Identification keys and the natural method: the development of text-based information management tools in botany in the long 18th century

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ABSTRACT

Botany was undergoing an information overload crisis in the late 17th century. By the beginnings of the 18th century, there were over 18,000 described kinds of plants – far too many for anyone to memorize. Botanists coped by shifting to text-based ways of organizing information about plants called "systems" or "methods."

Initially, botanists produced arrangements of plants meant to be simultaneously as congruent with nature as possible and as easy to use for plant identification. Over time, they saw that it would be more efficient to switch to using separate schemes optimized to perform each of these functions. These schemes grew into the natural method, reflective of plants' relationships, and identification keys, which are easier to use. Proponents of the two technologies split into rival camps. A mismatch between theory and practice in the 18th century hampered meaningful discussion about classification until the turn of the 19th century. At that time, when botanical instruction in France surged in the wake of the French Revolution, botany teachers combined the natural method and identification keys with alphabetical indexes to create the format of the modern field guide. This innovation was not accepted in Great Britain for another generation due to cultural and political factors.

During the long 18th century, botanists had to overcome many technological and psychological obstacles, including the high cost of illustrating texts, difficulties in obtaining

complete specimens, a lack of terminological and other standards, and prejudices against textual features that enhance readers' ability to search for information. Stumbling blocks preventing a balanced historical evaluation of the origin of botanical classifications, such as the reading back of disciplinary boundaries into the past, misunderstandings of terminology, and philosophical prejudices about what is important in history, are also examined. The work ends with a discussion of the multiple invention of a classificatory technology and its implications for the history of science.

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Introduction

Up to now, there has been no standard history of data management in pre-Darwinian natural history. Historians of biology, for instance, Ernst Mayr, David Allen, James Larsen and Frans Stafleu, have examine the history of botany in the long 18th century with a focus on the naturalists who produced discoveries now seen as the most relevant for modern science.[1] For instance, a typical such history starts with herbalists, moves on to late 17th century botanists John Ray, Joseph Pitton de Tournefort and perhaps Robert Morison, all striving to find the natural method, moving on to 18th century classification-oriented botanists, particularly Linnaeus (though sometimes with natural method seekers Michel Adanson and Antoine-Laurent de Jussieu as well), before switching focus to zoology. These histories enter the 19th century emphasizing Georges Cuvier's program of animal anatomy and how it was carried into Darwin's day by Richard Owen. Historians of botany, especially botanists writing about their predecessors, have been most interested in examining botany as the science of plants, that is, living organisms with a physiology, anatomy and ecology so different from our own. Historians of systematics in particular, such as Henri Daudin and Peter F. Stevens, focussed on the idea of continuity in nature and, in Stevens' case, contributions of Jussieu and his "natural method" to the development of taxonomy. Historians of a sociological bent, such as Lorraine Datson, and Emma Spary and Ann Shteir, have looked at economic botany, examining the interplay of colonies and colonizers, patronage relationships among botanists, women as botanists, and the teaching of botany to children. Professionalization of natural history in the 19th century drew the attention of Paul Farber and David Allen. Book history in this period has also received consideration in works by Sachiko Kusukawa, David Allen, Ann Blair, and many others. In addition, there have been many masterful (if intrinsically hagiographic) biographies of individual botanists produced, from Charles E. Raven's biography of John Ray, to D. J. Mabberley's

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[1] Historians generally use the term "long 18th century" to refer to the period approximately from the 1680s to the late 1820s due to the relative cultural uniformity of this pre-industrial revolutionary period. This time span corresponds to the time period I cover in this dissertation. For brevity's sake I will use the phrase "18th century" when referring to the long 18th century throughout this work.

biography of Robert Brown, to William Stearn's biography of John Lindley. More recently, historians of botany, such as Staffan Müller-Wille and Susan McMahon, have joined forces with historian of zoology Polly Winsor and several others to re-examine common assumptions about the philosophical frameworks that pre-Darwinian naturalists employed when they described plants and came up with plant classifications. Many other historians of biology have written excellent books and articles on the development of the science of botany and on its major expression in the 18^{th} century, botanical classifications. But until now, the history of where those classifications came from, what they were intended to do, how they changed over time and what they meant to the botanists who wrote them and used them has been neglected. These information management strategies, however, were of critical importance to how botany developed over the course of the 18th century. This work is my effort to tell their story, the story of how identification keys, the natural method, and field guides originated.

In the late 1990s, I was an undergraduate student specializing in botany. I really enjoyed classes on vascular plant morphology, plant families, and systematic botany. But there was one thing I could not stand, and that was using keys to identify plants. The keys never seemed to work properly. Specimens sometimes did not seem to have the features that were required to identify them definitively. Descriptive phrases about anatomical features of plants may have seemed clear to whoever wrote the keys, but they were opaque to me. I would go down blind alleys in the keys and get stuck there. Whenever that happened, I had to start over because there was no way to trace back to where I went wrong. Even with five years of Latin courses under my belt, I found the technical terms for plant parts confusing. The majority of my classmates were even more confused than I was. How come, I wondered, at the dawn of the 21^{st} century, we were still using this dinosaur of a technology? Who were the sadists who invented it? How did it become so pervasive in botany? Wasn't there anything better available?

If the inventors of keys were sadists, I suppose that makes me a masochist, because I have spent my entire Ph D researching these very questions.

The history of keys, the natural method, and field guides starts in the late 17^{th} century, at the origin of taxonomy as we know it. At that point, botanists had been struggling for decades to keep track of information about the increasing numbers of known plants. There were far too many plants to memorize – more plants, in fact, than any other kind of living thing thought to exist. And the number of plants thought to be unknown to science was even greater. Botanists had been writing down plant descriptions for centuries, but without any standard names for the plants or efficient ways to find information about them in books, it all seemed next to hopeless.

But a number of ideas about how to address these problems had been proposed. The first, beginning in the early 17^{th} century, concerned making better plant names.

The extents to which nomenclature should reflect classification and/or should be descriptive were recurring themes. The descriptive Latin phrases used to name plants were seen to be unsatisfactory for a number of reasons. They were too long, they were inconsistent, there were too many synonyms, etc. Several men experimented with various ideas to replace them. The new names were based on the principles used by authors of philosophical universal languages. Often, the same men were involved in both projects.

The new nomenclatures ranged from numerical "naming" systems associated with charts of plants to combinations of letters standing for coded descriptions of plant features. These were designed to eliminate the need for reference texts and to prevent synonyms from arising. Such schemes achieved their greatest prominence in the late 17^{th} century. Although they were shown to be ineffective, isolated botanists of many different backgrounds continued to come up with the same ideas well into the 19^{th} century. (I discuss the story of the multiple invention of these schemes in the final section of Chapter 6). Plant names only began to stabilize when botanists began to adopt binomial nomenclature on a large scale, that is, after Linnaeus began to popularize it in the mid-18^{th} century.

Simultaneous with late 17^{th}-century experiments in nomenclature, both old and new techniques for finding information in books started to be used for botanical purposes for the first time. Arrangements of material by subject, indexes, and tables of contents began to appear in botanical works that had heretofore been arranged alphabetically or according to orders used by ancient authors. The use of hierarchies as information-finding devices made the leap from books about concepts to books about organisms. This increase in the use of text navigation devices gradually reduced botanists' reliance on the memorization of plant names. Knowing all the names of plants was acknowledged to be far-fetched by the mid-17th century anyway, since over 6,000 kinds had been described by 1623. Instead, botanists shifted toward memorizing the names and details only of plants of interest to them. They used texts to look up unfamiliar ones. For many decades, however, textual arrangements of information about plants were regarded more as crutches for weak memories than as tools to extend memory's reach beyond what the fittest could do unassisted. This was just the first hint that a rift between the theory and practice of organizing information about plants was about to expand.

John Ray's contemporaries worked through these ideas with their universal language schemes. They left him in a position to react to the worst of their efforts and build on the best of them. Ray ended up producing the botanical work now seen as the first effort towards building a "natural method," a technique for grouping plants together according to how similar they are to each other.

This is not to say that Ray's method was perfect. It, and all efforts to follow, were all subject to a number of constraints that held them back from achieving the goals that Ray had in mind when he was writing his. Chief among these was the desire to produce a work that would enable anyone to identify plants easily and quickly, without needing a master's guidance. Ray wanted his book to be illustrated, comprehensive, detailed enough to describe plants specifically and unambiguously, and also inexpensive enough for students to purchase. Ideally the book would also be portable, to take into the field – or at least it would outline a simple way to organize plants that could be memorized and taken into the field in one's head. Many of these goals were contradictory. Ray and his successors struggled to produce botanical works that could meet as many of them as possible. Out of these struggles, two camps emerged.

One camp prioritized organizing plants according to how similar they were to each other. This approach became known as the "natural method." The natural method was supposed to keep related plants together. Doing so relied on background knowledge of what features were important indicators of relatedness among different kinds of plants, such as morphology, medicinal (chemical) properties, habitat and ecology. This made it notoriously difficult for beginners to use. At the same time, competence with the natural method was prestigious. The natural method was furthermore seen as the only framework that would eventually allow generation after generation of botanists to put all the plants in the world in their proper places.

The other camp focussed on identifying plants quickly and easily. The techniques they used became known pejoratively as "artificial" because related plants were not always kept together in such books. Dissimilar plants were sometimes printed alongside each other as well, so long as the way that the information was presented would make plant identification as simple as possible – particularly in local floras and for small-scale taxonomic analyses. Artificial techniques were often simple in design, making them memorable and easy to learn. They could also be structured so as to walk people unfamiliar with plants through the identification process, helping to yield the names of specimens without assuming the breadth or depth of background knowledge of botany that the natural method required. Unsurprisingly, these features made them popular with botanical novices.

Throughout the 18th century, followers of each of these two camps were widely perceived to be bitter rivals even though both sides made concessions to the other's methodologies: natural methods often had artificial components to facilitate plant identification, while artificial methods were seen as better the more natural groups of plants they could preserve.

Something unusual started to happen in post-revolutionary France, however. After four years of neglect during which education stagnated in France, it was decreed in October 1795 that an école supérieur was to be opened in every department of the Republic. Natural history -- a large part of which was botanical – became a mandatory subject.[2] Demand for botanical instruction surged. Teachers of botany found that using different books for different purposes helped their students learn about plants better than sticking to only one method permitted. They began to refer students to artificial methods to look up names of plants, but to books written using the natural method for information about how plants were related. Some enterprising instructors even compiled both kinds of method into one book, to reduce the cost to students and to enhance portability. The addition of alphabetical indexes to this mixture meant that the origin of field guides was complete.

Field guides

This story makes the development of field guides sound simple. In reality, there were many obstacles to overcome, both technical and psychological. The biggest practical obstacle was the expense of printing accurate illustrations. While the technology to create superbly detailed copperplate illustrations had existed since the 15th century,[3] it was far too expensive to use to illustrate more than a handful of plants per publication. The majority of botanical works designed for use in the field or studio (rather than as "coffee table books" for the rich and idle) were composed almost entirely of text. Since a picture is worth a thousand words, myriad technical terms were invented to describe all parts of plants in minute and precise detail. Glossaries grew up to explain these words to the uninitiated. Etymological arguments were invoked for the sake of making information about plants easier to learn and easier to find.

The major psychological stumbling block botanists had to get over was the notion that classifications that are true to nature present information in the best possible way. It followed that arrangements of plants done well should not require indexes, tables of contents, or any other aid to finding information within them. Anyone who included finding aids in his publication was admitting that he was not a good enough botanist to see the God-given patterns in nature.

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[2] Williams 1953, pp. 314, 316.
[3] Kusukawa 2000b, p. 90.

Historiographical myths and obstacles

There are also several pernicious historiographical myths about botany in the 18th and 19th century that need to be addressed in order to make sense of what was happening. These myths also help explain why so few historians of botany have devoted time to this fascinating subject. Understanding how these myths have pervaded the history of botany also accounts for why the development of identification keys was so slow compared to that of the natural method, and why the British clung to Linnaeus's sexual system of botany into the 1830s. These two situations were not fully explained for by previous histories of botany, even by helpful re-evaluations of past events. For instance, recent emphasis on the spread of the idea that pre-Darwinian naturalists were "essentialist" has shown how that terminology was used to forward a political agenda in 20th-century systematics while muddying our understanding of 18th- and 19th-century systematic practice. The re-evaluation of the role of essentialism in the history of systematics has added much to our understanding of how naturalists identified natural groups of organisms but precious little to that of these other two situations.

Essentialism

As Mary P. Winsor, Staffan Müller-Wille and several others have shown, a number of 20th-century historians of biology with their own political agendas have read Aristotelian and Platonic logic into the practices of 18th- and 19th-century naturalists, where it does not belong.[4] To quote Mary Winsor, "Much of the literature relating essentialism to systematics is seriously flawed by the failure to separate ontology and epistemology."[5] The "essentialism story" has Linnaeus and most if not all other pre-Darwinian naturalists searching for Platonic essences and exact logical definitions of groups of living things when they were constructing their classification schemes. They were described as what Popper would have called "methodological essentialists."[6] After Darwin

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[4] E. g. Winsor 2001, Winsor 2003, Winsor 2004, , Winsor 2006, Müller-Wille 1999 and Müller-Wille 2005, .
[5] Winsor 2003, p. 389.
[6] Winsor 2003, p. 388.

propounded the theory of evolution by natural selection, the "essentialism story" goes, this incorrect, static view of living things could no longer obtain among naturalists. Darwin single-handedly rescued taxonomy from "2,000 years of stasis."[7] So long as everyone agreed that pre-Darwinian naturalists' ideas followed this Aristotelian/Platonic pattern, there was no reason to investigate what Linnaeus or Jussieu really thought they were doing. Any ontological statements in favour of essentialism were taken to be in support of a methodologically essentialist program consisting of the classification of living things by "logical division." Classic examples of books and articles expounding this story include Mayr , Cain and Ereshefsky , though it is pervasive in almost all cursory treatments of the history of botany, including articles about the lives of 18th and early 19th century botanists and in botanical textbooks.

As M. P. Winsor has shown, however, pre-Darwinian naturalists formed natural groups of organisms by methods of "chaining," and "exemplars," not by logical division. Their polythetic species definitions and failures to revise generic characters when anomalous species were added likewise point to a prioritization of the maintenance of natural groups over any logical agenda.

Nevertheless, despite these advances in our understanding, the re-examination of the role of essentialism in the methodology of pre-Darwinian naturalists can explain only part of why so many different classifications were produced in the 18th and early 19th centuries, and why naturalists had so many – often inconsistent – ideas about them. It does not explain at all why the dominant ideas about classification changed over this time period, or were different from place to place or person to person. It also does not address why there were so few correspondences between naturalists' ontological commitments and their practically-oriented research programs.

A more complete explanation of these issues needs to take more than logic (or lack thereof) into account. As it turns out, pedagogical concerns, communication, and information storage and retrieval matters played much more significant roles than had previously been thought.

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[7] Hull 1965.

Dichotomy

Another point of linkage between botany and logic is the emphasis on the pedigree of dichotomously branching identification keys. Dichotomous divisions have long been used to divide up the world into mutually exclusive groups. Logicians employ them to exhaust all logical possibilities by phrasing choices as "A" versus "not-A." The fame of George Bentham as a botanist and as author of an influential logic textbook in the early 19th century adds to the appeal of this argument. But Bentham, as we will see, did not consider the kind of logic he wrote about to be applicable to botany. In addition, the first identification keys were neither logically exhaustive nor entirely dichotomous – though neither were the classifications of knowledge of the philosophical pedagogue Ramus or his followers, to which they have often been compared. In fact, there is only a loose historical linkage between Ramists and the development of identification keys. Instead, keys seem to have developed as a special kind of tables of contents honed over centuries of teaching practice into something more highly regarded for being a quick and easy way to identify plants than for being logical in the 20th and 21st century sense of the word. The connections between botany and logic are many, but tenuous at best.

Terminology

Finally, but perhaps most seriously, a number of terms used frequently by 18th- and 19th century botanists, in particular, "essential," "artificial," "natural," "system" and "method," have become associated with schools of thought and classificatory practices that they did not signify consistently in those days. Many historians have made the understandable mistake of taking botanists at face value when they defined these terms in the past. But past use was far from set in meaning and often associated with personal squabbles about how best to organize material about plants. To quote Adanson,

> From this diversity of opinions about [what are] the most essential parts of plants or of the fructification upon which to found a system come the disagreements of methodists [i.e. systematists], each praising the goodness of his own method, and

> regarding it as the most universal, or the easiest, or the one approaching nature the most, or even as the most natural.[8]

Nobody had a monopoly on any of these terms, or on being correct. It is necessary to examine the work of many botanists to determine who was in which camp and when, and to understand the implications of some of the terminology they used. In many cases, modern significations of terms or even significations preferred by particular famous individuals are not typical of how the majority of 18th century botanists used words.

Theory and practice

Modern ideas about what naturalists should (or should not) have been doing have also influenced how historians and philosophers of biology have approached this material. For instance, it has been customary to examine arrangements of living things considering the extent to which each one approaches what would *now* be considered a "natural" arrangement – keeping related plants together as much as possible, without intercalations of unrelated plants. The natural method itself is generally considered to be the precursor of what was recognized, after Darwin, to be the phylogenetic grouping of organisms. Ray, Adanson and Jussieu in particular have all been hailed as fathers of the natural method because all three prioritized keeping similar plants together. Historians of science are trained to be on the alert against "Whiggish" ideas such as these, yet it is far more common to see them expressed in print than one would expect. As a result, when modern scholars emphasize the philosophical approaches that helped or hindered naturalists with this task, the production and popularity of deliberately "artificial" arrangements look strange. They are considered impediments to progress, puzzling anomalies, or both. In particular, several historians of biology have wondered why Linnaeus's sexual system remained the dominant plant classification in Britain after Jussieu's natural method was published.[9]

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[8] Adanson 1966, p. xcvii.
[9] E.g. Allen 1976, p. 31, Mabberley 1985, p. 174.

As I will show, however, this attitude is itself a product of the long process of differentiation of identification keys and the natural method. During this period, name-calling and straw man building, along with the idealization of predecessors thought to embody a progressive spirit, were as prevalent as was anti-essentialist rhetoric after the Modern Synthesis. Artificial systems were demonized so that botanists could define the natural method more sharply as the most important goal of their discipline. Along the way, the real history of classificatory arrangements became obscured by a veil of self-congratulatory, present-focussed history. The majority of botanists brought up in this climate no longer paid attention to the real benefits that artificial methods brought to science.

An additional problem for modern historians is that, in the 18th- and 19th- century intellectual climate, different uses of terminology and different philosophical approaches did not map directly to classificatory practice.[10] Philosophically inconsistent behaviours and idiosyncratic uses of terminology abounded. And the most perplexing aspects of information management and classification turn out to rely less on how theory influences practice than on how practical problems limit possibilities. For instance, the common 18th century belief that the natural method would make acquiring knowledge about plants faster and easier for the very reason that its structure reflected real patterns in nature became untenable in practice long before it became unfashionable in principle, as the French teachers' actions showed.

Furthermore, as 20th century botanist V. H. Heywood pointed out, historians of systematics, in their "preoccupation with the development of the sciences of botany and zoology [in the post-Linnaean period]. . . diverted attention from the role of taxonomy as an information science" even though "this change in the nature of taxonomy from a local or limited folk communication system and later a codified folk taxonomy to a formal system of information science marked a watershed in the history of biology."[11] This watershed involved adapting classifications of plants from locally

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[10] As M. P. Winsor has repeatedly shown regarding 20th-century assertions that pre-Darwinian naturalists were "essentialists."

[11] Heywood 1985, pp. 11, 12.

important classifications based on ecological, utilitarian and morphological criteria into a globally understood association of standardized terminology, nomenclature, storage of specimens and layout. The entire apparatus was capable of allowing for the identification of hundreds of thousands of plants both known and yet to be discovered. The international community of botanists developed these mechanisms iteratively throughout the 18th century. While some of the botanists' desiderata remained to be fulfilled at the end of this period – for instance, accurate and up-to-date dictionaries of synonyms were still wanting – the technologies they had been refining for centuries certainly yielded better results at plant identification by the 1830s and even 1780s than they had at the beginning of the 18th century, in John Ray's time.

Disciplinary boundaries

Addressing these methodological issues required botanists to draw on expertise that now falls under different disciplines: linguistics, information studies, computer science, cognitive science, and mathematics, to name a few. But especially in the early 18th century, men of letters often dealt with these concerns on their own and all at once. Nomenclatural practices were not codified as they are now. Botanists did not have the option of outsourcing their information management to the computer guy down the hall, or even of purchasing a filing cabinet full of standard-sized index cards. Instead they learned from their predecessors, tweaked those techniques, and outright innovated their way to creating workable solutions to their communications and information management needs. Just as in plant physiology, anatomy, or any other aspect of botany, people contributed to the development of their field to different extents. Men who are now remembered as able botanists, such as W. J. Hooker, often did not add to the organization of botanical information, whereas authors who are best forgotten for their botanical blunders, such as John Hill, sometimes came up with important new ideas of this kind. Other botanists, already widely recognized for their research, such as John Ray, Jean-Baptiste Lamarck, Augustin-Pyramus de Candolle and John Lindley, assume roles of greater importance when those who contributed to the development of information management techniques get their due. A number of great botanical teachers, such as the Lestiboudois clan and F. A. N. Dubois, also gain recognition for the significant roles their pedagogical contributions played in the organization of botanical information. A focus only on botanical research as it is now understood would have missed their valuable gifts to the science.

As the altered emphasis on these historical figures shows, investigating the development of information management strategies and their interplay with botanical classifications requires a broader approach than has been taken until now. Purely chronological accounts, the traditional way to approach the history of botany, have not succeeded at untangling the mess of practices and ideologies invented, neglected, and re-adopted again and again during the 18th century. In particular, a purely chronological account of who did what and when would not do justice to the epistemological questions raised by multiple reinventions of the same technology, such as that of the combinatorial plant names I mentioned above. Botanists' understandings of "artificial" and "natural" classifications, for example, also varied more from person to person than from place to place or decade to decade for much of the 18th century. The iterative nature of change in systematic arrangements makes it simpler to examine one topic and its iterations at a time than to jump from topic to topic covering one generation of naturalists, only to repeat the process for the next. I have therefore found it most convenient to trace lineages of ideas and how they played out and interacted over the time period I cover. Instead of an "archaeology of ideas," indulge me in allowing me to call it a "historical ecology of ideas" as I follow particular strands through time, and note how they weave together and splay apart.

In order to make sense of the twists that eventually led to a separation of so-called "natural" and "artificial" classifications of living things, it is also necessary to use implements that lie outside of a historian's normal toolkit. Historians of science may find this practice unusual, but I found it necessary because the historical actors whose work I have examined frequently did not explain what they were doing. I cannot describe what the actors were doing in their own words when they used "natural" to describe their own work and "artificial" to describe their rivals', regardless of what each of them did in practice. Even those who explained themselves lacked a common vocabulary for the technical aspects of what they did. To paraphrase Adanson above, everyone claimed to have modified his scheme to make it "better." In order to characterize "better" in a clear and meaningful way, I have imported some analytical strategies and vocabulary from modern disciplines that specialize in the nitty gritty problems of information management. In this age of electronic databases and journals that 18th- and 19th-century botanists could only have dreamed of, I am doing my part toward making the research I have conducted useful not only to historians of botany, but to researchers in other disciplines likely to find it interesting. By using terms already in wide circulation, I hope to improve access to my findings by enabling anyone searching for these terms to find what they seek.

I will leave the final word to Geoffrey Bowker, communications guru and aspiring historian of science. He once wrote that

> Information infrastructures like classification systems are embedded both in work practice and in technological media. Their history cannot be told independently of the work practice that they constitute or the media that they are inscribed in.... In order to do historical justice to the development of information infrastructures, one must move between stories that historians traditionally tell of people and places and things and stories that are generally left untold of the woof and warp of the canvas on which historical dramas are painted.[12]

I hope that you will enjoy reading about the history of botanical information infrastructures as much as I have enjoyed the last six years I have spent researching and writing about them.

[12] Bowker 1996, p. 59.

Part 1. Identification keys, the natural method and the origin of field guides

Chapter 1. 17th century: too many plants

Emergence of taxonomy as a goal of natural history

Empirical science and information overload

> *Overload is more than the existence of very large amounts of information, enormous accumulations of publications, larger and larger data bases. Rather, it is a gap between what one can do and what one wants to do, a gap between what one can do and what one thinks one should do with existing information.*[13]

The development of formal ways of classifying living things is directly linked to the difficulty of remembering too much information. In the late 17th century, a number of factors came together such that there was no longer any hope that naturalists could learn everything they wished to recall by heart. They devised text-based data management techniques to help them keep track of information, both for their own purposes and in order to instruct others.

Reference texts "designed to be used, rather than read," had been developed by theological scholars late in the 12th century. They became common in the 13th century, and continued to increase in popularity in a variety of intellectual spheres as a response to a perceived "multitude of books" during the middle ages.[14] It has been suggested that the use of indexes and alphabetically arranged reference texts was indicative of "a significant change in attitude toward written authority" since these devices catered to readers interested in finding particular information at will. They were no longer content to assimilate a work's contents as a unit or necessarily in the order that its parts were presented to them.[15] But this

[13] Wilson 1996, p. 22.
[14] Blair 2003, p. 12.
[15] Rouse and Rouse 1991, p. 221. Library catalogues that indicated the location of texts in a library or in a region (rather than just whether a given book was in or out) first appeared also in the 13th century, and by the 14th century some universities' libraries' books were organized on the shelves and catalogued according to their

transition in botanical publications was gradual.

In the late 15th century, people writing about plants mainly tried to identify those described by the ancients. When this project proved increasingly difficult by the 1530s, they largely "shifted from identification to description" in a quest to find out exactly what those ancient authors, such as Dioscorides, were talking about. In the next few decades, the numbers of physicians and others interested in botany proliferated. They travelled through Europe, creating an explosion in the number of books about new kinds of plants, let alone the plants themselves.[16]

The compilation of information about plants soon became part of the Baconian enterprise of empirical science. The written word became recognized as a scientific tool. The changing use of the phrase "artificial memory" in the early 17th century illustrates this development. Until this time the term had referred to mnemonic tricks to boost one's "natural memory." These included visualizing items to be memorized in different locations of a room, or cosmological encyclopaedias arranged so as to supposedly aid memorization. Francis Bacon and René Descartes both asserted, however, that writing itself is a form of artificial memory.[17] Furthermore, Bacon wrote,

> Believing that the natural powers of the memory (without the help of ordered tables) are sufficient in the interpretation of nature ... is like maintaining that a man without the aid of writing can solve the calculations of a book of ephemerides.... a sound aid to memory can be of the greatest use even in the ancient and popular sciences.[18]

Bacon furthered his project of encouraging an empirical approach to understanding nature by acknowledging limits to human memory and saying outright that it is not necessary to store everything one knows in one's head in order to make use of it. It was perfectly proper to compile information as text to be dealt with later. Brian Ogilvie's research on botanical practice in the Renaissance has even shown that botanists grew "increasingly distrustful of

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subject matter (Rouse and Rouse 1991, pp. 226, 239), extending the grasp of readers interested in finding choice biblical quotations or commentators' passages on specific topics.

16 Ogilvie 2003, p. 30.

17 Rossi 2000, pp. 15, 23, 44, 124.

18 Bacon (Works, vol. I, p. 647), cited Rossi 2000, p. 106.

memory" and eventually became "deeply suspicious of memory" because they felt overwhelmed by the "number of plants they had to remember, and the number of characters used to differentiate them."[19] This heritage was such that until the middle of the 17th century, the emphasis in natural history was more on compiling information in "herbaria, illustrations, and careful field notes ... to prolong ... experience" than on putting it into a structured whole.[20] Figuring out relationships among organisms could wait.[21]

While there was no one standard way to organize botanical texts during this period, several traditional formats of arranging them existed. Some 16th century botanical works were, for instance, arranged similarly to ancient authors' texts. Mathias de L'Obel's *Plantarum seu stirpium historia* (1576) and the botanical section in *Rerum medicarum Novae Hispaniae thesaurus* (originally written prior to 1630, possibly as early as 1588)[22] follow the order used by Dioscorides,[23] while Caspar (Gaspard) Bauhin (1550-1624) arranged his *Pinax theatrum botanici* (Basilae Helvet: Ludovici Regis, 1623), a giant index of plant names, according to the plan followed by Theophrastus, only in reverse.[24] *Materia medica* were sometimes arranged "from head to heel," according to the diseases of the parts of the body the plant-derived medicines were supposed to treat.[25] Alphabetical herbals and *material medica* were also common from the ninth century on.[26] For instance, Peter Schöffer's *Herbarius* (1484), Otto von Brunfels' *Herbarium vivae eicones* (1530), Conrad Gessner's *Historia plantarum* (1541), and William Turner's *New herball* (1551) were all arranged alphabetically.[27] Nevertheless, the state of botany in those days was often described as "chaos." For despite even Bauhin's monumental efforts to bring some order to the

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[19] Ogilvie 2006, p. 181. Susan McMahon disagrees with Brian Ogilvie and me on this point (McMahon 2002, p. 8).
[20] Ogilvie 2006, p. 181.
[21] McMahon 2002, p. 2.
[22] de Renzi 2000, pp. 154-155.
[23] Hernández 1651, p. 3, L'Obel 1576. L'Obel does not anywhere explain the order that he used.
[24] Cain 1994, p. 346.
[25] MacKinney 1938, p. 243.
[26] MacKinney 1938, p. 250.
[27] Wellisch 1978, pp. 83-84, 91, 94.

multitude of botanical books, none of these arrangements was very useful for putting a name to a plant specimen.

Exponential increase in numbers of species

By the late 17th century, the numbers of known species of plants and animals were increasing exponentially. The rapid increase in the number of known plants at that time has been fingered by many authors as responsible for increased attention to arrangements of plants conducive to their identification.[28] There certainly were other factors at play in the stabilization of classification as "proper protocol for natural history ... in the late 17th century," for instance, a large community of naturalists with a common interest in plant identification in order to give proper credit to species identifiers.[29] The rate of increase of knowledge of new species, however, seems to have played an important part, in that anyone attempting to organise information about the natural world would have to take into account not only the increasingly large numbers of plants and animals known to exist, but also those that were expected to be discovered. By the turn of the 18th century, these numbers were so large as to be considered evidence of God's power in and of themselves.

Reasons for enumerating or estimating the total number of species

What drove 17th- and 18th-century naturalists to estimate the total number of species on the earth? They may have just felt a desire to count species, or had the idea that things that could be known should be enumerated so that they could better be investigated. They could also have taken inspiration from the Biblical story of Adam naming the animals as a step to acquiring power over them. More appropriate to the late 17th century, however, was the approach taken in John Graunt's *Natural and Political Observations Made upon the Bills of Mortality* (1662). Graunt's work was predicated on the idea that if information about

[28] Candolle 1813, pp. 38-39, McMahon 2002, p. 8 (she provides several other references), Mayr 1982, p. 158and Grandjean de Fouchy 1763, p. 53.
[29] McMahon 2001, p. 262, McMahon 2002, pp. 2, 9-10.

human populations could be quantified, trade could be better understood, conflict between nations and business partners would be reduced, and prosperity would follow.[30] This popular and influential work went through four printings within 5 years of its appearance, and earned its author – a shopkeeper of humble origins – membership in the prestigious Royal Society.[31] In this work, Graunt calculated the probable population of London, including the probable size of an army which could be raised – both of which were unknown at the time – from the numbers of christenings per year. He obtained this figure from the bills of mortality, combining it with some assumptions about the fecundity of women, the ratios of women of childbearing age to those too old or too young to have children, and the ratio of men to women within the population.[32] This kind of extrapolation was common in 17th-century accounting practices, but Graunt was apparently the first to apply it systematically to the study of human populations.[33] The same kind of reasoning was subsequently used by John Wilkins, F. R. S. in his *Essay towards a real character* (1668) to calculate the space required to house and feed all of the known species of "Beasts" and birds on Noah's ark. Wilkins concluded that the ark could have held enough animals to account for their present-day diversity. Enumerating the "innumerable," according to Jaap Maat, who has studied the *Essay* in detail, was Wilkins' way of bringing the natural world down to size so that it would conform to the biblical account.[34] And proving the truth of the biblical account was of paramount importance to the religious Wilkins.

But, to paraphrase Shakespeare, it turned out that there were more things in heaven and earth than there were dreamt of in Wilkins' philosophy. By 1691, his colleague John Ray was using species estimates to achieve the opposite purpose: he used the sheer

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30 Rusnock 2002, p. 15.
31 Rusnock 2002, p. 25.
32 Rusnock 2002, p. 33.
33 Rusnock 2002, p. 28.
34 Maat 2004, pp. 211-212 citing Wilkins 1668 p. 162.

magnitude of the number of species to demonstrate God's greatness.[35] Ray reckoned the number of described beasts to be around 150, and the number of birds "known and described" near 500, but he also believed that at least one third more species in both groups still existed and had not yet been described.[36] Ray estimated the numbers of known and existing species of many other kinds of animals, assuming that the ratios of animal species to plant species are constant everywhere on earth. That is, he reasoned,

> Supposing then, there be a thousand several sorts of Insects in this Island [Britain] and the Sea near it: if the same Proportion holds between the Insects native of England, and those of the rest of the World, as doth between Plants Domestick and Exotick, (that is, as I guess, near a Decuple) the *Species* of Insects in the whole Earth (Land and Water) will amount to 10 000, and I do believe they rather exceed than fall short of that sum....
>
> What can we infer from all this? If the number of Creatures be so exceeding great, how great nay immense must needs be the Power and Wisdom of him who form'd them all![37]

The more living things Ray catalogued, the greater his testament to God's glory. This belief was echoed by Thomas Sprat, who wrote in his *History of the Royal Society* (1722) that the classification of natural bodies as a way to admire God's power would lead to "the utmost perfection of *human Nature*."[38] These Englishmen were not alone in their beliefs. The Dutchman Pieter van Musschenbroek also included a section on the estimated numbers of kinds of living things in his *Oratio de sapientia divina*, given as an address upon his retirement from teaching at Leyden in 1744.

Pieter [Petrus] van Musschenbroek (1692-1761) was the son of the famous scientific instrument maker, Johan von Musschenbroek of Leyden. He himself became well-known as the inventor of the Leyden jar. Although he primarily taught physics and philosophy, he also lectured occasionally on zoological and botanical topics – not entirely surprising, given that he studied under the renowned physician and botanist Hermann Boerhaave (1668-1738).[39] In

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[35] For more about Wilkins' and Ray's personal relations, and differences in approach to dealing with the large numbers of known organisms, plants in particular, see Chapter 1.
[36] Ray 1691, 2 ed., p. 5.
[37] Ray 1691, 2 ed., pp. 7-8.
[38] Sprat 1722, 3 ed., p. 110.
[39] Rooseboom 1970, pp. 177-178.

turn, "He had much influence on students of many nationalities at three universities for more than forty years."[40] Although van Musschenbroek did not appear to be actively engaged in the study of living things himself, he was widely read in related topics and brought them to the attention of many others.

In the aforementioned *Oratio,* von Musschenbroek, following Ray, attempted to estimate the numbers of species of animals in order to demonstrate God's greatness. He based his estimates on information from experts on many different animal groups, and included the enumerations of many small animals described with the aid of microscopy in the years since the *Wisdom of God* had come out, such as foraminifera, small molluscs and echinoderms, mites, and many microorganisms, in addition to the birds, beasts, insects, and other groups mentioned by Ray.[41] To calculate the estimated number of animal species on earth, von Musschenbroek first added up all the estimates of animals other than insects. To this sum he added the estimated number of insects, which he derived from multiplying Boerhaave's estimated 13,000 species of plants by five, assuming that five species of insect live on each plant. Adding the insects to the other animals, he then multiplied the total by two, for "each animal serves another species as food." Estimating the number of unknown species of animals to be between two and ten times the number of known species yielded between 300,000 and 1,500,000 species of animals – mind-bogglingly enormous numbers for their time, all the better to illustrate God's power.[42]

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[40] Rooseboom 1970, p. 190.

[41] Rooseboom says that the experts whose works he consulted were "Gesner, Aldrovandi and Willughby (birds); Rumphius, Valentin, Richter and Lesser (horns [sic – ?] and shells); Bianchi, Dandridge, Derham, Ray, Réaumur, Lesser, Frisch, Trembley, Lyonet, Willughby, Redi, Klein, Swammerdam, Baker, Heister, Sachs, Leeuwenhoek, Joblot, Hartsoeker and others." She cites only a few of the specific works in question in a note. The organisms studied are mentioned in the titles of several of these works.

[42] Rooseboom 1970, pp. 180-182.

Ray's natural theology-motivated species estimates also influenced Reverend William Kirby and William Spence in their *Introduction to Entomology*, in which they not only cite him, but use the same methodology of estimating the number of insects based on the numbers of plants (which were better known) coupled with a knowledge of insects' feeding habits (Kirby and Spence 1828, 5 ed., vol. 4, pp. 488-491).

Cataloguing living things as a research tool

Not everyone involved in cataloguing or estimating the diversity of life necessarily thought about it in the same terms. Many may have been driven just as much by their desire to collect things for the sake of collecting, or to become connected to a community of like-minded hobbyists as a social activity as they were in the natural theological project of amassing information about living things to glorify God.[43] Aesthetics or curiosity about patterns in nature likely played just as big a role for many collectors as did religious motives, personal or national pride, or economics. Collecting and cataloguing activities at a local level augmented the efforts of political entities eager to boast of new discoveries at home and in their colonies, providing evidence of the richness of their earthly realms. For all of these people, estimating the numbers of organisms yet to be collected could inform choices about where and in which groups to look next for novelties.[44]

In addition to collecting efforts, the numbers of described species were also boosted by efforts to separate "nondescripts" – plants that had not yet been described by anyone – from plants previously described. This was a major concern,[45] especially in an intellectual climate in which clarifying the priority of discoveries was gaining in importance.[46] Not only did work on identifying plants lead to the discovery of new plants and the separation of different plants that had once been seen as belonging together, but the need to identify plants

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[43] Reeds 1976, p. 521.

[44] In fact, Ray wrote in the preface to Willughby's *Ornithology* that Willughby had agreed to the posthumous publication of his incomplete work on the grounds that it would

> conduce somewhat to, first, the illustration of God's glory by exciting men to take notice of and admire his infinite power and wisdom, displaying themselves in the creation of so many species and animals; and secondly, to the assistance of those who addict themselves to this most pleasant and no less useful part of philosophy; and, thirdly, also the honour of our nation in making it appear that no part of real knowledge is wholly balked and neglected by us (Raven 1986, 2 ed., pp. 309, citing Ray's unpaginated preface to Ornithology).

Ray's aim in this, as in his other natural history publications, was the easy identification of organisms. He wrote that he was engaged in "accurately describing each kind and observing their characteristic and distinctive notes, [so] that the Reader might be sure of our meaning and upon comparing any bird with our description not fail of discerning whether it be the described or no" (Raven 1986, 2 ed., pp. 320, citing Ray's unpaginated preface to Ornithology).

[45] Wotton 1694, p. 156 and Tournefort 1694, p. 3, cited McMahon 2002, pp. 10-11; Gray 1821, pp. v-vi.

[46] McMahon 2001, p. 262.

methodically become more pressing the more plant descriptions that had to be sorted through. This situation meant that classification at the turn of the 18th century "became an economical technology for the identification of new species."[47] Classifications that facilitated plant identification were valued not just for teaching purposes, but because they cut down on the work required to figure out whether something had been described before, when, and by whom.

Concomitant increase in kinds of information to track

The information naturalists were feeling overwhelmed with was not just about new taxa. New information about known taxa was also being recorded, such as information about the behaviour of animals, the growing conditions of plants, where they were found, what they did in different seasons, what they looked like in different parts of their life cycles, and so on. No one book contained all that information, even though various authors made efforts to provide all-inclusive texts.[48] This meant that the information that botanical authors had to keep track of was not just about plants *per se*, but also about particular descriptions and description writers, and where appropriate illustrations could be found. It was not unusual for botanists in the 18th and early 19th centuries to list the works of their predecessors in order to facilitate finding descriptions. As Alphonse-Pyramus de Candolle wrote, "up to the present time there have been about as many [classificatory] methods as there have been botanists. Their number is so great and their principles so diverse that, just to list them, it is necessary to classify the classifications themselves."[49] Linnaeus' classification of botanists in his *Bibliotheca botanica* 1736, for instance, was intended to help people find information. Naturally, this necessary tool was also a convenient way of staking a political claim. Linnaeus was sure to draw attention to how true to nature his own sexual system was. He

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[47] McMahon 2002, p. 10.
[48] E.g. Conrad Gessner, in his posthumous work on plants, and John Ray (as was clear in his private letters, though he was diffident about his ability to produce it to his high standards). Pavord 2005, p. 285, Ray 1848, pp. 139, 145, 155, etc.
[49] Candolle 1813, pp. 25-26.

also categorized botanists who based their classifications on variations in leaves or other parts of plants, or used alphabetical order, as "heterodox," and subdivided them into categories with silly names.[50]

As mentioned earlier, botanists were also well aware that whatever schemes they devised would need to accommodate many more plants than were already known to science. Though exactly how many plants existed, or what their features would be, were matters of conjecture, one thing was certain: "a method," as Colin Milne wrote in 1770, "to be universal, ought to accommodate itself no less to future discoveries than to those already made."[51] These same considerations were at the forefront of one of John Ray's friends when he wrote to Ray, the "reviver and restorer of method,"[52]

> I confess I judge it a very difficult matter to lay down such principles of method as will comprehend even the species of plants already known, and far less those that are yet undiscovered, or that will not be subject to change or admit of alteration; for I find, by the few observations that I made this year, that a plant must be viewed in all the seasons of it before one can venture to give a true and exact character of it; and it is not one single observation that is sufficient to constitute the character of a plant, for that may escape our sight or memory at one time which we may discover at another.... [and] how many new species have been discovered of late years both in East and West Indies never [383] known in the world before, and how many lie hid to this day?[53]

When he received that letter, Ray had finished but not yet published his *Historia plantarum*, an attempt at a "natural method," in which groups of plants were put together based on many features that they shared. Ray hoped that such a system would be capable of being amended to accommodate all possible plants, no matter how partial our understanding of their features.

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[50] Linnaeus 1736. See also Stafleu 1971, p. 41, for a partial English translation of the same categories used in later works of Linnaeus.
[51] Milne 1771, pp. 123-124.
[52] "Method" here has the standard 17th century sense of a technique for organizing material so as to make it easier to learn.
[53] Letter from Preston to Ray, Jan 13, 1701 (Ray 1848, pp. 382-383.

Iterations of improvements

That Ray included the ability for his proposal to be expanded and amended is important. The more refinement that "natural" groupings of plants undergo, the more uniform in composition they become. Anomalous plants get moved to their own groups, leaving only plants similar to each other behind. The more similar the plants in each group are to each other, the easier it is for people to remember which plants belong there. As well, the more that plants within each group are alike, the fewer descriptions of exceptional plants will need to be added.

But just how classificatory schemes were to be amended was a contentious issue. It brings us to one of the major storylines weaving throughout the history of botany, that of natural and artificial classifications. I will deal with these terms at length. For now, a brief summary should suffice to set the scene.

In short, in the early 18th century, people wanted classifications that they could use to do everything. They wanted classifications help them determine plants' relationships, identify them quickly and unambiguously, indicate areas in need of further research, and even hint at plants that were yet to be discovered. As appealing as all-purposes classifications are in principle, in practice they were impossible to make. Botanists split into two rival camps based not so much on what they wanted to achieve – a greater understanding of plants – but on how they tried to achieve it. The "artificial" camp prioritized the identification of specimens. The "natural" camp prioritized keeping groups of plants that looked similar together. Highlighting the relationships among plants, as determined mostly by morphological similarity, was their guiding principle when structuring their classificatory schemes.

"Natural method" – relationships as organizing principle

While the folk classifications of plants used the world over before and instead of "scientific" classifications employ ecological features, morphological and cultural salience, size, utility, etc. in grouping plants, the "natural method" is not so catholic in its organizing principles. Instead, it groups plants similar to each other in many ways without reference to how salient or useful they are. The natural method became more important the more information there was to organize.[54] It was explicitly adopted among botanists in the late 17th century. These botanists assumed that the relationships they saw among plants had a real existence in nature, independent of who was investigating them, that they were stable, and that even more of them could be perceptible by human investigators – if only they paid enough attention to detail. Those who followed this "natural method" could contribute piecemeal to the larger whole, which they, as Ray before them, recognized to be too complex for any one person to master.[55]

But problems remained. Though the one true way that nature was organized was thought to be stable, different investigators' knowledge of the relationships of which it was composed changed frequently. Unlike post-Darwinian views of the relationships among organisms, which are constrained by our knowledge of evolutionary history (phylogenetic lineages) to somewhat reticulating treelike patterns, pre-Darwinian naturalists had no such preconceptions of how God might have chosen to arrange his creations. In effect, they had far more freedom than we now do to arrange natural productions. They could take whatever patterns they thought likely and use those arrangements as hypotheses about how the world really was. These botanists and zoologists could see that groups of similar organisms, such as natural families like the lilies, roses, ferns, and so on, existed in nature, but they had no agreed-upon or even fully elucidated theories to account for why this was so. The result was confusion as multiple authors published one method or system after another, each of which

[54] Atran 1981, pp. 47-54.
[55] See, e.g. Linnaeus 2004 (not published), p. 8 to this effect.

had features supposedly more in tune with nature than the author's previous efforts, or those of his rivals. In addition to the "chaos" of plant names, every new publication unleashed new technical terms, new conceptions of relationships, new plants and new properties of plants on the increasingly weary botanophilic public. The "conceptual framework" of the natural method was unstable. It required those who wished to use it to constantly re-evaluate the relevance of information they already knew in light of every discovery – a contributing factor to information overload.[56] The sustained effort required to track the moving target of a natural system was tiring for botanical enthusiasts. For novices, it was often not worth the effort.

Identification keys – ease of identification foremost

In parallel with the development of the natural method, the first glimmer of identification keys also appeared on the botanical horizon. Like the natural method, key origins are manifold and the technology did not appear overnight. Because there are so many formats of identification keys, it is much easier to use one than to describe what one is. Most importantly, all identification keys are interactive ways of leading someone to the name of a specimen through a process of elimination. Keys work much like a game of 20 Questions. But since the "game" is text-based rather than oral, layout is important to their functionality.[57] A formal definition of an identification key could be: an algorithm, phrased as descriptive questions or statements, usually in the textual form of contrasting couplets, designed for ease of identification of particular items, say, plants. A key consists of a branching hierarchy of these questions or statements. At each branch point in the hierarchy, a user trying to identify a specimen must choose the description that best matches it from the descriptions on offer. The user is then directed down that "branch" to further descriptions. The process is repeated at each subsequent branch point in turn. This results in a process of

[56] Wilson 1996, pp. 22-23.
[57] Much as in a flow chart.

elimination that narrows down the description of the specimen. Finally, the reader reaches either a definitive identification, that is, the plant's name and perhaps additional information, or an impasse. Using a key in this way is known as "keying out" a specimen.

Note that identification keys do not need to be strictly dichotomous or strictly nesting in order to be effective at yielding the names of specimens. Three or more choices may be offered at any given branch point, and/or the same item may key out in more than one place, and/or branches of the hierarchy may "anastomose," and/or there may be cross-references in the key, all without compromising ease of identification.

A problem with defining identification keys is that it treats keylike structures themselves as if they fit neatly into categories, while really, some are not so easily categorized. In particular, the central criterion that a key be "designed for ease of identification" was often compromised by authors' preferences for printing similar-looking plants together in texts. As well, many early "keys" had a branching pattern but did not provide descriptions at each branch point to specify what features were to be contrasted. This made it unclear which branch to choose. Many hierarchies, both explicit and implied, suffer from one or more of these problems. A hierarchy of questions, descriptions, or groups of taxa in itself, therefore, should not be considered to be an identification key. A "true" identification key, in my opinion, must have some kind of apparatus to allow a user to key out specimens by a process of elimination based on the features that the specimen exhibits.

The realization of the importance of all of these features for quick and easy plant identification was long in coming. The natural method always received the lions' share of attention and approbation.

Proliferation of "artificial" "methods" and "systems"[58]

Although botanists saw that the natural method might bring stability in the long term, in the short term it proved to be frustratingly labile. It was, in fact, no easier to use than the

[58] In Chapter 5 I discuss the terms "method" and "system" and how they were used in greater detail.

profuse output of authors of so-called artificial systems or methods, who had, as Patrick Blair complained in 1720, "multiply'd *Methods* so fast in a short Time, that if *Botanick* Writers go on at this rate, e'er it be long they shall render *Plants* as unintelligible by *Method*, because of their great Plurality, as formerly it was to know the *Plants* without *Method*."[59] "For Method has of late Years," he continued,

> been so far multiply'd, the Plants so variously dispos'd, and the Authors of the several Distributions have had such Contests and Debates, [about] which ought to be preferr'd; that instead of informing, they have often led their Followers into the Errors they themselves had advanc'd, and encreased Faction and Division in a Science of it self so very innocent, that they have actually broke out into a Paper and Botanick War; by which an excellent Institution is perverted, and what was intended for its Welfare, is like to become its Ruine.[60]

These systems and methods were, to use an anachronistic expression, unhappy marriages of the natural method and keys. I will be discussing them at length. In the meantime, though, it can be seen from comments on the state of botany in the 17th and early 18th centuries that Blair's sentiments were widespread. Linnaeus described the state of botany prior to the publication of his own works in very similar terms. There were, he complained,

> So many controversies among authors! So many bad names! So much confusion! Such indeed was the state of things, that as often as a new System-maker arose, the whole Botanical world was thrown into panic. And I truly do not know if these System-makers produced more evil than good.[61]

Even botanists who tried to proceed empirically and did not promote any particular system of their own drew scorn from their colleagues. Adanson, for instance, was so focused on promoting the natural method that he saw any other way of arranging botanical information as an impediment to progress. He was particularly irritated by the 300 or so local catalogues of plants that came out between the end of the 16th century and the middle of the 18th century. These sloppy and unoriginal lists made botany "a vain science of names," Adanson wrote. "It is discouraging to have to burden the memory with a great number of names and phrases for the same plant, which each author named according to his whim." Many authors

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[59] Blair 1720, p. 72.
[60] Blair 1720, p. unpaginated preface.
[61] Linnaeus 2004 (not published), pp. 3, i.e. §8.

published all the names they could collate regardless of whether a name referred to several plants, or a plant had more than one name already in circulation. "This mania for catalogues," Adanson complained, "keeps growing to the point where it makes [me] fear that Botany will finally be smothered under the useless weight of these phrases, which are no more than a gangrenous crust of the science."[62] The treatment for this disease might be severe, but it would be necessary for the ailing patient to survive.

Florid metaphors aside, the abundance and variety of "methods" put forth, the great number of botanists agnostic about how plants should be arranged, and the great number of botanists slavishly following Tournefort's method or the sexual system were all symptomatic of a legitimate scientific problem. Everyone had a different idea of what constituted suitable groupings of plants. In the early 18th century there was no consensus on what constituted clear limits of either species or genera, or on the nomenclature for these groups. Authors without contact with well-known botanical experts or access to their books dealt with this problem in two different ways: they either went with their gut feelings and came up with new methods that never became popular, or they kowtowed to authority to the best of their abilities. The result was the nomenclatural confusion and system overload that well-connected botanists, such as Linnaeus and Adanson, hated so much.

Despite Adanson's grumbling, the uncertainty about nomenclature and generic limits was certainly reduced after Linnaeus's sexual system of botany became popular. His works made binomial nomenclature standard. Linnaeus also went out of his way to promote "natural" genera, which were also roughly the same from author to author and method to method. And well before Linnaeus, indexes in individual botanical works as well as indexes of synonyms, when well-constructed, allowed botanists to look up the same genera in different people's books even if they did not understand the fine points of each other's methodology.

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[62] Adanson 1966, pp. cxli-cxlii, clii-cliii.

All the same, relatively stable genera and indexes of synonyms did not solve the problem of managing the amount of information that botanists intended to corral. There were just too many species for the techniques that Linnaeus implemented to work smoothly. Botanists could not avoid balancing the elegant simplicity of their schemes with comprehensiveness and ease of implementation. But before they realized that these ideals could and often did conflict, they had to figure out how to handle many other, more pressing problems of classification.

What the exponential increase in numbers of species meant for taxonomy

It is often remarked that the number of known species was increasing exponentially throughout the 18th and 19th centuries.[63] Most of these assertions are based on anecdotal or fragmentary evidence. The exception is an article from 1953 that still has relatively few data points and dealt only with plants.[64] My more detailed examination of the numbers of known species and the numbers of species estimated to exist from 1620 to the present shows that previous commentators were correct. There were exponential increases. My data also shows that there were differences among the rates of increase for different taxa, and that the exponential increases began at different times. I will discuss these and other aspects of my findings in more detail in later sections. But first, let us analyze some hard data showing how these exponential increases occurred in different groups of organisms.

Exponential increases

For the past few years I have examined hundreds of natural history books and articles. I recorded every instance when an author mentioned the number of species or genera of plants, mammals, birds, fish, fungi, and insects described or estimated to exist at a

[63] E. g. McOuat 1996, p. 481, Atran 1987, p. 265, Shaw 1811, vol. 8, p. 12, Kirby 1802, vol. 1, p. xiv, Grandjean de Fouchy 1763, p. 53.
[64] Cailleux 1953.

certain point in time. I also counted the number of species of plants mentioned in several works of Linnaeus and those of several other authors purporting to record the totality of known species or genera in particular taxonomic groups. When authors mentioned the numbers of species or generá known to science or estimated to exist, I cited them as is. Otherwise, in books published in the 19th century and earlier, I counted the numbers of species or genera in the book by hand. Species or genera were often assigned numbers. This made my task easier. When species or genera were listed in an index, I counted index entries. I maintained distinctions in my records between species and genus counts and estimates, and among total plants, flowering plants, seed plants, cryptogams (including or excluding the fungi), fungi, birds, fish, mammals and insects.

Species and genus counts will always retain an element of uncertainty, owing to several factors. First among these are the differences in methodologies used to count published descriptions of taxa. Most published reports are not of actual counts of species such as I have used for old books with low numbers of plants. For instance, when André Cailleux examined the *Index Kewensis*, which lists flowering plant species and genera but does not provide any enumerations of them, he counted the average number of valid species and genus names described per page over a certain number of pages and then multiplied the average by the number of pages devoted to taxon descriptions. He did not use synonyms or the names of hybrids in his work.[65] Nevertheless, his estimates of the number of angiosperm species and genera are grossly inflated. (They form almost a straight line above all the other data points from 1885 to 1952 in Figure 5).

Many recent authors estimate both the number of species suspected to exist and the numbers of described species in various taxa using calculations that assume a constant rate of the description of new species.[66] Several studies discuss the merits and failings of this

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[65] Cailleux 1953, p. 45.

[66] E.g. Cailleux 1953, p. 47, Ainsworth & Bisby's Dictionary of the Fungi 2001, 9 ed., p. 359, Ainsworth, James and Hawksworth 1971, 6 ed., p. 405.

approach.[67] My own research shows that the rate of species description for the one taxon for which I have data – fish – to have been anything but steady. FishBase, an online resource for all fishy taxonomic information, provides a database that can be queried as to the number of species of fish now considered to be valid that were described every year since 1758.[68] Figure 1 shows the results.

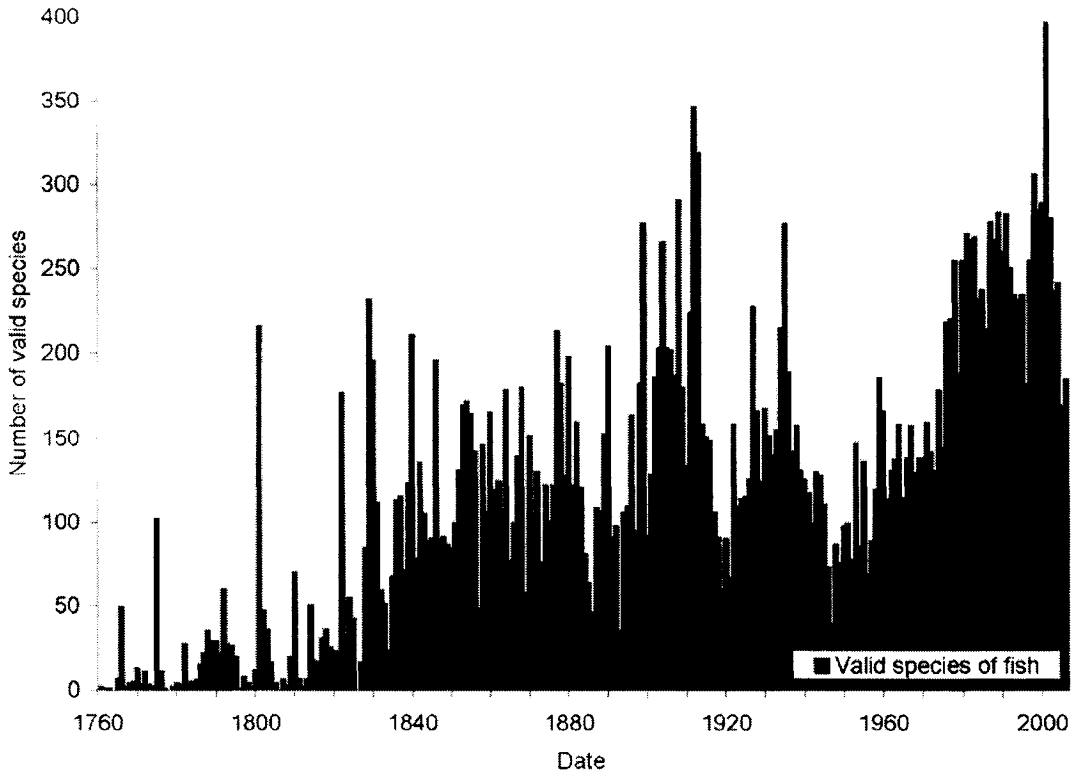

Figure 1. Number of valid species of fish described per year, 1758-2006. Notice the large dips in the numbers of species described during the World Wars.

Even though the number of taxa described in a given time period has fluctuated wildly, the number of fish species described in the past and now thought to be valid corresponds remarkably well to the number of fish species thought to be valid in the past, as shown in Figure 2. I assume that the relationships between the numbers of plant species described in the past and those now seen to be valid will show a similar correspondence as

[67] Alroy 2002, Stork 1988.
[68] Agbayani 2005.

they had done for fish, particularly because during the time period studied, there were more estimates made about numbers of plants than there were for fish.

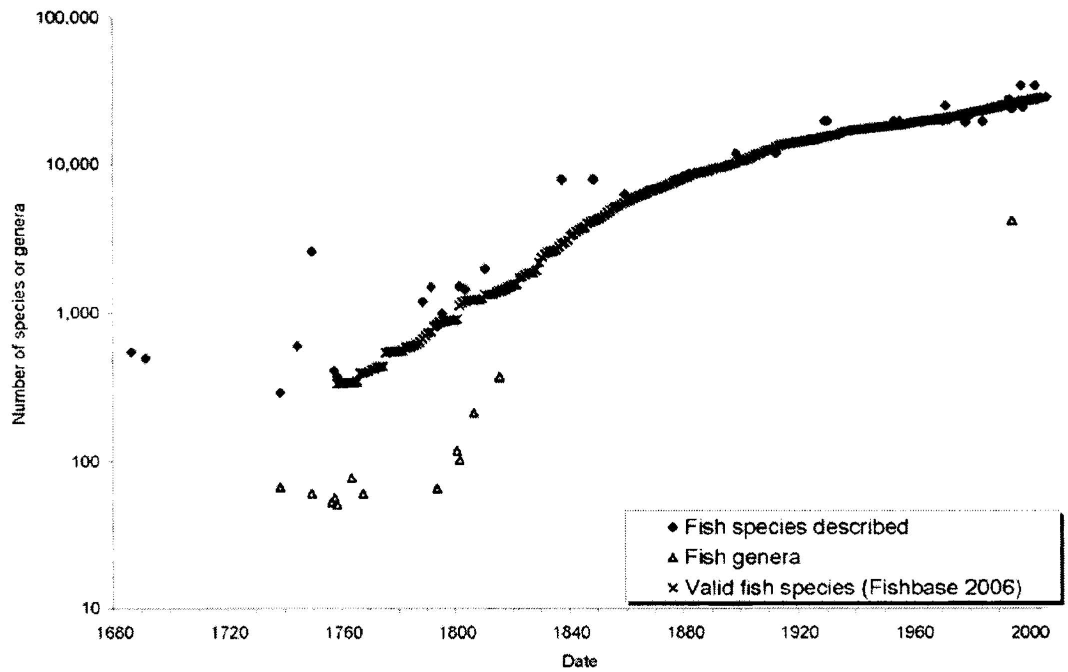

Figure 2. Logarithmic graph of numbers of species and genera of fish known to science during the years 1680-2006, compared with the number of species described each year from 1758 to 2006 that are now recognized as valid, according to FishBase (2006).

A number of other factors influence the numbers of species and genera recognized. These include difficulties in determining or agreeing upon taxon limits,[69] including changing ideas of what species (in particular) are,[70] the presence of synonymous names for the same taxa,[71] and the insufficiency of information available to authors of would-be comprehensive

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[69] Though Cailleux (Cailleux 1953, p. 43) wrote that the differences in species estimates between lumpers and splitters looking at the same geographic region is usually no more than 20%, he provided no supporting data for this assertion. Williamson (Williamson 1997, p. 3) suggests that we can expect a trend toward splitting in some taxa, such as birds, "as it is usually easier, for various reasons, to push for the conservation of a species than of a subspecies."

[70] May 1990, p. 301, discusses some of these problems in a modern context, but comparisons of what constituted a valid "species" in 1690 with what "species" meant in 1850 and today also raises the spectre of incommensurability. For many taxa, however, morphological features are still the hallmark in their taxonomy. While what may be called the "minimally genetically identifiably distinct taxon concept" has been gaining ground as of late, it seems to be just a variation on the old practice of splitting taxa when they get inconveniently large.

[71] For instance, Kirk et al. (Ainsworth & Bisby's Dictionary of the Fungi 2001, 9 ed., p. 359) wrote that

> amongst the approximately 320,000 names at species rank proposed for fungi, many have never been reassessed since their first description nor have they been adopted by modern

lists of taxa, including sampling bias.[72] Mnemonic considerations,[73] such as the desire for stability of taxon names,[74] and, in the case of genera, the tendency of monograph writers to split large genera so that each genus will contain only a manageable number of species, also play important roles.[75]

The changing circumscription of high-level taxa over time also introduces ambiguity into the data employed here. In the 12th edition of *Systema Naturae*, Linnaeus, for instance, included a number of what we would now unequivocally consider to be fish – puffer fish, box fish, rays, sharks, chimaeras, angler fish, seahorses and some others – in a separate category, "Nantes," which he put in the "Amphibia" along with reptiles and snakes. He also lumped spiders, crabs, scorpions, ticks, mites, centipedes, millipedes, and a few other creepy crawly organisms now not thought of as insects in with the insects.[76] In addition, although the distinctiveness of the fungi has been recognized for a long time, Linnaeus and many others after him still considered them to belong to the plant kingdom. He put them in the Order Cryptogamia. Their numbers of species and genera contributed to the total numbers in those respective groups.[77] I have made the judgment call of including the numbers of fungi in the numbers of cryptogams when authors placed them there. As for more modern treatment of the fungi, they sometimes include only the true fungi, and other times also

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> workers. Names in many accepted genera, especially some of the larger, have not been critically revised...
>
> In the absence of a world checklist of accepted fungi ... the possibility that as many as 100,000 or even 150,000 fungi are already described cannot be excluded.

Scotland and Wortley 2003, Govaerts 2003 and Alroy 2002 all discuss the extent of synonymy in late 20th and early 21st century estimates of the numbers of named species of seed plants and fossil vertebrates, respectively.

72 Walters addresses the issue of sampling bias not only in terms of which areas of the world have had their plants studied the most, but also suggests that higher numbers of genera in groups of greater salience, and more species per genus in those of lower salience, are recognized according to the culture of the people examining the plants (Walters 1986)

73 Stevens 1997b. I have not incorporated data on the number of "natural orders" (plant families) recognized, as their numbers tended to be much more "artificial" than either genus or species counts, often for mnemonic reasons. For instance, Jussieu limited the number of plant families to 100 in his *Genera Plantarum* (1789) (Walters 1986, pp. 532-533) while Bentham and Hooker limited the number of angiosperm families to 200 in their *Genera Plantarum* (1862-1883)(Stevens 1997a, p. 248).

74 Stevens 1997a, p. 247.

75 Stevens 1997a, p. 246.

76 Linnaeus 1766, 12 ed., vol. 1, p. 349 – not that "fish" is a monophyletic group, anyway!

77 E.g. Linnaeus 1754, "Editio quinta ab auctore reformata et aucta" ed., p. 483, considered the order of fungi to be equivalent to a class in distinctiveness, though he kept them as an order.

include other organisms dealt with by mycologists, such as the Oomycota. I have indicated in the reference notes which cases apply to which data points.

Despite these caveats, data about the number of taxa of each rank recognized or estimated to exist at different times are informative. They allow us to situate the naturalists working during those times in terms of their conceptions of the extent of biodiversity around them, how well they thought they were able to handle the influx of new information, and by what means.[78] They permit us to put the data management strategies that they adopted into the context of the ongoing sense of information explosion that they were experiencing, a feeling of being overwhelmed by the influx of new information that began centuries ago and continues today.[79] In particular, they let us see how utterly inadequate plant arrangements, and, later, arrangements of other taxa meant to be used without the assistance of text-based reference systems were becoming at the start of the 18th century. For instance, Figure 3 shows the number of species of all plants known to science (described) and the number of species of plants estimated to exist for different dates from 1620 to the present. There is a strong exponential increase in the number of described plants from the 1740s to the mid 19th century, in particular.

Results for all plants, flowering plants, mammals, birds, insects, and fungi

Figure 4 shows the same data set of species of described plants and the same time frame as in Figure 3, but this time compared with the number of genera of plants described throughout that period. The number of genera was also increasing exponentially throughout the time span measured. The exponential increase in the number of genera adheres even more closely to its trend line than the species do – rather predictably, as the delimitation of genera is expected to be more arbitrary than the delimitation of species. Notice that, until the last quarter of the 19th century, the number of genera remained around an order of magnitude

[78] Ogilvie 2003, pp. 33, 35, 36.

[79] Rosenberg 2003, p. 2. "It is strange to confront the urgency of ... sixteenth-, seventeenth-, and eighteenth-century projects to contain and to comprehend exploding worlds of knowledge. Equally strange is the persistence of the rhetoric of novelty that accompanies so old a phenomenon," Rosenberg wrote.

lower than the number of species. This is likely at least partly due to lumping tactics: new species were being assigned to old genera and not enough revision of genera was taking place to allow for splitting. After the mid-19th century, the exponential increase in the number of plant species described slowed relative to the exponential increase in the number of genera described. Were there more botanists active during this time? Were old genera being re-examined on a large scale? Was splitting very trendy during this period? It would be wonderful if Kew Gardens or another botanical source were to provide the same kind of detailed information about plants as is available for fish through FishBase. The information I have at present is too scanty to answer these and similar questions.

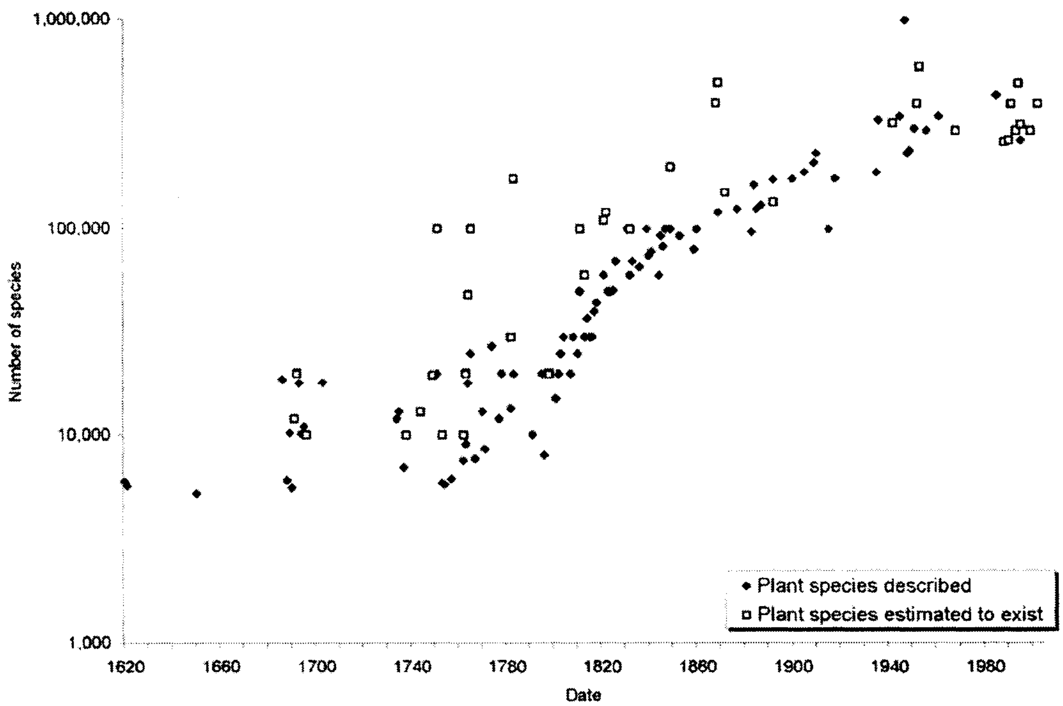

Figure 3. Logarithmic graph of numbers of plant species described and estimated to exist during the years 1620-2006.

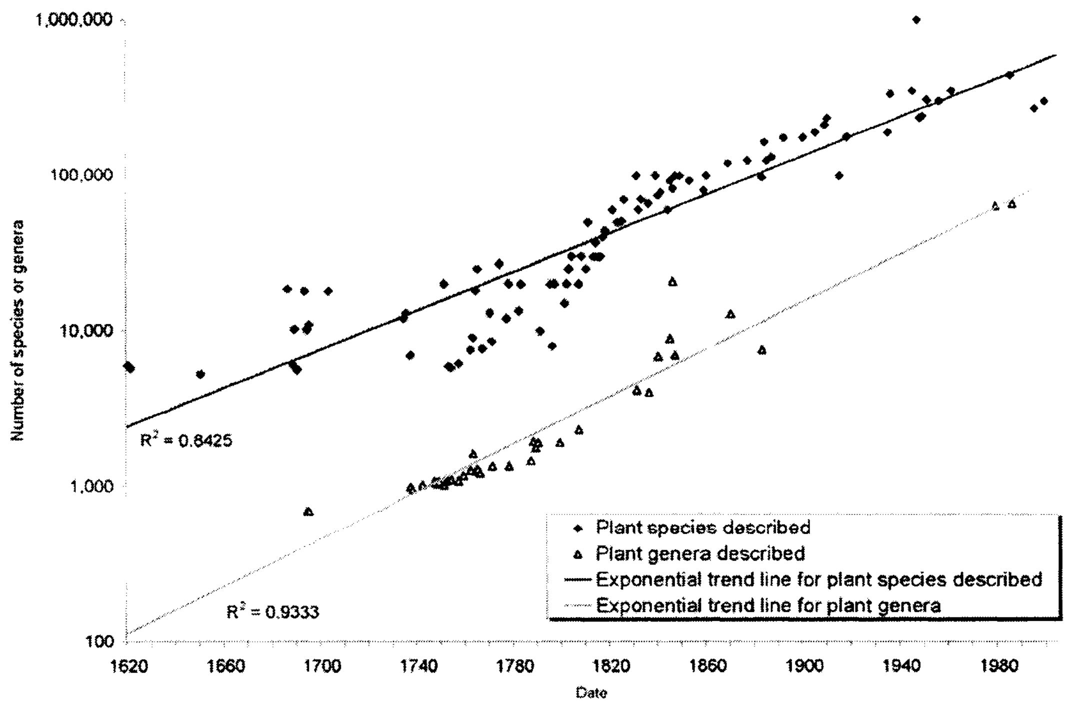

Figure 4. Logarithmic graph of numbers of species and genera of all plants known to science or estimated to exist during the years 1620-2006. Trend lines show a very good fit with an uncomplicated exponential growth pattern.

The numbers of flowering plant species and genera described and the number of flowering plant species estimated to exist, from 1740 to the present, is shown in Figure 5. Unlike the data for all "plants" shown in Figure 4, the description of flowering plants seems to have slowed significantly in the last hundred years. There were known to be many synonyms for flowering plants in circulation in the past two hundred years, as evidenced by lower numbers of flowering plants estimated to exist than numbers of flowering plants described during this time. This slowing trend is even more evident if Cailleux's data points for both species and genera are ignored. Botanists have every right to be confident that the vast majority of angiosperms have already been described.

Mammals are an even better-studied group than flowering plants. There are fewer of them, and they tend to be more obvious and sought out by greater numbers of people. The slackening trend in the description of new mammal species is even more evident in Figure 6 than it is for flowering plants in Figure 5.

Birds are perhaps the best-described group within the vertebrates. Ornithology attracts legions of amateurs as well as powerhouse theorists such as Ernst Mayr and even Jared Diamond. There are close to ten thousand species of birds described and ornithologists are pretty sure they have described all but a handful of them. In the past 60 years they have devoted a great deal of time to sorting out synonyms, which accounts for the large drop in the number of described species during this period, as shown in Figure 7.

Leaving those small groups behind, we can see a different kind of pattern in the two most speciose taxa of large eukaryotes, the insects and fungi. In both cases, the more species that became known, the more divergent and extreme became the estimates of the number of species that exist, as shown in Figures 8 and 9. But whereas naturalists have been estimating the number of insects that exist on earth since the 17th century, fungi did not inspire this sort

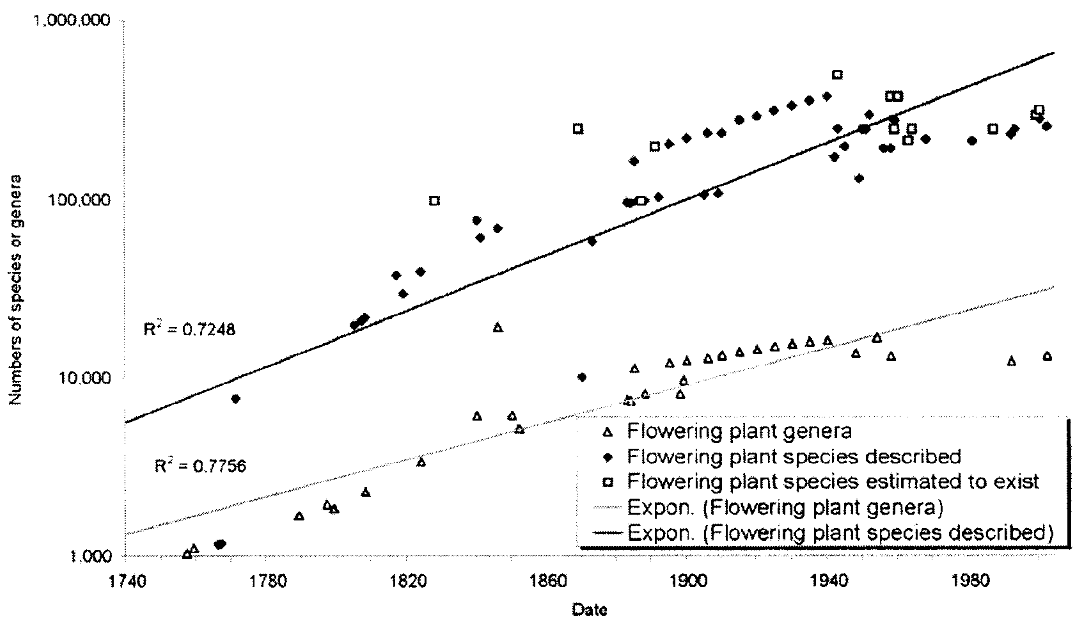

Figure 5. Logarithmic graph of numbers of flowering plant species both known and estimated to exist, compared with numbers of flowering plant genera described from 1740-2006.

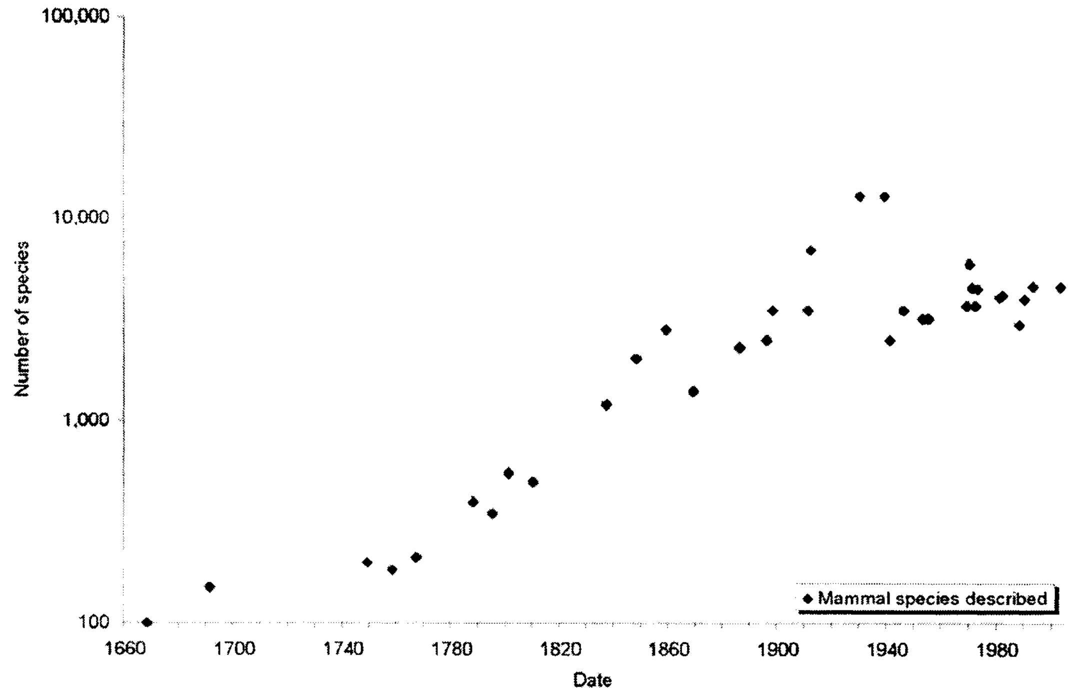

Figure 6. Logarithmic graph of numbers of species of mammals known to science from 1660 to 2006.

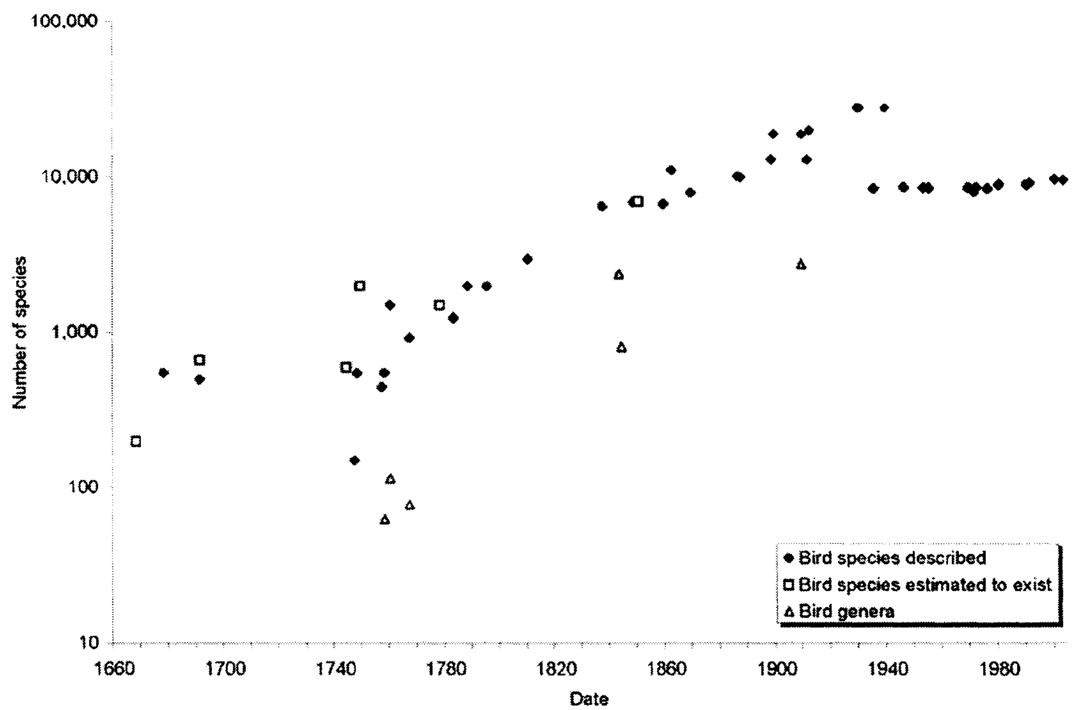

Figure 7. Logarithmic graph of numbers of species and genera of birds known to science and bird species estimated to exist from 1660 to 2006.

of curiosity in naturalists until the mid 19th century. And while it seems that entomologists today can agree on roughly how many species of insects they have already described, fungal taxonomy remains in such flux that mycologists are experiencing a level of disagreement about how many species they had already described comparable to the one in which botanists were mired in the 18th century. Now as then, new ideas about how to redo taxonomy and classification to make communication about species clearer are in the news. And, then as now, just because extended debates are raging about such theories does not mean that applying those theories in practice is any more feasible. Debates about nomenclature were particularly vibrant at the end of the 17th century.

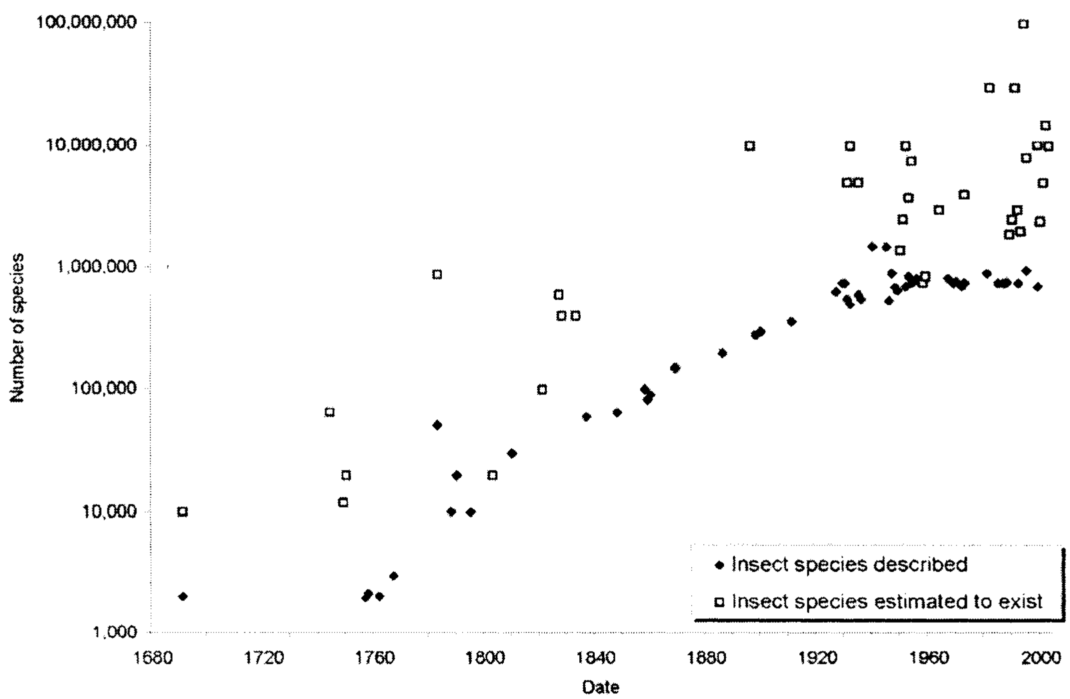

Figure 8. Logarithmic graph of numbers of species of insects known to science and estimated to exist from 1690 to 2006. Notice the difference in scale between this and the other figures.

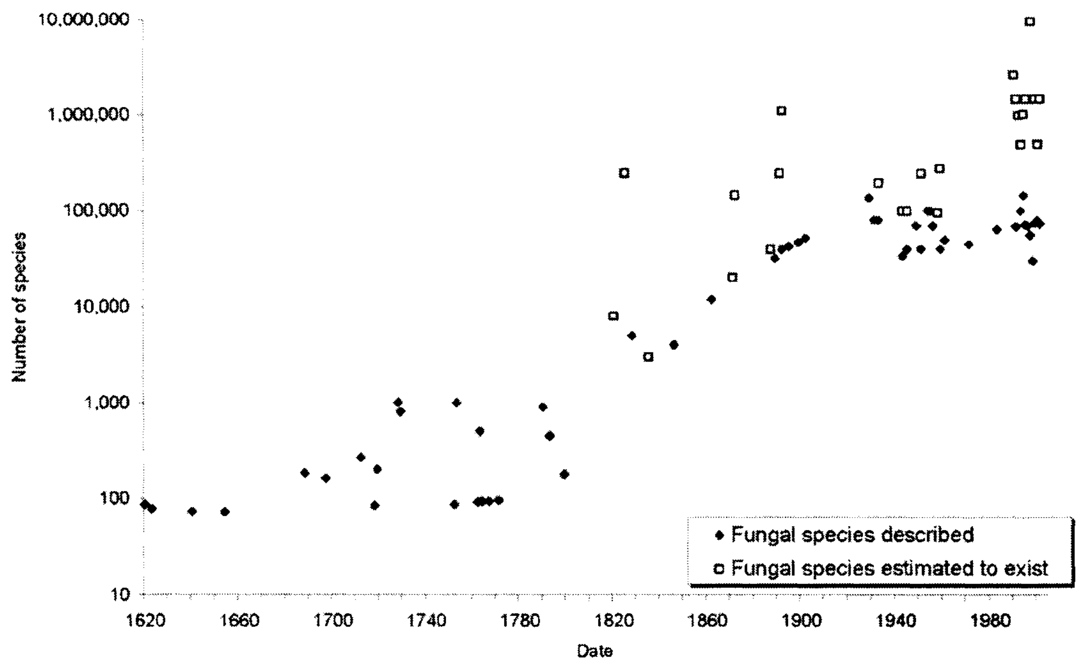

Figure 9. Logarithmic graph of numbers of species of fungi known to science and estimated to exist from 1620-2006. Notice the difference in scale between this and the other figures.

Better plant names: universal language schemes

Natural order should be easiest to remember and understand

In the 17th, 18th and even early 19th centuries, it was very common to believe that the human mind is primed to perceive nature as it is. Patterns in nature are sensed, remembered, and understood without any modification. Complex ideas are mere combinations of simple ideas. From all this it follows, first, that contact with disorderly subject matter leads to disordered perceptions of nature, but that information presented in its natural order will be easy to understand and remember. Second, it was theoretically possible to break any concept down into its constituent parts and understand it perfectly. These beliefs about the workings of the mind are part of the Ramean heritage and were echoed in later philosophers' work, such as that of Locke.[80] Third, a kind of corollary to the first two beliefs was the implication

[80] Ong 1958, pp. 184, 194, 203-204, 256.

that memory should know no limits. Though everyone was aware that humans are fallen, imperfect creatures, there were no preset limits to how many things a person could hold in his or her memory. (As far as I am aware, there were also no attempts made to quantify such limits). The result was the popular belief that the more natural a depiction of nature, the easier it should be to memorize and use, especially if the language involved were derived from first principles (simple ideas).[81] When problems with these hypotheses arose, they were blamed on human conventions and imperfections, such as laziness and insufficient education. There was no perception that there are inherent differences in the structures that work best for representing how things are "in nature," for describing how people perceive things, and for how they communicate these things to each other. We could eat our classificatory cake and have it too. Every feature of a scheme – reflecting the true relationships among living things, comprehensiveness, simplicity of structure leading to ease of memorization, ease of use, and ease of communication – could be optimized at once.

Naturalists only found out that there were problems with this view once they tried to develop schemes to depict and thereby understand nature based on these ideas. They soon saw that their schemes did not work as anticipated.

Specialized arrangements of plants

Ad-hoc schemes and non-alphabetical ordered schemes

Arrangements by part of the body treated by medicinal herbs, habitat, and other information external to a dead specimen work tolerably well for guiding readers to local or special-purpose knowledge of plants, particularly when the number of plants in the scheme is not large (on the order of hundreds). Ethnobiologists Scott Atran and Brent Berlin have shown that folk classifications like the European one from which these arrangements grew

[81] These ideas motivated the authors of numerous philosophical universal languages in the 17th century (Cram and Maat 2001, p. 50, Slaughter 1982, p. 113, Salmon 1966, p. 392, etc). Condillac also popularized the same ideas in the mid-18th century (Condillac 1746, pp. v. 1 p. 19, vol. 2 p. 240-246, 256-262, 282-289, Knowlson 1975). The influential zoologist Georges Cuvier and the writer Samuel Taylor Coleridge, among others, also promoted the same views in the early 19th century (Cuvier 1836, pp. 38-39, Coleridge 1818, pp. 13-14).

are used by peoples the world over.[82] But when the number of plants to be organized exceeds the bounds of common sense, or, in practical terms, what a person can keep in his or her memory, a more consistent mode of arrangement becomes necessary. Alphabetical arrangements of plants were one of the first such schemes to be used.

Ordered arrangements

I will discuss two kinds of ordered arrangements, alphabetical and hierarchical. Both of these arrangements list plants in a particular order. Although alphabetical and hierarchical arrangements are quite different in other respects, they both developed and rose to prominence in botany in a similar way. The history of the adoption of both alphabetical and hierarchical schemes helps explain the driving forces behind the increasingly elaborate classification schemes used in this discipline.

The slow adoption of complete alphabetisation and of hierarchical schemes suggests that the use of folk and *ad hoc* organization of plants is not a sign of laziness or confusion. To the contrary, why should anyone have bothered applying a consistent classification scheme if a rough guide were almost as functional, particularly if total systematization is costly to achieve in time and energy?

It is likely that the numbers of items in books or their indexes that needed to be sorted quickly had to become overwhelmingly large for those who had to deal with them before authors felt motivated to adhere strictly to a tedious system of reference. The balance of time invested in cataloguing something versus the amount of time required to find it must be tipped in favour of a consistent system by the sheer number of items to be searched through, and the necessity of sharing the use of the system with others. If the cataloguer is also the main user of the system, she can rely on mnemonic tricks that make sense only to her in order to remember where items are. There is little need to reorganize existing records into a new, more accommodating format unless other people, such as students, need to learn how

[82] Berlin 1992 and Atran 1990.

to use it. With ordered schemes, students do not need to remember where in a text any given plant is. They need be taught only how to look plants up. But again, putting plants into a predefined order takes time and energy. Historically, this investment was not initially seen to pay off.

