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Some philosophical questions about paleontology and their practical consequences

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SUMMARY

This paper attempts to objectively discuss the actual state of paleontology. The progress of this science stood still somewhat during the latter part of the last century which caused it to develop apart from the biological sciences, an area to which it is naturally tied. As a consequence, this bruought a long-term stagnation, because biology, during this century, has made great progress and paleontology has not. During the last 20 years paleontology as a science has recuperated some of this loss, but we as scientists need to be careful in various aspects of its development.

RESUMEN

En este trabajo se ha intentado hacer un balance de algunos aspectos del estado actual de la paleontología. La historia de la paleontología muestra un estancamiento a finales del siglo pasado; este estancamiento hizo que nuestra ciencia fuera alejándose cada vez más del área de las ciencias biológicas, a las cuales estaba ligada naturalmente. Esto trajo, como consecuencia, un importante atraso, pues la biología, durante este siglo, ha dado un gran paso hacia adelante, mientras que este no ha sido el caso de la paleontología. En nuestro país --y posiblemente en muchos otros-- la paleontología es, fundamentalmente, una herramienta para datar estratos. Sin embargo, una mala herramienta, pues la datación está basada en las especies fósiles y su determinación se fundamenta en el concepto de especie biológica; los fósiles, pero, en muchas ocasiones, son usados más bien como sellos o monedas. Salvo excepciones, incluso numerosas, en la práctica diaria de la paleontología faltan conceptos teóricos --mucha gente es impulsada a hacer trabajos paleontológicos sin conocimientos básicos de la biología de los grupos que tocan, ni de biología en sentido amplio--, lo cual da como resultado trabajos que, desde un punto de vista científico, son inconsistentes. Creemos que la paleontología es una ciencia y que su interès fundamental es la dimensión histórica de la vida, es decir, el núcleo de la paleontología no se puede realizar sin distinguir los acontecimientos «post-mortem» reflejados en los fósiles; es decir, sin tafonomía. Por otro lado, la historia de la vida sobre la tierra queda reflejada en los estratos ordenados en el tiempo, y esto es estudiado por la paleontología estratigráfica, cuyos resultados son muy importantes para la paleobiología. Tafonomía, paleobiología y paleontología estratigráfica constituirían pues la ciencia de la paleontología. Ciertamente, existe un paradigma para la paleobiología y existen los textos divulgadores de aquel, pero no ha tenido éxito. Y, desgraciadamente, en muchos casos —con excepciones, claro— se utiliza con la misma frivolidad con que se utilizaría una moda, pero sin cambios en las ideas de base, que en general tienen un mal fundamento teórico. Para empezar, se debería tener en cuenta algunas premisas muy básicas para los trabajos paleontológicos que se empiecen a hacer a partir de ahora: así el estudio tafonómico, la población y las muestras, el no caer en el abuso del actualismo y del uniformitarismo sustantivos, y últimamente, el uso crítico del aparato estadístico. Sin estos puntos de partida la paleontología no se podrá desarrollar. Sin embargo, la paleontología puede dar, al igual que en otras épocas, la visión de fenómenos biológicos a gran escala a nivel temporal. La biología, como tal, debería ser una síntesis de paleontología y neontología, lo cual debería reflejarse en la organización de los estudios universitarios de biología, a pesar de que en muchos casos nos encontramos con paleontólogos procedentes del campo de las ciencias geológicas.

RESUM

En aquest treball s'ha intentat fer un balanç d'alguns aspectes de l'estat de la paleontologia. La història de la paleontologia mostra un estancament a les darreries del segle passat; aquest estancament va fer que la nostra ciència s'anès allunyant cada cop mès de l'àrea de les ciències biològiques, amb les quals estava lligada naturalment. Això va portar, com a conseqüència, un important endarreriment, car la biologia, durant aquest segle, ha donat un gran pas endavant, mentre que aquest no ha estat el cas de la paleontologia. Al nostre país —i possiblement a molts d'altres— la paleontologia ès, fonamentalment, un estri per datar estrats. Tanmateix, un mal estri, car la datació ès basada en les espècies fòssils i la seva determinació ès fonamenta en el concepte d'espècie biològica; els fòssils però en moltes ocasions, són utilitzats mès aviat com si fossin segells o monedes. Llevat d'excepcions, fins i tot nombroses, a la pràctica diària de la paleontologia li manquen conceptes teòrics —molta gent ès impulsada a fer treballs paleontològics sense coneixements bàsics de la biologia dels grups que toquen, ni de biologia en sentit ampli—, la qual cosa dóna com a resultat treballs que, des d'un punt de vista científic, sòn inconsistents. Creiem que la paleontologia ès una ciència i que el seu interès fonamental ès la dimensió històrica de la vida, ès a dir, el nucli de la paleontologia no es pot realitzar sense distingir els esdeveniments «post-mortem» reflectits als fòssils; ès a dir, sense tafonomia. D'altra banda, la història de la vida sobre la terra queda reflectida als estrats ordenats en el temps, i això ès estudiat per la paleontologia estratigràfica, els resultats de la qual són molt importants per a la paleobiologia. Tafonomia, paloeobiologia y paleontologia estratigràfica constituirien doncs la ciència de la Paleontologia. Certament, existeix un paradigma per a la paleobiologia i del qual n'existeixen els textos divulgadors, però no ha tingut èxit. I, malauradament, en molts casos —amb excepcions, ès clar— s'utilitza amb la mateixa frivolitat amb que hom utilitzaria una moda, però sense canvis a les idees de base, que en general tenen un mal fonament teòric. Caldria, per començar, fer compte d'algunes premises molt bàsiques per als treballs paleontològics que es comencin a fer a partir d'ara: aix l'estudi tafonòmic, la població i les mostres, el no caure en l'abús de l'actualisme i l'uniformitarisme substantius, i darrerament, l'ùs crític de l'aparell estadístic. Sense aquests punts de partença la paleontologia no es podrà desenvolupar. Tanmateix, la paleobiologia pot donar, al igual que a altres èpoques, la visió de fenòmens biològics de gran escala a nivell temporal. La biologia com a tal hauria de ser una síntesi de paleontologia i neontologia, la qual cosa s'hauria de reflectir en l'organització dels estudis universitaris de biologia, malgrat que en molts casos hi ha paleontòlegs procedents del camp de les ciències geològiques.

INTRODUCCIÓN

Paleontology is a body of very complex knowledge in our century. This body is expanded on many fields and this is the cause, at the present moment, of the necessity of drawing any kind of schema which shows us the relationships among these fields. At the same time, this schema should show us the hierarchy of such relationships, which are established among the diverse studies made by the paleontologists. The absence of such a vision can result in a bad and useless work, because the paleontological research is carried on without giving previously some necessary footsteps. These questions are very well set in other sciences; nevertheless, in our poor paleontology, it is not so (or it is not so in such a way in many cases). Perhaps, it is necessary to ask if the word «science» is applicable to this body of knowledge called paleontology. I have the belief that paleontology is a science or may become a science, but the conditions in which this is possible must be specified.

Under the word «paleontology» there are other areas of knowledge, and they are really sciences, but the very paleontology may become a science too. Fossils —the primary data of paleontology— are geological as well as biological objects. A student may be paleontologist becoming a graduate in geology or in biology. That is possible because fossils are ambivalent objects. Fossils are found in sedimentary rocks and they are useful for important geological aspects: age of strata and indications about the sedimentary environment in which were formed the sediments where the organic remains were buried. Simultaneously, the fossil is another component of the sedimentary rocks. The actually fossilized organism could live in the place where the sediment which buried it, after its death, was deposited. But the organic remains of living organisms, after the death, could be transported and deposited in another different place from that where they lived. The energy of the sedimentary environment would be the cause of such a transport. In the new place, if sedimentation was active, the transported remains would be buried. This last feature makes fossils similar to inorganic clasts of the sedimentary rocks, while the former makes them similar to their authigenic components. On the other hand, the organic remains, before becoming fossils, play a role in the diagenesis of the sediment, because they have a mineral and an organic component which react with the other materials of the sediment until reaching chemical equilibrium. All these considerations make fossils interesting things in geology.

But fossils are more important as biological objects. Their very nature is the principal reason for this statement: they are the remains of old organisms and these belonged to populations of determinate species; their form was adapted, in general, to an environment and accomplished concrete functions in their ecological niches. These populations underwent changes in the time; i. e., they evolved. Such populations, on the other hand, were part of the old ecosystems; these ecosystems changed in specific composition with the time too; this change is called ecological succession.

All these reflections show that paleontology takes its knowledges from the fields of biology and geology. At the first glance, it is not autonomous. However, there are no autonomous sciences. Sciences like mathematics or logic, which apparently seem to depend only on the mind, are not autonomous; its foundations are derived from certain kinds of experience (Piaget, 1970). The problem arises in the primacy of treatment. Which of the aspects of fossils is more important, the geological or the biological one? The solution to that is not a manicheistical one. On the other hand, I claim to show paleontology, and concretely, paleobiology, as a possible science with specific points of view.

Fossils are actually in sedimentary rocks and we interprete them now as organisms that lived in an ecosystem placed in an area of the Earth surface where sedimentation occurred. The forming sediments buried the dead organisms of the ecosystem and they present themselves together as rocks and fossils in some place of the Earth. Man has not always considered this as an obvious truth; I shall speak about the reasons of it in the next pages. The interpretation of fossils as organic remains seems today as something of «common sense» —I use the term «common sense» with all the reserves—, but this interpretation raises a new problem: the interpretation of the biological and geological phenomena of the past with the key of the present phenomena.

In this way, we can ask ourselves about the degree of uniformity of the Nature during the times; in more familiar terms: is it valid the uniformitarianism? Paleontology can use information derived from the actual biological and geological processes, but we may only consider certain properties as uniform ones during the times. It means that the uniformitarianism must be applied in a restricted sense. This raises a new question: for the most of the paleontologists, fossils appear as pure facts. Rudwick, Gould and other authors have fougth against this coarse empiricism. I shall try to illuminate this with opinions taken from the philosophy, methodology and history of science.

A debate is opened. Perhaps, many things presented in this paper are very discussible. I shall comment the themes that I have presented in this INTRODUCTION and that I judged as principal ones. I hope that this paper could be useful for opening the discussion, but it does not cover all the possible and important themes, and I am conscious of its mistakes. This discussion is necessary at the present moment in paleontology, because the large amount of paleontological papers accumulated in this century requires a reflection about the bases —they are not always clear ones— of our work.

WHAT IS PALEONTOLOGY?

