Ehrenbergiana: problems of elusive types and old collections, with especial reference to diatoms

DAVID G. MANN

Royal Botanic Garden, Edinburgh EH3 5LR, Scotland, UK

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Abstract

Contrary to popular opinion and the recent appetite among some nomen- claturists for a unified BioCode, the main impediments to systematics research are not inconsistencies between the five current codes of nomenclature. Instead, one of the principal difficulties is the inordinate time spent searching for holotypes or lectotypes of taxa published before 1958, and the apparent pointlessness of this search, especially given the recent trend for 'current usage' to be preferred over authors' original intentions. Measures to increase the efficiency of the type method in practice could include the registration of types of new taxa and their deposition in designated repositories, and a time limit for registration of the types of older names, after which existing typifications would be non-mandatory. If 'current usage' can be defined satisfactorily and is to be preferred in the interests of stability, it seems logical to alter the laws of nomenclature to remove the automatic priority given to holotypes or lectotypes. To lessen the chance that current usage will drift away from correct usage in future, information about types must be made more readily available to users of taxonomy, e.g. as images disseminated via hard-copy and the Internet. Diatom types should only ever be single specimens.

INTRODUCTION

This volume celebrates the work of C.G. Ehrenberg and the welcome prospect that his remarkable collections, unique not only for their scientific value but also in the manner of their preparation and storage, may soon be restored to a state where they can be studied by taxonomists and also, perhaps, where they can be used for other kinds of research. I have little to add to the evaluations of Ehrenberg's work made here by others, since most of my essay is not directly about Ehrenberg, but about the problems old collections (including Ehrenberg's) pose for taxonomists, particularly those who study diatoms. However, first I should like to make some observations about Ehrenberg in relation to the study of diatom protoplasts, following an interesting recent paper by Jahn (1995).

Jahn describes how Ehrenberg came to the conclusion that diatoms are animals, with complex internal organs, and she documents the obstinacy with which he held to this view as scientific advances (from around 1840 onwards) showed, ever more clearly, the similarities between diatoms and other algae (e.g. Kützing, 1844; Smith, 1853–6; Pritchard, 1861) and began to reveal the true nature of the eukaryote cell. Ehrenberg seems to have been unable to adjust to new ideas and, even during his life, his interpretations of diatom cells must have come to appear not merely wrong but foolish, though he was not alone in his views (e.g. Meneghini, 1853, a translation of a paper published in 1845). But in one way diatom research took a step backwards when Ehrenberg's views were discredited and the diatoms (and many other microscopic algae included by Ehrenberg in the 'Polygastrica') were established to be autotrophic organisms, since there was then far less reason to study the organisms themselves, rather than their shells.

For as long as diatoms were thought to be animals, and while their chloroplasts, nuclei, vacuoles and storage products were still interpreted as digestive and reproductive organs, the structure of the diatom protoplast *had* to be studied, since only then could diatoms be compared with all the other organisms that Ehrenberg and his contemporaries included within the Infusoria – rotifers, ciliates, flagellates, amoebae, desmids, etc. Once diatoms were accepted to be algae, however, and shown to have the same basic cell structure as any other plant (save the blue-green algae), the main problem of classification appeared to have been solved. The main tasks for taxonomists seemed now to be to catalogue diatom diversity and to provide means of identification. In this, the fascinating complexity of the frustule and the convenience of using preserved, mounted material quickly led most people to use cleaned valves and frustules as almost the sole basis for taxonomy, and to ignore the cell itself. Thus, between 1850 and 1950, students of the protoplast were relatively few,

notable exceptions being Pfitzer (1871), Lauterborn (1896), Karsten (e.g. 1899), Mereschkowsky (1901, 1902–3, 1903, 1904a, b, 1906) and Geitler (e.g. 1937a, b) (see also Mann, 1996). Unfortunately, by ignoring the plastids, nucleus and other cytological features, phycologists denied themselves a rich source of systematic data and the opportunity to study many interesting processes, including mitosis and cytokinesis, plastid movements and division, and morphogenesis.

The change in approach can be illustrated by comparing Ehrenberg's beautiful plates of living (sometimes recently dead) diatoms in the *Infusionsthierchen* (1838) and Kützing's exact but dull drawings six years later (1844). In Kützing's monograph the cell contents are represented perfunctorily, if at all, in the plates and they are neither described nor labelled; in the text the main focus is on frustule morphology and colony formation. Ehrenberg's illustrations, on the other hand, show the protoplast in considerable detail and its features are described and carefully (though wrongly) interpreted in the lengthy species descriptions and figure captions.

The same trend is documented in Pritchard (1861), which contains plates from different editions of a *History of Infusoria*. The later plates, by Tuffen West, are more useful taxonomically and more accurate than the earlier ones (as they ought to be, with improvements in microscope design), but they lack the biological interest and cytoplasmic detail of the plates produced in 1841 and 1852 by Pritchard himself, who had clearly been inspired by Ehrenberg. In W. Smith's *Synopsis of British Diatomaceae* (1853–6), cellular detail is depicted in seven colour plates and discussed briefly in the Introduction, but it forms no part of the descriptions of species and genera. Curiously, another feature only seen in living material – colony formation – remained important in Smith's classification, as it did in Kützing's.

THE NEED FOR HISTORIC COLLECTIONS

The main purpose of this essay is to consider some general issues surrounding the value, use and curation of historic collections and the typification of diatoms. Most herbaria take especial pride in their older material and type specimens, particularly if these were collected or studied by eminent scientists like Ehrenberg. I suspect I am not the only one, however, who has wished rather more frequently than conscience should allow, that bomb, fire or flood had destroyed more herbaria and museums – or at least the types they contain – providing, of course, that this could have been done without hurting their curators! I admit that I have certainly had such wicked thoughts about the Ehrenberg Collection.