Alphabetization

Alphabetical arrangements make no claims to represent the ways that plants are related to each other. They are arbitrary, for there is not necessarily any connection between a plant and its name other than convention. But they are also simple to use: anyone who knows how to spell a plant's name also has the immediate awareness of where in the arrangement to look for information about it. They can also accommodate unlimited numbers of plants, so long as the plants are all named.

Alphabetization first became common in the 13th century, but not in botanical texts. Instead, the focus of attention was index entries in theological works designed to speed up the composition of sermons. This alphabetisation, however, did not often extend past the first, second, or third letter of the words, even when such texts contained thousands of entries.[83] Irregularly and over time, alphabetisation was then extended in certain texts until it finally became customary and expected to order every index entry word according to every letter in it.[84] Hieronymus Bock's *New Kreutter Buch* (1539), Conrad Gessner's *Historia plantarum* (1541), and Leonhart Fuchs' *De historia stirpium* (1542) were pioneering botanical productions in that their entries were all in strict alphabetical order.[85] Complete alphabetisation in alphabetically arranged herbals or in indexes to works of natural history did not become normal until the late 16th century. "From then onwards," historian of indexing Hans Wellisch remarked, "most authors of herbals, zoological treatises and other works on natural history sought to emulate the example set by Conrad Gessner," who was

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[83] Rouse and Rouse 1991, pp. 226, 240-241.
[84] MacKinney 1938, p. 259.
[85] Wellisch 1978, pp. 94, 96.

quite influential.[86] The only two strangely alphabetised indexes I have found in a natural history work after this time were first, in Adanson's *Familles des plantes* (1763), which follows the author's program of spelling reform. In Adanson's work, words beginning with an H (which is silent in French) were listed according to their initial sound, e.g. "hAller" under A, and words beginning with I, HI, or Y were put together in their own section.[87] The second unusual index was appended to Nathaniel Wolf's *Genera plantarum vocabulis characteristicis definita* (1776). Wolf's index, though published in 1780, is alphabetized to the first letter or two only, and appears to have been made in great haste with no editing whatsoever.[88] For instance, part of the section under M runs:

Mugwort
Muda
Muduan
Mudusa
Muffle de veau
Mukuna
Mughettus
Mugvet
Muchomor
Mugo
Muitis
Müntze
Münchskappe
Mulberry
Mullein
Münchusia....

Decent alphabetical indexes in botanical works were in demand, though. They were often advertised on the covers of books, especially in subtitles.[89]

When dealing with a large number of plants, however, synonyms are inevitable. Each one necessitates a cross-reference, and many cross-references make a work unwieldy. On the other hand, a lack of cross-references to synonyms when plants are known by several names can make it impossible to find information on a given plant. A number of polyglot dictionaries of plant names published in the 16th century attempted to address these

[86] Wellisch 1978, p. 97.
[87] Adanson 1966.
[88] Wolf 1776.
[89] E.g. Ray 1660 and Forsyth 1794.

problems.[90] But no matter how well-executed, alphabetical arrangements are poor at handling new species that lack common names. There is no obvious place where entries on such plants belong.[91] Finding places for unnamed plants became a pressing problem in botany at the end of the 17th century, when botanists felt overwhelmed by the thousands of undescribed plants that were coming to their attention.

Beginnings of hierarchical schemes for finding information[92]

Hierarchical schemes based on features inherent in plants helped solve this problem. A plant new to science can be slotted away in a hierarchical classification easily and quickly so long as the divisions in the classification are based on features that the plant has. Prior knowledge of plants' names or their relationships to each other are not necessary. Someone identifying a plant need only examine the plant's features to know where to look in the classification to find further information.

Hierarchical arrangements also offered the advantage of parsimony to botanists. Nested categories in a hierarchy allow descriptions to be compacted: features common to subgroups need be listed only once in the group that contains them.[93] This feature of hierarchical arrangements is also beneficial to the memory. Scott Atran elaborated on this idea in his discussion of the origins of the natural genus and family concepts. Both the grouping of species into genera (standardized by Tournefort in the early 1700s) and the grouping of genera into families (standardized by Linnaeus in the 1740s and 1750s) came about so as to reduce the number of plants a botanist would need to retain in memory at a time.[94] Even better, not only were natural groups useful for saving mental work, space and breath in descriptions, but they also appear to be inherently easy for people to recognize.[95]

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[90] Slaughter 1982, p. 75.
[91] Slaughter 1982, p. 56.
[92] See also the section on dichotomy in Chapter 6.
[93] Peter Stevens explained that Jussieu liked this particular aspect of organizing genera into natural families (Stevens 1994, p. 45).
[94] Atran 1990, pp. 165-167. As described below, Magnol introduced the terminology of a plant family as a group with a shared general likeness in 1689, though at the time, there was no real distinction between genus

Although Linnaeus is the most famous author of a hierarchical botanical arrangement, he did not come up with this structure in a vacuum. Before him, a number of other botanical authors had been experimenting with the format, notably John Ray and Joseph Pitton de Tournefort. And botanical texts featured a number of "messy," somewhat hierarchical structures before strict hierarchies became common and expected as a way to present information. These somewhat diagrammatic, somewhat hierarchical arrangements – usually printed as nested sets of curly bracketed categories at the beginning of books or chapters – served as tables of contents for the prose sections that followed. (The *Phytosophicarum tabularum* of Prince Federico Cesi, however, consisted *entirely* of curly bracket tables).[96] Actually, these textual features were next to useless for anyone but their authors for identifying plants. Their organization was terribly inconsistent. For instance, the order of the subjects treated in the introductory diagram often did not correspond to the order or even divisions of subjects treated in subsequent passages.[97]

Mathias de L'Obel, for instance, included curly bracket summaries at the beginning of most groups of plants in his *Nova stirpium adversaria* (1576), without explaining what they were for or how to use them to navigate the text.[98] Though the material was treated in the same order in the summary and prose sections, the plants were sometimes discussed

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and family. Scott Atran and other ethnobiologists are emphatic that folk-groupings of plants existed long before the scientific concepts of genus and family had stabilized. The names and approximate dates of the formal establishment of genera and families in scientific taxonomy are therefore not to be taken as absolute.

[95] Atran 1990, pp. 24, 40, 256, 257. Atran points out that plant families can have members of different "life forms," unlike animal families, where all of the members tend to have more in common in terms of general shape and size. For instance, plants in the pea family (legumes) can be small tender herbs or giant trees. Yet, he argues, people in diverse cultures recognize the relatedness of these plants even though they differ in appearance. He calls these recognized but unnamed groupings "covert taxa" and argues that we are evolutionarily primed to recognize them (Atran 1990, pp. 11, 33, 41, 191, Atran 1983, pp. 1, 54).

Note that both Stevens (previous note) and Atran emphasize the "stackability" of *natural* families and *natural* genera. The same benefits seem to hold true *to a lesser extent* for less natural, strict inclusion class hierarchies: we are not able to recognize them by gestalt, although they are in principle more "logical." Still, some historians and philosophers of biology have placed emphasis on botanical hierarchies obeying a class logic (i.e. having strict inclusion rules), whereas in reality, botanical classifications almost all featured a messy combination of natural taxa and oft-broken strict inclusion rules. I discuss these issues in more detail below.

[96]Caesius 1651, . Much of the subject matter enclosed in the curly brackets was in paragraph form, laid out in geometrical patterns. The arrangement of text in this work is more decorative than logical.

[97] The same is true of Bergeret's *Phytonomatotechnie* vol. III (Bergeret 1783-1784, vol. I-III, pp. vol. III, pp 5-25), in which Bergeret describes the plants he has mentioned, but not in the order he lists them on either page 3 or page 4 in his curly bracket table.

[98] L'Obel 1576.

under different names in each, and other plants were present in one section and absent in another. For instance, though "Avena nuda" appears in both the curly bracketed introductory section and the descriptive section, "Avena vesca & sterilis" is described but not present in the curly bracket table.[99]

Robert Morison likewise touted similar diagrams as useful for plant identification in his works.[100] But this confusion did not escape notice by subsequent botanists. For instance, Colin Milne wrote that Morison's "system, devoid of uniformity, is clogged with a multiplicity of characters; the classes are often not sufficiently distinguished from each other; and the key of the arrangement seems totally lost."[101]

Even the philosophical works of the medieval author and instructor Peter Ramus and his disciples, who believed it most proper to use dichotomously branching nested sets of curly brackets to summarise ideas about cosmology, the curly bracket tables at the beginning of books or chapters and the subsequent arrangement of the information in the texts do not always match. The diagrams are instead more of a rough schematic or thematic outline rather than a strict guide to the ordering of the subjects dealt with later.[102] Authors were therefore using hierarchical arrangements of their subject matter as text navigation devices in a loose and experimental fashion before strict nested categories became *de rigeur* – even in texts of which the subject matter was supposedly "logical."

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[99] L'Obel 1576, pp. 5, 9, 19.

[100] Morison 1680, p. praefatio ad lectorem. Edward Voss mentions a number of other keylike structures from this period (Voss 1952).
Barsanti (Barsanti 1988) reproduced one of the circle-and-line diagrams purported by Morison to aid plant identification, and wrote that the copies of this work in Florence (Firenze) and in the Bibliothèque Centrale of the Muséum Nationale d'Histoire Naturelle in Paris lack the pages with the diagram, which is true. A 1672 copy in the MNHN Phanerogamic Botany library has those pages, though: *Praefatio ad lectorem* 2-3 and *Cogitationis et affinitatis* tab. 1. f. t. in principio "chiave" p. 44. This copy is bound with *Plantarum historiae universalis oxoniensis pars secunda* (1680), and in this binding, the *Praefatio ad Lectorem* sections have been switched: the *Umbelliferarum* book is first, with the 1680 *Plantarum historiae Praefatio*, immediately followed by the *Plantarum historiae* title page, etc. but the *praefatio* in that section is the 1672 one that goes with the *Umbelliferarum* work (the exact same as the *Praefatio* in the 1672 copy of the *Umbelliferarum* book in the Bibliothèque Centrale).

[101] Milne 1771, p. 62.

[102] Ann Blair, personal communication – email of September 24 2004. An example of such a text is Zwinger 1594.

The adoption of tables of contents and other hierarchical diagrams that depict subjects of the same scope and in the same order as they are dealt with later took time to be instituted consistently. Eventually, economies of scale and pedagogical concerns grew enough in importance to make it worth botanists' while to ensure that tables of contents directed readers precisely to where the subject matter mentioned was located. An easy way of doing so was to print chapter titles in the order of appearance at either the front or the back of the book, usually numbered, and, sometimes along with the page numbers indicating the chapters' first pages.[103] It is not a big leap to move from this format to a more keylike structure in which the chapter titles are replaced by plant parts, divided into subsections according to their features. John Ray, for instance, used a table of contents of the kind I just described at the beginning of his *Methodus plantarum nova* (1682), while the rest of his work was made of curly bracketed groups of plant parts and their characters to which specimens could be referred. Subsequent botanists, such as Augustus Quirinus Rivinus (1532-1725), were even more consistent in the keylike curly bracket layout they used – though Rivinus was not consistent enough in his *Introductio generalis in rem herbariam* (1690) to avert sharp critiques from his colleagues.[104]

The increasing standardization of these techniques had already become a familiar part of botanical apparatus by the time of Linnaeus. Nevertheless, there was a debate from the outset about what standards should be adopted. This included the extent to which nomenclature should reflect classification.

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[103] Books with such arrangements include Hernández 1651, Réaumur 1734, vol. 1, Condillac 1746 and Buffon 1749, vol. 2.

[104] Rivinus was censured, e.g. by Colin Milne (Milne 1770, pp. 106-116) for the same reasons that Linnaeus drew scorn: he kept natural genera together even when they did not have the features of the artificial groups that contained them. (I discuss this issue in more length in Chapter 2). It was only in the mid- to late 18th century, when like-minded botanists gave voice to Rivinus's concerns to make plant identification as easy as possible no matter what, that Rivinus got a measure of respect for his system. For instance, Linnaeus had singled him out as one of the principal developers of systematic classification Linnaeus 2003, pp. 111, i.e. §153, and John Hill praised his method, though Hill also criticized him for the"confusion that arises in the Genera from this distribution: many of which are terribly broke and divided" (Hill 1770, 2 ed., vol. 1, pp. 23, 30).

Representing knowledge using nomenclature and classification

Devising a simple, stable, easy-to-use nomenclature suitable for describing any plant that might be found, a nomenclature that would make identification quick and unambiguous, was not a concern unique to the developing discipline of botany. These ideals of classification were also debated by philosophical universal language projectors in the mid-17th century. These men tried to devise universal languages based on "philosophical" principles. Many of them were also involved in botanical classification. Scholarly studies of the universal language projectors generally focus on linguistic or philosophical rather than botanical aspects of their work. The exception that proves the rule is Mary Slaughter. She devoted an entire book to discussing the links between "scientific taxonomy" and attempts to create a universal language in the 17th century, suggesting that "for the most part the motivation of the language projectors was more scientific than linguistic; their concern was more with nature than with language."[105] Although the accuracy of this view of the main aims and activities of the universal language projectors has been contested,[106] Slaughter was perceptive to draw attention to the goals that botanists and universal language projectors shared. These goals were to represent knowledge comprehensively, unambiguously, and in a way that is easy to learn and communicate.

The social circles of 17th century scientific language projectors and botanists overlapped. Sometimes the same people were experimenting with and making suggestions concerning both language reform and botanical classification. Such men included Marin Mersenne (1588-1648),[107] Cyprian Kinner (?-1649),[108] William Petty (1623-1687),[109] Seth Ward (1617-1689)[110] and John Wilkins (1614-1672).[111] This group was large and its

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[105] Slaughter 1982, p. vii.
[106] Maat 2004, pp. 69, 149, 172, 174, and especially pp. 214-215.
[107] Salmon 1966, p. 392.
[108] DeMott 1958, p. 6, Schulte-Albert 1979, pp. 47-52 – a brief biography, including birth and death dates.
[109] Slaughter 1982, p. 134.
[110] Slaughter 1982, pp. 133, 179.
[111] Slaughter 1982, p. 160. See also p. 135 for even more connections. Wilkins did his own botanical arrangement before the Great Fire of London in September 1666 destroyed his work (Maat 2004, p. 172).

members eloquent. Their discussions illuminate some of the problems that were and still are involved in devising classification schemes that are comprehensive, with the largest possible scope, stable, easy to use, reflective of natural relationships, and simple in structure – particularly regarding nomenclature and what its functions should be. A discussion of these issues helps to situate later botanical arrangements, particularly John Ray's, in terms of the debates current at the time Ray was devising them.

Functions of nomenclature

Essentialism comes from attempts to reduce ambiguity

Like botanists later, universal language projectors of the mid- to late 17th century were engaged in the quest for universal, accurate notions that could in turn be represented by a universally understood system of signs. Part of the inspiration for this quest was a reaction against the Aristotelian system of categories, which had been culturally dominant earlier. The prestige of this kind of logic, however, declined sharply as empiricism became popular.

By the mid- to late 17th century, the intellectual movers and shakers, such as Wilkins, Dalgarno, Ray, and Tournefort, had rejected Aristotelian categories as arbitrary and unsatisfactory.[112] They wanted to find an unambiguous basis upon which to found a classification of things that would be useful to all people, for all purposes. They sought a shared language free from the potential for misunderstandings like those that had contributed to so much bloodshed during their lives. They looked to incorporate some of the new ideas about language structure that were filtering into Europe from missionaries in East Asia familiar with ideograms.[113] This search for an unambiguous understanding of and means of communicating about nature was manifested in the quest to pin down the "essences" of

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[112] Cram and Maat 2001, p. 38, Maat 2004, p. 171, and McMahon submitted.
[113] Maat 2004, pp. 17-18.

things, a quest in which most of the participants acknowledged that their goal was as impossible as it was desirable.[114]

The search for "essences" in botany can therefore be considered as an offshoot of botanists trying to overcome the problems of ambiguity, vagueness, redundancy and confused meanings inherent in representing concepts so that others can understand them. Despite the acknowledgement that people with different educations – particularly the speakers of different languages – see the world differently, Renaissance and early modern thinkers generally subscribed to the ideas that certain concepts are universal and that "language is … basically a collection of names which refer to these concepts."[115] These ideas fit well with the longstanding conception of thought as the combination and rearrangement of simple ideas into more complex ones. Words could stand for concepts such that "linguistic expressions can be reduced to an underlying, logical form that agrees with this internal process."[116] Mid-17th century universal language projectors were all familiar with this way of thinking about thought, though they varied in how much they agreed with it.

Universal languages based on these premises offered hope to humanity. The idea was that the linguistic diversity foisted upon the world at the tower of Babel might have been a curse on mankind, but we could return to something like the pre-Babel ideal, or at least clarify communications, if the universal concepts at the heart of all languages could be identified. If universally unambiguous words could be applied to these concepts, the ways that these words could be manipulated would be something like manipulating the concepts themselves. Communication would be more literal and would rely less on metaphors with elusive meanings open to misinterpretation. Miscommunications and confusion would diminish, and all the social ills connected with them would be reduced. Humanity would be

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[114] Cram and Maat 2001, p. 28, Maat 2004, p. 58. These men were therefore, to use Popper's terminology, ontological but not methodological essentialists.
[115] Maat 2004, p. 21.
[116] Cram and Maat 2001, p. 50, Cram 1998, p. 136.

freed from the burden of Babel that had been holding back progress. The arts and sciences would flourish like never before.

After some initial experiments with universal languages from the 1640s to 1680s, however, the people paying attention to the poor results of such projects began to state that reality is too complex, and the human mind too feeble, to ever allow people to get at the real essences of things.[117] But our best efforts, coupled with rigorous empirical examinations of nature, could allow us to come pretty close.[118] For identification purposes, it was determined that the most informative properties of a thing either indicate something about what the thing is, such as how it came into being (causation) or its least variable features (description) or its most distinctive features (discrimination from other things).

These practical aspects of description as a means of communication introduced essentialism to botanical discussions. I have come to believe that discussions of essentialism in natural history are incomplete unless they consider this role of communication. Essentialism creeps in whenever people wish to communicate as unambiguously as possible about ambiguous entities – and plants were certainly ambiguous entities. Botanists throughout the 18th century, such as John Ray and later Linnaeus, distinguished between nominal and real essences (although not with this terminology).[119] The hope was that we would be able to approach the real, unambiguous, causal essences using whatever tools we had at hand that would give us access to nominal essences. Colin Milne expressed this common attitude when he wrote in 1770 that

> The fact is, various bodies, every way limited in their essential properties, are subject to change from occasional causes, in other properties less essential. Hence the enquirers into nature should give particular attention to

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[117] Cram and Maat 2001, p. 38.

[118] McMahon submitted, Maat 2004, p. 171. See also Adanson 1966, pp. cxci-cxcii for an 18th century example.

[119] Philip Sloan published an influential paper on Ray's and Tournefort's supposed essentialism. Susan McMahon examined the validity of this idea in its historical context and showed it to be wanting (Sloan 1972, McMahon submitted). Within the broader literature, Mary Slaughter attributed essentialism to John Wilkins, but Jaap Maat showed this also to be incorrect for both Wilkins and his contemporary and fellow universal language planner, George Dalgarno (Slaughter 1982, Maat 2004, pp. 58, 172, 260). Linnaeus and his different types of essences were likewise in this tradition of distinguishing between real and nominal essences, though with different terminology (Linnaeus 1751, pp. §§ 189-191).

> all the properties of bodies, and select from them the *most fixed and constant* for scientific distinction.
>
> The great umber of bodies to be investigated, requires that, in distinguishing them, we should collect many characters; that from these *placed in proper subordination*, we may lay the foundation of the differences and agreements of bodies.[120]

Botanists therefore set their sights on the least variable features or most distinctive features of plants – usually the most informative ones from the point of view of identification – as indicators of nominal essences. These features were often said to embody the essence of a plant according to whatever theories were in vogue at the time. Theories of plant physiology, which was poorly understood, yet critical to the life of plants, were sometimes invoked for this purpose, particularly after the 1770s. But few naturalists explicitly discussed what they believed were the embodiments of the essential properties of plants. One exception was Linnaeus in his *Fundamenta botanica.* He wrote that the essence of a plant is in its fructification, a stance echoed by Julien-Joseph Virey. (Virey added that the most essential part of an animal is its nervous system). A.-P. de Candolle was of a different opinion. He stated that the essence of a species is the mutual resemblance and interfertility of its members. [121]

These exceptional comments aside, remarks explaining what essences are, other than Linnaeus's definition of the essential character of a plant (as opposed to its factitious or natural characters) were quite uncommon. It was most common for authors to mention essences without explaining what they meant at all.[122] Those who did referred to essential features as the most constant properties as often as they referred to essences as properties that were necessarily unchanging. For instance, Colin Milne wrote that "The essential

[120] Milne 1770 under CHARACTER. This is the first use of "subordination" referring to characters that I have seen – earlier than Lamarck's use of the term in 1778 (cited in Stevens 1994, p. 65 note 13 as the first instance of it he has seen). My italics.

[121] Müller-Wille 2005, pp. 90-91, Virey 1818, vol. Tome XX. [MED-MIN]. p. 482, Candolle 1813, p. 157. Ernst Mayr might have agreed. Would that not have made him an essentialist about the Biological Species Concept?

[122] E.g. Lefébure 1817, p. 12, Lestiboudois 1781, p. xlvii, Lindley 1835, p. 40, Mouton-Fontenille de la Clotte 1798, p. 12.

characters never vary in the same species,"[123] Gerard stated that essential characters are those most distinctive and constant,[124] Jussieu wrote that essential characters are the most uniform, i.e. the sexual organs of plants, because they contribute to formation of the seed, which is the most essential part of the vegetable economy,[125] and Marc Dutour pronounced that essential characters are the most constant, and therefore the most useful taxonomically.[126] Naturalists were concerned with stability of their plant arrangements in practice as much as they were with loftier philosophical ideas. While many did search for nominal essences in the form of unambiguous features of organisms, their quest was usually motivated at least as much by a desire to communicate with sufficient accuracy and precision to enable others to know exactly what plants were being discussed as by supposed Aristotelian or neo-Platonist beliefs. Essentialism in this respect therefore did not have as pernicious an influence on the classifications of plants that were produced as the "received view" would have it.[127] In many ways, essentialism in practice is instead a symptom of the use of increasingly structured communications in which there must be a compromise between comprehensiveness and clarity.

Thus, although naturalists and other natural philosophers of the late 17th century and early 18th century often used the terminology of Aristotelianism, their practices were based on the idea that empirical exploration was required to dispel the accumulated misconceptions of the past. Aristotelianism inspired them, but it was also their point of departure.[128] Although botanists in the 18th century almost always believed that species each have some kind of unchanging essence, the "essences" early modern botanists wrote about searching for were Aristotelian only in that they were invoked for rhetorical purposes.[129] Botanists and universal language planners alike held no illusions that they were capable of detecting these

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[123] Milne 1770, p. METHODUS.
[124] Gerard 1774, vol. Tome Sixième p. 123.
[125] Jussieu 1789, p. 8.
[126] Dutour 1816, vol. 4 (BOE-CAL) p. 198.
[127] Winsor, 2003 and references therein.
[128] Maat 2004, pp. 14-15, 58.
[129] McMahon submitted, p. 4.

essences directly. A search for essences of species or the like was not the main aim of these endeavours.

To what extent should nomenclature reflect classification?

Instead of searching directly for essences, universal language projectors sought to develop a system of signs capable of describing any thing or concept to help people to amass information about the world, to describe its units in a meaningful way, and to enable clear communications about them. Botanists shared the same goals for botanical nomenclature, although, of course, they restricted their field of inquiry to the vegetable world. An examination of the problems that universal language projectors encountered while pursuing these goals can therefore illuminate the problems that the botanists faced when devising nomenclature along the same lines.

Since existing languages were full of imprecise designations and historically accumulated ambiguities and error, natural philosophers proposed a more thorough investigation of the natural world so as better to understand how to represent it clearly. Words in their languages would not just have universally accepted meanings. The way the words themselves were formed would also indicate those meanings at a deeper level.[130] Deriving basic words from first principles seemed to be the best way to do it. That way, the relationships of the words with the things they described would be as self-evident as possible. But what were the first principles to be? There was no consensus on this issue.[131] Furthermore, the philosophical universal language projectors, like many botanists after them, were also highly concerned with producing functional languages. It was not enough for the words in their schemes to indicate something about the things to which they referred. No matter what other criteria had to be met, they also wanted words to act as labels so that each and every thing would be distinguishable by name. Each name therefore had to be unique,

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[130] Slaughter 1982, p. 2.
[131] Maat 2004.

pronounceable, and preferably short and non-random, to help the memory. Names that describe their referents can meet these criteria, but only under certain conditions. If the description is too short, it will be ambiguous. If the description is too detailed or long, the name is hard to remember. People will find a way to shorten it, losing its meaning in the process.[132] But why should a name have to encompass all that information, anyway? If it is embedded in a classification scheme of some kind in which items that are similar are stored near each other, and the name points to a place in the scheme, then anyone familiar with the scheme can acquire background information about the named thing from its position, and the name will need only to distinguish the item from its neighbours. This situation – the same in universal philosophical language schemes and in botany – gives rise to the biggest question in constructing a stable and comprehensive system of nomenclature. To what extent should nomenclature reflect classification?[133]

Dalgarno versus Wilkins

The dispute between universal language projectors John Wilkins and George Dalgarno was largely concerned with the two men's difference of opinion on how much nomenclature should reflect classification. It is a microcosm of the techniques and philosophical positions that were debated among both universal language projectors and botanists, and therefore deserves a more in-depth examination than it has been accorded so far by historians of biology. Jaap Maat (2004) described their debate in great detail. I rely in large part on his work in the following section.

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[132] For instance, although it is a later example, Albrecht von Haller (1708-1777), respected author of a number of works on the plants of Switzerland, felt strongly that plants should go by descriptions of their properties, as opposed to the brief binomials favoured by Linnaeus. Yet his careful work was not very influential. "Although his phrases are very clear and short enough," A.-P. de Candolle wrote, "none of his most zealous disciples could learn them. [Instead] they used the numbers in his book as specific names, which proves by experience how much human memory needs to attach to some kind of short denomination in order to retain words. And yet everybody agreed that a specific name is better than an insignificant and arbitrary number" (Candolle 1813, pp. 224-225).
Under Note 145 I have reproduced Sir J. E. Smith's take on Haller's nomenclature.

[133] Or vice versa, in the case of alphabetical indexes.

George Dalgarno (1616?-1687) was a grammar teacher from Aberdeen who came to Oxford and became involved in the intellectual circles there in 1657 after making contact with Samuel Hartlib.[134] He had been working on universal language schemes before he arrived. Through Seth Ward, who was also keen on universal languages, he was introduced to John Wilkins. Wilkins used his position as warden of Wadham College to introduce Dalgarno to others with similar interests and to secure royal support for Dalgarno's project.[135]

Wilkins (1614-1672) was a powerful man. He was friendly, somewhat dogmatic, bright, and he had very influential friends – for instance, he was married to Oliver Cromwell's sister.[136] He was a founding member and the first secretary of the Royal Society, and became the Bishop of Chester by 1668.[137] He had been working on his own universal language based on Hebrew in the 1640s, but neglected that sort of research until after he met Dalgarno and collaborated on his project.[138] Although both men had similar ideas of how to come up with the individual words for things, they made different assumptions about how memory works and how much it can retain. These assumptions affected the degree to which they expected the vocabularies of their languages to reflect their classifications and how they thought it would best be employed. By the end of 1657, Dalgarno and Wilkins had a falling out about this issue and went their separate ways.[139] Interestingly, the number of kinds of plants known to exist played a role in the subsequent development of each of their schemes.

Names as descriptions, descriptions as names

Both Dalgarno's and Wilkins's schemes fell into a tradition of using descriptions of objects as their names. This avenue was pursued time and time again. The idea behind it was that if words or plants were to be put into groups according to their properties, and their

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[134] Maat 2004, pp. 33, 50.
[135] Maat 2004, pp. 50, 136.
[136] Maat 2004, p. 136.
[137] McMahon 2001, p. 240.
[138] Maat 2004, pp. 137-138.
[139] Maat 2004, pp. 50-55.

names were to reflect those properties, a thing's name would indicate which group it was in, that is, where to find it. The most immediate problems with this approach are those of choosing the properties upon which to base the names. Naming plants according to diagnostic features only could lead to existing names becoming ambiguous should new plants be discovered that share those features. The alternative would be to err on the side of caution and describe several properties of each plant, as in the descriptive names used in many herbals. But names like these were awkward because of their length, and it was difficult to keep descriptions consistent enough to memorize. Coding information about plant properties as syllables, letters or numbers was one way to reduce the length of such descriptive names to address these problems. Schemes using coded descriptions as names were re-invented multiple times from the 1640s to the 1830s. None was implemented on a large scale or became popular after John Wilkins' well-publicized and well-financed *Essay towards a real character* (1668) failed to achieve all that had been hoped of it. I describe the circumstances accounting for the reinvention and repeated rejection of such schemes in Chapter 6.

Coded lists of properties as names of plants

Syllables and letters

Letter- or syllable-based schemes of naming plants originated at least as early as the first universal language schemes. Both Marin Mersenne[140] and Cyprian Kinner[141] proposed schemes in which each letter or syllable of a plant's name would stand for a property of the plant, in 1640 and 1647, respectively. Neither man, however, attempted to implement such a scheme; perhaps they foresaw how awkward to memorize or pronounce the names would prove to be. While working on his philosophical universal language scheme, published in 1661, George Dalgarno also recognized that the length of the name-definitions would get too

[140] Salmon 1966, p. 392, Slaughter 1982, p. 129.
[141] DeMott 1958, p. 6.

long and unwieldy when they had to include many properties. He rejected them for this reason.[142] But John Wilkins used a variation on this theme to generate nouns in his *Essay*.

Numbers

The attempt to use numbers rather than letters to stand for plant properties and serve in their entirety as plant names was equally unsuccessful. In 1657, Cave Beck (1623-1706?) proposed a "universal character" by which any language could be translated into any other by giving each word a number and using the numerical dictionary to translate – a technique George Dalgarno dismissed as merely an "enigmatical way of writing the English language."[143] Leibniz also considered such schemes to be impractical.[144] Despite offering a substantial reduction in the length of each written description, numbered lists are monotonous, hard to memorize, and, furthermore, more prone to being misread and misinterpreted than lists of words are, as Dalgarno recognized. John Wilkins, however, made use of them.[145]

Large numbers of plants thwart universal language schemes

Both Dalgarno's and Wilkins' philosophical language schemes generated names in which syllables stood for properties of the named items. Syllables were individually meaningful in that they pointed to the categories to which the named item belonged. In

[142] Cram and Maat 2001, p. 35. Dalgarno was concerned with nouns in general, not just species names.
[143] Maat 2004, p. 49.
[144] Maat 2004, p. 291.
[145] Cram and Maat 2001, p. 28, Knowlson 1975, p. 63. In the 18th century, Albrecht von Haller (1708-1777), author of a respected classification of the plants of Switzerland, was soundly criticized for rejecting Linnaean binomials. "What did the great Swiss botanist substitute in the place of this contrivance?" asked Sir J. E. Smith – "A series of numbers, burthensome to the memory, destitute of information, accommodated to his own book only, and necessarily liable to total change on the introduction of every new-discovered species!" (Smith 1832, vol. 2 pp. 468-469). (I reproduced A. P. de Candolle's comments on Haller's nomenclature in note 132).

The problems with using numbers as names were also discussed at length in the 20th century, when similar proposals were being promoted again. As Arthur Cain explained, "A string of numerals is not only less easy to speak in ordinary discourse, it is also less memorable, and less easy to recognize a mistake in, than is a name. In a perfect taxonomy of fully analyzed entities such a number-code may be of great use, nevertheless. In biological taxonomy it has as yet little applicability" (Cain 1979, p. 26).

It is also interesting to note that apparently one of the very first uses of place value was devised by the Indian astronomer Āryabhaṭa, who devised a scheme for naming very large numbers whereby "he made up nonsense words whose syllables stood for digits in places, the digits being given by consonants, the places by the nine vowels in Sanskrit" Kaplan 1999, p. 43.

Dalgarno's scheme, every letter in a syllable indicated the place that the named thing occupied in an ordered list, whereas in Wilkins' scheme, only the last syllable had this function. A concept's place in the list served to distinguish it from other items.[146] But this is where most of the similarities between their schemes end.

In addition to vocabulary charts, Dalgarno provided users of his scheme with a separate section on mnemonic aids for his words. He thought that, although it would be possible to determine the meaning of each syllable in a word logically, "it would be an excessive torture for the mind to run through such a long process for every individual word." Dalgarno expected that conversation could only flow if words came to mind "spontaneously and without any act of thought or judgement."[147] For this reason, he wanted the vocabulary of his language to be small enough, and individual words short enough, to use and retain in the memory. An encyclopaedic enumeration of all things would be unsuitable as the basis for a lexicon, he argued, because Bauhin has already shown the plant world to consist of at least 6,000 plants, and adding the number of animals and manmade things, numbers, colours, sounds, passions of the soul, and so on, would make the vocabulary too big to use.[148]

Because Dalgarno had to compromise between length and descriptive ability of each word, the meaning of many words was ambiguous, that is, underdetermined. Dalgarno was also sceptical about whether individual words needed to be specified logically or defined precisely in order to optimize communication.[149] He therefore proposed that combinations of words could be used to refer to particular things. Users could decide for themselves which combinations met their purposes. These combinations of names were meant to distinguish things, not necessarily to state how each thing differs from every other one.[150] In other words, Dalgarno proposed the use of a kind of binomial nomenclature such that "that names for species of animals and plants... are to be composed out of a word signifying a generic

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[146] Maat 2004, pp. 43-44, 49.
[147] Maat 2004, pp. 91, quoting Dalgarno in The Art of Signs p. 59.
[148] Maat 2004, p. 69.
[149] Maat 2004, pp. 52, 90.
[150] Cram and Maat 2001, p. 46.

concept and a word signifying some salient feature of the species."[151] That salient features, not necessarily "logical" ones, were used to distinguish species in same genus from one another is critical. Dalgarno chose to take advantage of a common feature of language as he saw it in use. He avoided imposing an idealized picture of how the mind *should* work. If these names were ambiguous, he added, users could attach another single difference or descriptive phrase to make their meanings clear.[152]

Wilkins, on the other hand, insisted that there must be a unique and meaningful word for every single thing. Wilkins realized that the resulting vocabulary of his language would be large. He tried to compensate for this by trying to make it regular in outline, composed of small groups of either six or nine members. The idea was that this regular structure would be easy to memorize.[153] His scheme, however, was not as simple in structure as he claimed. The sections on plants and animals compiled by John Ray posed particular problems. In fact, more than 2300 names in his scheme are shared by more than one concept or thing, making them ambiguous. One word, for instance, means both mule and ass.[154] Wilkins acknowledged that coupling items under one name in this way reduced his language's descriptive powers, since users would have to specify which of the paired concepts they meant, but he did not offer any solutions to this problem.[155] His scheme was inconsistent with the philosophical precepts upon which it was supposed to be based in many other ways as well.[156] Not the least of these was that Wilkins or Ray realised – like Dalgarno, mentioning the 6,000 plants described by Bauhin as an incomplete enumeration of those that existed – that there were too many plants to include in the scheme as it stood. There were so many in the 'herb' category alone that it had to be split into 'herbs considered according to the leaf,' 'herbs considered according to the flower,' and 'herbs considered according to the

[151] Cram and Maat 2001, pp. 47 – referring to the Art of Signs, pp. 42-45. He is using "genus" and "species" here in the general, Aristotelian sense of the word, in which "genus" is a group to which one or more "species" belong(s), with no intimation of rank other than that genus is above species.
[152] Maat 2004, pp. 87-88.
[153] Maat 2004, pp. 172-173.
[154] Maat 2004, pp. 174-175.
[155] Maat 2004, p. 177.
[156] Maat 2004, pp. 179-197.

seed-vessel,' each of which was to be divided into subordinate groups[157] In fact, there were twice as many described kinds of plants as the number of words Wilkins intended to use as the vocabulary of his entire language![158] Jaap Maat has even suggested that "plants constituted the only kind of existing things which defeated Wilkins's approach on account of their great number."[159] But failing to include a good selection of plants would defeat Wilkins's desire for his project to be comprehensive. Wilkins – or Ray – was forced to compromise and "to take notice only of the *chief families* of Plants, to which the others are to be reduced." More specific kinds of plants had to be "expressed Periphrastically here as in all other Languages."[160] There was no way to provide a name for everything using Wilkins' language, as much as he had started out thinking that this idea was feasible.[161]

[157] Wilkins had originally also included a category of plants organized, as Maat wrote, "with respect to their use, such as bringing pleasure, or being edible, or medicinal, but he finally opted for the more 'truly Philosophical' classification presented in the tables." John Ray may have been responsible for convincing Wilkins to abide by these changes; Wilkins' original tables had been based on Merret's *Pinax*, which Ray had once described to a friend as "bungling." Raven 1986, 2 ed., pp. 143, 182, Maat 2004, p. 173.
[158] Maat 2004, p. 207.
[159] Maat 2004, p. 209.
[160] Maat 2004, p. 208.
[161] Wilkins ended up resorting to the combinations of radical words and words indicating distinctive features favoured by Dalgarno. His classification scheme was also modified in structure to accommodate 759 species of plants (Maat 2004, pp. 208-209), particularly the 546 species of herbs, which were too numerous to place in one category without further subdivision. Still, even with subdivisions, some of the resulting groups still had up to 18 members (Maat 2004, pp. 173, 176, 208). All of these modifications reduced the regularity of Wilkins' scheme's structure, making the classification upon which his nomenclature was based more difficult to memorize (Maat 2004, p. 177). Nevertheless, Wilkins' scheme was more complete than Dalgarno's. Dalgarno's lexicon consisted of at most 2600 words while Wilkins published almost 4200 radical words (Maat 2004, pp. 94, 175) – and Wilkins had more prominent backers. Wilkins' scheme soon became the main such language discussed by the intellectual elite at the time.

The result was that Wilkins won the universal language battle but lost the war. Dalgarno changed the organisation of his universal language vocabulary from its original format as verse – which he considered to be easy to memorize – to tables in a modified version of the predicamental order, in order to accommodate Wilkins' criticisms (Maat 2004, pp. 41, 49). And after Wilkins' *Essay* was published, Dalgarno and his scheme were generally dismissed by the intellectual elite in London and Cambridge – Dalgarno was even accused of plagiarizing Wilkins' scheme, despite having published first! (Cram and Maat 2001, p. 30). He soon fell into obscurity and was forgotten. But although Wilkins' scheme was more popular in the late 17th century than Dalgarno's, and remains better-known today, it also never achieved the kind of popularity Wilkins had hoped for it. Historians and linguists today regard Wilkins' scheme as the most complete attempt at an inherently impossible task ever published – whereas Wilkins regarded it as an incomplete attempt at a feasible task (Maat 2004, p. 142). Meanwhile, Dalgarno's attempts to take language and memory as he saw them in use into account when devising his scheme make him appear to have had a better grasp of the entirety of issues involved in representing concepts in communicable ways than did Wilkins.

Further problems with universal language schemes and their botanical counterparts (or, why we like binomials)

Names made with letter-, syllable- or number-based codes for properties of their referents, like those in both Dalgarno's and Wilkins' schemes, share further drawbacks than trouble handling large numbers of plants. Even when the properties on which the names are based are derived from supposed first principles, they are also inextricably tied to the classificatory schemes that generated them. This makes the names meaningless if taken out of the context of a scheme. The names are only understandable if people who use them have the scheme memorized. This is very difficult in practice, as the lack of success of universal languages based on these principles shows.[162]

Another major problem associated with remembering ordered lists of properties, whether coded as numbers or as syllables, is that it is impossible to insert new information into them without disrupting the place value, unless the names are to grow outrageously long. If the order in which the properties are listed is intended to be meaningful, appending newly discovered properties to the end of the list may be logically nonsensical, as well as aesthetically ugly. If place value is flexible, the names may grow so long that pronouncing them reliably becomes impossible.

Names that rely on place value also work only if everyone using them has memorized the same lists of properties and knows the code syllable or number as well as the contents of each position/item in the list/on the grid. This is an unlikely situation. Making unambiguous communication rely on a shared terminology alone is unrealistic because everyone must then have exactly the same background knowledge.[163] Without a way of expressing the same concept in multiple ways, it is very difficult to determine if a speaker and listener understand

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[162] Dalgarno came to this conclusion, as did Locke and, apparently, most members of the Royal Society by the 1680s (Maat 2004, p. 264). See also Chapter 6 for further criticisms of similar schemes proposed throughout the 18th century.

[163] As I discovered to my acute embarrassment when having a discussion about "pants" with a British friend.

a term to mean the same thing. A fixed terminology is also too inflexible to accommodate different nuances of expression necessitated by changes in context or a growth in understanding – changes such as new information about how plants function or are related to one another. The botanist A.-P. de Candolle recognized these problems. He had harsh words for such schemes, writing, that

> But even if ... significant names for every being could be obtained, this method would always be dangerous, in that it obliges naturalists to change all the names known up to now, by which it would sever the links between science and the public, and between the public with science, and above all because *names must always be changed as the real characters of beings become better known.*[164]

The binomial names that had become entrenched in botanical and zoological practice by the time de Candolle was writing thus seemed like the least of three evils: they were easier to pronounce and tell apart than names derived from coded lists of properties, more convenient than uncoded descriptive names such as those used in herbals, and more memorable, though more ambiguous, than both. Since names could not reflect enough properties of plants to allow them to describe how plants were to be arranged in their entirety, botanists were forced to acknowledge a certain amount of arbitrariness was inherent in the names they used. But there was still a desire that plant names should reflect plant classification to some degree. In this respect, limits to human memory seem to have favoured binomial names.

The common use of binomials was only reached long after a kind of consensus, or at least détente, about the extent to which nomenclature should reflect classification took effect in botany at the genus level. This followed the widespread acceptance of "natural genera" and binomial nomenclature after Linnaeus consistently employed them. Prior to the 1750s, the names the majority of botanists used were more than two words long, and the ways that plants were listed in texts were inconsistent. Nomenclature did not reflect classification because there was no real classification going on. But although he did not propose any

[164] Candolle 1813, p. 226, my italics. He was attacking J-P Bergeret's *Phytonomatotechnie universelle* (1783) in particular.

nomenclature reform in botany, John Ray was one of the first to embark on the project to organize his botanical texts consciously, and according to particular principles. It appears that Ray's main motive in doing so was to facilitate plant identification. But the changing ways in which Ray organized his texts over his career indicates that a number of other motives and constraints were at play. These deserve further scrutiny if we are to find out why classification *per se* later became such an important aspect of botany.

John Ray's changing ideas of how to make plant identification easy

It is unclear whether John Ray and George Dalgarno knew each other personally, though they certainly shared an interest in language and had friends and colleagues in common through the Royal Society (e.g. Francis Lodwick, another universal language projector).[165] But Ray was certainly familiar with the failings of Wilkins' classification scheme, even as he felt bound by duty and gratitude not to say anything negative about it publicly. Ray himself was so influential upon subsequent botanists, particularly in Britain, that it is worth examining these connections in further detail.

John Ray's name usually does not come up in discussions of plant classification except as the father of the "natural method." But not only did he achieve lasting fame as a botanist and zoologist (as editor and major contributor to the posthumous works of Francis Willughby),[166] he is also known among lexicographers today for producing a *Collection of English proverbs* (1670), a *Collection of words not generally used* (1674), and a trilingual Latin-Greek-English dictionary (1675)[167] that was exceedingly popular and ran to multiple editions.[168] He was particularly respected for his command of the Latin language.

Ray's collections of words and proverbs were as authoritative and nuanced as his collections of plant descriptions, if not of the same size. Although he left little in the way of personal thoughts and opinions that have been preserved, it is almost certain that he must

[165] Poole and Henderson 2005.
[166] Raven 1986, 2 ed., pp. 166-169, 309.
[167] Green 1996, p. 175.
[168] According to WorldCat, new editions came out in 1685, 1688 and 1696.

have thought considerably about the significance of names of things and about how to arrange them. Ray was cautious, however, not to overstep the bounds of his knowledge in this as in many other respects of his life – a wise choice, given the precarious social situations he was in almost continually.

Ray was a relatively unknown figure in the early 1660s. The son of an Essex blacksmith, he entered Cambridge on a scholarship. His first publication, an alphabetical flora of the Cambridge region, was printed anonymously in 1660 while Ray was still living there. Ray's biographer, Charles Raven, suggested that Ray chose an alphabetical arrangement of plants as a deliberate rejection of the "schemes manifestly defective or hurriedly extemporised" that his predecessors had produced; cautious as always, "he had not yet sufficiently studied either species or the structure [of plants] to be ready for the problems of classification."[169] Many years later, Raven wrote that Ray had explained that although his friend and fellow naturalists, Ralph Johnson, Vicar of Brignall, had suggested to him that

> The *Catalogue* should be arranged in the order not of the alphabet but of nature. 'I knew that this was good advice but at the time dared not take it: I was not ready for such an attempt: it demanded a close study of the systems of others, and a thorough survey of all species of plants, their relationships and differences: and this was a work of years.'[170]

An alphabetical arrangement was an embodiment of Ray's agnosticism respecting natural groups, not an expression of how he believed that plants should ideally be arranged for easy access. After the second edition of the *Catalogue* had sold out, Ray considered rearranging the plants in it according to Johnson's suggestion, but this never happened.[171]

While at Cambridge, Ray became close friends with his former student, Francis Willughby. The wealthy Willughby was also interested in natural history, particularly zoology. He invited Ray to travel with him through the north of England in the summer of 1660. The Restoration purge of Cambridge occurred at this time, and afterwards Ray found

[169] Raven 1986, 2 ed., p. 108.
[170] Raven 1986, 2 ed., p. 249.
[171] Raven 1986, 2 ed., p. 249.

himself unable to return.[172] Luckily for Ray, Willughby became his patron. The two of them embarked on a project to "reduce the several tribes of [living] things to a method and to give accurate descriptions of the several species from a strict view of them,"[173] touring Europe together in 1662. Ray's friendship with Willughby also "strengthened" his growing association with and "lifelong admiration" for John Wilkins, who knew both Ray and Willughby during their Cambridge days together.[174]

In October of 1666, while Ray was living with Willughby's family at Middleton, Wilkins recruited Ray to compile the tables of plants for his *Essay*, as detailed above.[175] Willughby was to help with the animals.[176] But Ray was not satisfied with Wilkins' scheme – which, after all, gave new names to fewer than one-tenth of the kinds of plants then described. In fact, he thought Wilkins' *Essay* ill-conceived and poorly planned, expressing his frustration in a private letter to his close friend Martin Lister. Given the pressure he was under because Wilkins' book was already being printed,[177] Ray wrote that it was not surprising that he was stuck producing

> error-filled confusing tables and imperfections, with only three weeks for the work; I had truly decided nothing beforehand about this thing, nor had I ever thought about it. Moreover, I am forced to follow these dictates (as I am not led by nature), but instead I must accommodate the plants to the method prescribed by the author....What now is the hope that this method will be complete, rather than most imperfect and absurd?[178]

Yet Ray continued to work on revisions to the tables despite his misgivings. A year later, he wrote to Lister,

> next week we expect the Bishop of Chester [Wilkins] at Middleton, who desires our assistance in altering and amending his tables of natural history. To make exact philosophical tables, you know, is a matter very difficult, not to say impossible; to make such as are tolerable requires much diligence and experience, and is work enough for one man's whole life, and therefore we

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[172] Raven 1986, 2 ed., p. 56.
[173] Raven 1986, 2 ed., pp. 123, citing a letter from Derham in Memoirs, p. 33.
[174] Raven 1986, 2 ed., p. 56.
[175] Raven 1986, 2 ed., pp. 181-182.
[176] Maat 2004, p. 135.
[177] Raven 1986, 2 ed., p. 182.
[178]Ray 1848, pp. 41-42 – Ray to Martin Lister, May 7, 1669. My thanks to Shana Worthen and Jean-Marc Drouin for translating this passage, which I found prior to locating Raven's translation of it (Raven 1986, 2 ed., p. 182).

> had need call in all the assistance we can from our friends, especially being not free to follow nature, but forced to bow and strain things to serve a design according to the exigency of the character.[179]

Despite these complaints, Ray's collaboration with and admiration for Wilkins endured. Wilkins helped Ray in many ways, for instance, he was instrumental in getting him nominated to the Royal Society in 1667 and having his entry fee waived,[180] trying to reconcile Ray with the church and other church officials with Ray,[181] and hosting Ray at his home for – according to one account – seven years.[182] Ray rushed to be with Wilkins when Wilkins was dying, and after his death, Ray completed a Latin translation of Wilkins' entire *Essay*. It was, however, never printed, and the manuscript was lost.[183]

Though Ray was quite loyal to Wilkins, it is possible that he was frustrated enough with the way that Wilkins' scheme dealt with plants and animals to put forth something more in accord with his own ideas of a "philosophical" take on nature. Ray's rivalry with 'King's botanographer' Robert Morison (1620-1683) also played a large role in Ray's decision to work on and publish his *Methoda plantarum nova* and *Historia plantarum*. In these works, Ray arranged plants according to the most "natural" method yet devised.[184] The two impetuses to aim for a natural method may, however, be linked: Morison seems to have initiated the rivalry in his *Praeludia botanica* (1669) by criticizing the tables that Ray had prepared for Wikins.[185]

The way readers were guided to make plant identifications in Ray's later books indicates that he had thought more about how to best arrange plants than he had when he first started working on the tables for Wilkins. That experience may have shown Ray both

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[179] Ray to Lister, April 28, 1670. (Ray 1848, p. 55).
[180] Raven 1986, 2 ed., p. 144.
[181] Raven 1986, 2 ed., p. 166.
[182] Raven 1986, 2 ed., pp. 56, cf p. 166.
[183] Maat 2004, p. 265.
[184] McMahon 2001, p. 268.
[185] Morison likely implied that Ray believed the format of the tables to reflect the order of nature. The text of Morison's criticism, however, has not been published in translation. Moreover, it is difficult to locate. Susan McMahon wrote to me that "there are lots of copies of Preludia Botanica, [sic] but many, if not most of them, have had Morisons's attack removed - including the copy in the Bodelian library that was used for the Early English Texts microfilm!" McMahon has located a copy of the offending letter and her translation is in preparation (McMahon, 9 December 2004, personal communication).

the advantages and disadvantages of using stepwise procedures to identify plants, and of making note of the "*chief families*" of plants.[186] He also made sure to avoid Wilkins's mistake of placing pre-set limits on the number of plants that his scheme could accommodate.

In the *Methodus* and the *Historia plantarum*, Ray adopted a mostly dichotomously-branching structure, but adapted it to suit his own ends. This stepwise path to a plant via a hierarchy of features examined was quite different from Ray's previous botanical publications, which had been alphabetical, and were more detailed and thorough than Morison's. But though the *Methodus* and *Historia plantarum* have an overall dichotomously branching structure superficially similar to Wilkins' scheme, Ray did not distribute plants according to a simple and rigid formula designed to be memorized easily, as Wilkins had tried to do. The differences by which a user is guided to find a plant in this work are not reified as categories, as they are in Wilkins' scheme. Nor did Ray deliberately omit any plants, as Wilkins had[187] – in fact, he included a number of plants incompletely described by other authors even though the descriptions are too vague for them to be of any use.[188] Ray instead aimed to organize as much botanical information as possible, in the simplest manner that he could think of – grouping plants with the most features in common together. It was based on techniques he had honed during decades of teaching students and friends how to recognise plants and describe them intelligibly to others.[189] He had long hoped to be able to achieve this goal, though he had not dared to attempt it earlier in his career.[190] And he never considered his work to be finished. He kept making insertions and revisions until the end of his life.

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[186] Maat 2004, p. 208. Certainly Wilkins' ideas about nomenclature did not impress Ray. Ray continued to use longhand descriptions of plants as their "names."
[187] See "Dalgarno versus Wilkins" above.
[188] Raven 1986, 2 ed., p. 225. Cf Brian Ogilvie's account of Ray excluding dubious animals from his and Willughby's zoological works (Ogilvie 2006, p. 262).
[189] McMahon 2002, pp. 4-5.
[190] Raven 1986, 2 ed., p. 249.

It was clear from Ray's correspondence what he wanted to achieve with the arrangements of plants he put forth in his *Methodus* and *Historia plantarum*. By "methodizing" plants – putting them into a hierarchical arrangement by which they could be identified on the basis of their features – he hoped to make plant identification clear and simple for everyone. He wrote of his *Historia plantarum* that

> my reasons for attempting this work were – 1. To satisfy the importunity of some friends who solicited me to undertake it. 2. To give some light to young students in the reading and comparing other herbarists, by correcting mistakes, and illustrating what is obscure, and extricating what is perplex and entangled, and in cutting off what is superfluous, or, under different titles, repeated for distinct. 3. To alleviate the charge of such as are not able to purchase many books; to which end I endeavour an enumeration of all the species already described and published. 4. To facilitate the learning of plants, if need be, without a guide or demonstrator, by so methodizing of them, and giving such certain and obvious characteristic notes of the genera, that it shall not be difficult for any man that shall but attend to them, and the description, to find out infallibly any pl[ant] that shall be offered to him, especially being assisted by [the] figure of it.[191]

In other words, Ray wanted the *Historia plantarum* to be an all-purpose text which both beginners and more advanced botanists who did not necessarily have the luxury of access to all the canonical botanical works could identify a plant with a degree of certainty unequalled in other texts, and without additional help. This was a radical departure from early modern herbals and other botanical texts. Earlier works were expected to supplement field demonstrations during which masters would introduce students to plants.[192] Ray's last comment above also indicates that he expected his work to be illustrated. His troubles finding the money to illustrate his *Historia plantarum* and subsequent *Synopsis* are indicative of the struggles faced by many authors after him.

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[191] Ray to Hans Sloane, Feb. 11, 1684. Square brackets in original. (Ray 1848, pp. 139-140, reprinted with no explanation in the same work, 160-161).
[192] Ogilvie 2006, pp. 74, 228.

John Ray's publication problems

Ray began working on his *Historia plantarum* at least as early as February 1684.[193] He was convinced of the value of illustrations to the utility of botanical works, writing to his friend Tancred Robinson that

> an history of plants without figures [is] as a book of geography without maps. A good figure conveys that to the mind suddenly, and with ease and pleasure, an idea whereof cannot be formed by the help of a description without time and pains, and a greater attention than most readers have patience to give it.[194]

Ray added that he wanted his *Historia plantarum* to be illustrated, even if it meant making it bulkier, more expensive, and of less general use. He thought that referring to illustrations in other books would "distract and interrupt the reader," and "but few readers would have the books referred to." But funds were scarce and Ray struggled to find ways to realize his wishes.

Part of the reason why Ray found money for illustrations so hard to come by were personal in nature. While Willughby's widow had paid for the publication of the lavishly-illustrated *Ornithology* (Latin version 1675; English version 1678), she was no longer interested in Ray's projects after she remarried, and, in fact, prevented him from accessing Willughby's manuscripts after she made Ray leave Middleton in 1676.[195] The Royal Society had financed the publication of Willughby's *Historia Piscium* (1686), which was heavily edited and reworked by Ray, and contained 113 plates by various artists. The publication of the plates went over budget and almost bankrupted the Society in the process, guaranteeing that it would no longer participate in such ventures again for some time, if ever.[196] There were not enough moneyed people interested in natural history in England's fragile political situation to come forward with the funds to allow the *Historia plantarum* to be illustrated,

[193] Ray 1848, pp. 139-140.
[194] Ray to Tancred Robinson, October 22, 1684 Ray 1848, pp. 155-156.
[195] Raven 1986, 2 ed., pp. 166, 213-214, 349.
[196] Kusukawa 2000b, pp. 187-188, 191-192. Even with more than half the Royal Society members contributing to the publication costs of the plates, slightly more than half the plates had been funded by an individual donor, Samuel Pepys (Kusukawa 2000b, pp. 187-190).

either. Ray seemed to sense this.[197] Still, a number of Ray's admirers wrote to him asking him to reprint the *Historia plantarum* with illustrations – going as far as offering to obtain subscriptions to help defer the cost. Ray anticipated many problems in arranging for illustrations to be made. For instance, he doubted competent engravers or supervisors could be employed at affordable rates.[198] After months of going over ways to make an illustrated edition possible, Ray felt forced to give up by the unfavourable circumstances.[199] His *Historia plantarum* was published unillustrated, much to his regret.[200] Several years later, Ray reaffirmed his belief that unillustrated botanical texts are incomplete, calling his *Supplement* (later included in his *Synopsis,* a partial continuation of *Historia plantarum*) "but a blind work, not illustrated by any figures, and so useless almost to any but the great proficients in botanics."[201] He apologized that he had to omit plates because of their expense.[202] An illustrated third edition of the *Synopsis* was published posthumously, but its 24 plates were a far cry from the figure per plant that Ray had originally envisioned.[203]

Problems finding the money to publish suitable illustrations in useful and affordable formats continued to plague botanists throughout the 18th century. For this reason, the majority of botanical works used for plant identification were mostly devoid of illustrations, save for a plate or two illustrating the technical terms that came to replace images as the main way of communicating morphological differences.

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[197] Raven 1986, 2 ed., p. 214.
[198] Ray to Sloane, December 16, 1702 Ray 1848, pp. 406-407. Ray had already participated in the publication process of illustrated books several times. Kusukawa (Kusukawa 2000a) describes the different steps in the publication of *Historia Piscium* in detail. Raven describes the publication of *Ornithology* (Raven 1986, 2 ed., p. 321).
[199] Ray to Sloane, Januar 20, 1703Ray 1848, p. 409.
[200] Raven 1986, 2 ed., p. 214.
[201] Ray to Hans Sloane, March 1, 1698 Ray 1848, pp. 336-337.
[202] Raven 1986, 2 ed., p. 302.
[203] Ray to Hans Sloane, Feb. 11, 1684 (Ray 1848, p. 140). This letter is reprinted with no explanation as to why in the same work on page 161. The *Synopsis* contained descriptions of around 10 000 new plants (Raven 1986, 2 ed., p. 304).

Split between identification and relationships

Being thorough, comprehensive and easy to use even without a master's help is a tall order for a textbook. Despite the lack of illustrations, Ray was optimistic about how useful his *Historia plantarum* would be for botanical novices. No prior botanical work had been as thorough as Ray intended his work to be. And Ray had nothing to lose by suggesting that representing data in a way that reflects how it really is in the pattern of nature will be the most informative and easy to communicate, for that assumption was common in his time, and remained so throughout the period when the adoption of the "natural method" was becoming popular.

In his *Methodus emendata* (published in 1703 but written several years earlier), Ray proposed six fairly common-sense rules for botany that embody these assumptions. They are that:

1. Names must be changed as little as possible: the multiplication of names produces mistakes and confusion.
2. Notes characteristic of the group must be clearly defined: those that depend upon comparison are to be avoided: if used, the difference between the groups compared must be large.
3. *Characteristics must be obvious and easily observable, the use of a Method being principally to help beginners.*
4. *Groups approved by nearly all botanists should be preserved: they are in fact natural, and have many common attributes besides that from which they are named.*
5. *Care must be taken that cognate plants are not separated, nor strangers united; valuable as is Rivinus's method for teaching, it fails in this regard.*
6. Characteristics must not be multiplied unnecessarily, and should not be more than is needed to determine the group.[204]

I have italicized rules number 3, 4 and 5 because, while they seemed like achievable goals in Ray's time, they have proved to be contradictory demands. What we now understand to be natural groups are not always united by obvious and easily observable features that beginners can grasp quickly. Identifying plants easily – Rivinus's principal aim[205] – sometimes requires a focus on features possessed by some plants but not by their relatives. If

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[204] Ray 1703, described and translated in Raven 1986, 2 ed., pp. 291-292. My italics.
[205] Rivinus 1690, pp. 3, 18.

this means separating similar plants with different numbers of petals in a text, so be it.[206] Ray did not see this problem, and, to be fair, neither did his most of his contemporaries or successors.

But even in this climate of optimism it was appreciated among botanists that different systems of botany had different advantages and disadvantages. As Hermann Boerhaave wrote,

> The End and Design of this [Tournefort's] Method is to help the Memory, and to range every thing distinctly in its proper Place. That System therefore is ever the best which is most easy; I do not say which is most natural, for we do not know what that is: But if you examine all Systems whatsoever, you will very easily find that of *Tournefort* to be preferable to all the rest, as having reduced every thing to a very few general Heads, and yet without Confusion. He took his Character and Mark from Fruit and Flower: Indeed it is not perfect, for no Man could ever perfect such a work, but yet it is most easy.
>
> ....
>
> In this [Ray's] System you must attend to a great many Things at once ... But if the *Genus* of Plants is to be taken from what they are alike (or agree) in, then *Ray's* Method must prevail; for, as I said before, *Tournefort's* Method has much the fewer general Heads, as being better accommodated to help the Memory; it is therefore the best of any for Beginners. But if a Physician would be more perfect in this knowledge, he must have recourse also to Mr. *Ray's* Method.[207]

These recognitions were one of the first blatant calls for a split between methods used to identify plants quickly and easily and those used to describe how they were related to one another.

At the cusp of the 18th century, therefore, botany was suffused with confidence about what arrangements of plants could achieve as scientific and pedagogical tools. But just as the nomenclature employed in universal language schemes was torn between saying something meaningful about the properties that characterize referents and being easy to remember and say, the development of taxonomic schemes was often fraught with problems. Botanists in the early 18th century hit the same wall Ray had run into: what they felt they could

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[206] Milne 1771, pp. 115-117.
[207] Boerhaave 1719, pp. 130-131.

accomplish in principle seemed ever less likely to be achievable in practice. Naturalists faced practical constraints on what their classifications could be made to accomplish. Regardless of whether they were aware of it, they were forced to prioritize their goals. The interplay between the criteria by which they judged classifications and practical exigencies affected how they organized their books.

Chapter 2. 18th century rift between theory and practice of taxonomy

> *Botanists split into groups and ... there were a large number of different arrangements proposed. The history of these different ideas must offer to the intellect a fairly amusing spectacle.*[208]

Although the natural method was seen to be botany's ultimate purpose from the time of John Ray, identifying plants quickly and easily has always been its most important short-term goal. It was certainly a significant concern of both Ray's and Tournefort's.[209] But, as Susan McMahon points out, this goal was often obscured in debates about the merits of different botanists' classification schemes. These debates usually revolved around how well such schemes kept similar plants together – even if the schemes had not been constructed with this end in mind.[210] Arrangements of plants were expected to be easy to use, simple in design (and, as such, easy to memorize), comprehensive, and reflective of plants' relationships, all at once. Naturalists who tried to make plant identification schemes meeting all of these criteria ran into problems for the same reasons that universal language planners did.

For one, most such authors in the 18th and early 19th centuries emphasised the simplicity or elegance of the theories by which they justified their choice of characters, rather than the ease of use of the schemes themselves. Ease of use was assumed to go along with cutting-edge and robust theories. So was the maintenance of natural groups. There was no need to prioritize ease of use in and of itself when it was seen to follow from sophisticated interpretations of nature's workings that kept natural groups together. When botanists criticized their rivals' schemes for failing to keep related organisms together, they

[208] Grandjean de Fouchy 1763, p. 53.
[209] McMahon 2002, pp. 2, 3, 10, 11.
[210] McMahon 2002, p. 11.

were expressing their belief that sound scientific theories, as embodied by classification schemes, should be congruent with nature.[211] And even if the classifications in question were more heavily slanted towards organizing information than explaining patterns in nature, they still expected natural groups to be accommodated as smoothly as possible. Related organisms should be grouped together in the schemes, while dissimilar ones should not be placed close together. Features of schemes that interfered with quick and easy identification were often excused and retained if they kept natural groups together. "A feeling persisted," Peter Stevens wrote, "that taxonomic groups had to be readily recognizable."[212] When they were not easily recognizable, botanists blamed each other for failing to understand nature well enough. Difficulties in identifying plants using the majority of 18th-century arrangements of plants, particularly Linnaeus's sexual system, embody all of the messy consequences of these contradictory desires.

Yet, over the course of the century, botanical practice changed. By the end of the period, the information management tools botanists had at their disposal became significantly easier to use both for plant identification and for keeping natural groups together. A number of theoretical assumptions about the shape of nature and the workings of the human mind had to be overcome in order for these technological developments to occur. For most of the century, the theories propounded in the prefaces of botanical works usually lagged significantly behind the adoption of new classificatory methodologies. Botanists ruminated about theory, struggling to digest how they could make sense of their more progressive practices. Their writings were filled with intellectually vapid phrases more symptomatic of attempts to sort out what was happening than of any firm theoretical commitments. I will address some of their more equivocal terms and convoluted arguments later on. For the moment, here is a quick primer on the two most loaded words – natural and

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[211] E.g. Lindley 1829a, p. 80.
[212] Stevens 1994, pp. 228-229 – he provides some references too.

artificial – before we discuss their implications for contemporary understandings of the sexual system and similar hierarchical arrangements of plants.

Botanical bafflegab obscures conflict between theory and practice

The gap between botanical theory and practice is evidenced by an expansion in popularity of the categories of "natural" and "artificial." These terms had many connotations, many of which were contradictory and some of which were devoid of all but phatic meaning. Ignoring the mud-slinging rhetoric, however, the significance of the terms is largely connected with how classification schemes were to be used.

Natural

In general, "more natural" systems were those aiming to capture the interrelationships among organisms as they exist in nature. They were necessarily informed by theoretical conceptions of how nature is structured. They were usually intended to include as many plants as possible. Pragmatic constraints, such as the need to identify organisms quickly and easily, were not a high priority. The phrase "natural method" was used – often interchangeably by the same naturalists – to mean either the technique to organize plants in the way most congruent with nature, or the ultimate results of so doing. Since the eventual aim of understanding all plants as they are related to each other was not an immediate possibility, it was fairly normal for botanists to write that different schemes varied in their degrees of naturalness.

The more groups of plants in a given arrangement were seen to share a family resemblance, the more natural the arrangement was seen to be.[213] These groups were almost always circumscribed by more than one character (feature). Characters shared by organisms seen to be related were valued the most, regardless of how obvious those characters were to

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[213] E.g. Candolle 1813, p. 197.

perceive. Such characters could include internal anatomical and/or microscopic details that required specialist training or equipment to see. They could also be based on information not visible in plant specimens at all, such as details of range of occurrence or behaviour, or information dependent on seasonal availability. Authors of more natural systems often hesitated to commit to descriptions of plant groups.[214] Sometimes they did not characterize them explicitly at all.[215] For these reasons, more natural works include reference texts aimed at expert users, such as those on biological theory, as well as some monographs and some floras.

Artificial

"More artificial" systems, on the other hand, were more constrained by practical issues. They ideally provided simple ways to divide nature into units that had functional importance to the systems' users. Such texts were usually explicitly aimed at beginners, focussed on the identification of organisms, and/or restricted in scope with respect to location or biological group – such as field guides, some floras, and some monographs.[216] The choice of characters by which to describe taxa therefore depended most heavily on how easy they were to perceive. External features visible to the naked eye were preferred,[217] as were features contained within specimens in the format in which users were likely to encounter them – that is, isolated and dead. Range distribution, behaviour, or other seasonally restricted observations of patterns exhibited by living organisms in the wild were generally not used. Visibly distinctive features were also favoured. In the case of flowering plants, flower parts are sufficiently distinctive to merit being used in artificial classifications, even though these features are not visible at all stages of a plant's life. Features from other

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[214] Smith 1832, vol. 2 p. 571, Candolle 1813, pp. 259-260.
[215] E.g. Linnaeus' *Fragmenta*, Linnaeus 1764, 6 ed., pp. 603-619, Linnaeus 1738.
[216] E.g. Hooker 1835, 3 (with additions and corrections) ed., p. vii, Smith and Sowerby 1832-1846, 2 ed., p. iii. Using these criteria, folk taxonomic classifications are necessarily more artificial than modern scientific (phylogenetic) ones. Scott Atran discusses this issue in some depth, though not with this terminology (Atran 1998).
[217] Candolle 1813, p. 36, Kirby 1802, vol. 1, pp. 29, 39.

parts of the plants were sometimes also employed to help reduce the time-dependency of the identification process.[218] Single characters or simple combinations of characters were used where possible. The idea was to make identification as simple and fail-safe as possible.

Botanists often designed their artificial systems to be regular in structure. Characters by which taxa were differentiated were often consistent.[219] For instance, Linnaeus's sexual system was based on the numbers and relative positions of stamens and pistils in flowering plants.[220] Structural regularity could also come from using the same characters that predecessors used within particular taxa.[221] Botanists over several generations described plants in terms of floral parts. Consistent patterns of one kind or another gave focus to arrangements. They also helped botanists to memorize the overall structure by which plants were to be arranged.

The choice of characters by which to describe taxa was frequently also linked to their perceived functionality in the lives of the organisms.[222] However, much of the talk of the harmony of structure and function and ease of classification using particular organs involves *a posteriori* justification of choices made on pragmatic lines, since certain organs were seen empirically to be more taxonomically informative than others, as I discuss below.[223]

Regardless of the naturalness or artificiality of a system, the characters used to describe organisms had to be relatively constant within taxa, yet subject to enough variation among taxa to enable them to be distinguished.[224]

The degree of naturalness or artificiality of a classification comes down to a question of priority. As outlined earlier, one of the first priorities that botanists made explicit was

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[218] Candolle 1813, p. 36.
[219] Candolle 1813, p. 36.
[220] Linnaeus 1751, p. 101 (=Philosophia botanica § 163).
[221] An equivalent example from zoology would be Linnaeus arranging mammals according to their teeth , and describing insect orders in terms of their wings, following Aristotle and (in the case of insects) Reaumur (Atran 1990, p. 114, Winsor 1976, p. 64).
[222]E.g. Jussieu 1789, pp. xliii-xlvii, cited in Stevens 1994, p. 37 and Lindley 1835, p. 40.
[223] Larson 1994, pp. 43-45.
[224] Candolle 1813, p. 36.

their wish to make schemes that would work to identify plants unequivocally. This desideratum, however, was complicated by a number of beliefs about correspondences between the shape of nature and the functions of the mind prevalent in the 18th century.

Schemes simple enough to be memorable should make identification easy ... but they don't

Natural method should be most natural to the mind

A common philosophical approach to classification in the 18th and early 19th century was that nature was the product of a rational creator, and could therefore be expected to yield to an equally rational arrangement.[225] As Aubert du Petit-Thouars put it, "Nature is simple; its progression must consequently also be so."[226] Rationality was also seen as the main feature of humanity that separated it from the rest of the animal kingdom. God, who was rational, created nature, and God created people, who were uniquely rational among his creations. As such, there was a widespread belief that the true order of nature would be conducive to understanding by the human mind. The best way to approach the study of nature was therefore through an examination of nature's productions, arranged in such a manner as to illustrate their relationships. The most natural arrangement of living things would be good for every purpose. It would be inherently easy to understand and to use. If people were to fail to develop an all-encompassing yet philosophically sound classification, it would be due to mental limitations or ignorance about lesser-known parts of the world. At the beginning of the 18th century (and even among some natural theologians in the 19th century) many naturalists believed that God had even put marks on his creations to indicate

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[225] Of course, this is a gross generalisation – different naturalists subscribed to the various premises I outline to various, sometimes only limited, degrees. Although the true thoughts coming from God idea was made popular by Descartes, many naturalists, particularly in France, believed that nature was far too vast and complex to ever be understood other than at a superficial level (e.g. Buffon 1749, vol. 2, p. 12). Nevertheless, the views I describe were widespread and influential even upon those who purported not to subscribe to them – very few naturalists were philosophically consistent.

[226] du Petit-Thouars 1811a, p. 32.

to mankind the place of each in nature. Tournefort, for instance, explained that "The Creator of all things, who gave us the faculty of giving names to plants, places in the plants themselves signifying marks from which should be sought that similarity required of species of the same genus. We can neither change these marks nor renounce examining and using them, if we would eliminate error."[227] He also wrote that the characters of genera should "be perceptible and easy to notice, without requiring the use of a microscope to find them."[228] Our desire to force natural productions into groups at our convenience was misguided. Care and attention to detail were what were needed to put plants and animals in their proper places.

It is largely for this reason that most naturalists did not see the utility of using separate techniques to describe organisms' relationships and to identify them. Early systems and methods, such as Ray's *methodus* and Linnaeus' sexual system of plants, were expected to accomplish both ends: a feasible goal in a rational world.[229] Most naturalists consequently used either "the" natural method or an artificial one. They saw their functions as equivalent. This situation held even though many of the leading naturalists of the late 18th century saw that natural groups did not necessarily exhibit features allowing them to be delimited sharply or identified easily – for, simultaneously, they maintained that doing so was possible in principle.

The debut of text-based schemes: from cramming aids to reference tools

Botanists shared this common goal and these perceptions of nature and memory with the universal language projectors who had come before them. Like the universal language projectors, they were eager to share knowledge with their peers and to pass it on to students. But instructing botanical tyros, let alone familiarizing oneself with these myriad vegetable

[227] Scott Atran noticed this widespread belief as well. Tournefort, *Institutiones res herbariae* 3rd ed, Paris: Imprimerie Royale, 1719, p. 54, cited Atran 1990, p. 166.
[228] Tournefort, 1694, *Élemens de la botanique*, pp. 13-14, cited Daudin [1926], p. 32.

[229] Although Linnaeus stated that his sexual system was artificial, it was held in high esteem partially because it maintained many natural groups. I discuss this issue in depth later on.

productions, was not a simple task. As George Arnott Walker Arnott, the author of the article on botany in the 7th edition of the *Encyclopaedia Britannica* explained, there were so many species of plants in the world that

> To obtain a knowledge of every one of these individually, and without any relation to any other, would be a Herculean task, for which the utmost extension of human life might not be sufficient, and which, though procured, could not be imparted to others. The study would thus be selfish, and the labours of a lifetime useless.... To remedy these evils, means must be resorted to for the classifying or arranging of vegetables.[230]

Arrangements of plants based on the rational world view therefore had a catch. The catch had to do with memory. By the early 18th century, classifications and arrangements of plants were not and could not have been based on memory alone.

17th-century and early 18th-century arrangements of plants, such as Ray's, Tournefort's, and Boerhaave's, were intended to be memorized entirely or at least used as guides to help students of botany to cram information.[231] Their relatively clear structures (compared to arrangements of plants that had come before them) were to have helped students learn about aspects of botany in an order that would facilitate understanding and retention of that information. But even as they were writing these guides, botanists came to realize that there were too many plants for anyone to memorize all of their names and descriptions. Botanists did not discuss what they were doing, but during this time period, there was a shift in what they expected systems and methods to do. The idea slowly took hold that, if all the names of plants could not be memorized, at least students should be able to memorize an algorithm they could use to identify plants. The algorithm would be used in conjunction with a text employed as external or artificial memory. Information about plants would be stored in the text, not in a student's head. The method or system would be like a table of contents in the botany student's mind, allowing him or her to consult the written entries at will.

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[230] Arnott 1832, 7 ed., vol. 5 p. 30. His estimate was 100,000 species of plants, but these concerns were shared by botanists even at the beginning of the 18th century who assumed that there were far fewer plants.
[231] This is what systems and methods in the time of Ramus were also intended to do (Ong 1958, pp. 213, 227).

In this way, the main objective of botanical arrangements shifted from facilitating the memorization of plant names to making it simple to look up those names.[232] Botanists such as Linnaeus may have boasted about having created ways to make plant descriptions easier to memorize in large numbers[233] but they also wondered who had the memory to remember all these plants without some kind of aid.[234] Whether they realized it or not, whether they liked it or not, botanists of the period had to move to a more text-based way of storing and communicating information about plants.[235] This shift in priorities is embodied in the growing obsession botanists and would-be botanists had with instruction through books alone. To put it another way, botanical instructors no longer expected it to be feasible for their students to memorize everything they needed to know about plants. Instead, texts could be consulted for every aspect of botany that a beginner might want to learn. And if a text contained enough information in an order easy enough to use, who would need a teacher? With demand for botanical instruction booming and beginning to outstrip supply, especially in remote areas, botanical texts that could even partially replace students' needs for mentors' attention would benefit everyone.

Growing importance of text-based instruction

Connoisseurship problem

The prefaces of botanical publications from the 18th and early 19th centuries almost always contain a remark or two about how the author's intent in presenting an arrangement of plants is to make botany – i.e. plant identification – easier to learn. Despite these

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[232] Cf Stevens 1997b.

[233] Linnaeus 1751, p. § 161, and in his *Critica botanica* (1738), cited Stafleu 1971, p. 42, and an appendix to the *Genera plantarum* (1764) (Stafleu 1971, p. 135 quoting Stearn's 1959 translation).

[234] Linnaeus 2004 (not published), pp. 4-5. Adanson and many others also expressed similar sentiments: "Right now the scope of botany is becoming greater and the immense quantity of plants is starting to overwhelm botanists. What memory can suffice for all these names?" (Adanson 1966, p. ciii).

[235] Although there were certainly mnemonic elements to the binomial nomenclature that became widespread during the 18th century, Linnaeus's first use of binomials was as an aid to looking information up in books in an index. He referred to them as a way to save paper (Stafleu 1971, p. 106).

intentions, methods of arranging botanical texts that were popular before the late 17th century left a lot to be desired on the identification front. While they often supplied interesting ecological or horticultural information, Samuel Frederick Gray wrote in 1821, these early botanical works were too often in alphabetical order or arranged according to other principles that did not provide "any means by which a student, in possession of a plant unknown to him, can discover its situation in the catalogue; and, of course, he is necessitated to have recourse for this purpose to the instruction of a living master, who may not always be at hand."[236] As much as the problem grated on early 18th-century botanists, it took decades before anyone devised a way of organizing information about plants that would free students from this necessity.

Hermann Boerhaave's *A method of studying physick* (1708; English translation 1719), for instance, was a textbook designed to facilitate the acquisition of knowledge useful for physicians. It is full of recommendations not only about what canonical medical authors to read, but suggests an order in which to read them, so as to speed up the rate of learning. When it came to acquiring knowledge about plants beyond the 200 or so he suggested were of greatest importance to medicine, however, Boerhaave preceded his list of recommended texts with a caveat concerning the futility of attempting to understand plants from books alone. "If ... a Physician would perfectly be acquainted with Botany," Boerhaave wrote,

> let him at first chuse a Master who may shew him *vivâ Voce*, and with his Finger the Names proper to every Plant; for he must not begin by mere Reading.... if the Plant with its Name be pointed out to me by the Finger, I have then a Foundation to remember its Name with such Descriptions, and so retain it... you must learn this from a Master.[237]

Boerhaave admired the work of Tournefort and Ray, but considered neither of their methods adequate for introducing students to plant identification – even though Ray had explained that one of his main goals in writing his *Historia plantarum* was

> To facilitate the learning of plants, if need be, without a guide or demonstrator, by so methodizing of them, and giving such certain and

[236] Gray 1821, pp. v-vi.
[237] Boerhaave 1719, pp. 125-127.

> obvious characteristic notes of the genera, that it shall not be difficult for any man that shall but attend to them, and the description, to find out infallibly any pl[ant] that shall be offered to him.[238]

Boerhaave's attitude was typical of his time. As William Wotton had also complained in a 1694 text with similar aims, "the Knowledge of Plants is a confused thing depending wholly upon an uncommon Strength of memory and imagination, and even with the Help of the best books scarce attainable without a Master."[239]

In the first half of the 18th century and earlier, botany was an activity of connoisseurship. The discrimination of similar plants was best learnt by practice as an apprentice to a master with a large collection of plants at his disposal. These resources were standard at university botanical gardens by the mid-17th century. Many also had greenhouse facilities for culturing exotics. With the guidance of a tutor, David Knight wrote, a student might eventually acquire an idea of natural groups that would enable him to identify plants to family by gestalt. But, as in any other type of connoisseurship, "the dictum applies that the self-educated man will have had a very ignorant instructor. What was needed was a system that was like the plants, cut and dried."[240]

This need for some kind of text-based way of becoming proficient at plant identification remained severe for botanical beginners who did not live close to centres of teaching such as university botanical gardens or who did not have the time or money to engage the help of an expert to guide them. Louis-Jean-Marie Daubenton, the anatomist who depicted graphic dissections for the natural history works of Buffon, described the situation in the second half of the 18th century when natural history was very much in vogue among men of letters. The great number of collectors of natural history specimens, he wrote – even those who were satisfied to admire specimens without theorizing about them –

> obviously proves the public taste for this science, which can be formed only though [sic] difficult research and considerable expense, as the price for

[238] Square brackets in the original. Ray to Hans Sloane, February 11, 1684, (Ray 1848, pp. 139-140).
[239] Wotton 1694, p. 254 cited McMahon 2002, p. 3.
[240] Knight 1981, pp. 55-59 (quotation on 58-59).

> such natural curiosities has risen to a high point. Such a use of time and money presumes the desire for self-instruction in natural history.[241]

Jean-Jacques Rousseau was one such person with a desire to learn more on his own. He was frustrated with the botanical texts available to him in the countryside. "Deprived of any instruction or human assistance, I have had recourse to books that assume knowledges [sic] I do not have, and I cannot learn about the unknown by way of the unknown," he wrote to a friend. Rousseau felt that he only learned anything about plants when he spent months at a time botanizing with an expert. He asked his friend to write a simple beginners' guide to plants that could free him and others like him from the need to invest so much effort in pursuing their favourite study.[242]

There were, in fact, far more people who wanted to learn more about botany than who could do so, the Abbé François-Noël-Alexandre Dubois (1752-1824) explained in the introduction to his flora of Orléans (1803). Obstacles arose "since," he wrote, echoing Rousseau, "botanists have written for experts rather than for those who want to become experts, and because it is next to impossible to study their works fruitfully without having a master to supervise" and provide guidance. Dubois concluded that "nothing could be as useful and pleasant for the public as a method with which one could learn to get to know plants that grow naturally in the field easily, and without a master."[243] A book using such a method might be called a field guide.[244]

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[241] Daubenton, « Histoire naturelle. » Diderot and Alembert, *Encyclopédie ou Dictionnaire raisonné des sciences, des arts, et des métiers*, 3rd ed. (Geneva: Jean-Léonard Pellet, 1779), 17: 589-590. Translated in Williams 2001, pp. 87-89.

[242] Rousseau to Dr. Clappier, 23 December 1768, *Correspondence complète* (Geneva: Voltaire Foundation, 1965-1984), 36: 214-215, cited in Williams 2001, p. 93.

[243] Dubois 1803, p. vii.

[244] The connoisseurship problem never goes away. It is part of the eternal quest to capture tacit knowledge and squeeze it into a textual form. As Candolle remarked about the "theory of natural classification, ... That which one can learn about it is reduced to several general ideas that botanists of a superior order display in their conversation more than in their books, and which belong among the number of opinions that Bacon called *floating*, for since they had never been set out methodically, they never could be discussed seriously" (Candolle 1813, p. 77).

In addition, although the natural method was more consistent than any relationship-based way of arranging plants that came before it, it still relied heavily on an expert eye at recognizing relevant features of plants. Techniques to reduce or eliminate the need for connoisseurship – those based on grouping plants not according to loosely defined perceived relationships, but rather using standard algorithms based on many measurable differences (numerical taxonomy, cladistics, maximum likelihood and Bayesian inferences) – only

Field guide function

As mentioned earlier, there was a desire for books that beginners could use to identify plants on their own long before texts that could function efficiently as field guides began to be produced. For instance, John Ray's first publication, his alphabetical *Catalogus Cantabrigiam* (1660) was designed to be used this way. As Ray's biographer Charles Raven mentioned, it had an index of common names, helpful to beginners, and a list of places where particular plants were to be found. Another section listed plants described by Gerard and Parkinson that grew in Cambridgeshire. Etymological and technical notes made the book appealing to expert botanists as well – "all this in a book that can be slipped into the pocket as a companion on any country ramble.[245] But Raven went too far in adding that even in his time, the 1940s, "no better system could well be devised," for no alphabetical list in isolation can provide a way for anyone to look up a plant for which he or she does not know the name. The only kind of arrangement of plants that can do this is a hierarchical scheme. And the most famous hierarchical botanical scheme of all time was made by Linnaeus.

Linnaeus

After both John Ray's and Joseph Pitton de Tournefort's successful botanical works were published, there was a lull in the production of influential plant classifications. The next man to really make his mark on the botanical scheme still evokes strong feelings in many people today: Linnaeus.

There are many excellent books and articles in circulation about Linnaeus's life and personality, his travels, his interest in growing exotic plants in Sweden, his influence on other botanists, his contributions to botanical nomenclature, and the supposedly insidious effects of his philosophically noxious mode of thinking. [246] What is of importance to this

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became functional on a large scale in the last third of the twentieth century, with the help of computers.

[245] Raven 1986, 2 ed., pp. 83-84.

[246] Koerner 1999 Stearn and Bridson 1978, Müller-Wille 2001, Müller-Wille 2003, Stafleu 1971, Stearn 1959. The accepted view of Linnaeus's impact here as outlined in Cain 1958, Mayr 1982 and Ereshefsky 2001is

story are four aspects of Linnaeus's oeuvre: his use of a relatively strict hierarchy, his popularization of binomial nomenclature tied to that hierarchy, the standardization of vocabulary and layout in his texts, and his promotion of natural genera and, eventually, natural families. It is important to look at the sexual system with these considerations in mind. An understanding of the sexual system is a necessary starting point for more intricate discussions of the role Linnaeus played in the differentiation of plant identification techniques and the natural method.

The sexual system

Hierarchy in the sexual system

In Linnaeus' sexual system of plants, the class and order of each plant are determined by examining the numbers and situations of particular floral organs. The names of the classes reflect the numbers and situations of stamens on the flowers, while the names of orders are tied to the numbers and positions of pistils that plants within them are supposed to have. A 24th class is reserved for plants without flowers (including the fungi). Figure 10 shows the schematic representations of different numbers and configurations of flowers' sexual parts from the first edition of *Genera plantarum* (1737) to be used to determine the class and order of any given plant specimen. Figure 11 shows Linnaeus' schematic representation the sexual system itself (the *Clavis classium*, or "key to the classes"), taken from the same work.

Although Linnaeus on occasion described his orders and classes as "genera of genera" and "genera of orders," respectively, Henri Daudin pointed out that this description is schematic at best: in the sexual system, the orders and classes are "artificial," based on variations in only pistils and stamens, whereas the genera are "natural," based not only on

deeply flawed. Winsor 2001 and Winsor 2006 provide an explanation of how this erroneous story came to be so widespread.

flower structure but also on other features of the plants.[247] The discrepancies in the delimitation of artificial and natural groups led to many problems for the sexual system and other classifications like it, as I detail below. However, in general terms, the sexual system works tolerably well for identifying flowering plants. It so happens that, in the flowering

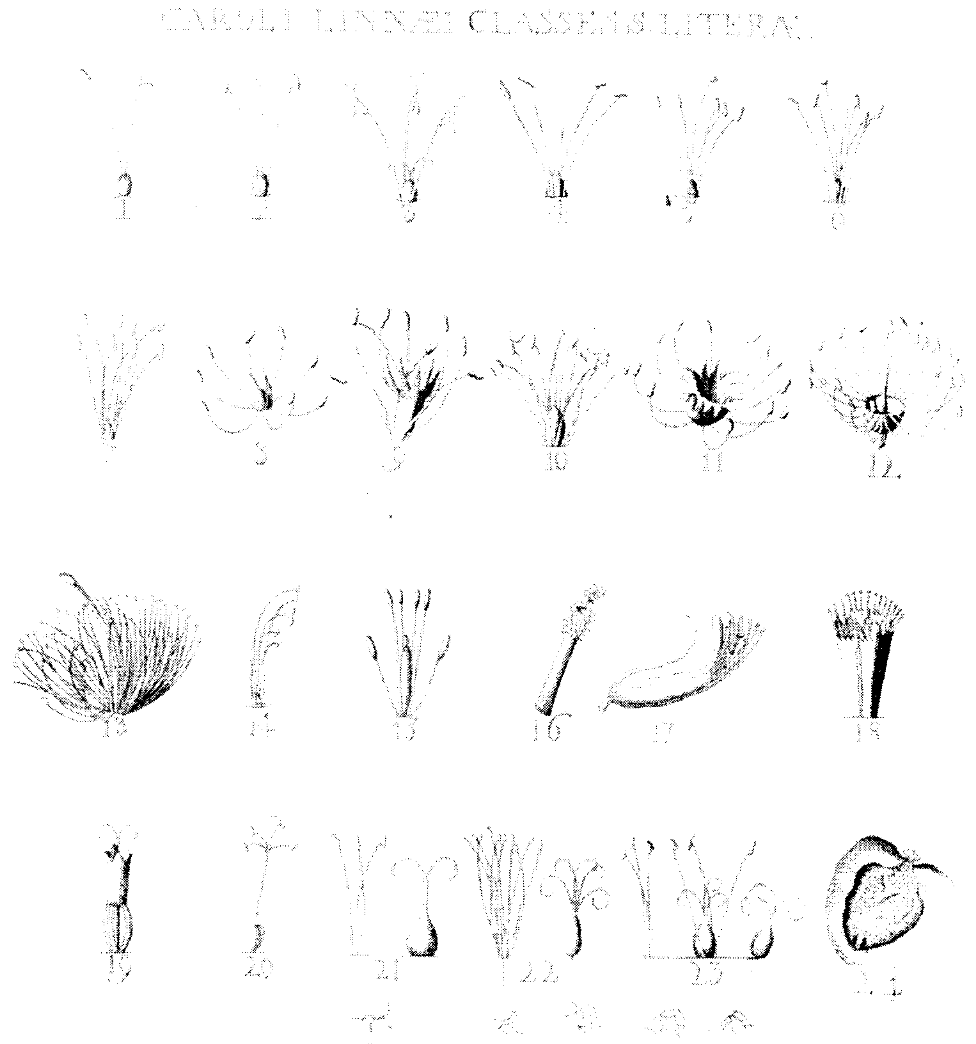
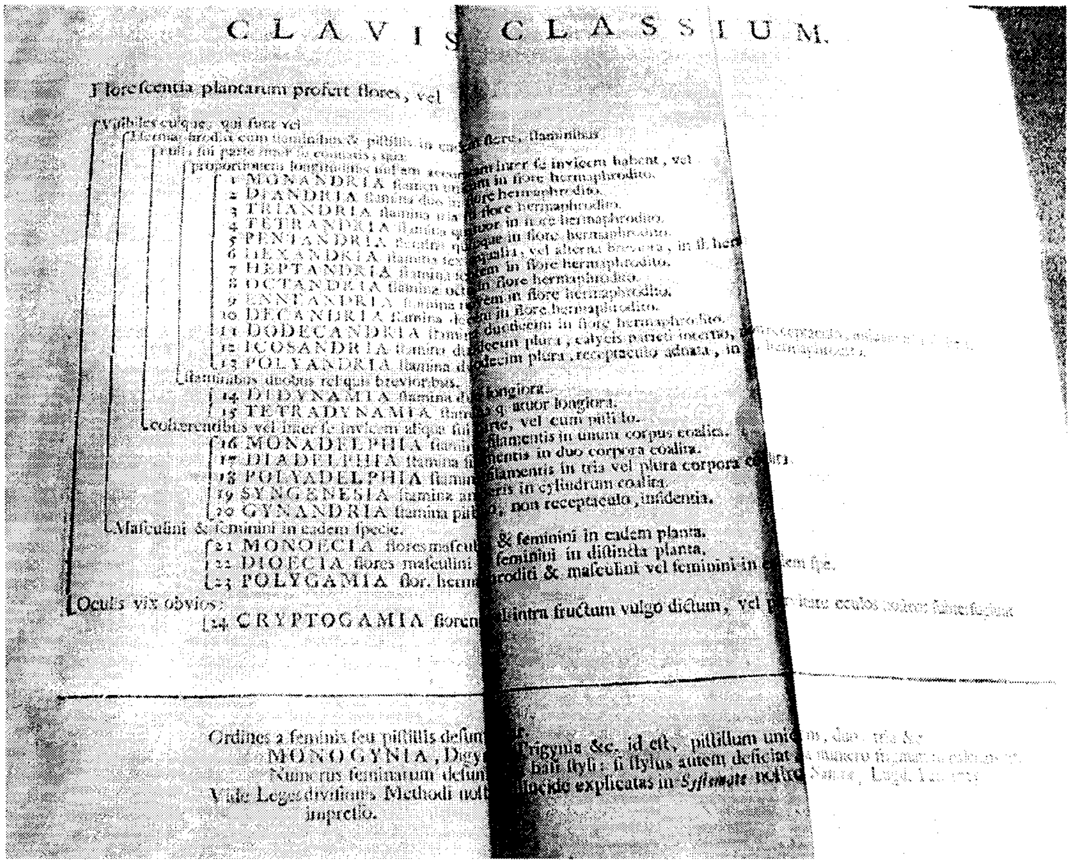


Figure 10. Plate of stamen and pistil configurations by Georg Dionysius Ehret from the first edition of Linnaeus's *Genera plantarum* (1737). Number 24 represents plants without obvious sexual structures (the cryptogams): the image is of the interior of a fig sliced in half, since in 1737 fig flowering structures were not understood. Image © and courtesy of Hunt Institute for Botanical Documentation, Carnegie Mellon University, Pittsburgh, PA.

