# Two Centuries of Systematics of Bryophytes - what will bring the Future? [1]

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**Summary:** The development of and the changes in the classification of bryophytes are shown over the last two centuries. It is shown that, except for an early period, the bryophyte system was never totally revised but was always only slightly improved, in which the ideas of a bryophyte system has undergone some kind of evolution but was never drastically changed. Neither the evolution theory in the last century nor the results of cytology, phytochemistry, computerbased studies or genetics in this century had drastically altered the bryophyte system. The reasons for the never totally changed but always slightly improved classification of bryophytes seem to be that recognition of systematic units is based on certain principles such as hierachical classification, types and algorithms of comparison. Therefore it is postulated that no general changes in the classification of bryophytes are to be expected in the future.

## 1. The history of bryophyte classification

Scientist have described approximately 2 millions of organisms in about 0.5 million of systematic categories. The bryologists described "only" about 80.000 species (57.000 species of mosses, Crosby et al. 1992). The greatest challenge was, to classify these species in a system.

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[1] Introductory lecture at the IAB symposium on 2000´s bryology , Beijing, China, 26-30 May 1997.

Vitt (1984) and Buck (1991) have already outlined the historical development of the systematics of mosses. Since 200 years, bryologists classified. The first classifications were based on **genera**. Dillenius recognized 6 genera in 1741, Linnaeus 8 in 1753. Hedwig already recognized 25 genera in 1782. And the trend continued and continues at present.

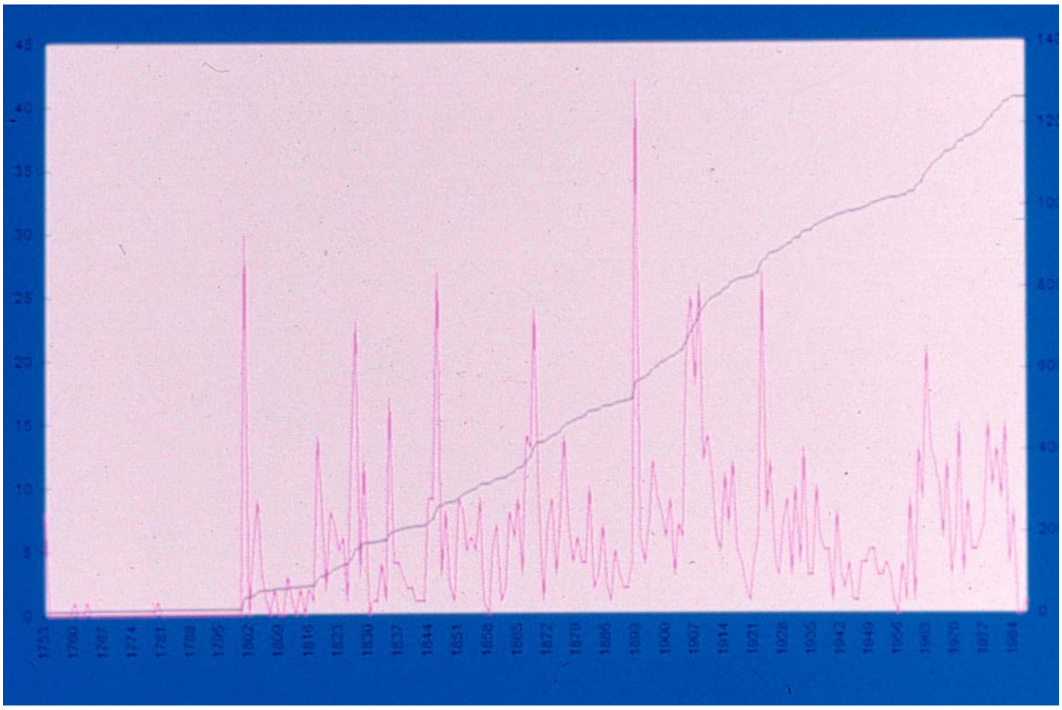

Fig. 1: Numbers of described genera of mosses over the past two centuries.

As an example, a curve (fig. 1) showing the number of described genera over the past 200 years is shown. It is based on the „Generic Names in Current Use“ for mosses, which was converted in a database format. The cumulative curve shows a more or less continuous increase from 273 genera in 1850 to 409 in 1870, 613 in 1900 und 999 in 1950 to about 1263 genera at present. Surprisingly, the description of genera of bryophytes was relatively constant over the past 200 years, and interestingly, the curve does not show yet a saturation. The numbers of genera described per year varies much. Peaks represent publication dates of books (e.g. Hedwig´s Species Muscorum, the Bryologia Europaea or the second edition of the Natürlichen Pflanzenfamilien). Minima occur at the begin and at the end of the last century and also in the period 1930-1960. The raise of the number of genera does not depend on the scientific exploration of the tropics. If we count the numbers of genera recognized in Europe over the past two centuries, we see (fig. 2) the same increase:

| | |
|---|---|
| Dillenius 1741 | 6 |
| Linnaeus 1753 | 8 |

| | |
|---|---|
| Hedwig 1782 | 25 |
| Hedwig 1801 | 32 |
| B.S.G. 1836-56 | 127 |
| Mönkemeyer 1927 | 183 |
| Corley et al 1981 | 226 |

Even between 1927 and 1981, the number of genera raised from 183 to 226.