After many years in which the Ehrenberg Collection has been preserved but unused, plans are now in hand to restore it to 'full working order'. There should soon be no need for anyone to be other than profoundly thankful that the Collection did not fall victim to enemy action in the last world war and that it has in fact survived remarkably well since Ehrenberg's death. But let us consider what would have happened if there had been no new plans to make it more accessible: what if there had been no perestroika and the Ehrenberg Collection had remained in East Berlin, effectively hidden from most of those who might have wanted or needed to study it, even within the eastern bloc itself? What if the scientific academies of the German Democratic Republic or their successors in the reunited Germany had made no special provision for this large and historically important collection of material? These are not idle speculations. There are many collections around the world that languish in old buildings, or in cupboards that are never opened. In western Europe, many universities no longer support any research in systematics and have no use for old collections of preserved animals and plants. What should be done with them?

In many cases, it is irresponsible to discard collections, since parts or all of a collection may be irreplaceable and even a modest collection represents a very considerable investment. I have suggested elsewhere (Mann, 1997) that herbarium specimens of angiosperms may cost around £25 on average to acquire and incorporate into a European collection. This is an acquisition cost, not a valuation, but it does give some idea of the investment that has been made in developing collections and the cost of replacing them, if indeed this were to be possible. Slides of diatoms are probably even more expensive, as a result of the work necessary to clean specimens using concentrated acids or other oxidizing agents, and the subsequent washing and mounting in high refractive index media.

Clearly, then, one should think carefully before throwing away a collection that has cost many thousands or millions of pounds to build up and may be even more expensive to replace. But there are many other reasons too why old collections should be kept. They are a resource for new research and they document and validate the work of previous generations of scientists and collectors. Battarbee (1979) was able to check and re-interpret early twentieth century studies of the phytoplankton of Lough Neagh, N. Ireland (Dakin & Latarche, 1913), by studying two of the original samples that had been preserved at the Natural History Museum in London (see also Flower, 1986). Specimens often record the date and place of collection, so that it is possible to work out changes in distribution through time, as a result of pollution, habitat destruction, the spread of alien species and so on. Sometimes, specimens that were collected

quite accidentally can be as useful or more useful than specimens collected deliberately. Thus, for instance, Van Dam & Mertens (1993) were able to use diatoms attached to herbarium specimens of aquatic macrophytes to determine the long-term effects of eutrophication in a nature reserve in the Netherlands. Their data gave useful insights on the likely outcome of a management strategy proposed for the reserve, involving reduction in phosphorus loading. The labels on herbarium specimens of angiosperms may record ethnobotanical information (e.g. Chaudhuri, Banerjee & Guha, 1977), which may be useful in the search for new plant products and pharmaceuticals.

Sometimes old collections contain species that are now extinct, such as the angiosperm *Trochetiopsis melanoxylon* (R. Brown ex Aiton f.) W. Marais, formerly endemic to St. Helena but extinct since *ca* 1780 and now represented by just five herbarium sheets (Q.C.B. Cronk, personal communication to Mann, 1997). In this and many less dramatic examples, museum and herbarium specimens are irreplaceable in an obvious and non-trivial sense.

However, for various reasons, parts of a valuable collection may not be actively studied for years or decades. We could suggest that in these circumstances, there may be no need for any curation to be done, beyond a minimum of care and maintenance. The collections may not be being used now, they may not even be usable, but one day they may be. I have heard of a university Head of Department who stopped short of suggesting that the collections in his charge should be thrown away, but did threaten to board them up. By accident or design, many old collections today are, in effect, boarded up, and perhaps we should simply be glad that they survive at all.

But although this laissez-faire attitude may be acceptable for some kinds of collection – specimens formerly used in research or teaching, for example, or the voucher specimens of ecologists and palaeoecologists – it is not satisfactory for collections that contain type specimens, such as the Ehrenberg collection. Type specimens are biological standards, defining how the names of taxa are to be applied, and it must be possible to refer to them when there is any controversy about which name is correct. Laissez-faire, as a policy for dealing with moribund collections containing types, is not merely unsatisfactory, it is intolerable. We must take steps, by whatever means are available, to make sure that information about type specimens is easily available, and that the types themselves are usable for their prime purpose: the standardization of nomenclature.

THE NEED FOR TYPES

The type method is now accepted as a fundamental tenet of botanical nomenclature. The current *International Code of Botanical Nomenclature*

[ICBN] (Greuter *et al.*, 1994) requires that the use of names is to be determined by reference to types, which are usually preserved specimens, unless it is impossible to preserve anything useful. Hawksworth & Kirk (1995) have called types the "keystones of unequivocal communication in biology". It is salutary to remember, however, that typification has been mandatory for only 40 years. Indeed, during the first 150 years after Linnaeus' invention of biological nomenclature, the type method scarcely existed at all. The type method was not part of the first laws of nomenclature (de Candolle, 1868). It was formally introduced in the *American Code of Botanical Nomenclature* (Arthur *et al.*, 1904, 1907) and has gradually won acceptance since (Perry, 1991), becoming fully established only on 1 January 1958 (Greuter *et al.*, 1994: Article 37). The progress of the method can be seen by examining issues of a suitable taxonomic journal, such as the *Notes from the Royal Botanic Garden, Edinburgh*. Between 1910 and 1920, a few authors designated types (e.g. Simpson, 1915) but most did not (e.g. Balfour, 1919). By the 1930s, types were usually specified (e.g. Cowan, 1932), but Tagg (1931) did not indicate types for several new species of *Rhododendron*. It should come as no surprise, therefore, that there are many problems of typification arising from before 1958, and it is no disgrace to taxonomists that a backlog of typification still exists.