[247] Daudin [1926], p. 45. That is, Linnaeus considered two species with different numbers of pistils or stamens to still be in the same genus if everything else about them was pretty much the same, as he did with different species of *Valeriana* with different numbers of stamens, for instance.



CLAVIS CLASSIUM.

Florefcentia plantarum profert flores, vel

1 MONANDRIA
2 DIANDRIA
3 TRIANDRIA
4 TETRANDRIA
5 PENTANDRIA
6 HEXANDRIA
7 HEPTANDRIA
8 OCTANDRIA
9 ENNEANDRIA
10 DECANDRIA
11 DODECANDRIA
12 ICOSANDRIA
13 POLYANDRIA
14 DIDYNAMIA
15 TETRADYNAMIA
16 MONADELPHIA
17 DIADELPHIA
18 POLYADELPHIA
19 SYNGENESIA
20 GYNANDRIA
21 MONOECIA
22 DIOECIA
23 POLYGAMIA
24 CRYPTOGAMIA

Figure 11. The "Clavis classium," or key to the classes of Linnaeus's sexual system, from the first edition of *Genera plantarum*, 1737. Image © Sara Scharf, courtesy of the Thomas Fisher Rare Book Library, University of Toronto.

plants, the numbers and situations of floral parts are quite taxonomically informative. Groups made using a system based on variations in these organs alone therefore have a high degree of overlap with groups formed using a greater number of characters. Linnaeus was able to keep many natural genera together even under his artificial orders and classes because of this.

Natural genera

Linnaeus believed that both species and genera were natural groups with a real existence in nature. He also believed it was possible to describe all of the genera of plants accurately and naturally. Linnaeus dedicated himself to the task. A large measure of the success of the sexual system comes from Linnaeus's hard work "fixing" genera. "Fixing" meant setting constant boundaries on the genera, based on a careful consideration of the features shared by – in principle – all the species in them.

Linnaeus did not invent the idea of natural genera. If a person can be credited with this idea, the honour goes to the 16th century botanist Andrea Cesalpino. Cesalpino, as Scott Atran noted, was concerned with making botany simpler to remember. He also wanted to make use of the way that people tend to recognize plants by overall impression, a technique that does not help at all when trying to find plants in an alphabetical scheme or most other non-hierarchical ordering systems. "One must unite everything within homogeneous genera," Cesalpino wrote. This is "very efficacious for the memory, because an enormous number of plants are thus enclosed in a résumé, ordered by genera; hence, although a plant may have never before been seen, anyone can place it in its appropriate category; and if a plant has no name, anyone can call it by its generic name."[248] This would be identifying the plant by gestalt.

Linnaeus, like Cesalpino and 17th and early 18th century botanists, expected competent botanists to memorize details of all the genera.[249] But even though he believed it possible for no more than 5736 genera of plants to exist,[250] he acknowledged in actions if not in words that the majority of botanists would not have memorized them before it would be necessary to use them. For this reason, Linnaeus's genera were firmly nested within orders and classes that would help users to identify them.

Natural genera had several advantages over artificial ones, Linnaeus wrote. They were internally consistent and not tied to anyone's particular hierarchy of orders or classes.

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[248] Atran 1990, p. 142, citing Cesalpino 1583, dedicatory epistle.
[249] Linnaeus 2003, p. 219 § 256.
[250] Linnaeus 2003, p. 130 § 167.

They would not change no matter whose method is used or how many new species were added. In fact, they become more uniform in composition the more species that are found.[251]

Stable natural genera with unchanging (or at least relatively stable) labels, he saw, were perfectly suited to be the "common currency" of botany.[252] If someone were to come up with an arrangement of plants completely unlike the sexual system, other botanists would still be able to consult that work with ease if the author used the same natural genera and the same names for them. All a reader would need to do would be to look up those names in the book's index. There would be no need to memorize every new system or method for plant identification, no need to understand the philosophical principles behind them or how they all differed. (The implication here was that, not only were plants too numerous to bother memorizing them all, but the methods and systems organizing those plants were also too numerous to be worth committing to memory). The natural genus would be the great leveller, and the binomial nomenclature affixed to those genera would be the universal language that botanists had wanted for so long. Linnaeus believed it could literally be so because of the loose link he proposed between binomial nomenclature and classification. As he put it,

> if it be desirable in general, that the terms of science should constitute an universal language, like the characters in Astronomy and Chemistry, the letters and signs in Algebra, and the figures in Arithmetic, it is particularly so in the Botanic nomenclature: where the names of the *Genera* are merely arbitrary, and can not therefore with propriety be rendered by an equivalent term into another language.[253]

Binomials were links to descriptions, not the descriptions themselves. Although they embodied the species and generic levels of classification in Linnaeus's sexual system, they would work equally well in other systems with different higher-level classifications. These

[251] Linnaeus 2004 (not published), pp. 7-8, Linnaeus 2003, p. 42 § 189. Although Linnaeus wrote that diagnosing organisms using unique marks is "easiest for the intellect," he also recognized that the discovery of new taxa sharing diagnostic features with known taxa would necessitate a re-examination and re-assignation of the marks by which each taxon could be separated from the others (Linnaeus 2004 (not published), pp. 6-7).
[252] Linnaeus 1751, p. § 284.
[253] Linnaeus 1787, vol. 1, pp. v-vi.

features of binomial names made them popular among botanists, far more popular than uninomials – which seem simpler at first glance – ever were.

Loose linkage between nomenclature and classification

Uninomial users and "species multipliers" bad, binomial nomenclature good

Since nature's productions seemed to extend far beyond the reach of human perceptions and memory,[254] the names of species could not be mere labels. There would simply be too many names to cope with. At the turn of the 18th century, botanists such as Ray and Tournefort, and later Boerhaave, all expressed dissatisfaction with the descriptive nomenclature of their day. Boerhaave, for instance, noted that there were more kinds of plants than there are Latin words.[255] But though none of them used a new nomenclature in their works, Tournefort in particular recognized that names could only become brief and memorable if they were separated from description. He even proposed a kind of binomial nomenclature, writing that,

> Names are like certain definitions, in which the first word expresses the genus, and the others the differentia.... Names ought to be short... some of these [Latin descriptive names] are so lengthy that they can scarcely be voiced in one breath .... It is one thing to name a plant, another to describe it.[256]

A number of 18th century naturalists, among them Buffon, took this idea of a separation of nomenclature and classification to the extreme and proposed the use of uninomials.[257] Uninomials, however, never became popular. Botanists gave a number of reasons for rejecting them. A.-P. de Candolle, for instance, was of the opinion that there are too many plants to have separate, memorizable names; nomenclature should bring related species together so that they are easier to remember.[258] Though Peter Stevens has suggested that

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[254] An alternative would be to deal with this superabundance of plants by restricting one's botanical knowledge to a smaller realm, sacrificing scope, as suggested by Mouton-Fontenille de la Clotte 1803, pp. 327-328.
[255] Boerhaave 1719, p. 128.
[256] Tournefort *Isagoge* vol. 1 pp. 63-64, cited Jussieu 1789, pp. xxiii, note 1, Jussieu 1994c, p. 510 note 115.
[257] Candolle 1813, p. 225.
[258] Candolle 1813, p. 225.

uninomials will always be attractive "when generic limits are in flux, because then only the specific epithet appears to be constant... [and] the name of a species is then independent of its place in the system,"[259] for the most part, 18th- and 19th-century botanists wanted plant names to be helpful for finding information about organisms. They saw linkages between nomenclature and classification as positive.

This viewpoint was most clearly expressed when botanists discussed the breakdown of taxonomy that they believed would occur if splitters or "species multipliers" had their way. Linnaeus, for instance, expected in 1737 that every competent botanist should be able to memorize all generic names and descriptions, while "few need remember specific names." Freed from the need to be memorizable, specific names, which, at that time, were still long Latin descriptions, could be as long and as technical as necessary.[260] Monotypic or small genera could wreak havoc with this plan. If, as Linnaeus put it, every dissimilarity among plants is considered significant enough to differentiate genera, there would be as many genera as species. When there are too many taxa to choose among and no system or labels to organize them, someone attempting to identify a plant can succeed only by chance.[261] Disengaging nomenclature from classification would therefore cause chaos. The memory-saving trick of including information about the classificatory hierarchy in the very names of plants would be lost – it would be almost as if the hierarchy were to lose a level.

Losing a level of the hierarchy would be a problem because even when items are grouped, human memory imposes limits on the number of items that can be remembered per group. Several 18th and 19th c. writers recognized this situation, including Leibniz.[262] He observed that it is easier for people to manipulate groups of things that fall under a common head or label than it is to consider each of them individually every time.[263] Linnaeus also

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[259] Stevens 2002, p. 20.
[260] Stearn 1959, p. 9.
[261] Linnaeus 2004 (not published), pp. 3 and 9, aphorisms 8 and 29 respectively.
[262] E. g. Boerhaave 1719, pp. 128-130, Senebier 1802, 2 ed., pp. 48-49, Kirby and Spence 1828, 5 ed., vol. 4, pp. 404-405. Kirby and Spence are explicit about it.
[263] Maat 2004, p. 354.

mused that "no one will deny that it is easier to distinguish a few genera than all at once."[264] A few decades later, he also wrote that grouping genera into orders is helpful because then "it does not turn out to be necessary at one and the same time to distinguish more genera than the mind can easily comprehend... for it is easier to distinguish 10 genera than 100."[265] Even Lamarck, who worked with the assumption that groups above the species level were products of the human mind rather than nature, recognized what was at stake. "The invention of genera," he wrote, "is a great help to relieve the memory by reducing the sum of terms used to make names."[266] Grouping genera into orders and classes simplifies finding information about plants. It substitutes the knowledge of a technique that helps users to find groups of plants for the knowledge of exactly where every plant is described. Though memorizing the features of the genera helps speed the process, it is not necessary to have memorized every generic character in order for a hierarchical structure to be useful for plant identification.

Standardization

Vocabulary and nomenclature

Another very important feature of an effective botanical work is the use of standard terminology. Standard terminology enables its users to communicate with others about what they have learned and what they would like to know. Without standard ways of communicating, it becomes difficult to specify what plant or plant property is of interest in person, by letter, or by reading complementary texts. Without standardization, knowledge about plant identification published by different people would still be confined to small groups of readers who shared the same vocabulary and ideas of plant morphology and anatomy as the author. Though Ray's use of Jungius's detailed anatomical terms helped

[264] Linnaeus 2004 (not published), pp. 3-4, aphorism 9. He also expresses a similar idea in Linnaeus 2003, p. §161.
[265] Linnaeus 2003, p. 115 § 161.
[266] Lamarck 1778, vol. 1, p. lxxxiii.

make communications among botanists clearer, and the works of Tournefort standardized botanical terms to a certain extent in France, Linnaeus was the person whose works imposed sufficient order on botanical terminology to enable the majority of botanists in all of Europe to speak the same language, as it were.

Layout

The order that Linnaeus brought to botany came not just from his use of binomial nomenclature, or a hierarchical format of information organization, coupled with indexes, but also consistency and standardization. Successful text-based techniques for plant identification –rudimentary keys – are much more than just hierarchical arrangements of data. They are based on a standard format and use a standardized vocabulary. Standardization is Linnaeus' greatest gift to botany, a gift that kept its utility long after the rationale behind his sexual system had ceased to be persuasive. To use a modern metaphor, Linnaeus, in many ways, was the Microsoft of botany. Even those who preferred to use other systems with different advantages had to know how to use his system and his terminology because once they became standard, there was no escaping them.

Despite being rife with inconsistencies, Linnaeus's natural history texts were sufficiently regular in structure to be much easier to use than those of his predecessors. His contemporaries appreciated that he "has great merit in the judicious disposition of his Matter; so that a glance of the eye catches in a moment the subject in question, without the fatigue of reading page after page."[267] David Knight has called him a "pioneer of information retrieval" because he "was prepared to take great pains over the organisation of material, and its presentation in a convenient and accessible form."[268] The layout of his books was designed to make skimming convenient. He standardized his descriptions of plants so that the parts of each were described in the same order, with a highly controlled

[267] Withering and Stokes 1787, 2 ed., vol. 1 and 2, p. xv.
[268] Knight 1981, p. 63.

vocabulary, and laid out consistently on each page, with the generic name of each plant in capitals[269] and the trivial names in the margins,[270] to make browsing more efficient. His use of binomial nomenclature was not only useful for teaching because of its mnemonic value, but because of the way it allowed the names of plants to flag further information about them visually on each page, as shown in Figure 12. And the hierarchical mode of presenting information that Linnaeus employed was also a form of data compression, because of inheritance of characteristics or nesting of groups in the hierarchy.

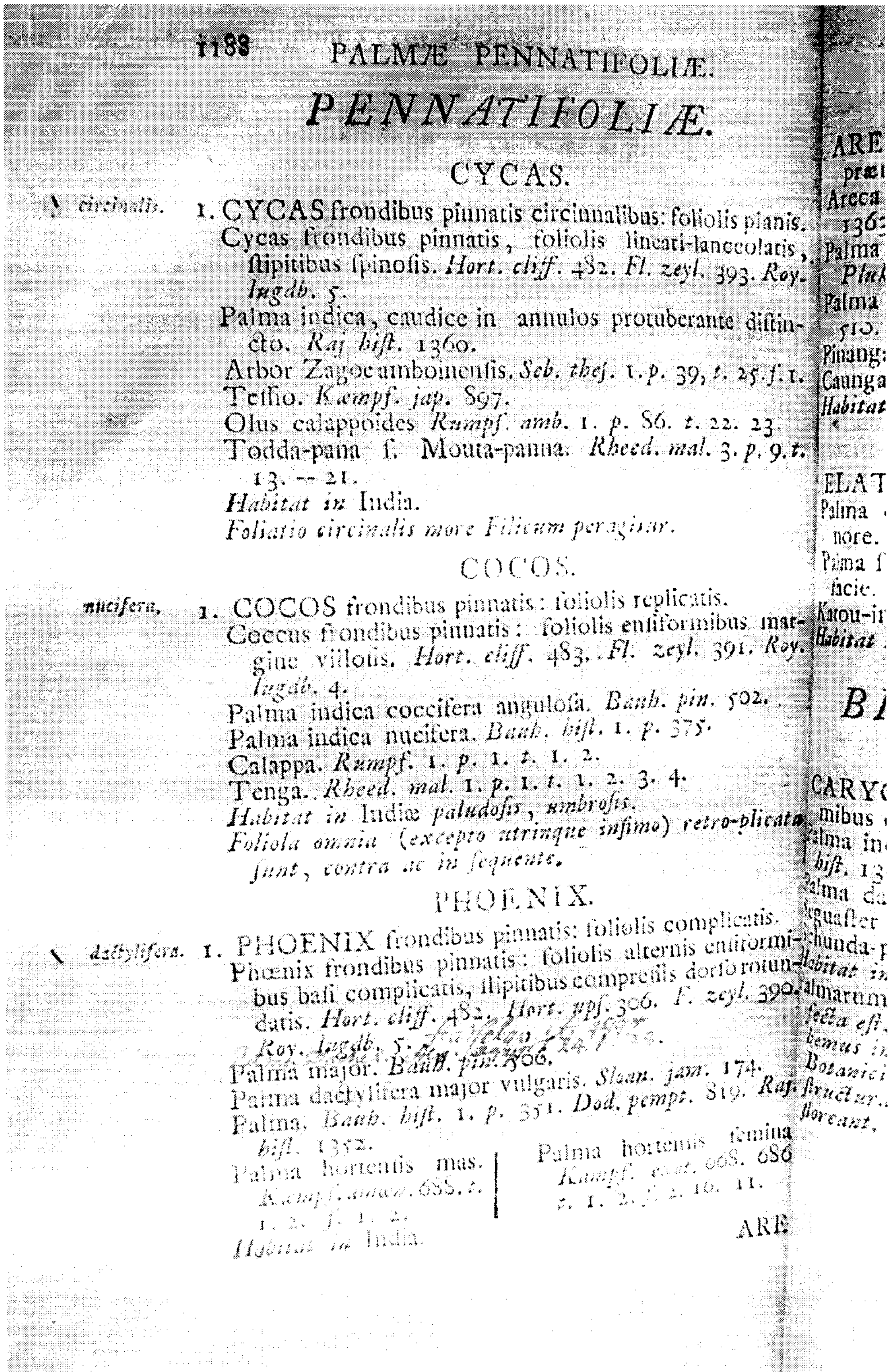
Nineteenth-century botanists, who were accustomed to identifying plants using the works of Linnaeus, welcomed these features of his texts and recognized the importance of these contributions. As medical journalist and botanical lecturer Samuel Frederick Gray (1766-1828)[271] wrote, Linnaeus introduced

> some very great improvements, . . particularly in the typographical execution of his works.... If we compare his manner of printing the synoptic tables of the genera, prefixed to each class, with the tables of Ray, or Knaut, the superiority of his method will be evident. The same superiority exists in the manner of printing the few descriptions he has published. By always observing the same order in treating of the several parts, breaking the description into short paragraphs, and using a different type for the leading word of the several divisions of a paragraph, the eye of a person accustomed to his works glances immediately to the information that is required.
>
> These real improvements, added to the industry which he manifested in publishing the successive improvements of his system, and the cheapness of his works, in which the expense of figures was avoided, brought his system into vogue, particularly in Germany and England.[272]

Linnaeus's works were more practical for learning and teaching botany than large, expensive volumes that could not be taken into the field.

Despite these advantages, in many ways the sexual system grew to be seen less and less as a representation of plants' relationships or even a handy identification scheme so much as just a general repository for information about plants. This was particularly the case

[269] E.g. Linnaeus 1754, "Editio quinta ab auctore reformata et aucta" ed..
[270] E.g. Linnaeus 1764, 3 ed..
[271] Father of John Edward Gray (1800-1875) of the British Museum, who contributed information about the non-vascular cryptogams to his father's botanical work (Stearn 1989, p. 31).
[272] Gray 1821, pp. 21-22.



1188 PALMÆ PENNATIFOLIÆ.

PENNATIFOLIÆ.

CYCAS.

circinalis. 1. CYCAS frondibus pinnatis circinnalibus: foliolis planis.
Cycas frondibus pinnatis, foliolis lineari-lanceolatis, stipitibus spinosis. *Hort. cliff.* 482. *Fl. zeyl.* 393. *Roy. lugdb.* 5.
Palma indica, caudice in annulos protuberante distincto. *Raj. hist.* 1360.
Arbor Zagoe amboinensis. *Seb. thes.* 1. *p.* 39. *t.* 25. *f.* 1.
Tessio. *Kæmpf. jap.* 897.
Olus calappoides *Rumpf. amb.* 1. *p.* 86. *t.* 22. 23.
Todda-pana f. Mouta-panna. *Rheed. mal.* 3. *p.* 9. *t.* 13. -- 21.
Habitat in India.
Foliatio circinalis more Filicum peragitur.

COCOS.

nucifera. 1. COCOS frondibus pinnatis: foliolis replicatis.
Coccus frondibus pinnatis: foliolis ensiformibus margine villosis. *Hort. cliff.* 483. *Fl. zeyl.* 391. *Roy. lugdb.* 4.
Palma indica coccifera angulosa. *Bauh. pin.* 502.
Palma indica nucifera. *Bauh. hist.* 1. *p.* 375.
Calappa. *Rumpf.* 1. *p.* 1. *t.* 1. 2.
Tenga. *Rheed. mal.* 1. *p.* 1. *t.* 1. 2. 3. 4.
Habitat in Indiæ *paludosis, umbrosis.*
Foliola omnia (excepto utrinque infimo) retro-plicata sunt, contra ac in sequente.

PHOENIX.

dactylifera. 1. PHOENIX frondibus pinnatis: foliolis complicatis.
Phœnix frondibus pinnatis: foliolis alternis ensiformibus basi complicatis, stipitibus compressis dorso rotundatis. *Hort. cliff.* 482. *Hort. upf.* 306. *Fl. zeyl.* 390. *Roy. lugdb.* 5.
Palma major. *Bauh. pin.* 506.
Palma dactylifera major vulgaris. *Sloan. jam.* 174. *Raj. hist.* 1352.
Palma. *Bauh. hist.* 1. *p.* 351. *Dod. pempt.* 819.
Palma hortensis mas. *Kæmpf. amæn.* 688. *t.* 1. 2. *f.* 1. 2. | Palma hortensis fœmina *Kæmpf. amæn.* 686. *t.* 1. 2. *f.* 2. 10. 11.
Habitat in India.

ARE

Figure 12. A page from the first edition of Linnaeus's *Species plantarum* (1753), showing trivial (species) names in the margins and generic names in bold and capital letters both centered before and beginning each description. Image © Sara Scharf, courtesy of the Thomas Fisher Rare Book Library, University of Toronto.

by the end of Linnaeus' lifetime, when the natural method was in vogue and Linnaeus's *Genera plantarum* alone was almost 600 pages. By then, the sexual system's standardized format and wide audience were its most valued features. Although some authors continued to arrange their local floras according to the sexual system, as the "simplest method" for plant identification, "useful in the field as well as in the closet,"[273] such praise applied only to works restricted enough in scope to enable them to be portable. For instance, the physician John Berkenhout (1726- 1791)[274] wrote in the introduction to a book of his based on a modification of Linnaeus's sexual system that "The young Botanist ... is to consider this Book, not as a sufficient system of English Botany, but rather as an index to that branch of Natural History; as a pocket-companion in his botanical excursions." He left out synonyms and references to keep the size of the book down so that it would be small enough to be used in the field, assuming and suggesting that students keep copies of Linnaeus's *Species plantarum* and Hudson's *Flora anglica* – both large reference works – at home for research purposes.[275]

When, at the turn of the 19th century, the sexual system was used in larger works, standardization again was key, and Linnaeus was praised instead for his comprehensiveness, good plant descriptions and synonym tracking.[276] But the sexual system had become so loaded with exceptions to its own rules by that time that botanists gave up on praising it for simplicity. Frustration with the sexual system and others like it because of these very issues was what drove botanists to experiment with other ways of identifying plants and representing their relationships.

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[273] Hooker 1830, p. vii.
[274] Henrey 1975, vol. 1, pp. 113-114 provides biographical information on Berkenhout.
[275] Berkenhout 1770, vol. 2, pp. iii-iv. This book was somewhat popular in England. It was apparently the first botanical work that Sir James Edward Smith used (Henrey 1975, vol. 1, p. 113).
[276] E.g. Lamarck and Mirbel 1803, vol. 2, p. 110.

Sexual system dysfunction: the complicated consequences of using a simple scheme

> *It seems to me that there is a confusion of ideas in what is urged in favour of the Linnean system, and that its theoretical simplicity is mistaken for practical facility of application. That the principles of the Linnean system are clear, and simple, and easily remembered is indisputable; that student indeed must be remarkably dull of apprehension, who could not master them in a day. But is its application equally easy? that is the point.*[277]

Although the description of the sexual system I have presented so far makes it look clear-cut and strictly hierarchical, it was anything but in practice. Boon that it was for botany, its flaws were hard to ignore. Only botanical dabblers believed it was the ultimate solution for categorizing living beings. While the identification of most European flowering plants using the sexual system is indeed a straightforward process – many commentators compared the sexual system to a dictionary, with which anyone could look up the name of a plant[278] – the sexual system was full of exceptions right from its first appearance in the first edition of *Systema naturae* in 1735. It also acquired more and more exceptions in Linnaeus' subsequent works, owing to both increased numbers of known species and greater quantities and types of information about species already known. And Linnaeus' haphazard approach to including anomalous taxa in his system did not help matters. Botanists familiar with using it were accustomed to having problems identifying plants. These problems stem from several sources. Most of the problems are common to all hierarchical schemes. I will discuss these first, and then explain how they played out in the sexual system.

Problems with hierarchical arrangements in general

No other paper-based technique is capable of guiding someone to identify an unknown plant based on the features in the plant itself with the degree of precision that a

[277] Lindley n. d. (between 1830 and 1845), 2 ed., pp. vi-vii.
[278] E.g. Duméril 1806, pp. 63, 65, Smith 1821, p. 32, Stillingfleet 1977, 3 ed., p. xxiii.

hierarchical arrangement can provide, so it is no surprise that they were popular. But every technique has its problems.

Hierarchy split up in books

One of the unavoidable issues associated with hierarchical arrangements is that users must approach them in a linear way because of printing constraints. Once a hierarchical arrangement reaches a certain size, it must be broken up physically and the components put into a linear order so as to be printed on separate pages. Some botanists complained that the necessity of printing botanical information in a sequential way implies that they believe in a linear order of nature, when that is not what was intended at all. A.-P. de Candolle, for instance, wrote, that "the relationships among living things are much more multiplied than it seems that the form of our books can indicate."[279]

Features must be examined in a pre-set order

Another problem with printed hierarchical schemes is that, regardless of whether the arrangement is printed on one page or many, a specimen's features must be examined in a particular order in order to use them to identify it. The choice of which features to focus on and the order in which they are presented are up to the author of the scheme, not the user. For instance, someone looking up a plant with the sexual system must look first at the number and arrangement of stamens in the flower to determine its order, and then the number of pistils, to determine the class. A user cannot look first at pistils and then at stamens, or first leaves and then pistils, or whatever else he or she finds most convenient. As well this linear mode of access provides no room for error.[280] There is no way for someone to trace back through the process to find out where they went wrong, say, for instance, by miscounting the number of stamens and looking under the wrong class. In such a case, it

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[279] Candolle 1813, p. 199. He says almost the same thing on page 201 as well.

[280] Most hierarchical arrangements, especially keys, do not provide a way for users to trace back the choices they have made if they suspect that they have made mistakes. This applies to the majority of 20th- and 21st-century paper keys as well, and I have not found a single key from the 1850s or earlier with this ability built in.

might not be obvious that the plant in question would not be found under the orders of the wrong class. Only by looking at each of the generic descriptions under the class and order he or she found herself examining, and then seeing that the specimen does not belong in any of them would the user realize that something was wrong. When users get stuck like this, they have to start from the beginning again. And getting stuck in the identification process is practically a certainty if one wishes to identify an organism that happens not to be in the scheme, or a specimen with missing features, if the scheme uses those particular features for diagnostic purposes. Organisms too poorly known to have had those features characterized may also be omitted from such schemes, even though they may already have been described recognizably enough by someone to have an accepted name. Hierarchical schemes work best for organisms that have been studied relatively well.[281]

Users must know technical terms

Even if the hierarchy itself is not memorized, the technical terms should be. Hierarchical schemes need to be accompanied by complete explanations of terms or users will get hung up on the ones they do not understand.

Taxon delimitation

Furthermore, even by the end of the 17th century, naturalists were confronted with the problem that natural groups are often difficult to delimit in simple ways.[282] It was difficult to say where one genus of, for instance, small and inconspicuous sedges ended and another one began. New specimens sometimes exhibited features intermediate between those of groups that had seemed distinct before.[283] Features that had been used to diagnose particular

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[281] The few instances in which I have seen organisms with unknown features included in hierarchical schemes have been those in which the plants in question were dumped in an appendix or *incertae sedis* section, which is only marginally more helpful to users than simply listing the plants' names in the index.
[282] As outlined in the discussion about essentialism in Chapter 1 (p. 53).
[283] An example familiar to supermarket-goers today could be the Asian pear, similar in appearance to an apple though with a normal pear texture.

organisms unambiguously suddenly appeared in new and unexpected places.[284] And many organisms, particularly fungi, could be recognised as distinct from one another only by combinations of inconvenient-to-perceive attributes.

Many botanists did not realize that there was a problem for a long time,[285] though the best of them had the feeling that they were confronting a mismatch between their expectations and what nature was really like. This hunch did not, however, always result in naturalists acknowledging that there was a problem with their expectations. Often they continued to assume that natural groups should necessarily be able to demarcate with appropriate care. Their philosophical inconsistency did not seem to trouble them.

Natural groups should be relatively easy to demarcate

For instance, Adanson wrote that it is "impossible" to produce good quality works in natural history without the use of a microscope because some features of plants and animals are simply too small to see without one, but he then asserted that if an investigator examines multiple aspects of an organism, he will always find at least one feature "remarkable" enough to avoid the need to resort to microscopy at all.[286] In other words, he purported that natural groups necessarily possess features that allow people to demarcate them clearly and without mechanical aids.

Similarly, Antoine-Laurent de Jussieu held the assumption that the best arrangement of plants will be most convenient for naming them and the most reflective of their relationships. But a group of plants must be well-demarcated to be named, while Jussieu believed that nature was continuous such that all forms were linked by imperceptible gradations. He believed that any apparent gaps between natural groups in the series he had laid out would eventually be filled by groups intermediate in character. As Peter Stevens has

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[284] Some modern examples, which we would now attribute to convergent evolution, include the pitcher-like structures in the carnivorous plant genera *Nepenthes* and *Sarracenia*, and succulent stems covered in spines in both cacti and euphorbs.

[285] E.g. Henderson 1832, Lefébure 1814, and also the zoologist Fleming 1822.

[286] Adanson 1966, p. cxciv.

pointed out, "Despite Jussieu's assertions, characters very frequently do not delineate groups, even when they are described as characterizing them....There is clearly a conflict between the family as represented by a description and the family as part of the natural series."[287] Jussieu, however, did not seem to be aware of, or bothered by, any such conflict. "Indeed," Stevens wrote, "almost every feature that Jussieu emphasized as being important in delimiting major groupings in the natural series transgresses slightly the lines dividing groups. Groups and characters are not coterminous, despite his claims."[288]

By the early 19th century, the belief that we should be able to circumscribe natural groups relatively easily was giving way to practical realities. Perhaps the best example of this change is furnished by the respected entomologist, the Reverend William Kirby. Kirby was a natural theologian. He even wrote a Bridgewater treatise discussing the habits of insects as a manifestation of God's work. Kirby believed that the creator had furnished organisms with particular signs indicating where they belong in the plan of nature. He explained that God had created all life and put every living thing "into a harmonious system, every member of which, although it has a separate place and office assigned it, is connected, by certain common marks and characters, with those which precede or follow it." Though Kirby's belief in God-given signs in nature was extreme, he was typical of working naturalists of his day in conceding that taxa were hard to delimit sharply in practice. The passage quoted above, for instance, turns into a lament that modern, fallen man has lost the Adamic ability to "discern the intention of his Creator: in this world and its productions, seen in their various affinities and economies... so that to him, the page of creation was a revelation by natural symbols and types." But despite our present-day degeneracy, there was hope: "if that glorious day of true and genuine science should ever come, we shall then behold each natural object in its proper place; we shall learn its history, economy, and uses,

[287] Stevens 1994, p. 58.
[288] Stevens 1994, p. 58.

its moral and spiritual signification."[289] It is almost as if Kirby believed that perfect delimitation of groups of organisms would be one of the effects that the return of the messiah would bring to the world. The more that naturalists tried to do a good job, the closer they would come to that joyous day, even if there was no guarantee that it would come in any particular person's lifetime.

Kirby was walking a fine line, trying to balance his strong beliefs about how nature was structured with his daily experience as a naturalist of the first order, constantly confronted with diversity and change. He and other naturalists like him thought hard about how to delimit the groups they saw in the most reasonable ways possible. But the more organisms that became known to science, the worse this problem became. Many organisms new to science had features intermediate between groups that had already been described. More features had to be examined in order to distinguish them all. Cross-references and/or double entries could keep track of such organisms within hierarchical arrangements, but they made schemes more complicated, more difficult to memorize, and less aesthetically pleasing.

Cross-references are blemishes and foster index learning

Because naturalists strove to present the groups they described in a meaningful order, they frowned on schemes that included cross-references to help users locate plants or animals split from the other members of natural groups to which they were seen to belong. Natural groups were meant to be kept together. The use of characters that split natural groups and necessitated cross-references was seen as a mark of laziness or incompetence of the schemes' makers.

This disdain for cross-references also fits in with the widespread belief that the order of nature should be most natural to the mind. It would necessarily be sufficiently easy to use on its own. It might not be as simple to use as a scheme in which only one or two characters were considered, but it should be comprehensible with appropriate care and attention. In this

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[289] Kirby 1802, vol. 1, pp. 1-3.

milieu, the use of additional text-navigation tools, such as indexes, was seen as a blemish by some naturalists.

Not only were indexes shortcuts that would enable insufficiently-educated people to skim through botanical works without understanding the profound ideas embodied by the order in which plant groups were presented, but they were seen as a smokescreen used by authors who had skimped on profound ideas as well. If an author knew that his audience was interested only in index learning, there was no need for him to attempt anything deeper. He could then justify his unphilosophical methodology by saying that he was trying to make the material accessible. Linnaeus would have exposed himself to this kind of criticism even more than he did had he not emphasized the importance of maintaining natural genera so much. But the maintenance of natural genera created its own set of problems, problems that only added to the difficulty of using the sexual system to identify plants.

Problems with Linnaeus's sexual system in particular

These problems are, first, that the sexual system is based on flower features, but most plants produce flowers for only part of their lives. Flowers may also be too small to see, modified into unrecognizable structures, or inaccessible. Some plants also never produce flowers. The sexual system also makes finding plants with unusual numbers of stamens or pistils difficult because natural genera often fit poorly into artificial orders and classes. Linnaeus did not provide adequate help for users to locate anomalous species in those natural genera in either *Genera plantarum* or *Species plantarum,* necessitating the use of multiple reference texts (or pure guesswork) to identify these plants. Finding generic descriptions among the large numbers of species per genus or large numbers of genera per order also posed problems.

Both Linnaeus and subsequent botanists influenced by Linnaeus encountered these problems and tried to work around them. These problems with the sexual system and their attempted solutions both tell us something about the priorities that 18th century botanists had.

Flower issues

Linnaeus based the sexual system on floral features. He believed that these were the most informative for classification, even if they were seasonal, and often small and difficult to see. He was in favour of using whatever technology was available – microscopes or lenses included – to help see the flower parts in instances where close examination would be most useful to identify natural groups.[290] This is just one of many instances in which Linnaeus kept natural groups together at the expense of keeping the sexual system easy to use. He received a great deal of criticism on this account, particularly after Buffon decided to mock the sexual system's usefulness in the field by remarking that someone using it would need a microscope to identify a tree.[291]

In addition, as many of Linnaeus's critics were quick to point out, flowers may be seasonally or yearly absent, they may be modified into confusing structures (such as in the fig),[292] they may be inaccessible (particularly in the case of preserved specimens and of tall tropical trees) and many plants do not have flowers at all (cryptogams). Adanson in particular was critical of the sexual system's inability to cope with the diversity of plants that he encountered on his trip to Senegal. Many of these had interesting structures that he had not seen before and was not sure how to fit in the sexual system. After his experiences in Senegal, Adanson came to believe that European plants are not typical of those found all over the world. He pointed out that some plant families are absent or nearly absent in tropical places. For instance, he found no mosses or buttercup relatives or orchids in Senegal, and only one umbellifer, whereas plants in these groups are very common in Europe.[293] If the sexual system could not accommodate the majority of plants found in these regions, how could it be expected to be of any use on voyages to other continents?

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[290] Linnaeus 2003, p. 143 § 192.
[291] Buffon 1749, vol. 2, pp. 27-28. This was reiterated by, e.g. Virey 1818, vol. Tome XX. [MED-MIN]. p. 479.
[292] Linnaeus had put the fig in the Class Cryptogamia by itself in the order Plantae in his *Hortus cliffortianus* because its flowers were not obvious (Linnaeus 1737d, p. xi). See Figure 10.
[293] Adanson 1966, p. clvii.

Generic descriptions often out of date

This problem was compounded by out-of-date generic descriptions. Sometimes reading a generic description would not be helpful in placing a plant. Linnaeus was not able to keep up with the influx of new species over the years. In many cases, he did not revise generic descriptions when he added new species to genera. As a result, those generic descriptions applied only to the species in the genera that Linnaeus had identified first. Many of Linnaeus's critics, for instance, Lamarck and Medikus, were incensed by the mismatch between generic descriptions and contents that resulted.[294]

Trouble finding anomalous plants

Mismatches between natural genera and artificial orders and classes

Another manifestation of the same problem arose from Linnaeus's firm conviction that natural genera had a real existence in nature and that it was important to keep related species together.[295] As a result, natural genera did not always work well with Linnaeus's artificial orders and classes. In hierarchical arrangements, the distribution of taxa is dependent on whichever of their characters are examined at each level of the hierarchy. In a strictly artificial hierarchy, such as one founded upon "logical division," no higher-level group will contain a lower-level group with properties in contradiction to its definition. But Linnaeus used natural genera in a system where the orders and classes were artificial. This created a problem for people following the hierarchy in order to identify plants.

Linnaeus differentiated the classes on the basis of anther number and arrangement, and the orders on the basis of pistil number. In certain genera of plants, some species differ in anther or pistil number from congeners to which they are very similar in every other respect.[296] If

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[294] E.g. Medikus, cited in Stafleu 1971, p. 261 and Lamarck Lamarck 1778, vol. 1, p. xviii. Mary P. Winsor discusses Linnaeus's methodology in this respect further in Winsor 2003.

[295] E.g. Linnaeus 2003, p. 114 § 159.

[296] Colin Milne 1770 lists examples of 24 such species in his *Botanical Dictionary* (under ESSENTIAL CHARACTER). Tim Dickinson points out that the North American species of *Crataegus* (hawthorn) that

the sexual system were followed strictly, two species in the same natural genus but with different numbers of pistils would turn up under different orders, and those with different numbers of stamens would turn up under different classes. Linnaeus followed nature rather than the artificial tenets of his scheme, however, and had species that belonged to the same natural genus printed as a cluster in all of his books. The result was that readers unfamiliar with those anomalous plants would end up looking in the orders or classes with the right numbers of pistils or stamens, not where the plants' descriptions were actually printed. One of the most problematic genera in this respect was *Valeriana*, valerian. The species Linnaeus had described first had three stamens, so he put it in the class Triandria. He put all subsequent species of *Valeriana* he described in with it despite their differences in stamen number. For instance, *Valeriana rubra* has only one stamen, and should therefore be located in the class *Monandria*, but Linnaeus instead put it in Triandria with its three-stamened relatives (See Figures 13 and 14). If the sexual system were to be followed strictly such that only plants with the right numbers of stamens and pistils were to be admitted to the classes and orders, the different species of *Valeriana* would have to be split up. As Colin Milne wrote,

> This genus affords one of the strongest objections to the Linnaean method of arrangement, and distribution of the genera. No less than six different species pertain to other classes [than Triandria]. The diversity too that obtains in the other parts of fructification is so remarkable, that it is not possible to give a generical description that shall include every species. Such is the inconvenience of adopting natural genera in an artificial method.[297]

John Berkenhout explained the type of problem that Linnaeus caused for botanical novices by keeping related species together in natural genera instead of rigorously following his own class and order definitions. "Supposing him, therefore, well-acquainted with the characteristics of the Twenty-four Classes," he wrote,

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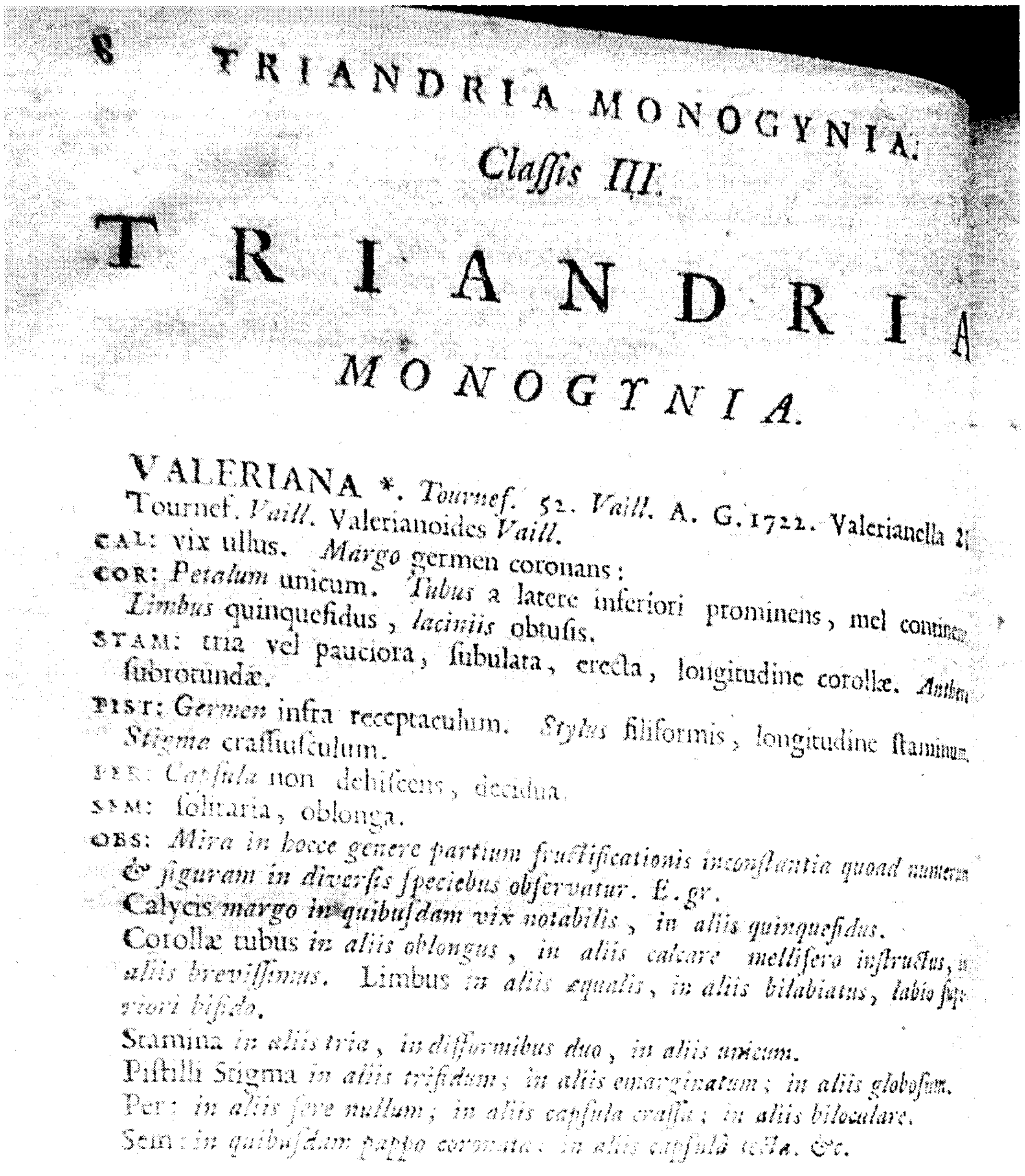
Linnaeus described are another example of how Linnaeus "wilfully ignored variation at odds with his generic concept," and that Medikus also criticized Linnaeus for this. (Personal communication).

[297] Milne 1771, p. 254.



8 TRIANDRIA MONOGYNIA.

Classis III.

TRIANDRIA

MONOGYNIA.

VALERIANA *. *Tournef.* 52. *Vaill.* A. G. 1722. Valerianella 2; Tournef. *Vaill.* Valerianoides *Vaill.*

CAL: vix ullus. *Margo* germen coronans:

COR: *Petalum* unicum. *Tubus* a latere inferiori prominens, mel continens. *Limbus* quinquefidus, *laciniis* obtusis.

STAM: tria vel pauciora, subulata, erecta, longitudine corollæ. *Antheræ* subrotundæ.

PIST: *Germen* infra receptaculum. *Stylus* filiformis, longitudine staminum. *Stigma* crassiusculum.

PER: *Capsula* non dehiscens, decidua.

SEM: solitaria, oblonga.

OBS: *Mira in hocce genere partium fructificationis inconstantia quoad numerum & figuram in diversis speciebus observatur. E. gr.*

Calycis *margo in quibusdam vix notabilis, in aliis quinquefidus.*

Corollæ tubus *in aliis oblongus, in aliis calcare mellifero instructus, in aliis brevissimus.* Limbus *in aliis æqualis, in aliis bilabiatus, labio superiori bifido.*

Stamina *in aliis tria, in difformibus duo, in aliis unicum.*

Pistilli Stigma *in aliis trifidum; in aliis emarginatum; in aliis globosum.*

Per: *in aliis fere nullum; in aliis capsula crassa; in aliis biloculare.*

Sem: *in quibusdam pappo coronata; in aliis capsula tecta. &c.*

Figure 13. Entry for the genus *Valeriana* on page 8 of the first edition of *Genera plantarum* (1737). Linnaeus has described the stamens ("STAM") as "three or fewer" and added an observation ("OBS") that the numbers of floral parts in this genus are prone to variation. Image © Sara Scharf and courtesy of the Thomas Fisher Rare Book Library, University of Toronto.

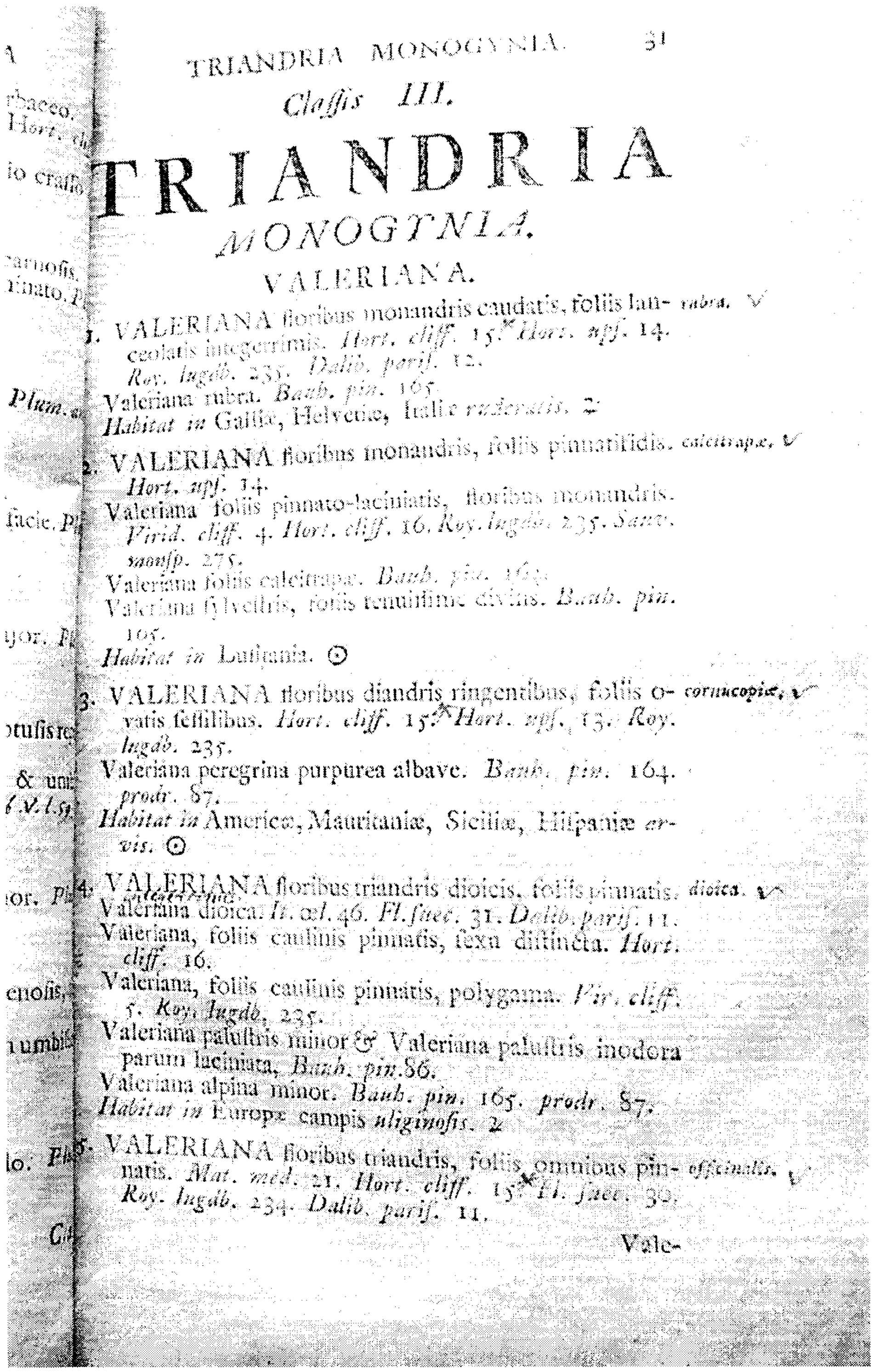


Figure 14. Page 31 of the first edition of *Species plantarum* (1753): the beginning of the Triandria monogynia section showing the entry on species in the genus *Valeriana*. Supposedly all plants in Triandria monogynia have three stamens and one pistil. However, the very first species listed, *V. rubra*, has one stamen, as does *V. calcitrapa,* and *V. cornucopiae* has only two stamens. Image © Sara Scharf and courtesy of the Thomas Fisher Rare Book Library, University of Toronto.

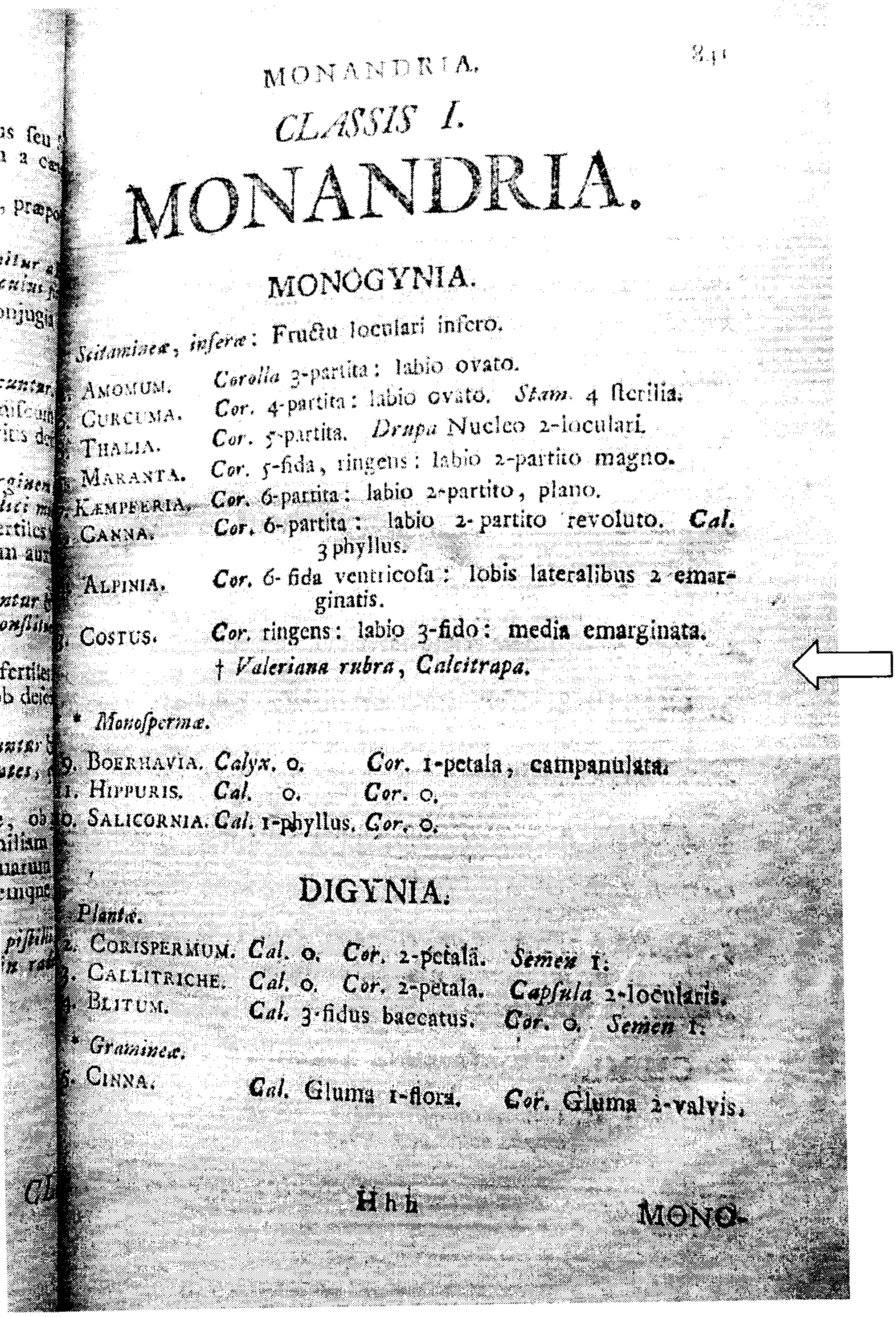
> having gathered a plant with which he is unacquainted he first examines the parts of fructification, and, without much difficulty, determines the Class. He then fixes the Order; and so proceeds. But if it should unfortunately happen, that the whole Class and Order to which his plant evidently belongs exhibits no such Genus, he is immediately bewildered, and begins to flatter himself that he has discovered a plant unknown to Linnæus: Nor is there any method of finding his mistake, except by comparing the plant in question with every generic character in the whole System.[298]

This problem applies even more forcefully for plants in the appendices to the sexual system. Linnaeus never mentioned the existence of these appendices in his "*Clavis classium*," or table of contents, included at the front of the first through fourth editions of *Genera plantarum* and all editions of *Species plantarum*, although at least he consistently listed the names of the plants in the appendices in the alphabetical indexes at the back of his books. Users ignorant of the existence of an appendix or of the name of the plant they were trying to locate information about would have no way of finding it unless they flipped through the book, found the appendix, and then read through every entry in it.

No cross-references

This clustering of natural genera at the expense identifying anomalous species using the sexual system's hierarchy did not necessarily have to present a problem. Cross-references under the orders and classes where those anomalous species would appear could indicate where those species' descriptions were. But although Linnaeus sometimes put cross-references in his publications, he did not do so consistently. There are cross-references to anomalous plant species in Linnaeus's *Methodus sexualis*[299]and editions of the *Systema naturae* published at least since 1748, as shown in Figure 15.[300] Most frustratingly, there are no cross-references in any edition of *Genera plantarum* or *Species plantarum* published

[298]Berkenhout 1770, vol. 2, p. v. This is a simplification. I discuss how the mistake could be found below.
[299] Linnaeus 1737c, bound with the first edition of *Genera Plantarum*.
[300] There are no cross-references in the first edition (Linnaeus systema 1735), but they are present in the sixth Stockholm edition (Linnaeus 1748, Editio sexta, emendata et aucta ed.), sixth Leipzig edition (Linnaeus 1748, Secundum sextam Stochholmiensem emendatam & auctam editionem ed.), 1756 Lugduni Batavorum edition (Linnaeus 1756, Editio multo auctior et emendatior ed.), and subsequent editions (Linnaeus 1766, 12 ed., vol. 1, Linnaeus 1767, 12 ed., vol. 2, Linnaeus 1768, 12 ed.), including the *Opera Varia* (Linnaeus 1758) modified fourth edition. (I have not had a chance to examine the second (1740) or third editions).



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MONANDRIA.

CLASSIS I.

MONANDRIA.

MONOGYNIA.

Scitamineæ, inferæ: Fructu loculari infero.

1. AMOMUM.	*Corolla* 3-partita: labio ovato.
2. CURCUMA.	*Cor.* 4-partita: labio ovato. *Stam.* 4 sterilia.
3. THALIA.	*Cor.* 5-partita. *Drupa* Nucleo 2-loculari.
4. MARANTA.	*Cor.* 5-fida, ringens: labio 2-partito magno.
5. KÆMPFERIA.	*Cor.* 6-partita: labio 2-partito, plano.
6. CANNA.	*Cor.* 6-partita: labio 2-partito revoluto. *Cal.* 3 phyllus.
7. ALPINIA.	*Cor.* 6-fida ventricosa: lobis lateralibus 2 emarginatis.
8. COSTUS.	*Cor.* ringens: labio 3-fido: media emarginata.
	† *Valeriana rubra*, *Calcitrapa*.

* *Monospermæ.*

9. BOERHAVIA.	*Calyx.* o.	*Cor.* 1-petala, campanulata.
11. HIPPURIS.	*Cal.* o.	*Cor.* o.
10. SALICORNIA.	*Cal.* 1-phyllus.	*Cor.* o.

DIGYNIA.

Plantæ.

12. CORISPERMUM.	*Cal.* o.	*Cor.* 2-petala.	*Semen* 1.
13. CALLITRICHE.	*Cal.* o.	*Cor.* 2-petala.	*Capsula* 2-locularis.
14. BLITUM.	*Cal.* 3-fidus baccatus.	*Cor.* o.	*Semen* 1.

* *Gramineæ.*

15. CINNA.	*Cal.* Gluma 1-flora.	*Cor.* Gluma 2-valvis.

Hhh MONO-

Figure 15. A chart on page 841 in volume 2 of the 10th edition of *Systema naturae* (1758), showing the beginning of the Monandria (plants with one stamen) section. Note the cross-reference indicating that *Valeriana rubra* will key out under Monandria monogynia. All of the species of *Valeriana* are listed under Triandria monogynia. Image © Sara Scharf and courtesy of the Thomas Fisher Rare Book Library, University of Toronto.

under Linnaeus's watch.[301] These two works are the ones that botanists would be most likely to purchase and use expressly to identify plants.

No double entries: pressure to keep species in natural genera physically printed together

In the absence of cross-references, Linnaeus still could have allowed anomalous species in natural genera to appear under the classes and orders that reflected their numbers of stamens and pistils had he employed double entries and included the genus under all relevant classes and orders. But Linnaeus did not do this. Since he and the majority of botanists after him acknowledged the benefits to their discipline of using natural genera, splitting them was anathema. Still, some followers of Linnaeus instituted a twist on the cross-referencing system that Linnaeus had sporadically used. For instance, John Berkenhout printed some species in one part of their texts and others in another while keeping the same name for the genus and explicitly writing that all of the species formed a natural genus.302 This use of double entries was apparently offensive to most other botanists. Linnaeus did not try it, and neither did any other modifier of his sexual system (to my knowledge). The consensus seemed to be that anyone who presented organisms in his book in an order other than the one assumed to be that of nature demonstrated something less than mastery of the subject, no matter what his protestations. Double entries would not be philosophical enough.

This is not to say that Linnaeus's sexual system was philosophically consistent in any way, even with its own explicitly-written tenets. The way Linnaeus treated the plants now known as palms and cycads embodies everything wrong with the sexual system.

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[301] Linnaeus 1737b, Linnaeus 1742, Editio secunda aucta & emendata ed, Linnaeus 1743, 2 ed, Linnaeus 1752, 4 ed, Linnaeus 1754, "Editio quinta ab auctore reformata et aucta" ed, Linnaeus 1764, 6 ed, Linnaeus 1753, Linnaeus 1762-1763, 2 ed., vol. 1 and 2, Linnaeus 1764, 3 ed..

[302] Berkenhout 1770, vol. 2. John Hill and John Stuart, Earl of Bute also used double entries in their keylike schemes (Hill 1770, 2 ed., vol. 1, Stuart 1787).

Palms and cycads

The palms were relatively unknown to scientific botany in the early 18th century. Although their vegetative morphology is quite distinctive, European botanists had few chances to see their inflorescences. There is only one species of palm – Chamaerops humilis – native to Europe, and it grows only around the Mediterranean. Still, Linnaeus knew that the palms are flowering plants even though he did not know much about their flowers. Unlike the cryptogams, or plants without obvious sexual parts, palms have huge reproductive structures. Palm inflorescences can be meters long and contain thousands of flowers. But even though some palms were grown in northerly European greenhouses, Linnaeus did not have ready access to palm flowers on these plants. He did not have access to dead palm flowers, either. The specimens brought back from exploratory voyages consisted almost exclusively of leaves, as it is quite difficult to preserve fleshy plant parts, such as palm flowers, under hot and humid conditions. They simply rot. Even into the 19th century, collectors were often unable to access the inflorescences because the trees were not blooming, or they were hard to access due to their height or where they grew.303 This lack of information prevented the palms from fitting within Linnaeus' sexual system except as a special group.

Initially Linnaeus placed the palms in the Vagae ("things of uncertain position") with other, completely unrelated plants.304 Soon afterward he moved them to their own "Appendix Palmae."305 This palm appendix was his way of tacitly recognizing their distinctiveness and coherence as a natural group. However, the palm appendix never appears in the "Clavis classium," or key to the classes of the sexual system, that was published as a table of contents at the beginning of the first through fourth editions of Genera plantarum.

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[303] Lamarck and Mirbel 1803, vol. 2, pp. 105-106.
[304] Linnaeus 1737d, p. x.
[305] Linnaeus 1737b, p. 354.

This was a serious flaw in terms of using the sexual system as an information retrieval device.

By 1737, Linnaeus also knew enough about the flowers of five of the palm genera to enable him to place them in the main body of the sexual system, had he so desired.306 But instead he highlighted the distinctiveness and coherence of the palms again by keeping them in their own appendix. Fourteen years later, he wrote plainly that the palms form a distinct group, writing that: "VEGETABILIA comprehendunt Familias VII: Fungos, Algas, Muscos, Filices, Gramina, Palmas, Plantas." That is, roughly, "plants comprise seven families: fungi, algae, mosses, ferns, grasses, palms, and plants proper." 307 The first four natural orders he mentions, fungi, algae, mosses and ferns, all fall under his 24th class, the cryptogams. They were accorded no higher rank during Linnaeus' lifetime. In the 5th edition of Genera plantarum, however, Linnaeus wrote in the introduction to the Cryptogamia that these four groups were very diverse structurally and should each be considered as a class.308 Linnaeus was sending very mixed messages. While he recognized these groups as distinctive from one another and from everything else, he suppressed them in rank so that the sexual system could continue to divide up classes based on fructification or lack of it. This is a case of Linnaeus privileging the hierarchical structure of the sexual system over what he regarded as the natural relationships of the plants being classified.

As for the palms and their unique features, Linnaeus kept them distinct even in sixth edition of the *Genera plantarum*.[309] They remained in the palm appendix even though this time he included notes next to the generic descriptions of several of the palms indicating the different classes and orders in the sexual system where they should go based on new

[306] Linnaeus 1737b. He wrote that *Phoenix* has three filaments in its male flowers but he had not examined its female flowers, and that *Chamaerops* has six filaments and three pistils in the hermaphrodite plant, and six filaments in the male plant. *Corypha* has six filaments and one pisitil, while *Coccus* [sic for *Cocos*] has six filaments and one pistil, with male, female and hermaphrodite flowers on same plant. *Borassus* has six filaments and one pistil, with male and female flowers on separate plants, and *Caryota* has many filaments and one pistil, with separate male and female flowers on same plant.

[307]Linnaeus 1751, p. 37.

[308] Linnaeus 1754, "Editio quinta ab auctore reformata et aucta" ed., p. 483.

[309]Linnaeus 1764, 6 ed., pp. 571-576 [576 is misnumbered 566].

information he had about their flowers. He did not, however, put cross-references in those orders to indicate that some plants with the appropriate numbers and arrangements of stamens and pistils could be found in the palm appendix instead of where a user might expect to find them. Linnaeus was clearly more interested in maintaining the integrity of this particular group, with its distinctive leaves and trunk, than he was in the integrity of his hierarchical scheme or its use for plant identification.

Linnaeus added a new genus to the palms in 1753. This genus is named *Cycas*.[310] We now know it as a cycad, an ancient lineage of plants related to both conifers and flowering plants but belonging to neither. While true palms are flowering plants that generally produce large drooping panicles of flowers each with parts in multiples of three, cycads produce scaly-looking cones. But when Linnaeus first encountered *Cycas*, he knew nothing of its reproductive structures. This is evidenced by how he treated it in his books. In *Species plantarum* (1753) he described only its leaves, as shown in Figure 12 above. In the fifth edition of *Genera plantarum*, published the next year, the entry under the name *Cycas* is a blank space, whereas in the sixth edition, the *Cycas* entry contains nothing but the plant's name and a complaint that Linnaeus knows nothing of its character, as shown in Figure 16.[311]

Some time between 1754[312] and 1763[313] Linnaeus added another genus of plants now recognized as a kind of cycad to the palm appendix. This was the genus *Zamia*. By 1763,

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[310]Linnaeus 1753, p. 495, genus number 1087. Linnaeus mentioned the genus *Cycas* without comment in *Classes plantarum* (Linnaeus 1738, p. 192) and in *Hortus cliffortianus* (Linnaeus 1737d, p. 482), he noted only where it supposedly grows. He also noted it in *Philosophia botanica* in 1751, but exclusively as an example of a generic name derived from Greek (Linnaeus 1751, p. 174). He did not describe the plant or where it belongs in any of these works. *Cycas* reproductive structures were depicted in Rheede et al's *Hortus malabaricus* (1678-1703), but although Linnaeus commented in 1738 that Plumier and Rheede had illustrated all palms other than those in the genera *Corypha, Borassus, Coccus* [sic], *Chamaerops, Phoenix* and *Caryota* (Linnaeus 1738, p. 490), he did not mention having seen these figures until 1775 (Linnaeus 1775, p. 516).

[311] (Linnaeus 1764, 6 ed., p. 572).

[312]The 5th edition of *Genera plantarum*.

[313]Linnaeus 1762-1763, 2 ed., vol. 1 and 2, p. 1659; the palms do not appear at all in the *Systema naturae* of 1748 or 1756. The 1767 (12th) edition of *Systema naturae* has only three palms in the appendix, and no entries for *Cycas* or *Zamia*.

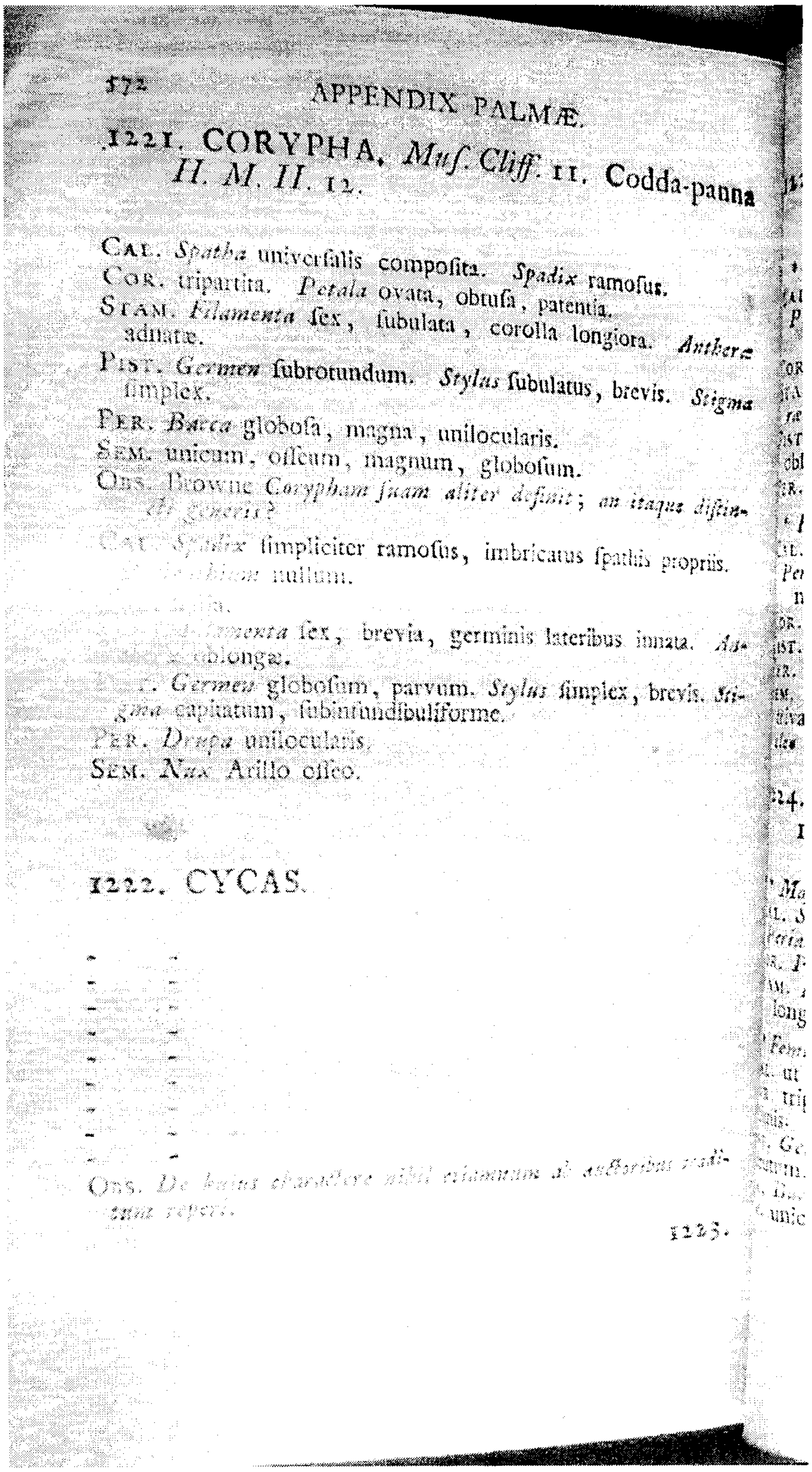

Figure 16. The entry for *Cycas* on page 572 of the 6[th] edition of *Genera plantarum* (1764). Image © Sara Scharf and courtesy of the Thomas Fisher Rare Book Library, University of Toronto.

Linnaeus was familiar enough with *Zamia's* fruiting structures to write that they resemble the cones produced by *Cupressus* (cypress), a conifer that at the time was in the sexual class and order Didynamia gymnospermia.[314] Still, it is easy to see why Linnaeus put these organisms in with the palms, when they could have gone into Didynamia gymnospermia or even the perfectly good category of Cryptogamia. At a gross morphological level, they look very similar to palms (see Figure 17).

Figure 17. *Cycas circinalis* (left) and *Phoenix loureirii* (right), two of many cycad and palm species with similar looking leaves and overall appearances. Photos © and courtesy of K-Palms Inc.

Both *Cycas* and *Zamia* remained lumped with the palms in the palm appendix even though it was evident by 1763 that their fructification is quite un-palmlike and they have a different mode of leaf development: new palm leaves unfold like paper fans whereas new *Cycas* leaflets unfurl in a spiral the way fern fronds do. Linnaeus himself had noted this as early as 1753.[315] In other words, Linnaeus appended *Cycas* to the palm family appendix

[314]Linnaeus 1762-1763, 2 ed., vol. 1 and 2, p. 1659.
[315]Linnaeus 1753, p. 495, genus number 1087.

purely on the basis of its leaf shape and general appearance. The hierarchical structure of the sexual system based on fructification, at least regarding these plants, was not a priority for him.

Linnaeus's treatment of palms and cycads thus highlights several flaws with the sexual system. First, the sexual system does not work to classify or even identify plants, no matter what their size or shape, if the flowers are unavailable. Second, when Linnaeus kept species in natural genera together but without cross-references or double entries or even mentions of the existence of appendices, he made it difficult for users to find anomalous plants even if they tried to key them out by counting stamens and pistils.

No all-in-one book for identifying plants

As described above, it was difficult if not impossible to identify anomalous plants using either *Genera plantarum* or *Species plantarum.* However it was possible to look at the end of the orders in either Linnaeus's *Methodus Sexualis* (1737) or a post-1758 edition of *Systema naturae* for lists of names of anomalous genera and indications of where they were described in *Genera plantarum.* Species could then be looked up in *Species plantarum.* This reliance on multiple texts to identify plants made the sexual system more awkward than it needed to be. Botanical novices were unlikely to know that they had to follow this convoluted procedure to identify certain plants unless they were shown how to do it. This procedure also made teaching botany using Linnaeus's texts problematic: a student would need access to at least three texts, possibly published decades apart. Botanists wished for something simpler.

Trouble finding generic descriptions

Finding a particular genus in a large order, or finding a species in a large genus could be very time-consuming, especially for users who did not already know the genus name or generic characters (in the case of searching within an order) or species name and specific characters (when searching within a genus) of the plant sought. If the user knew the genus or species name, respectively, it would be faster to look it up in the index rather than having to read through every description until finding one that fit. This was especially true if the organism were anomalous, as I describe below.

Book order should reflect natural order

These two sources of trouble were further compounded by the desire Linnaeus shared with his contemporaries of listing species within genera and genera within orders in a series so that adjacent groups would always share features with each other. The idea that nature is a continuum and that natural groups fade into each other via imperceptible gradations – based on the belief in plenitude – had a strong influence on botanical practice. But it was not the only influence. A number of botanists believed that natural groups were discontinuous.316 Yet even these botanists tended to arrange their groups in linear series of sorts. These botanists were most likely swayed by the practical aspects of this approach. That is, that books present information in an inherently linear way, and that keeping like with like was a mark of competence. Splitting natural genera in particular was seen as evidence of incompetence. This even applied to where species descriptions were physically printed in books. Species in natural genera were printed together partially because of peer pressure among botanists.

Inherently linear presentation of information

In the case of the sexual system, the sequence in which the species were listed in each genus in *Species plantarum* and the genera were listed within each order in *Genera plantarum* were up to Linnaeus. He had written that "an order must place next to each other the genera that are more closely connected."[317] The principles he followed in the arrangement of species within genera is likely to be the same; Stafleu wrote that the genera

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[316] Stevens 1994, p. 80.
[317] Linnaeus 1751, p. aphorism 208, cited Linnaeus 2003, p. 150.

in both *Genera plantarum* and *Species plantarum* follow the same sequence as in Linnaeus's *Fragmenta methodi naturalis* (Fragments of a natural method).[318] It appears that the sequence of these natural groups was intended to represent a pattern in nature, though Linnaeus also had suggested at the beginning of his presentation of the *Fragmenta* that "All plants exhibit their contiguities on either side, like territories on a geographical map."[319] Linnaeus was quite typical of botanists of the 18th century in both attempting to represent the order of natural groups in his books in a meaningful linear order while denying that this order was congruent with the true and more complex natural order. The inherently linear structure of printed lists in books – much cheaper to reproduce than diagrams illustrating complex relationships were – can account in large part for this apparent paradox.

Confrontation with messy natural groups led to a divergence of theory and practice

As the 18th century wore on, published classification schemes increasingly reflected the fact that keeping track of cross-references, particularly to features of plants that were not always apparent (i.e. flower parts) was becoming more complicated than keeping track of multiple features that could be used in concert to identify plants during a greater part of their life cycles. In addition, though the binomial nomenclature consisting of generic and trivial names is mnemonically helpful and works well with hierarchical organizations of plants, it only labels the genus and species. But Linnaeus's sexual system and other hierarchical schemes that followed it rely on many more levels of organization. Botanists quickly realized that there were, in fact, too many *genera* of plants to bother memorizing all their names. French Linnaean Marie-Jacques-Philippe Mouton-Fontenille de la Clotte (1769-1837), for instance, wondered, "How is it possible to hope that a man, who is not uniquely

[318] Stafleu 1971, p. 76.
[319] Linnaeus 1751, p. aphorism 77, cited Linnaeus 2003, p. 40.

occupied with this part of Natural History, can keep in memory the names and the attributes of such a great number of plants?" An expert familiar with the rules and conventions of botany may memorize many names, but "he who wishes to acquire solid knowledge in botany must have the courage to restrict himself to a number of genera and species proportionate to the extent of his memory, and, above all, relative to the needs which he fills in society," especially, for example, medicinally useful plants, common plants, or unusual plants.[320]

As a Linnaean, Mouton-Fontenille came to this conclusion decades later than most of his compatriots, and did not emphasize the utility of natural families to reduce the mnemonic load. But most other botanists saw the value in switching from artificial classes and orders to natural classes and orders. This meant that, especially after the 1760s, they increasingly emphasized features inherent in the plants rather than adherence to the step-wise procedures that had originally been devised to make identification easier. With natural genera bursting at the seams, natural families were seen as the way to go.

Natural orders or plant family fragments

The plant family was first introduced by Magnol in 1689 in his *Prodromus historiae generalis plantarum.*[321] In this work he talked of plant families as groups of plants sharing similar forms, analogous to the family-like groups of animals, such as the dogs and pigeonlike birds. Linnaeus was the next botanist to emphasize plant families. He conceived of orders (including natural orders, or plant families) as genera of genera.[322] The order, or family in general, was just another level in the hierarchy that would reduce the number of groups to memorize by an order of magnitude, much as the grouping of species into genera had done earlier.[323] As Adanson put it,

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[320] Mouton-Fontenille de la Clotte 1803, pp. 327-328.
[321] Magnol 1689, p. preface.
[322] Linnaeus 2003, p. 148 § 204.
[323] Atran 1990, pp. 10, 167.

"These families considerably abridge the work of botany, because they are the summary of all present-day knowledge in this science. They reunite the object of all systems ever made and all those yet to be made, and in this they simplify as much as possible the manner of grasping the totality of all generic relationships."[324] Like natural genera, natural families were intended to be constant no matter what botanist's system or method they were in. Also like natural genera, people already familiar with the features of particular groups of plants could use their gestalt impressions of new plants to place them in the correct natural family. This would make botany much easier, proponents of the natural method were sure to emphasize.[325] This technique for identifying plants relied on plant families having a relatively uniform composition. Only genera that were alike in many respects could belong in the same family.

Internally consistent groups

The internal consistency of natural families was the lynchpin of the natural method. It enabled people familiar with plants to see immediately where a plant new to them would belong. For instance, Adanson wrote that natural families would make botany easier than ever because it would make it possible to know "all of botany" without "knowing all plants." "It suffices," he explained,

> to know fundamentally, i.e. in all their parts, 1, 2 or 3 genera of each family, that is, the one that occupies the middle, and two that occupy the extremities, to be conversant with all the diverse forms of plants, and to be able to distinguish the new ones from those which are known, and to place them in their stations. For example, once you know jasmine in depth, you can easily determine that lilac and privet are of the same family.... the same with palms, lilies, grasses, etc. once you know one or two plants from these families.
>
> Moreover, in following this linkage, the connection that there is between these families, you pass by degrees from known things to the unknown, and from one truth to those which depend on it.[326]

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[324] Adanson 1966, p. cxciii.
[325] E. g. Adanson 1966, p. cxciii.
[326] Adanson 1966, pp. cxciii-cxciv.

Botanists, Adanson suggested, could recognize the gestalt of different plant families by becoming familiar with a few members of each used as types. It is very clear from this passage that, not only would the recognition of natural families make use of cognitive pathways that humans tend to use anyway,[327] but the resulting groups would be useful for making predictive inferences by dint of the very same processes.

Perhaps the biggest deciding factor in preferring the natural method, with its main emphasis on empirical research, to more artificial schemes – no matter how ingenious – was this recognition that the more taxa that are added to a natural classification, the more stable it becomes over time. The more organisms a naturalist examined, and the more closely they were examined, the clearer it became which features were informative for keeping like with like within those groups. Organisms that did not exhibit such features were split off. The makeup of each group of organisms consequently became more homogeneous and resistant to change the more attention was paid to it. Conversely, it only takes one new taxon different or deficient in the feature used in an artificial classification, or answering to the same brief characterisation as a taxon already in the classification, to destabilise it.[328] To use a metaphor from gaming, a hierarchical arrangement is like referring to each card in a house of cards according to what layer it is in and how far it is to the left or right. Adding or removing cards changes how to refer to nearby cards. It can even destabilize the entire structure. But with the natural method, each card retains its unique identity regardless of how the cards are laid out. The deck can be reshuffled and reordered according to different principles (suit, face or number, colour, etc.) but each card or "module" (see below) still goes by the same name, based on some feature it has regardless of where it is. All of this taken together shows that what made natural families of plants "natural" is that each family was circumscribed so that it was self-contained.

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[327] Rips 2001, pp. 834, 836, Daston 2004, p. 179, Rosch 1978, – Lamarck also describes putting families together in a similar way, as described in Stevens 1994, p. 15.
[328] Brown 1867a, p. 12, Kirby 1802, vol. 1, p. xiv.

Modularity: the hallmark of the natural system is keeping like with like, not with any other ordering principle (e.g. simple to complex or vice versa, Quinarian circles, etc).

"Natural" groups, be they genera or families, are held together by similarities among their members. This internal cohesion allows the groups to be picked up and moved around, so to speak, without destroying their internal structure. They could be rearranged alphabetically, or according to the relationships seen to exist among them at any point in time, or according to other criteria, all without changing their compositions or (often) even their names. The modularity of natural groups is the hallmark of the natural method. It is the reason why naturalists could hold so many conflicting views of the forms of relationships that existed among groups, and yet could continue to talk to each other intelligibly about those groups. "Among plants," Lamarck explained,

> the natural method is extremely difficult to establish because of the obscurity that reigns in the interior characters of organization of these living things, and in the differences that plants of different families can offer in this respect. Nevertheless, since the learned observations of M. Antoine-Laurent de Jussieu, we have made great steps toward the natural method in botany. Numerous families have been formed according to the consideration of relationships. But it remains to determine solidly the general disposition of all these families among themselves, and, consequently, that of the overall order. In truth, we have found the beginning of that order, but the middle and end of the main order are still at the mercy of arbitrary [decisions].[329]

Botanists saw that the best way to figure out the overall pattern of natural families would be to "think globally, act locally:" they continued to amass information and figure out in which family each plant belonged, with the hope that the more families were described, the clearer the pattern among the families would be. Although some naturalists, particularly in the 1780s, believed that this would eventually result in a linear series of families, this idea was not popular for long. There were too many similarities among "distant" families for naturalists to argue convincingly that all of them would be best represented in one

[329] Lamarck 1809, vol. 1, p. 24.

unambiguous series. By the early 19th century it was widely acknowledged that linear orderings of natural families were instead an undesirable artefact of publication in books.

Linearity in the natural method

Expecting a natural series

Twentieth- and 21st century researchers of 18th and early 19th century "natural" arrangements of plants have emphasized their linearity – and it is true that Adanson, Linnaeus, Lamarck and Jussieu all presented their natural orders in a linear sequence or series, in the case of Lamarck and Jussieu in particular, in an attempt to show plants in an order reflecting their anatomical complexity.[330] To a certain extent, however, this linear listing of families of plants may have been necessitated by the inherently linear structure of books and the high cost of printing diagrammatic plates.

As Giulio Barsanti has demonstrated, linear conceptions of organisms' relationships began decreasing in popularity in the 1750s (with the exception of a brief fascination with strictly linear interpretations of how nature is organized in France in the late 1770s and early 1780s) in favour of non-linear ones. These were represented as ramifying threads, bundles of sticks, maps and nets.[331] In particular, the map metaphor grew in popularity once naturalists recognized that the different organs or other features of adjacent organisms

> *do not all have the same level of complexity* and that it would be erroneous to affirm that one [organism] is entirely "*superior*" to the preceding one and entirely "*inferior*" to the one that follows. After this, each species could no longer be conceived of as an intermediary between *two* other species, because it possessed affinities with *numerous* different groups to which it was linked by *particular rapports*. In other words, people discovered *that*

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[330] Étienne Pierre Ventenat (1757-1808), writing in the 1790s, and Achille Richard (1794-1852), writing in the 1820s, among many others, also favoured arranging plants in a series based on their relative complexity Stevens 1994, pp. 97, 173-175. Adanson, on the other hand, seemed to think that such a pursuit was futile because of the variability and unequal distribution of features among organisms, although he still believed that a natural series of plant families based on resemblance among the genera within them was possible (Adanson 1966, p. clxv).

[331] Barsanti 1988, pp. 57, 66, 69, 71. Although a number of French naturalists in the late 1770s and early 1780s aside from Jussieu, particularly Félix Vicq d'Azyr, proposed linear arrangements meant not only as teaching aids, but to reflect the order of nature, the French fascination with such arrangements was not of long duration. A number of naturalists who had earlier been enamoured with such arrangements – for instance, Daubenton and Geoffroy Sainte-Hilaire – had abandoned them by the early 1790s (Barsanti 1988, pp. 72, 73, 75).

> *affinities are not distributed uniformly but rather that instead they are enmeshed with each other.*[332]

This was particularly true by the late 18th century. Peter Stevens noted that "It was a matter of frequent comment from Jussieu's time onward that the linear arrangement of families in books was artificial; however, given the nature of the printed page, this was the only way in which they could be arranged."[333]

The linearity of schemes proposed during the 1770s and beyond was therefore usually with respect to the particular features of organisms being discussed, not the organisms on the whole. There was no overarching *scala naturae*; scales of complexity were context-dependent. The "natural" groups being rearranged according to these criteria, however, were meant to be permanent no matter which of their characters were being examined or what order a given author felt like using. Even Jussieu, whose belief in continuity in nature and the appropriateness of a linear arrangement of natural orders has been emphasized by 20th-century commentators, especially Henri Daudin and Peter Stevens, was more concerned with keeping his groups internally consistent than with ranking them according to particular criteria.[334]

Jussieu tried to connect each of his natural orders to those before and after in his series – and was surprisingly good at doing so. Each of his natural orders was composed of a centre of typical genera but also ragged edges composed of genera that had some features different from those of the genera at the centre but in common with the genera in the next

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[332] Barsanti 1992, p. 265. Barsanti's italics.

[333] Stevens 1994, p. 165.

[334] Daudin (1926) and Stevens (1994) in particular have emphasized Jussieu's and others' belief in continuity in nature as a driving force in the arrangements of plants they produced. But individual botanists' beliefs about continuity in nature had very little role to play in terms of providing functional means to identify plants. When identification is a priority, classification-makers must emphasize differences, that is, distinctive features or combinations of features that plants possess. This is a practical reality that affected the shape of arrangements far more than did botanists' stated commitment to the belief that taxa naturally flow into one another. The only exception to this general rule would be in the case of a top-down classification meant to highlight 'holes' in a God-given plenitude of natural productions – but even in this case the taxa in the classifications still have to be parcelled out according to particular features they have or do not have. Beliefs about continuity in nature therefore play only a miniscule role in the history of information management in botany.

natural order adjacent to that edge.[335] What connected natural orders in Jussieu's series was not some overall measurement of complexity but rather the sharing of certain features among different genera – i.e. keeping like plants with like.

Even the tendency for naturalists of this period to talk about the relative dignity or station of taxa is not necessarily as closely linked with notions of a *scala naturae* or other linear arrangement as it might first seem. "It cannot be overstated how prevalent this style of discussion was," Peter Stevens pointed out, "whether or not the authors believed in relationships that were reticulating or circular."[336] At a local level in particular, "highness" was often associated with a taxon seen to be the type – the ideal or centre of a group of organisms – while "lowness" was associated with taxa with less characteristic features. Such types were ways to visualize or summarize and abstract information about a group of plants. They did not need to be seen as real in a Platonic sense or situated on a ladder of dignity or anything else in order to fulfil these functions. This kind of typology may involve a local linear ordering of taxa, but among groups at a larger scale is compatible with the overall non-linear depiction of relationships – e.g. islands, knots in a network, and circles of affinity – that became popular during the last quarter of the 18th century.

Disparaging linear arrangements of families

By the beginning of the 19th century, the linear representation of plant families was seen as more of a necessary evil than a fact of nature. It was used to maintain the normal structure of books, for teaching purposes and for arranging collections of plants,[337] but no longer in theoretical discussions of the shape of nature. But at this time, naturalists of all stripes were aware that, even if they themselves did not believe in a linear order of nature, their readers tended to interpret groups that were listed closely together as being closely related. Failing to keep groups believed to be related to each other close together in a text

[335] Stevens 1994, pp. 55-56.
[336] Stevens 1994, p. 173.
[337] Candolle 1813, p. 203.

would provoke criticism. Botanists therefore made efforts to list taxa in a way so that any two adjacent ones in their lists had something in common.[338] But this led to a vicious cycle of feeding into the public expectation that natural groups did indeed fall into a meaningful linear order in nature.

As usual, the Rev. William Kirby was quite eloquent in summing up the thoughts of many of his contemporary naturalists regarding the implications of putting groups of living things in lists. He despaired of depicting the multiple and even multi-dimensional relationships that they believed to obtain among organisms, much less finding the one true natural way of arranging them that he believed to exist. After quoting the philosopher Sir J. F. W. Herschel, who wrote that the most scientific arrangements of organisms "have for their very aim to interweave all the objects of nature in a close and compact web of mutual relations and dependence," Kirby mused that

> When so many eminent men form *different* conclusions from the *same* premises, we may rest assured that there is something in the subject of their lucubrations that admits of hesitation, and justifies variety of opinions; and this is clearly the case with respect to the arrangement of natural objects; for whether we consider the productions of our globe, in all their affinities, as best represented by a branching tree, a net, or a sphere formed of an infinity of larger and smaller orbits, connected on every side, and placed *ad infinitum* wheel within wheel; if we set ourselves to arrange and describe upon paper the individuals composing any department of the three kingdoms, we shall find that it is above us either to conceive or delineate it so as to maintain all its connections undisturbed and unbroken. We *must* do it in a *series*, which can only be a series of mutilations and dislocations. It will be like cutting off every branch and twig of the tree to place them end to end; like tearing up the net to place all the meshes one after the other; like blowing up the whole sphere, and unraveling, as it were, all its orbits great and small, to make a continuous thread of them. So that it is a hopeless case to attempt an arrangement according with nature *in all its parts*; vain man, with all his boasted powers of intellect, cannot conceive, much less utter and embody it. All that he can accomplish is to give some general idea of it, and to describe some fractions of it. He can also attend to the composition of his groups, and keep those together that are really related; but as to conterminous groups, he will often be at a loss which is nearest to the one in question, for from different parts of the same group, a variety of others will often branch off in different directions.[339]

[338] Another reasons why separating similar-looking plants caused outcries was that it interfered with readers using natural groups within their texts to bypass using the hierarchical arrangement to identify plants (what I call the "shadow system" of identification by gestalt, below).
[339] Kirby 1837, p. xxv. Kirby's italics.