We could answer this question in two very different ways: in a genetic one and in an actual one. I think that the history of a science could bring more light on the very nature of it than a simple and an atemporal definition. It is for this reason that I try by a genetical way to answer this question.

Rudwick (1972) has supplied the materials for such a genetic response; i. e., by telling us a history of paleontology. This history shows as many concepts and responses to questions set up by the fossils, that we see now them as obvious ones and of «common sense», had been discussed during more than two centuries.

Rudwick begins his history at the moment in which Gesner, in the second half of the XVI[th] century, published a little opuscule about the nature of fossils. The main problem in that epoch was, above all, the classification of the «fossil objects» in a spectrum which was going from the organic to the inorganic. The causal explanations of the form of fossils were secondary ones. On the other hand, the neoplatonism and the aristotelism supplied sufficient explanation for the thinking of that epoch. The neoplatonism interpreted fossils with organic form as an effect of the «*sympathy*» between stones and living organisms, but did not interprete them as *remains* of living organisms. As Rudwick remarks, it is an alternative interpretation. The aristotelism saw fossils as the result of the growth of «seeds» or «gems» of the corresponding living organisms, in the fissures of the rocks of the Earth.

After a century, neoplatonism and aristotelism were overcome by a new form of interpreting and understanding the reality: the *scientific method* developed principally by Galileo Galilei. In this method, there is a neat separation between observations and hypothesis or explanatory conjectures with reference to a concrete problem. Such a form of conceiving fossil objects was carried to the end by Stensen; for this naturalist, then, fossils —objects resembling living animals or vegetals— were only more or less altered remains of old organisms; these remains were subjected to some kind of petrifying action, which would explain their stony aspect. At the same time, he understood the nature of the fossiliferous strata sequences and conceived them as a prove of the history of the Earth and the life on it. Hooke was the campion of these new ideas in England. However, the precarious knowledge of living organisms —in those moments, *Nautilus* was not yet seen, for instance— and the materials from which many fossils were formed —very strange, if the origin of fossils was a biological one— were an impediment for recognising fossils as remains of old living organisms.

On the other hand, natural theology was ambivalent: Nature does not play and its acts are useful; thus, a stony object with shell form (a fossil) would not be a ludic activity of the Nature, but a thing which served for protecting an animal. Therefore, it was correct to interprete fossils as organic remains. But, on the other hand, natural theology considered living species as the most perfect work of the Creator. If fossils should be interpreted as remains of old living organisms, then there should have existed extinct species today, and this possibility of extinction would be an imperfection of the Creation and its Creator. Natural theology was very important in England as a frame of reference of thought and science, as neoplatonism and aristotelism, for instante, were important in the same sense during the XVIth century. The debate and the discussion were opened among the competent naturalists. Fossils, as interpreted as organic remains, showed that species could become extinct; at the same time, *fossil remains had no living similar relatives in many cases.* To accept them as organic remains was difficult for all these reasons.

On the other hand, biblical chronology raised a lot of problems to the students of these questions. However, biblical chronology impelled the research of the origin and history of the Earth. Nevertheless, near the end of the XVIIIth century, the organic nature of fossils and the temporal broadness of the history of the Earth were already irreversible concepts. At the same time, in this epoch and at the beginnings of the XIXth century, Cuvier set seriously the question of the extinction of the species. He supplied important proofs by means of the big fossil quadrupeds, because the big living quadrupeds were well known in all the world. In this research, he made use of his new form of conceiving the study of the animals: the comparative anatomy. This one claimed to be a reflexion of the rigour of the Newton's mechanics as well as the conceptions of Bacon's empirical philosophy, and the ability of reduction of the variability in a classification, supplied by the works of Linne. Cuvier wanted to explain the extinction of the species by means of catastrophical events or *revolutions* —which were nothing more than a translation into the Nature of something which was a familiar one for the men of that epoch: the political revolutions—, which were not worldly ones. Species which replaced the extinguished ones came from other areas —not affected by the revolution— by migration. Cuvier distinguished several revolutions in the history of the Earth, and he conceived this history like a «steady state» as in Hutton's ideas. Revolutions would be sea invasions on the Earth, which would alternate with calm periods. Hutton's ideas would be in accordance with Newton's image of the Nature (undirectional changes). The same concepts dominated the geological science of Lyell.

Both Lyell and Cuvier became conscious about the need of setting the opposite but complementary problem of that of the extinction of the species: the problem of the origin of species. Cuvier, however, thought that the moment of setting this problem had not yet come, because he judged not having enough data for it. Nevertheless, for Lyell, the problem of the origin of species could not be resolved in that moment, because no *actual similar process* was known.

However, there had already been attempts of formulation of hypothesis on the origin of species during Cuvier's time. One of them was Geoffroy Saint-Hilaire's. He conceived the transformation of species as oriented by a directional environmental change, from a warm and uniform climate towards cold and diversified one through the history of the Earth (this came from the old theory of the cooling of the Earth), which influenced —in an irreversible way— in the transformation of living species of the Earth. This last one reflected a lamarckian concept of life and, the most remarkable: the diversification of faunes and flores was a direct function of the environmental diversification produced by the progressive cooling of the Earth. On the other hand, the whole process showed a strong random aspect.

These random features of the evolutionary thinking were not compatible with the model or image of life of that epoch. Life was something perfect and, above all, this perfection was shown, in a striking way, in the adaptation. The argument of designfulness was a common place and the naturalists did not understand how perfect adaptations could arise by chance. Another problem was the place and the origin of Man. If Man would have had an origin by chance, he would not have been morally responsible for his actions and this would be cause of trouble of the whole society, which was built on this foundation. However, during the coming of Darwin's theory, the opposition was, above all, of scientific character, because it was not seen the sufficiency of the explanations by means of the natural selection theory.

In the middle of the past century, the german geologist Bronn studied the fossil record in a phenomenological way. He tried to see its regularities and from this study inferred the existence of *creative forces* («Schöpfungs-Kraft») of life. The character of these forces was considered as a physical one, not vitalist. These forces would be of the same nature of gravitation or chemical affinity. They were *creative* because they would make innovative changes. Observing its effects, it would be possible to know something about its way of action and its very nature. From alle these aspects, Bronn concluded two laws for the development of life on the Earth: an *intrinsec law* and an *extrinsec law.* Wallace had observed regularities in the fossil record too. For instance, for a given species there was, in an immediately anterior time in the fossil record, another species very similar to the former. When Wallace elaborated, with independency of Darwin, his evolutionary ideas, he had no difficulty to explain that regularity as a descendency relationship.

However, the origin of species rested as a problem before Darwin. The theories of Geoffroy Saint-Hilaire and Lamarck were seen as a very inconvenient at philosophical level. This was the reason of the emerging of alternative theories. One of them was the theory of the *archetypes* (of platonical inspiration) of Owen, which gave explanations for many phenomena in the general thinking perspective of the middle XIXth century.

On the other hand, Darwin did not see how the fossil record could supply any test to his evolutionary theory. This prejudice was a consequence of his lyellian conceptions. However, other authors obtained good proof of the evolutionary theory from the fossil record. We could cite Gaudry and Huxley. Nevertheless, although evolution was already unquestionable, the explanation of its mechanism did not satisfy. The reasons of such an unsatisfaction were, in first place, scientifical ones, and in second place, ideological ones. The state of knowledge for deciding an evolutionary mechanism was judged unsufficient and the scientific community preferred to avoid the explanation based on natural selection for the origin of species. Other alternative mechanisms emerged as evolutionary theories. The *directionalist* theories (finalists, etc.), which called only for *internal* mechanisms as explanation of organic change —internalism and directionalism are not necessarily depending— were an example of such alternatives (Gould, 1977). These alternatives were characterized by its image of life as something different from the rest of the physical world; i. e., as vitalistic conceptions.

In the second half of the XIX^th^ century, however, the naturalists claimed to «read» the fossil record in an evolutionary framework. Terms like adaptive radiation were born in that epoch. But emerged too speculative undesirable concepts, like the recover of the «scale of beings» concept and its reconversion, by Haeckel, in the biogenetical law.

The Rudwick's history ends in the second half of the XIX^th^ century. From the same author, a history for paleontology in the XX^th^ century would be a complex one —and I agree with him— and it is still for making. Unfortunately, there are not yet historical studies for this last period. In the onnly one which I know (Valentine & and Campbell, 1979), the authors recognize that their study is only historiographical.

If we observe this Rudwick's historical study, we could observe that the first problems which emerged about fossils, were problems of classification. Fossils should be placed in a broad spectrum from these with a neat organic appearance to those with a purely inorganic aspect, as crystals. It did not preoccupy very much, in the XVI^th^ century, that similarity with living beings could be an indication for the possibility that fossils mere organic remains in its origins. To ad mit this possibility for fossils with appearance of marine organisms brought perplexity to many students of fossils, because it showed them that there could have existed a different distribution of seas and continents. At the end of XVII^th^ century, many authors accepted that fossils of neat organic appearance should be considered as remains of old organisms, but it led to think that, in other times, there were species actually extinguished. This was conflictive with natural theology, which considered species as something perfect and thus, imperishable. In the XIX^th^ century, it was clear that species could become extinct ones, but at this moment emerged the problem of their origin, which the naturalists tried to solve by means of creationist hypothesis (secondary causes arbitred by the Creator) or evolutionary hypothesis. These last ones entered in conflict with several features of the «spirit of the epoch».

This little summary of the history of paleontology is very important for realizing that the same «facts»; i. e., fossils, have been considered in very different ways according to: a) different scientific theories, and b) different philosophical, theological or, even, political conceptions which, at the same time, influenced the status of scientific theories or the models valid in each epoch and which served for interpreting the meaning of fossils. We should not be surprised that before a systematic knowledege of the living world, it was difficult to distinguish the living things from those which were not living ones. This means that it was not very important for the students of the Nature in the XVI^th^ century, to recognize the organic origin of fossils.

On the other hand, Rudwick's book has shown us that, in each moment of the history of the study of fossils, it is given a conceptual schema about them, according to the status of the knowledge and the «spirit of the epoch». A conceptual schema is a theoretical body which is developed from observations of facts, even though it transcends them. According to Kuhn (1957), the functions of a conceptual schema are of two kinds: a) *logical ones*, and b) *psychological ones*, and its evolution as such a schema depends on the form in which it accomplishes its functions. Conceptual economy would be one of its logical functions; therefore, the naturalists of the XVI^th^ classified the fossil objects and thus reduced the variability inherent to them: the individuals were grouped forming classes and this involved conceptual economy; on the other hand, in a neoplatonic image of the world, fossils represented general affinities with other elements of Cosmos. Psychological functions depend on the beliefs or the incredulities of the scientifist at the time in which he is working (we must remember the case of the *intranquility* produced in the scientific circles by the trouble represented by the acceptation of the possible extinction os species that, together with biological reasons, avoided the recognization of fossils with organic aspect as organic remains of old organisms until the XVIII^th^ century). However, as stated by Kuhn (op. cit.), neither economy nor satisfaction, which the conceptual schema is able to give to the scientifist, are guarantees of its adequacy with the reality.

