



# The role of stratotypes in stratigraphy

## Part 3. The Wood Committee, the Berkeley school of North American mammalian stratigraphic paleontology, and the status of provincial golden spikes

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### Abstract

The North American Land Mammal Ages, defined in 1941 by the Wood Committee, provide good examples of the use of strict nominal stratotypes in fixing the names and establishing the minimum durations of provincial biochronologic units. The lithostratigraphic “type sections” of each Age served a name-bearing and exemplary function, not a boundary-defining function. Despite some inconsistencies in their work, the common charge that the Wood Committee defined many of their Ages as lithochrons is a myth. Nevertheless, the potential or actual temporal overlap of some of the original nominal stratotypes of the Ages is a significant problem, and their redefinition is permissible in order to preserve the original intentions of the Wood Committee.

The Berkeley school of North American mammalian stratigraphic paleontology has been the most influential force in theoretical discussions in this field since the 1960s. D.E. Savage retained most of the Oppelian views of R.M. Kleinpell, while M.O. Woodburne and D.R. Prothero departed from that tradition and adopted some of the views of H.D. Hedberg and W.A. Berggren, thus emphasizing single-taxon definitions, “datum-planes,” and boundary stratotype definitions for “stages.” However, recommendations by members of the Berkeley school that we turn our land mammal ages into “formal stages” are ironic because their own writings show they still hold the anti-Hedbergian view that “stages” are flexible biostratigraphic/biochronostratigraphic entities. Nevertheless, in partial agreement with at least some members of the Berkeley school, the transformation of our provincial biochronologic units into golden spike-defined Ages/Stages would defeat their purpose, which is to summarize our evolving knowledge of the true sequence of important paleobiological events in each major fossil group that occurred in a given province.

In principle and in practice, Hedberg’s attempt to restrict the terms Age/Stage to golden spike-defined entities has failed. However, Hedbergians and traditional stratigraphic paleontologists can find common ground if they will admit that aurichronologic and biochronologic units are both legitimate *subcategories* of geochronologic units. If so, Hedberg’s major program would be vindicated in that golden spikes would still be used to define the Phanerozoic Standard Global Geochronologic

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Units, while traditional stratigraphic paleontologists would retain the ability to modify the boundaries of their provincial Ages/Stages as they saw fit, provided that such modifications did not violate the strict nominal stratotypes of those Ages/Stages.

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*Keywords:* land mammal ages; nominal stratotype; golden spike; biostratigraphic unit; biochronologic unit; aurichronologic unit; age/stage

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## 1. Introduction

In the first two parts of this paper, Walsh (2005a,b) discussed the three major functions of stratotypes in stratigraphy (the boundary-defining, example-providing, and name-bearing functions), documented the origin of the Berkeley school of stratigraphic paleontology in the writings of H.G. Schenck and R.M. Kleinpell, and contrasted the views of these men with the sometimes very different views of H.D. Hedberg, whose stratigraphic philosophy is largely incorporated into current major stratigraphic codes and guides (e.g., Hedberg, 1976; North American Commission on Stratigraphic Nomenclature [NACSN], 1983; Salvador, 1994). Walsh (2005b) noted that the treatment of biochronologic units in those documents was superficial, and proposed a scheme for their codification. He suggested that provincial biochronologic units with geographic names need not be defined by golden spikes, and could legitimately be called Ages/Stages as long as they had a designated nominal stratotype or nominal type locality.

Good examples of the use of nominal stratotypes and provincial biochronologic units are provided by the provincial mammal ages for the North American continental Tertiary named by Wood et al. (1941) (subsequently renamed the “North American Land Mammal Ages” [NALMAs] by Savage, 1962). Although heavily criticized, the Wood et al. time scale has proven to be robust, and to varying degrees has influenced the development of similar schemes in South America (Flynn and Swisher, 1995), Europe (Mein, 1975, 1990; Lindsay and Tedford, 1990; Bernor et al., 1996; Sen, 1997), and Asia (Russell and Zhai, 1987; Ting, 1998; Lucas, 2001). Because the nature of the original definition of the NALMAs continues to be widely misunderstood, a clarification of the intention of Wood et al. (1941) will be relevant to mammalian time scales established throughout the world, and will help to further illustrate the concepts of nominal stratotypes discussed by Walsh (2005a).

An important subclass of the Berkeley school of stratigraphic paleontology may be called the Berkeley school of mammalian stratigraphic paleontology. The origin of this school can be traced to several prominent vertebrate paleontologists who graduated from U.C. Berkeley in the late 1940s, 1950s, and 1960s, and who were originally influenced by R.M. Kleinpell. This school has been by far the most influential force in theoretical discussions of mammalian stratigraphic paleontology since the 1960s, and continues to be the most influential force today. Its members are by no means perfectly homogeneous in their views, but on the whole have distinguished themselves mainly in terms of their foundational criticisms of the work of Wood et al. (1941).

The purpose of this paper is to correct the distorted view of Wood et al. (1941) that has been perpetuated by many members of the Berkeley school, to explore the origin and evolution of this school, and to show where its views conflict with current stratigraphic codes and guides. Using these discussions as background, I will then criticize the requirements in these stratigraphic codes and guides that our provincial biochronologic Ages/Stages should be redefined by golden spikes.

## 2. The Wood Committee

### 2.1. Historical background

Wood and Colbert (1938) proposed to the Section of Vertebrate Paleontology of the Paleontological Society that a committee be appointed to develop a provincial time-scale for the North American continental Tertiary, based on fossil mammals. This proposal was quickly accepted (Romer, 1938). The two main purposes of such an independent time scale would be: (1) to enable the use of a nomenclature for intracontinental correlation that would avoid the difficulties involved in trying to apply poorly defined temporal terms based on European marine rocks (i.e.,

the Lyellian epochs and various “stages”) to the continental rocks of North America; and (2) to end the common but logically invalid practice of using lithostratigraphic names and unit terms for time-stratigraphic concepts (e.g., Kleinpell, 1934). Building on previous summaries such as those of Osborn and Matthew (1909), Matthew (1924), Simpson (1933), and Stirton (1936), the report of the Wood Committee was nearing completion by the time of the Pittsburgh meeting of the Section of Vertebrate Paleontology in December 1939 (Simpson, 1940a), a meeting at which G.G. Simpson would present an important paper entitled “Types in modern taxonomy” (published as Simpson, 1940b; see Walsh, 2005a for discussion). The final report of the Wood Committee was published in January 1941 (Wood et al., 1941), by which time the Section of Vertebrate Paleontology of the Paleontological Society had formed its own organization, the Society of Vertebrate Paleontology (Simpson, 1941).

Although Kleinpell (1938) was not cited in the references in Wood et al. (1941), it seems likely that at least one member of the Wood Committee was aware of *Miocene Stratigraphy of California*, namely, Chester Stock of Caltech, where Kleinpell was a guest lecturer in 1939 and 1940 (Arthur, 1985, p. 79; Meyerhoff, 1987). Another member of the Wood Committee was the paleobotanist Ralph W. Chaney of U.C. Berkeley, who was a close colleague of Stock’s (Gray and Axelrod, 1974), and was presumably aware of Kleinpell’s work. Whether any of Kleinpell’s ideas were discussed by the Wood Committee is a question worth pursuing by an interested historiographer with access to the relevant documents and correspondence, but one beyond the scope of this paper.

In contrast to the work of Kleinpell (1938), Wood et al. (1941) necessarily constructed their Mammal Age scheme “from the top down.” Their job was to name, typify, and describe only the most obvious continent-wide faunal changes in the North American Cenozoic record. They could only deal with the changes that stood out above the noise caused by the inevitable regional differences in coeval mammal faunas existing across a large continent. In other words, the work of Wood et al. (1941) was not at the “zone” level, and indeed their Ages were based almost entirely on genera rather than species. Even though they were aware that some faunal subdivisions were already recognizable within several of their Ages, data were still sparse at

that time and the formal recognition and naming of any Age subdivisions were not their task.

Wood et al. (1941) named a total of eighteen mammal ages for the Tertiary of North America, and Savage (1951) named two additional mammal ages (Irvingtonian and Rancholabrean) for the North American Pleistocene. A more detailed treatment of these mammal ages was in preparation by the “second Wood Committee” throughout much of the 1950s (Gregory, 1958), but the project was stalled for various reasons (A.E. Wood, 1975; Wilson, 1975), and had to wait 30 years for completion by others, most of who were by then working under a substantially different paradigm (Woodburne, 1987).

## 2.2. A nomenclatural debate

Although both Kleinpell (1938) and Wood et al. (1941) used geographic names for their ages/stages, a fundamental difference lies in the fact that Kleinpell (1938, p. 91) avoided using lithostratigraphic names as the basis for his time/time-stratigraphic names. This important subject was debated at length by Tomlinson (1940, 1941), Schenck et al. (1941), Hedberg (1941), Eaton (1941), and White (1941), too late for consideration by the Wood Committee. Like Kleinpell, Schenck et al. (1941) and White (1941) objected to using lithostratigraphic names as the basis for time/time-stratigraphic names, warning that such practice would inevitably lead to confusion. This view can be traced all the way back to d’Orbigny (Monty, 1968, p. 691). In contrast, Tomlinson (1940, 1941), Hedberg (1941), and Eaton (1941) did not think this to be a major problem, in that with a vigilant use of the appropriate “class noun,” one’s meaning should always be clear (e.g., Socorro Formation vs. Socorro Stage), such that it would even be unnecessary to use suffixes like “-ian” for the time/time-stratigraphic names.

The nomenclatural approach adopted by Wood et al. (1941, p. 5) was consistent with the prevailing American stratigraphic code (Ashley et al., 1933, Article 24d), which explicitly allowed modifications of lithostratigraphic names to be used for the names of roughly corresponding time units (Wilson, 1975, p. 199). Indeed, one member of the Wood Committee (J.B. Reeside, Jr., of the U.S. Geological Survey) was also a co-author of the 1933 Code. Nevertheless, it is important to correct a common misinterpretation

of the Wood Committee's nomenclature. For example, Lindsay (2003, p. 220) stated that "The name given for each provincial age was intended to be independent of rock stratigraphic terms, but most of the provincial ages were named after rock units." However, Wood et al. (1941) never intended the names of most of their Ages to be independent of their typifying lithostratigraphic units. Rather, their purpose was simply to stop the use of a name like "Wasatch Formation" for the *chronostratigraphic* concept of all strata in North America deposited during the time when mammals typically found in the Wasatch Formation lived. To achieve this, Wood et al. coined the name "Wasatchian Age," which means something very different from the name "Wasatch Formation." Of course, the fact that clear thinkers such as Wood et al. (1941) and H.D. Hedberg had no trouble distinguishing the meaning of these two names did not guarantee that future workers would not confuse them. Indeed, NACSN (1983, Articles 7b, 77b) now recommends that "a stage preferably should have a geographic name not previously used in stratigraphic nomenclature." Kleinpell (1938), Schenck et al. (1941), and White (1941) therefore seem to have been proven to be correct on this point.

