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STRATIGRAPHIC CONCEPTS IN VERTEBRATE PALEONTOLOGY

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ABSTRACT. Vertebrate paleontologists use "local fauna" for the totality of taxa found at one or a few neighboring localities and prefix a geographic name; i.e., Garvin Gulley local fauna. If the fauna can be traced more or less continuously within a sedimentary province, the word "local" is dropped; i.e., Garvin Gulley fauna. This would be equivalent to the biostratigraphic term range zone.

Type sections in stratigraphic classification should have no more significance than name bearers. They are one dimensional samples of three dimensional bodies of rock.

A worker in vertebrate paleontology is sandwiched between geology and zoology. He is usually regarded as a biologist when speaking before geologists and therefore a person out of the field of the audience; as a geologist when talking before biologists, so again, tolerated but not to be taken seriously. But there are advantages; he gains from seeing how things are progressing in the two adjacent sciences, to his right and to his left. Speaking in a political sense I would place stratigraphy well to the right.

The stratigraphic concepts of which I wish to speak, I feel are somewhat leftish, that is toward, or perhaps better words would be, more analogous with, concepts held by some biologists. These concepts are not confined to vertebrate paleontology; some of my colleagues would deny them. They do, I am sure, play a role in the thinking, although I have not seen them so expressed in print, of some vertebrate and some invertebrate paleontologists. These ideas were arrived at, in part, over numerous cups of coffee with Drs. W. C. Bell and K. Young at The University of Texas. I am grateful for their stimulating help but assume full responsibility for this paper.

The continental sediments along the Texas Coastal Plain furnish a record of successive vertebrate faunas that very nicely illustrate certain points in stratigraphy. Vertebrate paleontologists have used the term "local fauna" to apply to the totality of species collected from one important site or cluster of sites. Thus the name Garvin Gulley local fauna was given to the vertebrates found on the Garvin farm just north of Navasota. Over a period of several years additional collections have been made. Now elements of the Garvin Gulley local fauna can be identified from localities (fig. 1) distributed over an area 250 miles long. Such a distribution is no longer "local" so the term Garvin Gulley fauna has been applied to the sum total of the local faunas.

The term is comparable to assemblage zone but instead of using a biologic name with all the vicissitudes to which it is subject, vertebrate paleontologists use a geographic name. Thus we have a biostratigraphic unit, the Garvin Gulley fauna, based on collections of vertebrate fossils similar to those at Garvin Gulley.

To the south is the Burkeville fauna similarly traceable over an area 300 miles long. Still farther south is a third fauna, named after the town of Cold Spring in San Jacinto County.

All three faunas have some vertebrates in common with one or both of

Fig. 1. Diagrammatic representation of distribution of fossil localities in the Miocene of the Texas Coastal Plain.

the others: turtles, gar fish. Each fauna shows evolutionary changes in some of the mammals from those of the preceding fauna: the horses, for example. Each fauna has some new forms introduced or old ones missing. It is this change in total composition that enables us to differentiate the faunas one from the other.

For purposes of convenience of expression I can draw lines separating each of these faunas (fig. 2). These lines can only be found on this diagram and on a couple of others in print. They cannot be found in the field anymore than contour lines can be found in the field. Each collecting locality is a point

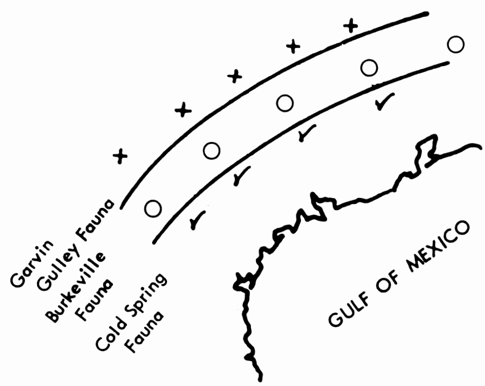

Fig. 2. Diagrammatic representation of grouping of localities containing similar fossils; biostratigraphic units.

of reference, the line drawn between is an interpolation only, subject to change with additional collecting, just as a contour line is subject to change with more precise elevation data. I now have on the diagram (fig. 2) a sequence of biostratigraphic units. They do not illustrate lithology and do not illustrate time.