I shall come back on these grounds and insist deeper in this paper. We can see now that fossil students had, in each epoch, a conceptual schema of their materials (fossils) and such a schema was according to its time (to the intellectual, scientific and ideologic frame of its time). There could be isolated advanced conceptions or new facts, but in this case, they passed without notoriousness, because the epoch had no capacity for integrating them in its general framework of thinking.

I think that this is the best form —and the most instructive— of conceiving the history of science: we must not look for the «highway» that runs from the Old age up to a concept or theory that we consider correct today, but we must look for, in each time, how objects became accessible to the analysis (Jacob, 1970). I remark that this «to become accessible to the analysis» is, in many cases, a simple change of vision of the things, as says Jacob, and *it does not necessarily coincide with a technological innovation which allowed us to obtain new sources of data.*

It we return to our history, Rudwick (1972) points out an important change of vision at the end of the XIX^th^ century: the search for mineral and energetic resources and their exploitation made of paleontology an important tool for stratigraphy, wich is intimately related with this search. This made paleontology to be transformed into a mere instrument for dating strata; this situation goes on still in general today. The consequence of it is that paleontology is farther and farther from the field of the biological sciences, which were very related to that un til the second half of the XIX^th^ century. This fact contributed, by speaking with the same words of Rudwick, «*to narrow its intellectual horizons*».

As we have seen throughout this history and the last remarks, paleontology should be considered like something closely related to biology. The intense use of fossils for dating strata only served for deviating paleontology from its true scientifical interests, including stratigraphical paleontology. We can see that such interests had been placed on the grounds of biology. The use of fossils for dating rocks had begun very recently, in the first years of the XIX[th] century. William Smith and Georges Cuvier were the first naturalists who tried the correlation and dating of sedimentary strata; Cuvier was interested in such an objective because he looked for evidence in order to prove the very nature of his «revolutions».

We can now answer in a more concrete way the question: what is paleontology? *Paleontology*, we could say, *is fundamentally paleobiology*, or in other terms, *it is the study of the historical dimension of life on the Earth.* As Rudwick (1972) and Gould (1977) state, the problems of a determined science are well already set from the first men who worked seriously in that area. Thus, the classification of fossils was one of the first problems faced by their students, but today, systematics is a very important activity. The ecological and functional problems raised by bossils preoccupied Cuvier and other naturalists; the problem of the origin of species was more and more urgent when the fossil record was studied deeper and more and more extinct species were discovered. On the otjher hand, the use of fossils for the chronology os the history of the Earth was already made by Stensen. As a last question, how fossils were formed was one of the first problems already set since Gesner's times.

As a conclusion, we realize that paleontological problems are of three basical kinds: those referred to the origin of fossils, those raised by its biological meaning and those raised by their position in the geological time. This last one is very important when we want to consider life as a temporal and evolutionary process. Therefore, paleontology can be conceived as it follows:

1. The study of the way how organisms pass from the biosphere to the lithosphere, or *taphonomy.*
2. The study of the biological meaning of fossils, or *paleobiology.*
3. The study of the place of fossils in the geological time, or *stratigraphical paleontology.*

We should remark that paleobiology is the nucleus of palentology; when we know how is produced the fossilization —or taphonomic process— of the remains found in a deposit and which its place in the geological time, the problems raised by those fossils will be absolutely biological ones. Only the resolution of such problems will allow to use fossils in geological applications, such as more precise subdivisions of the geological time. If we work in this way, paleontology shall come back to occupy its place as *biological science the Earth,* which was lost in the second half of the past century, when paleontology became a simple and rutinary instrument of application to geological problems.

In the next paragraph, I shall justify and make clear all these statements.

MUST THE ACTIVITY OF THE PALEONTOLOGIST FOLLOW AN ORDER?

The basic problems of paleontology, as we have set them at the end of the last paragraph, do not suppose an indiscriminated accessibility. We can distinguish here a hierarchical structure, although there are some feed-bak in it. It is important to remark that it is impossible to make paleobiological studies without having previously realized the taphonomic study. This has been stated several times (Lawrence, 1971; De Renzi, et al., 1975; De Renzi, 1978) and it is necessary to attack in this way the problem. However, to recognize the diagenetic aspects suppose a previous paleobiological knowledge: that of the chemistry and mineralogy as well as the microstructures of the hard parts of the organisms, which we found posteriorly fossilized. This means that those aspects (chemistry, micrôstructures, etc.) have to be considered as reasonably invariable for each phylum. For this reason, Sorauf (1971) had doubts about the primary mineralogical nature of the Rugosa. The problem was that a very well preserved specimens of rugose corals, showing very delicate features of the septa, were preserved in calcitic material. In Scleractinia, all the members of the order build their skeletons with aragonite, and they have trabecular microstructures as well as the rugose corals object the considerations. Thus, there arises the problem of if trabecular microstructures could only be constructed with aragonite or it is possible to build them with calcite too. The conservatism of aragonite for actual and fossil (no Rugosa) trabecular skeletons of corals is a motive of the discussion of Sorauf's paper, which deals with the possibility of a diagenetic ion-to-ion process. In the Conodontophorida, the radular hypothesis was discussed in one of its points, because conodonts are phosphatic ones, with evidence of primary mineral, while mollusc radules are chitinous ones, although convergence between radules and certain assemblages of conodonts could be remarkable (Tasch, 1973). A last example: when in a fossil mollusc is seen a layer of sparry calcite among prismatic or foliated layers, we consider, with whole probability, that it was aragonitic material and that this shell was a bimineralic one, because it is never found a mollusc or other remains of hard animal parts with a sparitic microstructure.

All these examples shows us how certain kinds of paleobiological aspects supply information about taphonomic processes. It is not only about diagenesis, but biostratinomy too; there are some biotic aspects which can tell us something about transport. For instance, the organisms in life position; determined organisms have positions not due to mechanical origin during life; thus, many infaunal bivalves live buried with the commisure orthogonal to the surface of the sea floor; this floor will become the surface of a bed or paralell to it. This position is an anomalous one; if in a fossiliferous bed this position is repeated with a high frequency for all the individuals of the same species and these are more or less regularly separated among them by the sediment, this concludes that, with security, such specimens were in life position and we can consider them as autochtonous; i. e., we are faced with elements of a paleobiocenosis. An example would be *Pseudomiltha* (?) *corbarica* (Leymerie) from the beds of the lower Ilerdian of Tremp (Catalunya, Spain) (De Renzi, 1975).

Nevertheless, the taphonomic study must precede not only the paleobiological one, but the biostratigraphic too. The application of the criterion of curves of size frequency is well known (the distinction —by means of these curves— between paleobiocenosis and thanatocenosis, as explained by Johnson (1960) and Fagerstrom (1964)) in taphonomy. Really, these curves masked paleobiological features which *must be elucidated a posteriori* of the taphonomical study. Raup and Stanley (1971) show how these curves are the result —in a biocenosis— of the combination of a growth curve and a survivorship curve; so, these curves can be explained as biological phenomena and not as demostrative of a transport, which is a purely sedimentological event. We must remember that a good biostratigraphy must recognize the autochtonous, resedimented or reworked character of the fossils used in order to date strata. In this sense, we have two papers on jurassic biostratigraphy that distinguish taphonomic events as useful ones in the biostratigraphic interpretation (Fernández-López and Suárez-Vega, 1979; Fernández-López, 1979).

All these questions bring us to speak about certain properties of organisms —and of many biological phenomena— as reasonably invariable during the geological time. For this motive, we can use them as orientative in the research of taphonomic phenomena, but it is in a minimum grade.

If we reconsider the history of paleontology, we can observe certainly, that the first problems set by the fossils were those of preservation or origin (today, we would speak about them in taphonomy). On the other hand, we would have problems about the situation of fossils in the geological time. These problems drive directly to those of the extinction and the origin of species and also to the problems of adaptation and so on. Thus, we can recognize the priority of placing fossils in a determinate chronological scale. However, this makes clear that *the nucleus of paleontological problems is of biological nature.*

Why is it necessary, in most of the cases, a chronostratigraphy before attacking paleobiological problems? Because many of these problems suppose *processes* and these last ones are developed during the time. On the other hand, similar processes could take place in very different temporal intervals. If we do not precise our chronology, we could easily confuse two different processes in only one. For instance, speciation events, as conceived by Eldredge and Gould (1972) and Gould and Eldredge (1977), involve the knowledge of the paleobiogeographical distribution of species during temporal intervals well known. Paleoecological problems, at a synthesis level, require good datations; so, opportunistic species, for instante, are restricted to narrow chronostratigraphic intervals (Alexander, 1977). It should not be necessary to say that paleobiogeography studies the distribution of animal or vegetal groups and its causes in the whole world, or in a part of it, during their existence in the geological time.

RELATIONSHIPS OF PALEONTOLOGY TO ITS CLASSICAL AND MODERN APPLICATIONS

The dating of strata is made by means of fossils too. We must remark that this application of paleontology is an important one when we want to make precise divisions in the geological time. As a logical conclusion, the disposition of the biological events on the Earth in a precise order, requires this application, although it would not be possible without the basic body of paleontology. The fundamental problem of such a kind of dating strata is that of having, in each moment, species with broad spatial distribution and narrow vertical distribution, or with well known point of departure, although the vertical distribution was large. However, important features of the actual species remain unknown for many paleontologists, and this is unfortunately a fact. These problems, in the case of fossils, become paleobiological ones and the lack of knowledge of many practisers of «applied paleontology» in this field has not allowed, in many cases, a good use of fossils as time markers. So, such important features like the polymorphism of the species and its related consequence, the clines, which in many cases are confused with phyletic evolution (with a gradualistic character). This is the consequence of a naïve «reading» of the sedimentary and fossil records in their vertical dimension and it could be (and it is still) the cause of many mistakes when fossils are used in biostratigraphy, as remarked Eldredge and Gould (1977).

I want to set another important question in this comtext: the evaluation of the absence of an index fossil. It can be caused by two factors: I) Ecological or biogeographical conditions, which prevented that organism to live in that area. II) Biostratinomical or diagenetical conditions (which belong to taphonomy) which prevented the preservation of that organism. In this last case, we must be prudent —it is necessary when we are in doubtful or limotrophy situations—, because we shall not be able to decide surely its absence.

