time scales (Harland et al., 1990; Cande and Kent, 1992, 1995) have built-in potential calibration errors of up to ± 1 m.y. for much of the Paleogene (Harland et al., 1990, p. 154; cf. Berggren and Aubry, 1998, pp. 27–28; Aubry, 1998, p. 49).

5.4. Option 4

Option 4 of Aubry (2000a) is similar to Option 3 in that a new age/stage of about 1 m.y. duration would be added to the chronostratigraphic framework, and its beginning/base would again be defined to correspond to the CIE. However, this new age/stage would be regarded as latest Paleocene rather than earliest Eocene because under this option, the P/E boundary would be defined the same way as in Option 1. Option 4 would still conflict with the 1989 vote of the Subcommission on Paleogene Stratigraphy, because although the Ypresian would again be the oldest Eocene age/stage, the Thanetian would no longer be the youngest Paleocene age/stage (see Jenkins and Luterbacher, 1992).

5.5. Option 5B

Option 5B was referred to by Aubry et al. (1999, Fig. 1, column 5B), but for some reason was not specifically illustrated in Aubry (2000a, Fig. 2). Option 5B simply involves defining both the P/E boundary and the Standard Global Thanetian/Ypresian Age/Stage boundary to correspond to the CIE (Fig. 4). This alternative seems to have been viewed sympathetically by Berggren and Aubry (1998, p. 23), who stated:

> While not favoring one criterion over another at this point, we note that the choice of a δ^{13}C excursion/spike in mid-Biochron NP9 ... would have the virtue of unifying the Paleocene/Eocene Series boundary in these two disparate stratigraphies at a common level that is stratigraphically and temporally midway between the top of the Thanetian Stage and the base of the Ypresian Stage.

Note that in this quotation, the authors use "Thanetian Stage" in the restricted sense of the Thanetian unit stratotype of Curry (1981), as did Remane (2000a, p. 681), but not in the equivocal, expanded sense of Aubry (2000b, p. 684). However, Option 5B has since become repugnant to Aubry et al. (1999, 2000b), Aubry (2000a,b), and Aubry and Berggren (2000a), mainly because the beginning/base of the Ypresian Standard Global Age/Stage would be about 1 m.y. older than the base of the Ypresian synthem. Aubry (2000b, p. 685) stated:

> The result is that if the CIE is used for correlation of the P/E boundary, the privileged relationship between stages and series advocated by the Guide—in which the base of a stage [meaning synthem] defines the base of a series, not the opposite—cannot be respected.

However, this "privileged relationship between stages and series" (in which the base of a traditional synthem supposedly must define the base of a series) cannot be found anywhere in the International Stratigraphic Guide (Hedberg, 1976; Salvador, 1994). Indeed, contrary to the claims of Aubry (2000a, p. 470), Aubry (2000b, p. 685), and Aubry and Berggren, 2000b, p. 14), Option 5B would be perfectly consistent with Hedberg (1976, Fig. 13), who showed new boundary stratotypes for several hypothetical ages/stages being located approximately midway between the traditional unit stratotype boundaries. This is exactly where the CIE is situated; midway between the top of the Thanetian unit stratotype and the base of the Ypresian synthem (Fig. 4). Curiously, Aubry (2000a, p. 470) complained about the "significant inconvenience" that would result if the beginning of the Ypresian were to be made older by a mere 1 m.y., when the very use of the term "Ypresian Stage" for a *global chronostratigraphic unit* is a much greater violation of its historical meaning as a *synthem of merely regional scope*. Surely, this would be a good argument for using subepochs/subseries under Option 2!

Although Aubry (2000a, p. 469) objected to Option 5B on the grounds that chronostratigraphy must allegedly be "independent of any aspect of Earth history, either paleobiologic, tectonic, or climatic," it is important to note that this view is not shared by most of the members of the P/E Boundary Working Group. Thus, Schmitz (1994, p. 39) stated:

> [At the symposium "Stratigraphy of the Paleocene" held in Goteborg, Sweden in 1993], it was stressed that the formal division of the Paleocene should reflect the true sequence of lasting, global environmental changes during this period. Ideally, each stage should represent a period of time on Earth characterized by distinctly different environmental conditions (manifested, for example, in faunal, floral, and geochemical conditions) compared to adjacent stages.

This passage expresses an Earth history-*dependent* chronostratigraphic philosophy almost completely opposite to that of Aubry et al. I am somewhat sympathetic to this view, but would also note that if taken too far, it could result in an unnecessary multiplication of the number of ages/stages in a given epoch, a situation touched upon by Schmitz (1994, p. 39).

5.6. Option 6

To finally complete this exploration of some of the relevant P/E boundary permutations, yet another arrangement of ages/stages is possible, and is portrayed in Fig. 4 as Option 6. Under this scenario, the Thanetian Age/Stage would be restricted in time scope to the Thanetian as actually used by most British stratigraphers, as well as by Feugueur (1963) and Pomerol (1969, 1977; see Aubry 2000a, Fig. 1). In other words, it would correspond to the age span of the composite unit stratotype of the Thanetian Stage as designated by Curry (1981). A new age/stage would then be inserted between the restricted Thanetian and the base of the Ypresian synthem. Importantly, the CIE would play no part in this definitional scheme, which would be more consistent with Aubry's (2000a, p. 469) own claim that "a [traditional, pre-GSSP] chronostratigraphic boundary cannot be relocated in order to reflect any aspect of Earth history." Other consequences of Option 6 would be that the duration of the Thanetian Age/Stage would be reduced to about 1.2 m.y., while the new intermediate Age/Stage would have a duration of about 2.2 m.y. Obviously, therefore, my discussion of Option 6 does not mean that I advocate its implementation; I do not, because it would again unnecessarily multiply the number of Standard Global Ages/Stages of relatively short duration. I am only pointing out that Option 6 would be *required* by a consistent application of the avowed unit stratotype-sanctifying chronostratigraphic philosophy of Aubry et al.

Importantly, this philosophy has already been applied by Rio et al. (1991, 1994, 1998) to the Pliocene time scale. Because the top of the unit stratotype of the (traditionally late Pliocene) Piacenzian Stage was found to be about 0.8 m.y. older than the Plio–Pleistocene boundary of Aguirre and Pasini (1985), Rio et al. (1998) chose to insert a completely new Gelasian Age/Stage into the global chronostratigraphic framework between the Piacenzian and the Pleistocene. The arguments of Rio et al. (1991, p. 1057) are revealing:

> The Piacenzian Stage can be retained as a useful chronostratigraphic unit, but the usefulness of extending it to the Pliocene/Pleistocene boundary is debatable. We propose that the Piacenzian Stage be limited only to that stratigraphic interval present in the stratotype section... Utilizing this definition of the Piacenzian Stage results in the interval from 2.5 to 1.6 Ma not being represented by a chronostratigraphic unit. To rectify this situation, we propose a threefold subdivision of the Pliocene Series, with the Zanclean and Piacenzian representing the lower and middle Pliocene, respectively, and an unnamed stage for the upper Pliocene. This

proposal represents a major change in Pliocene chronostratigraphy, but we believe that the three-fold stage system provides a more practical subdivision of the Pliocene.

Contrary to Rio et al. (1991), the recognition of three ages/stages is *not* "more practical." Stratigraphers around the world are constantly faced with the practical question: What is the shortest duration geochronologic unit of the formal geological time scale to which I can confidently assign my local strata to? For a geologist working on nonmarine strata in, say, South America, it might be "Neogene," or "Pliocene," or if fortunate, possibly some subdivision of the latter. But with a three-part Pliocene, it is now more difficult to assign a given stratum to any particular one of these subdivisions. A rock unit in South America that might once have been confidently assigned a late Pliocene age (previously about 1.8–3.6 Ma), might now only be assignable to the Pliocene in general, because the available geochronologic methods are insufficient to resolve its assignment to either the now Middle Pliocene Piacenzian (3.6–2.6 Ma) or to the now Late Pliocene Gelasian (2.6–1.8 Ma).

