CHARLES LYELL'S DREAM OF A STATISTICAL PALAEONTOLOGY

by M. J. S. RUDWICK

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ABSTRACT. The terms *Eocene*, *Miocene*, and *Pliocene* were first used in 1833 in Charles Lyell's analysis of Cainozoic earth-history. Their original meaning and intention in Lyell's mind is here reconstructed, against the background of other contemporary research on the Tertiary strata. Lyell's terms were not originally intended to define contiguous periods of geological time: instead they defined relatively short isolated 'moments', randomly preserved from a far longer span of time. The ages of these 'moments' of preserved time were thought to be determinable in quantitative terms (however approximate and uncalibrated), by reference to the percentage of extant molluscan species that each group of strata contained. This palaeontological 'chronometer' depended on a biological theory of the continuous piecemeal formation and extinction of species at a uniform rate. Lyell originally hoped that his 'chronometer' could be extended backwards beyond the Tertiary and into even earlier periods. Although this ambitious project soon failed, it is conceptually important as an early attempt to use palaeontology to make a general quantitative time-scale for geology.

THE name of Charles Lyell (1797–1875) is well known to geologists and palaeontologists for at least two reasons: he is widely considered a pioneer of so-called 'uniformitarianism' in the earth sciences; and he is remembered as the originator of the terms Eocene, Miocene, and Pliocene, which even in the modern era of radiometric dating still dominate the descriptive stratigraphy of the Cainozoic (for historical re-evaluations of 'uniformitarianism', see Hooykaas 1959; Cannon 1960*a*; Rudwick 1971). This paper aims to show that Lyell's stratigraphical terms are 'conceptual fossils'; they are fragmentary relics of an ambitious theoretical project in stratigraphical palaeontology. This project faltered and failed almost as soon as it was launched, and the stratigraphical terms were 'metamorphosed' almost out of recognition. But Lyell's project is worth reconstructing none the less, because it serves to tie his practical work in applied palaeontology firmly into his broader programme for research on all aspects of the earth sciences.

Lyell himself had few personal followers and founded no distinct 'school' or research tradition. But it is difficult to over-estimate the influence of his compendious *Principles of geology* (1830–1833) and its later offshoot the *Elements of geology* (1838), which were widely translated and repeatedly updated in successive editions through nearly half a century. Lyell's persuasive interpretations of the accumulating empirical research of the mid nineteenth century were absorbed, selectively but pervasively, into the thinking of the first generations of professionalized geologists and palaeontologists; and his general approach has remained an essential element of the 'taken-for-granted' tacit knowledge of earth scientists to the present day.

THE PROBLEM OF THE TERTIARY FAUNAS

It is no accident that Lyell's permanent legacy to stratigraphical terminology should concern the Tertiary strata ('Tertiary' was used to cover all the 'Cainozoic' of modern

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geology except what were later interpreted as glacial and post-glacial deposits). This was not, however, because he sought to make an original 'contribution' by specializing in an 'under-developed' research field. Yet when Lyell first entered the community of active geologists centred on the Geological Society of London (Morrell 1976), Tertiary stratigraphy was in a certain sense under-developed. If the term 'paradigm' is used with proper caution in its historical sense (Kuhn 1962, *not* the palaeontological sense of Rudwick 1961, etc.) to describe a dominant style of research in a particular science at a certain period, then stratigraphy around 1830 was developing with great rapidity and success by following a paradigm that was primarily 'structural' in its cognitive goal (Rudwick 1976*b*) and increasingly palaeontological in its method. In other words, attention was focused on the discovery of the correct order of succession of formations, seen as a problem of three-dimensional structure; while 'characteristic fossils' were being used with increasing confidence as the most reliable (though not the only) criterion for the correlation of formations in different regions. The first aspect reached back to the highly fruitful research tradition of Werner and his followers (Ospovat 1969); the second aspect, while not altogether original to William Smith (1769–1839), certainly gained increasing emphasis from the manifest value of Smith's great geological map and its palaeontological illustrations (Smith 1815*a*, *b*; 1816–1819).

This paradigm was centred, however, on the strata with which Smith himself had had his greatest success: the so-called 'Secondary' formations (roughly equivalent to the Mesozoic and Upper Palaeozoic of modern geology). These could be divided relatively easily into formations of distinct and diverse character, many of them containing equally distinct and diverse fossils in some abundance. Below the Secondary strata, however, were the confusing 'Transition' strata and 'Primary' rocks (in modern terms, pre-Carboniferous strata and many metamorphic and igneous rocks), often highly disturbed and with few if any fossils. Above the Secondary strata were the Tertiary, which often seemed almost equally confusing, though for different reasons.

The Tertiary strata had indeed provided the paradigm of stratigraphy with an 'exemplar' that was at least as influential as Smith's, namely the study of the Paris region by Georges Cuvier (1769–1832) and Alexandre Brongniart (1770–1847). Their work showed the possibility of identifying a series of distinct formations over a wide area, using characteristic fossils as a tool of major importance; and their results were actually available in published form before Smith's (Cuvier and Brongniart 1808; 1811). But it soon seemed as if the Tertiary formations differed rather fundamentally from the Secondary. Tertiary strata seemed always to be confined to scattered 'basins'—the term was used in a quite literal sense, since the sediments were envisaged as having gradually filled pre-existing hollows in the underlying rocks. Cuvier and Brongniart described the Paris basin; Thomas Webster (1773–1844), the draughtsman at the Geological Society of London, soon afterwards described analogous Tertiary strata in the London and Hampshire basins (Webster 1814; 1816); and other basins were quickly added to the list during the 1820s. This apparent contrast between isolated Tertiary basins and widespread Secondary strata was heightened by the fact that the uppermost Secondary formation, namely the Chalk, was the most widespread and most distinctive formation of all.

A further difficulty derived from the very success of the French research. The most striking faunal distinctions among the strata of the Paris basin were given an interpretation that went far beyond the merely structural level of most stratigraphical work, reaching instead for a causal explanation. Using analogies with present-day faunas and floras, Cuvier and Brongniart interpreted the Parisian strata as an alternation of marine and non-marine formations; and a somewhat similar alternation was recognized by Webster in the Hampshire basin. This ecological dimension, coupled with the great difficulty of correlating the formations of one basin with those of another, led to a generally implicit but widespread belief that the Tertiary strata had accumulated under conditions that somehow differed radically from the Secondary.

