



Alice in Taxonomyland: Systematics in mid-twentieth century microbiology

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ABSTRACT

This article recovers some of the key moments and contributors in the convoluted and consequential debates in microbiological taxonomy in the mid-twentieth century. By reconstructing and refining some of the critical transnational debates about the nature of microbiological systematics in a pre-phylogenetic age, it reveals how prominent and influential individuals in mid-twentieth century microbiology were preoccupied with discussion about the merits of different approaches to bacterial taxonomy and nomenclature. Drawing on previously unstudied archival materials, as well as scientific publications and textbooks, it sheds light on how a small but highly active group of microbiologists continued to wrestle with exactly how, or even whether, to classify microbial species. It also examines the parallel development of practical tools to support the vital work of identifying organisms in laboratory settings. These devices were designed to make sense of a vast volume and range of morphological, metabolic, biochemical and serological data.

1. Introduction

'Taxonomists are jealous of their preserve and make things difficult for the newcomer by the use of a complicated (and often unnecessary) terminology. Few will agree on a definition of taxonomy itself, and each is a merry sharp—shooter of his colleagues.' (Cowan, 1968, p. 104)

On May 5, 1969 the Curator of the UK's National Collection of Type Cultures, Samuel Tertius (S. T.) Cowan (1905-1976) delivered a seminar entitled 'Alice in Taxonomyland' at the University of Maryland. His departure point was that, in his words, 'taxonomy can – and does – drive taxonomists to a topsy-turvey [*sic*] Wonderland' (Cowan, 1970, p. 145). In this witty and provocative address Cowan, who was the standard bearer of a so-called heretical approach to taxonomy, and centrally involved in postwar taxonomic decision-making amongst the international community of microbiologists, outlined ten principles to be applied to the naming and classifying of bacteria.

These principles, derived from his then two decades' experience in senior positions within the International Committee on Bacteriological Nomenclature, were deceptively simple, yet hugely wide-ranging. For example, first, in the 'least controversial of my heresies', Cowan asserted that 'Bacteria are not plants' (Cowan, 1970, p. 147). This observation was based on extensive wrangling, many decades earlier, about the most appropriate starting point for any classification system of microbes:

were they best treated as animals or plants, with the application of corresponding conventions of taxonomy? As he went through these principles, Cowan revealed to his audience – as his published version does still to us – the chief objects of controversy, debate, and consequence in microbial taxonomy, many of which have remained unexplored by historians of biology, and are still hotly disputed over half a century later.

Despite his centrality in these post-war debates, Cowan has scarcely featured in histories of microbiology or in accounts of its taxonomy. The same can be said for several other individuals who we consider in this paper. This is perhaps reflective of a wider preoccupation by historians with taxonomy in botany and zoology in this period, with classification of microbes being comparatively neglected (Hamilton and Wheeler, 2008; Kranke, 2024). The imperative for biologists to classify not just new, but new *kinds* of organisms in the twentieth century – microorganisms being a particularly fluid and uncertain category – sparked a second wave of classificatory interest and wrangling within the biological sciences following substantial earlier work in botany and zoology during the eighteenth and nineteenth centuries. In a pre-genetic age articulating relationships between bacterial species proved a major challenge and created an environment ripe for disagreement and debate about microbial classification and even the stability of bacterial species themselves (Jan, 2009a).

In a precise analogue to Maureen O'Malley's important corrective work in introducing microbiology into the fold of broader philosophies

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of biology and taxonomy (O'Malley, 2014, especially pp. 45-94), this paper sets out to highlight the complexity and consequences of mid-twentieth century debates amongst selected systematists about how bacteria should be organised in classificatory systems, and what they should be called. Most notably, it does so by drawing scholarly attention to the visibility of active and outspoken taxonomists such as Cowan, and the persistence of uncertainty even in an age of supposed clear rules for microbial systematics. In contrast to the claim that '[t]axonomy, like nomenclature, is a theory-neutral activity', we demonstrate how changing theoretical proclivities amongst microbiologists manifested in major overhauls of all aspects of classification, nomenclature and taxonomy (Ingraham, 2017, p. 49). As O'Malley and Parke note, and as was certainly experienced by researchers in microbiology in the mid-twentieth century, '[m]icrobes ... provide a treasure trove of problems for species concepts', with taxonomic efforts strongly influenced by prevailing theoretical concepts within microbiology and the highly contingent scientific infrastructure of microbial culture collections (O'Malley and Parke, 2020, drawing on other studies such as Ereshefsky, 2010). There were, as we also show, practical consequences to this complexity: an ever more convoluted system of nomenclatural rules and principles, which had started out as a key means of legitimising the discipline of microbiology, and a mass of underpinning data which threatened instead to fundamentally undermine it.

There are several reasons why a close, detailed focus on the key figures in this paper is warranted. In the first place, Cowan was not alone in being preoccupied with microbial taxonomy, classification and nomenclature. Rather, he was just one, albeit influential, figure in an expansive international network of microbiologists for whom there was a great deal at stake in the organisation and naming of microbial species and strains. Second, the question of how, and even whether, a taxonomic system should be organised reveals important features about microbiology more widely, not least its desire to shake off the shackles as the new pretender to longer-standing scientific disciplines such as zoology and botany. By attempting to articulate robust classificatory frameworks which deviated from those for animals and plants, microbiologists sought to legitimise their enterprise as a scientific discipline, not just a practical field.¹ Third, pronouncements – known in quasi-legal terms as 'opinions' – by groups convened to arbitrate on matters of bacterial nomenclature reveal the points at which groups of organisms were formally recognised, or ceased to be recognised. This reinforces our understanding of the naming and packaging of bacteria into categories as a highly contingent, subjective activity which reveals the interdependence between microbial ecology and other disciplines, such as biochemistry, which were frequently invoked in differentiating between species and strains. Finally – at least for the purposes of this paper – the postwar obsession with classification was illustrative of wider attempts to establish more comprehensive transnational cooperation in science more widely. The organisations involved in microbial management were part of a more concerted programme of integration across national contexts, where the role of culture collections, systematics, and nomenclatural debates have yet to be considered.²

While the main period in question begins with the publication of the first International Bacteriological Code of Nomenclature (known

informally then and since as the "Bacteriological Code") in 1948, it is important to outline some of the developments which preceded this.³ We conclude in 1976, which marked the year from which microbiologists were required to designate new species with a taxon and name in accordance with the Bacteriological Code. Coincidentally, this year also marked the death of the retired but still-active Cowan, and the year before Carl Woese and George Fox published their landmark paper on the phylogenetic reconfiguration of the domain of prokaryotes (Woese and Fox, 1977).⁴ Prior to 1948 several competing, contradictory, and controversial systems of classification and nomenclature co-existed amongst bacteriologists. While the first formulation of the Bacteriological Code was far from successful in serving as a wholly-accepted source of reference, it nevertheless marked the culmination of preceding decades in which bacteriologists had attempted to find common ground. The following thirty years were characterised by continuing wrangling about the form, purpose, and desirability of producing a definitive approach to microbial systematics, leading Cowan and other "heretics" to become progressively more cynical about whether the enterprise of producing a taxonomy of microbes was feasible or even necessary.

For a paper focused on aspects of nomenclature, it is especially important to establish the specific differences between four terms which might be otherwise easily conflated. These are: nomenclature, taxonomy, classification, and systematics. The purpose of distinguishing between these is to highlight how they were used by historical actors at a time when issues of microbial taxonomy were of such vital importance to the field.

