

THE TAXONOMY OF FOSSIL PLANTS (INCLUSIVE SPORAE DISPERSAE) IN THE INTERNATIONAL CODE OF BOTANICAL NOMENCLATURE, 1956

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THE same subject was treated in "Palaeontologische Zeitschrift" (Vol. 30, pp. 69-87) but it was in relation to the International Code adopted at Stockholm in 1950. The present paper deals with the International Code adopted in 1954 at Paris as it will be necessary to take into account publications of several authors, which since then have expressed their views on this subject.

Because the genera and species of fossil plants (inclusive Sporae dispersae) must agree with the rules of the International Code, it is wrong if authors who make new proposals at once make use of them in their palaeontological papers. The proposals must first be accepted by the International Botanical Congress.

GENERAL RULES

To agree correctly with the International Code its rules and recommendations must be observed in whole. Consideration of only some single sentences produces errors because there are exceptions of the rules.

"The Rules and Recommendations apply throughout the plant kingdom, recent and fossil. However, special provisions are needed for certain groups" (I.C., 1956, p. 11, Preamble, 7). "Special provisions concerning fossil plants in Appendix II" (I.C., 1956, p. 11).

However, spore nomenclature and palaeobotanical nomenclature follow exactly the same rules. In both there are difficulties which must be understood historically.

"The general rules applicable to the naming of recent plants apply also to the names of fossil plants and to those of organ-genera and form-genera" (I.C. 1956, p. 55, Art. PB2).

The genera of Sporae dispersae are organ and form-genera. Where they are legitimate, they cannot be replaced by names, e.g. proposed by Erdtman. The names

saved by the International Code always have priority.

Valid publication of names for fossil plants is treated as beginning from the 31 December 1820 (STERNBERG, Flora der Vorwelt, Versuch 1: 1-24. t. 1-13) (I.C., 1956, p. 17, Art. 13, j).

"Schlotheim, Petrefactenkunde, 1820, is regarded as published before 31 December 1820" (I.C., 1956, p. 18, Note 1).

"Publications by indelible autograph before 1 Jan. 1953 is accepted" (I.C. 1956, p. 27, Art. 29; 2).

"The date of a name or an epithet is that of its valid publication. When the various conditions for valid publication are not simultaneously fulfilled, the date is that on which the last is fulfilled" (I.C. 1956, p. 32, Art. 45).

"A name is not validly published when it is not accepted by the author who published it" . . . "Note 1. Provision No. 1 does not apply to names or epithets published with a question mark or other indication of taxonomic doubt, yet published and accepted by the author" (I.C. 1956, p. 29, Art. 33).

Fr. Thierngart, R. Potonié and others have published combinations, e.g. *Cyatheaaceae* ? *sporites* Thierngart, 1938; they had not the intention to accept such names.

"In order to be validly published, a name of a new taxon of recent plants, the bacteria and algae excepted, published on or after 1 January 1935 must be accompanied by a Latin diagnosis or by a reference to a previously and effectively published Latin diagnosis" (I.C., 1956, p. 29, Art. 34).

This means palaeobotanists may continue without Latin diagnosis.

"When a taxon of recent plant, algae excepted, and a taxon of the same rank of fossil or subfossil plants are united, the correct name or epithet of the former taxon must be accepted, even if it is antedated by that of the latter" (I.C., 1956, p. 40, Art. 58).

ORGAN AND FORM-GENERA

The difficulties in the taxonomy of palaeobotany cannot be understood without some historical remarks.

In 1909 Henry Potonié published proposals concerning palaeobotanical nomenclature which were supported by F. Beyschlag, A. Engler, E. Gilg, W. Gothan, H. Harms, O. Horich, R. Pilger & J. Urban. They were only personal additions to the rules and were published after the International Botanical Congress of 1905 in Vienna. In spite of the fact that even later these proposals were in a great deal not introduced in the International Code, many authors followed them.

There were distinguished in the 1909 proposals:

- (1) "Good" species, genera, families, etc.
- (2) Provisional "dilemma" groups (Verlegenheitsgruppen, groups d'embarras).

In modern terms the first concerned "Organ-genera" and the epithets above them and the second "Form-genera" and all epithets above.

It has become evident, however, that the two cannot always be distinguished. Some form-genera can be transferred into organ-genera but often this may be discussed because of the changes in scientific viewpoint.

The rules concerning the palaeobotanical organ and form-genera are:

"Since the names of the species, and consequently of many of the higher taxa of fossil plants, are usually based on specimens of detached organs and since the connection between these organs can only rarely be proved, organ-genera (organo-genera) and form-genera (forma-genera) are distinguished as taxa within which species may be recognized" (I.C., 1956, p. 55, Art. PB1, 1).