On the other hand, plate tectonics, paleontology, and their relationships with dating of rocks are very complex. The sin of many biostratigraphers is to forget that «the stratigrapher-paleontologist observes age relationships only in a local section where visible superposition establishes a succession of species. These successions form the central pier of correlation; *all else is deduction and hypothesis*» (Beerbower, 1968; the underlined of his text is ours). In a given moment of time —delimitated by means of common index fossils— we can know, by means of distributions of concrete organisms, the degree of union or separation of plates. However, this would require precise systematic determinations of species of the groups with which we deal. We must also evaluate the absences, referred to impossibility of presence, because environmental conditions are not available in a determined place as well as by the action of the taphonomical process. In this last point, Raup and Marshall (1980) remark as Marsupialia and Rodentia do not give easily preservable forms; it is not so in the case of Perissodactyla.

Lately, since a few years ago, it is frequent the research on the geophysic aspects of past geological times, concretely about Earth's rotation. We must remark several authors and papers as pioneers in this field, as Farrow (1971, 1972) up to those published in Rosenberg and Runcorn (1975). It is interesting, in the first place, to place chronologically the astronomical events, and for this it is necessary to obtain good chronostratigraphical datations. But the crucial question is to recognize lamellae or growth rings in bivalves, corals and other groups. We must evaluate the seasonal or daily character of the production of such features; it is a biological (and paleobiological) problem, but taphonomy must indicate us the primary or diagenetic character of the structures in this evaluation. The obtained results shall have also paleontological consequences.

All these questions are not the only ones of the very broad extension of the relationships and hierarchies among the several fields of paleontology and the relationships among paleontology and its classical or more modern applications. However, paleobiology, which is the central point where we are always going to end up, appears to us as a black box. The dilucidation of the origin of fossils involves fundamentally sedimentological data, because the dead organisms behave in an inert way; the environmental agents play with them as if they were clasts. On the other hand, the basic philosophy of stratigraphical paleontology is, since Stensen, the strata sequence, but the correct determination of fossil species is the most important in stratigraphical paleontology, but it is a *paleobiological problem.* In a later paragraph I shall speak about paleobiology as a central part of the paleontological science. Nevertheless, before I attack this problem, I shall have to discuss broadly some previous aspects involved in the present paper. Figure 1 is an attempt to show a schema of relationships among the parts of paleontology and between paleontology and some of its applications to geological sciences.

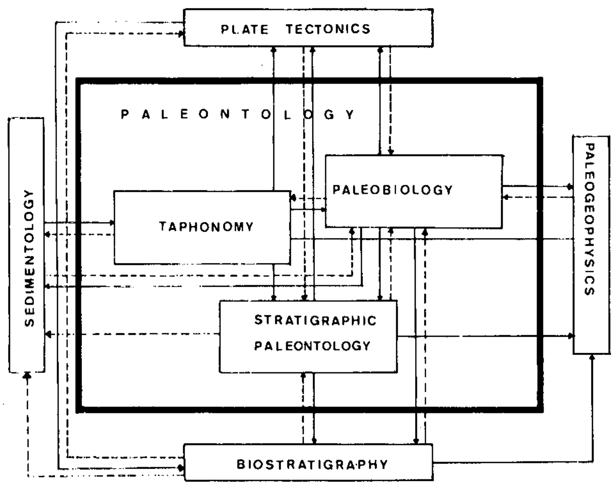

Fig. 1. Some possible relationships among the diverse parts of paleontology, and among these and some of its applications to geological sciences. The continuous arrows indicate the primary sense of the relationship. The dot lines indicate the need of the relationship in inverted sense, when the first type of relationship has already been established.

INTERMEZZO: THE NATURE OF SCIENCE AND THE SCIENTIFIC METHOD

Our principal preoccupation is the scientific character of this that we speak of under the name paleontology. We have just said that paleobiology would be its central part. It is for this reason that we must already attack —before trying to establish the nature and methods of paleobiology— two questions: a) What is science? and b) What is the scientific method? It is evident that I do not treat these themes in its whole broadness. Such a matter would be above this simple paper. On the other hand, the exhaustive treatment of these themes overcome completely the author's competences. However, I think that every scientist should set himself some basic reflections about science and the scientific method. This should not be a spontaneous matter, but a serious one. Every scientist has a philosophy of science, and this philosophy guides him —although, in many times, he is not conscious of it— in his form of acquiring knowledge from the world. In many cases, it can underlie a very coarse empiricism and pragmatism in the way of acting of many scientists. At the same time, they seem to ignore the name of their attitudes; these ones are not philosophical questions for them, but obvious bases of science.

In this paragraph I want to give a vision of the nature of science, which could be useful for making to advance paleontology as a science. I have said that the problem is complex and it is not meant for giving an exhaustive vision of it. Nevertheless, I want to centre the problem in two fundamental disjunctives: there would be two basical alternatives about science; the first of them states that we can «read» directly in the Nature, while the second one states that such a direct «reading» is not possible. For those who support the first alternative, the induction —for deciphering and understanding the world which is around of us— is enough. The induction is accumulation of particular cases and, from them, obtention of general statements; for the inductivists, scientifical theories are only a summary of data and can be directly obtained from the tables of numbers coming from laboratories (or observations in the field). This means that theories are «discovered» and not «created» (Bunge, 1973). This position would be impregnated of *empiricism,* because the direct «reading» of the Nature involves a faith without limitations in the «pure» observations and facts. This faith is the cause of the fact that scientists who embrace it, think that concepts in a determined science have only meaning in the case which they can be defined operationally. Thus, any concept not defined by means of empirical operations, has no meaning in that science —this has been shown in physics by Bunge (1973).

On the other hand, the second position claims that the reality is not given to us immediately, but the resolution of the scientifical problems requires hyothesis which go farther on of facts for explaining them. From these statements, all observation (Popper, 1958) is brought under the light of theories (it is the opposite of the inductivists, who believe in a language which speaks about facts (phenomenistic), free of theories). The previous Popper's statement is paraphrased by Eldredge and Gould (1972): «all observation is colored by theory and expectation».

Must we believe in any of these alternatives? Which is the state of our science? Paleontology has gone through a long way —during our century— in the field of its biostratigraphical applications, as we saw at the end of the summary of Rudwick's history, in this same paper. Biostratigraphical research was needed in an utilitarian way and paleontology was limited to the search of fossils in order to date rocks. This involved, in a large scale, a direct «reading» of the fossil record and the primacy of observation above theorization (search for oil, with the competition of rival companies, was not the ideal situation for a correct practice of science). However, paleontology, as a science, had not gone through its way; paleontology depended on the development of biology and that of the related geological disciplines; thus, the «reading» of the fossil record could be mistaken by preconceived and unverified ideas. Therefore, the use of fossils could give good results in a short period, but in a long period, serious incoherences could be detected. Such incoherences —as I shall show in the following paragraphs— I think that are due to the lack of a good theory which rules the search for facts and observations, and which was susceptible of criticism for its foundations. On the other hand, incoherences have a theoretical ground: that which ruled when the development of paleontology was stopped as biological source of problems. Here, history of science is useful again, because it shows us a development —a *rational reconstruction* of it— and from it we can judge or evaluate two rival methodologies (interpreting such a development *normatively*; i. e., from a determined methodology; see Lakatos, 1971) in a manner that we can see which way is a cul-de-sac and which abandoned way, if we come back it, can lead to real progress in the considered scientifical field.

The idea of the scientifical method, which I am going to expose here will be that which gives priority to the theory and does not search for data and observations only for itself. Such an idea is a summary of the methodological concepts exposed by Bunge (1969), but acctually they come since Galileo Galilei. Briefly, we can say that science begins with *problems*; a problem is set when in a field of our knowledge there arises a question which is not explained by the previous knowledge (for instance, a new object —what is this?—; localization of something —where is this?—; how it takes place the action of something?, and so on). From here, we shall make conjectures which allow us to find the solution of the problem (for instance, to suppose that the action of something is the succession of a series of well specified events imagined by us); these conjectures are called *hypothesis.* However, and this point is essential, hypothesis must be *tested.* Hypothesis cannot be made in whatever way, as several currents of philosophy of science state, but *they have to be compatible ones with the body of relevant knowledge in the moment in which we formulate the hypothesis.* For testing hypothesis, we shall arbitrate *techniques* —this is our true link with experience, which is *fully* necessary; *we cannot build a factual science with only paper and pencil—* which can be *empirical* or *conceptual* ones. These techniques are available in the previous body of knowledge. Empirical techniques are referred to the use of instruments, chemical reactives, and so on, for throwing light on the validity or invalidity of our hypothesis. Nevertheless, the conceptual techniques allow to enunciate with precision problems or make procedures for obtaining consequences from the hypothesis and evaluating results from observation and experimentation which, with a simple glance, we cannot evaluate. In this sense, an important conceptual technique is statistics, and we shall latter speak about it, referring to its use (good and bad) in Paleontology.

Thus, hypothesis are not only very interesting in itself, but they are in their consequences too. It is important to remark that hypothesis are more and more useful when they have less and less observational terms in its formulation. Thus, useful hypothesis have many non-empirical terms. History of biology illustrates us with the example of the genetical discoveries of Mendel. He postulated the existence of *not visible* particles as responsible for the transmission of hereditary characters; nevertheless, *consequences* derived from their existence *were susceptible to observation* —and thus, hypothesis could be tested— by means of adequate techniques which made them evident. The process led to success in the dilucidation of the heredity (Jacob, 1970).

Definitely, among several hypothesis, we can evaluate them by means of testing them; it is also necessary to do the same thing for the techniques used for this purpose. It is evident that our problem will be explained in the general frame of knowledge which provoked it, and this solution will increase our body of knowledge. However, our research shall have only solved the problem partially. On the other hand, this research raises new problems and their solutions would be able to show that hypothesis used for the solution of previous problems were valid ones only in a partial way or

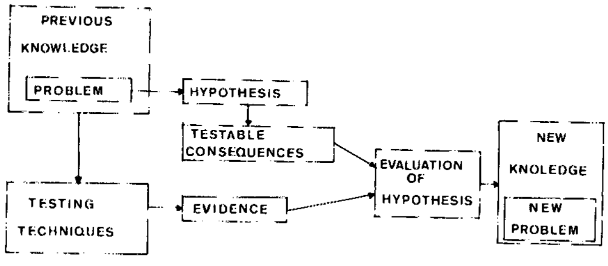

Fig. 2. Schema of the scientific method (from Bunge, 1969).

absolutely invalid ones (we must remember that the concept of fossil, in the Renaissance, explained correctly many things in the context of knowledge of that epoch).