However, in the last ten years there has been a change to the ICBN which makes me wonder what role types now play or will play in future. Article 57 of the current ICBN (Greuter *et al.*, 1994) says "a name that has been widely and persistently used for a taxon or taxa not including its type is not to be used in a sense that conflicts with current usage unless and until ... [proposals for conservation or rejection have been] submitted or rejected" – a presumption in favour of re-typification. Article 24.1 of the Draft BioCode (Hawksworth, 1996) also says that when there is a conflict between the current usage of a name and 'correct' usage, as determined by the type, current usage is to be preferred. This surely means that, if the identity of the type is known and the use of the name that it typifies is uncontroversial and correct, the type is unnecessary (since standardization of nomenclature is essentially complete); if the identity of the type is not known, the type is a fiction; and if the identity of the type becomes known and would require a change to current usage, the type is to be ignored and replaced. Perhaps the authors of this change to the Code considered that there would be only a few cases where 'current usage' conflicts with correct usage, but this is not true in diatoms: there are many. For diatoms at least, the type method seems to have been seriously undermined by Article 57 and the type itself no longer seems to have much importance. True, when the type of one taxon is found to belong to another at the same rank with an earlier name, the type method still holds sway, and the later name will have to be replaced or conserved with a new type, but in diatoms this is

probably not common, since there are many fewer names available than there are taxa needing names. When taxa are divided, the type is used to determine which of the daughter taxa bears the original name. However, if a taxon is found to be heterogeneous, assigning names to the segregate taxa might as well be arbitrary, since the information gathered about the composite parent taxon cannot usually be apportioned between the taxa segregated from it. Presumably too, if current usage were again to drift after a taxon has been conserved and neotypified to make 'correct usage' conform to 'current usage', Article 57 would require typification to be adjusted yet again. If so, types are a strange kind of standard.

The principle of using types as a basis for nomenclature is excellent, but in practice it has been weakened by changes in the International Code, especially the extension of conservation to the species level. It would be weakened still further by the adoption of lists of names in current use (see Hawksworth, 1996). One wonders why time and money should be spent in searching for holotypes, isotypes or syntypes if, in the end, all that matters is current usage. A great deal of effort could be saved by designating neotypes. Indeed, there seems to be a philosophical inconsistency in the present version of the ICBN. Article 57 and other measures that have been proposed seek to stabilize nomenclature in current use. On the other hand, according to Article 9.13, a holotype or lectotype is always to take precedence over a neotype. Yet, if there is a neotype, it is more likely to correspond to 'current usage' than any holotype or lectotype that may subsequently be discovered. Article 9.13 thus creates the kinds of problem that Article 57 then attempts to circumvent, via the slow procedures of conservation and rejection.

All these difficulties arise because of a fundamental weakness in what we could call the 'type system', i.e. the application and operation of the type method day-to-day. The type system is weak because typification and current usage are almost decoupled. When I use a ruler to estimate length, I am using a near facsimile of a standard rule, itself calibrated directly or indirectly by reference to the definition of a metre as the distance travelled *in vacuo* by light in 1/299 792 458 of a second. We can calibrate our clocks every day by reference to standard time, broadcast via radio or television. But those who use biological units – genera, species, varieties, etc – have little or no opportunity to calibrate their identifications and usage. For ecologists, biochemists and other users of taxonomy, the types are out of sight, masked by layers of interpretation and exposition in monographs, floras and field guides. Their material nature is described but not experienced; in many cases it is not even illustrated. To some extent the standardization of biological nomenclature is inevitably weaker than the standardization of units of time or physical

measurement, since each taxon is not a single, unvarying quantity, like a metre or a kilogram, but a group concept. The type offers no help in standardizing the concept of a particular species or other taxon: its only, but vital, function is to standardize nomenclature. However, although names will change their meaning from time to time, as the groups they refer to are made more or less inclusive, there would certainly be less doubt and error, and far less conflict between current and correct usage, if types could be made as easily available as rulers and standard time.

Types have to be specimens unless preservation is impossible and there are good reasons for this. Descriptions are derivative and subjective, and contain only as much information as was present in them at the instant of their creation. Illustrations also allow only limited new interpretation and research. Specimens, on the other hand, usually yield more and more information with further study. Thus, if a new character is found, which helps separate a taxon into two or more daughter taxa, this can often be studied in a type specimen, whereas no information about that character may be evident in the protologue or illustration. The penalty of using specimens as types is that the types themselves cannot be distributed (except in limited numbers, as isotypes). Images and text, on the other hand, can be duplicated indefinitely, either electronically, or as photographs, or in printed material. Ironically, therefore, now that light and electron microscopy can provide a detailed photographic inventory of the morphology of even the smallest cell, the nomenclature of unpreservable, wall-less algae and protists may in future prove more stable than that of vascular plants, bryophytes and macroalgae, preserved in their millions in herbaria.

Thus, one of the most important contributions that could be made to nomenclatural stability, perhaps more important than any other, would be for herbaria and museums to produce illustrated catalogues of their types, as recommended also by Williams (1993). This has been done for some diatom collections, e.g. by Williams (1988) and Simonsen (1987, 1992). In future I hope that such catalogues will be made freely available via the Internet, as well as being produced as hard-copy. In diatoms, a photograph (or a series of photographs, taken in different focal planes) can show virtually all of the features necessary for identification. In this special case, we should ask ourselves which should be considered the real nomenclatural standard: the image and any accompanying textual material in type catalogues (which will probably be the principal influence on future usage), or the specimens from which the images were derived? For diatoms, I would prefer the image. If a type photograph did not show some feature subsequently found to be important for diagnosis, further photographs could be added as epitypes (Greuter *et al.*,

1994: Article 9.7), or other defining data, such as nucleotide sequences (as yet, the ICBN does not allow this).

THE TYPE METHOD IN PRACTICE

Although the type method seems to have been seriously undermined by Article 57 of the current ICBN, let us assume for the remainder of this essay that the type method will continue to play an important part in botanical nomenclature.

I wrote to someone once that Ehrenberg's collections are the greatest single obstacle to progress in diatom taxonomy. This was an exaggeration, but it contained more than a grain of truth. The species and genera that he described have been, and are still, a source of confusion, dispute and nomenclatural instability. Ehrenberg lived for a long time in an age of exploration and discovery, and almost inevitably he described many new species and genera. Unfortunately, in most cases we do not know what his names mean. We use them, sometimes consistently, sometimes inconsistently, but we often have no idea whether our use of his names corresponds to his.