When 18th- and early 19th-century botanists had the opportunity to depict the relationships they believed to hold among plant groups using small diagrams rather than normal typographical characters over many pages, they frequently illustrated complex interrelationships that are not reducible to a linear series.[340] Even Jussieu eventually disavowed the depiction of natural groups in a series. In the posthumously published revised introduction to his *Genera plantarum* (1837), he wrote that natural affinities are not linear, as he had suggested in 1789, but were rather like bundles or aggregates of taxa, which were impossible to represent adequately in books or in botanical gardens[341] – one or two dimensions being insufficient.

The eventual recognition that arrangements of plants in books are not necessarily connected to beliefs about plant relationships came about over a long period of time. Before the turn of the 19th century, botanists were taking advantage of the modularity of natural groups "under the table," as it were, at the same time as they employed other means of classification.

The "shadow system"

The major way that botanists used the modularity of natural taxa to their advantage before the natural method became the dominant mode of arranging plants in books was through what I like to call the "shadow system" – keeping natural groups together even in a hierarchical scheme. "Shadow system" is a good name for this use of natural clusters of taxa even within artificial hierarchical arrangements because it was ubiquitous but almost never discussed openly.

The shadow system was a pragmatic solution to a number of problems botanists faced. But, as its name implies, it had a sinister side as well. As long as botanists took

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[340] Some graphical depictions of non-linear relationships among taxa produced in the 18th and early 19th century are reproduced in Stevens 1994, pp. 164, 167, 168, 171, Stevens 1983, Barsanti 1988, Barsanti 1992.
[341] Stevens 1994, p. 388.

advantage of the shadow system, their plant arrangements were doomed to mediocrity. The shadow system gave them some of the advantages of both the natural method and hierarchical schemes, but all of the disadvantages of both, and none of the clarity of either one taken independently.

The first pragmatic solution that the shadow system yielded was to make plants easier to identify using hierarchical schemes. Anyone attempting to use a hierarchical scheme for plant identification purposes is likely to encounter trouble. Despite all the promotional talk about ease of plant identification using the sexual system or Lamarck's *méthode analytique* (dichotomous key – discussed below), all hierarchical schemes suffer from a major problem. In a hierarchical scheme, the features of the object being identified must be examined in a predetermined sequence in order to find its description. If the specimen a user wishes to identify is not exhibiting one of the features necessary to proceed to the next step in the hierarchical identification sequence, identification using the hierarchical scheme fails. For instance, if determining the class and order of a plant requires it to be blooming, and it is not, the plant cannot be identified. Users are left to thumb through all the taxon descriptions until they find what they want, just as in the case of using an alphabetical scheme to find information about a plant of which the name is not known to the reader. Unlike in the case of alphabetical schemes, however, users were often aided in this respect by authors who clustered similar plants together in natural genera or unnamed sections within their "artificial" hierarchies. These sections acted as modules: the shadow system allowed users familiar with the natural groups to skip to sections of interest without descending through the hierarchy or using the alphabetical index. It reduced the amount of guesswork required to locate anomalous species whose descriptions were printed with their congeners despite not keying out through the hierarchy to the same order or class. (This made it particularly valuable in works without cross-references or double entries). But here is where the shadow system's troublesome aspects come into play. It fed into botanists' beliefs that it was possible for an arrangement of plants to maximize ease of identification and best represent plants' relationships at the same time.

The shadow system is a side effect of printing related species and/or related genera as close together as possible in books. It played a very large role in 18th century botany. It was, in fact, probably the greatest hindrance to both the development of identification keys and the natural method as independent technologies. So long as the shadow system was in place, botanists could get some of the benefits of each of these methods of arrangement. Even in an imperfect hierarchical scheme, so long as plants are grouped more or less "naturally," users with some background knowledge of botany could flip to the section of the work where plants like the specimen they wanted to identify were described. Authors of schemes like this could claim that their arrangements were natural, since natural genera were kept together. They could also reap the benefits of using simple to delimit and remember artificial orders and/or classes.

But a hierarchical arrangement of plants with a shadow system also shares the disadvantages of both its components. Users must have background knowledge of plants' relationships if they are to take advantage of the shadow system. For everyone else, the shadow system may as well not exist. Anomalous taxa would be near impossible to find without cross-references. Species would be hard to find within large genera, and genera would be hard to find in large orders.

The inclusion of both more natural and more artificial components in systematic works, even in the absence of explicit justification for doing so, served to acknowledge that different techniques were required to achieve the most effective means of classifying organisms according to relatedness and identifying them. That is, regardless of whether the belief was common that one system could be both all-encompassing and easy to use, in practice, naturalists implicitly acknowledged that different methods suited the different purposes of representing nature and of presenting taxonomic information in a useful way. In light of the different capabilities of hierarchy-based and relationship-based (modular) classifications, the makers of classification schemes were forced to prioritise comprehensiveness, adequate representation of organisms' interrelationships, and ease of use.

Chapter 3. Origin of field guides

Separation of techniques for identifying plants and for representing their relationships

> *Anybody who has ever used an encyclopedia, or a library catalogue, or the membership list of a society, or has hunted frustratingly round a department store, will be aware that different classifications suit different purposes, and that some systems are a good deal better than others.*[342]

The first identification keys: Nehemiah Grew, Christian Gottlieb Ludwig, John Hill, and Lord Bute

One of the features necessary for a field guide to be effective is a way for someone unaware of the name of a plant to be able to find out further information about it. Linking a plant to its name is undoubtedly the most important step in this process; the name is critical for looking up further information. Identification keys – stepwise procedures directing users through an examination of different features of their specimen, and culminating in the organism's name – serve this purpose. As I explained in Chapter 1, identification keys do not need to be called "keys," nor be dichotomous, nor bracketed or indented, but the information in them does need to be arranged in a hierarchy of features examined, in order for the stepwise process of elimination to function. The English plant anatomist Nehemiah Grew proposed a clear outline of how to make such a key in 1682: his "Method *proposed, for the ready finding, by the* Leaf *and* Flower, *to what Sort any* Plant *belongeth.*" He did not implement his suggestions himself.[343]

Grew's method was unusual in that he designed it as a plant identification tool, and

[342] Knight 1981, p. 131.

[343] Grew 1682, pp. 174-176. John Ray's biographer, Charles Raven, upon hearing about Nehemiah Grew's *Method*, sought to find out whether it may have had an influence on Ray's classifications. He was unable to find any indications that Ray and Grew even knew each other, let alone were familiar with each other's classificatory ideas (Raven 1986, 2 ed., p. 201). Recall also that Ray had tried to make his scheme both as natural and as easy to use as he could.

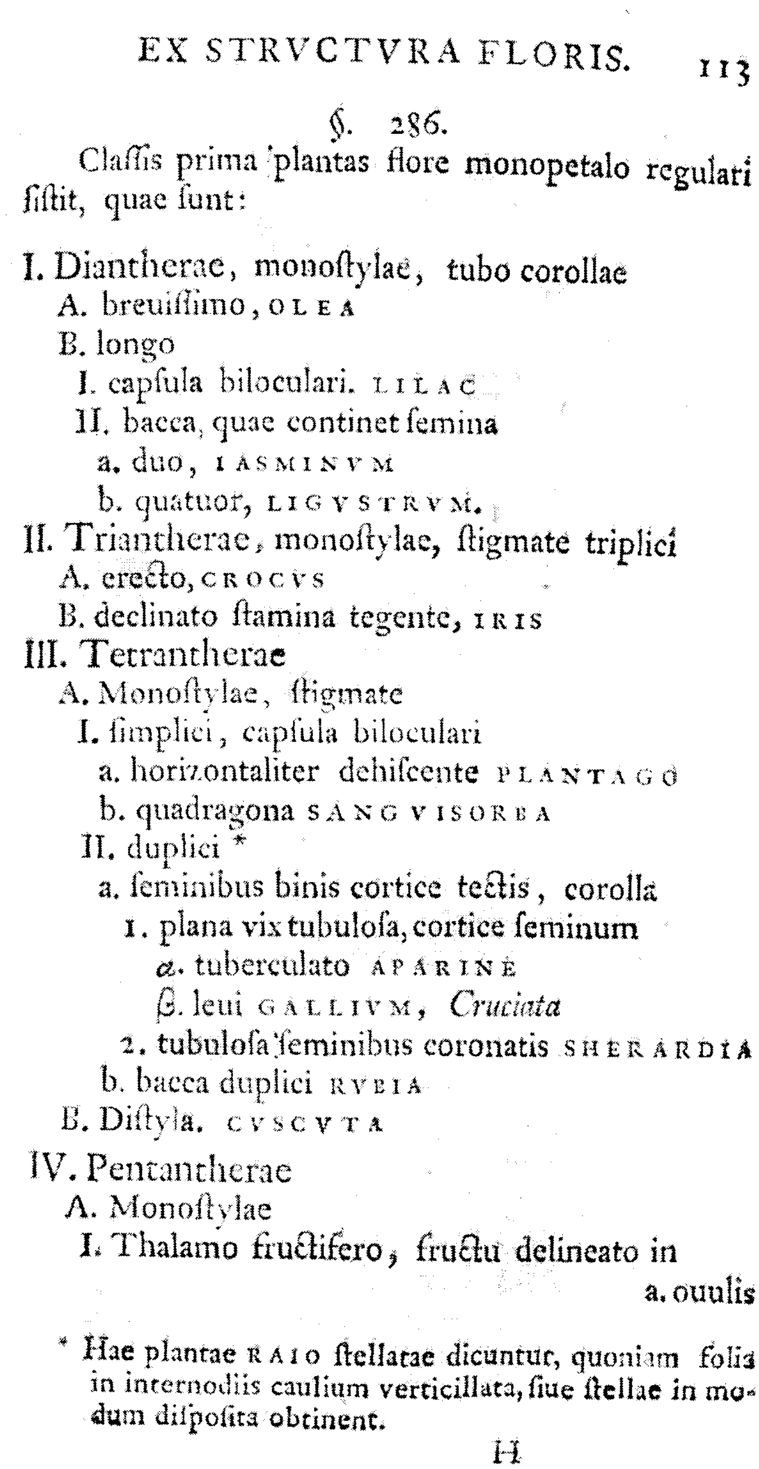
nothing more. As I outlined earlier, it was much more common for the hierarchical arrangement of plants in late 17^{th} century and early- to mid-18^{th} century botanical texts to serve more than one function.

The next functional key I have seen was produced by Christian Gottlieb Ludwig (1709-1773). He published it in his *Institutiones historico physicae regni vegetabilis praelectionibus academicis accommodatae* (1757). Its format can be seen below in Figure 18. Unfortunately, it lacks both cross-references and double entries.[344] It was not well-received,[345] probably in part because of this and because Ludwig did not explain clearly how and why ease of identification and the maintenance of natural groups conflict, even if he understood the reasons for it himself.

John Berkenhout, as mentioned in the section on double entries above, pursued another avenue to facilitate plant identification. He modified Linnaeus's sexual system into a more functional identification tool by using double entries so that it would be possible to key out each genus and species under the class and order matching its number of stamens and pistils. He avoided the problems that Linnaeus had caused by keeping natural genera together under artificial classes and orders, and respectfully faulted Linnaeus for doing so. "The first grand divisions of Class and Order," Berkenhout explained, "should be absolute, without exceptions; otherwise the system serves only to perplex the investigation it was meant to facilitate." He therefore "transferred several single Species to their proper Class and Order, referring the reader for their generic character to the Class and Order where the rest of the same Genus are to be found. In a Natural system," he continued, "this separation would be unpardonable; but in an Artificial one, provided I am thus enabled more readily to

[344] Ludwig 1757, Editio altera aucta et emendata ed., p. 113. I have not had the opportunity to see the first (1742) edition of this work.

[345] E. g. Adanson complained that Ludwig's arrangement, like many others he deemed to be artificial, split natural genera Adanson 1966, p. c, while Colin Milne called his contributions to botany useless Milne 1771, p. 124.



EX STRVCTVRA FLORIS. 113

§. 286.

Claſſis prima plantas flore monopetalo regulari ſiſtit, quae ſunt:

I. Diantherae, monoſtylae, tubo corollae
 A. breuiſſimo, OLEA
 B. longo
  I. capſula biloculari. LILAC
  II. bacca, quae continet ſemina
   a. duo, IASMINVM
   b. quatuor, LIGVSTRVM.
II. Triantherae, monoſtylae, ſtigmate triplici
 A. erecto, CROCVS
 B. declinato ſtamina tegente, IRIS
III. Tetrantherae
 A. Monoſtylae, ſtigmate
  I. ſimplici, capſula biloculari
   a. horizontaliter dehiſcente PLANTAGO
   b. quadragona SANGVISORBA
  II. duplici *
   a. ſeminibus binis cortice tectis, corolla
    1. plana vix tubuloſa, cortice ſeminum
     α. tuberculato APARINE
     β. leui GALLIVM, *Cruciata*
    2. tubuloſa ſeminibus coronatis SHERARDIA
   b. bacca duplici RVBIA
 B. Diſtyla. CVSCVTA
IV. Pentantherae
 A. Monoſtylae
  I. Thalamo fructifero, fructu delineato in
   a. ouulis

* Hae plantae RAIO ſtellatae dicuntur, quoniam folia in internodiis caulium verticillata, ſiue ſtellae in modum diſpoſita obtinent.

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Figure 18. A sample page from Ludwig's 1757 *Institutiones*, showing the indented layout of his key. Image © and courtesy of Hunt Institute for Botanical Documentation, Carnegie Mellon University, Pittsburgh, PA.

identicate the plant in question, it is certainly of no importance that there are other Species of the same Genus in another part of the Book."[346]

Berkenhout effectively turned the sexual system into the efficient identification key that it could have been had Linnaeus not been bent on keeping the species making up his natural genera physically printed together in his works.

Berkenhout's countryman John Hill (1714?-1775) also devised an identification key. Hill called it an "artificial method, or index of plants."[347]Hill was an apothecary-turned-actor-turned writer of many tracts on a staggering range of subjects. He is almost as famous for his obnoxious personality as his attempts to shame the Royal Society into focusing more on science and less on trivialities through risqué satires of its *Transactions*. Hill offended so many powerful people with these and similar confrontational works that a large number of his publications had to be issued under pen names – he wrote 96 books with 29 different publishers.[348] His contemporaries reacted to his self-aggrandizement by calling him a quack and a fraud. Despite Hill's considerable talent as a botanist and microscopist, he failed to garner the three requisite recommendations to allow him to join the Royal Society.[349] It even appears that there was a campaign of sorts to write him out of botanical history.[350] It is, perhaps, due to personal circumstances such as these that Hill's innovative treatment of plant identification in his *Vegetable System* was ignored by almost everyone who came after him.

Hill claimed that his method would be easier for anyone – *anyone* – to use for plant identification than Linnaeus's sexual system. He explained that he wanted to make "a method of ranging Plants" that,

> with very little time or trouble, would enable a person unacquainted with Botany, to find out an unknown Plant as certainly as he would a word

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[346] Berkenhout 1770, vol. 2, p. vi.
[347] Hill 1770, 2 ed., vol. 1, p. 45.
[348] Rousseau 1991, p. 299.
[349] Most biographical information about Hill from Henrey 1975, vol. 1, pp. vol. 2., pp. 90-109.
[350] Historical and biographical sketches of the progress of botany in England, from its origin to the introduction of the Linnaean system. By Richard Pulteney, M. D. F. R. S. 8.vo. 2 vols. pp. 380 in each. 10s. Boards. Cadell. 1790 1791, p. 367.

> in a dictionary. This will suit alike all the purposes of beginners; for the young Naturalist ought certainly to know the Plant by sight, before he attempts to find out its place in the scale of vegetative nature and there are many people of distinction possessed of great variety of Plants, that have neither time nor inclination for botanical researches, and yet wish to name a Tree or Herb that draws their attention. For these, and for the fair sex, this index is calculated.[351]

Unlike in the sexual system, where genera did not always share 100 per cent of the characters of the classes in which they were placed, Hill, like Berkenhout, would put genera only in classes that describe them accurately. He would repeat genera under different classes if species within them showed features of more than one class, and he would keep generic descriptions as short as possible, using easily visible floral features to differentiate among genera. "Let not some critick tell us, after this declaration," wrote Hill,

> that we break all natural Classes, and separate near relations; we mean to do so. We mean, in the following work, to have no mercy upon any Class, any Order, any System, that stops a minute our pursuit. With the utmost reverence for Nature, we chuse in this performance an easier guide.[352]

A more natural arrangement, in Hill's opinion, is not necessarily the easiest way to proceed.

Hill's illustrated table – he actually calls it a key[353] – divides plants into classes similar to the groups proposed by John Ray, but in a deliberately step-wise fashion and according to the rules Hill had outlined. Hill wrote, however, that he hoped his "mere artificial index" would "pave the way" to a "more natural" system, implying that botanists would eventually reach a point where they could discard all artificial methods and just use "the" natural one.

John Stuart, Earl of Bute (1713-1792) also published an identification key. Lord Bute was a correspondent of Linnaeus, patron of botanists William Curtis and John Hill, and instrumental in the establishment of Kew Gardens.[354] Bute and Hill were close for most of Hill's life, but fell out over the costs involved in the production of Hill's *Vegetable System*. After Hill died in 1775, his widow Lady Hill wrote a letter condemning the Earl of Bute for

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351 Hill 1770, 2 ed., vol. 1, pp. 43-44.
352 Hill 1770, 2 ed., vol. 1, pp. 43-44. Hill's spelling.
353 Hill 1770, 2 ed., vol. 1, p. 44.
354 John Stuart, 3rd Earl of Bute (1713-1792) .

refusing to cover the costs of writing and illustrating the 25-volume work as the Earl had originally promised. She was left in great debt because he reneged on his pledge.

Ten years later, the Earl of Bute published Botanical tables, containing the different familys of British plants, distinguish'd by a few obvious parts of fructification rang'd in a synoptical method at his own expense. The nine large quarto volumes with over 650 illustrations apparently cost £12 000 to produce, since each copy was said to cost £1000 and only 12 copies were ever printed.[355] The book was designed to be a functional key to plant identification according to a plan similar to that used by Hill in his Vegetable System. I was fortunate enough to see the Earl of Bute's Tabular distribution of British plants (1787), a compact derivative work made with the tables (keys) from the original, stripped of introductory matter and modified to include the characters of the genera in the tables. The Tabular distribution was, the Earl of Bute wrote, "composed entirely for the convenience of those who have the work in quarto, and who may wish to carry it with them into the field." If it is indeed representative of the larger work, it provides strong evidence that the Earl of Bute was very influenced by John Hill's ideas. His keys feature double entries of genera under different orders and classes, as shown in Figures 19 and 20. Of the more than 200 botanical works from the late 17th and early 18th century I have read, only those by Berkenhout and Hill also featured double entries.

Despite its interesting features, both of the Earl of Bute's books were too hard to find and too expensive to have much influence on the botanical world. Sir James E. Smith, founder of the Linnean Society, dismissed the Earl's efforts, writing, "it is easier to make a rare book than a good one." Smith also noted that, even if the work had been better, its small publication run made whatever impact it could have otherwise had inconsequential.[356] Thus, despite these early English (and one German) attempts at creating identification keys, it turns out that this technology was first popularized by a Frenchman.

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[355] Henrey 1975, vol. 1, p. 243 provides several references, Stuart 1787, pp. 306-307
[356] Smith 1824-1828, p. xviii.

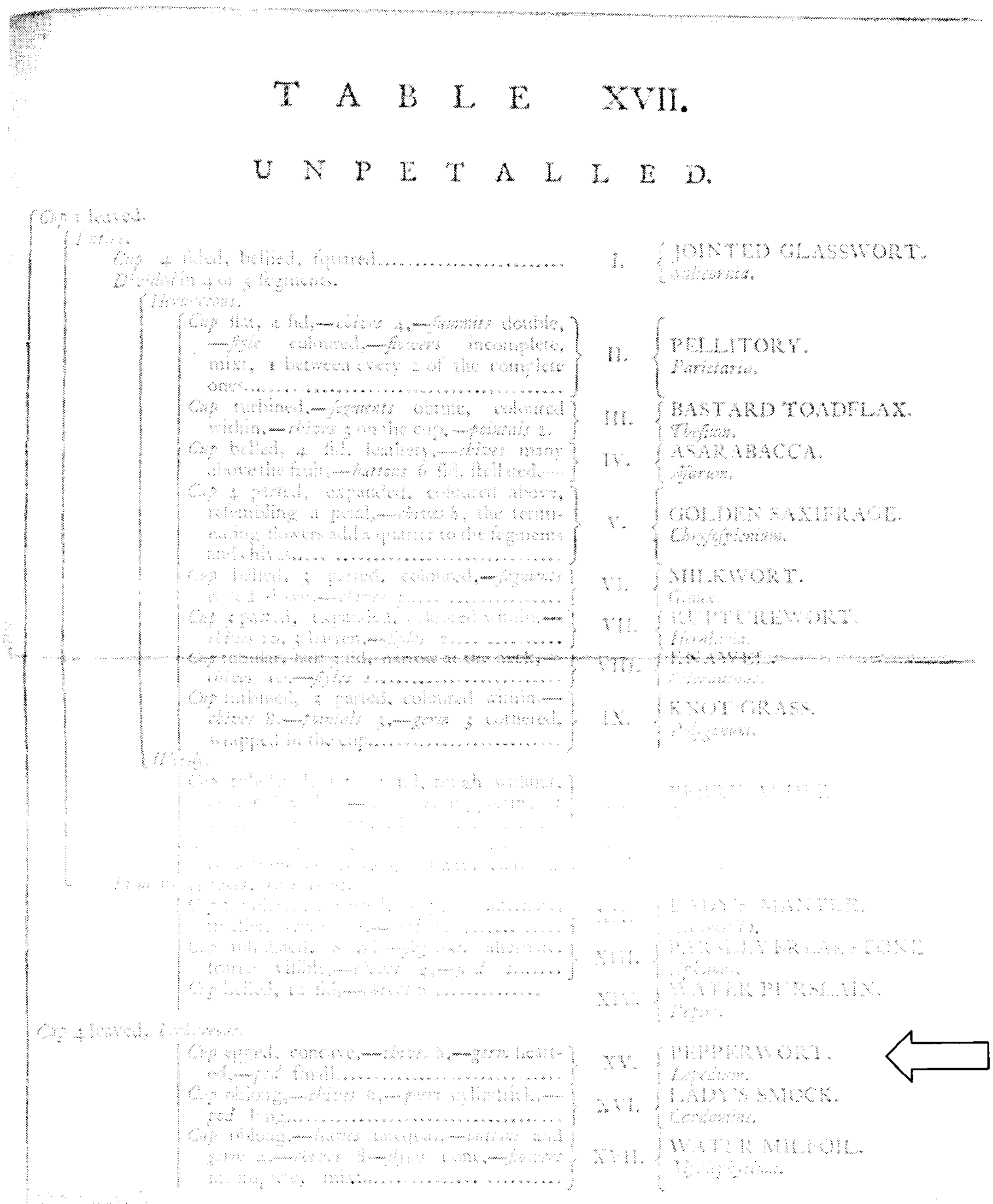

TABLE XVII.

UNPETALLED.

Cup 1 leaved.

Cup 4 lobed, bellied, squared........................ I. JOINTED GLASSWORT. *Salicornia.*

Divided in 4 or 5 segments.

Hermaphrodite.

Cup flat, 4 fid,—chives 4,—filaments double,—style coloured,—flowers incomplete, mixt, 1 between every 2 of the complete ones........................ II. PELLITORY. *Parietaria.*

Cup turbined,—segments obtuse, coloured within,—chives 5 on the cup,—pointals 2. III. BASTARD TOADFLAX. *Thesium.*

Cup bellied, 4 fid, leathery,—chives many above the fruit,—buttons 6 fid, stellated. IV. ASARABACCA. *Asarum.*

Cup 4 parted, expanded, coloured above, resembling a petal,—chives 8, the terminating flowers add a quarter to the segments and chives........................ V. GOLDEN SAXIFRAGE. *Chrysosplenium.*

Cup bellied, 5 parted, coloured,—segments .... VI. MILKWORT. *Glaux.*

Cup 5 parted, expanded, ... VII. RUPTUREWORT. *Herniaria.*

.... VIII. KNAWEL. *Scleranthus.*

Cup turbined, 5 parted, coloured within,—chives 8,—pointals 3,—germ 3 cornered, wrapped in the cup........................ IX. KNOT GRASS. *Polygonum.*

.... XIII. PARSLEY BREAKSTONE. *Aphanes.*

.... XIV. WATER PURSLAIN. *Peplis.*

Cup 4 leaved.

Cup egged, concave,—chives 6,—germ heart-ed,—pod small........................ XV. PEPPERWORT. *Lepidium.*

Cup oblong,—chives 6,—germ cylindrick,—pod long........................ XVI. LADY'S SMOCK. *Cardamine.*

Cup oblong,—chives .... XVII. WATER MILFOIL. *Myriophyllum.*

Figure 19. Part of Table 17 from John Stuart, Earl of Bute's *Tabular distribution of British plants* (1787), showing the general organization of his charts. The arrow indicates an entry for Pepperwort (*Lepidium* sp.). N.B. "chives" was the English word used for stamens at the time. Image © and courtesy of Hunt Institute for Botanical Documentation, Carnegie Mellon University, Pittsburgh, PA.

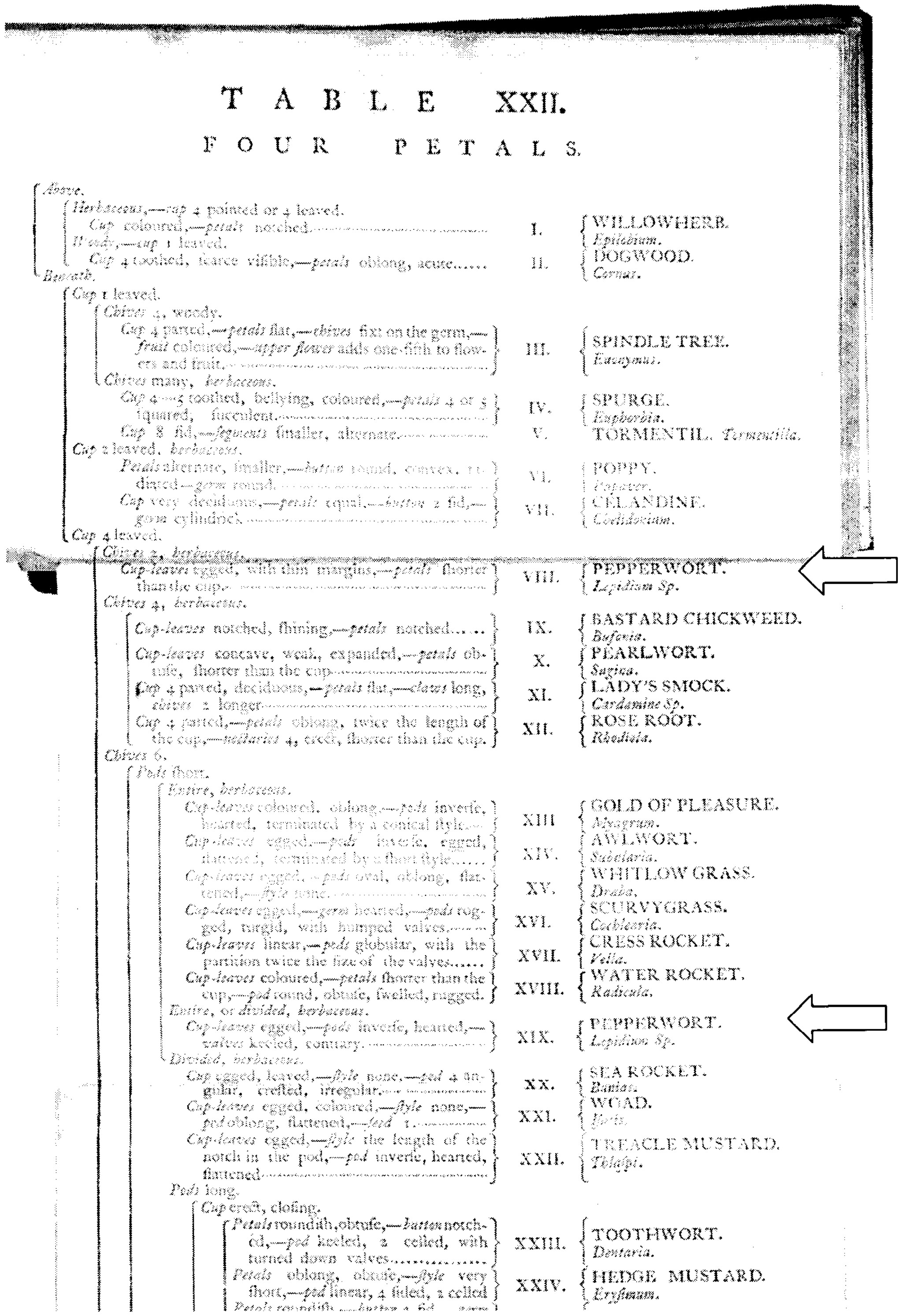

TABLE XXII.

FOUR PETALS.

Above.
Herbaceous,—*cup* 4 pointed or 4 leaved.
Cup coloured,—*petals* notched. — I. WILLOWHERB. *Epilobium.*
Woody,—*cup* 1 leaved.
Cup 4 toothed, scarce visible,—*petals* oblong, acute...... — II. DOGWOOD. *Cornus.*
Beneath.
Cup 1 leaved.
Chives 4, woody.
Cup 4 parted,—*petals* flat,—*chives* fixt on the germ,—*fruit* coloured,—*upper flower* adds one-fifth to flowers and fruit. — III. SPINDLE TREE. *Euonymus.*
Chives many, *herbaceous.*
Cup 4—5 toothed, bellying, coloured,—*petals* 4 or 5 squared, succulent. — IV. SPURGE. *Euphorbia.*
Cup 8 fid,—*segments* smaller, alternate. — V. TORMENTIL. *Tormentilla.*
Cup 2 leaved, *herbaceous.*
Petals alternate, smaller,—*button* round, convex, radiated,—*germ* round. — VI. POPPY. *Papaver.*
Cup very deciduous,—*petals* equal,—*button* 2 fid,—*germ* cylindrick. — VII. CELANDINE. *Chelidonium.*
Cup 4 leaved.
Chives 2, *herbaceous.*
Cup-leaves egged, with thin margins,—*petals* shorter than the cup. — VIII. PEPPERWORT. *Lepidium Sp.*
Chives 4, *herbaceous.*
Cup-leaves notched, shining,—*petals* notched...... — IX. BASTARD CHICKWEED. *Bufonia.*
Cup-leaves concave, weak, expanded,—*petals* obtuse, shorter than the cup. — X. PEARLWORT. *Sagina.*
Cup 4 parted, deciduous,—*petals* flat,—*claws* long, *chives* 2 longer. — XI. LADY'S SMOCK. *Cardamine Sp.*
Cup 4 parted,—*petals* oblong, twice the length of the cup,—*nectaries* 4, erect, shorter than the cup. — XII. ROSE ROOT. *Rhodiola.*
Chives 6.
Pods short.
Entire, herbaceous.
Cup-leaves coloured, oblong,—*pods* inverse, hearted, terminated by a conical style. — XIII. GOLD OF PLEASURE. *Myagrum.*
Cup-leaves egged,—*pods* inverse, egged, flattened, terminated by a short style...... — XIV. AWLWORT. *Subularia.*
Cup-leaves egged,—*pods* oval, oblong, flattened,—*style* none. — XV. WHITLOW GRASS. *Draba.*
Cup-leaves egged,—*germ* hearted,—*pods* rugged, turgid, with humped valves. — XVI. SCURVYGRASS. *Cochlearia.*
Cup-leaves linear,—*pods* globular, with the partition twice the size of the valves...... — XVII. CRESS ROCKET. *Vella.*
Cup-leaves coloured,—*petals* shorter than the cup,—*pod* round, obtuse, swelled, rugged. — XVIII. WATER ROCKET. *Radicula.*
Entire, or divided, herbaceous.
Cup-leaves egged,—*pods* inverse, hearted,—*valves* keeled, contrary. — XIX. PEPPERWORT. *Lepidium Sp.*
Divided, herbaceous.
Cup egged, leaved,—*style* none,—*pod* 4 angular, crested, irregular. — XX. SEA ROCKET. *Bunias.*
Cup-leaves egged, coloured,—*style* none,—*pod* oblong, flattened,—*seed* 1. — XXI. WOAD. *Isatis.*
Cup-leaves egged,—*style* the length of the notch in the pod,—*pod* inverse, hearted, flattened. — XXII. TREACLE MUSTARD. *Thlaspi.*
Pods long.
Cup erect, closing.
Petals roundish, obtuse,—*button* notched,—*pod* keeled, 2 celled, with turned down valves............... — XXIII. TOOTHWORT. *Dentaria.*
Petals oblong, obtuse,—*style* very short,—*pod* linear, 4 sided, 2 celled — XXIV. HEDGE MUSTARD. *Erysimum.*

Figure 20. Part of Table 22 from John Stuart, Earl of Bute's *Tabular distribution of British plants* (1787), showing two more entries for Pepperwort (*Lepidium* sp.). Image © and courtesy of Hunt Institute for Botanical Documentation, Carnegie Mellon University, Pittsburgh, PA.

Lamarck's méthode analytique

Lamarck introduced his *méthode analytique* in his *Flore Françoise* of 1779.[357] It was the first botanical work to use only contrasting couplets all the way through, from the first question to the attainment of a plant's name. Unlike the sexual system or any other hierarchical system published before, there were no classes or orders breaking up the *analyse* along the way. In fact, the entire book is structured as one giant dichotomous key. The first page of the *analyse* is shown in Figure 21.

Lamarck was candid in explaining to his readers exactly why he chose that format. A reader, he wrote, has to choose between only two characters,

> of which one applies to the plant to the exclusion of the other, and of which the coexistence in the same individual would imply a contradiction. This is what distinguishes my method from all the others, which, without talking about the great number of objects among which they most frequently leave the observer indecisive and disoriented, offers him a choice to make among characters that are ordinarily similar to each other, or are, at the very most, different but rarely incompatible.[358]

This structure is also quite different from the dichotomous but not fully contrasted characters employed in earlier works, such as Ray's and l'Obel's.

Lamarck had written the *Flore* in only six months, after some of his friends challenged him to devise and publish a method that would allow someone without any particular knowledge of botany to find out the names of plants.[359] Despite these odd and frivolous beginnings, the *Flore* had a huge impact on the rest of Lamarck's life. The ease with which botanical novices could identify plants with it won him instant recognition. It impressed Buffon sufficiently that he took Lamarck under his wing. Buffon arranged for the Imprimerie Royale to print the work at the cost of the government, with all profits going directly to the author,[360] and was instrumental in getting him into the Académie des

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[357] The title page says 1778 but it came out in 1779 (Métraux 1996, p. 543).
[358] Lamarck 1778, vol. 1, p. lxxxi.
[359] Dayrat 2003, p. 86.
[360] Duris 1997, p. 255.

MÉTHODE ANALYTIQUE.

PLANTES ADULTES, ou dont les Fleurs ſont dans un état de développement parfait.

1. ANALYSE.

Fleurs diſtinctes.	*Fleurs indiſtinctes.*
Fleurs dont les étamines & piſtils peuvent aiſément ſe diſtinguer. 2.	Fleurs nulles, ou dont les étamines & piſtils ne peuvent ſe diſtinguer. 1240.

2. *Fleurs diſtinctes*	Fleurs conjointes; fleurs raſſemblées dans un calice commun, & dont les étamines ſont réunies par leurs anthères	3
	Fleurs disjointes; fleurs non raſſemblées dans un calice commun, ou dont les anthères ſont disjointes.	137
3. *Fleurs conjointes*	Fleurettes ayant, outre leur calice commun, des calices particuliers..	4
	Fleurettes n'ayant qu'un calice commun, mais point de calices particuliers.	7
4. *Fleurettes ayant, outre le calice commun, des calices particuliers*	Fleurettes dont la corolle eſt monopétale & tubulée.........	5
	Fleurettes dont la corolle eſt compoſée de cinq pétales........	6

Tome II. A

Figure 21. The first page of Lamarck's *méthode analytique,* or key, from his *Flore françois* (1779). The entire book, with the exception of the introductory discourse and index, is laid out like this. Image © Sara Scharf and courtesy of the Thomas Fisher Rare Book Library, University of Toronto.

Sciences. Buffon also arranged for Lamarck to accompany his son on a tour of the botanical gardens of Europe.[361] This trip increased Lamarck's experience and stature significantly.

Buffon and classification

This association of Lamarck with Buffon as a result of the *Flore Française* may bring to mind the contrast between Buffon's classificatory ideas and those of Linnaeus that is often emphasized in works on the history of biology.[362] Buffon has been portrayed as the ultimate anti-Linnaean for his polemical attacks on the sexual system, although Linnaeus's *oeuvre* was diverse enough that the term "anti-Linnaean" can mean just about anything. Buffon's particular problem with the Linnaean program to classify all living things was that he believed it to be impractical and irrelevant to everyday life. His valid argument that the sexual system was difficult to use without a microscope was in this vein. However, Buffon's own ideas on classification were not well developed. He did not understand the unique problems that the massive influx of new plants into Europe in particular was creating for naturalists. Instead he believed that naturalists should continue using old methods of description and classification based on personal familiarity. He was incapable of proposing any workable alternative to the sexual system for this reason.

Although his erudition made his natural history works quite popular, a large number of Buffon's contemporaries saw that he held many misconceptions about Linnaeus and the aims of classification.[363] For instance, botanists such as A.-P. de Candolle and Achille Richard both wrote that Buffon did not understand the differences in purpose between natural and artificial classifications.[364] More recent writers have documented further evidence that Buffon's contemporaries thought his writings on classification misinterpreted

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[361] Dayrat 2003, p. 88.
[362] E.g. Stevens 1994, p. 401 note 5, Duris 1997, p. 132.
[363] E.g. Aubert du Petit-Thouars, who wrote upon hearing of Buffon's death, "though his systems were attacked, one was also forced to admire the spirit with which he imagined them" du Petit-Thouars 1811a, p. 40.
[364] Candolle 1813, p. 64, Richard 1826, vol. 10 p. 494.

Linnaeus and had more style than substance.[365] Historian of biology Henri Daudin in particular damned Buffon's writings on Linnaeus's work as "unfortunately almost thoroughly superficial, as must be recognized right away. . . the assurance of the writer, coupled with the casualness of a gentleman, today could not conceal how imperfect [Buffon's] knowledge is, not only of real facts, but also, in the same sense, of the question asked."[366]

Buffon's belief that only individuals exist in nature and that all classifications, including species, are arbitrary, provided no help to anyone attempting to communicate about those very individuals. Lamarck's work therefore probably impressed Buffon very much in that Lamarck stated that all groups of organisms are arbitrary, yet he still came up with a practical and homegrown way to identify plants. While Buffon and his friend, the Abbé René-Just Haüy, likely contributed stylistic touches to the *discours préliminaire* of Lamarck's *Flore*, the substance of the *discours* is purely Lamarckian, dealing with subject matter beyond Buffon's realm of expertise.[367]

Success of the Flore Françoise

Although "the logic of ordering" in the *Flore* offended many French naturalists intent on figuring out the natural order,[368] the original April 1779 printing of 1200 copies quickly sold out. Botanists liked how, in the words of Aubert du Petit-Thouars, "the ingenious *analyse* of M. de Lamarck . . . leads you as if by the hand."[369] Lamarck had been preparing to put out a second edition in 1788 when the events of the French Revolution presumably put a halt to his plans. Instead the first edition was reprinted in year III of the Republic, i.e.

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365 E.g. Daudin [1926], pp. 127-129, 131, Malesherbes, cited in Williams 2001, p. 51, Miller 1988, p. 229, and Rousseau 1991, p. 289, citing an anonymous author who in 1780 wrote of Buffon that he was impressed by "the beauty of the Author's style, which is always enchanting, even where it berays marks of negligence."
366 Daudin [1926], p. 126.
367 Daudin [1926], p. 191, Stevens 1994, p. 14.
368 Métraux 1996, p. 543.
369 du Petit-Thouars 1811a, p. 31.

1795.[370] The *Flore* continued to garner enthusiastic reviews thirty years after its first publication. In 1809, for instance, the French Linnaean Mouton-Fontenille de la Clotte wrote that the *Flore Françoise* was

> made with care. It is rare to find the author making mistakes with synonyms. His descriptions ... are really characteristic, offering only the attributes that can lead to the certain diagnosis of a plant ... Even though this flora is not complete in the current state of science in France, it will always be seen as one of the best works published in the eighteenth century.[371]

Nevertheless, identification of plants with the *Flore* could be tedious, as Lamarck had put more effort into making identifications definite than speedy. This is a problem with all dichotomous arrangements, as I discuss in more depth in the section in Chapter 6 on dichotomy.

A very natural method

Lamarck's *méthode* certainly had its faults. But what was the competition like?

When Jussieu's *Genera plantarum* came out in 1789, it was hailed as the most "natural" arrangements of plants to date. But, as I mentioned earlier in the shadow system section, the natural method is no good for plant identification except for people already familiar with plants. A number of features of Jussieu's' *Genera plantarum*, featuring the most influential arrangement of natural plant families (most of which are still considered natural to this day) demonstrate this situation.

The hierarchical structure of Jussieu's natural method is full of anomalies that make finding information difficult

Jussieu was determined to produce as natural an arrangement of plants as he could. His zeal for the natural method eclipsed his desire to produce anything of use to other

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[370] Duris 1997, p. 255.
[371] "Table alphabétique des auteurs de Botanique" in Mouton-Fontenille's *Linné François ou tableau du règne végétal*, Montpellier, 1809, t. 5. p. xxxii, cited Bange 1997, pp. 220, note.

naturalists, especially those in need of a consistent set of principles to guide them. Although Jussieu recognized that using a hierarchical "method" with strict inclusion could be helpful in identifying genera, Jussieu did not seem to appreciate that it was useful *because* it was a strictly nesting hierarchy. Instead, he was concerned that the strict hierarchical groups within it were useful only insofar as they were natural. Jussieu therefore circumscribed classes and orders as he saw fit. He often used features in his family descriptions that he had not introduced earlier. He also excluded features of plants he considered peripheral to the main body of natural orders (*genera affinia*) from his descriptions of families, in order to make his family descriptions more clear-cut.[372] Users of Jussieu's' *Genera plantarum* found this particular feature of his arrangement frustrating. It was hard to locate these genera using the natural method because higher level groupings that included them did not necessarily appear to contain anything with their features.

At the time that Jussieu published his *Genera plantarum*, he did not see the problems that he caused for others by assuming that natural groups would fall into a neatly nested hierarchy but not producing this kind of hierarchy himself. His explanations of how botanist should proceed are full of contradictory advice, such as, "methodical distributions of plants are valuable only insofar as they are natural, easy and uniform ... By nature's tacit command, whatever method that preserves undivided any of these [*natural orders*] is more perfect the more it admits."[373] That Jussieu praised plant arrangements designed to identify plants quickly not for their facility at accomplishing that goal but for how well they kept related plants together indicates that he believed that the two ends were not only compatible, but could best be achieved in one arrangement. Jussieu's' criticisms of Tournefort and Linnaeus also show that he expected an ideal arrangement of plants to enable users both to name plants and to understand how they are related to one another: he pointed out both where those two authors deviated from the simple principles they used to sort plants into nameable

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[372] Stevens 1994, p. 58.
[373] Jussieu 1789, pp. xxvii cited Stevens 1994 p. 346-347.

groups, and where the same principles induced them to split groups of plants that Jussieu thought belonged together.[374]

Jussieu's natural method thus suffered from the same problems that Linnaeus's sexual system and, for that matter, any arrangement of plants relying on the shadow system of natural groups embedded in a supposedly strict inclusion hierarchy. There was only one way to get rid of the pernicious influence of the beliefs that the shadow system fostered once and for all: throw it out.

Combinations of complementary text navigation devices in field guides

Getting rid of the shadow system involved taking the best of each of hierarchical and modular systems and optimizing each of them separately. There was no better way to arrange empirical data about plants than to combine the strengths of each of the data management strategies available: not just the natural method, but analytical (hierarchical) identification schemes, i.e. keys, as well as alphabetical indexes. Once each of these components was adjusted for maximum utility, they could be used at will, in concert but not blended together. The strengths of each were complementary, and none of them shared the same weakness. Identification keys in particular increased in popularity once their usefulness when coupled with the natural system and a common index was discovered.[375] Texts using

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[374] Jussieu 1789, pp. xxix-xxxiii cited Stevens 1994 pp. 348-353.

[375] Combining hierarchical and "natural" methods in one text without a common index does not confer the same advantage of speed of looking up information that using a common index does. Perhaps the most awkward use of hierarchical and "natural" schemes with indexes in one book can be found in William Jackson Hooker's *Flora scotica* (1821), which consists of two parts, the first according to the sexual system and the second according to the natural method, each with its own index to the genera. Plants are described both in the Linnaean part of the book and in the "natural" part, though readers looking at specific descriptions in the "natural" section were referred by page number to the places where the same species were discussed in the Linnean section, "where fuller descriptions and more particular habitats are given" (Hooker 1821, pp. 162, note). Cryptogams pose an exception, since they are described only in the "natural" part of the book.

In the index to the Linnaean section, each genus connects with two page numbers, the first one indicating where the genus is first mentioned under its order and class, and the second, the page on which it is described more fully. In the index to the "natural" part, which also contains the names of plant families, page numbers refer to the pages in the "natural" part of the book only. In practical terms, this means that if a user knows the family of a plant and looks up more about it using this information, he or she would have to use the this three-part structure were adopted at different times in England and France.

Using two or more texts at once

In France, there was a long tradition of experimentation in botanical arrangements. Budding French botanists in the 1790s who wanted to learn about the flora of their country could take their pick of the "natural methods" put forth by Adanson (1763) and the Jussieus (1789), or Tournefort's system (first published in 1693 and still being taught in some French schools a good hundred years later),[376] or Lamarck's *méthode analytique*, i.e. a giant dichotomous key, first published in his *Flore François* (1779). Furthermore, even into the early 19th century in France, A.-P. de Candolle noted that "the sexual system has continued to be generally used, and since the majority of books are arranged in this order, it is necessary to know it thoroughly, whatever opinion you might have about its real merit."[377] Botanists often used several of these texts in combination to identify plants – a strength of Lamarck's, Linnaeus's and also Tournefort's – and to read about their features, such as what other plants they resemble -- strengths of Adanson's and especially Jussieu's arrangements. It was not uncommon for botanists to refer to this practice of cross-checking in their textbooks, especially when discussing the process for the sake of students.[378] As Nicolas Jolyclerc

"natural" index to find the place where the family was circumscribed, and then read through all the generic descriptions in the family to find the right genus, and all the genera to find the right species. He or she would then have to look up this name in the index to the Linnaean part of the book in order to find the species description. Meanwhile, although the artificial arrangement in the first part of the book can be used to determine the name of a plant specimen, a reader who wants to find out what family it is in has to use the index to the "natural" part of the book to find where in that section its genus is described.

This book is a nightmare to use and I have not seen any others in which this structure was replicated. John Lindley helped Hooker with both the content and arrangement of the "natural" section of the text (Hooker 1821, p. x); it is likely that Lindley was inspired by this experience to come up with something more convenient.

376 E.g. using such texts as Ventenat 1797 and Tournefort and Jolyclerc 1797, among others – Williams 2001, pp. 105-129 provides several other examples from the 1780s to the 1810s.

377 Candolle 1813, p. 47.

378 E.g. Lestiboudois 1781, p. xlv, Tournefort and Jolyclerc 1797, pp. 313-315. Colin Milne, who could clearly read French (he often paraphrased Adanson without attributing the source of his information) likewise did so, writing,

> Every artificial method has necessarily defects, voids, and obscure points. But two methods, such as those of Tournefort and Linnaeus, so well conceived, so judiciously executed, and founded upon observation, must enlighten each other mutually. They cannot err on the same subject; if the one wanders but for a moment,. the other immediately sets him in the right path.

explained, "Every method doubtless has its advantages and merit. My opinion is that the comparison of many of them leads an amateur to know plants according to a greatest number of relationships, and in this way to get to know them with the most certainty."[379] And as Jean Senebier put it,

> I believe that naturalists should study the two methods [natural and artificial] with care: if the artificial one facilitates the means of recognizing each of the beings that one encounters, the natural method, which is really more philosophical, opens the way to important discoveries; it makes known the beings that it indicates by their relationships; it provides ideas about their organization [and] on how to study them... In the study of nature, which is so difficult, is it not pleasing to surround oneself with all the means which compete to make it easier, and to augment its progress in all respects?[380]

The inclusion of both more natural and more artificial components in systematic works, even in the absence of explicit justification for doing so, served to acknowledge that determining relatedness and identifying organisms quickly are best achieved with different techniques. That is, regardless of whether the belief was common that one system could be both all-encompassing and easy to use, in practice, naturalists implicitly acknowledged that different methods suited the different purposes of representing nature and of presenting taxonomic information in a useful way. Using two complementary systems in one book was a practical way to offer a one-stop source for different types of information.

Two or more navigation devices in one book

Books combining several different classifications had been published since at least the 1770s. The combination of different methods in one book eliminated the need for students to purchase several expensive tomes written for more advanced botanophiles. The

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> The same reflection occurs in comparing several other learned and ingenious methods; such as those of Haller, Van Royen, Sauvages, Ludwig, Adanson and Duhamel; and the observations scattered thro' the works of Jussieu, Guettard, Dillenius, Gerard and others; so certain is the maxim with which I shall conclude this article, that a multiplicity of methods and observations compared together, leads us to distinguish plants under a greater number of relations, and consequently conducts us, with greater ease, to their knowledge.

(Milne 1770 under METHODUS). Milne's opinion was not common among Anglophones at the time.

379 Tournefort and Jolyclerc 1797, pp. xiv-xv.

380 Senebier 1802, 2 ed., pp. 54-55.

earliest such texts were also designed to introduce students to botany in their own language, French, rather than Latin. For instance, the Lyonnais monk-turned-botany professor Nicolas Jolyclerc's 1797 publication has the full title *Élémens de botanique, ou méthode pour connoitre les plantes, par Pitton de Tournefort. Edition augmentée de tous les Supplémens donnés par Antoine de Jussieu; enrichie d'une Concordance avec les Classes, les Ordres du Systême sexuel de Linné, et les Familles naturelles créés par Laurent-Antoine* [sic] *de Jussieu; mise à la portée de tous les hommes par l'interprétation française du texte grec ou latin des Espèces admises dans les Auteurs, par des additions très-considérables au Dictionnaire des termes du Botaniste, etc. etc. etc.* [381] Jolyclerc was spurred to compile this text because he realized that the species descriptions in Tournefort's *Élémens* were in Latin only, and were therefore useless in practice for a large part of the population.[382]

Innovative local floras: les Lestiboudois

The best of these books also presented students with information only about plants that they would find in their regions. Eliminating information about plants that do not grow in particular areas helped keep these works small, which enabled them to be taken into the field during plant identification exercises. The Lestiboudois family of Lille provides a number of examples of how pedagogical concerns led to the production of successful botanical works.

Jean-Baptiste Lestiboudois (1715-1804) was an innovative teacher much loved by his students. These included the brothers Dupetit-Thouars, Palisot de Beauvois and his own son, Jean- François Lestiboudois (1759-1815). J.-B. Lestiboudois instructed botany courses in

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[381] Jolyclerc died in 1817. More biographical information is available in Davy de Virville 1954, p. 67. His book's title translates as: *Elements of botany, or method to get to know plants, by [Joseph] Pitton de Tournefort. [This] edition augmented with all the supplements given by Antoine de Jussieu, enriched with a concordance with the classes and orders of Linnaeus's sexual system, and the natural families created by Laurent-Antoine [sic] de Jussieu; put within reach of all men by the French interpretation of Greek or Latin text of species admitted by authors, by very considerable additions to the dictionary of botanical terms, etc. etc. etc.*

[382] Tournefort and Jolyclerc 1797, p. xiii. Roger Williams (Williams 2001) mentions a number of other French botanical works from the late 18th century that employ more than one classificatory method – usually combinations of Linnaeus's and Tournefort's, though sometimes also including the natural method.

Lille from 1770 until his death, without a break, even during the French Revolution. He had switched from using the works of Tournefort to teach when he found that Linnaeus's sexual system was easier for students to use.[383] Soon afterwards, he decided to reconcile the two systems in a "*Carte botanique*," published in 1773 to acclaim, and then in a book called *Abrégé élémentaire de botanique, à l'usage de l'École de botanique de Lille* (1774, Lille: J. B. Henry).[384] Jean-François Lestiboudois recollected that "the celebrated Valmont de Bomart explained the botanical part of his natural history course in Paris" using his father's *Carte*.[385]

François-Joseph Lestiboudois soon published his own effort, *Botanographie Belgique* (1781). This work was an abridgement of Lamarck's *Flore François* that included fold-out illustrated curly bracket keys to 22 "natural families" similar to Tournefort's groupings, synonymy with Linnaeus and Tournefort, and an index.[386] *Botanographie Belgique* was popular and sold out completely.[387] It also earned praise for its clarity and ease of use from respected botanists, for instance, Augustin-Pyramus de Candolle recommended it to students.[388] Nevertheless, the 22 natural families that Lestiboudois introduced do not correspond directly with those used by any other author. The widespread approval of Antoine-Laurent de Jussieu's 100 natural families published in his *Genera plantarum* of 1789 rendered Lestiboudois's work obsolete. After the formation of central schools following the Revolution, Lestiboudois was asked to produce a second, updated edition of the *Botanographie Belgique*.[389] Unfortunately, this work, in which he reconciled his old 22-family system with those introduced by Jussieu by making his own families into classes, consists of three bloated volumes bulging with extraneous material in numerous

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[383] Leclair 1908, pp. 40-43.
[384] Unfortunately, as far as I have been able to determine, only two copies of this book are available in libraries, one in the Smithsonian in Washington and the other at the Institut de France (in Paris, but next to inaccessible).
[385] Lestiboudois 1799, 2 ed., vol. 1, pp. tome 1, v.
[386] Lestiboudois 1781, p. iv.
[387] Lestiboudois 1799, 2 ed., vol. 1, pp. tome 1, xiv.
[388] Gray 1821, p. 31.
[389] Lestiboudois 1799, 2 ed., vol. 1, pp. tome 1, xiv.

subsections.[390] Instead of referring only to the local flora, this edition includes an entire *species plantarum*. Though Lestiboudois explained that he had it printed in parts so that relevant sections could be taken on "botanical walks"[391] it is more suited for use as a classroom text than as a field guide. Nevertheless, it was still popular; "new editions" (word-for-word reprints with a few changes to the title) of the *Botanographie Belgique* came out in 1804 and, posthumously, in 1827, under the direction of his son, Thémistocle, a famous botanist in his own right.[392]

Jussieu's keys

Combinations of hierarchical and "natural" methods were not published exclusively in regional floras, however. In addition to being the most widely-accepted book on the "natural method" ever published, it has only infrequently been noticed that Antoine-Laurent de Jussieu's *Genera plantarum* also contained an identification key – though only to a small number of genera. It seems from many comments throughout the introduction to this work that Jussieu saw hierarchical arrangements of plants as imperfect attempts at the natural method, rather than as arrangements with different aims. Nevertheless, Jussieu clearly acknowledged in a footnote to one such comment that he did, in fact, appreciate that hierarchical ways of organizing information could be useful for showing people where in the natural method plants could be found. Referring to a hierarchical "index" at the back of his

[390] Volume 3 is actually bound in two parts because it is too long to be bound as one. Lestiboudois's advice about what to bring on *herborisations* is typical of the cram-it-all-in approach taken in the second edition. Aside from detailed instructions about where to go and when, and the kinds of books, vascula, hats, knives, magnifying glasses, material for note-taking, and walking sticks that one should bring, he also recommends that since anyone botanising is going to encounter insects, one might as well take along insect trapping equipment, pins and wax to pin them on, and a net (Lestiboudois 1799, 2 ed., vol. 1, pp. tome 2, vii-x). A botanist heeding his advice would be ridiculously overburdened, much as his book is.
[391] Lestiboudois 1799, 2 ed., vol. 1, pp. tome 1, xiv-xv.
[392] Leclair 1908, pp. 52-53. Other books combining different famous botanical methods continued to be published into the 19th century, but by the 1810s they were more curiosities than helpful student manuals or cutting-edge science, e.g. Louis F. Henri Lefébure's *Atlas botanique, ou clef du jardin de l'univers, d'après les principes de Tournefort et de Linné réunis* (Paris, 1817).

book, which he included to facilitate finding information about his genera *incertae sedis*,[393] he wrote that

> Plants of an uncertain position are here arranged in such a manner... which is arbitrary until their true affinities become known. I intend to add a similar [index] to the entire work so that those genera that are noted and placed in orders only with difficulty by Beginners, may be quickly named with certainty by the use of an index of a few obvious ["impensis"] signs: but the imminent publication of this work left no time to compile such an index, which however is indispensable and I am working on it continuously so that it will, therefore, be published separately, or be appended to a second edition, if in due course one appears.[394]

This key to all of the genera in *Genera plantarum*, just like the second edition of the *Genera plantarum* he was preparing in the 1790s, was never published, due to the French Revolution and subsequent social uproar.[395] In the decade that followed, Jussieu had to give up botany in order to fulfil administrative responsibilities. When he resumed work on botanical topics he produced mostly monographs, though a revised introduction to the *Genera plantarum*, in which he discussed more general ideas about plant anatomy and physiology, as well as the principles according to which plants should be arranged, was published posthumously in 1837.[396]

Jussieu's arrangement as the ultimate natural method

Just because Jussieu himself did not devote much thought or effort to creating identification keys to his work does not mean that he did not benefit from them, however. The natural method as presented in Jussieu's *Genera plantarum* succeeded not only because of the knowledge of plants Jussieu had over and above that of his predecessors, it also had to

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[393] This "key to the genera Jussieu could not place in a family" (Stevens 1994, p. 518) is on p. 416 of the *Genera plantarum*. It is laid out as three sets of curly brackets all on one page. The first branch point is based on the number of petals the plants have: one, more than one, or none. The second branch point for both mono- and polypetalae is seed location. The key leads to species names. No page numbers are provided.
[394] Jussieu 1789, p. lx, Jussieu 1994c, p. 380 and note 181 on page 518. All square brackets are Rosa's and Stevens'.
[395] Stevens 1994, p. 41, du Petit-Thouars 1811a, p. 14. Du Petit-Thouars kept hoping into the first decades of the 19th century that Jussieu would publish it du Petit-Thouars 1817, vol. 5 p. 222.
[396] Stevens 1994, pp. 1, 384.

do with techniques of presenting that information that had developed over the previous century.

Like Ray, Jussieu wrote in Latin. But unlike Ray, Jussieu made an effort to use layout to help his readers at least find where different groups of plants above the generic level began and ended. Despite his beautiful Latin, Ray's method was hard to navigate, with many dichotomous branch points. It was widely acknowledged (especially in the Francophone world) that Tournefort's less-natural system was easier to use.[397]

The next major promoter of natural groups was Adanson. But Adanson was a difficult character. He insulted everyone who came before him in his *Familles des Plantes* for not making their systems natural enough, including Ray, Tournefort and Linnaeus. Even worse, his system was very difficult to use for plant identification, not the least because of his idiosyncratic French spelling. Adanson also did not have institutional support, especially after A.-L. Jussieu came to Paris to live with his uncle Bernard, in whose house Adanson had been staying. This forced Adanson to move out of the home in which the best herbarium in France was kept.

Antoine-Laurent de Jussieu benefited from being part of a long-recognized botanical family by the time he published his natural method. This gave him the name recognition and clout that Adanson lacked. The sexual system had also been in use long enough by the time Jussieu published for most botanists to be familiar not only with its flaws but with Linnaeus's *Fragmenta* and his dictum that the natural method was to be the "*primum et ultimum*" of botany. On top of these advantages, Jussieus' method was the most comprehensive natural method when it was published. It was written in Latin, enabling it to catch on in places other than France. Jussieu justified his divisions of plants using trendy physiological arguments about cotyledons. And French students confused about how to find plants using the natural method also had the opportunity to look them up using either the

[397] An example of an Anglophone writer with the same opinion is Colin Milne 1770 (under METHODUS).

sexual system or a panoply of homegrown French alternatives, e.g. Lamarck's, Tournefort's, or various hybrids developed during the 1780s and 1790s, many of which also explained Jussieu's classification in lay language, as described earlier. Jussieu's success at promulgating his version of the natural method had very much to do with the general state of botany, the French education system, his family's reputation, and the state of identification technology in France when he published. It was in this milieu that the first modern field guides were developed, building not only on the same developments in botanical communication that Jussieu had built on, but on Jussieu's natural method itself.

First modern field guides

At the turn of the 19th century, the idea that combining strictly utilitarian keys with the most natural arrangement of plants available (Jussieu's) and an index in one textbook occurred to a number of people, including F.-A.-N. Dubois (1803) and Candolle (1805, 3 ed.). They may have come up with this idea independently, or they may have been inspired by Jussieu's comments on combinations of methods in his *Genera plantarum*. The result was evidently well-received. Dubois's combination of the natural method with an "*analyse*" was described as "a double advantage for lovers of botany."[398] Candolle may even have copied Dubois consciously or without being aware of it.[399] He did not cite Dubois in 1805 but he was familiar enough with his work in 1813 to write that

> M. Lestiboudois, in his *Flore de Belgique* [sic], and ... M. Dubois, in that of Orléans ... both linked the *méthode analytique* with a more or less natural method, and presented M. Lamarck's series of questions in the form of step-by-step tables. These books are perhaps the easiest to use of all elementary

[398] Guillemin 1825, p. 100.

[399] Dubois wrote in the introduction to his *Flore* that "To get to know its defects, I put it into the hands of a large number of people, above all young people, who wanted to learn botany, and I took care to correct it every time that it lost them, and that it did not bring them with certainty [*sûrement*] to the name of the plant that they were looking at. It is these multiple corrections that gave me the right to give it the title *Méthode éprouvée* [Proven method]. Those who used it were so pleasantly surprised to find themselves [not] obliged to have to take lessons from a master [sic], that they all pressed me for several years to print my work. There are, just the same, a large number of people who, seeing that I did not respond quickly enough to their desires, have copied or are copying my method. I did not believe I ought to resist such repeated instances for much longer, and I hope that the public, impatient with what I yielded up, will truly judge me with indulgence" (Dubois 1803, pp. vii-viii).

> works in the botanical literature, but their use is unfortunately strongly held back because they pertain to very restricted regions.[400]

The restricted scope of these floras, though a blessing for students with access only to their local environs, simultaneously made it less likely for their innovative structure to become familiar to botanists interested in plants growing elsewhere. This situation, however, is not likely the leading one why combinations of different plant arrangements in one textbook were slow to catch on in Britain.

Britain

Preference for using only the sexual system

In Britain, the sexual system alone was dominant for a lot longer and it was subject to fewer challenges from other arrangements of plants than was the case in France. A number of cultural factors were at play in this situation: an "anti-theoretical attitude among naturalists"[401] that discouraged speculation about or experimentation with new plant arrangements, the preference to avoid conflict by correcting and modifying existing structures rather introducing new ones, the belief that producing or using "the" natural method was dependent on a perfect knowledge of plants, which could never be attained, and, in the 19th century in particular, an overwhelming amount of inertia and tradition embodied by Sir James Edward Smith and the Linnean Society. As well, a widespread belief in natural theology, according to which arrangements of plants that both reflect nature and are understandable by people are not only possible, but ideal, may have been a factor. These ideas, combined with the dominance of the sexual system in the world of botany in the 1780s, and in Britain in particular in the early 19th century, may have led to the "if it ain't broke, don't fix it" attitude expressed by so many British botanists regarding the sexual

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400 Candolle 1813, pp. 51-52.
401 Stevens 1994, p. 223.

system. William Withering touched on most of these issues in the introduction to his *Botanical arrangement of British plants* (1787), writing that in his work,

> All controversies about system are here studiously avoided. Mankind are weary of such unprofitable disputes. Every System yet invented, undoubtedly may glory in its peculiar beauties, and, with no less reason, blush for its particular defects. It is sufficient for the present purpose that the system of LINNAEUS is now very universally adopted; and though confessedly imperfect, it approaches so near to perfection, that we may perhaps never expect to see any other improvements, than such, as will be founded upon his plan.[402]

Withering added, "the generality of mankind are tired with disputes about Systems, and the vegetable productions of Europe are pretty well arranged: It is time therefore to think of turning our acquisitions to some useful purpose" such as studying their medicinal uses.[403]

In addition to cultural differences between Britain and France, there were two further obstacles to diffusion of the natural method. One was mutual loathing due to frequent warfare. In the first three decades of the early 19th century, showing appreciation for anything French was akin in some English circles to traitorous admiration of Napoleon. There was also a language barrier. Though Tournefort and Jussieu published in Latin, the works of Adanson, Lamarck and Candolle, as well as lesser figures such as Dubois, Senebier and Lestiboudois, were available only in French. The sexual system was the lingua franca of botany – why learn something else that was not only associated with enemy propaganda but difficult to master? Patriotic declarations and name-calling were often a greater part of the debates about botanical arrangements in Britain than were discussions of each scheme's practical merits and problems. William Roscoe's harangue against the natural method, "On artificial and natural arrangements of plants: and particularly on the systems of Linnaeus and Jussieu," first read in 1810, printed in 1815 and again in 1830, epitomizes this genre. The article, for instance, gave the ominous warning to British Linnaeans that

> A rival has of late risen up, and has already become truly formidable. – Under the patronage and by the influence of a neighbouring nation, this

[402] Withering and Stokes 1787, 2 ed., vol. 1 and 2, p. xxvi.
[403] Withering and Stokes 1787, 2 ed., vol. 1 and 2, pp. xx-xxi. William Smellie uses the same arguments in reference to arrangements of animals in his *Philosophy of natural history* (1790) (Smellie 1790, p. 8).

> rival now comes forward, and demands universal homage. Its advocates are not only numerous, but learned; not only acute, but earnest. – That their influence is daily increasing cannot be doubted; and the crisis is now arrived when their opinions must be either submitted to, or resisted.[404]

Roscoe was evidently comparing Jussieu to Napoleon. He concentrated on building up Linnaeus's reputation as a sage who understood all possible aspects of botany, while ridiculing any attempts at a more natural arrangement as useless for plant identification, incomplete, overly hasty, foolish and doomed to failure, quoting Linnaeus who wrote that

> "Natural orders ... cannot constitute a method without a key. In distinguishing plants, the artificial method is alone of any avail; a natural method being scarcely, or rather not at all, possible. Natural orders are useful in acquainting us with the nature of plants, but an artificial method is requisite to their discrimination."... [405] And to this he has added, in language that must for ever remove all ambiguity on this head, "Those persons who, instead of a natural method, have arranged plants in fragments of such a method, and reject an artificial one, seem to me to resemble those who, having a convenient and well roofed house, overturn it, in order to build one in the place of it which they are unable to finish the roof."[406]

Roscoe also argued that, unlike the sexual system, the natural method is neither easily intelligible nor communicable. In fact, the natural method is a hindrance to scientific progress because it is so hard to use, "incongruous and absurd," that it cannot possibly be *really* natural.[407] "The method of Jussieu," he explained, "is not in fact a natural, but an artificial one," and "as an artificial method, the system of Jussieu is inferior to that of Linnæus."[408]

Even as Sir J. E. Smith began to concede that the natural system might have some merit (see above), he was still inclined to cite Roscoe's arguments favourably regarding "the question of the natural or artificial character of Jussieu's System." Both men argued that since Jussieu's arrangement of plants was not perfectly natural, there was no point in

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404 Roscoe 1830, p. 17.

405 Roscoe reproduced Linnaeus's original Latin in a note, without indicating which work he was citing. This, and the following quotation, are from Linnaeus's *Ordines naturales* section in the sixth edition of *Genera plantarum* (1764) (Linnaeus 1764, 6 ed., p. [602]). Linnaeus was likely reacting to Adanson's *Familles des Plantes*, published 1763-1764, in which Adanson insisted that botanists should not put their energies into anything other than the one true natural method.

406 Roscoe 1815, pp. 22-23.

407 Roscoe 1815, p. 183.

408 Roscoe 1815, p. 98.

adopting it over the tried-and-true sexual system, imperfect though it may be in its own ways. According to Smith, the sexual system still had an advantage in terms of ease of use. "We require a cabinet," Smith wrote,

> as it were, with cells or drawers, where we may find each Order as we want it; and Jussieu's classification, with all it's [sic] unavoidable imperfections, goes much beyond any system previously invented, in the natural assemblages which it produces. Nevertheless .... There remains ... in the study of natural classification, only a choice of difficulties ... we can, as yet, obtain but very confined and imperfect views. Hence therefore I am almost inclined to revert to the idea of Linnaeus, that we are not competent to define technically any natural orders, without so many, and such paradoxical exceptions, as to destroy all consistency.[409]

Smith later criticized Jussieu's *Genera plantarum* on the grounds that an index was necessary to find one's way around it, unlike the books of Linnaeus, in which "the analytical mode of inquiry" could serve as a guide.[410]

James Ebenezer Bicheno (1785-1851), Secretary of the Linnean Society,[411] similarly wrote in 1827 that he believed that including any artificial or analytical components in a text would somehow debase the purity of the natural method. Bicheno was concerned that English naturalists, newly enamoured of the natural method, would mount a backlash against the sexual system simply because it had dominated botany in their country for so long. "The danger to be now apprehended," he wrote, "is, that those who adopt other arrangements will forget the advantages to be derived from what is old, in their love of that which is new."[412] Bicheno's solution was to use the sexual system and the natural method independently of one another.[413] Since, according to Bicheno, arrangements of organisms were good either because they were natural or because they made identifying organisms easier, there was no point in trying to make the natural system easier if doing so would make it less natural. Instead of stooping to such an unproductive compromise, he proposed that naturalists

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409 Smith 1821, pp. 193-195.
410 Smith 1832, vol. 2 pp. 571-572.
411 1824-1832.
412 Bicheno 1827, p. 481.
413 While this idea may have sounded appealing to those attempting to maintain the dignity of the sexual system without denying the merits of Jussieu's approach, it had the fault of being utterly impractical. Few shared Bicheno's opinion, and the most successful British introductory botanical works in the following years – John Lindley's – benefited from a combination of "natural" and hierarchical approaches.

interested in naming organisms use the sexual system while keeping in mind that "the chief object of the natural system ...[is] its utility as an instrument of general reasoning."[414] The natural method would be philosophically worthwhile because it would allow users to think about organisms in terms of their relationships, and thereby make predictions about their properties, or about new organisms yet to be discovered.

Bicheno thus criticised Robert Brown's *Prodromus florae Novae Hollandiae* – a widely admired work on the flora of Australia and Tasmania, arranged according to Jussieu's natural method – for being too difficult to find species in, even for people with reasonable botanical experience. But he then added that, "to have made it subservient to this purpose, would have been to have rendered it less beautiful and complete as a work of synthesis."[415] Bicheno's reasoning seems to have been that since plants' relationships are complex, so must the natural method be complex. Any attempts to simplify *using* it would make it less natural and therefore imperfect. Bicheno's implicit assumption is that the representation of relationships among organisms and the best method to retrieve information about those organisms are necessarily the same: an ideal arrangement embodies the relationships among organisms, and its ordering principles are sufficiently obvious to those who are familiar with those relationships or those who wish to become familiar with them that no other help is needed. Attempts to make finding information in the arrangement a priority would therefore have to change the arrangement itself. This would make the representation of relationships "subservient" to ease of use, or automatically artificial and bad.

Candolle regarded as ridiculous the fanatical Linnaeans' name-calling and perverse focus on the advantages of the sexual system as compared with the disadvantages of the natural method. These Linnaean enthusiasts, he wrote, "saw every work on the natural method as a kind of sacrilege against their master." Candolle took pains to point out that, after all, Linnaeus said that the natural method is the ultimate goal of botany. He even

[414] Bicheno 1827, p. 494.
[415] Bicheno 1827, p. 490.

condemned "certain zealous partisans of natural orders, such as Adanson and Buffon, [who] censured the sexual system beyond measure and without justice because they did not understand the difference in aim and means between natural and artificial methods." Although he tried to explain the differences,[416] these arguments held little sway in the English-speaking world until Anglophones themselves began to propound them.

Large-scale introduction of the natural method

Robert Brown effectively introduced the natural method to Britain by forcing anyone who wanted to learn about the botany of Australia to become familiar with his treatment of it, since his groundbreaking *Prodromus florae Novae Hollandiae* (1810) using this arrangement was never completed, and lacks an index.[417] Although serious botanists were impressed with Brown's work, it took much longer for the natural method to gain in popularity in Britain. Part of the reason for this was a lack of entry-level textbooks about it in the English language; Brown's *Prodromus* was "written in Latin, unlike the popular works being produced in England at that time, suggesting that the book was aimed at the international scholarly world, including the sympathetic French and other Continental botanists."[418] Even the proponents of the natural method acknowledged that it was hard to use, particularly for novices. Novices also had little help or encouragement to learn it, since most important and influential British botanists – Smith and W. J. Hooker, in particular – published works arranged according to the sexual system well into the 1830s.[419] Though S. F. Gray tried to remedy this situation in 1821 by publishing *A natural arrangement of British plants*,[420] his text lacked a hierarchical section and was difficult to navigate.

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[416] Candolle 1813, pp. 59-64, quotations on 64.
[417] An index to the genera in the *Prodromus* was published along with the first (unfinished) part of that work in a German edition of a collection of works of Robert Brown, *Robert Brown's vermischte botanische Schriften* (vol. 2, 1826), edited by Christian Nees von Esenbeck (Mabberley 1985, p. 253).
[418] Mabberley 1985, p. 162.
[419] The English Flora. By Sir J. E. Smith. . . vols i-iv. 1824-1828; the British Flora, by W. J. Hooker . . . vol. i. 3rd edition, 1835; the English Flora. Vol. v. part i. (Or, the British Flora, vol. ii, part i). By W. J. Hooker . . . 1833. Same works, vol. i. part ii. By W. J. Hooker, and Rev. M. J. Berkeley. . . 1836 1837, pp. 96-97.
[420] Gray 1821.

Still, in 1821, there was some indication that, despite a climate of rivalry between the proponents of the sexual system and the natural method, a compromise could be reached. As Sir J. E. Smith had suggested, botanists began using the two methods together. Sometimes botanists arranging their texts according to the sexual system included a synopsis of the natural method.[421] Sometimes the sexual system and natural method sections of the texts were self-contained and independent from each other.[422] It was only a small step to move from books arranged in these ways to those incorporating non-Linnaean identification keys – usually called "analytical tables" or "artificial analyses" – in lieu of the more exception-plagued sexual system.[423]

Lindley's contribution

The proliferation of introductory-level books including the natural method certainly had an effect on the way botany was taught and how the relative merits of the sexual and natural systems were understood in Britain in the 1830s. John Lindley (1799-1865) was the most influential populariser of the natural method, especially through his texts for the botany courses he taught at the University of London, starting in 1829.

Lindley was born in Catton, near Norwich, the son of the unsuccessful nurseryman George Lindley. He had wanted to join the military but his father's debts prevented him from buying John a commission in the army. He was, as his biographer William Stearn wrote, "a warrior at heart .... even when a famous professor, Lindley drilled at weekends as a volunteer in the south Middlesex militia; apart from weekend gardening and archery, this seems to have been his major recreation!"[424] The frustrated Lindley waged war on anyone who

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[421] E. g. Smith 1821, Smith 1824-1828, Hooker 1830, Hooker 1831, 2 (with additions and corrections) ed, Hooker 1833, Hooker 1835, 3 (with additions and corrections) ed..

[422] E. g. Hooker 1821. I have not seen an English work in which the Linnaean component was integrated so as to function as a key to the natural arrangement in the rest of the book, as Lamarck and Mirbel had done much earlier (Lamarck and Mirbel 1803, vol. 2, MacKay 1836).

[423] E.g. Lindley 1832, Lindley 1829b, Smith 1821, p. 61, Brown 1867b, vol. 2, Brown 1867a, . See Lindley's comment at the beginning of the section 'Sexual system dysfunction' to this effect (Lindley n. d. (between 1830 and 1845), 2 ed., pp. v-vi).

[424] Stearn 1999, vol. Bicentenary celebration volume p. 18.

disagreed with him in botanical matters, and his rigid beliefs about what constituted honourable behaviour did not always serve him well.

Lindley's entry onto the botanical scene was typical in this way. He forever won the ire of Sir J. E. Smith because, in the preface to his translation of a work by Louis-Claude Richard on fruit and seed structure, he praised Jussieu's natural method and bluntly stated that naturalists adhering to the sexual system were behind the times.[425] Smith responded with insults and indignation. After an acrimonious exchange of letters in the *Philosophical Magazine and Journal* in which neither man softened his position,[426] Smith essentially blacklisted Lindley for life.

Another example of Lindley's hot-headedness involves the souring of his relations with Robert Brown. Though Lindley worked in the Banksian herbarium under Brown from January 1819 to Joseph Banks' death in June 1820, and Brown was one of the three people upon whose recommendations Lindley became a Fellow of the Royal Society of London in 1829, Lindley refused to apologise for a comment he wrote in 1830 that Brown perceived as an insult. Lindley thought it sufficient to say that he had not meant to be insulting. Even when advised by friends and colleagues to make peace by apologizing, Lindley refused to back down, writing: "I think myself a better judge than they of what concerns mine own honour, and I am not likely to take them into my counsels as to what it is, or is not befitting to do." Lindley and Brown were estranged for the rest of their lives, Brown in particular going out of his way to avoid Lindley.[427]

John Lindley also "courageously" but unwisely took on the legal burden of paying off his father's debts in the 1820s in order to save him from bankruptcy. His prolific output of books "can be regarded as part of John Lindley's efforts to pay off debt. He moved among men of wealth as an intellectual equal but was never one of them. For most of his life financial anxiety was probably never far from his thoughts," especially as he also had to

[425] Richard and Lindley 1819. This was written before Lindley's 20th birthday.
[426] Lindley and Smith 1825.
[427] Stearn 1999, vol. Bicentenary celebration volume pp. 17, 18, 32, Mabberley 1985, pp. 289-290, 317.

support his wife, three children, and a number of domestic staff. When the newly-opened University of London (now University College, London) hired him as a professor in May, 1828, the income was a welcome addition to that from the post as Assistant Secretary of the Garden of the Horticultural Society at Turnham Green, Middlesex, an appointment he had held since early 1822.[428]

"Lindley, when appointed professor in London," wrote William T. Stearn, "like [William Jackson] Hooker [1785-1865] in Glasgow, had never heard an academic lecture in his life. Unhampered by tradition, both lectured well."[429] The same can be said about Lindley's approach to textbook-writing. Lindley wrote that he had felt compelled to produce new texts because nothing suitable for teaching the natural system was available in English, and that the foreign-language texts available were too specialized.[430] In particular, he made it his personal mission to persuade anyone who would listen that the natural method was the way of the future, whereas the sexual system was old news. For this reason, he specifically banned his students from using books by the late Sir J. E. Smith (who had died in March 1828) because of his favourable attitude toward the sexual system.[431] Instead, Lindley used identification keys ("analyses") and alphabetical indexes in conjunction with the natural system in texts such as *A synopsis of the British flora* (1829) and *An introduction to the natural system of botany* (1830) to make the natural method within them navigable. In this way, Lindley made plant identification for Anglophones easier and faster than ever before, a recipe for success at the booksellers', as evidenced by the multiple printings and editions of most of his introductory botanical works.[432] "In the first [1830] edition of this work,"

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[428] Stearn 1999, vol. Bicentenary celebration volume pp. 18, 28, 32.
[429] Stearn 1999, vol. Bicentenary celebration volume p. 33.
[430] Lindley 1846, pp. vi, x.
[431] Stearn 1999, vol. Bicentenary celebration volume p. 34.
[432] *A synopsis of the British flora* (1829) ran to three editions, and the third edition was reprinted. *An outline of the first principles of botany* (1830) was translated into German (1831) and Italian (1834), and a second English edition appeared a year after its initial publication. *Nixus plantarum* (1833) was originally published in Latin but it was also translated into German. Both *Nixus* and *Outline*, being out of print by 1835, were incorporated into *A key to structural, physiological, and systematic botany* (1835), which was also reprinted. The second edition of *An introduction to the natural system of botany* (1830) appeared under the title *A natural system of botany* (1836). Another work called *An introduction to botany* (1832) ran to four editions. *Ladies' botany*

Lindley wrote in the introduction to the second edition of *A natural system of botany* (1836), "I entered into some explanation of the fallacy of the common opinion that the artificial system of Linnaeus is easy, and the Natural System difficult of application. Within the last five or six years, however, the sentiments of the public have undergone so great a change upon this subject that I no longer find it necessary to go into such details."[433] What Lindley did not explicitly mention is that the natural system in his texts was made easy to navigate with the aid of his "analytical tables" (keys) and indexes.

Quick and easy plant identification in the field (without a master) – a recipe for selling books and for inspiring botanical minds

As I discussed earlier, Nehemiah Grew and John Ray were both concerned with devising techniques for quick and easy plant identification in the field and without a master.[434] And both Ray and Tournefort agreed that classification provided the best means to acquire new knowledge of plants and that classification was an instrument which "leads with certainty to the knowledge of plants by the shortest way, and without the help of a master."[435] Many other botanists, both novices and experts, shared similar beliefs, as evidenced by the success of botanical texts that allowed users to identify plants quickly, easily, and as independently as possible. Examples of such books from the 18th century were therefore not as easy to use to identify plants as later texts that employed the natural method, a hierarchical section, and an alphabetical index all together. Still, readers clearly thought they were sufficiently more convenient to use than their predecessors had been, for such texts often sold out and were reprinted, and sometimes out-of-print editions cost several times more than they did when they first were issued. For instance, as Sir J. E. Smith wrote,

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(1834) ran to six editions, and two abridgements were also published. Lindley published many more botanical textbooks than these in the 1840s. For a complete list of Lindley's publications (including the details mentioned above), see Allford 1999, .

433 Lindley 1836, 2 ed., p. vii.

434 Grew 1682, p. 174, Ray 1848, pp. 139-140.

435 McMahon submitted, pp. 19, citing Tournefort, 1699, p. 6: "Une méthode simple, uniforme, & qui conduise certainement à la conoissance des Plantes par le chemin le plus court, & sans le secours d'aucun Maître."

> The *Flora Anglica*, by Mr. William Hudson, F. R. S. an apothecary in Panton-street, Haymarket, published in 1762, marks the establishment of Linnaean principles of Botany in England, and their application to practical use. With this book in his hand, any one conversant with the Latin language, and with the first rudiments of systematic knowledge, might reduce a wild plant to its class, order, genus and species. By turning to the books indicated under each species, he would become acquainted with everything relating to its characters, history, or properties, and might confirm his own determination of the plants, by the figures and descriptions of former writers. This is the use of a systematic arrangement, and therefore the more clear and easy it is the better. Hudson's work became extremely popular, and rose in process of time to near twenty times its original price.[436]

William Withering's *Botanical arrangement of British plants* (1776), in which the sexual system was first applied to the flora of Britain in English, repeated the success story, going through "five full-sized editions... and three smaller ones" by 1836, though by then it was "almost obsolete as a work of science, although bearing a name still very good for the book-market."[437]

Perhaps the best example of a book that became both popular and influential because of its ease of use for plant identification was Lamarck's *Flore Françoise*. Michel Foucault has even remarked that Lamarck's emphasis on the distinction between the processes involved in plant identification and in "discovery of the real relations of resemblance" among plants in this work was more important to the development of natural history than were Lamarck's later ideas about transformatism.[438] The story of the origins, development and influence of the *Flore François* is strange. In addition to Lamarck writing it on a dare and impressing Buffon enough to secure his future career, as described above, the book went on to inspire and help the careers of other botanists. For instance, The Lillois botanist Jean-Baptiste Lestiboudois became familiar with the *Flore* shortly after it was published. He

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[436] Smith 1824-1828, pp. xiii-xiv.

[437] The English Flora. By Sir J. E. Smith. . . vols i-iv. 1824-1828; the British Flora, by W. J. Hooker . . . vol. i. 3rd edition, 1835; the English Flora. Vol. v. part i. (Or, the British Flora, vol. ii, part i). By W. J. Hooker . . . 1833. Same works, vol. i. part ii. By W. J. Hooker, and Rev. M. J. Berkeley. . . 1836 1837, pp. 95-96.

[438] Foucault 1970, p. 230, corresponding to pp. 242-243 in the 1966 French edition (*Les mots et les choses*). I thank Jean-Marc Drouin for bringing this reference to my attention. Foucault's obersvation was perceptive. However, I believe that he was overly optimistic in his assessment that Lamarck's clear explanation of the differences between techniques to identify plants and techniques to describe their *rapports* "within a few years . . . [rendered] natural history and the preeminence of *taxonomia* obsolescent." Foucault assumed a greater rate of change within taxonomy than I have seen, particularly if developments in the Anglophone world as well as France are taken into account (Foucault 1970, p. 230).

called it the "easiest and most entertaining" method of plant identification (as compared to Tournefort's and Linnaeus's systems).[439] Less than two years after reading the *Flore*, Jean-Baptiste's son, François-Joseph, also a botanist, published his highly successful *Botanographie Belgique* (1781), in which he employed some of the principles that Lamarck had brought to his attention. Aubert du Petit-Thouars – like F.-J. Lestiboudois, a former student of J.-B. Lestiboudois[440] – was also impressed by the *Flore.* He wrote to Lamarck, "I will always regard you as my master, having drawn the principles of botany from your *Flore Française* [sic]."[441] Augustin-Pyramus de Candolle also found the *Flore* of great use in his professional development, as I describe below.

Although Lamarck had done well in his botanical endeavours, the aftermath of the French Revolution forced him to change focus. Buffon, his protector, was long gone. When the Jardin du Roi became the Muséum National d'Histoire Naturelle in 1793, plum botanical positions went to others more in line politically with the new administration. Lamarck was able to keep a job there only by accepting to do research on the least prestigious animals, invertebrates.[442] He outdid everyone's expectations and excelled at zoology. Still, the public was not favourably inclined towards Lamarck's theorizing and the theological discomfort that it stirred up. He became increasingly isolated socially.

In the winter of 1796, the 18-year-old Genevois botanist Augustin-Pyramus de Candolle (1778-1841) arrived in Paris.[443] He contrived to get to know Lamarck. Candolle determined that Lamarck usually ate alone at a particular restaurant before meetings of the Académie Française. He arranged with a friend to sit "as if by chance" at the same table as Lamarck and talk at length about how useful he found the *Flore françoise.*[444] The ruse worked. Lamarck invited Candolle to visit him. Lamarck was impressed with Candolle, and

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[439] Leclair 1908, pp. 57-59.
[440] Leclair 1908, p. 43.
[441] du Petit-Thouars 1811b, p. 4.
[442] Williams 2001, p. 63.
[443] Candolle 2003, p. 85.
[444] Candolle 2003, p. 91.

some time later, asked him whether he would be interested in working on a new edition of the *Flore*. At the time, Candolle had turned him down, but in the summer of 1802, newly married and in need of money, he went back to Lamarck to ask him if he still wanted him to work on the project. Lamarck agreed right away, giving Candolle free rein to implement whatever revisions he thought necessary.[445] Candolle not only updated the descriptions of plants included in the work, but also made drastic changes to its structure. "In the original work," he wrote,

> the "*méthode analytique*" [dichotomous key] runs from one end of the book to the other, and the descriptions were placed wherever the dichotomy tossed them. This way of proceeding had many inconvenient aspects, some practical, others logical... Anyway, since the number of plants had increased so much since the first works of M. de Lamarck, the book printed according to the old plan would have been too bulky and impossible to take along into the field. But what's more, this *méthode* had the inconvenience of splitting up the families and in no way indicating their relationships. I adopted a course at once more convenient and more logical. In a series of volumes I described all the plants arranged in families and in the order of their real relationships, then, in a separate volume that one could take out botanizing, I completely redid the "*méthode analytique*." I see this system of arrangement as one of the principal reasons for the success that my book had, and I dare to talk here of this success as a material fact because despite its high price (50 francs), it sold 5000 copies![446]

Candolle's version of the *Flore*, now *Française*, was published in September 1805.[447] It in turn inspired George Bentham to become a botanist. After picking up his mother's copy by chance, young George

> was struck with the dichotomous tables for the determination of the plants, a plan which at once commended itself to his methodical mind. Gathering the first plant he saw, he tried to run it down by the aid of that book, and was long hindered by the articulation of the stamens of his subject, *Salvia pratensis*; but persevering, he succeeded in determining it, and his success induced him to prosecute the study.[448]

The third edition of the *Flore* was reprinted once in 1815. There was a market for it to be reprinted again, but the Candolles decided it was not worth the bother. As A.-P. de Candolle's son, Alphonse, also a botanist, explained, it

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[445] Candolle 2003, pp. 204-206

[446] Candolle 2003, pp. 206-207.

[447] Croizat 1945, p. 69.

[448] Jackson 1886, p. 91.

> sold out so fast and the work kept such a high value, despite the intervening changes in the science, that in sales of old books, examples cost three or four times the original price. A bookseller asked us insistently around 1833 or 1834, to come up with a new edition, and he offered us 14 000 francs for it, but the cause that made my father resolve to do it [i.e. poverty] did not exist any more and this kind of work was not convenient for him or me.[449]

It could also be that the Candolles got swept up in the enthusiasm for all things physiological that was popular at the time, and felt that reissuing a flora would require a great deal of revision that would not be worth the effort professionally.

