NOTES ON THE BASIDIUM

by

M. A. DONK*

(Rijksherbarium, Leiden)

(Issued Oct. 2nd, 1958)

INTRODUCTION.

The terminology of the basidium became complicated when Neuhoff (1924: 256) introduced the term epibasidium for various extensions (exclusive of sterigma-tips) from basidia. Such diverse organs as (i) extensions from probasidia, like the four-celled body producing the sterigmata in *Auricularia* Bull. ex Mérat, and (ii) extensions from metabasidia, like the sterigmata (minus tips) of *Tremella* Fr. and *Tulasnella* J. Schroet., all became epibasidia. Neuhoff's views were enthusiastically defended by Rogers (1934), then a pupil of Dr G. W. Martin, and they have been upheld by Martin himself (1938) and other mycologists of his school, which concerns itself particularly with the Heterobasidiae.

Neuhoff's terminology has been rejected or criticized by various authors: Donk (1931: 78—81; 1954; 1956: 366), Linder (1940), and Talbot (1954: 249—263). They all rejected the term epibasidium. Recently Martin (1957: 27) concedes that it "has in some instances been incorrectly applied", and restricts it, presumably, to strongly developed sterigmata minus their tips, that is, to category (ii). This removes one of the principal issues of divergence in opinion between Martin's conception on the one hand and Talbot's and Donk's on the other. For the organs of category (i) originally included under the term epibasidium, Martin emends Donk's term metabasidium, thus restricting it to metabasidia that arise from probasidia that remain morphologically distinct. A few questions remain to be considered.

HOLOBASIDIA.

Martin (1957: 25) reproaches me for calling the *Tulasnella* basidium a holobasidium. Let me confess that I had not realized that it ever has been called a phragmobasidium, although I was aware of the fact that Rogers (1932; 1933: 1) had insisted on the presence of septa across the bases of the sterigmata and had considered "the septation of the basidium" an important feature of the Tulasnellaceae. Mycologists now follow

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* The author is much indebted to Dr R. W. G. Dennis, Kew Herbarium, and Dr P. H. B. Talbot, Pretoria, for corrections and suggestions.

Patouillard, who emphasized that in this family the basidial body remains undivided as compared with true phragmobasidiate "Hétérobasidiés": *Prototremella* Pat. [= *Tulasnella*] "diffère essentiellement par ses basides simples et non cloisonnées ..." (1888: 269); "la cavité reste simple", "basides subglobuleuses, non septées" (1900: 3, 26). Heim (1949: 6) in 1939 combined the "Tulasnellacées et Caloceracées sous la denomination de Protoclavariales afin de rapprocher tous les Basidiomycètes dont la baside est à la fois complexe et entière, autrement dit les champignons à hétérobaside non cloisonnée". It is this very situation—the undivided basidial body—that I translate into 'holobasidia', although I am fully aware of the presence of septa across the bases of the sterigmata. This is Martin's pertinent paragraph:

> "Despite the fact that Donk recognizes that the swollen, spore-like bodies which produce basidiospores in *Tulasnella* and *Gloeotulasnella* are definitely cut off by septa from the basal portion before the basidiospores are produced, he is forced by his peculiar interpretation to call them holobasidia, to say, that is that they are divided undivided basidia. Surely, this is to make the distinction phragmobasidia and holobasidia meaningless ...."—Martin (1957: 25).

In my opinion, in a phragmobasidium the basidial body becomes divided into cells by true cross-walls. In *Auricularia* the resulting cells are superimposed, in *Tremella* the basidia appear (usually) cruciately divided into four cells if viewed apically, or the cells may be less regularly arranged. In *Tulasnella* and *Gloeotulasnella* Höhn. & L. the basidial body remains undivided, hence, with my definition, I cannot call them phragmobasidia by any stretch of imagination. I cannot accept a definition, like the one implied by Martin, that calls for homologizing the septa separating the sterigmata from the basidial body in *Tulasnella* and *Gloeotulasnella*[1] with the septa in the basidial body of *Auricularia* and *Tremella*. I draw attention to the holobasidia in *Clavulina* J. Schroet. where "the secondary septum, which cuts off the completely vacuolate distal half, or third, of the basidium after spore-discharge, is a constant and noteworthy feature. In some species it is followed by several more septa formed successively in the proximal part as the protoplasm withdraws."—Corner (1950: 65). Here the septa develop in the basidial body itself, but still I see no reason to speak of phragmobasidia in this case.