I will take the genus *Diploneis* as an example. Ehrenberg described several species that are now referred to the genus *Diploneis* and the genus itself is also his. Among the species Ehrenberg described are the marine diatoms *D. didyma* (Ehrenb.) Cleve, *D. crabro* (Ehrenb.) Ehrenb. ex Cleve and *D. bombus* (Ehrenb.) Ehrenb. ex Cleve. These names are commonly used today and there has been a fair degree of consensus in the last 60 years about what they refer to. The consensus began to develop from 1875 or so onwards, and hence after Ehrenberg's death; but although these three diatoms, like many *Diploneis* species, are coarsely structured and quite large relative to many other pennate diatoms (so that it is less likely that Ehrenberg and other early authors could misinterpret each other's illustrations and text), today's consensus may bear no relation to Ehrenberg's original intentions. Some other Ehrenberg names, such as *Diploneis entomon* (Ehrenb.) Cleve, which also refer to large, coarsely structured diatoms, can be found only in old Floras, nomenclatural catalogues and indices. For some reason they, unlike *bombus, crabro* and *didyma*, have been neglected and are now uninterpretable, a source of instability for the future. As noted above, it is now possible to conserve a name of a species in such a way as to exclude its original type and thus allow 'current usage' to be protected and continued. Thus, whatever Ehrenberg meant when he used the names *didyma*, *crabro* and *bombus*, could be made irrelevant by conservation. However, it now appears that the species concept in diatoms is generally too broad (Mann, 1989; Droop, 1994; Mann & Droop, 1996), so that the consensus concepts of *didyma*, *bombus* and *crabro* are no longer tenable and therefore

must not be protected. These species are heterogeneous and need to be divided into several, more narrowly circumscribed entities, each with its own distinctive morphology, ecology and distribution. Which, if any of them, is Ehrenberg's *crabro*, which is *bombus*, which *didyma*, which *entomon*? Only type material can give the answers.

Another species described by Ehrenberg, *Navicula lyra*, has become the type species of a new genus, *Lyrella*, which was described by Karayeva (1978). *Lyrella* species share a number of characteristics of frustule structure, chloroplast arrangement and chloroplast division patterns, and sexual reproduction that link them together and separate them from *Navicula* Bory species, and also from superficially similar diatoms now classified in *Fallacia* Stickle & D.G. Mann. In all *Lyrella* species the striae are interrupted by a lyre-shaped area and it is the shape and ornamentation of this area, together with valve shape and size and striation density, that form the basis for species differentiation. But in fact, we do not know what Ehrenberg was looking at when he described *Navicula lyra* from material collected in the Falkland Islands in the 1830s: we know only what other people have thought he meant. Type material has not been examined, because it has been inaccessible, as a result of the history of the Ehrenberg collection and the complex politics of twentieth century Europe. We do not even know whether Ehrenberg's species would fit within *Lyrella* as this genus is now circumscribed, though we can be certain it does not belong to *Navicula* (Cox, 1979; Round, *et al.*, 1990).

None of these nomenclatural problems can be solved until Ehrenberg's collections can be consulted. Similar problems exist in relation to many other collections and names. They are the kinds of problems every taxonomist faces and their solution is an essential step that cannot be circumvented. In a taxonomic research programme, we do the 'science' first, examining many specimens and many taxonomic characters. Then a preliminary classification is formed, which if possible is tested against other evidence and further specimens. Once we are satisfied with our classification, we have to find the correct name for each taxon. From available floras, monographs and papers, and any catalogues, indexes and databases that have been made, we try to find all the names that could conceivably be relevant. Then we examine type material to see how these names should be applied. Finally, we use the rules of nomenclature to determine what names are in fact legitimate for the taxa we wish to retain. This last, purely nomenclatural phase is the shortest of all, unless it leads to an application for conservation or rejection, and it could be made even shorter by the development of an interactive computer program that embodies the pseudolegal framework of the relevant code of nomenclature. Even if conservation or rejection procedures are initiated, the nomenclatural

phase is slow rather than time-consuming: while a decision is awaited, one can get on with other things. And because it is a short phase, the application of the rules of nomenclature is inexpensive relative to the primary research (studying the variation pattern and producing classifications), and also relative to the middle phase – the search for relevant names and the examination of type material. It is here that the research programme often grinds to a halt. What is the type material? Did anyone ever designate a type? Does it still exist and if so, where is it? Will anyone lend it to you? Will they even reply to your letters and faxes? If they won't lend it, or say that it's up to you to come and find it, because it should be somewhere in the herbarium, can you afford to travel to see it? Can you get a visa? Will there be anyone to show you how to find the types when you arrive and will there be any facilities available to you to examine them?

I can illustrate the difficulties from our experience in Edinburgh. Recently, my colleague Stephen Droop has been trying to revise various groups within the genus *Diploneis*, during the preparation of accounts for a Flora of British Marine Diatoms. New collections of marine diatoms have been made, of which several hundred have been examined so far, using the light microscope, in order to establish the pattern of variation and occurrence of *Diploneis* around British coasts. Many new taxa will undoubtedly have to be described (Droop, 1994, 1995), but before this is done comparisons must be made with existing species and varieties (Droop, 1996). Stephen has therefore needed to study type material of various taxa, including some described by W. Gregory (1856), e.g. *Navicula splendida* Greg. It is well known among diatomists that Gregory's collection is in the Natural History Museum in London, and it is well known too that Gregory's new species were illustrated not by Gregory himself but by R.K. Greville, who received slides from Gregory, illustrated the specimens indicated by him, incorporated the slides into his own collection and, fortunately, labelled them meticulously. The Natural History Museum has indexes to these collections and curators who are willing and able to find slides and lend them. Here the system worked well and we now know what Gregory's name *Navicula splendida* refers to, from studying a slide in Greville's collection (Droop, 1996).