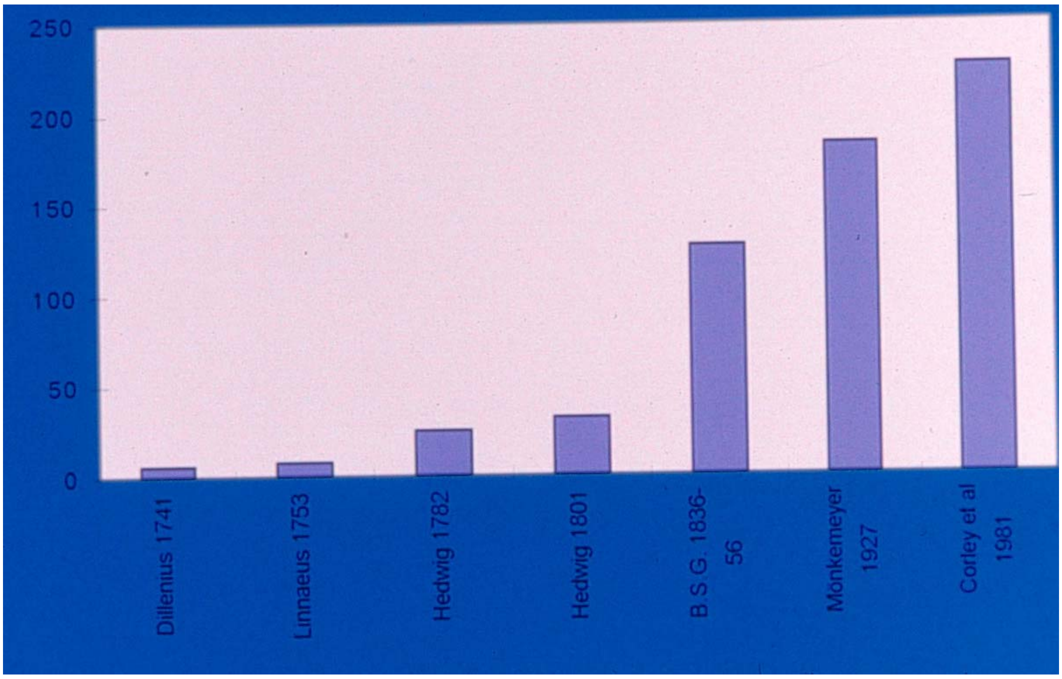


Fig. 2: Numbers of genera of mosses recognized in Europe over the past two centuries.

It would be interesting to compare this curve with the same for species, since this will probably look different. However, an evaluation on a species level is not possible, since the data e.g. of the Index Muscorum or Index Hepaticarum are not available in computerized form (as those of the „Index Kewensis“).

Similar curves can be obtained for bryological publications. Therefore the increase of genera is related with the increase of bryological activities as expressed in the numbers of publications. If the numbers of publications per year of local floristical bryological publications for Germany are shown in a curve (fig. 3), an enormous increase of bryological activities is shown during this century, interrupted by curve minimas during the time of the world wars.