The lithologic units (fig. 3) found in this same area are the Oakville

Sandstone Formation and the Fleming or Lagarto Formation. The lithology of the Oakville interfingers to the east with lower Fleming lithology so that an arbitrary cut off is necessary. The top of the Fleming Formation is covered by Pleistocene deposits.

In the central part of the area the Garvin Gulley fauna coincides by accident of occurrence with the Oakville Formation but the fauna can be extended eastward into the lower part of the Fleming Formation independent of the lithology. The Burkeville and Cold Spring faunas are both entirely within the lithologic unit, Fleming Formation.

As yet I have made no interpretation of age. An observant stamp collector, given the fossils and the locality records, could probably make the map of the biostratigraphic units. The names of the beasts would not be necessary for him nor would a knowledge of the evolutionary sequence.

The strike and dip of the lithologic units on the Coastal Plain show that the rocks farthest inland are the oldest, by the law of superposition. The same law applies to the fossils that are in the rocks so here is a first interpretation of relative time. The Garvin Gulley fauna is older than the Burkeville and it, in turn, is older than the Cold Spring Fauna.

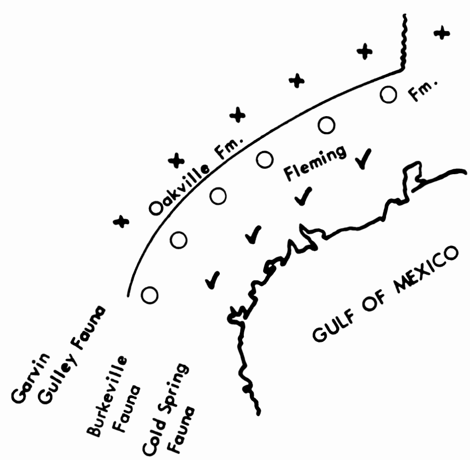

Fig. 3. Diagrammatic representation of lithologic units.

A further interpretation can be made by comparing each of these faunas to others whose positions in the timescale has been laboriously worked out by the same method of superposition. The Garvin Gulley fauna seems to have lived during an Age called Arikareean or Early Miocene, the Burkeville and Cold Spring faunas seem to have lived during the Hemingfordian Age or Middle Miocene. This is my *interpretation*. The fossils did not come labeled as to time. I determined their respective ages by an interpretation of several lines of evidence. The rocks containing these faunas constitute Stages so I can now draw Age/Stage boundaries on a map (fig. 4). Again, this line is no more visible in the field than is a contour line. Its position is subject to change as is

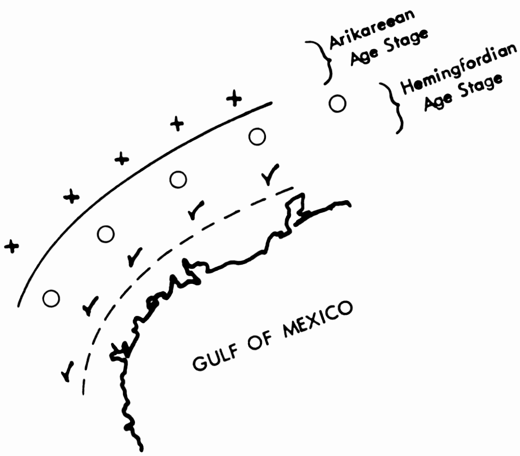

Fig. 4. Diagrammatic representation of time-stratigraphic units.

the biostratigraphic unit line between the Garvin Gulley Fauna and the Burkeville Fauna. The two here coincide, but they do not have to.

The term Arikareean Age is based on an arbitrary time during which certain mammals lived. The term Arikareean Stage is based on the rocks deposited during the time those mammals lived. These Arikareean rocks and fossils are ones that happened to be found earlier in Nebraska, that is, the type section of the time-stratigraphic unit Arikareean Stage is in Nebraska. The time, Arikareean Age, is defined as that characterized by the presence of *Parahippus*, a horse, living with certain dogs, rhinos, rodents, etc. The type section of the Hemingfordian Stage is also in Nebraska and is similarly defined from rocks and fossils overlying the Arikareean Stage. One of the keys to the recognition of the Hemingfordian Age/Stage is the sudden appearance of another more advanced horse, *Merychippus*.