This belief was probably reinforced by the analogous but even greater problems surrounding the most 'superficial' deposits of all. The unconsolidated, highly irregular, and often peculiar deposits that are now interpreted as glacial and post-glacial seemed naturally to suggest a major break in geological processes in the relatively recent past. Such an episode was sometimes labelled 'diluvial'; but when links with the biblical Deluge were expressed, this involved no mere literalistic interpretation (e.g. Buckland 1823), and in any case the phenomena themselves seemed to make some kind of drastic causal explanation inescapable (Rudwick 1970*b*; 1972, chapter 3).

At an early stage in his geological career, Lyell became dissatisfied with the prevailing tendency to assume the kinds of causal discontinuity that have just been summarized. It is difficult, however, to judge the relative importance, or mode of interaction, of theoretical and practical components in the development of his outlook. He may have been influenced by John Playfair's (1802) reinterpretation of the 'geological' aspect of the wide-ranging 'natural philosophy' of James Hutton. But since Lyell entered the field of geology in a period of self-consciously rapid development, he might well have found these authors merely 'old-fashioned' (see Porter 1976; 1977). He is likely to have been more impressed by seeing for himself the Tertiary strata around Paris. In 1823 he had as his guide Constant Prévost (1787–1853), who had already argued in public that the ecological explanation of the strata given by Cuvier and Brongniart could be brought even more closely into line with present-day analogies (Prévost 1823).

Lyell's first major scientific paper (Lyell 1826*a*) was devoted to showing that one characteristic Parisian lithology, a freshwater limestone, had a close modern analogue in the lakes near his Scottish family home. In his first essay on geology written for a more general audience (Lyell 1826*b*) Lyell extended this into the suggestion that Cuvier and other illustrious geologists had been premature in concluding that such analogues between present and past could not be found and used for explanation throughout geology. Lyell's confidence in this conclusion was strengthened by a persuasive case-study of the already classic area of Auvergne, published by his friend the political economist George Poulett Scrope (1797–1876) (Scrope 1827; Rudwick 1974). Lyell's review-essay on Scrope's work (Lyell 1827) shows clearly how he was adopting Scrope's picture of the very gradual development of the physical geography of central France, and trying to extend it to incorporate a picture of similar gradual change in its fauna and flora. Lyell explicitly used this example to illustrate how it was unnecessary to postulate sudden events of uncertain character in the past: all the observed phenomena of the Tertiary strata and their fossils could be explained without recourse to such events, simply by reference to processes observable at the present day.

At about this period, Lyell conceived the idea of writing a book that would reorientate geological interpretation along these lines. After many delays, and a change of plan that transformed it from a short elementary introduction into a massive three-volume work, the book was published as the *Principles of geology* (Lyell 1830–1833). Significantly, it was subtitled 'an attempt to explain the former changes of the earth's surface, by reference to causes now in operation'. Within this work, Lyell's reinterpretation of the Tertiary occupied a crucial position (Rudwick 1970*a*). In the first two volumes, Lyell surveyed the varied 'causes now in operation' that make for change in both inorganic and organic spheres, arguing that this repertoire of present-day processes was much more varied and more powerful than most other geologists had realized. In the final and culminating volume (1833), these processes were put to work in the causal interpretation of the stratigraphical record. Lyell illustrated this mode of interpretation chiefly by the example of the Tertiary. He chose the Tertiary because it was important for him to demonstrate the validity of his approach for the relatively recent epochs of earth-history. By doing so, he could both eliminate the supposed 'diluvial' break between the Tertiary and the present, and also go on to show that the contrast between the Tertiary and the Secondary was merely a difference of degree, not kind.

Within this over-all strategy, Lyell's concepts of Eocene, Miocene, and Pliocene played a crucial role, but a role that has often been misunderstood. To uncover Lyell's original intentions in coining these terms, it is necessary to look in more detail at previous interpretations of the Tertiary strata and at the development of Lyell's own ideas.

In analysing the formation of any novel scientific theory or project, it is important first to survey the range of other relevant theories or projects that were actually available as 'resources' that the principal figure under study could have utilized in his own construction. In the present case, at least four such existing pieces of work seem to have been relevant.

Firstly, the already classic description of the Parisian strata by Cuvier and Brongniart (1811) yielded a picture of the gradual accumulation of strata within a single basin under alternately marine and non-marine environmental conditions. Cuvier and Brongniart had given these ecological changes a 'catastrophist' interpretation, inferring that they had been caused by sudden changes in physical geography. Yet on Prévost's reinterpretation (1823) these variable conditions were to be expected, on present-day analogies, in any shallow gulf of the sea near the mouth of a large river. Not only was this the kind of explanation that Lyell intended to deploy more generally, but it also suggested that each Tertiary basin would have to be treated in the first instance as a separate entity. In other words, Lyell probably realized that it would be futile to search for exact correlations between the formations in different basins, if they owed their characteristics to essentially local factors.

A second major 'resource' for Lyell was the work of the Italian naturalist Giovanni Battista Brocchi (1772–1826), who had published a superb monograph on the molluscs of what he called the 'Subappenine' strata in north Italy, soon after the Parisian strata had been described (Brocchi 1814). It is probable that Lyell was familiar with this work at an early stage in his career: it was still perhaps the best monograph on any Tertiary fauna even ten years after publication. Its Italian was almost certainly no serious barrier to Lyell, since his father was a noted Italianist and he himself knew the language well enough in the 1820s to review a new edition of Danté (Wilson 1972, p. 187); and he also borrowed extensively from Brocchi, with scant acknowledgement, when later he was writing the historical introduction to the *Principles* (McCartney 1976). Brocchi prefaced his systematic work with a very long introduction, which included important interpretative comments on the relationship between his Italian fauna on the one hand and the Parisian and the present-day faunas on the other. Brocchi was a good enough naturalist to be aware of present-day regional faunal variations, and so he attributed the difference between the Subappenine fauna and the Parisian fauna to what we would term biogeographical factors. This was entirely reasonable, since there was no evidence to suggest any difference of age between the two basins. On the other hand, he was also aware that many of his Subappenine species had no living counterparts, and were probably extinct. However, he rejected Cuvier's 'catastrophist' explanation of extinction as inapplicable to marine molluscs—Cuvier had suggested that the extinct Tertiary land mammals had been annihilated by sudden marine incursions—and he found it a superfluous hypothesis anyway. In its place, Brocchi suggested an organismic explanation: species, like individuals, probably have a limited life-span, eventually losing their reproductive vigour and therefore dying out. On this model, extinction would be an essentially piecemeal process. This, he thought, might explain why his Subappenine fauna contained a mixture of extant and apparently extinct species. Furthermore, he pointed out that individuals of different biological groups have life-spans that vary from a few hours to a few centuries (e.g. insects and trees), and he thought that by analogy the average life-spans of species belonging to different groups might also vary widely. This could explain why the Tertiary strata contained many extant molluscan species, whereas their mammalian fossils were apparently all extinct.

