The Historic Development of Concepts of Biostratigraphic Correlation

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In stratigraphic procedure, it is not what terms an author uses that matters, but whether he knows what he is talking about (Arkell, 1956a, p. 466).

THE DISCOVERY THAT FOSSILS CAN BE USED FOR CORRELATION

Only rarely nowadays can it be said that a new concept originates with one man alone, but the science of biostratigraphy was founded by William Smith, and he owed nothing to earlier writers. It is true that the law of superposition is contained within the seventeenth-century ramblings of Steno (1669), but to the end of his life Smith had probably never heard of Steno. By 1796, Smith had discovered from his wanderings around Bath in Somerset that " . . everywhere throughout this district, and to considerable distances around, it was a general law that the 'same strata were found always in the same order of superposition and contained the same peculiar fossils' " (Eyles, 1969; Phillips, 1844). In true British style, he first made note of his ideas while in a pub, the Swan near Dunkerton. This momentous dual discovery became known through two Wiltshire friends of Smith, the Reverend Benjamin Richardson of Farleigh Hungerford and the Reverend Joseph Townsend of Pewsey. It was at Townsend's house in 1799 that Richardson drew up, at Smith's dictation, the succession of strata near Bath from the Chalk down to the Coal Measures, and listed "the most remarkable fossils which had been gathered in the several layers of rock" (Phillips, 1844). An outline of Smith's succession was first published in 1801 in Warner's *The History of Bath.* But for many years Smith's ideas spread by conversation. In March 1808, Smith showed his collection of fossils, arranged stratigraphically in his London house, to members of the Geological Society. It was not until 1813 that Smith's full table of 1799 was published by Townsend, who enthusiastically acknowledged that both the facts and ideas originated with his teacher Smith, who ". . made known his discoveries to everyone who wished for information."

Smith, a civil engineer, found writing a great labor. It was not until 1815 that his results appeared under his own name as an accompaniment to his great geological map of England.

The accurate surveys and examinations of the strata . have enabled me to prove that there is a great degree of regularity in the position and thickness of all these strata; and although considerable dislocations are found in collieries and mines, and some vacancies in the superficial courses of them, yet that the general order is preserved; and that each stratum is also possessed of properties peculiar to itself, has the same exterior characters and chemical qualities, and the same extraneous or organized fossils throughout its course (Smith 1815, p. 2.).

Smith's best known works, from which he can now be judged, were published between 1815 and 1819. In June 1816 he started to publish *Strata Identified by Organised Fossils, containing Prints on Coloured Paper of the most characteristic specimens in each Stratum* (1816-1819), which was never completed. In many respects this contains the purest and most complete account of his own observations; but the nearest to a general account in his own words appeared in 1817 in his *Stratigraphical System of Organized Fossils*:

By the tables it will be seen which Fossils are peculiar to any Stratum, and which are repeated in others (p. iv).

The organized Fossils which may be found, will enable him to identify the Strata of his own estate with those of others . (p. v).

My observations on this and other branches of the subject are entirely original, and unencumbered with theories, for I have none to support . (p. vi).

Note that the "Strata" had first been delineated by mapping, and then their characteristic fossils collected. The fossils could be used to distinguish two strata of similar lithology that lay at different positions in the succession, e.g., the Oaktree Clay (Kimeridgian) from the Clunch Clay (Callovian-Oxfordian), but the idea of different facies being correlated by fossils in common did not as yet exist.

On the other hand, it is clear that Smith had further recognized two important principles that were long overlooked by many subsequent geologists. First, he realized that even a single lithic formation could be subdivided on fossil distribution alone:

The Cornbrash, though altogether but a thin rock, has not its organized fossils equally diffused, or promiscuously distributed. The upper beds of stone which compose the rock, contain fossils materially different from those in the under (1819, p. 25–26).

Second, he recognized that fossils from the same facies of different ages could resemble one another:

The upper part of this thick Stratum contains large incurved oysters or Gryphaea, so much resembling others I have collected from remote parts, of a clay which now appears to be Oak-tree clay, as to be distinguished with difficulty; but this is only one of the many instances of the general resemblances of organized Fossils, where the Strata are similar (1817, p. 22).

The importance of Smith's work is greater than that of any subsequent contributor to the theory of our science. His lack of a formal education and his very limited reading of earlier authors meant that he was remarkably unbiased by previous ideas. His approach was completely empirical, and he never tried to draw conclusions beyond the evidence he had found himself, let alone to speculate why each stratum had its own peculiar fossils. His work is completely free from grandiloquent attempts at a unitary theory of all geology, and equally free from any interaction with theology. He himself pointed out that a person did not need to be able to read and write to use his methods, and as Arkell (1933, p. 8) has aptly mentioned, "That he could turn fossils to such useful account although he had no names for most of them is an object-lesson which many modern workers might with advantage take to heart."

Smith's achievements were well advertised in 1818 by an article in the *Edinburgh Review,* which, although unsigned, was known to have been written by the widely respected W. H. Fitton (who, nevertheless, suffered socially for his courage). The textbook by Conybeare and Phillips (1822) shows how much the new principles had become common knowledge, and the respect that was felt for William Smith as the discoverer that strata could be correlated by fossils. One would never realize this from the books by Mantell (1822) or Young and Bird (1822). Mantell was a surgeon at Lewes in Sussex who felt too strongly about his station in life. Smith was socially beneath him, and so there is but one reference to him among the hundreds of observations and opinions of "gentlemen," and then he does not even accord him the title of "Mr"! Mantell makes no reference to using fossils for correlation, and his lack of understanding is shown in his sole discussion on correlation in the whole work. He is commenting on Buckland's table of Tertiary strata in England with their probable continental equivalents (based on marine or nonmarine faunas):

It is not implied that the above five subdivisional parts of the Tertiary formations, maintain the same relative order of succession in England, and on the Continent;

most of them probably alternate, but they are all more recent than the chalk of England, France and Italy (Mantell 1822, p. 249).

This is a far cry from Smith's words of 1815!

