CHAPTER SEVEN

Classification and natural history: 1800–1850

After our examination of the scope, theories, methods and geography of Coenraad Temminck's work and the ups and downs of his authority, the question that remains to be explored is: how do Temminck's discussions of systematics fit within the wider domain of the natural sciences? In other words, what was the role and status of systematics within natural history between 1800 and 1850? In order to tackle this point, we first need to define natural history itself. There are several possible approaches: looking at the definitions of natural history given by historians of science, looking at the definitions of—and attitudes towards—natural history of the naturalists themselves during this period, and exploring the ideas of the general public, patrons, institutions and government officials. All these are necessary to better understand the development of systematics, but in this chapter I will focus on the views of natural history and its branches of their practitioners, which is necessarily only a partial view. However, a look into the available definitions of natural history from the period between 1800 and 1850 will suffice to demonstrate that defining *natural history* is more difficult than it may seem. Defining it was by no means resolved half way through the nineteenth century, and it may still not be resolved. As Stevens remarks: "In order to talk about natural history in this era, at the very least one must qualify the use of the word. There is no monolithic natural history in the nineteenth century."[1]

Defining *natural history*

When Temminck published his first monograph on pigeons and gallinaceous birds in 1813, he justified the time and effort that had gone into his work by appealing to religious and utilitarian arguments. Temminck claimed that the study of natural history,

[1] Stevens, *Development of Biological Systematics*, 218.

or, more precisely, the "étude des trois règnes de la nature," brought not only joy and inner peace, it also brought its practitioner closer to the Almighty and kept young people safe from unspecified but tantalizing "séductions."[2] He also listed the advantages that natural history brought to men and society. Among other benefits, it allowed man to know himself and the workings of his internal organs, it made it possible to domesticate various species of animals for his profit and pleasure, it helped in finding fertile grounds for his crops, it helped him to navigate around the globe and brought him the most useful gifts, like additional foodstuffs and medicines. In this sense, that of usefulness, the study of pigeons and gallinaceous birds was of particular interest, being indispensable and of the greatest service to society, especially to the elite: this order of birds includes game birds, fowl and poultry.[3]

These kind of spiritual and utilitarian considerations preceding a work on natural history were common in late eighteenth and early nineteenth century books, but after 1813 Temminck would never again write an introduction like this. Just two years later, without a word on the benefits of ornithology, neither for the spirit nor for society, Temminck presented his *Manuel d'ornithologie* as what it actually was: a systematic work on birds to increase the knowledge of the European avifauna. His aim, he said, was to correct the errors and gaps in the existing works, deal with the nomenclatural chaos and improve the description of genera, and without much ado, offered an Index of the treated genera. Although Temminck dispensed with the spiritual preambles, he did highlight the utility of natural history on several occasions, and for different reasons. In 1835, for example, he wrote: "We seem to forget that the study of nature, apart from its intrinsic importance from the moral and scientific point of view, greatly influences civilization and industry, driving its springs, many of the arts and the propagation of enlightenment to all classes of society."[4]

But what did Temminck actually mean by natural history? He was never very explicit about what natural history actually included, or did not include, nor the place his work had in it and its relation to the other branches of natural history, like physiology or paleontology. From the *Discours préliminaire* from the *Histoire naturelle générale des pigeons et des gallinacés*, however, we can deduce that, for Temminck, natural history comprised anatomy, systematics, applied botany, physiology, geology and geography, but not astronomy.[5] A quick and by no means exhaustive look at some other definitions from this period, from 1790 to 1840, reveals that natural history was not as

[2] Temminck, "Discours préliminaire," *Pigeons et gallinacés*, vol. 1, 7, 9.
[3] Temminck, "Discours préliminaire," *Pigeons et gallinacés*, vol. 1, 13-14, 16.
[4] Temminck, "Introduction troisième partie," *Manuel d'ornithologie*, 2 ed., vol. 3, xxxvi-xxxvii.
[5] Temminck, "Discours préliminaire," *Pigeons et gallinacés*, vol. 1, 7.

well-defined a concept as its widespread use suggests.[6] Naturalists disagreed on what natural history was, what branches of the natural sciences were to be included in it, and whether or not it was part of their work to delve into philosophical considerations and *a priori* theoretical thoughts. The relative importance of each of its branches was also far from established. As we will see later on, the issue of delimiting the scope of the naturalist's domain greatly depended on whether one believed that the natural system could be achieved by the *practice* of classification, without a theoretical framework, or not at all.

In 1817, Georges Cuvier expressed his concern about this lack of consensus in defining natural history and felt the need to clear it up: "Few people have an accurate idea of natural history, it seems necessary to start our work by clearly defining the goals of this science and establishing the distinct limits between natural history and its related sciences."[7] He defined natural history, or "physique particulière" as the study of natural objects, including minerals and living beings, while the other natural sciences included the "physique générale" (physics, optics, astronomy and meteorology), and chemistry.[8] All these branches of the natural sciences pursued one goal, the discovery of the general laws governing nature, but each did so by different means and methods. Cuvier's classification of the natural sciences can be found almost literally in other textbooks from the beginning of the nineteenth century, like in John Stark's *Elements of Natural History* (1818) or Jan van der Hoeven's *Handboek der dierkunde* (1827).[9]

Jean-Baptiste de Lamarck held similar ideas regarding the scope of natural history. For Lamarck, it was the branch of natural sciences that pursued "the general and detailed knowledge of our globe and all its products born or made on its surface and its interior."[10] He excluded physics, chemistry and astronomy from natural history, and divided it into three big fields: zoology, botany and mineralogy.[11] For the first two fields, the study of living beings, Lamarck introduced the term *biology*—in quite the same sense we use it today.[12] Zoology encompassed comparative anatomy, physiology, the

[6] See the references previously given in the Introduction, "A brief review of historiography."
[7] Cuvier, "Introduction," *Règne Animal*, vol. 1, 1.
[8] Cuvier, "Introduction," *Règne Animal*, vol. 1, 2.
[9] John Stark, "Introduction," in *Elements of Natural History: Adapted to the Present State of the Science, Containing the Generic Characters of Nearly the Whole Animal Kingdom, and the Descriptions of the Principal Species*, vol. 1 (Edinburgh: W. Blackwood, 1828); Van der Hoeven, "Inleiding," *Handboek der dierkunde*, vol. 1.
[10] Jean-Baptiste de Lamarck, "Sur l'Histoire Naturelle en général," *Journal d'Histoire Naturelle* 1 (1792): 3-4.
[11] Lamarck, "Sur l'Histoire Naturelle en général," 4.
[12] Jean-Baptiste de Lamarck, *Hydrogéologie, ou Recherches sur l'influence qu'ont les eaux sur la surface du globe terrestre; sur les causes de l'existence du bassin des mers, de son déplacement et de son transport successif sur les différens points de la surface du globe: enfin sur les changemens que les corps vivans exercent sur la nature et l'état de cette surface* (Paris: Agasse et Maillard, 1802), 188.

description of life histories, geographical distribution and classification.[13] With regards to methodology, Cuvier and Lamarck agreed, if in nothing else, that physics, optics and chemistry were sciences based on experimentation and mathematical calculations, while natural history was based solely on observation and comparison, although it made use of the principles governing the rest of the natural sciences.[14]

The botanist Augustin P. de Candolle had a slightly different view of the matter. For example, he considered natural history to fall within the experimental sciences, together with physics and chemistry, all intimately linked and depending on each other's progress. Similarly, entomologist William Sharp MacLeay had also questioned whether natural history was a science of observation only, as it also depended on experience, while chemistry could not be considered only a science of experience, but one of observation, too. He also wondered whether mineralogy shouldn't be included in chemistry instead of in natural history. Much of what had been discovered in that field depended on calculation, and its affinity with chemistry was, for him, more evident than its affinity with natural history.[15] For de Candolle, astronomy, geology, mineralogy and meteorology were all branches of natural history dealing with inanimate matter, while zoology and botany were the branches studying living beings.[16] He also explained that, as a consequence of the differences between animals and plants, anatomy played the most important role in zoology, but botany had to settle for external characters and geographical distribution. De Candolle divided botany into three main subdisciplines: *glossology*, dealing with the terminology of the parts of plants; *taxonomy*, or the theory of classification; and *phytography*, the "art of describing plants in the way most useful to the progress of science." Interestingly, he regarded the "Botanique descriptive" and the "Synonymie Botanique" as "applied branches," in other words, as a practical field within botany.[17] De Candolle also considered vegetal physiology and pathology, and all applied disciplines like agriculture and botanical pharmacy, as branches of botany. All these disciples were, as Lamarck and Cuvier remarked, related to each other.[18]

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[13] Jean-Baptiste de Lamarck, "Discours préliminaire," in *Philosophie zoologique, ou exposition des considérations relatives à l'histoire naturelle des animaux; à la diversité de leur organisation et des facultés qu'ils en obtiennent; aux causes physiques qui maintiennent en eux la vie et donnent lieu aux mouvemens qu'ils exécutent; enfin, à celles qui produisent les unes le sentiment, et les autres l'intelligence de ceux qui en sont doués* (Paris: Baillière, 1809), 1-2.

