Heretical Taxonomy for Bacteriologists*

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SUMMARY

In taxonomy, the most subjective branch of a biological discipline, rigid application of the principles of the microbiologists to bacteria has created puzzles and problems. Too much attention has been given to nomenclature, and too little to the bacteria themselves, their characters, and what they do. Different principles, which some will regard as heretical, should be applied to the classification and nomenclature of bacteria; ten of these principles are described briefly, but defy further summarization.

INTRODUCTION

A hitherto undetected similarity exists between Lewis Carroll's Alice and taxonomists, and bacterial taxonomists in particular. Alice lived (or dreamed) in a world of fantasy in which the eating or drinking of various delicacies gave her the ability to magnify or minimize characters, and she not only upset preconceived ideas of relationships in size, but also disturbed the significance or importance attached to hereditary factors (such as monarchy) and the normal (as distinct from the decapitate) state of the whole organism. Taxonomists also seem to do these things, though they do not need to bite mushrooms or drink magical concoctions in the laboratory. Like the people of Wonderland they tend to be argumentative, and one taxonomist seldom sees a problem through the same spectacles (whether rose tinted or not) as his colleagues. Again like Alice, the taxonomist makes an individual approach to the world around him, and while each is extremely conservative in his views about bacteria, he is by inclination a radical if not an anarchist, and an opportunist if not a dreamer. These may seem to be inconsistent and impossible combinations, but the proof of them should become apparent as I try to convince you that taxonomy can—and does—drive taxonomists to a topsy-turvy Wonderland.

I shall make two assumptions about readers who have got so far, namely, that they are all taxonomists at heart, and that they do not know anything about taxonomy (for anyone who thinks he does would have stopped reading before reaching this sentence). If these premises are true—and I believe they are true of most people—then an ignorance of the art or science of taxonomy should be an advantage to a taxonomist. And this is so, for the less one knows of one's subject, the easier it is to understand and accept its philosophies and conceits.

Because all taxonomists have different ideas on the subject it is necessary to remind readers what I mean by taxonomy (Cowan, 1965*a*). I distinguish it from systematics, which embraces all facets of biology intended to lead to the greater organization of

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organisms (Cowan, 1968). Taxonomy is divisible into three parts: first, there is the sorting of individuals into likes and unlikes; this is pigeon-holing or classification. Next, there is the labelling or naming of the groups sorted (nomenclature); the third part is the comparison of the unknown with the known and the identification, where possible, of the unknown with the previously recognized and named specimen, group, or population.

Although unrecognized as such, these three things are what everyone does, more or less subconsciously, from the cradle onwards, and explain my premise that everyone is a taxonomist at heart. Their recognition and acceptance lead to another very important point; we are not looking for a single classification of universal application, but for a series of classifications, each with a different objective. One set of objects can be classified in several ways for different purposes, so it is important to realize that we are not looking for THE classification of bacteria but for A classification bespoken for a particular purpose. It is surprising how many so-called microbiologists look upon the schemes published in *Bergey's Manual* (1923–1957) as if they were not only useful general classifications of bacteria, but ones that have received universal approval, both on earth and in heaven. I am assured by my colleagues that approval of *Bergey's Manual* is not universal, even on earth; I am not yet able to judge its reception in heaven.

It is now opportune to consider some of the absurdities of bacterial taxonomy, many derived from the macrobiologies, in the hope that by drawing attention to them we can make taxonomists think more deeply about the general implications of their work, and so avoid some rather stupid consequences.

Historical background

The history of bacteriology has been traced in detail by Bulloch (1938) and parts of historically important papers have been reprinted by Brock (1961) and it is from these useful books that much of this summary of historical background is taken. By listing these milestones of bacterial taxonomy we can hope to see the subject in proper perspective.

1675. van Leeuwenhoek first observed bacteria (he had seen protozoa in 1674); what he saw must have been some of the largest bacteria, for combinations of his best lenses magnified only about 300 times (Dobell, 1923).

1753. von Linné (Linnaeus) published his *Species Plantarum* and, although not the first to do so, he used a binominal nomenclature. His classification of plants was supported by names much shorter than the descriptive polynominals then in general use. Because of the convenience of 'binomials' the publication of this work became the official starting date for botanical nomenclature, and later of bacteriological nomenclature. What are called Linnaean classifications (those with binominal nomenclature) start from this date. But we should not blame von Linné for all the faults of what has become known as the Linnaean system.