Young and Bird (1822) were clearly familiar with Smith's works and some of their phrases are very close to his, although they do not mention him by name and did not accept Smith's ideas:

The attempt to identify the several strata by their respective fossils must be confined within narrow limits. the assistance thus furnished must be very limited; for if some of the fossils appear peculiar to certain beds, there are others very extensively diffused; and how can we be sure, that such as we deem peculiar to the beds which they occupy, may not be discovered, like their companions, in other beds of a very different description? (Young and Bird 1822, p. 300).

The same year saw a publication by Cuvier and Brongniart (1822) that shows as well as any contemporary work the possibilities of Smith's techniques. Some authors have accorded Cuvier an equal, or even superior, share in the honor of discovering the basic principles of biostratigraphy (e.g., Grabau, 1913), and the impression is repeated in the recent study by Wilson (1972). The error seems to originate by giving a modern meaning to Cuvier and Brongniart's undoubtedly original and outstandingly important discovery that the Tertiary deposits of the Paris basin contain alternations of marine and freshwater fossil assemblages, and hence that for the Paris basin one can draw up a stratal succession which shows that there have been "revolutions" of life on earth (Cuvier and Brongniart, 1808). It is clear from reading the original work that Cuvier was not concerned with correlating strata, but with the history of life on earth:

If there is any circumstance thoroughly established in geology, it is, that the crust of our globe has been subjected to a great and sudden revolution, the epoch of which cannot be dated much further back than five or six thousand years ago (Cuvier, 1817, p. 171).

It would certainly be exceedingly satisfactory to have the fossil organic productions arranged in chronological order, in the same manner as we now have the principal mineral substances. [He is referring to the succession of strata according to Werner.] By this the science of organisation itself would be improved; the developments of animal life; the succession of its forms; the precise determination of those which have been first called into existence; the simultaneous production of certain species, and their gradual extinction—all these would perhaps instruct us fully as much in the essence of organisation, as all the experiments that we shall ever be able to make upon living animals. And man, to whom only a short space of time is allotted upon the earth, would have the glory of restoring the history of thousands of ages which preceded the existence of the race, and of thousands of animals that never were contemporaneous with his species (p. 181).

As for stratigraphy, even in the revised edition of 1822, Cuvier was thinking in terms of fitting the strata into a Wernerian scheme. He went to great trouble to prove that the three main *terrains – la craie, le calcaire marin grossier,* and *le gypse avec les sables* – were not parallel, and hence were truly separate formations according to the principles of Werner. There was no mention of fossils, even though Cuvier knew that different levels in *le gypse avec les sables* contained different fossils; these alone were not sufficient to make them separate formations.

This interpretation of Cuvier's place in the history of stratigraphy is supported by Young and Bird (1822), who discuss the theories of Cuvier and Smith as separate, unconnected subjects, and by Brongniart himself, who acknowledges the advances of Smith and other geologists in England. But Brongniart, as distinct from Cuvier, realized the tremendous possibilities of making long-distance correlation by fossils independently of facies (although the word "facies" did not yet exist in the literature). Brongniart compared the fauna of the white chalk of Gravesend and Brighton with that of the white chalk of Meudon and Dieppe; the fauna of the greensand of Folkestone was compared with that of the chloritic chalk of Rouen, Le Havre, and Honfleur. He goes on to compare certain beds in Poland with "   the system of the lower or chloritic chalk with which I believe it can be correlated. In one of these beds the mineralogical characters disappear entirely, the stratigraphic position is obscure, it [the correlation] depends only on the zoological characters " (Cuvier and Brongniart, 1822, pp. 326-327). In a footnote following this paragraph, Brongniart argues in favor of using fossils over all other characters, particularly over lithologies, to correlate rocks. He points out that at the present day you can see different types of rock forming at the same time in different places, and that equally in Calabria, over 38 years, it had been possible to see beds formed lying at an angle on earlier sediments, yet no one would treat them as belonging to different stratigraphic epochs.

The following year (1823) Brongniart showed that nummulitic facies of the Vicentine Alps in Italy were of the same age as the Tertiary of the Paris basin.

FACIES

The development of the concept of facies is more elusive than that of correlation by fossils. Young and Bird (1822) remarked,

> Some parts of the strata are so nearly allied, that they often pass into one another, and occupy the same beds. This observation applies particularly to sandstone and sandy shale, to coal and bituminous shale; and, we may add, to oolite and grey limestone; which, as we have observed (p. 270), pass into one another, at Kirkdale and other places (pp. 294, 295).

But there was no mention of fossils in this context.

The same concept of facies in a lithic sense was worked out independently by Eaton in 1828 for the Devonian of New York State, where he recognized that the red Catskill rocks are a lateral facies of the marine gray beds to the west (Wells, 1963).

Similarly, Constant Prévost in France recognized in 1839 that the Wealden of England must be the freshwater equivalent of the marine Neocomian of France and Switzerland, because they were both sandwiched between the Jurassic and the Chalk. In 1841 he pointed out that the Muschelkalk (of Germany) was absent over much of Europe, but must be represented in these other regions by clays and sands. Prévost's logic was sound, and historians such as Zittel (1899) and Hoelder (1960) have honored him as a major innovator of the idea of facies. This is unjustified: not only had Young and Bird and Eaton preceded him, but Prévost, like these earlier geologists, had little influence on his contemporaries (Wegmann, 1963), and other geologists already had much the same ideas, e.g., De La Beche (1839, p. 129):

> Where we find limestones terminating abruptly against slates, and corals form a large portion of such limestones, we may be led to infer that the limestones were formed in the manner of modern coral reefs, mud and silt having accumulated against them laterally

We have seen how Brongniart correlated different facies with fossils, and similar ideas can be found in the work of Fitton (1827) on the Cretaceous of southeast England, and Phillips (1829) on the Jurassic of Yorkshire. However, it was not until 1838 that the word "facies" appeared in the literature and the concept was discussed in any detail. This was the work of Amand Gressly on the Middle and Upper Jurassic of the Soleurois Jura in Switzerland.