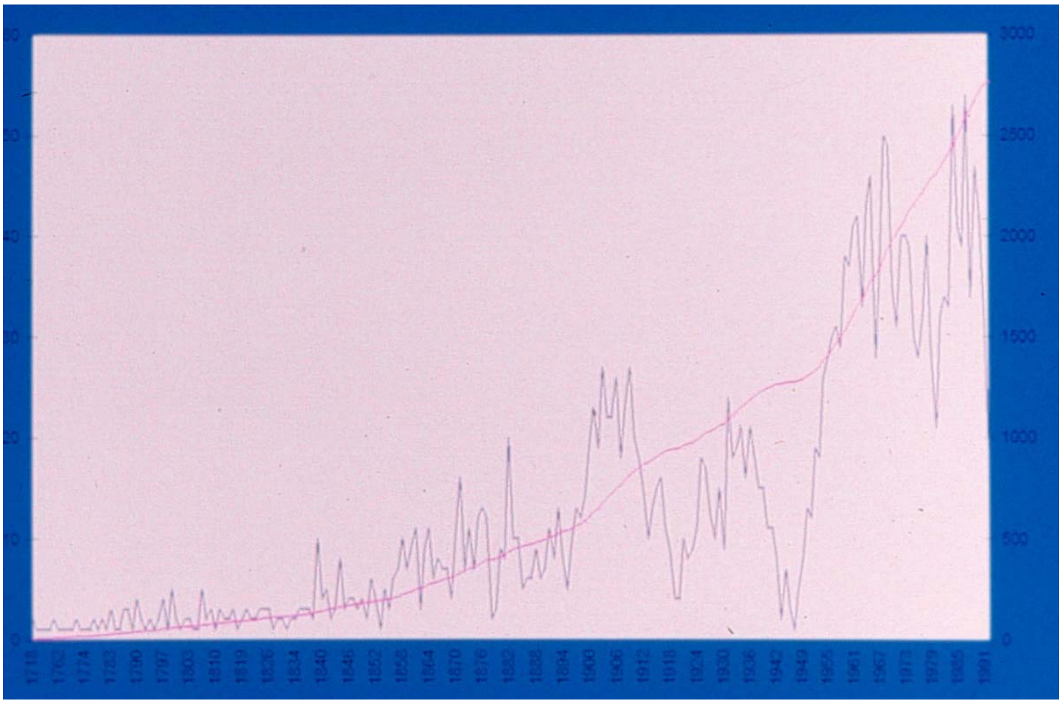


Fig. 3: Numbers of bryological publications in Europe during the past two centuries.

An important influence on the bryology (if not the most important) was the use of microscopes. It has to be considered, that so called microscopes existed already 150 years before the start of scientific bryology in the times of the dutch scientists Huygens, Swammerdam and Leeuwenhoek, but these microscopes were no compound microscopes and hardly more than strong magnifying hand lenses. The first microscopes in our modern sense came up at the end of the 18th century. These first microscopes must have been comparable with Scanning Electron Microscopes of today with regard to price and availability. Hedwig as a physician could not effort a microscope personally, but got one from Schreber, professor at the university of Erlangen. The only picture of Hedwig shows him together with a microscope, and a microscope was illustrated at the cover page of the „Species Muscorum“. The efforts of Hedwig to bryology are usually contributed to him personally but are in fact due to the use of a microscope and the recognition of new microscopic characters. The invention of the Scanning Electron Microscope has later had not that influence on bryology. Apart from the nice pictures, the scientific input for systematics was small, it opened no new dimension as the compound microscope.

ISSN 0945-3466

Bridel in 1819 was the first to classify bryophytes in **families, orders** and **classes**. Until the midth of the last century and before the recognition of the life cycle of bryophytes by Hofmeister, bryophytes were taken for small flowering plants. Interestingly, this wrong point of view had not much influence on classification. The consequence was, however, that mosses were mainly classified by the sporophyte, since the classification of flowering plants was based on flowers. So it was apparently not the merit of older authors such as Hedwig, Palisot de Beauvois, Taylor, Bridel and others to classify by the sporophyte but it was the common trend at that time. The first bryologists, who put more emphasis on the gametophyte, were Schimper and Mitten, and this was after the recognition that bryophytes are spore plants.

The use of gametophytic characters was also caused by the low number of available sporophytic characters. Therefore Schimper, Mitten and Lindberg used sporophyte characters for higher taxonomic units and gametophytic characters for lower ones. At the end of the last century, Philibert published his „Etudes sur le péristome“, which influenced the classification by Fleischer in 1904 and by Brotherus in his second edition of the „Natürliche Pflanzenfamilien“ in 1924. All the changes in moss systematics were in fact never alterations but slow and steady improvements and refinements, beginning with Nees von Esenbeck, who classified into „Acrocarpi“ and „Pleurocarpi“ to arthrodontous and nematodontous mosses introduced by Mitten to Hapololepidei and Diplolepidei introduced by Philibert.

It has often been stressed that no major alterations in the systematics of mosses happened since Fleischer in 1904. Crosby (1980) and Vitt (1982) proposed new models, but these are hypothetical evolutionary models and no pure classification systems.