[14] Lamarck, "Sur l'Histoire Naturelle en général," 8-9; Cuvier, "Introduction," *Règne Animal*, vol. 1, 4-5, 7.

[15] William Sharp Macleay, *Horae Entomologicae or, Essays on the Annulose Animals*, vol. 1 (London: S. Bagster, 1819), 174-75.

[16] Candolle, "Introduction," *Théorie élémentaire*, 3, 8-9.

[17] Candolle, "Introduction," *Théorie élémentaire*, 19.

[18] Candolle, "Introduction," *Théorie élémentaire*, 16-19.

The study of mankind posed similar problems. Jan van der Hoeven for instance, published—besides many other things—a series of articles on the natural history of man. In the first of these articles he warned the reader: "*Anthropology* should not be confused, as sometimes happens, with the natural history of man."[19] The former, he explained, covered a bigger area and it included the latter. Natural history dealt with the differences between man and animal, as well as with the differences between "people and people," that is, between racial and ethnic groups.[20] In order to address the first issue, comparative anatomy and physiology were essential, while the study of the different human groups required knowledge of the races populating the earth. The natural history of man was an "anthropological science," but was not Anthropology itself.[21]

The term *naturalist* was as elusive as *natural history* was. Entomologist William Sharp MacLeay wrote in 1821:

> Naturalists, it is said, may be classed like the objects of their study into genera and species; and in this classification places may be found for the comparative anatomist and physiologist to the mere collector who hoards a shell or pebble, simply that he may be gratified in the possession of that which his neighbour wants. The variety of pursuits embraced in the comprehensive term Natural History, is without doubt multifarious; but it may be questioned whether the title of *naturalist* be merited by the cultivator of any one or two of these pursuits, or indeed by any person who deems even the most ignoble of them unworthy of his attention.[22]

Temminck saw himself as a naturalist, but of what kind? He had turned from a 'mere collector' into an ornithologist working on bird systematics, to become the founder and director of one of the most prestigious natural history museums in Europe. Perhaps the failure to pin down what 'naturalist' actually meant, lay not so much in semantics but in the assumption that the role of the naturalist was static, as was the case for the field naturalist. But definitions did not need to be too complicated. For Temminck, a naturalist was an "observateur de la nature"—not terribly precise, but undeniably inclusive.[23] Temminck was not as keen on definitions as academics were. His books were not text books for students, he wrote for fellow naturalists dealing with the classification and life history of birds, so he offered no classical definition of the terms he

[19] Jan van der Hoeven, "Bijdragen tot de natuurlijke geschiedenis van den mensch. 1ste Bijdrage," *Tijdschrift voor natuurlijke geschiedenis en physiologie* 1 (1834): 87.
[20] Van der Hoeven, "Bijdragen tot de natuurlijke geschiedenis van den mensch," 88.
[21] Van der Hoeven, "Bijdragen tot de natuurlijke geschiedenis van den mensch," 89.
[22] Macleay, *Horae Entomologicae*, vol. 1, 327.
[23] Temminck, "Introduction troisième partie," *Manuel d'ornithologie*, 2 ed., vol. 3, lviv.

employed, as one can find in the writings of Cuvier or Van der Hoeven. Not incidentally, as both were lecturers with appointments in academic institutions. For a professor and author of textbooks, explications are mandatory. But Temminck had no obligations regarding teaching, the museum took no part in the activities of Leiden University and had no influence in it. Nor did Temminck wish to have any. His systematic monographs were written for systematists, not students, so quite possibly, defining natural history or explaining at length his classification methods seemed to be a superfluous exercise.

These few and admittedly random examples indicate that, although most early nineteenth century naturalists agreed on what natural history roughly was, the concept was not definite. For most of them, natural history was a branch of the natural sciences that included zoology, botany and mineralogy or, as most naturalists put it, the study of animals, plants and minerals. Zoology in particular dealt with the description, classification and arrangement of living beings, as well as with the discovery and description of their life histories and geographical distribution. Besides the scope and arrangement of each branch of the natural sciences, there remained three other points of discussion that proved to be much more significant for the practice of natural history. Firstly, should natural history include philosophical or theoretical discourses? Secondly, what was the status of anatomy and physiology in zoology? Thirdly, what was the status of classification and nomenclature in relation to the other branches of natural history?

The issue of philosophical arguments

The acceptance of philosophical considerations in natural history was, by far, the most prickly of the three questions. Some examples will suffice to illustrate the tension between the exponents of some form of natural philosophy and the adherents of a more empirical approach. From Lamarck's and Geoffroy Saint-Hilaire's ideas on organic evolution to the *Naturphilosophen* and the Quinarians, there was a variety of theoretical proposals during the first half of the nineteenth century. Although locally popular at some point or other, none of them were to last, and none of them provided naturalists with a framework that was both predictive and generally applicable in zoology and botany. As a result, systematists grew increasingly weary of *a priori* theories. MacLeay admitted as much when presenting what would be known as the Quinarian System: "This part of my work […] takes more the character of an hypothesis, and as such

deserves more suspicion."[24] Yet he was quick to clarify that this was not an hypothesis in the "Newtonian sense," but one "entirely dependent upon observed facts which its object is to connect," He added, though, that there were just not enough observations yet to remove the hypothesis-label.[25]

Cuvier's theory of the *conditions d'existence* and the principle of correlation of characters might be the most striking exception to the systematists veto against *a priori* theories. Interestingly, his theories never came to be perceived as philosophical, for several reasons. Firstly, Cuvier, an authoritative figure in and outside the Muséum, claimed that his findings were based on observation and comparison—just as MacLeay did. And authority mattered, at least until the 1840s. Secondly, Cuvier's ideas did provide systematists with a reliable starting point for the classification of animals, one that when applied to the higher taxonomic categories, gave classifications stability and predictability, hinting at the naturalness of these categories. Finally, his opposition to organic evolution as proposed by Lamarck and Geoffroy Saint-Hilaire reaffirmed the image of Cuvier as someone opposed to speculative philandering. As Paul Farber observes: "Most naturalists, however, were not very philosophical, and the impressive elucidation of what appeared to be an order in nature gave them the confidence that a natural system would be forthcoming. Perhaps more than anyone the person who did the most to reinforce this view was Georges Cuvier whose comparative anatomy was widely perceived to be the tool that would make the study of animals into a rigorous science."[26] So the validity of Cuvier's theories remained largely undisputed in zoology. Cuvier had his challengers, though.

Lamarck, for one, saw the search of universal laws as an integral part of natural history. So did Cuvier. But according to Lamarck, natural historians should also be enquiring into the causal principles behind the forms they observed, and into the relations between living beings and their environment. He believed that most naturalists were neglecting to do this:

> It would be a real hindrance for the natural sciences to persist in seeing only the observed objects, their form, their size, their external parts, even the smallest, their color, etc.; and if those dedicated to such studies refrain from rising to higher considerations, such as to investigate the nature of the objects they study,

[24] Macleay, *Horae Entomologicae*, vol. 1, 167.

[25] Macleay, *Horae Entomologicae*, vol. 1, 167. Macleay quoted Newton: "Quicquid [enim] ex phaenomenis non deducitur, hypothesis vocanda est" (Whatever is not deduced from the phenomena, has to be called an hypothesis). See William Thomson and Hugh Blackburn, eds., *Sir Isaac Newton's Principia: reprinted for Sir William Thomson ... and Hugh Blackburn* (Glasgow: J. Maclehose, 1871; reprint of the third edition 1726), 530.

[26] Farber, *Emergence of Ornithology*, 81; see also Stevens, *Development of Biological Systematics*, 197-209.