1823. This was the year in which Bizio described, in a letter to a most eminent priest, the results of his microscopic examination of the phenomenon known as bleeding polenta in which, when the air was damp enough, a red colour appeared after 24 to 30 hr exposure. The importance of the date is that it was in this letter that Bizio gave the name *Serratia marcescens* to the 'many clusters of very small hemispheres' (translation by Merlino, 1924) which he thought were stemless fungi.

1881. Robert Koch described the plating technique by which he obtained colonies of bacterial growth on nutrient media solidified with gelatin. By this method he was able to obtain a pure culture of a bacterium more readily than by the earlier dilution methods in which cultures were (hopefully) expected to develop in broth from a single bacterial cell.

1887. The plating method was improved by Petri's invention of his dish, and at about the same time by the introduction of agar-agar as a gelling agent at the suggestion of Frau Hess, the wife of one of Koch's assistants. Thus began the era of pure cultures of which Perkins (1928) remarked 'The discovery of the principles of pure-culture study resulted in such a sudden burst of investigation that it was a lost month in which a new organism was not described, catalogued, and laid away, very frequently in the wrong grave.' But we do not assume that, thereafter, all cultures were pure; far from it, for as recently as the 1950s Muriel Robertson remarked that it was doubtful whether a strict anaerobe had ever been isolated in pure culture. Her pessimism was justified, for when she worked with anaerobes in the 1920s most of the descriptions of 'species' were really descriptions of the characters of mixed cultures.

1928. The year in which Griffith published his experiments in which he changed the serotype of pneumococci, a process now known as transformation. This was the beginning of the era that was to challenge the fixity-of-species concept, and was to make bacterial variation respectable and not a confession of poor technique.

Some bacterial heresies

This is the place to introduce a series of ideas about bacteria, taxonomy, and taxonomists that can, at best, be described as heretical, but will be thought by some to be anarchistic.

Bacteria are not plants. The least controversial of my heresies but one that needs to be stated to declare our complete independence of botany and the botanists. For a long time bacteria were regarded as plants and the earliest attempts at bacterial taxonomy were made by botanists such as Cohn (1872), who applied the principles and rules of botanical nomenclature to the bacteria. A more modern view, expressed by van Niel (1955), is that bacteria are different from both plants and animals, and are descendents of precursors of both. For those who like to put micro-organisms into a kingdom the name Monera has been used (for a detailed discussion see Whittaker, 1969). To me, living in a country with a monarchy, it seems quaint that the inhabitants of the great North American republic should accept kingdoms so readily and prefer them to the idea of separate, independent (taxonomic) states.

Hierarchical systems are unrealistic in biology. It is convenient and practical to catalogue spare parts for automobiles on a hierarchical basis, but misleading to do so with living units, because hierarchies imply phylogenies and phylogenies of bacteria are purely speculative. We think of hierarchies as groups broken down into subgroups, subgroups divided into sub-subgroups and these divided again into lesser groups, but in bacteriology this is literally stuff-and-nonsense. Exactly the opposite course is followed to create a hierarchy; individuals are said to be similar and to form a group which is labelled a species; several species with some common characters are combined to form a genus; several genera are combined to form a family, and so on. Hierarchies are built up, not broken down. Moreover the different groups are not sacrosanct; taxonomists build, with bricks of their own fabrication, walls, divisions,

and structures of their own design; they may build a pyramid but it is likely to be less enduring than those of the Pharaohs of the fourth dynasty.

The hierarchy is not appropriate for living things because it would presuppose that all are descended from one original living cell, whereas life almost certainly started in many different places by many independent syntheses. Apart from their evolutionary connotation, hierarchies are attractive to taxonomists because, by the separate listing of group characters, the descriptions of subgroups can be shortened.

Another (antihierarchical) view of the relations of living things is to regard them as different segments of a spectrum blending one with another but having patches or zones of greater concentration that form the smaller units such as species. The spectrum concept allows for, and indeed expects, differences between individuals, and identical units are the exception rather than the rule (Cowan, 1959).