The Burkeville and Cold Spring Faunas in Texas both contain *Merychippus* and enough other mammals in common with the type Hemingfordian fauna to correlate them. The Garvin Gulley Fauna has *Parahippus* but 10 out of every 100 single teeth of the same size and proportion possess the morphological character that makes them *Merychippus*. Moreover, on the Gulf Coast individual skulls of these horses have been found with some teeth in the same dentition identifiable as *Parahippus* and the others as *Merychippus*.

The Gulf Coast *Parahippus* is obviously in the process of evolving into *Merychippus*. Since *Parahippus* is absent in the Burkeville Fauna it seems obvious that it became extinct by evolution into *Merychippus*. Once *Merychippus* had evolved it migrated northward and lived alongside surviving parahippines in Nebraska.

The appearance of *Merychippus* then at the type section in Nebraska is one of migration from the Gulf Coast northward. The appearance of *Merychippus* on the Gulf Coast is by evolution and must be the earlier.

What does this do to our line between the time-stratigraphic units, now that the time-line between the Arikareean and Hemingfordian in Texas is demonstrably older than that line in Nebraska? In another way of speaking the so-called isochronous surface between the ages/stages by the best possible paleontological evidence is not isochronous.

What now, is the significance of the Nebraska type section? I should like to examine the concept of types in stratigraphy.

That order could be made of the stratigraphic section and of the multitudinous variety of fossils has never been doubted by stratigraphers and paleontologists. It was and still is, in itself, a tremendous act of faith which we have more or less unconsciously accepted. With the scientists of the last century this faith was perhaps stronger and more certain. They proposed numerous laws, often gave them fancy Greek names, and believed these laws to be of world-wide application. With a small amount of data such interpretations could reasonably be made, and perhaps many of the so-called laws would have stood up if the amount of data had remained small.

Unfortunately, in one narrow-minded sense, a great deal more data have been accumulated since the so-called laws were proposed and one by one they have been demoted. Large amounts of data necessitated the development in the twentieth centry of statistical concepts, especially when the data included a large number of variables. Developing with statistical theory has been the theory of sampling.

Let us look back from our vantage point of today and see what has happened to these data or these samples during the development of stratigraphy, and paleontology and biology. By a long series of events in history these sciences began in England and Western Europe. Classifications of rock and of animals, both living and fossil were begun by a few brilliant men living in this part of the world.

A classification of animals and plants was invented and quickly adopted. It was proposed by Linnaeus who at that stage in his career believed in the fixity of species. A classification of sedimentary rocks, the time scale, grew out of the efforts of men who believed in catastrophism. Both of these classifications are still in use because better ones have not yet come along.

Within these independent classifications grew the *type* concept. In a local area with a small amount of data there were different and distinct types of animals and of sediments. The early classifier either of rocks or animals accepted as axiomatic that the things he was classifying conformed to a "type".

To quote from Mayr, Linsley and Usinger (1953, p. 26):

> The original concept of a species, the species of the local naturalists Ray and Linnaeus, was a species without the dimensions of space and time. Such a species is always separated by a complete gap from other sympatric species. In its purest form it is clear-cut and has objective criteria, because it is defined by the gap that separates it from other sympatric species. This local species is the yardstick [type, JAW] by which all other situations are measured. Lacking the dimensions of space and time such a species is not evolving, it is static. It is for this reason that the nondimensional species has a great deal of objectivity and can be defined unequivocally (Mayr, 1949).

Concepts of time units in stratigraphy were early arrived at in a similar way. To quote from Dunbar and Rogers (1957):

> Fuchsel divided the Flötzgebirge of Thuringia into 9 rock units which he called series, 6 of which had subordinate basal units . . . Then, after objectively describing these rock units as such, he proceeded to erect precisely corresponding time units . . . .

The time units used by Fuchsel were easily distinguishable locally because they were defined by just those local gaps or lithologic changes that separated the rock units.

This equating of time units with type sections of rock has been explicit or implicit in all stratigraphic codes and is present in some of the modern textbooks. To quote from Moore (1958) p. 135:

> Like the Cambrian, the rocks called Ordovician have their type locality in Wales. . . . The section represented by outcrops there furnished *the* (italics mine) standard for comparison with deposits of similar age elsewhere in Europe and on other continents, *even though* (italics mine) many features of the Ordovician record are more simply and fully recorded elsewhere.