Nomenclature was concerned with determining and allocating the most appropriate names to specific organisms. As we shall see, it proved a contentious and complicated process. This was distinct from taxonomizing, which was the process of determining the relations between different species: which groups there should be, and which species belonged where. Cowan's framing of the distinction is helpful in this regard. Writing in 1952, he confirmed that 'when the taxonomic groups have been defined, the problems of nomenclature begin' (Cowan, 1953, p. 111). His later close collaborator, K. J. Steel, articulated this in more hierarchical terms: the more subjective nomenclature should be subservient to the science of taxonomy, since 'nomenclature should be our servant, not our master' (Steel, 1962, p. 405). Classification, by contrast, was the overarching enterprise of establishing both taxonomy and nomenclature, while systematics 'embraces all facets of biology' in attempting to position bacteria in relation to other organism types (Cowan, 1970, p. 145). This is critical, since the precise question about differences and similarities between zoological, botanical, bacterial and, later, virological systems of nomenclature and taxonomy were a significant preoccupation of microbiologists in the mid-century, with those engaged in debates about nomenclature frequently overreaching their remit of adjudicating names of species.

Finally, a word is needed on sources and structure. In the first place, what follows is based on two parallel strands of evidence. The first comprises the official publications of taxonomists and groups tasked with establishing systems of microbial classification, such as the *International Bulletin of Bacteriological Taxonomy and Nomenclature* (abbreviated later as the *International Bulletin*), published first in 1951 and

¹ This was also true in the parallel case of mycology which for some decades laboured under the requirements of botanical codes of nomenclature under which mycological classification rested.

² The immediate postwar period saw the subversion of many long-standing conventions in bacterial taxonomy and nomenclature. For example, the term "Bacterium" as a taxon was first introduced by Christian Ehrenberg in 1828, before being formally jettisoned as a term of any taxonomic value in 1954 (Ehrenberg, 1831; Judicial Commission, 1954).

³ The renaming of the Code as the International Code of Nomenclature of Prokaryotes was formally agreed in 1999, confirming its long-recognised scope beyond simply bacteria (Parker et al., 2019).

⁴ Although there was not an immediate transformation following the Woese and Fox publication, this is nevertheless one of those instances where we can make reasonably concrete distinctions between pre- and post-phylogenetic taxonomic efforts, each period characterised by different assumptions and debates. As far as the definition of "prokaryotes" is concerned, this was similarly in flux, changing from a descriptive, morphological label in the early twentieth century to a class that spanned the kingdoms of Archaea and Bacteria, catalysed by Woese's work.

renamed in 1966 as the *International Journal of Systematic Bacteriology*. The second is correspondence between individual taxonomists, focusing on those associated with the UK's National Collection of Type Cultures (NCTC) which was both an essential repository of microbial strains and a site of significant taxonomic research, and which Cowan led for almost the entirety of the period under consideration. Collectively these sources reveal the public and private views of those engaged with microbial classification at a time when it was one of the most significant features of the field.

The article is arranged in three sections which are broadly chronological. The first, "Creating a Dump Heap and Overhauling Microbial Collections", outlines Cowan's early tenure as Curator of the NCTC from the mid-1940s and his attempts to overhaul the collection and its presentation in a catalogue of strains circulated throughout the world. The second, "The International Bulletin and Judicial Opinions", examines the origins and eventual publication in 1948 of the first Bacteriological Code, and the subsequent establishment of the *International Bulletin* as a means of securing international compliance with decision-making in microbial classification conducted by several judicial committees. The final section, "The Determinator and Microbial Data", charts how the pragmatic work of identifying microbes in a laboratory setting was practised through the period, indicating some of the practical aids developed by, and for, microbiologists and how these reflected the processes of working with increasingly large volumes of data even in a pre-genetic era.

We begin, though, with how Cowan and other staff members of the NCTC came to be sifting through microbes in a farmhouse on the outskirts of post-war London.

2. Creating a dump heap and overhauling microbial collections

At the outset of the Second World War in 1939 the UK's national collection of microorganisms – the National Collection of Type Cultures (NCTC) – as well as other critical scientific and industrial infrastructure in major urban centres, was at risk. In consequence, this sizeable collection of microbes of scientific, industrial and agricultural importance was relocated from its well-established surroundings in the Lister Institute on Chelsea Bridge Road in central London and deposited in a farmhouse in Elstree under the continued care of its long-standing Curator, Dr Ralph St. John-Brooks (1884-1963), who had been in post since 1920 (Stark, 2025). These premises were far from the stable and clean conditions ideal for the maintenance of a collection of microbial organisms with exacting nutritional and environmental requirements. It is perhaps therefore unsurprising that, when Cyril S. Brindle, a newly-appointed Assistant Curator, first visited these temporary premises in 1946 he found them in a state of dishevelment. He wrote to the new Curator, S. T. Cowan, who succeeded St. John-Brooks that same year, that he had 'learnt to balance a slide on a tin lid' and 'became a carpenter' to repair a desk which could otherwise only stand up straight when propped against a wall.⁵

Cowan was no stranger to working with a range of microorganisms in poorly-equipped surroundings. He had spent the period from April 1941 to December 1943 touring throughout the Mediterranean and Middle East, providing diagnostic and clinical services. His diaries from the time record how, working in a 'most unimpressive' and 'shabby' laboratory in Lydda (Lod, then located in the British Mandate for Palestine), he isolated and identified numerous strains of *Bacillus dysenteriae* during examination of over 3,000 stool samples between October and December 1941.⁶ During this time, Cowan encountered several novel strains. In around January 1943, for example, he 'isolated an unusual Salmonella

from a blood culture; it fermented glucose and mannitol; indole production was variable. The "H" antigen appeared to be a, b and d, a combination unknown to me."⁷

Returning from his experiences with bacterial identification, and working in similarly less-than-ideal conditions at the NCTC, one of Cowan's first acts as Curator was to assess the collection. Having established the state of the microbial cultures, the following summer in 1947, 'Miss Shaw [another new member of staff at the NCTC] and I went through the boxes of cultures and threw away many buckets – full of obviously contaminated or dried up cultures.'⁸ Unknown to him, Cowan's predecessor St. John-Brooks had already been tasked by the Medical Research Council (MRC) with developing a set of priorities for the collection in the post-war period. In a paper to the MRC, written on February 14, 1944, St. John-Brooks advocated for the collection to be somewhat more limited in scope.⁹ This included maintaining all of the 'non-medical fungi, soil and industrial bacteria' currently in the collection, but not actively expanding in this area and, most controversially, that the 'the distribution [of strains] ... be restricted to approved research workers and should not include commercial firms and teaching bodies.'¹⁰

Cowan was clearly preoccupied with making a fresh start of the NCTC, both as an institution allied to microbiological research and in the organisation, preservation, and scope of its collections. At the same time as he was clearing out the numerous contaminated cultures he pondered what updates might be required to the catalogue of organisms, last published in 1936, and in need of updating. His first task was to determine how the revised catalogue should be organised. On February 26, 1947 he wrote to noted bacteriologist G. S. Wilson to outline a key element of his proposal. In it, Cowan identified a simple problem: 'what to call the many organisms that are at present labelled Bacterium for want of a better name', the majority of which were still awaiting more formal designation.¹¹ His solution was simple: to 'create a dump heap for unclassified Gram-negative bacilli', for which he proposed a couple of almost-comical names: either '*Incertum*' or '*Dubium*'.¹² This would be just one of many 'considerable changes in the new catalogue', reflecting both the change in taxonomic approaches, the realignment of the collections to focus on bacteria of medical and veterinary relevance, and Cowan's own preferences.¹³

Wilson (1895-1987) was then Director of the Public Health Laboratory Service and co-author with his long-standing collaborator, W. W. C. Topley (1886-1944), of the landmark bacteriology textbook *Principles of Bacteriology and Immunity* (latterly known informally simply as *Topley & Wilson*), to which Cowan looked as his guiding light for taxonomic purposes (Topley and Wilson, 1929).¹⁴ He was not, though, impressed with Cowan's suggestion, noting in response that 'I don't very much like the idea of creating a generic name for a dump heap.'¹⁵

The concept of a "dump heap" has frequently been attributed to Stephen Jay Gould's provocative 1985 article where he suggested that a "taxonomic wastebasket" might serve an important purpose and the position of some organisms under any given taxonomic structure may

⁷ Cowan, *Kephissia to Chapel-en-le-Frith*, p. 26.