Fructifications, leaves, cuticles, stems, roots and Sporae dispersae are such "detached organs". The connection between these organs and the Sporae dispersae can only rarely be proved. To use for an organ, such as a spore, the name of an organ or a plant which only perhaps is the mother plant is unscientific. In such cases we must use for the Sporae dispersae organ-genera and form-genera as taxa within which may be recognized organ-species and form-species.

"An organ-genus is a genus whose diagnostic characters are derived from single organs of the same morphological category or from restricted groups of organs connected together" (I.C., 1956, p. 55, Art. PB1, 2).

Here it is said that the diagnosis of an organ-genus can speak only about *one* organ, e.g. spores. Other organ-genera concern the complete fructifications with the sporophylls and the spores. These are "restricted groups of organs connected together". An organ-genus is not allowed to contain elements other than such organs which are "of the same morphological category", may it be single organ or restricted groups of organs, they must be in an organ-genus that has the same "organ" as genotype. To emphasize that point the I.C. adds a recommendation:

"An author describing organ-genera should clearly indicate for which kind of organ the genus is established."

"It is desirable that the name should indicate the morphological category of the organ (for leaves a combination with *phyllum*, for fructifications with *carpus* or *theca*, etc.)" (I.C., 1956, p. 56, Recomm. PB 6A).

One who follows this recommendation indicates already with the name of the genus that the genotype and holotype is only a certain kind of organ, so that in this genus other organs may not be put.

Till now the kind of organ of spore genera has been indicated by suffixes as *pollenites*, *pollis*, *spora*, *sporis*, *sporites*. Here must be said that a clear differentiation between spore and pollen genera is not possible, so that a suffix as *sporis* or *sporites* in many cases would be sufficient.

Names without such a suffix are not invalid.

"A form-genus is one that is maintained for classifying fossil specimens that lack diagnostic characteristics indicative of natural affinity but which for practical reasons need to be provided with binary names. Form-genera are artificial in varying degree" (I.C., 1956, p. 55, Art. PB1, 3).

So form-genera differ from organ-genera only in lacking diagnostic characteristics. Further study sometimes finds out that such characteristics could still be given for some or all species of the genus, in spite of the first statement, and so a form-genus may later become an organ-genus.

"It is necessary to distinguish both organ-genera and form-genera since the former are held to indicate a certain degree of natural affinity, while the later may — and in many cases do — include species belonging to different families or even groups of higher rank,

e.g. ferns and pteridosperms" (I.C., 1956, p. 55, Art. PB1, Note 2).

W. L. Norem (1954, p. 143) says, "such a classification is confusing because materials of known affinities are classified according to phylogenetic relationships and those of unknown parentage according to morphological characteristics. No clear-cut distinction is made in the nomenclature between fossils classified in the natural and the artificial systems."

The impossibility to make a clear-cut distinction between organ-genera and form-genera is not astonishing. This is a question of scientific recognition and not an agreement to be adopted for ever. Each author must realize, whether he will put a genus in the natural system or not. But, on the other hand, we are allowed to put altogether provisionally organ and form-genera in a morphographical system which only serves the review of the whole material.

In every case organ-genera are those which can be placed in a certain family, while form-genera are held to indicate none or only a lesser degree of natural affinity. So, as a matter of fact a part of the genera of *Sporae dispersae* can be put in the natural system. These are the true organ-genera (see R. POTONIÉ, 1954). Other genera only agree with the groups of higher rank, e.g. orders, classes, etc. "But *form-genera* have been recognized as pertaining to a special morphological category since 1828 (*Adolphe Brongniart*). Since that time they have been constantly used in taxonomic and morphological literature and they are quite indispensable" (I.C., 1956, p. 55, Art. PB1, Note 2).

The word "morphological" in this case means "morphographical" (see H. POTONIÉ, 1912). Unfortunately the notion of morphological has been enlarged since Goethe and Brongniart.

"In descriptions of organs of uncertain nature or affinities, a name suggesting definite relationship with a recent plant should be avoided" (I.C., 1956, p. 56, Recomm. PB6 D).

This recommendation is not often followed in the case of genera of fossil woods and the genera of *Sporae dispersae*. It concerns chiefly the form-genera. But it has been forgotten to add in this recommendation (PB6D) that it should also be avoided to use a name suggesting definite relationship with another fossil plant. Such names would

always stay valid even if later on it is proved that the relationship does not occur.

"The purpose of giving a name to a taxonomic group is not to indicate its characters or history, but to supply a means of referring to it and to indicate its taxonomic rank" (I.C., 1956, p. 11, *Preamble*).

