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PART THREE

A place for systematics

CHAPTER SIX

Temminck's intellectual landscape

On whose authority?¹

By the middle of the century, natural historians were spending quite some time and no less energy discussing nomenclature, the use of sections and genera, and the validity of their classification systems. These were not merely technical questions; these issues were echoing fundamental thoughts on the organization of nature. Before 1830 disputes were often settled when an influential naturalist won the argument. With only a handful of naturalists dedicated to animal classification, debates were partly solved by argument and partly by social status, the most famous example being Georges Cuvier and his conflicts with Jean-Baptiste de Lamarck and Étienne Geoffroy Saint-Hilaire.² Despite the occasional challenge by his colleagues, Georges Cuvier remained a renowned authority on the subject, thanks to his work on comparative anatomy as well as his social status. Figures of stature such as Linnaeus, Buffon or Cuvier ruled their institutes and set the rules of engagement. Twenty years later, as the number of naturalists studying classification swelled, the situation had changed.

Natural history and with it, classification, had since the end of the eighteenth century become a matter of general interest. Many factors contributed to this trend, including economics, politics and technology. Advances in printing techniques made iconographic books even more beautiful and, more importantly, cheaper to produce and to purchase.³ There was a great fascination with exotic faunas, fueled by the accounts of voyages of discovery to the tropical colonies. With it, public and private zoological collections grew exponentially. This was also the time when new national museums were established, like the Zoological Museum in Berlin in 1810 and 's Rijks Museum van Natuurlijke

¹ This section is based on my previously published article, Gassó Miracle, "On Whose Authority?" Changes have been made to include new insights and references, and to avoid duplications in this book.

² See, for example, Appel, *The Cuvier-Geoffroy Debate*; Coleman, *Georges Cuvier, Zoologist*.

³ Farber, *Emergence of Ornithology*, chap. 5.

Historie in Leiden in 1820. An increasing number of natural historians, whether as amateurs or as professionals within a scientific institution, dedicated themselves to the task of naming and classifying natural objects, and the literature on the subject mushroomed. New zoological and botanical clubs and associations were founded, each with their own specialized journals as vehicles of communication and platforms for debate. This was the rich milieu from which different views on classification emerged. Authors disagreed on how to practice natural history, scientific names for animals and plants multiplied and debates took place in journals as well as in the sessions of zoological and botanical clubs and societies. As Peter Stevens puts it: "One signal that all was not well was the unending proliferation of new systems and the failure to develop any way of evaluating characters and groups other than by trying to achieve a consensus with present and past masters of the art."⁴

In Britain, the situation was accentuated by the polarization between reformers like Vigors and the more conservative wing. Scientific journals were established to allow naturalists to publish their views—provided they were in line with those of the editors.⁵ The 1837 volume of the *Magazine of Natural History* included eight articles exclusively devoted to discussing issues of nomenclature and the status of new genera, including Hugh Strickland's first set of *Rules of Nomenclature*. In 1838, the journal included in a single volume a back-and-forth of views in a series of articles by Strickland and the Irish naturalist William Ogilby discussing these rules. The tone of their writings was harsh and personal, and in all probability, raised more than a few eyebrows.⁶ Ogilby summarized his position as follows:

In short, these "Codes for Nomenclature" put me in mind of nothing so much as Mrs. Malaprop's account of the "laws of CONFUSION, the Chinese Philosopher, Where you feel like a needle going astray, With its one eye out, through a bundle of hay."⁷

⁴ Stevens, *Development of Biological Systematics*, 265.

⁵ McOuat, "Species, Rules and Meaning."

⁶ William Ogilby, "Observations on 'Rules for Nomenclature'," *Magazine of Natural History* 2, no. 2 (1838); "Further Observations on 'Rules for Nomenclature'," *Magazine of Natural History* 2, no. 2 (1838); "Letter in Reference to Mr. Strickland's Observations on the Application of the Term Simia," *Magazine of Natural History* 2, no. 2 (1838); Hugh Edwin Strickland, "Reply to Mr. Ogilby's 'Observations on Rules for Nomenclature'," *Magazine of Natural History* 2, no. 2 (1838); "Remarks on Mr. Ogilby's 'Further Observations on Rules for Nomenclature'," *Magazine of Natural History* 2, no. 2 (1838); "A Few Words of Explanation in Reference to Mr. Ogilby's Letter," *Magazine of Natural History* 2, no. 2 (1838).

⁷ Ogilby, "Further Observations on 'Rules for Nomenclature,'" 284. Mrs. Malaprop is a character of Richard Sheridan's 1775 play *The Rivals*. The character became known by her verbal blunders, or *malatropisms*.

Entertaining as this exchange may be, it also reflects the huge impact that regulating zoological nomenclature had on naturalists. The time when a Linnaeus or a Cuvier could impose his views for decades by using his social and scientific position was gone.