### 2.3. Intention

Despite future misunderstandings, the intention of Wood et al. (1941, p. 6) was actually quite clear:

*"The type of each age necessarily belongs to it, and the sequence and approximate scope of the ages are thus intended to be definitely fixed (barring discoveries which should lead to radically different interpretations). However, the ages are not necessarily coextensive with their types, and the precise limits between successive ages are intended to be somewhat flexible and may presumably be modified in the light of later discoveries. Thus the Wasatchian age is more extensive than the known mammalian faunas of the type Wasatch and probably less extensive than the time equivalent of the entire Wasatch group in the type area. The Barstovian age includes units which may be older than any part of the Barstow formation, and are certainly older than any of the faunules now known from the Barstow [italics mine]."*

Given the preceding discussions, it should be clear from this quotation that the type sections designated by Wood et al. (1941) were intended to have a strict name-bearing function (as well as an exemplary function), but *not* a boundary-defining function. Thus, the type sections of Wood et al. (1941) served to prevent subsequent workers from coining unnecessary new names for Cenozoic mammal ages intended to apply to all of North America, established a minimum duration for each Age that future revisers were implicitly asked to respect, and at the time of their designation, generally also served as the best available sections in which to observe the faunas of each age. As such, Lindsay's (2003, p. 221) claim that "the most significant mistake committed by the Wood Committee was the lack of provision for periodic review and updating of terms and concepts" is false. The very fact that the type sections of Wood et al. (1941) were *not* golden spike-defined *unit* stratotypes shows that these authors fully anticipated "periodic review and updating of terms and concepts." Indeed, the second Wood Committee was fully engaged in this activity during most of the 1950s (Gregory, 1958).

### 2.4. Misinterpretation

Unfortunately, the explanation of Wood et al. (1941, p. 6) was also misunderstood by Savage (1962, p. 55), who stated:

"Wood et al. (1941) proposed that their time scale for North American continental Tertiary would have 'purely temporal significance' and would not face the 'continual dilemma between trying to conform to the European standard time scale or using American formation names as time units.' This proposal was not effected. Wood et al. formally proposed that Puercan age be based on the Puerco formation, Torrejonian age on the Torrejon formation, Clarkforkian age on the Clark Fork Member, Wasatchian age on Wasatch group, *et cetera*. In all, fourteen of their eighteen ages were typified by lithostratigraphic units ... The proposal of a Continental age based solely on a locally exposed lithostratigraphic unit is self-vitiating; for if the age is to be identified in all parts of the continent, the criteria for its demonstration should be equally widespread ... So, with

this delusive definition: – *Age-based-on-formation* – the Wood Committee actually rejected its very purpose; because the purpose of the Committee was to demonstrate successive chapters in North American Tertiary history by means of the fossil mammal record [*italics in original, with Savage’s original bracketed remarks deleted*].”

This myth has been perpetuated by numerous authors (Tedford, 1970; Krishtalka et al., 1987; Emry et al., 1987; Tedford et al., 1987; Woodburne, 1987) and has assumed the status of a virtual dogma among some members of the Berkeley school (Prothero and Emry, 2004). However, the italicized passages in the above quotation from Wood et al. (1941, p. 6) clearly indicate that the lithostratigraphic “types” of each age served a *name-bearing* and *exemplary* function rather than a *boundary-defining* function, as emphasized by Wilson (1975, p. 198). Indeed, in several subsequent papers (Evernden et al., 1964; Savage, 1975, 1977), D.E. Savage never repeated his earlier claim that Wood et al. (1941) defined the NALMA boundaries by lithostratigraphic boundaries. Perhaps this was partly a result of his realization that Kleinpell (1938) had also placed most of the boundaries of the type sections of *his* stages at lithostratigraphic contacts (Savage and Russell, 1983, p. 4), and that if he were to be consistent, Savage would also have to place Kleinpell’s stages in quotes (see below).

To show why the “Savageian myth” is just that, consider the following statement of Emry et al. (1987, p. 118):

“The time units (NALMAs) are therefore technically geochrons (i.e., the time values of rock units), and because the rock units on which they were based may be separated by disconformities or unconformities, the time scale does not ‘cover all of Tertiary time.’”

This claim can be partially refuted upon inspection of Wood et al. (1941, Plate 1). In this chart, the base of the Chadron Formation is clearly depicted as being younger than the beginning of the Chadronian Age, and the base of the Barstow Formation is depicted as being younger than the beginning of the Barstovian Age. Wood et al. (1941, Plate 1; p. 7) also attempted to show that some of the type

sections of their Ages had disconformable bases and tops by using wavy lines for their boundaries (e.g., the Arikaree Group). These disconformities were admittedly depicted as being synchronous with the corresponding Age boundaries, but that fact is best explained as the simplest drafting solution available to Wood et al. in the absence of more definite information on the exact ages of some of their type lithostratigraphic units.

Most of the confusion over the intentions of Wood et al. (1941) has undoubtedly stemmed from their use of the ambiguous phrase “based on” when giving the “types” of each age (e.g., “Torrejonian age—new provincial time term, based on the Torrejon Formation”). Many workers have again interpreted such statements to mean that the Torrejonian Age is literally defined by the lithochron of the Torrejon Formation (e.g., Woodburne, 1987, p. 2). However, given the discussion of Wood et al. (1941, p. 6), what was meant by such a statement was simply that their concept of the Torrejonian Age was based on the *fauna* of the Torrejon Formation, and that the Torrejon Formation is the *name-bearer* of the Torrejonian Age. It is perfectly acceptable to say that our concept of a given biochron is “based on” the faunal content of a particular lithostratigraphic unit, without at all implying that the beginning and ending of that biochron are literally defined by the ages of the base and top of that lithostratigraphic unit.

### 2.5. Clarification

Given the above, the function of the “types” of Wood et al. (1941) was in principle the same as those of the Tertiary New Zealand ages/stages of Finlay and Marwick (1940), as discussed by Fleming (1953, p. 101). Carter (1970, pp. 352–353) explained the nature of this kind of type section (strict nominal stratotype of Walsh, 2005a) as follows:

“This [designated fossiliferous formation] then becomes the type formation of the new Age, and the name-bearer; the type formation is central to the concept of the age in the same way that the holotype is related to the concept of the biological species. Hence, it provides an objective reference to the fauna of the Age, but in no way defines the boundaries of the Age in time.”

Curiously, Tedford (1970, pp. 687–688) endorsed the misinterpretation of Wood et al. (1941) by Savage (1962), but then correctly stated (p. 688):

“It is evident from the remarks of various recent authors that the nature of the Mammal “Ages” is widely misunderstood. Despite the reference to a type lithostratigraphic unit, the characterization of the Mammal “Ages” is actually based on the temporal span of a unique suite of mammal genera. These genera are a composite assemblage derived from the type fauna for the age and from many other approximately correlated assemblages drawn from as wide a geographic region as possible, to circumvent the problem of local ecological and zoogeographic facies. The Wood Committee intended to characterize spans of time by means of a suite of coexisting organisms.”

Tedford’s (1970) analysis here is almost identical to the view later expressed by Wilson (1975). Given the above, Wood et al. (1941) echoed Kleinpell’s (1938) own practice in that none of the stratotypes of these workers was intended to be *boundary-defining* stratotypes. It is therefore amusing that Wood et al. (1941) have been repeatedly criticized for placing the boundaries of the type sections of their Ages at lithologic contacts (Prothero, 1995, 1998a; Prothero and Emry, 2004), and Kleinpell (1938) has been praised for properly establishing biostratigraphically defined stages based on formal type sections (Prothero, 2001a, p. 6), when in fact the actual practice of type section designation used in these two studies was virtually identical. Thus, Schenck and Kleinpell (1936, p. 218) defined the base of the type section of the Refugian Stage to correspond to a lithostratigraphic boundary (the base of the Gaviota Formation) and not to a documented biostratigraphic boundary. Kleinpell (1938, p. 108; 1980, p. 17) defined the base of the type section of the Zemorrian Stage to correspond to the base of the Temblor Formation. Kleinpell (1938, p. 121) defined the base of the type section of the Luisian Stage to correspond to the base of the Monterey Formation, and Kleinpell (1938, p. 127) defined the base of the type section of the Mohnian Stage to correspond to the unconformable base of the Modelo Formation. Was Kleinpell (1938) therefore guilty of concocting an unacceptable

“hybrid of lithostratigraphy and biochronology,” the same charge that Prothero and Whittlesey (1998, p. 39) and Prothero and Emry (2004, p. 163, 165) made against Wood et al. (1941)? Of course not. Like Wood et al. (1941), Kleinpell (1938) was defining exemplary stratotypes for *biochronologic/biochronostratigraphic units* (Walsh, 2005a).

Another point requiring clarification concerns the following statement of Wood et al. (1941, p. 6)

“The same terms [i.e., the mammal age names] can also serve for the corresponding stages, *sensu stricto* (Renevier, 1901, p. 196), if any one wishes to use them in that sense; however, the committee believes that, unlike widespread marine deposits in which a stage may be a very real entity, **a stage as applied to the more variable continental deposits is usually an abstraction, consisting simply of all the continental deposits dating from a given age** [boldface mine].”

This explicitly chronostratigraphic conception of a stage was illustrated by Walsh (2001, Fig. 1), although Wood et al. of course did not define the boundaries of their Ages by golden spikes. However, Cione and Tonni (1995, p. 143) stated:

“There seems to be confusion between physical and conceptual levels in Wood et al.’s statement (see Bunge, 1976). A chronostratigraphic unit is always a conceptual construct, an artifact. A stage, for instance, is a class (in the conceptual level; i.e., a concept) that refers to all the strata (in the physical level, i.e., real things) that are supposed to be deposited during a time span. Notwithstanding that the rocks are real things, a stage is not a “real entity” and the referred rocks that were deposited during a time span are not necessarily interrelated in some form in nature...”