Sometimes septa are formed in sterigmata under abnormal conditions, especially when the sterigmata tend to be long and more or less hypha-like. One will come across these septa in *Dacrymyces* C. Nees ex Fr. and also in *Tulasnella* (Patouillard, 1888: 269 f. 2; Juel, 1897: 7 fs. 11, 17). One of the septa Juel observed occurred precisely between protosterigma and secondary protosterigma. Juel remarked that the cell contents had moved into the terminal portions of the tubes and that these plasma-filled portions had produced septa which separated them from the empty basal portions. He correctly added that no real cell divisions had occurred. These septa are in my opinion of the same nature as those formed in *Clavulina* and I would call them 'false' septa. Patouillard persisted in

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[1] In *Ceratobasidium* D. P. Rog. the sterigmata are "occasionally with an adventitious basal septum."—Rogers (1935: 4).

describing the *Tulasnella* basidium as undivided, although he reported occasional septa in the sterigmata (but not those across their bases). It will not do to use the term phragmobasidia in these cases either. And yet the only difference between these septa and those across the bases is the place where they are formed: as soon as the protosterigmata in *Tulasnella* have absorbed the contents of the metabasidia the septa separating the plasma-filled protosterigmata from the empty (or completely vacuolate) metabasidium are formed. These basal septa, too, are false septa *in optima forma.* Here is another essential difference between the septa in the *Tulasnella* sterigmata and the septa in phragmobasidia: the latter are true septa formed after true cell divisions! Not only would I conclude that the *Tulasnella* basidium is an undoubted holobasidium, I believe it to be incorrect to call it a phragmobasidium.

Basidiospores have often been accepted as part of basidia, for instance by Martin (1938: 684); this is a matter of definition and such an arrangement of facts has much to recommend itself. It may then be recalled that in *Dacrymyces* and *Exobasidium* Woron. the basidiospores may become divided by cross-walls, but it seems very unlikely that anyone will start calling the holobasidia in these genera phragmobasidia, although we are dealing here with 'true' septa.

METABASIDIA.

Martin restricts the term metabasidium to thin-walled extensions from a "basidial cyst". Then the four-celled body producing the spores in *Iola* would be a metabasidium, but the same body in *Mycogloea* L. Olive would not. It also implies that the four cells of the mature *Tremella* basidium are not homologous with the four cells constituting the metabasidium in *Iola.* This will not be acceptable to many a mycologist. The question has already been ably discussed by Talbot (1954: 260) and I understand that Dr P. H. B. Talbot will soon return to this matter.

There is very little in Martin's definitions indicating the differences between his metabasidium and his epibasidium; the first beng "a thin-walled extension from a basidial cyst" and the second, "a tubular or swollen extension from the body of the basidium to the hymenial surface, where it produces a sterigma". Martin does not discuss the distinction between a 'basidial cyst' and a 'basidial body'. It also appears from his paper that by 'hymenial surface' the surface of a gelatinous fruit-body is meant. If such a surface is lacking (when the basidia are not imbedded in a matrix) one would conclude that no epibasidia are formed. Yet, Martin seems to admit the presence of epibasidia in *Tulasnella, Ceratobasidium* D. P. Rog., and *Cerinomyces* G. W. Mart., while "a strkingly similar development may be found in certain gelatinous Homobasidiomycetes, notably in *Myxomycidium* [Mass.]". The words 'hymenial surface' in Martin's definition thus appears to be of restricted value only. Given this situation, and the fact that one is left at a loss to find the difference between a 'basidial cyst' and the 'body of the basidium', the definitions appear inadequate.

In practice one will recognize Martin's metabasidia by various features not mentioned in his definitions: number per cyst (one); division into spore-bearing cells; or if they are undivided, a set of apical sterigmata.

In very special cases such as occur in *Septobasidium grandispora* Couch (1938: 64, for cytological data), *S. purpureum* Couch, *Platygloea unispora* L. Olive (1944: 17 pl. 4 fs. 1—11), where the metabasidia remain undivided and each produces only one apical sterigma, Martin's definitions are particularly unconvincing. Cytological criteria would be of much help, but these Martin does not consider essential (see also p. 103).

STERIGMATA.

If one were forced to use the term sterigma in connection with basidia in as wide a sense as possible without making it meaningless and keeping as close as possible to common usage, one would come to the following definition for it, I believe: any extension from a metabasidium that bears a basidiospore at its tip, or as Linder (1940: 421) would have it, "that structure which arises from the basidium and bears the basidiospore", which means precisely the same. These definitions cover exactly those structures which have been usually called sterigmata and which may be variable in development, form, and size. Linder thought it superfluous to go any further and to extent terminology in this regard.