6. Silurian/Devonian parallels

The position of Aubry et al. on the Paleocene/Eocene boundary is reminiscent of that of many British stratigraphers of the 1960s, who were accustomed to the Silurian/Devonian boundary being defined by the Ludlow Bone Bed (local base of the Old Red Sandstone) by both British and French workers since the early part of the twentieth century (see White, 1950, pp. 60; 63). This horizon was also very close to R.I. Murchison's (somewhat variable) original concept of the Silurian/Devonian boundary (Holland and Richardson, 1977, p. 36). When, however, for the sake of better global correlatability, these stratigraphers were confronted with the proposal (e.g., Holland, 1965) to redefine the Silurian/Devonian boundary to approximate the base of the *Monograptus uniformis* Zone (a horizon significantly younger than the Ludlow Bone Bed), they could not tolerate the thought that any "Silurian" rocks could be younger than the Ludlow Bone Bed, and complained about their literature becoming outdated and historical priority being ignored. J. Shirley even pleaded that "recent work demonstrated that [the Ludlow Bone Bed] can be correlated by means of ostracodes and other microfossils with the graptolitic sequence of Central Europe and that it can therefore no longer be considered as unsuitable on the grounds of difficulty of correlation. He maintained that *stability and priority demanded that this horizon has strong claims to be regarded as the boundary*" (McLaren, 1977, p. 11; my emphasis). The similarity of these arguments to those now being used by Aubry et al. should be obvious.

The point of this analogy is that, eventually, most of the British workers came around to the new *M. uniformis* criterion for the sake of improved correlation and better global communication (McLaren, 1977, p. 11). It is also interesting to note Hedberg's reaction to the Silurian/Devonian debate. If he believed as Aubry's portrayal of him would have us believe, then surely Hedberg would have opposed such a history-destroying redefinition of this traditional boundary. Instead, here is what Hedberg (1973, p. 177) said:

> I think this was an excellent procedure. A biostratigraphic horizon which appears to be very widespread and sharply time-significant was chosen as the guide to the boundary... the standard for the boundary was stated to be, not the base of the *M. uniformis* Range-zone in general, but the base of the *M. uniformis* Range-zone in a certain specifically designated boundary stratotype section. The procedure used coincides with that recommended in ISSC [1972] Report 6, Chronostratigraphic Units, pp. 13–18.

Hedberg did not decry the fact that given the redefinition, up to 500 m of the (traditionally Devonian) Old Red Sandstone had to be reassigned to the Silurian (Westoll et al., 1971, p. 287). Even a brief comparison of Allen and Tarlo (1963, text-Fig. 1) with Holland and Richardson (1977, Fig. 1) will reveal the major change in the traditional concept of the Silurian/Devonian boundary that British stratigraphers had to accept. But again, Hedberg (1973) did not even mention the original British definition. As noted by Holland (1986, p. 8), the new Silurian/Devonian boundary was in part a compromise: "British stratigraphers had to accept a level higher than that to which they had become accustomed; Central European colleagues had to be prepared to lower their traditional level.'' Again, this was a conventional decision that was settled by a vote. Aubry et al.'s consistent misrepresentation of Hedberg's chronostratigraphic views will be explored in detail in the next section.

7. Aubry vs. Hedberg (and the ICS) on chronostratigraphic principles

7.1. Boundary stratotypes vs. GSSPs

Aubry et al. (1999, pp. 108–110) charged that the phrase ''Global Stratotype Section and Point'' of Cowie et al. (1986) is nothing more than a new name for the boundary stratotype of Hedberg (1976). This is incorrect. A boundary stratotype is a general concept applicable to boundary definitions of several kinds of stratigraphic units, including lithostratigraphic, unconformity-bounded, and both regional and global chronostratigraphic units (Hedberg, 1976; Salvador, 1994). In contrast, GSSPs are a specific kind of boundary stratotype, dealing only with the golden spike-defined Standard Global Geochronologic/Chronostratigraphic units of the geological time scale. Requirements for GSSPS are much stricter than the requirements for boundary stratotypes of other kinds of stratigraphic units (Cowie et al., 1986; Salvador, 1994, pp. 90–91), so the coining of the phrase ''Global Stratotype Section and Point'' was entirely appropriate.

It is interesting to note that the definitions of ''boundary stratotype'' given by Hedberg (1976) and Salvador (1994) differ significantly. For Hedberg (1976, p. 24), a boundary stratotype was ''a specific **point** in a specific sequence of rock strata that serves as the standard for definition and recognition of a stratigraphic boundary [emphasis mine].'' In contrast, Salvador's (1994, p. 26) definition is: ''A specified **sequence** of rock strata in which a specific point is selected that serves as the standard for definition and recognition of a stratigraphic boundary [emphasis mine].'' Thus, for Hedberg, a boundary stratotype was the boundary itself, being a dimensionless point having no thickness, whereas for Salvador, a boundary stratotype is an actual stratigraphic section that contains the boundary point. The self-defining phrase ''Global Stratotype Section and Point'' therefore eliminates any semantic confusion that might result from these two different definitions.

7.2. Does the content of a stage determine its boundaries?

Aubry (2000a,b), Aubry et al. (1999, 2000b,c), and Aubry and Berggren (2000a) argued that ''Hedbergian'' principles are commonly violated by the ICS when system, series, and stage boundaries of the formal global chronostratigraphic framework are defined independently of historical unit stratotype or synthem boundaries. Aubry et al. (1999, p. 128) stated:

> Stages cannot be defined by their lower and upper boundaries only, although Remane et al. (1996, p. 78) claim that 'Chronostratigraphic units of the Phanerozoic Global Standard can only be defined through boundary stratotypes' and regret that unit stratotypes played an important role in Hedberg's chronostratigraphic framework (see also, e.g., Cowie, 1986; Cowie et al., 1986). We must recognize that we have the ability today to define boundary stratotypes only because the content of stages for which we set boundaries (the GSSPs) have been well-documented. We can, of course, conceive of stratigraphic units that are defined only by their boundaries and without knowledge of the rocks in between (see below), but this cannot apply to stages . . . Thus, we simply cannot introduce new stages defined solely by their boundaries. The content of a stage is what determines its boundaries.

After reading this discussion of Hedberg's alleged protection of the priority of ''stages'' (meaning traditional unit stratotype or synthem boundaries) over series and systems, and his alleged opinion that in chronostratigraphy, boundary definition alone is unworkable, one will be very surprised to go back and read the following passage from Hedberg's (1977, p. 231) reply to Van Couvering (1977):

> The unit-stratotype of a stage is of little importance in its definition except as its upper and lower limits

> constitute its upper and lower boundary-stratotypes. It is the boundary-stratotypes that really count (see Guide, pp. 71, 83–84). The internal character (lithofacies, biofacies, etc.) of a stage has no bearing on its time scope and cannot possibly be typified by any one section...

Here, Hedberg clearly states that the content (lithologic, paleontologic, etc.) of the traditional unit-stratotype of a stage is irrelevant to its time scope (and thus also irrelevant to where the boundaries of the stage will occur far away from the unit stratotype). Insofar as the unit stratotype of a traditional stage might be used in the global chronostratigraphic framework, it is the *boundaries* of this unit stratotype that will define the duration of its corresponding Age, which will then in turn conceptually define the set of all strata on Earth (Stage) that were formed during this Age.