This is an extreme statement and Moore admits elsewhere, p. 27, that "the span of *some* (italics mine) divisions is modified by data derived from other parts of the world.

I am fully aware that a biologic type specimen is not the same as a type section. You can lop a few feet off the top or bottom of a section and still have a section, but it is not considered good technique to lop off the head or tail of an animal, or to break pieces off a type ammonite if it doesn't look like it is supposed to. Even so the thinking behind typology in biology and in stratigraphy leads to strikingly similar conclusions.

As Mayr points out, the nondimensional species has a great deal of objectivity and can be defined unequivocally. Both of these are highly desirable attributes. They make taxonomy easy; compare an unknown with the type—if it matches, label it by that name, and file it in a drawer. If it doesn't match, give it a new name. It is separated from other species of the same local area by a complete gap and its limits are defined by that local gap. Such species are static and nonevolving. With the accepntance of evolution the gaps between species had to go, and when the gaps went those comforting attributes objectivity and unequivocalness went too.

Stratigraphic typology leads to similar dead end conclusions but it offers the same tempting attributes—objectivity and unequivocalness. The logic of typologic stratigraphy proceeds like this. In the type section fossil zones A, B and C are found. One hundred miles away fossil zones A and C are found to be continguous, zone B is missing. The typological conclusion is that there is an unconformity between zones A and C. If no evidence for an unconformity can be found in the field there is now an easy out, because it can be said that there is a paraconformity between them. This takes care of the hiatus typologically called for by the absence of zone B. Our objectivity and unequivocalness are preserved and our new section is really, after all, like the type.

The end of this is that the rigid inflexible stamp of the type must be imposed throughout the world. If the type marine Triassic is divisible into three parts then the continental redbed section must be also, even though an unrecognizable disconformity must be placed in the section to create a hiatus in order to account for a reputedly absent Middle Triassic (Moore, 1958, p. 336). The alternative, that the European vertebrates that lived while the Muschelkalk was being deposited did not get to western North America, is not considered. Good typology assumes that they did get to North America and lived exactly during the time when an unrecognizable disconformity was being formed.

I can not claim that there are no occurrences where a Zone B is absent because of erosion, nor can I claim that there is no such thing as a paraconformity, but this method seems too glib. Paraconformities should be a last resort when all means of positive explanation fail. And unrecognizable disconformities should also.

To return to our analogy between the classifications of biology and stratigraphy: The acceptance of evolution meant that biologic taxonomists were in fact dealing with a continuum, life; from its earliest forms to its living forms, from its simplest to its most complex, one inconceivably vast complex of closely or distantly related forms. The modern concept of a species then became multidimensional moving, and dynamic but, alas, necessarily lost much of its objectiveness.

The type specimen in a modern concept of a species (Simpson, 1940) plays a different role, an important one but not much more so than that of other museum specimens of the same species. With a species now thought of as a population, the type specimen is just a single sample of this population. All other collected specimens are also valid samples and from this total known sample, inferences are drawn which form the basis of the greater concept that is the species. The totality of a species is really unknowable. How could you tell whether or not you had shot the largest possum? or the smallest? Even the species of whooping crane is really unknowable because we can not examine those that were living 100 years ago.

A type then in modern taxonomy is a sample; a sample that is useful as the basis for a description, useful as a fixed point to attach a name and that is about all. For comparison an unknown must be compared not only with that part of the sample known as the type but with all other known samples of the species, or at least a statistically representative sample.

Is not a type section in stratigraphy also a sample? Are not the Cretaceous rocks of England but a sample of the sum total of Cretaceous rocks deposited? Is not the type section of the Austin Chalk just a sample of a larger three dimensional lithologic unit? A sample of the Austin Chalk near Dallas is just as valid as samples found at San Antonio or Austin in furnishing the concept, Austin Chalk Formation. After all the type section is only a one dimensional sample of this three dimensional thing called Austin Chalk Formation. The formation should be a concept based on inferences drawn from all known samples, both surface and subsurface. We can never observe all of the formation, Austin Chalk, any more than we will ever observe all of a species. The Austin Chalk would have to be floating in the air so we could see the bottom of it but not too high but what we could jump on it and observe the top. Its boundaries over practically all of its extent will remain vague since we can observe only a minute fraction of the Austin Chalk Formation.