Cione and Tonni (1995) are correct that, being a chronostratigraphic unit, a stage is indeed a class (Walsh, 2001). However, Wood et al. (1941) were not being inconsistent when they suggested that certain marine deposits called “stages” might be very real entities. As usual, the problem is entirely semantic (Walsh et al., 2004, p. 205–207). At the time of the Wood Committee report, the term “stage” was often used for the concept of what is now called an unconformity-bounded unit (e.g., a synthem; Chang,

1975; Salvador, 1994), or for a “natural” depositional sequence of sedimentation resulting from a single marine transgression and regression (e.g., Van Wagoner et al., 1988). This latter, mainly European view of a stage was exemplified in the influential work of Gignoux (1955; first published in 1925), and can be traced all the way back to d’Orbigny (Monty, 1968; Aubry et al., 1999, p. 103). This usage persists among many workers, but conflicts with the modern establishment of the stage as a member of the chronostratigraphic hierarchy (Hedberg, 1976; NACSN, 1983; Salvador, 1994; Walsh et al., 2004).

## 2.6. To quote or not to quote

Savage (1962, p. 54) redesignated the provincial ages of Wood et al. (1941) as the North American Land-Mammal “Ages” (NALMAs). Savage (1962) insisted that several specific things had to be accomplished in order for something to be called a formal Age/Stage, and that Wood et al. (1941) had not done those things—thus, quotation marks should be placed around the word “Age.” Almost all subsequent workers have followed this convention, including Walsh (1998a). Curiously, however, this convention was not employed by Evernden et al. (1964), Savage (1975, 1977), and Savage and Russell (1983). Indeed, if current stratigraphic codes and guides are strictly adhered to, the Ages/Stages of Kleinpell (1938) would also have to be placed in quotes, because even though Kleinpell designated stratotypes for his stages, they were not *unit* stratotypes or *boundary* stratotypes, and so his ages/stages are not “true” ages/stages sensu Hedberg (1976) and NACSN (1983, Article 80). This point also seems to have been recognized by Berggren (2000), who placed Kleinpell’s stage names within single quotes.

Walsh (2005b) discussed this problem and concluded that legitimate geochronologic/chronostratigraphic units (including biochronologic/biochronostratigraphic units) with the rank of Age/Stage can be given geographic names and defined by different methods. In any paper dealing with Ages/Stages, we should specify whether each entity is a rigid, golden spike-defined aurichronologic unit or a flexible, paleobiological event-defined biochronologic unit, and then use the unmodified terms Age/Stage for the rest of the paper. The important

thing is to explain exactly how each geochronologic unit under discussion is defined, and exactly what functions its stratotype is intended to perform.

## 2.7. The strict nominal stratotypes of Wood et al. (1941) in theory and practice

Although it is clear that Wood et al. (1941) did not literally define the boundaries of the NALMAs by the ages of the boundaries of the typifying lithostratigraphic units, how were their strict nominal stratotypes actually supposed to work in theory? In addition to establishing the priority of a name, a strict nominal stratotype also brings a certain amount of stability to our concept of a given biochron by limiting the options for adjusting the temporal boundaries of that biochron (Wood et al., 1941, p. 6). For example (and excluding consideration of the preceding and succeeding NALMAs for the moment), if the “fossiliferous tuff member” of the Barstow Formation (now the upper member of the Barstow Formation; Woodburne et al., 1990) is designated as the strict nominal stratotype for the Barstovian NALMA (Wood et al., 1941; Tedford et al., 1987, p. 187), then, in principle, anyone can define the beginning and ending of this biochron in any way that they please, as long as all of the upper member is still included in the Barstovian. If for some reason we wished to define, say, the ending of the Barstovian in such a way that would exclude the upper part of the upper member from this Age, such a definition would violate name-typology—it would be analogous to cutting off a piece of a holotype specimen because some morphological feature was inconvenient to our preferred revised diagnosis of the species. This interpretation is clearly mandated by the statement of Wood et al. (1941, p. 6) that “The type of each age *necessarily belongs to it*... [italics mine].”

In practice, of course, any revised characterization of the boundaries of one NALMA must also take into account the nominal stratotypes of the preceding and succeeding NALMAs, so as not to violate the name-typology of *those* biochrons. For example, the nominal stratotype of the Hemingfordian NALMA (immediately preceding the Barstovian) was given by Wood et al. (1941, p. 12) as “the Hemingford group, including the Marsland and, especially, the limited or lower Sheep Creek fauna...” (the latter being of late

Hemingfordian age; see [Tedford et al., 1987](#), p. 186). Thus, in order to respect name-typology, any adjustment of the Hemingfordian/Barstovian (H/B) boundary must situate this boundary so as to be younger than the top of the stratigraphic interval yielding the lower Sheep Creek fauna ([McKenna, 1965](#), p. 16), and older than the base of the upper member of the Barstow Formation.

Given the above facts, and if the reader can tolerate a few more necessary details, it will become clear how an adherence to the principle of strict name-typology can help to invalidate unnecessarily drastic redefinitions of traditional biochron boundaries. [Lindsay \(1995\)](#) decided to lower the prevailing H/B boundary level in the Barstow Formation to correspond to the lowest observed occurrence in this unit of the rodent *Copemys*, which he estimated to correlate with the beginning of polarity chron C5Cn.2n based on the magnetostratigraphy of [MacFadden et al. \(1990\)](#). The revised calibration of the age of the beginning of polarity chron C5Cn.2n is 16.5 Ma ([Cande and Kent, 1995](#), Table 2). However, according to [Tedford et al. \(2004, p. 220\)](#), a new Ar–Ar date of 16.4 Ma has been obtained on the Sheep Creek Tuff, which approximates the top of the stratigraphic interval yielding the lower Sheep Creek fauna ([Tedford et al., 2004](#), Fig. 6.2). Taken at face value, therefore, these facts indicate that [Lindsay's \(1995\)](#) proposed H/B boundary (now estimated at 16.5 Ma) is actually 0.1 m.y. older than the top of stratigraphic interval (16.4 Ma) serving as the strict nominal stratotype of the Hemingfordian NALMA. In other words, [Lindsay's \(1995\)](#) definition of the H/B boundary apparently violates Hemingfordian name-typology; thus [Tedford et al. \(2004, p. 215\)](#) were justified in rejecting it.

### 3. Problems with the definitions of [Wood et al. \(1941\)](#)

Despite my general defense of [Wood et al. \(1941\)](#), some of their “types” are nevertheless highly problematical. Because they were usually specified to be entire formations rather than specific stratigraphic sections, different interpretations could be made by various authors as to exactly what rocks and faunas were and were not intended by [Wood et al. \(1941\)](#) to be included in those “types.” Examples of the confusion

resulting from these ambiguous original definitions have been discussed by [McKenna \(1965\)](#), [Tedford et al. \(1987\)](#), and [Hunt \(2002\)](#). More specific problems are addressed below.

#### 3.1. *The problem of rocks vs. faunas*

Some of the definitions of [Wood et al. \(1941\)](#) were ambiguous because they simultaneously used both a lithostratigraphic unit and the known fauna from that lithostratigraphic unit as the “types” of a given Age. This ambiguity is seen in the definition of the Uintan NALMA, which was “based on the Uinta formation of northern Utah, i.e., the time of deposition of Uinta A–C, with its included faunas” ([Wood et al., 1941](#), p. 10). But Uinta A is now known to be barren ([Robinson et al., 2004](#)), so is the name-bearing type of the Uintan NALMA really Uinta A–C, or just that part of the Uinta Formation that has yielded mammalian faunas, i.e., Uinta B+C?

Similarly, [Wood et al. \(1941, p. 9\)](#) stated that “The Wasatchian covers the time during which the faunas of the Sand Coulee, Gray Bull, Lysite, and Lost Cabin were deposited.” But, what if the faunas of the Sand Coulee do not occur in the basal part of this unit, and the faunas of the Lost Cabin Member do not occur in the uppermost part of this member? Then, the “type” of the Wasatchian based on known faunas would be different from the “type” of the Wasatchian based on lithostratigraphic boundaries. This tension is also present in the original definitions of the Bridgerian and Barstovian Ages, and can lead to legitimate disagreement on just exactly what the name-bearing type of a given Age really is (cf. [Tedford, 1970](#), p. 688).

#### 3.2. *The problem of conformably superposed type sections*

Although I have disputed the general validity of the “Savageian myth” in Sections 2.3–2.5, the Wood Committee did inadvertently define some of their Age boundaries by lithostratigraphic boundaries. For example, the “types” of the Orellan and succeeding Whitneyan NALMAs were respectively given as the Orella and Whitney members of the Brule Formation, northwestern Nebraska ([Wood et al., 1941](#), p. 11). However, these two members occur in direct and

apparently conformable superposition with one another (Wood et al., 1941, Plate 1; Emry et al., 1987, pp. 120, 143), such that even if these members were intended to be strict nominal stratotypes, no adjustment of the mutual boundary between the Orellan and Whitneyan Ages would be possible without violating the strict name-typology of one of them. As such, the boundary between the Orellan and Whitneyan in effect became fixed, and functionally equivalent to a strict boundary stratotype. A similar situation is evident with the conformable contact between the Uinta and Duchesne River Formations, respectively used as the types of the Uintan and succeeding Duchesnean Ages by Wood et al. (1941, Plate 1).

### 3.3. *The problem of temporal overlap of type sections*

The most serious problem with the definitions of Wood et al. (1941) involves the potential or actual temporal overlap of some of their type sections. For example, the type of the Bridgerian NALMA was defined as consisting of Bridger A–D, inclusive, and the type of the succeeding Uintan NALMA was defined as consisting of Uinta A–C, inclusive (Wood et al., 1941, p. 10). However, the Bridger and Uinta formations occur in separate depositional basins, and the barren nature of Uinta A makes its age uncertain. So, what would happen if the upper part of Bridger D was actually coeval with the lower part of Uinta A? Then, the upper part of Bridger D and the lower part of Uinta A would necessarily be both Bridgerian and Uintan in age, which was obviously not the intention of Wood et al. (1941). If this situation were to actually materialize (as I predict it will; Walsh, 1996), then line priority should be invoked to assign a Bridgerian age to the relevant interval of Uinta A (Wood et al., 1941, p. 10).

An actual case of name-typological overlap was discussed by Tedford et al. (1987, p. 184–186) regarding the types of the Arikareean and Hemingfordian NALMAs. The stratigraphic problems involved are complex (McKenna, 1965; Tedford et al., 1987; Hunt, 2002) and need not be reviewed here. The important point is that in this particular case of overlap, by emphasizing a modified biochronological definition of the Arikareean/Hemingfordian boundary, the top of the strict nominal stratotype of

the Arikareean was implicitly redefined by Tedford et al. (1987, p. 186) to correspond to the lowermost part of the “Marsland” Formation, rather than to the top of the “Marsland” as originally designated by Wood et al. (1941). Although this action might appear to violate Arikareean name-typology, the lack of a clear definition of “Marsland” by Wood et al. (1941) justified the proposed solution (Hunt, 2002; Tedford et al., 2004).

