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Conclusions

“Taxonomy is described sometimes as a science and sometimes as an art, but really it’s a battleground.”

Bill Bryson, *A Short History of Nearly Everything*.¹

When Louis Napoleon was still King of the Netherlands, Temminck was denied admittance to the Koninklijk Instituut van Wetenschappen. The reason for Temminck’s exclusion was, seemingly, his lack of formal academic training. True, Temminck was tutored at home and had never set foot in a university as a student. But by 1836, he was finally admitted to the First Class of the Koninklijk Instituut. Temminck was by then waving in one hand two honorary doctorates (one from the University of Groningen and another from the University of Jena) and in the other a bundle of royal distinctions and awards. He was also none other than the Director of ’s Rijks Museum van Natuurlijke Historie, with collections that ranked amongst the best in Europe. He had published titles that had given him international prestige. Twenty years later, however, his fame was diminishing and his authority dimming. His peak-shaped career has been looked at from different angles, each author emphasizing a different phase in his life, leaving us with incomplete and contradictory stories. But Temminck’s career is best understood when put in the context of the development of systematics and only if looked at in its entirety. From this perspective it becomes clear that his path, his priorities and his status depended directly on the path systematics was taking. This epilogue uses the analysis provided by the previous chapters to evaluate the question of systematics as an emerging discipline within natural history.

During the first decades of the nineteenth century, Temminck received most praise for straightening out the chaotic nomenclature in ornithology and for his clear, unambiguous definitions of genera in his first monographs. The accuracy and consistency with which he upgraded existing ornithological works gained him a place on the international stage. Standardization was, after all, an important, if not the most important, pursuit in systematics during the nineteenth century. This is what Farber

¹ Bill Bryson, *A Short History of Nearly Everything* (London: Doubleday, 2003), 319.

means by noting that Temminck's main contribution to ornithology was to increase the rigor of zoological classification. Zoological nomenclature was in such disarray that it became one of the most intensely discussed subjects in systematics. The first rules for nomenclature set down by Linnaeus and amended by Fabricius and De Candolle were widely acknowledged, but at the turn of the nineteenth century these rules were becoming increasingly deficient. Naturalists were making exceptions as they went, especially to the principle of priority, and there was no agreement on how to proceed when a species had been labeled several times. The process of working in ever-growing collections confronted Temminck with a body of literature that was incomplete, heterogeneous and sometimes unreliable. He proceeded to match the original descriptions of Linnaeus, Pallas, Brisson, Levaillant, Illiger, Meyer, Latham and other naturalists with the specimens they had used, which were dispersed all over Europe. He did so by traveling for months from cabinet to cabinet, while building an extensive network of correspondents, something he would have never been able to do had he not been born into a patrician family. Years of effort paid off, and his first monographs included indexes of species with complete synonymies and literature references, which were generally welcome and exceedingly useful for others coming after him.

There were, however, a number of points that ignited heated discussions. In the first place, Temminck overruled the principle of priority on several occasions: if a name didn't match with the characteristics of the species (for instance, if the description had been based on juveniles), if a name placed a species somewhere on the planet where it did not occur, or when the description had been based on damaged specimens. In such cases, Temminck re-christened the species to correct the errors. The rub was, some naturalists, such as Frédéric Cuvier or George Robert Gray, did not agree with him and admitted no exceptions to the rule of priority, while others, such as William Swainson or Thomas Hosfield, thought it appropriate to change any incorrect epithets—especially, if these were not their own. The fact that even such a seemingly simple rule as the principle of priority (the first name ever given to a species prevails) stirred such debates reveals how complicated the issue of standardization was and how seldom naturalists agreed. Several efforts to standardize the nomenclature were made during the first half of the nineteenth century, but success was not possible until naturalists found a way to solve their individual differences as a community, that is, when the BAAS institutionalized the code of nomenclature as proposed by Strickland, Darwin, Owen and nine others. This was not a mere technical matter: any change meant that one's credibility and authority was being questioned.

Which name is given to which species might seem a problem of lack of agreement on regulation, but names are linked to descriptions, and groupings based on the wrong

descriptions make the ensuing classifications confusing. Names are important because they transmit ideas and reflect relations between varieties, species, genera, and so on. The disputes over nomenclature reflected different views about the definition and significance of each of these categories. and any challenge to a naturalist's proposed arrangement touched on much deeper questions. Classifications were meant to mirror the natural order, which, after all, was the main goal of systematics: to understand the natural diversity and God's plan behind it. There was a general expectation that in the end an order would be revealed in the apparent disarray of forms. This natural order was an intuitive idea and naturalists were confident that laws similar to those for, say, planetary harmony would eventually be forthcoming. How to achieve this, however, would keep naturalists occupied for decades.