> the causes of the modifications and variations to which these objects are subject, the relations of these same objects among themselves, and with all the others that we know, etc.[27]

Lamarck thus considered that both zoology and botany included two closely dependent fields: philosophy, devoted to the development and discussion of the principles of biology, and observations, the descriptive and classificatory part, based on observation and comparison.[28] Although some naturalists agreed with Lamarck on the need of a philosophical basis for natural history, his *Philosophie Zoologique* and his transmutation theories were controversial and in the end, ridiculed. Cuvier himself considered Lamarck's incursions into philosophy nothing short of capriciousness: "A system established on such foundations may amuse the imagination of a poet; a metaphysician may derive from it an entirely new series of systems; but it cannot for a moment bear the examination of any one who has dissected a hand, a viscus, or even a feather."[29] The Cuvierian view endured.

Severe criticisms of the theories proposed during this period (Lamarckism, numerical classifications, *Naturphilosophie*) made systematists cast aside any theoretical approach to classification and focus on a more empirical approach to the study of natural diversity.[30] Amongst them was Temminck, who considered Cuvier's work as a "light to be followed," but further despised any incursion into the realm of philosophy.[31] In this Jan van der Hoeven wholly agreed with Temminck. Van der Hoeven noticed the tendency of the human mind to look for order and how it resorts to hypotheses, too impatient to first gather the necessary facts. But science should be based firstly on accurate and comprehensive knowledge, and then "the true theory will follow it, without anybody knowing how and when, and the solid building of science will be erected on the broad and firm basis of experience, to remain standing when the fairy castles of systems and opinions have evaporated and have disappeared from memory."[32]

Karl Illiger's thoughts went in the same direction. He pointed out the necessity of enlarging the bulk of described species as the only means available to increase the empirical basis for the naturalists' observations, and therefore the way to confirm or reject the existing systems, "for we can never have enough observations if we want to

[27] Lamarck, "Discours préliminaire," *Philosophie zoologique*, 12-13.
[28] Lamarck, "Sur l'Histoire Naturelle en général," 8-9.
[29] Georges Cuvier, "Biographical Memoir of M. de Lamarck," *Edinburgh New Philosophical Journal* 20 (1836): 15.
[30] Farber, *Emergence of Ornithology*, 140-41; Mayr, *The Growth of Biological Thought*, 203; Stevens, *Development of Biological Systematics*, 222.
[31] Temminck, "Introduction troisième partie," *Manuel d'ornithologie*, 2 ed., vol. 3, xv, in response to Quinarianism.
[32] Van der Hoeven, "Bijdragen tot de natuurlijke geschiedenis van den mensch," 91.

use them as bases for entirely reliable conclusions."[33] Remarkably, the initial departing point of quinarians like William Sharp Macleay was quite similar, or so he claimed: "In natural philosophy a system has usually indeed been considered as synonymous with an hypothesis; but the two ideas expressed by these words have of late been very properly distinguished by observing that though a mere fiction or hypothesis may explain phænomena, yet a system is a certainty that must be deduced by these."[34] MacLeay's quinarian system was, for him, not an hypothesis, but the result of the discovery of the elusive natural system—after all, he noted, "the strength of this beautiful theory" was irrefutable because "different persons have respectively stumbled upon it in totally distinct departments of the creation."[35]

The line separating hypothetical conjectures from theoretical premises was quite blurred during this period. Although Temminck emphatically defended the idea of an empirical, observation-based approach in systematics, rejecting any *a priori* construction, he did embrace typological thinking as the basis for his genera and therefore, as the foundation of his classificatory practice. His law on the distribution of genera around the globe was also born from this type concept. Temminck argued that his law was based on the observation of the facts, as MacLeay did for the quinarian system, but this was not the case. It was an hypothesis, based on the *idea* of morphological types and the extrapolation of a few observations. The Belgian botanist August Drapiez, for example, grasped the strong philosophical background of Temminck's classification in the *Manuel d'ornithologie*, which was, apparently, in strong contrast with the luxurious iconographic works: "The work we are reviewing here is, in this respect, beyond reproach, and the second edition, which closely followed the first, proves that to the present day, these studies have a truly philosophical purpose."[36]

Temminck had tried to marry the type concept to a perception of a continuous nature by meticulously identifying within each group, whether it was an order, a genus or a species, variations that deviated from the types and then deciding which type these variations resembled the most, and classified them accordingly. By 1840, Temminck had started doubting the possibility of delimiting genera and species in a clear-cut way, which led him to wonder if species as such even existed in nature and recognized the

[33] Illiger, "Einige Gedanken über die Begriffe," xxxiii; translated by Mayr, "Illiger and the Biological Species Concept."
[34] Macleay, "Horae Entomologicae," vol. 1, x.
[35] William Sharp Macleay, "Remarks on the Identity of Certain General Laws which have been lately Observed to Regulate the Natural Distribution of Insects and Fungi," *Transactions of the Linnean society of London* 14 (1825): 63. For a discussion of numerical classification in botany, especially in Victorian Britain, see Endersby, "Classifying Sciences."
[36] Drapiez, "Manuel d'ornithologie," 276.

problems posed by defining genera based on a morphological type concept.[37] This idea had forced him to try to define as precisely as possible the set of characters that should be used to delimit genera, and to keep his genera as big as possible (in the sense of the number of species it included). Nonetheless, he carried on, confident the truth would reveal itself with time and patience, even when his theoretical framework was being shaken by the sheer amount of forms and varieties being discovered each passing day.

While Temminck was convinced of the possibility of achieving a natural classification by this method, others, like Jussieu and Lamarck, concluded that continuity in nature implied, by definition, that any classification had to be artificial.[38] Which starting point one adopted inevitably led to different definitions of what constitutes a natural taxon, to a different methodology and correspondingly, to different classification systems. In any case, continuity was as much an hypothesis as were Temminck's morphological types. Over one hundred years later, Carl Hempel appropriately explained the relation between classification and assumed hypothesis: "If a particular way of analyzing and classifying empirical findings is to lead to an explanation of the phenomena concerned, then it must be based on hypotheses about how those phenomena are connected; without such hypotheses, analysis and classification are blind."[39]

In Britain, Hugh Strickland also openly declared his aversion towards philosophical ideas in natural history. In 1844, he envisioned a time when "more definite principles of classification may hereafter be discovered, and meantime all that we can do is to arrange our systems according to sound reason and without theoretical prepossession."[40] Temminck would have emphatically agreed. However, Strickland felt some sympathy for those taking the innovative road. He thought that the Quinarian system, however wrong, had taken ornithology further by helping define the terms *analogy* and *affinity*, as well as by promoting fruitful discussions, much in the same manner that alchemy had been the precursor of chemistry, and astrology of astronomy. When speaking of Lorenz Oken and his system of animal classification, based on the five senses of man as sketched in his *Grundriss der Naturphilosophie* of 1802, Strickland did celebrate his creativity: "This doctrine is far too fanciful to stand the test of common sense, but it is certainly very ingenious, and we may admit that *se non é vero é ben trovato*."[41] But

[37] Temminck, "Introduction troisième partie," *Manuel d'ornithologie*, 2 ed., vol. 3, xix.
[38] Jean-Baptiste de Lamarck, *Encyclopédie méthodique. Botanique*, 8 vols. (Paris, Liège: Panckoucke, Plomteux, 1783–1808), 630; Stevens, *Development of Biological Systematics*, 25-28.
[39] Carl G. Hempel, *Philosophy of Natural Science*, Foundations of Philosophy Series (New Jersey: Prenctice Hall, 1966), 13.
[40] Strickland, "Report on the Progress," 217.
[41] Strickland, "Report on the Progress," 176.

Temminck could not bring himself to be amused by these ideas, while the botanist Joseph Hooker had reached, it seems, a boiling point: "I have been groaning over 'Rejuvenescence' que Diable! When is this German rubbish to end?"[42]

FIGURE 7.1. Portrait of Hugh Edwin Strickland (1811–1853), by Thomas Herbert Maguire.

In the end, Strickland ultimately got trapped in the same paradox as Temminck had been, but he did so knowingly. While claiming to steer clear from any metaphysical concepts, the foundation for his classification system was based on admittedly ill-defined concepts, including the concept of species and the notion of the essential characters used to define species. In 1841, in an article confidently entitled "On the true method of discovering the natural system in zoology and botany," he remarked:

[42] Joseph Hooker to Thomas Henry Huxley, 12 September 1854, in Leonard Huxley, ed. *Life and Letters of Sir Joseph Dalton Hooker, O.M., G.C.S.I. Based on Materials Collected and Arranged by Lady Hooker* (London: John Murray, 1918), 425. Hooker had just read a translation of Alexander Braun's *Betrachtungen über die Erscheinung der Verjüngung in der Natur* (Leipzig: W. Engelman, 1851), or the "Rejuvenescence of Plants." Also in Endersby, *Imperial Nature*, 217.