⁸ S. T. Cowan, '[Untitled Manuscript]', n.d., p. 3, Box 4, NCTC.

⁹ R. St. John-Brooks, 'Medical Research Council. National Collection of Type Cultures. Memorandum by the Curator', 14 February 1944, MRC 44/31, The National Archives, Kew (hereafter: TNA).

¹⁰ St. John-Brooks, 'Medical Research Council', TNA.

¹¹ S. T. Cowan to G. S. Wilson [Letter], 26 February 1947, FD 1/992, TNA.

¹² The two names were not in use at the time and Cowan clearly suggested these to highlight uncertainty ('*Incertum*') and doubt ('*Dubium*') about the classificatory position of these ambiguous microbes.

¹³ Cowan to Wilson, 26 February 1947, FD 1/992, TNA.

¹⁴ This text subsequently went through numerous versions, and is still, in its 10th edition, a standard reference work the field. (Topley, Wilson, Collier, Balows and Sussman, 2007).

¹⁵ Wilson to Cowan [Letter], 27 February 1947, FD 1/992, TNA.

⁵ Cyril S. Brindle to S. T. Cowan [Letter], 4 November 1946, Box 4, NCTC Archives, UK Health Security Agency, London (hereafter: NCTC).

⁶ S. T. Cowan (ed. Tony Cowan), *Kephissia to Chapel-en-le-Frith* (April 1941-December 1943) MS 8346, pp. 13-17.

inevitably remain uncertain. (Gould, 1985). Yet it is revealing that Cowan, despite being committed, as we shall see, to rigorous and thorough taxonomic principles, was a far earlier advocate for the collation of uncertain species under a separate heading. He referred time and again in his writings to the benefits of such a taxonomic “dump heap”: many years later in his *Dictionary of Microbial Taxonomic Usage* he highlighted how the term “bacterium” was ‘debased to the category of a dump heap’ with little to no meaning (Cowan, 1968, p. 13). It is revealing of the state of taxonomy in the 1940s that it was almost exactly fifty years later, in 1994, that an identifier of non-specific taxonomic rank – *Candidatus* – was proposed, and accepted, for those organisms which could not be described in sufficient detail to warrant being assigned a new taxon (Murray and Schleifer, 1994). Cowan’s vision of *Incertum* or *Dubium* was, therefore, ultimately realized, albeit only after half a century of further searching, ultimately fruitlessly, for a neater solution.

In the immediate post-war period, Cowan’s more theoretical musings on the nature of taxonomy were intimately connected to his vision for what the scope of the NCTC should be: a collection of bacteria of medical and veterinary interest. His classification system sought to create a clear distinction between microbes and other forms of life, principally fungi and plants, and to establish bespoke principles which were not borrowed or adapted from other taxonomies, but which treated microbes as a unique starting point. He had, too, an expansive vision of what taxonomies involved, not just developing a system, but ‘classification, nomenclature, and identification’, leading him to consider the *use* of a classificatory system and its accompanying conventions of nomenclature as an important feature of taxonomists’ work (Cowan, 1968, p. 1).

First, however, he had to overhaul the collection of microbes held at the NCTC itself. Most of the fungi were transferred to the Imperial (shortly afterwards renamed as the Commonwealth) Mycological Institute, at the Royal Botanic Gardens in Kew, while fungi of medical interest were sent to James T. Duncan (1884-1958) a medical mycologist at the London School of Hygiene and Tropical Medicine (Anon, 1958). Cowan arranged for the plant pathogenic bacteria to be transferred to botanist W. J. Dowson at the Department of Plant Sciences at Cambridge, while he rationalised the collection of medical and veterinary bacteria by examining strains and those ‘that have lost their typical characteristics are being replaced by new ones, and atypical cultures are being discarded.’¹⁶

None of this activity was occurring in isolation. At almost the same time that Cowan was clearing out buckets full of dead or impure bacterial cultures from the National Collection of Type Cultures and rationalising its scope, his predecessor, Ralph St. John-Brooks, was marking the triumphant culmination of his own collaborative endeavours to create international networks and standards in bacteriology and microbial collections, on the cusp of the publication of the first edition of the Bacteriological Code.

3. The Bacteriological Code and International Bulletin of Bacteriological Nomenclature and Taxonomy

In the fevered postwar age of transnational collaboration – the United Nations (1945), NATO (1949) and numerous other specialised international bodies being established in the aftermath of World War Two – microbiologists followed suit. One major outcome of the International Congress for Microbiology in Copenhagen in 1947 was

¹⁶ S. T. Cowan, ‘National Collection of Type Cultures of Micro-organisms’, 18 September 1948, FD 1/989, TNA.

agreement on the first Bacteriological Code of Nomenclature (the Code). This represented a further step in microbiology’s change from ‘an applied science, empirical in outlook, and isolated from the mainstreams of biological thought’ to a fundamental pillar of the biosciences (Stainer et al., 1957, p. v).¹⁷ Although this was a long-standing ambition of the bacteriological community since the interwar period, beginning with the Microbiology Society of America in early taxonomic reports of 1917 and 1920, it nevertheless represented the first more formal effort towards international standardisation (Winslow, Broadhurst, Buchanan, Krumwiede, Rogers and Smith, 1920).¹⁸

The purpose of the Code was to create a set of agreed rules and principles for the naming of bacterial species and strains. The chief architects of this new Code were Ralph St. John-Brooks, first Curator of the NCTC, and Robert Earle (R. E.) Buchanan (1883-1973), Director of the Agricultural Experiment Station at Iowa State College (later University). These two figures represented two different areas of microbiology. John-Brooks was trained as a physician while Buchanan was primarily a botanist. What each shared was a strong interest in both transnational collaboration and standardisation; Buchanan – centrally involved in the 1920 scheme initiated by the Society of American Bacteriologists (SAB) – gained a reputation more as an administrator who was consistently concerned through his career with systematics and nomenclature.¹⁹

Given his interest in systematics in general, and nomenclature in particular, Buchanan was joining a lengthy historic cast list of predecessors who had attempted to impose order and structure on the microbial world. He was well aware of this fact. His 1925 text, *General Systematic Bacteriology*, running at more than 550 pages, was a synthesis of his earlier ten-article series published between 1916 and 1918 in the *Journal of Bacteriology*.²⁰ In *General Systematic Bacteriology*, Buchanan took pains to first outline, in nearly 100 pages, the structure of different classificatory approaches to bacteria expounded over the preceding 150 years. He argued that, ‘if science is to be defined as a system of *classified* knowledge, the subject of bacteriology is labouring under a serious handicap in lacking ... a satisfactory system of terminology or of nomenclature’ (Buchanan, 1925, p. 9). This was despite innumerable attempts in the past to provide just such a framework. Ranging from Otto Friedrich Müller’s two volume treatise, *Vermium Terrestrium et Fluviatilium* (1773-4), which Buchanan claimed to contain ‘the first definite named description of any organism included now among the bacteria’, to Ferdinand Cohn’s classification of bacteria in 1872, and beyond, he then introduced his own system (Cohn, 1872; Müller 1773-74).²¹

¹⁷ This text outlined how ‘microbiologists have come to realize that micro-organisms, whatever their singularities, obey the same general laws as other living systems’, though interestingly the essentially evolutionary nature of classification and taxonomy which predominated in botany and zoology was decidedly not integrated within bacterial taxonomy until considerably later (Stainer et al., 1957, p. v).