Botanical "bait"

Artificial methods of plant identification continued to have appeal within the setting of botanical courses, however. Botany teachers realized that identification keys could give students a measure of independence which could persuade them to pursue botany with greater interest than through studying plants passively. Lyonnais botanical instructor Marie-Jacques-Philippe Mouton-Fontenille de la Clotte's (1769-1837) remarks on this matter sound as though they could have been written recently. Students, he wrote,

> do not want to take pains to determine them [plants], that is, to bring them methodically to their classes, orders, genera, species and varieties. They prefer, while botanising, to make the professor say the names of plants, which they retain for a while, but they always finish by forgetting, because they did not study them according to any principles. From this they do not develop a taste for the science, and this is why, of the great number of students who flock to the opening of botanical courses every year, there remain at most five or six who finish the course and who follow the professor regularly in his lessons and botanical field trips....
>
> The remedy that we have used to stop such a great harm was to show the students the advantage of the synoptic tables of Linnaeus's *Systema vegetabilium* to determine the genera, and to return to its artificial characters, without overburdening the memory. We have seen those who, at the end of eight days, managed to determine plants, and confessed that they felt great satisfaction, when they discovered the names of those that they were looking for. It could be, in some respects, a kind of bait that compels students to bite [*hameçon qui les forçoit de mordre*] at botany.[450]

That is not to say that many students did not find such "bait" distasteful. Then, as now, the process of keying out specimens was used by instructors keen on familiarizing students with

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[449] Candolle 2003, p. 207 note 412

[450] Mouton-Fontenille de la Clotte 1803, pp. 186-187.

characteristics of each species, genus or family. What had originated as a quick way to identify plants became more of a tour of the features of plants under study once it became co-opted for didactic purposes. François-Noël-Alexandre Dubois was one textbook author and teacher who used keys this way. He was proud to say that his key was structured so that "one cannot arrive at the name of a plant except by having examined with care all the characters that lead to it. Young people who study botany with my book are therefore forced to become observers."[451] The enthusiasm by which he describes how keys can impart knowledge to impatient youngsters brings to mind students squirming in their seats, daydreaming about anything other than keys.

The tedious aspects of identifying plants with printed keys have yet to be overcome even now. And certainly the state of affairs by the mid-1830s was not one in which all botanists had recognized that hierarchical and "natural" classifications were best separate, but used in combination. John Lindley still had to explain in an 1836 introductory botanical work not only how to use an identification key, but what readers should not expect an identification key to do:

> This table is of no other use than to shew you how to analyse the characters of the subjects you examine; it does not give you, as you must remark, a correct notion of the *essential* characters of any of the tribes, but it states clearly how they *differ* from each other. They differ from each other in many other respects, but it was not necessary to express any thing further in order to enable you to know them from one another.[452]

And even as late as 1837, Cottrell Watson recommended that flora writers use either a natural or artificial arrangement exclusively.[453] But the stage was set for others who saw the advantages of optimizing ease of use separately from reflecting plants' relationships to develop further refinements simplifying access to information about plants as soon as technical advances in illustrating books and, most recently, digitizing the process, permitted.

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451 Dubois 1803, p. xiv.
452 Lindley n. d. (between 1830 and 1845), 2 ed., pp. 69-70.
453 Watson 1837, p. 427.

Students may hate keys now, but looking back on their history, it is a wonder how botanists coped before their invention.

Part 2. Mental blocks and practical hurdles in the history of botanical classifications

Chapter 4. Patterns in nature, patterns in classifications

When arrangements of plants are meant to capture aspects of the way plants are arranged "in nature," there must, of course, be some correspondence between the two. This correspondence took many forms. One was the numbers and sizes of divisions within classifications. These were intimately linked to the number of plants and their relationships with each other. The ability of classifications to not only express our knowledge of the shape of nature but to predict what it is like in its entirety was another. Experimentation with rigid, regular patters showed, especially by the 1830s, that it was more difficult to predict the shape of nature *a priori* than was previously thought.

Taxon size as a mnemonic problem

As evidenced by the Figures in Chapter 1, the number of species of plants described and estimated to exist were increasing exponentially well into the 20th century. The number of genera of plants also increased exponentially ever since the genus concept was introduced. While some botanists decried the proliferation of new genera, others considered it to be inevitable. George A. W. Arnott pointed out in 1842 that the increase in genera was "unavoidable, from the increasing numbers of known plants. Nor does it appear that the number of genera is increased in proportion to the discovery of new species."[454] Decades earlier, A. P. de Candolle drew attention to the fact that

> The number of known plants will always grow, and consequently it is not extraordinary to assert that the number of genera tends to increase... [in fact]

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[454] Arnott 1832, 7 ed., vol. 5 p. 59.

today we have fewer genera in proportion to the number of known plants as there were in the time of Linnaeus.[455]

In the works of Linnaeus from 1750 to the death of his son (who continued his work) in 1783, the rates of increase of new species and new genera were growing in tandem so that the average number of plant species per genus increased geometrically, as shown in Figure 22. Although the average number of plant species per genus varied from author to author, it stayed in the low teens well into the 19th century. These were lower numbers of species per genus than Tournefort had had in 1694. It should not have been difficult for anyone familiar with a particular genus to memorize the species that belonged there.

But the low average number of species per genus does not tell the whole story of the greater botanical information management crisis. Aside from the sheer number of genera, the

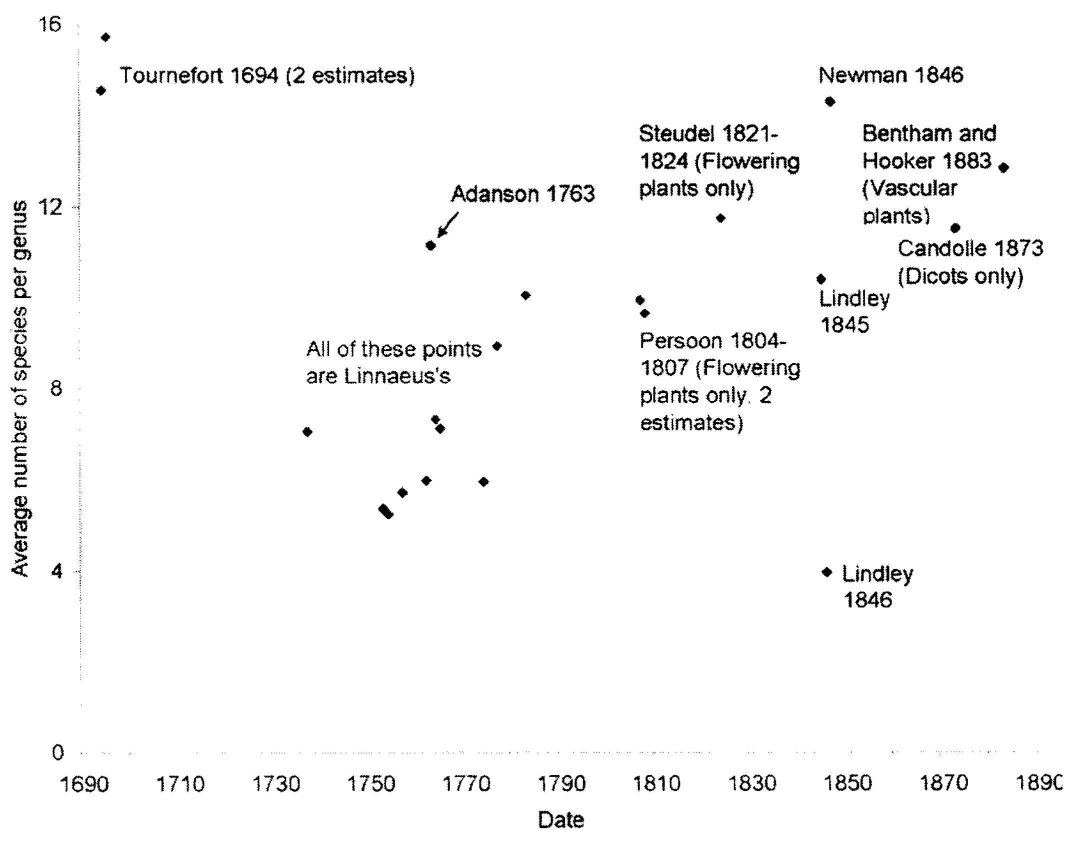

Figure 22. Average number of plant species per genus, 1690-1890.

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[455] Candolle 1813, p. 190. Candolle compared only the number of species and genera described by Linnaeus in the second edition of *Species plantarum* (7,540 species in 1,260 genera, a ratio of approximately 6 species per genus) with those described by Persoon in his "last work" (i.e. published in 1807), that is, 22,000 species in 2,280 genera, for 9.6 species per genus.

distribution of species among genera was not even. Botanists saw that the unequal distribution of taxa posed practical problems for anyone using the ranks in the hierarchical structure of the classification to find information. In Linnaeus's sexual system of plants, for instance, there were many monotypic genera and several large genera.[456] As well, Linnaeus himself noted that certain classes, particularly Pentandria and Syngenesia, "are absolutely enormous; and so with this system it is more difficult than with others to distinguish the genera" in them. "Classes and orders that are too long [i.e. contain too many genera] or too numerous cause great difficulties," Linnaeus wrote.[457] He did not provide any way to solve this problem, so botanists looking for particular genera in, for instance, the class Pentandria, "sometimes were at pains to find them."[458] Colin Milne likewise lamented that in certain large plant families such as the mint family, "I discover so many resemblances and so few differences [among the genera], that I am almost tempted to make one enormous genus of the whole, till I reflect that by such proceeding I gain nothing in point of facility, as the trouble thus spared by the diminution of genera is more than equalled by the prodigious multiplication of species."[459] "The enormous weight of genera," Milne later wrote, "is, in fact, no less prejudicial to Botany on the one hand, than the unnecessary multiplication of

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[456] An impressive amount of ink has been devoted to the size distribution of taxa, as part of a larger project to determine the degree to which human biases influence the subdivision of taxa at different ranks. If this influence can be quantified, it may be possible to examine patterns in evolutionary divergence by comparing taxa on the basis of the number of subtaxa they contain. Minelli, Fusco and Sartori 1991summarized much of this literature, including the suggestion by Quentin Cronk (Cronk 1989) that the patterns we see come 50% from human factors in classification and 50% from evolutionary history (largely from extinction of taxa).

This project was started by botanists, beginning with J. C. Willis in the 1920s. Willis plotted genera according to the number of species they contained and noticed a "hollow curve" pattern: there are a few genera containing many species, and many genera containing few species. S. M. Walters, impressed with the recent results of ethnobiological research, expanded on this issue in an influential 1961 paper in which he suggested that the pattern results more from European bias in angiosperm classification rather than evolutionary history. The hollow curve, he argued, resulted from the excessive splitting of salient taxa, and excessive lumping of relatively unknown foreign, inconspicuous, and/or economically unimportant taxa (Walters 1961). A number of interesting contributions focussing on human factors have been published since Walters reviewed the debate in 1986 (Walters 1986), for instance, Peter Stevens (Stevens 1997b, pp. 294-296, Stevens 2002, Stevens 1997a, pp. 247-249) discussed how cognitive constraints played a role regulating the sizes and numbers of plant genera and families in the *Genera plantarum* produced by both Jussieu (1789) and Bentham and Hooker (1863-1883).

[457] Linnaeus 1751, p. aphorism 207, cited Linnaeus 2003, p. 149.

[458] Candolle 1813, p. 47.

[459] Milne 1771, p. 29.

their number is on the other. To avoid either extreme ought to be the care of every systematic writer, who cannot be too often reminded that the science is in equal danger from both."[460]

Uneven taxon size

These problems of large genera on one hand and large numbers of monotypic genera on the other were the same in both botany and entomology, two fields of natural history dealing with very large numbers of species, as 18th-century naturalists recognized. "The insects," Jean-Antoine-Nicolas de Caritat, Marquis de Condorcet (1743-1794) explained in1777, "by the number of their species, the diversity of their forms, [and] the varied structure of their parts, must be studied using the same method as plants, and classed like them in methodical divisions."[461] Botanical practice was used as a model for entomologists because problems associated with keeping track of information about huge numbers of species surfaced in botany first. In fact, the number of described plant species exceeded the number of described insect species until the last decades of the 19th century, as shown below in Figure 23.

The number of insect species estimated to exist, however, began to exceed the estimated number of plant species among entomologically-minded naturalists in the second quarter of the 18th century, when they began to appreciate the enormous diversity of insects. For instance, in 1734, René-Antoine Ferchault de Réaumur suspected that "the number of different insect species must be in the thousands ... for surely they outnumbered the twelve or thirteen thousand plants already named."[462] Buffon also recognized that the number of species of insects was likely to exceed that of plants.[463] Insect taxonomy looked to pose immense problems.[464] That is not to say that everyone recognized the situation. Some late

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460 Milne 1771, p. 131.
461 Condorcet 1780, p. 5.
462 Winsor 1976, pp. 58 citing Réaumur 1734, vol. 1 p. 2.
463 Buffon 1749, vol. 2, p. tome II p. 10.
464 John Ray, who had studied both plants and insects closely, had an inkling of this diversity decades earlier, but he could not decide either way: in *The Wisdom of God* he says in one place that there are likely more insect species than plant species, but on the next page he says the opposite thing (Ray 1691, 2 ed., pp. 4 and 5-6).

18th-century botanists, including Antoine-Laurent de Jussieu, and Aubert du Petit-Thouars, still believed that there were more plants than animals.[465] The popular belief that there were more plants than insects likewise persisted at least into the 1830s, as shown below in Figure 24.[466] Insect taxonomy received far less attention than plant taxonomy did – far less than it deserved.

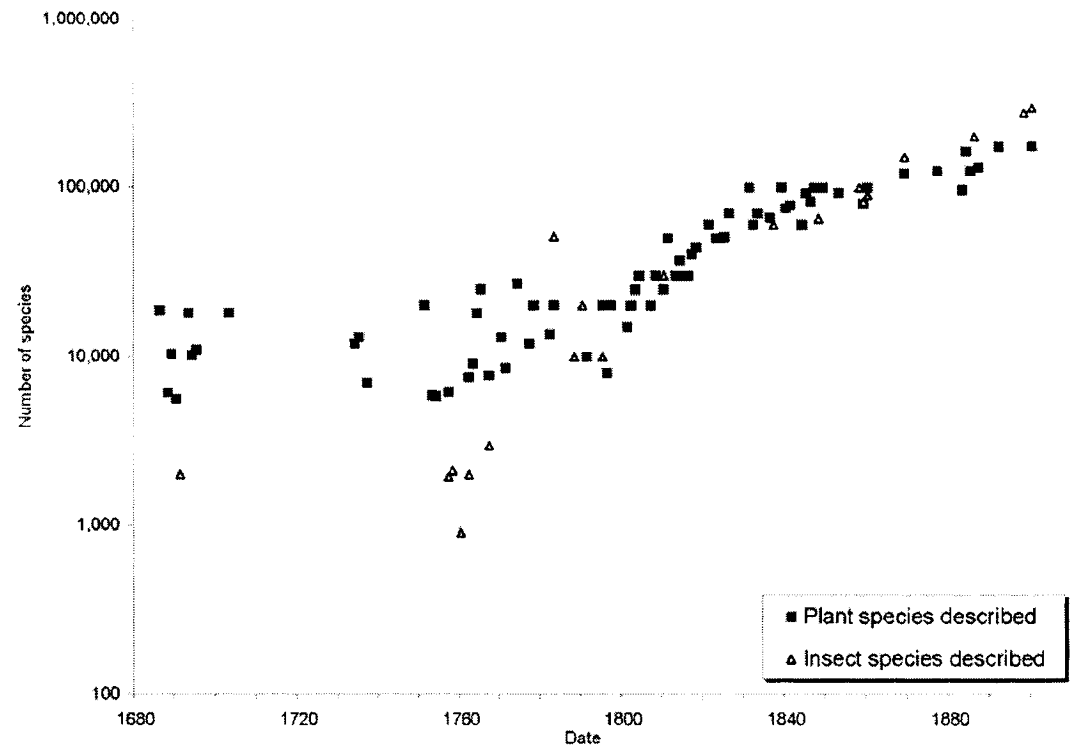

Figure 23. Numbers of species of plants versus numbers of species of insects known to science, 1680-1900.

Linnaeus's classification of insects, for example, was not very elaborate. By the late 1820s and early 1830s, entomologists were confronting the fallout from the lack of organization head-on.

As the lepidopterist James Duncan wrote, "The Linnean distribution was vague and unsatisfactory, even at the time when it was first produced, and soon became utterly

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465 Jussieu 1994b, pp. 175, 196 cited Stevens 1994 pp. 295, 331; du Petit-Thouars 1811a, p. 27. Du Petit-Thouars had changed his mind by 1811, writing, "the insects are becoming uncountable" (du Petit-Thouars 1811a, p. 46).

466 Henderson 1832, p. 160.

inapplicable when the amount of known species was increased."[467] The famed author of *Horae Entomologicae*, William Sharpe MacLeay, had cursed out James Ebenezer Bicheno, lawyer, botanist and Secretary of the Linnean Society, for suggesting in 1827 that Linnean genera should not be divided. "I happen to have seen more than 2000 species of the Linnean genus Scarabaeus, when Linnaeus himself saw little more than 80," he added.[468] The

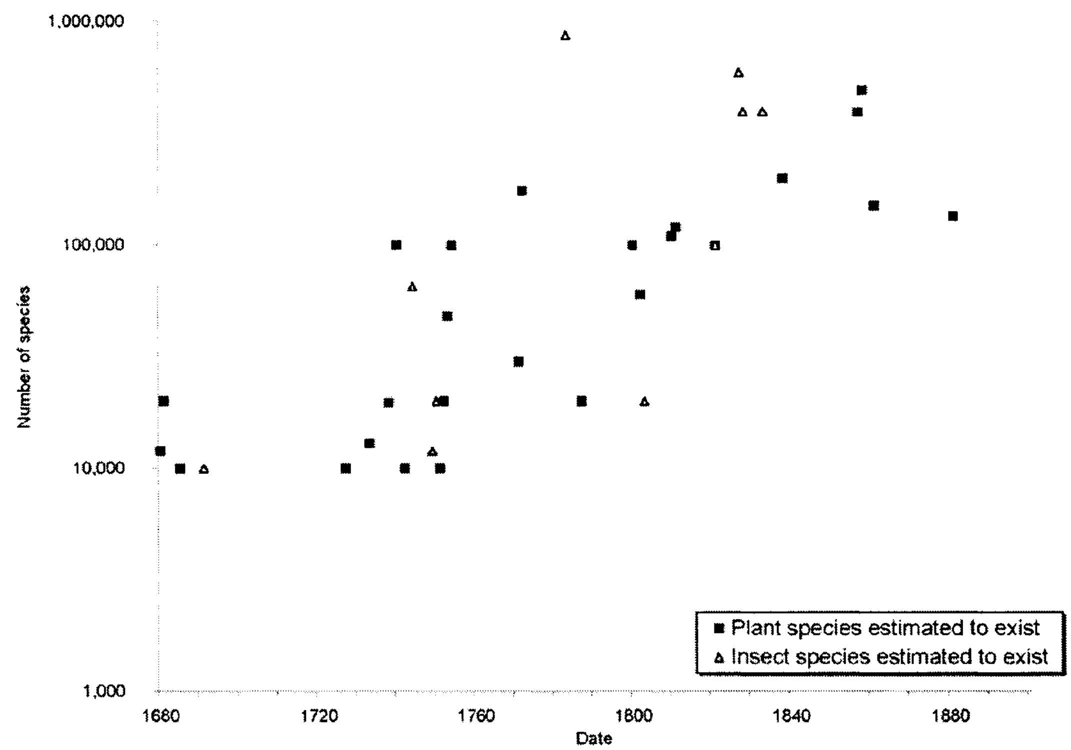

Figure 24. Numbers of species of plants versus numbers of species of insects estimated to exist, 1680-1900.

Reverend William Kirby and William Spence agreed about the scope of the problem. Though normally extremely polite and reserved, these authors of the popular guide, *Introduction to entomology,* suggested that the number of species per genus of insects posed a serious problem for the memory. They drew attention to the frustration that they felt when dealing with some of the larger genera, complaining,

[467] Duncan 1837, p. 77.
[468] MacLeay 1829, p. 206.

> To be obliged to compare a single individual with the descriptions of from 100 to 300 species, to ascertain its name, seems enough to make you start aside with horror from the employment, and be content that your species should remain unnamed, rather than expose yourself to such a waste of time and patience.[469]

Eighteenth- and 19th-century botanists certainly felt the same. The answer, it seemed, was to reduce the number of subtaxa in each taxon – but how?

Coping with uneven taxon size

Adding levels to the hierarchy

Botanists' first approach was to introduce more levels in the hierarchy for *all* taxa, both large and small. This process is akin to the one that Scott Atran suggested had happened when species were organized into genera in Cesalpino's and later Tournefort's work, and when genera were later put into families. In all cases, the rationale was to reduce the number of taxa to something memorisable.[470]

For instance, Linnaeus wrote in the introduction to the first edition of *Genera plantarum* that his sexual system, which included orders as well as classes above genera, would be "preferred" because it is one that

> leads to the genera by the more certain and trouble-free path, and the one that is the most universal. For I believe there is hardly anyone born with such a memory, that he could retain the genera without a System. The Method must therefore lead the way.... And no one will deny that it is easier to distinguish a few genera [at a time] than all at once.[471]

Likewise, John Hill liked the idea of using orders to subdivide classes in his 1770 "index" (identification key), since they, "by dividing the number of Plants in the Class, will make any one that is sought, the easier to be found."[472] But, as I have explained above, this solution was not adequate on its own. There were still large numbers of species in some genera and large numbers of genera in some orders of the sexual system.

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[469] Kirby and Spence 1828, 5 ed., vol. 4, pp. 562-563.
[470] Atran 1990, pp. 9, 10, 139-142, 167, 263, Atran 1981, pp. 47-48, Atran 1987, pp. 256, 265.
[471] Linnaeus 2004 (not published), pp. 3-4.
[472] Hill 1770, 2 ed., vol. 1, p. 46.

Subdividing large taxa

The problem of unmanageably large taxa seems to have crept up on the sexual system over time. When Linnaeus published the *Species plantarum*, the largest genera were *Lichen* (80 species) and *Polypodium* (a fern, 58 species), both of which were in the relatively under-studied class of Cryptogamia. In the flowering plants, the genus *Euphorbia* had 56 species, "and only thirteen other genera have thirty species or more," as S. M. Walters indicated. Linnaeus did not need to consciously avoid creating large genera or split large genera because his predecessors had already done the job for him by giving distinct names to many European plants that, botanically speaking, would probably have been considered to be in the same genus, had they been less familiar. Each large genus that was relatively small when Linnaeus described it therefore "represents relative taxonomic ignorance at the time of Linnaeus;" genera that later exploded in size tended to be non-European, or inconspicuous and economically unimportant, or both.[473]

One such genus was *Solanum,* which is largely South American and includes the potato. In 1778, Lamarck complained that finding this genus in the class and order Pentandria monogynia meant searching through 130 genera. Evidently this was an irritating task despite Linnaeus's having subdivided the order into unnamed divisions to make finding genera within it easier.[474]

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[473] Walters 1961, p. 80.

[474] Lamarck 1778, vol. 1, pp. xlv-xlvi. Linnaeus's insect genera posed an even bigger problem. A.-M. Constant Duméril remarked in 1806 that a particular family of beetle had started out as a genus, but that since its debut it had grown to more than 1200 species and was in need of further divisions in order to keep track of them (Duméril 1806, p. 226). Kirby and Spence took issue with the inadequacy of Linnaeus's insect genera to manage the numbers of species within them on a larger scale – Linnaeus's 83 genera, containing an average of 35 species each, were inadequate to accommodate the 100 000 species of insects estimated to exist by the end of the 1820s. Kirby and Spence proposed that Linnaeus's genera be elevated in rank, and many subordinate genera created. They even flirted with quinarianism for this reason, despite the problems that rigid grouping by fives would pose when new taxa were discovered: "could the trifling difficulty occasioned sometimes by the discovery of a new group, be set against the advantage of having only 2000 names to commit to memory instead of 10, 000?" (Kirby and Spence 1828, 5 ed., vol. 4, pp. 404-405).

Jussieu: adding levels to the hierarchy and subdividing large taxa (including splitting)

By the 1780s, there were enough such gigantic orders and genera to merit a fresh approach to arranging them. When Antoine-Laurent de Jussieu published his *Genera plantarum* in 1789, he took such a new tack. First, he changed the hierarchy to add more levels. At the highest level, he split all plants into three groups according to their number of seed leaves: the Acotyledones (no seed leaves), Monocotyledones (one), and Dicotyledones (two). "The Acotyledons stay undivided until their organization is better known," he wrote, so they consisted of one class. He split the monocots into three classes on the basis of how stamens were inserted, while "the exceedingly numerous Dicotyledones, whose number is almost ten times greater than that of the preceding [Monocotyledones], very truly require a multiplication of classes." He split them into 11 classes, depending on the number of petals they had and their insertion of flower parts.[475]

While he increased the numbers of levels of the hierarchy used to organize plants, Jussieu restricted the number of orders of all plants to 100, and ensured that none of them contained more than 99 genera. Furthermore, he insisted that these orders be natural, that is, that plants in them would have many features – not just stamen and pistil numbers – in common with each other. This would save space in writing generic descriptions and mental effort in remembering them.[476] He also strove to keep the groups that he made within manageable size limits, writing that botanists should look for characters to use to split up plant classes, orders and genera that get too big. For example, Jussieu went as far as to split the Compositae (daisy family; a natural group recognized as such by earlier botanists, including Linnaeus) into three groups, likely for this reason.[477] Conversely, Jussieu did not allow monotypic families, and "the small size of a group might make him hesitate to divide it," so as to keep the total number of plant families and the number of genera per family

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[475] Jussieu 1789, pp. liii, lxxi, Stevens 1994, pp. 373, 383.
[476] Jussieu 1994a, p. 239, Stevens 1994, p. 294.
[477] Still, as Stevens points out, Jussieu was not the first to split the composites (Stevens 1994, pp. 46, notes 120 and 121).

relatively small and manageable.[478] "The genius of the method lay in its capacity to provide indefinite flexibility without violating naturalness," one twenty-first century commentator wrote, emphasizing that since Jussieu believed nature to be continuous, he could draw the lines between groups of plants wherever he pleased, without feeling that the groups he was delimiting were more arbitrary than those that would have resulted from following any other procedure.[479]

Jussieu sanctioned his explicit plan to take the limits of human memory into account when delimiting "natural" plant groups by his belief that nature is continuous and that the composition of any groups we perceive is therefore arbitrary. But thinking about how to make groups memorable was assisted by the "clumpy" and uneven way that natural groups are distributed as much as it was necessitated by the problems finding information about plants in large taxa using hierarchical methods of data management alone. This problem was particularly acute in taxa which contained plants with diverse features. In such cases, it made more sense to separate out identifiable groups than to include them in large and heterogeneous taxa. Robert Brown did just this when he separated the Asclepiadeae from the Apocinae of Jussieu.[480] Close attention to "obvious and important" characters present only within a subset of the larger group of plants (in this case, the unique pollen-bearing structures of the Asclepiadeae) had led Brown to produce smaller groups of plants easier to delimit and remember. This action and others like it quietly struck a blow against the idea of continuity in nature.

Despite Linnaeus's, Jussieu's, and others' efforts, the problem of finding species in large genera remained. It was a necessary evil if "natural" genera were to be maintained.[481] By the turn of the 19th century, botanists were arguing for divisions of genera to help sort

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[478] Stevens 1994, pp. 45-48. Stevens provides many examples in which Jussieu adhered to these principles, and a few in which he ignored them.
[479] Williams 2001, pp. 60-61, Stevens 1994, pp. 32, 45.
[480] Brown 1867a, p. 12.
[481] In the *Flore Françoise* (1779), Lamarck also insisted that nature was continuous, but suggested that botanists use *artificial* genera that were as easy to delimit as possible, for plant identification purposes (Lamarck 1778, vol. 1, pp. xvii-xviii, lxxiv, "cxi" (xci)-xcii).

through those containing more than a handful of species. For instance, François-Noël-Alexandre Dubois presented his readers with divisions of genera purely for the sake of plant identification in a particular key,[482] while Charles-François Brisseau de Mirbel recommended limiting the number of species in each genus while making sure that the number of genera themselves did not become a burden to memory.[483] Whether botanists believed genera to be real entities or figments of the imagination matters little in the information management context.

Ambrose-Marie-François-Joseph Beauvois de Palisot used the same reasoning when he countered objections to his having created two new fungal genera, *Favolus* and *Microporus,* from species within *Agaricus* (Juss). If distinctions as marked as those that he used to separate *Microporus* were considered insufficient to demarcate genera, he argued, *Agaricus* would contain more than 500 species. And although *Favolus* "strictly speaking, appears hardly natural to some botanists ... its formation makes the study of a genus very rich in species easier." He agreed, "along with LINNAEUS and all educated botanists, that one must be very hesitant to multiply genera, but I also believe that this principle is subordinate to the ease that must be introduced to the study of plants."[484] With these concerns in mind, it is understandable why A.-P. de Candolle – who strongly believed that natural groups were discrete – could write that

> it must be admitted that, in the case ... [of] some ambiguity, one must readily establish or admit a new genus, composed of several species, rather than counting each one of them as a single [genus and].... in an ambiguous case, give some importance to the number of species in the formation or adoption of genera.[485]

This is a roundabout way of saying, as Colin Milne had done years before, "if an unnecessary multiplication of the genera is to be avoided on the one hand, an unnecessary

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[482] Dubois 1803, pp. x-xii.
[483] Lamarck and Mirbel 1803, vol. 2, pp. 252-253.
[484] Palisot de Beauvois 1804, vol. 1, pp. 1, 13.
[485] Candolle 1813, p. 189 and also 194.

reduction of them is no less to be shunned on the other."[486] Botanists would do well to balance the size of families they form with the number of genera they create to organize them; too many genera are difficult to locate within a family, whereas too few genera make finding species within them difficult.[487]

Balancing numbers of levels in the hierarchy with taxon size

Despite the value of restricting taxon sizes to aid the memory, the ideal classification for information retrieval purposes does not depend only on cognitive constraints on the *size* of taxa.[488] Increasing the number of levels in organizational hierarchies to keep taxon numbers down could only go so far. For instance, increasing the number of botanical families to cope with growing numbers of genera was only a temporary measure. On average, for instance, there were approximately 12 genera per family in Linnaeus's *Classes plantarum* (1738) and 16 genera per family in *Philosophia botanica* (1751),[489] 26 genera per family in Adanson's *Familles des plantes* (1763),[490] 20 per family in Jussieu's *Genera plantarum* (1789), but 470 genera per family in Bentham and Hooker's *Genera plantarum* (1883).[491] It was also important to minimize the number of hierarchical levels that a user would need to descend through in order to find a taxon. Too few ranks and taxa would be difficult to sort through; too many get confusing in and of themselves, especially if they all have to be named and memorized. This problem was recognized as early as the turn of the 18th century. Tournefort, for instance, was careful to make sure that the number of genera he put forth would be large enough so as not to overstuff each one with species. He saw that

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[486] Milne 1770, p. GENUS.
[487] Lamarck 1778, vol. 1, p. xxxviii makes a similar statement.
A recent mathematical analysis comparing the size distribution of many taxa suggests that botanists have heeded this advice (Scotland and Sanderson 2004, p. 643).
[488] Cf Stevens 1997a, Stevens 1997b, Stevens 2002, Atran 1990, pp. 9, 10, 142, 167, especially 263, Atran 1981, pp. 47-48, Atran 1987, pp. 256, 265.
[489] My calculations; data from Williams 2001, p. 26.
[490] My calculations; cf Stevens 1997a, p. 248, who says there were "almost 11 genera in the next highest taxon."
[491] Stevens 1997a, p. 246.

there must be a balance between the number of species in a genus and the number of genera to memorize.[492]

Lamarck also acknowledged this problem, as did J. E. Bicheno, but not everyone writing on this topic agreed.[493] Peter Stevens suggested that George Bentham had removed this difficulty by interpolating informal groupings among the formal ranks, so that the small groups that would result "could readily be compared and their distinguishing features recalled;" A.-P. de Candolle, J. E. Bicheno and the Rev. Leonard Jenyns had suggested a similar approach even earlier.[494] Although this technique works well to help direct users in monographs and other restricted-scope classifications, on a large scale and with many interpolated ranks I am doubtful that its mnemonic usefulness would hold, regardless of whether the ranks are named.[495]

Furthermore, regardless of the number of levels of a hierarchy that people can keep in memory, the textual format of botanical works also makes it desirable to minimize them. Almost every new grouping, be it a rank or a level in a key, requires users to flip a page or read at least one new description. Since the vast majority of botanical works did not (and do not) provide a means to trace one's path back in the case of error, the more ranks there are, the greater the chance of error, and the greater the necessity of starting the identification at the beginning again. In other words, the higher the number of levels in a hierarchy, the more tedious the identification process.[496]

Calculating the ideal balance

Under this scenario, all else being equal, many groups of roughly equal size at each rank in an inclusion hierarchy would be preferable to either many small groups (e.g.

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[492] Tournefort 1694, p. 38 cited Atran 1990, p. 166.
[493] Lamarck 1778, vol. 1, pp. liii, lxxvii-lxxix, Bicheno 1827, p. 489. See Chapter 6 section on dichotomy for more details.
[494] Stevens 1997a, p. 246, Bicheno 1828, p. 495, Jenyns 1833, p. 390, J. S. 1829, pp. 349-351.
[495] The entomologist J. O. Westwood argued that "it has been ascertained by practice, that it is much easier to recollect these minor groups when named, than when designated by stars, daggers, &c., after the old fashion" (Westwood 1833, p. 120).
[496] As Colin Milne pointed out, particularly regarding Ray's, Morison's and Magnol's arrangements (Milne 1771, p. 29).

monotypic ones) or a few very large groups. If all groups of taxa are the same size, the optimal (minimum) number of ranks above the species level for organizing *n* plants in groups of *m* members is the smallest integer not less than the base *m* logarithm of *n*. This "integer above" function is known as the "ceiling." The minimum number of ranks (r) required would then be

$$r = \text{ceiling}\ (\log_m (n))$$

Using this formula, a dichotomously branching hierarchy would need, for instance, at least 14 ranks above the species level to organize 10 000 species. This was the number of total plant species that Linnaeus thought existed as of 1738.[497] Calculations using the same formula show that Linnaeus's ideal "system," interpreted as grouping by tens rather than twos at each rank, would need only 4 superspecific ranks to accommodate the same number of species.[498] A dichotomously branching hierarchy would also require a minimum of 16 ranks above species to accommodate 40 000 or 50 000 plants, the numbers of species said to be known to science when Jeremy Bentham's *Essay on nomenclature and classification* and its partial French translation were respectively written. Although neither Jeremy nor George Bentham did the calculations, both of them assumed that this was too many bifurcations to make a dichotomous arrangement of plant species worthwhile (I discuss the particulars of how they came to their decision in chapter 5).[499]

Unfortunately, all else is never equal. Naturalists generally wanted to make sure that species in particular, but also genera and sometimes larger groups, were as "natural" as

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[497] Linnaeus 1738, p. lectori s. author.

[498] Stevens 2002, p. 13.

[499] Bentham 1823, p. 71, Bentham 1983, p. 252. Curiously, in Bentham's autobiography he mentions having looked forward to the "arrival of successive livraisons of 3 vols of a *Dictionnaire d'Histoire Naturelle*, then in course of publication" in the summer of 1818. He credited this dictionary with opening his eyes to "the wonderful variety and combinations of natural objects" and inspiring him to start a herbarium. The editors of the autobiography identify this work as "*Nouvau Dictionnaire d'histoire naturelle, appliquée aux arts, à l'agriculture et à l'économie rurale et domestique, par un société de naturalistes et d'agriculteurs*, 36 vols. (Paris: Deterville, 1816-1819)." However, this dictionary has only the most rudimentary articles on botanical topics. The *Dictionnaire des sciences naturelles, dans lequel on traite méthodiquement des différens êtres de la nature* [etc.] on the other hand, was originally published from 1804-1806 and reissued in amended form starting in 1816. Du Petit-Thouars's article *Botanique* came out in a volume issued in 1818. The reasoning that George Bentham used less than five years later when he published his reworked version of his uncle's *Essay* is quite similar to that used in this particular article. Based on these similarities, I suspect that the dictionary Bentham consulted was the *Dictionnaire des sciences naturelles*.

possible. This meant taking distinctiveness into account when circumscribing taxa, and characteristic features and salience, including familiarity, economic importance and obviousness, do not fall into neat patterns.[500] Calculations of the minimum possible numbers of ranks are therefore only approximate at best. Still, some botanists found other ways to optimize the identification process. For instance, Dubois did his best to organize the dichotomous key in his flora of Orléans so that users would have to read through a minimum of descriptions in order to find descriptions of their specimens. "In all the analyses," he wrote,

> you will continually find two characters bracketed together, between which you must choose. They are combined such that one will direct you to the plant that you have before your eyes, and the other does not direct you to it, and it is only after having gone through all the characters that pertain to that plant that you will get to its generic name. I took care to present, for the first character, the one of the two bracketed characters that directs you to the larger number of plants. Here is the reason that made me decide to do this: when analysing a plant, you are always obliged to read the first of the bracketed characters. So, you must then read it [up to] 48 times uselessly if, out of 50 plants that you were analysing, there were only two to which it applied. Nevertheless, I distance myself from this rule, because I could not follow it without becoming obscure, or without giving rein to confusion.[501]

It seemed that even well-thought out rules about how to divide plants up in the most convenient ways fell to pieces when they faced the sheer diversity they were intended to manage. But that did not stop botanists from trying.

Good arrangements are predictive; changes in arrangements of organisms show a lack of foresight on the part of their authors

Botanists faced another mismatch between theory and practice in their attitudes toward how predictive arrangements of organisms should be. While it was a common belief

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[500] Aubert du Petit-Thouars clearly understood this (du Petit-Thouars 1817, vol. 5 pp. 227-228).

[501] Dubois 1803, p. x. Similarly, A.-M. Constant Duméril tried to keep the number of branch points in his keys to all animal genera in his *Zoologie analytique* (1806) to eight or below (Duméril 1806, pp. xviii-xix) – for which he was praised by George Bentham (Bentham 1823, p. 70).

that taxa that appeared anomalous would eventually be connected to other known groups by intermediate forms yet to be found, naturalists also frequently promoted each new method or system as the be-all and end-all of plant arrangements. Since systematic schemes were expected to stand the test of time, the need to revise an arrangement of plants was seen by some botanists as a failure on the part of its author to think the choice of characters through, rather than as a necessary consequence of learning more about previously poorly-known taxa. In this respect, naturalists failed to accept in principle what they clearly knew in practice: that each arrangement of organisms they produced was a step in a community-wide process in which it was perfectly normal for botanists to revise their schemes, or develop new schemes, in order to incorporate new information about plants.

Along similar lines, it was popular before the mid-18th century to play up the *a priori* aspects of classifications even when authors had used plenty of empirical data in their construction. Too much emphasis on the empirical would presumably detract from the virtuosity of a well-made scheme, which would be manifested in its author's ability to have predicted new organisms or aspects of organisms of which he was unaware when he proposed it. In this sense, explicitly empirical, *a posteriori* schemes requiring repeated revisions were not regarded as adequately scientific.

That Linnaeus secretly made use of features other than those of the fructification when assembling some plant genera, as his son recalled,[502] indicates that he, too, was trying to present the world with the impression that he had shown botanists all they needed to know about delimiting genera when he devised the sexual system. Having to consult plant parts other than stamens and pistils in order to delimit genera would be an admission that his first set of instructions was inadequate.

By the end of the 18th century, however, the increasing adoption of the explicitly *a posteriori* natural system demonstrates the decreasing privileging of the use of *a priori*

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[502] Stafleu 1971, p. 71.

arrangements in practice. This is not to say that predictiveness lessened in importance. Botanists continued to seek patterns in nature with predictive properties, only now, patterns formed *a posteriori* from empirical research became more acceptable. There was greater interest in what was shown to work well, rather than in what was supposed to work, but could not be proven to do so.

As such, proponents of the natural system banked on its proven predictive ability to draw in new adherents. Jussieu, for instance, suggested that the natural method could act as a map for explorers, in which relatively unknown areas could be plotted with respect to known ones and then subjected to further inquiry,[503] and medicinal properties going along with natural groups.[504] These predictions, however, were suggestions to be tested empirically rather than statements to the effect that organisms to be found using the natural method as a guide *will* exhibit certain combinations of features.

Despite the growing acceptance of the natural method by the end of the 18th century, however, the intellectual climate in favour of *a priori* schemes took a lot longer to change. Some authors of natural systems acknowledged this pressure to devise schemes that were *a priori* predictive by emphasising that their schemes were "fragments" or "*prodromi*" (introductory sketches) rather than finished works,[505] likely to defuse criticisms of incompleteness and inadequate theoretical support. For instance, Antoine-Laurent de Jussieu explained that his uncle Bernard had not published the details of the natural method according to which he had arranged the plants at the Paris Trianon in 1759 because "this way of proceeding does not allow divisions that are too methodical... this modest man has not published his work because he believed himself too little advanced in the science."[506] And when Sir James Edward Smith, President and founder of the Linnean Society, was forced by

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503 Jussieu 1789, pp. xxxvi-xxxvii, Jussieu 1994c, p. 356.
504 I give more examples of this below.
505 E.g. Linnaeus 1764, 6 ed, Brown 1810.
506 Jussieu 1994b, p. 192, Stevens 1994, p. 309.

the tide of scientific opinion to include information about the natural system of botany in his 1821 *Grammar of botany*, he put in the disclaimer that

> the reader must not consider this publication as any thing like a complete view of a Natural System, but rather, to use a French idea, as *Memoirs towards a System*. Much still remains to be done by future observers, and still more by future systematic writers.[507]

Even into the 1840s, John Lindley felt pressured to explain to critics why he continually rearranged plants within his books about the natural system rather than putting forth an immutable arrangement, once and for all. The takes on the natural system that he had put forth in the past were flawed, he wrote, but they were honest efforts at describing the relationships among plants known at the time. He was not so presumptuous as to assume that he could do better than that. Lindley wrote, however, that his critics "have imagined that a natural classification of plants is something which is to suddenly start into existence, perfect in all its parts, and their criticisms betray a total ignorance of the difficulties by which such a subject is surrounded.... for perfection is the attribute, not of man, but of his Maker." Lindley added that he had never claimed to have come up with a permanently applicable system,

> In fact, there is no such thing as stability in these matters. Consistency is but another name for obstinacy. All things are undergoing incessant change. Every science is in a state of progression, and of all others the sciences of observation most so... This is inevitable in a science as that of Systematic Botany, where the discovery of a few new facts or half a dozen fresh genera may instantly change the point of view from which a given object is observed. The Author cannot regard perseverance in error as commendable, for the sake of what is idly called consistency; he would rather see false views corrected as proof of their error arises. His object, and, he thinks he may say that of every one else who has turned his attention to this question of late, has not been to establish a system of his own, which shall be immutable, but to contribute to the extent of his ability towards that end ... All that we can do is to throw our pebbles upon the heap, which shall hereafter, when they have sufficiently accumulated, become the landmark of Systematical Botany.[508]

A belief in the superiority of intellectual skills required to come up with *a priori*-type methods for arranging plants and animals over more empirical ones relied highly on a static view of both nature and, as Lindley intimated, the workings of naturalists themselves. The

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507 Smith 1821, p. xiii.
508 Lindley 1846, pp. xi-xii.

idea that a few well-chosen characters will serve to pick out organisms in a given taxon assumes that no new members of that taxon lacking those characters or bearing ambiguous characters will be discovered, or that those discoveries will have no impact on subsequent classifications. The "landmark of Systematical Botany" was to be a system which would incorporate all of this complex information, provided piecemeal by many different researchers, and would keep on growing as knowledge about plants grew. But how would it balance incorporating this information with providing users with a relatively stable way of accessing it?

Predictiveness comes from keeping like with like

Finding information about plants quickly and easily was a pressing need, but some critics wanted arrangements of plants to let them do more than that. Plant identification on its own was laudable but not "philosophical" enough. An ideal classification would accomplish this mundane – if difficult to achieve – goal and then some. It would be a guide not only to the items already in it, but to items in the world beyond: it would be a guide to things yet to be found.

In the 18th and early 19th centuries, there were two main approaches to making classifications as predictive as possible. One relied on keeping like organisms with like at all costs, as described earlier. The other combined and tempered this practice with arranging organisms into patterns, for instance, in an order of increasing or decreasing complexity, or in clusters of fours or fives. Almost all of these schemes were called "natural," at least by their proponents. Whether the patterns revealed were strict and numerical and discernible mostly *a priori* or messy and unpredictable and detectable only *a posteriori*, their promoters all shared the assumption that a scheme is more likely to be predictive of further organisms to be found the more it resembles the patterns found in nature.

The implicit standpoint that these naturalists took was that the true arrangement or distribution of living things has some kind of regular pattern. It might be complex and difficult to understand, but it is not chaotic or random. I will discuss further implications of this assumption later on, but first, let's take a look at some 18th century perspectives about predictiveness in classification schemes.

Similar plants, similar properties

Arranging plants according to their external similarities also directed attention toward their internal similarities. Long before the "correlation of characters" had ever been described as a principle of scientific investigation, those familiar with plants had recognised that plants with similar external features often shared virtues, or medicinal properties. (These are now known to come from similar secondary compounds, i.e. physiologically active chemicals). Grouping similar plants together, regardless of how their similarities were thought to have come about, could have practical, useful consequences, as proponents of the "natural method" liked to point out. Noticing the similarities among plants was particularly desirable for physicians.[509] This knowledge could help a physician make educated guesses about which "simples" – medicines made from a single plant – could be substituted for others, whereas classification schemes concerned only with plant identification were useless in this respect.[510] Comparing unknown plants with known plants could also yield hints about the properties of new plants discovered during voyages – or could draw attention to local plants resembling exotics with valuable properties. For instance, a ship's doctor armed with knowledge of the properties that particular groups of plants tend to share could guide his crew to eat nutritious plants and avoid poisonous ones in foreign lands.[511] Physicians made

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[509] Kirby and Spence 1822, 4 ed., vol. 1, pp. 46-47, Arnott 1832, 7 ed., vol. 5 p. 30.

[510] Candolle 1813, p. 30. In fact, alphabetical guides for physicians explaining which local or common plants could substitute for rarer or more expensive ones in medicinal preparations were produced long before the "natural method" became an issue. The plants involved were often not related but were, for instance, all astringents or vermifuges. Loren Mackinney wrote that she has seen more than twenty works of this kind. "Although they were most popular during the ninth, tenth, and eleventh centuries, *Antebalumina* continued to be used in the late Middle Ages" (MacKinney 1938, p. 260). Mathias de L'Obel appeared to be working with this tradition when he assembled charts in his *Plantarum seu stirpium historia* listing plants that could be substituted for each other as simples, arranged by plant part or medicinal function (L'Obel 1576, pp. 657, 662, 665-669). It is the earliest tabular presentation of non-numerical data that I have seen so far.

[511] Lindley n. d. (between 1830 and 1845), 2 ed., pp. 58-62.

aware of the purgative power of an imported bindweed were encouraged to examine local bindweeds that could potentially be just as useful.[512]

Keeping similar plants together and paying attention to features they had in common was therefore a strategy to predict economically important properties of newly discovered plants as much as it was an expression of appreciation of natural groups for their own sake.

Reasoning with and about the shape of nature

But for many botanists, predicting what properties new-found plants would have was not enough by itself. Keeping like plants with like would presumably not only yield information about the relationships among them, but could be in turn used to infer something about the shape of nature. After all, it is only a short mental distance from grouping organisms together in order to keep track of them and then seeing a pattern emerge, to speculating about the causes of that pattern. Those causes would be long-sought natural "laws" of living things.[513] While some naturalists thought that there were mere wisps of a pattern and that more organisms would have to be discovered and described before anything could be said about nature's overall shape or plan, others believed that they had enough information to sketch outlines of it. These men had examined living things, seen patterns, and extrapolated those patterns to the rest of the plant kingdom or even all of living nature. Gaps in the pattern predicted what kinds of new organisms would be found. The fit of newfound organisms to the patterns would be a test of whether the patterns were real and the men who made them, botanical prodigies.

In practice, however, a practical demonstration of virtuosity did not seem to be the driving force behind the majority of schemes. Instead, the convenience of arranging plants or animals according to variations in only one of their features led to people with little familiarity with their subject matter publishing a spate of "systems" and "methods" in the

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[512] Hill 1772a, vol. 7, p. 8.

[513] Assuming, as naturalists in the 17th, 18th, and early 19th centuries generally did, that whatever patterns emerged were free from other influences.

18^{th} century and early 19^{th} century. Complaints about the poor quality of these arrangements – usually called "natural" by their promoters and "artificial" by their detractors – were common. But the complaints focussed on different failings of the schemes than a modern reader might be led to expect. Most of them, in fact, were limited to pointing out that related organisms were not kept together, rather than challenging the assumptions that naturalists should know *a priori* which features to privilege in a classification in order to keep them together, or that basing arrangements of organisms on variations in only a few features could successfully demarcate natural groups and show what features new groups might have. Far from being seen as intrinsically doomed to failure, the concept that all of nature could be organized according to a set of simple yet ahistorical rules bore a certain amount of prestige.

Scala naturae

One kind of scheme that was very appealing in the 18^{th} century was a linear arrangement of organisms. Every organism or group of organisms was expected to occupy a definite place in the line. Each one's place would be determined by arranging it so that every one had similarities (affinities) with those on either side of it. Group after group would lead into each other, like links in a chain. Living things could be arranged from simple to complex or complex to simple, even within small group of organisms – the Great Chain of Being did not have to lead to humans at the top if all that a naturalist was looking at was plants.

Michel Adanson, for instance, explained that he had organized plants in as natural a progression as possible, "with the aim of imitating the gradual progression of nature in its operations and in the linkage and sequence of families.... Nevertheless," he wrote,

> despite the care that we have taken to find the relationships among these families, and to bring them together by their degrees of resemblance, there are still many that do not follow [others], and which leave holes among

> them which indicate that we are still missing families that are unknown to us, and which must be created.[514]

Adanson believed his linear arrangement of plant families to be natural enough that gaps between families in his sequence would indicate families yet to be found – not errors on Adanson's part or an inherently messy shape to nature.

Granted, not all linear arrangements were intended to highlight gaps to be filled. For instance, Lamarck wrote that his "sample" of a linear arrangement of plant genera was tentative, artificial, and only meant to give readers an idea of his conception of the results of the natural method.[515] Many other linear arrangements of plants, particularly after the 1780s, were not even meant to represent nature at all, but were the unwanted but unavoidable consequence of presenting textual material in the pages of a book. But even with these caveats, the idea of a linear progression from simple to complex (or a degeneration from complex to simple) had a powerful grip on both the scientific and public imaginations during the 18th century. Even today it is not uncommon to hear organisms described as "higher" and "lower," with the implication that they fit into this kind of scheme – even when the person making the statement intellectually accepts evolutionary phylogenies and has only a vague idea of what the *scala naturae* is.

Returning to predictiveness, not all "natural" classifications intended to be predictive were linear. Highly structured,[516] non-linear classifications were also popular. Such arrangements were more common in zoology than in botany, and persisted longer in zoology than in botany, but there certainly were elements of this kind of reasoning in botanical schemes even into the 19th century. Since most of the quotable quotes about these schemes come from zoologists, however, I will rely on them extensively in this section.

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[514] I.e. created by the botanist, not by God (Adanson 1966, p. clxxxviii). See also a similar statement he makes about arranging plants in a series where gaps will show species to discover or those that have become extinct (Adanson 1966, p. clxv).

[515] Lamarck 1778, vol. 1, pp. cxiii-cxvii.

[516] These classifications have often been called *a priori*, but since they are not entirely *a priori* I prefer to use the terms "highly structured" and "rigid" because the patterns they picked out in nature were highly structured and inflexible.

Quinarianism

In some rigid schemes, organisms were grouped in clusters containing a certain number of members. If the number of members in a given group was less than the ideal number, the gap indicated species yet to be found. If a group contained too many, it was split. The tables used in John Wilkins' *Essay* were one such scheme.[517] Another was Elias Fries' division of the fungi into groups of four.[518] The entomologist William Sharpe MacLeay, founder of Quinarianism, [519] suggested that Fries had assumed that fungi were to be grouped in, as he put it, pairs of groups of two. MacLeay thought that Fries had seen something in the nature of fungi similar to what others had observed empirically in "the construction of vegetables... Botanists," wrote MacLeay,

> have found the typical number of parts of fructification in the acotyledonous plants [i.e. cryptogams] of Jussieu to be two, that in monocotyledonous plants to be three, and that in dicotyledonous plants to be five, or multiples of these numbers. Consequently the existence of a determinate number in the distribution of the plants themselves might have been argued *à priori*.[520]

Relatively constant numbers of floral organs in particular groups of plants do not now automatically imply constant numbers of subordinate taxa per taxon. But from MacLeay's perspective, any numerical or geometric patterns exhibited in or among organisms were inherently meaningful clues to the structure of nature. He assumed that nature's underlying structure would necessarily be regular as well, such that all living things are clustered in groups with the same number of members in each. "Instead of subjecting Nature to arbitrary rules of our own invention, we should humbly receive her laws as she clearly proclaims them,"[521] Macleay wrote. Since it was clear to him that nature's "laws" would be manifested

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[517] Wilkins 1668, pp. Epistle Dedicatory, sig. a.
[518] Lindley 1832, pp. 336-348. Despite its relatively regular structure, Linnaeus's sexual system of botany was not a rigid scheme. Linnaeus laid down no laws about the numbers of genera to be found in each order or the numbers of species to be found in each genus, so the numbers of genera or species in a given order or genus did not say anything about whether further related plants remained to be discovered. He did not name any taxa yet to be found, either.
[519] Quinarianism was very popular among zoologists in the U. K. in the late 1820s and 1830s. Although its influence lingered into the 1850s, its dominance was short-lived.
[520] MacLeay 1823, p. 199.
[521] MacLeay 1823, p. 261.

in the repetition of a particular number, "It only remains for the naturalist to discover from observation what this number is."[522] MacLeay was certain that the special number was undoubtedly five, but other naturalists experimented with schemes based on other numbers, such as the aforementioned Fries (four), the entomologist Rev. William Kirby (seven) and Lorenz Oken, the founder of Naturphilosophie (four).[523] Quinarianism, however, was the most influential of all of these schemes.

In Quinarianism, a natural group always had five members. Proof that the group was natural was that its members could be arranged in a five-membered series that folded back in on itself, with the last member connecting to the first to form a circle.[524] The different members of the circle each had parallelisms (analogies) with one another in addition to the direct linkages (affinities) tying them together. These analogies and affinities could be morphological, behavioural or ecological. Quinarians believed that incorporating ecological and behavioural information into their classifications gave their scheme a stronger grounding in reality than other classification schemes had. They banked on the fact that naturalists could perceive many more kinds of relationships among living things than other classification systems considered. Instead of discarding ecological, smell, taste and other data purposefully omitted from, for example, the sexual system, Quinarianism incorporated every bit of information available. Circular arrangements therefore seemed to account for the complexity of nature through the manifestation of a simple pattern. Many British zoologists in the 1820s and 1830s, in particular, found Quinarianism and similar schemes appealing.

According to Quinarian principles, each member of a circle also connected loosely to a member of another group of five. MacLeay insisted that these groupings of five were real features of the natural world. The regularity of this pattern in fact proved to him that his

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[522] MacLeay 1823, p. 199.
[523] MacLeay 1830, p. 435, MacLeay 1823, p. 199. Some more examples can be found in Stevens 1994, p. 186. Most British zoologists before and after Quinarianism's heyday, and most botanists and continental naturalists even during its peak in popularity, saw such a rigid approach to classification as inadequate to represent the complexities of nature.
[524] MacLeay 1830, p. 206.

ideas were correct and just as real as the parallelisms composing it.[525] "The naturalist," MacLeay wrote, "cannot have a more admirable test of his accuracy, or a stronger rein on his fancy, than this parallelism of analogous groups in a contiguous series of affinity."[526] The resulting interconnecting pattern of pentagons did not just hold the fabric of nature together. In MacLeay's view, it was the fabric of nature. Closing a five-membered circle was therefore rather like completing a clue in a cryptic crossword: knowing the meaning of the clue, the position of the word in the grid, and the word length all contribute to demonstrating when an answer is correct. There is only one way to be correct. Quinarians were quite sure they had figured out what that pattern in nature was.

Analogical reasoning with parallelisms

Both numerical schemes like this and *scala naturae*-type arrangements of organisms allowed relationships among organisms in "parallel" groups to be compared analogically. Parallel arrangements could predict what features as-yet-undiscovered organisms would have: if the counterpart of an organism in one group were unknown in a parallel group, it perhaps existed and had yet to be discovered. Parallels could exist between kingdoms, or between closely related groups. For instance, the animal and vegetable kingdoms, wrote entomologists William Kirby and William Spence in 1828, "are extended in a nearly parallel direction till they reach their extreme limits . . . each showing its kindred with the other by certain resemblances observable between *opposite* points."[527] Lamarck had published diagrams showing these relations between ascending series of animals and plants in the mid-1780s. Such diagrams were popular for decades.[528] Similarly, in the botanist John Lindley's flirtation with Quinarianism, he arranged the five classes of plants he recognized in the form of a circle and expanded on the way their affinities could be shown as parallelisms among

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[525] MacLeay 1823, p. 198.
[526] MacLeay 1823, p. 196.
[527] Kirby and Spence 1828, 5 ed., vol. 4, p. 370.
[528] Lamarck's diagrams are reproduced in Barsanti 1992, pp. 262-263. Another such diagram graces Turpin 1820 – this book is of such bad quality that it likely reflected common beliefs when it was printed more than it influenced them.

the classes in a chart.[529] He did not believe that there were equal numbers of genera or families in those groups, but he hinted that unpaired families might indicate the existence of families with similar features to be found in other classes.[530]

Systems facilitating the discovery of parallelisms in nature were popular not only because of their predictive value. As Paul Lawrence Farber has noted, the "massive literature in natural history" prompted zoologists such as William Swainson to turn to Quinarianism in the belief that a new classification was necessary to replace earlier schemes unable to cope with the huge increases in empirical knowledge. Quinarianism, Farber suggested, was a project initiated when there was too much information for any one person to handle, but no proper institutional infrastructure to compensate for it.[531] It seemed to offer a way not just to look for undiscovered organisms, but to organize those already known to science in meaningful ways according to properties that the system of organization itself could help reveal.

This was particularly true for handling physiological and ecological patterns among organisms, which were generally neglected in other taxonomic schemes.[532] Arranging organisms so as to highlight their parallelisms could also bring previously unnoticed physiological and ecological patterns among them to naturalists' attention. Quinarianism embodied the zenith of such schemes, but others with similar guidelines to analogical comparisons existed more than 150 years earlier. For instance, it was long held that parts of plants and parts of animals of similar composition must have similar functions. Both plants and animals have liquid components, sap and blood, so plants, Nehemiah Grew reasoned in

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[529] Lindley 1835, p. 45.
[530] Lindley 1835, p. 75.
[531] Farber 1985, pp. 55-56.
[532] Farber 1985, p. 55. Farber describes Swainson's attention to animal behaviour as one such example. Lindley, too, was interested in using non-traditional criteria to classify plants. Lindley based his classes on vegetative characters, notably those of vascular tissue, if present. (This way of dividing up the plant kingdom never became popular). In *A key* (1835), he explained that "the axis and leaves, without which plants could not exist, are of the first importance" (Lindley 1835, p. 40). Lindley probably felt bolstered in this opinion by the knowledge that plants can survive – and reproduce – perfectly well through grafts, suckers, fragmentation, and other asexual means. I also have the suspicion that he liked this take on the natural system precisely because it was a sharp break with all tradition in botany privileging floral features – Linnaeus's sexual system in particular, of which Lindley was the most outspoken critic in his time.

1672, must have an active, peristalsis-like way of moving their sap around.[533] Antoine-Laurent de Jussieu believed that there were seven parallel divisions of animals and plants, which "agree in their plan and progression" the same way that "branches grow from the same trunk." The two kingdoms "are simple and constant and certainly co-equal, and so the principles assigned to the one also suit the other, and in both, from the important organs and similarly, the function, comparable divisions both primary and secondary may be drawn."[534]

Along these lines, since animals eat solid foods and excrete solid waste, plants take in liquids, so plant excrement, "if there is any," A.-P. de Candolle wrote in 1813, must be liquid.[535]

John Lindley used this idea of parallels between the plant and animal kingdoms to reason that gymnosperms, with their naked seeds, bear the same kind of relationship to other woody plants as "frogs and similar reptiles," in which fertilization is external, do to other, internally fertilized animals.[536]

What exactly analogies of this nature meant to naturalists is debatable. The theories current at the time considered analogies to be highly significant, but did not explain why they existed. Better naturalists of the early 19th century came to see that rigid schemes meant to help extract meaning from patterns of analogies were products of wishful thinking. Still, nobody had an obvious answer as to why God's plan for nature was so intractable.

Even dabblers in botany took these ideas to heart. Samuel Taylor Coleridge complained in 1818 that botanists and zoologists were not putting enough effort into looking for parallels and contrasts between the plant and animal kingdoms. The result was that, despite the enormous amount of research into plants and animals over hundreds of years, the disciplines were insufficiently methodical to uncover the laws that would allow the subject

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[533] Letter about Nehemiah Grew to John Ray from Henry Oldenburgh, July 6, 1672, cited in Ray 1848, p. 98.
[534] Jussieu 1789, p. xlix, Jussieu 1994c, p. 369.
[535] Candolle 1813, p. 13. N. B. Candolle also cautions that "One must not transport all the principles of zoology into botany, or of botany into zoology, without attentive examination, on the pain of committing grave errors" (Candolle 1813, p. 18); by 1833 he was arguing the numbers of species in each division of the plant and animal kingdoms, respectively, show that the fact that the kingdoms can each be easily divided into 4 groups is a coincidence, and not evidence of parallelisms (Candolle 1833, vol. 54 p. 268).
[536] Lindley 1836, 2 ed., p. xi.

matter to be taken up easily by the mind. "What is BOTANY at this present hour?" he demanded. "Little more than an enormous nomenclature; a huge catalogue, *bien arrangè* [sic], yearly and monthly augmented, in various editions, each with its own scheme of technical memory and its own conveniences of reference!" Clearly, even a highly technical reference scheme was not good enough without philosophical guiding principles.

Plenitude

It is clearer now than it was in the early 19th century why rigid schemes did not work: they have little place for contingency in the existence of living things. Living things are the way they are today because of the vagaries of environmental conditions and other contingent events affecting their ancestors. But evolutionary perspectives of any kind were uncommon in the late 18th and early 19th centuries. Instead, naturalists making rigid schemes were species fixists who held an often implicit belief in plenitude – but plenitude only within certain orderly physical limits.[537] They expected gaps between kinds of living things to be either predictable or non-existent. That is, some gaps would eventually be filled by yet-to-be-discovered living things in line with God's plan, while other gaps would never be filled because they represented combinations of organs that were physically constrained from ever working together to make a functional plant or animal. Plenitude would therefore subsist within tidy classificatory boundaries. This meant in practice that some systematists came up with schemes in which they described and named groups of organisms that had not yet been discovered. These organisms were expected to exhibit combinations of features that the systematists had seen in other organisms. The reasoning seems to have been that, if nature were shown to be capable of producing a theme, it was expected to have explored all possible variations on it.

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[537] Peter Stevens has also noted unusual ideas of plenitude in rigid systems (Stevens 1994, p. 186). It could have been that such beliefs, when coupled with reliance on a rigid picture of how nature is organized, could hamper further investigation by giving researchers the idea that there were no gaps to be filled in a particular cluster of organisms – they could relax and stop looking. But I have not seen any evidence in support of this speculation. Instead, the classifications presented all eventually were contested and/or superseded by new arrangements, made either by the original authors or by others.

The Abbé de Las, for instance, proposed an arrangement of plants in 1783 in which flowering plants would be assigned to classes based on the form of the corolla. There were twelve "simple" kinds of flower, five composite "double" kinds of flower, made from two simple flower types, and five "triple" kinds of flower made from three simple flower types. This yielded 23 classes in all, including a class of anomalous plants.[538] Sections within each class would be based on the number of petals in the flower: 1, 2, 3, up to 12 or more, or none. Each "simple" class would therefore contain 13 sections. The double classes would contain nine sections each, because certain flower types cannot be combined in the double classes, and the triple class would contain only eight sections "because it cannot be combined with radiate flowers." De Las calculated that his arrangement would comprise 91 (= 7 x 13) sections of simple flowers, 45 (= 5 x 9) sections of double flowers, 40 (= 5 x 8) sections of triple flowers, and three sections of anomalous flowers, for a total of 179 sections of plants. No plants exhibiting some of these combinations of features had been described, but de Las explained,

> because plants that are lipped, layered and crested all at once have been brought to my attention, I conclude that all the other combinations could exist. All the same, even though botanists have not yet discovered a multi-petaled labiate plant, I do not feel exempt from giving this class as many divisions as the other ones. Nature is covered with an immense veil of which we are at pains to lift a corner. And who will assure us that what is still hidden does not differ at all from what we see?[539]

It did not seem to occur to de Las that it would be impossible to predict what kinds of organisms exist based on combinations of the features of organisms that he already knew about. Plenitude in this case was strictly circumscribed by variations on what was already known.

Another wonderful example of bounded plenitude as a guiding principle in a classification is Bernard Germain Étienne de la Ville, Comte de Lacépède's (1756-1825) 11-volume opus on fish, *Histoire naturelle des poissons* (an VI-an XI de la République: 1798-

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[538] de Las 1783, p. 22.
[539] de Las 1783, p. 33.

1803). In this work, Lacépède defined his subject matter as animals with red blood, no lungs, and fins.[540] Animals that did not have these characteristics might exist, but were not fish as far as he was concerned (finless ones excepted), so his method does not deal with them.

Lacépède divided fish in two classes, bony and cartilaginous, each of which contained four subclasses. Each subclass was made up of four divisions, for a total of 32 orders. Fifteen of these orders had never been seen in the flesh by the time the last volume of his scheme was published.[541] "You can see that some orders do not have a single described genus in them yet," he wrote,

> But I believe that I had to give the general plan all the regularity and scope that it could possibly have, and that nature seemed to me to demand. Anyway, I did not want my method to have to be revised as an even greater number of fish are discovered. I wanted it to be able to serve as [a place] to write down all the species that will be observed in the future, and I was all the more confirmed in this idea when, after I started to use the table that I am publishing, many recently discovered genera came, so to speak, to fill in several gaps.[542]

But Lacépède never was able to fill all the gaps. Despite his expertise at describing individual fish, he was ridiculed for his system even by the likes of John Fleming (1785-1857), himself notorious for promoting a strictly dichotomous classification of living things (see Chapter 6). "Zoologists," Fleming wrote in his *Philosophy of Zoology* (1822), "have not yet ventured to form species without individuals, or genera where there are no species; but in a valuable work on fishes, fifteen *orders* may be observed, instituted and named which have no representatives in nature."[543]

The Abbé de Las's amateurish efforts aside,[544] most botanists in the 18th century were confronted with far greater numbers of subjects than zoologists had to face, and had figured out much earlier that the shape of nature was not regular or predictable. It became quite clear that attempting to build predictive value into classifications of organisms by using

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540 Lacépède 1798, vol. 1, p. xxxii.
541 Lacépède 1803, vol. 11, pp. 192-193, Lacépède 1798, vol. 1.
542 Lacépède 1798, vol. 1, pp. ccviii-ccx.
543 Fleming 1822, pp. 146-147.
544 I discuss other awkward aspects of his proposal in Chapter 6.

schemes with regular structures was not very effective. The discovery of anomalous organisms meant that rigid schemes, relying as they did on hierarchies of characters, were often foiled. The features used to define higher level groups, for instance, the presence of fins in Lacépède's scheme for fish, or constant numbers of sexual parts in flowers in Linnaeus's sexual system of plants, were not present in every member of lower-level groups with them. For instance, flowering plants in the genus *Valeriana* have a variable number of stamens, as mentioned above. Even in supposedly natural schemes, apparently clear-cut divisions were not always so. For example, it was unclear whether the water lily *Nymphaea* was a monocot or a dicot.[545] *Scala naturae*-type schemes, with their rigid structures, were even less equipped to accommodate anomalous organisms. They were largely surpassed in popularity by nonlinear, flexible schemes by the end of the 18th century.[546] Even though a number of botanists produced *scalae naturae* well into the middle of the 19th century, what seemed plausible for small numbers of organisms often failed to scale up without falling apart.[547] The analogies that held organisms in place did not always occur in ways that suited the schemes. Rigid schemes also turned out to be far less useful in practice than in principle at predicting what organisms were yet to be discovered.

The realization that rigid schemes did not live up to their promise of capturing patterns in nature and making natural history more predictable did not, however, dampen naturalists' desire to make their classification schemes resemble what they understood to be the natural order as closely as possible. For this purpose, they sought to identify taxonomically important features that would help identify natural groups. Even this supposedly empirical endeavour was fraught with the tension between theory and practice.

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[545] Ray 1848, p. 4, Smith 1821, p. 85. The difference between monocots and dicots was recognized and considered fundamental by many 18th and 19th century botanists, including Jussieu, who based his natural method on cotyledon number. But some plants, including *Nymphaea,* exhibit features intermediate between monocots and dicots. Linnaeus, for instance, found it confusing: he left it out of his natural families in 1764 (Linnaeus 1764, 6 ed.) and put it in the "miscellaneous" natural order in 1767 (Linnaeus 1767, 9 ed.). Monocots are now recognized to be a monophyletic group within the polyphyletic dicots. As for Nymphaea, it is now considered to be a basal angiosperm, neither a monocot nor a eudicot (Wing and Boucher 1998, pp. 393, 394, 397).

[546] Barsanti 1988, Barsanti 1992.

[547] For examples of such schemes, see Stevens 1994, pp. 185-193.

Characters good for demarcation must have functional importance

A major cause of this strain came from late 18^{th}-century and early 19^{th}-century beliefs about form and function, or organisms' physiology and identity. It was important for naturalists to show that the characters they used in drawing up classifications were connected to deep truths in nature. The way they expressed this idea was through the belief that characters demarcating taxonomic groups must necessarily have high functional importance. Even though most taxonomic classifications were based on plants' most constant morphological features, functional importance was used to justify the choice of those features after the fact. Botanists often described particular parts of plants as "essential" for both these reasons, although they usually put emphasis on the functionality.[548]

By the late 18^{th} century, physiological research was yielding many of the most exciting theoretical developments in the study of living things. For instance, tentative steps toward understanding photosynthesis were taking place.[549] It is therefore no surprise that classification schemes of that period usually incorporated, or claimed to incorporate, ideas of the relative importance of particular organs in plant or animal functioning. The longstanding assumption that features of functional importance should also necessarily be useful in the arrangement of organisms was rarely challenged. For example, Linnaeus justified forming groups of plants according to variations in their floral parts on the basis of the then-new theory of plant sexuality. Since floral parts are the sexual organs of the plant, and reproduction is the most essential function of the plant, plant classifications should be based on floral parts, he reasoned.[550]

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[548] Tournefort was an exception in that he was explicit that constancy mattered more than functionality (Atran 1990, pp. 167-168, i.e. Tournefort 1694, p. 26).
[549] Morton 1981, pp. 334, 335, 336.
[550] Müller-Wille 1995, p. 45, Müller-Wille 2005, pp. 90-91. While arrangements of plants based on floral organs had been popular even before the discovery of the sexuality of plants, and continued to remain the case in botany for a long time, talking about the sexual organs of animals (though clearly they are as "essential" to the reproduction of animals as those of plants are) was taboo, even according to Linnaeus. "The great

Antoine-Laurent de Jussieu also believed that an understanding of plant reproductive functioning was a necessary foundation for delimiting plant groups. He explained that the cotyledons are the most essential part of the plant body and therefore most suitable for dividing up the plant kingdom into its highest taxa because they are also the most essential part of the seed. Seeds, after all, carry out reproduction, and "The procreation of beings is the highest design of Nature, and her primary function." He took the fact that all or most sexual organs are shed after fertilization or maturation of the seed as proof of his point.[551]

While Jussieu recognized that forming exception-free groups of plants was impossible if plants were grouped according to "parts that are the most obvious and easy to observe," as was the "received prejudice." Nevertheless, he still believed that arranging plants according to their features of fructification would yield such results because the parts of fructification were "the most essential and invariable" features (after the cotyledons). He considered flower parts to be the most essential because "they cooperate in forming a new individual, which begins a new life in germination and which is not accounted perfect until it has produced another individual like itself." Arranging plants according to these essential features, he believed, would yield natural groups in which taxa that had a few anomalous features would not be split from their relatives. Although discovering the manifestation of those essential features in many groups of plants might be difficult, it was not impossible. A

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command, 'increase and multiply,' will direct us to those parts which constitute the essence of an insect; but these, if it were possible, it would be improper to use for characters," wrote the Rev. William Kirby in his *Monographium apum angliae* (1802), citing Linnaeus's *De Mammalibus* on this point. In the animal kingdom, the organs of nutrition – for no animal can survive without feeding itself – therefore became the organs of choice upon which to base arrangements. Kirby added, "Since then the *Instrumenta cibaria* [mouthparts] do not constitute the essence of an insect, it is consequently a matter of indifference whether the generic characters be taken from them, or other parts that are more obvious. In these small animals, I call that a generic character which is constant through a genus from whatever part it be taken" (Kirby 1802, vol. 1, pp. 38-40, including citation of Linnaeus "de Mammalibus" without page or section number). Although Kirby wrote that he chose the mouth parts to divide up the bees because those features were constant within genera but different among them, he also justified his choice according to a convoluted symbolism that he worked out based on his understanding of Hebrew as the closest language to the Adamic ideal. As he explained, "The Hebrew name of the bee, דבורה [dvorah], derived from דבר [daber], to speak, seems to direct us to the *tongue* for its *Essential Character*" (Kirby 1802, vol. 1, pp. 119, note) – divine confirmation that Kirby had chosen to base his arrangement on the right features. (In Hebrew, most vowels are not written and the consonants "B" and "V" look alike (ב)). This is a prime example of how a naturalist felt that taxonomically useful features had to be connected to deeper truths in nature in order to be justifiable to his peers. (For Kirby, these deeper truths also involved the truth of Scripture).

551 Jussieu 1789, p. xliv, Jussieu 1994c, p. 364.

botanist might have to try hard at "combining with essential, but sometimes hardly apparent, characters, accessory characters that indicate the existence of the former and are constant and always visible."[552] Jussieu therefore recognized that plant features "essential" to their reproduction were not always the most obvious for use in circumscribing sufficiently natural groups. In describing the natural method, however, he insisted on the importance of essential features, as he conceived them, by invoking "accessory characters." These accessory characters effectively reinstate the invariability criterion that got buried under Jussieu's justificatory rhetoric of functional importance. As Stevens noted, however, Jussieu's classifications in practice relied more heavily on the relative invariability of certain plant features than on any searching for essences, especially in an Aristotelian sense.[553]

Other French botanists shared Jussieu's belief that features of functional importance would be of the greatest use in delimiting natural plant groups. André-Marie Constant Duméril even wrote that organs of fructification are considered in almost all of the numerous systems of botany, since "that is really the only function that we understand well in plants, and in general it has produced very natural arrangements." This implies that natural groupings *must* be founded on features of organisms for which the functions are understood (though how naturalness was to be judged was a moot question).[554] Similarly, Augustin-Pyramus de Candolle asserted that an awareness of the relative importance of plant organ functions is necessary in order to place them in natural groups, and that "simple reasoning" *a priori* from those functions can yield the relative importance of given plant organs for classificatory purposes.[555] Lamarck had also devised a technique of numerically weighting the relative importance of floral organs *a priori* in order to determine degrees of relatedness among plants, but he did not take further steps toward arranging plants according to this

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[552] Jussieu 1994b, pp. 179-180, 192-193, Stevens 1994, pp. 299, 308-309. Stevens (Stevens 1994, pp. 66-67) describes in more depth how the influential idea of subordination of characters, used as a guiding principle in Jussieu's botanical classifications and Cuvier's zoological classifications in the 1790s, is partially based on such assumptions.
[553] Stevens 1994, pp. 33, 41.
[554].Duméril 1804, p. 57. Duméril also said the same thing in different words two years later (Duméril 1806, p. xi).
[555] Candolle 1813, pp. 78, 86.

scheme.[556] Despite flirting with *a priori* ways of determining the degrees of relatedness among plants, however, both Lamarck and Candolle recognized that doing so could be, at best, a guide to delimiting natural groups.[557] Lamarck, in fact, conceived of it as more of a thought experiment to guide research, since he regarded the results as approximate and conjectural.[558] Delimiting "natural" taxa remained an empirical endeavour that required a lot of time, patience, and attention to detail.

A similar situation to that in late 18th- and early 19th century France played out in the works of John Lindley in the mid-1830s. At that time, Lindley proclaimed that one of the principles of modern botany was that "the characters by which natural affinities are ascertained, are valuable in proportion to their importance to the existence of a plant."[559] Lindley was likely influenced by the success of Cuvier's zoological programme of subordination of characters according to functionality in the lives of the organisms[560] (in turn inspired by Jussieu's earlier program) and also Quinarian ideals linking physiology and classification. Nevertheless, his functionalist approach to choosing characters upon which to base his classifications smacks of philosophical justification after the fact. The same year as he put this statement forward, he also wrote that proportional functional importance has little to do with taxonomic utility. Instead, "In all departments of natural history," he wrote,

> our systems are of necessity founded upon those parts which present the greatest variety between species and species, and the greatest constancy between individual and individual. Now it matters not what function you

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556 Lamarck 1778, vol. 1, pp. xciv-cvi.
557 Peter Stevens (Stevens 1994, p. 88) cites Candolle 1827, pp. 1:vi-viii, 2:242-244 and Candolle 1832, p. xiii to this end.
558 Lamarck 1778, vol. 1, p. xcii.
559 Lindley 1835, p. 40.
560 Cuvier is often discussed in 20th- and 21st- century history of biology works as a great example of someone whose research program broke with the Great Chain of Being/*scala naturae* by dividing the animal kingdom (animalcules excepted) into four parallel *embranchements* of mollusca, vertebrata, annulosa (roughly analogous to arthorpods) and radiata (all animals with radial symmetry). The *embranchements* were separated from each other according to body plan; each *embranchement* featured a different arrangement of body parts integrated into a functional whole. Intermediaries between *embranchements* could not exist, since they were expected not to be able to perform all of the necessary life functions to survive. Cuvier's excellence as a comparative anatomist inpired many others to take animal anatomy seriously and helped lift zoology to the forefront of natural history during his lifetime. Peter Stevens (Stevens 1994, pp. 66-67) explained how Cuvier drew inspiration from Jussieu's idea of subordination of characters to develop his functionality-based research program. But Cuvier, like Jussieu, could provide only speculation as to why the natural groups he described were, of all possible groups, the ones that existed.

> take as the basis of a natural classification, for one will lead as well as another to a natural result, provided we thoroughly understand it.... The study of natural affinities is nothing but the observation of the constancy, more or less great, of certain combinations of organs; and, following out that principle, the naturalist places side by side all those beings which possess the greatest number of identical or similar organs, and he separates those which have a smaller number in common; whence it results, that ... a natural arrangement is ... the more perfect, the greater the number of ideas connected with the characters of the classes.[561]

Although examining the functions of plant parts should illustrate how plants are related to one another, keeping like with like on the basis of whatever features exhibit the appropriate level of variation among groups was and should be the naturalist's first priority in working on the natural system. The natural system was real, but figuring it out was a strictly *a posteriori* affair.

[561] Lindley 1999, p. 103.

Chapter 5. "Natural," "artificial," and textbook features

The last few decades of the 18th century in botany were characterized by a struggle to determine the best ways to communicate about plants. Building on the discoveries of their predecessors, botanists of this period were equipped to figure out which techniques worked best for which purposes. But constrained both by technical limitations and by the prejudices of their predecessors, they had many obstacles to overcome before they succeeded. One of the results of the struggle to determine the best ways to proceed in the face of so many plants to organize was that the natural method became the rallying point for the incipient discipline. The roles and value of other, more artificial technologies, though crucial for coping with the amount of information, were played down. Botanists also showed that their science could hold its own against the increasing advances in zoology, especially comparative anatomy, by placing greater emphasis on what would now be called the biological aspects of plants and less on their organization. Botanists working on the chemical and physiological aspects of plants considered themselves to be contributing to the study of the natural method, since it had long been recognized that the more closely related plants are, the more such traits they would be likely to share.

The changing implications of "natural" and "artificial" during this period, as well as the differentiation of distinct types of botanical texts and text navigation aids within them, illustrate the coalescence of standardized information management strategies within the nascent discipline of botany. Eventually, botanists came to take these tools for granted. This is a major reason why the history of information management in botany has so far been neglected by modern historians of science as well.

Conflicting goals influence what is meant by "natural" and "artificial"

Despite the standardization that Linnaeus brought to the description of plants and their nomenclature, botanists continued to talk past each other when discussing the faults and merits of each other's work. In particular, "natural" and "artificial" were both highly loaded terms in the rhetoric of 18th- and 19th-century natural history. Few botanists – or zoologists, for that matter – recognised that different techniques of arranging organisms were best suited to different purposes. Rather than focussing on how well a given scheme accomplished what it purported to do – making plant identification easy – botanists often accused their rivals of producing schemes that were not suitably "philosophical" or reflective of nature.[562]

John Hill was apparently the first[563] to explain in detail that

> Systems of Plants seem to be necessary for two distinct purposes; the one to assist the memory [and] the other to range vegetables, in such a manner, that every Tribe, Division, and Family may seem to be allied to those that precede, as well as follow it. In this last distribution, no arbitrary character of Tribes, &c. is upon any account to separate Plants naturally connected; this System is therefore more adapted to a natural history, than to facilitate the discovery of an unknown Plant.
>
> We have seen, in running over the various methods [proposed by previous authors], that they have been reputed good or bad, according as they approach Nature; hence the disputes between writers on Botany. Whereas, in truth, though some approach nearer the mark than others, no system extant hitherto at all deserves the appellation of natural.
>
> All have had in view the two very different purposes mentioned above, which appear incompatible with one another: they must therefore be separated, and Nature severely followed in the latter, though attended with a thousand difficulties to the learner: not but that ways will be found to obviate many of them.

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562 McMahon 2002, p. 11.

563 Linnaeus had written that "Natural Orders instruct us in the nature of plants; artificial ones teach us to know one plant from another," but he did not clearly explain the circumstances under which each would be best used . Sir James Edward Smith was fond of citing this "Natural Orders" passage to show that Linnaeus was aware of the importance of the natural method before Jussieu had published on it. For instance, Smith put it on the title page of his *Grammar of Botany* (1821) and discussed it to this end in the introduction. He both cited and alluded to it for this purpose in "A Review of the modern state of Botany" as well. Steadfast Linnaean William Roscoe also quoted this passage, but only as part of a polemic arguing that since the Jussieus' method was not completely natural, one should use the sexual system because it serves both to name plants and to give some ideas about their relationships. This is most likely the opposite of what Linnaeus had meant to imply (Linnaeus 1764, 6 ed., p. [602] aphorism 10, Roscoe 1830, pp. 16, 183, Smith 1821, pp. title page, x, Smith 1832, vol. 2 pp. 442, 590).

Hill explained that, since we do not yet know enough about nature to form a truly natural system, "an artificial one must be formed, merely to assist the memory, and make us certain of the Plants we examine for reason tells us, we must know a thing by sight, before we can pretend to assign it a proper place." This artificial system was to be called "a botanical index or dictionary." It would contain natural genera but artificial higher ranks, delimited strictly on the basis of features shared by all plants within them. Hill would accommodate anomalous plants by using double entries. Genera would appear in more than one class or order if necessary, so that each higher taxon would contain only species that matched its definition. "Thus," Hill wrote, "we shall have a universal dictionary to the vegetable kingdom, equally useful to the young beginner, and the real Botanist."[564] That is, Hill conceived of orders and classes in his "universal dictionary to the plant kingdom" not as ranks within a nested set hierarchy meant to represent how plants were related to one another, but as branch points in an identification key. This arrangement of plants was designed to identify plants quickly and easily. It would have the same advantages of natural genera that the sexual system had, but would eliminate all ambiguity that Linnaeus had introduced to his system when he maintained natural genera at the expense of either modifying his class and order definitions or including more cross-references.

Jean-Baptiste de Monet de Lamarck,[565] Augustin-Pyramus de Candolle and John Lindley were rare examples of subsequent botanists who picked up on the utility of separating techniques for describing plants' relationships and for identifying them, as opposed to expecting plant arrangements to do both and then complaining that they are not natural enough or sufficiently easy to use to identify plants. As A.-P. de Candolle wrote, methods with a given purpose were subject to "unfair criticisms" from people who "judged by the laws of another kind of method" far too often, "even in the most serious discussions. Even when botanists today appear to offer really large differences [of opinion] on the

[564] Hill 1771, vol. 2, pp. 42-43.
[565] Lamarck 1778, vol. 1, pp. lii, liii, especially lviii.

comparative merits of the different methods, it is because, in reality, they are talking about different things under similar names."[566] He explained that "All these different [classification] methods have their kind of utility and of perfection relative to the goal that guided their author; there are no bad ones except for those written without a well-determined goal."[567]

Most botanists, however, did not have as clear an idea of the conflicts inherent in trying to get one arrangement of plants to work as both a way of describing how plants are related to one another an to identify them. The ways in which they used the terms "natural" and "artificial" to describe plant classifications illustrate many of the assumptions that they made.

"Natural method" vs. "Artificial system"

There has been some confusion in the secondary literature about the use of the terms "natural" and "artificial" and "method" and "system." The "natural method" (the sensible, "bottom-up" precursor of phylogenetic classifications) is often contrasted with "artificial systems" (the *a priori*, "top-down" arrangements made obsolete by the natural one).[568] Ernst Mayr was the major disseminator of this idea, and it has had a hold on philosophers of biology in particular for several decades.[569] Recently, however, it has been subject to increasing criticism, for instance, Michael Ghiselin wrote that "The very idea that systematists must either work "upward" or "downward" is a preposterous travesty of what they actually do, which is repeatedly re-evaluate the classification in terms of new evidence."[570]

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566 Candolle 1813, p. 27.
567 Candolle 1813, p. 31.
568 E. g. Bicheno 1827, pp. 489, 491.
569 Mayr 1982 and, e.g., Hull 1965, Ereshefsky 2001, Lefèvre 2001, vol. 220 .
570 Ghiselin 1997, p. 179. I have yet to see a 20th- or 21st- century commentator write anything about what Colin Milne explained in 1770:

it is clear how natural things are to be investigated, and how communicated. The discoverers of natural things have evidently proceeded from the consideration of individuals to that of species and genera, and so on to the

Participants in the debates and discussions about what these terms meant have all too often turned to etymology rather than history for support.[571] Father Ong, however, has shown that the terms "method" and "system" are roughly the same age in the western world and have always had roughly the same meaning.[572] Both of them were associated with the order in which something is arranged in order to *make it naturally easier to memorize* for several hundred years before the debate exploded in the 18th century.

"Artificial" and "system" are also frequently associated by modern historians of science with Linnaeus's sexual system of botany to the exclusion of other plant arrangements. It is true that in early 19th century France, "artificial" arrangements were often called "systems" and "natural" ones "methods," regardless of whether the words "artificial" or "natural" were used – possibly following the lead of Antoine-Laurent de Jussieu, who was fairly consistent in his use of these terms in the introduction to his *Genera plantarum* (1789).[573] Lamarck and Mirbel, for example, explained that while systems are founded upon variations in a single part of a plant, methods use all available features to achieve their goals.[574] But early in the 18th century, there was little distinction between the way the terms "method" and "system" were employed. Boerhaave, for instance, used the terms interchangeably in referring to Ray's and Tournefort's work, and Linnaeus did likewise, writing, for instance, "I believe there is hardly anyone born with such a memory, that he could retain the genera without a System. The Method must therefore lead the way."[575]

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higher arbitrary divisions: and a like method do all adopt, who would improve natural knowledge by further discoveries.

But when invented and proposed to be communicated to others, we take a different route, and beginning with the higher arbitrary divisions, as classes and orders, descend to genera, species, and varieties, in a direction rerograde to what we followed in invention. This method is found most adapted to the capacity of learners. (Milne 1770, p. METHODUS).

[571] E.g. Mayr and Bock 2002, Kirby and Spence 1828, 5 ed., vol. 4, pp. 364-365 and Coleridge 1818, pp. 2-3.

[572] Ong 1958, p. 299.

[573] Jussieu 1789, Stevens 1994, pp. 319-383.

[574] Lamarck and Mirbel 1803, vol. 2, pp. 208-210. See also Dubois 1803, p. 126, Mouton-Fontenille de la Clotte 1803, pp. 26-27, Duméril 1804, p. 3and Virey 1819, vol. XXXII [SPH-TAZ] for similar explanations.

[575] Boerhaave 1719, pp. 130-131, Linnaeus 2004 (not published), pp. 3-4. Stevens 1994, p. 12 includes more instances of Linnaeus using the terms interchangeably; see also Linnaeus 1764, 6 ed., p. [602] for another 2-sentence example in which he mentions both natural and artificial methods – but cf Kirby and Spence quotation below.

France

In France, several authors in both the late 18th century and early 19th century also used "method" and "system" interchangeably. Jean Emmanuel Gilibert even referred to "la Méthode de Linné" ("Linnaeus's method,") in his *Démonstrations de botanique* (1797), though he calls it a system elsewhere in the same work.[576] Some botanists referred to a "méthode artificielle" (artificial method),[577] while a number of others used "method" to refer to a plant arrangement technique in general, without discussing how "natural" or "artificial" it is.[578] Mouton-Fontenille used "système" the same way throughout his work, and so did Bergeret and Lefébure,[579] though Lefébure also referred to the "systême naturel"[580] and generally usually used "méthode" in the context of his own scheme, which he considered to be relatively "natural."[581] Several authors also used the term "méthode analytique" to mean an efficient plant identification device that makes no attempt to reflect plants' relationships, i.e. an artificial contrivance;[582] Duméril called such a device a "systême," though he also used the word to refer disparagingly to arrangements based on modifications of only one organ.[583]

Britain

Anglophone use of the terms was not consistent, either, particularly in the early 19th century,[584] although "system" started to become associated exclusively with Linnaeus's sexual system by the 1820s. That is, in 1770 John Berkenhout referred to both "natural" and "artificial" systems, "system" being any kind of deliberate arrangement. Colin Milne wrote

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576 Gilibert and de la Tourette 1797, 3 ed., pp. vii, x.

577 Paulet 1790, vol. 1, p. xx, Jussieu 1789, p. 12, Gilibert and de la Tourette 1797, 3 ed., pp. 13, 17, Mouton-Fontenille de la Clotte 1803, p. 128.

578 Lestiboudois 1781, pp. xxxii, xliii, Tournefort and Jolyclerc 1797, pp. viii, xiv, Lamarck 1873-1808, vol. 3 pp. ii, xii, xviii, xix, Mouton-Fontenille de la Clotte 1803, p. 328, Lefébure 1817, p. 215.