Hedberg (1958, p. 1885) argues:

> An equally critical point, which continually seems to need emphasis, is that all types of stratigraphic units require actual *type* or *reference rock sections* for adequate definition and that without these and adequate descriptions, they and their boundaries will always be vague and controversial if not actually meaningless.

With the necessity for type *and* reference rock sections I agree, but I can not see that they in themselves clearly fix the position of a boundary ten miles away. Because the fish I caught last Saturday weighed one pound and the type weighs two pounds does not mean they are not both white bass.

Formation boundaries are not always observable. In a gradational change from one lithology to another it is common practice to place an arbitrary boundary. But an arbitrary boundary is not observable. If it were observable it would not have to be arbitrarily placed.

Like biology, stratigraphy is also dealing with a continuum. Stratigraphy has the continuum, time, which has been divided into continguous time units. It also has the continua deposition and erosion, both continuous in time but discontinuous geographically. Nondeposition is similar in both respects.

Time of course has no type section. But time-stratigraphic units do. Must we still stamp a rigid, inflexible conformity on all stratigraphy? Can we finally break away and recognize that fossils in themselves do not magically tell us accurate time? We are now dealing with data that can prove this. An appeal to what is found at the area that was accidentally described first will no longer work. Let us recognize type sections for what they really are, samples, useful samples, but still just one of many known samples of rocks and fossils. Let us realize that there is no easy, objective, unequivocal way to classify the products of processes that are continuous.

The present terminology of the Ashley Code is sufficient to deal with most problems of stratigraphy. What is needed most is a change in thinking, a realization that deposition and erosion, and nondeposition are dynamic processes going on all the time. These processes used to be thought of two dimensionally instead of four dimensionally. Stratigraphy needs to shake off the bonds of typology that leads only to sterile repetitious description of the same things in different areas.

A different way of expressing the nomenclature of stratigraphy is shown here.

Time Unit		Time-stratigraphic Unit	Composed of Rock Units Group, Formation, Lentil, etc. and Biostratigraphic Units Zone or Fauna
Era			
Period	greater than ——	System	
Epoch		Series	
Age		Stage	

To return to the Gulf Coast Miocene faunas, by interpreting the Garvin Gulley Fauna as Arikareean, I mean that this fauna is most nearly like that found not only in the type section but in other sections determined by my colleagues as Arikareean, and that my inclusion of the Garvin Gulley Fauna into the Arikareean stage modifies somewhat but does not destroy the concept of Arikareean as held by my colleagues. To place it above or below would certainly destroy the concept of those Ages/Stages. To create a new one is unnecessary.

It is the necessity of communicating ideas that forms the stabilizing influence once typology is relegated to its proper place. My assignment of the Garvin Gulley Fauna to Arikareean implicitly carries with it the sense that part or all of the type Arikareean is correlatable with my section. I could assign my biostratigraphic units to the Waucoban, but I would have a very difficult time conveying the idea Early Miocene with that word.

The clear-cut objectiveness, the unequivocalness, is gone. The isochronous surface is not traceable, but I think stratigraphy can gain a more flexible, workable, and more accurate approach to what is found in the field. Not just in the field at the type area but all over the world. Let stratigraphic taxonomy catch up with biologic toxonomy.

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ERRATA

In the article by George C. Kennedy, "Phase relations in the system Al_2O_3—H_2O at high temperatures and pressures," (this Journal, October 1959, p. 570) the boundary on figure 3 labelled Kaolin/Pyrophyllite + Sillimanite + H_2O should be labelled Kaolin/pyrophyllite + corundum + H_2O.

In the article by E. F. Osborn on the "Role of oxygen pressure in the crystallization and differentiation of basaltic magma" (this Journal, November 1959), figures 7 and 10 were inadvertently interchanged by the printer. The diagram on page 631 is figure 7 and should appear on page 625; that on page 625 is figure 10 and belongs on page 631.