¹⁸ The “American system” was subject to significant criticism, though many researchers did indeed adopt some of their radical suggestions for changes in nomenclature.

¹⁹ Upon his death in 1973, none other than S. T. Cowan penned a memorial to Buchanan, which appeared in the 1990 revision of the Bacteriology Code. In this, Cowan noted that ‘Buchanan could never understand why anyone should make light of ... or be flippant about bacteriology, and worse, about its nomenclature’, and that his views on such matters ‘were rigid and he was inflexible’ (Cowan 1992, p. xiii).

²⁰ *General Systematic Bacteriology* had the added virtue of being endorsed by the SAB and published as the first in their new series on the topic, adding further reach and authority to Buchanan’s perspectives.

²¹ Danish naturalist Müller (1730-1784) introduced conceptual frameworks of species and genera, and made a comprehensive schema to account for diversity of the infusoria which he observed. Meanwhile, more than a century later, German botanist Cohn (1828-1898) was the first to classify algae as plants and established the fundamental division of bacteria – which did not clearly belong to the domain of either botany or zoology, nor on their own – into rods, spheres, spirals and threads which continues to form the basis of many classifications.

Buchanan had previously outlined his reservations at a meeting of the Annual Meeting of the SAB in Urbana, Illinois, in an address on December 28, 1915. There he argued that ‘the classification of bacteria is in a chaotic condition’ and railed against those bacteriologists who were ‘ignoring the well established customs of biological nomenclature’ (Buchanan, 1916, p. 591). Buchanan’s rigid taxonomic philosophy was laid bare:

‘Everywhere we find disregard of law and precedent, and everywhere the loose thinking and writing which are the consequence. ... Our present system, or lack of system, leads to inaccuracies, misconceptions, and misstatements. We can not long continue to violate the principle that for every kind of living thing there shall be a single valid name, without causing confusion.’ (Buchanan, 1916, p. 592)

Of principal importance to this enterprise was Buchanan’s belief in the alignment between bacteria and plants (Buchanan, 1917, p. 155). In this respect he was drawing on a decades-long tradition of aligning bacteriological nomenclature with botanical conventions. One can easily imagine him gazing wistfully at parallel discussions in contemporary botany where there was far more widespread consensus about the need to have – and to adhere to – such a set of rules.²²

During the 1930s both Buchanan and St. John-Brooks had not just an international agreement about nomenclature in mind, but far broader collaboration. In a co-authored, unpublished draft text dating from the Second World War they argued that ‘the expansion and co-ordination of the existing Type Culture Collections of micro-organisms must, of necessity, take a prominent part’ in postwar reconstruction.²³ This mirrored precisely the origins of St. John-Brooks’ own institution, the NCTC, which was specifically established as a piece of British imperial scientific infrastructure in 1920 (Stark, 2025). Buchanan and St. John-Brooks’ unpublished paper presented a vision for the ‘formation and endowment of type culture collection research institutes ... one in North America and one in Europe ... [to enable] the utmost facility in exchange of cultures and information’.²⁴ This was coupled with Buchanan’s existing, and explicitly North American, system of nomenclature, which he compiled with a view of introducing it to the Third International Congress for Microbiology, held in New York in September 1939 (Buchanan, 1939).

Infrastructural and taxonomic plans were intimately connected. At precisely the time when Buchanan and St. John-Brooks were finalising the Bacteriological Code for the subsequent Fourth International Congress for Microbiology, held in Copenhagen in 1947, the most recent edition of *Bergey’s Manual of Determinative Bacteriology* – an authoritative but much criticised manual for bacterial identification – was in preparation. It is impossible to imagine that those overseeing the updated edition of *Bergey’s Manual* were unaware of the imminent publication of the Code. Nevertheless the 1948 sixth edition of *Bergey’s Manual* noted that ‘there has been international agreement (in so far as this can be achieved) that bacteriologists should follow the botanical or zoological codes in the naming of bacteria to the extent they are applicable’ (Breed et al., 1948, p. 40). The continued reference to botanical and zoological codes of nomenclature as an agreed position internationally is highly surprising since one of the three editors of the *Manual*, Robert S. Breed,

was a close collaborator of both Buchanan and St. John-Brooks, who had by this time both become fervent supporters of microbiology’s independence from these other sciences. *Bergey’s Manual* also made explicit reference to transnational cooperation: ‘future development of taxonomic work holds several interesting possibilities of increased international cooperation such as between the various National Type Culture Collections’ (Breed et al., 1948, p. vii). The authors bemoaned the ‘large number of ... poorly described species [which] suggests that there has been much unsatisfactory work done in the field of bacteriological taxonomy’ (Breed et al., 1948, p. vii).

An elaborate reconstruction of the circumstances surrounding the finalisation of the first Bacteriological Code itself is not necessary here (Buchanan & John-Brooks, 1947).²⁵ Suffice to say that through successive International Congresses for Microbiology, largely US-led efforts to develop a common code of nomenclature came to bear fruit.²⁶ What is critical is that once a bacteriological code of nomenclature was established following the Copenhagen Congress in 1947 the question remained: how to ensure connection and observance of the Code between bacteriologists? January 15, 1951 marked the culmination of over twenty years of activity of the International Committee on Bacteriological Nomenclature (ICBN): the publication of the first issue of a new journal, the *International Bulletin of Bacteriological Nomenclature and Taxonomy*. This came in the wake of ongoing wrangling about the content of the first Bacteriological Code, and was intended by its founders to ‘contribute to the stabilization of bacteriological nomenclature by opening up a channel of free communication between those concerned in the naming and classification of bacteria’ (The Editorial Board, 1951, p. 1). The Editorial Board consisted of three key figures in bacteriological nomenclature and culture collections: R. E. Buchanan, R. S. Breed, and Cowan. Cowan had seamlessly moved to take the position in the ICBN previously held by St. John-Brooks, who had occupied several leading roles within the ICBN since its founded at the first International Congress for Microbiology, held in Paris in 1930. At this meeting, St. John-Brooks and Breed were elected as the two permanent secretaries of the Nomenclature Committee, with a remit to adjudicate in cases of disagreement or uncertainty about the appropriateness of names for particular species or strains. It was, then, a largely deliberative group, though there were only limited and limitedly visible mechanisms for bringing such instances to the attention of the Nomenclature Committee, and for the communication of their verdicts.

It was this particular need – the dissemination of pronouncements – which was especially at the forefront of the intentions behind the *Bulletin*. However, it was only in light of the publication of a taxonomic guide – the Bacteriological Code – that a more public forum made sense. As Buchanan, Breed, and Cowan noted in their opening Foreword, ‘the Bulletin was authorized by emendations of the Bacteriological Code’ (The Editorial Board, 1951, p. 1). Prior to this, judgements rested much more strongly on the individual preferences of Nomenclature Committee members. Of even greater importance was the assertion from Buchanan, Breed and Cowan that ‘the living forms with which the microbiologists concern themselves are in part plants, in part animals, and in parts primitive’ (The Editorial Board, 1951, p. 1). This presented a problem, since the difficulties in establishing dedicated standards in bacteriological nomenclature stemmed precisely from the conflation of these categories of living organisms. The rationale for treating bacteria as a separate case for the purposes of classification, taxonomy and nomenclature was, according to the Editors, precisely the rationale for ‘the

²² The origins of overarching botanical nomenclature are frequently traced to the publication of *Lois de la Nomenclature Botanique* in the aftermath of the International Botanical Congress held in Paris in 1867 (de Candolle, 1867). Subsequent texts refining and, in some cases, challenging these original principles appeared over subsequent decades, and the dominant set of rules when Buchanan was active in the late 1910s was agreed at the Vienna Congress in 1905, subsequently amended in Brussels in 1910 (Briquet, 1912).