Gressly completed his monumental work in the summer of 1837, when he was only 23, and took it to Neuchâtel with the intention of reading it at the reunion of the Natural Science Society of Switzerland. He was so excited by everything that he saw and heard there that he forgot to read his own paper! Luckily, in spite of being timid, quiet, and withdrawn, he had the courage to show the manuscript to the famous Louis Agassiz. Agassiz was highly impressed; he got the manuscript published and invited Gressly to join him as his co-worker (Wegmann, 1963).

> To begin with it is two main facts, which characterise the whole harmony of modification that I call *facies* or *aspects of the formation:* one means that *such or such a petrographic aspect of a formation necessarily implies, wherever it is encountered, the same palaeontological assemblage;* the other that *such or such palaeontological assemblage rigorously excludes genera and species of fossils frequent in other facies* (Gressly, 1838, p. 11).

These are somewhat sweeping statements, and reading the full memoir shows that Gressly had knowingly exaggerated his basic definition. In fact, he recognized almost all the basic problems of facies known today. He showed that each of his facies reflected a different environment of deposition. He emphasized that the vertical and horizontal relations of facies must be compatible in terms of the environments each represents, this 56 years before the publication of Walther's law of the correlation of facies. Unhappily, more have paid lip service to Gressly than have taken his ideas into their own work.

FAUNAL SUCCESSIONS NOT TIED TO A LITHIC SEQUENCE

Although Smith had noted that there were distinct fossils from the lower and upper Cornbrash, most of his faunal lists were tied to undivided formations. The idea of being able to subdivide a broadly homogeneous formation on fossils alone did not exist in Conybeare and Phillips's general study (1822); the Carboniferous Limestone, the Lias, the Oxford Clay, the Chalk were all left undivided.

The general realization that rock successions could be divided on their fossils alone really came from studies of Tertiary sediments. Cuvier had realized the desirability of this, but no one man has the honor here; within just three years, Deshayes in France (1830), Bronn in Germany on the basis of the Italian Tertiary (1831), and Lyell in England on the basis of Deshayes's tables (1833) had all proposed divisions of the Tertiary based on fossils alone. Indeed, as is well known, Lyell's names for the Tertiary systems were based on the proportions of extant living species the rocks contained.

In theory, at least, stratigraphy was now free from a lithological control, and the possibility of a pure biostratigraphy had been attempted. In practice, the next introductions, the concepts of the stage and the zone, were developed without reference to Tertiary rocks. This was the beginning of a split approach to stratigraphy by students of the Mesozoic and the Tertiary, which has lasted to the present day.

CONCEPT OF THE STAGE

Although it was seen during the 1830s that one could find a succession of fossil assemblages within a single lithic formation, the erection of a systematic pile of fossil assemblages, independent of facies, and for the whole geologic column, was introduced by the French geologist Alcide d'Orbigny. D'Orbigny saw that a consequence of Gressly's "facies" was that stratigraphy must be freed of the vagaries of formation names, each of which was based on a local facies body.

> Geologists in their classifications allow themselves to be influenced by the lithology of the beds, while I take for my starting point the annihilation of an assemblage of life-forms and its replacement by another. I proceed solely according to the identity in the composition of the faunas, or the extinction of genera or families (d'Orbigny, 1842–1851, p. 9).

From this, d'Orbigny introduced the concept of the stage; but in discussing his contribution to biostratigraphy it is as well to separate his practice from the ideas behind it. The theoretical vision behind d'Orbigny's stages was conservative, even backward looking, in his own day; by present-day standards it is positively peculiar. Happily, even his contemporaries ignored his idea of repeated catastrophic destructions of life on earth, with repeated new creations, each of his stages representing a state of rest between one creation and the next destruction.

To understand what d'Orbigny meant in practice by a stage, let us take the Cenomanian in the Cretaceous, and read his mature account of it. This passage by d'Orbigny has been much abbreviated, but the terse style, the seminote form, and the odd use of capitals, or lack of them, are all true to the original.

20th Stage: Cenomanian, d'Orb. First appearance of the genera [list of 13 genera]. Reign of the order of cyclostegous Foraminifera, of the genera [8 names, chiefly bivalves]. Zone of *Nautilus triangularis,* of *Ammonites Rhotomagensis, Mantelli, varians;* of *Turrilites costatus,* of *Strombus inornatus,* of [10 species, chiefly bivalves]. Second zone of Rudists.

Derivation of the name. Here again it is the petrography which has served as a basis for the different names given to this stage, and which have prevented it being clearly distinguished as a geological horizon. One has called it *chloritic Chalk,* when it is full of green grains, as at Havre, at Honfleur; but the albian stage of Escragnolles is of the same lithology, whilst the beds of this same geological level are elsewhere bluish, marly, or represented by white chalk and by quartzose sands, red, green or white The Sandstones belong, by their stratigraphic position and by their palaeontologic characters, to all cretaceous stages . We give to the . beds the stage name *cenomanian,* the town of Le Mans (*Cenomanum*) being built on the most characteristic type and the most complete of the stage which concerns us

Synonymy . it is . a part of the *chloritic Chalk,* of the upper green Sand, of the glauconitic Chalk, of several french geologists;. . it is the *Tourtia* of Tournay of belgian miners; nervian system . of Mr. Dumont; the *Chalk-marl* and *Fireston of Upper-Green-sand* of Mr. Mantell (Sussex);. . *french type.* Le Mans, Saint-Calais (Sarthe); cap la Hève (Seine-Inférieure); [four more French type-localities]. *english type,* at Blackdowne . *portugese type,* banks of the Tage, near Lisbonne, etc. (1849–1852, pp. 630–652).

D'Orbigny follows this with a long list of localities where Cenomanian occurs, a discussion of its upper and lower limits, its relations to the overlying and underlying stages, its thickness and variety of lithologies, the paleogeography. He rounds off his essay with details of the fossil content: he lists numbers of genera of the underlying Albian and the overlying Turonian, which are absent in the Cenomanian; in more detail he gives all the genera of the Cenomanian unique to the stage; he notes that there are 841 species characteristic of the Cenomanian stage, which, he says, is quite enough to enable one to recognize the stage whatever facies it presents.