Talbot (1954: 255—259) and Donk (1954; 1956: 366, 373) agree with Linder that the term sterigmata should be maintained for the structures currently so called, but they think that there is scope for finer distinctions especially when the complexity of the sterigmata has to be carefully analysed. In the most complex sterigmata they distinguish three successive portions, viz. protosterigmata, secondary protosterigmata, and spicula. The secondary protosterigmata may be lacking and the (primary) protosterigmata may be hardly developed and often little more than hypothetical: the sterigmata then are almost completely represented by the spicula. All transitions exist between these extremes. The conception of the protosterigma as the initiating stage of the sterigma is supported by the facts: every sterigma, however thin when fully developed, starts as a bud-like outgrowth, not as a spiculum. The protosterigma is an essential developmental stage and admitting it as a portion of the sterigma provides an additional base for defining the latter organ as Donk (1931: 73—75, 115) and Linder did, in agreement with common usage. This is not to say that in fully developed sterigmata the protosterigmata are always morphologically conspicuous: quite often they are not. The protosterigma may almost immediately produce a sterigma (*Agaricus* type), or it may grow out considerably into a hypha-like body (*Tremella* type), or it may swell into a voluminous spore-like structure (*Tulasnella* type). The protosterigma is more essential than the spiculum. The latter may be absent, for it is characteristic only of a sterigma that bears a ballistospore: for instance in Gastromycetes with sterigmate basidia the spicula are lacking. Every deviation from a long-prevailing definition should be well founded and a proposal to transfer the corresponding term to, let us say, a not too essential portion of the organ that may even be lacking, must be rejected. In my opinion, this is the case with Neuhoff's emendation of the term sterigma.

Martin now substitutes for sterigmata in the above traditional sense what he regards as two different kinds of organs, both extensions from the basidial body, to wit (a) true sterigmata (*Agaricus* type) and (b) epibasidia (*Tremella* type, and, presumably, *Tulasnella* type). The epibasidia usually become tipped by sterigmata or presumably by secondary epibasidia tipped with sterigmata. In his view the two kinds of basidial extensions do not merely represent two steps in degree of development, no, they represent essentially different organs.

On a previous occasion (Donk, 1956) I have brought together under the designation of 'the tulasnelloid fungi' such genera as *Tulasnella* and *Gloeotulasnella*; *Ceratobasidium*; an unnamed group that recently has been called *Heteromyces* L. Olive[1]; and *Thanatephorus* Donk and *Uthatobasidium* Donk, the latter two genera segregates from *Botryobasidium* Donk (*Pellicularia* Cooke sensu D. P. Rog.) and like the rest possessing spores exhibiting repetition, although they closely agree in other respects with *Botryobasidium*, a genus in which the spores do not exhibit repetition. I have also stated as my opinion that it is easy to arrange these genera in a continuous series as far as the development of the sterigmata is concerned, viz. sterigmata rather tenuous, curved (*Botryobasidium* restricted sense), sterigmata rather stout, often still horn-shaped, i. e. curved (*Heteromyces*), sterigmata rather strongly developed (*Thanatephorus, Uthatobasidium*), sterigmata perhaps sometimes slightly more voluminous (*Ceratobasidium*), and sterigmata strongly developed, voluminous, separated from the basidial body by cross-walls (*Tulasnella, Gloeotulasnella*). When Juel (1897) separated the Tulasnellaceae as a remarkable family he had every reason to do so. Since then the gap between *Tulasnella* and the Corticiaceae has become filled by an imposing number of intermediates and *Tulasnella* now stands out as an extreme term in an intergrading series, at least if one arranges these forms according to the degree of development of their sterigmata.

Martin has not attempted to explain the *morphological differences* between the 'epibasidia' of *Ceratobasidium* and, for instance, the 'sterigmata' of *Thanatephorus* (except in terms of shrinkage of the basidial body). This is a regretable omission because in nature these extensions of the basidium are not labelled. An increasing number of mycologists find difficulty in grasping the essential distinction between the 'epibasidia' of *Ceratobasidium anceps* and the 'sterigmata' of *Thanatephorus cucumeris*[2] For instance, Jackson (1949: 243) remarks that "it would seem more logical to include *Pellicularia* in the family Ceratobasidiaceae ...". Talbot and Donk are unable to find any fundamental difference between the sterigmata of the two species. More recently Pilát (1957: 81) and Olive (1957a: 431) even transferred the second species to *Ceratobasidium*, thus indicating that

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[1] This is Donk's group (vi) of the tulasnelloid fungi (1956: 373) which he temporarily entered into a heterogeneous genus *Ceratobasidium*. In the meantime it has been raised to generic rank and called *Heteromyces* L. Olive (*1957a*: 432).