If a name is legitimate, it must be used even if it mentions what is not to be seen in the type, e.g. the false relationship with a recent plant.

"Organ-genera based on detached parts may be distinguished not only by morphological characters, but also by reason of different modes of preservation" (I.C., 1956, p. 55, Art. PB1, Note 1).

This signifies in the case of *Sporae dispersae* that where the relationship is not clearly to be seen, it would be allowed to create both, e.g. a genus for spores without and another with a perispore, or a genus for spores gained by maceration and one for spores seen only in a coal slide in reflected light (see E. STACH).

"In order to be validly published, a name of a genus of recent plants must be accompanied (1) by a description of the genus, or (2) by a citation of a previously and effectively published description of the genus, or (3) by a reference to a previously and effectively published description of the genus as a subgenus, section, or other subdivision of a genus, etc." (I.C., 1956, p. 31, Art. 39).

This shows that palaeobotanists must follow another Article:

"From 1 January 1953 the name of a genus or of a taxon of higher rank is not validly published unless it is accompanied by a description of the taxon or by reference to a previously and effectively published description of it" (see Art. 39) (I.C., 1956, p. 55, Art. PB3).

Till 1 January 1953 a palaeobotanical genus or a taxon of higher rank could be considered as validly published without any description if in other respects it was right. So it was a fault of Thomson & Pflug (1953) to put aside such names of genera published before 1 January 1953.

"A description of a new species assigned to a monotypic new genus is treated also as a generic description if the genus is not described" (I.C., 1956, p. 31, Art. 41, Note 1, 1).

But palaeobotanists must besides use the following articles:

"The name of a monotypic genus of fossil plant published after 1 January 1953 must be accompanied by a description of the genus indicating its difference from other genera" (I.C., 1956, p. 56, Art. PB6).

"In order to be validly published, a name of a new taxon of fossil plants published on or after 1 January 1912 must be accompanied by an illustration or figure showing the essential characters in addition to the description, or by a reference to a previously and effectively published illustration or figure" (I.C., 1956, p. 30, Art. 36).

We have seen in Article PB3 that a description of a fossil plant *genus* is only necessary from 1 January 1953. But Article 36 demands that every taxon of fossil plants already in existence since 1 January 1912 must have an illustration, etc., in addition to the description of the *species*.

"In certain circumstances, an illustration with analysis is accepted as equivalent to a generic description" (see Art. 41) (I.C., 1956, p. 31, Art. 39, Note).

"The publication of the name of a monotypic new genus based on a new species is validated either by (1) the provision of a combined generic and specific description (descriptio generico-specifica), or (2) for generic names published before 1 January 1908, by the provision of an illustration with analysis showing essential characters" (I.C. 1956, p. 31, Art. 41).

"Single figure of microscopic plants showing the details necessary for identification are considered as illustrations with analysis showing essential characters" (I.C., 1956, p. 31, Art. 41, Note 2).

Art. 41, Note 2, should only carefully be applied with figures of fossil spores, but it is to be observed.

"A name of a taxon below the rank of genus is not validly published unless the name of the genus or species to which it is assigned is validly published at the same time or was validly published previously" (I.C., 1956, p. 32, Art. 42).

The names *Sporites* and *Pollenites* have been validly published and so also the names of the species below them. It is another question to discuss the present worth of these taxa as form-genera (see below).

"A specific epithet is not illegitimate merely because it was originally published under an illegitimate generic name, but must be taken into consideration for purposes of priority if the epithet and the cor-

responding combination are in other respects in accordance with the rules" (I.C., 1956, p. 45, Art. 70, Note 3).

"A legitimate name or epithet must not be rejected merely because it is inappropriate or disagreeable, or because another is preferable or better known, or because it has lost its original meaning" (I.C., 1956, p. 41, Art. 62).

Reissinger (1950) has rejected the legitimate name *Pityosporites* and proposed *Pityopollenites* merely because the genus contains pollen grains. This is not possible. In the same way a genus name mentioning a relationship, which later on is found erroneous, cannot be changed.

"When a name has been proposed but not validly published by one author and is subsequently validly published and ascribed to him by another author, the name of the former author followed by the connecting word *ex* may be inserted before the name of the publishing author, etc." (I.C., 1956, p. 34, Recomm. 46A).

E.g. *Trilites* (ERDTMAN 1947) *ex* Couper, 1953, p. 129.

"When it is desired to indicate the name of a subdivision of the genus to which a particular species belongs in connection with the generic name and specific epithet, its epithet is placed in parentheses between the two; when necessary, its rank is also indicated" (I.C., 1956, p. 23, Recomm. 22B).