The inclusion, in the stage, such as we conceive it, of all the points indicated of its geographical extent is based on stratigraphical considerations of superposition, and on the assemblage of palaeontological characters of all these places. To prove that this reunion is nothing arbitrary, but that it is properly the result of the identity of contemporaneous species, we are going to give here the names of the most common, the most characteristic species which are found everywhere, not merely around the anglo-paris basin, in the Pyrenean basin, but in the Mediterranean basin [here follow localities from England to north Africa] (d'Orbigny, 1849-1852, p. 644).

This example has been given at some length because there is still argument today over what a stage is. When one examines d'Orbigny's own account, it is clear that his Cenomanian stage was conceived and based on its fossil content. The same is true of all his other stages: each is the major body of strata less than a system (anywhere in the world) that contains at least some of a long list of fossils which are peculiar to that piece of the total stratigraphic column.

To emphasize the certainty of his results, d'Orbigny gave multiple definitions of each stage. This has led to different usages because some authors have emphasized the use of type localities*; others have noted d'Orbigny's own emphasis on the use of fossils independently of lithologic variations; still others have read sedimentological cycles into his palaeogeography and stratigraphy. In recent years some have even, in effect, gone back to d'Orbigny's catastrophic theory: the stages are "the expression of the divisions which nature has delineated with bold strokes across the whole earth" (d'Orbigny, 1842-1851, p. 603). Hence the curious idea that the stages are terms of "time-scope" independent of their fossil content (e.g., Hedberg, 1959); no such meaning was given to d'Orbigny's stages by his contemporaries, nor for the next hundred years.

CONCEPT OF ZONES

The medieval words "zone" and "stage" were used by a number of geologists in the first half of the nineteenth century. The use of zone as a biostratigraphic term originates, like stage, with d'Orbigny. In this sense, he used it as an alternative word to stage: "I . . became convinced that the Jurassic rocks were divisible into ten zones or stages . . " (d'Orbigny, 1842-1851, p. 601). However, he also continued to use it, although less commonly, in the usual nontechnical sense of a band or belt, or natural division of the earth's geography, as when he wrote

In each stage, in fact, some fossils are characteristic of terrestrial deposits, others of marine deposits; in these latter deposits, floating species characterise the inshore sediments, at the level of high seas; others belong to zones deposited a little below tide-level; whilst some series belong entirely to the deep zones of the oceans (d'Orbigny, 1849-1852, p. 258).

*D'Orbigny invented the concept of type localities, yet he never used the phrase, but simply referred to "types." He was so confident in his correlations that he quoted many types for each stage. The more recent use of a single stratotype is not in d'Orbigny's own works, although he may refer to one of them as "the best type" or "the most beautiful type."

Various meanings for zone in stratigraphy might have continued for years, but in 1856 there started in Germany a work that gave the word a definite meaning, and which was to alter stratigraphic practice forever – Albert Oppel's *Die Juraformation Englands, Frankreichs und des Südwestlichen Deutschlands* (1856-1858). Oppel was only 27 when the whole of this work had been published, but his ability to observe and to draw conclusions from those observations, combined with tremendous energy to travel around Europe, had not been found in any previous stratigrapher since Sedgwick and Murchison. He died of typhoid fever when he was only 34.

Even today a brief perusal of Oppel's book impresses with its spread of detail. In eight separate districts of western Europe, the Jurassic rocks are subdivided into 33 zones correlated on the basis of their fossil content. Oppel's contemporaries outside Germany were completely bowled over; even the French admitted that it was pertinent to France and published a tabular summary (Laugel, 1858). Oppel explained his method within a mere seven-page forward:

Comparison has often been made between whole groups of beds, but it has not been shown that each horizon, identifiable in any place by a number of peculiar and constant species, is to be recognised with the same degree of certainty in distant regions. This task is admittedly a hard one, but it is only by carrying it out that an accurate correlation of a whole system can be assured. It necessarily involves exploring the vertical range of each separate species in the most diverse localities, while ignoring the lithological development of the beds; by this means will be brought into prominence those zones which, through the constant and exclusive occurrence of certain species, mark themselves off from their neighbours as distinct horizons. In this way is obtained an ideal profile, of which the component parts of the same age in the various districts are characterised always by the same species (Oppel, 1856, p. 3; translated in Arkell, 1933, p. 16).

Today we can recognize that these words mark the birth of biostratigraphy as a separate discipline. Whatever d'Orbigny may or may not have meant by his stages, it was Oppel who introduced the "ideal profile" based on fossil successions independent of any local palaeontologic or lithic succession. And in that same hot summer of 1858, Alfred Russell Wallace and Charles Robert Darwin read their joint paper to the Linnean Society of London, "On the Tendency of Species to Form Varieties, and on the Perpetuation of Varieties and Species by Natural Means of Selection." Immediately, Oppel's ideal profile becomes synonymous with the record of irreversible evolution of life on earth. Oppel himself remarked that the more accurately the fossils are examined and species defined, the greater the number of zonal divisions that could be recognized.

Each zone was named after a fossil, generally an ammonite species, but Oppel said that they could equally well have been named after places. The index species was merely a name for the zone: it was only one species in the assemblage of fossils that actually defined the zone, although one must be careful to avoid the mistake of Woodward (1892, p. 298), who wrote, "Zones are assemblages of organic remains of which one abundant and characteristic form is chosen as an index," which, as Buckman tartly commented, is a definition that embraces fossils in museum drawers! In practice, Oppel quoted 10 to 30 species as characteristic of each of his zones.

D'Orbigny's stages were fitted into Oppel's scheme: groups of zones were gathered to form each stage. Thus the zones of *Ammonites macrocephalus, A. anceps,* and *A. athleta* together form the Callovian stage. It is important to understand that Oppel's approach was to build up each stage from zones, not to take each stage and then subdivide it into zones (Miller, 1965). At the same time, he recognized that each stage named at that time already had a particular meaning attached to it, and occasionally a zone did not fit into one of d'Orbigny's stages as then defined. Thus his zone of *Diceras arietina* fell between the Oxfordian and Kimeridgian stages.