[2] These two species will be found under various names in recent literature: the first, *Ceratobasidium anceps* (Bres. & H. Syd.) H. S. Jacks. = *Ceratobasidium vagum* (Berk. & C.) Pilát sensu Pilát; the second *Thanatephorus cucumeris* (Frank) Donk = *Hypochnus solani* Prill. & Del. = *Botryobasidium solani* (Prill. & Del.) Donk = *Pellicularia filamentosa* (Pat.) D. P. Rog. = *Ceratobasidium solani* (Prill. & Del.) Pilát = *Ceratobasidium filamentosum* (Pat.) L. Olive.

they do not consider the two even generically distinct, although they maintain the Ceratobasidiaceae as a family! The omission of a morphological character that would enable one to differentiate between the 'epibasidia' of *Ceratobasidium* and the 'sterigmata' of *Thanathephorus* is the more unexpected because in Martin's paper under discussion (1957: 27) he emphatically states, that "in taxonomy, the morphological data always have been, and presumably will continue to be, the immediate bases for classification".

Martin admits that series of all intermediate conditions exist between his 'sterigmata' and his 'epibasidia'. He writes that "it is not unusual to find a single basidium of the auriculariaceous type in which the lower segments bear a distinct epibasidium while the upper segments form sterigmata directly". Similar and other considerations lead Talbot and Donk to assess Martin's 'epibasidia' (protosterigmata) as variable and subordinate parts of sterignata, rather than as independent structures. Martin (1957: 26) continues, "The transition is evident, but the argument that makes it impossible to recognize epibasidia applies equally to protosterigmata as defined by Donk." I entirely agree, but I would point out that the transition induced Donk to subordinate the protosterigmata to the sterigmata, while Martin considers them structures completely different from his sterigmata (spicula).

The difference between the two evaluations becomes more evident on application of the terms. If Martin speaks of a species merely as having "relatively large sterigmata", he first has to decide whether or not these sterigmata are seated on 'epibasidia' and, then, commit himself by implicitly, but definitely, denying the presence of the latter. When Donk speaks of sterigmata, the organs he has in mind are much more clear-cut structures and he need not commit himself as to whether or not 'epibasidia' (protosterigmata) are developed, unless he has made up his mind and explicitly wants to state his views on them. If I accepted Martin's conception of 'epibasidia', I would still contend that they were present in the "relatively large sterigmata" of *Thanatephorus* and *Uthatobasidium.* Donk can leave out the problem and merely speak of sterigmata in this case.

To conclude this chapter I would summarize my opinion by stating that it is preferable to continue calling sterigmata just sterigmata, instead of two distinct structures whether or not combined in addition into more complex superstructures. The loss of the term epibasidia would not have been lamented by many: it is a pity not only that it has been maintained, but also that it has got a new meaning without the original sense being yet abandoned. "Continued use in both senses would only compound the confusion." — Olive (1957b: 668).

THE TULASNELLA BASIDIUM.

As for the strongly developed and inflated, not yet sporulating sterigmata (protosterigmata) in *Tulasnella* and two related genera, Martin comments upon them as follows:

> "Despite the differences in place of septation, the effect of the septa in *Tulasnella* is to localize in (usually) four cells the protoplasm and included nuclei that eventually will enter the spores, exactly as happens in a typical tremellaceous or auricularious

basidium ... [The spore-like bodies in *Tulasnella* and *Glocotulasnella*] are morphological equivalents of the four cells into which the *Tremella* ... or the *Auricularia* basidium is divided."—Martin (1957: 26, 28).

I would put forward again, as a mere suggestion, that functionally the rôle of the *Tulasnella* protosterigmata is likely to be comparable with that of the club-shaped terminal portion of the *Tremella* protosterigmata in species with strongly gelatinous fruit-bodies, rather than with the part-cells of the *Tremella* metabasaidium which has become highly vacuolate (appearing 'empty') when the basidiospores are being formed. In both cases most of the plasmatic contents of the metabasidia will be found driven into the protosterigmata.