## 2. The influence of different scientific disciplines

### 2.1 The influence of the discovery of the life cycle

Buck (1991) mentions that bryological classification in the beginning of bryology was much hampered by the understanding of the life cycle. But has this ever had an drastical influence on moss systematics? Aside from some errors in the early bryophyte systematics (e.g. the separate position of cleistocarpous mosses) there were no fundamental errors, independent whether bryophytes were understood as small flowering plants or as spore plants. And this is because classification has nothing to do with real recognition. This is shown by the fact that bryophytes were originally regarded as small flowering plants in the 18th and the first half of the 19th century. Moss capsules were anthers for Dillenius and Linnaeus. Hedwig discovered the sexuality of mosses as the first but for him the archegonium was the gynaecium and the antheridium was an anther, as expressed in the word. Consequently the sporophyte was the fruit with a capsule and seeds in it. Older bryologist used the term fruit for the capsule even at a time when it was clear that the sporogon is not a fruit and some of us use still the abbreviation „c.fr.“ (cum fructibus) to indicate that a specimen has sporophytes.

The life cycle was first detected in 1851 by Hofmeister, professor at the university of Tübingen in Germany. However, his results were long time ignored. The reason is, that Hofmeister was a very modest person. In his famous publication, for which he would have gotten a noble price if this price would have existed at that time, Hofmeister (1851) deals with the description of the anatomy of *Anthoceros*, „leafless and foliose Jungermannias, ferns, horsetails, lycopods and conifers, illustrated by numerous plates. Only in the last few pages he gives a summary in a comparative review that „bryophytes and ferns show a regular change between two generations“. The cytological background was not known at that time.

Two years later, C. Müller in his book „Deutschlands Moose“ ignored Hofmeisters results totally and explained in the chapter „what is a bryophyte?“ that archegonia are for sure gynoecia but the explanation of the antheridia with spermatozoids as found in animals could not be male pollen.

Apparently the discovery of the life cycle of the bryophytes had no essential influence on the classification, and for a classification it was less important whether bryophytes were flowering plants or spore plants. The interpretation for the organs changed, but without consequence for classification. So it was apparently not important whether bryophyte spores were pollen, seeds or spores.

**2.2 The influence of the evolution theory**

Also the evolution theory was never essential for the classification of bryophytes. It can hardly be traced, when or how the evolution theory influenced bryology. The adaptation of Darwin´s and Wallace´s principles in bryology apparently took long time. It seems as if the principles of evolution theory were widely ignored by bryologists during the fifty years after publication of Darwin´s "Origin of Species". It seems as if this subject was "too hot" for bryologists, or there was no necessity seen to deal with this new theory, or part of the bryologists were too conservative. Some of the important bryologists are named as "creationists", e.g. Carl Müller. This is, however, not true, at least not for C. Müller. He wrote (Müller 1853) in the preface to his book on German mosses: „How could it be that the 3000 bryophyte species known to us were created?“ He explained that the organic world is composed of (at that time) 64 elements, that we have 10 numbers, 24 letters, 3 colours and that the whole nature is a product of combination, in bryophytes a combination of different expressions of stem, leaves and capsule.This implies something of the (re)combination of characters in the modern biology but no theological background.

It is perhaps less known that one bryologist corresponded with Darwin. This was Hermann Müller from Lippstadt in Germany. Müller was high school teacher and had studied intensively the pollination biology of plants. Because of the observation of the perfect correspondence of insect morphology and flower morphology, he was much convinced from Darwin´s theory. Since he was also studying bryophytes, he made the attempt to apply the principles of evolution theory on bryophytes (Müller 1867). Müller argued for the first time in a less known publication, that species may not have varieties or forms if they were created and that the presence of varieties is the result of mutations, which lead to new species. The presence of „good“ and „bad“ species is interpreted

ISSN 0945-3466

for the first time as an expression of an evolutionary species concept. Interestingly, his opinion was totally ignored. Later Grebe (1917), a German forester, explained the morphology of bryophytes in some kind of Neodarwinism as an evolutionary response to their habitats, e.g. upright capsules as result of adaptations to light habitats and inclined capsules as an adaptation to shady habitats.

Although evolution principles were never really incorporated in bryology, the classification was nevertheless not wrong or had to be substantially revised.

**2.3 The influence of the knowledge of plate tectonics**

The continental drift allows to explain disjunct ranges separated by oceans. It seems today as if the explanation of disjunctions by the continental drift was mainly developed during the past thirty years. This is, however, not the fact. Disjunct ranges were already known by De Candolle in 1835 at the begin of the last century, who introduced this term. He explained these as the result of drowned continents, similarly as the so called Atlantis theory, which postulated a drowned continent between Europe and North America. Earlier Willdenow in1798 proposed a connection between Australia and Southern Africa because of species in common. Later Hooker postulated former connections between the landmasses in the antarctic.