Many palaeobotanists use in parentheses between the generic name and specific epithet the name of a genus to which the species formerly belonged (see GOTHAN, 1953, p. 61). This is producing errors with the recommendation above. I, therefore, propose to add in such cases "al", e.g. *Paripteris* (al. *Neuropteris*) *gigantea*.

THE TYPE METHOD

Appendix IV of the I.C. 1956, p. 294, gives the method of the determination of the types. I shall not repeat all this. It is necessary to see there the details concerning the type method.

"The typification of organ-genera, form-genera, genera based on plant microfossils (POLLEN, SPORES, ETC.), genera of imperfect fungi, and any other analogous genera or lower taxa does not differ from that indicated above" (I.C., 1956, p. 15, Art. 7, Note 5).

We see that the type method is provided not only for organ-genera but also for form-genera and for genera of pollen and spores.

"The application of names of taxa of the rank of order or below is determined by means of nomenclatural types" (I.C., 1956, p. 14, Art. 7, 1 sentence).

"The principles of priority and typification do not apply to names of taxa above the rank of order" (I.C., 1956, p. 20, Art. 16).

This is concerning the natural system of recent plants; in palaeobotany also the taxa above the genera do not follow the principles of priority and typification as far as they do not show suffixes as *aceae*, etc.

Ever the names of order and of all taxa below (excepted the "dilemma" groups or turma) are applicable only when a nomenclatural type can be found.

In palaeobotany it is not necessary that the type has been mentioned by the author: only it must be possible to find one.

"Publication on or after 1 January 1958 of the name of a new taxon of recent plants of the rank of order or below is valid only when the nomenclatural type is indicated" (I.C., 1956, p. 30, Art. 35).

This Article does not concern fossil plants, so that here also in future a taxon otherwise free from objection is valid without indication of the type. But other rules of the I.C. demand that the publication must be such that it is possible to determine the type, where a taxon is compared with other taxa.

Concerning Sporae dispersae it was tried to introduce the type method already in 1931 (see R. POTONJÉ, zur Mikroskopie der Braunkohlen — Zeitschrift Braunkohle, Halle). It was said that it is necessary to preserve the preparation of the specimen which has been used for description and figure, and that for each figure the preparation and the place where it has been deposited should be mentioned.

For palaeozoic genera of Sporae dispersae the type method firstly has been used by my collaborator Ibrahim.

The rules concerning the type method are, as we have seen, joined with those of priority. Where the type method is not used, it is not possible exactly to find out the priority.

Also with the organ and form-genera the priority cannot be used without the method of typification.

A "genus", which is created with the intention not to use typification, cannot be

called a genus under the rules of the I.C. Such unities can only serve as heads (turma) to range the legitimate organ and form-genera in a morphographical system.

All this concerns the Sporae dispersae as well as the whole palaeobotany.

As an example I mention *Gothan* (1953, p. 9). Here the form-genus *Neuropteris* is cut in two parts. *Gothan* creates in its place (in accordance with a proposition made by him already in 1941) two new genera (which he calls expressly genera); these are *Imparipteris* and *Paripteris*. So *Neuropteris* is placed out of the rank of a legitimate genus and becomes something of higher rank. As an excuse for this handling *Gothan* (1941) mentions *Florin*, who in the same way cut the genus *Walchia* only for such cases where the species could not be put in the new genera. For *Neuropteris* it would have been possible to find a genotype (see ANDREWS JR., 1955). Therefore, the arrangement could have been as *Gothan* deals in the same paper (1953, p. 9) with *Linopteris* Presl 1838. *Linopteris* is restricted and a part of this old genus is put in the new genus *Reticulopteris*.

Concerning Sporae dispersae there have been made some very large unities for which a genotype cannot be chosen without changing completely the intention of the authors and therewith confusing the older literature. Such unities are *Sporites*, *Pollenites*, *Saccites*, *Monosaccites*, *Disaccites*, *Aletes*, etc. All these unities contain, according to the "original meaning" of the author, so great a material (PB 6C) that they include many of the legitimate form-genera existing beside them.

These unities now are outside the rules of priority, because a genotype would be unsuitable. They are used in the morphographical system of the Sporae dispersae as unities of a rank higher than the genera (as turma). They are no more genera, but also not families, etc., of the natural system. They only allow a purely morphographical arrangement.

Only those organ and form-genera, whose types have been fixed or can be found in a wise way, are genera in the proper sense of the I.C.

It would be no more good to use the notion "form-genera" in the sense of the International Code, for unities which have or shall not have a nomenclatural type.