Despite being a self-proclaimed "Hedbergian," Van Couvering (2000, p. 173) is still unclear about Hedberg's actual views on stages, as evidenced by the following passage:

> It follows, however, that we must then go beyond the present Guidelines [Hedberg, 1976] and accept that certain marine-based stages—and not others—will be elevated into "global" status, as the building blocks of the worldwide chronostratigraphic hierarchy. The stages that are selected for this special role should therefore receive a distinguishing name such as "Global Standard Stage", or GSS for short. The GSSP at their base must meet two standards: (1) that of global correlatibility, and (2) equally that of historical appropriateness.

One can only wonder where Van Couvering had been for the previous 24 years, because Hedberg (1976, pp. 76–77) clearly spoke of the "Standard Global Chronostratigraphic (Geochronologic) Scale" and stated that "Such a scale should be extended to include standard series (epochs) and stages (ages)" (see also Salvador, 1994, p. 85). Therefore, Van Couvering (2000) seems to have overestimated his own originality in claiming that we must "go beyond" the Hedberg Guidelines because "certain marine-based stages—and not others—will be elevated into "global" status, as the building blocks of the worldwide chronostratigraphic hierarchy." This was thoroughly understood by Hedberg long ago, and in fact was clearly explained to Van Couvering (1977) by Hedberg (1977, pp. 231–232)! Van Couvering's (2000) phrase "Global Standard Stage" is therefore clearly an unnecessary synonym of Hedberg's (1976) "Standard Global Stage." Van Couvering's (2000) additional claim that GSSPs for Standard Global Stages must be globally correlatable and historically appropriate implies that these two criteria have equal importance. In fact, 11 criteria were listed by Hedberg (1976, p. 80, item 5) for this purpose, and among them, "historical appropriateness" was listed next to last.

7.3. What does "base defines boundary" mean?

Aubry (2000a, p. 465) claimed that:

> boundaries are defined by the base of the younger stage (Hedberg, 1976; see also Salvador, 1994). This implies that boundaries between series, systems and erathems are the corollary of specific stage boundaries, or more exactly the base of specific stages. It follows that the P/E boundary is defined by the base of the Ypresian Stage [meaning Ypresian synthem] as stratotypified in Belgium, with its base corresponding to that of the Mont Héribu Member.

Similar claims were made by Aubry et al. (1999, pp. 111–112), Aubry (2000b, p. 685), and Aubry and Berggren (2000a, p. 110). In other words, Aubry et al. want the boundaries of the formal divisions of the (Cenozoic) geological time scale to be determined by the bases of the historical "stages" (synthems) of Europe, a situation illustrated in Fig. 6. But contrary to what these authors claim, neither Hedberg (1976) nor Salvador (1994) have ever maintained that a new boundary stratotype for a stage (or higher-ranked chronostratigraphic unit) must be chosen so as to approximate the age of the base of the younger of two consecutive stage unit stratotypes or synthems. For example, in Fig. 13 of Hedberg (1976), new boundary stratotypes for various hypothetical ages/stages are shown approximately midway between the traditional unit stratotype boundaries, and most certainly do not correspond to the bases of the younger unit stratotypes. In Salvador's (1994, Fig. 14) version

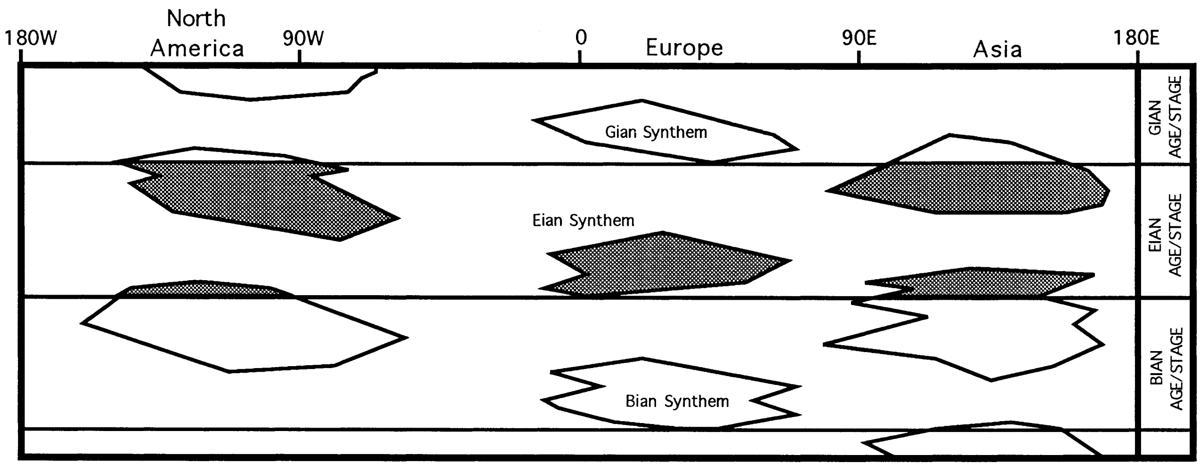

Fig. 6. Modified version of Fig. 2 illustrating the distorted "base defines boundary" argument of Aubry et al., who claim that the beginnings of the Standard Global Ages/Stages (and corresponding higher-level units) should be dictated by the bases of the historical European synthems. Such a view is untenable for reasons discussed in the text, and contrary to their claims, was never held by H.D. Hedberg.

of this diagram, the boundary stratotypes for the bases of Stages B and D are shown to correspond not to the bases of the unit stratotypes of B and D, but rather to the tops of the subjacent unit stratotypes of Stages A and C. Thus, the claims of Aubry et al. are plainly contradicted by the very publications of Hedberg and Salvador that they cite.

Further proof that Hedberg did not hold the narrow view attributed to him by Aubry et al. is supplied by his response to the following question asked by Hancock (1966, p. 179):

> For example, between the highest beds in the type Cenomanian at Le Mans, and the lowest beds in the type Turonian near Tours, there is at least one whole ammonite zone. In which stage would Dr. Hedberg place this zone?

If Hedberg had really held the "base of younger historical unit stratotype defines stage boundary" position that Aubry et al. insist he did, Hedberg would have answered that Hancock's stageless ammonite zone should be assigned to the Cenomanian, because this zone is older than the base of the unit stratotype of the Turonian. Instead, Hedberg (1968, p. 195) stated:

> I think it is simply a matter of reaching international agreement that some certain point in a section of continuously deposited strata is the type Cenomanian–Turonian boundary (Cenomanian–Turonian boundary stratotype) and then using all available evidence to the best of our ability to decide the age relation of Hancock's ammonite zone to this point, and hence its proper stage assignment.

Clearly, Hedberg did *not* insist that the golden spike-defined Cenomanian–Turonian boundary stratotype must be as close as possible in age to the base of the unit stratotype of the Turonian Stage.

In discussing potential problems in locating GSSPs, Aubry (2000a, p. 471) asked:

> But should an unconformity occur [at the level at which a golden spike is driven], the boundary will correspond to two independent horizons (the unconformable surfaces). Which of the two will serve for the definition?

Here Aubry has misunderstood the "base defines boundary" principle of George et al. (1967, p. 81). This principle simply means that once a boundary stratotype is selected, then *if* there happens to be an unrecognized disconformity right at the golden spike level, the golden spike will be understood to be (figuratively) hammered into the very base of the *upper* bounding surface of the disconformity, rather than hammered into the top of the *lower* bounding surface of the disconformity. This point was also clearly discussed by Ager (1973, p. 71; quoted but evidently misunderstood by the first author of Aubry et al., 1999, p. 106), McLaren (1977, p. 20), Cowie et al. (1986, p. 8), and Salvador (1994, p. 90). Unfortunately, as shown above, Aubry has distorted this "base defines boundary" principle in an effort to convince others that a new boundary stratotype should always be selected so as to correspond in age to the base of the younger of two traditional unit–stratotypes or synthems.