According to Martin, the combined protosterigmata[1] of a single basidium constitute a complex *morphologically* equivalent to the mature (divided) basidial body of the *Auricularia* basidium, that is, to that portion and stage of the basidium that Martin agrees to call a metabasidium. This is an old and long buried conception revived. Schroeter (1888: 397) foreshadowed it ("... Sterigmen, welche ... den Theilbasidien gleichen ..."); Patouillard (1888) tentatively suggested it; and Boudier (1896) accepted it more definitely. After the cytology of the basidia of *Tulasnella* had become known, this interpretation ceased to find support.

To remain consistent Martin should proceed to call the combined set of protosterigmata of a *Tulasnella* basidium, a metabasidium. Thus we would come back very close to Neuhoff's original terminology, with a remarkable exchange of the word 'epibasidium' for 'metabasidium'. Or, else, if one calls the *Tulasnella* protosterigmata epibasidia, to remain consistent, why not call the part-cells of a divided metabasidium, epibasidia too?

This conception, of the protosterigmata of a *Tulasnella* basidium being *morphologically* equivalent to a metabasidium, will prove unacceptable to many who admit cytological criteria into their definitions of the basidial terminology: the fusion of the dikaryon and the subsequent meiosis takes place not in the sterigmata but in the basidial body, which, therefore, is equivalent to the phragmobasidial body of *Tremella*. That a third division of the diploid nucleus of the basidium may occur in the protosterigmata of a few species of *Tulasnella* is in line with the 'autonomous' status of the protosterigmata and on which I have commented before (Donk 1956: 367). The first division of the only nucleus in basidiospores is, conceivably, equivalent to the third division of the diploid nucleus that may occur in the basidial body and to the division that occurs halfway in the protosterigmata of some species of *Tulasnella*. This does not alter the fact that the spores, the protosterigmata, and the basidial body are different organs.

Recently an additional example of strongly developed, more or less 'autonomous' (in this case, even deciduous) protosterigmata was described by Bandoni (1957) for *Sirobasidium* Lagerh. & Pat. The situation is as follows, to use Bandoni's own words: more or less fusiform and deciduous epibasidia produce short tubes tipped with sterigmata, or the sterigmata

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[1] Martin nowhere explicitly calls these protosterigmata either 'epibasidia' or 'metabasidia', but from his definitions I would conclude that he considers them to be epibasidia.

arise directly from the epibasidial wall; the basidiospores are violently discharged. In my terminology the epibasidia are protosterigmata; the short tubes, secondary protosterigmata; and the sterigmata, spicula; and all three organs together constitute sterigmata. Conceivably an author agreeing with Martin on the interpretation of the *Tulasnella* basidium would call the protosterigmata in *Sirobasidium* morphological equivalents of the *Tremella* or *Sirobasidium* metabasidium. But, in that case, how can this be true, when both sets of organs occur in the same basidium as morphologically distinct ones?

CYTOLOGICAL CRITERIA.

Martin objects to a definition of the basidium that includes cytological criteria, which would fail, he writes, in instances in which karyogamy and meiosis are lacking. His definition of the basidium reads as follows:

> "*Basidium.* The organ of Basidiomycetes which bears the basidiospores, either directly or on extensions from the primary basidial cell. Nuclear fusion and meiosis usually occur in the earlier stages, but neither such occurrence nor the places in which they occur is an essential part of the definition."—Martin (1957: 28).

This definition and other remarks made in his paper allow us to rewrite Martin's definition in all fairness as, 'the organ of Basidiomycetes which bears the basidiospores', neither the extensions nor the cytology being essential. No definitions of a Basidiomycete or a basidiospore are given. This leaves us with nothing essential at all to recognize a basidium. Cytological criteria, it would seem, — even if they may fail sometimes — are after all essential to prevent the definition from becoming completely meaningless. Martin has failed so far to supplement his definition with other criteria that would have made it workable without cytological criteria. The following arguments are raised in favour of keeping karyogamy and meiosis as essential features of the basidium.

(i) In all cases where mycologists doubted the basidial nature of certain spore-producing cells, it was the cytological data which proved to be decisive. To mention a few instances of conidiophores wrongly identified with basidia: *Microstroma* Niessl (cf. Maire, 1913; Wolf, 1929), *Protocoronospora* Atk. & Edgert. (cf. Wolf, 1920).

Another example is *Syzygospora* G. W. Mart. The basidia which led Martin (1937) to establish the latter genus as a member of the Auriculariaceae proved to be in reality conidiophores. The true basidia appear to be quite different and are holobasidia. These conclusions are based on cytological data (Kao, 1956).