579 Mouton-Fontenille de la Clotte 1803, Bergeret 1783-1784, vol. I-III, Lefébure 1817, pp. 81, 169, 172, 204, etc.

580 Lefébure 1817, p. 221.

581 Lefébure 1817, pp. 185, 187, 200, 221, etc.

582 Lamarck 1778, vol. 1, V[an] D[er] S[tegen] de P[utte] 1792, p. iii, Lamarck and Candolle 1805, 3 ed., vol. 1.

583 Duméril 1806, pp. ix, xv.

584 Stevens 1994, pp. 12-13.

in 1771 that "The terms System and Method are frequently used, without any precise idea being affixed to either," before going on to define systems as based on variable principles and methods on invariable ones.[585] In 1791 James Edward Smith used both "system" and "method" to mean any kind of arrangement of plants, and in 1810, William Roscoe referred both to "natural" and "artificial" systems and methods.[586] In the 1820s and 1830s, Sir James Edward Smith and W. J. Hooker talked of both natural and artificial systems[587] and natural and artificial methods,[588] but in Hooker's *Flora Scotica; or a description of Scottish plants, arranged both according to the artificial and natural methods* (1821) he used "system" exclusively in the phrase "Linnean system."[589] S. F. Gray used "method" to mean any kind of botanical arrangement[590] and also referred to the "Linnaean method" and "sexual method"[591] and the "natural system,"[592] though usually when he used "system" it was with reference to the sexual system.[593] In 1829 John Lindley used "method" only with reference to the natural method and "system" with reference to the sexual system,[594] but several years later he published a popular textbook called *A natural system of botany* (1836 2nd ed) in which he favoured the use of "system" with an adjective to describe all sorts of botanical arrangements. Kirby and Spence, entomologists of an etymological bent, summed up the early 19th-century state of confusion when they wrote that "method" and "system" "have often been confounded and used indifferently to signify the same thing." They suggested that naturalists should follow Greek etymology and "call every arrangement whose object is confessedly artificial, a *Method*; and that which aims at the plan of nature, a *System*."[595]

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585 Milne 1771, pp. 25-26.
586 Berkenhout 1770, vol. 2, pp. iii, vi, Smith 1791, pp. 14, 16, 29, 36, 38, 53, Roscoe 1830, pp. 16, 17, 98, 183.
587 Smith 1824-1828, p. xxv, Smith and Sowerby 1832-1846, 2 ed., p. iii, Smith 1832, vol. 2 p. 463, Hooker 1830, pp. viii-ix.
588 Hooker 1830, pp. viii-ix, Smith 1832, vol. 2 p. 442.
589 Hooker 1821, pp. ix-x, 292.
590 Gray 1821, pp. v, viii, 24, 27.
591 Gray 1821, pp. viii-ix.
592 Gray 1821, p. 29.
593 Gray 1821, pp. vii, viii, 22.
594 Lindley 1829b, pp. vii, x, xi.
595 Kirby and Spence 1828, 5 ed., vol. 4, pp. 364-365.

These suggestions did not catch on, and ambiguity reigned. Owing to this state of affairs, I will continue my discussion of "artificial" and "natural" arrangements without ascribing any particular implications to the words "system" or "method."

What is natural? What is artificial?

"Nothing is as simple as the arrangement of nature, and nothing, perhaps, is more difficult than to recognize and to conform to it."[596]

In the battle of words that was 18th- and 19th-century botany, "artificial" was often portrayed as nothing more than "not natural," and therefore a poor substitute for "natural." But "natural" meant many different things, depending on the context – sometimes contradictory things. The naturalness of any given arrangement of plants could depend on how many features of the plants were considered in grouping them, how many plants the scheme could accommodate, especially ones about which little information was known, how close the arrangement approached the ideal of perfectly representing all relationships among plants, how much background information about plants was required to use it, how much information about the plants was embodied in the construction of the arrangement, or just how much the arrangement resembled any one writer's conception of "naturalness" at any given time. I have split up these contrasting uses of "natural" in order to discuss them more clearly, but most authors invoked more than one of them at once. I also do not pretend to have found all of these senses of "natural" or "artificial." I hope that this discussion will clarify some of the confusion that pervades the use of these terms in the 18th and 19th centuries.

Keeping like with like

The belief that "natural" meant uniform in keeping like with like, whereas artificial meant mixing unlike with like or separates like from like[597] was very strong, but often not

[596] Grandjean de Fouchy 1763, p. 58.

stated openly. Instead, it was manifested in the form of criticisms directed at rivals' arrangements for not being natural enough, as evidenced by splitting apart groups of plants that belonged together, or mixing unlike plants together.

Number of parts considered

Classifications that took large numbers of plant features into consideration were believed to be natural, whereas classifications based on differences in a single feature, or on only a few features, were seen to be artificial.[598] Because artificial classifications were based on a small number of plant parts, the different combinations of these parts were seen to be simple to memorize.[599]

Since artificial classifications were formed using differences in only a few features, Michel Adanson reckoned that multiple artificial schemes used together could work like a natural scheme. He compared groups of plants formed under 65 artificial systems, each based on variations in a different plant part. He considered the groups of plants that reappeared in different schemes as natural.[600]

Comprehensiveness

While "the" natural method was supposed to include all plants, artificial ones could and did exclude plants, whether by design or because the features upon which the classification was based were not visible or known to science. As Jussieu explained,

> An unknown plant cannot be arranged in a[n artificial] system unless it exhibits a character of a class, and most importantly, unless it has a complete flower. In the natural method, on the contrary, many signs are admitted that are often auxiliary to the primary ones, so while some of these are evanescent, others, persisting and making up the habit of the plant, are sufficient for the determination of its order and genus.[601]

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[597] Jussieu 1994a, p. 215, Stevens 1994, pp. 274-275, Jussieu 1789, p. lvi.
[598] E.g. Jussieu 1994a, p. 215, Stevens 1994, pp. 274-275, Jussieu 1789, p. lvi.
[599] Jussieu 1789, p. 15.
[600] Winsor 2004, , Larson 1994, pp. 35, 41 provides other such references.
[601] Jussieu 1789, p. lvii, Stevens 1994, p. 377.

Adanson had earlier expressed the same ideas, adding that the natural method alone would have no problem accommodating plants from the tropics that bear no resemblance to temperate or arctic families.[602]

In other words, classification schemes that do not make room for known groups of plants or that are too inflexible to accommodate new plants that could have unforeseen features are artificial.[603]

Singular and universal perfection

The natural arrangement was seen as perfect. Artificial arrangements were only humans' imperfect attempts to organize living things. Adanson, for instance, forcefully declared in his *Familles des plantes* (1763) that

> The natural method must be unique, universal or general, that is, it must not suffer a single exception, and it must be independent of our will, but rather based upon the nature of beings, which consists in the totality of their parts and their qualities. It is not doubtful that there cannot be a natural method in botany except that which considers the totality of all the parts of plants.[604]

Other botanists concurred that a truly natural system is free from exceptions, reflects the order of nature perfectly, and is independent of human will or theorising.[605]

A number of botanists said that all arrangements of plants devised by humans are artificial, since there is only one natural arrangement, and it cannot be known until all plants are discovered. For instance, Linnaeus, according to Sir J. E. Smith, "contended that human science was not yet competent to give definitions, or technical characters, of Natural Classifications,"[606] while A.-L. de Jussieu wrote that "The natural method, the truest and best aim of science, has been and still is hidden in part from Botanists on account of a lack of knowledge of all the plants to be arranged, and of the principles of arrangement."[607] And

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[602] Adanson 1966, pp. cxcii-cxciii.
[603] Smith 1832, vol. 2 p. 493.
[604] Adanson 1966, p. clv.
[605] E.g. J. C. P. Erxleben, foreword to *Anfangsgründe der Naturlehre*, 2nd ed., Gottingen: Dieterich, 1777, cited in Larson 1994, pp. 33-34, Roscoe 1830, p. 183.
[606] Smith 1821, p. 31.
[607] Jussieu 1789, p. xxxvi, Jussieu 1994c, p. 56.

every true naturalist, John Fleming wrote, knew that "The establishment of a *System of Nature*, to which all his labours are directed, can only be completed when all the creatures of the globe shall have become known."[608]

Some botanists, wary of sounding overconfident, made sure to indicate that their attempts to describe the natural system were just sketches. Antoine-Laurent de Jussieu also wrote that "the most perfect method is that which is closest to the natural order, or, rather, it will be the natural order itself, of which all artificial methods are nothing but very imperfect imitations."[609] He, too believed that the one true natural method would be perfect and stable, whereas humans' attempts would always be provisional, but he regarded that as an incentive to investigate nature in greater depth rather than as an obstacle to it.[610] Many other botanists agreed, for instance, Lefébure wrote,

> These ongoing discussions about the difference established between an artificial method and a natural method are pretty pointless, since there is only one natural one, and it will not be people who invent it. As to the others, they are necessarily all artificial, but more or less close to the universal plan, which encompasses the entire vegetable kingdom at once.[611]

Lefébure continued that, no matter how unnatural, all methods can bring to light valuable information about plants' relationships, and that this information should be recorded for future use. Others, however, believed differently. Duméril noted that natural classifications cannot be constructed for groups that are poorly known,[612] or until all organisms have been completely characterised.[613] William Roscoe went a step further in suggesting that the one natural method is unknowable, and we should not even pretend to approach it, but instead use a tried-and-true means to organize the plants: Linnaeus's sexual system.[614]

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608 Fleming 1822, p. 2.
609 Jussieu 1994b, p. 178, Stevens 1994, p. 297.
610 Jussieu 1789, p. xxxvi, Stevens 1994, p. 356.
611 Lefébure 1817, p. 221.
612 Duméril 1804, p. 208.
613 Roscoe 1830, pp. 183, Duméril 1804 , Duméril 1804, p. 3.
614 Roscoe 1815, p. 183.

All-purpose versus special purpose

Natural groups are real; artificial ones are arbitrary or convenient (special-purpose)

Reverend Leonard Jenyns explained that in "an *artificial* system... the sole object is to facilitate the student in recognising the species he meets with, ...[while] in a *natural* one [,] Every group ...must be shown to exist in nature."[615] This distinction also encompasses the observation that natural classifications can allow predictive inferences to be made about the groups they contain, since members of natural groups are expected to have many features in common,[616] whereas the groups formed with artificial classifications are arbitrary with respect to natural relationships and therefore have no predictive value.[617] It is important to note that artificial classifications are not, however, arbitrary with respect to ease of use. Many naturalists recognized that artificial arrangements of plants functioned well as naming devices.[618]

Natural is profound (all-purpose); artificial is superficial

Marie-Jean-Antoine-Nicolas de Caritat, marquis de Condorcet wrote that the natural method would not be a mere "simple nomenclature, a kind of artificial memory – it would become the foundation of a science ... established according to the general laws of nature."[619] In the *Extrait des Registres de la Société Royale de Médecine* at the beginning of A.-L. de Jussieu's *Genera plantarum,* the signatories Geoffroy, Jeanroy, and Jean-Noël Hallé (1754-1822) explained that the difference between artificial methods and the natural method is that the natural method has the goal of improving and perfecting physical science, while artificial

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[615] Jenyns 1833, pp. 389-390.
[616] Stevens 1994, p. 154, Lindley 1835, p. 39, Candolle 1813, p. 31.
[617] Lindley 1835, p. 39, Duméril 1804, p. 2.
[618] E.g. Cuvier 1836, p. 3, Bicheno 1827, p. 481, Candolle 1813, p. 27.
[619] Condorcet 1780, p. 15.

ones have the goal of ease of classification and of getting to know species.[620] Similarly, Candolle described natural arrangements as useful only to people with prior experience with the numerous features of the organisms being classified; those with only superficial knowledge should stick to artificial schemes based on fewer features.[621]

Gestalt recognition difficult for beginners

The main reason why the natural method is difficult in practice for beginners is that beginners have more trouble extrapolating the affinities of an unknown specimen from the few groups of plants they are familiar with than do people who were aware of a larger number of plants and their features. This problem of extrapolation was, in fact, was practically the same as the one caused by old-style Latin comparative names. It had the same advantages and problems, but was capable of working on a much larger scale.

It is true that describing one plant in terms of another works well enough with small numbers of plants, and when the plants being used as examples are widely known. In such cases, its major advantage is that it is, as Mouton-Fontenille de la Clotte put it, a form of "artificial memory" formed "by linking less-known objects with better-known objects." This is why early modern Europeans had used this technique to name exotic plants by analogy to familiar ones. But this method, he explained, is useless unless the plants being used for comparison are familiar to readers. Typically this was not a problem, "since the comparisons were almost always taken from the most common plants, [and] it followed that they were sufficiently ordinarily known to everyone." The "small inconvenience" of the occasional analogy with an unfamiliar plant must be weighted against the "invaluable advantage of fixing ideas in the most certain and most rapid manner." Mouton-Fontenille further added lustre to this idea by mentioning that "immortal *Buffon* used it advantageously in his methodical consideration of birds: he had the great skill of putting the European species first,

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[620] Jussieu 1789, p. 15.
[621] E.g. Candolle 1813, pp. 54-55, 260-261.

and to refer the exotic species that had points of analogy or resemblance with them to those." It must therefore have been "truly philosophical."[622]

But philosophical daydreams aside, Linnaeus had recognized decades earlier that the number of known plants was too large to permit everyone to know enough common ones to compare – let alone the fact that many naturalists were far from Europe and could not use its plants for comparison at will. He had the good sense to forbid comparative names and unfamiliar analogies in descriptions.[623] But the "artificial memory" that comparisons with other plants could confer on botanists was not eliminated. It was instead shifted from resting on the similarity of individual features – fleshy leaves alone, for example – to larger numbers of similar features, such as would be found in plants of the same genus, or, later, natural order (plant family). This allowed comparisons to be made on a much broader selection of plants. It also, of course, made it more difficult for beginners to acquire the background knowledge required.

Natural method is difficult to learn and to communicate

Another aspect of the profundity of the natural system that some botanists promoted was the impossibility of communicating its principles. This idea was particularly appealing to fanatical Linnaeans in 19th century Britain, who were looking for reasons to continue teaching the sexual system of botany even though the natural system was regarded by leading botanists in every other country as more scientific. They argued that since botanists could understand the mysteries of the natural method only after a long study of plants and all their myriad connections with one another, it made sense to identify plants using the sexual system, which everyone knew worked fairly well. William Roscoe of Edinburgh was one such Linnaean who explained that artificial arrangements are meant only to name plants,

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622 Mouton-Fontenille de la Clotte 1803, pp. 8-9, i.e. "Affinitates Plantarum."
623 Linnaeus 2003, pp. § 198, 226.

which they do well, since they are based on obvious external features of plants. "The" natural system, on the other hand,

> must be founded on a long and intimate acquaintance with the nature of plants.... the system then proceeds to arrange the individuals of the vegetable kingdom, not by their exterior phænomena, but by those primitive and secret alliances by which nature has bound them together .... an artificial method, devised and completed by one person, may readily be communicated to another and is as intelligible to the student as to the preceptor; whilst, on the contrary, the knowledge of a natural system is chiefly confined to the author, and cannot be fully attained by any other person without entering into the same investigations, and ascertaining the same facts; many of which might perhaps afford different results, or lead to different conclusions.[624]

If the natural method were unique to each person's understanding, it could never be communicated clearly. It was hopeless to even think about it. It would be a better use of one's time and energy to stick to the tried and true sexual system.

Opponents of the sexual system turned this argument on its head. For them, the sexual system's ease of use was an indication that it was not sufficiently natural to be worthwhile. Conversely, the difficulty of using the natural method to identify organisms became a selling point for certain naturalists keen to separate themselves from such trifling amateurs; to be proficient in it was a mark of competence.

John Lindley ridiculed all of these ideas in his introductory lecture to the first class of botany students at the University of London on Thursday, April 30, 1829. Lindley had announced that he was going to teach the class the natural method. "I am told," he said,

> that in this I shall surely fail; that it is impossible to explain to a Botanical class the mysteries, if they be mysteries, of the natural affinities of plants, and that all experience is against the attempt. But I confess I am not of this opinion .... The characters upon which this redoubtable [natural] system depends, are after all just as obvious and as easily detected, as those of the most superficial mode of study that has ever been invented.[625]

Even so, Lindley had to concede,

> there will always be more difficulty in acquiring a knowledge of the Natural System of Botany than of the Linnaean. The latter skims only the surface of things, and leaves the student in the fancied possession of a sort of

[624] Roscoe 1830, p. 183.
[625] Lindley 1829a, pp. 13-14.

> information which it is easy enough to obtain, but which is of little value when acquired; the former requires a minute investigation of every part and every property known to exist in plants, but when understood has conveyed to the mind a store of information, of the utmost use to man, in every station of life.[626]

The profundity of the natural system, according to Lindley, was within reach of anyone who cared to pay attention to the same features of plants that users of the sexual system had to pay attention to. The difference was that the investigation of nature required by students of the natural method would be useful for any purpose imaginable, not just for trivial name games.

My way is natural, yours is artificial

The ornithologist Edward Newman summed up the feelings of many naturalists of both the 18th and early 19th centuries when he wrote that

> I know, from my own feelings, that philosophical, clear, sound, correct, natural principles or views mean one's own views, or views that coincide with one's own; while, by unphilosophical, indistinct, unsound, incorrect, artificial principles, we simply mean the opinions of others when in opposition to our own.[627]

But, as seen above, while the *ways* in which artificial classifications did not reflect the presumed total natural order of things were often described in great detail, the *reasons* for this disjoint were usually not explored.[628] It is well worth considering why botanists so frequently used artificial techniques for arranging plants, if, as the rhetoric would imply, all they were doing was exhibiting their ignorance and laziness. In fact, while the natural-artificial dichotomy is appealing in its simplicity, it obscures the main reason why artificial components of schemes were used, namely, to simplify plant identification. For, as detailed earlier, though the "natural method" has the marked advantages of accommodating anomalous or incompletely known organisms and stores growing, large data sets much better than any other framework, it can be difficult to use on its own for plant identification

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[626] Lindley 1829b, p. xi.
[627] Newman 1833, p. 485.
[628] E.g. Mouton-Fontenille de la Clotte 1798, pp. 11-12, Smith 1791, p. 44, and this approach continues today, e.g. Larson 1994, p. 35, Stevens 1994, p. 265.

purposes. Eighteenth and 19th century botanists were aware that finding a plant in a book arranged according to the natural method requires so much background knowledge that it is difficult and often confusing for beginners[629] – not to mention that often the arrangements within well-accepted natural groups are so author-specific that they stymied even experts.

Natural-artificial is not a dichotomy, it is a continuum

As we have seen, 18th- and 19th-century naturalists used "natural" to mean representative of plants' relationships (keeping like with like, keeping dissimilar things separate), comprehensive (using comparisons of many features), and in tune with recognized theories of the organization of nature[630] – though not necessarily all of these meanings at once. On the other hand, they used "artificial" to refer to arrangements that did not reflect plants' interrelationships, either by separating those that were seen to belong together, or by putting together those that were seen to be distinct.

Schemes generated by grouping plants on the basis of one or a few features tended not to group plants "naturally," and so were called artificial. So were schemes that split plants into groups with a particular number of subgroups that had this result.[631] But all arrangements of plants are compromises between being "natural" and "artificial," because the "artificial" components of the arrangements were precisely those that were implemented in order to make them easier to navigate. These included symmetrical arrangements of small groups, the uniform use of specific features, similarities to earlier arrangements, etc. For instance, Adolphe Brongniart reminded readers that, when Antoine-Laurent de Jussieu occasionally deviated from the fundamental principles according to which he conceived the *Genera Plantarum,* "he does so as a concession to facility of study, or to the old systems, rather than from real conviction." Jussieu had also increased the number of classes so that

[629] E. g. Jussieu 1789, p. lvi cited Stevens 1994 p.376, MacKay 1836, p. ix.
[630] This did not necessarily mean congruent with what we now think of as natural, e. g. for Quinarians, "natural" meant grouped in fives.
[631] E. g. Ray's comments that Wilkins' highly structured scheme was "manifestly imperfect and ridiculous" (Ray 1848, p. 41).

there would be fewer plants in each, and tried to ease the transition to the natural method for followers of Tournefort's method, by emphasizing their similarities, since Tournefort's "was generally known, not only to the pupils, but to the majority of the botanists of that era."[632] Furthermore, the groups formed using "natural" or "artificial" methods were sometimes identical, for some natural groups have unique features found in no others (e.g. legumes have their special kind of fruit, and among flowers with radial symmetry, only the Asclepiadaceae have pollinia).

The extent to which any given arrangement of plants was "natural" or "artificial" was, therefore, very much dependent on what it was meant to accomplish, and under which constraints it was written. These circumstances varied widely, but must be taken into account for the terms "natural" and "artificial" to have anything more than rhetorical meaning.

Book features: stand-alone or not

Although monographs, floras, and indexed lists of synonyms already existed in the late 17th century,[633] most botanical texts were meant to fill the roles that are now occupied by several different genres. They served as combinations of :

- introductory texts with didactic functions covering the scope and history of botany;
- instructions about how to collect and preserve plants;
- descriptions of where plants grow and when they bloom (floras, floral calendars, ecology texts, biogeography texts), or what their parts are and how they function (plant anatomy and physiology texts);
- explanations of technical vocabulary (glossaries);

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[632] Brongniart 1838, p. 299. The work Brongniart refers to is: Jussieu 1994b, p. 184, Stevens 1994, p. 302, but Jussieu explains what his uncle did and why in more detail in Jussieu 1789, p. lv, Jussieu 1994c, p. 375.

[633] E.g. Bauhin 1623, Ray 1660, Morison 1680.

- descriptions of the relationships among plants, as understood by cutting-edge researchers (monographs, but also genera/species plantarum);
- repositories for information about as many plants as possible (genera/species plantarum), or lists of particular plants present in a given collection or area (floras, collection lists);
- devices to find out the names of specimens (keys);
- portable guides to plants, especially for use in the field (field guides); and
- coffee table books (florilegia), full of pretty pictures.

Some books also included information about medical and/or agricultural, horticultural, or other economic uses of plants.

Comprehensive vs. portable

Most botanical texts purported to be able to help people identify plants, to be easy to use by beginners, and to be arranged in a "natural" way, though rivals were always quick to point out exactly where each one failed. It should be no surprise that no text was perfect, for many of the aims of texts listed above are at cross-purposes. It is difficult, for instance, to make a text both comprehensive and portable. A comprehensive reference text might be expected to have everything in it that someone would want to look up – synonyms, illustrations, technical terms, and so on – whereas a portable guide to plant identification, capable of being taken into the field where the plants were growing, would need to be small and light, with the emphasis on naming specimens. Users could be referred elsewhere for detailed information.

Vade mecums

Botanical *vade mecum*s were a kind of book meant to be taken into the field. They exemplify early attempts to mix the right proportions of detail and brevity in botanical texts. The first *vade mecums* produced for general (as opposed to medical) audiences began to appear in the early 18th century. Despite attempting to address the demand for botanical arrangements conducive to self-instruction in plant identification, these books were not as effective as guides as they claimed to be. The physician Martin Daniel Johren (?-1718), for instance wrote a work called *Vade mecum botanicum* (1710), with the full translated title: *Botanical vade mecum, or a botanical guide, a book most useful not only to lovers of botany, but also to all other students of any faculty: a book by which both those who zealously search for plants and those who wander far and wide for mere enjoyment can consult [their] memory in naming plants, can easily impress upon themselves the character of plants, and can, in this way, acquire an understanding of plants: according to the Tournefortian method.* Note that Johren intended his book to supplement the memory, not to somehow enable a budding botanist to memorize everything he would need to know.

Although Johren's book contains one of the earliest known, if not the first use of "if-then" statements for plant identification purposes,[634] the order in which the plants appear in the text is – as advertised – identical to that used in Tournefort's *Institutiones*. The text is accordingly divided into sections according to flower shape. But the *Vade mecum* lacks a table of contents or outline of Tournefort's system, making it confusing for people unfamiliar with Tournefort's arrangement to find the part of the text that applies to their plant. (It does, however, have a 44-term glossary to help beginners sort out what the section heads mean, if not where they are). The if-then statements about plants are in a uniform typeface, except for the Latin names of plants, which are in italics. The French plant names are indented, but there are no spaces between paragraphs or any hanging sections. It is hard to scan for information in this text. In later years the work was, according to A.-P. de Candolle, "all but ignored, and although clever, in effect merits little attention because of the weakness of its execution."[635] Nevertheless, Johren's *Vade mecum* was popular enough to be

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[634] A.P. de Candolle Candolle 1813, p. 48, called it the first work to use the "méthode analytique" while S. F. Gray referred to this text as "the first attempt at an analytical method of plants" (Gray 1821, p. 27). To 21st-century eyes it resembles a text-only flow chart.

[635] Candolle 1813, p. 49.

reprinted after seven years. It also may have influenced Linnaeus in designing his texts to be used as portable guides: it is one of the books that Linnaeus is known to have taken with him on botanical excursions during his time in Lund, in 1727-1728.[636]

A number of other abridgements or popular reinterpretations of grand systems, and local renditions of larger floras, were published with the express aim of helping students or beginners to learn about plants in the field.[637] As François-Joseph Lestiboudois put it, referring to the 3-volume *Flore Françoise* (1778), "What use is it to carry and flip through three volumes while botanizing?"[638] But just as with Johren's effort, authors had to be careful not to strip such books of too much useful information, or they would fail to meet users' needs. A reviewer, for example, complained that Sir James Edward Smith's *Compendium of the English Flora*, "one thin 12mo volume, containing little more than the specific descriptions taken from the larger work, with the duration, time of flowering, and usual situation of each species, expressed by abbreviations.... was insufficient without access to the larger, and the larger work was inconvenient in use without the smaller and more portable one."[639] Achieving the right balance between comprehensiveness and portability could be tricky.

Cost

Cost was a major factor as well: the people putting the book together must be able to afford to publish it, and the intended audience must be able to afford to buy it, in order to justify the amount of work required. The high costs of illustrations greatly affected the format of botanical texts, for introductory texts in particular – aimed at a student audience – had to be produced at a modest cost or risk not being sold.

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[636] Stearn and Bridson 1978, p. 6.
[637] E.g. Lestiboudois 1781, Dubois 1803, Johren [1710].
[638] Lestiboudois 1781, p. iv.
[639] The English Flora. By Sir J. E. Smith. . . vols i-iv. 1824-1828; the British Flora, by W. J. Hooker . . . vol. i. 3rd edition, 1835; the English Flora. Vol. v. part i. (Or, the British Flora, vol. ii, part i). By W. J. Hooker . . . 1833. Same works, vol. i. part ii. By W. J. Hooker, and Rev. M. J. Berkeley. . . 1836 1837, p. 94.

All of these considerations must be taken into account when pronouncing a botanical arrangement "natural" or "artificial," for arrangements of plants are only one component in texts of which the larger structure and context are also important in accomplishing their aims.

Illustrations

The role of illustrations in natural history instruction was ambiguous in the early 18th century and even into the 19th century, though not always for the same reasons. For instance, Hermann Boerhaave's instructions to students not to trust illustrations on the grounds that ancient authors knew what they were doing, but modern ones did not – a continuation of arguments typical in the 16th century.[640] He warned that figures purported to be of plants described by the ancients were drawn entirely by modern authors, who could have made identification mistakes, and lamented that Colonna's "Books can scarce ever be printed again, because no one now in the World can engrave them so well."[641] Though quibbles about whether modern illustrators were capable of producing figures up to old standards disappeared later in the 18th century, other problems remained. Linnaeus' sexual system in particular was blamed for turning botany into nothing better than an elaborate game of words. But was it avoidable, given the technological constraints at the time?

Inherently misleading

Linnaeus, for instance, was against using illustrations in natural history texts because he saw them as inherently misleading. As far as he was concerned, illustrations in natural history texts were eye candy only, with little, if any, scientific value. He did, however, concede that the "figures or drawings" used by Tournefort "render his genera much more intelligible.... Most of the Painter's figures elucidate more parts, more marks, the shape of the

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[640] Reeds 1976, p. 529.
[641] Boerhaave 1719, pp. 127, 133.

flower, etc., than the [written] description."[642] In the next sentence, however, Linnaeus went on to say that he absolutely rejects the use of drawings for determining generic limits, and although illustrations are "of great importance to boys and to those who have more brain-pan than brain; I confess that they convey something to the unlearned," it is still "an easier and surer way to communicate ideas by writing,"[643] for "Who could ever deduce a firm argument from a drawing? But from written words, it is easy."[644] According to Linnaeus, writing not only permitted attentive authors to convey more precise ideas, but also allowed them to generalize. For although he was quite skilled at producing summaries describing groups of plants in general terms, Linnaeus's approach to representing plants with drawings was surprisingly literal. In the first edition of *Genera plantarum* (1737) he wrote that, since the numbers and shapes of plant parts vary among the different species within each genus, botanists would have to include figures depicting all such differences if they wanted to illustrate a genus – a ridiculously expensive proposition, not to mention that "Who would be able to extract any certainty from such a multitude? But," Linnaeus wrote, "to omit the differing parts from a description and to describe those agreeing is a much easier task, and easier for the intellect... We will therefore try to express by words all marks just as clearly – if not more clearly – as others with their splendid drawings."[645] Generalizations were best restricted to words; a generalized illustration (if such a thing could be possible) would be misleading because it would not be capable of showing the range of variation exhibited by the organisms it was meant to represent.[646]

Other botanists likewise found written descriptions capable of expressing more precision than drawings. "Sensible as I am of the importance of figures in the present state of

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[642] Linnaeus 2004 (not published), p. 5.
[643] Linnaeus 2004 (not published), pp. 5-6.
[644] Linnaeus 2004 (not published), p. 6.
[645] Linnaeus 2004 (not published), p. 6.
[646] Still, Linnaeus included references to where illustrations of the organisms he was describing could be found. For instance, the 12th ed of the *Systema naturae* had an "appendix synonymorum" to help readers look up illustrations in a few entomological treatises that he recommended. He provided volume numbers and page numbers for figures in Shæffer's *Iconum Insectorum Ratisbenensium* and Seba's *Masei tomus quartus* (Linnaeus 1766, 12 ed., vol. 1, p. tome I vol. II p. 1355).

botany," the physician Jonathan Stokes, compiler of a list of references to figures for the second edition of William Withering's *Botanical arrangement of British plants* (1787) wrote,

> I am inclined to believe, when the Linnaean principles of discrimination have been carried to their full extent, that species, as well as genera, may be distinguished in so clear and unambiguous a manner, that figures may be rendered of far less importance, and their use principally confined to the Fungi, Lichens, and Medicinal plants, which last it is of importance to know in every stage of their growth.[647]

These comments echo Linnaeus's statement that communicating with pictures is more primitive than using written words,[648] that illustrations were too vague – fuzzy – to really be useful in botany. They were a safety blanket of sorts, comforting to botanists in botany's juvenile state, but ultimately to be discarded as botanical knowledge matured.

Bad quality

Some naturalists warned that bad quality illustrations were worse than no illustrations. John Ray, for instance, was persuaded by his friends to avoid illustrating his *History of Plants* at all – contrary to his original intentions – because he could afford no better than second-hand woodcuts, which would have devalued the rest of the work that he put into it.[649] As William Withering put it nearly a century later, "bad plates conveyed false, or insufficient ideas."[650]

Skilled renditions of poor quality material were especially pernicious because although the images would be pleasant to look at, they would also be easy enough to take for granted without further inquiry, leading to taxonomic errors. Illustrations in some natural history publications were known to be "deceiving, fictitious, or over-adorned by artists,"[651] or to have been drawn from specimens that the artists had never seen alive[652] and which

[647] Withering and Stokes 1787, 2 ed., vol. 1 and 2, p. xxxvi.
[648] Linnaeus 2004 (not published), p. 6.
[649] Raven 1986, 2 ed., p. 214.
[650] Withering and Stokes 1787, 2 ed., vol. 1 and 2, p. xvi.
[651] Kusukawa 2000b, p. 186.
[652] Knight 1981, p. 54.

might look very different from the living organisms. Some were suspected to have been damaged or faked without the artists' knowledge.[653] The resulting drawings would be unrealistic, no matter how earnest the artists were or how hard they tried to capture reality. For these reasons, Kirby and Spence advised aspiring entomologists that figures

> should never be referred to in the first instance, but be regarded as a resource when the ordinary methods leave the subject of inquiry doubtful. Those who begin their entomological studies by turning over figures usually end them there, and never attain to that nameless tact in making out insects that can only be the result of patient study.[654]

In other words, illustrations at best provide a visual complement to the knowledge acquired the tried and true way, through comparisons of written descriptions with actual specimens.

Too expensive to produce good-quality works

But even people who believed that illustrations could be helpful in plant identification often chose or were forced to omit them, as the costs of publication were prohibitive. Cesalpino's *Sixteen books on plants* (Florence, 1583), for instance, was not illustrated (due to the expense) but came with a little herbarium instead – not something that could be repeated on a large scale.[655] Good quality illustrations were expensive, particularly copperplate engravings or etchings, which require a separate printing press from the text, and hence were printed on separate pages.

Making a profit or even breaking even on books full of copperplate engravings was difficult. Potential audiences – particularly for works in Latin – were small, and few interested people could afford them.[656] This was particularly so in England in the 1680s and 1690s, when the political situation was unstable. Robert Morison, for instance, "reduced himself almost to penury in order to provide proper illustrations, copper-plates not woodcuts, for his work,"[657] and not much later, the Royal Society found itself in serious financial

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[653] Duncan 1837, p. 206.
[654] Kirby and Spence 1828, 5 ed., vol. 4, pp. 568-569.
[655] Kusukawa 2000b, p. 107.
[656] Raven 1986, 2 ed., pp. 325, 330 quoting Ray's Further Correspondence p. 249.
[657] Raven 1986, 2 ed., p. 213.

difficulties after its members paid for the illustrations of John Ray's *Historia piscium* (1686)[658] – not to mention that fewer people seemed likely to be interested in financing an illustrated book of plants than on those full of pretty birds or even fish, even in a political climate more conducive to book-buying.[659]

Similar situations kept occurring so long as poor-quality woodcuts and expensive copperplates remained the only choices for botanical illustrations. Even then, there was no guarantee that copperplate engravings would come out as well as planned. Aubert du Petit-Thouars complained in 1811, for instance, that after he had ten plates engraved for the first *livraison* of his *Mélanges de botanique et de voyages,* the quality of illustration, particularly of crucial flower parts, degraded during every step of the process of transferring the drawings to prints. He decided to hire an artist to do the engravings directly. Even though the cost of publishing the printed text was low, he could not afford to do the same thing again for the second *livraison.* He ended up engaging the services of first one bookseller, then another, to have the plates done to his satisfaction, incurring losses of time, money and independence along the way. The second bookseller reworked the plates to be able to print them in colour, at the expense of the detail that du Petit-Thouars thought essential for scientific purposes. In total, he spent so much time and effort on these two *livraisons* that he was not able to publish a third. "Science," he wrote,

> thus lost out as far as exactitude goes. As for elegance, it also gained nothing, since [the engravers] traced exactly the outlines that I gave [them], but I never could redo the details of the fructification. So, the only result of this operation was to increase the price of the work by more than double, without any real advantage. It seems that the bookseller did not do well from this change either, since this work is buried away in his stores."

Du Petit-Thouars ended up selling his book at a discount in order to recoup some of his losses. He also began it with an apology for publishing "such imperfect sketches" when other botanical works appeared to "compete for preeminence in beauty and exactitude." Du Petit-Thouars continued, however, that he was heartened that true botanists still encouraged

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[658] Raven 1986, 2 ed., pp. 350-357, especially 365.
[659] Raven 1986, 2 ed., p. 243.

him to publish because they were aware of the scientific value of his work.[660] Even four years after the disastrous encounter with the booksellers, he was still too much in debt to print the rest of the plates of plants he had described during his years in Madagascar, Bourbon (Réunion) and Ile de France (Mauritius).[661]

Despite the commonality of publishing horror stories such as these, copper plates "remained the norm for accurate illustrations down to the middle of the 19th century"[662] despite the difficulties involved in recruiting and running a team of artisans qualified to produce suitable figures[663]; in 1813, for instance, Candolle insisted that copperplate engravings are the only way to illustrate plant parts with a sufficient level of detail to allow for the identification of previously undescribed plants.[664]

Too expensive to produce works with multiple illustrations

There were also other barriers to producing illustrated botanical works. Not everyone could afford, as Rivinus had done, to spend 80,000 florins on furnishing his botanical works with 500 copperplate illustrations![665] A book full of illustrations, even if they were low-cost woodcuts, could become expensive because of their sheer number, not to mention too bulky to be useful as a portable guide. Written descriptions were also easier to reproduce and disseminate than good-quality illustrations, assuming that such things were even capable of representing generic characters adequately. As Linnaeus wrote, "If one wants to use or review a generic character in some book, one cannot always easily paint, engrave, print, and

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[660] du Petit-Thouars 1811a, pp. 5-6.
[661] du Petit-Thouars 1811a, p. 28.
[662] Knight 1981, p. 53.
[663] See Raven 1986, 2 ed., p. 321 for an account of Ray's difficulties in getting the brass plate illustrations for Willughby's *Ornithology* produced up to standard.
[664] Candolle 1813, p. 286.
[665] Sachs 1890, pp. 74, note. Sachs added that Rivinus lived in Halle (in Saxony). According to Spangler 2002, florin was another word for guilder, although there were various kinds of florins/guilders in circulation, and they were not always equivalent to each other. Stanley, Newton and Ellis 2004 [1702] provides a table of currency equivalencies for 1702, written in part by Isaac Newton. Interpreted according to the currency and silver weight conversion tables in Jordan n.d., 80,000 Saxon guilder at the end of the 17th century were roughly equivalent to 9,373 pounds, 3 shillings and 19 pence, or 37,167 troy ounces of silver! I am not convinced that Rivinus really had this kind of money – perhaps the number given in Sachs is a misprint, or the kind of florins Rivinus used were not represented in any of the currency conversion charts I have found.

publish a picture; however, it is easy with a description."[666] And, perhaps most frustrating of all, including illustrations in introductory botanical texts, where they could be welcoming to beginners and useful in instruction, could price them out of the budget of their intended audience of students. Botanical writers and publishers had to balance all of these considerations when deciding how and the extent to which a particular work should be illustrated.

Making reference to figures in other books was one way to bridge this gap between a desire to include illustrations and financial constraints preventing the engraving of suitable plates. In the preface to the second edition of William Withering's *Botanical arrangement of British plants* (1787), Withering was proud to announce that a new set of references to figures "make one of the most valuable parts of the present Edition"[667] because "It is not in every one's power to become possessed of a complete collection of figures, and those who are, cannot easily transport them to situations in which their possessor might wish to consult their contents." Even so, the number of references to figures was restricted by the size of the text.[668] And though references to descriptions and illustrations in other books were helpful, they remained insufficient for poor or geographically isolated people unable to access the books cited, just as Ray had pointed out a century before.[669] Until the price of illustrations came down sufficiently to permit introductory systematic texts to use them at will, the constraints on the publication of accurate illustrations forced a reliance on words, and words alone, to describe plants well enough to allow their identification. This remained the case for more than a hundred years. As Mouton-Fontenille said of figures in botanical works,

> reducing their number would make their acquisition less costly, and useful to a greater number of people.
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> The costliness of botanical works will always be an obstacle to the progress of this science. Dr. Gilibert calculated that an amateur who wants to procure only the fundamental works for himself would be obliged to

[666] Linnaeus 2004 (not published), p. 6.
[667] Withering and Stokes 1787, 2 ed., vol. 1 and 2, p. v.
[668] Jonathan Stokes, compiler of said list of references, in Withering and Stokes 1787, 2 ed., vol. 1 and 2, p. xxix.
[669] Ray 1848, p. 156, Letter to Tancred Robinson, October 22, 1684.

> spend 3000 francs. How many people who, with a decided taste for plants, cannot allow themselves this expense![670]

He suggested that illustrations should be used to confirm species identification only after other means had been used to get that far.

This is not to say that botanical texts in the 18th and early 19th centuries did not contain images of plants – far from it. In England, "monster" illustrated botanical texts such as Sir J. E. Smith's *English Botany* found an audience. The first edition, published from 1790 to 1814, ran to 36 octavo volumes. Smith's *English Flora* (1824-1828) – considered "the standard contemporary text" in England at the time[671] –

> merely continued an established tradition. Like Albin's showy tomes or Pennant's *British Zoology* they were aimed at the shelves of affluent gentlemen-scholars. They were not working manuals; and the high price that went with their temptingly lavish character put them beyond the reach, as usual, of many field-workers who genuinely had need of them.[672]

They were "too bulky for the valise of the tourist, and too expensive for the pocket of the young student."[673] Smith tried to reduce the size of the second, "small," edition to make it handier, leaving out many illustrations in order to bring its price down sufficiently to put it within the reach of students. In place of these pictures, he included expanded descriptions of the plants that were not illustrated, as well as references to other botanical works that have illustrations.[674] The substantially smaller edition – 12 volumes published from 1832-1846 – seems to have found favour with the public; the third edition, published 1841-1852, was also 12 volumes.[675]

But such books were still far too cumbersome to take on plant identification trips, such as field botany classes instituted at universities in Glasgow, Edinburgh and Cambridge

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670 Mouton-Fontenille de la Clotte 1803, p. 193.
671 Allen 1976, p. 109.
672 Allen 1976, p. 95.
673 The English Flora. By Sir J. E. Smith. . . vols i-iv. 1824-1828; the British Flora, by W. J. Hooker . . . vol. i. 3rd edition, 1835; the English Flora. Vol. v. part i. (Or, the British Flora, vol. ii, part i). By W. J. Hooker . . . 1833. Same works, vol. i. part ii. By W. J. Hooker, and Rev. M. J. Berkeley. . . 1836 1837, p. 94.
674 Smith and Sowerby 1832-1846, 2 ed., pp. iv-v (unpaginated).
675 Information on publication dates and the number of volumes per edition obtained from WorldCat.

in the early 1820s,[676] and botanical outings organized by private societies slightly later.[677] As David Allen wrote, the 1820s were characterized by "a substantial unsatisfied demand for inexpensive works of identification. It was thus a singular stroke of luck that just at that point there should have occurred a breakthrough in publishing economics."[678]

Technological advances

By the mid-1830s, changes in printing technology in Britain – particularly the widespread use of the steam printing press, which had been introduced from France in 1814 – and a reduction in the high taxes on books and journals by 1836, allowed more authors than ever before to publish good-quality illustrations at affordable prices.[679] William MacGillivray (1796-1852) trumpeted his plan to take advantage of the new technology in his "Address to the Public" announced in the *Edinburgh Journal of Natural History* on October 24, 1835:

> The taste for Natural History is now universally diffused throughout the empire. Within the last few years, various cheap publications on this subject, illustrated by engravings on wood, have led all classes to observe and to enjoy the ever-varied beauties of the creation. But no description, however correct, or no wood-cut, however well executed, can give that complete idea of a natural object, which is effected by an engraving on steel, when coloured with accuracy. The enormous price at which these illustrations of a higher order are usually sold, has alone prevented them from becoming extensively popular. Hitherto, coloured engravings, executed with beauty and correctness, have been accessible to the wealthier classes alone. It is proposed, in this work, to place the elegant engravings of the choicest productions of Nature, within the reach of all classes of the community; and thus, in small towns and in the country, where museums are unknown, and libraries very scarce, the lover of Nature will be enabled to extend his knowledge of the works of the Creator beyond the limited sphere of his own observations.[680]

The use of lithography was another factor in bringing the price of illustrated works down. This technique, invented in Germany at the turn of the century and employed in English natural history works in the late 1810s and early 1820s, enabled subtle effects to be

[676] Allen 1976, pp. 107-109.
[677] Allen 1976, pp. 160-162.
[678] Allen 1976, pp. 95-96.
[679] Allen 1996, pp. 111-112, Allen 1976, p. 96.
[680] The Edinburgh journal of natural history, and of the physical sciences 1839, p. 1.

reproduced economically and on a large scale. Though its results were uncertain at first, "Gradually it came into use as an accepted alternative to copper-plate engraving (the method favoured, for example, by the Sowerby family) and eventually overtook and largely superseded it .... the great increase in accurate coloured illustrations in natural history works which occurred in consequence helped to stimulate still more interest in the subject."[681]

Integrating illustrations and text

In terms of using illustrations as observation aids, however, much remained to be desired. The "bulging picture-books which had been produced so generally till then for people with more cash than knowledge – or even literacy" may have been enabled by the new printing technologies to become "scientific tools of the requisite precision and exactness,"[682] but they nevertheless continued to bulge. Though illustrations were less costly than before, the texts in which they appeared were not designed to facilitate the identification of organisms. Instead, they showcased all the lavish detail that it was possible to produce, at the expense of portability, ease of use in identification, and student-friendly prices.

This was particularly true in the case of books about birds – William MacGillivray, for example, described two beautifully-illustrated ornithological works as "less useful to the world than a good octavo volume of plain letterpress would be, for they are beyond the reach of ordinary students."[683] Books about showy plants sometimes suffered the same fate. Illustrations were still expensive in quantity. Their numbers and sometimes the quality of reproduction had to be reduced in order that they could be useful as identification tools, and they needed to be integrated with the text to a higher degree than had been common before. Authors of botanical texts had to conceive of accurate illustrations as one of several means to identify plants, and turn away from the idea that a complete illustration of a living plant was an end suitable for all purposes. It was necessary to switch from attempting to depict all

[681] Allen 1976, p. 99.
[682] Allen 1976, p. 99.
[683] The Edinburgh journal of natural history, and of the physical sciences 1839, p. 152.

aspects of a plant at once, as perfectly as possible, to showing only the portions of the plant in as much detail as necessary to convey the idea that needed to be expressed to achieve an author's goal, whether that goal be to point out a unique aspect of morphology, a feature illustrative of a technical term, or another attribute useful in plant identification. It was necessary to introduce the same kind of minimalist, functionalist aesthetic to plant illustrations that had been so successful in plant descriptions.

Along these lines, A.-P. de Candolle had praised the advent of beautiful modern coloured illustrations (which appeared in France earlier than in England) but cautioned that

> This perfection can be obtained only by a series of processes that make these books so extravagant that the majority of those who devote themselves to the study of botany cannot obtain them. This circumstance puts the point to which botanical plates should be brought to perfection into question again. In this respect it looks to me, as to many others, that the nature of the works must be distinguished.[684]

Candolle continued that introductory texts should have simple drawings, copied from monographs if necessary, but never coloured, while monographs should have detailed illustrations of plant organs, copperplate if possible, and shaded to show their depth and form. Finally, illustrations designed to show plants as they are when alive should be drawn and coloured as perfectly as possible, so that they could stand as type specimens for people restricted to the libraries of big cities and universities.[685] What Candolle did not mention is that in order to be most useful in plant identification, illustrations must be coupled with a structured way of proceeding through texts, for, as Jonathan Stokes noted, "figures, without a system to conduct the reader to them, are almost useless."[686]

Lists of synonyms

Reducing synonymy was very important in the 18th century. Synonyms confused communication and added to the number of plant names. But botanists could not just reject

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[684]Candolle 1813, p. 285.
[685] Candolle 1813, p. 285. Adanson had come up with similar but simpler guidelines much earlier (Adanson 1966, pp. clxxxiii-clxxxvii).
[686] Withering and Stokes 1787, 2 ed., vol. 1 and 2, p. xxxiii.

all synonyms without losing the knowledge attached to those names. Lists of synonyms, which would allowing cross-referencing with older works as well as contemporary ones using different nomenclature (a particularly pressing problem before nomenclatural standards were implemented in stages beginning in the 1840s) were common in medieval and early modern botanical texts[687] and also standard in botanical texts in the 18th and early 19th centuries. It was particularly common to put in Linnean synonyms, and even the classes and orders to which plants belonged in the sexual system, in books using other arrangements.[688]

Since compiling an accurate list of synonyms requires an intimate acquaintance with the plants themselves as well as the literature about them, good synonymists make good botanists.[689] But when publishing botanical works, it was not a case of 'the more synonyms, the better.' Authors had to balance the number of synonyms they included with ideas of what their intended audience was looking for in terms of the level of complexity, size, cost and portability of the finished work. For example, in the second edition of his *Botanical arrangement of British plants* (1787), William Withering left out

> The Latin Synonyms of Bauhine, Gerard, Parkinson, and Ray, which occupied so much space in the first edition ... partly because they could be of little use to those who do not understand the Latin language, for they who do, may get the references from HUDSON's *Flora Anglica*; and partly to make room for a new set of references to figures.[690]

A.-P. de Candolle even provided a guide to botanical authors about the quantities and sorts of synonyms that were appropriate to include in different genres of texts.[691] Though the

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687 Ogilvie 2003, p. 36.
688 E. g. Gilibert and de la Tourette 1797, 3 ed, Tournefort and Jolyclerc 1797 and many of the other regional floras mentioned in Williams 2001.
689 Candolle 1813, p. 225.
690 Withering and Stokes 1787, 2 ed., vol. 1 and 2, pp. v-vi.
691 The extent and the direction that one gives to synonymy vary greatly according to the nature of the work. If it is a matter of a simple catalogue or a book destined to be popular, one must use the name without synonyms. If it is a question of an elementary or classic [classificatory] work, the principal synonyms must be indicated, such as those that came after the era of Linnean reforms, that of the first botanist who discovered each plant, those where the best descriptions or the best figures are found, and finally those that tend to elucidate the native land or history of a species. If one wants to write a work of botany applied to medicine or to some particular art, the synonyms of authors who considered the plants according to the same point of view should preferably be researched, and above all common names

trimming down and focussing on what was relevant for each type of book – catalogue or popular work, introductory text, systematic review, flora, medical book or monograph – was in line with what was happening regarding the use of illustrations and descriptive information in other facets of plant identification, these suggestions did not address the question of how to let readers know definitively whether a name was a synonym or not.

Dictionary of synonyms wanted

Proposals for a new *Pinax*

Botanists yearned for the days when Gaspard Bauhin's *Pinax theatrum botanici*, which "was meant, as its name imports, as an index to all the botanical knowledge then in the world"[692] could be consulted as an almost certain way to find the name of any plant ever described. An up-to-date central repository of synonyms would also reduce the needless multiplication of further synonyms by reducing the research involved in determining whether a name had already been used. Considering that it took Bauhin 40 years to catalogue the synonyms of around 6,000 plants, which he then printed in 12 books, the prospect of an 18th- or 19th-century equivalent to his *Pinax* looked increasingly futile. It was common to lament the incompleteness and insufficiency of contemporary lists of synonyms, as well as the time necessary to look up early plant descriptions. Some botanists, such as Adanson, took this so much to heart that they were pessimistic that Bauhin's feat could be replicated. An update to the *Pinax,* according to Adanson, would be "so immense and so difficult to sort out that it would be useless and unnecessary. We do not counsel anyone to undertake it."[693] Still, a number of botanists toyed with the idea of writing one, though no such work was published. Lists of synonyms remained scattered and partial.

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should not be neglected, of which the utility in this part of the science is very important. Those working on a flora of a country should cite preferentially the authors who wrote on the plants of that country, and furthermore, the common names native to the aforementioned country should be collected with care, a kind of research too much neglected by botanists. Finally, in writing a monograph, all the synonyms that one can find out about should be brought together (Candolle 1813, pp. 253-254).

[692] Smith 1791, p. 13.

[693] Adanson 1966, p. clxj.

For instance, the Linnean Antoine Gouan (1733-1821) of Montpellier tried to persuade Henri-François-Anne Roussel (1748-1812), director of the Jardin botanique in Caen, to produce a continuation of Bauhin's *Pinax*, writing, "It would be very advantageous for botany if someone were to undertake this work."[694] Mouton-Fontenille wrote in 1810 that he wanted to offer a new *Pinax,* and described its scope, though nothing came of this project.[695] A.-P. de Candolle also complained that there was no way to recognize what modern name any old name corresponds with except by a protracted search. Though there were occasional indexes of synonyms for particular works, for instance, to the works of Matthioli, Dillenius and Bauhin, these partial works did not suffice. "There remains to be desired," Candolle wrote,

> that some hard-working learned man would like to start making a dictionary, in which every author's name [and] every common name will be linked to the name accepted by botanists today. Such a book would contribute greatly to making the science useful and popular. It would establish a connection between the public and the language of the learned, and, in many cases, would enlighten the learned themselves, by giving them an easy way of reading the writings of travellers and ancient authors.[696]

Candolle's countryman Aubert du Petit-Thouars had envisioned a similar work. It would be an alphabetical list of all generic synonyms in which a description of each recognized genus would be given and all synonyms cross-linked to it. The reasons for any changes in generic name would be explained. Main entries would also have notes on the etymology of the names and on the peculiar features of the plants they designate. The book would be in French so that anyone could use it.[697] But until such an all-encompassing book of synonyms could be compiled,[698] authors had to make hard choices about how much instructive and descriptive material to include in their texts. They had to think about not only how many

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[694] A. L. M. 1795, p. 329.
[695] Mouton-Fontenille de la Clotte 1810, p. viii.
[696] Candolle 1813, pp. 256-257.
[697] du Petit-Thouars 1817, vol. 5 pp. 230-231.
[698] No such list was published until 1885, when the *Index Kewensis* – which encompasses all seed plant genus and species names issued since 1753 inclusive – began to be issued (International Plant Names Index 2004).

synonyms to list, but also the extent to which they explained the technical vocabulary they used – that is, how detailed to make their glossaries.

Glossaries

Herbals and materia medica

Glossaries – alphabetical or topical lists of technical words used in a text, and their definitions – have been made at least since the middle ages. They appeared in illustrated as well as unillustrated texts.[699] A number of Renaissance herbals also contained glossaries, notably Leonhard Fuchs' his *De historia stirpium commentarii insignes* (1542), in which he "included explanations of such 'difficult words' (*vocem difficilum explicatio*) as *arbor, herba* and *fructus*."[700] Fuchs "set the parameters for botanical descriptions down through Linnaeus," such that later botanists put more effort into using standard terms when describing plants, and included multilingual glossaries and indexes of synonyms more frequently.[701]

18th century botanical texts

> *A rustic knows plants and so maybe does a brute beast, but neither can make anyone else the wiser. The botanist is distinguished from the layman in that he can give a name which fits one particular plant and not another, and which can be understood by anyone all the world over.*[702]

Making sure that anyone the world over could understand a botanical work was important. For this reason, botanists devoted a great deal of time and effort to making sure their readers would understand the terminology used. Glossaries of words followed by their

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[699] In the middle ages, "Glossaries, concordances, dictionaries, encyclopaedias, and *summa* were more prevalent than monographs" (MacKinney 1938, p. 240). Glossaries of technical terms were quite common in many different kinds of texts, e.g. alchemical ones, *materia medica*, and descriptions of diseases. Still, there was no standard name for this book feature. They were known as *synonyma, nomina, vocabularia, glossaria, index*, and other terms. Unfortunately for historians of the book, the presence of these reading aids is hardly ever recorded in catalogues, making it difficult to judge their prevalence (MacKinney 1938, p. 265). Glossaries continued to go by multiple names into the 19th century.

[700] Reeds 1976, pp. 528-529 n. 40.

[701] Ogilvie 2003, p. 36.

[702] Linnaeus *Critica botanica* (1737), aphorism 210, cited Stearn 1959, pp. 7-8.

definitions were only one of the solutions employed. For example, Linnaeus included the definitions of 224 terms in sections 79 to 85 of his *Philosophia botanica* (1751), but in the 12th edition of *Systema naturae*, he provided a "TERMINI ARTIS" instead. This was a list of terms with references to the pages on which those terms were used, rather than definitions.[703]

Sections of texts devoted to explaining the use of technical terms became much more common in the late 18th century, and much longer. By then, botanists were concerned that botanical terminology was getting too complicated for anyone to navigate without extra help. The ability to use terminology to describe plants precisely was extremely important, not only to understand written descriptions, but to communicate with others about specimens. Being able to employ technical vocabulary this way was also requisite of acquiring respect within the botanical community, as much as incompetence in this respect was grounds for derision. "It is easy to recognize a TRUE BOTANIST", wrote Lestiboudois,

> by the manner in which he describes a plant. In effect, a man who is not accustomed at all to observe plants often does not know which term to use, and gives an idea about the object that he wants to designate that's so vague that it is frequently impossible to recognize it.[704]

A major function of glossaries was therefore to familiarize people with the parts of plants that are important in their identification. To help beginners acquire proficiency in the use of technical vocabulary, Lamarck explained 722 distinct uses of French terms and 799 distinct uses of Latin terms (joined to the French ones), in a section called "Principes élémentaires de Botanique" (Elementary principles of botany)[705]. Similarly, Nicolas Jolyclerc (1746-1817), a Lyonnais Benedictine monk turned botanical lecturer, emphasized the importance of

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[703] Linnaeus 1766, 12 ed., vol. 1, pp. 1353. Tomus I part II, published 1767 and Linnaeus 1751, pp. §§79-85, counted by hand.

[704] Lestiboudois 1799, 2 ed., vol. 1, pp. 31, in a footnote.

[705] Lamarck 1778, vol. 1, p. 8. I counted the number of definitions in his "table des matières" (index of French terms) and his "table des termes latins." Some terms have more than one use; I counted each one as separate.

The "Principles élémentaires de botanique" section makes up 215 out of the 342 pages of introductory matter in the first volume, which also contains 130 pages of his méthode (key), one of additions, 27 pages of indexes to terms, and eight plates. (The second volume has four pages of introductory matter and 660 pages of méthode; the third volume is 654 pages of méthode, 1 page of errata and 20 pages of indexes to plant names (10 each for French and Latin).

explaining specialized terms in order to make botany accessible to as many people as possible, writing,

> All the additions that I have made in the dictionary of French technical terms [glossary], as well as the Latin vocabulary [dictionary] that I have added, have the aim of putting this book within the reach of everybody, to make it intelligible to anybody, to women as well as men, to the child led to the study, as well as to the man whose education is developed, to the man without knowledge as well as to him whose education has raised to be able to do without me.[706]

Jolyclerc's glossaries were alphabetical, but some glossaries were topical. For instance, Lamarck's was arranged by major plant part, from root to seed, with the adjectives and smaller parts of the plant that were associated with each part explained under each heading. Lestiboudois's "Table fondamental des descriptions" (Fundamental table of descriptions) followed a similar plan, but was laid out as a table.[707] Paulet's "EXPLICATION *Des principaux termes employés dans cet Ouvrage*" (Explanation of the principal terms used in this work) was also partially topical. Its headings are arranged alphabetically, but some terms explained under the headings are not cross-referenced. For instance, "Feuilleté," "Régulier," "Irrégulier," and "Simple" are explained under "CHAMPIGNON," which itself is defined biologically and etymologically; they do not appear elsewhere in the glossary, even though they could be applied to more specific parts of mushrooms.[708]

Sometimes glossaries also included references to plates in which the terms described were illustrated.[709]

By the second decade of the 19th century, bloated glossary sections explaining not only the usage, but sometimes also the etymology and pronunciation of many terms took up large proportions of some natural history texts. Notable examples include Candolle's "Glossologie, ou exposition des termes consacrés dans la langue de la botanique" (Glossology, or exposition of terms established in the language of botany), which takes up

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706 Tournefort and Jolyclerc 1797, p. xiv.
707 Lestiboudois 1799, 2 ed., vol. 1, p. table opposite page 33.
708 Paulet 1793, vol. 2, pp. iii-viii.
709 E.g. Paulet 1793, vol. 2, p. iv, Kirby 1802, vol. 1, p. 219.

41%, or 203 out of 498 pages of the entire work, Gray's "Explanation of the terms used in botany" (14%, or 191 out of 1539 pages), and Kirby and Spence's chapter 46, "Orismology, or explanation of terms" (16%, or 98 pages out of 614 in volume 4 alone).[710] In earlier books the percentage of pages occupied by glossaries – when present – had generally been much lower (e.g. 0.8% ("table alphabétique") Dubois , 4% ("orismological index") Kirby).[711] A possible explanation for this phenomenon is that the botanical community had become sufficiently fragmented by this time that authors were largely either writing handbooks or introductory texts for total amateurs, who would require large glossaries to explain technical vocabulary, but not as many synonyms, or they were producing larger reference works which did not necessarily contain glossaries but would be more useful if they contained extensive lists of synonyms and references to other works.

Scope: global or local (or otherwise restricted)

Ease of plant identification, as discussed before, is often a major, if not the primary, goal of botanical texts. But which format makes such a book the easiest to use? In many respects, it is a question of scale as much as purpose. As Sir James Smith explained,

> A Flora can afford but a broken and partial view of a Natural System, nor can such a system answer the first purpose of a Flora, which is to enable unpractised students to investigate and determine unknown plants.[712]

Although Smith's agenda was to promote Linnaeus's sexual system, he has a point. In a flora, many plant groups are likely to be represented by single species, with no obvious relationships to others. In this case, using only the natural method would not help anyone to identify such plants. Some other kind of reference tool would be necessary. In the superheated atmosphere of late 18th- and early 19th century botany, when the choice of arrangement of plants was often a political statement, justification of that choice was more often than not a complicated business.

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710 Candolle 1813, Gray 1821, Kirby and Spence 1828, 5 ed., vol. 4.
711 Page counts excluding indexes.
712 Smith 1824-1828, p. xxv.

Reference tool battle of metaphors

Artificial classifications: disposable, dumbing-down devices or respectable reference tools?

Botanists attempting to use the natural method positioned it as a better alternative to "artificial" classifications, particularly the sexual system.[713] Prior to the last decade of the 18th century, attempts at organizing plants according to a natural method (other than Linnaeus's *Fragmenta*) were also almost always published in separate books from those using the sexual system. Each technique was purported to provide the answer to its readers' plant identification and relationship-illuminating needs on its own. There was little recognition that their different main functions were best suited to different purposes.

Although earlier in the century Linnaeus had differentiated between natural classification schemes, in which the arrangements of organisms reflect their relationships with each other, and artificial ones, such as his sexual system, in which the arrangement has the primary function of helping people to identify organisms,[714] Linnaeans usually described the sexual system as not only simple to use, but also natural in some respects. Linnaeus and his followers portrayed this naturalness as a good thing. They also called the so-called natural methods confusing to use and insufficiently natural to deserve the name. Proponents of the natural method countered by describing artificial methods, and the sexual system in particular, as nothing more than mere plant identification devices. But plant identification devices were sorely needed to deal with the sheer number of plants, it was countered, and the natural method was not capable of handling them without confusion. A battle of metaphors ensued, with each side putting its own spin on the roles that a guide to the natural world should play.

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[713] Adanson 1966, Jussieu 1789.
[714] Linnaeus 1764, 6 ed., pp. [602] (i.e. Genera plantarum (Ordines Naturales, §§ 9-11).

Scaffold metaphor

One of the most prevalent metaphors used when referring to the differences between artificial and natural classifications was that of a scaffold. Linnaeus had written that the natural order was the *primum et ultimum,* or most important and ultimate goal of botany.[715] This was generally taken to mean that artificial techniques were to be used only until botanists had assembled a complete natural method. Artificial techniques would then be abandoned because they would no longer be necessary. Adanson wrote, for example, that "Methods are not science, but rather sorts of ... dictionaries, scaffolds to arrive at science."[716] And John Hill wrote that:

> There may be a thousand artificial methods devised; and though that will be certainly best, which, with equal distinctness, comes the nearest nature, yet none of them are to be valued for more than they intend, nor preserved longer than they are wanted... when they have served their purpose, let them be rejected. Though particular knowledge is useful, it is general knowledge that is great: and when the structure shall be raised, let no one wonder we throw down the scaffold.[717]

As far as Aubert du Petit-Thouars saw it,

> You must not regard it [Lamarck's *analyse*] as anything but a purely mechanical method that you must well distinguish from the knowledge that it helps us acquire. Though with its help you are brought to distinguish a great number of plants, you must be able to imitate the celebrated Conquerer of Mexico, who, landing where his ambition drove him, burned the ships that brought him there. In the same way you must abandon and try to forget the routes that you took, and no longer admit classes [of plants] other than those that appear sanctioned by Nature.[718]

Likewise, Antoine-Laurent de Jussieu called the sexual system and other "systems"

> botanical preludes, or well-arranged catalogues or non-alphabetical indexes, some more convenient than others, in which... plants are tentatively arranged in an agreed order until that happier day when, through continued meditation, they can be arranged in a truly natural series.[719]

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[715] *Philosophia botanica* § 77 (Linnaeus 1751, p. 27).
[716] Adanson 1966, p. cliii.
[717] Hill 1772b, vol. 5, p. 4.
[718] du Petit-Thouars 1811a, p. 31; less dramatically he calls it just a scaffold in du Petit-Thouars 1817, vol. 5 p. 227.
[719] Jussieu 1789, p. xxxiv, Jussieu 1994c, p. 354.

A.-P. de Candolle similarly advised students that once they had become familiar with the basics of plant identification, they should "stop using artificial methods except in difficult cases and in those in which the books arranged according to the natural order that exist today are insufficient,"[720] because "The real science of general natural history consists in the study of the symmetry proper to each family, and in the relationships among these families. Everything else is nothing but a more or less industrious scaffolding to reach this goal."[721]

Linnaeus's *primum et ultimum* aphorism was most frequently quoted by early 19th-century botanists fed up with the sexual system but still respectful of Linnaeus (or wary of offending his supporters too much). For instance, S. F. Gray also invoked Linnaeus's authority in suggesting that the sexual system is a mere stopgap to be used only until the true relationships among plants are discovered, writing that

> [The] undue extension of the sexual method is contrary even to the declared opinion of Linnaeus himself, who expressly says, he considered it only as a temporary substitute until the natural method, or that which considers the mutual affinities of plants, be so far improved as to admit of a clue being applied to it, by which the student may investigate the place of a plant in the method without any other help.[722]

He also advised readers who wanted to begin assembling a herbarium that,

> While the collection is yet in its infancy, the alphabetical order is not improper; as the student advances in the science, the artificial system adopted as a guide will be found more convenient; but he cannot too soon endeavour to become acquainted with the natural method, and arrange his collection by it: this being the ultimate goal of the science, to which the two other arrangements are merely subordinate.[723]

Similarly, John Lindley wrote that "analytical tables are mere artificial aids in investigation, to be abandoned as soon as they cease to be indispensable."[724] Simultaneous to such assertions that artificial techniques are mere scaffolds for the natural method, defenders of the sexual system referred to how comparatively easy the sexual system was for beginners to

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720 Candolle 1813, p. 66.
721 Candolle 1813, p. 206.
722 Gray 1821, p. ix.
723 Gray 1821, pp. 229-230.
724 Lindley 1830, p. vii.

use. They positioned it as an inoffensive reference tool. A few words are therefore in order about how naturalists perceived one of their most common reference tools, the index.

A few words about indexes

Indexes, as in alphabetical arrangements of names or terms, directing users to other parts of the book where those words are used, went by several different names in 17th, 18th and 19th century natural history texts. "Index" was the most commonly used,[725] but the same kind of device also went by "index alphabeticis ordinum,"[726] "table,"[727] "table raisonné,"[728] "table alphabétique,"[729] "alphabetical table,"[730] "catalogue alphabétique,"[731] "alphabetical catalogue,"[732] "table des noms",[733] "table des genres,"[734] "table des termes,"[735] "table des matières,"[736] "contenta,"[737] "Nomina botanici tomi hujus posterioris,"[738] and "concordantia botanica."[739] Sometimes, separate indexes (often in different languages) went by different designations even within one publication. "Index," however, could also refer to several different kinds of text navigation devices, such as keys or tables, e.g. "A TABLE of the DISTINCTIONS and CHARACTERS of CLASSES in the ARTIFICIAL METHOD; or

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725 E.g. "index" Johren [1710], p. 217, Johren 1717, p. 217, Linnaeus 1737a, Linnaeus 1742, Editio secunda aucta & emendata ed, Linnaeus 1743, 2 ed, Linnaeus 1752, 4 ed, Linnaeus 1753, Linnaeus 1754, "Editio quinta ab auctore reformata et aucta" ed, Linnaeus 1762-1763, 2 ed., vol. 1 and 2, Linnaeus 1764, 6 ed, Linnaeus 1766, 12 ed., vol. 1, Linnaeus 1767, Linnaeus 1771, p. 576, Hill 1770, 2 ed., vol. 1, p. 151, Hill 1771, vol. 2, p. 123, Withering and Stokes 1792, 2 ed., vol. 3, pp. v. 3 p. 499, 501, Paulet 1793, vol. 2, p. 477, Forsyth 1794, Gilibert and de la Tourette 1797, 3 ed., p. 441, Persoon 1805, vol. 1, p. 527, Persoon 1807, vol. 2, p. 657, Gray 1821, pp. 777, 779, 801, Hooker 1821, p. 293, Smith 1821, pp. 61, 217, Smith 1824-1828, pp. 341, 353, 368, Lindley 1829b, p. 312, Hooker 1830, pp. 467, 478, Hooker 1831, 2 (with additions and corrections) ed., p. 465, Smith and Sowerby 1832-1846, 2 ed., pp. 81, 85, etc.

726 Jussieu 1789, p. 454 .

727 Linnaeus 1743, 2 ed, Lestiboudois 1799, 2 ed., vol. 1, p. v. 3 p. 413, Gilibert and de la Tourette 1797, 3 ed., pp. vol. 2 p. lxix, 673, Duméril 1806, pp. 313, 331.

728 Adanson 1966, p. 411.

729 Gilibert and de la Tourette 1797, 3 ed., p. v. 1 p. xxviii, Dubois 1803, pp. 567, 576, Lamarck and Mirbel 1803, vol. 2, pp. 515, 549, Candolle 1813, p. 499.

730 Boerhaave 1719.

731 Seringe 1830-1832, pp. 337, 341.

732 Linnaeus 1787, vol. 1, p. 809.

733 Lamarck 1778, vol. 1, pp. v. 3 pp. i, xi, Bergeret 1783-1784, vol. I-III, p. 241, Paulet 1793, vol. 2, pp. v. 2, p.468.

734 Lestiboudois 1781, p. 315.

735 Gilibert and de la Tourette 1797, 3 ed., pp. v. 1, p. xlvii.

736 Lamarck and Mirbel 1803, vol. 2, pp. vol. 15, p. 113.

737 Linnaeus 1751.

738 Gunnerus 1772, p. 149.

739 Wolf 1776, p. 1776

INDEX of PLANTS,"[740] "Index methodi ordines naturals complectentis,"[741] "Index generum, familiarum, specierum plantarum fungosarum,"[742] and "Linnaean index,"[743] or any unspecified technique for referring to stored information.[744]

Indexes were only rarely discussed as parts of the plant identification apparatus in their own right. Even then, the comments were not so much in praise of good indexing practice as complaints that otherwise valuable natural history texts were made harder to use when they lacked an index.[745] Though many botanists seemed to have ambivalent feelings about indexes of any kind, indexes were common in botanical texts and seemed popular enough with readers. Indexes were simultaneously valued as tools to direct users to information about plants, and despised because they enabled people with only superficial "index learning" to pass for those who had studied material, thought about it, and come away with real knowledge. Anything that enabled index learning made common what had formerly been the near exclusive bailiwick of scholarly gentlemen. It was vulgar and threatening.[746] Adanson was explicit about what he thought the vulgarization of botany was doing. The study of botany, he wrote, is naturally "restricted to a small number of ... creative geniuses" who alone are capable of extending the progress of the science. The widespread use of index- or dictionary-like artificial methods, however, has "spoiled the geniuses made to go deeply, and has dragged them down to the most common tastes, which lead only to brushing against the surface of things, so that now only with difficulty can one distinguish the profound man, the learned man, from he who merely has the veneer [*écorce*] and the appearance of one."[747] Yet, despite this threat, it was difficult to deny how useful actual indexes were – Adanson's *Familles des Plantes*, for instance, has three.

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740 Hill 1770, 2 ed., vol. 1, p. 45.
741 Jussieu 1789, p. lxxi.
742 Paulet 1793, vol. 2, p. v. 2 p.477.
743 Smith 1832, vol. 2 p. 591.
744 E.g. Fleming 1822, p. ix.
745 E.g. Smith 1832, vol. 2 p. 467 and 591, H. E. S. and J. D. 1833, p. 437.
746 Botanists were not the only ones concerned with index learning. Lund (Lund 1998) discusses its perception in contemporary literary sources.
747 Adanson 1966, p. cliii.

Further comparisons of artificial systems to "mere" indexes reflect this ambivalence. For instance, John Hill wrote that "it matters not how an artificial System is formed: it is an Index, and little more,"[748] and Antoine-Laurent de Jussieu described the sexual system as

> an analytical table ["table raisonnée"] where plants are arranged following conventional features that allow Botanists to understand one another. It has been adopted by many of those who would prefer more to follow a ready-made order, rather than to think of a new one or to reform older ones.[749]

In other words, Jussieu suggests that the sexual system is for people who prefer index learning to a real consideration of nature. But though Jussieu continued in calling the sexual system "a prelude to Botany or a methodical index," in his *Genera plantarum* of 1789, he added in a footnote that "Just as in any book, an index conveniently and briefly reveals the arrangement of the contents, so a systematic order, or methodical index, is useful in botany, since it produces an easy distribution of plants in which what is wanted is very quickly found."[750]

Jean Senebier (1742-1809), early plant physiologist and librarian (!) in Geneva, similarly started out by saying that "the study of a classification is very long, very annoying, very useless; it is an enormous labour to learn by heart a long *Index* that does not even yield complete knowledge about the material of the work of which it is the table."[751] But over the course of his career, and likely because of discussions he had on these matters with Augustin-Pyramus de Candolle,[752] Senebier had a change of heart about the value of nomenclature, classifications and other information-finding tools. By 1802 he suggested that not only "natural history cabinets or libraries ... are more or less complete indexes to the great book of nature that it is also very important to see and to get to know"[753] but that the

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[748] Hill 1770, 2 ed., vol. 1, p. 30.
[749] Jussieu 1994b, p. 177, Stevens 1994, p. 297.
[750] Jussieu 1789, p. lx, Jussieu 1994c, p. 380 and note 181 on page 518.
[751] Senebier 1775, pp. 35-36.
[752] Lamarck had introduced the two by giving Candolle a book to take to Senebier in Geneva. Candolle met Senebier in the spring of 1797, and though he found Senebier's writing "diffuse, incoherent, without clarity or style," he appreciated the importance of Senebier's work on plant physiology and was touched by his open-heartedness. The two became friends and corresponded to the end of Senebier's life (Candolle 2003, pp. 91, 94).
[753] Senebier 1802, 2 ed., p. 348.

act of indexing can illuminate previously undetected relationships among the objects being arranged. "Nomenclatures," he wrote,

> are nothing but indexes, more or less good, more or less extensive; they are the threads that unite the parts of nature, but they cannot be considered as means of explaining it. It is, nevertheless, not impossible that these indexes serve this use, by bringing together objects that one would never think to see together.[754]

Playing around with indexes or nomenclature in this way could be like idly flipping through an encyclopaedia, a sort of textual brainstorming that could lead to interesting ideas to pursue. Generally, however, botanists and other naturalists still believed that information was best organized in a manner that reflected its meaning. By the second and third decades of the 19th century, for instance, proponents of the natural method in Britain described people who clung to the sexual system because it was easy to learn as lazy, dull, and uneducated[755] – index learners under a new guise.[756]

Natural arrangements should be easy to navigate

The true extent of the prejudice against indexes and book learning that existed in the 18th and early 19th centuries can be appreciated when it is considered that, not only was the natural method seen as arrangement of plants best suited to memorization and understanding, but also to navigating textual information.

For instance, Sir James Edward Smith suggested that Jussieu's natural method was difficult to use even for experts, since "neither he nor any other mortal has a perfect clue" about the true relationships of plants. Jussieu's reluctance to describe unequivocally how particular plants are related "greatly interferes with the practical use of his book, except for

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754 Senebier 1802, 2 ed., pp. 61-62.

755 Lindley n. d. (between 1830 and 1845), 2 ed., pp. v-vi.

756 Though Lindley was extremely vocal in his disdain for the sexual system, he was one of the biggest proponents of cross-references in analytical tables (keys). In his *Natural system of botany* (1836), which was a much-reworked second edition to his *An introduction to the natural system of botany* (1830), he wrote, "The analytical table has been entirely reconstructed, and I trust upon a better principle. One of the great faults of that prefixed to the first edition consisted in no provision being made by it to meet cases of exceptional structure. This has now been attended to" (Lindley 1836, 2 ed., p. xiv).

the learned." Beginners would be forced to use his index to find the plants they sought.[757] This was clearly a mark of failure on Jussieu's part, if the natural method were to be self-evidently the best source for everything concerning human knowledge about plants.

As late as 1822, the naturalist the Rev. John Fleming, though admittedly somewhat backward, still boasted that in his *Philosophy of zoology,* "The analytical Table of Contents exhibits so fully the method which has been followed, and the subjects which have been treated of, as to supersede the necessity of an Alphabetical Index."[758] Fleming clearly subscribed to the idealistic belief that what he construed as the natural method would make information easy to find on its own.

Thus, the alphabetical index, while indispensable to finding information quickly, remained in the shadows of arrangements of plants that were based on plant features rather than plant names. Quite aside from its practical benefits, many botanists saw the presence of an index as an admission that the arrangement followed in the rest of a book was far from perfect. It was almost like a third arm – a useful blemish. Meanwhile, the sexual system was lumped in with alphabetical arrangements as "at best ... but an index to nature."[759] The changing use of alphabet, code, and dictionary metaphors in the natural history community reflects the same beliefs, as, gradually, anything other than the natural method became seen as a "mere" text navigation device.

Alphabet, code, dictionary and table of contents

Initially, the alphabet, code and dictionary metaphors were used by defenders of the sexual system to demonstrate not only how easy it was to use, but what foresight Linnaeus had employed in devising a scheme that could decode the imprints that God had impressed in every plant. Invoking the code metaphor therefore helped forward the cause that the sexual system was based on natural principles. By the turn of the 19th century, however,

[757] Smith 1832, vol. 2 pp. 571-572.
[758] Fleming 1822, p. xvii.
[759] Dr. Lardner's Cabinet Cyclopedia. Natural History. 1. On the geography and classification of animals. By W. Swainson, Esq. -- 2. Classification of quadrupeds. By W. Swainson, Esq. 1836, p. 547.

when the natural method was making huge headways, such claims would have appeared ridiculous. Botanists who employed the dictionary metaphor dropped the code component. 19th-century botanists instead expanded the dictionary and table of contents metaphors to show exactly how the sexual system could be used to look up a plant by examining particular features in turn, emphasizing its ease of use in practice.

Linnaeans knew that Linnaeus had discussed writing plant descriptions in terms of using a botanical code or alphabet. This floral alphabet described the functions of the flower and fruit in sexual reproduction, with an emphasis on the organization of the parts. The idea was that the very organization of flower parts allowed reproduction to be mechanically possible.[760] This view was substantiated by, for instance, Colin Milne, who wrote that the sexual system "would be absolutely unintelligible" if it turned out that stamens and pistils do not perform sexual functions in flowering plants.[761] Whether the sexual system was fundamentally based on deeper truths about plant physiology was, however, of little importance to Benjamin Stillingfleet (1702-1771), who claimed simply that it was the best for identifying plants.[762] But like many proponents of the sexual system coming after him, he had to address critics' assertions that the system is difficult to use because obscure features sometimes have to be examined in order to fit plants into its classes and orders. Stillingfleet brushed these concerns aside by saying that, in such cases, the sexual system "is obscure merely from its consonancy to nature. If Providence has thought fit to write in cyphers, shall he be blamed who endeavors to give a key to its works, because some men cannot distinguish one stroke from another in the cypher?"[763] In Stillingfleet's opinion, Linnaeus

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[760] Müller-Wille 2005, pp. 90-91.
[761] Milne 1770, p. 189.
[762] Benjamin Stillingfleet (1702-1771) was born in Norfolk and educated in Cambridge. He published English translations of six of Linnaeus's essays, plus some of his own work on grasses, as *Miscellaneous tracts relating to natural history, husbandry, and physick*, in 1759. This work was, according to Sir James Edward Smith, one of the most, if not the most, important English-language works to have diffused the ideas of Linnaeus (Smith cited in Henrey 1975, vol. 1, p. 120).
[763] Stillingfleet 1977, 3 ed., pp. xxiii-xxv.

had devised a system that could decode patterns in nature, and ease of use was secondary to preserving the impression that the system was both the most useful and the most "natural."

John Hill, both a proponent and critic of the sexual system, was too well aware of its flaws to suggest that Linnaeus had read God's code. Hill believed that "a universal dictionary to the vegetable kingdom" would be something excellent for botany, "equally useful to the young beginner, and the real Botanist for where is the man blest with a sufficient memory to retain the marks of so many thousand Vegetables." But the sexual system was not of this calibre. Although it was "an alphabet, a new language, for this delightful science," it was a poor one, full of exceptions to its own rules.[764]

Colin Milne, Jean Emmanuel Gilibert and Louis Claret de Fleurieu de la Tourette, and A. M. C. Duméril all re-examined the sexual system's requirement of examining plant parts in a particular order when they compared the steps taken to look up a plant in the sexual system to those used when looking up a word in a dictionary. "This method of proceeding," wrote Milne, "is similar to that which is observed in turning over a dictionary, where, in searching for a word, as SPACE, we first look for the letter S, next P, then A, and so successively the C and E. S may represent the class; P the order; A the genus; C the species, and E the variety." Or, as Duméril put it, "Every plant features its character written in its flower. You have to spell it out, so to say, and bring the flower successively to its class, order, genus, and species, and for that, you sometimes have to do 8 or 9 successive searches."[765]

In these works, the emphasis on the stepwise path to plant identification indicates an increased understanding of the particular features of the sexual system that make it useful for that purpose. In works subsequent to the ones mentioned above, both Milne and Duméril wrote that analytical hierarchical systems and the natural method are like dictionaries, one

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[764] Hill 1771, vol. 2, pp. 42-43.
[765] Milne 1770, p. METHODUS, Gilibert and de la Tourette 1797, 3 ed., p. 13, Duméril 1804, p. 63; see also Milne 1771, p. 163 for a similar statement.

arranged alphabetically and the other arranged thematically or etymologically, according to the meaning of the words. Wrote Milne,

> Artificial methods are to the natural, what the alphabetical dictionary is to the etymological. The characteristical marks of the classes correspond to the letters of the alphabet, and the order of their succession: the natural families and the order of their arrangement, to the etymological arrangement of words. The analogy is strong; and the difference, in point of facility, may be explained, in both cases, on the same principles.[766]

S. F. Gray went into even more detail on this point, penning a long discussion of the sexual system and natural method in terms of how far using each one to look up information about a plant resembled using each kind of dictionary, respectively. He saw the natural method (and topical dictionaries) as insufficient guides for looking up information when an isolated plant specimen, or "the letters of the word are, by hypothesis, the only guide."[767] Gray suggested that the sexual system is useful only as an index, or "finder" of information about plants, and a disposable one at that: it was to be discarded once the natural method was better understood.[768]

Similarly, John Fleming wrote that "the true value of the 'Systema Naturae' of Linnaeus" is its function as a "well-arranged *table of contents of the book of Nature*, where the student will find that every *entry* refers to a *species*," but that the actual book to which the table of contents refers does not yet exist because not enough is known about the physiology, habits, and other aspects of living things.[769]

Smith

The way that Sir James Edward Smith used the dictionary metaphor in the 1820s and 1830s shows his struggles to maintain a place for the sexual system in scientific botany, which was increasingly embracing the natural method. In his *Grammar of Botany* (1821), Smith finally had to admit that not only was Jussieu's natural method here to stay, but it was

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[766] Milne 1771, pp. 31, 164-165, Duméril 1806, p. xvi.
[767] Gray 1821, pp. ix, note.
[768] Gray 1821, p. viii.
[769] Fleming 1829, p. 302.

much better than the sexual system was at keeping similar plants together. "Natural affinities," Smith wrote in his introduction, "cannot now be overlooked, by those who contemplate the Vegetable Kingdom with any degree of philosophical attention."[770] If he did not give in and discuss the natural method in his book, he would be seen as hopelessly old-fashioned. But all was not lost for Smith. He still claimed that the sexual system was the best at something – identifying plants. The sexual system might be "understood merely as a dictionary, to enable him to make out any plant that may fall in his way,"[771] but it was the best dictionary available, whereas he characterized the attempts to date at tackling the natural method as preliminary and tentative, declaring "There is scarcely a principle which can be assumed as universal, or without exception, in Natural Classification."[772] In fact, Smith suggested, the natural method was so full of confusing and conflicting information that the sexual system, in its function as a dictionary, was essential for anyone attempting to understand the natural method. Smith emphasized these points in his introduction, writing, after he characterized the natural method as incomplete,

> It is evident that no such mode of classification can, at present, serve the purposes of analytical investigation, to make out an unknown plant. That is the exclusive object of the Artificial System of Linnaeus, which, of all the schemes hitherto contrived, is alone, perhaps, universally applicable to the end in question.

But Smith also wrote that Linnaeans and Jussieuans should try to get along and work together, for "A dictionary quarrels not with a grammar, nor a history with a chronological table. It is pernicious, as well as foolish, to set them at variance."[773] He discussed similar ideas of reconciling and pairing the sexual system with the natural method in his *Review of the modern state of botany* (1832), explaining, that, unlike Linnaeus,

> The authors of most plans of botanical classification have ... seldom considered the questions of natural and artificial arrangement, as opposed to each other. The system of every such author seems to have appeared to himself the most consonant to nature, as well as the most convenient in

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[770] Smith 1821, p. x.
[771] Smith 1821, p. ix.
[772] Smith 1821, p. 33.
[773] Smith 1821, pp. xiii-xiv.

> practice; yet nothing betrays a more absolute incompetency to the subject than such an idea.

God was the only one who could possibly understand all of nature, therefore, nobody could claim to have truly understood the natural method. Just the same, teaching botany without either the natural method or the sexual system would be just as ineffectual as teaching "composition without a grammar, or philology without an alphabet." Though neither artificial nor natural methods were perfect,

> they may combine their powers, and cooperate in instruction. The one may trace an outline which the other may correct and fill up. The first may propose, and the second elucidate; the former may educate and improve the memory and observation, for the use of the latter. When they oppose each other, their several defects and weaknesses appear; by mutual assistance they strengthen themselves.[774]

In other words, Smith was certain that there was still a place for the sexual system in modern botany. It would simply go from being the only classification worth considering to a means of interpreting the otherwise confusing natural method.

By the second and third decades of the 19th century, some naturalists were writing of hierarchical arrangements of organisms in general as dictionaries, without even referring to the sexual system. Cuvier, for instance, compared hierarchical arrangements of living things based upon features of the things themselves to "a dictionary, wherein the properties of things are an index to their names, being the reverse of ordinary dictionaries, in which the names are given, as an index to their meanings or properties,"[775] while Kirby and Spence wrote that

> A good system of insects containing all the known species, arranged in appropriate genera, families, orders, and classes, is in fact a dictionary, putting it within our power to ascertain the name of any given insect, and thus to learn what has been observed respecting its properties and history as readily as we determine the meaning of a new word in a lexicon.[776]

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[774] Smith 1832, vol. 2 pp. 564-565. N. B. how this echoes Milne 1770, p. METHODUS (note 374 above).
[775] Cuvier 1836, p. 3. Cuvier's *Le règne animal* was first published in 1817, and Cuvier died in 1832. I have seen only the 1836 English edition of this work.
[776] Kirby and Spence 1822, 4 ed., vol. 1, p. 46.

John Lindley hammered the final nail in the coffin of the sexual system when he indicated that, even as a text navigation aid, it was too full of exceptions to be worth using at all:

> if a system of Botany is to be nothing more than a contrivance to help those who will to master the elements of the science, to determine the name of a plant; and if it is really necessary to have a mental rail-road on which such persons may be impelled without any exertion of their own; then indeed the analytical tables of the French are infinitely better contrivances than the Sexual System: because if well executed they meet every case and lead with certainty to positive results.[777]

All of these instances show that naturalists had moved from considering the sexual system as the be-all and end-all of plant arrangements to acknowledging that it is one of many schemes with both good and bad features, and that its hierarchical component is responsible for its ability to allow users to look up the names of plants.

Filum ariadneum (Ariadne's thread) through the labyrinth

Another favourite metaphor used by botanists in their discussions of what methods and systems were good for is the labyrinth and Ariadne's thread leading out of it. Just as with the dictionary metaphor, the ways in which it was used changed according to what the major concerns in natural history were at the time.

Although James Larson 1971, p. 151 wrote that the labyrinth, cryptogram and alphabet metaphors used by Linnaeus and other Enlightenment writers are symptomatic of "thinkers convinced of the fundamental rationality of nature," the authors may simply have been expressing their frustration at the confusing multitudes of things to know. I have found that, while "filum Ariadneum" (Ariadne's thread) metaphors usually imply a rational approach to figuring out a huge, yet somehow understandable universe, labyrinth metaphors on their own often express a sense of being overwhelmed by disorder – particularly disorder generated by people's haphazard way of arranging things, such as books, or information within books. For instance, Rivinus wrote that Christian Mentzel's compilation of a "universal index" to the names of plants offered, "as it were, Ariadne's thread to those who

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[777] Lindley n. d. (between 1830 and 1845), 2 ed., pp. v-vi.

want to enter the Botanical labyrinth."[778] Mr. Samber wrote in his preface to Boerhaave's *Method of Studying Physick* (of which a large part is devoted to reviewing botanical texts) that a good "Method of Study" is of utmost importance in finding one's way through the "Multitude of Books." Studying the right books in the right order would avoid time wastage and mere superficial understanding of the material. The use of improper study habits, however, "has crowded People's heads with an Infinity of unshapen confused Ideas, and has plunged them into such endless Labyrinths that they have not been able to extricate themselves as long as they lived."[779] Jean-Jacques Paulet similarly complained that large, decorative illustrated works about fungi, in which the authors paid no attention to overall organisation, are a labyrinth.[780]

The ancient metaphor found new life in broader botanical arguments after Linnaeus used it in his *Philosophia botanica* (1751), writing that "a system is the *filum Ariadneum* of botanists, without which botany is chaos."[781] Many botanists invoked this idea during passionate debates about the relative merits of the sexual system and the natural method during the early 19th century. Several shared Linnaeus's opinion, and described the organization that classification and nomenclature bring to the study of living things as a *filum Ariadneum*, e.g. Sir J. E. Smith and J. E. Bicheno.[782] And though Candolle lamented, "What guide can we find in this frightening labyrinth?" of around 60 000 living species of plants, he immediately provided the answer: the regular structure of botanical taxonomy, of course.[783] In this, he was possibly building on Antoine-Laurent de Jussieu's statement that synonymous names in botany "function like Ariadne's thread, revealing the labours of those gone before, and exposing errors to be avoided."[784]

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778 Rivinus 1690, p. 40.
779 Boerhaave 1719, p. unpaginated preface.
780 Paulet 1790, vol. 1, pp. ix-x.
781 Linnaeus 1751, pp. 98, §156. Similarly, Leibniz frequently described his proposed philosophical language as the *filum Ariadneum* that would be an infallible guide to thought (Maat 2004, p. 303), but it is doubtful that the metaphor came into natural history through him.
782 Bicheno 1827, p. 485, Smith 1824-1828, p. 354.
783 Candolle 1813, pp. 23-24.
784 Jussieu 1789, p. xxvi cited Stevens 1994 p. 345.