To further illustrate the dubious consequences of Aubry's distorted "base defines boundary" principle, let us suppose that, as maintained by Hansen (1979) and Thomsen (1981), and as illustrated by Hardenbol and Berggren (1978, Figs. 3 and 4), the age of the base of the historical Danian unit stratotype at Stevns Klint in Denmark is about 0.5–1 m.y. younger than the Cretaceous/Paleogene boundary as currently conceived. Although there now seems to be a consensus that such is not the case (Schmitz et al., 1992, pp. 241–242), let us assume for the sake of discussion that such a disconformity is indeed present at Stevns Klint, and then follow Aubry's arguments to their logical conclusions. If the base of the traditional European unit stratotype should determine the base of every formally defined global chronostratigraphic Stage (and its corresponding series and system boundaries), then under the above scenario, the base of the Standard Global Danian Stage *and* the Cretaceous/Paleogene (= Mesozoic/Cenozoic) boundary should both be defined to correspond to the significantly younger age of the base of the Danian unit stratotype (mass extinctions and iridium layer be damned). The obvious question would be: *Why*?! Why should geologists around the world be held hostage to the historical accidents of European stratigraphy?

7.4. Is the stage really the basic unit of chronostratigraphy?

M.-P. Aubry (International Subcommission on Paleogene Stratigraphy, 2000, p. 38) stated:

> The International Stratigraphic Guide (Hedberg, 1976; Salvador, 1994) establishes the stage as the basic unit of chronostratigraphy to which chronostratigraphic units of higher ranks are subordinated. In accordance, the base of a stage determines the base of a series.

Similar claims were made by Aubry (2000a, pp. 465, 469), Aubry et al. (2000b, p. 204), and Aubry and Berggren (2000a, p. 107). However, the only statement I can find in the International Stratigraphic Guide that approximates the claims of Aubry et al. is from Hedberg (1976, p. 71) and Salvador (1994, p. 78):

> The stage has been called the basic working unit of chronostratigraphy because it is suited in scope and rank to the practical needs and purposes of intraregional chronostratigraphic classification. Furthermore, it is one of the smallest units in the standard chronostratigraphic hierarchy that in prospect may be recognized worldwide.

In noting that the stage *has been called* the basic *working* unit of chronostratigraphy, I interpret these passages to mean only that numerous coeval regional "stage" schemes have been defined around the world to aid stratigraphers in their practical correlations. Far fewer regional series and systems have been defined, because only one set of global series and systems have generally been thought necessary for most Phanerozoic rocks. In other words, when stratigraphers argue about the age of a given stratum, they will most often be arguing about what stage it belongs to, rather than what series or system. Hedberg's (1976, p. 78) statement cannot be interpreted as a proclamation that the stage is the basic unit of chronostratigraphy in any necessary, foundational sense.

To illustrate further, Robert Knox (International Subcommission on Paleogene Stratigraphy, 2000, p. 33) has argued that the stage should be regarded as the basic building block of chronostratigraphy (as is the formation in lithostratigraphy) and that the Series should be regarded as a basically arbitrary grouping of stages (in the same way that the group in lithostratigraphy is basically an arbitrary assemblage of formations). Unfortunately, Knox's provocative analogy is not quite valid. The formation is indeed the basic building block in lithostratigraphy because a formation must exist before it can be subdivided into any component members, and a group does indeed consist only of a stipulated set of previously or simultaneously defined formations. However, the stage does not (and need not) play an analogous role in chronostratigraphy. First, we obviously must still develop a geochronologic/chronostratigraphic classification of the Precambrian (~ 85% of recorded geologic time), in spite of the fact that ages/stages have not been and probably never will be recognized for this interval. Even within the Phanerozoic, it is acceptable to define a period/system or epoch/series boundary without reference to a regional or standard global age/stage, as noted by Hedberg (1976, pp. 72–73) and Remane (2000a, p. 682). Knox's position is also refuted by the fact that as soon as the Ordovician/Silurian and Silurian/Devonian GSSPs were ratified (Bassett, 1985; McLaren, 1977), the Silurian Period/System was automatically defined, long before the names and boundaries of all of its component ages/stages were agreed upon. The same is also true of the Cambrian Period/System (Brasier et al., 1994; Landing, 1994; Geyer and Shergold, 2000; Cooper et al., 2001).

While the stage cannot be regarded as the basic unit of chronostratigraphy for reasons discussed above, the scopes of existing, widely used stages should of course be taken into account (along with many other considerations) when defining corresponding series and system boundaries. On this point, I broadly agree with Knox (1994) and Aubry (2000a,b). However, the assertion that historical and usually unconformable "stage" (synthem) boundaries should *necessarily determine* their corresponding series and system boundaries (Aubry, 2000a, pp. 465; 469) is a very different and unsupportable claim. Indeed, it was properly dismissed by Remane (2000a, p. 682), who noted that "Recognizing the stage as basic unit of [standard global] chronostratigraphic classification means that the boundaries of all units of higher rank have to coincide with [standard global] stage boundaries, no more."

7.5. Is there a limit beyond which traditional stages cannot be expanded?

In discussing Fig. 13 of Hedberg (1976), Aubry et al. (1999, p. 133) stated:

> Furthermore, in the search for stage boundaries, it is in fact acceptable (as shown by Hedberg, 1976, Fig. 13) to move the base of the stage slightly downwards (to a level as much as 200,000 to 300,000 years older) in order (1) to construct a chronostratigraphic framework without temporal gaps, and also (2) to improve the correlation potential.

Aubry et al. (1999) imply here that according to Hedberg (1976, Fig. 13), traditional stage unit stratotype boundaries can be adjusted by no more than 200,000–300,000 years when defining new formal stage boundaries with GSSPs. But where did Aubry et al. (1999) get the figures "as much as 200,000 to 300,000 years"? One can only wonder, because no numerical ages whatsoever are shown in Fig. 13 of Hedberg (1976). In fact, when one actually measures (with a millimeter scale) the "duration" of the unit stratotype of Stage D on the left side of this figure, and then measures the increased "age" of the new boundary stratotype between Stages C and D on the right side of this figure, one obtains an increase in duration of about 14%. Thus, for example, assuming a current duration of 5.5 m.y. for the Ypresian (Berggren et al., 1995), a 14% increase would extend the beginning of the Ypresian by some 0.8 m.y., which is nearly the same as is actually being proposed for the Ypresian expansion (~ 1.0 m.y.) given the age of the CIE. I should emphasize that A. Salvador (personal communication) has indicated that neither Fig. 13 of Hedberg (1976) nor Fig. 14 of Salvador (1994) were meant to be taken literally, but were drafted only to communicate a general concept. Nevertheless, the allegedly maximum allowable limit of 300,000 years for extending age/stage boundaries is clearly an invention of Aubry et al. (1999), not Hedberg. Such an artificial limit would be particularly unreasonable when adjusting formal age/stage boundaries in the Paleozoic, where temporal resolution is often at least an order of magnitude greater than 300,000 years.

7.6. Should definition really precede correlation?

Aubry et al. (2000b, p. 204) quoted the following statement from Hedberg (1976, p. 86):

> Only after the type limits (boundary stratotypes) of a chronostratigraphic unit have been established

> can the limits be extended geographically beyond the type section.

and claimed that it stood in sharp contrast to the "Correlation Precedes Definition" principle of the ICS, which was framed by Remane et al. (1996, p. 78) as follows:

> To define a boundary first and then evaluate its potential for long-range correlation (as has been proposed in some cases) will mostly lead to boundary definitions of limited practical value.