Temminck was convinced that the study of the collections and the practice of classification, when combined with knowledge about the life histories and geographical distribution of animals, would eventually reveal nature's order, in the same way that a beautiful autumn landscape appears when one completes a jigsaw puzzle. Temminck's first incursions into systematics, however, had never been fueled by the urge to answer philosophical questions nor been the result of pondering the divine blueprint of nature. His first classifications had a more prosaic origin: he wanted to bring order to his father's collection of stuffed birds. Theoretical questions emerged later from the problems he encountered while carrying out *practical* work: identifying species to catalogue his collection, finding similarities between groups of birds to arrange them in the cabinets, treating birds skins with arsenic soap to preserve the characters he needed for identification, and browsing the literature for that one bird he could not name, finally to discover it had never been described before. From this experience, he understood that knowledge emanated from the very act of classifying, and consequently his was a thoroughly hands-on method that left no room for natural philosophy or any hypothesis made *a priori*. Most of Temminck's peers agreed: Carl Illiger or Hugh Strickland, for instance, promoted patience and careful work. Other naturalists, however, believed that the natural classification system should derive from a theoretical approach, and not the other way around. Amongst them, we find the quinarians and the *Naturphilosophen*. In any case, it was the search for order in nature that dominated systematics in Temminck's time, and for many, its discovery required the standardization of rules, methods and definitions.

A particularly difficult issue naturalists faced when attempting to uncover order was the circumscription of "natural" groups. In general, the principle of continuity was accepted and while Cuvier's four *embranchements* applied to the higher ranks in zoological classification, species and varieties of related organisms formed a continuum

that was increasingly difficult to cut up into natural and not arbitrary groups, especially when it came to genera. As we have seen, Temminck believed that a genus comprised a natural group of species that belonged together as manifestations of an ideal type, the basic form of organization. Species were manifestations of this mold with some variations from the original plan. Temminck's *type* was also a fixed category, as immutable as the species were. It carried with it the key to unlock the organization of nature: his understanding of the type as a blueprint induced Temminck to look above the levels of species and variation and to focus on shared similarities between different faunas. This led him to formulate a law concerning the geographical distribution of animals—similar *types* can be found along the same latitude on the globe. His general law remained virtually unnoticed, but Temminck's more descriptive work presented a relatively new panoramic view for naturalists after him to study those regions. Temminck's *Coup-d'oeil* and *Fauna Japonica* were very valuable precisely because they provided a birds-eye view of the faunas of the Sunda Islands and Japan. He laid the foundation for other naturalists like Murray, Wallace and Darwin to develop their own interpretations.

We should note here that Temminck's type concept as an organizing category is not necessarily at odds with his idea of continuity in nature: without groupings, nature becomes shapeless, while Temminck believed nature to be structured. He found continuity between species belonging to different genera. Similarly, his belief in separate acts of divine creation was useful to explain the geographical patterns of distribution, but it did not imply that he saw nature as made up of discrete groups. What Temminck's writings suggest is that he believed not in a linear chain of beings but in a web, a reticulum of related groupings, although Temminck never offered a clear description of how he perceived nature. His classificatory work and his law of the patterns of distribution of mammals reveal that, despite the emphasis he put on defining groups, his focus actually was on relationships, similarity and the overall organization of the fabric of nature, stressing continuity above divisions. This also explains Temminck's reluctance to split big genera, and the fact that his interpretation of the distribution of mammals around the globe was the opposite of that of Buffon, Humboldt and de Candolle: different forms occupied different regions, separated by insurmountable barriers. They focused on fauna's made up of different species where Temminck saw faunas comprising similar genera.