This would be a general schema of scientific method which —I believe— can drive us towards a good development of paleontology; figure 2 gives a plastic idea of it. I believe to detect such a form of conceiving paleontology in many modern papers published in determined journals (*Palaeontology, Journal of Paleontology, Paleobiology, Lethaia,* and other ones). Nevertheless, on the other hand, there is a lot of rutinary paleontological work: description of faunes and flores from more or less wide areas, purely biostratigraphic papers, etc. Apparently, they seem rutinary work, but latter, I shall show their scientifical or non-scientifical character, according to what immediately I am going to develop.

The next footstep is referred to the hypothesis highly tested and verified. In this situation, we can accept them as true objective schemes of the reality, and these hypothesis are those which we call *laws.* There are not only laws; science begins establishing regularities —for example, that discovered by Wallace: a fossil species has another very related with it in an immediately anterior geological time— and these regularities are only summaries of very concrete data. The propositions which enunciate such regularities are called *empirical generalizations* or *level 0 hypothesis* (Woodger, 1978).

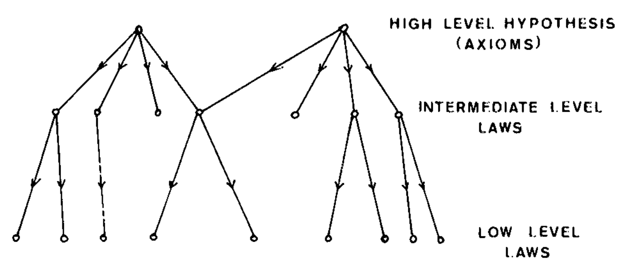

Fig. 3. Schema of the logic structure of a scientific theory (from Bunge, 1969).

When in a science we have a set of hipothesis os high degree of confirmation or laws, then it is very important to try the articulation among them; i. e., we must know if we can deduct all these laws from a minimum of initial suppositions. This deductibility leads us to show relationships among laws, that in another way they cannot be shown. These relationships affect those concepts on which those laws are based. For this reason, concepts appearently no related, become linked. Such new relationships can open, in its time, new ways of research. If we succeed that the body of knowledge could attain such a structure, which allows to deduct all its laws from a little number of basic principles which, in its time, contain the basic concepts of that science, we shall say that we have built a *theory* on the body of knowledge of some area of the reality. The basic principles from which we build the theory are not generally deductible among them; they are called axioms. The concept of axiomatic theory is not related with a sclerotic structure, impossible to correct it. The ordination of a set of laws as a theory or system allows to clarify and ordinate all the knowledge from that field, in such a way that easily all the weak points of such a system come to light, and thus it points out to us new problems and research lines. All this can even affect the axioms of the theory in such a way that it changes one or several axioms, or introduces new ones. Therefore, the knowledge of a field of reality changes its form, though the first successes could remain in a radically new theory. I give only two examples: the relativity theory would be a general one which would cover all those phenomena studied by the newtonian mechanics, and another area of phenomena which had not been considered by the newtonian mechanics at first. On the other hand, newtonian mechanics failed as explanation of this last kind of phenomena. From new bases, Einstein built a different theory which contained newtonian mechanics.

Another case would be genetics. Mendel formulated several hypothesis about the hereditary material, which he considered as immutable. The discovery of mutations by De Vries altered radically the primitive conception of the heredity. However, in absence of mutations, Mendel's laws went on accomplishing themselves and that was a particular case of heredity.

All this seems to indicate that the progress in a science is founded in the obtention of basic concepts for this science; the basic concepts are really not referred to observable things either. The way of conceiving the hereditary material by Mendel was not something susceptible to direct observation— and it is not susceptible either now (Ruse, 1973), but the consequences of such a conception are valid and foud all the modern genetics. For all this, I believe that a scientifical paleontology must be based on such principles. Thus, it is difficult that the mere search for data could conduce to a right term. If we come back to the history of paleontology for evaluating methodologies (in the sense of Lakatos before cited), we shall see that in our century there flourished the accumulation of a big mass of data, but without any rational conceptual schema which allows us to organize them. When the things have escaped far from our hands, it is difficult but not impossible, to try looking for a form of organization of our knowledge about the history of life, under the light of some basic principles.

PALEOBIOLOGY AS THE CORE OF PALEONTOLOGY

Paleobiology means «biology of old organisms». The basic problem consists in knowing if it is possible, a knowledge of such a biology. However, another and principal problem will be what we understand as biology.

Old organisms present to us themselves as fossilizable hard parts; the animal which was protected by those or which contained them is not preserved in general. As for the fossil vegetals, we have major information at histological level. In the case of the animal fossil remains we have, nevertheless, the chance that, in many times, these hard parts preserved as fossils are considered to be very important elements in the life of the organism to which they belong. Such an importance is shown by the strong correlations among these hard parts and the rest of animal. Thus, a bivalve shell possesses a repartition of muscle scars in its interior, and this repartition is very well correlated with the anatomy of living animal, in such a way that we can reasonably predict it in absence of the animal (Kauffman, 1969). Another example of group which allows such a kind of predictability are brachiopods. Muscle scars, brachidia, and other features of the shell of these animals are very correlated with the living animal too; if we consider that they have a very little representation, all these aspects well understood open to us the way for understanding of the numerous extinct brachiopods (Rudwick, 1970).

However, we must advance something about that which we understand as biology, before speaking on paleobiology. From a general point of view, we shall be able to say that biology studies living organisms, and they have character of living systems; according to Waddington (1968), they would be characterized by containing codified information, responsible of the main feature of living systems: reproduction. This codified information is hereditarily transmitted and is susceptible to become altered and transmit later hereditarily such alterations. This information, codified in a *genotype*, origins specific substances which interact among them and with the physical environment, and this is the origin of the *phenotype*.

Living systems, as defined above, are susceptible of undergoing changes which can transmit to their descendents, and this means *evolution*. At the same time, Waddington remarks that phenotype results from the interaction of the genotype with the physical environment. On the other hand, it is important to remark that each living system is not isolated, but it interchanges matter and energy with its environment and, at the same time, coexists with other living systems. In a certain place of Earth, we shall generally see many living systems, which can put together in discrite groups; i. e., each discrete group belongs to a *population* of a determined *species*. The set of living systems in a determined area, which changes matter and energy with its physical environment and which is maintaining its structure —based on the rélationships established among its component populations— is called *ecosystem*. Ecological theory is established about the ecosystem and its properties, such as stability, diversity, and so on.

Living organisms transmit their information, but another basic characteristic, as we said, is evolution. However, the modern trends in evolutionary studies remark more and more the need of considering organic evolution into the frame of the ecosystems; thus, evolution must be considered in the context of ecological theory. Organic changes would not be, in many cases, direct responses to variations of physical environment (physicalism, physicalists, as Gould (1977) called this position and those which hold it), but caused by modifications of the structure of the ecosystem.

We have here two great theoretical bodies: evolutionary theory and ecological theory. Ruse (1973) remarked that evolutionary theory has a wholly unified structure which allows to reach its axiomatization. If we do not have in account his reductionism of organic evolution to population genetics, we believe that his idea about the unified character of the evolutionary theory is true. The distinction of macroevolutionary phenomena from those microevolutionary ones means recognizing different levels in the evolutionary context. This idea has been formulated years ago in some papers and books (Eldredge and Gould, 1972; Gould and Eldredge, 1977; Stanley, 1975, 1979; Gould, 1980, etc.). That is no obstacle in order that those new ideas could produce, and in fact produce, a unified vision in the new level, and relationship with the lower and upper levels Gould (1980) recognizes three levels in the evolutionary hierarchy, separated by two discontinuities: the Goldschmidt break (between change in populations and speciation) and the Wright break (between speciation and trends as differential success among species).

On the other hand, referring to ecological theory, Margalef (1968, 1980) has drawn an important line in this context. This author remarks as the actual hknowledge of ecology can acquire a meaning and an integration in the framework of an ecological theory. He also states that such a theory overcomes the field of classical empirical ecology. The same author refers to systems as objects of study of ecological theory. Thus, we have that, the other principal aspect of living organisms, that of forming assemblages of individuals inside of the different physical environments, in is also covered in a very concrete way at theoretical level.

The features considered until now bring us to conclude that *present* biology, as body of knowledge, could be organized as a theory. In this last consideration, the two partial theories —the ecological and the evolutionary theories— would integrate all the bodies of existent biological data (figure 4). Such a synthesis could be called biology in a restrictive sense. I shall speak of a broader sense when is fully accepted a temporal dimension of life on the Earth.

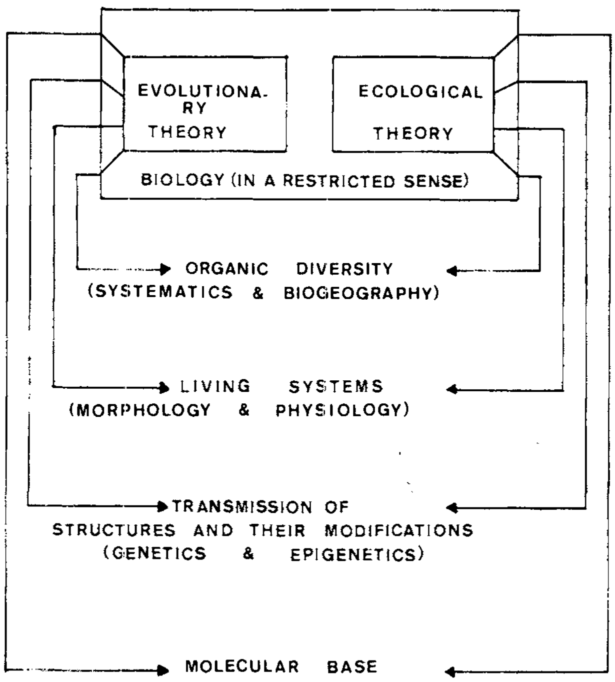

Fig. 4. The theoretical organization of biology (in restricted sense).

Gould (1977) has shown as a very important phenomenon for the evolution, such as heterochrony, is very related to life strategies r or K, which are followed by the species in which heterochrony takes place. The same author, in the same work, and Frazzetta (1975), remark the importance of these strategies for explaining the adaptation events in general. Schopf (1977) treats evolution of Bryozoa in identical terms. These statements show only a sample of the affirmation that realized before.