### 3.4. *Status of the type sections of the Wood Committee*

In view of the problems discussed above, it may be concluded that some of the type sections of the NALMAs designated by Wood et al. (1941) were inconsistent with their stated intentions in several respects. This conclusion has been previously reached by several other workers (e.g., Emry et al., 1987; Tedford et al., 1987), although again mainly not for the reasons claimed by Savage (1962). To what extent these inconsistencies can be attributed to genuine conceptual confusion on the part of the Wood Committee, or to differences in emphasis and writing style among its members, or to the limitations of drafting detail imposed by the complex correlation chart, we will probably never know. Nevertheless, given that these inconsistencies do exist, must we continue to respect the “types” of any of the NALMAs designated by Wood et al. (1941)? Clearly, North American mammalian stratigraphic paleontologists will apply the principle of the strict nominal stratotype to the “types” of Wood et al. (1941) only if those “types” are still thought to have normative weight. This is not a matter that can be legislated, and the ambiguity in some of the original definitions may leave room for legitimate disagreement. However, my point is that despite their inconsistent expression, the principles enunciated by Wood et al. (1941, p. 6) are quite valid, and modern workers should be aware of the stated intentions of that committee when proposing to revise the boundaries of a particular NALMA. The best approach would be to use the “types” of Wood et al. as a kind of null hypothesis. That is, their strict name-bearing function should not be violated unless (“barring discoveries which should lead to radically different interpretations”) there are overwhelming reasons for doing so. As stated by Tedford et al. (1987, p. 186), “Departure from typology and

established usage of the Wood et al. (1941) mammal ages should be avoided whenever possible to reduce confusion.”

#### 4. The Berkeley school of North American mammalian stratigraphic paleontology

##### 4.1. Introduction

Walsh (2005b) discussed the origin of the Berkeley school of stratigraphic paleontology in the writings of H.G. Schenck and R.M. Kleinpell. An important subclass of the Berkeley school may be called the Berkeley school of mammalian stratigraphic paleontology. The origin of this school can be traced to R.A. Stirton (see Camp et al., 1967), the Kleinpell-influenced D.E. Savage, and several other prominent vertebrate paleontologists who graduated from U.C. Berkeley in the late 1950s and 1960s, including M.C. McKenna, W.A. Clemens, M.O. Woodburne, R.H. Tedford, and E.H. Lindsay. Another important member of this school is D.R. Prothero, a former student of M.O. Woodburne, who by virtue of his active professorship at Occidental College (California) and his publication of numerous papers and textbooks, has been training a new generation of stratigraphers in the tenets of the Berkeley school.

The Berkeley school of mammalian stratigraphic paleontology has been by far the most influential force in foundational discussions of North American mammalian stratigraphic paleontology since the 1960s, and continues to be the most influential force today (e.g., Savage and Russell, 1983; Woodburne, 1987, 2004). Remarkably, however, many American paleomammalogists seem to be unaware that such a school even exists, let alone that some of its tenets are contrary to those of the North American Stratigraphic Code and International Stratigraphic Guide. In particular, members of this school often claim to be following these (Hedberg-influenced) codes and guides when in fact they are often following the very different stratigraphic philosophy of R.M. Kleinpell.

Before discussing the writings of specific members, it will be appropriate to note some general idiosyncrasies of the Berkeley school, most of which

can only be understood in the context of the Schenck–Kleinpell influence.

##### 4.2. The veneration of Schenck and Muller (1941)

The paper of Schenck and Muller (1941) is historically important, but its significance has been routinely exaggerated by members of the Berkeley school, who often seem to regard it as the font of all stratigraphic wisdom (e.g., Prothero, 2001a, p. 5). Indeed, Lindsay (2003, p. 214) claimed that Schenck and Muller (1941) “invented a way to resolve correlation of Devonian sediments, whether in England, Germany, or any other global location, with a complete Devonian Period. They did this by proposing a new ‘chronostratigraphic’ hierarchy (System, Series, and Stage) for the stratigraphic representation of equivalent chronologic intervals (Period, Epoch, and Age) of the Devonian.” Lindsay’s claim is quite false. First, the term “chronostratigraphic” was not coined until much later, by H.D. Hedberg (1954, p. 205). Second, the hierarchy used by Schenck and Muller (1941, Table 2) was not “new,” but, as clearly indicated by Schenck and Muller (1941, p. 1422), was identical to that recommended by the 1900 International Geological Congress (Renevier, 1901), with the trivial exception that “phase” was not used by Schenck and Muller (1941) for the time-equivalent of “zone.”

There are numerous other errors of fact and interpretation in Lindsay (2003) which cannot be dealt with here. Most importantly, however, Lindsay (2003) and to my knowledge all other Berkeley school members have neglected to mention the *fundamental flaw* in Schenck and Muller (1941); namely, their failure to separate time-stratigraphic units from biostratigraphic units. This subject was discussed by Hedberg (1941, p. 2206; 1965, p. 456) and Chang (1974), and has rightly been a fundamental part of all recent (Hedberg-influenced) stratigraphic codes and guides (Hedberg, 1976; NACSN, 1983; Salvador, 1994). The reason that most members of the Berkeley school have not recognized this flaw in Schenck and Muller (1941) is that they do not think it is a flaw at all, because along with R.M. Kleinpell, they have often believed that chronostratigraphic units *are a kind of biostratigraphic unit*. This claim will be vindicated in Sections 4.4 and 5.

#### 4.3. The unshakable belief that chronostratigraphic units define geochronologic units

A characteristic feature of the Berkeley school is the continuing confusion among many of its members about the nature of a chronostratigraphic unit (Walsh, 1998a, 2001). Some of this confusion doubtless stems from R.A. Stirton's brief discussion of the concept in his book *Time, Life and Man*, which influenced many U.C. Berkeley vertebrate paleontologists (e.g., Lindsay, 2003, p. 223). Stirton (1959, p. 77) stated: "Time represented as precisely as possible by selected rock sequences, preferably those that are continuously fossiliferous, are time-rock or chronostratigraphic units." This definition is incoherent, but Stirton's discussion indicates that he believed chronostratigraphic units are "selected rock sequences," or presumably what we would now call unit stratotypes. Not surprisingly, Stirton's student D.E. Savage (1975), using a definition similar to that of Stirton (1959), believed that "chronostratigraphic units" necessarily defined the time spans of their corresponding geochronologic units. Despite Walsh's (1998a, p. 162; 2001) demonstration that this belief stems from a series of simple equivocations in language, it has been retained by prominent members of the Berkeley school, including Lindsay (2003, p. 214) and Woodburne (2004, pp. xii, 2, 8). The source of the equivocation remains perfectly clear, and is revealed by Woodburne's (2004, p. 3) statement that chronostratigraphic units are "sections of rock that document intervals of geologic time." This continuing confusion of the concepts of "unit stratotype" and "chronostratigraphic unit" simply defies rational explanation, and I can only refer the reader to Walsh (2003, 2004) and Walsh et al. (2004) for additional arguments against it. *No conceptual progress in stratigraphy is possible until this fallacy is abandoned.*

#### 4.4. The misunderstanding of chronozones

A related idiosyncrasy of the Berkeley school lies in their consistent misunderstanding of the nature of a chronozone (chronostratigraphic zone). Their most common interpretation has been that a chronozone is a kind of *biostratigraphic zone* whose boundaries are

synchronous within the available limits of resolution, as opposed to other biostratigraphic zones whose boundaries are not synchronous within the available limits of resolution and so supposedly are not chronozones. For example, Savage and Russell (1983, p. 3) stated:

"chronozone: (paleontologic chronozone = "OppeIzone") A stratal interval identified by the *congregation* (W. Berry, 1966, 1968) and bounded above and below by "horizons" that are isochronous so far as paleontological record can determine."

This formulation is similar to Kleinpell's (1979, p. 15–16) notion of a Zone. To my knowledge, Kleinpell never used the term chronozone in his own work, but he did regard OpeI zones as time-stratigraphic (biochrono)zones (Kleinpell, 1979, p. 20). Similar misunderstandings of a chronozone by Woodburne (1987, p. 15) and Lindsay (1990, p. 5) were noted by Walsh (1998a,b, p. 163). Unfortunately, Prothero and Tedford (2000, p. 7) have added to the confusion by indicating that chronozones are temporal ranges of taxa, but this is incorrect. Chronozones (note the suffix "-zone") are actually sets of *rock* that were formed during a given chron, or span of time (Salvador, 1994, p. 113; Walsh, 1998a, 2001, 2003).

The essential distinction between biostratigraphic and chronostratigraphic zones is extremely simple, but has nothing to do with the presence or absence of synchronous boundaries (Walsh, 2001). Recall that according to Hedberg (1976, p. 48) and NACSN (1983, Article 49), a biostratigraphic zone is an explicitly *epistemic* category—it consists *only* of the strata in which specified fossils *have been observed by humans* (Walsh, 1998a, p. 155). In contrast, a *chronostratigraphic* zone is much more abstract, as it consists of *all* strata in a specified geographic area that were deposited during a given chron (Walsh, 2001). As such, a chronozone will necessarily include vast amounts of strata that do *not* contain observed fossils diagnostic of the purely biostratigraphic zone on which it might be based, and so such a biostratigraphic zone *cannot itself* also be a chronozone (Hedberg, 1965, Fig. 1; Johnson, 1981; Salvador, 1994, Fig. 13; p. 102; Walsh, 1998a, Fig. 8).

## 5. Prominent members of the Berkeley school

While [Tedford's \(1970\)](#) review of North American mammalian stratigraphic paleontology remains invaluable, I will restrict my discussion here to the work of D.E. Savage, M.O. Woodburne, and D.R. Prothero, the three members of the Berkeley school of mammalian stratigraphic paleontology whose writings have been most influential in shaping current practice.

### 5.1. Donald E. Savage

Donald E. Savage (1917–1999) was a student of R.A. Stirton's at U.C. Berkeley, earning his PhD there in 1949 ([Savage, 1951](#), p. 216; [Sanders, 1999](#)). His thesis on the late Cenozoic vertebrates of the San Francisco Bay area was published in 1951, wherein he named the Irvingtonian and RanchoLabrean Land Mammal Ages ([Savage, 1951](#)). Later, [Savage \(1955\)](#) published a paper in which he named the Montediablan and Cerrotejonian mammalian stages of California. R.H. Tedford (pers. comm., 2003) has pointed out that [Savage's \(1955\)](#) paper was written in the context of the then-ongoing revision of the [Wood et al. \(1941\)](#) time scale. According to Tedford, [Savage \(1955\)](#) was attempting to demonstrate to the second Wood Committee how the North American Land Mammal Ages could be redefined in Kleinpellian terms, using (supposedly) rigorous biostratigraphy and properly designated type sections. Savage's approach agreed with Kleinpell's in four major ways: (1) in the need to designate type sections for stages, which nevertheless did not define the stage boundaries; (2) in the use of the overlapping ranges of many species to characterize his stages; (3) in the consequent rejection of the "datum plane" concept; and (4) in the belief that our standard global geochronologic units need not be precisely defined.