The International Code demands since 1 January 1953 for all those taxa (as order and below), which follow the type method, the clear indication of the rank of the taxon as *nov. gen.*, *nov. spec.*, etc. "A new name published on or after 1 January 1953 without a clear indication of the rank of the taxon concerned is not validly published" (I.C., 1956, p. 32, Art. 44).

This only concerns the ranks adopted by the International Code. Not adopted are: *nov. spm.*, *nov. spt.*, group, subgroup, turma, etc.

I propose for all palaeobotanical unities outside the I.C. to add *nov. turma* [as Erdtman says for the *Sporae nov. spormorpha*, etc., and Pant (1954) speaks of "groups". But it would be better to have one term for the whole palaeobotany]. Who adds such indications shows that his unities stand outside the type method and the rules of priority.

"In descriptions of new species it is desirable to mention which specimen is regarded as the type and to indicate in which Museum or collection the type is to be found (I.C., 1956, p. 56, Recomm. PB 6E).

This is only a recommendation for the palaeobotanist, but many authors follow it and so necessarily those taxa also are influenced which were till now treated without regard of a type.

There has been considerable resistance to treating the taxa now called form-genera and form-species under the same rules as the other genera, but this is now required by the International Code. It now seems certain that for both organ-genera and form-genera the type-method and, therefore, priority must be used.

"When diagnostic characters are altered or circumscription changed in taxa of fossil plants, the type is determined by reference to the original specimen figured in validation of the name of the taxon. If more than one figure is supplied in validation of the name, the emending author must indicate from the specimens originally figured the one he regards as constituting the type" (I.C., 1956, p. 56, Art. PB5).

"For the name of a fossil species, the lectotype, when one is needed, should, if possible, be a specimen illustrated at the time of the first valid publication" (I.C., 1956, p. 15, Art. 8, Recomm. 8D).

"The type of a genus of fossil plant is the first described species which shows such

characters as are necessary for distinguishing the genus from other taxa. The type of a species of fossil plants is the first described and figured specimen showing such characters as are necessary for distinguishing the species from other species" (I.C., 1956, p. 56, Art. 4).

This means, we should not absolutely designate as type the first described specimen but the first showing the characters mentioned above.

In the determination or selection of the nomenclatural types of previously published taxa "mechanical systems such as the automatic selection of the first species or specimen cited" ... "should be avoided as unscientific and productive of possible future confusion and further change. The original description of the taxon concerned should be the basic guide" (I.C., 1956, p. 294, Appendix IV, 4).

This sometimes has not been observed. A type was chosen without any other judgement because it was the first picture that had been published. "Designation of a lectotype should be undertaken only in the light of an understanding of the group concerned" (I.C., 1956, p. 294, Appendix IV, 4).

An example is provided by *Trudopollis pompeckji* in which the type must be the picture in R. Potonié (1934, 4, p. 78, Pl. 4, Fig. 12) and not the first bad photo of 1931 as designated by Krutzsch (1954, p. 286). The description of *T. pompeckji* concerns the picture of 1934 and also current usage corresponds therewith.

"Whenever the type material of a taxon is heterogeneous, the lectotype should be so selected as to preserve current usage unless another element agrees better with the original description and (or) figure" (I.C., 1956, p. 15, Art. 8, Recomm. 8C).

It is dangerous to introduce specimens inadequately described and figured in lists of synonyms. It is uncertain whether other material does correspond. The I.C. says expressly:

"Palaeobotanists should exercise great caution in applying to well-preserved specimens' names which have been originally attached to poorly preserved specimens or to specimens which have been inadequately described or figured" (I.C., 1956, p. 56, Recomm. PB 6J).

Names attached to bad specimens are not illegitimate but we should forget them more and more, only applying names attached

to better preserved materials. In this way we eliminate unpleasant changes of names and bad holotypes without much trouble.

Van der Hammen has designated recent pollen grains as genotypes of a several new pollen taxa, e.g. *Calluna vulgaris*. He has also given specific names to pollen grains taken from fructifications of recent plants. Neither of these procedure is valid under the I.C.

Without following the type method in a correct way we can never retain a synopsis of the fossil Sporae dispersae. Many writers today emphasize the type method (BHARDWAJ, COUPER, GUENNEL, HORST, HUGHES, IBRAHIM, KLAUS, KREMP, PFLUG, SCHOPE, SURANGE, THOMSON, TRAVERSE).

THE POSSIBILITIES TO CORRELATE ORGAN-GENERA HAVING GENOTYPES OF DIFFERENT ORGANS

Faegri (1956, p. 652) and also R. Potonié (1956, p. 69) point out that the rules of the I.C. already contain the necessary provisions. But the I.C. says nothing concerning the cases where the taxa of spores are considered in relation with taxa of other organs (*see* POTONIÉ, 1956c, p. 10).