Importance of variation. Because it directly opposed the basis of the typological concept of the ideal invariant form which made the unit, variation was generally ignored by bacterial taxonomists until fairly recently and was, in fact, regarded as a nuisance which introduced intermediate forms. An invariant unit was the foundation stone of the type concept (type method of botanists) by which a low category taxon such as a species was based on a single specimen or strain. Being derived from the botanical, the type of a species could be a drawing or other non-living specimen; bacteriology (and perhaps virology) stands alone in having living type specimens; in theory at least, the type specimens of the microfungi are dried-up specimens.

The *type method*, which once received my support (Cowan, 1956), is responsible for many of the faults of modern taxonomy. It makes a permanent link between a name and a single specimen, but inevitably the name becomes attached to a taxon of a particular rank so that the type strain (or specimen), intended as a name stabilizer, becomes the fixed point of a taxon. By the rules of nomenclature (for the type method is a purely nomenclatural device) the type specimen or strain should be designated as such by the original author of the name (who is not necessarily the first describer of the taxon); if he does not do this, the type (a neotype) can be designated by subsequent workers; originally a nomenclatural attachment, the type is now more often regarded as attached to a taxon such as a species or serotype. The important point is that the strain chosen as type does not need to be typical (in the sense of characteristic) of the taxon. This ridiculous situation can be overcome by ignoring the type method and treating the taxon as a population, as described below.

The population concept. Several strains are needed to represent a taxon above the individual, and Gordon (1967) believes that a species can be defined only as 'a group of newly isolated strains, old stock strains, and their variants that have in common a set of reliable characteristics separating them from other groups of strains'. This is a recognition of a taxon as a population and means that it can only be typified (if that word must be used) by several strains chosen, as far as possible, to show the normal limits of variability within the taxon. Even if the strains are not specially chosen but are a random sample, taken together they must be more representative of the taxon than any one strain could possibly be.

Overlapping of taxa, by which we mean that a taxon may belong to more than one taxon of the next higher category, is a concept unacceptable to the typologist or to the conservative taxonomist. It is a hypothesis that accepts the idea that micro-organisms can be arranged like a spectrum or as a series of intergrading forms (Cowan, 1955); to

be realistic the spectrum should be multidimensional as are the models made by Lysenko & Sneath (1959) to express visually the relations of the Enterobacteriaceae and of some pigmented bacteria. It can also be shown simply, but less convincingly, as a series of circles, the overlapping of which represents areas common to more than one taxon (Cowan, 1959).

Inadequate technique has been too much in evidence in bacteriological laboratories; like the car driver, no one questions his own ability or his own technique. We have had the era of the biochemist-turned-bacteriologist who never knew, or seldom cared, if he had a pure culture or was working with a glorious mixture or even a replacement culture. We now have the era of the molecular biologist and wonder (hopefully) if he is better than the ordinary biochemist.

As taxonomists we believe that our descriptions should be based on the examination of pure cultures, but the day has arrived when the possibility of a stable culture is questioned, and the older concept of a 'pure culture' is dying out. We accept the parasitization of our strains by minute bacteria, as *Bdellovibrio*, and even smaller biological units such as bacteriophages as normal events of everyday occurrence, but such acceptance is only possible when our techniques are beyond reproach. Yet it is only just over ten years since the publication of annotations to the Bacteriological Code (Editorial Board, 1958) in which a culture infected with a phage was given as an example of an 'abnormality' (annotation to Rule 24h).

Technical ability determines the facts on which descriptions are based; and good descriptions are essential to good taxonomy. One of the first principles of nomenclature concerns priority, which requires that the oldest validly published name should be used. This may seem sensible but its application can be vexatious because the interpretation is subjective. A requirement for valid publication of a name is that it should be accompanied by a description or a reference to a description. What passed for a good description in, say 1900, may not be recognizable now, but some workers will accept a name with even the vaguest description of what is named. The situation is complicated because the rules are said to be retroactive, so that the poor bacteriologist of 1900 had to anticipate rules—and changes of rules—made 50, 60, or 70 years later!