But what is a *genus*? In order to deal with the question of how to define genera, Temminck advocated the use of a few characters, carefully selected, that were to be used for all genera of a particular class. He spent years perfecting this selection, considering which to use for genera and which for species, and testing it throughout all his

groupings. He followed Cuvier's principles of the correlation of parts and of subordination of characters, trying to figure out which characters were most important for the survival of a particular group. In this way, he succeeded in defining genera in a uniform manner, with a limited set of characteristics. Each genus so defined was an unambiguous entity, but necessarily big because of the use of a small number of characters for each one. Temminck resorted to *sections* to divide large genera into smaller groups, especially when he was not sure he had found the definite characters to make new genera. Vigors, on the other hand, preferred to split genera into smaller units and found the use of sections confusing and unnecessary. Breaking up the large Linnaean genera became a battlefield, like nomenclature, precisely because of this underlying belief that systems should reflect, in the end, the divine plan behind all these wonderful organisms. Only those naturalists who believed that classification systems were in fact an aid, a tool, not unlike a dictionary—that is, artificial by definition and necessity—felt free to divide big genera into small units and move species from one genus to another. Classifications should be, first and foremost, useful. Among these naturalists were Sebald Brugmans and Carl Reinwardt, who had a more holistic, but also more utilitarian approach to natural history. But generally speaking, the concept of genus and how to define it was a very charged issue. By 1844 Hugh Strickland still lamented, tellingly, that the general approach to defining genera was an intuitive one, a matter of convenience more than a natural principle, and Alfred Wallace concluded that the type was neither fixed nor eternal. Interestingly, in the end the search for perfectly defined genera even took Temminck to doubt the existence of types and even of species. Although the species concept had received considerable attention, the concepts of genus and type were decidedly essential in nineteenth-century systematics. They carried the basic premises for constructing the elusive natural system and ultimately, for understanding the natural order. When genera were seen as natural, fixed elements of nature, naturalists' needed to succeed in defining them. Otherwise, the natural system would never materialize.

It is important to stress that the idea of improving the foundations laid by Cuvier does not imply that zoological systematics up to 1850 was simply producing an increasingly complex version of Cuvier's system, as it were by way of some kind of long and fastidious book-keeping. The main issues of systematics were the integration of a myriad of diverse systems of classification, to arrive at universally accepted rules of nomenclature, to find a way to harmonize geographical patterns of distribution and classifications, to clearly define a method to set boundaries around genera and a satisfactory procedure to weight the characters used in classification. Standardization was crucial, although it had not yet been achieved by the mid-nineteenth century, but it

was slowly emerging as zoological systematics developed. Such efforts to set guidelines and methods for classification were concentrated in the hands of an expanding but definite group of naturalists, working in certain settings. For instance, Temminck's approach was definitely collection-bound, and it marked the course of 's Rijks Museum in Leiden. He never did any field work, nor did he feel the need to. His travels took him to explore other collections, not nature. Even the one theoretical work that Temminck ever published, his law on the geographical distribution of animals, was the direct result of comparing European and Southeast Asian mammals with Japanese ones while working in the Leiden collection. The very fact that the 's Rijks Museum was established as a separate institute from the University of Leiden, although not completely independent, was the outcome of Temminck's understanding that different disciplines needed different types of collections. Anatomical preparations were not suited to answer the questions of classification, and systematically arranged collections of stuffed specimens could never help the anatomist to figure out the working of the organisms. In the Netherlands, comparative anatomy was practiced and taught at the universities, systematics in the Leiden museum. This is relevant because this divorce practically excluded systematics from the universities and made it the core business of 's Rijks Museum—for nearly two hundred years.

Although the situation in Leiden was very different from that in the Muséum National d'Histoire Naturelle, where the professors also held posts at the academies, classificatory endeavors in Paris were also exclusively collection-based, the approach so fiercely defended by Cuvier when he tried to minimize Humboldt's contributions. Cuvier clearly stated the need of comparing at first hand a broad array of specimens and literature to arrive at useful conclusions, which could only be done in a collection, not in the field. Similarly, the tasks carried out by the members of the Natuurkundige Commissie in the field were different from those they planned to undertake when—and if—they returned to the Leiden museum. In the field, they were observing, collecting, preserving and shipping animals and plants. All of these activities, although essential, were preparatory. But the practice of systematics itself, which consisted of identifying, naming, describing and recognizing relations, was a comparative exercise. While the first step was carried out in the field, the second one required extensive collections and a comprehensive library. Access to large collections that provided a panoramic view of natural diversity was indispensable for classification, while Humboldtian naturalists necessarily needed the experience and the measurements from fieldwork to understand the interactions among organisms and their environment. In other words, naturalists were claiming different spaces for different practices. This also implies that *naturaliste-voyageurs* could be occupied with very different subjects while in the field, either with

the preliminary work for systematics, or other activities needed for what we today would call ecological studies. The categories *field naturalists* and *cabinet naturalists* need to be defined not only by the space they were working in, but also by the disciplines with which they were concerned. In general, naturalists were aware of these separate approaches and they also recognized they had different goals. Agreeing on which goal was the most desirable and important at the time was another matter altogether.