It would not be necessary to add something to the rules, if we were of the opinion, that the taxa of different organs never ought be put together. Often indeed it seems as if this was the clandestine intention of palaeobotanists.

It is possible in certain cases to say how different *organa dispersa* are related, but to express this taxonomically is difficult and has already caused confusion in stratigraphy.

The stratigraphical incongruity of different organs of plants from one and the same genus or family has recently been discussed (R. POTONIÉ, 1956a, p. 88). Not all parts of the body of the plant, which as fossil we mostly find dispersed (as *organa dispersa*), have the same diagnostic and, therefore, also not the same stratigraphic significance.

This is a further demonstration of how much we need the organ and form-genera for stratigraphical as well as botanical purposes. It would often be a risk to amalgamate definitely organ or form-genera of different organs even if the I.C. provided rules for it.

So also there remains as ever before uncertainty whether a *Spora dispersa* should be placed in a genus specially created for spores

(organ or form-genus) or in a plant-genus of which the genotype is not a spore.

In the latter case if a fossil spore diverges in any respect from the spores of the genus concerned, it is unscientific (apart from consideration of the type method) to extend this genus without any further knowledge of the plant. *An author should always use a special Spore-genus if he uses a specific name valid only for the spore.*

If the author chooses a genus where the type is, for example, a fossil fructification of the whole recent plant, *he should not choose a special specific name for the spore.*

Faegri (1956, p. 650) observes clearly that: "If a pollen grain can be identified as belonging to a known taxon, living or fossil, no special name need be or can be attached to it." I have emphasized the same idea (1956c, p. 8).

Traverse (1957, p. 256) says: "Potonié (1956) has clearly condemned the use of extant generic and specific names for fossil pollen." That is not right. Both are allowed and I have said it in several papers. But there is an important restriction:

It is not suitable to use for fossil spores and pollen grains generic names of recent plants if we give the spores such specific names whose holotypes are Sporae dispersae.

If we use a generic name of a recent plant, we also should use a specific name of one of the species of that genus. Or better it would be only to mention the generic name without addition of the specific name.

On the other hand, the I.C. will never forbid to put fossil specific names in genera of recent plants. This will ever be possible if the material of the fossil is more or less complete and does not only consist in a very little part of the plant which sometimes only shows characters of *contested* diagnostic value.

So it is even not forbidden officially to introduce specific names of fossil Sporae dispersae in genera of recent plants, but the moment this is done, the specific name of the spore or pollen grain will no more have any scientific sense. I, therefore, proposed for such cases no more to use specific but generic names and perhaps to mention which of the recent species of the genus the fossil spore resembles most. In this point I agree entirely with Bhardwaj, Brown, Faegri, Firbas, Hughes, Iversen, Kirchheimer, Rudolph and Thomson.

Sometimes it happens that all spores of a genus show approximately the same features indistinguishable by present methods. There is surely then no point in using a special specific name. Accuracy can only be advanced to the name of a genus although that may still have considerable bearing.

If the shape of a fossil spore or pollen grain occurs in a recent genus and there too only in one of the species, then this name of a recent species could be applied. But if the fossil form is produced by several recent species of that genus and never occurs in other genera, then it is sufficient to say to what a section or series of the recent genus the fossil form belongs. More we learn of pollen morphology, more it is becoming possible that certain fossil pollen grains (*Sporae dispersae*) may be put in recent genera and more it becomes clear that there they do not need a proper specific name.

It is different, however, if a single form of spore concerns several genera: there a specific name is advisable but with a spore as genotype.

This occurs also with all those forms of unknown affinities.

Faegri says (1956, p. 649): "The only grains that, strictly speaking, can be identified to "mother species" and thus be assigned to their proper place and name in the system of plants, are those taken directly from anthers. Even with recent species, specification of pollen grains found isolated from their 'mother plant' is only rarely possible. We have to rest satisfied with genus, tribe or even family, unless phyto-geographical or other auxiliary evidence indicate that only one species is present." He thus repeats what some palaeobotanists have often said.

Hence it is unscientific to place the species *hiatipites* Wodehouse in the genus *Taxodium*, as it would mean that the genus *Taxodium* is present everywhere we find *hiatipites*. Placing a spore with only a few characters in a recent genus can thus cause stratigraphic confusion as well as difficulties in nomenclature and taxonomy (R. POTONIÉ, 1956).