Like Kleinpell, [Savage \(1955, p. 10–11\)](#) emphasized the need to designate "type sections" for stages:

"A (mammalian) stage, as the term is used in this text, is best typified in a locality wherein the type lithology bears a relatively complete mammalian assemblage and exhibits useful and demonstrable stratigraphic relationships to contiguous stages. The stages here described are considered to be the stratal occurrence and representation of significant phyletic change, a

peculiar and particular interval in the evolution of land mammals . . . An attempt is made to characterize the stages by mutual overlap of vertical range of two or more species (joint occurrence control) correlative with [Kleinpell's \(1938\)](#) interpretation and application of "Opellian" [sic] stratigraphy . . . The physical requirements for the type of a (mammalian) stage are very stringent. Two or more mammalian assemblages must be present; they must be in successive superpositional arrangement; and they must be in a restricted columnar section of sedimentary rocks."

[Savage \(1955\)](#) was clearly using the term "stage" in its traditional complex biostratigraphic/biochronologic/biostratigraphic sense, as [Savage \(1955, p. 11\)](#) soon made clear:

"The stratigraphy commission (Hedberg et al. [[American Commission on Stratigraphic Nomenclature, 1952](#)], p. 1634) has recommended the definition of boundaries for stages. This is a convenience, but it may impart a false sense of precision. I do not wish to define the boundary of a (mammalian) stage on the basis of a local change in lithology; for actually the mammals in the stratum make the best boundary—a boundary for the uncalibrated sequence between this and the next biostratigraphically controlled stratum."

In other words, Savage did not believe that the ages of the base and top of the type section should rigorously define the stage boundaries, but were merely used to constrain the transitional interval between successive stages. Thus, [Tedford's \(1970, Fig. 7\)](#) depiction of [Savage's \(1955\)](#) Cerrotejonian and Montediablan Stages clearly shows a temporal gap ("uncalibrated sequence") between them, and as noted by [Woodburne \(1987, p. 18\)](#), "as yet, none of their boundaries have been defined." Like Kleinpell, [Savage \(1955\)](#) viewed the type section of a stage as an *exemplary* stratotype, *not* as a *unit* stratotype. This conclusion is further supported by the fact that [Savage and Curtis \(1970\)](#) referred to the European Villafranchian Land Mammal Age as a true Age/Stage (because it had a formally designated "stratotype"), even though they actually used the Villafranchian as a *biochronologic unit*, because they recognized that as conceptually defined by the immigration into Europe of *Equus*, *Elephas*, and *Bos*, the beginning of the Villafranchian was older than the base of the Villa-

franchian stratotype. As such, the Villafranchian stratotype is clearly not a boundary-defining stratotype, and therefore the Villafranchian is *not* an Age/Stage sensu Hedberg (1976) and NACSN (1983), even if it is an Age/Stage in the sense of Savage and Kleinpell.

Savage's use of the terms "Age/Stage" for biochronologic/biostratigraphic units is also demonstrated by the fact that Savage (1977) recognized an allegedly formal Age/Stage (Wasatchian) without bothering to explicitly define the boundaries of that age/stage in any specific stratigraphic section. Again, Hedberg (1976) would have regarded this step as the most important of all when defining an Age/Stage. Therefore, contra Woodburne (1987, 2004) and Prothero and Tedford (2000), Savage's Cerrotejonian, Montediablan, and Wasatchian "Stages," although they are Stages in Kleinpell's sense, are *not* stages according to Hedberg (1976) and NACSN (1983).

By using the overlapping ranges of many taxa to characterize the Wasatchian Stage, Savage (1977, p. 439) believed that the boundaries of this Stage were inherently fuzzy:

"It is axiomatic to remind ourselves, also, that these respective lowest and highest of genera do not demark planes; the boundaries thus defined are most likely to be fuzzy and must be picked probably in a paleostratigraphic interval of disconcerting thickness!"

As a corollary, Savage rejected the "datum plane" approach, whereby a single distinctive, common taxon is chosen to "rigorously" define a zone or stage boundary. Savage and Russell (1983, p. 4) concluded:

"Although examples such as the "*Hipparion* datum," the "first appearance of the Rodentia datum," the "*Orbulina* global datum", the Coderet niveau repère," FAD, and LAD are in the public eye and used frequently in this decade, we believe that these concepts overreach the real resolution inherent to the paleontological discipline in chronostratigraphy and geochronology."

Savage's views on this subject were identical to those of Kleinpell (1979, p. 16–17). Savage (1975, p. 18) also opposed Hedberg and shared with Kleinpell the view that "we leave the erathems–eras,

systems–periods, and series–epochs as the vaguely bounded bulk units that we inherited..." This view was already criticized by Walsh (2005b) in regard to Kleinpell's philosophy.

Clearly, D.E. Savage was a Kleinpellian through and through. The most important point about his philosophy of mammalian stratigraphic paleontology is that his "stage" concept was of the traditional, flexible, biostratigraphic/biochronostratigraphic variety. Contra Hedberg, Savage did not believe that a stage must be rigorously defined in terms of unit- or boundary stratotypes in order to be valid. Instead, Savage believed that a stage could be valid only if taxonomic elements of the sub- and superjacent stages could be documented to occur in superposition. Thus, the main reason that Savage (1962) placed quotation marks around the "Ages" of Wood et al. (1941) was because those authors had not documented the biostratigraphic transition between their "Ages" in specified sections. This pre-Hedbergian, flexible view of what a "stage" is has persisted in some form among virtually all members of the Berkeley school.

## 5.2. Michael O. Woodburne

Michael O. Woodburne was a student of R.A. Stirton at U.C. Berkeley, obtaining his PhD there in 1966 (Woodburne, 1966). A major cladogenetic event within the Berkeley school occurred when Woodburne (1977) departed from the Kleinpell–Savage philosophy and recommended single-taxon definitions for mammalian paleostratigraphic units. Woodburne (1977) was impressed by the First Appearance Datum concept of Berggren and Van Couvering (1974), as well as the use of the lowest occurrence (LO) of a single taxon in a boundary stratotype section to serve as the guiding criterion for a Standard Global Geochronologic/Chronostratigraphic boundary (for example, the Silurian/Devonian boundary; McLaren, 1977; Murphy, 1977).

Woodburne's (1977, 1987, 1996) advocacy of the use of single-taxon and boundary stratotype definitions for zones, biochrons, and ages/stages has profoundly influenced North American (and recently, South American) mammalian stratigraphic paleontology for the last 27 years. This influence is exemplified by the single-taxon and/or boundary stratotype based definitions proposed by Rose (1981), Evander (1986),

Flynn (1986), Archibald et al. (1987), Lindsay (1995), Cione and Tonni (1995), Tedford et al. (1996), Madden et al. (1997), Prothero and Whittlesey (1998), Lofgren et al. (2004), and Prothero and Emry (2004).

Criticisms of the single-taxon/boundary stratotype approach in mammalian stratigraphic paleontology have been made from different perspectives by Lucas (1992), Alroy (1998), and Walsh (1998a), and need not be repeated here. What is important for historical reasons is that in his belief that paleostratigraphic boundaries should be defined in terms of single taxa (and preferably in a boundary stratotype section), Woodburne (1977, 1987, 1996) departed from the Kleinpell–Savage tradition and adopted a position conceptually much closer to that of H.D. Hedberg and certain advocates of the “datum plane” concept (e.g., Berggren and Van Couvering, 1974). In one very important respect, however, Woodburne’s philosophy differs fundamentally from Hedberg’s and agrees with that of Kleinpell. While Hedberg always believed that a boundary stratotype (that is, a golden spike) for a stage boundary should be permanent, Woodburne (electronic comm., 1998) believes that if a lower fossil of the guiding criterion of a mammalian age/stage should be found in the boundary stratotype section, or if an older fossil of the guiding criterion should be found elsewhere on the continent, then we should be able to pull out the golden spike and hammer it in to the level corresponding to the new fossil. The same view was previously expressed by Erben (1972, p. 94–95):

“The opponents against the concept of stratotypes or boundary types are scared particularly by the slogan of the “golden spike” and by the strict demand that such types, once established, should remain unchanged forever ... If for some serious reasons (e.g., the discovery of the index fossil below the boundary) it should appear necessary to modify slightly this boundary then this should be possible.”

However, this view blurs the crucial distinction between permanent, golden spike-defined *aurichronologic* units and traditional, flexible *biochronologic* units (Murphy, 1994; Walsh, 1998a, 2001, 2005a,b), a distinction which Hedberg spent much of his career trying to explain. Why go through all the time and

effort involved in deciding upon the best taxon to use, finding the most complete and fossiliferous section in which this criterion is expressed, and then hammering in the golden spike amid cheers and celebrations, if this golden spike can be immediately pulled out and re-driven if an older fossil of our guiding criterion is discovered somewhere else? If this view is accepted, then flexible biochronologic units would clearly be sufficient for all of our needs, and golden spikes would be a pointless waste of time.

That Woodburne still does not fully appreciate Hedberg’s concept of an Age/Stage is revealed by Woodburne’s (2004, p. 16L) disagreement with Walsh’s (1998a) argument that the Wasatchian and Clarkforkian NALMAs as respectively defined by Savage (1977) and Rose (1981) were not true stages in the sense of Hedberg (1976) and NACSN (1983). Savage’s (1977) work was already discussed above. Rose (1981) did not define the Clarkforkian as a formal Age/Stage sensu Hedberg (1976) and NACSN (1983) because, although Rose (1981) did depict the *apparent* boundary between the Tiffanian and Clarkforkian in a specific section as of 1981, he *defined* this boundary *biochronologically*, by the first appearance (immigration) of the Rodentia. Thus, if a new rodent fossil was found that was older than the apparent boundary level as of 1981, then that now-obsolete “boundary” would be demonstrated to be younger than the true beginning of the Clarkforkian. In other words, Rose’s (1981) intent was not to drive in a golden spike to define the beginning of the Clarkforkian, and this fact was explicitly acknowledged by Archibald et al. (1987, p. 62L). So even though Rose (1981) called the Clarkforkian an Age/Stage, it was still not an Age/Stage sensu Hedberg (1976) and NACSN (1983).