This localization of systematics was accompanied by a process of community building. Naturalists dealing with zoological classification were aggregating into a collective with its own dynamics, and platforms. By 1840, the time of champions like Linnaeus, Buffon and Cuvier was giving way to an altogether different model of organization. The transition was set in motion when the number of naturalists dealing with classification increased, especially in Britain. Societies, magazines and clubs were proliferating, and articles on the classification and reviews on systematics works were as numerous as other subjects in journals like the *Magasin de zoologie, d'anatomie comparée et de palaeontologie*, the *Annals of Natural History*, the *Magazine of Natural History*, or even in the all-embracing *Tijdschrift voor Natuurlijke Geschiedenis en Physiologie*. This was the place to discuss, debate, criticize and review each other's works. This is a subject that needs to be explored further and that calls for comparative studies, but we have already had a glimpse of this process in the previous discussion on authority.

The example of Strickland's code of nomenclature shows how naturalists would not lightly accept one man's attempts to set rules, on nomenclature or any other matter, however good the reviews his work had received nor how high he stood in the chain of command of a museum, or any other institution. Community building implied, necessarily, community reviewing. Any proposed name, system of classification or general principle was open to discussion. Consensus was necessary, if ever elusive, for the much-needed process of standardization. Naturalists had to present very clear lines of thought and reasoning to defend their systems of classification. Temminck, Vieillot, Vigors, Swainson and Strickland, all needed their best arguments to convince each other of the correctness of their systems and their classification categories. They needed to have robust lines of reasoning, extensive knowledge of their fields and a sharp pen to respond to their reviewers. This situation, chaotic as it might seem at first glance, was the fertile ground from which systematics evolved.

Disputes were a necessary factor for the elevation of classification into a scientific discipline on its own. But after 1835, Temminck was noticeably absent from this arena. He continued publishing extensive monographs that took him decades to finish, partly because of his fixation on detail and partly because of his lack of time due to the management of the museum. He never took up the writing of short articles that dealt

with theoretical issues. He answered Vigors' criticisms about his use of *sections* with nearly a decade of delay. It took him more than twenty years to complete the second edition of his *Manuel*, the installments of *Nouveau Recueil* appeared over a period of twenty years, too, and he also needed fifteen years to complete the *Monographies de Mammalogie*. At that pace, it is hardly surprising that his manuscripts had already fallen out of line with the mainstream even before they were published. He published them nonetheless, inevitably introducing species already described by others. This delay prevented him from actively participating in the debates that took place in a fast tempo in zoological clubs and journals. Temminck's lack of urgency might seem a paradox, especially taking into account that he seemed very much aware of the need of resolving the chaos created by different zoological classification systems, but it makes sense in the context of his self-perception. He saw himself as part of an elite group of naturalists somehow authorized to study and comprehend nature: Cuvier, Illiger, Meyer, Latham, Wolf, himself... On top of this, Temminck felt a certain disdain for British naturalists or, at least, for British natural history. He routinely cast aside their work, even when Britain began to take the lead in natural history. Temminck was in the middle of this transition, as were Van Marum, Reinwardt or Strickland, but unlike the latter, he could not adapt. This, together with his failure to provide a well-articulated framework for his classifications and ideas on distribution patterns, gnawed away his authority.

There were a series of factors that influenced the quality and the quantity of Temminck's publications that were beyond his control. The most evident is the economic crisis that followed the first years after the foundation of the museum. Lack of money was a constant headache for Temminck, who had to spend an inordinate amount of time and creativity to obtain specimens, to publish his works, and to keep the museum and the *Natuurkundige Commissie* up and running. The problems the Commission faced during the thirty years of its existence also influenced Temminck's ability to deal with the material from the Dutch colonies. When specimens arrived at Leiden, they had often been separated from the corresponding manuscripts, so vital for the description of the specimens, and only letters and other scraps of information were available to work on. Also, the governmental decree ordering the publication of scientific results in Dutch, by Dutch nationals, did not help much in positioning the museum as a leading institute in Europe, even though that had been the intended result of the policy. But perhaps the most difficult of these obstacles was the change of the attitude of those in power towards museums.

