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Summing-up

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Two dangers beset discussions on the principles of classification: they may degenerate and deal with the simpler issue of unequivocal labelling or they may become too impractical and get bogged in the philosophy or even metaphysics of classification. Unequivocal labelling is of the greatest importance but, however perfect the system becomes, it does not lead to an intellectual comprehension of the field covered. After a time the growth of knowledge and the accumulation of specimens or phenomena forces people to try to classify. Classification may be arbitrary, as when the names are arranged alphabetically, or it may be quasi-logical; but in the end a system with some sort of logical background is likely to prevail if only because it is easier to remember. Logic involves some arrangement in which things or concepts are associated in the system to an extent that parallels their resemblance to each other. Some would say their natural resemblance, but natural in this context seems pleonastic. Resemblance can, however, depend on many different factors and the judicious selection of relevant resemblances is the essence of any system of classification.

To this extent classification is arbitrary, we classify from a point of view and with an end in view; no system is ever unique, though the practical convenience of some may give them the appearance of uniqueness. In choosing the features on which to base a classification, preference is likely to be given to properties that distinguish the domain that is being classified from all others, or at least to properties with especial prominence in that domain. The most complete system of classification that we know is that of the metabionta and most biologists nowadays accept evolution as the peculiar feature of this domain. There is no reason to think that all organisms evolved from one ancestor, but each phylum presumably did and common ancestry probably goes back much further than that. A good classification of the metabionta should, therefore, make evolutionary sense and many speakers at this symposium have assumed explicitly or tacitly that classifying was the same thing as arranging species in a phylogenetic order. Very few people now dispute this, and these few seem to most of us to be perverse. But at one time their's was the accepted view. The early classifiers, Ray, Tournefort, Linnaeus, and de Candolle, believed in the fixity of species although various evolutionary ideas were already in the air. It is important to remember that Charles Darwin did not originate the idea of evolution; he suggested an important mechanism for what many scientists and even more educated non-scientists thought had happened. But the taxonomists before Darwin would have none of it. Linnaeus believed that the number of species was fixed at the

Creation and that this belief was an essential part of a sensible classificatory system. His system was a *tour de force* of observation and co-ordination with no logical foundation at all unless we choose to look on it as a logical attempt at deductive theopsychology. It is clear, therefore, that there is nothing impossible about classifying a domain before a body of knowledge has been accumulated about it which, in retrospect, seems necessary for a comprehension of the subject. The attempted classification is indeed often the stimulus for the acquisition of this knowledge.

Attempts to classify the metabionta ran long ago into the difficulty that it is not easy to decide what a species is. This difficulty obtruded itself acutely in our discussion of microbial classification and all shades of opinion were expressed between a disbelief in the usefulness of the species concept as total as that of Buffon, and an acceptance as naïve as that of an elementary textbook. The problem of species is primarily one of labelling rather than of classification; if there were no species there would be nothing to classify. So long as labels can be agreed it is not fundamental to the process of classification whether they represent distinct and unconnected entities, or nodes about which individuals group even although there is continuity between the nodes, or arbitrarily chosen type individuals in a continuous distribution without significant crowding at certain points.

The reality of the categories needs a little more attention than it often gets. Only individuals exist and with varying degrees of certainty these are grouped into species according to rules about interbreeding and so on. At this level a type specimen can be kept, but this is no longer possible at the higher stages of a classification. All the members of a genus or order may share an attribute and this may be absent from the members of another order but no type specimen of even a genus any longer exists. According to the phylogenetic view, and this seems the only logical one, all the individuals in a genus share an ancestor which they do not share with individuals in another genus. The same applies, but more distantly, to orders, classes and so on. We must assume that there was once an object (or pair of them) which was this common ancestor. In principle, except when multiplication is by binary fission, it could have been put in a bottle, after reproduction, and kept as a type specimen. But unless that had been done there is no object corresponding to a genus or higher step in classification. The position is nearly as simple even when there is binary fission, because samples of the original culture could, again in principle, be kept frozen for comparison later with the products of evolution. From a phylogenetic standpoint microbial classification will have a completely different certainty if it should prove possible to reconstruct experimentally the presumed course of evolution. The absence of fossils and embryology does not exclude the possibility of establishing a microbial phylogeny and of testing such schemes as that proposed by Bisset, because these organisms multiply rapidly enough to make the experimental approach possible.

As soon as we have decided how many species we are concerned with it is easy to say how many qualitites have to be observed for unequivocal labelling; n qualities would ideally permit differentiation between 2^n species. Thus ten qualities might suffice for 1024 species and we would almost certainly be safe with fourteen or fifteen qualities. No similar *a priori* approach is possible for classification rather than labelling. The number of qualities that have to be observed is certainly larger but it need not be very much larger and with the metabionta the number to choose from is vast. Thus the mammals can be labelled without looking inside, though dissection is necessary for classification. Things are not so easy with bacteria and viruses; a year's work may be needed to get as many pieces of information about a culture as can be got by a glance at a flower. This is the origin of part of our difficulty.