Given his misunderstandings of the status of the Wasatchian and Clarkforkian, Woodburne (2004) reveals that he is still thinking in terms of the flexible Kleinpellian stage concept that he learned at U.C. Berkeley, and does not appreciate the difference between it and the very different stage concept of Hedberg (1976) and NACSN (1983). Numerous other errors of fact and interpretation in Woodburne (2004) cannot be corrected here. Nevertheless, I have always believed that Hedberg’s concept of an Age/Stage is too narrow (Walsh, 1998a, 2001), and that is why I have proposed that provincial biochrons with geo-

graphic names may be called Ages/Stages as long as they have a designated nominal stratotype or nominal type locality (Walsh, 2005b).

### 5.3. Donald R. Prothero

Donald R. Prothero was a student of M.O. Woodburne's at the University of California, Riverside, and later completed his PhD at Columbia University in 1982 (Prothero, 1982). Although Prothero's stratigraphic philosophy is essentially identical to Woodburne's, it is necessary to examine his views in detail because he is the author of several popular textbooks (Prothero, 1991, 1998b; Prothero and Dott, 2004), two major review chapters (Prothero, 1995, 1998a), and several additional papers that have promulgated the tenets of the Berkeley school (Prothero and Whittlesey, 1998; Prothero, 2001a,b). Unfortunately, Prothero's stratigraphic writings are anachronistic in several respects, and his understanding of several key publications is seriously flawed. Therefore, in order to prevent unnecessary confusion, the idiosyncrasies of his views must be pointed out.

In the biostratigraphy chapter of his textbook *Bringing Fossils to Life*, Prothero (1998b, p. 173) stated:

“As stratigraphers have become more careful in documenting ranges and describing type sections, inevitable disputes over boundaries have arisen. The type section or areas of two successive *biostratigraphic* units are usually in two separated areas, and there may be no overlap. Often, neither section preserves the boundary between the two units, so biostratigraphers must search for a third area where the transition is recorded. Ideally, this section should be as continuous and fossiliferous as possible, with several taxonomic groups to compare.” [italics mine]

Clearly, Prothero is here discussing *chronostratigraphic* units, although he calls them “biostratigraphic units.” Confirmation is provided on the very next page. After mentioning the definitions of the Ordovician/Silurian, Silurian/Devonian, and Devonian/Carboniferous (chronostratigraphic) boundaries, Prothero (1998b, p. 174) then stated: “Many other *biostratigraphic* boundaries are currently under discussion . . . [italics mine].” Such statements would

make Schenck and Muller (1941) proud, and not surprisingly, Prothero's (1991) detailed treatment of the definition of the Silurian/Devonian boundary appears in his chapter on biostratigraphy, not in his chapter on chronostratigraphy (where it clearly belongs).

A further confusion of biostratigraphic and chronostratigraphic units is evident in Prothero and Tedford (2000, p. 1), who stated:

“In this paper, he [Savage, 1955] abandoned the hybrid biostratigraphic–biochronologic scheme of the Wood Committee and defined formal biostratigraphic units, complete with type sections, that could be regarded as true “stages” in the sense of the North American Code of Stratigraphic Nomenclature.”

First, contra Prothero and Tedford (2000), Savage (1955) defined no biostratigraphic units of any kind, either formal or informal. Indeed, Savage (1955, p. 13) admitted that “Very little definite information may be presented in regard to the stratal range of the mammals identifying the Cerrotejonian Stage in this [the type!] district.” Second, according to NACSN (1983, Article 48c) and the writings of H.D. Hedberg upon which most of that Code is based (Hedberg, 1965, 1968, 1976), biostratigraphic units are a completely different category from chronostratigraphic units (such as stages), and a biostratigraphic unit can *never* be regarded as a chronostratigraphic unit, even if a “type section” has been formally designated for that biostratigraphic unit. Third, as noted above, Savage (1955) never rigorously defined the boundaries of his stages in a specified section, and so those stages, by definition, are simply *not* stages in the sense of Hedberg (1976) and NACSN (1983).

A further misunderstanding of the nature of chronostratigraphic units is exemplified by Prothero (1998a, p. 9), who stated:

“As Tedford (1970) and Woodburne (1977, 1987) have pointed out, these units were not true time-stratigraphic ages, which must be based on biostratigraphic zones and stages (according to Western stratigraphic codes such as the 1983 North American Code of Stratigraphic Nomenclature). Because they were not true stratigraphic ages, the North American

land mammal “ages” should properly be put in quotes in all publications. Instead, the Wood Committee’s methods resemble a system called “biochronology,” first proposed by H.S. Williams (1901), which attempts to reconstruct the sequence of occurrences of taxa without necessarily documenting every event in a stratigraphic section. Demonstration of stratigraphic superposition was noted when available, but detailed work in the European biostratigraphic tradition was not considered essential to their “provisional” system.”

First, Prothero’s (1998a) use of the terms “time-stratigraphic ages” and “true stratigraphic ages” is improper. The correct terms would be either “time-stratigraphic *stages*” or “*geochronologic* ages” (NACSN, 1983, Article 74). Second, Prothero (1998a) stated that Williams (1901) proposed a “system” called “biochronology” that attempted “to reconstruct the sequence of occurrences of taxa without necessarily documenting every event in a stratigraphic section.” However, Williams (1901) proposed no such system, and although he did coin the term “biochron,” he did not use the term “biochronology.” Third, contra Prothero (1998a), nowhere in the North American Stratigraphic Code does it say that geochronologic/chronostratigraphic units “must” be based on biostratigraphic units. If R.M. Kleinpell, O.H. Schindewolf, or J.M. Hancock had written the Code, that claim might have turned out to be true, but they did not. According to NACSN (1983, Article 66c), any age-significant feature in a given section may serve as the guiding criterion for the boundary of a geochronologic/chronostratigraphic unit. Thus, although biostratigraphic data will most often provide the primary guiding criteria for the definition of such units, a variety of other means have been used for this purpose (e.g., Walsh et al., 2004).

Berggren (2000) observed the difficulty which many members of the Berkeley school have had in distinguishing biostratigraphic from chronostratigraphic units. In fairness to workers in the pre-Hedberg (1976) era such as Kleinpell (1938) and Schenck and Muller (1941), a clear distinction between biostratigraphic, biochronologic, and biochronostratigraphic units was generally not reflected in terminology, and all of these concepts were usually

considered to be “biostratigraphic.” However, modern workers who continue to confuse these categories have no excuse for doing so, and it must be emphasized that the phrase “biostratigraphic stage” (e.g., Prothero, 1998a, p. 14) is just as much of a non sequitur as is “magnetostratigraphic formation” or “lithostratigraphic system.”

Prothero (1995, p. 315; 1998a, p. 14) has scolded his colleagues for not obeying the North American Stratigraphic Code in their failure to designate type sections for their “biostratigraphic units.” In his own work, however, Prothero has failed to indicate exactly what functions his own type sections are supposed to perform. Prothero and Whittlesey (1998, p. 55) stated:

“We recommend that the beginning of the Orellan be defined by the FAD of *Hypertragulus calcaratus*, and characterized by the rest of the taxa listed above . . . In compliance with the North American Code of Stratigraphic Nomenclature, we formally designate the earliest Orellan as the *Hypertragulus calcaratus* Interval Zone, using the concept of interval zones that are based on the successive first occurrences of index taxa as described in the Code (NACSN, 1983, Fig. 4C1). This type of zone was also used by Archibald et al. (1987) to zone the Paleocene. We designate the type section for the *Hypertragulus calcaratus* Interval Zone as the strata 7 to 17 m (20–50 ft) above the 5 tuff in the Reno Ranch East section . . . The top of this zone is marked by the FAD of the name-bearer of the overlying zone, *Miniochoerus affinis*.”

There are numerous problems with this passage. First, Prothero and Whittlesey (1998, p. 55) misused the term “index taxon” (see Wood et al. 1941, p. 8; Tedford, 1970, p. 687; Walsh, 1998b). By definition, *Hypertragulus calcaratus* is *not* an index taxon for the *H. calcaratus* Interval Zone, because as shown by Prothero and Whittlesey (1998, Fig. 6), this species is not restricted to this zone, as it also occurs in the overlying *Miniochoerus affinis* and *M. gracilis* Interval Zones. Instead, *H. calcaratus* is one of the *defining* taxa of the *H. calcaratus* Interval Zone (NACSN, 1983, Article 54d), as well as the *nominal* taxon for this Zone. Second, Prothero and Whittlesey (1998) did not define the highly ambiguous term “FAD.” Prothero (pers. comm., 2000) explained that this term was intended to denote the lowest known

fossil of a taxon in a given section (see also Prothero, 1998b, p. 420; equivalent in meaning to the LO<sub>k</sub> of Walsh, 2000). This usage of “FAD” is very different from most other workers such as Berggren and Van Couvering (1974) and Woodburne (1996) (see Walsh, 1998a). Third, in designating the earliest Orellan (a time unit) as the *H. calcaratus* Interval Zone (a biostratigraphic term), Prothero and Whittlesey (1998) are using the term “Zone” for a temporal concept, so they are in clear violation of NACSN (1983), not in compliance with it (see discussion in Ziegler and Sandberg, 1994). Recall that the NACSN (1983) does not formally recognize biochronologic/biochronostratigraphic units, which is why Walsh (2005b) has made a proposal to do just that.

The designation of a “type section” for the *H. calcaratus* Interval Zone by Prothero and Whittlesey (1998) raises several additional questions, because Prothero and Whittlesey (1998, p. 39) stated: “Instead of the hybrid of lithostratigraphy and biochronology created by the Wood Committee (Wood et al., 1941), it is now possible to designate biostratigraphic zones and true chronostratigraphic stages and geochronologic ages.” Do Prothero and Whittlesey (1998) therefore claim to have defined the Orellan as a true (auri)chronostratigraphic stage in the sense of Hedberg (1976) and NACSN (1983), using the lowest observed fossil of *H. calcaratus* (as of 1998) in the Reno Ranch East section as the level of their golden spike? If so, then the beginning of the Orellan Age would be fixed at this level, such that the Chadronian/Orellan boundary would be immune to redefinition from any future discoveries of *H. calcaratus* in North America that might be older than the lowest observed fossil (as of 1998) of this species at Reno Ranch East. Or, despite references to “true chronostratigraphic stages and geochronologic ages,” did Prothero and Whittlesey (1998) only designate a “type section” for the *H. calcaratus* Interval Zone because the North American Stratigraphic Code required it, with no intention of placing a golden spike at the base of this type section?