Stephen also wanted to look at type material of *D. bomboides*, described by Adolf Schmidt (1874). This proved more difficult, since we do not know whether Schmidt's collection survives. Stafleu & Cowan (1985) suggest that Schmidt's herbarium is in Halle, but the curator at the herbarium of the Martin-Luther-Universität in Halle told us that the information in Stafleu & Cowan is wrong. Where this information came from is unknown. Schmidt's collection may have been deposited in the Botanical Institute of the Martin-Luther-Universität after Schmidt's death in 1899, but when the collections of the

university were rearranged after the 1939–45 war, no trace of Schmidt's material was found. Stafleu & Cowan also say that there is some Schmidt material in Bremerhaven (BRM), one of the best curated diatom collections world-wide. Searches on our behalf among the Schmidt slides at Bremerhaven showed that there are indeed some that come from the right places (Hvidingsoe and Sölsvig) and they have the right taxa in them. The slides came from Dr Gründler, not Adolf Schmidt, but since Schmidt (1874) acknowledges Gründler's help in preparing samples, it looks as though the Bremerhaven slides contain isotype material. But of course, we can't be sure that we have found the right material. There will always be a nagging doubt that an attic somewhere harbours the real Schmidt collection and that any type we designate, using the Bremerhaven slides, may eventually have to be replaced or conserved. Perhaps Schmidt's original collection was removed from Germany as war reparations. In 1995, I was in Sevastopol and was shown the library of Professor Max Hartmann, one-time editor of *Archiv für Protistenkunde* and Director of the Kaiser-Wilhelm Institut für Biologie in Berlin – an eminent biologist and an exponent of theories of sexuality, who played an important role in phycology in central Europe between the two World Wars (e.g. Garbary & Wynne, 1996). The library was with Hartmann in Berlin until the end of the Second World War, when the two must have parted company: Hartmann himself ended up in West Germany, while his library went to the Ukraine. Were Schmidt's collections also taken, as spoils of war?

Stephen Droop also wanted to look at material of *Navicula splendida* var. *heemskerkiana*, described by Brockmann (1928). Initially he tried the Geological Survey of the Netherlands because we knew they had a small collection of Brockmann slides (de Wolf, 1993). They did not have specimens of var. *heemskerkiana* and referred Stephen to the Niedersächsisches Institut für historische Küstenforschung, Wilhelmshaven, which also has slides made by Brockmann. The curators took the trouble to check all their slides but found nothing corresponding to the sample numbers given by Brockmann (1928). They suggested, therefore, that we try another museum holding Brockmann material, the Senckenburg Institut, and that does indeed appear to be where the slides we wanted are usually kept. But they were out on loan.

For yet another species of *Diploneis*, it was clear where the type material ought to be, since the author's collection has survived where it was deposited, together with his notebooks. After some months waiting for a reply to our request for a loan, we sent a reminder; eventually, the curator replied that type material could not be found. We can either accept the information we have been given, that the original material is truly lost, in which case we could designate a neotype, or we must find the money and time to go and check for ourselves.

Such quests for original specimens take a great deal of time and are very expensive. The cost of searching for Schmidt's and Brockmann's collections must be well over £1000. This represents the time Stephen and I spent trying to identify where collections might be, writing to curators, and the time these curators spent examining their collections. It excludes the cost of the research Stephen did on *Diploneis* that led him to need to refer to type material and it excludes the cost of the research and manuscript preparation following those parts of the search that were successful. And this is for half a dozen names. The number of entries in VanLandingham's *Catalogue of the Fossil and Recent Genera and Species of Diatoms and their Synonyms* (1967–79) is well over 40,000, and a significant number of these entries refer to names invented by less well-known and less careful workers than Schmidt and Brockmann!

In a way, of course, it can be great fun to be a taxonomic detective, tracking down old collections and types, but I can't help feeling there must be better ways to organize the 'type system'. Admittedly, the next time we try to find Schmidt and Brockmann material, we will know better where to look, but this kind of benefit is small compensation for the amount of work, money and time spent searching for and through old collections. Furthermore, if a search reveals a type and this supports the current usage of the name, we have merely confirmed the *status quo* and any resulting publication is not going to attract much interest nor result in many citations! On the other hand, if the type does not agree with current usage, in a way our search will often have been in vain, since the likelihood is that the type will have to be changed (unless we ignore Article 57, by interpreting 'widely and persistently' in an extreme way).

Accurate nomenclature is essential and we do not need to be apologetic about spending money to achieve it. The case for taxonomy and systematics has been made frequently and well in the last few years, for example in Systematics Agenda 2000 documents (1994), and the long-term benefits of biodiversity research are certain, although there may be no immediate improvement in wealth creation or in the quality of life. Those, like Ehrenberg, who classified diatoms in the nineteenth century and early twentieth century, provided us with a basis for detecting and monitoring environmental change, including pollution and changes in climate (e.g. Flower & Battarbee, 1983; Fritz, Juggins, Battarbee & Engstrom, 1991; Laird, *et al.*, 1996). They were not far-sighted philanthropists and had little or no idea what use would or could be made of their research. Even the most inspired Technology Foresight (Chancellor of the Duchy of Lancaster, 1993) would have failed to predict and provide for present needs. So there is no need to be defensive about research into plant systematics. And to be able to do any research in systematics – or any other field of biology,

pure or applied – we need unambiguous nomenclature, so that we can accurately communicate information about the organisms we study and use.

But as in any activity, we should certainly try to achieve our goals efficiently, since, even within plant systematics, there are many other demands on limited financial and intellectual resources. In the Newly Independent States (NIS) of the former Soviet Union many scientists, some of them very able indeed, now receive reduced salaries or none at all, and there is very little money for equipment and consumables. A major taxonomic work on diatoms in the NIS, meant to help provide a foundation for ecological studies and the monitoring of water quality in the rivers and lakes of a badly polluted country, waits in a box where it has been for months. It is likely to remain there for months or years longer, because there is no money to publish it. In such situations the search for types is an unaffordable luxury. Hence we must always be searching for better ways to organize taxonomy.

I am not the first taxonomist, of course, and certainly not the most prominent, to suggest that we need to put our house in order. In 1990, Clifford, Rogers & Dettmann caused an outcry when they said that many herbarium specimens could and should be pulped, and Max Walters (1993), in a more moderate commentary, has suggested that it would be easy to reduce the content of European herbaria by up to 20% by judicious weeding. But actually, creating extra space in overcrowded herbaria is not a major problem. Storage in itself is fairly cheap and often it would probably be more expensive to do the judicious weeding that Walters suggests than to leave the weeds alone. The best way to improve the quality of collections is to concentrate on improving the quality of the intake. Do the weeding if there is time, by all means, but don't get too worried about the weeds – they're dead and won't proliferate! The point is that we must be careful to ensure that any new measures we take to streamline the 'type system', no matter how attractive they may be in theory, will lead to real improvements in practice, in the economy, efficiency and effectiveness of taxonomy.