CONSOLIDATION DURING THE LATE NINETEENTH CENTURY

D'Orbigny had first introduced stage names in 1843, when he used them in describing Gastropoda from the Cretaceous, but it was not until the publication of his textbook of stratigraphy from 1849 to 1852 that his method became well known. Since it was only four years after this that Oppel began to use zones in a modern sense, the gradual acceptance of stages and zones into stratigraphy forms a single subject. It is a tangled subject, because even before d'Orbigny there were geologists who used stages names as geologic jargon for lithic formations, or some combined lithic and fossil unit. Merely putting "-ien" endings on place names had been introduced by Brongniart in 1829 (e.g., Oxfordien and Portlandien), and the practice spread independently of d'Orbigny (e.g., Néocomien by Thurman in 1836, Séquanien by Marcou in 1848, and Maestrichtien by Dumont in 1850; but these authors did not give them the biostratigraphic meaning that d'Orbigny did). On the other hand, Trautschold in Russia was still using plain Kimmeridge and Portland as late as 1877, but giving them the meaning of Kimeridgian and Portlandian.

Equally, because authors quoted Oppel's zones when writing about the Jurassic is no proof that they understood them (e.g., Woodward, 1876). In France, as early as 1857, Hébert was referring to the "zone of *Am. primordialis*" in the Jurassic, and he clearly only gave a stratigraphical meaning to "zone." But Hébert used *zone, couche, assise,* and *horizon* interchangeably, more or less at random, throughout his life; sometimes he made them lithic divisions (e.g., *ces assises glauconieuses,* (Hébert, 1857b). Oppel himself in the subheadings of his chapters referred to "Strata of Ammonites exus" not Zone of *Ammonites exus.*

The history can be summarized by saying that stages were introduced more rapidly than zones, but were more often misunderstood, and met with more opposition than zones.

Outside France and Belgium, d'Orbigny's stages were either ignored, or received with hostility, for many years by most geologists. Quenstedt, in Germany, wrote, Of what avail is it if a man has seen the whole world, and he does not understand aright the things which lie in front of his own doors? To compare faithfully two beds, each a hand in height, one on top of the other in their true order, can effect a more fruitful development of science than the use of stratigraphic catalogues from the furthest regions of the earth. Right from the outset one has to admit that such records are not reliable (Quenstedt, 1856, pp. 23–24; translation based on Arkell, 1933, with corrections).

Let us not weary of searching our strata; let each one of us collect as much as he can in his own neighbourhood, labelling the specimens exactly with their localities, and compare then with material collected by others; then at least the first goal of all geological research should not remain far from our reach – *a true table of the succession of the strata* (Quenstedt, 1857, p. 823; translated in Arkell, 1933, p. 14).

Note that this critic equally believed that there was some natural "true table of the succession of the strata." His real complaint was that d'Orbigny's foresight had forestalled those with more patience who would produce an accurate record. In fact, Oppel's improvement on d'Orbigny's work was published in the same years as these remarks of Quenstedt.

In Britain and Ireland, the stage names of d'Orbigny and other French geologists came into use only very slowly. Tate (1865) used them for the Upper Cretaceous of Ireland but referred to them as "formations." In 1867 he used Renevier's stage name Hettangian but he still refrained from using Sinemurian for the next beds upward in the Jurassic (Tate, 1867). As late as 1896, Strahan could remark about the Cenomanian stage " that English geologists were having reason to repent the introduction of Continental names into their Cretaceous nomenclature" (Jukes-Browne and Hill, 1896, p. 178).

In the United States, the concept of stages was completely ignored evèn as late as 1912 in the comprehensive *Index to the Stratigraphy of North America* (Willis, 1912).

These three countries – Germany, Britain, and the United States – were the slowest to introduce stages into their stratigraphy. By contrast, in Russia, Trautschold (1877) was only the first of a line of stratigraphers who used stages, and in their proper sense. Resistance to the use of stages was partly because outside France they were not always the self-evident divisions; partly because d'Orbigny was ahead of his time in recognizing the need for standard international divisions smaller than systems (d'Orbigny's Paleozoic stages were, in fact, synonymous with systems); and partly because in England and Germany local divisions were already strongly established. In the United States, correlation of Mesozoic faunas with their European counterparts was not developed until the 1920s, and without this the stages could not be identified in America.

Such opposition was not encountered by "zones," although like most novel concepts its adoption was relatively slow. Almost all stratigraphic theory was initially developed from work on Jurassic, Cretaceous, and Tertiary rocks. For these systems, and especially in France, Belgium, and Germany, the recognition and use of zones came in smoothly, limited only by the small number of stratigraphers to do the necessary detailed work. It is in this context that the work of Lapworth (1878, 1879–1880, 1879) is important. In this series of papers he showed how graptolite distributions could provide reliable zonal divisions of the Ordovician and Silurian; indeed, it was in the last of these papers that he introduced the Ordovician as a system. This was the first systematic erection of zones in the Paleozoic; it disproved Barrande's theory of "colonies," which could have meant that graptolites were without correlative value; and the British could no longer consider zones to be merely some sort of foreign idea (for many years the only detailed work on Cretaceous zones in Britain was by the French geologist Barrois, 1876). Lapworth recognized 19 zones grouped into seven stages, although these were not always given "-ian" endings. He explained how you could find the top and bottom of each of his zones. He also emphasized the chronological value of fossils: " . we have no reliable chronological scale in geology but such as is afforded by the relative magnitude of zoological change . . ." (1879, p. 3). It was still a slow business to get such an idea into the British; in the country of William Smith one could still find Woodward in 1887 being openly reluctant to use zones: he regarded lithology as safer for cross-country correlation!