Already 200 years ago it was supposed that Africa and South America were connected. Humboldt (1807) stated (translated from the German edition): „The geography of plants makes it probable that South America was separated from Africa and that both continents were joined with ist eastern and western shores“. Many facts especially concerning the relation of the African and South American continent were already known in the last century, but there was no evidence for a common origin of the continents. It was the German metereologists Wegener, who collected all available information on this subject but also made first experiments. During his epeditions to Greenland he used the measurement of the duration of transmission of radio waves between Europe and Greenland in different years that Europe and North America were drifting away from each other 1 cm per year. As in the results of many other famous scientists, his continental drift theory published in 1915 was laughed at.

A remarkable and less known exception is a paper by Irmscher (1922), professor of botany in Hamburg. Only a few years after Wegener, he applied the move theory for the explanation of disjunctions of flowering plants.

Although land bridge theories were discussed and proposed already in the last century, they were interestingly not applied to bryology. Therefore Carl Müller and Franz Stephani went on and on to describe the same species from different parts of the world under different names. And although Wegeners book was published in 1915, Herzog in his Geography of Bryophytes (1926) just mentioned that Wegeners theory has still to be proved for bryophytes. In his chapter about disjunctions, he mentions simply that disjunctions are the result of spore dispersal or separation of a range. He writes that „A breakup of the former ranges is the explanation for the majority of disjunct bryophytes“, but did not apply the continental drift theory, neither the landbridge theory nor the move theory. And Herzog must be charged for this fault, since his textbook was the authority for the next 50 years and seriously delayed the progress in bryology.

**2.4 Influence of cytology**

Tuomikoski stated in 1958 in his paper concerning the present state of moss systematics that the knowledge of chromosome numbers includes (at that time) only 1% of the moss species and that the previous results have offered valuable insights for the generic and familiar placement of species and the distinction of species. He supposed that „cytology will open new perspectives for bryophyte systematics in the future“.

However, as we know today, the idealistic idea that chromosome counts could be used as great help for taxonomy was in vain. Today we have chromosome counts of 830 taxa of liverworts and 1550 taxa of mosses (Fritsch 1991), which is about 15% of the species but 15 times more than 33 years ago at the time of Tuomikoskis statement. Although so far only part of the species has been studied, a large deal of bryophytes have the same chromosome numbers. Smith (1978) has reviewed the importance of chromosome counts for bryophyte systematics. In the mosses, the major systematic groups are reflected by basic numbers of n=6 and n=7 and derived numbers, but the phylogenetic scheme derived from the chromosome numbers is the same than that derived before by comparative morphology.

Tuomikoskis other hope, that culture experiments could solve problems of delimitation of taxa, remained not fulfilled. This method, as easy as effective, is apparently to simple to gain interest.

**2.5 The influence of phytochemistry**

Most publications on the phytochemistry of bryophytes deal with chemical constituents and have no systematic interpretation and thus only little value for taxonomy. As outlined by Mues (1985) „the discussion [in phytochemical papers] is more often limited to their chemical properties with little or no mention of their possible systematic significance. Contributions to the classification of bryophytes are rare. A very spectacular result of the phytochemistry was that *Takakia* must be a moss according to its flavonoid chemistry, which was published by Markham & Porter in 1979, long before the sporophyte was known. In some cases, phytochemical data can contribute arguments for the systematic positions of groups of higher categories, e.g. the flavonoid chemistry of the Sphaerocarpales, which indicated that this morphologically very strange group belongs to the Marchantiidae and not to the Jungermanniidae. Most evaluations of phytochemical data were made for liverworts. They supported classifications of higher categories (orders, families). e.g. by presence or absence of flavonoids supported existing classifications without altering them. On the species level, the phytochemistry has often supported but also refined the delimitation of taxa, because the phytochemical evolution e.g. of sesquiterpenes went hand in hand with the morphological evolution.

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**2.6 The influence of cladistics**

It has been argued that systematics has lost its importance during the first half of this century as compared with other biological disciplines, not because the problems are all solved but because the methods are unsufficient. The error in this statement is that disciplines like physiology or molecular biology cannot be compared with systematics. This arguments has lead to „more scientific approaches“ to systematics. One of these approaches was cladistics, which argued that science must have principles and a theory. Since the introduction of cladistics by Hennig (1966), many publications dealt with theory of biological systematics and the application of computerized phylogenetic systematics. But has this brought any changes? No. In spite of the attempt of the cladists to make systematics to an „Exact Science“, the systematic biology and also the ecology has more and more separated from the rest of the biology, which is expressed in the recent trend to establish institutes for evolutionary biology and ecology for the organismic biology.

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Another problem with cladistics is that it is commonly misunderstood. This starts with the fact that the English translation of Hennig´s (1966) book, which was published before the original German version (Hennig 1982), was not identical with the original. In the foreword to the German edition, the editor, Hennig´s son, remarks that parts of the German edition were not translated and that his father was not satisfied with the English text, which he did not see before publication. If you read Hennig (1982: 35) carefully, you will find, that Hennig entitled phylogenetic systematics as a task, which final solution will at least be in unreachable distance, and that phylogenetic systematics must be postulated, however will never be final, and that he entitles provisional systems as phylogenetic systems as provisional.