> It is probable that most naturalists at the present day have an instinctive belief in the existence of a natural system in Zoology and Botany, but there are very few who if questioned on the subject could give any clear explanation of the grounds of their belief, of the nature of that system, or of the mode by which a knowledge of it may be attained. The uncertainty which hangs over the subject is doubtless owing to the obscure and metaphysical nature of some of the principles involved, and still more to the vague conceptions and crude theories which have been promulgated on the subject.[43]

And immediately after that, he added: "The postulate with which I commence the inquiry is, to let it be granted that there are such things as species, distinct in their characters and permanent in their duration." After admitting this, he then proceeded to define the natural system as an arrangement of species according to the resemblance of their essential characters. Nevertheless, he omitted to define species or what made a character essential, except that the latter question was linked to "an estimate to the physiological importance" of the characters, that is, in a Cuvierian sense.[44]

Despite the fact that zoological and botanical classification were, necessarily, based on a set of basic assumptions, often unquestioned, natural philosophy and systematics remained formally separated niches for most naturalists. Cuvier's views were universally accepted as empirically proven, with the exception, possibly, of catastrophism. With the arrival of the Lyell's *Principles of Geology*, published in three volumes between 1830 and 1833, this particular discipline launched itself into the theoretical, speculative arena after decades of controversy, when geologists had sought to avoid discussions by focusing mostly on stratigraphy. According to James Secord, the *Principles* was "the most thoroughgoing vindication of geology as a sophisticated and philosophical enquiry."[45] Botany was in a similar predicament as zoology. Joseph Hooker, for example, strongly wished for new ideas to arrive that would take botany out of its impasse. In 1856, in a review of Alphonse de Candolle's *Géographie botanique*, he lamented "the vagueness of its principles, the inexactness of its methods, the puzzling complexity of its phenomena, and the purely speculative character of those hypotheses upon which all inquirers base their efforts to explain its facts and develop its laws."[46] Until the time came that botany would overcome such shortcomings, Hooker

[43] Strickland, "On the True Method of Discovering the Natural System in Zoology and Botany," 184.
[44] Strickland, "On the True Method of Discovering the Natural System in Zoology and Botany," 184.
[45] James A. Secord, *Visions of Science Books and Readers at the Dawn of the Victorian Age* (Chicago: University of Chicago Press, 2015), 144.
[46] Joseph Dalton Hooker, "Notices of Books. Géographie Botanique Raisonée [...] par M. Alph. de Candolle. Concluding remarks," *Hooker's Journal of Botany and Kew Garden Miscellany* 8 (1856): 249.

mainly worked on defining the principles on which his classification system was based, focusing mostly on genera, orders and classes.[47]

Classification was not atheoretical. As Paul Farber puts it in *The Emergence of Ornithology*: "The lack of agreement on the foundations of a natural system, even on the method of establishing such foundations, should not cause us to overlook the fact that it was such a search that occupied a significant portion of naturalists throughout the ornithological world."[48] Temminck was convinced that by elaborating on Cuvier's methods and enlarging and refining classifications, one could finally build the natural system, the perfect classification with objective groupings and a stable nomenclature, that reflected the true and divine order in nature. Yet, Temminck's work was bound by his own concepts of species and genera, typological thinking and the method he used to weight the characters to delimit any taxonomic group. So were all naturalists working on zoological classification. Their classifications were based on particular concepts (genus, species, essential characters, affinity, the natural system), but not necessarily on a well-articulated explanatory theory about the overall structure of the natural world. Even for those who avoided *a priori* hypotheses, the understanding of cause and effect, of the diversity they saw, its geographical patterns, the origin of (and reason for) similarities between groups, all of this was based on these pre-existing concepts. But universally accepted theories remained elusive.

The general mood was one of expectation, a period of waiting for that moment when naturalists could take a step back from their classifications and see, visualize, the true, natural order according to which groups were arranged and species related to each other—not unlike a painter taking a step back from a giant mural, or somebody finishing a colossal jigsaw puzzle without having beforehand a picture of what it represents. In other words, around the 1840s there was a general belief that more knowledge was needed, not more hypotheses. The understanding of the big picture would come from more specimens, more data on their geographical distribution, especially from those unexplored corners of the planet, and more research on anatomy and physiology. No theory would instantaneously answer all their questions about the general organization of the natural world. With their classification systems, Temminck, Strickland and Hooker were recognizing an order that was already there, a structure slowly being uncovered, while Oken and MacLeay used their knowledge to create a new one that might explain nature; but in general, systematists were looking for explanations in their classificatory systems and in the physical objects that constitute

[47] Endersby, *Imperial Nature*, 211-13.
[48] Farber, *Emergence of Ornithology*, 113-14.

them. The rise and great popularity of experimental biological sciences might be partly a consequence of this expectation.

The status of anatomy and physiology

Anatomy and physiology were disciplines that had been closely linked to medicine for something like two hundred years , but they had only recently been applied to systematics, mainly thanks to the work of Cuvier and Geoffroy Saint-Hilaire. Most naturalists, if not all, agreed in including physiology and anatomy under the umbrella of natural history, especially zoologists—at least, during the first half of the nineteenth century. After that, things quickly changed.

Karl Illiger pointed out in 1800 that Cuvier's principles were to be taken as a starting point for choosing the characters to define species and genera. Most weight should be given to physiologically important characters, those fundamental for the vital functions of the organism (*Lebensverrichtungen*).[49] Similarly, as we have seen earlier, Temminck thought that these two fields were "the fundamental sciences and starting point" for the construction of zoological classifications, and he focused on elaborating on the groups laid down by Cuvier.[50] Likewise, zoologist Henri Milne-Edwards from the Muséum (as devoted to Cuvier's principles as Temminck), defined anatomy and physiology "as the foundation of Natural History."[51] Similarly, for the entomologist John Obadiah Westwood, comparative anatomy was so important that he defined entomology as "that branch of zoology which treats of the insect tribes, as restricted by the knowledge obtained by the elaborate researches of modern comparative anatomists."[52] After Cuvier, then, zoology was based on anatomy and physiology, complemented by systematics, the life histories and the distribution of animals. It seems surprising then, that whether physiology and anatomy truly belonged within natural history could be an elusive question. Yet it was, possibly because these disciplines had been connected to medicine for much longer than they had been connected to zoology or botany.

We can have a glimpse of the different views of the relations between the natural sciences—in the broadest sense—by looking at the subjects included in natural history journals. For example, in 1824, an all-embracing journal of natural sciences had been

[49] Illiger, "Einige Gedanken über die Begriffe," xli.
[50] Temminck, "Introduction troisième partie," *Manuel d'ornithologie*, 2 ed., vol. 3, xv-xvii.
[51] Henri Milne-Edwards, "Cours Élémentaire de Zoologie. Notions préliminaires," in *Cours élémentaire d'histoire naturelle. A l'usage des Collèges et des Maisons d'Éducation, rédigé Conformément au Programme de l'Université du 14 septembre 1840. Zoologie* (Paris: Fortin, Masson et Cie, 1841), 11.
[52] John Obadiah Westwood, "Introduction," in *The Entomologist's Text Book. An Introduction to the Natural History, Structure, Physiology and Classification of Insects, Including the Crustacea and Arachnida* (London: W. S. Orr and Company, 1838), 3.

launched in France, the *Annales des Sciences Naturelles: comprenant la physiologie animale et végétale, l'anatomie comparée des deux règnes, la zoologie, la botanique, la minéralogie et la géologie.* The editors of the *Annales* explained that botany and zoology had divergent classification methods as a direct consequence of the anatomical differences between plants and animals. Zoologists were able to classify animals by means of comparing their internal organs and using the characters most relevant to their survival, therefore grouping them according to their similarities. Botanists, however, had been obliged to use external characters and the sexual system of Linnaeus, because of the obvious lack of such internal organs in plants. Therefore, the editors noted, zoological anatomy was focused on function, but botanical anatomy studied form, and while animal physiologists concentrated on pathology, vegetal physiologists worked basically on chemistry. As a consequence, zoology was far from standardized and animal classification seemed to be in disarray. Ichthyologists, ornithologists, conchologists and entomologists were all using their own systems, oblivious of the classification principles used by others. Botanists, within their constraints, had achieved a uniform system of classification, but anatomists treated plants as if they had no organic functions, and chemists studied plants as if they had no organs. And as a result, the editors concluded, the two organic kingdoms had been drifting apart from the beginning. The same was true for the geological sciences. Hence the need of a journal encompassing all disciplines.[53] This summary by the editors of the *Annales* offers an accurate snapshot of the difficulties of settling methodology and standardizing processes within natural history.