Thirdly, Lyell was almost certainly familiar with the first full description of the basin in south-west France, published in 1825 by the young Parisian naturalist Barthélemy de Basterot (1800–1887). This memoir not only added another Tertiary basin to the growing list, but also included an important theoretical interpretation. Basterot derived his analysis explicitly from the new quantitative biogeography of the Swiss botanist August-Pyrame de Candolle (1778–1841). In his celebrated article on 'Géographie botanique', Candolle (1820) had used a wide range of published floras, from all parts of the world, to construct quantitative tables based on the numbers of genera and species common to various areas. These tables enabled him to distinguish twenty major biogeographical regions with distinct indigenous floras. He borrowed the then current meaning of the term 'statistics'—it denoted the collection of quantitative economic data for political use by the 'statist' or statesman—and he called his own work 'statistique végétale'. Basterot applied Candolle's botanical 'statistics' to his own palaeontological problem of the Tertiary molluscan fauna of the Bordeaux region. He had identified a total of 330 species in this fauna, and he analysed them quantitatively in two distinct ways, in relation to living species and in relation to other Tertiary faunas. Of the total number of species, only 45 were known to live in European seas and another 21 elsewhere in the world. Divided the other way, he noted that 91 species were also reported from Italy, 66 from the Paris basin, 24 from England (the Hampshire and London basins were not distinguished), and 18 from the Vienna basin; while 110 appeared to be peculiar to the Bordeaux basin. He suggested on the basis of these figures that the faunal similarity between any two basins might be roughly proportional to their geographical proximity.

Basterot's figures are not even completely consistent, and he did not attempt any more sophisticated analysis of them. His remarks are important, however, because they suggested at least the possibility of giving quantitative or 'statistical' precision to Brocchi's earlier inference that the faunal differences between the various Tertiary basins might reflect a pattern of Tertiary biogeographical factors. In other words, the diversity of the Tertiary faunas might be a function of space, not of time.

The fourth and last major 'resource' for Lyell's construction of his own interpretation of the Tertiary was the work of Scrope (1827), which has been mentioned already. Although Scrope was not concerned at all with the palaeontological aspect, he did describe the Tertiary strata of the Massif Central in enough detail for Lyell to realize that they were exclusively non-marine. More significantly, however, Scrope analysed the long history of the area, and showed that sporadic outbursts of volcanic activity had punctuated the gradual and continuous erosion of the Tertiary sediments. He argued that the lava-flows, isolated by subsequent erosion at various heights, formed 'a natural scale' or chronometer for estimating the relative *age* of the eruptions (Rudwick 1974). In other words, a quantitative measuring device, however approximate and uncalibrated, could be discovered within the geological phenomena themselves, and could convert the appearance of discontinuity into evidence of underlying continuity.

LYELL'S CONSTRUCTION OF A FAUNAL CHRONOMETER

Lyell's construction of his own distinctive theory for the Tertiary faunas, integrating the pre-existing 'resources' that have just been summarized, can be dated to the successive phases of his most important season of geological fieldwork. This was his long expedition through France and Italy in 1828–1829, partly in the company of his friend Roderick Murchison (1792–1871), who was not yet a rival. Lyell's development of his theory, which underlay his later terms Eocene, Miocene, and Pliocene, will be reconstructed here on the basis of the accessible records of his journey, namely his subsequently published letters from this period (K. Lyell 1881, pp. 182–251), some brief published extracts from his notebooks (quoted in Wilson 1972, pp. 187–261, and a few unpublished letters.

Lyell and Murchison first studied the areas that Scrope (1827) had described in the Massif Central. Lyell at least was completely convinced by Scrope's arguments for the very gradual erosion of valleys (Lyell and Murchison 1829*a*), and he must surely have recalled, when seeing the ancient lava-flows with his own eyes, how Scrope had used them as a quantitative 'natural scale' to measure geological time. Certainly he was impressed by the span of time that was implied by the hundreds of feet of thin-bedded Tertiary limestones, which were clearly the product of slow and tranquil deposition. All this confirmed his growing conviction, shared with Scrope, that most other geologists were seriously underestimating the sheer magnitude of geological time.

Lyell's interests, however, were more palaeontological than Scrope's, and he developed Scrope's pattern of interpretation in more biological directions. He checked that the Tertiary strata were indeed all freshwater in origin. Although strikingly similar to some of the Parisian limestones, they were also comparable to modern lake-marls, and contained similar freshwater molluscs and plants. On the other hand, the mammalian fauna of the Massif Central had evidently changed as much as it had in the Paris region, since local naturalists had discovered and described a fine fauna of extinct mammals (Croizet and Jobert 1826–1828; Deveze and Bouillet 1825–1827). But since there was no sign of any marine incursion into the area, Cuvier's explanation of the extinction of similar mammals in the Paris region was clearly invalid for the Massif Central. This may have confirmed Lyell's suspicion that some more gradual cause of extinction must be found. A further indication was that the mammalian fossils had not come from the freshwater limestone formation, but from a much younger—though still 'ancient'—river-gravel (Lyell and Murchison 1829*a*). The area thus contained a relatively recent mammal fauna that was full of extinct species, while the older Tertiary lake-deposits contained freshwater molluscs and plants much closer to those of the present day.

This evidence probably made Lyell recall Brocchi's suggestion that extinction might be caused by the intrinsic 'old age' of individual species, and that species in some groups (such as mammals) might have much shorter 'life-spans' than species in other groups (such as molluscs). Some such speculations along Brocchian lines are strongly suggested by the fact that while he was still in the Massif Central Lyell wrote a short essay in his notebook entitled 'On the laws which regulate the comparative longevity of species' (Wilson 1972, p. 215).

A few weeks later, Lyell and Murchison had left the Massif Central and reached Nice, where Lyell made use of the local knowledge of Giovanni Antonio Risso (1777–1845). He studied a thick Tertiary conglomerate formation and speculated on its causal origin; and he mentioned in a letter home that 'in the intervening laminated sands are numerous perfect shells, more than 200 in Risso's cabinet, 18 in a hundred of which are *living* Mediterranean species, whose habits are known' (K. Lyell 1881, p. 199). Risso himself, in his five-volume description of the natural history of the Nice area, had published long lists of molluscan species from the 'Formation Tertiaire' (Risso 1826, vol. 1, art. 4); but he had not distinguished the extant species among them, and the percentage expression was therefore probably Lyell's own gloss on Risso's work. It is in fact the first hint that Lyell was beginning to apply the quantitative or 'statistical' approach that Basterot had borrowed from Candolle. The context of Lyell's remark suggests, however, that he was using the numerical proportion purely as an ecological criterion, not as an indicator of geological age. The 'habits' of the extant species helped to show that the conglomerate formation had been deposited in conditions differing little from those probably still existing offshore.