²³ R. E. Buchanan and Ralph St. John-Brooks, ‘Suggestions for the Development of Type Culture Collection Research Institutions’, p. 1, uncatalogued, NCTC, Box 4.

²⁴ Buchanan and St. John-Brooks, ‘Suggestions’, p. 3.

²⁵ A detailed account of the proceedings of the Copenhagen meeting, and prior work, is included in the substantially-revised 1958 edition of the Code. See: International Committee on Bacteriological Nomenclature [1958], pp. v-xviii).

²⁶ For a general overview of the phases of development, largely taking place at and between major international microbiology meetings, see: Lapage, Sneath, & Lessel et al., 1992a, 1992b.

elaboration of special descriptive criteria' for bacteriology ([The Editorial Board, 1951](#), p. 1).

Far from being an open publication in which original pieces of systematic research might appear, its primary purpose was rather 'maintaining the necessary contacts between and among the members of the Judicial Commission, of the International [Nomenclature] Committee, and of the several subcommittees on nomenclature and taxonomy of special groups of microorganisms of economic and medical significance.' The *Bulletin* existed to communicate decisions and to explain their rationales, as well as to provide updates to the Bacteriological Code, and thereby 'bring order out of the present chaos in bacteriological nomenclature' ([The Editorial Board, 1951](#), p. 3). Although the meeting in Paris and subsequent versions of the International Congress for Microbiology in London (1936), New York (1939) and Copenhagen (1947) were the forums in which the Nomenclature Committee put forward proposals and received critique and suggestions for amendments, it was clear from the outset that the resulting journal was designed primarily to facilitate communication between a small group of committed taxonomists.

One of the principal motivating factors behind establishing the Bacteriological Code in the first place was to enable bacteriologists to remove their reliance on the systematics underpinning botany and zoology. However, this was not solely in the gift of microbiologists themselves. As Buchanan, Breed and Cowan noted in their report on the major Botanical Congress which took place in Stockholm in July 1950:

'the botanists retain their interests in the bacteria as plants, and the microbiologists recognize that the nomenclature of many groups of microorganisms such as the yeasts, fungi and algae is governed by the botanical rules. ... The microbiologist may well note with some care those changes in the botanical rules that have to do with fungi, yeasts and algae and also those which may suggest desirable changes.' ([Buchanan et al., 1951a](#), p. 11)

While botany and zoology shared core basic terminology, some of these were anathema to bacteriologists. For example, as Buchanan, Breed and Cowan noted in a set of proposed amendments to the Bacteriological Code, even a term such as 'taxon is quite unfamiliar in bacteriology', highlighting the fundamental differences involved in microbial systematics ([Buchanan et al., 1951b](#), p. 34).

As well as high-level conceptual debates about the framing, meaning and scope of terminology in bacteriology, there was equal attention afforded to seemingly more niche matters of nomenclature. On the following page from their proposition that the term taxon be allowed but not required in bacteriology, there appeared a report on 'the correct spelling of the specific epithet in the species name *Bacillus megaterium* de Bary 1884', recommending that '[t]he spelling *megaterium* ... is to be preferred to the spelling *megatherium*' ([Buchanan et al., 1951](#), p. 35). This settled the long-standing and challenging dual use of both *B. megaterium* and *B. megatherium*, reflected in numerous publications prior to this decision ([Ehrlich and Knight, 1951](#)).²⁷

The role of culture collections in microbial systematics comes through clearly in a large proportion of articles in the *Bulletin*. Writing shortly after stepping back from his role on the ICBN, the then-Emeritus Professor Breed harked back as far as his personal copy of the 1900 catalogue of bacteria from the Kral collection in Prague – later relocated to Vienna and then Chicago, by which time most of the cultures had died – to identify the appropriate type culture of *Micrococcus* ([Strasser, 2019](#), p. 37). In his account, Breed noted that the properties listed in two

cultures of *Micrococcus luteus* held at the American Type Culture Collection, one originally isolated in 1892, did not accord what the Kral catalogue would lead him to expect ([Breed, 1952](#)). The collections of type cultures were therefore critical in establishing principles and specific instances of nomenclature, reflecting the importance of decision-making about which strains to keep, and where. However, this case also demonstrates how the desire for these collections to be sources of authority – changing nomenclature accordingly – posed practical difficulties for even the most experienced recipients and beneficiaries. The wholesale renaming of species and strains meant that for many it was a challenge to even understand which strains were available.

Cowan meanwhile clearly viewed the Bacteriological Code as a flexible set of guidelines rather than fixed principles and rules, and noted as much in his introductory remarks to the revised catalogue of the National Collection of Type Cultures published in 1958. In his approach to nomenclature he proposed to use terms which, in his opinion, were 'probably correct by a reasonable interpretation of the Rules of the Bacteriological Code': he noted that '[n]omenclature [in the catalogue] does not follow the usage of any one textbook of determinative bacteriology' ([Medical Research Council, 1958](#), p. 1). It is perhaps understandable that Cowan was sceptical about extant systems of nomenclature. Writing just a year earlier, a trio of high-profile Professors of Bacteriology at the University of California – Roger Stainer, Michael Doudoroff and Edward Adelberg – introduced a chapter entitled 'the major groups of bacteria' by lamenting that '[b]acterial taxonomy is still in a very poorly developed state, and there is no universally accepted system of bacterial classification.' They were scathing of 'serious differences of opinion among bacteriologists' which they blamed on 'much guesswork' needed to construct evolutionary relationships between groups of bacteria and led to 'endless unprofitable disputes between the holders of different views' ([Stainer et al., 1957](#), pp. 296-7).

The Bacteriological Code and the accompanying *International Bulletin of Bacteriological Taxonomy and Nomenclature* – crucially including taxonomy in the title as well as nomenclature – had been formulated precisely to build international consensus in bacteriological systematics. At a time when microbiology as a discipline was enjoying a period of security as a legitimate specialist science following its earlier distribution across a range of applied fields, the failure to arrive at anything approaching consensus revealed continuing deep-seated difficulties in defining, classifying, and understanding microbial relationships. Nowhere was this more visible than in the far-reaching revisions to the 1948 Code in its second edition, published a decade later in 1958, with Buchanan and Cowan comprising half of the four-person editorial board ([International Committee of Bacteriological Nomenclature, 1958](#), p. xviii). Regardless of this ongoing uncertainty, the pragmatic need to accurately identify specific strains remained, and this was an equally important feature of practice in the period.

4. The Determinator, microbial data, and practical tools of identification

'The classification of bacteria has reached a point at which there is a need for some mechanical aid to sorting the data.' ([Sneath, 1957](#), p. 201)

Whether formally recognised as such or not, philosophies of taxonomy existed since well before the appearance of large-scale taxonomic enterprise. This is the territory of both the descriptive and the normative: what do, and what should, taxonomies attempt to capture, and what do, and what should they include or exclude? The observation above, from the instigator of computer-aided numerical taxonomy of microbes, Peter Sneath, indicated that by the mid-1950s the volume of data needed to appropriately classify a microbe had become

²⁷ No doubt the members of the Judicial Commission and other mid-century microbial systematists would be appalled by the continued co-existence of both terms in places, sometimes even within a single publication. See: [Deng, Chen, Gong et al., 2021](#). To add further complexity to this aside, the organism was formally renamed as *Priestia megaterium* in 2020 ([Gupta, Patel, Saini and Chen, 2020](#)).

prohibitively large.²⁸ This data represented an accumulation of morphological, serological and biochemical information about microbes' appearance and behaviour. It had long been used to differentiate between microbial strains and species, but now the question arose: what sort of mechanical aid could help achieve reliable identification of bacteria?