EARLY ATTEMPTS AT INTERNATIONAL AGREEMENT

At the first International Geological Congress in Paris in 1878, Stephanesco of Roumania appealed for the establishment of a uniform nomenclature in stratigraphy. A 14-man commission, representing 16 countries, was set up to report on this. It met in Paris in April 1880, and recommended that the definitions of general terms be first attempted.

The congress at Bologna, Italy, in September-October 1880 decided on definitions of stratigraphic words like series and stage, and listed their synonyms in several languages (pp. 196–197). Rocks, considered from the point of view of their origin, were formations; the term was not part of stratigraphic nomenclature at all, but concerned how the rock had been formed (e.g. marine formations, chemical formations). Stratigraphic divisions were placed in an order of hierarchy, with examples, thus: group (Secondary Group), system (Jurassic System), series (Lower Oolitic Series), stage (Bajocian Stage), substage, assise (Assise à *A. Humphresianus*), stratum. A distinction was made between stratigraphic and chronologic divisions. The duration of time corresponding to a group was an era, to a system a period, to a series an epoch, and to a stage an age.

Some terms never got to be discussed at Bologna, including zone, horizon, and deposit, but subsequently the secretary of the commission, G. Dewalque of Belgium, attempted to draw up a consensus from reports of individual national committees, and this was published with the *comptes rendu* of the congress in 1882 (pp. 549–559). A revised version of this consensus report by Dewalque was published for the congress in Berlin (in 1886), and an English translation of this revision was published by the American committee in the same year (Frazer, 1886).

This appearance of unity was deceptive. The individual reports from the national committees show that even the agreed resolutions taken at Bologna were unacceptable to geologists in many countries. The British committee complained that those who had actually got to attending meetings of the commission were only a few of those who should have been there. This is the prime lesson of these early congresses: unless real agreement exists, international rules will be ignored.

TWENTIETH-CENTURY DEVELOPMENTS

Most of the concepts used in biostratigraphy today had already been developed by 1900. Three widely used textbooks of that time, Lapparent (4th edition, 1900), Geikie (4th edition, 1904) and Haug (1908-1911), show that stages and zones were being used as a matter of course in Europe for the Mesozoic and Tertiary, but for the Paleozoic their adoption had been haphazard; in America, lithostratigraphy still dominated. The gradual spread of biostratigraphic methods for other systems, and into other countries, would hardly be of general interest. Suffice to say that it would be a long story; in America, as recently as 1970, Berry and Boucot felt it necessary to publish a lucid explanation of the basic techniques of biostratigraphy. This was 114 years after Oppel had done the same for the Jurassic of western Europe!

Only the more important refinements of the twentieth century are discussed below.

Special Sorts of Zones

For many years no one worried that some zones were defined in one way, perhaps by the occurrence of a floral assemblage, whereas others were defined in some quite different way, such as by the stratigraphic range of an individual ammonite species. That such different sorts of zones existed was fully realized by Woodward in 1892, but it was not until 10 years later that a restricted type of zone was introduced by Buckman (1902, p. 557). This was the "faunizone," but by it Buckman meant no more than what many geologists of the time (and today) meant by the plain word "zone." Indeed, in defining faunizone, Buckman started by taking Marr's widely quoted definition of a zone, thus:

> Faunizones are, to paraphrase Mr. Marr, "belts of strata, each of which is characterized by an assemblage of organic remains," with this provision, that faunizones may vary horizontally or vertically, or the strata may not vary and yet may show several successive faunas. So faunizones are the successive faunal facies exhibited in strata, (Buckman, 1902, p. 557).

The first special type of zone that was quite distinct from Oppel's zones was the "epibole" introduced by Trueman in 1923 (p. 200) for the strata accumulated during the acme of a dominant species. The next year Frebold (1924) introduced "teilzone" for the strata at one locality deposited during the range of one species at that locality. Both types of zones were aimed at the finest possible divisions of strata on the basis of fossils. Whereas epiboles have little use except in regions where fossils are abundant through considerable thicknesses of sediments, teilzones can be used anywhere; with what effect is a matter of dispute.

More zonal terms have been introduced since 1924, a plethora by the late 1960s. For those who enjoy "geologese" there is a glossary of them patiently compiled by Hedberg (1971). There are discussions of their use by Arkell (1933), Weller (1960), and Shaw (1964). In spite of Arkell's statement as long ago as 1933 that "The term 'zone' by itself has now become a kind of family term, which may be very ambiguous unless qualified," a quick perusal of almost any stratigraphic journal will show that unqualified zones are still the norm.

Stages Named After Fossils

In 1898, Buckman named the ages, corresponding to stages, after groups of ammonites (e.g., Parkinsonian, after *Parkinsonia,* for the age equivalent of the Bathonian stage). Later he elaborated this scheme to fit several ammonite ages into each stage. Since Buckman believed in the nearly instantaneous world migration of new species, and hence in the absolute synchronity of beds correlated by ammonites, his ages became a sort of substage without a type locality, and usually without a rigid definition.

This habit was followed by Spath (e.g., 1923-1943) and others, but in a rather half-hearted way. The technique is now dead, largely because older stage names were too well entrenched, but more importantly because it presupposes that ammonites are the most perfect, or even sole, group of fossils for correlation.

Confusion of Stratal and Time Terms in Biostratigraphy

The earliest biostratigraphers were little concerned with time in geology, but with the practical problem of subdividing the stratigraphic column and correlating its parts from place to place. They had little idea of the immensity of geologic time. When Huxley (1862) wrote that " . neither physical geology nor palaeontology possesses any method by which the absolute synchronism of two strata can be demonstrated. All that geology can prove is local order of succession," nobody of the time could disprove him. Hence the need for a distinction between "homotaxis" or "similarity of arrangement" and "synchrony" or "identity of date." But as Huxley himself observed, "It may be so; it may be otherwise," and the example he offered must have been as nearly as provocative to his audience as it is to us today:

> For anything that geology or palaeontology are able to show to the contrary, a Devonian fauna and flora in the British Islands may have been contemporaneous with Silurian life in North America, and with a Carboniferous fauna and flora in Africa. Geographical provinces and zones may have been as distinctly marked in the Palaeozoic epoch as at present, and those seemingly sudden appearances of

new genera and species, which we ascribe to new creation, may be simple results of migration (Huxley, 1862, p. xlvi).