After crossing the Appenines, Lyell and Murchison were able to study another important collection at Turin. Here Franco Andrea Bonelli (1784–1830) had already noted the faunal similarity between some of the local strata and Basterot's in south-west France (Wilson 1972, p. 221). Lyell recognized the Turin strata as similar to those he had been studying along the Mediterranean coast. In a letter from Milan shortly afterwards he referred back to 'the sub-Appenine beds from Montpellier to Savona, containing as they do nearly twenty per cent of decided living species of shells' (K. Lyell 1881, p. 201); and the context makes it clear that he was now using that percentage as an indication of *age*. This was because the strata near Turin were highly tilted; and Lyell already suspected from what he had seen in the Massif Central that there was a causal connection between tectonic disturbance and volcanic activity (Lyell and Murchison 1829*b*).

In pursuit of this hypothesis, Lyell left Murchison in northern Italy, and turned south towards Sicily to study active volcanoes, for in their vicinity he explicitly anticipated finding still more recent strata elevated above sea-level. This expectation was duly fulfilled (Wilson 1969; Rudwick 1969); and in the development of his hypothesis of elevation he made increasing use of the proportion of extant species in a fossil fauna as a guide to the relative age of the formation in which it was preserved. Brocchi had died only two years earlier, and so Lyell was denied the chance of a personal discussion with him; but Brocchi's Subappenine fauna, with its mixture of living and extinct species, was clearly playing a key role in Lyell's mind. Furthermore, unlike Basterot and even Brocchi himself, Lyell was now comparing different Tertiary areas primarily in terms of their relative ages, rather than in biogeographical terms. For example, after concluding his fieldwork in Sicily, he told Murchison how the strata he had studied there must be much younger than the Subappenine formation: 'I am come most unwillingly to this conclusion. But the numerous extinct species which characterise the Subapps. are wanting [missing] here, & living shells are present too plentifully, to admit a doubt that it is more related to our own epoch' (K. Lyell 1881, p. 233).

Up to this point, it seems that Lyell was using the proportion of living and extinct species in a purely empirical way and as a purely geological tool. But on his journey back through Italy a meeting with the botanist Domenico Viviani (1772–1840) at Genoa apparently turned his thoughts in a much more biological direction (K. Lyell 1881, p. 243). Lyell was so excited by what he called 'my new geologico-botanical theory' that he crossed the Alps in the depths of winter specially to talk about it with Candolle in Geneva. His report of his discussions with Viviani and Candolle merits quotation at some length.

I am now convinced that geology is destined to throw upon this curious branch of inquiry [i.e. biogeography], and to receive from it in return, much light, and by their mutual aid we shall very soon solve the grand problem, whether the various living species came into being gradually and singly at insulated spots or centres of creation, *or* in various places at once and at the same time. The latter cannot, I am already persuaded, be maintained. Viviani was puzzled to account for Sicily having so much less than its share of *peculiar* indigenous species; but this [is as it] should be, for *I* can show that three-fourths of this isle [i.e. Sicily] were covered by the sea down to a period when nine-tenths of the present species of shells and corals (and by inference of plants) were already in existence. Such an isle, like Monte Nuovo [the 'new' volcano formed in 1538 near Naples], has been obliged to borrow clothes from its neighbour, having scarcely had time to furnish any yet for its own nakedness. It has not yet seen out a tenth, perhaps not a twentieth part of a revolution in organic life. Give it the antiquity [i.e. as a *land* area] of the high granitic mountains of Corsica, and it will also boast its indigenous unique plants, unknown elsewhere either in the Mediterranean or other part of the globe [K. Lyell 1881, p. 246].

This important passage records the first rough outline in Lyell's mind of a wide-ranging *theory* of organic change, which would integrate evidence from biogeography, palaeontology, stratigraphy, and structural geology.

Like almost all naturalists at this period, Lyell believed that species were real entities, intra-specific variation being often considerable but always finite. There seemed to be good empirical grounds for rejecting Lamarck's earlier postulate of limitless variation.

The problem of accounting for the *origins* of these discrete units was therefore acute. Lyell probably shared the general belief that the production of new species must have been under divine providential control, in order to account for the precise adaptation of each species to the environment in which it was placed. Certainly he expressed this view, probably with complete sincerity, during his first public lectures on geology only three years later (Rudwick 1975; 1976*a*). But in the contemporary understanding of 'providence', such a view was quite compatible with a belief that God could have used some natural process or 'secondary cause' to achieve this end (Cannon 1960*b*). In the passage just quoted, Lyell seems to imply that new species would somehow originate spontaneously, if the appropriate ecological niches were vacant for long enough, as they might have been on an ancient and rather isolated island like Corsica. This process had not yet occurred on Sicily, he thought, because it had emerged from the sea-bed so recently, and it was so close to an existing land-mass that existing species had simply spread to it. The important point about Lyell's speculations, however, is that he clearly envisaged a process of *piecemeal* production of new species, in appropriate ecological situations that would tend to be scattered in both space and time. Also implicit in the quoted passage is Lyell's belief that the extinction of species was a process similarly piecemeal in character. As already mentioned, he had probably derived this idea from Brocchi and he may still have been using Brocchi's notion that the cause of extinction was the 'old age' of each species.

These two processes in conjunction produced an over-all pattern of continuous piecemeal change in the whole fauna and flora. With such a process of organic change, the specific composition of (say) the molluscan fauna, if followed from any given moment in past geological time, would gradually change until all the old species had been replaced by new ones. Lyell referred to such a cycle of change as a 'revolution'. For him this word carried no overtones of sudden violence; he used it in the older but still current sense of a complete cycle, such as the turning of a wheel or the circling of the Earth around the Sun. In the present context it meant a complete turnover in the specific composition of the fauna or flora (of some particular biological group).

This concept of slow cycles of organic change was given a quantitative dimension in Lyell's mind, by being geared to his previously empirical use of the proportion of extant species in various Tertiary molluscan faunas. This now became implicitly a *measure* of geological time. Thus in the passage quoted above, Lyell was measuring the age of Sicily as a land-mass by reference to the mere 5 or 10% of extinct molluscan species in its youngest marine strata; he was saying in effect that this represented an age of only 5 or 10% of one complete turnover or 'revolution', conceived as a major unit of geological time.