Cowan – deeply wedded to the more niche enterprise of microbial taxonomy but running a culture collection used by a large, international community of microbiologists – was caught between large-scale theorising about taxonomy and the practical business of enabling microbiologists to do their work. He outlined his taxonomy vision across two principal publications: in the introduction to his *Dictionary of Microbial Taxonomy* (first published in 1966, the second edition appearing posthumously in 1978), and in an earlier paper entitled simply “The Philosophy of Taxonomy”, which appeared in the *Journal of General Microbiology* in 1955.

The latter was based on a series of discussions which Cowan orchestrated at the annual meeting of the Society for General Microbiology in September 1954. As well as Cowan's contribution – which laid the foundation for consideration of various approaches to microbial taxonomy – various other prominent microbiologists, including C. H. Andrewes and G. S. Wilson, penned essays, of which there were fourteen in total, covering topics from nomenclature to viruses and genetics (Andrewes, 1955; Wilson, 1955). Addressing his audience at the meeting, Cowan explicitly stated that it was his ‘purpose to sow doubt in your minds’ (Cowan, 1955, p. 319).

As Curator of a pragmatic scientific resource – the National Collection of Type Cultures – one of Cowan's central roles was to make the collection intelligible, useable, and accessible to a broad range of microbiologists. Cowan was acutely aware of the need for bacteriologists to be able to determine precisely which strains they were dealing with, a particular challenge given the rapid proliferation of biochemical and serological methods of identification, added to earlier morphological approaches. For Cowan, while ‘there is not one classification (made by God, Nature, or by man) but any number of classifications, all made by men, each with a particular purpose in mind’, the pragmatic nature of much of microbiology necessitated useable and accessible tools unburdened by the finer points of microbial systematics (Cowan and Steel, 1974, p. 4).

To this end, he and collaborator K. J. Steel, Deputy Curator at the NCTC during much of Cowan's tenure, developed a practical tool to help bacterial identification. This involved combining existing diagnostic tables of microbial data with a physical device – The Determinator – which allowed researchers to compare the characteristics of organisms under observation with known strain behaviours. The Determinator (Fig. 1) was a relatively simple device, consisting of a wooden frame box into which researchers could place different diagnostic tables, a Perspex sheet laid on top, and a moveable slider on which records of various reactions were to be recorded. Researchers could thus straightforwardly determine which genus and species were present, assuming that the individual tests were carried out reliably.

On its own Cowan and Steel's device was, they admitted, useless (Cowan and Steel, 1961, p. 357). It was its conjunction with masses of authenticated bacterial data that rendered it a useful tool. Such tables, meanwhile, had often been extremely large and complex (Fig. 2), having grown in scale to cover an ever-increasing range of microbial strains, and a hugely expanded range of morphological, serological and biochemical properties. The principal impact, as both Cowan and Steel later noted, was arguably on the refinement of their own schema for

identifying bacteria, published first in 1966 and later reissued shortly after Steel's death in a second edition (Cowan and Steel, 1974). In this, they attempted to lay out robust and readily replicable principles and tests in a manner akin to *Bergey's Manual*. However, what was distinctive about their endeavour was the acknowledgement that for identification purposes they had attached ‘much weight to some characters, regarding them as having great distinguishing value, give less weight to others, and no weight at all to some features’ – this being decidedly not the case in classification where all characters and features were viewed as being of equal significance (Cowan and Steel, 1974, p. 22).

Although Cowan and Steel did ‘not think that anyone (including ourselves) used this device ... the tables [which they developed to accompany it], which can be used by simple inspection, led to the development of this *Manual*’ (Cowan and Steel, 1974, p. 22). By recognising the value in ascribing different levels of weighting to sets of microbial characteristics, they were able to adopt a stepwise system of bacterial identification, and to produce a manual which could be more readily followed by those unfamiliar with all the possible tests which might be applied to microorganisms. Cowan had already been experimenting with electronically-aided numerical taxonomy for over fifteen years, and he readily acknowledged the potential of computation in comparing across ‘an almost unlimited number of characters or features’ (Cowan and Steel, 1974, p. 22). Cowan's earlier work in this area, in collaboration with Peter Sneath (1923–2011), who ‘revolutionized the practice of bacterial taxonomy’ through his work with numerical approaches, began with the publication in 1958 of a co-authored ‘electro-taxonomic survey of bacteria’ (Jones and Grant 2013, p. 337; Sneath and Cowan, 1958, p. 551). In essence, Cowan remained an ardent proponent of computer-aided numerical taxonomy, but favoured a different, and more subjective approach to identification, writing in 1974 that ‘the results of [observations between strains] can be analysed laboriously by making a large number of calculations, or more easily by letting a computer do the hard work’ (Cowan and Steel, 1974, p. 22).

Cowan and Steel were not alone in attempting to develop a manual for identification which could be widely used. Another taxonomist, the Australian microbiologist V. B. D. Skerman, also developed a well-recognised manual of bacterial identification, with the significant inclusion of ‘a complete list of the techniques needed for the purpose’ (Skerman, 1967, p. ix). In Skerman's work we can see strong evidence of the need to align theoretical and philosophical discussions of microbial taxonomy and nomenclature with the practical job of microbial identification. It is telling that, whilst critical elsewhere of *Bergey's Manual*, he expressed thanks to both the Board of Trustees responsible for its publication, as well as R. S. Breed and R. E. Buchanan, the latter provided ‘a very critical perusal of the manuscript’ (Skerman, 1967, p. ix).

Skerman's *A Guide to the Identification of The Genera of Bacteria* was straightforwardly a text which advocated an approach rooted in numerical taxonomy. However, in keeping with recent scholarship which has shown the need to move beyond a characterisation of his period as the so-called Systematics Wars, he also recognised the importance of ‘conventional methods’, providing they were appropriately standardised (Skerman, 1967, p. 17). It was this feat of standardisation in practice to which Skerman aimed. At the same time, while numerical taxonomy itself encountered heavy resistance in its application to multicellular organisms, Sneath was sufficiently confident that in the second edition of his major text on the subject with Robert Sokal, he asserted that ‘there have been rather few criticisms of numerical taxonomy applied to bacteria’ (Sneath and Sokal, 1973, p. 424).

Bacteriologists were not alone in wrestling with interrelated, but distinct, challenges of taxonomy, classification, and nomenclature. Mycology, for example, was far more strongly wedded to the classificatory system of plants, governed by long-standing conventions as part of the Botanical Code. This manual, traceable back to the publication of *Lois de la Nomenclature Botanique* (“Laws of Botanical Nomenclature”) in 1867, was the result of sustained transnational dialogue, and placed requirements on mycologists which sat at odds with their preferred

²⁸ Numerical taxonomy was an approach which aimed to develop quantitative relationships between characteristics. It was developed with the goal of lessening reliance on subjective weighting of species' characteristics and therefore claiming to remove a degree of subjectivity which had previously characterised taxonomy.