Meanwhile, in London, the editorial board of the *Zoological Journal* was debating whether certain articles should or should not be included in the first issue of their magazine, which was due to appear in October 1824. In the Introduction, they stated that they considered comparative anatomy, zoological classification, monographs on particular groups, animal chemistry and "palaeontography" as the most important subjects, in this particular order. Comparative anatomy "claims a very high rank in Zoological researches.[...] It has, at length, in great measure rescued one branch of natural history from the confusion and absurdity in which, whilst the structure of the habitation only and not that of the inhabitant was considered, its arrangements were involved."[54] And yet, the board chose a subject completely different to open their first

[53] Jean Victor Audouin, Adolphe Brongniart, and Jean-Baptiste Dumas, "Introduction," *Annales des Sciences Naturelles: comprenant la physiologie animale et végétale, l'anatomie comparée des deux règnes, la zoologie, la botanique, la minéralogie et la géologie* 1 (1834).

[54] Thomas Bell, John George Children, James de Carle Sowerby et al., "Introduction," *The Zoological Journal* 1, no. 1 (1824): iv.

issue. It began with an article entitled "An inquiry respecting the True Nature of Instinct, and of the Mental Distinction between Brute animals and Man," by John Oliver French. The essay explored the relation between consciousness, rationality, morality and intelligence. Hardly the stuff one might expect in a British zoological journal now, but Fréderic Cuvier had also dedicated quite some time to the study of the limits between instinct and intelligence, and he even designed an experiment using, rather unexpectedly, beavers as subjects.[55] Regarding French's essay, the editors of the Zoological Journal noted: "It necessarily enters into the detail of facts purely Zoological and in the highest degree interesting—and if the main question be metaphysical, it is from natural history alone that its ingenious author derives his arguments in discussing it."[56] Apparently, philosophical and moral considerations were interesting enough to be considered in zoology, as long as they did not affect methodology nor the classification systems.

Ten years later, in the Netherlands, Jan van der Hoeven and his friend, the doctor and botanist Willem Hendrik de Vriese, launched a new journal devoted to natural history, and they struggled with the same questions. The new journal was meant to be a continuation of the *Bijdragen tot de Natuurkundige Wetenschappen* (Contributions to the Natural Sciences) which had stopped being published two years before, in 1842. The *Bijdragen* covered all branches of the natural sciences, from natural history to astronomy, hydrology, chemistry and even engineering. The title of their new journal was rather succinct compared to its French counterpart. They entitled it *Tijdschrift voor Natuurlijke Geschiedenis*, or Journal of Natural History, indicating already a substantial reduction of the number of subjects they were willing to include in it. In the introduction to the first issue, the scope was described as follows: "natural history of the three kingdoms, plant and animal anatomy and general physiology."[57] Articles on pathological anatomy were also welcome because animal anatomy was closely related to human anatomy. Essays on applied and medicinal botany were equally interesting and fell within their scope. The editors concluded that, in the end, they would not make any "fearful choice," as their goal was only to promote science and all scientific research was, in one way or another, of practical interest.[58] But after the first year, the editors considered it wise to change the title of the journal into a slightly expanded one: *Tijdschrift voor Natuurlijke Geschiedenis en Physiologie*, adding physiology to the original title and, with that,

[55] Pierre Flourens, *Résumé analytique des observations de Frédéric Cuvier sur l'instinct et l'intelligence des animaux* (Paris: C. Pitois, 1841); David Rains Wallace, *Mountains and Marshes: Exploring the Bay Area's Natural History* (Berkeley: Counterpoint, 2015), 104.

[56] Bell, Children, de Carle Sowerby et al., "Introduction," vi.

[57] Van der Hoeven, "Voorberigt," *Tijdschrift voor Natuurlijke Geschiedenis* 1, no. 1 (1834): ii.

[58] Van der Hoeven, "Voorberigt," i-ii.

divorcing physiology from natural history. In a *Naschrift*, or Post Scriptum at the end of the first volume, Van der Hoeven and De Vriese declared that nothing had changed from the original plan, but the title was extended "to avoid misunderstandings."[59] Unfortunately, the editors did not elaborate on the nature of these misunderstandings, but their amended title does reflect at least the existence of doubts on the inclusion of physiology and anatomy in natural history, which was apparently regarded as mostly descriptive and classificatory.

If Van der Hoeven and De Vriese set physiology apart from natural history, the German professors Carl Theodor von Siebold, from the Freiburg University, Ernst Heinrich Ehlers, from the University of Erlangen, and Albert Kölliker, from Würzburg, went a step further. They founded the *Zeitschrift für wissenschaftliche Zoologie*, or Journal for Scientific Zoology, in 1848 (only four years behind the *Tijdschrift voor Natuurlijke Geschiedenis en Physiologie*). They introduced their new journal as follows:

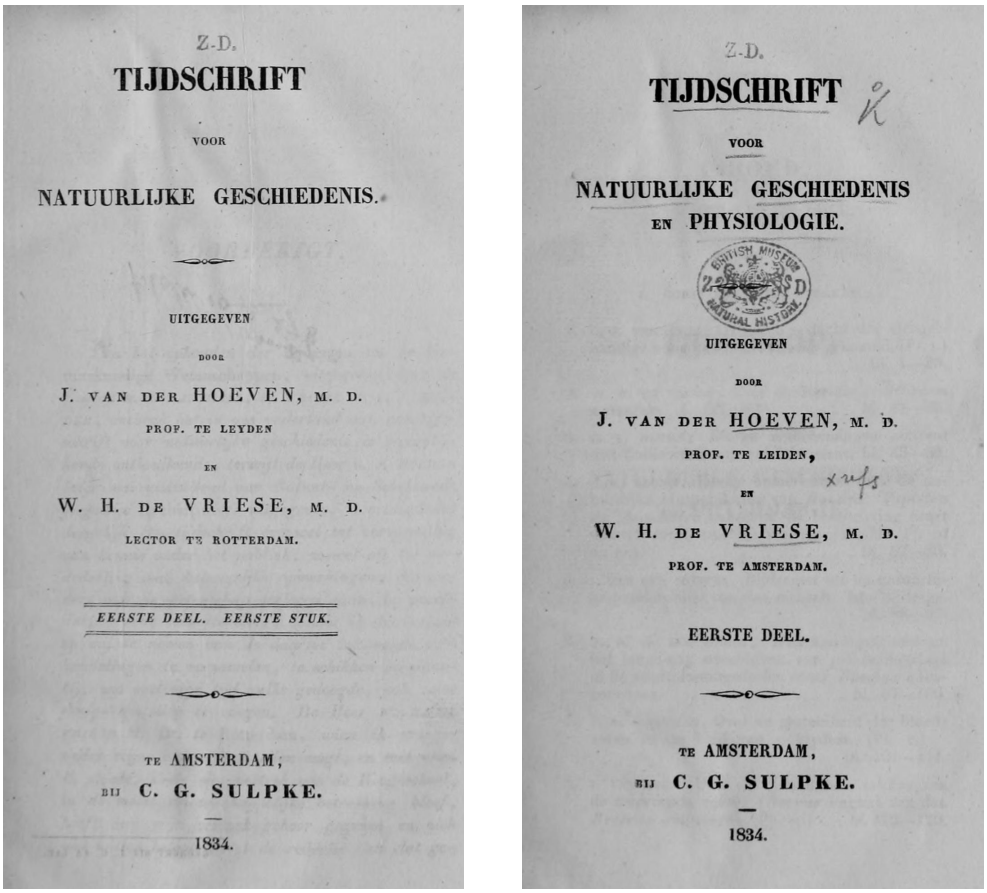
> We desire to give our journal the most scientific character possible [...] To this purpose we exclude all announcements of new genera and species that do not relate to this task, unless these offer us a more thorough-going insight into plant and animal structure, into the life history of animals and plants, or in the lawful organization of the organic realms.[60]

The editors remarked that there was clearly a distinction to be made between "simple notes and natural history news" on the one hand and "the truly scientific side of botany and zoology" on the other. Anatomy and physiology received undisputedly the stamp of "scientific," but classification and nomenclature fell short, apparently. Morphological studies were no longer included in natural history, which was synonymized with classification, as Van der Hoeven and De Vriese had done. This was a significant shift, as Lynn Nyhart puts it: "When researchers choosing to separate themselves from natural history identified the part (taxonomy) with the whole (natural history), they made invisible much of the intellectual work that gave taxonomic work its life."[61]

This process started to gather momentum in the 1850s and this would continue during the second half of the nineteenth century; it had a profound effect on the status of both the experimental sciences and systematics. By 1900 the notion that natural history was mainly occupied with classification and life histories was ingrained in the

[59] Jan van der Hoeven and Willem Hendrik de Vriese, "Naschrift van het eerste deel," *Tijdschrift voor Natuurlijke Geschiedenis* 1, no. 1 (1834).
[60] Ernst Heinrich Ehlers, Albert von Kölliker, and Karl Theodor Ernst von Siebold, "Prospectus," *Zeitschrift für Wissenschaftliche Zoologie* 1, no. 1 (1848); quoted by Lynn K. Nyhart, "Natural History and the 'New' Biology," in *Cultures of Natural History*, ed. Nicholas Jardine, James A. Secord, and Emma C. Spary (Cambridge University Press, 1996), 429.
[61] Nyhart, "Natural History and the 'new' biology," 429.

scientific and public minds, and biology became the term that encompassed all disciplines dealing with living beings, just as natural history had been in the 1800s, but in those days also including geology and mineralogy.[62]


Z-D.