Too much attention is given to names, and not enough to what the bacteria do. Names are only labels and they can be replaced by numbers, or by a mixture of letters and figures (Cowan, 1965*a, b*). Names do not have any significance except as a means of communication; it is naïve to expect that the name of a bacterium will have a meaning, or that it is descriptive. Quite elaborate (many think too elaborate) rules have been drawn up in codes of nomenclature to try to regulate the formation and use of names, but these codes would delight the hearts of lawyers because they are too detailed and try to cater for all eventualities. In the event, they are confusing and self-contradicting; for example, Principle 4 of the Bacteriological Code says that names are taken from Latin or Greek words, but if taken from any other language or formed arbitrarily, they are to be treated as if they were Latin, and Latin terminations should be added. In practice many specific epithets are formed in an arbitrary manner, or are unaltered modern words or place names without any attempt at latinization; all these are acceptable because the Code gives permission to correct faulty latinization, improper connecting vowels or endings, without taking away any credit due (in an author citation) to the original author who had mishandled, or been frightened off, the Latin. (The zoologists, in their code, are much less permissive.) Since names for bacteria can

be formed in an arbitrary manner, all the rules about latinization, correction, and so on, are really unnecessary elaborations.

This heresy can be summarized thus: since scientific names may be formed in an arbitrary manner, all names printed in the appropriate letter-characters should be acceptable; there should not be a need to worry about latinization or Latin endings. The corollary is that names, as names, would cease to be centres of argument and debate, and this would free the taxonomist to devote more time to the more important characterization and description of the organism or taxon.

The heresy of importance. This heresy is not concerned with the weighting (or importance) of characters, a subject adequately discussed by Sneath (1957), but asks the question, What is important in taxonomy? Is it the name, the description, or the ability to recognize (identify) the organism? The last heresy dismissed the name as important only in communication; here we are considering the search for practical day-to-day characters that enable us to visualize and recognize the organism we have isolated. Two things are important: the ability to search out characters (i.e. technical acumen and prowess), and the literary ability to describe clearly and unambiguously the characters found. What the taxonomist and his colleagues need to know is the nature of the organism as revealed by the characterization.

The heresy of Serratia marcescens, or names of the pre-bacteriological era. What were the organisms seen by Bizio and described in 1823 as stemless fungi, hemispherical capsules occurring in clusters (Merlino, 1924); were they yeasts or some of the smaller bacteria? In trying to answer these questions we should bear in mind the quality of microscopes in the early part of the nineteenth century, and the fact that the bacterium later named *Monas prodigiosus* was so small that it became the standard test organism to detect faulty bacterial filters. Which, if either, of these organisms did Bizio see, a bacterium or a yeast? Mycologists unhesitatingly implicate a yeast as the cause of bleeding polenta; we also know that the late Dr R. S. Breed reproduced the phenomenon with a bacterial culture. It is reasonable to suppose that the cause of the phenomenon is non-specific, and may be a yeast or a bacterium; but it seems unlikely that Bizio saw a very small bacterium and more probable that the hemispherical capsules were yeast cells. Therefore, I believe that the name *Serratia marcescens* was not applied to a bacterium, but may have been applied to a yeast. However, Bizio's description is so vague that it is impossible for us to be sure what organism he saw; it follows that the name he applied to his ill-defined organism should not be accepted but should be rejected under Rule 24f of the Code, which says that a name is illegitimate 'If its application is uncertain'.

The fact that a name such as *Serratia marcescens* was ever considered by so practical a bacteriologist as Breed suggests that there is something wrong with the rules of nomenclature that allow such old names to be valid. It is an example of a name applied to something before bacteria were isolated or studied scientifically, but it serves a purpose in drawing attention to the scientific uncertainty that may arise from the use of names of the pre-Pasteurian era.

The starting date of bacteriological nomenclature, 1753, is much too early; it would be more realistic to consider a date in the present century.

Although Dobell (1923) described Leeuwenhoek as the first bacteriologist and the founder of bacteriology, it would be unwise to regard his observations as the beginning of scientific bacteriology. Fortunately Leeuwenhoek did not apply any latinized names

to his animalcules, but if he had done so his drawings would have been good enough to satisfy Rule 9d(1) Note 1 of the latest Code (Editorial Board, 1966), which says that 'For a species which cannot be maintained in laboratory cultures... the type is the original description, preparation or illustration.' Which shows that 'the wind of change' has not yet swept through the Bacteriological Code.