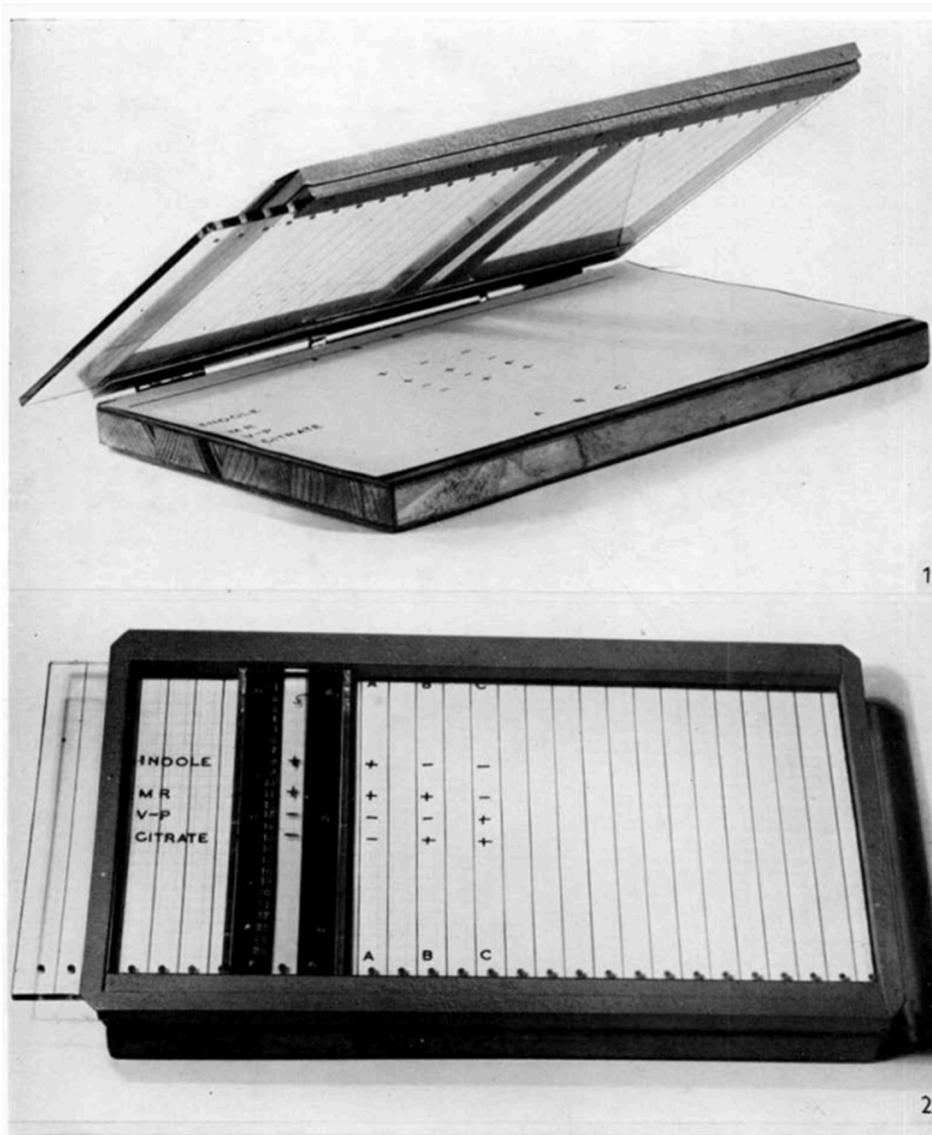


Fig. 1. Cowan and Steel's "Determinator", first introduced in a 1960 publication in *The Lancet* and later simplified to yield less specific results but without such a large number of tests being required (Cowan and Steel, 1961, p. 373).

practices (de Candolle, 1867). For example, botanists insisted that any type specimens conformed to botanical standards: essentially they had to be dried, preserved specimens rather than living cultures (Bessey, 1950; Bisby, 1953).²⁹ This posed significant challenges for mycologists, for whom maintaining living cultures in collections was standard practice. Similarly, as Cowan noted in his posthumous 1978 publication, the significantly expanded *A Dictionary of Microbial Taxonomy*, '[v]iral nomenclature continues in a long gestation' (Cowan and Hill, 1978, p. 276).

The tension between, on the one hand, a desire to develop a flexible taxonomy, and, on the other, to maintain consistency in means of identifying organisms was an intrinsic part of the operation of culture collections and their use in systematics. It was most visible in the separate contributions of Cowan and Steel to the Twelfth Symposium of the Society for General Microbiology, which took place at the Royal

Institution London in April 1962.

Until the 1962 Symposium, most literature on classification was highly specialised and restricted to individual groups (Porter, 1962). The expanding scale of taxonomic research, noted by J. M. Sherwan, who was Curator of the UK's National Collection of Industrial Bacteria at the Torry Research Station, was visible in the number of papers tackling the subject.³⁰ The Symposium, which was focused specifically on classification, considered all groups of microorganisms beyond bacteria, reflecting both facts and uncertainties and was the most comprehensive work on the topic, with the express intention of arriving at a clearer and far-reaching account of microbial classification (Knight, 1963). Steel and Cowan's contributions distinguished between the chaos of the natural world and the problems that arise when scientists attempt to create order from it. The character of the natural world in biological science, they noted, was often incommensurate with the scientist's need for standardisation. As such, they argued, a flexible approach to classification and taxonomy was required to embrace this chaos and provide the

²⁹ Bessey is particularly noteworthy since his position was actually Emeritus Professor of Botany, reflecting the close connections between that field and mycology, particularly in matters of plant pathology. Bessey noted, pithily, that any specimen designated as a type 'should be preserved' (Bessey, 1950, p. 16).

³⁰ J. M. Sherwan 'Report on the National Collection of Industrial Bacteria M.D. No. 60/37, 1960', DSIR 40/46, TNA.

Fig. 2. A large-scale table of microbial data produced in around 1960, demonstrating the increasing number of methods involved in more precise bacterial identification. (Uncatalogued, held at the National Collection of Type Cultures and UK Health Security Agency.)

resilience to encompass unknowns, mistakes and alterations to procedures. Changes in approaches to taxonomy as an intellectual pursuit, rather than routine procedure, were also seen as important to making these improvements.

In his entry, Steel argued that identification should be the priority of microbiologists, done swiftly and objectively to be put to practical use. Taxonomy, on the other hand, was subjective, less scientific, and not strictly necessary prior to application. He underscored the ultimate purpose of classification: to enable clinical and epidemiological research, advocating for multidisciplinary understanding in microbiology. Knowledge of how an organism was used in practice aided identification, enabling accuracy and utility (Steel, 1962, p. 427). The Symposium itself functioned largely to educate scientists working across specialisms (Routen, 1962). Cowan also highlighted the importance of a microorganism's practical usage: the specificity of information needed about a bacterium, for example, was different for epidemiological surveys and the treatment of an individual patient (Cowan, 1962, p. 440).

In his typically light-hearted and forthright paper, Cowan set about disrupting matters of nomenclature, questioning its underlying purpose and advocating the necessity of flexibility.³¹ He discussed the artificial nature of the designation 'species', invoking Lockian and Darwinian admissions that the term was arbitrary, its usage illustrating the taxonomist's desire for convenience over accuracy (Cowan, 1962, p. 435). This also applied to strain typing as it was not actually possible to designate a typical representative of a species: 'only the individual is real' (Cowan, 1962, p. 447). Cowan envisaged a future where new technologies such as the computer aid in classification using a numerical rather than hierarchical system. Once problems with taxonomy were

resolved, he argued, attention should then be turned to nomenclature and Cowan considered in favourable terms A. J. Cain's proposal of an uninomial, rather than binomial system.³² Despite the supremacy of accurate identification, on standardising this procedure, Steel indicated that the goal of 'whole truth' in some cases of notifiable disease and pathogens must come second to 'partial truth which is of greater diagnostic value' (Steel, 1962, p. 413). A major source of criticism of *Bergey's Manual* was its attempt to combine classificatory and identification systems with indefinite descriptions of organisms. Therefore, The Determinator was designed to simplify the process of identification through comparison of unknown organisms with known characters of existing ones.

As custodians of one of the largest microbial culture collections in the world in the mid-1960s, Cowan and Steel were responsible for circulating thousands of strains to researchers internationally. In his report on activities in 1963, for example, Steel noted that they had issued upwards of 8,000 culture samples, of which around 1700 were distributed to workers outside the UK, spread across 53 countries.³³ It is therefore unsurprising that they were distinctively well-placed to produce an effective aid to bacteria identification. They worked closely with specialist teams tasked with bacterial identification, who were part of the Public Health Laboratory Service, located on the same site as the National Collection of Type Cultures. Nor is it surprising that, given the significant disagreements between microbiologists – driving taxonomists into a 'topsey-turvey [sic] Wonderland' – that persisted throughout the 1960s and beyond, that virologists experienced precisely the same set of challenges when breaking ties with the bacteriologists. There is

³¹ As has been noted elsewhere, Cowan was not alone in his scepticism of the bacterial species as a concept (Jan 2009a; Jan 2009b, p. 168). However, his leadership of a prominent national culture collection which depended on a successful use of such a species concept makes his scepticism all the more significant.