Many different types of information can be, and should be, woven into the classificatory scheme, and there is no obvious advantage in attaching overriding importance to any one type. We may consider host range and effects on the host, analytical composition and enzymic make up, or morphology, or the production of antigens and toxins. The effects on the host are obviously important because these effects are the reason for most of the financial support for microbiology. If they are made paramount, those saprophytes that seldom or never attack a host would not be included and the position of non-virulent strains becomes equivocal. Furthermore, host range produces many surprises. Thus Elrod & Braun (1941) found that the same bacterium was a cause of disease in tobacco plants and several mammals and it is now generally accepted that some plant viruses multiply in their insect vectors. Finally, if we tied microbial classification to the nature of the disease caused, we would stop arguing about microbial classification until we had settled the principles of disease classification. One of the conclusions of that discussion would probably be that diseases could only be satisfactorily classified in terms of the causative organisms. New possibilities arise when the two wings of our subject attack one another. The bacterial viruses could be classified in terms of their hosts or the hosts in terms of their viruses. Stocker's paper shows that the latter is the more productive direction for the argument.

Biochemical resemblances cannot be taken as overriding or we would put tunicates among the plants because of the importance in them of cellulose, and pigeons among the mammals because they secrete milk and the secretion is controlled by a similar hormonal mechanism. The more detail there is in biochemical information the more useful it becomes; Elsden asked for information about the mechanism of actions and Clarke asked that the statement that acid and gas are produced should be supplemented by information about which acid and which gas. It would be even better if we got rates of production as well, even if it were only an indication of the order, e.g. 1, 100, 10,000 or 1,000,000 molecules per cell per second. This would prevent similar weight being given to the presence of widely different amounts of enzyme.

Morphology and the production of macromolecules with recognizable distinctive properties are less likely to lead classification astray, because, being essentially imprecise qualities, their users are more aware of the existence of gradations and of the way in which a structure or specificity shades into adjacent structures and specificities. Antigenic specificity is particularly interesting; it is a trustworthy guide in one direction. If an organism does not share an antigen with others it can hardly be classified with them, but the existence of Forssman antigens and similar phenomena abundantly demonstrates that the possession of a common antigen is no evidence for relationship.

Classification presents a slightly different aspect to those with a chemical rather than a purely biological training, partly because of intrinsic differences and partly because chemistry has explored more of the classificatory possibilities. In the same way that evolution is the peculiar feature of the metabionta, the peculiar feature of the simpler chemical substances is that they contain a precise number of atoms arranged in a determinable way (cf. Pirie, 1952). Before Dalton even the first peculiarity was not recognized and when, in the eighteenth century, chemists wearied of elaborate and ambiguous nomenclature—the vitriols, livers, butters and such gems as *spiritus fumans Libavii*—attempts at rationalization followed several lines. The most interesting was made by Bergman (1784) who, 31 years after Linnaeus and also in Uppsala, tried to classify chemicals on Linnaean lines with genera, classes, and so on. To us, thinking of elements, he seems to perpetrate names as absurd as those he was eliminating and we see that he was trying to classify before he knew enough about the properties of the things classified. This is the odd irony of the situation. The simpler a system is, that is the fewer components it has, the more we have to know about it before sensible classification becomes possible. Hence much of Linnaeus' classification stands now because there are so many attributes of the complex organisms that bad pieces of classification stick out obviously. One can get away with worse fits where, as in chemistry, there are fewer obvious properties to look at.

The attempt is relevant to virus classification because viruses are nearly part of chemistry. People with chemical knowledge, on seeing attempts at Linnaean virus classification, are apt to mutter 'Isn't this where we came in?' And indeed it is. Bergman's grand attempt to force chemistry into a Linnaean strait-waistcoat was bound to fail, not so much because it is not that kind of subject, as because he did not know enough about the intrinsic properties of the things being classified. It is important to remember that, although much of chemical nomenclature is binomial, it is not, in most fields, Linnaean because both words in the name carry equal weight. It is as logical to classify all the chlorides together as it is to classify all the sodium salts, whereas nonsense obviously results if we start grouping together all organisms with *versicolor* as their second name. The argument sometimes advanced that viruses may be classified binomially because so many chemicals already are, is therefore fallacious.

We do not yet know what the peculiarities are that make some substances act as viruses. A fully logical classification is not, therefore, possible. But, just as Linnaeus classified before Darwin and some chemical classification was valid before Dalton, so at least parts of the viruses' domain could probably be classified. If we fail it may be because those who try have not Linnaeus's comprehensive grasp of the subject; alternatively it may be because they are not using enough properties. Even to label the 200 odd plant viruses we would have to use eight qualities and for classification probably a dozen or more. Virus classification is not, therefore, likely to be put on a satisfactory general basis until many more qualities of each are systematically studied.

A few principles of virus classification seem to be clear. Binomial nomenclature of the Linnaean type, in which the first name unites the members of a genus, can only be used rationally to unite those viruses that are likely to have had a common or similar origin. This may permit a few genera to be set up; Andrewes has made a start and so has Bawden though he prefers to say that he is grouping the varieties of a species. These beginnings will be both scientifically illuminating and practically useful. Attempts to carry co-ordination beyond the genus are not likely to succeed until we know whether viruses originated by variation from one another, by degeneration from bacteria, by the undisciplined behaviour of a normal cell component, or by yet another route. Premature attempts will produce such obvious anomalies that they will keep people from realizing that a few pieces of restricted classification are both useful and valid. From such restricted schemes a comprehensive system will grow gradually. This, besides being of importance in microbiology, will probably be of general use because a classification of the smaller viruses may well serve as a stimulus and model for the classification of other proteins.

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