TIJDSCHRIFT

VOOR

NATUURLIJKE GESCHIEDENIS.

UITGEGEVEN

DOOR

J. VAN DER HOEVEN, M. D.

PROF. TE LEYDEN

EN

W. H. DE VRIESE, M. D.

LECTOR TE ROTTERDAM.

EERSTE DEEL. EERSTE STUK.

TE AMSTERDAM;

BIJ C. G. SULPKE.

1834.

Z-D.

TIJDSCHRIFT

VOOR

NATUURLIJKE GESCHIEDENIS

EN PHYSIOLOGIE.

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EERSTE DEEL.

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BIJ C. G. SULPKE.

1834.

FIGURE 7.2. *Left*: Title page of the first issue of the *Tijdschrift voor Natuurlijke Geschiedenis. Right*: The same journal's title page for the first volume, including issues one and two, with its new title, now including *Physiologie*.

[62] For literature on the decline of systematics toward the end of the nineteenth century up to today see for example W. Ford Doolittle, "The Practice of Classification and the Theory of Evolution, and what the Demise of Charles Darwin's Tree of Life Hypothesis Means for both of them," *Philosophical Transactions of the Royal Society of London. Series B, Biological Sciences* 364, no. 1527 (2009); Kevin de Queiroz, "Systematics and the Darwinian Revolution," *Philosophy of Science* 55, no. 2 (1988); Mayr, *The Growth of Biological Thought*, chap. 6; James A. Secord, "The Crisis of Nature," in *Cultures of Natural History*, ed. Nicholas Jardine, James A. Secord, and Emma C. Spary (Cambridge University Press, 1996); Stevens, *Development of Biological Systematics*, chap. 9.

It is very well possible that the editors of the *Annales des Sciences Naturelles* had pointed to the source of the problem, or at least one of them. The different methods used in botanical and zoological studies, mainly in systematics, physiology and anatomy, may have been responsible for the confusion about the exact meaning of each of these disciplines. The boundaries of anthropology and even the very definition of what being *human* meant were also blurry. Some defined natural history by the object of its research, others by its goals, and then others did so by focusing on methodology. To complicate matters more, there was no consensus on whether theoretical or idealistic premises should be accepted as a valid starting point. The experimental sciences were on the rise, and classical terms like natural history were being re-defined. All these issues had far-reaching consequences, because the status of each discipline determined its place within the natural sciences, the authority of its practitioners and the scientific institutions they related to, its public image, its stability and its progress.

Systematics within natural history

Peter F. Stevens describes the practice of botany at the turn of the nineteenth century as "ahistorical natural history," that is, as mainly occupied with description and classification: "as the nineteenth century began, botany proper and classification were near synonyms; they excluded physiology and certain kinds of anatomy and were distinct from philosophical botany. And classification was seen by many naturalists themselves as an endeavor that owed little to theoretical considerations."[63] Stevens also notes the diminishing prestige of botanical systematics both among naturalists and the general public, but the situation was different for zoology.[64] At the turn of the nineteenth century, zoology included other disciplines besides classification—not only in theory but also in practice. The highly praised work of Cuvier on comparative anatomy and paleontology, Lamarck's and Geoffroy Saint-Hilaire's theories of organic change, the German *Naturphilosophie* and the British Quinarian system, pathological anatomy, physiology and ethology, were all included in zoology. What is more, all of them contributed in some manner to the development of zoological classification. This situation lasted until the second half of the nineteenth century, when things started to shift and natural history acquired a different meaning rather quickly. So, what happened? What was the status of zoological classification within natural history before the 1850s, amidst all this activity? Was it the main focus of naturalists, only part of it, or just a practical tool?

[63] Stevens, *Development of Biological Systematics*, 208.
[64] Stevens, *Development of Biological Systematics*, 209-12.

Between 1800 and 1850, there were two main currents of thought regarding the status of systematics within natural history or, in other words, two different attitudes towards it. Within the first one, systematics was seen as a refinement of the groundwork of comparative anatomy and paleontology laid by Georges Cuvier, and it was regarded as the sure path to uncover the true organization of nature. Although based on anatomy, systematics was considered to be a discipline in and of itself. A second group of naturalists saw classification and nomenclature as a practical tool for natural history, and not as a discipline on its own. But again, nothing was as simple as this apparently straightforward dichotomy seems: systematics was either a discipline, or a tool. It all depended on a single fundamental question: should the point of departure for classification be natural philosophy? Other issues in systematics (its goals, methods, practice, the subject of its study, or a combination of all of them) were influenced by this primary question.

Adherents of the first perspective, who considered systematics to be a true discipline of natural history, included, as expected, Cuvier himself, but also Karl Illiger, Coenraad Temminck and Hugh Strickland. Cuvier made the distinction between anatomy and systematics quite clear in 1805 when he wrote:

> The development of methods is the goal of natural history itself. Anatomy receives them, as it were, ready-made. It is from them that anatomy takes its first directions: but soon it illuminates them with its own light; anatomy is the best test for their truth; it is by applying a method of natural history to anatomy that we are able to judge whether or not it is in accordance with nature.[65]

By "methods" Cuvier was referring to the natural classification systems arrived at by following his principles of correlation and the *conditions d'existence*. Anatomical laws and classification systems emerged independently and each of them could be used to test the truthfulness of the other, that is, to see whether they were reflecting nature's organization. In practice, however, Cuvier focused mostly on comparative anatomy to characterize the higher ranks of animal taxa, without working out the details of the lower groups like genera and species.[66] Or, as MacLeay put it, a touch disappointed: "no person of such transcendent talents and ingenuity ever made so little use of his observations towards a natural arrangement as M. Cuvier."[67] Finishing what Cuvier left in the pipeline was the task Temminck had set himself to carry out, for the birds and the mammals.

[65] Georges Cuvier, *Leçons d'anatomie comparée*, vol. 1, 64.
[66] Farber, *Emergence of Ornithology*; Coleman, *Georges Cuvier, Zoologist*, 67.
[67] Macleay, *Horae Entomologicae*, vol. 1, 326.

Illiger, Temminck, Milne-Edwards and Strickland all thought this was the way to go in systematics, Cuvier-style. By borrowing from anatomy the knowledge needed to weight the characters for their classifications, which gave each anatomical feature its physiological importance, they could construct the natural system and elaborate on the basic groups defined by Cuvier. Comparative anatomy would guide them in their major groupings, while systematics would either prove or disprove the solidity of these higher taxa. It was at the very base of systematics, but comparative anatomy was *not* synonymous with systematics. Temminck remarked repeatedly that while his classification methods were built upon the principles of comparative anatomy, osteology and physiology, this was not sufficient on its own. The following passage gives a hint of Temminck's feelings on systematics being an independent discipline:

> Many anatomists find themselves hindered in their research because they have neglected to study the characters provided by zoology: they believed that comparative anatomy alone would guide them in their studies of the secondary levels [lower taxa]; and they have lost sight of this auxiliary science because of their underlying belief in the importance of anatomical research. Their research cannot be of use to us today, as it is often impossible to determine with exactitude on which species of animal they based their observations, while those that are still carrying on without the help of any basic notions of zoology are not contributing to this field, which pursues the perfection of the natural system.[68]

Here, Temminck used the term *zoology* as a synonym of systematics. In it, he included the careful comparison of external characters, the scrutiny of varieties (geographical as well as variations due to age and sex), life histories and geographical distribution, all indispensable for defining the lower taxa, especially genera and species.