A PARADIGM FOR PALEOBIOLOGY

«Paleontology is not geology, it is zoology or botany; it succeeds only so far as it is pursued in the zoological and biological spirit».

OSBORN quoted by SCHOPF (1972).

If the synthesis of ecological and evolutionary theories would constitute an integrating paradigm of the principal aspects of biology, I think that it would be possible to do something similar for paleobiology. In this last case, fossils can be interpreted under the form of temporal succession of living forms and, at the same time, the geological paradigm of plate tectonics (Valentine and Campbell, 1979) allows to place spacially the living organisms in any moment of the past. Thus, just as biology —in restrictive sense— can only have in account the spacial dimension, paleobiology can place their events in a space-temporal framework.

Some years ago, Margalef set a schema for paleontology, which would consist in an integration of paleobiogeography, paleoecology and evolutionary paleontology, for any moment of the geological time. Margalef's conception was taken by Lawrence (1971) and we claim to present it as a base of a paradigm for paleobiology.

After Kuhn (1963), a paradigm is a theory or an embryo of theory emerging in a concrete moment which, giving reason of enough phenomena of an area of the reality —without connections until that moment— influences in a positive way in the scientific community that studies that sector, in such a form that this theoretical body manage to rule the research in that area in a coherent manner, because it is open to new ways. I want to make now two statements: a) that the paradigm, in its first condition (theory or embryo of theory) already exists, during several years, for paleobiology, and b) that the principal geological application of paleontology in this century —to date rocks by means of fossils— cannot ignore this paradigm without becoming a useless routine. We could summarize this last statement in the sense that a paleontologist is not necessarily a biostratigrapher, but, however, a biostratigrapher *must be a fortiori* a paleontologist (in the sense of including taphonomy and paleobiology in his daily work); i. e., he must use fossils as remains of old organisms, not as mere «fossil objects» Rudwick (1972) talks about «fossil objects» in referring to the ideas on them along the second half of XVI[th] century; in that moment, students of fossils were preoccupied in placing those in a very broad spectrum. That was very progressive in that time; however, such an attitude would be today radically negative; see the paragraph WHAT IS PALEONTOLOGY? at the beginning of this paper).

I want to justify the two statements a) and b) that I have enunciated before. As to a), a sketch of an organization of the paleontological studies, on the grounds of the integration of paleoecology, paleobiogeography and evolutionary paleontology, is already given. We would have to say that paleobiogeography would be already covered by paleoecology and plate tectonics. This would be according to the synthesis of ecological and evolutionary theories, which we proposed before, like an organization of biology as a scientifical theory in a restrictive sense. Thus, the evolution is shown in the space and time by paleobiology. In that sense, Krohn (1979) has remarked several ideas about integration of evolutionary and ecological theories.

According to Kuhn (1963), the vectors of paradigms are the textbooks. A very interesting sketch of *modern* paleobiology was published several years ago under the form of textbook (Beerbower, 1960). On the other hand, the second important pioneer in this sense is the book of Raup and Stanley (1971). In this last one, the study of organic form receives a special treatment. If we have in account that the Beerbowers's textbook is previous to those first Rudwick's papers on functional morphology and Raup's papers on theoretical morphology, its originality is shown in a patent manner. Another important synthesis is Valentine's (1973) book; in it there appears a connection among ecology, evolution and plate tectonics. In the three textbooks, ecological and evolutionary aspects cover all the other themes. The organic form displays its characters, ruled directly or indirectly by the genes; mechanical aspects in the development of the organic form (Thompson, 1917 and Gould, 1970, in biology and paleontology, respectively) do not mean that such aspects are not genetically governed. This would be translated in the concept of *adaptability* (Frazzetta, 1975); thus, these genes are going to be object of natural selection, and the result of their action will be the adequation of living organisms possessing such a form —resulting from the development— in the diverse ecosystems which constitute the biosphere. In Europe there have been textbooks in order to diffuse these conceptions; I want to cite, among other books, those of Babin (1971) and Roger (1974).

Fossils and their deposit conditions supply us data for reaching such a paleobiological integration. Here, a second problem menaces us: that of the uniformitarianism, which we shall treat apart, by reason of its capital importance in the practice of paleobiological studies.

Thus, we have a proposition well founded and real of elaborating a science of paleobiology. The inclusion of the temporal dimension would distinguish paleobiology from biology in a restrictive sense; i. e., biology of actual living organisms, which supposes them as the result of an evolutionary process and even knows the very process described by paleontologists, although without connection in many cases. Such a biology is normally called *neontology* and it is the opposite of paleontology (of paleobiology, more exactly). Neontology studies only a very thin temporal stratum —the present— of the whole history of life on the Earth. That is very important; aspects such as evolutionary trends, evolutionary rates, emergency of higher taxonomic categories, and so on are only possible to see and face them in a paleobiological context with neontological foundations. On the other hand, ecological changes along the geological time —they are not similar to those of the classical succession (Gould, 1980 a)— would be a specific object for paleobiology too, which could not be covered by ordinary ecology.

In this sense, the proposition of this paradigm is really exciting. Multiple problems arise when we consider life in geological time. They are problems that need solution and that they can have it. In fact, we have an open theory embryo, and that is a contition for the paradigm; this can attract the intelligence of the youth, anxious for displaying their creatives energies. However, bad habits are a big load on us; I spoke before about «direct reading of nature» and such a «reading» is a very troubling charge and it goes on loading very much our minds. Paleontologists are still very compromised with the belief on «brut facts»; that is to say, fossils in succession. I want to bring here a beautiful example about this: fossils in succession appearently show a progressive increase of organic diversity in geological time; this is a «brut fact», but as Raup and Stanley (1978) remark, the outcrop conditions seem to represent the execution of a rarefaction experience by the Nature (rarefaction is a statistical tool which predicts the composition of samples more and more little from a known universe), and if it is so, the progressive diversity is only apparent; this means that a same «brut fact», considered from sound theory could represent something against «common sense» or «common knowledge». After this little disgression, I want to make some remarks; I do not know what is the worse, if to embrace «classical» and inductivistic points of view of paleontology or embrace *superficially* those modern concepts, which I consider as a new paradigm for paleobiology. This frivolous way of work makes me suppose, in many cases, that nothing has changed, except the form of showing the facts, a more elegant and less boring one. We can speak, from a «classical» position, on functional morphology, paleoecology, phylogeny, and so on; they are beautiful ornaments to add to papers and monographs. The paradigm fails, thus, in its psychological objetive; we go on organizing paleontology around objects more than ideas, as eight years ago Schopf (1972) stated; in the present year, Gould (1980 a) says that «we can mouth Kuhnian modernisms about the role of theory, but we remain inductivists in our heart of hearts». I would like to be wrong, but perhaps, many of these observations could be very well applied to spanish paleontology, although there are many hopeful exceptions to this sad rule.

I want to say something about the propositions of Gould (1980 a): these are referred to an higher level of paleobiological synthesis; i. e., that that involves geological and not ecological time —that is to say, the very object of paleobiology, as considered in this paper— for evolutionary and paleoecological studies. We cannot extrapolate, for instance, concepts of ecological succession —colonization by pioneers, ...— to a temporal strata sequence in a basin. The ecological succession events are given in the ecological time; nevertheless, the events here considered, take place in the geological time. Mere analogies cannot bring us to the error of supposing that both kinds of events are interpretable in the same way (briefly, it has no logical support that the phase of colonization by pioneers had a duration of some hundred of thousand —or even some millions— of years). However, in the kind of paleontology that we practice in Spain, we are not yet in conditions of attacking problems of this class, but we can —and it is wholly necessary— attack punctual paleobiological problems. This is related with the statement b), at the beginning of this paragraph: we cannot yet speak on biostratigraphy without paleobiology, except that we desire to go on into the routine. I want to explain myself: in private and informal conversations with spanish paleontologists (above all, with those who have the direction —from the places of major responsibility— of the paleontological research), they told me on many occasions that it is necessary previously to make systematic monographs about the various fossil taxa of Spain. This is so in other countries since the past century. I consider the true and urgent need of such monographs. More, I trhink that a work in this sense is a good exercise for any future paleontologist, *because it serves for familiarizing him with fossils and its paleobiological meaning and problems,* because —and this is the nucleus of the question— systematic problems involve problems on the form and its generation, its phylogenetical history and the whole range of restrictions that affected the form during its construction; systematic problems involve, on the other hand, ecological and biogeographical ones. All that which is not in this way means a regression towards an old systematics of typological or nominalistical conceptions, which nobody claims today seriously. As biostratigraphy founds the goodness of its dating, in a great part, in the goodness of the species that determines the biostratigrapher, it is very necessary to take in account all the biological implications that the modern concept of species brings itself. It is enough that any neophyte reads shallowly a classical as Mayr (1969) for realizing him on the existence of the very important ecological, evolutionary and biogeographical phenomenon called subspeciation, whose lack of valoration can conduce to the illusion of facing new species; of such «new species» unfortunately, the fossil record is densely overcrowded. At the same time, the confusion of clines with phyletic transformations can drive us to consider successions, with appearance of gradual change, as evolutionary phenomena. The consequence of such a confusion —for the lovers of the application of fossils to dating of rocks— will be that of giving as heterochronous beds those which really should be considered as isochronous ones.

And the consequence of all this is, at least in our country (unfortunately, I guess that many criticisms that I have made from a real knowledge of facts, would be applicable to the way of making paleontology in other countries; however, as I have no occasion of doing concrete constatations, I prefer, at the moment, to centre myself in the case of spanish paleontology), to do these paleontological monographs and biostratigraphical researches, except in those hopeful exceptions that I talked before, can mean a double work: the finished monographs or papers can show their lack of paleobiological foundations, whose result will be serious incoherences in the geological applications that we hoped from these works. Thus, this will mean to make all the work again —or in a great part—, because much of it will not be suitable in a short periode of time. The pure and simple morphology can serve very little, if it is not enlighted by a real comprehension of its meaning. And this meaning, let's not deceive ourselves, is a biological one. In many cases, paleontologists baptise again the old species with the new generic names from the *Theatise on Invertebrate Paleontology,* or other books, if they are dealing with vertebrate or plant fossils; this can be an illusion of progress, but if all is reduced to this, it will only be as I say: pure illusion.

TERATOLOGY OF PALEONTOLOGY

Paleontology, like an organism, is developing itself. It is evident that such a development, if it is not carried on harmoniously, can result in grave malformations that do not allow the survival of the organism which possesses them. We have seen that the new ideas can be used as a frivolous mode, and here is the danger. Little by little, it seems that the modern is being introduced. Nevertheless, how is it introduced? and above all, how is it used?