However, Aubry et al. (2000b, p. 204) have misinterpreted Hedberg's (1976, p. 86) statement. Its true meaning was more clearly expressed by Hedberg (1958, p. 1892):

> . . . can you imagine anything more ridiculous than the reams of paper and hours of time which have been wasted in arguments and controversies about where to draw the boundary in a certain region between the Oligocene and the Miocene, the Cretaceous and the Tertiary, the Cambrian and the Precambrian, etc., when no one knows exactly what is included in the *type* Oligocene, or what is included in the *type* Miocene, or what is included in the *type* Cretaceous? Until standard reference sections of actual rocks have been designated for these series and systems and these have been accepted as definitions by some international authority, **how can anyone possibly identify or trace their boundaries with any validity?** [italics in original, boldface mine]

Thus, Hedberg (1976, p. 86) simply meant that a *single* boundary must be *formally* agreed upon by *everyone* before we can *meaningfully* correlate it beyond the stratotype. If there are multiple competing concepts of a given boundary, then it is impossible to *meaningfully* correlate such an ambiguous "boundary." Hedberg (1976, p. 86) in no way meant to say that we must define a boundary before evaluating its correlatibility! It is difficult to see how Aubry et al. (2000b) could have misinterpreted Hedberg on this point in view of the fact that Hedberg (1958) was explicitly cited by Aubry et al. (1999).

Nevertheless, Aubry (2000a, p. 471) insisted:

> Following Hedberg, the procedure is straightforward: the lithostratigraphic boundary horizon is directly selected, the means for correlation are then determined. Even if the base of a stage is slightly adjusted so as to define boundary stratotypes, the boundary is relocated at the closest level that is easily correlatable. Following Hedberg's principles, stage definition has precedent. Even if the means used to correlate the boundary level are shown to be inappropriate, the definition stands. There can never be a real problem of definition, particularly in view of possible reference to the unit-stratotype (even if the boundary stratotype is located outside of the type area). There can only be a problem of correlation, which of course may be acute (e.g., correlation of the Miocene stages).

Similar claims were made by Aubry et al. (1999, p. 100) and Aubry et al. (2000b, p. 207). However, contrary to these assertions, Hedberg plainly believed that the global correlatability of a geochronologic/chronostratigraphic boundary was a very important matter to consider before its formal definition. Thus, of the 10 "Principal points in the ISSC recommended procedure for the definition of systems (or other units) of the Standard Global Chronostratigraphic Scale," points 4 and 8 state (Hedberg, 1976, pp. 80–81):

> 4. Review of potential widespread correlation horizons in the general boundary zone between the two systems and their probable value for regional or global time-correlation . . . 8. Selection in the field of the precise position of the boundary-stratotype in the chosen section, so as to best express the appropriate concepts of the two adjacent systems, and so as to be most practicably correlatable as an approximately isochronous horizon worldwide.

Further proof that Hedberg did not hold the views attributed to him by Aubry et al. was provided by Remane (2000b, pp. 211–212). Finally, Hedberg's consistent opposition to the placement of formal geochronologic/chronostratigraphic boundaries at unconformities can only be interpreted as a logical consequence of his view that the correlatability of these boundaries was a very important factor in their selection (see below).

7.7. Can golden spikes be placed at unconformities?

In view of Aubry's (2000a, p. 471) question (quoted above) about unrecognized unconformities at the level of a golden spike, consider this puzzling statement by Aubry et al. (2000b, p. 208):

> Surprisingly enough, these [unconformable] surfaces have the same age over large areas. There is little if any diachrony involved in many instances (see Poag, 1993; Aubry, 1995a; Aurisano et al., 1995). For this reason, since base defines chronostratigraphic units, an unconformable surface may constitute a horizon of reference. The base of the marine Trubi marls that represents the base of the formally defined Zanclean Stage is an horizon that rests unconformably over the upper Messinian alluvial/lacustrine Arrenazollo Formation [Van Couvering et al., 2000]. The base of the Ypresian Stage could very well be defined by the base of the London Clay Formation. If continuous boundary sections are deemed more suitable, however, we know how to correlate these horizons in more continuous stratigraphic sections.

Along with Remane (2000b, p. 212), I am unable to decipher the meaning of the proposition that "since base defines chronostratigraphic units, an unconformable surface may constitute a horizon of reference," but my first point is that the question posed by Aubry (2000a, p. 471) is impossible to reconcile with the statement of Aubry et al. (2000b, p. 208). On one hand, Aubry (2000a, p. 471) is worried that if we define a GSSP in an apparently conformable section, the chosen level *might* actually correspond to a disconformity (heaven forbid!), in which case we supposedly will not know how old the boundary really is. On the other hand, Aubry et al. (2000b, p. 208) stated that we could very well define the base of the Ypresian Standard Global Stage to simply *be* the base of the *known unconformity-bounded* London Clay! Is Aubry uncertain about how old *this* boundary would be?!

Hedberg (1976, p. 84) would clearly have rejected the proposal of Aubry et al. (2000b) in view of the fact that he stated: "The worst possible boundary is an unconformity; it not only does not represent a sharp point in time but also tends to change in age laterally" (see also Salvador, 1994, p. 90). Also, given that the Ieper Formation and London Clay are deposits resulting from a marine transgression from the west and north (Berggren and Aubry, 1998, p. 28), then the bases of these units are indeed diachronous, even if our current geochronologic methods are unable to detect this diachrony (although all of the relevant lithostratigraphic units are shown to be perfectly isochronous in Fig. 4 (after Aubry, 2000a, Fig. 2), this depiction is almost certainly an oversimplification of reality). Thus, the verbal definition for the beginning/base of the Standard Global Ypresian Age/Stage proposed by Aubry et al. (2000b, p. 208) would have to be in theoretical terms, e.g., "the beginning of the Ypresian Age is defined as the moment of deposition of the oldest particle of sediment belonging to the London Clay." No GSSP could be designated for this purpose, because at least some part of the London Clay would always be slightly older than the base of the London Clay at any particular boundary stratotype section available to humans (see also Fig. 1). Unfortunately, such a verbal, theoretical boundary definition would be potentially unstable, as it would be subject to change if our lithostratigraphic definition of the lower boundary of the London Clay were also to change (as it has several times in the past).

As for the tradition-upholding, deliberate placement of the Miocene/Pliocene boundary GSSP at the basal Zanclean unconformity by Van Couvering et al. (2000), Hedberg (1977, p. 230) stated in his reply to Van Couvering (1977):

> I think it important to be sure that the type boundary between two stages is designated at a single point in a continuously deposited sequence of strata, not at an unconformity or hiatus in deposition, even if it should be necessary to put it in the middle of a bed to make certain. If the type boundary was unwittingly placed at an unconformity, then it could not have been a valid chronostratigraphic boundary and should be corrected.

This was certainly one of Hedberg's most fundamental chronostratigraphic principles, and it is difficult to see how Aubry et al. (2000b) and Van Couvering et al. (2000) can consider themselves "Hedbergians" in view of their evident disregard for it. As Aubry et al. are well aware, if a golden spike is placed at a significant unconformity, then we can correlate it by means of the geochronological evidence available in the boundary stratotype section on only one side of the boundary. In contrast, if the golden spike is placed in a conformable section, then we can (more accurately) correlate it by using the geochronological evidence available on *both* sides of the boundary. Whether or not the Miocene/Pliocene GSSP defined by Van Couvering et al. (2000) turns out to be as easily correlatable as they claim remains to be seen, but even if it is, that does not demonstrate the general validity of defining GSSPs at unconformities in order to preserve traditional usage. Indeed, it would appear that only the exceptional astrochronological *correlatability* of the Trubi Formation has allowed the placement of the Miocene/Pliocene GSSP at the base of this unit. If the Trubi Formation did not contain such a record, then the unconformable contact between it and the underlying Arrenazollo Formation would never have been seriously considered for the formal Miocene/Pliocene boundary.