Prothero and Emry (2004) had every opportunity to clarify the intent of Prothero and Whittlesey (1998), but did not. It is therefore difficult to know which of the above interpretations is correct, because Prothero (2001b, p. 379), in discussing Mallory’s (1959) Ynezian benthic foraminiferal stage, inexplicably stated that “. . . ages do not have to be coextensive

with their type sections . . . [italics added].” This statement brings us back full circle to the identical statement of Wood et al. (1941, p. 6), whose work Prothero (1998a, p. 9) has so often criticized for exactly this reason—namely, that Wood et al. did not define type sections to correspond to rigorously documented biostratigraphic boundaries, and therefore their Land Mammal Ages were not “true chronostratigraphic stages and geochronologic ages” as defined in the Code. But in suddenly reversing himself and joining Wood et al. (1941) in the belief that “ages do not have to be coextensive with their type sections,” Prothero (2001b, p. 379) only reveals his misunderstanding of the Code. According to NACSN (1983), geochronologic ages *most certainly do* have to be coextensive with their type sections, because Article 80 explicitly states that the “beginning and ending [of a geochronologic age] correspond to the base and top of the referent.” Again, this point involves the fundamentally different functions of a “type section” as envisioned by Hedberg (1976) and NACSN (1983) on the one hand (strict unit- and boundary stratotypes) vs. Kleinpell and the Wood Committee on the other (exemplary and nominal stratotypes; Walsh, 2005a,b).

The nebulous and self-contradictory statements of Prothero (2001b) and Prothero and Whittlesey (1998) exclude the possibility of further rational analysis. My conclusion is nevertheless clear. The failure to carefully distinguish between biostratigraphic, biochronologic, and aurichronologic units, together with the failure to understand the different kinds and functions of a “type section,” will inevitably lead to stratigraphic discussions that are incoherent.

#### 5.4. Summary: the main misconception of the Berkeley school, and the lesson to be learned

The views of M.O. Woodburne and D.R. Prothero documented above reveal that the main misconception of these preeminent members of the Berkeley school of mammalian stratigraphic paleontology lies in their desire to “obey the Code” and establish boundary stratotypes in order to define formal ages/stages, but without realizing that the age/stage concept they have in mind is really more akin to the flexible concept of Kleinpell and classical stratigraphic paleontology, and not to the rigid concept of Hedberg and the stratigraphic codes and guides that

he inspired. D.E. Savage's view of the stage was even more similar to Kleinpell's in that he did not believe that the designation of boundary stratotypes was even necessary, and I share this view for provincial biochrons. Given Woodburne's and Prothero's confusion, it seems necessary to emphasize again that Hedberg's view of the function of boundary stratotypes was *unambiguous*, and was expressed with great clarity in virtually every paper he ever wrote on this subject. For example, Hedberg (1973, p. 176) stated:

“Comment: Is not the use of boundary-stratotypes to define a stratigraphic unit expressive of geologic age (chronostratigraphic unit) undesirable, because it fixes or freezes the scope of the unit too strictly? All we need is a general concept of its time scope based on its biostratigraphic character.”

“Response: Such a philosophy may be comforting in its laxness but is exactly responsible for the endless futile argument and confusion about the limits of named stratigraphic units expressing geologic age, which we are trying to avoid. Knowledge of fossil ranges and taxonomic concepts may change, but a stratotype in the rocks is fixed. We need *fixed standards* for the scope of these units, which will *always* mean the same thing to everyone [italics added].”

There can be *no doubt* about Hedberg's view of the function of a boundary stratotype and the meaning of a formal chronostratigraphic unit, and it is Hedberg's concepts that are formalized in current major stratigraphic codes and guides, *not Kleinpell's*.

In the opinion of Prothero (1995, 1998a), Prothero and Whittlesey (1998), and Prothero and Emry (2004), for something to be called a “stage,” it must be defined in terms of constituent biostratigraphic zones. But this is the view of Oppel, Kleinpell, and traditional stratigraphic paleontology; it is *not* the view of Hedberg (1976), NACSN (1983), and Salvador (1994), who reject the position that stages (chronostratigraphic units) can be composed of, or defined in terms of, *inherently unstable biostratigraphic* zones. The difference again being

that if a new fossil of *H. calcaratus* is found that is older than the lowest fossil of *H. calcaratus* (as of 1998) in the Reno Ranch East section, that new fossil locality will by definition belong to the *H. calcaratus* (biostratigraphic) range zone, but it will most certainly *not* belong to the (chronostratigraphic) Orellan Stage, *if* we are using the term “stage” in the sense of Hedberg (1976), NACSN (1983), and Salvador (1994).

The views of many members of the Berkeley school of mammalian stratigraphic paleontology have been shaped by a kind of institutional memory, and the influence of H.G. Schenck and R.M. Kleinpell continues to be felt long after their deaths. As such, many of the fundamental ideas of H.D. Hedberg (arguably the most important stratigrapher of the 20th century) have passed certain members of the Berkeley school almost completely by. That Prothero (2003, p. 3) can claim that “Schenck's most important student was Robert M. Kleinpell” and not even mention H.D. Hedberg proves my point (see Walsh et al., 2004; Walsh, 2005b). Nevertheless, I am in no way claiming that all members of the Berkeley school must now agree with everything that Hedberg has ever written, for I also disagree with Hedberg on several issues (Walsh, 2001, 2003; Section 6). Rather, I am only saying that members of the Berkeley school *must be aware* that their views on the relationship between stages, biostratigraphic units, and the functions of stratotypes are often very different from those of Hedberg and the stratigraphic codes and guides that he inspired, and to bring that fact to the attention of their students.

Despite my criticisms of certain members of the Berkeley school, there are also aspects of their philosophy that are well worth keeping. The most important of these are their advocacy of detailed biostratigraphic work in local sections, of detailed biostratigraphic studies of the transitions between successive NALMAS, and of the development of meaningful temporal subdivisions of each NALMA (e.g., Tedford, 1970; Woodburne, 1987; Prothero and Whittlesey, 1998). Such positions are not unique to the Berkeley school, of course, but have been part of standard practice in stratigraphic paleontology for more than a century. Again, however, contra Woodburne (1987, 2004), Prothero (1995), and

Prothero and Whittlesey (1998), the fulfillment of these goals will *not in itself* automatically turn the NALMAs into formal Ages/Stages in the sense of Hedberg (1976), NACSN (1983), and Salvador (1994).

## 6. The bigger picture: are provincial golden spikes necessary?

### 6.1. The problem of normative weight

My findings regarding the Berkeley school and its notions of “stage” and “biostratigraphic units” would be relatively insignificant were it not for the fact that similar conflicts between classical stratigraphic paleontology and Hedberg’s philosophy continue to occur throughout the world. Given Salvador’s (1994, p. 87) recommendation that “Local or regional chronostratigraphic units should adhere to the same rules established for the units of the Standard Global Chronostratigraphic Scale,” stratigraphic paleontologists need to decide if their traditional, provincial biochronologic/biochronostratigraphic Ages/Stages really should be redefined with golden spikes. This problem has recently been debated by several New Zealand stratigraphers (Carter and Naish, 1999; Beu, 2001; Scott, 2001), and is also relevant to the status of the North American West Coast Cenozoic molluscan stages of Clark and Vokes, 1936 (e.g., Squires, 2003). I now wish to examine this problem in more detail, and will again use examples from North American mammalian stratigraphic paleontology.

Suppose that a Hedberg-oriented stratigraphic paleomammalogist, tired of the unstable biochronologic definitions of the Hemingfordian/Barstovian NALMA boundary, published a paper in which he proposed to permanently define the H/B boundary by means of a golden spike hammered into an exactly specified level of the Barstow Formation. Evander (1986) attempted to do just that. But, to *permanently* define a boundary is a serious matter, and raises some serious questions (Schoch, 1988). Why should any of us necessarily have to follow Evander (1986), or Prothero and Whittlesey (1998), or anyone else on the definition of the boundaries of a provincial stratigraphic unit? What role, if any, does priority play in

maintaining *boundary definitions* as opposed to merely the *names* of the units? Should boundary definitions reached by a committee of “experts” be given more weight than boundary definitions proposed by one or two authors working in isolation? Should journal editors enforce a particular definition of the Hemingfordian/Barstovian boundary, or should they let authors propose alternative definitions if they so desire?

Starting with the Standard Global Geochronologic Units, geoscientists have more or less agreed to let committees organized by the International Commission on Stratigraphy define the boundaries of these units so as to be widely correlatable (Salvador, 1994, p. 30; Remane et al., 1996). These boundaries are then intended to be permanent, and because they were agreed upon by a long process of debate followed by a democratic vote among numerous experts, they are deemed to have normative weight. That is, other geoscientists are expected to use and respect the ratified boundaries and the names of the units for the purposes of global communication. Editors of journals are within their rights to insist that authors follow the formally ratified definitions of, say, the Silurian Period and the Paleocene Epoch, because such definitions are arbitrary and conventional.

But, does the principle of normative weight apply to the boundaries of provincial ages/stages as proposed by one or two workers working in isolation? I do not think so (Schoch, 1988). Getting back to the Hemingfordian/Barstovian example, does the H/B boundary definition of Evander (1986), because he was the first worker to drive in a golden spike for this boundary, compel other workers to use his definition, and are journal editors within their rights to require authors to use that definition? The answer to these questions must be “No,” and not for the mostly procedural reasons given by Schoch (1988; see also Woodburne et al., 1990, p. 475). For even if Evander (1986) had followed all existing provisions of the stratigraphic codes to the letter, the answer would still be “No.” To answer “Yes” to the above questions would be disastrous, because any worker with his own agenda could then go out and formally define the Xian/Yian provincial boundary with a golden spike and rest assured that his definition would be binding on everyone else. Human egos being what they are, all

of our provincial boundaries would then be defined by golden spikes very quickly, but most of them would rightly be resented.

In agreement with this conclusion, Salvador (1994, p. 30) implicitly rejected the normative validity of boundary stratotypes for provincial chronostratigraphic units defined by isolated workers when he stated: “stratotypes of units of only local extent and interest may only require approval from local or national surveys or stratigraphic commissions.” It is unclear, however, what would give, say, the California Division of Mines and Geology, or the U.S. Geological Survey, or the North American Commission on Stratigraphic Nomenclature the time, money, authority, expertise, or inclination to rule on boundary stratotype definitions for hundreds of provincial aurichronologic units.