I think that a good point of departure, apart from the interest of paleontologists for the biological concepts which can be interesting in their activity, would consist in centring their daily work in four important points, on which I shall comment a little.

1. *Taphonomy.* After all that is said, anyone can think that a correct taphonomical study is the *sine qua non* condition for a well founded paleontological study —paleobiological one as well as of application in other fields of geology—. I do not want to be insistent, but I desire to remark that the absence of a determined fossil in a bed *is necessary to evaluate it.* Biostratinomical phenomena can destroy concrete kinds of fossils before they become buried (so, species of little size, with hard parts built with instable material, young forms, and so on). This is very important before inferring consequences, in paleobiology (evaluation of population dynamics in fossil populations, evaluation of diversity and structure of old ecosystems, etc.) as well as biostratigraphy (determined index fossils, like planktonic forams). If our taphonomical study is consistent with the hypothesis of the possible destruction of some species or determined young specimens, we must abstain from making concrete statements about the fossil assemblage which we are studying. Only when the taphonomical study does not show conditions that mean the necessary destruction of some concrete forms, then we can judge such an absence from a pelobiological point of view. This last one will be very important when make biostratigraphy: an index fossil cannot appear in a bed for two reasons; the first one is because this bed has not the age indicated by the index fossil; the second one is because the considered species could have been incompatible in the ecosystem which existed in that place of the Earth or simply, because the species could not arrive there for biogeographical reasons (barriers). It is evident that the analysis of these two last paleobiological points of view can bring us to decide if the considered species could live in that place or not. If it could really live there, then its absence can be evaluated as true —thus, the corresponding bed can be considered as anterior to the appearing of this species or posterior to its extinction—, but if this absence can be inferred to by reasons of competition or biogeographic barriers (see Valentine, 1977), then we must abstain from basing the age of that bed in the absence of the considered index fossil.

The previous considerations show as taphonomy and paleobiology are interrelated in the most common applications to geology. There should be paid more attention to taphonomy; sedimentary petrologists study many diagenetical processes that affect organic remains in the rocks, but biostratinomy is not practiced so intensely. For a review on biostratinomy, with abundant bibliography, see Martinell, et al. (1980).

2. *Populations and samples.* It is not useless to remember that a species is a set of populations displayed on a geographical area, with real or potential gene flow among them, and reproductively isolated from other similar sets. If these populations are distributed on more or less wide areas, environmental heterogeneity will be more a rule than an exception, and the populations of this species become adapted to these conditions. Those populations submitted to a common patern of environmental conditions inside that area, will tend towards a similar pattern of adaptation. This pattern will be often reflected in the morphology of the individuals of the considered populations. This makes that the species' morphology varies from one place to another inside the area of distribution, forming groups of populations, related to geographical places, with uniform morphology; these groups are subspecies of considered species. Between two of such areas we shall find transitional morphologies. At the same time, if there is an environmental factor with a gradient in a determined direction in the range of distribution of a species, we shall observe morphological gradients or clines, which will be in geographical correspondence with the factor gradient. The subspeciation can be still more complex than we show it here, but perhaps it is enough for beginning. Selection and mutation events in populations and subspeciation can be explained in the frame of the synthetic theory of evolution.

The paleontological consequences of all this are very important. In the first place, it is necessary to classify species according to these criteria. The existence of a homogeneous population, morphologically different from other populations of a given species, is not enough for deciding if it belongs to another species different from that. If two morphotypes of a same genera coexist in a same area and there are constant and accused differences among their individuals, we can think reasonably in a case of «displacement character» (see Schindel and Gould, 1977), which assures us that we are really in the presence of two different species; the converse test is to search in areas where only we shall find one of the two morphotypes; in such a case of separated areas, the different populations will show morphological convergences. When we find separated fossil populations which present morphological differences between them, we must take care in saying that one of them —if it is morphologically unknown— is a new species. The paleobiogeographical context of forms of the same age can be a good guide for our decision. It is true that there could be important losses in the fossil record, but if populations —with intermediate morphology between those considered as typical and the problematic form— appear in outcrops of the same age, then we can think that this last one is rather a subspecies of that well known.

This is important for biostratigraphy: it is a very dangerous affair to differentiate taxa for defining divisions of the geological time. There is a circular reasoning inherent to it: age is defined by fossils, but after fossils are defined by age. There are paleontologists who, unconsciously, reason like this. It is very possible that phyletic gradualism, that we often invoke, is the responsible of this kind of biostratigraphical reasoning —in the sense of associating a little change to irreversible evolution and thus, to the time—. I was criticised (in my Ph. D., De Renzi, 1972) for considering as synonimous —previous consultation with Dr. M. Glibert and direct comparison with specimens of type— the meridional species *Turritella trempina* Carez and the meridional species *T. dixoni* Deshayes. Criticism (from the french colleagues) were in the sense that we —Dr. Glibert and my— have no in account the *stratigraphical part* of the problem for both species. This discussion was some years ago; now this could be seen as a beautiful example of morphological stasis.

However, the biological species is based on the population concept; the knowledge of the population includes the intrapopulational variabilty, dynamics and life history strategies. Only careful sampling plans of units as concrete as possible can give us idea on all this aspects. It is evident that the fossil population is something interpreted by us in each fossil assemblage (Fagerstrom, 1964; De Renzi, et al., 1975); the fossil assemblage is restricted to a narrow stratigraphic interval and to a very determined geographical area. As a narrow stratigraphical interval is thought —ideally— something like to a single bed. In this cases, the sample is convenient to be as punctual as possible, for avoiding mixing of generations (the fossil assemblage would represent, in general, a natural sample of old ecosystems, with more or less «selection» operations —biostratinomical or diagenetical destruction, transport, etc.— whose maximum level action would tend to minimize highly the relation between the assemblage and the primitive ecosystem). The need of random sampling (simple, systematic, stratified and cluster) is very important for setting questions on fossil populations. For a determined purpose it is necessary some concrete sampling plan, but not whatever. Thus, it would be sufficient a ramdom simple sampling in a fossil assemblage for telling something about mean values, covariances and correlations of biometrical magnitudes from different growth stages in a population. However, elucidation of regular changes in time or in space in populations of a given species needs systematic random sampling, and so on (the classical work Krumbein and Graybill (1965) provides the base for good sampling plans).

Another important aspect involved in this discussion is that of working with individuals from a population *grouped in growth stages*. It is obvious when we evaluate population dynamics (it is always possible if we, in turn, can decide that taphonomical processes did not affect the young individuals, because if these processes eliminated forms of little size, such a study is absurd), but it is also very important for biometrical studies: about this theme, see Gould (1966, 1970) and De Renzi and Martinell (1979). It cannot make biometrical comparisons among species with samples which have mixed individuals of different ages. A sample from a population must be divided in subsamples in relation to age. Thus, we must compare the subsamples of the same age from different populations (or individuals with the same major dimension, as alternative).

A last point: the most of organisms, when they grow, become deformed (they alter their relative proportions among their magnitudes); this is known as allometric and anisometric growths). For this cause, it is absurd to apply simple linear regression to bivariate plots between biometrical magnitudes, for young and adult individuals, from a population (in this case it is obviously necessary the mixing of measures, because we are interested in the *whole* growth). Margalef (1953) remarked this question in neontology —although allometric growth was treated during the first half of this century— because it have received little attention from many systematists, which went on defining species by means of index (quocient between magnitudes); it is absurd, because the index varies if the form changes during the growth.

3. *Substantive uniformitarianism.* I treated this theme anteriorly in another paper (De Renzi, 1978) and I am going to refer to it in a good part. The distinction between a blind uniformitarianism, which is limited to export actual processes and their rates to the past, without any kind of valuation, and a more critical position, it is something advised several times (Lawrence, 1971; Gould in Valentine, 1973). Such a kind of actuation is that which Gould has called *substantive uniformitarianism.* I would prefer to j maintain the distinction accomplished by Hooykaas (1963) between *actualism* (which is only referred to acting causes; actualism means that the same causes which act in the present, acted in the past) and *uniformitarism* (which is distinguished from the common term *uniformitarianism,* that involves actual causes and their uniformity for the anglo-saxon geologists and paleontologists; uniformitarism is related with the rates of the processes, without changes during the geological time).

Thus, we must speak separately of *substantive actualism* and *uniformitarism.* The use of substantive actualism (or uniformitarism) means untested hypothesis, and this is the most important failure of such an image or model of the Nature. If the acting causes (and their rates) on the Earth and the biosphere have remained constant in all their levels, it must be tested, because *it is only an hypothesis,* it is not a *natural law* (Hooykaas). However, the history of the Earth and life could not be investigated without a guide of aspects that could have remained constant during the geological times. Natural laws are proved as such laws because they can apply to a crowd of *very different* situations and in *very different times.* These natural laws are that which we can consider reasonably as constant in the time. This is explained because those laws are founded on *immanent properties* (as expressed by George Gaylord Simpson) of matter and energy; this is valid for the Earth and life which is developed on it. Life has several properties which reasonably must be considered as invariable ones; if these properties would have changed during the geological times, paleontology would not be possible. The constant properties for life would be the following ones: reproduction of individuals similar to their parents; similarity is based on protein replication by means of DNA molecules and in the limited valid interactions of gene products among them, which gives as result the adult form; another aspect is the organization on the Earth of ecosystems composed of populations —constituted by individuals— of different species; these ecosystems have the property of autorregulation; this last one is consequence of the supposition of each individual nourrishes itself, and those of a determined population are part of a more general trophic structure. If we suppose that fossils mean old organic remains, all these statements can be applied to them. We see at once that the simultaneous action of immanent properties *in determined conditions* results in an *immanent process* (thus, the limitation of genic interactions in the development of an organism, because it is only viable a single principal manner; this would be the cause of its immanency. The intemporality of natural laws and this of the resultant processes of their acting in determined conditions, is the base of that which Gould calls *methodological uniformitarianism* and that we would distinguish in two separared aspects: *methodological actualism* and *methodological uniformitarism.* The methodological approach is, in our opinion, the correct one.

We can see now many processes on the emerged lands and in the seas, whose character is possible to show as an immanent one. We have only their responses recorded as fossils and sedimentary rocks. We must set thus, hypothesis or models which allow to explain causally —not as a «black box»— the observed effects or responses. Such models can be formulated on the grounds of those immanent properties or processes which we suppose to give the observed effects. In these conditions, the process will be the hypothesis to be tested as compatible with those responses. Johnson (1960) gave three models, with the possible responses in each case, for testing transport and exposition in fossil assemblages.