Subsequent nineteenth-century geologists sometimes acknowledged that their correlations were homotaxial rather than temporal, but, in the absence of a reliable means of measuring absolute ages of rocks, there was little more they could do about it. Of course, they realized that each thickness of sediment must represent a certain quantity of time, and there gradually developed a set of terms to represent the time equivalents of stratal divisions. D'Orbigny had used "époque" for the time of one of his stages, but by 1880, at the Bologna congress, it was agreed that the geologic time equivalent of "stage" was "age"; and for "zone" the Swiss committee had suggested "moment." However, in 1901 the code of the Paris congress made "phase" the time equivalent of a zone (Renevier, 1901), a decision apparently unknown to Jukes-Browne (1903), who suggested "secule" for the time equivalent of a zone. As early as 1893, Buckman had introduced "hemera" to be "used in a chronological sense as a subdivision of an 'age' " (1893, p. 518), and in 1902 he introduced (but apparently never used) the word "biozone," "to signify the range of organisms in time as indicated by their entombment in the strata" (1902, pp. 556-557). In America, a similar concept was expressed by Williams (1901): ". . . the time-value of the species *Tropidoleptus carinatus* would be the *Tropidoleptus* biochron."

All these introductions of new words prove that stratigraphers around the turn of the century had a clear distinction between stratal terms (system, stage, zone) and time terms (period, age, phase). In fact, in 1898, when Buckman submitted a paper to the Geological Society of London in which he used stage names, such as Bajocian and Bathonian, and called them "ages," the council of the society objected that he was using stratigraphic terms in a chronologic sense. Equally, the lack of use of "phase," "moment," and "secule" shows that no need was felt for them. As for "hemera," very few geologists either then or later ever used it, and for Buckman himself it was synonymous with "subzone," because he believed his ammonites to be perfect chronologic correlators.

No real confusion between time and stratal terms seems to have developed until the 1950s. When Arkell published his classic essay in 1933 (on which we all depend so much), he believed that there was a subtle distinction between stage and zone, but they were both based on fossil distributions, and hence both were biostratigraphic terms. He expressed the distinction more bluntly in 1946:

A stage is an artificial concept transferable to all countries and continents; but a zone is an empirical unit. If the zonal index species and its associated fauna are absent we cannot record the zone as present. . . In summary zones are of more restricted function than stages. Attempts to give them universal application are misdirected; such attempts merely make zones synonymous with subdivisions of stages and at the same time deprive them of their special qualities as the basis of correlation from one province to another (Arkell, 1946, p. 10).

Ten years later he seems to have realized that, in emphasizing that zones are not subdivisions of stages and neglecting to add to the passage that stages are built from zones, he left the impression that the two are quite different sorts of stratigraphic units.

> Just as it is convenient to group together formations into Series, so it is convenient to group like zones together and reduce the numbers for practical purposes, and above all to have a grouping which enables several zones to be correlated in a general way over long distances when the zones individually are too precise. Such groupings of zones are Stages (Arkell, 1956b, p. 7).

But it was too late! In 1952, at the international congress at Algiers, Hedberg proposed the setting up of an international commission "to establish principles and harmonize practice in stratigraphic nomenclature and terminology," and Hedberg became the first and, to date, only chairman.

There is now a series of papers by Hedberg, later to become a series edited (but still unmistakably dominated) by Hedberg (1948, 1951, 1954, 1959, 1961, 1964, 1965, 1968, 1971, 1972a, 1972b). These papers have provoked a lively discussion and for this reason alone they have been immensely valuable (e.g., Schindewolf, 1955, 1970; Arkell, 1956a; Hupé, 1960; Wang, 1964; Verwoerd, 1964, 1967; Callomon and Donovan, 1966; Hancock, 1966; Wiedmann, 1968, 1970a; and codes of stratigraphic nomenclature in 16 countries in all parts of the world, listed in Hedberg, 1972a, 1972b). Hedberg's chief innovation is the proposal that there should be set of "chronostratigraphic" units, which of itself is outside the scope of the history of biostratigraphy. Unhappily, among the formal units that he picked for his chronostratigraphic scale was "stage." Really, if Hedberg were not the gentleman I know him to be, I should be tempted to accuse him of a form of scientific theft. Whether "zone" and "stage" are different grades of the same concept, or whether they are different concepts, there is no question but that they are both biostratigraphic. As justly emphasized by Wiedmann (1968, 1970a), this was decided a long time ago, both by international agreement and common practice. Confusion now exists because a number of biostratigraphers have rushed to the defense of certain groups of fossils by claiming that their distribution is chronostratigraphic in Hedberg's sense (e.g., Sylvester-Bradley, 1967; Callomon and Donovan, 1966, for zones based on ammonities; Berry and Boucot, 1970, for graptolite zones). This is poor logic. Zones are not merely the most accurate known scale of stratigraphy, and by implication of stratigraphic time; they are the sole scale, other than radiometric dates in years, but they are still based on the distribution of fossils, and as such are biostratigraphic. The fact is that chronostratigraphic units are an imaginary entity of no value, and to claim an ammonite zone to be chronostratigraphic is to debase a practical stratigraphic unit, as well as to deny the biological characteristics of the origin, dispersal, and extinction of species populations – none of which is likely to be isochronous.

Making Biostratigraphic Units Objective

It will be remembered that one way by which d'Orbigny defined each of his stages was to quote type localities where a particular formation yielded a fauna that would enable one to recognize the same stage elsewhere. This objective manner of defining a stage has always appealed to many geologists, partly because most zones, in contrast, have been subjective; the recognition of any type of zone depends on the correct identification of certain fossils. French gelogists, in particular, have long placed emphasis on the use of type localities (stratotypes).