MAKING TYPES AVAILABLE

The first problem we must address is how to make types more readily available. Over the last 250 years, taxonomists have carefully developed a system which, potentially, can standardize nomenclature through the use of types. What has not been done so effectively is to ensure that types can be found and used, efficiently and economically. It is as if the proponents of SI Units, having persuaded everyone to adopt the kilogram as the base unit of mass, then made it as difficult as possible for anyone to find out what a kilogram is, by hiding the international prototype of the kilogram. Types are essential

to biological nomenclature and yet the international community lays down no rules determining where type specimens can be kept, nor does it demand any minimum standards for their curation, nor any measures to promote their accessibility and use. The International Code does not say what should happen if a collection becomes moribund and is no longer curated. The fate of our supposedly precious biological standards, these 'keystones of unequivocal communication', can apparently be left to chance. All we get from the latest version of the Code (Greuter *et al.*, 1994) is Recommendation 7A:

> "It is strongly recommended that the material on which the name of a taxon is based, especially the holotype, be deposited in a public herbarium or other public collection with a policy of giving *bona fide* botanists open access to deposited material, and that it be scrupulously conserved."

These are worthy thoughts, but they do not really help, since 'public' is not defined and the Recommendation has no legal force. I could start a new diatom herbarium tomorrow to house my new types. I could make the types available to other scientists, even advertise on the Internet, and so satisfy the requirement for open access. And in a few years' time, if cuts in funding for systematics got worse, I might get even more disillusioned than I have been at times in the last 10 years and, in a fit of childish pique, destroy the lot – and not tell anyone. In 20 or 100 years' time, some diatomist might search for the vanished collection, fail to find it, but never know for sure whether it might still exist somewhere and awake to create havoc with his latest revision or flora.

Some people may think that, by and large, the situation is not too bad and that, with modern communications, there will be fewer problems in the future. This may be so. But let us learn from Ehrenberg's posthumous example. His ghost has haunted diatom taxonomists for over 100 years, through no fault of his own. Many diatomists know next to nothing about him, yet Ehrenberg was famous in his own day and his contributions to science were immense, as is clear from this volume. Ehrenberg's papers are now difficult to obtain, but they were published by the Imperial Prussian Academy of Sciences in Berlin, in what was scarcely a low-profile journal. His collections have been essentially inaccessible for many years and now need careful restoration and curation, yet they were left in good order to a highly esteemed university. Time passes and what once seemed impossible comes to be. I think it is very likely that many collections containing types – collections that today are well used and well curated – will sooner or later fall into decay and become as much of a problem as any from the eighteenth or nineteenth century that we now curse. There is, after all, very little sign of an overall improvement internationally in funding for herbarium curation and taxonomic research.

As a first step, certain herbaria could be designated as type repositories. After an agreed date, a type would have to be deposited in at least one of the type repositories in order for the taxon to be validly published, just as names will have to be registered after 1 January 2000 (subject to ratification by the XVIth International Botanical Congress, to be held in 1999: Greuter *et al.*, 1994, Article 32.1). Providing this condition were met, isotype material could also be deposited in any number of other herbaria.

If governments and funding agencies were involved in the choice of repositories, there might be some guarantee that the 'type system' will be financed more securely in the future. The list of repositories need not be short, but it would probably be sensible for the types of some groups of organisms to be concentrated in a few institutes, if special expertise is needed for their curation. The repositories would have to meet certain standards of curation and guarantee access, and they would have the duty of registering and advertising the types they hold. If a repository were unable to meet its responsibilities, there would have to be mechanisms that ensured that the type specimens could be removed and transferred elsewhere.

Tightening up the rules on the deposition of types of new taxa will not improve the efficiency of the 'type system' in relation to names that have already been published. For these, we could perhaps set a time limit for registering, cataloguing and illustrating existing holotypes, lectotypes and isotypes, after which any that remained unregistered would have no status under the Code, opening the way to neotypification.

Alternatively, a new 'appeals system' could be introduced, like the current procedures for rejection and conservation, through which types that are unavailable or inconvenient (because of their nature, condition or accessibility) could be set aside in favour of a neotype, which would then be placed in one of the type repositories. Whole collections could be declared nomenclaturally redundant. This may sound like a 'dictatorship of the proletariat', but the Code already proscribes certain names – some because they are nomenclaturally inconvenient, others because they were published in the wrong place or in the wrong way; Article 32.8 makes some names invalid because they appear in certain proscribed books and articles (Greuter *et al.*, 1994), though currently there are very few of these. So why not have rules restricting the siting and curation of types, if they are really the valuable biological standards we always say they are? The Ehrenberg Collection is now being restored and made available for study. However, I believe we should have been able to remove the nomenclatural status of the collection many years ago, so that use of Ehrenberg's own specimens as types was voluntary, not mandatory under the International Code.

Finally, if the philosophy exemplified in Article 57 of the current ICBN continues to hold sway, we might modify the guidance on neotypification. At present, a holotype or lectotype is always preferred to a neotype. Why? Of course, because the holotype or lectotype is the best guide to the author's original intentions. But, as already discussed, the implication of Article 57, and moves to establish lists of names in current use, is that we should place less emphasis on original intentions than on established practice. If no type has been designated, and often even if one has, our concept of how a name should be used will be derived from interpretations of the protologue and later published accounts. In such cases, what was in the author's mind is almost irrelevant. So one could argue, perhaps, that, rather than searching for original material, one should simply designate a neotype, preferably with many duplicates that can be deposited in several herbaria (including one or more type repositories), and protect it against the holotype or lectotype. The neotype would only be replaced if it could be shown, as in Article 9.13 of the present Code, that the neotype is in serious conflict with the protologue or current usage.