7.8. Arbitrariness in defining geochronologic/chronostratigraphic boundaries

Among others, Aubry (2000a) and Walsh (2001) have argued that geochronologic/chronostratigraphic boundaries are arbitrarily defined. However, these authors are using the term "arbitrary" in different ways. Webster's Seventh New Collegiate Dictionary defines "arbitrary" as follows:

> **1**: depending on choice or discretion; determinable by decision of a judge or tribunal. **2a**: arising from will or caprice; **b**: selected at random and without reason.

Clearly, when we say that geochronologic boundaries are arbitrarily defined, we do not mean that they arise from will or caprice, or are selected at random and without reason. Instead, we mean that they are consciously selected, usually from a considerable number of possible candidates, through the discretionary process of a democratic vote. Thus, there is no such thing as a geochronologic/chronostratigraphic boundary that is, a priori, the One True Boundary (Ager, 1973: Chapter 7; Remane, in press). Instead, formal boundary choices are made by an appropriate commission of stratigraphers weighing many relevant factors (Salvador, 1994; Remane et al., 1996). Probably each member of such a commission will give slightly different subjective weights to each factor, for example, some emphasizing historical tradition, others emphasizing global correlatability, others emphasizing their own favorite methods of correlation (e.g., certain types of fossils as opposed to others; chemostratigraphic horizons; magnetic polarity reversals, etc.). However, when a formal vote is taken by such a commission, all of these considerations "come out in the wash," and the result is an arbitrarily defined boundary in the sense that is a *conventional* boundary (Remane et al., 1996; Remane, 2000a,b).

In contrast, Aubry (2000a, pp. 467; 472) stated:

> However, it was one of Hedberg's most important insights to foresee the instability of a chronostratigraphic framework that would be based on the history of life, and to install objectivity (arbitrariness) through the recognition of stages as the central units of chronostratigraphy.
>
> In conclusion the main difference between a chronostratigraphic framework established based on Hedberg's principles (ISSC) and one based on the ICS's guidelines is that only the former is truly arbitrary, independent of any aspects of Earth history, and thus able to provide the stability required to study Earth history. As I see it, as long as the correlation potential remains central to chronostratigraphy, there will be reasons to argue for changes to the locations of chronostratigraphic boundaries.

Here, Aubry seems to think that geochronologic/chronostratigraphic boundaries are arbitrary in the sense that they are and should be preordained by the random accidents of historical usage, i.e., that they should correspond to traditional unit stratotype or synthem boundaries (see also Aubry et al., 1999,

p. 133). But this view simply cannot be taken seriously. First, the exact boundaries of many unit stratotypes are still controversial, having often been changed several times since their original designation (even assuming they were originally designated at all; see, for example, Berggren's (1964) discussion of the multiple competing definitions of the Maastrichtian unit stratotype). Second, as she is well aware (e.g., Aubry et al., 1999, pp. 141–142; Aubry, 2000a, p. 469; Aubry et al., 2000b, p. 206), even if the boundaries of a given traditional unit stratotype are agreed upon by all workers, many of these unit stratotypes are unconformity-bounded and/or consist of nonmarine and/or brackish-water facies that do not provide the degree of global correlatability that the modern subdivisions of the geologic time scale require. The matter was clearly put by Remane et al. (1996, p. 78) as follows:

> There is no formal priority regulation in stratigraphy. Therefore, in redefining boundaries, priority can be given to the level with the best correlation potential ... This does not mean that priority should be totally neglected. Practical considerations will incite us to limit changes to the necessary minimum. If, however, the interregional correlation potential of a traditional boundary does not correspond to the needs of modern stratigraphy, its position has to be changed.

Ironically, this reasonable view toward priority is consistent with a view expressed long ago by Berggren (1964, p. 110):

> Voigt (1956, p. 16) concluded with the perceptive observation that usage of the rule of priority in stratigraphy would lead to impossible consequences and anomalous results. To accept an original definition of a stratigraphic term (or unit) as a rigid, unalterable stratigraphic concept may, in some instances, hinder further identification and elucidation of the variables involved. (The original definition of such a term as Paleocene and the stage names generally included here—Sparnacian, Thanetian, Montian, and Danian—and subsequent concepts applied to them by stratigraphers and paleontologists may serve as an example here.)

Nevertheless, Aubry et al. (2000b, p. 208) claimed that "it is irresponsible of the ICS to deprive chronostratigraphy of its roots, by insisting that historical priority has no ground in setting its subdivisions." But this claim is false (Remane et al., 1996, p. 78; Remane, 2000a,b). Of course historical priority has a role. It just does not have the *only* role (and not necessarily the most important role). See Salvador (1994, pp. 22–23; 91) and Remane (2000a,b, in press) for additional remarks on the problem of priority in chronostratigraphy.

As for Aubry's (2000a, p. 461) assertion that Hedberg believed that "chronostratigraphy must remain objective and as arbitrary as possible, and thus independent of any aspect of Earth history, either paleobiologic, tectonic, or climatic," Hedberg (1976, p. 71) actually stated:

> If major natural changes ("natural breaks") in the historical development of the Earth can be identified at specific points in sequences of continuous deposition, these may constitute desirable points for the boundary-stratotypes of stages.

There cannot be a more conclusive refutation of Aubry's (2000a) claim (Remane, 2000b, p. 212).

Aubry et al. (2000b, p. 204) cited with approval a paper by Naidin (1998),[5] who claimed that the ICS has not explicitly outlined its stratigraphic philosophy. As shown above, however, the stratigraphic philosophy of the ICS is clearly the same as that of the ISSC, as exemplified by Hedberg (1976) and Salvador (1994) (see also Vai, 2001). So, what specific recommendations would Aubry et al. add to the International Stratigraphic Guide, or to the Guidelines of the ICS? I can only conclude from their writings that they would support an explicit mandate that GSSPs should always be placed so as to approximate the age of the

[5] Unfortunately, I can understand little of Naidin's (1998) argument, which contains few specific recommendations but includes several misunderstandings of the ICS Guidelines. These cannot be addressed here for space reasons, but answers to most of Naidin's objections are readily found in Cowie et al. (1986), Salvador (1994), and Remane et al. (1996). Naidin's (1998, pp. 1029–1030) claim that there is a distinct difference "between the procedures of convention and voting" is potentially relevant to the present discussion of arbitrariness in boundary definition, but his arguments for this claim are unclear to me.

base of a traditional European unit stratotype or synthem. Fortunately, Hedberg was not so dogmatic. He did not decree that GSSPs should always correspond to the base of a historical unit stratotype, or to the top of a historical unit stratotype, or should be placed exactly midway between the boundaries of two successive unit stratotypes. He was perceptive enough to realize that each boundary situation was unique, and therefore deliberately left the placements of GSSPs to the stratigraphers most knowledgeable about a given boundary, trusting them to reach reasonable decisions through open debate and a democratic vote. If Aubry et al. have a better approach, they have not presented it. Far from the GSSP process resulting in a "locked system" (Aubry, 2000a, p. 472), it is in fact the "base of European synthem necessarily defines Age/Stage boundary" dogma of Aubry et al. that would impose a locked system on all other stratigraphers throughout the world.