In the foregoing interpretation of Lyell's thought, I have made explicit a theoretical structure that was only hinted at in the available documentary record, and my interpretation has been guided by Lyell's own explicit statements of a slightly later date (particularly in the *Principles of geology*). Nevertheless, I believe that there is enough evidence to indicate that by the time he left Geneva early in 1829, Lyell had already constructed a theory of organic change that in principle provided the basis for a faunal chronometer for geological time.

From Brocchi he had drawn the idea of piecemeal extinction, and the notion that the molluscan species had intrinsically longer life-spans than, for example, the more spectacular mammalian species. He had then used Brocchi's emphasis on the mixture of extant and extinct molluscan species in the Subappenine fauna as the basis for a measure of geological age. He had seen the value of Basterot's quantitative comparisons of the faunas of different Tertiary basins; but he had reinterpreted Basterot's (and indeed Brocchi's) comments on these differences, seeing them not as a reflection of biogeographical factors, but primarily as a result of the different geological ages of the basins. Yet he had welcomed the biogeographical insights of Viviani and the great Candolle, integrating their stress on the spatial dimension with his own temporal emphasis, to produce a theory in which the piecemeal production and extinction of species was closely geared to the ever-changing local environment. Finally, he had seen the various Tertiary basins, like Scrope's sporadic lava-flows in Auvergne, as preserved 'moments' in a much longer history, for which a 'natural scale' or chronometer could be constructed.

Having sketched the theory just outlined, Lyell realized that its development required above all a sound knowledge of fossil and living molluscs. On his way back to London he therefore stayed in Paris to learn all he could from one of the best conchologists in Europe, Paul Gérard Deshayes (1797–1875). He was perhaps disappointed, yet also encouraged, to find that both Deshayes and another Parisian naturalist, Jules-Pierre Desnoyers (1800–1887), had independently reached somewhat similar conclusions about the Tertiary strata and their fossils: disappointed, because it might detract from the acclaim he hoped to receive for his work; encouraged, because the independent conclusions of others did at least confirm the validity of his own formulation. Although apparently Lyell did not know it at the time, yet another geologist, the young Heidelberg professor Heinrich Georg Bronn (1800–1862) had also been working on somewhat the same lines.

On the face of it, this looks like a striking case of what some historians and sociologists of science have analysed as 'simultaneous discovery' (e.g. Merton 1957; Kuhn 1959). There is indeed an element of simultaneity, but it is hardly surprising that several naturalists should have been working at the same time on the problem of the Tertiary strata and their fossils. The sheer accumulation of descriptive papers and monographs was making it increasingly evident that the Tertiary was not just a single major formation, but a highly complex series of formations, and this knowledge was readily accessible to the whole European geological community through an already well-developed system of scientific periodicals. Yet beyond this general concern to reduce the Tertiary to greater order and coherence, a detailed comparison of Lyell's work with that of other naturalists greatly reduces the element of coincidence.

Desnoyers had discovered a stratigraphical overlap that proved, by the ordinary principles of superposition, that the Tertiary strata of the Touraine region were younger than those in the Paris basin to the north. Desnoyers (1829) generalized this into a theory of the successive existence and filling of the various Tertiary basins, and used this theory to explain, for example, why the Crag of East Anglia was much closer faunally to the Touraine strata than to the London basin, although it was much further away geographically. For such a case, Desnoyers's view was clearly superior to Basterot's biogeographical explanation. Yet this palaeontological aspect played only a very minor role in Desnoyers's argument; and, furthermore, he attributed the formation of new basins to sudden tectonic events—the kind of interpretation that

Lyell was most concerned to eliminate from geology. So although Desnoyers added a footnote to his article in proof stage (1829, pp. 214–215), expressing his pleasure that Lyell had reached similar conclusions on the successive filling of the Tertiary basins, there was really only a small degree of overlap between their work. In the part of Desnoyers's memoir that he had completed before Lyell's stay in Paris, there is no sign of Lyell's more comprehensive vision of a complex pattern of continuous environmental and organic flux. Desnoyers was there concerned with the more conventional goal of dividing the Tertiary strata into successive *periods* of formation.

Deshayes shared this aim, complementing Desnoyers's stratigraphical fieldwork with a museum-based palaeontological study of Tertiary molluscan faunas. Before Lyell arrived in Paris, Deshayes had apparently already distinguished three groups of Tertiary formations on the basis of their fossils. Yet a detailed analysis of his published conclusions shows that he interpreted these groups of formations in conventional terms as the products of three successive and temporally contiguous periods of geological time. In his first brief announcement of his results, Deshayes (1831*a*) distinguished a first epoch with 3% extant species (e.g. Paris and London basins), a second epoch with 19% extant species (e.g. Basterot's Bordeaux strata and Desnoyers's Touraine strata), and a third epoch with 52% extant species (e.g. Brocchi's Subappenine strata and the English Crag). He referred to these as 'three great zoological epochs, completely distinct by the assemblage of species in each, and by the constant proportions between the number of living species and those that are lost' (Deshayes 1831*a*, p. 186). In fact Deshayes also mentioned a fourth group of strata with 96% extant species (e.g. the most recent of Risso's strata at Nice and Lyell's in Sicily), but he evidently thought these too recent to deserve the rank of 'epoch'. It is therefore clear that Deshayes was using his quantitative faunal analysis to define sharply *distinct* epochs: it was a useful tool for determining the temporal order of formations, in cases where the more conventional criteria of stratigraphical superposition and characteristic fossils happened to fail. In fact this is explicit in his fuller memoir (Deshayes 1831*b*), where he urged selection of characteristic fossils from the larger assemblages in which they occurred, and at various levels of specificity (e.g. '*Lucina divaricata*' for all the Tertiary strata, '*Cardium porulosum*' for the Parisian strata, and '*Cucullea cravatina*' for the lower *Calcaire grossier*). In other words, Deshayes was clearly concerned to use fossil assemblages simply to divide the Tertiary into discrete epochs; formations could then be assigned to the correct epoch by appropriate characteristic fossils, if the criterion of superposition was not available.

This project differed fundamentally from Lyell's in its cognitive goals and structure of thought. But the contrast has been masked by the fact that Lyell paid Deshayes, who badly needed financial support, to supply him with a complete 'statistical' analysis of the Tertiary molluscan faunas, for Lyell to use in the *Principles of geology*. This arrangement necessitated a certain compromise in Lyell's presentation of Deshayes's results.