The starting date 1753 follows botanical practice, and is a relic of the days when scientific bacteriologists (as distinct from medical ones, who regarded the subject as a branch of pathology) accepted the Botanical Code in matters of nomenclature. Botany was, and is, the science of living plants, but its nomenclature is based on pressed, dead specimens, descriptions, and illustrations; bacteriologists are fortunate in being able to study and preserve living specimens so that our reference material is much more satisfactory than that of the botanists. We developed our own code, but only by modifying the botanists' code; why did we not start afresh with a less complicated, less ambitious code? And why did our predecessors accept a starting date before bacteria had been grown? Other dates have been suggested, and I think that a reasonable one would be 1920, when the Winslow Committee of the Society of American Bacteriologists (Winslow *et al.* 1920) issued its final report on bacterial classification; the nomenclature used in this report was obviously based on the work of one of the Committee's members, R. E. Buchanan, who had published ten papers on the subject between 1916 and 1918 (Buchanan 1916, 1917*a-c*, 1918*a-f*). The names were mostly those in use at the time the report was published, and it would have been reasonable to conserve them and to ignore (reject is the correct technical term) all other names published before 1920.

That the *Bacteriological Code* is too detailed and tries to anticipate every problem that may arise will seem to some to be a heretical statement, but it is justified by the failure of the Code, as shown by the frequent revision at almost every international congress for microbiology, to meet the needs of bacteriologists. The Code is divided into four parts: 1, General Considerations; 2, Principles; 3, Rules and Recommendations; 4, Provisions. Of these, Chapter 2, Principles, is much the best and most useful section; it is the part least like the Botanical Code and shows that bacteriologists would have done better for themselves and the discipline if they had paid less attention to the practices of the botanists.

This heresy is the expression of a wish for a much simplified Code, based on the Principles of the present Code together with a means of putting teeth into it; it could be improved by simpler language and less pseudo-legal phraseology. To achieve a measure of compliance some form of registration of names would be required and, if the conditions for registration were stringent enough, we should stop having names without descriptions or with only inadequate descriptions. This is not a new suggestion, it is merely the echo of one made for fungi by Ainsworth & Ciferri (1955).

My last heresy in this paper, number 13, should really not be regarded as a heresy. Taxonomists are lucky in that their work is personal, varied, and above all, full of interest; it is work that can and should be enjoyed by those who take part in it, whether as individuals or as members of a team. Taxonomy is a subject that tends to stimulate heated arguments and nomenclature is the most temperature-raising branch of all. For some unknown reason it seems to be impossible to discuss nomenclatural problems calmly and with the quiet detachment expected of scientists; arguments become acrimonious, tempers frayed, and blood pressures raised, but seldom do taxonomists

become spiteful or lose their dignity in personalities; that has happened, but fortunately only occasionally. So, while we argue at length and appear to be mortal enemies, we keep a sense of judgement and recognize that there is always more than one side to every argument. We know we are lucky in our work for it is work to be enjoyed and, what may surprise non-taxonomists, is not to be taken too seriously.

To round off my list of heresies I shall describe some principles of heretical taxonomy in the hope that readers will think about them, and compare them critically with the taxonomy learnt from the cradle onwards. In commending these principles let me remind you that they are Utopian and come, almost directly, from Alice in Wonderland.

TEN PRINCIPLES OF HERETICAL TAXONOMY

1. We should not attempt to classify bacteria in a rigid hierarchical system.
2. The relations between bacteria can be expressed only in a spatial concept, but can be visualized simply (if inaccurately) by a series of spectra, and of overlapping taxa.
3. The type method, as now practised, is not suitable for bacteriology; it should be replaced by a method in which several strains represent the taxon and indicate the limits of its variability or spread.
4. Good bacteriology is dependent on good technique and pure cultures; too much attention cannot be given to these qualities.
5. Names are important only as labels; they have no significance and cannot have a meaning.
6. Similar organisms cannot be recognized by the similarity of their names; they may have been mislabelled or misidentified. Clear, concise descriptions are the corner-stones of good taxonomy, and they alone enable similar organisms to be recognized and identified.
7. Accurate characterization and good descriptions are dependent on the use of good technique and pure cultures; clarity of description is dependent on literary adequacy.
8. The Bacteriological Code should be simplified by deleting the Rules and Recommendations. It should consist of Principles, and discretion should be given to bacteriologists to apply them intelligently.
9. The starting date of bacteriological nomenclature should be changed to some time when bacteriology was being done scientifically; I suggest that 1920, when the S.A.B. Winslow Committee reported, would be a suitable starting date.
10. Names of taxa should be acceptable only when (i) they are accompanied by an adequate description, and (ii) representative strains are deposited in at least two culture collections. Acceptance of names should be made by a committee which would sit to ensure that (i) and (ii) had been carried out, and that names were not duplicated. Accepted names should be registered

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