Fig. 7 is a proposed revision of Fig. 14 of Salvador (1994) that more clearly shows the meaning of "arbitrariness" in defining boundaries of the formal ages/stages of the geologic time scale (Walsh, 2001). Given the obvious gaps and overlaps in the age spans of several historical unit stratotypes (A–F), there are numerous conceivable ways to define contiguous temporal pigeonholes. Two possible solutions are illustrated. Solution 1 adopts a "splitting" approach, wherein a total of seven ages/stages are recognized, one of which is new (although similar in duration to the others). In Solution 2, a "lumping" approach is used, wherein five ages/stages of longer duration are recognized, and Stage B is dispensed with. In some cases, the new boundaries approximate the age of the base of a historical unit stratotype; in other cases, the new boundaries approximate the age of the top of a historical unit stratotype, and in still others, the new boundaries are intermediate in age between two historical unit stratotypes. The decision as to exactly how each age/stage boundary is defined will depend on numerous factors, including original definition, historical usage, current usage, and global correlatability.

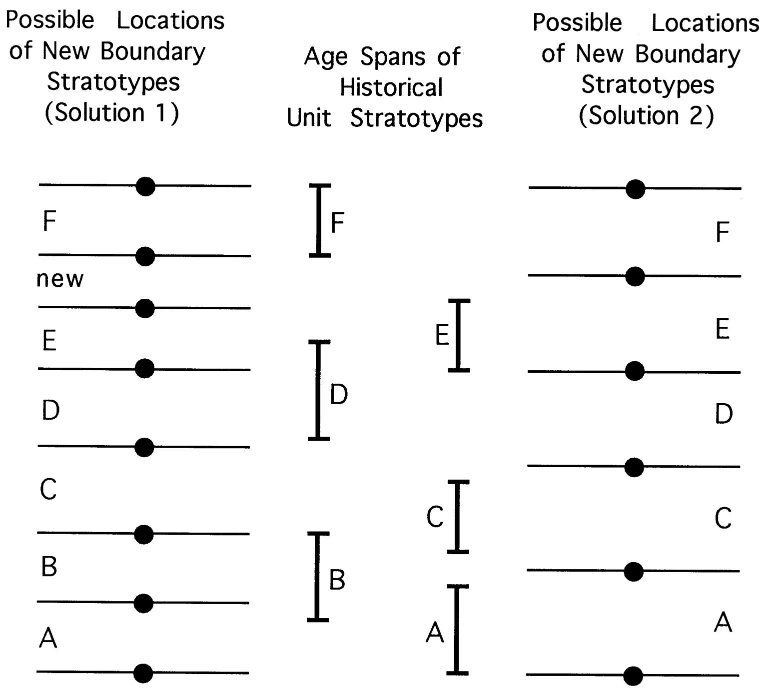

Fig. 7. Proposed revision of Fig. 14 of Salvador (1994) showing the many different possible ways in which contiguous temporal units might be defined using boundary stratotypes. Such boundary stratotypes may approximate the age of the base or the top of a given historical unit stratotype, or may be intermediate in age between two temporally disjunct or temporally overlapping unit stratotypes. See text for discussion.

Most boundary decisions will involve some degree of compromise on these factors. That these decisions are indeed arbitrary simply means that they can be settled by a vote (Remane et al., 1996, p. 78; Remane, 2000a,b). Concrete examples of such decisions are illustrated by the discussion of Menning et al. (2001) regarding the optimum number of Carboniferous series and stages.

7.9. The permanency of golden spikes revisited

Aubry et al. have sometimes misunderstood the very nature and purpose of the golden spike, or GSSP. Aubry et al. (1999, p. 135) stated:

> What will happen in the case of GSSPs that have been defined on criteria that future work may show to be diachronous, or distorted by unrecognized hiatuses? New GSSPs will be erected and the framework of correlations established around the ill-suited GSSP will disintegrate, but this instability is no way to maintain a global time-scale.

Similarly, Aubry et al. (2000b, p. 206) stated:

> We are therefore concerned that the present rules governing the erection of GSSPs will affect the stability of the Cenozoic chronostratigraphic scale. For example, chronostratigraphic units of long standing usage will be subject to redefinition each time a more powerful element of correlation is discovered.

However, Aubry et al. (1999) are simply mistaken that "new GSSPs will be erected" to replace any "tarnished" golden spikes. On the contrary, such golden spikes will remain in place (Murphy, 1994, p. 268; Remane, 2000a,b; Walsh, 2001). The whole purpose of the golden spike is to locate it in a conformable section containing as many independent lines of age-significant information as possible (e.g., diverse fossils, radiometrically dated horizons, magnetostratigraphic and chemostratigraphic horizons, etc.). That way, if the primary guiding criterion does turn out to be diachronous, the golden spike level will still be readily correlatable on the basis of other data already observed in the boundary stratotype section (Cowie et al., 1986, p. 7; Salvador, 1994, p. 91). From this standpoint, the primary guiding criterion for a golden spike can become "tarnished," but the golden spike itself cannot. In this context, I can only agree with Van Couvering's (2000) warning that "care must be taken . . . to avoid associating the GSSP too closely with any one correlation criterion." However, this position had already been made clear in the guidelines of Cowie et al. (1986) and Remane et al. (1996), and was further emphasized by Remane (2000a,b).

Aubry (2000a, p. 472) claimed that "the Hedbergian [read: her own] procedure allows for constant improvement as new information accumulates," and implied that the procedure of the ICS does not, because it results in a "locked system." This claim is refuted by the facts cited in the above discussion. Indeed, contrary to Aubry et al. (1999, p. 135) and Aubry (2000a, p. 472), even a golden spike with a somewhat diachronous primary guiding criterion located in a conformable and (for example) diversely fossiliferous boundary stratotype section would still be far preferable as a globally correlatable boundary than the base of a traditional unconformity-bounded synthem.

Aubry et al. (2000b, p. 206) were understandably concerned about the stability of GSSPs when they stated that "chronostratigraphic units of long standing usage will be subject to redefinition each time a more powerful element of correlation is discovered." It is true that some stratigraphers unfortunately do hold this view, as Aubry (2000a, p. 472) has noted concerning the recent proposed revision of the Plio–Pleistocene boundary GSSP. Indeed, Remane's (1997, p. 4) statement: "If the level of 2.6 Ma, favored by many Quaternary stratigraphers, would improve the correlation potential of the boundary, then a change should be seriously envisaged, but only then" gave additional credence to these concerns (Aubry et al., 1998a,b). Fortunately, however, according to the ICS guidelines (Remane et al., 1996, p. 80), there are only two scenarios under which a GSSP can be changed, and neither involves the discovery of "a more powerful element of correlation." See Remane (2000a, p. 682) for a more complete response to Aubry et al. (2000a) on this point. Finally, although Aubry (2000a, p. 472) stated that "The similarity between the problem that awaits resolution regarding the P/E boundary and the recent dispute concerning the location of the Pliocene/Pleistocene boundary is striking," the unmentioned fundamental difference between them lies in the fact that unlike the P/E boundary, the Pliocene/Pleistocene boundary had already been formally defined by a GSSP (Aguirre and Pasini, 1985) long before the redefinition arguments of Partridge (1997), Suc et al. (1997), and Morrison and Kukla (1998) were presented (see Vai, 1997 and Aubry et al., 1998a for rebuttal).