Traverse says (1957, p. 258): "Potonié's argument that inclusion of new organ species in an extant genus involves a broadening of the genus does not seem correct to me, because the circumscription of the genus is established by its description." Traverse would have understood me if he had seen that in his case a new organ species only is

necessary if it is believed not to fall absolutely within the circumscription of the recent genus in question, so that other genera contain or may contain equal features, or if the shape of the spore till now was not exactly represented in the genus, so that indeed it would be a broadening of the genus to put in the spore.

It is no improvement to make as Rouse (1957) does combinations like *Gleichenia concavisporites*. The suffix *sporites* added to the specific name serves no useful purpose and should not be confused with the addition of the same suffix to generic names as proposed by Henry Potonié (1909, p. 535). We must, in this case, only give the name of genus as did Kirchheimer and Ingversen or mention which of the forms of the genus more or less agree with the spore, by using "cf." in front of the name. To give a special name to the spore is superfluous. If a spore is placed in such a genus, the author should be able to say which spore or spores of the genus it must resemble and why it should not be placed in another genus. So everything is done what could be done. Rudolph followed this method in mentioning the species with which the spore agreed so that everything possible was done.

Traverse (1957, p. 255) says: "Palaeobotanists studying megafossil organs have placed the organs in extant taxa, where they felt that the organs fell within the limits of the taxa concerned." That as we have seen is not expressly forbidden by the I.C. But it has here, as everywhere, produced taxonomical and stratigraphical difficulties if a fossil of only little diagnostic value received a new specific name and so was placed in the mentioned recent genus.

Also older palaeobotanists have felt that. Many of them have created organ-genera even where they compared fossil organs with species of only one recent genus.

In every case it is risky to place together different fossil organs in one genus. As palaeobotanists commence to treat the type method with absolute correctness, this becomes plain.

It is sure that in the whole palaeobotany a correct dealing with the type method will affirm the opinion that parts (organs) of plants cannot be put in a fossil genus of another organ if the parts have received a specific name which was not previously contained in the genus. A fossil organ with its own specific name should be in a genus which

has the same organ as genotype, otherwise (that is clearly to be seen) there must result taxonomical and stratigraphical difficulties.

Another clean method will not be possible with the present palaeobotanical rules of nomenclature; they do not contain proposals how to put together organ-genera of different organs. So it is still only the silent opinion of palaeobotanists that a part of a plant (an organ) being in an organ-genus of its own organ and later put in a genus of a more complex organ (e.g. a single fossil spore put together with a fossil fructification containing such spores) should lose its specific name even though if this name is the older one. The part of an organ should ever accept the specific name of the more complex organ. Till now such rules are not in the I.C. If it were so, it would be the most scientific method to put a *Spora dispersa*, which since long has a proper specific name, in the genus of a fructification if in this genus has been found (even later) a fructification which contains absolutely the same spores (and that too only this fructification and not such of other genera or species). Then the spore may have the later specific name of the fructification and may lose its older name.

This is not in the rules, but already today we should never put spores in genera of more complex organs when in these genera are not contained fructifications which have the same spores. We must in this case use the specific name of the fructification and not only the name of the genus. Never we ought combine the name only made for the spore species with the genus name of a fructification.

OUTSIDE THE TYPE METHOD

We have seen that a new genus or a new species since 1 January 1953 is validly published only if there is added *nov. gen.* or *nov. sp.* or if in another way is said what taxon among those adopted by the Code is in question.

It is, therefore, unrealistic to present systems of "dilemma" groups, instead of form-genera, as have been done by Erdtman as well as Pant. This is possible only for dilemma groups (turma) above the genera. One who does not say, e.g. *nov. gen.* and *nov. sp.* shows that he creates taxa not saved by the rules of priority of the I.C.

In some of my earlier papers, like Erdtman, I also used *Sporomorphae* (*spm.*, *spl.*) for certain unities of *Sporae dispersae* in the rank of genera (*spt.*). Since 1 January 1953 this is no longer possible under the rules of the I.C. and must be abandoned. In the same way the term "subgroups" is no more permissible in place of genera validly published. (PANT, 1952, speaks of "subgroups").

Balme & Hennelly (1956), however, still create new taxa designated as *n. spl.* and *n. spm.* Fortunately Balme & Hennelly designate genotypes (and use the word genotype) and thus their "sporotypes" can be regarded as valid genera and their "Sporomorphs" as valid species.

"Thus each species belongs (is to be assigned) to a genus, each genus to a family (certain artificial groups of fossil plants excepted), etc." (I.C., 1956, p. 13, Art. 3).

This means that a fossil genus can never be assigned to a family but may belong to "certain artificial groups of fossil plants" (turma). For this artificial groups the type method cannot be used, and so also it is felt that the type method cannot be applied to the nomenclature of taxa above the rank of order (I.C., 1956, p. 20, Art. 16).