In contrast to Hennig, modern cladists claim that they are reconstructing phylogeny, but it must be kept in mind that this method is hypothetical. It was obvious in the older „hand-made“ Hennigian cladistic analyses that the whole was based on circularity: the assumption which characters are apomorphic and which are plesiomorphic. Nevertheless the „trees“ gave a nice synoptical graphical translation of the authors view. Some of the modern computerized cladistic analyses do not use anymore this a priori assumption.

Interestingly, phenetic and cladistic analyses based on the same morphological data (Frahm 1991) show the same results (same dendro- viz. cladogram), which is not surprising, since similarity is in most cases an expression of the same descent. This counts especially for bryophytes, but not for some other groups of organisms e.g. insects. It has to be kept in mind that Hennig, the father of cladistics, was entomologist and if he would have tried to classify insects, which have different stages in their development, these stages such as butterflies and caterpillars would have been separated. Metamorphism, allomorphism or polymorphism play, however, no role for bryophytes. For example, if one performs a cluster (=phenetic) analysis with the data matrix of the peristome types of Fissidens (Bruggeman-Nannenga 1988), which was performed with the cladistic program TREESEARCH, the resulting cladogram viz. dendrogram is almost absolutely identical! Apparently such test was not made before, since „cladist“ and „pheneticists“ belong to other schools and would not use methods of the other school.

Authors have usually used only one cladistic program for a study and relied on the results of this program. Frahm et al. (1996) in a revision of *Conostomum* used the same data matrix with three different cladistic programs (PAUP, HENNIG86 and TREESEARCH). TREESEARCH produced 12 trees. The last one was interesingly almost identical with the results of a cluster analysis obtained from the same data matrix. HENNIG86 calculated 15 dendrograms, which were totally different from those obtained from TREESEARCH. PAUP calculated also 15 dendrograms, of which only 3 were identical with those from HENNIG86. The strict consensus tree proposed by PAUP was one which seemed to be the most unlikely one.

The question is, has 20 years of cladistic studies influenced bryophyte systematics? The answer must be: generally not. Interestingly, cladistic analyses of higher categories have usually confirmed previous classifications. The cladistic analysis of the Hookeriales (Hedenäs 1996) dealing with 80 species and 75 character states took 120 hours (that means 5 days !) on a Pentium 100 Mhz processor without reaching an end of the analysis. The resulting strict consensus tree showed basically the arragement of genera into Hookeriaceae and Callicostaceae, a classification obtained before by human brain and without a Pentium processor.

**2.7 The influence of molecular systematics**

Molecular systematics is now said to be the ultimate test for biological classification. But makes it really a difference whether I have the statement „flowers white“ or a gene code for it, and whether I put the character „flower white“ in a data matrix for a phenetic analysis or the gene code in the computer for a cladistic analysis ?

The former studies on the molecular systematics using DNA sequences included mainly higher categories due to the low number of sequenced species. One of the remarkable results were that the marchantialen liverworts turned out to be separated from the rest of the liverwort (Capesius & Bopp 1997). The authors compare their molecular data with morphological data and argue that also the morphology of the Marchantiales and Jungermanniales are so different that they need recognition as different classes. Thus it seems as if the separation of Marchantiatae was overdue like the separate classification of the Anthocerotae before. I assume that a detailed phenetic analysis would have shown the same results, because of the high number of unique morphological characters in the Marchantiales, and if you compare a transverse section of the thallus of *Pellia* and of *Marchantia,* one can hardly find any homologies.

Current studies apply DNA sequencing to test the relationships of genera within a family. Interestingly the molecular systematic is presently mainly proving the results of former classifications. Preliminary and still unpublished results of DNA sequences from Dicranaceae by M. Stech show that the cladograms reflect the classification in subfamilies, which is used since almost hundred years.

ISSN 0945-3466

Recently Spagnuolo et al. (1997) measured the length of internal transcribed spacers (ITS1) and found that „the length matches to a great extent recent evidence on the circumscription of the genera based on morphology“.

The molecular (or cladistic) systematics shall be a phylogenetic systematic, as the representatives of these fields argue. But if the results of these fields are more or less identical with that of the idealistic morphological systematic, must not the classical systematics also be phylogenetic or is it just by chance that these results agree? In a phenetic analysis of the Campylopodioideae (Frahm 1991) it turned out that the genera *Microcampylopus* and *Campylopodium* are separate from the rest of the subfamiliy and has to be placed with *Dicranella* in the Dicranelloideae. The molecular data now simply confirm this.