8. Conclusions

The recent papers of Aubry et al. have certainly been provocative. On the positive side, they have provided a comprehensive review of the Paleocene/Eocene boundary debate, and have also been valuable to the extent that they have made us think more carefully about chronostratigraphic practices in general. It is always good to discuss basic principles, check our assumptions, and see if established views can be defended. These were the goals of my own recent discussions of certain theoretical aspects of this subject (Walsh, 2001, in press). Unfortunately, the arguments of Aubry et al. on the Paleocene/Eocene boundary and on chronostratigraphy in general are irreparably flawed.

Aubry et al. have misused the term "unit stratotype," and have frequently committed the fallacy of equivocation by using the term "stage" for the very different concepts of "synthem" and "(global chronostratigraphic) stage." A unit stratotype is just a very local stratigraphic section (or composite section) of outcrop scale. This concept is fundamentally different from the concept of a *synthem*, which is an unconformity-bounded unit of relatively major geographic scope, generally consisting of several superposed and/or laterally interfingering lithostratigraphic units, such that a complete section of the entire synthem is rarely if ever present in any one local area. Finally, a synthem is not in itself a global chronostratigraphic unit of the formal geologic time scale, such as a Stage. This is the even more abstract set of *all of the existing strata in the world* that were formed during a given Age, such an Age itself being defined by golden spikes. It is important to use the correct terms when referring to these three different concepts in order to avoid confusion.

Aubry et al. have frequently referred to the importance of the Ypresian unit stratotype, but there is no such thing. One may properly refer to the base of the unit stratotype of the Mont Héribu Member of the Ieper Formation, but this horizon is somewhat younger than the oldest part of the Ypresian synthem as generally understood (Willems and Moorkens, 1991). Synthem boundaries are not immutable, so it may be perfectly appropriate to lower the base of the Ypresian *synthem* to include the Zoute Silt and Harwich Formation if the disconformity below these units represents a significantly greater hiatus than the disconformity at the base of the Mont Héribu Member of the Ieper Formation and the Walton Member of the London Clay. Such a revision of the base of the Ypresian *synthem* would have no bearing whatsoever on the definition of the P/E boundary and the beginning/base of the Ypresian Age/Stage.

I am sympathetic to the suggestion of Aubry et al. (1999) that subepochs/subseries (e.g., "Early/Lower Eocene") be formalized as the standard lower-level global geochronologic/chronostratigraphic units for at least some parts of the Phanerozoic time scale. An even better alternative would be to formalize terms such as "Early/Lower Eocene Age/Stage," thereby reserving historical stage names (e.g., "Ypresian") for the European unconformity-bounded synthems of traditional scope.

Aubry et al. (1999) claimed that the "Global Stratotype Section and Point" of the ICS is an unnecessary synonym of the "boundary stratotype" of Hedberg (1976), but this claim is false. A boundary stratotype is a general concept applicable to the definitions of boundaries of several kinds of stratigraphic units, whereas the GSSP refers only to the boundaries of the formal, golden spike-defined Standard Global Geochronologic/Chronostratigraphic units of the geological time scale.

Aubry et al. (1999) claimed that the content of a stage is what determines its boundaries, but this claim is false, confuses synthems with Standard Global Stages, and was rejected by H.D. Hedberg.

Aubry et al. have frequently cited a distorted "base defines boundary" principle to support their claim that a new boundary stratotype for a stage (or higher-ranked chronostratigraphic unit) must always be chosen so as to approximate the age of the traditional base of the younger of two unit stratotypes or synthems. This claim is flatly contradicted by Hedberg (1968, 1976, 1977, Fig. 13) and Salvador (1994, Fig.

14). What the base defines boundary principle really means is that once a boundary stratotype is selected, then if there happens to be an unrecognized disconformity right at the golden spike level, the golden spike is understood to be hammered into the very base of the upper bounding surface of the disconformity.

Aubry et al. (1999) implied that according to Hedberg (1976, Fig. 13), traditional stage unit stratotype boundaries can be adjusted by no more than 200,000–300,000 years when defining new formal age/stage boundaries with GSSPs. However, no basis for this conclusion is evident in Hedberg's (1976) figure or his discussion of it, and such an artificial limit would be completely unworkable when adjusting age/stage boundaries for most of the geologic time scale.

Aubry (2000a) and Aubry et al. (2000b) claimed that Hedberg believed that the definition of a geochronologic/chronostratigraphic boundary should precede its correlation, in the sense that traditional boundaries should be retained or closely approximated, whether they are easily correlatable or not. However, Hedberg actually believed that the global correlatability of a formal geochronologic/chronostratigraphic boundary must play a major role in its selection, as evidenced by numerous statements in Hedberg (1976).

Aubry et al. (2000b) claimed that traditional unconformable "stage" boundaries may be suitable horizons for GSSPs. This position violates one of Hedberg's most fundamental chronostratigraphic principles, because the choice of such a boundary stratotype restricts our means of correlation to the geochronological evidence available on only one side of it.

Geochronologic/chronostratigraphic boundaries are arbitrarily defined in the sense that they are consciously selected through the discretionary process of a democratic vote. However, Aubry et al. (1999) and Aubry (2000a) have proposed that geochronologic/chronostratigraphic boundaries are arbitrary in the sense that they are and should be preordained by the contingent accidents of historical usage, i.e., that they should correspond to traditional unit stratotype or synthem boundaries. But this view is untenable for two reasons. First, the exact boundaries of many unit stratotypes are themselves controversial. Second, many traditional unit stratotypes are unconformity-bounded and/or consist of nonmarine and/or brackish-water facies that do not provide the degree of global correlatability that the modern subdivisions of the geologic time scale require.

Aubry et al. have claimed that GSSPs are inherently unstable in that they are subject to redefinition whenever a more powerful element of correlation is discovered. This fear is justified only in view of the common misunderstanding of the very purpose of the golden spike, which is to locate it in a conformable section containing as many independent lines of geochronologic information as possible. That way, if the primary guiding criterion does turn out to be diachronous, the golden spike will still be readily correlatable on the basis of other data already observed in the boundary stratotype section. Therefore, the discovery of more powerful elements of correlation will have no effect on the position of an already formally defined GSSP.

If the chronostratigraphic philosophy of Aubry et al. were to be taken seriously, it would require the creation of dozens of new Phanerozoic ages/stages of relatively very short duration whenever there was a significant gap between two successive historical stage unit stratotypes. As explicitly stated by Aubry (2000b, p. 686) regarding the P/E boundary problem, "The introduction of a new stage is simply a necessity imposed by the very principles of chronostratigraphy." However, these so-called "principles of chronostratigraphy" are hers, not Hedberg's, because in Figs. 13 and 14 of Hedberg (1976) and Salvador (1994), respectively, no examples of this practice were discussed or illustrated, because they were correctly assumed to be unnecessary. Boundary stratotypes were depicted as being located in a compromise way such that the introduction of new ages/stages of relatively very short duration was not needed.

The claim of Aubry et al. that there are significant differences in the chronostratigraphic philosophy of the International Subcommission on Stratigraphic Classification (Hedberg, 1976; Salvador, 1994), and that of the International Commission on Stratigraphy (Cowie et al., 1986; Remane et al., 1996) cannot be supported. As shown above, Hedberg's own published statements and diagrams prove that he did not share the very narrow chronostratigraphic views of Aubry et al. This is really unimportant in itself, however, because what Hedberg did or did not believe has no logical bearing on the more fundamental question of whether or not the chronostratigraphic philosophy articulated by Aubry et al. can stand on its own merits. It cannot. Their philosophy is easily refuted by virtue of its own internal contradictions and equivocations, by its misunderstandings of the literature, by the fact that it does not serve the needs of the modern geological time scale, and by the fact that its actual application would result in the creation of numerous unnecessary, virtually useless ages/stages of relatively short duration.

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