There is little doubt that to relax the rule on neotypification would make it easier to get away with poor scholarship. As soon as neotypes are allowed to take precedence over lectotypes, let alone holotypes, we to some extent sacrifice knowledge and integrity to convenience. There would also, inevitably, be discrimination against systematic research done in smaller or poorer countries and published in non-European languages or in journals with low circulations. The question is whether this is a worse tragedy than to use the time of our few remaining systematists to search for types that may no longer exist, in herbaria and museums that may welcome the prestige of holding types but do not accept the responsibilities that come with it.

THE PROBLEM OF COMPOSITE TYPES

Regardless of what action is taken to improve the availability of types, we should also ensure that any research that is done to typify taxa and standardize nomenclature is effective. Our typifications should be unambiguous and documented in such a way as to minimize the chance that our work will need to be repeated. Here, significant improvements can be made in current practice among diatomists, which will save considerable effort in the long-term.

In diatoms, there is a tradition of designating a whole slide preparation as the type, rather than individual specimens. I have done this myself (Mann 1981, 1990), and other authors doing the same include Foged (e.g. 1984), Håkansson (e.g. Håkansson & Mahood, 1993; Håkansson & Kling, 1994), Hendey (e.g. 1974), Kociolek & Stoermer (e.g. 1993), Lange-Bertalot (e.g. Lange-Bertalot,

1993; Lange-Bertalot & Moser, 1994), Round (e.g. Round & Basson, 1995; Bukhtiyarova & Round, 1996), Williams (e.g. Williams & Round, 1987) and many others.

Other diatomists have been more specific, indicating particular specimens by marking the slide in some way (e.g. the typification of *Thalassiosira simonsenii* by Hasle & Fryxell, 1977), or by giving a finder reference (Sims, 1994). R.K. Greville often identified particular specimens by ringing them, and this makes it easier to select a lectotype (Williams, 1988; Droop, 1996). Simonsen's (1987) catalogue of Hustedt's types gives finder references for each type or authenticated specimen, and each is illustrated photographically; this is a model to follow.

There is no barrier to the designation of a whole slide preparation as the type. Article 8.1 of the ICBN states that "for small herbaceous plants and for most non-vascular plants, the type may consist of more than one individual, which ought to be conserved permanently on one herbarium sheet or in one equivalent preparation (e.g. box, packet, jar, microscope slide)" (Greuter *et al.*, 1994; see also the discussion by Molloy *et al.*, 1992). Thus, as Williams (1993) has pointed out, "an entire population (or more accurately that part of the population or clone that has been collected and preserved) becomes the element [that bears the name]." However, this practice is flawed and should be stopped, since it introduces an unnecessary element of interpretation into typification.

The problem with designating a slide or population as a type is that the concept of a species (variety, form, etc) may change; hence, a type that is unambiguous when designated may become ambiguous later. In the recent work by Lange-Bertalot (1993), there are several cases where an array of photomicrographs is said to show the holotype. In most cases all the valves illustrated are very similar and could even represent a single dispersed clone, at various stages of the size reduction sequence. However, discordant elements seem to be present within the 'holotypus' of *Navicula canariana* (*op. cit.*, pl. 55, figs 2-10, particularly fig. 5). Again, do all the valves illustrated of *Brachysira calligraphica* or *B. hofmanniae* (Lange-Bertalot & Moser, 1994: pl. 37, figs 1-9, pl. 8, figs 1-18) really belong to the same species? If the species is split, which of the segregate species will bear the names *calligraphica* and *hofmanniae*? The typification is legal but 'soft', and some of Lange-Bertalot's types will probably have to be re-examined and re-defined. Even so, the excellent micrographs Lange-Bertalot provides will usually make it clear what choice is available to anyone who seeks to clarify the typification of the species he has described or revised.

A further example may be given. The type of *Sellaphora pupula* (Kütz.) Mereschk., designated by Ross (1963), is a slide containing many taxa, made from material Kützing collected at Nordhausen. This slide (BM 17918) and another slide of material authenticated by Kützing (BM 18725: see Ross, 1963) both bear valves of various shapes and sizes, which were probably all included in *S. pupula* (as *Navicula pupula*) by Kützing (1844). He appears to have studied live cells rather than cleaned frustules and all known *Sellaphora* species have a similar, characteristic type of chloroplast (Mann, 1989), which Kützing did not describe but drew (Kützing, 1844: pl. 30, fig. 40). The concept of *S. pupula* adopted by Ross was broad (Ross, 1963: 88) and, as a result, lectotypification via BM 17918 as a whole, rather than by a single specimen on BM 17918, was sufficient. Thus, when Schoeman & Archibald (1976–80) reinvestigated BM 17918 and photographed three valves, they labelled all of them as 'Type' (*op. cit.,* figs 5-8). One of the specimens they illustrate (fig. 8) is unlike the other two, but Schoeman & Archibald too had a broad concept of *S. pupula*. However, there is now evidence that the variable morphology of *S. pupula* reflects the presence within it of many subtly different entities, some of which deserve to be recognized as separate species (Mann, 1984, 1989; Mann & Droop, 1996). One of the segregate species will have to be chosen to retain the name '*pupula*' and a type specimen selected accordingly. This cannot be done without re-examination of BM 17918 and other authenticated material, in spite of the fact that Schoeman & Archibald provided excellent micrographs. Ross does in fact give a reference to a particular specimen on the slide label, but this is not mentioned in the 1963 paper, nor is there information on the slide as to how to use the reference to find the specimen (the figures are apparently the stage coordinates for an unspecified microscope). The studies by Ross (1963) and Schoeman & Archibald (1976–80) were very careful – I can vouch for this, since it took us 1.5 working days to find all of the very few specimens of *S. pupula* on BM 17918, including the three illustrated by Schoeman & Archibald – and, by most standards, they are well documented. But they were not definitive because they did not designate a single specimen as the lectotype. In contrast, Ross's (1963) typification of *Capartogramma rhombicum* Ross is beyond reproach, like almost all his nomenclatural work.

We can minimize future problems by deciding now to typify only via single valves or single frustules, whose locations must be specified and published effectively. Diatoms are unicellular organisms and there is no problem in defining individuals for the purpose of typification. Admittedly, to designate single diatom valves or frustules as types can be technically difficult. Engraving a ring on the cover slip, as done by Greville, is one possibility, though few people have the necessary equipment or skill to do it well. Furthermore, if several taxa are to be typified on the same slide, ringing may be impractical.