## 3.The theory of classification

It could be shown, that all new disciplines in biology such as the evolution theory, plate tectonics, cytology, phytochemistry etc. had no serious influence on the bryophyte systematics. Therefore the question arises, how the classification of bryophytes could survive almost unchanged for such a long time.

Classification already starts at the species level as the base of a system. Surprisingly, there was generally a relatively constant circumscription of species.

It is a trivial observation, but most of the species accepted today were already in use in the last century. Differences are met in certain "difficult" genera such as *Bryum*, which shows a strong genetic variability and *Sphagnum*, which underlies strong modifications by the habitat.

If we look at the species numbers recognized within the genus *Bryum* in Europe, we see that the authors of the Bryologia Europaea distinguished 59 species before 1850, however, in 1927, Mönkemeyer treated 192 species and named 28 more species, which he did not number. Podpera in 1954 recognized again only 75, Corley et al (1982) 68 and Frey et al. (1995) 71. Surprisingly the species number remained almost constant over a period of 150 years. This means also that 150 years ago, almost all species from Europe were described in this genus and the species distinguished today are basically the same as in the last century.

- But why is there so much consistency in the species concept?
- Why is there so much correspondence amongst different bryologists over the time?
- How can so many different personalities come to the same conclusions, if so many different classifications are possible?
- Why are all recognitions made in the past are also usable for the future?

It is conspicuous that many bryologists and just those who had enormous influence on the classification were no botanists but amateurs. Hedwig and Gottsche were practicing physicians, Brotherus and Limpricht were high school teacher, Fleischer was painter, C. Müller was pharmacist. They made their classifications not by applying theories but from the experience which they gained from the objects. And even today the human mind can still compete with computers in terms of classification.

Classification is grouping of objects by similarities. It is a typical intellectual capacity of man. The mankind is confronted with an enormous diversity of phenomena. All these phenomena cannot be named; they require abstraction. Abstraction is a fundamentum of human live.

Usually the abstraction is as detailed as required for the daily live. Indios from the Andes may have 50 different names for different sorts of potatoes but perhaps only one for mosses and lichens together. Classification is nothing else than abstraction.

Biological systematics is born from experience and intuition. It is therefore much different from other disciplines of natural sciences, which are based on ideas or hypotheses, which must be proved. Systematics is empiric and can hardly be proved.

Thus our perception leads us automatically to structured classes of similarities. Similarities in biosystematics, however, are usually homologies, and therefore the biological classification comes very close to a reflection of evolution. And since similarities are often based on homologies, a phenetic analysis by percentage similarity can also be interpreted in phylogenetic respects.

Surprisingly taxonomic units are generally (with comparably few exceptions) accepted by all bryologists. Why are all the different bryologists (even over the time) recognizing most taxa in the same way? It must be that all of them have the same way to compare and to group. And this seems to be inborn to the humans, perhaps since we are also part of the evolution, which produced the taxa which we are classifying. If we group thousands of species in a satisfying system which causes no contradiction, the method of classification must be correct and the order must be real.

Theoretically, several billions of different classifications of bryophytes are possible and could be proposed by a computer. The question is, why never serious alternatives were offered. Apparently, it is the difference between a computer and the human mind, which eliminates these unrealistic alternatives.

The change of systematic views has thus been small and (perhaps with the exception of the classification into cleistocarpous and stegocarpous mosses) never fundamental. It consisted of a sum of smaller corrections, which changed slowly from earlier to present opinions. By this drift of small systematic changes, the probability of the classification raised. Therefore it can be expected in the future, that the classification of organisms will never be fixed but will be continously altered in small steps, the steps probably getting smaller and smaller and the probability higher. Changes include fluctuations in the species concept, which goes up and down in almost regular intervalls, but has no real effect on classification since this is only a matter of taxonomic levels.

Almost all of the families created by Schimper are still used, almost all genera, almost all species. This is a surprising common sense amongst the bryologist from the last century and nowadays.

ISSN 0945-3466

How can that be? It sounds, that there is an order in the nature, which is seen by all taxonomist in the same way. This is surprising, since systematic studies are synthetic. Systematics combine the various results of morphology, anatomy, geographical distribution and ecological observations. This makes the difference between systematics (and also field ecology) with true sciences. The old bryologists used their "feeling", and although the modern bryologists nowadays are using to set up large tables of coded characters, the computerized calculation of similarity has not yet changed something substantially. Computer analyses can hardly result in something different, because the numerous characters which the taxonomist have integretated in a taxonomical unit, are just encoded. It comes out what you have put in, and this is not much enough, because one cannot encode everything.