Before analysing Lyell's use of Deshayes's work and the articulation of his own very different theoretical framework for Tertiary earth-history, a brief comment on Bronn's work is appropriate, although Lyell did not hear of it until later. Bronn toured northern Italy at about the same time as Lyell (though they did not meet), and used many of the same local informants. After his return to Heidelberg he published an elaborate quantitative analysis of the complete fossil record, with a more detailed analysis of the Tertiary faunas of the various European basins (Bronn 1831). Bronn's tables are an outstanding example of the 'statistical' approach in palaeontology at this time (remembering that 'statistics' meant simply a compilation of quantitative data). For each fauna he counted the numbers of genera and species belonging to various major groups, and converted these numbers into decimal fractions for easier comparison. For the Tertiary formations he calculated degrees of faunal affinity between the various basins; tabulated these affinities in terms of orders of similarity; and constructed an elaborate matrix to display them numerically. Yet only incidentally in the midst of all this '*Tabellenstatistik*' (as its critics scornfully called it) did Bronn record the fractional proportions of extant species in the various basins. Only these figures—a minor feature of one of his seventeen separate tables (Bronn 1831, table 11)—are conceptually equivalent to the percentages that Lyell was to use in his later analysis of the Tertiary. Bronn did use the proportion of extant species to indicate that the strata near Turin were quite separate from the Subappenine strata (as Lyell had also concluded); but he did not develop this into a general criterion of geological age, and his published work shows no theoretical structure underlying his 'statistical' analysis. Like some more recent palaeontologists who have been keen to exploit statistics in the modern sense of the word, Bronn gives the impression of having had more figures than he knew how to handle, and of not having had any clear theory that he wished to test.

THE ARTICULATION OF LYELL'S FAUNAL CHRONOMETER

Lyell described his period of collaborative work with Deshayes in 1829 in the following terms: 'We planned together a grand scheme of cataloguing the tertiary shells of various European basins, that I might draw geological inferences therefrom.' The intended division of labour is here quite clear! Later in the same letter, Lyell made equally clear the ambitious scope of his project: 'My results will be an induction from nearly (perhaps more than) 3000 species in the tertiary formations alone, and I hope by other aid than Deshayes to carry it on through older strata also' (Mantell MSS., Lyell to Mantell, 24 Feb. 1829). In other words, Lyell hoped that quantitative faunal comparison—a 'statistical' palaeontology—would yield a *general* faunal chronometer for the whole of the fossil record, not just the Tertiary. Given Lyell's concept of successive turnovers or 'revolutions' in the specific composition of, say, the molluscan fauna, there was no reason in principle why the chronometer should be limited to the most recent complete cycle of organic change. If some earlier 'moment' of geological time were taken as the base-line for comparison, in place of the present, it would be possible to estimate the ages of still older strata by their degree of faunal approximation to that base-line. In this way the chronometer could in principle be extended backwards indefinitely through one faunal cycle after another. Such an ambitious scheme was never again explicitly stated by Lyell, except very briefly and much later (Burchfield 1975, p. 68). Yet my analysis of his intention is consistent with what Lyell did make explicit, and I believe it represents the full scope of Lyell's original 'dream' of what his faunal chronometer might achieve.

Lyell had to make this grandiose project concrete in the first instance, however, by dating the Tertiary strata alone. During a period of intensive work after his return to

England, hints in his letters reveal how far his conception of the Tertiary stratigraphical record differed from that of Deshayes and Desnoyers. For example, he told Murchison: 'My last tour persuaded me that altho' on enlarging our Geology in new countries the epochs will multiply, yet in any one country the groups [of strata] will be found to belong to much fewer geological epochs than our predecessors imagined' (Murchison MSS., Lyell to Murchison, 5 Oct. 1829). Here the word 'epoch', like the words 'statistics' and 'revolution', needs careful interpretation. In the early nineteenth century the older sense of 'epoch', meaning a *point* or moment in time, still co-existed alongside the secondary (and modern) meaning of a *period* of time. Lyell's use of the word in the passage just quoted was in a sense a blend of the two meanings. Of course he did not think that any group of strata had been deposited in a 'moment' of time, even in geological terms: on the contrary, he was centrally concerned to emphasize how slowly all sediments have accumulated. Yet the quotation does imply that he envisaged that the time taken for the deposition of any one formation was quite short in comparison with the totality of time represented by all the formations of the same general age in different regions. To put it another way, Deshayes and Desnoyers hoped that by faunal and stratigraphical comparison the Tertiary formations of different regions could eventually be linked together in an overlapping 'chain' that would provide a *continuous* record of sedimentation and marine life. Lyell, in contrast, believed that these formations represented mere isolated 'moments' (relatively speaking), separated from each other by vast spans of geological time of which no record had been preserved; and even future exploration would merely multiply the number of these 'moments' of preserved time without ever achieving a continuous record.

Lyell returned to Paris in 1830 to work with Deshayes again, but the publication of his interpretation of Tertiary earth-history was delayed by the unexpected magnitude of his task of describing and classifying the agents of change at present observable in the inorganic and organic realms. This work was published in the first two volumes of the *Principles of geology*, and only in 1833 did Lyell finally publish the last volume, in which the Tertiary was used as an 'exemplar' of his method of interpretation of the past 'by reference to causes now in operation'.

In his exposition of his new system for the Tertiary, Lyell used once more the Brocchian analogy between species and individuals, and wrote quite casually about the 'birth and death of species' (Lyell 1833, pp. 32, 33). He explained his theory that the extinction of species is a normal part of nature, and that it happens in a piecemeal manner at all times. In the previous volume he had in fact rejected Brocchi's 'old age' explanation of extinction and replaced it with a more 'modern' theory based on environmental change (Lyell 1832, pp. 128–130). But this scarcely reduced the value of Brocchi's general analogy, and Lyell used it with great effect in his exposition, adapting it into a *human* analogy based on the contemporary concern with population censuses (Lyell 1833, pp. 31–33; Rudwick 1977). Lyell likened the continual shifts in the areas of sedimentation (and hence of the possibility of preservation of the fauna) to the movements of itinerant 'commissioners' taking censuses of the population in different regions of a country. Each preserved fauna in a given Tertiary basin was thus like the 'statistical documents' that such officials might leave behind them, to record the state of the population in a given province at a certain time. On their next visit, the constituent individuals in the population would have changed, by deaths and births, in proportion to the time since the previous census. Likewise the change in the fauna between two successive formations would also be proportional to the unrecorded interval of time between them.