Above the genera the type method in palaeobotany is only to be followed with those taxa which announce by their suffix (e.g. *aceae*) that they belong to the natural system of plant families; others are still "dilemma" groups (VERLEGENHEITS-GRUPPEN, turma) outside the type method.

Henry Potonié (1909, p. 534) had already suggested that "dilemma" groups above the rank of genera should never bear the suffix—*aceae*. This proposal has been introduced into the I.C. Just the taxa directly above the genera must show it by their suffix, if they are not taxa of the natural system. So we see that they do not correspond with the rules of the I.C. and, therefore, also not with the priority in the frame of the valid taxa of the I.C.

"Form-genera should not be used as types on which natural taxa of higher rank are established."

NOTE — While organ-genera may be grouped in families bearing names taken from one of the genera and ending in *aceae*, form-genera should not be placed in groups with names implying the status of natural taxa" (I.C. 1956, p. 56, Recomm. PB 6C).

This means, to range form-genera (which frequently appear between the genera of *Sporae dispersae*) we need a morphographical system. In every case it is not allowed to place them in the morphological system of natural taxa, as long as they are form-genera and not transferred to organ-genera.

The Recommendation PB 6C shows that the I.C. knows that besides the taxa of the natural system and the form and organ-genera are needed further "artificial" unities (turma) to place form-genera in groups with names not implying the status of natural taxa. Therewith the I.C. mentions an artificial (or better morphographical) system beside the natural system. A genus may be placed by one author in the natural system, by another author in the morphographical one.

But in order to handle the large number of legitimate form and organ-genera it is best to arrange them all in a morphographical system which is similar to a key. As heads or divisions (turma) of this system may be chosen as far as possible names of the old "dilemma" groups which appeared earliest to the literature. These are intended to be outside the order of ranks of the I.C., and, therefore, rules of priority do not apply. We should, however, use the oldest names where possible and these are often the terms of Naumova.

The legitimate taxa of the I.C. always have precedence. If anywhere the oldest names of "dilemma" groups are chosen, this only occurs in their own frame.

"The names of form-genera should as a rule be used only in their original meaning, and subsequent alteration of the diagnostic characters of the form-genera is not desirable" (I.C. 1956, p. 56, Recomm. PB 6B).

This recommendation does not ever agree with Article PB 2 and Article 7, Note 5, where clearly it is said that also with form-genera the typification does not differ from that indicated for other genera. So alternation of the diagnostic characters of the form-genera will often be inevitable in spite of that recommendation.

As a matter of fact, the palaeobotanical literature till today has not observed the type method with many of the form-genera; it followed the original meaning, that is what the author has said in the diagnosis.

The nomenclatural type fixes the point from which the name of the taxon is not

allowed to be taken away. It would often not be possible to follow this rule if we follow the "original meaning". In every case the type method is to be accepted where both methods are in collusion.

On the other hand, it may be good concerning taxa, which have been used for many years without any consideration of a type, not to designate one. Such a taxon often contained from the beginning much heterogeneous material; the first specimens introduced were often poorly preserved and other species only temporarily included are now legitimately included in other genera. In choosing a type, all better preserved forms are found to have been put in other genera, leaving for the old "Dilemma" groups only forms with which no one could work. It is surely unscientific to legitimize a genus with an unsuitable type; likewise it is unwise to put new and good material into a genus with bad genotype (I.C. 1956, p. 56, PB 6F). For instance, in the case of the two taxa *Sporites* and *Pollenites*, Faegri (1956, p. 650) is of the same opinion as the present author that "they have no definite meaning today". They are not suitable as names of genera in the legitimate sense of the I.C. It would be completely arbitrary to select a genotype from such heterogeneous material as from the beginning was introduced here. In spite of this Andrews (1955, p. 215) believed that *Pollenites iliacus* was suitable because this species has the smallest figure number on the plate in the first publication. In addition to the advice given above, this designation of lectotype is inappropriate because *Pollenites iliacus* was already put into the legitimate genus *Ilexopollenites* Thiergart in 1937.

Sporites was believed to have the genotype *Sporites plicatus* Schopf (1938; see ANDREWS, 1955, p. 242) although Schopf correctly suggested that a type was not necessary. *Sporites* was erected by Henry Potonié in 1893 (see R. POTONIÉ & G. KREMP, 1955, pp. 31, 33). The species then mentioned in *Sporites* were already erected by Dawson in 1866. They were the species *papillata* and *glaber*. But these species were never adequately described or figured.

Sporites and *Pollenites* should, therefore, only be used in future as higher "Dilemma" groups (turma) containing legitimate genera.

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