Now, I would like to express some criticisms about certain kinds of practicing paleontology. These kinds involve substantive actualism (and uniformitarism); perhaps, it is not so, but there is no apparently another clear alternative. Thus, Murray (1979), although he speaks of adaptations, which are impressed on the shell morphology of foraminiferids, he finishes stating that, although relationships between assemblages of living and fossil benthic forams are complex, it is still possible to use benthic forams for paleoecological interpretations of old deposits: «the ease of interpretation is greatest for Neogene deposits. Because of evolutionary changes, the interpretations are more subjective for Mesozoic deposits and even more so for those of the Paleozoic». Murray uses the taxonomical category of genus in his interpretations. In my opinion, I think that it would be again *substantive actualism and uniformitarism.* Genus is a group of species related very closely; however, these species do not mean necessarily only one kind of adaptations. The morphological similarity of many species inside each genus considered as a good environmental marker, would be a reflect their similarity of adaptation, and in many cases we would apply this last consequence, although it would be an inconscious application. However, such a statement should be tested. Several delicate characters of foram shells should be tested as dependent on chlorinity, temperature, and so on, and this could illuminate many things about these questions. On the other hand, although Murray argues that the application of his interpretations to assemblages of Mesozoic and Paleozoic benthic forams would be very subjective, I think that it would be not advisable, because, as he says, evolutionary differences with actual forams should be very important; such differences would mean unknown adaptive differences; these last one however, can be investigated, and this research must be based on morphology. Numerous papers have tried to light immanent properties of the organic form and their possible investigation on fossils. Such a way can show the adaptive features and causal environmental correlations of the organic form; thus, it can show us modes of life and, as a consequence, valid environmental indications.

In another recent paper (Parker, 1979) is stated explicitly, referring to molluscs, that «modern shallow-water species of mollusks, for instance, have remained relatively unchanged in *external morphology* and by inference, therefore, in their life processes since middle Miocene times. Many have close relatives from Eocene times» (the underlined is mine). The reasons seems more interesting, but I go on thinking that only the investigation of the meaning of the organic form could make clear us, in a more convenient manner, the problem.

All these questions are not new ones. Lawrence (1971) remarked already the invalidity of the more frequent paleoecological reconstructions, founded on the indiscriminate exportation of properties of actual organisms to the past. This it the substantive actualism and uniformitarism. He criticized rightly the inference of the old environment based on the following reasoning: the fossils of a given deposit —as living organisms— would be similar —in environmental requisites— to their nearest actual relatives. It is not necessary to comment this reasoning. Referring to fossil brachiopods, Ager (1965) said that «paleontologists cannot live by uniformitarianism alone. There is ample evidence to suggest that the brachiopods have —for the most part— changed both their habits and their distribution since Mesozoic times. Comparison with what little is known about modern brachiopods is full of difficulties and inadequacies. On the whole it is much better to look at the evidence provided by the fossils themselves». We think that these statements do not need any kind of comment after all this discussion; they should have in account by all the paleontologists.

4. *Biostatistics.* Statistics, in paleontology, is a tool which serves for answering very primary questions. The most sophisticated methods of multivariate analysis are referred, in many cases, to know, for instance, if there are significant simultaneous differences among several populations, based on a series of magnitudes which we think they are important in the taxonomy of that group. In other cases, statistics serves us for seeing structures and relationships among variates taken on organisms, populations or ecosystems; however, such variates are seen before as important ones in the respective biological theory.

Statistics in an important technique (Bunge, 1969); in this sense, it helps us in the testing of determined hypothesis formulated in the teoretical contexts of our sciences. These hypothesis are those which involve samples of populations, where determined parameters of biological importance in the population, are part, because their nature, of probability distributions of variates measured on such populations (I consider here «population» as the statistical concept; «biological population» would be one of its correlates). That I try to say, definitively, is that statistics is not an «universal panacea» which could serve us for avoiding the work of creating and testing hypothesis. It is simply *another testing* technique. Quantitative data are, on the other hand, paraphrasing Gould (1980 a), only other data, although they could allow a great conceptual precision. If data are not related to the corresponding theoretical context, they would be, in general, a poor information.

I would like to remark again that statistics serves us for seeing features of our quantitative (and also qualitative) data, which are not detectable in a simple glance on our laboratory or field tables. More, it would be something temerary and not advisable to act only guided by the first glance on the data arrays. However, to accept significant differences, for instance, or correlations, and so on, that statistics could supply us, without an analysis in our theoretical context, it is also absurd. Scott (1980) points out the possible lack of theoretical bases in the «widespread use of gross dimensions and similar point-to-point measurements in biometric studies» and he considers such a practice as due to instrumental limitations and preceding studies; he claims that it is better, in a theoretical sense, the study of outlines and applies it to *Globorotalia puncticulata* (Deshayes).

Thus, significant differences do not indicate, automatically, taxonomic differences at the species level. It is necessary to explicit, in the first place, the sympatric or allopatric character of the considered populations; the taxonomical value of the characters which we use for distinguishing those populations, and so on. Sylvester-Bradley (1977) criticizes Brinkmann's ideas on *Kosmoceras*; Brinkmann thought to reognize very well five phylogenetical lines in the Oxford Clay (middle Jurassic) at the English locality of Peterborough. It is a classical and very cited example. Brinkmann recorded specimens and he placed each one of them in those five groups; these groups, however, were subjective ones and «as Callomon (1963) points out, having thus imposed a classification on his material, it is hardly surprising that the satatistics confirmed it!». Thus, it is very important to have very clear that it is in the core of the respective biological theories, where we obtain the variates with biological meaning (systematics, ecology, etc.) and it is on these where we shall apply the statistical concepts.

* * *

I think that these four points are very primary and they should have in account by the paleontologists in their daily work. There exist many more points, but in my opinion, these four would be the more urgent. Any departure from them will be the origin of abnormal developments in paleontology which shall not be able to survive. Unfortunately, it seems that «the modern» has become a mode, and this is the worse could occur, because are derived paleobiological consequences without having in account all the requisites for making a correct paleontological work. Normally, many authors do not show in their papers some important points of their researches which, perhaps, would make them invalid ones. Thus, criticism becomes very difficult. However, a correct record of data in the area which was investigated, it is sufficient in general for the discredit of a bad work.

EPILOGUE

Epistemological and methodological questions are generally very useful when a science is searching for its foundations. This is the case of paleontology. Paleontology is a phoenix that is born again from its ashes; an utilitarian fire destroyed it in the second half of the past century. Our science tries to come back to its natural contacts with biology, and biology has advanced very much as a science since that epoch. Biology is gone far away from pure facts and it is trying to constitute itself as a scientifical theory. However, although paleontology must take many the new concepts created by biology in this interval of time, biology is not complete without paleontology. Paleontology, in its time, set the problem of the extinction and, as a consequence, that of the origin of species, which was the origin of one of the theories which is considered as unifying of biology: the evolution theory. For this reason, I think that it is not concevaible a science of biology without paleontology today (paleontology must not be only a simple «intellectual curiosity» for biologists). Paleontology supplies explicitly the temporal dimension to the study of life; our actual vision in biology is placed in the ecological time, something like to one microsecond, if we consider the whole temporal rank of the history of life on the Earth. I think rather than we must speak on neontology and paleontology; both study complementary aspects of life on the Earth. About it, I desire to make a not traditional suggestion: the introduction of paleontology, in an obligatory manner, in the universitary studies of biology. According to all that I have said in this paper, biology without a temporal dimension; i. e. neontology, it is something incomplete. The possibility of paleobiology as a nomothetic science has been explicited sufficiently by Gould (1980 a); here I think that I have not to comment anything more about this question. The reading of this Gould's paper by any biologist conscious of the scientific character of his work, can suggest him a deep reflection on the signification of the correct study of fossils in biology.

Unfortunately, I think that this is utopic now. There are many exceptions: in several countries, included ours, there are paleontologists coming from the field of the biological sciences. Nevertheless, the most of paleontoiogists have realized our formation in the field of geology. After there has emerged a variable conscience about the primarily biological character of the affairs with which we treated. In many cases, however, the most part of paleontologists are true collectionists of stamps —any variation in the characters of fossils becomes the origin of a new taxon— and the maxium achievement is a bad systematic work. In these cases it plays an important role the desire of creating new species, genera, and so on. This reflects a false concept of scientific discovery, whose more delator translation is that of the sentence «new species for Science» (yes, Science in capital letters). On the other hand, that utilitarian fire has converted paleontology in a mere auxiliar of stratigraphy, and this auxiliary character is becoming worse and worse. It is necessary to remember to all those that use fossils for stratigraphical purposes, that fossils had been living organisms, not coins or stamps. Only in this way they can be used as good tools for dating rocks, because they, as such organisms, belonged to populations of species integrated in old ecosystems and occupied a variable biogeographical rank in a determined interval of the geological time. All these factors, and the previous possibility (or impossibility) of preservation, must be have in account when we work in biostratigraphy; if we do not act in this sense, we shall only accumulate printed paper which the time will discredit it.

If paleontology passes from a protoscientific stage to a scientific one, it is very necessary to explicit and, above all, to apply the scientific method in it. There are no pure descriptions nor pure data record; Rudwick and Gould have shown us that in a very demonstrative and authentic way for paleontology in their critical and historic papers. I think that is not necessary to insist more about it; the papers of these authors would be according to a good sector of the soundest actual philosophy of science; they sustain their thought in it. Their merit is to have known how applying the concepts of philosophy of science to a critical revision of paleontology. If paleontology should remain during several or many years as a fee of faculties or high schools of geology —except those cases of paleontologists coming from the biological field or paleontology practiced in biological universitary or research centres— it is necessary to make conscious the geologists in a very concrete sense: geological thought is essential in paleontology, but in a secondary level (as physics or chemistry in geology or biology). Fossils are not understandable out of a geological context (sedimentologic, tectonic, paleogeographic, and so on), but, in first place, they have to be considered as that they are: as remains of old organisms, and this is radically a biological question. The geologist seems, on the other hand, absurdly reluctant for understanding that it must be so. It is very much easy to make understand to a biologist the need of studying sedimentology and historical geology for good paleontological research, than a geologist understands the need of a good biological foundation for this kind of investigations.

I do not want to insist more on these questions of scientifical politic; I think that along this paper have been explicited its bases enough. I do not desire to be dogmatist, although I declare that I have a deep faith in the form of conceiving paleontology that I have exposed in the previous paragraphs. The display of paleontological works and the judgement of them through the history of science will be, at least, that which will pronounce the last word.

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