Exactly which approach to systematic botany constituted the best *filum Ariadneum* was not always clear. Julien-Joseph Virey remarked that not only is synonymy in natural history a labyrinth,[785] but that the study of innumerable species is doomed to be a labyrinth if "the route indicated by nature" is abandoned.[786] Virey, however, also wrote that an artificial system, such as the sexual system, directs one through the "immense labyrinth" of the multitudes of creatures.[787] A. M. Constant Duméril was likewise ambiguous about the best way to get through the labyrinth of the sheer number of natural productions;[788] he showed students how to find information about plants using both the sexual system and Jussieu's natural method. William Kirby similarly went wild with ambiguous metaphors when he wrote that, "If it is our wish really to trace the labyrinth of nature, we can only accomplish it by a careful perusal and examination of her various groups." This involved paying close attention to characters in specimens, for "all characters ... within certain limits lead us right, and are an index to a natural group." But if naturalists put undue influence on any character rather than letting nature guide them, they would become "perplexed in the mazes" of an inadequate and misleading artificial classification.[789] By the early 19th century, it seemed, though the shape of nature was acknowledged to be complex, it was also recognized as the real standard against which arrangements of plants were to be judged. And while the techniques to understand nature were necessary, they in turn grew to be complicated enough that both nature and classifications of natural productions became seen as at once both the problem (labyrinth) and the solution (thread). For instance, Aubert du Petit-Thouars called the natural method "an inextricable labyrinth" without Ariadne's thread of an artificial arrangement.[790]

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785 Virey 1817, vol. 13 [GEN-GUE] p. 6.
786 Virey 1818, vol. Tome XX. [MED-MIN]. p. 478.
787 Virey 1819, vol. XXXII [SPH-TAZ] p. 329.
788 Duméril 1804, p. 2.
789 Kirby and Spence 1828, 5 ed., vol. 4, p. 400.
790 du Petit-Thouars 1817, vol. 5 p. 226.

John Lindley took the labyrinth metaphor in a new direction when he wrote that "a beginner ... is often fairly lost in a labyrinth of resemblances, differences, and exceptions" of the natural system. Unlike his predecessors, he did not present anything as a particularly simple thread to follow out of it. Instead, he recommended a careful study of the basics of how plants are related, which he went on to describe.[791] In the same text, Lindley had called the sexual system a "contrivance" and "mental rail-road"[792] – something to take passengers from one place to another, but which is not conducive toward getting to know the regions in between. The sexual system was recognized as, at best, good only for naming plants, despite the groups of related plants that it sometimes kept together. As mentioned above, Lindley believed that its numerous exceptions meant that it was not even the best way to find out plant names. The implications are that while the diversity of plant life may still be overwhelming to beginners, a novice who followed a sensible program of study no longer needed to cling blindly to a thread to get out. Rational consideration of empirical data could turn anyone into a Daedalus.

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[791] Lindley n. d. (between 1830 and 1845), 2 ed., p. iv. In the introduction to his *Synopsis of the British flora* published several years earlier, Lindley said the same thing but without the metaphor of the labyrinth:

> Whatever the difficulties may be of becoming acquainted with plants according to this [natural] method, they are inseparable from Botany, which cannot be usefully studied without encountering them. A mineralogist may as well complain of the necessity of a blowpipe, or a chemist of the infinite variety of apparatus which he is compelled to employ, as a botanist of the microscope and dissecting knife. It would, undoubtedly, be more convenient, if knowledge could be acquired with greater facility; but we must take things as we find them, and submit patiently to the difficulties of the road we are forced to pursue (Lindley 1829b, p. xi).

Earlier on the page he introduced identification keys ("analytical tables") as part of the botanist's toolkit.

[792] Lindley n. d. (between 1830 and 1845), 2 ed., pp. v-vi.

Chapter 6. Botany and logic

A number of the most interesting questions I encountered during the course of this project are directly tied to problems of botany and logic. I do not here refer to the well-meaning but hopelessly naïve attempts to reconcile hierarchical arrangements of plants with class logic or symbolic logic that keep getting published.[793] Rather, most of the logical problems of botany have to do with synonymy. Synonyms have a special place reserved for them in Taxonomy Hell because they multiply the number of plant names. This compounds the problems caused by too many plants known to science I discussed earlier. Botanists widely recognized that eliminating synonyms would do many great things for them. It would cut down the number of plant names in circulation, clear up priority disputes, consolidate partial descriptions so that poorly known species could be understood better, and reduce the amount of time botanists had to spend tracking names – something that everyone agreed was a tedious bore.

Synonyms come about for practical reasons: first, not everyone becomes aware of the existence of a given plant at once, and, second, identifications are not always precise. One way to solve the first problem would be to somehow broadcast the name and a description of a plant around the world once it had been described and named. But during the 18th century, there was no central registry for names, no formally codified rules about who can name what, based on how partial or complete a specimen, etc., not to mention slow transit times of books, letters and journals to the totality of the community of naturalists inhabiting every continent save Antarctica. Eventually, naturalists' frustration with these very issues culminated in the establishment of codified rules governing nomenclature, the ICBN (for plants) and the ICZN (for animals) – though the transmission of this information still lags to

[793] Of which there are unfortunately many, e.g. Davidson 1880, Gregg 1954, Arabie and Hubert 1992, Priss 2003, , Roberty 1968and others. There is no book or article by a botanist, to my knowledge, with "logic of botany" in its title, though there are at least two called "grammar of botany," (Smith 1821 and Loudon 1829). This is just one of many hints that legitimate botanists sought generative rules subject to exception – not absolute rules – to organize their material.

this day for those without an internet connection, and brawls are still more common at systematists' meetings than they probably should be. But until nomenclatural rules first began to be codified formally, with the Strickland rules of 1842,[794] natural history was an especially vibrant free-for-all of interesting ideas and experiments to bring synonymy and related problems under control. Arrangements based on features inherent in plants are one way to address the problem of synonyms being generated when botanists find what they think are "nondescripts," or plants that had never been described.

Arrangements based on inherent features: ease of use versus simple or logical structure

One such experiment that attracted a certain amount of attention in the 18th century was combinatorial plant names. These were names derived from coded combinations of plant features, designed to prevent synonyms from being formed in the first place. With such a system, a nondescript would get only one name, regardless of who was naming it. Useless, time-wasting synonyms would not have a chance to be created. I will discuss how this kind of nomenclature works and how such attempts fared later on. For now, it suffices to say that they did not perform as planned. Although some isolated people were still promoting combinatorial plant names into the 19th century, the majority of botanists and even lay people by that time were cynical enough to greet proposals of this type with sarcasm. For instance, a notice in the *Athenaeum*, a popular magazine, stated, "We understand that some German botanists are labouring at the invention of cabalistic characters for plants ... we suppose the Algebraic botany will be called a natural system!!"[795] The two exclamation marks leave no doubt about how many combinatorial schemes and so-called 'natural systems' the public had seen come and go before. However, as I discuss below,

[794] McOuat 1996.
[795] Algebraic botany 1828.

combinatorial descriptions of plant features not tied to nomenclature did find a more receptive audience among botanists of the 18th century.

The other approach, making descriptions more precise, was based on what was generally acknowledged to be the natural method. When materials are arranged by subject, or plants are arranged according to some physical features that they have in common, as in the natural method, it is possible for someone with only a vague idea of what he or she is looking for to execute a local search in part of a text that contains information on plants similar to those he or she wishes to find out more about. Because the natural method keeps natural groups together, it is usually necessary to compare only a small number of plants differing in small numbers of features at any given time in order to narrow down an identification. (Large taxa excepted). The relative uniformity of natural groups helps make identifications definitive. And if the identification process were definitive, no new synonyms would be made because of confusion about a plant's identity. Old synonyms could be eliminated over time, and the future of botany would be secured. This is the process that most botanists used, and it still survives today. However, in the 18th century and early 19th century, it was not uncommon to add a twist to the procedure. Instead of focussing only on keeping similar plants together, which was taken for granted, some men paid extra attention to picking out differences one at a time in order to make the process of identification simpler. The simplest way to pick out a difference is to contrast two things: a dichotomy. Several different men experimented with dichotomous, or binary divisions of plants, as they were also known.

Of course, there were problems with this approach. Chief among them was that, even if the overall arrangement of plants follows the natural method, keying out a plant by working through successive dichotomies highlights particular traits in turn. It shifts attention from the gestalt impression of plants that the natural method without such fine divisions fosters. This makes the system more difficult to use since it is harder to mentally separate plant traits and deal with them one by one than to think of where a plant belongs according to the overall impression it makes. Furthermore, some of the traits highlighted may not be noticeable to anyone but experts. All of these considerations make what seems initially like a sure-fire, simple approach to a complex problem into a logistical horror show in practice. Rampant dichotomous divisions work against the senses that people automatically rely on when they try to identify plants. Schemes based upon these principles are a disaster for organizing large numbers of plants because of dichotomies' low error tolerance and tediousness.

All of these issues highlight difficulties that botanists faced when they tried to apply logically generated descriptions or algorithms to the results of natural processes. The specific problems, however, were different for combinatorial plant names and for the use of dichotomies to arrange botanical information. I will discuss the history of dichotomous arrangements of plants first.

Origins of topical (natural) arrangements in books

"Natural" arrangements of plants correspond in function to topical arrangements of information in other fields of knowledge. Topical arrangements in other fields, such as cosmology or the study of the relationships among fields of knowledge, are older than the natural method in botany. They were often closely linked with ideas of how the universe was seen to be organized. Just as in 18th century botany, the more a scheme reflected the order of the universe, the easier deriving it from first principles, memorizing it and understanding it were assumed to be.

Topical arrangements of information in fields of knowledge other than botany even predate alphabetical arrangements.[796] In the Middle Ages, a particular kind of topical arrangement of knowledge became popular for the first time, one based explicitly on how their creators believed the world to be organized. During this period, alphabetical

[796] Hüllen 1999, p. 15.

arrangements were uncommon enough that authors often prefixed explanations of how to use them. Instead, medieval scholars preferred to organize their books upon what they saw as the same "logical" principles that God had used when He created the harmonious universe. The linear order that alphabetical organization implied conflicted with the assumption that the most important information should be presented first. It made no sense to put *filius* [son] before *pater* [father], or *angelus* [angel] before *deus* [God]. Alphabetical order was seen by advanced scholars as a sop to less-educated readers, and they disdained it until its usefulness as a data management tool became apparent to them during the 14th century.[797] Until then, "an author who arranged materials on the illogical, or rather nonlogical, basis of the alphabet," the book historians Mary and Richard Rouse wrote,

> seemed either to deny the logical relationships, or to confess himself incapable of perceiving them.... even in cases where alphabetization would have been advantageous, it was rejected, or, more accurately, it was not considered as a possibility.[798]

The texts Rouse and Rouse described were mostly theological encyclopaedias, commonplace books and concordances. That is, they were intended to present a comprehensive overview of the universe of knowledge based on philosophical principles. Ramus's works were similarly, for the most part, classifications of knowledge, for he and his followers believed that "the chief business of all classification is the classification of the arts and sciences themselves; such classification is the starting point of all philosophy."[799] The most philosophical arrangement of subjects was also assumed to be the easiest to learn.

Although such arrangements "dominated the field" in the Middle Ages and to the beginning of the Renaissance, there remained other genres of texts organized along different lines. These included "wordlists, such as dictionaries, lexicons, *herbals*, lapidaries, medical recipes, mnemonic verses (*all items that defied logical classification*)."[800] Nevertheless, even in "logical" works, the order in which topics were addressed (not to mention how the topics

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[797] Rouse and Rouse 1991, p. 241.
[798] Rouse and Rouse 1991, pp. 240-241.
[799] Ong 1958, p. 197.
[800] Rouse and Rouse 1991, p. 241. Emphasis added.

were partitioned) was very author-dependent.[801] Although such texts usually shared many common features – for instance, in classifications of everything, God came first, followed by the angels – each one was different, because every author had his own ideas about the scope and relative importance of topics and the order in which they should be best addressed.[802]

These logically-organized texts often included lists of chapter titles, which functioned as an early form of tables of contents. Often these tables were presented in curly brackets. Since the books were organized logically, these lists, in which the order of the chapters was preserved, were generally presented logically as well,[803] though not always strictly so. The works of Ramus and his followers, discussed below, follow this pattern.

Navigating logically-arranged texts

Schemes based on this kind of *a priori* or top down approach could confuse readers, especially when following the author's idea of how the universe was arranged was the only way to access the information in it. This problem is inherent in all hierarchical schemes. It can be remedied by including alternative means for finding information in the same work. An alphabetical index serves this purpose very well: it circumvents the need to know the details of an author's classification scheme in order to find information.[804] As one astute 19th century botanist noted, and 21st-century scholars agree, the easiest and fastest way to get to an entry in a topically-arranged book is still usually the alphabetical index.[805]

Problems of this nature have been recognized since at least the 17th century. George Dalgarno, in his *Art of Signs* (1661) – a work devoted to arranging information about the world in a manner best suited to memorizing it – struggled extensively with the difficulties of representing concepts in meaningful and useful ways. He became convinced that, contrary to popular belief,

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801 Hüllen 1999, p. 14, Freedman 1993.
802 See Freedman 1993 for many examples of Ramist works of this type.
803 Rouse and Rouse 1991, p. 241.
804 Hüllen 1999, p. 15.
805 du Petit-Thouars 1817, vol. 5 p. 230, Hüllen 1999, p. 27.

> an accurate analysis of all notions of *nature* and *art,* which are extremely *complex*, resolving them into their first elements, and all the mutual relations of these *simple* elements....is of little use for the immediate apprehension of a notion as a single unified whole, by a single act of the mind, without requiring much mental ado.[806]

Dalgarno's scheme was supposed to contain items arranged so logically, and named in accordance with that logic, that their names would provide definitions or at least descriptions of them. Nevertheless, he recognized that it would still be easier and faster to memorize the terms and where the items belong than it would be to go through the classification step-by-step.[807] That is, *even proponents of logical classifications recognized that logical principles of organization on their own were not sufficient for finding information quickly*. This was evident in the sexual system and other schemes like it that had a top-down analytical component coupled with natural groups (in this case, species and genera). "Logical division" was rarely used: natural groups were often kept together at the expense of a strictly nesting hierarchy, as I described earlier. This made the "shadow system," as detailed above, valuable both as a way to find anomalous plants and as a way to avoid the tediousness and error-prone work of keying any plants out through the hierarchy. This discovery that a logical pathway does not guarantee swift or certain results in practice turned out to apply even to the "purest" form of logical classification, that of dichotomy. In fact, when botanists justified using dichotomous divisions to facilitate finding information about plants, they also recognized that the number of dichotomous divisions necessary to reach a plant when the number of plants in the arrangement was large and many plants were anomalous would make identification tedious, would strain the memory, and, as such, could lead to errors based on inattention. They therefore restricted contradictory bifurcating arrangements to relatively small numbers of plants and/or recommended the use of shortcuts. This effectively undercut the logical rationale behind using dichotomous divisions.

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[806] Dalgarno 2001, pp. 191 (pp. 32-33 in the original).
[807] Dalgarno 2001, pp. 221 (pp. 58-59 in the original).

Dichotomy

Tree of Porphyry

Dichotomous divisions of knowledge have been around since antiquity.[808] Plato, for instance, gave an example of a way to describe angling (fishing) in terms of its place in a dichotomously divided arrangement of the arts.[809] Aristotle discussed similar principles in his *Categories*. In the third century CE, a man called Porphyry wrote a work called *Isagoge* in which he explained Aristotle's nested categories of genus, species, difference, property and accident in simple terms. The *Isagoge* became a standard of scholastic education for centuries, and Porphyry's explanation of the categories – showing how Socrates is a man, a man is a rational animal, animals are sensitive living things, living things are animated bodies, and so on, up to bodies being corporeal substances – became known as the Tree of Porphyry.[810]

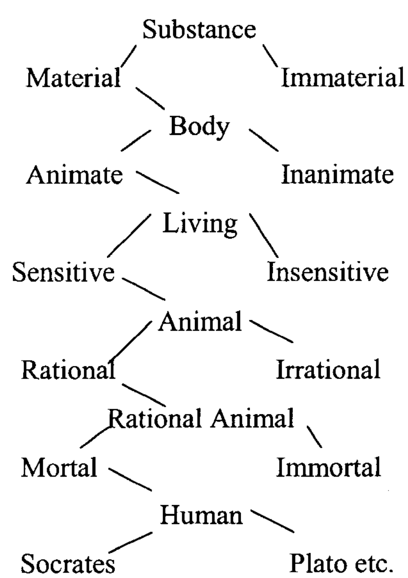

Figure 25. The Tree of Porphyry (from Hacking 2003).

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[808] E.g. Denizot 1997, p. 229.
[809] Panchen 1992, p. 18 provides Plato's description mapped onto a cladogram-like structure.
[810] Cram and Maat 2001, p. 36. The Tree of Porphyry actually begins with substance and works down to man. I have reversed the order in the text here for simplicity.

Later authors often illustrated Porphyry's example along the trunk of a tree.[811] This language of "trees" has fed into the association of logic with botanical matters, particularly phylogenetic trees, although there is little evidence of a direct connection. In particular, the Tree of Porphyry, just like Plato's angling example, consists of only a very few instances of categories nested in a hierarchy. None of the categories are elaborated except for the ones directly referred to in the example. In these partial hierarchies, we do not know, for instance, what the divisions under immaterial substances are, or what their subdivisions are, and so on for the other divisions. As we will soon see, however, examining only partial hierarchies, or what can be visualized as one "strand" of a tree diagram, as Plato and Porphyry did, does not give an adequate picture of the complexity involved in categorizing everything in this way. When Ramus began to popularize dichotomously divided diagrams in the 16th century, he fell into exactly this trap.

Ramism

As I indicated earlier, Petrus Ramus made the technique of dichotomous divisions popular in his many works on philosophy and pedagogy. In his didactic and philosophical texts he presented knowledge according to a dichotomously-branching hierarchy intended to mirror "the order found within things themselves."[812] His curly bracketed tables of contents were mostly dichotomous too.[813] Their regularity on the page gives them a veneer of logical purity.

Father Walter Ong's masterful studies of Ramus and the influence of Ramism, however, show that Ramus was a shoddy logician who was far more concerned with teaching well than with philosophical consistency. He and his followers were quite

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[811] 18 such diagrams are reprinted in Hacking 2005.
[812] Ong 1958, p. 194.
[813] E.g. Freedman 1993, pp. 102-105 – seven different examples of classifications of knowledge from Ramist texts, Ramus 1995, 2 ed., pp. "Typus prioris partis grammaticae" – the page before the title page has a dichotomous curly bracketed table of contents, though this is a language instruction text.

successful in popularizing the conception of knowledge and teaching as "reducible by rough analogy to some simple spatial arrangement or rearrangement of intellectual atoms."[814] But Ramus's "method" of dichotomous curly bracketed tables seemed to catch on more because their layout was supposed to aid the memory than because of any logical purity they were purported to possess, or any empirical assessment of their utility. In fact, as Father Ong has shown at length,

> "logical" method has no really honest ancestry at all in scientific logic.... method puts in its appearance as the product of rule-of-thumb pedagogical adjustments rather than of abstract reason. The original promoters of a purportedly airtight and all-inclusive logical method had no really ascertainable warrant for supposing such a thing existed, or could exist.[815]

Ramists did not get into pairing every term with its opposite in order to exhaust the logical possibilities of everything in the world. Instead, the method was more of a teaching aid or study tool to help students cram the relatively small amounts of material that could be represented graphically on a page. Ramus never generated dichotomous tables that contained thousands of terminal items. If he had tried to do so, he may well have run into the problems that botanists faced later.

Botanical texts by Ramists

If, as has been suggested, botanical identification keys come from or were at least intimately connected to Ramist logic at their origin, one would expect plenty of dichotomies in botanical works by Ramists. After all, they wasted no time in applying their beloved "method" to everything else. Yet, this is clearly not the case. It was some time before botanists began to use dichotomous tables in their diagnostic works. And when they did, the keys seemed to grow out of lists of headings and then tables of contents much as had Ramus's method.

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814 Ong 1958, p. 247.
815 Ong 1958, p. 307.

Ramus himself and the majority of his followers published almost exclusively on grammar, rhetoric, philosophy, education, classical authors, and law. They produced guides, commentaries, manuals, and large-scale encyclopaedic works, though they generally did not venture beyond humanistic scholarship into natural history. One of Ramus's followers, though, was the Swiss physician Theodor Zwinger (1533-1588), a "faithful correspondent" of the naturalist Conrad Gessner. Zwinger produced dichotomously branching books about philosophy and also a dichotomous "choose-your-own-adventure"-type travel guide to the Basel region. His botanical work, an expanded edition of one of Matthioli's works based on Dioscorides,[816] however, was arranged as the earlier texts of Dioscorides had been, without a single dichotomy inside.[817]

The only botanical book written by a Ramist in the form of dichotomies that I have found was by a Bohemian physician and professor with the velvety name of Adam Zaluziansky ze Zaluzian (1558?-1613).[818] His 1592 publication was designed to be as practical a classroom guide as possible. In it, Zaluziansky prepared dichotomous curly bracketed tables for only 10 of the 23 groups of plants he covered.[819] He found these Ramus-inspired divisions and their spatial layout helpful for showing students how to tell apart different kinds of plants. These arrangements of plants, however, were not what would now be considered identification keys proper.[820] The text was meant to supplement classroom instruction under the tutelage of an experienced botanist. It was not intended as a self-instruction guide and it would not have worked well as one: the curly bracket tables were keylike, to be sure, but without specifying which features were to be examined at each level

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[816] Zwinger 1594, Pavord 2005, p. 293. Gessner did not produce any guide to living things structured dichotomously, either.
[817] Zwinger 1744. I have not seen the original 1696 version. The second edition (1744) was edited and expanded by his son, the botanist and physician Freidrich Zwinger. Both were based on Mattioli's commentaries on Dioscorides.
[818] Westfall, Zaluziansky a Zaluzian 1940, vol. 1. Zaluziansky also goes by Adam Zaluzansky ze Zaluzan and Zaluzanius.
[819] There are 23 chapters on different groups of plants, though they only number up to 21 because there are two different chapter 10s and two different chapter 11s.
[820] Cf. Ogilvie 2006, p. 227. Brian Ogilvie suggested that "Zaluziansky's history resembles nothing so much as the artificial keys found in modern field manuals."

of the dichotomy, they were not always useful for determining which step to take next in the analysis of a plant. Not that this mattered much in Zaluziansky's time – the largest of his tables contained only 30 plants.

Zaluziansky's book was mostly unknown and not at all influential in either English or French-speaking botanical circles. Adanson, however, mentioned it in his *Familles des Plantes* (1763). He considered Zaluziansky's work to have been good for his time at putting plants in natural groups.[821] But Adanson said nothing about the book's layout.

A smoking gun?

Perhaps what counts as the biggest "smoking gun" linking the development of dichotomous identification keys with Ramism is this: that the encyclopaedist Johann Heinrich Alsted (1588-1638), an avid Ramist, was the teacher of Jan Amos Komenský (Comenius, 1592-1670),[822] who employed Cyprian Kinner (?-1649), also a colleague of Alsted's, to work on a language reform textbook. Kinner later published a plan for a universal language for plants (described below) that likely influenced John Wilkins's universal language scheme, both directly and via George Dalgarno, who was also familiar with Comenius's work.[823] Wilkins in turn got his friend John Ray to work on the tabular distribution of plants for that scheme, while John Ray went on to produce his *Historia plantarum*, laid out mostly in dichotomously branching couplets.

"Aha!" a philosopher of science might say. "This is clear evidence that Ray's use of dichotomies was most likely to have been influenced by Ramist logic." A closer examination of this chain of influence does not quite merit this conviction. For one, George Dalgarno, as we shall see, had doubts about the efficacy of dichotomies in conveying information. Wilkins generally did not employ dichotomies, whereas John Ray, as described earlier,

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[821] Adanson 1966, p. xiii.

[822] Ong 1958, pp. 163, 298, Rossi 2000, pp. 132, 136-137, 171. N. B. Rossi says that the mnemotechnic tradition embodied by Alsted and Comenius was "*eliminated from European culture*" (his italics) by the end of the 18th century. Rossi 2000, p. xviii.

[823] Schulte-Albert 1979, pp. 53, 55, Maat 2004, pp. 48, 50 note 20, 61.

thought Wilkins' tabular scheme was Procrustean. And the oppositions in Ray's couplets were, in turn, as Henri Daudin pointed out, "not at all reducible, as a general rule, to the rigorous form of an alternative between two contradictory propositions"[824] – though neither were Ramus's. Could Ray and other botanists have been influenced by Ramist thought? Yes – insofar as Ray's method, like Dalgarno's and Wilkins', was reliant on the trope of breaking down complex elements into simple ones and assuming that learning the simple ones in their most natural order would best help students learn. Was Ramist "logic" a leading source of inspiration for how they turned out? Very likely not. There are quite a number of reasons why the majority of botanists shied away from employing strictly dichotomous divisions in their works.

Doubts about dichotomies: how useful are they?

At least since the mid-17th century, doubts were being raised about how well a dichotomous scheme could aid the memory or reflect the order of the universe. George Dalgarno had praised Ramus for using dichotomous division consistently. Dichotomy, he wrote, is "the best and most natural *division* of the genera ... without any doubt ... because it is the *first*, for it is removed from *unity* by one step alone." But Dalgarno resorted to some legalese hair-splitting when it came to the practical utility of dichotomies as memory aids. Dichotomies, he wrote, are commonly known to confuse the memory, but this cannot be so; it must be "not so much the *memory* that is confused as the *intellect*, which has not yet clearly enough perceived the reason and underpinning of the division."[825] Dalgarno wanted to maintain dichotomous divisions' logical pride of place, but he also acknowledged that using them does not come naturally. In fact, Dalgarno thought that trichotomies and other arrangements of data might be "no less perfect than the dichotomy" in certain circumstances.[826] His stance seems to have been that, while one should aim for aesthetically

[824] Daudin [1926], p. 31.
[825] Dalgarno 2001, pp. 187, 189 (pp. 29-30 in the original).
[826] Dalgarno 2001, pp. 197, 199 (37-38 in the original).

pleasing dichotomous divisions, they were no more important than any arrangement suggested by the data themselves, particularly when ease of memorization was important.

Until the early 19th century, most books featuring a dichotomous path were philosophical, pedagogical and/or cosmological in nature and had nothing to do with natural history. Still, a few 17th- and 18th-century botanists employed bifurcating schemes for plant identification purposes[827] or at least described them.[828] The useful aspects of these schemes were the methodical approaches employed, not the degree to which they approached dichotomous perfection; none of them was strictly dichotomous. The dichotomies in many often consisted of couplets that are not fully contrasted. Magnol's *Prodromus,* for instance, contains the division "bulbosae/bulbosae affines" (plants with bulbs/plants like the plants with bulbs). It is enough to make any good logician squirm. Furthermore, all of these works featured alphabetical indexes in a tacit acknowledgement that the dichotomous divisions were not adequate on their own to help readers find the plant descriptions they sought. (Alphabetical indexes to botanical works had been common since the middle ages).[829]

As mentioned in Chapter 3, Lamarck was the first botanist to employ a relatively strict dichotomous scheme, the "*méthode analytique,*" in his *Flore Françoise* (1779). As Lamarck envisioned it, the way his scheme forced a choice between two and only two conceptually opposed possibilities made it idiot-proof. He did not appeal to philosophy, other than to the "contradiction" he mentioned above, to justify his approach, although the conjunction of a character and its opposite at every choice in his *méthode analytique* logically exhausts the possibilities. He also chose to use contrasting pairs of attributes because they led to certainty, not because it was a fast way to proceed. Though he aimed to devise a way to identify plants as definitively and quickly as possible, he wrote that he would always choose a more tedious, certain path to identification over a faster, less certain

[827] E.g. Ray 1686-1704, Magnol 1689, Hill 1770, 2 ed., vol. 1.
[828] Grew 1682.
[829] Wellisch 1978, p. 81. Zaluzianky's work also has two alphabetical indexes (Zaluziansky a Zaluzian 1940, vol. 1).

one.[830] Lamarck was explicit that his intention was to provide a clear-cut way to identify plants, and nothing more. In fact, he did not care if the groups of plants identified with his *Flore Françoise* had some kind of separate reality in nature or not, so long as they were easily and predictably identifiable.[831] Lamarck used logical principles as long as they suited his aims. Circumscribing natural groups was not one of them.

Lamarck's *Flore* won praise for how easy it was for beginners to use, even as the book's step-by-step structure made it boring and annoying for people already familiar with plant identification. As Augustin-Pyramus de Candolle wrote about the *méthode analytique*,

> I believe that because of its extreme facility, it is preferable to all others [i.e. kinds of arrangements] for beginners, but scarcely as soon as one acquires a certain amount of strength, then one becomes disgusted with it. One tires of the monotony of often uniform questions; one becomes impatient with the length of certain searches; one becomes bored with the attention the reference numbers demand from one question to the next; finally, one complains of not being able to follow easily the route by which one arrives at a name, and of only rarely finding resting places for the memory and attention.[832]

Lamarck acknowledged these defects of his *méthode analytique* well before Candolle pointed them out. In fact, in the very first edition of the *Flore*, Lamarck instructed users on how to cheat his key. Given that there are about 4,000 plants in the *Flore*, Lamarck wrote, arranging them dichotomously is not overly taxing. In fact, it "only bothers those who ignore the nature of geometric progressions. In effect," he wrote,

> if you divide the sum 4096 continually by twos, you will arrive at unity as soon as the eleventh division, and if you should find that there are more than eleven divisions to get through for each plant, one supporting the other, I observe that this work can be abridged by at least one level in a multitude of cases.

Lamarck directed readers to the numbers placed next to his couplets. These could be used as place markers. Users familiar with his text could remember which ones were associated with

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[830] Lamarck 1778, vol. 1, p. lxxiii; but see below.

[831] Though Lamarck made efforts to keep natural groups together (Lamarck 1778, vol. 1, pp. lxxvi, cxii), he explained that this was only secondary to dividing genera "simply, cleanly, and as evenly as possible" (Lamarck 1778, vol. 1, p. lxxvi). As a result, when he did not succeed in keeping natural groups together, he wrote "it's not my fault, nor that of the principles I am using" (Lamarck 1778, vol. 1, p. lxxvii).

[832] Candolle 1813, p. 51.

which plant features and skip to appropriate places in the text without reading all the descriptions along the way. "With a bit of use," Lamarck wrote, "you can, in a glance, skim through four or five divisions, which, in certain cases, greatly abridges the operation." Students could easily learn these "reference numbers by heart ... and thus the divisions to which they refer: an advantage that further dispenses with a part of the work to do."[833]

François-Joseph Lestiboudois also suggested that users familiar with his key skip the first part of it. "Looking up a plant with this method might appear a bit difficult to follow at first, [but] it also has the advantage of being infinitely shorter than the simply analytical way," he advised.[834]

That is, both Lamarck and his follower, Lestiboudois, acknowledged that a dichotomous arrangement of information is often not the most convenient or practical. A simple structure alone is not necessarily mnemonically salient or simple to memorize or use.

George Bentham, logician and botanist

In the early 19th century, botany and dichotomous divisions crossed paths again in the economist and philosopher Jeremy Bentham's *Chrestomathia*[835] (1817), a book advocating education reform along the lines of a new approach to the organisation of knowledge. In the fourth appendix to this work, "Essay on Nomenclature and Classification," Bentham extolled the benefits of using contradictory bifurcations to describe things, given that the results of each division would be exhaustive, instructive and distinct throughout.[836] This chapter was later substantially reworked by Jeremy's nephew, George Bentham (1800-1884), who later published an influential logic textbook before becoming one of the most outstanding

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[833] Lamarck 1778, vol. 1, pp. lxxix-lxxx. Aubert du Petit-Thouars concurred, writing in 1817 that people who have used Lamarck's *analyse* "have proven that after a bit of exercise they have not needed to return to the beginning of the *analyse* to look things up, and that they would go right away to a particular group" (du Petit-Thouars 1817, vol. 5 p. 227).
[834] Lestiboudois 1781, p. xliv.
[835] A chrestomathia is a collection of choice passages useful for teaching (Shorter OED, 3rd ed. 1950).
[836] Bentham 1983, p. 225, Bentham 1827, pp. 107-109.

botanists of the 19th century. George's version was published in French as *Essai sur la nomenclature et la classification des principales branches d'art-et-science* in 1823.

Despite his admiration for contradictory bifurcating divisions, Jeremy Bentham was aware of its faults. He even conceded that, though a number of logicians had made note of exhaustive bifurcation before, it was only to ridicule it. When translating this passage, however, George inserted a comment that various works in the natural sciences employed contradictory bifurcations, mentioning Lamarck and de Candolle's *Flore Français* (1805) and singling out Duméril's *Zoologie philosophique* (1808) as having used it to its full effect.[837]

"There remain however still, on every occasion, two questions," Jeremy Bentham wrote, "viz. how far this useful process can be, and how far it ought to be carried on" – the questions of "*impracticability*, the impracticability of the operation: the other may perhaps be termed the *uneconomicalness* of it," or whether it is worth the effort.[838] Jeremy Bentham divided impracticability into two separate problems, the those of "uncognoscibility" – the great difficulty of knowing with certainty whether "this or that property being stated as having place in all the individuals contained in one of the two branches, and as *not* having place in any of those contained in the other" is possible, and the problem of finding a surface area big enough to display a branching diagram with a very large number of "extreme branches" (lowest-level categories). In both cases the example he uses of a kind of thing that is so numerous as to cause difficulty with the bifurcation is "the number of supposed different *species* of plants," then estimated to be around 40 000.[839] (George Bentham mentions 50 000 plants).[840] Jeremy Bentham wrote that, as to the uncognoscibility question, no person could ever properly compare one plant's "points of agreement and disagreement with reference to every other" – there were simply too many of them. Furthermore, an

837 Bentham 1817, p. 244, Bentham 1823, p. 70.
838 Bentham 1817, p. 252, cf. Bentham 1823, p. 71.
839 Bentham 1817, p. 252.
840 Bentham 1823, pp. 71-72.

exhaustive bifurcating classification of plants could be exhaustive only so long as no new plants were discovered from year to year, and this was clearly not the case. As to the surface area question, he struggled with the math before leaving the matter to "any ready arithmetician." Regarding uneconomicalness, Jeremy Bentham wrote that each individual must decide for himself how far to pursue the exhaustive binary classification "in regard to net profit and convenience."[841] Neither Jeremy nor George suggested a maximum number of items beyond which it would become impracticable to use a dichotomous approach. Both Benthams, however, agreed that a bifurcating division of the arts and sciences is both desirable and feasible, since this branch of knowledge "comprehends principally the operations of the human spirit, and ... the effects produced by this instrument are nothing in comparison to those produced by nature in number or variety."[842]

Four years after George Bentham's *Essai* was published, he produced a short *Outline of a new system of logic* (1827).[843] He discussed "logical division" at length. George still thought highly of dichotomous divisions in which the choices are logical opposites, since they are the only method that leads to an exhaustion of all possible combinations of attributes at each step.[844] But clearly he had put more thought into when and where they were appropriate than he had in his earlier work. Of classifications in general he wrote that

> The mode of division to be adopted on each occasion will be found to depend upon the logical purpose of the operation. If the object be to give an exact and complete exposition of the parts into which the subject is divided, the only mode which can answer the purpose is that which may be termed *contradictory bifurcate* division. Where either *retention* of the names of the species and of their reference to the genus, or *exemplification* of the genus, is the chief purpose in view, the loose or irregular multifurcate division may be found adequate or even more appropriate.[845]

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[841] Bentham 1817, p. 253.

[842] Bentham 1823, p. 72. I chose to translate this from the French because George Bentham says in one sentence what it takes Jeremy several pages to say!

[843] This was George Bentham's only publication on logic. It sold only 50 copies – most copies were destroyed when the publisher went bankrupt (Stevens 2003, p. 193) – and was hardly influential at the time. By the 1850s, however, George Bentham was seen among philosophers as an important figure in logic reform, since in the late 1840s it was revealed in a priority debate among logic reformers that some of them may have been influenced by Bentham's *Outline* in the 1820s – one had even reviewed it. Bentham stayed clear of these controversies, though he kept a scrapbook of newspaper articles following the debates (McOuat 2003).

[844] Bentham 1827, pp. 107-109, 111.

[845] Bentham 1827, p. 106.

A contradictory bifurcating analysis might be the only logically satisfying way of exhausting a field of inquiry, but it was not ideal for retention in the mind for something such as species of plants. He continued,

> The point where contradictory bifurcation should cease, and multifurcate division (which is, in fact, mere exemplification) take its place, – as well as that where even multifurcate division should stop altogether, – are considerations which must be determined, upon each occasion, with reference to the particular object in view. They will, of course, depend upon the comparison of the advantages gained by, with the inconvenience of, the operation.[846]

Above all, Bentham cautioned against the reckless application of symmetrical or regular divisions to groups of biological entities. The "operations of nature," he wrote, "cannot be reduced to any rule of uniformity." There were too many aberrations for inductive conclusions drawn about living things to ever be certain.[847] When deciding between following nature or following logic, Bentham wrote that nature trumps logic for forming groups of organisms, much as Dalgarno – and Ray – had discovered before him. In fact, it seems that all serious naturalists who toyed with describing nature in terms of dichotomous divisions for logical reasons eventually rejected this approach as both inadequate to reflect organisms' relationships and to serve as a convenient guide to identifying or naming them. The exception that proves the rule, one might say, is the case of John Fleming.

John Fleming, devotee of dichotomy

John Fleming (1785-1857), born in Linlithgowshire, received his Doctor of Divinity degree from the University of Edinburgh in 1805 and was licensed to preach by the Church of Scotland the next year. But he was also passionate about natural history, particularly zoology and geology, and for the next two years his employment consisted of surveying the economic mineralogy of the Orkney and Shetland Islands for the Scottish Board of Agriculture. Fleming stayed in the Shetlands as a preacher until 1810. After that time he

[846] Bentham 1827, pp. 116-117.
[847] Bentham 1827, p. 188.

moved to the ministry of Flisk in Fifeshire, where he stayed for the next 22 years while writing many zoological articles and his most famous book, *The philosophy of zoology* (1822). He was elected to the Royal Society of Edinburgh in 1814.[848]

The philosophy of zoology has been praised for its ecological focus and for dealing with extinction as an "established fact,"[849] as well as for Fleming's ideas on geology – Charles Lyell (1797-1875), the most eminent British geologist of the 1830s, was an admirer.[850] The work, however, is, perhaps, better known for other reasons – notably Fleming's extreme and embarrassingly naïve promotion of a strictly dichotomous classification of organisms. He explained that representing the relationships among organisms as contradictory dichotomous divisions of as equal sizes as possible would be the most time- and space-efficient way possible. He believed it could be done strictly so that "The characters of the class will comprehend all the properties common to its orders, genera, and species," and suggested using "those characters which are of easiest detection, and which produce the most equal division of species."[851]

Fleming ignored the existence of anomalous taxa that do not share all of the properties of the higher groups in which they belong, and never mentioned the cross-references that would be required to keep track of such taxa. He also did not seem to realize how frequently it is difficult to find characters that not only split collections of species into two equal groups, but that are obvious as well.[852] These are all problems wrestled with by the producers of earlier attempts at dichotomous arrangements of organisms, especially Lamarck.[853] Fleming even went as far as to say that his dichotomous arrangement better represents nature than any other because of its predictive value. Within it, he wrote, genera

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848 Rehbock 1985, pp. 130-131.
849 Rehbock 1985, p. 133.
850 Corsi 1978, p. 222.
851 Fleming 1822, pp. 142-143.
852 Fleming 1822, p. 146.
853 It does not appear that Fleming read Lamarck's botanical works, in which Lamarck discussed these problems in detail. Fleming appreciated the physiological insights Lamarck published in his *Histoire naturelle des animaux sans vertèbres* (1815-1822) but was very critical of Lamarck's materialism (Corsi 1978, p. 223). He apparently modelled his *Philosophy of Zoology* more on fellow Scot William Smellie's *Philosophy of natural history* (1790) than on Lamarck's *Philosophie zoologique* (1809) (Rehbock 1985, p. 133).

"will seldom contain more than two species. When genera occur greatly exceeding in species this limited number, we may safely conclude, that the structure of the species is imperfectly understood."[854]

Attacks on John Fleming: dichotomy's detractors

Articles sympathetic to dichotomous arrangements of animals had been printed in respectable scientific journals in the 1820s,[855] with a number of zoologists claiming to have come upon the idea independently.[856] Papers promoting circular systems, however, had overtaken them in popularity by the end of the decade. Circular systems, Quinarianism in particular, promised to explain the relationships among animals and fungi better than could dichotomies. These rigid systems also were structured so as to highlight the features that as-yet-undiscovered organisms would have, as discussed earlier. As well, groups of fives fall right within what is now recognized as the usual number of items retained in ordinary people's short-term memory – between three and seven.[857] Nested groups of this size were a practical compromise between picking one's way through the chaotic masses of species accumulated in what used to be small Linnaean genera and working through the extensive number of divisions necessitated by a dichotomous treatment.[858]

Meanwhile, Fleming continued to write as late as 1829 that "the dichotomous method is the exhibition of a process of thought universally practiced by the human mind" that would convey a student to the answers he sought by the surest and "shortest road" [!] He

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[854] Fleming 1822, p. 155. To Fleming's credit, he did not believe that ranks had any meaning when comparing different taxa, and that it was perfectly sensible to elevate a taxon in rank if new subtaxa were to be discovered (Fleming 1822, pp. 146, 159).

[855] Haworth 1823, Haworth 1825b, Haworth 1825a, Haworth 1825d, Haworth 1825c.

[856] Haworth claimed not to have known of Duméril's mostly-dichotomous *Zoologie analytique* (Duméril 1806) or anyone else's dichotomous arrangements until after he had come up with his own (Haworth 1823, p. 430). MacLeay also suggests that Fleming claimed that his binary system was entirely without precedent (Fleming 1830, p. 433), though MacLeay's assertions need to be taken with a bit of salt.

[857] Cowan 2001 and commentaries. These mental limits are not rigid.

[858] For more on this note, see the Rev. William Kirby's comment to this effect, note 469 above.

expressed surprise that so few naturalists were taking it up, despite admitting that "The labour of an author who pursues the dichotomous method is greatly increased."[859]

When Fleming not only persisted in his praise of dichotomous divisions, but called the circular system "arbitrary," ridiculed MacLeay's explanation of the meaning of different kinds of gaps between groups of organisms, and called the analogies upon which the quinary system is based "deceitful,"[860] MacLeay could not contain his contempt. He soon fired off what must qualify as one of the nastiest personal attacks in a scientific publication of all time, "On the dying struggle of the dichotomous system." This letter was addressed to his friend and fellow quinarian, N. A. Vigors. It was printed in several parts in the *Magazine of Natural History*. The first instalment came out in June of 1830. It lampoons Fleming's ideas, character, nationality, competence and intelligence at length, pointing out the inconsistencies in his stance, and adding a few of MacLeay's own unusual and frequently contradictory opinions in his attempts to defend and clarify his position. The editor intervened in the second instalment, in July 1830, removing most of the invective and irrelevant ranting. MacLeay still managed to say several times that he had proved the dichotomous arrangements of living things to be ridiculous in his *Horae Entomologicae*, among other reasons because it "has the advantage of making as many species as there are individual beings" – defeating the very purpose of a classification.[861] Though the edited letter was a relatively tame (by MacLeay's standards) discussion of the failings of the dichotomous system, an unedited version had been circulating privately for some time before it was printed in the magazine.[862] It was influential in spite of its ungentlemanly tone. Other contemporary zoologists later criticized Fleming and his beloved dichotomous system as well, believing MacLeay to have shown "Binary or Dichotomous systems ... to be not only among the most artificial of all arrangements, but as even incompetent to answer the purpose

859 Fleming 1829, p. 312.
860 Fleming 1829, pp. 323-326.
861 MacLeay 1830, pp. 423, 437, 203 and 207.
862 MacLeay 1830, p. 53.

of a mere index to genera and species."[863]

Thus, instead of saving natural history from confusion and philosophical error, dichotomous arrangements of large numbers of plants or animals were seen as nothing short of irrational by the end of the 1830s. By this time, identification keys were being used and promoted for smaller numbers of organisms at a time, in conjunction with the natural method, and without the added baggage of necessarily reflecting the relationships among those organisms in nature.

Combinatorial arrangements

The search for patterns in classifications of plants as a way to make fruitful predictions about unknown plants existed even before the quest to compile and understand natural relationships among plants became paramount. Flowering plants are an evolutionarily young group composed of members much more closely related to each other and more similar in appearance to each other (on the whole) than, for instance, animals in different phyla. Plants are also modular. They are composed of a few basic parts, such as stems, leaves, and floral organs, each of which vary in number and position. This aspect of plant form, plus the large number of plant species (not to mention hybrids), means that plants lend themselves, as it were, to being described in combinatorial ways.

Before combinatorial techniques were applied to botanical schemes, they were used to generate new *ideas*. Combinatorial approaches had been tried in religious contexts since at least the 13th century, when Ramon Llull (1232? 1236?-1315) and his followers popularized his "art." This was a way of looking at the world in terms of the attributes of God that could be found to varying extents in any given thing. The "art" was based on kabbalistic numerology, but Llull explained it with diagrams of concentric wheels with letters standing for divine attributes arranged on them. Rotating the wheels with respect to one another

[863] Dr. Lardner's Cabinet Cyclopedia. Natural History. 1. On the geography and classification of animals. By W. Swainson, Esq. -- 2. Classification of quadrupeds. By W. Swainson, Esq. 1836, p. 552.

revealed different combinations of these attributes.[864] When this was combined with the Ramist assumption of concepts as units that can be manipulated and the desire to methodize all knowledge so as to render it easier to memorize, the appeal of a combinatorial technique to discover new things about the world became even stronger. Johann Heinrich Alsted, Llullist, Ramist, and prolific writer of all manner of encyclopaedic texts, spread the word until the mnemotechnic/combinatorial/unified system of knowledge tradition was prevalent all over Europe.[865] Alsted, for instance, was the teacher of Comenius, the language reformer who went on to influence a number of universal language projectors in the mid- to late 17th century including Dalgarno and Wilkins, as mentioned above. But the development of an all-encompassing combinatorial approach to knowledge was to hit a major snag. Gottfried Wilhelm Leibniz's (1646-1716) experience with it illustrates what went wrong.

Leibniz was certainly inspired by Llullist and Ramist conceptions of knowledge, especially through his readings of Dalgarno and Wilkins.[866] He took the idea of combining unit attributes in many different ways to a new level. He not only envisaged the generation of new ideas by combinatorial means, by transporting the permutation and combination techniques he developed in mathematics to the realm of human thought in general; he also wrote that if all ideas could be reduced to their basic components, and then if those basic elements of thought could be recombined systematically, all known ideas as well as new and interesting ideas could be generated.[867] Leibniz's emphasis on as-of-yet-undiscovered ideas becoming obvious because of the possibility of exhausting all logical combinations was unusual; most of his predecessors and contemporaries did not quantify the number of ideas that would be generated or make recourse to logic in this way. Most importantly, Leibniz also perceived that many of the ideas generated this way would be irrelevant. He saw that their number would also be so vast that nobody would have the time to deal with them all

[864] Yates 1966, p. 176.
[865] Rossi 2000, pp. 131-136.
[866] Maat 2004, pp. 69, 184, 270.
[867] Maat 2004, p. 273.

one by one. He suggested that a "method of exclusions" would be a way to separate the wheat from the chaff.[868] This mental winnowing technique, like many of his other ideas concerning the complete enumeration and systematization of everything and the perfection of human thought, was more of a desideratum than a functional algorithm. Neither he nor anyone else was successful in developing an *a priori* method to separate the useful from the fanciful or the existent from the possible. Despite the centuries of efforts put into them, combinatorial idea generation techniques began to fade into oblivion by the 18th century.[869] The evolutionary epistemology of trial and error – the only way that worked to pick out good ideas[870] – was simply not quick or logical enough.

It did experience a certain form of revival, however, in a discipline in which it still promised some utility – botany.[871] But it survived in botany not because of any absolute predictive value but rather because it provided a tool for describing plants that worked well with the knowledge of plants that was available in the 18th and 19th centuries.

Botanists in the 18th and early 19th centuries (and even earlier) found it convenient to describe plants in terms of the numbers and positions of their parts, as well as their general external shapes. This approach had many practical benefits. First of all, in plants, parts can vary from individual to individual within the same species – even within cuttings taken from the same source. This is one reason why it is much more difficult to distinguish different kinds of plants than different kinds of animals: most animals have fixed numbers of parts and are subject to less environment-induced variation (phenotypic plasticity). Botanists who wished to tell different plant kinds apart had to devote a great deal of effort to finding out which arrangements of parts are the most constant within plant groups. 18th-century naturalists such as Buffon attributed the contrast between the elaborate and reasoned

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868 Davies 1986, p. 267.
869 Rossi 2000, pp. xviii, 44.
870 Campbell 1974, .
871 Rossi 2000, p. 171.

classifications in botany and the lag in development of similar schemes in zoology to this difference between plants and animals.[872]

Using descriptions of plants in terms of their outer parts was also a matter of exigency. As Scott Atran has pointed out, plants are composed of different numbers of parts arranged in patterns on the outsides of their bodies where they are easy to see.[873] Other approaches to understanding plants did not develop fully during the 18th or early 19th century. Internal anatomy began to be used with great success in the late 18th century to characterize animals. Physiologists also made headway toward understanding animal organ function. These approaches, however, turned out to be far less useful for plants. While many animal body functions are localized to particular organs, occurring in particular places and particular numbers, plant functions are more distributed throughout their bodies. Until microscopy became relatively cheap and widespread, and cell functions were better understood, plant anatomy and physiology were sufficiently alien to preclude causal explanations of plant growth, development and structure.[874] As Aubert du Petit-Thouars put it in 1788, systems and methods were first and most extensively developed for keeping track of plants because,

> Among animals the breaks [between taxa] appeared to present themselves without effort. Here [among the plants] one must grope and search for them; and because plants are more numerous, a greater uniformity appears to reign among them. The same colour, this modification of the body which appears so mutable, [is] so fixed, and green dominates for the most part... [Among animals,] the mouth, this organ which allows them to take up their food, also furnishes the facility [of classification] that we do not find among the plants. It is only at the extremities of the capillary tubes of their roots, that Nature has placed imperceptible pores by which they draw in nourishing sap [from] the breast of the earth, just like she has placed others on their leaves to breathe in from the air the principles that flutter about there. These parts are too difficult to observe, and, consequently, cannot serve to characterize them....[But] nature seems to have compensated for the kind of monotony widespread in the rest of the plant [with the flower]....After having admired its *ensemble*, [a botanist] will decompose it...

[872] Buffon 1749, vol. 2, p. tome II p. 10.
[873] Atran 1990, p. 191.
[874] E.g. Duméril 1806, p. x and Candolle 1813, p. 59. Stevens 1994, p. 252 note 87 provides more references.

> It is therefore only in this interesting part that it is possible to find a physiognomy [sic] presenting characters numerous enough to differentiate species. As well, all the methods and systems that can be imagined to facilitate the study of Botany function upon the different parts of which we have seen flowers are made.[875]

This situation made classification according to the numbers, shapes and positions of parts particularly useful as a framework of investigation. It is not necessary to know what leaves or stamens do in order to notice their shapes and modes of attachment, or to count them.

Linnaeus's sexual system was one of the first attempts to codify the description of plants based heavily on the numbers and arrangements of their parts.[876] Aspects of this tradition live on in the form of floral diagrams and floral formulas. All of these techniques are useful tools for describing plant structure in a compact and efficient way – like shorthand. Although they may look like old-style combinatorial ways to generate new ideas, the way they are used is purely descriptive.

Separating description from prescription and description from naming, however, have always been tricky. The appeal of combining the two turned out to be more than many 17th, 18th, and 19th-century botanists could resist.

Re-inventing the dud: combinatorial plant names

Contingency and inevitability in scientific discovery

Multiple discoveries of a natural phenomenon or multiple inventions of a technology are a staple of the history of science.[877] Multiple discoveries or inventions – "multiples," for short – are usually explained in terms of the similarities of social experiences of the inventors and their awareness of earlier research that serves as a foundation for their work. "The innovations," Robert Merton wrote, summarizing the findings of previous research into

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[875] du Petit-Thouars 1811a, pp. 27-30.

[876] I.e. Linnaeus 2004 (not published), pp. 5-6.

[877] The literature about multiples treats multiple inventions and discoveries as equivalent because it is so difficult to differentiate between common contingencies coming from a shared reality or a shared perception of reality. The multiple I discuss here is the reinvention of a technology.

multiples, "became virtually inevitable as certain kinds of knowledge accumulated in the cultural heritage and as social developments directed the attention of investigators to particular problems."[878]

Merton, like others who looked into multiples and their causes,[879] wrote almost exclusively about successful research programs. He was concerned with demonstrating the reality, ubiquity and importance of multiples in the sociology of science. He was not interested in multiple independent failures. But multiple independent failures exist. In fact, there is nothing quite like a multiple, independent failure – reinvention of a dud – to highlight the assumptions inherent in a researcher's "cultural heritage." Multiple duds are background assumptions brought to the foreground and writ large. They illustrate attempts to solve common problems with inappropriate tools, or to combine common ideas in unsuccessful ways. Their existence testifies to an unequal distribution of knowledge among scientific practitioners, since inventors of multiple duds are not only unaware of the invention of previous, similar duds, but also of their failures. (Even those inspired by previous duds or who plagiarize earlier duds misjudge their proposals' potential for success). Multiple duds also support an evolutionary epistemology for scientific development: duds are popular enough to be invented multiple times but they are also repeatedly shown to be flawed, and rejected. Duds can, in fact, be seen as "dead ends" in the evolution of ideas. In this case, multiple duds are an inevitable by-product of the "constrained stochastic behavior"[880] of scientific creativity.

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[878] Merton 1961, p. 475.
[879] Such as Simonton 1986, Simonton 2003 and Lamb and Easton 1984.
[880] Simonton 2003.

Evolutionary dead ends: special case solutions do not always scale up well

Evolutionary dead ends occur frequently in biology.[881] There are millions of species on this earth, and within each, hundreds to billions of organisms, each a living test of the viability of particular features under particular conditions. Each is constrained in its variation by its history as well as its current circumstances. One feature of the most successful organisms on earth is the exchange of genetic information. Sexual reproduction ensures that, every generation, the young are genetically different from their parents. A population composed of such individuals, each with slightly different genes and a slightly different phenotype, is more likely to contain some that will survive drastic changes in their environment than would a genetically uniform population. Variations less favourable in one environment might do extremely well in another. And this is where the dead ends come in. There are some living things that have lost the ability to exchange genetic information. Asexual "species" result when particular genotypes outcompete all others within their populations, to the point where only one genotype remains. Asexual species have evolved repeatedly, in organisms as diverse as bdelloid rotifers, aphids and lizards – and hundreds to thousands of species of plants and fungi. Such populations have done well enough to survive, in some cases, for millions of years.[882] But the minute conditions change – the temperature gets above their range, or a parasite discovers a new trick – these organisms are doomed. Their genetic isolation does not prepare them for a bigger slice of the world than the one to which they had grown accustomed. And so, to an extent, it goes with scientific theories. Just as some chemical experiments work well on a small scale but fail to function as expected in industrial quantities, some theories conceived of in relatively isolated circumstances are very attractive within the limited world view of their inventors.

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[881] For the sake of simplicity I am not going to address the effects of genetic drift, mutation, migration, sexual selection or bacterial conjugation, transformation or transduction here, though these processes also play roles in gene exchange and species formation.
[882] Mark Welch, Mark Welch and Meselson 2004, p. 1621.

Conventions about publication and priority among scientists encourage the inventors to elaborate on and publicize their ideas.[883] But nothing in these inventors' experience prepared them for the poor reception their ideas would have in the broader scientific community. Reinvention of duds is both contingent and near-inevitable when evolutionary epistemologies occur such that isolated offshoots can develop.

This scenario has played out dramatically in the history of science. Over and over again, creative scientists, isolated from the demands of reconciling theory and practice on a large scale, devised techniques that work far better on paper than in real life. In the history of botany, between the end of the 17th century and the late 1820s, at least seven different men living in four different countries and writing in four different languages independently invented – or claimed to have invented – a particular technique for naming plants in a meaningful way. This technique was intended to generate meaningful, unique and memorable names for plants. The more similar the plants, the more similar their names. The inventors, however, were not worried about how hard it would be to remember tens to hundreds of thousands of plant names that all sound similar – among other problems. They had all come up with something that worked for them for tens or hundreds of plants and extrapolated its potential without accounting for such limits. Multiple duds were born.

As to be expected, the particulars varied from inventor to inventor. These differences, along with biographical details, lend credence to the likelihood that their inventions were independent. Still, there were sufficient similarities among the inventors to merit grouping them together as expressions of the same idea.

Common technology

All of the schemes they proposed were supposed to make botany easier to learn and communicate than ever before. They each involved a highly structured algorithm – a set of

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[883] Merton 1961, p. 480.

rules – for giving plants names that embodied their descriptions. The rule set would be short and easy to remember:

1. Different plant features would each be assigned a different letter:
   A consonant would come first,
   then a vowel,
   then a consonant,
   then a vowel, and so on.
2. Each letter would have place value so that if it appeared more than once in the word, it would have a different meaning each time.
3. The combinations of letters that the technique would generate would then be pronounceable, compact descriptive names of plants.

The rules were simple to use. Botanists needed remember only a few traits of plants. Better yet, the meaningful names would be brief and memorable, far more so than the long Latin descriptions current before the 1730s. They would be superior to Linnaean names as well. With both the Latin descriptions and Linnaean binomials, botanists have to search through books to determine what to call a specimen. With the new technique, anyone anywhere who knew the rules about which letters stood for which properties and the order in which plant features should be named would be able to name a plant in exactly the same way, whether or not he had seen it before. The new system of naming would eliminate the nomenclatural mess caused by both synonyms and the giving of the same name to different plants. Botanists would finally be freed from the need to buy and pore over expensive Latin books to identify specimens.

The benefits that these schemes would have brought to botany, had they functioned as described, would have been great indeed. Yet, not one of these techniques became popular among botanists at large, and some were downright ridiculed. But clearly this kind of dud was attractive enough to inspire many different men to invent or claim to have invented it. How did they end up travelling down the same blind alley, and what can their misadventures tell us?

Let's start with a brief discussion of each of the schemes in question, the circumstances under which it was produced, and how the botanical public received it.

The first schemes of this nature originated in the 1640s and 1650s amid a group of corresponding intellectuals who wanted to develop a universal language. Many of the men experimented with and made suggestions concerning both language reform and botanical classification. They included Marin Mersenne (1588-1648),[884] Cyprian Kinner (?-1649),[885] William Petty (1623-1687),[886] Seth Ward (1617-1689)[887] and John Wilkins (1614-1672).[888] Details of most of these schemes are scarce, however, Cyprian Kinner laid out his plan in a letter dated June 27th, 1647 to Samuel Hartlib (?-1662), the chief disseminator of scientific ideas of his age.

Cyprian Kinner's scheme (1645)

Kinner (?-1649) was a Silesian lawyer and physician. He lived most of his life during the 30 years' war. Few details of his life are available, though he is known to have come from a wealthy noble family from the Silesian town of Brieg, where he studied at the same school as Samuel Hartlib. He travelled extensively, and had many intellectual correspondents. He was long interested in education and language reform, and had worked for a time with Alsted the encyclopaedist in Transylvania. After he lost his family and fortune when the Imperial army invaded Silesia, "he offered his service in 1644 to the Czech reformer-in-exile Jan Amos Comenius (Komenský)," Bishop of Brethren, who hired him 1645 as collaborator on the revision of a language textbook.[889] It was around this time that he developed his scheme for naming plants.[890]

[884] Salmon 1966, p. 392.
[885] DeMott 1958, p. 6, Schulte-Albert 1979, pp. 47-52.
[886] Slaughter 1982, p. 134.
[887] Slaughter 1982, pp. 133, 179.
[888] Slaughter 1982, p. 160.
[889] Biographical details from Schulte-Albert 1979, pp. 42, 47.
[890] DeMott 1958, p. 1958 .

Kinner's letter outlining his idea is paraphrased in a 1958 article by Benjamin DeMott. A few years before he wrote the letter, Kinner had explained,

> in a period when he was considering ways of helping students of botany, he, too, had thought of devising technical words (*voculas technicas*). His notion was to fashion them so that every letter or syllable in them would have a specific meaning. Consonants in the first syllable would denote primary and secondary qualities ... vowels would denote the degree of qualities. The second syllable would express the peculiar power of the plant – curative, preservative, nutritive, or the like. The third syllable would signify even more particular details, as for example when and where the plant grows and how it is gathered. Some letters would necessarily be repeated in different syllables (the Latin alphabet not providing enough letters for all the information that would be expressed in the word), but the repeated letters could be arranged so that they would have different signification according to the syllable in which they appeared. And the syllables might be made to vary in length from one to three letters, so that the meaning of letters would then depend upon their place within the syllable: by such techniques the problem of repetition could be solved.
>
> Kinner observes that a symbol made on this plan would be more than merely a new name for a plant, for to remember such a term would be to possess a compendium of the plant's powers and uses. And he contends that similar terms could be made for other classes of existing things [such as] elements [and] astronomical bodies.[891]

Kinner gave a few tentative examples but did not expand on the scheme any further. His scheme may have influenced many people, though, care of Hartlib – including John Wilkins, author of the famous, Royal Society sponsored *Essay towards a real character and a philosophical language* (1668).

John Ray's contribution to John Wilkins' scheme (1668)

John Wilkins' *Essay* was the largest and best-financed attempt at a universal language put forward during the last half of the 17th century. In many respects, it is a test case for what happens when a dud has strong institutional backing.

As mentioned in Chapter 1, Wilkins had originally completed his *Essay* in 1666. In September of that year, the printer had almost finished with it when most of the printed sheets and a good part of the manuscript were destroyed in the Great Fire of London.[892]

[891] DeMott 1958, p. 6.
[892] Maat 2004, p. 135.

After the fire, Wilkins resolved to put the book together again, bigger and better than before. He enlisted his friends to help with different sections. The botanist John Ray worked on the tables of plants.

Many historians, linguists, cryptographers and the like have written on Wilkins' scheme, and I have described it above as well, so I'll keep this commentary short.[893] Wilkins assembled words for concepts into tables in groups according to a hierarchy of kinds of his devising. There were four levels in the hierarchy: genus, difference, species and numerical position. Within each species, items were mostly grouped in nines. Each had a particular place in the table. It could be expressed using numerical notation, for instance:

> 'elephant' occurs under the genus 'beast', and under the first difference, that is, 'whole footed', as the fourth species. To locate 'elephant' on the tables, one could write '18.1.4', since 'beast' is the 18^{th} genus on the list of genera, 'whole footed' the 1^{st} difference under that genus, and 'elephant' the 4^{th} species under that difference.[894]

Wilkins had also provided for each of these location indicators to be expressed in words. Each genus was represented by a two-letter word. Each difference was indicated by adding a different consonant, and species were distinguished by the use of different vowels at the end. Using this notation, '18.1.4,' equivalent to "elephant," could also be spelled "zibi."[895] The names for plants would be formed in an analogous way.

Wilkins knew that his plan was imperfect, though he believed it to be achievable in principle.[896] John Ray thought otherwise. A major complaint of his was that there were far too many plants to describe using such a scheme.[897] Even by including only major groups of plants, and modifying the tables in the plant section to permit groups of up to 18 per 'species,' rather than the usual limit of 9, the "Real Character" could distinguish among only one-eighth of plants known and described at the time it was published.[898] It is likely that

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[893] See Maat 2004 for a thorough treatment.
[894] Maat 2004, p. 167.
[895] Maat 2004, p. 158.
[896] Maat 2004, p. 142.
[897] Maat 2004, p. 207.
[898] Maat 2004, p. 208, Wilkins 1668, p. 67.

Ray's frustration with Wilkins' scheme for plant identification purposes played a large part in spurring him to develop what grew into his own "natural" method for classifying plants. And despite the Royal Society's backing and some friends' of Wilkins using it in their correspondence for a few years, Wilkins' real character and philosophical language never caught on with the public.[899] It was, in effect, a dud.

This dud was, of course, reinvented. A similar scheme specifically designed for use with plants came to light nearly three-quarters of a century later, in Sweden. But its inventor was not who you might expect it to be.

Christopher Polhem's schemes (1739, 1741)

Christopher Polhem (d. 1751) was born in December 1661 to a merchant's family on the island of Götland, just east of the Swedish mainland in the Baltic Sea.[900] His father died when he was a young child. Soon afterwards, the uncle who had raised him for several more years died as well. He had to work on a farm to support himself. Polhem was very bright and self-motivated. Some friendly priests spotted his potential and instructed him in Latin. He was admitted to the University of Uppsala in 1687 at the age of 25.

At university, Polhem excelled at mathematics. Repairing a medieval clock in the Uppsala cathedral secured his reputation as an engineer. For the rest of his life he made a name for himself designing original mechanisms for clocks as well as machinery for the mining, sawmill and textile industries. He even designed and had built a water-powered clock assembly factory almost a century before the industrial revolution. He was considered the leading mathematician in Sweden for much of his life.

Polhem was one of the founders of the Swedish Royal Academy of Sciences in the late 1730s. A painting of Polhem, made by Johan Henrik Scheffel in 1741 and now seen by

[899] Jaap Maat describes several examples of the general disdain for the real character and schemes like it by the 1680s Maat 2004, p. 265.

[900] Biographical details translated by Hanna Martinsen from Sten Lindroth's *Svensk Lärdomshistoria* (1975. Stockholm: Norstedts, 1997, vol. 2, 534-552) and Tore Frängsmyr's *Svensk Ideéhistoria: Bildning och vetenskap under tusen år* (Stockholm: Natur och Kultur, 2000, vol. 1, 178-182).

Swedes on the back of their 500-kronor note, shows him wearing the Order of the North Star. He later shared this honour with Linnaeus.

Some time after 1739, his seventy-eighth year, Polhem approached the Swedish Royal Academy of Science with a manuscript on "Suggestions for Botanical Names." He wrote that he had been inspired by reading one of Linnaeus's works – possibly *Classes plantarum* (1738) – to devote time to an old idea of his for a universal language to describe plants. The system he proposed had different consonants at the beginning of words to indicate different kinds of substances. K in particular would stand for trees and G for grasses and other herbaceous plants. The initial consonant designating the size and rigidity of the plant would be followed by a vowel. Each vowel would signify a different sense that the plant would affect in the human observer such that A would stand for sight, E for hearing, I for smell, O for taste and U for touch. The letters of these names could be written in different typefaces, "So that if *ga* should exist, it necessarily signifies an herb which sight alone can indicate or please, such as a beautiful flower, according to the approximate size that the size of the consonant will express."[901] Along these lines,

> *ge* can indicate an herb which man has heard about but not yet gained knowledge of its actual properties; *gi* indicates an herb that smells pleasant or appalling; *go* an herb that according to taste is good enough to eat; and *gu* [a herb] which has a soft or sharp feel etc. However, if a man wants to describe any herb's figure as follows, then the first syllable must be *gå*, because å is a composite of a and u [such] as the French write, and according to a and u, vision and feel give the most reliable knowledge about everything ....
>
> It happens sometimes that that one needs two vowels together, such as when an herb both smells and tastes good, which or consequently is written *gio* and their size as the situation requires.[902]

To the third position would be assigned one of the "semi-vowels,"

> such as s for the intellect, l for all operations by the tongue in general living, n for hands, m for feet or *Modus localis* and r for the whole body's labour of internal and external nature. To follow up this with examples becomes too extensive, therefore only one example here and there is included which can demonstrate the method for everything else; e.g.: *gas* denotes a flower which the intellect has much to reason about, *gal* represents a flower which

[901] Polhem 1954, vol. 4, pp. 346-347.
[902] Polhem 1954, vol. 4, p. 348.

> has much to be talked about; *ges* represents a flower which much is heard about and much talked about; *gan* a flower which can be used for manufacturing and yarn colouring and which has been transported from another place; and *gar* a flower which serves our body as medicine or poison. The entry *gis* marks a sensible chemical mixture which causes good odours, etc., *gil* an herb which a charlatan can talk about for his own profit, *gin* an herb of good or nasty smell which, however, serves for some preparations or manufacture, *gim* an herb that smells and which can serve to promote speed, such as hemp for ropes for riding, horse tackle, and rigging and sails, etc., *gir* an aromatic herb for medicinal purposes. The rest is easy to work out from this.[903]

Other intellectuals were not as confident of the scheme's success as Polhem was. This particular manuscript was not mentioned in the Academy's *Handlingar*, its monthly publication, or in its minutes.[904]

But Polhem did not give up. Two years later, in 1741, he felt the need to revise his older manuscript after he saw a description of a collection of 100 plants that Linnaeus had discovered in Gothland, Öland and Småland published in the *Handlingar*. This new version of Polhem's was called "Suggestions for such Names for Herbs and Grasses that will in a Concise Way Point out their Virtues and Qualities in General." The main ideas of how alternating consonants and vowels assigned place value were to represent qualities that observers note in plants remained the same. He also added some comments on how the use of three different fonts and five different font sizes could increase the number of plants it would be possible to describe with the system. For instance,

> *bu* points out an externally recognizable herb, and can be ranked for better or worse with the largest vowel for cotton, followed by linen, and so hemp, thistle and nettle.
>
> These two letters, such as one consonant and one vowel, should satisfy the common goal as they can, with their 3 kinds and 5 sizes, make 15,625 variations. But to entertain the inquisitive, one will add the 6 semi-vowels for how each branch conducts itself, such as the top or the crown which is characterized by l, seed or fruit by n, leaves with m, the trunk or stem with s and the root by r.
>
> When these are divided into 3 kinds and each kind into 5 sizes, [there] appear 90 variations, and when the aforementioned 15,625 are multiplied with this, [there] occur 1,406,250. More differences should not

903 Polhem 1954, vol. 4, pp. 348-349.
904 Polhem 1954, vol. 4, p. 346.

> exist.[905] Nevertheless, so that nothing is omitted, a vowel then follows so each name will consist of 4 letters in total....
>
> I should perhaps continue with examples, but as I am neither a botanist nor an apothecary, I can not support to go further outside my field.[906]

Polhem's successors seemed to agree. His ideas on this matter were neglected.

Nathanael Matthaeus Wolf's scheme (1776, 1782)

The next person to have published on this topic was Nathaniel Matthaeus von Wolf (or Matthaeus Nathanael Wolf, Nataniel Mateusz Wolf, Nathaniel Matthew Wolf and all variations upon these names, (1724-1784)). Wolf was born in Konitz, a town in western Prussia, on January 24, of 1724. He was the son of an apothecary.[907] He got his medical degree in 1748 in Erfurt and built up a good reputation as a physician. Wolf went to England from November 1759 to July 1760 in order to pass the six months on English soil required at that time to allow him to qualify as a Fellow of the Royal Society of London.[908] Although he was a respected physician, his true love was astronomy. While in England, he seems to have been closest to John Hyacinth de Magalhaens (Magellan), F.R.S., Portuguese expatriate and nephew of the famous explorer, who shared his passion for this subject.

Upon his return to Poland, Wolf served as the personal physician of the prince-bishop of Posen, the congressional marshal of Poland, and of Prince Czartorisky. He became a member of the nobility in the Polish Diet in 1766. In 1769 he went into private practice in Dirschau. Forced out of Dirschau by the partition of Poland, he was able to set up private practice – and a small but well-equipped astronomical observatory – in Danzig (Gdańsk) in 1772. While in Gdańsk, he inoculated townspeople against smallpox as early as 1776, 20

[905] Linnaeus used this style of reasoning in his *Philosophia botanica* (1751, pp. 98, 99, and 101) to explain that 10 classes of 10 orders of 10 genera with 10 species in each would be enough to encompass all existing plants (Stevens 2006, Stevens 2002, p. 13) whereas in *Classes plantarum* (1738), he simply stated that he thought there were no more than 10,000 plants in existence (Linnaeus 1738, p. first page). It is possible that Linnaeus developed his mathematical accounting for this number after talking with Polhem.

[906] Polhem 1954, vol. 4, pp. 350-351.

[907] Most of the biographical details are from Poggendorff 1863, vol. 2, p. 1356 and Kasia 2004.

[908] Royal Society of London 2006.

years before Edward Jenner's better-known efforts.[909] He was elected as a Fellow of the Royal Society of London on April 10, 1777.

In 1776, Wolf came out with *Genera plantarum vocabulis characteristicis definita*, a Latin publication expanding on ideas of plant classification he had been working on for several years. (Two brief, earlier manuscripts on related subjects, written in 1770 and 1771, still exist in the archives of the Royal Society).

He began his treatise by describing the sounds that each of the letters he was about to employ should indicate, and stating that notions could be either numerical or comparative. For the numerical notions, he assigned "A" to mean "first," "Æ" to mean "second," "Y" to mean "third," and so on. A long vowel, designated by a circumflex (^) above it, was to indicate quantity, for instance "Â" would mean "one" and "Ŷ" would mean "three." Comparative notions included substance, superficial qualities, location, shape, smell and flavour. Each manifestation of these notions was to be represented by a letter. For instance, for the different kinds of substance that plant parts could manifest, Wolf assigned the following letters:

Substance type	**Letter**
Hard	R
Somewhat hard	V
Elastic	F
Soft	P
Sticky	W
Liquid	B

The ensemble of his technique was likewise displayed as a chart (Figure 26).

Instead of using uninomials, as the other inventors of similar schemes did, Wolf split his into two parts so that they were a kind of binomial. One name stood for the plant's family and the other for the genus, though in other respects the generation of the names was the

909 Januszajtis 2002, Kasia 2004.

same. He suggested that plant family names should be two letters long, the first indicating the number of pistils (or pistilliform stamens), the second, the number of stamens. Adding a prefix "h" would indicate an unequal number of stamens, while adding "z" would indicate an absence of stamens. For generic names, the first syllable would describe the fruit. The next syllable would describe the number of involucres, the next the corolla, and so on. He provides the example of a plant with one naked seed: according to his system, the botanist

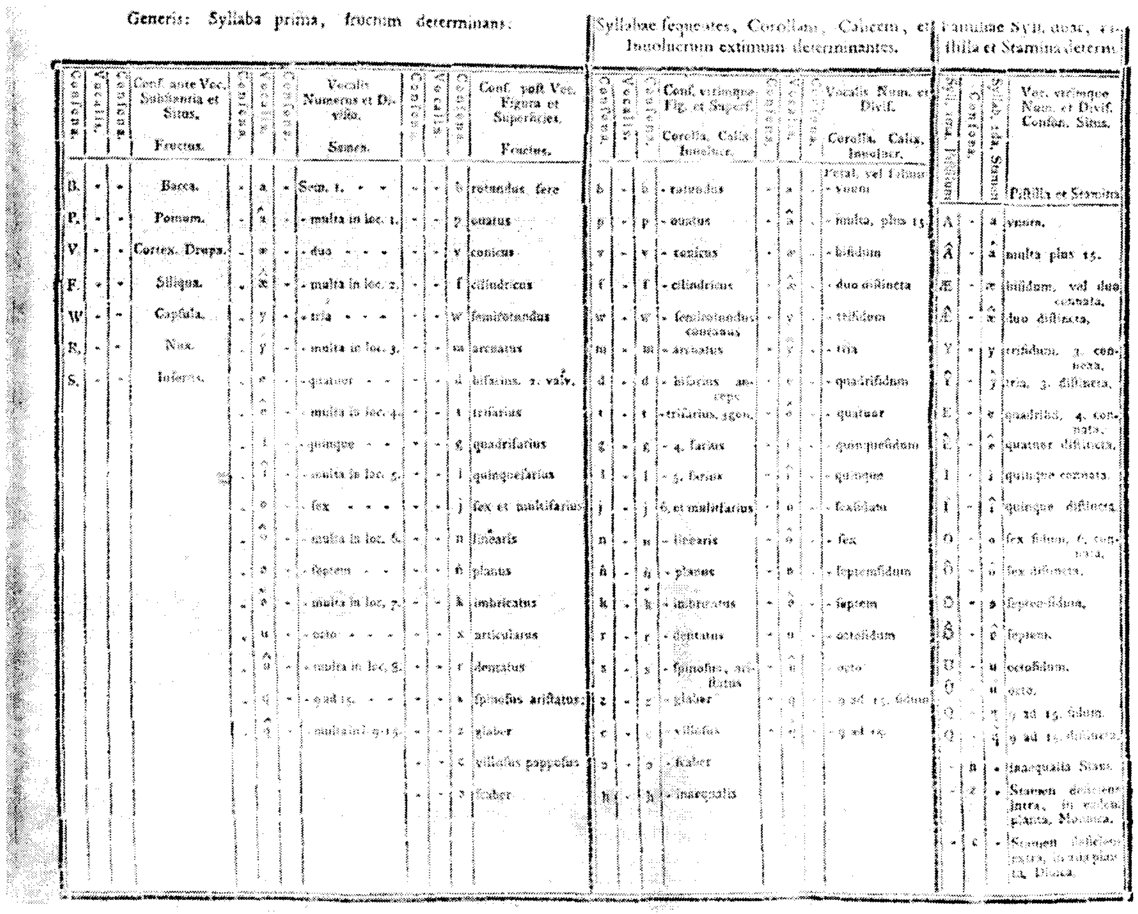

Figure 26. N. M. Wolf's chart explaining which letters stand for which plant properties. Image © and courtesy of Hunt Institute for Botanical Documentation, Carnegie Mellon University, Pittsburgh, PA.

would first write "A" and "ń." Noticing that this particular flower has five simple petals in a tube, he would add "f" and "i" to make the generic name, Ańfi. The flower has one pistil and five stamens, making the next syllable, or family name, Aî. The botanist could then look up a more detailed description for the plant under its family and genus (Aî Ańfi), where he would also find its Latin generic name, *Calligonum*.[910] Under his system, samphire (*Salicornia*) would be Aæ Anga, and pipewort (*Eriocaulon*) would likewise be Yŷ Apvye.

Wolf used his technique to briefly describe and name new "families" and to provide new "generic" names for hundreds of plants. Still, his dedication to the botanical cause seems ambiguous. The *Genera plantarum* was initially published anonymously, and on very bad quality paper. The index – published separately in 1780 – is a marvel of disorder, lacking alphabetization beyond the first letter and missing many entries. It shows at a glance that some plants have multiple names and other plants share one name. Wolf's friend Magellan nevertheless made a gift of Wolf's book to the Royal Society on November 9, 1780.[911] The Abbé des Houssayes (1727-1783) also, quite astonishingly, did not mention anything unusual about the index in his scathing review of Wolf's work in 1781 – though he called Wolf's nomenclature "hieroglyphics,""barbarous," and "unintelligible."[912]

Despite these drawbacks, Wolf's work was popular enough to merit a second, expanded edition published in 1782 and titled *Genera et species plantarum vocabulis characteristicis definita*.[913] This work was like the first edition except that Wolf also introduced three-syllable names for plant species. The first syllable would describe the plant's overall appearance, the second, its leaves, stipules, bracts, etc. and the third, the arrangement of flowers in its inflorescence. These syllables would likewise be generated by combinations of letters standing for particular plant properties. For instance, for the genus *Equisetum* (horsetails or scouring rushes), Wolf proposed the genus name Ånu. The various species to be found in Ånu were Vyljaffe, Væxfe, Vyxjafzi, Svyxjafpu or Svæxjapû (one

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910 Wolf 1776, p. 7.

911 Presents made to the Royal Society from November 1780 to July 1781; with the names of the donors 1781 "N. M. de Wolff, M. D. , F.R.S" also personally gave an octavo copy of the second edition of his work to the Royal Society on November 13, 1783 Presents made to the Royal Society from November 1783 to July 1784; with the names of the donors 1784.

912 Deshoussayes 1781, p. 405.

913 Wolf 1781. According to WorldCat, though the date given on the title page is 1781, it was published in 1782.

species with two names), Svexjaffu, Svexzpu, Vyxɔpu and Viljaffê.[914] I suspect that these names were tongue-twisters even to multilingual mitteleuropeans.

Doctor Jonathan Stokes, compiler of references to figures in the English botanist William Withering's very popular *Botanical Arrangement of British Plants* (1787), was familiar with Wolf's 1776 edition. Of "Wulff," he wrote in an aside that he had

> once formed a botanical language on a plan somewhat similar, but I soon discovered that in proportion as plants resembled each other, the difficulty of distinguishing the sounds or combinations of letters expressive of them, must proportionately increase. Languages formed on plans of this kind, must be full of such ambiguous names as Clutia, and Clusia.[915]

Wolf's work remains a bane to systematic botanists to this day. It is as rare as its quality is poor, yet even des Houssayes conceded that it could have "some utility."[916] Modern botanists must still occasionally consult it since it contains the original description of the genus *Eragrostis*, the ornamental plant popularly known as "love grass."

Wolf did not appear to publish further botanical works after 1782. He continued to practice medicine and make observations at his private astronomical observatory in Gdańsk until his death in December, 1784, during a flu epidemic.

Jean-Pierre Bergeret's scheme (1783)

Probably the most famous botanical scheme of this type was produced by Jean-Pierre Bergeret (d. 1813). There are several different versions of the details of his early life. According to Weiss, he was born on November 25, 1751, in Lasseube, Auch, in the southwest of France near Pau.[917] He took courses in surgery, anatomy and natural history at Bordeaux, then moved to Paris. Dayrat indicates that he was born in 1752, in Oléron, in the Béarn district, slightly south of Lasseube, though his parents moved to Paris when he was young. After they died, he decided not to return to his birthplace with his relatives but

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914 Wolf 1781, p. 337.
915 Withering and Stokes 1787, 2 ed., vol. 1 and 2, p. xlvi. Wolf had sent Michel Adanson a copy of his 1776 publication in 1780 (Hunt Institute of Botanical Documentation 1963, vol. 1, p. 307) but it does not appear from existing records that Adanson corresponded with anyone about it.
916 Deshoussayes 1781, p. 406.
917 Weiss 1854, 2 ed., vol. 4 .

instead to stay in Paris to study surgery and take courses in botany with Bernard de Jussieu. He was never enrolled in a school or faculty of medicine.[918] Either way, in 1776 he undertook a description of the plants of the Paris region, but set aside this work to prepare and then teach a botanical course he started.

It was during this time that he started to compile the material that would grow into his masterpiece, "*Phytonomatotechnie universelle* [a universal technique for naming plants], or, the art of giving plants names taken from their characters; a new system by the means of which one can, by oneself, without the help of any book, name all the plants that grow on the surface of the earth."[919] Bergeret tested his system on his students before having it published. "On different field trips," he wrote in the prospectus to his book,

> having put the principles of my system into their [i.e. students'] hands, I had the sweet satisfaction of seeing them grasp it, and I saw with pleasure that, by the means of these very principles, they were brought without trouble to name to me phytonomatotechnically all the plants that they found at their feet. The displays of joy on their part were not at all equivocal; they saw themselves, so to speak, as the creators of names, and this joy became even more evident when they recognized the appropriateness of the application of the letters to the different characters, according to the conformity of these names with those that I had already laid down.[920]

A major selling point for this kind of nomenclature, according to Bergeret, was that the principles of it were simple enough to memorize easily. In fact, the rules for naming genera, he wrote, "can be written on fewer than twelve playing cards."[921] But though a major selling point of his system was that it would allow botanists to name plants the field without lugging books around, Bergeret did not take that to mean that his work had to be pocket-sized.

Phytonomatotechnie universelle was published in three volumes in-folio from 1783-1785. It is a sumptuous work of which only 200 copies were printed. The most complete versions contain 328 plates, some in colour. Bergeret drew them all himself. It was to be issued in 30 livraisons. The last two and the twenty-first were never produced. The twenty-

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[918] Dayrat 2003, p. 160.
[919] Bergeret 1783, p. 157.
[920] Bergeret 1783, pp. 157-158.
[921] Bergeret 1783, p. 158.

first was to have contained an explanation of his botanical system, which Dayrat erroneously characterized as "unique in the entire history of botany."[922] Figure 27 shows the first chart from the book, explaining how the system works.

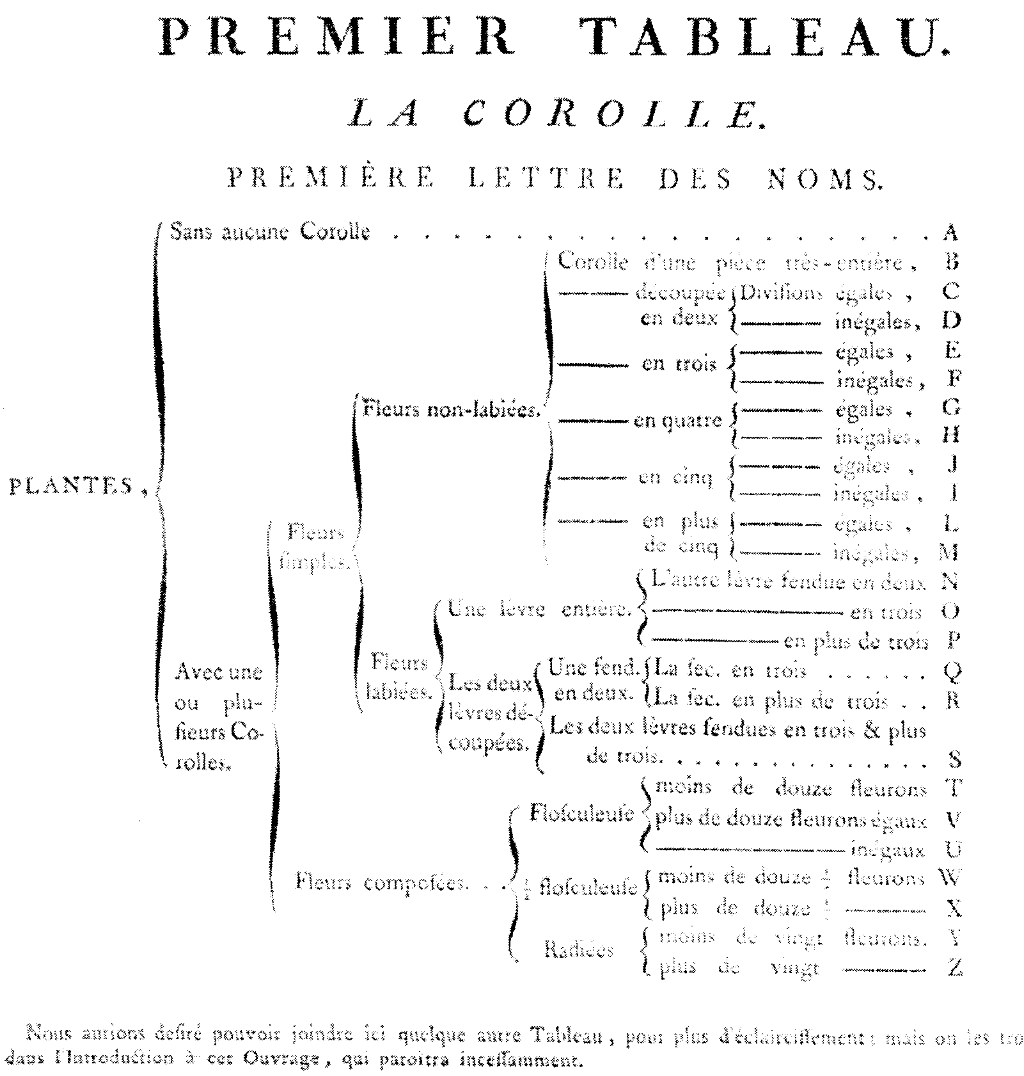

Figure 27. Bergeret's first table, "the corolla," from his *Phytonomatotechnie* (1783). Each variation of the corolla is assigned a different letter, with no regard for the pronounceability of the resultant plant name. This table was to be followed by 14 more like it, one for each of the subsequent parts of the plant examined. Image © and courtesy of Hunt Institute for Botanical Documentation, Carnegie Mellon University, Pittsburgh, PA.

[922] Dayrat 2003, p. 160.

Bergeret explained that he had gone over

> all the principal states (*manières d'être*) of the corolla, the stamens, the nectaries, the pistils, the floral envelope, the calyx, the pericarp and the seeds. To each of these different states, we attributed a consonant; this yielded eight large alphabetical charts. We then saw that the majority of these different states could also be subjected to closer examination. This new consideration produced seven other small charts for us, in which there is nothing but vowels.
>
> All these charts are put together in a manner such that each one gives a letter to the plant, according to its characters [i.e. the plant's features]; the resulting name can be spelled easily.[923]

Each name was to be 15 letters long. Unlike all the other schemes discussed here, his did not involve strict alternation of vowels and consonants. Names containing several of the same letters in a row – such as those in which many parts were assigned "A" for "absent" – could be condensed by adding a superscript number over the first repeating letter. This would indicate how many of the same letters were to follow. For instance, the phytonomatotechnical name for the fly agaric mushroom, AAAAAAAALAAAAYZ, could be spelled A^8LA^4YZ for short.[924] He then ran through how to generate these names, using the example of *Belladonna officinalis* (deadly nightshade):

> We compare the corolla with the characters of the first table; we see that the letter J indicates a corolla in five sections. We write J and move on to the second table, which gives the vowel E because the sections of the corolla are not cut very deep. We put the E next to the J, and so we have JE. We go to the third table, and find that the letter Q indicates the insertion of the stamens under the germ, by means of the corolla. We write Q, and have JEQ. The number of stamens is five, so we find in the fourth table that the letter L indicates those for this plant. We write it and we have JEQL. The side opening of the anthers is expressed by the letter Y in the fifth table; we write it down and have JEQLY. Finally, by this method, we travel through the five tables in turn, each furnishing a letter; we write them down, and obtain the name JEQLYABIAJISBEV, which is equivalent to the entire description that Linnaeus gives us of the genus of deadly nightshade, since each of these letters expresses, as we will see, a character of the plant.[925]

Although Bergeret paid attention to the pronunciation and ordering of individual letters, he was apparently not as concerned with the pronounceability of phytonomatotechnical names as he could have been. This was a source of consternation to his readers, for instance,

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[923] Bergeret 1783-1784, vol. I-III, p. i [unnumbered].
[924] Bergeret 1783-1784, vol. I-III, p. i [not numbered].
[925] Bergeret 1783-1784, vol. I-III, p. ii [not numbered].