Strickland's *Desiderata* from his 1844 "Report on the State of Ornithology" tells us that his thoughts went along the same lines. While comparative anatomy provided naturalists with the most important precepts, they still needed more ecological, behavioral and geographical information. Anatomy alone, as important as it was, was insufficient to achieve the main goal of systematics: the uncovering of the still hidden natural system.[69] Nonetheless, Strickland wished for a better understanding of bird anatomy and suggested that someone—a student, he specified—should make an arrangement of birds based on their anatomical characters alone. If such a Cuvierian classification agreed with one based on external characters, "its reality and truth" would

[68] Temminck, "Introduction troisième partie," *Manuel d'ornithologie*, 2 ed., vol. 3, xv.
[69] Strickland, "Report on the Progress," 177, 219.

then be proven, he concluded, just as Cuvier had done almost forty years before in the first volume of his *Leçons d'anatomie comparée*.[70]

Lamarck, on the other hand, stressed the concepts of transmutation and continuity, and from this perspective, he inevitably had a different idea of systematics than Temminck. By putting continuity in the foreground, classifications had to be, necessarily, artificial constructs. The supraspecific taxa (genera, families, orders and classes) did not exist in nature, they were instruments to facilitate the study of nature.[71] Each grouping was defined according to a series of agreements amongst naturalists in order to manage diversity and understand each other, but he prophesied that, with time and more specimens, the limits between groups, even those that seemed to be totally isolated from others, would disappear.[72] For Lamarck, classification was a tool, and the process of creating it, was guided by conventions. His focus was on his theories explaining the diversity of living beings, in cause and effect, in relationships. In all probability, Lamarck did not think that by itself the classifying effort merited a high status within natural history.

At the other side of the channel, the quinarians also hoped that classifying living beings would lead them to filling the gaps they now saw in their model of nature. But these were not the same gaps that Lamarck had perceived. For the quinarians, the puzzle consisted in completing their perfect mathematical circles, each with five groupings in it. Therefore, the quinarians' hope of filling the gaps was fundamentally different than Lamarck's. Quinarians believed in the existence of a model in nature, a geometrical arrangement that included separate and well-defined groups, and their classification was a representation of this order in nature. Lamarck, on the other hand, was classifying a natural world that he perceived as continuous, and the groupings he made were therefore never meant to be a reflection of nature's structure: there was no other structure than a linear succession of beings that evolved up a ladder of complexity. But for both, as their approach to classification was set up *a priori*, systematics on its own, without a philosophical component, was not a scientific discipline, it was a means to an end. Macleay, for example, lamented that classification as such had become the goal of many naturalists:

> The almost exclusive attention which has of late years been unfortunately lavished on Nomenclature and Systematic Arrangement—on the means in short,

[70] Strickland, "Report on the Progress," 219.

[71] Lamarck, *Philosophie zoologique,*, nouvelle éd., 33-34.

[72] Lamarck, *Philosophie zoologique*, nouvelle éd., 26. Beside this passage, Charles Darwin noted in his copy of Lamarck's book: "Fallacy common to Swainson & Macleay," edition of Charles Darwin's Reading Notes by Di Gregorio & Gill (Darwin Manuscripts Project: darwin.amnh.org). See also Macleay, *Horae Entomologicae*, vol. 1, ix-x.

> and not on the end of the science—has with ignorant persons diminished the importance of the study of Natural History itself. Let us hope that the slur will be soon entirely obliterated by those naturalists who have already shown that they are not to be deterred from the investigation of affinities by great names, because, forsooth, these may have preceded them in the annals of science.[73]

He insisted that "nomenclature is not a department of natural history, but only a convenient instrument whereby an acquaintance with it may the more easily be cultivated."[74] Vigors could not have agreed more.[75]

In the meantime, in the Netherlands, Jan van der Hoeven was working on the definitions of natural history and its subjects for his own textbook on Zoology, the *Handboek der dierkunde, of Grondbeginsels der natuurlijke geschiedenis van het dierenrijk*, first published in 1827. Regarding systematics, for which he used the term "Taxonomia," he considered it to be a *kunst*, a skill or art—as opposed to a scientific practice.[76] Van der Hoeven discussed the importance of achieving a natural classification system using Cuvier's definitions of *méthode* and *analyse*, but in his view, classification was not a scientific discipline as such, however indispensable. In France, Milne-Edwards shared this line of thought with Van der Hoeven. He had defined anatomy and physiology "as the foundation of Natural History," although he found them on their own insufficient to really understand the natural order, not unlike Temminck or Strickland. To "know" animals and plants, one had to study the relations between living beings as well, describe the "peculiarities they offer, which can be used as characters," learn about their geographical distribution, and then name and classify them.[77] But Milne-Edwards saw classification as a method, a tool that helped zoology (which he broadly defined as "the Natural History of Animals") move forward, and by helping distinguish between different kinds of animals, it "pour soulager la mémoire" (to unburden one's memory).[78] Concerning its importance in natural history, he wrote:

> The introduction of natural methods for the classification of living beings is one of the greatest services rendered to natural history: it has changed the image of

[73] Macleay, *Horae Entomologicae*, vol. 1, 10.
[74] Macleay, *Horae Entomologicae*, vol. 1, 24.
[75] Vigors, "Observations on the Natural Affinities," 180.
[76] Isaac Marcus Calisch and Nathan Salomon Calisch, *Nieuw woordenboek der Nederlandsche taal; bevattende: de meest gebruikelijke woorden, spraakwendingen en spreekwoorden* (Tiel: H. C. A. Campagne, 1864), 667.
[77] Henri Milne-Edwards, *Cours élémentaire d'histoire naturelle*, 281-91.
[78] Milne-Edwards, "Cours Élémentaire de Zoologie. Notions préliminaires," in *Cours élémentaire d'histoire naturelle*, 11.

> this science and has made compelling that part of botany and zoology that had been up to now the most arid.[79]

The relevance of classification, for him, was closely linked to its being based on "natural methods," that is, classification systems based on anatomy and physiology (as opposed to artificial classifications), but it was nonetheless a tool for zoologists to move forward in understanding nature.

There was yet another field of study contending for leadership in natural history, and it proposed a whole new approach to the study of nature. Its highest representative was Alexander von Humboldt. He stressed unity and relations, cause and effect, and what we would now call a multidisciplinary approach. The laws of nature were to be discovered in a steady process of field work, observing, measuring and mapping phenomena. This was a line of work that had altogether different subjects of study and was being practiced mostly in the field, so that, as we have seen in the previous chapter, comparisons can be problematic.[80] Humboldt recognized the importance of systematics as "la base principale de l'histoire naturelle descriptive," suggesting that he did not regard it as a discipline within natural history, but as an instrument. He described descriptive botany as indispensable for the advancement of botanical medicine, culture, art and even philosophy, but it was, he noted, not less relevant to establish the *Geography* of plants, which he understood as a field in itself.[81] In practice, this meant that, from his point of view, systematics was mostly useful because it could be used for medicine, geobotany and other disciplines.

In the Netherlands, no-one embraced Humboldt's approach more than Caspar G. C. Reinwardt. Reinwardt's inaugural lecture as a professor at Leiden University was a tribute to field work in the Humboldtian sense, where attention to the relations between the earth, living beings, the climate and man were central for anyone aiming at understanding nature. Museum workers were limited in their knowledge and perceptions, their objects of study having been removed from nature and with that, lost their context.[82] His discourse stressed the importance of measuring, observing and analyzing cause and effect. For Reinwardt, as for Humboldt, systematics was a lesser endeavor and, although it had its place from a practical point of view, it played a minor role in the advancement of science.[83]

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[79] Milne-Edwards, *Cours élémentaire d'histoire naturelle*, 288.

[80] See also Outram, "New spaces in Natural History"; Laura Dassow Walls, *The Passage to Cosmos. Alexander von Humboldt and the Shaping of America* (Chicago: University of Chicago Press, 2009).

[81] Alexander von Humboldt and Aimé Bonpland, *Essai sur la géographie des plantes: accompagné d'un tableau physique des régions équinoxiales* (Paris: Levrault, Schoell et compagnie, 1805), 13.

[82] Reinwardt, *Redevoering van C. G. C. Reinwardt*, 23.

[83] See also Weber, "Hybrid Ambitions," 187-92.