³² Cain proposed a revision of Linnaeus' original approach to taxonomy which centred the genera as the primary category. He lamented the state of taxonomic training by imitation rather than through instruction in taxonomic principles (Cain, 1959, p. 235).

³³ K. J. Steel, 'Notes from the National Collection of Type Cultures – 2', 1964, uncatalogued, NCTC Archives, Box 4.

much more to be said about microbial taxonomy in the immediate postwar period, but as a teaser for another story as yet untold, let us consider what the Chair, British virologist Peter Wildy (1920-1987), presented at the second meeting of the Executive Committee of the International Commission on the Nomenclature of Viruses at their meeting in London in April 1968:

'In taxonomy we must realize that we are dealing with virologists as much as with viruses. There are manifold ways of classifying and naming viruses and everyone has his own prejudices as to the best. Add to that the innate inertia and reaction to change that we mostly suffer from and it becomes obvious that the wise strategy is to progress slowly with frequent wide consultations. People will no longer tolerate an authoritarian approach; they want to participate.'³⁴

Although he was writing about virology, Wildy was no doubt equally conscious of the decades-long debates – still ongoing – within the broader discipline of microbiology. While only a minority of microbiologists were centrally concerned with taxonomy in their everyday scientific work, the outsized influence which Cowan and others held in shared spaces of scientific infrastructure, management of microbial data, and official pronouncements meant that the impact went far beyond the niche of classification.

5. Conclusion

'In its simplest form an impure culture will show features of all the organisms present so that the characterization of a supposed strain or culture may well be so bizarre or exotic as to suggest a new species or even a new, higher taxon.' (Cowan, 1968, p. 6)

For large swathes of the middle part of the twentieth century a number of highly skilled, knowledgeable and committed microbiologists expended a great deal of energy and scientific credibility attempting to develop comprehensive systems of classification, taxonomy and nomenclature. From the publication of the first comprehensive survey, with recommendations, in 1920, to the viral schism of the 1960s, bacteriologists simply could not agree on how to organise the objects of their investigation. By the end of the period in question many were railing against the reality of microbial species as intelligible units of study or classification. The prospect of a resolution came into view only from the 1970s with the twin advent of recombinant DNA technology – in which analysis of bacteria was at the vanguard – and a three-kingdom proposal from Carl Woese which carved off the new kingdom of archaea from bacteria. These efforts built on changing understandings of microbial genetics in the immediate postwar period.

The influence of expansive international networks ostensibly concerned with microbial nomenclature had appreciable impacts on matters of taxonomy. This was coupled with the development of practical tools to manage more complex aspects of microbial culturing and identification, which resulted in an explosion of microbial data long before large-scale genetic investigations. In both of these, microbial culture collections were central, serving as suppliers and arbiters of necessary type culture strains, and centres of taxonomic research and authority. Just as Kristin Johnson has noted the centrality of museums and their collections in entomological taxonomic enterprises during the mid-twentieth century, so too did collections of microbes serve as critical pieces of infrastructure, shaping the organisation, purpose, and detail of microbial taxonomies (Johnson, 2022). Our account of microbial systematics

lays bare the protracted but also fundamental nature of these debates, which highlight why it matters that historians and philosophers of science continue to explore such discussions.

Why, though, did microbial systematists encounter such distinctive challenges during the post-war period? And why did microbiologists advocate so strongly that they should decouple their discipline from botany and zoology in matters of taxonomy and nomenclature? Without intending to resort to arbitrary distinctions, both more scientific and more social elements were central factors, and it is no exaggeration to say that microbial culture collections – and those responsible for them – were at the heart of such debates.

In the first place, it is obvious that as microbiologists began to uncover new means of differentiating species and subspecies from one another – successively through morphological, biochemical, and serological approaches – it led inevitably to controversies as to whether this should mean they should be categorised as distinct families, genera, species, and subspecies. How, exactly, should a bacterial species be delineated, with what threshold, and at what levels? Tellingly, systematists working elsewhere in biology did not seem keen to get involved in the problematic areas of microbial taxonomy. Set against these successive findings was the international microbiological community which faced several challenges.

First, the field of bacteriology itself had stemmed largely from totemic figures who had little to no interest in classification. As was repeated in their highly influential 1946 edition of *Topley and Wilson's Principles of Bacteriology and Immunity*, 'a clever and positive microscopist, who told Pasteur in very cautious language that a certain organism which he had taken for a coccus was in reality a very small bacillus, was much astonished to hear him [Pasteur] reply: "If you only knew how little difference that makes to me!"' (Wilson and Miles, 1946, p. 9). Against this backdrop – and a deep-seated veneration of the foundational figures of their discipline and the field's practical accomplishments – microbiologists struggled to make sense of precisely *how* to classify bacteria, particularly in an environment in which botanists and zoologists had seemingly clear, long-established principles for how to organise the objects of their interest, which did not appear to be appropriate to bacteria. We see here the origins of microbiology as primarily a practical field, largely disconnected from the priorities of systematists and evolutionary biologists in the period in question.

Second, the communities who took upon themselves the task of adjudicating matters of taxonomy, nomenclature and classification were deeply divided. While systems of nomenclature arguably mattered only to a small subsection of microbiologists, that community was outsized in its influence, and established vocal and influential channels of communication, embedded within transnational professional networks, and bolstered by a sense of officialdom by a Judicial Commission which had the authority to make official, "legal", pronouncements on issues of nomenclature (see, for example: Buchanan et al., 1951). Many of the leading figures were associated with prestigious culture collections, and this gave them privileged access to large repositories of bacterial strains and expertise in laboratory cultivation and identification. Broader taxonomy, meanwhile, was left largely untouched by such structures, and became a progressively messy and contentious affair. Into this, taxonomic heretics such as Cowan waded enthusiastically, arguing the folly of focusing on principles of naming before organisation.

Finally, one feature of latent microbial classification controversy which cannot perhaps be so readily explained stems simply from the relatively small number of individuals involved and their entrenched views. While the concept of the so-called Systematics Wars from the mid-1960s to the 1980s has received recent and compelling criticism in relation to botany and zoology, it is no exaggeration to note the occurrence of such conflict within the community of microbiologists in the immediate postwar period (Hull, 1988; Sterner and Lidgard, 2017). Numerous first-hand sources attest to this fractious environment. Attempts to create agreed international standards in both taxonomy and nomenclature did not reach a neat solution, despite the wide

³⁴ P. Wildy, 'E2/2a President's Progress Report', 1 April 1968, International Commission for the Nomenclature of Viruses: Minutes, GC/168/D/1/1, Wellcome Collection. Wildy was later centrally involved in the establishment of the UK's Neuropathogenesis Unit, and connected to a number of other important scientific infrastructure initiatives during the 1980s (Anon, 2000).

acknowledgement of a decades-long deficiency in those areas. A more complete understanding of microbial systematics in this period is similarly, therefore, long-overdue. Such attention will yield important insights into how microbes themselves were conceptualised, how microbiologists positioned their discipline, and how classification continued to be seen as a crucial legitimising enterprise of scientific domains and the natural world.

CRedit authorship contribution statement

James F. Stark: Writing – review & editing, Writing – original draft, Supervision, Resources, Methodology, Investigation, Funding acquisition, Conceptualization. **Isobel Newby:** Writing – review & editing, Investigation.

Declaration of competing interest

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No data was used for the research described in the article.

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