Erasmus Darwin, writing on behalf of the members of the Lichfield Botanical Society in England.[926] "Thus," he wrote in 1787,

> *Ieqlyabiajisbey* [sic] expresses the description of *Belladonna* ... In like manner *Gypmyabeahuftez* is both the name and description of *Draba* [a weed in the mustard family]. It is easy to foretell, that words of such enormous length, though they may serve well for a botanic shorthand, must be too difficult to be remember'd, or to be pronounced; and thence can never come into general use as a botanic language.[927]

Bergeret's countrymen were no kinder in their assessment of his work. In 1811, Baron Ambroise-Marie-François-Joseph Palisot de Beauvois (1752-1820), a member of the botanical section of the Académie de France since 1806,[928] wrote that the

> Phytonomatotechnie of Bergeret, the Phytographie of l'As,[929] and many other, similar ones, are so many books confined, as it were, to libraries, and [which] no longer have any other use than that of serving to complete the history of the science by showing its progress and extent, although they do not combine anywhere near the benefits that botanists find in the analytical method of the Flore français of M. de Lamarck.[930]

A biographical article written about Bergeret in 1854 mentions the numerous awards and royal acknowledgements he received for his surgical work, but gives much the same assessment of *Phytonomatotechnie*: "The execution is very remarkable for the time... [but] this work is now not much sought after, even though it is the most important of the author."[931] Even today, copies are quite rare and expensive, though they are valued more for the high production standards of the plates than for Bergeret's systematic treatment of plants.

The Abbé de Las's scheme (1783)

I have not been able to find out very much about the Abbé de Las – not even his first name.

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[926] This "society" consisted of three members. It was founded by Erasmus Darwin, who was the translator of the works of Linnaeus that the Society published Browne 1989, p. 599.
[927] Linnaeus 1787, vol. 1, p. xii.
[928] Crosland 1981, p. 619.
[929] I.e. de Las 1783. See below.
[930] Séance du lundi 10 juin 1811 1832/1835, p. 486. Palisot de Beauvois had been a student of the inspirational botany instructor Jean-Baptiste Lestiboudois (Leclair 1908, p. 43) at Lille and likely had plenty of first-hand experience using far better texts than these for identifying plants in the field.
[931] Weiss 1854, 2 ed., vol. 4 .

He seems to have lived in Arras and may have also taught chemistry there during the 1780s.[932] In 1783, he published his botanical scheme in a work called *Phytographie universelle, ou systême de botanique fondé sur une méthode descriptive de toutes les parties de la fleur: avec une nouvelle langue antho-phyllographique.* It did not seem to have attracted much notice other than Palisot de Beauvois's dismissal and scorn. But de Las himself was quite proud of his work and convinced of its importance. Of all the dud-makers I have found, he is the only one who acknowledged the existence of other authors' similar schemes. In particular, he wrote off Bergeret's *Phytonomatotechnie* as inadequate and insisted that he had not plagiarized from him or from Wolf. Clearly, he saw Bergeret and Wolf as his rivals in the development of the best variation of an entirely feasible project. He believed all this despite producing the most impractical and bizarre dud of all.

"I wanted," Abbé de Las wrote, "to establish a rapport between plants and their names, a rapport that could serve as a support for the memory."[933] Las believed that remembering all the names of plants – he considered there to be 20,000 known kinds – would be impossible. "The more the sphere of botany expands, the more one perceives that its study goes beyond the limits of the human spirit," he wrote.

> I know quite well that botany does not consist only of the knowledge of [names of] plants, but I also know that it is the most necessary part for beginners.... [but] memory refuses to keep track of an infinity of words that have no relationships among themselves nor with the things they signify. If an algebraic formula does not bring up the principles that served to build it, do you think I can retain it? The connection it has with its principles fixes it in my mind....
>
> Not only will every man instructed in the characters of flowers as my method indicates be able to recognize the plant that each anthographic word signifies without worrying about making mistakes, he will be equally capable of composing the name of each plant that he will have before his eyes, with the certitude of making it known to all people of whatever nation they are who are familiar with my characters and the signs that they represent.[934]

To do this, he invented not one but two languages, each with

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[932] Dalmasso 2005.
[933] de Las 1783, p. 38.
[934] de Las 1783, pp. 50-54.

> different signs and principles. The first, which I call anthographic [*anthographique*], or language descriptive of flowers, has only a very small number of characters, uniquely representing the form, situation, and parts of the flower. It serves to compose the generic name of plants. The second, which I call phyllographic, because its characters are taken for the most part from leaves, serves to distinguish the species [when] united with the first.[935]

His anthographic rules for naming genera were sufficiently complex that it took de Las 17 pages of text to explain all of its principles, corollaries and exceptions. In brief, they involved a modified Greek alphabet in which each letter stood for a character of the plant and had place value. Vowel sounds were added between the consonants at will to make the name sound harmonious. His phyllographic characters, or specific names, each consisted of two consonants, standing for two outstanding features of the plant's leaves. Again, vowels were to be added to these consonants so that the resulting two-syllable name would be easy to pronounce. He included a 9-page "Phyllographic alphabet" indicating which letters stood for which features and when, supplemented by a 255-word "Phyllographic dictionary" defining each of the words describing the features so as to remove all ambiguity.

The Abbé was aware that his proposal was not mainstream. In fact, he wrote that "fear of the censure of intellectuals on this matter would have made me hold back printing [it], if M. Bergeret had not published the prospectus of his *Phytonomatotechnie*, which my work resembles too much, to put me at risk of being reproached for plagiarism if I were to defer publication any longer."[936] Bergeret's prospectus was published in May of 1783.[937] De Las quickly went on the defensive, preparing to publish as soon as possible. He ensured that his readers knew that he had given copies of his project to Adanson and Jussieu in November 1782, and that Jussieu had given him suggestions on how to improve it. He listed a number of other men of letters with whom he had communicated about his ideas, including several from Arras and others from Dijon and Lyon. "Almost all" of these men, he wrote, "have done me the honour of writing me that they would permit me to call them in proof of the

[935] de Las 1783, p. 39.
[936] de Las 1783, p. iv.
[937] Phytonomatotechnie Universelle 1783.

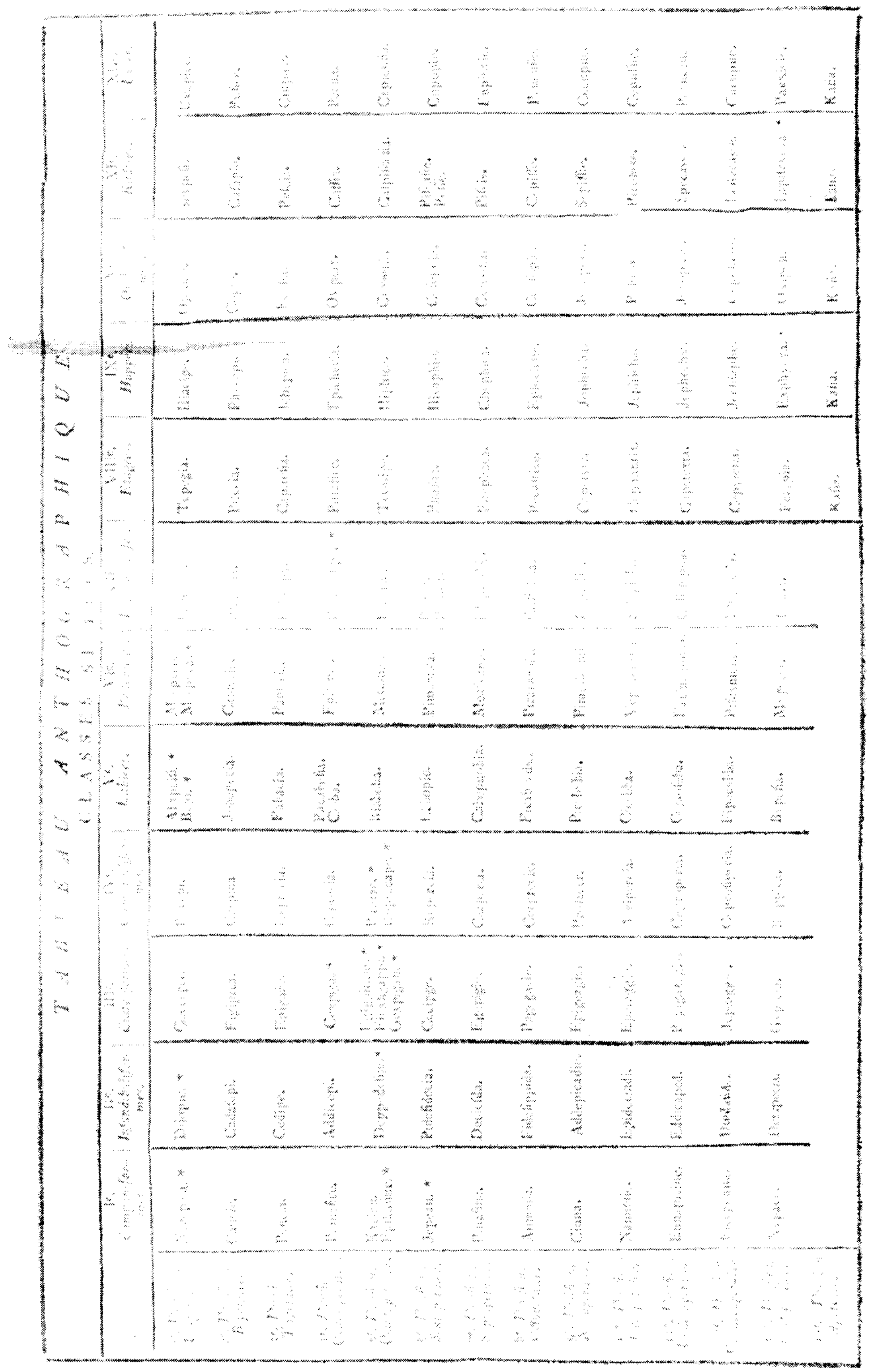

Figure 28. The *Tableau Anthographique* (Anthographic table) from de Las's *Phytographie universelle* (1783), showing the names of "simple classes" of plants. Image © and courtesy of Hunt Institute for Botanical Documentation, Carnegie Mellon University, Pittsburgh, PA.

knowledge that they had of my work, long before Mr. Bergeret published the prospectus of his *Phytonomatotechnie*." He reprinted the attestation of M. de la Tourette, perpetual secretary of the Académie de Lyon, "against whom I doubt that M. Bergeret would dare to bother to argue with me over [*s'élever pour me contester*] the glory of having conceived at the same time as him a project favourable to the progress of botany."[938]

As well, de Las was sure to explain that he had devised his scheme before he had ever heard of "Mr. Wolf, the Polish physician." Adanson, de Las wrote, first made him aware of Wolf's work

> only in conversation. M. de Jussieu showed me the book, which did not rest in my hands, as the same M. Jussieu can attest, for more than around four minutes. However brief this time was, I believe it is possible to assure that it sufficed for me to see how barbarous his method is. It is more of a writing than a language. How to pronounce, in effect, a language that is made up of twenty-five vowels, five *A*s, five *E*s, etc., each of these *A*s and these *E*s differing only in the pronunciation taken from its neighbours, of the sort that to speak the language of Mr. Wolf, one must know not only German, but also all of its dialects. What confusion! Could the author ever promise that his language would be adopted?

De Las was emphatic that his own method was easier to pronounce than any other. He demonstrated that, for instance, according to Wolf's plan, the letter *B*

> signifies that the fruit is a berry, *Â* with a circumflex accent, that the flower has four petals, *W*, that the calyx is conical, and plain *E* that the calyx has four segments. [The Linnean genus] *Actaea*, placed by this author in the second class, distinguished by all its characters, is named by Mr. Wolf Båê We, which is equivalent in pronunciation to *Baaeeoue*, and consequently cannot be pronounced. The same plant is named *Cucagexpi* in my method, and is more completely described, because this word includes again the form that the petals take among themselves and the number of stamens and the pistil.[939]

Combining the Abbé de Las's anthographic genus names and phyllographic species names with his plenitude-influenced top-down classification of flower shapes described earlier, results simply and unambiguously in the most colossal mess of a classificatory scheme for plants that I have ever come across.

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[938] de Las 1783, p. v.
[939] de Las 1783, p. 58.

So far, we have looked at botanical schemes produced by a 17th century Silesian and Englishman, and a Pole, a Swede and two Frenchmen who lived in the 18th century. What did they have in common? First of all, Kinner, Wolf and Bergeret were physicians. This automatically gave them some familiarity with plants. Even the men who were not physicians received formal educations. Though details of their formative years are few, they were likely all exposed to the same mnemonic technique used in philosophy classes. It was normal until the logic reforms of the 19th century for the medieval names of categories of syllogisms, called moods, to be explained in university courses through the use of odd-sounding mnemonic names.[940] Each of the three vowels in the name of a mood indicates in turn the categorical forms of the two different premises and the conclusion of all syllogisms that are of that mood. The consonants yield information about how to convert syllogisms of the last 20 moods into syllogisms of the first four moods through a series of logical operations somewhat akin to proving trigonometric identities.[941] However, like trigonometric identities in modern life, these names and the tricks used to generate them probably were all but forgotten by our dud-makers the moment they were no longer required to demonstrate proof that they could use them. None of the dud-makers referred to the naming of syllogistic moods in their works, and none claimed to have made systems that were intended, as syllogistic moods were, to exhaust all logical possibilities. Chances are that whatever influence knowledge of the syllogistic mood names had on the dud-makers was not in the forefront of their minds when they were constructing their schemes. There must be other factors at play to differentiate the dud-makers from their contemporaries who shared similar university educations but who did not go on to create duds.

One such similarity among the dud-makers was that they were intellectually adventurous and motivated to succeed in their respective professions. However, the men

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[940] The 24 names are: Barbara, Celarent, Darii, Ferio, Barbari, Celaront, Cesare, Camestres, Festino, Baroco, Cesaro, Camestrop, Darapti, Disamis, Datisi, Felapton, Bocardo, Ferison, Bramantip, Camenes, Dimaris, Fesapo, Fresison and Camenop.

[941] Spade 2002, 1.1 ed., pp. 14-25 for a detailed explanation of how this system worked..

grew up in different economic and cultural circumstances and also had different interests. Also, only two thirds of them succeeded in having their schemes published. This does not give us much to go on. What sort of person would be next to devise such a scheme? And how, after more than a century of such schemes being circulated, can we be sure that the next invention would be independent?

The case of John Henderson contains some similarities to those of his predecessors, to be sure. But his ideas about botanical classification were clearly developed independently.

John Henderson's scheme (1830-1831)

Dr. Henderson (d. 1836) arrived in Hobart, Van Diemen's Land (Tasmania) as Ship's Superintendent on the *York* on Aug. 29, 1829. Not much known about his life prior to this point, though a detailed study by Michael Edward Hoare – the source of most of the following details of Henderson's life – paints a picture of a restless traveller eager to explore and to help his fellow men. Between 1815 and 1826 Henderson lived in eastern India as a military medical officer. He became surgeon to the Bengal Army in June 1826. By 1829 he had been stationed in several cities, mostly in the Uttar Pradesh region. He was involved in many projects to improve local agricultural techniques and to introduce new crops, such as indigo, to his adopted homes. Most of these ventures failed. In 1829, Henderson "received permission to go on leave to Australian colonies and came 'with shattered health, and in embarrassed circumstances' to Van Diemen's Land."[942]

Henderson had already written several articles on natural phenomena for the *Transactions of the Medical Physical Society of India* and *Alexander Tilloch's Philosophical Magazine* before he arrived in Hobart Town.[943] He wasted no time in bringing his enthusiasm for the natural world to the town's inhabitants. Hobart Town was then only 24 years old. Its population was largely rough and uncultured, as befitted this former penal

[942] Hoare 1968, pp. 10-11.
[943] Catalogue of Scientific Papers, Royal Society of London, 1800-1863, 1869, v. 3 p. 273.

colony in such an isolated location. Still, by January 1830, Henderson had persuaded 'most of the respectable settlers throughout the Island' to form the Van Diemen's Land Society, otherwise known as the Philosophical Society or the Van Diemen's Land Scientific Society.[944]

Henderson, as President of the Society, gave a long speech before more than 100 of the most influential islanders, gathered in the court house for the first meeting. The Hobart Town Courier reported on January 23, 1830, that at the heart of Henderson's talk was a proposal for

> an entirely new system for introducing one general and determinate form of expression by which those who collected new plants, animals and other curiosities, through at a distance from each other, might infallibly be enabled to give the same name to their discoveries... . [He had waged] war against 30,000 arbitrary names of plants received in the nomenclature of botany, and had suggested the substitution of certain syllables and letters, of which might be compounded names expressive of the diagnostic marks of each particular plant.[945]

Henderson later claimed to have been inspired to develop these ideas not from experience with botany, but from observing that judges presented with practically the same legal arguments were likely to interpret them differently. He surmised that a logical outline of how to proceed in each instance would help reduce uncertainty and variation in each legal decision. If this system were in place, a judge

> would only stand in need of common sense to direct his decisions, instead of requiring as now, a protracted attention to legal lore, until the mental faculties almost cease correctly to recognise the distinction betwixt truth and error. Legal language would also become brief and determinate; each legal document, henceforth constituting a series of definitions, arranged in regular and successive order.[946]

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[944] Hoare 1968, p. 12.
[945] Hoare 1968, pp. 14-15.
[946] Henderson 1832, pp. 176-177.

Henderson thought that botany was adrift in a sea of unintentional synonymy caused by a lack of guidelines on how exactly to name plants. He believed it could benefit from this kind of logical overhaul as well.[947]

The talk was "received politely and well," though "opinion was divided" about it. Adam Turnbull, a prominent physician, argued in favour of naming local plants according to Henderson's principles, while James Ross, an eclectic and relatively educated man who published the Hobart Town Courier, thought that classifications developed in Europe by expert botanists were better. He recommended the Linnaean Sexual System. There is no record of anyone in attendance mentioning Robert Brown's well-respected *Prodromus florae Novae Hollandiae et Insulae Van-Diemen* (Introduction to the flora of New Holland [Australia] and Van Diemen's Island), a Latin work published in England in 1810 and arranged according to Jussieu's "natural method."[948]

Printed transactions of Society meetings were supposed to be issued, but none ever was published.[949]

Henderson left Van Diemen's Land shortly after the first meeting of the Scientific Society. He was disappointed with the people for caring only for wealth and status rather than the natural riches around them. First he travelled to New South Wales, where he spent more than a year exploring. He compiled notes on its flora, fauna, geology and climate, as well as sociological descriptions of aborigines and settlers alike. He then left Sydney for Bengal in January, 1831, on his way to "resume his service with native regiments in Agra

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[947] Henderson explained his ideas in detail in a letter, described below. He also devoted several pages to explaining how dividing plants into successive pairs of groups of equal size would be the ideal way to organize information about them. His fixation on dichotomous divisions may have been inspired by similar proposals put forth in Jeremy Bentham's *Chrestomathia* (1817), or John Fleming's *Philosophy of Zoology* (1822), with which he shares the unusual terminology of "positive and negative" divisions. (His apparent inability to read French makes familiarity with George Bentham's more extensive and more botanical *Essai sur la nomenclature et la classification* of 1823 unlikely). Henderson's proposal also resembles Gottfried Wilhelm Leibniz's 18th century idea for a universal language designed to prevent illogical legal decisions from being made, which was, in turn, inspired by Bishop John Wilkins' *Essay towards a real character* (1668) (Maat 2004, pp. 156, 301-302). Henderson's proposal, however, was not lifted wholesale from any of these works. It also contains enough original elements – including flaws addressed in some of his predecessors' schemes – to suggest that he devised its details on his own even if he had heard of similar ideas from others.

[948] Hoare 1968, p. 15.

[949] Hoare 1968, p. 16.

and Ludhiana."[950] By the first of November of that year, had distilled the ideas he had presented in Hobart Town into a treatise, 'On Nomenclature.' He sent it from Juanpore (Kanpur) to Antoine-Chrysostome Quatermère de Quincy (1755-1849), Perpetual Secretary to the Institute of France in Paris.

This English-language treatise speaks well to what those familiar with Henderson's dealings in Hobart Town characterized as his "censorious and dogmatic" character.[951] He began by stating bluntly that he preferred to send his ideas to France, the home of Lavoisier, reformer of chemical nomenclature, than to the Royal Society in England. He did not expect a "favourable reception" from the English, who, he wrote, had "firmly rooted prejudices" "inimical" to nomenclature reform.[952] He followed these remarks with a typical – if bloated and vituperative – list of botanical complaints: there are too many kinds of plants to remember, too many absurd and arbitrary plant names, too many synonyms and disputed names, too many technical terms, and too many botanical works to look through in order to identify plants. Henderson proposed that a study of plant physiology was what really mattered, and it would be best served if botanists abandoned artificial systems written in dead languages.[953]

Discussing "artificial systems" in this context would have been the equivalent of waving a red flag before a bull. French botanists had been, for the most part, quite proud to have abandoned the arch-artificial Sexual System of Linnaeus several decades earlier. To speak of botany without mentioning the celebrated "natural method" of Jussieu was to insult all French intellectuals. As well, while England had been a stronghold of the Sexual System until the mid-1820s, botanists were switching to Jussieu's method in large numbers by the time Henderson sent his letter. Samuel Frederick Gray and John Lindley had already published several English-language botanical texts arranged according to its principles. Even

[950] Hoare 1968, p. 11.
[951] Hoare 1968, p. 19.
[952] Henderson 1832, p. 156.
[953] Henderson 1832, pp. 157-159.

Sir James Edward Smith, President of the Linnean Society of London from the day he founded it in 1788 until his death in March, 1828, came to praise Jussieu. In the introduction to his *Grammar of botany* (1826), Smith wrote that "Natural affinities cannot now be overlooked, by those who contemplate the Vegetable Kingdom with any degree of philosophical attention." English botanists would be best served by catching up with their French counterparts in this respect.[954]

Assuming that Quatermère de Quincy had continued reading past this gaffe, he would have come to the crux of Henderson's argument. Henderson wished to propose "the formation of a regular system of nomenclature" such that it would be clearly working

> *when an individual, who might in one country discover a new chemical substance, mineral, plant, insect, or other animal, should be enabled by means of the system to give it the self-same appellation that another individual, having no communication with the first, would have assigned to it, had he discovered its co-partner in any other part of the world* [Henderson's italics].

He wrote that he intended "to prove, not only that such a system was perfectly practicable, but that it could be likewise reduced to simple and easy regulations" – unlike the Linnaean Sexual System, which he then criticized again at length.[955]

Henderson's explanation for why his proposed system would be easier to use than the Sexual System involved an outline of how he believed human memory to work. He acknowledged that oral histories passed on as poems are "less easily corrupted, and more easily recollected than prose," attributing this to their regular structure: a narrative "chain of ideas" reinforced "through the ideal associations afforded by the rhyme." Linnean nomenclature, however, offers "neither line of connection betwixt the specimen and the name, or betwixt one name and another." When names are given willy-nilly like this, "the mind endeavours to supply the defect by an ideal chain of its own construction." In general

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[954] Smith 1826, p. x.
[955] Henderson 1832, p. 160.

terms, an "unharmonious" order of materials presented to a learner causes difficulties in recollection.[956]

Henderson consequently proposed "to simplify and establish a general systematic nomenclature in science" by "rendering as determinate as possible our methods of arrangement" such that each name would "describe ... the exact position of the specimen in the established system of classification." Since he wished his classification scheme to be as natural as possible, the classification and nomenclature for each plant needed to be based on "some fact or quality in the class or specimen."[957] Henderson considered it feasible for each class, order and genus of plants to be defined according to a property shared among all its members.[958] His next step was to designate each class with a different consonant, and assign a different vowel to each order. The genera in each order would likewise each be represented by a consonant, while the species would be labelled with vowels, "resorting to the employment of double letters, dipthongs, or even to consonants, wherever the higher numbers may be called for."[959] This method of combining alternating consonants and vowels, each with its own descriptive significance, would generate meaningful and pronounceable names of organisms. As Henderson explained,

> the name 'Bal' will clearly represent the first subdivision of Monandria Monogynia [plants with one stamen and one pistil]; and Balba will equally represent the first species, in the first genus, in the first subdivision, in the first order of the first class of the Linnaean system. One point more yet requires to be determined; namely, to what science this name may belong; and we must therefore append to it some termination, such as 'na,' or Balbana, in order to indicate its being the name of a plant, and to distinguish it from that of an animal or mineral. The name 'Dombina' will in the same manner represent the third species of the second genus, in the second subdivision of Tetrandria Tetragynia [plants with four stamens and four pistils]; the termination 'na' becoming the symbol of the class-word, indicating thereby, its relation to the department of Botany.

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[956] Henderson 1832, pp. 162-163.
[957] Henderson 1832, pp. 168, 175.
[958] He respected the Sexual System enough to propose classes based on the number of stamens and orders based on the number of pistils, but clearly he was not familiar enough with it to realize that there were numerous plants that have different numbers of stamens and/or pistils from other plants considered to belong to the same natural genera. One example discussed in botanical circles at length is *Valeriana rubra.* It has one stamen instead of the three found in other species of valerian, as discussed earlier. There was no consensus on how best to classify such plants.
[959] Henderson 1832, p. 176.

Any living thing or mineral could easily be named in this way, achieving Henderson's goal of a system of nomenclature that would enable anyone, anywhere, to spontaneously come up with the same name for the same kind of organism or mineral.[960]

There is no record of a reply to Henderson from the Institut de France. In all likelihood, Quatermère de Quincy ignored the missive as yet another waste of paper from an uneducated crank. Henderson, meanwhile, continued to travel, proposing civic improvement projects wherever he went. After four years in India, wanderlust overtook him again. He embarked on an exploratory trip outside of British-controlled territory through Kashmir and Ladakh, into what is now Pakistan. After hostile encounters with both locals and British soldiers, he died from exhaustion in March, 1836.[961]

Why was this dud reinvented?

Well, what are we to make of these 'duds'? Clearly they did not work as well in practice as in their inventors' conceptions of theory. Names cannot be and should not be detailed descriptions of things. Other, more worldly botanists pointed out that the descriptive names generated by these schemes would be similar for similar-looking plants, or too long, or unpronounceable, or even just useless. Wolf's, Bergeret's, de Las's and Henderson's would also have to overcome the entrenched standard of Linnaean binomials in order to gain acceptance – not to mention the problems that these schemes would have with giving distinct names to plants with absent, unclear or unusual features.

Botanists with better access to collections and greater networks of collaborators than the men whose schemes I described realized that schemes based on memorization alone were not going to work. In the 17th, 18th, and early 19th centuries, species of plants were thought to be the most numerous kind of thing in the world.[962] Caspar Bauhin had published his

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[960] Henderson 1832, pp. 176-178.
[961] Hoare 1968, p. 11.
[962] Whether the "species" hold up as valid species today is beside the point, since what matters here is the number of kinds of things that needed to be distinguished in the schemes. The number of described plant

Pinax theatrum botanicum in 1623, and it was said to have contained descriptions of 6,000 plant kinds. This work was frequently mentioned by authors of botanical schemes. They were aware that whatever schemes they were to develop had to distinguish these memory-busting numbers of kinds. And although each of the proponents of meaningful plant names was concerned with the pronounceability, regularity and brevity of the names they designed, the names themselves were far from memorable. Kinner, Polhem, Wolf and the others had all made the same mistakes regarding human memory and the "artificial memory" of the written word.

As well, while Henderson was correct that memory is helped by narrative, rhyme, and order of a sort , he and all of the other scheme-builders described here neglected to take into account that imagery, spatiality, dramatic elements, metaphor and metonymy are also effective mnemonic aids.[963] What their names made up for in regularity, they lost in salience. Essentially what these men had done was to take techniques that work well in print – a fixed order, coded short forms, tabular layouts – and assumed that they would transfer well to memory and speech. The results "speak" for themselves.

A friend of mine, systematic botanist Tim Dickinson, explained to me years ago that any good system of botanical organization has to have local memorizability, so that botanists can recall the names of plants that they work with, and global "look-upability," so that unfamiliar plants can be identified quickly. The sheer number of plant kinds necessitates a written record for this purpose. And if a record is written, it is best recorded in terms that a casual reader can understand, that is, in ordinary words, which do not require users to memorize both technical distinctions and their letter codes. Kinner, Polhem, and the others

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species exceeded the number of described insect species until the last decade of the 19th century. The popular belief that there were more plants than insects persisted at least into the 1830s (e. g. Henderson 1832, p. 160), though the number of insect species estimated to exist by competent entomologists began to exceed the estimated number of plant species in the second quarter of the 18th century.

[963]Rossi 2000, (original Italian publication 1975) describes the centuries-old tradition of *ars memoria,* based on these techniques. It was used by literate and unlettered folk alike. No 'dud' maker described in this paper refers to this tradition.

did not work hands-on with enough plants when they were writing their schemes to have encountered the limits of human memory.

Curiously, one might say, duds of the kind described earlier did not remain duds forever. Rusty and abandoned technologies can be refined and transported to new contexts where they will shine. Tweaking the naming systems these men devised a little yields a way to make call numbers. Schulte-Albert's work on Kinner praises him for developing a "faceted classification" much like the kinds now used in library catalogues.[964] Nobody memorizes call numbers, but they are useful written reminders of where books are located. Modifying the schemes in another way produces "hash tables." These are essentially one-line, computer-generated summaries of database entries, also used as their addresses. Evidently, substituting written "memory" or computer memory for human memory could give these duds new life.

I am not going to stretch my original metaphor of asexual species beyond this hint that horizontal gene transfer can revive a potentially moribund lineage by giving it new tools to survive. But it does bear consideration that yesterday's multiple dud can have a chance at being tomorrow's multiple invention – giving "reinvention of the dud" a whole new meaning.

If this is the case, then a flag should be raised for historiographers. Cases of multiple duds becoming multiple inventions support Greg Radick's assertion that "it seems, in principle, that *any* unsuccessful theory from the past has to be regarded as a potentially successful theory.... and that every failed but potentially successful theory can be described as a variant – more or less extreme – of a successful theory."[965] If duds can be converted to success stories merely by the addition of new technologies, we need to look in more detail at

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[964] In fact, Aristotle's predicaments or categories – substance, quantity, quality, relation, place, time, position, state, action, and passion – upon which Wilkins' early collaborator George Dalgarno originally intended to base his classification scheme (Cram and Maat 2001, p. 36) bear an uncanny resemblance to the colon classification of S. Ranganathan, the man now hailed as the father of faceted library classifications, as Hans Schulte-Albert pointed out (Schulte-Albert 1974, p. 324). (Ranganathan's five facets are personality, matter, energy, space and time).

[965] Radick, p. 31.

theories that were rejected in their own times. Only then will we be able to give a more complete account of what processes give rise both to the contingency and the inevitability of both dud and successful multiple inventions.

Conclusion

As should be evident by now, the development of the information management tools and strategies in botany in the 18th century was a long and complicated process. We have seen how the numbers of plants known at the turn of the 18^{th} century was instrumental in goading botanists to work on making arrangements of plants easier to use. We have watched botanists throughout the 18^{th} century invent variations on hierarchical arrangements of plants, botanical nomenclature, textbook format, and text navigation devices. We have noticed the contributions of innovative botanical instructors to these endeavours with new interest. We have observed how some of these techniques were accepted and diffused throughout the botanical community in Europe, how some techniques were ignored and forgotten, and how others were reinvented and lost time and time again. We have marvelled at repeated attempts to link hierarchical arrangements of plants to class logic, and shaken our heads at the terminological confusion about what "artificial" and "natural" mean that extends from the 18^{th} century to the present. And now, it is time to move on.

I would be gratified if anyone reading this study were to think just a little more about the efforts hardworking 18^{th}- and 19^{th}- century botanists devoted to managing large amounts of information when they fire up a database or even riffle through a card catalogue. We can not only learn from their failures, but we can also rejoice at how they managed to pull together a workable system with many parts still in operation today.

I do not wish to imply that the only meaningful result of this study has been to show how, in pre-Darwinian botanical classifications, the medium has been the message. I do hope, however, that 21^{st}-century readers will cast a more critical eye on the technologies they employ to organize their information, botanical or otherwise. Our solutions are only as good as our conceptions of what is going on. If we do not recognize our limitations, both technological and conceptual, we will be no better off than botanists were in the early 18^{th}

century. In many cases in modern biology, methods of organization presented as panaceas are just repackaged 18th century ideas with the trickings of the digital age, doomed to fail for the same practical reasons. But, just as these men were able to build on the works of their predecessors, we can, too. Now that we know more about how they proceeded and what they understood of their situation, we can do the same to ours. I think they would be proud of us.

Appendix A

Sources of data points for Figures 2 to 9 and 22 to 24

Below I list the references to the sources yielding data points used in Figures 2 to 9 and 22 to 24 in the chronological order of the data points. Full titles of the works cited are given in the Bibliography. Each reference refers to one data point unless otherwise noted. For instance, a note of "high and low estimates" means that one author gave information for two data points in the same year.

In all cases I have made attempts to consult original sources. When I was not able to consult particular sources, I describe where I took the number from and its attribution. I reproduce incomplete and/or incorrect attributions as is. I use page numbers whenever available.

Works in which I counted the entries myself are marked "hand counted." In all other cases, either the number is mentioned explicitly in the text, either in passing or as genus numbers in various *Genera plantarum*-type works, or I explain next to the citation how I obtained the number I used in the Figures. For instance, I mention when taxon limits differ significantly from modern limits for the same taxa, when numbers of species in one taxon are described in terms of a multiple of the numbers of species in another taxon, and when different sources disagree at the about how many species there are or there were described by a given date or in a particular work.

Fish species described (Figure 2)

Data for 36 different years from 1686-2006; 42 references in total.

1686 McMahon 2002, p. 9, citing Willughby, *Historia piscium,* Oxford, 1686.
1691 Ray 1974, p. 4.
1738 Cuvier and Valenciennes 1828, pp. 97-98, citing Artedi (no date given). According to WorldCat, Artedi's only work was published posthumously in 1738.
1744 Rooseboom 1970, p. 181, citing Pieter [Petrus] van Musschenbroek, *Oratio de sapientia divina* (Leiden: S. Luchtmans, 1744).

1749 Biberg 1749, 3 ed., p. 125.
1758 Cuvier and Valenciennes 1828, p. 107,
Simon 1982, p. 68, and
Soulsby 1933, p. 10, all citing Linnaeus 1758, the 10th edition of *Systema naturae*. Soulsby says there were 378 species; Cuvier and Simon say there were 414 species.
1767 Linnaeus 1766, 12 ed. Not including the Tetraodontiformes, sharks, skates, rays, chimaeras or other members of the Nantes category (76 nantes in all, giving a total of 476 species now recognized as fish).
Cuvier and Valenciennes 1828, p. 117 say that there were 477 fish species in this work.
1788 du Petit-Thouars, p. 20, citing de la Mettrie's article, « Discours sur nos connoissances en Histoire naturelle » to be published in the *Journal d'Histoire Naturelle*.
1791 Stillingfleet 1977, 3 ed., p. xv, describing the "last edition of Systema Naturae." Unsure of which version he saw.
1793 Cuvier and Valenciennes 1828, p. 157, citing Gmelin's edition of *Systema naturae*. This work was published 1788-1793; I am unsure of which volume(s) discussed fish.
1795 Berkenhout 1795.
Lacépède 1795, p. ccviii, same figure.
1801 Cuvier and Valenciennes, p. 151 citing Bloch 1801.
1803 Lacépède 1803 Tome 11. Hand counted.
1804 Shaw 1803; Shaw 1804 vols. 1 and 2 of each of both years. No more "nantes" category.
1810 Delamétherie, p. 11.
1837 Jenyns 1837, p. 4, citing Agassiz, *Rapport sur les poissons fossiles, &c.* p. 57.
1848 Simon 1982, p. 68, citing
Schilder, p. 44, in turn citing Leunis 1860 (for year 1848). Simon does not give any page numbers.
1859 Simon 1982, p. 68, citing Schilder 1922, in turn citing Agassiz, Bronn 1859.
1886 Simon 1982, p. 68, citing Leunis and Ludwig 1886.
1898 Simon 1982, p. 68, citing Moebius 1898.
1912 Pratt 1912, p. 468.
Simon 1982, p. 68, citing Schilder 1922, in turn citing Pratt 1911, same number.
Henshaw, p. 318
1929 Simon 1982, p. 69, citing Hesse 1929.
1930 Koller 1949, p. 54, citing Arndt 1930.
1953 Simon 1982, p. 69, citing Mayr et al 1953.
1955 Simon 1982, p. 69, citing Kaestner 1955.
1969 Simon 1982, p. 70, citing Mayr 1969.
1970 Simon 1982, p. 70, citing Barrington 1970.
1971 Simon 1982, p. 70, citing *das Leben* 1971.
1972 Simon, p. 70, citing *Biological Data Book* 1972.
1979 Migdalski, Fichter, and Weaver 1979, p. 14, citing Cohen (no date given). High and low estimates.
1982 Simon 1982, p. 70.
1984 Hendrickson 1984, p. 42, estimated number of described species.
1994 Nelson 1994, 3 ed., p. 2 , number of valid species and high estimate of fish suspected to exist.
1998 Eschmeyer 1998, p. 7, number of valid fish species and high estimate of fish suspected to exist.

2003 *Grzimek's Animal Life Encyclopedia* 2003, 2 ed., p. 11.
FishBase , accessed November 2003.
2006 *FishBase* 2006, accessed October 2006.

Fish genera described (Figure 2)

Data for 10 different years from 1738-1815 (plus 1994); 13 references in total.

1738 Cuvier and Valenciennes 1828, pp. 97-98, citing Artedi (no date given). According to WorldCat, Artedi's only work was published posthumously in 1738.
1749 Cuvier and Valenciennes 1828, p. 113, citing Jacques-Théodore Klein, *Missus historiae naturalis piscium promovendae,* 1740-1749.
1756 Cuvier and Valenciennes 1828, p. 102, citing Laurent-Théodore Gronovius, *Museum ichthyologicum,* Leyden, 1754 and 1756.
1758 Soulsby said there were 51 genera of fish in the 1758 10th edition of *Systema naturae,* "According to John D. Sherman, Jr, (1928) Linnaeus in Tom. 1."
Cuvier and Valenciennes 1828, p. 107, said there were 57 species in it.
1763 Cuvier and Valenciennes 1828, p. 102, citing Laurent-Théodore Gronovius, *Zoophylacium*, 1763.
1767 Cuvier and Valenciennes 1828, p. 115, citing the 12th edition of Linnaeus's *Systema naturae.*
1793 Cuvier and Valenciennes 1828, p. 157, citing Gmelin's edition of *Systema naturae,* 1788-1793 (dates and volume(s) not indicated).
1801 Cuvier and Valenciennes 1828, p. 151, citing Marc-Eliezer Bloch. *M. E. Blochii systema ichthyologiae iconibus CX illustratum post obium autoris opus inchoatum, absoluit, correxit, interpolarit,* Joh. Gottl. Schneider, Berlin, 1801.
1803 Lacépède 1803 tome 11.
1806 Duméril 1806 Hand counted.
1815 Cuvier and Valenciennes 1828, pp. 209, 214, citing Rafinesque, Palermo, 1815.
1994 Nelson 1994, 3 ed., p. 5.

Plant species described (Figures 3, 4, 23 and 24)

Data for 63 different years from 1596-1999; 95 references in total.

1596 Ray 1974, p. 7.
Adanson 1966, p. xiii.
Smith 1791, p. 14.
All citing Bauhin's *Pinax* (1596) and referring to 6,000 plant species.
Cain 1994, p. 322 said there were 5,740 species.
1650 Adanson 1966, p. xv.
Gilibert and de la Tourette 1797, 3 ed., p. 14.
Both citing J. Bauhin, *Histoire universelle des plantes*, also 6,000 species.
1686 Adanson 1966, p. xix, citing Ray in his 1682 [sic] *Methodus naturalis Plantarum.* Gilibert and de la Tourette 1797, 3 ed., p. 15, same figure (probably taken from Adanson without attribution).
1688 McMahon 2002, p. 9, citing the number of plants in first two volumes of John Ray's *Historia plantarum* (to 1688).

Cailleux 1953, p. 44, citing Tournefort 1688.
1690 Adanson 1966, p. xxvii, citing Hermann, *Florae Lugduno Batavae flores*, 1690.
1694 McMahon 2002, p. 9, citing the total number of plants described in Ray, *Historia plantarum*, 1694.
Boerhaave 1719, p. 129, citing Tournefort (work and year not given, probably the *Isagoge*).
Adanson 1966, p. xxx, citing Tournefort, *Isagoge*, 1694. Different figure from Boerhaave.
1703 Cailleux 1953, p. 44, citing John Ray, 1703.
1734 Réaumur 1734, p. 2. High and low estimates.
Winsor 1976, p. 58, citing Réaumur.
1737 Adanson 1966, p. xxxix, citing Linnaeus, *Methodus sexualis S. systema a staminibus et pistillis*, Ludguni Batavorum.
1751 Daubenton 1751, p. 341.
1753 Stearn 1959, p. 5 and
Stevens 1994, p. 208 both citing Linnaeus, *Species plantarum*.
Cain 1994, p. 334 cites the same work but gives a different figure.
1757 Cailleux 1953, p. 44, citing Linnaeus, *Systema naturae*, 1757.
1762 Candolle 1813, p. 190, citing Linnaeus, *Species plan*tarum 2nd edition, 1762.
1764 Linnaeus 1764, 3 ed. Hand counted.
Adanson 1966, p. cccxxivol. 1 (published 1764).
Atran 1990, p. 188, citing Valmont-Bomare, *Dictionnaire d'histoire na*turelle, 1791, II: 397 that Adanson recognized 25,000 species of plants. Same figure as Adanson (i.e. 18,000).
Daudin [1926], p. 115, citing Bonnet, *Contemp. Natur*, 2e, p., ch. X: *Oeuvres*, 1764, t. 7. p. 54.
1767 Broberg 1990, p. 55, citing Linnaeus, *Systema natu*rae, 12th edition.
1770 Hill 1770, 2 ed., p. 3vol. 1.
1771 Saccardo, p. 173, citing Linnaeus, 1771.
1775 Senebier 1775, pp. 33-36, citing Linnaeus (date and work not mentioned) and Commerson (likewise).
1778 Flourens 1857, pp. 118-119, describing the number of plants known in 1778.
Wight 1840, p. iii, referring to the number of plants known when Linnaeus died; Linnaeus died in 1778.
1783 Berkenhout 1795, p. viii, referring to the approximate number of plant species described by Linnaeus, father and son together. Linnaeus the Younger died in 1783.
de Las 1783, p. 51.
1791 Stillingfleet 1977, 3 ed., p. xiii.
1795 Berkenhout 1795, p. viii.
1797 Gilibert and de la Tourette 1797, 3 ed., p. 1. High and low estimates.
1802 Senebier 1802, 2 ed., p. 60.
1803 Lamarck and Mirbel 1803, pp. vii, 206. High and low estimates.
Mouton-Fontenille de la Clotte 1803, p. 327.
1804 Duméril 1804, p. 57.
1805 Cailleux 1953, p. 44 says Persoon had 20,000 species of plants in 1805.
1807 Candolle 1813, p. 190 citing Persoon 1807.
Cailleux 1953, p. 44 and
Delamétherie, p. 22, both citing C. H. Persoon, *Synopsis plantarum, seu enchiridium botanicum*, Paris, 1805-1807.
1808 Delamétherie, p. 22. This is a figure for plants in herbaria, both described and as yet undescribed.

1810 Mouton-Fontenille de la Clotte 1810, p. xvii.
Delamétherie, p. 13. This is a figure for plants in herbaria, both described and as yet undescribed.
1813 Candolle 1813, p. 23.
1814 Lefébure 1814, p. 1.
Brown 1865, p. 536 (or 7 in the reprint) says there are 33,000 plants (incl. cryptogams), not counting the Australian ones (of which he's seen 4200 so far).
1815 Lamarck and Candolle 1815, p. 7.
1816 Lefébure 1816, 2 ed., p. 43.
1817 du Petit-Thouars 1817, p. 227.
Lefébure 1817, p. 8.
Cailleux 1953, p. 44 and
Delamétherie, p. 17 both citing Humboldt 1817.
1821 Kirby and Spence 1828, 5 ed., p. 488, citing Candolle, *Essai elémentaire de géographie botanique*, 1821, p. 62.
1823 Bentham 1823, p. 71.
1824 Candolle 1824, p. v.
Jussieu 1824, p. 467 says that the number of known plants has doubled in the past 10 years to around 20,000, but this figure seems extremely low. Likely the article was written for the dictionary when it first came out, 1804-1806, and Jussieu did not revise the figure for 1824. This data point not used.
Candolle 1833, p. 266 note, citing Steudel, *Nomenclator botanicus*, 1824.
Saccardo, p. 173, also citing Steudel 1824. Candolle said that Steudel had given 50,534 species of plants; Saccardo said 70,000.
1832 Henderson 1832, p. 157.
Arnott 1832, 7 ed., p. 30, high and low estimates.
1836 Cailleux 1953, p. 44, citing Meyen 1836.
1840 Wight 1840, p. iii.
Daston 2004, p. 171, citing William Whewell, *Philosophy of the inductive sciences*, 1840, 1: 507.
1841 Cailleux 1953, p. 44, citing Romers 1841.
Cailleux 1953, p. 44, citing Steudel 1841.
1846 Lindley 1846, p. 800.
Newman 1846, p. 6.
1849 Brongniart 1849, p. 49.
1860 Stevens 1997a, p. 246, citing a Bentham manuscript circa 1860, 169 recto. High and low estimates.
1869 Reclus, p. 505.
1883 Stevens 1994, p. 208, and
Stevens 1997a, p. 246, both citing Bentham and Hooker, *Genera plantarum*, 1862-1883, but giving two different estimates.
1884 Cailleux 1953, p. 44, citing Van Tieghem, 1884.
1885 Saccardo, p. 173, citing Duchartre, *Elem. Bot.*
1887 Saccardo, p. 174, a compilation of estimates for different plant groups by experts in those groups, the most recent of which dates from 1887, and including fungi, algae and lichens.
1892 Saccardo, p. 179, a compilation of estimates for different plant groups by experts in those groups, the most recent of which dates from 1892, and including fungi, algae and lichens.
1900 Vines, p. 461. He updates Saccardo's 1894 (i.e. 1892) estimates.
1905 Cailleux 1953, p. 44, citing Gaston Bonnier. *Traité de botanique*, Paris, 1905.

1910 Bessey, p. 669, and, a weeks later,
Bessey, p. 966, citing J. C. T. Uphof, *Die Pflanzengattungen*. Leipzig: Wiegel. (Different figures).
1918 Cailleux 1953, p. 44, citing Constantin, 1918.
1936 Jones, p. 234, using 1936 data. High and low estimates, i.e. with and without bacteria counted as plants.
1944 Brown, p. 542, citing Stanley A. Cain, *Foundations of Plant Geography,* 1944.
1948 Wieviel Tierarten gibt es? 1948, p. 78. Sum of numbers of species of "higher" (133,000) and "lower" (100,000) plants.
1949 Palmer, p. 70.
1956 Haupt 1956, 3 ed., p. 10.
1985 Wilson, p. 700. Includes algae and fungi.
1995 Hawksworth and Kalin-Arroyo 1995, pp. 117-118: 270,000 land plants.
1999 Mangenot and Boureau 1999, p. 1357. Mangenot's figure.

Plant species estimated (Figures 3 and 24)

Data for 28 different years from 1691-1999; 33 references in total.

1691 Ray 1974, p. 9.
Kirby and Spence 1828, 5 ed., p. 488. They cite Ray as above, but claim that he said there were 20,000 plants in the world. What Ray actually said is that there is probably double the number of plants in the world as were described in Bauhin's *Pinax* (and he says there are about 6,000 plants in the *Pinax*). This would yield 12,000 plant species worldwide. But Ray also says (p. 4) that God makes fewer less perfect things than perfect things, and that insects are more perfect than plants, and also that (p. 5) the numbers of insects may rival the numbers of plants, and he estimates the worldwide number of insect species to be 10,000 or greater (p. 7).
1696 Rivinus 1696, 2 ed., p. 30.
1738 Linnaeus 1738, p. lectori s. author.
1744 Rooseboom 1970, p. 181, citing Pieter [Petrus] van Musschenbroek, *Oratio de sapientia divina*, Leiden, S. Luchtmans, 1744.
1749 Biberg 1749, 3 ed., p. 125.
1751 Daubenton 1751, p. 341.
1753 Linnaeus 1753, p. in unpaginated Lectori Aequo.
1762 Linnaeus 1762-1763, 2 ed., p. unpaginated Lectori aequo.
1764 Linnaeus 1764, 3 ed., p. unpaginated Lectori aequo.
1763 Adanson 1966, p. 1: cccxxiii.
Atran 1990, p. 188, citing Valmont-Bomare, *Dictionnaire d'histoire naturelle,* 1791, II: 397, that Adanson recognized 25 000 species of plants "and projected at least four times that number."
Daudin [1926], p. 115, citing Bonnet, *Contemp. Natur*, 2e, p., ch. X: *Oeuvres*, 1764, t. 7. p. 54.
1783 Broberg 1990, pp. 58-59, citing Eberhard August Wilhelm von Zimmerman. High and low estimates.
Stevens 1994, p. 184 in turn citing Broberg.
1798 Delamétherie, p. 31.
1810 Delamétherie, p. 13.
1813 Candolle 1813, p. 23.

1824 Kirby and Spence 1828, 5 ed., p. 488, citing Candolle, *Essai elémentaire de géographie botanique,* 1821, p. 62. High and low estimates.
1832 Arnott 1832, 7 ed., p. 30
Lindley 1832, p. 434, same figure.
1849 Brongniart 1849, p. 50.
1869 Reclus, p. 505. High and low estimates.
1872 Cooke 1895, p. 319, citing De Bary, 1872.
1892 Saccardo, p. 179. Plants excluding fungi. With the fungi included, his estimate is 400,000 "plants" in total.
1942 Cailleux 1953, p. 44, citing Bach, *Course de botanique générale,* Paris, 1942.
1953 Cailleux 1953, p. 44.
1968 Cronquist 1968, p. 3. "More than 300,000 species of organisms that are generally considered to be plants."
1988 Raven, p. 549
1990 Hawksworth 2001, p. 1422. Vascular plants in 1990.
1991 May 1991, p. 475. Vascular plants.
1995 Hawksworth and Kalin-Arroyo 1995, p. 118. Land plants. High, low and "working" estimates, with "good" accuracy.
1999 Mangenot and Boureau 1999, p. 1348. This number is Boureau's, not Mangenot's.

Plant genera described (Figures 4 and 22)

Data for 28 different years from 1694-1883 (plus 1979 and 1986); 49 references in total.

1694 Adanson 1966, p. xxx, citing Tournefort, *Isagoge,* 1694.
Stevens 1997b, p. 26, citing
Stearn 1960, 5 ed. (no page given), same number.
Atran 1990, p. 306, same number.
Boerhaave 1719, p. 129, different number. He does not mention a specific work of Tournefort's.
1737 Linnaeus 1737b and
Linnaeus 1737a together. Hand counted.
Adanson 1966, p. xxxix, says that in Linnaeus' 1737 *Methodus sexualis* there were 1174 genera.
1738 Williams 2001, p. 26, citing
Linnaeus 1738. Williams cites
Adanson 1966, pp. xliv-xlviii, as saying that there were 746 genera in this work, but I counted 748 in his *Fragmenta* in that work and 955 in the sexual system part.
Atran 1990, p. 306 says there were 746 genera in the *Fragmenta methodi naturalis* published that year.
1742 Soulsby 1933, p. 34, citing Linnaeus, *Genera plantarum,* 2nd edition, 1742.
1747 Adanson 1966, p. lviii, citing Ludwig, *Definitiones generum auctae & emendatae and Institutiones historico-physicae regni vegetabilis*, Lipsiae, 1747.
Adanson 1966, p. lix, citing
Wachendorff 1747. Adanson did not count plants in the omissa at the end (which do not show up in the index).
1749 Adanson 1966, p. lxiv, citing Gleditsch, *Histoire de l'Academie Royale des Sciences de Berlin*, 1749, p. 109.
1751 Williams 2001, p. 26 citing
Adanson 1966, pp. xliv-xlviii, citing Linnaeus, *Philosophia botanica* 1751.
1752 Soulsby 1933, p. 32, citing Linnaeus, *Genera plantarum* 4th edition, 1752.

1753 Walters 1986, p. 534, citing Linnaeus, *Species plantarum,* 1753.
1754 Linnaeus 1754, "Editio quinta ab auctore reformata et aucta" ed.
1757 Cailleux 1953, p. 44, citing Linnaeus, *Systema naturae,* 1757.
1759 Jussieu 1789, pp. lxiii-lxx, citing Bernard de Jussieu 1759. Hand counted.
1762 Candolle 1813, p. 190, citing Linnaeus, *Species plantarum* 2nd edition, 1762.
1763 Adanson 1966, p. 8.
1764 Stafleu, p. ix, says that Adanson (1763) recognised 1700 genera, but this figure includes those in the second volume of Adanson's work, published in 1764.
Linnaeus 1764, 3 ed..
Linnaeus 1764, 6 ed..
Atran 1990, p. 306, says there were 1344 genera in the *Fragmenta* published in the 6th edition of *Genera plantarum.*
1766 Cailleux 1953, p. 44, citing Crantz, 1766.
1771 Linnaeus 1771. The highest genus number in the book, appearing on p. 553.
1778 Soulsby 1933, p. 34, citing Linnaeus, *Genera plantarum.* 7th edition. Ed. D. Joanne Jacobo Reichard. Francofurti: Varrentrapp Filium et Wenner, 1778, published posthumously.
1787 Linnaeus 1787.
1789 Soulsby 1933, p. 34 and
Mouton-Fontenille de la Clotte 1803, p. 167, citing Linnaeus, *Genera plantarum.* 8th edition. Ed. D. Jo. Christiano Dan. Schreber. Francofurti: Varrentrappii et Wenneri, (1789). Mouton-Fontenille says 1767 genera ; Soulsby says 1766 genera.
Jussieu 1789This number is the 1754 genera in the main body of the text, plus 137 genera of *plantae incertae sedis*, plus 11 genera added late and in an appendix.
Johanneau An V [1797] citing Jussieu (1789). The number is the number of genera in the main body of the work, plus those in the *plantae incertae sedis* and 34 very *insedis* trees and shrubs.
1799 Lestiboudois 1799, 2 ed. Hand counted, including the ones at the end added at the last moment.
1807 Delamétherie, p. 22, citing Persoon, *Synopsis plantarum, seu enchiridium botanicum,* 1807.
1831 Cailleux 1953, p. 44, citing Sprengel, *Genera plantarum,* 1831.
1836 Seringe and Guillard 1836, p. 3.
1840 Cailleux 1953, p. 44, citing Endlicher, *Genera plantarum,* Vienna, 1836-1840.
1846 Lindley 1846, p. 800.
Newman, p. 9.
1870 Cailleux 1953, p. 44, citing L. Pfeiffer, *Synonymia botanica,* Kassel, 1870.
1883 Reveal 1998, citing Bentham and Hooker 1883.
1979 *Index nominum genericorum (plantarum)* 1979, p. xi
1986 *Index nominum genericorum (plantarum). Supplementum I* 1986, p. viii

Flowering plant species described (Figures 5 and 22)

Data for 38 different years from 1766-2002; 53 references in total.

1766 Cailleux, p. 44, citing H. I. Crantz, *Institutiones rei herbariae*, Vienna, 1766.
1767 Cailleux 1953, p. 44, citing Linnaeus. *"Genera plantarum,"* 1767 – but which edition is this? Cailleux doesn't say.

1771 Saccardo, p. 173, citing Linnaeus, 1771.
1805 Cailleux, p. 44, citing Persoon, 1805.
1807 Brown 1865, p. 7 (586 in original), citing
Persoon 1807 as having described 21,000 species.
Candolle 1813, p. 190 says Person described 22,000 species.
Saccardo, p. 173 says Persoon described 20,000 species.
1817 Cailleux 1953, p. 44 and
Delamétherie, p. 17, both citing Humboldt, 1817.
1819 Saccardo, p. 173 citing Candolle 1819. (*Théorie élémentaire* – a reprint? A 2nd edition?)
1824 Arnott 1832, 7 ed., p. 59, and
Candolle 1833, p. 266, citing Steudel, *Nomenclator botanicus,* 1821-1824.
Cailleux 1953, p. 44, cites him as 1821 and with 39,700 species.
1841 Cailleux 1953, p. 44, citing Romers 1841.
1846 Lindley 1846, p. 800. Includes conifers, which he put in with the flowering plants. There were 210 species of "Gymnogens."
Saccardo, p. 173 said Lindley "reckoned" 79,837 species of phanerogams.
1870 Cailleux 1953, p. 44, citing L. Pfeiffer, *Synonymia botanica.* Kassel, 1870.
1873 Walters 1986, p. 534, citing Candolle, *Prodromus*, 1823-1873. This is dicots only.
1883 Saccardo, p. 174 citing Durand, *Index gen. phan.*, 1888, who took his data from Bentham and Hooker, 1862-1883, says there are 78,200 dicots, 2,600 gymnosperms, and 19,600 monocots. That makes 100,400 plants of these 3 kinds, or 97,800 phanerogams.
1884 Cailleux 1953, p. 44, citing Van Tieghem, *Traité de botanique,* Paris, 1884.
1885 Cailleux 1953, p. 44, citing *Index Kewensis,* 1885. (165,000 spp).
Saccardo, p. 173, citing Duchartre, *Elem. Bot.*, 1885. (100,000 spp).
1892 Saccardo, p. 179.
1900 Cailleux 1953, p. 44, citing *Index Kewensis,*1900.
1905 Cailleux 1953, p. 44, citing *Index Kewensis,* 1905.
Cailleux 1953, p. 44, citing Gaston Bonnier, *Traité de botanique.* Paris, 1905.
1910 Bessey, p. 670, and, several weeks later,
Bessey, p. 966, citing J. C. T. Uphof, Die Pflanzengattungen. Leipzig: Wiegel. Same figure for both.
1910 Cailleux 1953, p. 44, citing *Index Kewensis,* 1910.
1915 Cailleux 1953, p. 44, citing *Index Kewensis,* 1915.
Moore, Clark, and Vodopich 1998, 2 ed., p. 580, citing Engler and Prantl, 1915.
Ainsworth & Bisby's Dictionary of the Fungi 2001, 9 ed., p. 359, citing Martin, 1915.
1920 Cailleux 1953, p. 44, citing *Index Kewensis,* 1920.
1925 Cailleux 1953, p. 44, citing *Index Kewensis,* 1925.
1930 Cailleux 1953, p. 44, citing *Index Kewensis,* 1930.
1935 Cailleux 1953, p. 44, citing *Index Kewensis,*1935.
1940 Cailleux 1953, p. 44, citing *Index Kewensis,* 1940.
1942 Cailleux 1953, p. 44, citing Bach, *Course de botanique générale*, Paris, 1942.
1949 Palmer 1949, p. 86.
1950 Cailleux 1953, p. 45, citing Turrill, *Adv. Sci.* 26: 1950.
1952 Cailleux 1953, p. 45, citing Salisbury, *Litteris*, June 1952.
1956 Haupt 1956, 3 ed., p. 11.
1959 Coulter and Dittmer 1959, 2 ed., p. 255.
Stafleu 1959, p. 65. He says that there are probably at least as many fungi as flowering plants (the number of which he does not mention), and that he estimates that only one out of 7 fungal species has been described. 7 * 40,000 = 280,000.

1968 Cronquist 1968, p. 127.
1981 Cronquist 1981, p. 2.
1992 Thorne 1992, p. 250.
1993 Heywood 1993, p. 10.
2000 Prance, p. 353, citing
Prance et al. .
2002 Thorne 2002, p. 511.

Flowering plant species estimated (Figure 5)

Data for 10 different years from 1828-2000; 12 references in total.

1828 Kirby and Spence 1828, 5 ed., p. 488. Phaenogamous plants.
1869 Reclus, p. 505.
1887 Gray 1887, p. 177.
1891 Stevens 1994, p. 477 n. 64, citing Baillon (« Taxonomie » in *Dictionnaire de botanique*. Vol. 4. Paris, Hachette, 1891, p.155. Phanerogams only.
1958 Gould 1958, p. 332.
1959 Coulter and Dittmer 1959, 2 ed., p. 88.
Stearn 1959, p. 8. High and low estimates. Low estimate same number as Coulter.
1964 DeWolf, p. 151 High and low estimates.
1987 Meyers, p. 224
1999 Favre-Duchartre 1999, p. 60.
2000 Prance, p. 353, citing
Prance et al. .

Flowering plant genera described (Figures 5 and 22)

Data for 29 different years from 1757-2002; 31 references in total.

1757 Cailleux 1953, p. 44, citing Linnaeus, *Systema naturae*, 1757.
1759 Jussieu 1789, pp. lxiii-lxx, citing Bernard de Jussieu 1759. Hand counted.
1789 Jussieu 1789 Figure for total genera of plants, minus Class 1 genera (acotyledones).
1797 Cailleux 1953, p. 44, citing Linnaeus, *Species plantarum*, 1797. (Edition unknown).
1799 Lestiboudois 1799, 2 ed. Hand counted.
1807 Candolle 1813, p. 190, citing Persoon (date not mentioned) in his last complete inventory of plants. Flowering plants only.
1824 Arnott, 7 ed., p. 59, citing Steudel, *Nomenclator botanicus*, 1821-1824.
Cailleux 1953, p. 44, cites Steudel as having 3400 genera, and publishing in 1821.
1840 Cailleux 1953, p. 44, citing Endlicher, *Genera plantarum*, Vienna, 1836-1840.
1846 Lindley 1846, p. 800. Includes conifers, which he put in with the flowering plants. There were 37 genera. of "Gymnogens."
1850 Walters 1986, p. 534, citing Endlicher, 1836-1850.
1852 Cailleux 1953, p. 44, citing Dietrich, *Synopsis plantarum*, Vimaria, 1852.
1873 Candolle 1873, p. 311. Dicots only.
1884 Cailleux 1953, p. 44, citing Van Tieghem, *Traité de botanique*, Paris, 1884.
1885 Cailleux 1953, p. 44, citing *Index Kewensis*, 1885.
1888 Cailleux 1953, p. 44, citing T. Durand, *Index generum phanerogarum*, 1888.

1895 Cailleux 1953, p. 44, citing *Index Kewensis*, 1895.
1899 Cailleux 1953, p. 44, citing A. Engler and K. Prantl. *Die naturlichen Pflanzen familien*, Leipzig, 1899.
1900 Cailleux 1953, p. 44, citing Dalla Torre and Harms, *Genera siphonogarum*, 1900.
Cailleux 1953, p. 44, citing *Index Kewensis*, 1900.
1905 Cailleux 1953, p. 44, citing *Index Kewensis*, 1905.
1915 Cailleux 1953, p. 44, citing *Index Kewensis*, 1915.
1920 Cailleux 1953, p. 44, citing *Index Kewensis*, 1920.
1925 Cailleux 1953, p. 44, citing *Index Kewensis*, 1925.
1930 Cailleux 1953, p. 44, citing *Index Kewensis*, 1930.
1945 Cailleux 1953, p. 44, citing *Index Kewensis*, 1945.
1940 Cailleux 1953, p. 44, citing *Index Kewensis*, 1940.
1953 Cailleux 1953, p. 44.
1958 Gould 1958, p. 332.
1992 Thorne 1992, p. 250.
2002 Thorne 2002, p. 511.

Mammal species described (Figure 6)

Data for 31 different years from 1668-2003; 35 references in total.

1668 Wilkins 1668, p. 162. "Beasts" (i.e. vertebrates other than birds or fish.)
1691 Ray 1974, p. 5. "Beasts," including "serpents."
1749 Biberg 1749, 3 ed., p. 125. Estimated "quadrupeds" (not including "amphibians").
1758 Soulsby 1933, p. 10. There are 39 genera of mammalia in the 1758 10th edition of *Systema naturae,* "According to John D. Sherman, Jr, (1928) Linnaeus in Tom. 1."
1767 Linnaeus 1767, 12 ed.
1788 du Petit-Thouars, p. 20, citing de la Mettrie's article, « Discours sur nos connoissances en Histoire naturelle » to be published in the *Journal d'Histoire Naturelle*.
1795 Berkenhout 1795
1801 Shaw 1800vols. 1-2 and Shaw 1801vols 1-2.
1810 Delamétherie, p. 11.
1834 Jenyns 1837, p. 4, citing L'Institut, 1834, p. 138.
1848 Simon 1982, p. 68, citing
Schilder, p. 44 (Simon does not mention the page number), in turn citing Leunis, 1860 (for year 1848).
1859 Simon 1982, p. 68, citing Schilder 1922 citing Agassiz and Bronn 1859.
1869 Reclus, p. 562.
1886 Simon 1982, p. 68, citing Leunis and Ludwig 1886.
1896 Gill, p. 582, citing P. Z. S., 1896.
1898 Simon 1982, p. 68, citing Moebius 1898.

1912 Pratt 1912, p. 468.
Simon 1982, p. 68, citing Schilder 1922, citing Pratt 1911 – same figure as Pratt, 1912.
Henshaw, p. 318
1930 Koller 1949, p. 54, citing Arndt 1930.
1939 Simon 1982, p. 69, citing Arndt 1939.
1941 Arndt 1941, p. 78, citing H. Pohle. "Wieviele Saugetierarten kommen in Deutschland vor?" *Zool. Anz. Im Druck.* 1941.
1953 Simon 1982, p. 69, citing Mayr et al 1953.
1955 Simon 1982, p. 69, citing Kaestner 1955.
1969 Mayr 1969, p. 12, also cited in
Simon 1982, p. 70.
1970 Simon 1982, p. 70, citing Barrington 1970.
1971 Simon 1982, p. 70, citing *Das Leben* 1971.
1972 Simon 1982, p. 70, citing *Biology Data Book* 1972. High and low estimates.
1982 Simon 1982, p. 70.
Wilson and Reeder 1993, 2 ed., p. 3, citing James H. Honacki, Kenneth E. Kinman, and James W. Koeppl, eds. 1982. *Mammal Species of the World: a taxonomic and geographic reference*. Lawrence, Kansas: Allen Press and the Association of Systematics Collections.
1988 May 1988
1990 *Grzimek's encyclopedia of mammals* 1990, p. 4.
1993 Wilson and Reeder 1993, 2 ed., p. 3.
2003 *Grzimek's Animal Life Encyclopedia* 2003, 2 ed., p. 11.

Bird species described (Figure 7)

Data for 38 different years from 1676-2003; 51 references in total.

1676 McMahon 2002, p. 9, citing *The Ornithology of Francis Willughby Esq. of Middleton*, London, 1678. The Latin edition appeared in 1676.
1691 Ray 1974, p. 5.
1748 Stillingfleet 1977, 3 ed., p. 125, citing the 6th edition of *Systema naturae* in a translation note to Biberg 1749, 3 ed., p. 125. The 6th edition came out in 1748.
1758 Simon 1982, p. 68
Soulsby 1933, p. 10, both citing Linnaeus, *Systema naturae*, 10th edition, 1758. Simon says 444 birds and cites the same book, but Soulsby says there were 554 species of birds "According to John D. Sherman, Jr, (1928) Linnaeus in Tom. 1."
Mayr, p. 64 says there were 564 species of birds.
Farber, p. 72 says there were 545 spp.
1760 Farber, p. 12, citing Mathurin-Jacques Brisson, *Ornithologie*, 1760. Species and varieties.
1767 Berkenhout 1795, p. vi, citing approximately the number in the last edition of *Systema naturae* done by Linnaeus. That would be the 12th edition.
Farber, p. 72 says there were 931 birds in this work.
1783 Farber, p. 22, citing *Planches enluminées* [sic] of Buffon's *Histoire naturelle des*

oiseaux, 1783.

1788 du Petit-Thouars, p. 20, citing de la Mettrie's article, « Discours sur nos connoissances en Histoire naturelle » to be published in the *Journal d'Histoire Naturelle.*

1795 Berkenhout 1795.

1810 Delamétherie, p. 11.

1837 Jenyns 1837, p. 4, citing *Edinburgh New Philosophical Journal*, Vol. xv. p. 223.

1848 Simon 1982, p. 68, citing
Schilder, p. 44, (Simon does not mention the page number), citing Leunis 1860 (for year 1848).

1859 Simon 1982, p. 68, citing Schilder 1922, citing Agassiz and Bronn 1859.

1862 Stevens 1994, p. 477 note 64, citing Barber, *The heyday of natural history: 1820-1870.* Garden City, NY: Doubleday, 1980, p. 65. He says that there were 20 times more birds known in 1862 than in 1758 (i.e. the 10th edition of *Systema naturae*).

1869 Reclus, p. 562.

1886 Pratt 1912, p. 467, citing Ludwig's revision of Leunis.
Koller 1949, p. 54, citing Leunis and Ludwig 1886, different figure.
Simon 1982, p. 68, same figure as in Koller.

1898 Simon 1982, p. 68, citing Moebius 1898.

1899 Williamson 1997, p. 3, citing R. B. Sharpe, 1899. Williamson seems to get this number from Bock and Farrand, "The number of species and genera of recent birds: a contribution to comparative systematics." *American Museum Novitates*. 1980, 2703: 1-29.

1909 Mayr, p. 64 citing R. B. Sharpe, *Handlist of the genera and species of birds*, 1909. 5: xii.

1912 Pratt 1912, p. 468.
Simon 1982, p. 68, citing Schilder 1922, citing Pratt, 1911. Same figure as Pratt, 1912.
Henshaw, p. 318

1929 Simon 1982, p. 69, citing Hesse, 1929.

1930 Koller 1949, p. 54, citing Arndt, 1930.

1935 Mayr, p. 64, citing Mayr 1935.

1939 Simon 1982, p. 69, citing Arndt, 1939.

1946 Mayr, p. 67

1953 Simon 1982, p. 69, citing Mayr et al, 1953.

1955 Simon 1982, p. 69, citing Kaestner, 1955.

1969 Mayr 1969, p. 10, also cited in
Simon 1982, p. 70.

1970 Simon 1982, p. 70, citing Barrington, 1970.

1971 Simon 1982, p. 70, citing *Das Leben,* 1971.

1972 Clements 2000, 5 ed., p. xi, citing *Birds of the world: a checklist* (first publ. 1972).

1976 Griffiths 1976, p. 172.

1980 Williamson 1997, p. 3, citing Bock and Farrand, "The number of species and genera of recent birds: a contribution to comparative systematics." *American Museum Novitates*. 1980, 2703: 1-29.

1990 Imboden 1990, p. 7. "More than 9000" species of birds.

1997 Williamson 1997, p. 3, citing Howard and Moore, *A Complete checklist of the birds of the world*, London, Academic Press, 1991.

2000 Clements 2000, 5 ed., p. xi.

2003 *Grzimek's Animal Life Encyclopedia* 2003, 2 ed., p. 11.

Bird species estimated (Figure 7)

Data for 6 different years from 1668-1850; 6 references in total.

1668 Wilkins 1668, p. 162.
1691 Ray 1974, p. 5.
1744 Rooseboom 1970, p. 181, citing Pieter [Petrus] van Musschenbroek, *Oratio de sapientia divina*, Leiden: S. Luchtmans, 1744.
1749 Biberg 1749, 3 ed.
1778 Bernier 1975, p. 159, citing Buffon, *Des époques de la nature, septième et dernière époque, in Histoire naturelle générale et particulière, supplément.* Paris. Imprimerie royale. 1778. Tome 5, p. 246.
1850 Farber, p. 116.

Bird genera (Figure 7)

Data for 5 different years from 1758-1909; 8 references in total.

1758 Linnaeus . Hand counted.
Soulsby says there were 63 genera of "According to John D. Sherman, Jr, (1928) Linnaeus in Tom. 1."
Farber, p. 72 says there were 63 genera.
1760 Farber, p. 12, citing Mathurin-Jacques Brisson, *Ornithologie*, 1760.
1767 Farber, p. 72 referring to 12th ed. of Linnaeus's *Systema naturae.*
1844 Farber, p. 109, citing George Robert Gray, *The Genera of Birds,* 1844. Gray "described eight hundred and fifteen [genera], in an attempt to bring order to the over two thousand, four hundred genera [sic] names compiled from other authors." Both data points used.
1909 Mayr, p. 64, citing R. B. Sharpe, *Handlist of the genera and species of birds*, 1909, 5: xii.

Insect species described (Figures 8 and 23)

Data for 43 different years from 1691-1999; 55 references in total.

1691 Ray 1974, p. 6. Insects described by Martin Lister. Unclear from Ray what Lister's publication date is.
1758 Sabrosky 1952, p. 2. Says Linnaeus had 1,937 "kinds" of insects in the 1758 10th edition of *Systema naturae.*
Soulsby, p. 10 says there were 2109 of them in that work.
1760 Daudin [1926], p. 145, citing the last issue of *Systema naturae* before 1762. (There is a pirated edition c. 1760, according to the Linnaeus Link website (Soulsby 1933, p. 11).
1762 Daudin [1926], p. 145, citing E.-L. Geoffroy, *Hist. Ins. Env. Paris.* T. 1, pp. 13-14.

1767 Linnaeus 1766, 12 ed. This figure (2,964) includes arachnids, crabs, scorpions, ticks, mites, centipedes, millipedes, and a few other things now not thought of as insects.
Kirby and Spence 1828, 5 ed., p. 404 write that "In the last edition of the Systema Naturæ, and in its appendixes" there are 2,840 species of insects, including the arachnids.
1783 Broberg 1990, p. 59, citing Eberhard August Wilhelm von Zimmerman.
1788 du Petit-Thouars, p. 20, citing de la Mettrie's article, « Discours sur nos connoissances en Histoire naturelle » to be published in the *Journal d'Histoire Naturelle.*
1790 de la Métherie, p. 22.
1795 Berkenhout 1795.
1810 Delamétherie, p. 11.
1837 Jenyns 1837, p. 4, says that the number of known insects is at least 20 times the number in the 12th edition of *Systema naturae*, but doesn't say what that number is. By my calculations it should be 59,280 (= 20 * 2,964).
1848 Simon 1982, p. 68, citing
Schilder, p. 44, (Simon does not mention the page number), citing Leunis 1860 (for year 1848).
1858 Sabrosky 1952, p. 2. "Nearly 100,000 kinds of insects had been identified" in total in the hundred years since 1758 edition of *Systema natura.*
1859 Pratt 1912, p. 467, citing Agassiz and Bronn, 1859.
Simon 1982, p. 68, citing Schilder 1922, citing Agassiz and Bronn, 1859.
1869 Reclus, p. 562.
1886 Simon 1982, p. 68, citing Leunis and Ludwig 1886.
1898 Simon 1982, p. 68, and
Wieviel Tierarten gibt es? 1948, p. 78, both citing Moebius, 1898.
1900 Sabrosky 1952, p. 2. Estimate for 1900.
1912 Pratt 1912, p. .468.
Simon 1982, p. 68, citing Schilder 1922, citing Pratt, 1911. Same as Pratt, 1912.
1929 Simon 1982, p. 69, citing Hesse, 1929.
1930 Koller 1949, p. 54, citing Arndt 1930.
1931 Arndt 1941, p. 42, citing Aust 1931.
1932 Brues and Melander 1932, p. 8.
1935 Simon 1982, p. 69, and
Wieviel Tierarten gibt es? 1948, p. 78, both citing Pratt, 1935.
Wieviel Tierarten gibt es? 1948, p. 78, citing Eidmann, 1935.
1940 Metcalf 1940, p. 221.
1946 Wieviel Tierarten gibt es? 1948, p. 78, citing Shwanwitsch, 1946.
Metcalf, p. 329
1948 Sabrosky 1952, p. 6, for 1948.
1949 Palmer 1949, p. 382.
1952 Muesebeck 1952, p. 56.
Sabrosky 1952, p. 1.
1953 Simon 1982, p. 69, citing Mayr et al, 1953.
1954 Brues, Melander, and Carpenter 1954, p. 6.
1955 Simon 1982, p. 69, citing
Kaestner 1955, p. 12. (Simon does not give the page number).
1967 Kaestner 1967, p. 10.
1969 Mayr 1969, p. 10, cited also in
Simon 1982, p. 70. Mayr (p. 11) also says there are 838,000 arthropods.
1970 Simon 1982, p. 70, citing E. J. W. Barrington, *Invertebrate structure and function*,

London: Nelson, 1970.

1972 Simon 1982, p. 70, citing *Biology Data Book,* 1972, 2nd ed. Ed. P. L. Altmann and D. S. Dittmer. Bethesda, Maryland: Federation of American Societies for Experimental Biology, 1970. High and low estimates.

1973 May 1990, p. 295, citing V. Grant, *The origin of adaptations*. New York: Columbia University Press, 1973.

1982 Simon 1982, p. 70.

1985 Wilson, p. 700, citing R. H. Arnett, 1985. *American Insects*. New York, Van Nostrand Reinhold.

1988 Adis 1988, p. 115, citing E. O. Wilson, "The current state of biological diversity" in *Biodiversity*, Ed. E. O. Wilson. National Academic Press, Washington, DC, 1988, pp 3-18.

1992 Williamson 1997, p. 4, citing E. O. Wilson, E. O. *The diversity of life*. New York.,W. W. Norton and Co, 1992.

1995 Hawksworth and Kalin-Arroyo 1995, p. 118.

1999 Mangenot and Boureau 1999, p. 1357. Mangenot's figure.

Insect species estimated (Figures 8 and 24)

Data for 23 different years from 1691-2003; 32 references in total.

1691 Ray 1974, p. 7.

1744 Rooseboom 1970, p. 181, citing Pieter [Petrus] van Musschenbroek, *Oratio de sapientia divina*, Leiden: S. Luchtmans, 1744.

1749 Biberg 1749, 3 ed., p. 125.

1783 Broberg 1990, p. 59 citing Eberhard August Wilhelm von Zimmerman.

1803 Mouton-Fontenille de la Clotte 1803, p. 327.

1821 Kirby and Spence 1828, 5 ed., p. 490,
Jenyns 1837, p. 4, citing MacLeay, *Horae entomologicae,* part ii, 1821, p. 46, "annulose animals," i.e. including the crustaceans.
Westwood 1833, p. 117 cites page 469, not 46.
Cuvier 1836, p. 2 using the same figure. He probably got it from MacLeay, 1821, but does not mention that work.

1828 Kirby and Spence 1828, 5 ed., p. 488, high and low estimates. Kirby and Spence say that there are between 110,000 and 120,000 plants, and each plant has more than one insect associated with it. They give a ratio of British plants to insects of 1:6.
Westwood 1833, p. 118 also cites Kirby and Spence.

1833 Westwood 1833, p. 118.

1896 Gill, p. 583, citing "the late Dr. C. V. Riley."

1932 Brues and Melander 1932, p. 8. High and low estimates.

1943 Bisby and Ainsworth 1943, p. 18, citing Bessey's 1935 textbook (not named).

1952 Muesebeck 1952, p. 56.
Sabrosky 1952, p. 2. Both high and low estimates. Also cited in
Gaston 1991, p. 289.

1954 Brues, Melander, and Carpenter 1954, p. 6. High and low estimates. Also cited
Gaston 1991, p. 289.

1959 Stearn 1959, p. 9. High and low estimates.

1964 Wilson, p. 700 citing C. B. Williams 1964. *Patterns in the balance of nature*. New

York: Academic Press.
1973 May 1990, p. 295, citing V. Grant, *The origin of adaptations*. New York: Columbia University Press, 1973.
1982 Gaston 1991, p. 284, citing Erwin. "Tropical forests: their richness in coleoptera and other arthropod species." *Coleopterists' Bulletin*. 1982, 36: 74-75.
1990 May 1990, p. 300, citing I. D. Hodkinson and D. Casson. 1990. "A lesser predilection for bugs: Hemiptera (Insecta) diversity in tropical rain forests." *Biological Journal of the Linnean Society*. High and low estimates.
1992 Williamson 1997, p. 4, citing B. Groombridge, Ed. *Global biodiversity, status of the earth's living resources. A report compiled by the World Conservation Monitoring Centre*. London: Chapman and Hall, 1992.
1991 Gaston 1991, also cited by
Williamson 1997, p. 4.
1995 Hawksworth and Kalin-Arroyo 1995, p. 118. High, low and "working" estimates.
2000 Ødegaard 2000, pp. 592-593.
Ødegaard et al., p. 1186. High and low estimates.
2003 *Grzimek's Animal Life Encyclopedia* 2003, 2 ed., p. 11.

Fungus species described (Figure 9)

Data for 41 different years from 1620-2005; 51 references in total.

1620 Cain 1994, p. 343, citing Bauhin, *Pinax,* 1620.
1623 Paulet 1790, p. 88, citing G. Bauhin, *Pinax*, 1623.
1650 Paulet 1790, p. 90, citing Jean Bauhin, *Historia plantarum universalis, tribus tornis exhibita*. Ebroduni, 1650. (The publication date is mentioned as 1650 once and 1640 multiple times).
1654 Paulet 1790, p. 98, citing Loësel, *Catalogus plantarum in Borussiâ sponte nascentium*. Regiomonti, 1654.
1688 Paulet 1790, p. 181, citing John Ray, *Synopsis methodica*. 1686-1688.
1697 Paulet 1790, p. 203, citing Tournefort, *Élemens de botanique,* 1697.
1712 Paulet 1790, p. 118, citing Sterbeeck, *Theatrum fungorum ost het tonneel, &c.* Francius Van-Sterbeeck priester; Antwerpen. (This work was started in 1654, published 1675 and republished 1682 [Verflandegen hovenir] and 1712).
1719 Adanson 1966, p. lxxxiii, and
Paulet 1790, p. 218 , both citing Dillenius, *Catalogus plantarum spontè circà Gissam nascentium, cùm appendice pro supplendis institutionibus Rei herbariae Jos. Pitt. Tournefort*. Francofurti ad Moenum, 1719. Paulet says there were 84 species, Adanson says there were 200 species.
1729 Adanson 1966, p. lxxxiv, and
Paulet 1790, p. 343, both citing Micheli, *Nova plantarum genera juxta Tournefortii methodum disposita, &c. auctore Petro. Anton. Michelio, flor. ejusd. r. c. botanico*. Florentiae, 1729. Paulet says there were 1007 species, Adanson says there were 800 species.
1753 Linnaeus 1753. Hand counted.
Adanson 1966, p. lxxxvii, citing Gleditsch, *Methodus fungorum,* Berlin, 1753.
1763 Linnaeus 1762-1763, 2 ed.
Adanson 1966, p. lxxxiii.
1764 Linnaeus 1764, 3 ed.
1767 Linnaeus 1767 This number not actually in the book – but no new species of fungi

added to the *Mantissa*. Hand counted.
1771 Linnaeus 1771. Hand counted. 2 new species added to those in *Genera plantarum* 6th edition.
1790 Paulet 1790, p. xxxiii. Species and varieties.
1793 Paulet 1793. Hand counted.
1799 Lestiboudois 1799, 2 ed. Hand counted.
1828 Magnin-Gonze 2004, p. 171, citing Fries, *Systema mycologicum*, 1821 and *Elenchus fungorum,* 1828, combined.
1846 Lindley 1846, p. 797.
1862 Cooke 1895, p. 319, and
Saccardo, p. 174 citing Streinz, 1862. Cooke's and Saccardo's figures are very slightly different.
1889 Earle, p. 508, citing Saccardo, *Sylloge Fungorum*, as of 1889, as having described 40,000 species.
Saccardo himself wrote in Saccardo, p. 175 that there were 39,663 species in this work, the 10th volume of *Sylloge Fungorum*.
1892 Cooke 1895, p. 319, citing Saccardo, 1892.
1895 Current literature, p. 507, citing Saccardo, *Sylloge Fungorum*, as of 1895.
1899 J. C. A. , citing Saccardo, *Sylloge Fungorum*, as of 1899.
1902 Arthur, p. 68, citing the cumulative contents of the 16-volume set of Saccardo's *Sylloge fungorum*, Patavii, 1902. (The 16th volume just came out.)
Bessey, p. 794, same figure for Saccardo.
Earle, p. 508 referring to the same work says there were "almost 50,000 species" in it.
1929 Scientific notes and news, p. 307, citing *Sylloge Fungorum*, all fungi in 24 volumes.
1931 Martin 1951, p. 175, citing P. A. Saccardo, *Sylloge fungorum*, Pavia, vols. 1-25, 1882-1931.
1933 Bisby 1933, p. 248. Known seed plants outnumber known fungi 2:1.
1943 Bisby and Ainsworth 1943, p. 18.
1949 Palmer 1949, p. 50.
1956 Romagnesi 1956, p. 9. and
Ainsworth and Ciferri, p. 3, same figure.
Haupt 1956, 3 ed., p. 11, different figure.
1959 Stafleu 1959, p. 65.
1961 Ainsworth 1961, 5 ed., p. 277.
1971 Ainsworth, James, and Hawksworth 1971, 6 ed., p. 406.
1991 May 1991, p. 475.
1995 Hawksworth and Kalin-Arroyo 1995, p. 118.
1996 Blackwell, Vilgalys, and Taylor 1996
1998 Hudler 1998, pp. 13-14.
1999 Mangenot and Boureau 1999, p. 1356 Mangenot's figure.
2001 *Ainsworth & Bisby's Dictionary of the Fungi* 2001, 9 ed., p. 359.
Hawksworth 2001, p. 1423 Says that Kirk's estimates (2001) are a bit too high.
2005 Blackwell, Vilgalys, and Taylor 2005

Fungus species estimated (Figure 9)

Data for 19 different years from 1817-2000; 23 references in total.

1817 Cooke 1895, p. 319, citing Humboldt. No year given; assuming 1817.
1825 May 1991, p. 475, citing Fries, 1825.

1835 F. 1835, p. 81.
1872 Cooke 1895, p. 319, citing Cooke, 1872.
Cooke 1895, p. 319, citing De Bary, 1872.
1887 Cooke 1895, p. 319, Cooke, 1887.
1892 Saccardo, p. 178. High and low estimates.
1933 Bisby 1933, p. 248.
1943 Bisby and Ainsworth 1943, p. 16.
1951 Martin 1951also cited in
Ainsworth & Bisby's Dictionary of the Fungi 2001, 9 ed., p. 359. Martin says that the number of fungi is on the order of "good" seed plants known, which Kirk says was around 250,000 at the time. (Martin's article does not mention any number of seed plants at all. The number does seem valid when compared to seed plant estimates current in 1951).
1958 Gould 1958, p. 332.
1959 Stafleu 1959, p. 65. He says that there are probably at least as many fungi as flowering plants (the number of which he does not mention), and that he estimates that only one out of 7 fungal species has been described. 7 * 40,000 = 280,000.
1990 Hawksworth 2001, p. 1423, citing Pascoe, 1990.
1991 *Ainsworth & Bisby's Dictionary of the Fungi* 2001, 9 ed., p. 359, citing Hawksworth, 1991. Hawksworth's 1.5 million includes Oomycota and other organisms traditionally studied by mycologists.
1992 *Ainsworth & Bisby's Dictionary of the Fungi* 2001, 9 ed., p. 359; Hawksworth 2001, p. 1423, citing Hammond, 1992.
1994 Hawksworth 2001, p. 1424, citing Rossman, 1994.
1995 Hawksworth and Kalin-Arroyo 1995, p. 118, and
Hawksworth 2001, p. 1423, both citing Hammond, 1995.
1997 Hawksworth 2001, p. 1424, citing Cannon, 1997.
1999 Hawksworth 2001, p. 1424, citing Frohlich and Hyde, 1999.
2000 Hawksworth 2001, p. 1425, citing May, 2000.
Hawksworth 2001, pp. 1425, 1430, citing Arnold et al, 2000. Hawksworth accepts and promotes this figure. He says that 5.1 million species is a decent upper limit, but we should stick to the 1.5 million estimate for most purposes.

Fungus genera described (Figure 9)

Data for 31 different years from 1697-1971; 39 references in total.

1697 Paulet 1790, p. 203, citing Tournefort, *Élemens de botanique*, 1697.
1719 Linnaeus 1738,
Adanson 1966, p. lxxxiii, and
Paulet 1790, p. 217, all citing Dillenius, *Catalogus plantarum spontè ircà Gissam nascentium, cùm appendice pro supplendis institutionibus Rei herbariae Jos. Pitt. Tournefort.* Francofurti ad Moenum, 1719. Linnaeus gives a different number of genera, Adanson and Paulet give the same figure.
1729 Linnaeus 1738, p. 603,
Adanson 1966, p. lxxxiv, and
Paulet 1790, p. 266, all citing Micheli. *Nova plantarum genera juxta Tournefortii methodum disposita, &c. auctore Petro. Anton. Michelio, flor. ejusd. r. c. botanico*, Florentiae, 1729. Linnaeus said there were 31 genera, Adanson said

there were 30, and Paulet said there were 28.

1735 Linnaeus 1735.

1737 Linnaeus 1737b.

1738 Linnaeus 1738 Both the sexual system and the *Fragmenta* part have the same number.

1742 Linnaeus 1742, Editio secunda aucta & emendata ed. Includes *Byssus*.
Paulet 1790, p. 352, citing Haller, *Enumeration stirpium Helvetiae indigenarum*, Gottingae,1742.

1752 Linnaeus 1752, 4 ed. Includes *Byssus*.

1753 Linnaeus 1753.

1754 Linnaeus 1754, "Editio quinta ab auctore reformata et aucta" ed..

1758 Linnaeus 1758 Includes *Byssus.*

1759 Jussieu 1789, p. lxiii, citing Bernard de Jussieu 1759.

1762 Linnaeus 1762-1763, 2 ed.

1763 Adanson 1966, p. 8. Hand counted. Also cited in
Paulet 1790, p. 387.

1764 Linnaeus 1764, 6 ed.
Linnaeus 1764, 3 ed.

1767 Linnaeus 1767 *Mantissa plantarum.* This number not actually in the book , but no new genera of fungi were added to the *Mantissa*.

1771 Linnaeus 1771. *Mantissa plantarum.* No new genera of fungi added.

1787 Linnaeus 1787, p. 738.

1790 Paulet 1790, p. xxvi.

1793 Paulet 1793The number of genera he recognises as valid. Hand counted.

1799 Lestiboudois 1799, 2 ed. Hand counted.

1800 B 1816, Nouvelle ed., p. 39. Says that Ventenat in his *Tableau du Règne végétal* [1799-1800] has 19 genera of fungi, to which Rafinesque added 6 new genera. [He may be referring to Rafinesque's *Analyse de la nature, ou, Tableau de l'univers et des corps organises*, 1815.]

1807 B, Nouvelle ed., p. 39. Says that Persoon had 74 genera (hand counted; he lists them) – but did not say which work of Persoon's he was citing. Assuming 1807.

1824 Br. 1824Hand counted.

1846 Léveillé 1846. Hand counted.
Lindley 1846, p. 797.

1889 Earle, p. 508, citing Saccardo, *Sylloge Fungorum*, as of 1889.

1902 Earle, p. 508, citing Saccardo, *Sylloge Fungorum*, as of 1902.

1943 Bisby and Ainsworth 1943, p. 18. Estimate as of 1940.

1955 Ainsworth and Ciferri, p. 3

1958 Gould 1958, p. 332.

1961 Ainsworth 1961, 5 ed., p. 277.

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