Perhaps the most extreme position regarding the status of systematics was that of another Dutch scholar, the Leiden professor Sebald Justinus Brugmans, who had such a contempt for systematics that it verged on aversion. Although he had obtained his doctor's degree from the University of Groningen by studying and classifying minerals from the region of Groningen, he slowly turned against systematics to favor applied research. So much so, that in a lecture at Leiden University of 1787 he advocated more research on the role plants played in nature, their function and their usefulness, and while recognizing the advances made by "systematics in botany," he claimed to be much less interested in their names and classification.[84] Brugmans promoted a more accurate study of the local flora, and he warned his colleagues that he did not mean "a catalogue of herbs or a mere index, nor a simple Flora, as it is called."[85] He wanted research on every kind of plant, what soil it grew on, its relation to other living beings and to man. Or as Hans de Jonge summarizes it, "the message was, less systematics and more research on the utility of plants."[86]

Brugmans was convinced, it seems, that no classification system would ever lead to knowledge of nature, or worse: "the zeal for system is the grave of truth," as his biographer dramatically phrased it (*systematis ardor veritatis sepulcrum!*).[87] However, Brugmans could not get rid of all classification, if only because he needed some structure for his lessons at the University and he had to name the plants. He just followed the by then already outdated *Systema Vegetabilium* en *Philosophia Botanica* of Linnaeus.[88] Incidentally, his dislike for systematics did not seem to trouble his relation with Temminck. In 1813, after receiving a copy of Temminck's *Histoire naturelle des pigeons* Brugmans wrote to him: "All ornithologists are in your debt for this beautiful Monograph, and all will eagerly await its continuation."[89] Temminck had always been welcome to work in Brugmans' collection whenever he needed to collect anatomical information from Brugmans' skillfully made zoological preparations.

Stevens notes that around 1830, systematic botany had lost much of its allure and

[84] Sebald Justinus Brugmans, *De accuratiori plantarum indigenarum notitia maxime commendanda, habita Leidae ... MDCCLXXXVI: altera valedictoria, de natura soli Frisici exploranda, dicta Franequerae ... MDCCLXXXVI* (Leiden: S. et J. Luchtmans, 1787), 6; Hans de Jonge, "Sebald Justinus Brugmans (1763–1819)" (Master's thesis, Leiden University, 1999), 26.

[85] Brugmans, *De accuratiori plantarum indigenarum notitia maxime commendanda*, 10.

[86] De Jonge, "Sebald Justinus Brugmans (1763–1819)," 26.

[87] Abraham Capadose, *Lofrede op Sebaldus Justinus Brugmans*, vol. 7, Werken der Hollandsche Maatschappij van Fraaie Kunsten en Wetenschappen (Leiden: Hollandsche Maatschappij van Fraaie Kunsten en Wetenschappen, 1822), 165.

[88] De Jonge, "Sebald Justinus Brugmans (1763–1819)," 35.

[89] Brugmans to Temminck, 24 December 1824, Naturalis Biodiversity Center Archives, Archive C. J. Temminck, NAT_ARC_TEM_00254.

scientists were more excited about experimental research than about descriptive work.[90] For zoology, things were more complicated. Because the search of the general order in nature was the central preoccupation in natural history during this period, one might have expected that it was this goal that would define systematics as a discipline. This was not the case. Naturalists decided upon the status of systematics as a scientific discipline not by its main goal (the natural system), but by its methods: empirical or philosophical. If one believed that a natural system was to be attained by descriptive and classificatory zoology, within agreed rules and by Cuvier's anatomical principles, then yes, systematics was a scientific discipline. But if the pursuit of the natural system was guided by *a priori* hypotheses, then no, systematics was not a true discipline but a tool to test one's natural philosophy. The elaboration of a general model of the organization of nature required first an overview of nature, and then the specialist examination of one or a few groups. It also involved defining the relations between species and higher taxa, and looking at cause and effect. None of this is necessary in an empirical process, where, at least in Temminck's case, it borrows its philosophy and the definitions of concepts from Cuvier's comparative anatomy. It is because this extra intellectual exercise is necessary for the search for the natural system that natural philosophers could not see bare systematics as a discipline. Among them were Lamarck, MacLeay, Swainson, Oken and Van der Hoeven. The main requisite to raise systematics to the status of a scientific discipline was, therefore, not the search for the natural system, but the rejection of philosophy as a method to uncover it.

The status of any particular classification system (not of systematics itself) depended, on the other hand, on whether or not it approached that ideal, the ever evasive natural system. A natural classification as defined by Temminck and Strickland (based on anatomical and physiological characters, complemented with external characters, life histories and geographical distribution), was regarded as the highest achievement. A natural system uncovered by similar methods but based on an *a priori* model of nature—the quinarian system, for example—also ranked very high in the eyes of naturalists. Yet, the road to the natural system was long and bumpy, and even if classifications still did not reveal the expected natural order, there was hope that with more information and new specimens, these classifications would become natural.

These in-between classifications were seen by naturalists as temporary tools, a work-in-progress in the anticipation of more knowledge and material yet to come.[91] If the system was good enough, it would hold against new discoveries and become a natural

[90] Stevens, *Development of Biological Systematics*, 209-15.

[91] Temminck, "Introduction troisième partie," *Manuel d'ornithologie*, 2 ed., vol. 3, xi; Strickland, "Report on the Progress," 177.

one. The construction of artificial systems was, however, a very different thing altogether. When a classification was based on one or a few characters chosen almost at random, it was regarded as a lesser, although useful, product of natural history. A commonly used analogy during this period for the artificial system was that proposed by Georges Cuvier: an artificial classification was like of a "dictionary," where words are arranged alphabetically and not according to their meanings.[92] An artificial system was useful for finding the species within the systems (one can easily find words in a dictionary), but it provided no insights into the relations between the organisms. Therefore, the relevance of a particular classification system depended on whether it was made for practical purposes like a dictionary, or it was a stepping stone towards a final natural classification. But as the methods in classification and nomenclature were being constantly improved, standardized and polished, the relative importance of the classification systems was in constant flux.

The issue of the status of systematics during the first half of the nineteenth century, at least in zoology, is more complex that one might think at first sight. Natural history was being fragmented into several disciplines, each with its own set of goals and practices, and systematics was indeed losing the hegemony it had held during the eighteenth century, which is not the same as saying that it had the lowest of statuses. Since the work of the Parisian professors Cuvier, Lamarck and Geoffroy Saint-Hilaire, zoological systematics was flourishing. Museums, monographs and journals multiplied, and systematists became a paid pool of professionals.

In Victorian Britain, the tradition of gentlemanly science lingered, and as Jim Endersby notes, "only a gentleman could be philosophical." Therefore, in Britain the status of classification was necessarily related to gentility: "although the mathematical sciences were the most prestigious and best-rewarded, numbers alone did not bring status […]. It was more important that a discipline be considered 'philosophical.' "[93] Systematics was in turmoil, both in Britain and in continental Europe. The natural system seemed to be around the corner but it was nonetheless still frustratingly intangible, the tensions between field and museum work—and between amateurs and professionals—also contributed to changing the discipline's position. Towards 1850, systematics had become a more democratic field and, as a result, the platforms used by systematists to communicate also changed. Disagreements about natural philosophy and the general reluctance to tackle the question of universal laws would ultimately accelerate the decline of the status of systematics.

The breaking up of natural history into subdisciplines brought various changes in its

[92] Cuvier, "Introduction," *Règne Animal*, vol. 1, 9-10.
[93] Endersby, "Classifying Sciences," 64.

wake. With the emergence of a physical natural history *à la* Humboldt, the predominance of anatomy and physiology, and the relatively new experimental sciences, systematics had to compete for visibility, resources and hegemony.[94] Stevens describes a very similar situation in classificatory botany, although the decline of its status between 1800 and 1850 was more pronounced than that in zoology.[95]

The main question was, it seems, whether all these disciplines of natural history were regarded as complementary to each other, as Temminck or Strickland did, or seen as subordinate to each other. In that case, the pecking order was far from established towards 1840, and by the end of the century, systematics had fallen almost to the bottom of the ranking list. This would have a profound impact on systematics in the twentieth century. The status of a discipline determines where, when and by whom it is being practiced, it delimits the way it is being communicated, the channels it uses, how it contributes to scientific debates, the available resources, and whether or not it is being taught and practiced in higher academic institutions. The more prestige a field has, the more attention and resources it receives, and one effect will be that knowledge and skills can be passed on, not only through publications and specialized journals, but also through the curricula of the universities, molding the next generation of practitioners. The consequences of the low status of systematics towards the 1850s are still felt today.

[94] Farber, *Emergence of Ornithology*; Nyhart, "Natural History and the 'New' Biology"; Stevens, *Development of Biological Systematics*.

[95] Stevens, *Development of Biological Systematics*, 216-18.