Temminck had been close friends with the minister of Education, Industry and Colonies, dined with the Commissioner General for Public Education and rubbed shoulders with Louis Napoleon. But by the 1840s, his precious "national monument"

was referred to as “the monkey house” by Rudolph Thorbecke, arguably the most influential political figure in the country. Things had definitely changed. Stresemann’s conclusion that Temminck did not have the intellect needed to deal with the treasures stored in the Leiden museum can be countered by the statement that Temminck’s capacity to work on it was impaired by the economic and political changes, and seriously obscured by his method of slow, fastidious revision of classification systems, a method that at first, we should not forget, gave him his prestige. By 1840 a meritocracy was on the rise and quickly replaced the old autocracy to which Temminck had belonged for several decades.

The transformation of systematics during Temminck’s life can be understood as the emergence of a new discipline, one that had been traditionally part of natural history and even synonymized with it. If we look at the definition of *discipline* I gave in the Introduction, we see how systematics gradually began to fit it. By 1850, systematics had a set of goals, methods, terms and concepts unique to its subject matter; there was a developing self-regulated social structure that, although struggling to establish order, was already crystallizing into what we now call a peer-review system. It had its own journals and societies, and a space in which to practice: the natural history collections. Naturalists seemed to be conscious of the fact that, with their efforts at creating structure and standardizing methods, they were forming a new discipline. Whether systematics was included in academies remains to be examined. In Dutch universities systematics was part of natural history and medicine courses, mainly because it was necessary for applied botany and comparative anatomy, but the *practice* of systematics remained the core business of the Rijksmuseum in Leiden. The collections laid down by the universities were, for this reason, substantially different in materials, objects, conservation and arrangement than those kept in Leiden. Further studies will be very useful in elucidating the relation between museum and universities, in the Netherlands as well as in other continental and British institutions.

The main drive behind the development of systematics was standardization, an increasing rigor and specialization, and the rise of a meritocracy. Temminck’s influence in these processes faded away after the 1830s for the reasons mentioned above, but he did play a central role in the early decades of the 19th century in the standardization of names and methods, and by promoting the establishment of a museum independent of the university—dedicated exclusively to the practice of systematics. Taken together, these facts are, in my view, symptomatic of the process of discipline-building: standardization, community building and a particular location.

If systematics was developing into a separate discipline, how was natural history defined towards the mid-nineteenth century? There was no universal definition of

natural history nor an agreement as to which disciplines it should or should not include. Some naturalists included chemistry, physiology or behavioral studies in natural history, while others considered these to belong to the experimental sciences or natural philosophy. Within zoology, comparative anatomy was sometimes excluded from the scope of natural history, perhaps because it had been traditionally associated with medicine. Botany, on the hand, was often understood as a purely descriptive discipline; for naturalists in the 1850s, plants had no 'organs' to help elucidate the relation between form and function. In addition, new approaches were sometimes incorporated into natural history, like Humboldt's *Phytogéographie*. With the breaking up of natural history into a number of separate domains, the disciplinary pecking order shifted. Regarding systematics, its status depended not on its connection to anatomy nor on its promised reward, the understanding of the natural order. It depended on whether or not it relied on a hypothesis formulated *a priori* to achieve its goals. If systematics itself—the very act of describing and classifying—could eventually lead to explanations about order and causality, it was acknowledged as a true discipline. Naturalists who believed this to be the case defended systematics as independent of natural philosophy and free of any theoretical assumptions. Temminck, Illiger and Strickland, for example, were adherents of this idea.

On the other hand, systematics was sometimes regarded as a tool, an aid to help prove—or disprove—theories about the organization of nature. Lamarck, the *Naturphilosophen* and quinarians like MacLeay or Vigors considered the practice of classification as a useful instrument and therefore, for them systematics ranked very low among the other disciplines. For those naturalists who considered classifications and explanatory arguments as separate entities, systematics ranked very low in the hierarchy of natural sciences, while the reverse was also true: including theoretical explanations of the natural order in zoological classifications raised the status of systematics from a descriptive to an explanatory discipline. In other words, the status of systematics within natural history depended on whether it could by itself be an explanatory discipline, rather than only descriptive. It may even have been the case that Temminck formulated a law on the distribution of animals in an attempt to raise the status of systematics.