Clearly, the only way that permanent boundaries for provincial ages/stages might be respected by workers in a given field is if those boundaries were approved in a democratic manner by a committee of experts in that field, preferably holding a variety of different views and philosophies. Unfortunately, as with all human endeavors, we know that in reality the memberships and proceedings of such committees are often influenced by politics. As a result, such committees are capable of ratifying boundary definitions that can be just as questionable as those that might be proposed by any one scientist. But even if we disregard that fact, there would often be serious difficulties involved in even forming such committees. The most important of these is the general lack of funding available to do the enormous amount of field and lab work required to evaluate numerous potential boundary stratotypes for each provincial Age/Stage (R.L. Squires, pers. comm., 2004). It is very doubtful that the relatively academic transformation of our provincial biochronologic Ages/Stages into aurichronologic Ages/Stages would ever have any economic benefits. Indeed, Kleinpell’s (1938) Ages/Stages were never defined by golden spikes, but that did not affect their utility for finding oil (Berry, 1999). Interestingly, however, the Institute of Geological and Nuclear Sciences (New Zealand) is currently involved in a program to define the boundaries of the Cenozoic stages of New Zealand by golden

spikes (Beu, 2001, p. 116), and although I am skeptical of the need for such a program, it will be interesting to see how this provincial experiment in aurichronology turns out.

## 6.2. Provincial aurichronologic units are inappropriate and redundant

In view of the above, while I think it is appropriate to hold our noses and swallow the often distasteful medicine of politics, commissions, and votes for the sake of a stable standard global time scale (Walsh, 2004; Walsh et al., 2004), it would be unnecessary to turn our provincial biochronologic units into aurichronologic units. We just need to accept the fact that Evander (1986) has a right to propose one definition of the Hemingfordian/Barstovian boundary, Woodburne et al. (1990) have the right to propose another definition, and Lindsay (1995) has the right to propose yet another definition, and so on, as long as those definitions do not violate Hemingfordian and Barstovian name-typology (Section 2.7). Just as the morphological “boundaries” of a species as defined by any given worker have no normative weight, boundaries of provincial biochronologic units can have no normative weight, no matter who defines them. Just as the name-bearing function of a type specimen in biology is the sole restriction on the scope of a species name, so should the name-bearing function of a nominal stratotype or nominal type locality be the sole restriction on the scope of provincial biochronologic units (Walsh, 2005a,b).

Golden spikes are not appropriate for redefining the boundaries between provincial biochronologic units because such units would then become immune to the revisions that all biochronologic schemes must constantly undergo if we are attempting to discover the true sequence of paleobiological events that occurred in a given geographic area (Kleinpell, 1979). In addition, such provincial aurichronologic units would be redundant (Walsh, 1998a; J. Alroy, pers. comm., 1998; Van Dam et al., 2001, p. 383), because the Standard Global Ages/Stages of the geological time scale are already being defined by the International Commission on Stratigraphy in order to provide a theory-neutral, stable common language for all Earth scientists (cf. Carter, 1974). Finally, for Cenozoic workers, the geomagnetic polarity time-

scale provides numerous supplementary pigeonholes that can be used for conveniently expressing temporal resolutions finer than many Standard Global Ages, again rendering provincial golden spike-defined ages/stages unnecessary (cf. Scott, 2001).

### 6.3. A terminological solution

Despite my criticisms of the Berkeley school, the writings of its members have helped to convince me that Hedberg's attempt to restrict the terms Age/Stage (and geochronologic/chronostratigraphic units in general) to golden spike-defined entities has failed in practice. This is not necessarily a bad thing, of course. After all, Hedberg's use of Age/Stage only for entities that are defined by golden spikes violated his own warning about the restriction of general terms for narrow meanings (Hedberg, 1976, p. 15). Indeed, the term "stage" was used by Oppelians in a biochronostratigraphic sense long before the golden spike concept was invented (e.g., Arkell, 1933; Hancock, 1977; Walsh et al., 2004). Similarly, the term "geochronologic unit" etymologically means "geologic time unit" and not "golden spike-defined geologic time unit" (Walsh, 2003). Clearly, Hedberg's restricted meaning of "stage" has often been ignored. Many traditional stratigraphic paleontologists continue to call provincial biochronologic/biochronostratigraphic units Ages/Stages, even though they have no intention of defining them by golden spikes.

In view of the above, I say it is time to acknowledge the merit of principled intellectual inertia and accept Hedberg's defeat on this relatively minor point. Strict Hedbergians and traditional stratigraphic paleontologists can still find common ground if they will admit that aurichronologic and biochronologic units are both legitimate *subcategories* of geochronologic units, and that the appropriate lower rank unit terms for geochronologic and chronostratigraphic units *in general* are Age and Stage (cf. Scott, 1960; Walsh, 2001). Hedberg's major positions would still be vindicated in that golden spikes would continue to be used to define the Phanerozoic Standard Global Geochronologic Units, and a sharp separation of empirical biostratigraphic units from all subcategories of chronostratigraphic units would always be necessary. I have seen

no compelling argument against this view, and so have proposed that provincial biochronologic/biochronostratigraphic units with geographic names can logically be called Ages/Stages, as long as they have a designated nominal stratotype or nominal type locality (Walsh, 2005a,b).

## 7. Conclusions

The North American Land Mammal Ages of Wood et al. (1941) provide good examples of the use of strict nominal stratotypes in fixing the names and establishing the minimum durations of provincial biochronologic units. Nevertheless, the nature of the NALMAs has been routinely misunderstood, especially by members of the Berkeley school of North American mammalian stratigraphic paleontology. It has been widely assumed that Wood et al. (1941) literally defined the time spans of their Ages by the time spans of their typifying lithostratigraphic units, but this interpretation is incorrect given explicit statements of Wood et al. (1941) to the contrary. Despite some genuine ambiguity in their work, analysis of the NALMAs shows that the original definitions of these Ages were little different in basic form than those of the stages in the classic work of Kleinpell (1938). That is, the lithostratigraphic "type sections" of each Age served a name-bearing and exemplary function, not a boundary-defining function. The common charge that the Wood Committee defined many of their Ages as lithochrons is therefore a myth. Nevertheless, the potential or actual temporal overlap of some of the original nominal stratotypes of the Ages is a significant problem, and their redefinition is permissible in order to preserve the original intentions of the Wood Committee. In general, however, revisions of NALMA boundaries should not violate the original nomenclature established by Wood et al. (1941).

The Berkeley School of North American mammalian stratigraphic paleontology has its roots in the writings and teachings of H.G. Schenck and R.M. Kleinpell, and has been the most influential force in foundational discussions of North American mammalian stratigraphic paleontology since the 1960s. Characteristic idiosyncrasies of the Berkeley school include (1) a veneration of Schenck and Muller (1941) and a general unawareness of that paper's failure to

separate biostratigraphic and chronostratigraphic units; (2) an insistence on the obsolete equation of “unit stratotype” and “chronostratigraphic unit” and the resulting fallacious belief that chronostratigraphic units define geochronologic units; and (3) a misunderstanding of the chronozone concept. All of these beliefs hamper conceptual progress in mammalian stratigraphic paleontology.

The three most influential members of the Berkeley school have been D.E. Savage, M.O. Woodburne, and D.R. Prothero. D.E. Savage retained most of the Oppelian views of R.M. Kleinpell, while M.O. Woodburne and D.R. Prothero departed substantially from that tradition and adopted some of the views of H.D. Hedberg and W.A. Berggren, thus emphasizing single-taxon definitions, “datum-planes,” and supposedly rigorous boundary stratotype definitions for “stages.” Nevertheless, Woodburne and Prothero agree with Kleinpell in rejecting Hedberg’s view that once defined, formal age/stage boundaries must be immune to new discoveries of the guiding criterion. However, Woodburne’s and Prothero’s view would collapse the crucial categorical distinction between inflexible, golden spike-defined *aurichronologic* units and traditional flexible, paleobiological event-defined *biochronologic* units, a distinction that H.D. Hedberg spent much of his career trying to explain (Walsh, 1998a, 2001).

What most members of the Berkeley School of mammalian paleontology do not realize is that when they cite the authority of the North American Stratigraphic Code on the alleged need to designate type sections and boundary stratotypes for “stages,” they are uncritically assuming that the Code reflects the flexible Kleinpellian philosophy taught to them (directly or indirectly) at U.C. Berkeley, when in fact the Code is plainly a clear reflection of the very different golden spike-requiring philosophy of Hedberg. As a result, recommendations by members of the Berkeley school that we “obey the Code” and turn our land mammal ages into “formal stages” are superficial, because their own writings clearly show that they are still holding the pre-Hedbergian view that “stages” are flexible biostratigraphic/biochronostratigraphic entities.

Ironically, however, contrary to Hedberg and in partial agreement with some members of the

Berkeley school, the transformation of our provincial biochronologic/biochronostratigraphic units into golden spike-defined Ages/Stages would defeat the whole purpose of having biochronologic units, which is to summarize our ever-changing understanding of the true sequence of paleobiological events in each major fossil group that occurred in a given province. As illustrated by the difficulties routinely encountered by the International Commission on Stratigraphy (ICS) in defining the Standard Global Geochronologic Units, there would be enormous practical and political problems involved in the attempt to establish golden spike-defined Ages/Stages for each provincial biochron for each major group of fossil organisms in each province throughout the world. Furthermore, even if all of these hundreds of provincial Ages/Stages could be established, they would all soon be redundant, because the Standard Global Ages/Stages are already being defined by the ICS to provide a stable common language of geologic time at this level of resolution.

Both in principle and in practice, Hedberg’s attempt to restrict the terms Age/Stage to golden spike-defined entities has failed. Nevertheless, Hedbergians and traditional stratigraphic paleontologists can still find common ground if they will admit that aurichronologic and biochronologic units are both legitimate *subcategories* of geochronologic units, and that the appropriate lower rank unit terms for geochronologic and chronostratigraphic units *in general* are Age and Stage (Walsh, 2001). Hedberg’s major positions would still be vindicated in that golden spikes would still be used to define the Phanerozoic Standard Global Geochronologic Units, and a sharp separation of empirical biostratigraphic units from all subcategories of chronostratigraphic units would still be necessary. Traditional stratigraphic paleontologists would be vindicated in that they would retain the ability to modify the boundaries of their provincial Ages/Stages as they saw fit, provided that such modifications did not violate the principle of name-typology as established by the nominal stratotypes of those Ages/Stages. In order to fully realize these recommendations, major changes will have to be made to existing stratigraphic codes and guides (Walsh, 2005a,b).

## Acknowledgements

I thank W.B.N. Berry and S.G. Lucas for their reviews, which have enabled me to clarify the presentation of these ideas. R.J. Emry, R.H. Tedford, and R.L. Squires provided helpful information. I have benefited from previous electronic arguments with S.G. Lucas, D.R. Prothero, and M.O. Woodburne, as well as J. Alroy's unpublished manuscript describing his quantitative North American mammalian time scale. Of course, none of these individuals necessarily agree with any of my opinions.

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