Identifying a specimen via a finder reference is another possibility, though finders do not work equally well with left-handed and right-handed microscope slide holders (see Droop, 1996). Furthermore, some finders are now obsolete, such as the Maltwood Finder used by Greville, O'Meara and others in the nineteenth century, or the Zeiss finder used by Simonsen (1987), though with care any set of references can be translated into those of another type of finder, or into the coordinates of a microscope stage. If neither engraving nor finder references are possible, one could use a series of context photographs, at progressively lower magnifications, to fix the position of the type. Alternatively, specimens could be selected and mounted individually on slides. This is the most certain method, but also the most difficult and expensive. Furthermore, if single specimens are selected and mounted as types, strewn slides of the populations from which they were derived should also be preserved, since selected slides are virtually useless for any purpose other than to act as nomenclatural standards. Strewn slides, on the other hand, can yield information about population variation and frustule ontogeny, size spectra and ecological communities, etc, which may be useful for many fields of research.

In addition, photographs of holotypes and lectotypes of diatoms should always be made and published, as hard-copy or via the Internet, to minimize future need to refer to type specimens and to help ensure that current usage does not drift. I now regret not doing this for *Amphora arcus* Greg. (Mann, 1995).

FINAL THOUGHTS

Some of my readers may not recognize the taxonomic world I have described; my experience may not be yours, especially, perhaps, if you work with higher plants. Algae, especially microscopic algae, do have their own special problems and some of these have already been dealt with by special provisions in the ICBN – e.g., different starting dates for Latin descriptions. I think we should consider further exceptions and special provisions, which take account of particular difficulties in particular groups. In diatoms huge numbers of varieties and forms have been described (the category of subspecies is very rarely used), but I would guess that less than 10% of them are ever used and far fewer have been typified; to search for all the types, catalogue and photograph them, and publish the results is obviously an enormous task. Meanwhile, of the estimated total of 200,000 species of diatoms world-wide (Mann & Droop, 1996), less than 10% have yet been described. It may well be that some of the new species have already been described as varieties or forms, but our experience suggests that this will account for well under half the total. The enormity of the task facing us as we attempt to document and classify the diversity of diatoms world-wide must be set against the fact that there are less than 50 people world-wide, perhaps less than 20, who spend more than half their working

week, on average, on diatom taxonomy. I hope that there will be some improvement in staffing in the future, in view of the ecological and biogeochemical importance of diatoms and their utility as indicator organisms in ecological and palaeoecological monitoring, but I suspect that any improvement will be slow. What is the priority for the few taxonomists that exist: to attempt to produce a complete inventory and classification of diatom species and their distributions, or to clear the backlog of nomenclatural difficulties resulting from 150 years of untypified infraspecific taxa? What about wiping the slate clean for all varieties and forms in diatoms, by establishing a new starting date for these categories?

Curiously, at the very time when it would be easier than ever to allow for exceptions and special rules, as a result of information technology, the efforts of some nomenclaturists are being directed towards uniformity. A new unified *Code of Bionomenclature* has recently been proposed (Hawksworth, 1996) and the Introduction extols the virtues of harmonization between the five existing Codes. I believe these efforts at unification are profoundly misguided. Although the application of the rules of nomenclature is not the longest, nor the most difficult phase of taxonomic research, it seems curious when taxonomic expertise is in short supply, to propose changes to the International Codes of Nomenclature that will decrease the effectiveness of taxonomists for the foreseeable future. Whatever the failings of the existing Codes, at least most taxonomists only have to deal with one of them. Specialists in dinoflagellates and other groups of protists that contain many heterotrophic as well as autotrophic representatives are not well served by the separation of the Zoological and Botanical Codes (Patterson & Larsen, 1991), but they too will be little better off under any new 'BioCode'. For the simple fact is, that everyone will still have to learn the old Codes and old terminology, while also learning the new. Ehrenberg's names are some of the few that diatomists have to consider in relation to the Zoological Code; otherwise, the Botanical Code alone is relevant. If a new BioCode is introduced, the Botanical Code will apply to all plant names published up to a certain date, the new BioCode to all names published afterwards. Diatomists will have two codes instead of one; dinoflagellate and euglenoid systematists will have three instead of two. So, those proposing a unified Code for all life, living and fossil, are not setting us free from the ball and chain of the existing, imperfect codes; they are giving us a new ball and chain to wear alongside those we already wear and to which we have grown accustomed. This alone should be enough to make us reject 'harmonization' and seek other ways to deal with the few serious problems in nomenclature, such as ambi-regnal organisms (where the simplest solution is to assign each disputed group to one or other 'nomenclatural kingdom').

To return to Ehrenberg: I believe it would have been pure vandalism to have destroyed the Ehrenberg Collection and it still would be. It is part of the national heritage of Germany, a milestone in the scientific development of a nation, and a resource for new scientific research. I look forward to using it.

CONCLUSIONS

- Old collections, such as Ehrenberg's, often represent a major capital investment, contain irreplaceable material, and are important resources for future research. They should not be discarded lightly.
- The type method is designed to standardize nomenclature and can do so. In practice it is sometimes inefficient, because types and information about types are unavailable or difficult to find, leading to drift of 'current usage' away from the standard. Typification and current usage are decoupled, leading to 'nomenclatural drift' away from the author's original intentions.
- Nomenclatural stability in diatoms could be greatly improved through wide dissemination of high-quality photographic images of types, via publications and the Internet.
- Type repositories should be designated. After an agreed date, types of all new taxa should be required to be lodged in at least one repository.
- A deadline for finding, registering, cataloguing and illustrating existing types should be set, after which any unregistered types would have no status under the ICBN.
- Alternatively, new mechanisms could be introduced, whereby types and whole collections could be declared nomenclaturally redundant, making neotypification possible.
- Diatom types should only ever be single specimens.
- Establishing the identities of the myriad varieties and forms in diatoms seems a waste of limited taxonomic expertise; there should be a new starting date for these categories in diatoms.
- Proposals for a unified BioCode of nomenclature are ill-judged.

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