Curiously, the quinarians claimed that their system had been derived from evidence (we know now this could not have been the case), while Temminck and Illiger insisted on the hypothesis-free character of their classifications, although they were actually building upon the morphological *type* concept and the principle of continuity. In fact, all classifications were based, at some level or other, on predetermined underlying ideas and concepts like the species and genus concepts. In general, systematics was

subordinate to comparative anatomy and morphology while striving to provide natural history with some earth-shaking discoveries of its own. Further studies on the relation between natural philosophy and zoological systematics may shed more light on this question and help explain, among other things, the reception of uniformitarianism, or of the theory of evolution by natural selection and their influence—or lack of it—in the daily practices of systematists during the second half of the nineteenth century.

Taken together, the previous chapters provide a broad and detailed view of Temminck's career and the main developments in zoological systematics during his lifetime. We now can take a step back from the details and return to the three wider themes mentioned in the introduction: the development of systematics as a discipline, the rise of meritocracy in natural history and the status of systematics within natural history and natural philosophy.

Firstly, I suggest that systematics had, by the mid-nineteenth century, become a discipline within natural history. Systematists' main concerns by then were the standardization of rules and concepts and the pursuit of the natural system. Their main goals were to formulate the laws that governed nature, to understand how and why certain groups of organisms were seemingly related to others, and to find patterns in their geographical distribution. To achieve this, the most important point of departure was the concept of *genus*, not the *species*. The definition of a genus hinged on whether or not it was viewed as a natural entity, which was inextricably connected to the *type* concept. The concept of type depended, in turn, on each naturalist's idea of the divine blueprint for creation. Thus, I want to argue that the whole enterprise of systematics between 1800 and 1850 depended on the genus concept.

Secondly, I argue that the rise of meritocracy in natural history played a decisive role in pushing forward the development of systematics. As beliefs about the genus and the type concepts varied, classifications proliferated. Discussions were settled by argument and by debate, and the process of democratization of natural history cast aside those who claimed authority in the matter on the basis their past successes, their social status or in Temminck's case, both. As Britain took the lead in natural history, the social structure of the naturalist community changed, as did the way they communicated and the language they used to communicate. Systematics became of age by the mid-nineteenth century partly because of this development within the social infrastructure of natural history.

Thirdly and finally, the blurring of the boundaries between descriptive natural history and explanatory natural philosophy determined the hierarchy within natural history disciplines, especially systematics. Systematics had the status of a scientific discipline only if it incorporated philosophical elements and a conceptual framework to

explain nature's order. Without it, it was no more than a tool, a manual to help naturalists put some order into the natural world they were classifying. Cuvier's principles from comparative anatomy seemed to hold the key to unlock the order in the chaos, but as more and more animal forms were being discovered, the simplicity of the principles emanating from anatomy could only be of help for the larger groups: the classes and the families. The challenge that naturalists faced when trying to harmonize their classification systems with natural philosophy was exceedingly difficult. And as systematics proved unable to offer explanations of cause and effect, its status plummeted.

The consequences of the declining status of systematics towards the middle of the century are still felt today. In fact, systematics has been trying to gain prestige for the past two centuries by trying to incorporate into its domain the most pressing conceptual questions of the time. By the mid-nineteenth century, systematics pursued the discovery of order in nature and a century later, for instance, it attempted to redefine itself as Evolutionary Systematics, as the discipline to provide answers to evolutionary questions like the processes behind species formation and population genetics. Certainly, politics also played a role here. The prestige of any discipline is intimately linked to the amount of attention and resources it receives and the number of students it can attract. Similarly, the lower status of systematics has also resulted in the paucity of historiography: in fact, the history of the discipline has yet to be written. In the Netherlands, for example, there is a wealth of knowledge and much insight to be gained from studying figures like Hermann Schlegel or Samuel C. Snellen van Vollenhoven, leading systematists of their day. Additionally, the institutional histories of the Leiden University and the Leiden museum—in its passage from 's Rijks Museum van Natuurlijke Historie to the relatively new Naturalis Biodiversity Center—can provide valuable insights into the evolution of systematics through Darwinism up to molecular phylogeny and population dynamics. These studies would finally help putting the history of Dutch natural history on the agenda for further comparative research.