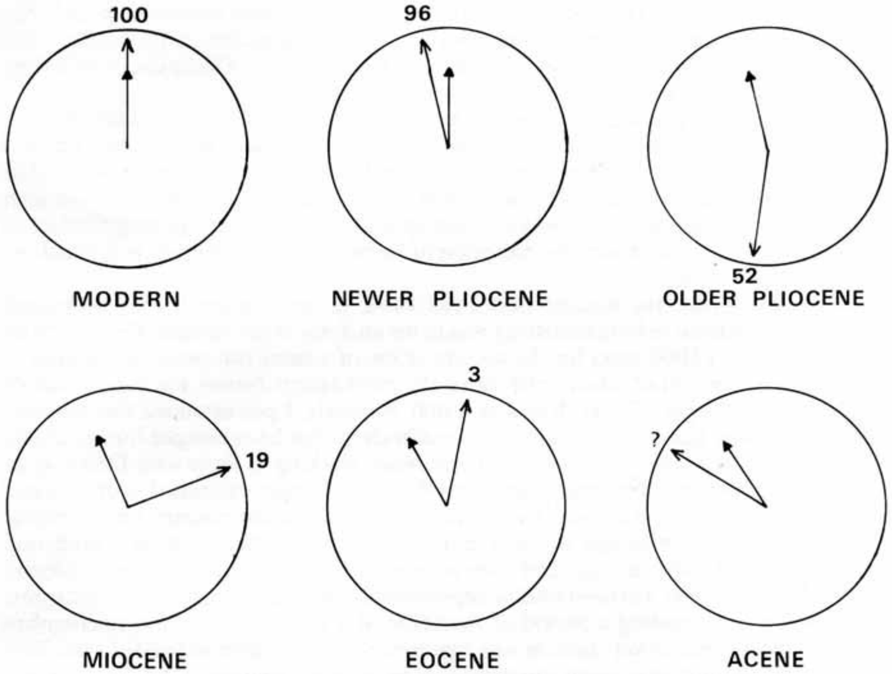
Obviously the analogy—like all analogies—had its limitations. Lyell was not postulating that the regularity of censuses had any analogue in the highly irregular fluctuations in the areas of Tertiary sedimentation. Conversely, he did want to use a quantitative comparison of faunas as a measure of time-differences not only between formations in one basin, but between formations in different basins. Nevertheless, the analogy was a helpful expository device for Lyell's readers, and it may also have been an important heuristic device at an earlier stage in Lyell's own mind. In any case, the population analogy served to make clear Lyell's distinctive concept of the various Tertiary formations as little more than momentary *samples* preserved from a vastly greater span of unrecorded time.

Lyell's introduction (1833, pp. 52–56) of the terms Eocene, Miocene, and Pliocene must be seen in the light of this conception of an extremely fragmentary geological record. Like other British scientists of this period, Lyell had applied to the polymathic William Whewell (1794–1866) for advice on his scientific nomenclature. Their letters reveal the barbarous Greek-based terms that might have entered the geologist's vocabulary—for example, 'Meiosynchronous', 'Meioneous', and 'Meiotautic'—before Whewell as an afterthought suggested 'Miocene', and of course 'Eocene' and 'Pliocene' too (quoted in Wilson 1972, pp. 305–307). Lyell also originally wanted an earliest 'Asynchronous' epoch, which in Whewell's hands became 'Acene'. Although the Acene was dropped from Lyell's published scheme, it is important for reconstructing his conception of the Tertiary epochs.

In earlier brief analyses of Lyell's work on the Tertiary, I interpreted Lyell's application of Deshayes's quantitative data in terms of a quite sophisticated conception of statistical survivorship rates (Rudwick 1970, pp. 24–26; 1972, fig. 4.5). This now seems likely to be anachronistic, since such statistics, in a form that Lyell might have recognized as relevant to his concerns, were not developed until the 1840s by the pioneer Belgian statistician Adolphe Quetelet (1796–1874). Lyell probably had a much simpler conception of the continual turnover of species during geological time, by which the percentage of extinct species in a Tertiary fauna would be directly proportional to its age.

It is probably consistent with Lyell's conception to depict his successive epochs in terms of a chronometer with a decimalized 'minute' hand (text-fig. 1). Suppose then that each 'hour' represents one complete faunal cycle or Lyellian 'revolution'. If the present (Lyell's 'modern' period) is represented by twelve noon, we can infer that Lyell conceived Deshayes's original estimates as follows: Deshayes most recent strata, which Lyell termed 'Newer Pliocene' (and later, 'Pleistocene'), were only 4% of the last 'hour' before 'noon'; his third epoch, Lyell's 'Older Pliocene', was just after the half-hour (52%); his second epoch, Lyell's 'Miocene', was only 19% through that last 'hour'; and his first epoch, Lyell's 'Eocene', was only 3% after 'eleven o'clock'. The provisional 'Acene' would then have represented some time *before* 'eleven o'clock', i.e. during the previous 'revolution', for it contained *no* extant species.

This analogy with a chronometer does not imply that Lyell thought that each of



TEXT-FIG. 1. Diagrammatic interpretation of Lyell's original conception of the 'epochs' of Tertiary time, in terms of a faunal 'chronometer'.

Deshayes's epochs represented no more than 1% of the total duration of a 'revolution'; he was certainly aware that Deshayes's figures could not claim any such accuracy. Further collecting was continually enlarging the fauna known from each formation, and the figures that Deshayes finally submitted to Lyell for publication in the *Principles* differed slightly from his earlier estimates: Eocene $3\frac{1}{4}$%, Miocene 18%, and (Older) Pliocene 49% (Lyell 1833, Appendix I, pp. 47–52). Yet even if Lyell realized the margin of error intrinsic to Deshayes's estimates, the suggested analogy with a chronometer does serve to emphasize that he did not think of his four periods (Deshayes's three, plus the Newer Pliocene) as periods that collectively recorded most of Tertiary time. He did, indeed, believe that the Newer Pliocene graded insensibly into the present. This was essential to his aim of breaking down the conceptual barrier between present and past, just as in Deshayes's view it disqualified the Newer Pliocene from being a real separate epoch. But between his four periods Lyell certainly believed that there were long spans of unrecorded time. He stated explicitly that the definition of these four periods was essentially a result of the accidents of preservation and collection, so that the periods were in a sense arbitrary; and he anticipated that intermediate periods would need to be defined in future (Lyell 1833, pp. 56–58). This makes it clear once more that to Lyell the known Tertiary faunas were no more than scattered samples from a far longer time-span of continuous change; they were *not*, as they were for Deshayes, a relatively complete record of a sequence of faunally distinct periods.

Lyell was always prudently cautious in public about suggesting any estimate in *years* for the magnitude of the geological time scale. Such reticence was prudent, not because Lyell had any reason to fear persecution or even ridicule by religious conservatives, but he knew that no estimate could be more than an enlightened guess, and that any such guess would be criticized by other geologists as a mere speculation running counter to the spirit of empirical science (the reception of Darwin's later rash guess is instructive: see Burchfield 1974).