Without such data one would homologize different organs, as Brefeld did when he made *Polyporus annosus* Fr. a special genus, *Heterobasidion* Bref., characterized, he thought, by two types of conidiophores, of which one, the basidia, were merely conidiophores producing a relatively small and fixed number of spores: "die Basidie ist nichts anderes, wie der zur typischen Form und zur bestimmten Sporenzahl fortgeschrittene Conidienträger ...". This may or may not be true, but in any case it is now possible to draw a much clearer demarcation between the two types than Brefeld was able — or willing — to do; we need not doubt that the nuclear behaviour of the basidia of *Polyporus annosus* conforms with the usual pattern.

(ii) Obviously, the main reason that caused Martin to exorcize cytological data as not essential for definitions of the basidium and its major portions or stages, is the lack of karyogamy and true meiosis in haploparthenogenetic forms. Yet, even in these forms the behaviour of the single nucleus and its division retain so many of the characteristic features of the normal cytological course that it may be assumed that many features of the normal pattern are never wholly lacking. To mention some items: the single original nucleus in parthenogenetic basidia swells before division to a considerable size, comparable to the swelling of the diploid nucleus in normal basidia; and moreover the position and length of the division spindle too is remarkably alike in parthenogenetic cases and in their non-parthenogenetic counterparts.

There is an obvious relation between shape and size of the basidium on the one hand and position and size of its diploid nucleus and the first division spindle on the other. In parthenogenetic cases nothing essential is altered in this respect: it is not possible to distinguish between haplontic and dikaryontic basidia from the outward development and shape of the basidial body alone; not even volume of the basidia seem to change materially. Kühner (1938: 127) has remarked that he was originally inclined to consider the smaller volume in two-spored basidia as a consequence of parthenogenesis, as Bauch did for *Hygrophorus virgineus*. But when comparing his data with corresponding ones obtained from two-spored, but originally dikaryontic, basidia, he concluded that the difference in volume between two- and four-spored basidia of the same species is precisely of the same order in both haplontic and dikaryontic cases. (There are indications that the swollen nucleus in parthenogenetic cases may be smaller than the diploid nucleus in normal basidia.) It is reassuring that the cytological data do not completely desert us in parthenogenetic cases! For details, compare, for instance, Maire (1902: 116 pl. 4 fs. 19-28, pl. 7 fs. 1-3, two species of *Hygrophorus* referred to *Godfrinia* Maire), Bauch [1926; *Hygrophorus virgineus* (Wulf. ex Fr.) Fr.], Smith [1934; *Mycena megaspora* Kauffman, pl. 37 fs. 1-10; *M. leptocephala* (Fr.) Gill., pl. 37 fs. 28, 30, 36-38; '*M. dissiliens* (Fr.) Quél.,' pl. 37 fs. 32, 33, 39-41], and Kühner (1938; *Mycena atromarginata* Lasch, pl. 9).

(iii) It is true that karyogamy and true meiosis may be lacking in a relatively small number of cases, but this does not make cytological data 'not essential' in those many cases where nuclear behaviour is 'normal'.

(iv) Few biological definitions will be tenable if one is to insist that they must be literally applicable to hunderd procent of cases.

Not so long ago Martin himself (1952: 3) considered the cytological criterion all-important and wrote: "The characteristic organ of the Basidiomycetes is the basidium. This is essentially a cell in which there is a fusion of two nuclei, followed by meiosis, the four daughter nuclei so produced migrating into protrusions of the cell wall and there becoming the nuclei of the basidiospores."

Secondary basidiospores.

Patouillard called the capability of a basidiospore of producing a secondary one on a sterigma-like outgrowth, 'germination par rénovation', and Martin (apud Rogers 1933: 183) rendered this expression as 'germination by repetition'. Donk (1956: 363) thinks that the use of 'germination' is incorrect here: the basidiospores repeat themselves without the actual process of germination setting in. He uses such expressions as 'basidiospores exhibiting repetition'. Martin (1957: 29) thinks that this seems to be an unnecessary quibble. However, as the process is of importance because it is often considered of high taxonomic value, I can see no sufficient reason to uphold an implication that is perhaps not strictly correct and appears superfluous. Martin agrees that the homology of the sterigma producing the secondary spore is obvious and strictly comparable with the production of a primary basidiospore on a basidial sterigma. Decidedly it would be no improvement to follow this terminology to its logical conclusion and speak of 'germinating' basidia when one meant that they form the usual type of basidiospores.

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