Attractive as this sort of stratotype approach might sound, it has met with practical difficulties. The top of a stage at one locality is not likely to coincide with the base of the overlying stage at *its* type locality; they may overlap or there may be a gap. To get over this difficulty, a group of British geologists suggested in the 1960s that one should define only the base of each stage and that this should be done by inserting a "golden spike" as a marker in an actual section (Ager, 1964). Hence one would have type localities to fix the base of each stage. Having defined the base of the stage, all strata are included in that stage as high as the base of the next stage upward. The idea was inserted in the first report of the stratigraphical subcommittee of the Geological Society of London (George et al., 1967), and the idea of "boundary stratotypes" as well as "unit stratotypes" is included in the report on stratotypes published by the Montreal Congress (Hedberg, 1970). The first examples of boundary stratotypes ("type horizons") were introduced by Sylvester-Bradley (1964), but at that time he did not use the principle of defining only the base of each stage. The Silurian-Devonian boundary is now fixed by a golden spike hammered in at Klonk in Czechoslovakia (Chlupáč et al., 1972).

Stages are groupings of zones. Therefore, the concept of defining only the base of each unit should start at zonal level. Thus one defines, with the help of a boundary stratotype, the base of each zone. The top of that zone is then defined by the base of the overlying zone, itself fixed at another boundary stratotype. The base of each stage is defined by the base of its bottom zone. Type localities for zones were introduced by Callomon (1964).

DISTINCTIONS BETWEEN CONCEPT AND PRACTICE

(Students of the cut and dried should jump to the summary).

When it came to writing this paper, I realized that some of the great innovators in biostratigraphy have put their conceptions into practice in such a way as to allow subsequent workers to make quite different interpretations of the supposed underlying concepts. The difficulty is a classic one for the historian: how do you distinguish what people say from what they do? Moreover, both may be ambiguous or inconsistent. Hence the student who seeks definite dates for the introduction of concepts is chasing a chimera.

The editors asked me to include some statements pointedly showing the historical derivations of various types of zonation, but there is no single point in history for the introduction of concepts like "concurrent-range zones" or "lineage zones." Certainly, the first time that these phrases were printed does not mark the first use of such zones, any more than to state that there was no biostratigraphy before 1910 when Dollo invented the word. Nor are such concepts introduced only once; most stratigraphic concepts are periodically rediscovered, so that the literature is littered with superfluous synonyms. Thus "biomere" of the 1960s and 1970s is only a new word for d'Orbigny's "stage." The current controversy over "chronostratigraphy" is largely an atavism: over the years 1850–1862 the same problems were discussed, albeit without the benefit of such twentieth-century words as "biostratigraphy," "epibole," or "negative association zone."

Oppel's ideas were so comprehensive that no simple definition of "zone" as used by Oppel is possible. He had already introduced, and therefore must have conceived without explicitly stating it, special sorts of zones such as assemblage zones and concurrent-range zones. He saw no need to distinguish such different types of zones, and did not feel it necessary even to define "zone." He visualized "an ideal profile," a concept sufficiently charismatic to attract, and sufficiently vague to allow us each to construct our own zonal succession, while thinking that we are getting closer to some fixed truth. European stratigraphers have been doing this for a century. Many of them, perhaps most, have never bothered themselves with just what sort of zones they have been erecting or handling. Some German stratigraphers (e.g., Schindewolf, 1950a, 1955) have even interpreted a zone as a time term to designate an interval during which was deposited the sediment that contains certain index fossils.

Such mystical inexactitude has dissatisfied twentieth-century American pragmatic stratigraphers. Hence the introduction of a variety of types of zones with precise meanings, among which the usual European practice is left as an almost undefinable "Oppelzone." To what degree this movement toward exact meanings for zones will improve our stratigraphic correlations remains to be seen; much of Shaw's apparently novel quantitative zonation (1964) is to be found in earlier European arguments. But it can hardly be doubted that exact definitions of the zones we use will improve our knowledge of their accuracy and limitations.

SUMMARY

1796	William Smith recognizes that formations are arranged in a regular order and each is characterized by its own peculiar fossils.
1833	Charles Lyell, with the help of Paul Deshayes, successfully subdivides the Tertiary of western Europe on fossil assemblages. He calls his Eocene, Miocene, and Pliocene divisions "systems," but because the divisions could be applied equally to separate basins of deposition, he and Deshayes had really anticipated d'Orbigny's invention of "stages."
1848	Amand Gressly introduced the word "facies," and shows that different facies of the same stratigraphic age yield different faunas.

1849-1852	Alcide d'Orbigny systematically subdivides all the Phanerozoic rocks of the world into stages based on the succession of their fossil assemblages.
1856-1857	Albert Oppel conceives stratigraphic divisions based on the general succession of fossils independently of the actual succession at any one place. He calls such divisions "zones" and applies them to the Jurassic.
1862	Thomas Huxley points out to geologists that their correlations with fossils show identity of arrangement (homotaxis); they do not prove synchronous deposition.
1879-1880	Charles Lapworth successfully applies Oppel's zonal methods to the Lower Paleozoic.
1880	Bologna: first international agreement on definitions of stratigraphic terms.
1893	Sydney Buckman introduces the "hemera," the first unit of geologic time, but it is unrelated to any stratigraphic division then in use.
1899	Arthur Rowe provides an early example of lineage zonation in his study of the evolution of the echinoid *Micraster* in the Upper Cretaceous of England.
1954	Hollis Hedberg appeals for a renewed effort to reach international agreement on procedure and terminology in stratigraphic classification, but purloins the "stage" from biostratigraphy for his new "chronostratigraphy."

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Note on References Cited

The actual date of publication of many nineteenth-century works was not that given on the title page. Quite commonly, an author would republish in book form, with a new date, work that had first appeared in parts, but nothing in the book shows this. In general, the dates on the title pages in English and German works give the latest year in which any of the pages were published. In France, this rule did not apply; d'Orbigny, in particular, would often publish the title page before the contents, all of which are later than the date given; when the work was complete he would re-issue it with a new date on the cover and the original date on the title page.