In the *Principles*, the nearest that Lyell came to any calibration of geological processes in relation to human history was in his analysis of the volcano Etna. Here he estimated some 12 000 years for the accumulation of a small (unspecified) fraction of the volcanic cone, all of which post-dated the most recent Newer Pliocene strata of Sicily (Lyell 1833, pp. 97–101; Rudwick 1969). Privately, Lyell extended this estimate slightly in a way that hints at the kind of calibration that he envisaged for his faunal chronometer. In a letter to his sister, written while working in Paris with Deshayes in 1830, Lyell reported: 'This morning all my Etna shells were examined; out of sixty-three, only three species are not known to inhabit the Mediterranean; yet the whole volcano nearly is subsequent to them and rests on them. They lived on a moderate computation 100 000 years ago and after so many generations are unchanged in form' (K. Lyell 1881, p. 308). This and similar unpublished comments (see Tasch 1977) suggest that Lyell was estimating a period of the order of a million years for one complete faunal cycle or 'revolution', such as was represented by the whole of the Tertiary. This order of magnitude may seem unimpressive by modern standards, but it records a significant phase in the gradual stretching of the scientific imagination that enabled ever longer estimates to seem to geologists first conceivable, then plausible, and finally inescapable.

CONCLUSION

The first edition of Lyell's *Principles of geology* thus introduced the terms Eocene, Miocene, and Pliocene in a form that superficially resembles other stratigraphical terms of the nineteenth century. On the surface they seem to denote a sequence of contiguous periods of geological time, each with a distinct fauna. It might seem that the only difference between these Tertiary terms and the terms introduced for other parts of the stratigraphical record was that the Tertiary strata generally lacked easily identifiable 'characteristic fossils', so that they had to be distinguished by reference to the over-all character of the whole faunal assemblage. This was, roughly speaking, Deshayes's conception of the Eocene, Miocene, and Pliocene; and the later history of the terms shows that most other palaeontologists and geologists interpreted Lyell's terms in this way.

I have tried to show, however, that such an interpretation profoundly mistakes Lyell's own original intentions. In his early letters and notebooks, and even in his explanation of his terms in the first edition of the *Principles*, it is possible to detect a quite different and profoundly innovative conception. Lyell interpreted the Tertiary earth-history of Europe in terms of a continual flux of the physical and biotic environment, in which areas of sedimentation—and hence areas of preservation of the fauna and flora—had shifted irregularly in an almost aleatory manner. All the while the composition of the fauna and flora had been changing continuously but gradually, by the piecemeal introduction of new species (by *means* that remained obscure) and the equally piecemeal extinction of old ones. From this extremely complex network of interrelated inorganic and organic change, the fossil record hitherto examined had, as it were, 'caught on the wing' a few spatially scattered samples from four temporally isolated epochs. These were the Eocene, Miocene, Older Pliocene, and Newer Pliocene. Further collecting would doubtless add new samples, but the record could not be expected ever to approach completion. But such new samples could be fitted into a quantitative framework of geological time already provided in outline by the known Tertiary molluscan faunas: the slow but broadly uniform rate of faunal change provided a chronometer for the whole of Tertiary time, which constituted the last faunal cycle or 'revolution' in organic life.

Lyell's dream of a 'statistical' palaeontology that could provide a quantitative chronology for geology faded very quickly. The complete faunal discontinuity between the oldest Tertiary (Eocene) and the youngest 'Secondary' strata (Maastricht) perhaps discouraged him from even attempting to carry out his original project of extending his 'chronometer' backwards in time beyond the last faunal cycle. Even for the Tertiary faunas, his quantitative method found little understanding and much criticism from his contemporaries; and Lyell gradually diluted his original conception of the Tertiary epochs, stressing 'the per-centage test' of age less and less, until the epochs became almost indistinguishable from other more conventional stratigraphical units (e.g. Lyell 1838, pp. 286–289).

This fading of Lyell's original 'dream' cannot be traced here, but one major reason for it can be summarized briefly. The percentage test depended on the strict comparability of the specific units on which it was based. Although Lyell, like almost all other naturalists, believed in the ultimate reality of species, the distinction between taxonomic 'splitters' and 'lumpers' was already well known. Lyell therefore emphasized that the value of his percentage figures depended on the fact that they were based on identifications made by one single palaeontologist, Deshayes, who could be presumed to have used the same criteria of specific limits throughout (Lyell 1833, p. 51). Problems arose, however, as soon as other palaeontologists of a more 'splitting' disposition started calculating the percentage of extant molluscan species in the same Tertiary faunas (see Wilson 1972, chapter 14).

Lyell's quantitative analysis of the Tertiary molluscan faunas therefore failed as a result of the empirical difficulty of defining the limits of intra- and inter-specific variation. It failed at precisely the same period—the later 1830s—when Lyell's younger friend Charles Darwin (1809–1882) was privately struggling with a theory of trans-specific change that converted the taxonomists' empirical difficulty into a theoretical answer (see e.g. Limoges 1970; Gruber 1974). Darwin's evolutionary theory was based precisely on pursuing the taxonomists' problem to its limits, questioning the assumption of Lyell (and others) that there must be *some* intrinsic limit to variation. Thus the failure of Lyell's project for a quantitative faunal chronometer, and the success of Darwin's project for explaining the origin of new species, were both parts of a much broader collective enterprise in which most naturalists in the 1820s and the 1830s were engaged.

Lyell's dream of a 'statistical' palaeontology is worth recalling because it records an important early attempt to construct a quantitative time-scale for the history of the earth, against which other events and processes, both inorganic and organic, could be plotted and measured. Although Lyell's attempt failed, his insistence on the vast span of geological time kept the problem alive. It was largely in response to his persistence that the project of constructing a geological 'chronometer' was taken up again in earnest in the second half of the nineteenth century, using a variety of mainly physical methods (Burchfield 1975). It was that tradition that led directly to the radiometric dating of the twentieth century, which has freed palaeontology from its subservience to stratigraphy and enabled it to 'come of age' as the temporal dimension of biological science.

Acknowledgement and note. I am greatly indebted to the Council of the Palaeontological Association for the invitation to give the Twentieth Annual Address. I have rewritten the lecture into a form more suitable for publication, while retaining the content essentially unchanged. At the specific request of some who heard the lecture, I have included fairly full cross-referencing to my other publications in this field, since most of them are in periodicals that will be less familiar to palaeontologists than they are to historians of science.

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M. J. S. RUDWICK

Unit for History and Social Aspects of Science
Vrije Universiteit
De Boelelaan 1083
Amsterdam
The Netherlands

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