Systematics is more than coding characters and putting it into a matrix. All questions involved with computerized systematics such as weighting of characters or ignoring characters is done automatically by the scientist. In addition, many other aspects as ecology and geography, matters, which can hardly be encoded, are considered at the same time.

The question is, is traditional classification true? My opnion is: yes. The answer lies in the evolution of man. The evolution of our sense organs was adapted over millions of years to our environment. This is the reason why we see the world as it is. Thus we as scientists have not the problems as the philosophers have. Part of them argue that the world exists only in our imagination. The natural selection, a fundamental in the evolution theory, would have prohibited this. For example, the monkey, who jumped on a branch, which only existed in his imagination, fall down the tree - and did not participate anymore in evolution. This gives us the safety that we see the nature as it is, and not that nature exists only in our imagination, as some philosphers say. And this recognition of nature, which is the same in all scientists, is the base of taxonomy and systematics. And this is the reason that all systematists come more or less to the same conclusions. And this is the reason that our classification is a natural one.

## 4. Conclusion

In past reviews of the classification of bryophytes, especially concerning „modern“ methods (Koponen 1978, Szweykowski 1978, Tuomikoski 1958), the authors have always outlined that the present results of modern methods are not yet useful for higher categories but at specific level, but „promise to give a major impetus to the classification, especially at higher levels“ and „modern taxonomic methods may yield a more natural classification“. (Koponen 1978). However, is our present classification not natural or what kind of changes in the classification do we expect? I am expecting slight additions in our knowledge over the time and slight improvements of our classification but never a fundamental change.

Other bryologists were even more pessimistic. Anderson (1974) and Greene (1976) stated that bryophyte taxonomy is years behind the taxonomy of other groups of plants. But is this really true or is simply less controversy amongst bryologists concerning bryophyte taxonomy? Greene (l.c.), who has never worked taxonomically himself, even proposed the omega taxonomy instead of the alpha taxonomy. But as we could see, did not the „modern“ disciplines of taxonomy generally support older views of classification (with a few exceptions)? It is some kind of bryopessimism if we argue that „our knowledge is still too slight to make effective applications“ and that „relatively few genera have been revised even by traditional methods“ (Anderson 1974). Is the percentage of revised genera in other groups of plants higher? I doubt. And propagating this pessimism leads to the effect that other botanists regard bryology as behind.

As we could see, the results of the different disciplines of taxonomy correlate. Is this not an indication that we have a natural order in bryophyte systematics?

In 1958, at the occasion of a symposion held at the university of Uppsala in commemoration of the 250th anniversary of the birthday of Carl von Linné, Tuomikoski (1958) explained the present state of moss systematics. He figured out that there were no special developments, and there was no kind of "modern systematics" in bryology. The systematic improvements in bryology in the time 1938-58 concerned microsystematics. He called bryology as relatively conservative with respect to modern systematics. He argued that bryology has not that reputation that it attracts money and implies somehow that lacking progresses in bryophyte systematics are a matter of funds.

But is this true?

Systematics of mosses during the past 150 years was a cumulative process. It accumulated over generations , was stepwise completed. It was not a matter of a change between wrong and right insights, a change from false to other false or even from right to false insights. The increase of taxonomic knowledge was, however, steady. This is a consequence of the principles of classifications. Biological classification can not be compared with other fields of natural sciences, where we had dramatical changes in the points of view. The biological classification has never undergone dramatical changes, had had no revolutions in the way of thinking, no re-interpretations.

## 5. Outlook

What will now bring the future? If bryology will not be put aside by socalled modern biological disciplines, and if bryology survives, we have to expect:

- realistic estimations of the present number of species of bryophytes in the world. It has to be remembered that the phytogeographical evaluations based on modern revisions and monographs give the opportunity for a worldwide view of distribution patterns, which can perhaps be integrated by help of computers. It can give the world patterns of bryophyte species and throw light upon the history of bryophytes.

- hopefully we will leave the level of microsystematics, that means monographs and revisions of single genera, and will advance to mesosystematics, that means to revise classifications not only on a genus but on levels of a family or even order.

ISSN 0945-3466

However, science is unpredictable. We do not know what the future will bring, especially concerning technical innovations. So we know virtually nothing about the limits of systematics and how will bryophyte systematics look like in 100 years.

The systematics are like a puzzle: but of how many parts consists the whole picture and do we have a all parts? How complete is our picture of bryophyte systematics now?. How much is known, how much unknown?

I like to thank Jens Mutke for preparing figs 1 and 3 and Dr. U. Drehwald for converting ING data into a database.

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Online Jan. 16, 2012

ISSN 0945-3466