The process through which an agreed-upon code of nomenclature eventually emerged exemplifies this shift. When Strickland published his first set of rules for nomenclature in 1837, it received almost exclusively negative reactions. The main reason for this, as Gordon McOuat points out, is that the rules were laid down by a single man and therefore, had no social or political weight. On his own Strickland had not enough scientific authority. In 1842, he suggested that the British Association for the Advancement of Science appoint a committee to work on the problem of zoological nomenclature, following "the parliamentary practice," by which the committee's proposal would be reviewed and approved as if it were a law.⁸ This committee, consisting of Hugh Strickland, Charles Darwin, Sir Richard Owen, the Reverend Leonard Jenyns and eight others, presented their set of rules in a Report to the BAAS on June 27, 1842. This report was published in 1843 in the Proceedings of the BAAS and it has been known since as the Strickland Code or Strickland's Rules.

The rules rapidly spread; they were published in a number of journals and translated into several languages, which, however, does not mean they were automatically accepted. These rules were discussed, criticized and altered. Finally, in 1895, the Strickland Code became the foundation for the modern International Code of Zoological Nomenclature.⁹ Strickland's parallel with a parliamentary *modus operandi* in zoology was an appropriate metaphor for the situation at that time. No single man had enough authority anymore to dictate rules, of nomenclature or of any other kind, without a consensus from his fellow naturalists. The Strickland Code, backed up by colleagues, the BAAS and soon published in the *Annals and Magazine of Natural History* and the *Philosophical Magazine*, on the other hand, had enough weight to be translated, spread, discussed and eventually, accepted. In his report on the Rules of Nomenclature of 1842, Strickland pictured the situation very accurately:

By emanating from the British Association, it is hoped that the proposed rules will be invested with an authority which no individual zoologist, however eminent, could confer on them. The world of science is no longer a monarchy, obedient to the ordinances, however just, of an Aristotle or a Linnaeus. She has

⁸ McOuat, "Species, Rules and Meaning," 506.

⁹ McOuat, "Species, Rules and Meaning," 512; Melville, *Towards Stability*, 10. For more information on the gestation and evolution of the Strickland's code of zoological nomenclature, see Sandra Knapp, Gerardo Lamas, Eimear Nic Lughadha et al., "Stability or Stasis in the Names of Organisms: the Evolving Codes of Nomenclature," *Philosophical Transactions of the Royal Society of London B: Biological Sciences* 359, no. 1444 (2004); Rookmaaker, "Early Endeavours by Hugh Edwin Strickland."

now assumed the form of a republic, and although this revolution may have increased the vigour and zeal of her followers, yet it has destroyed much of her former order and regularity of government. The latter can only be restored by framing such laws as shall be based in reason and sanctioned by the approval of men of science; and it is to the preparation of these laws that the Zoological Section of the Association have been invited to give their aid.¹⁰

Natural history was not a *monarchy* or *autocracy* anymore; agreement had to be reached by consensus and, even more difficult to realize, by making concessions, very much as in a republic. This was unfortunate for Temminck, to say the least. He was born in 1778 into an influential patrician family in Amsterdam and his father was a respected man, a treasurer of the Dutch East India Company. Temminck never went to school; he received private education at home. He had powerful friends in politics, who helped him realize his dream of a national natural history museum and put him at the head of the new institute. He had looked up to Georges Cuvier, from whom he had learned that scientific authority could lie in a single man.¹¹ However, Temminck did not wish to be a “new Cuvier,” as he saw natural history more as an autocracy, where science remained in the hands of competent naturalists, whose achievements would be remembered forever:

It is particularly the eloquent writings of Buffon that have given new appeal to this pleasant science, sounding a wake-up call around the world; and the order and the harmony in classification from the great Linnaeus, have equally helped increase the number of enthusiastic amateurs; illustrious scholars, who by following the writings of these renowned men, have earned the glory of seeing their names inscribed in the temple of memory.¹²

Temminck perceived himself as a member of this distinguished elite, mainly because of the success of his systematic works as well as his absolute confidence in his approach to systematics and his very conscientious method of classification. He was particularly proud of the fact that he had based his classification systems on his examination of thousands and thousands of specimens in nearly all European cabinets. He believed that his search for the natural systems relied directly on observation and comparison, on the ability to extract typical forms from diversity, and finding the true diagnostic characters. This is not to say that Temminck was advocating objectivity in its modern sense; he was

¹⁰ Hugh Edwin Strickland, Charles Darwin, John Stevens Henslow et al., “Report of a Committee Appointed to Consider of the Rules by which the Nomenclature of Zoology may be Established on a Uniform and Permanent Basis,” *Report of the British Association for the Advancement of Science* 12 (1843): 3.

¹¹ Temminck, “Avant-propos,” *Coup-d’œil général*, vol. 1, ix.

¹² Temminck, “Avant-propos,” *Manuel d’ornithologie*, 1 ed., vii.

in fact claiming authority precisely because of his unique talents: his scientific status was there by reason of his experience and his own views. Daston and Galison have coined the phrase “truth-to-nature” for late eighteenth and early nineteenth century naturalists who, like Temminck, believed in a hidden truth in nature that could only be uncovered by observation *and* by the particular skills of the experienced observer: “To see like a naturalist required more than just sharp senses: a capacious memory, the ability to analyze and synthesize impressions, as well as the patience and talent to extract the typical from the storehouse of particulars, were all key qualifications.”¹³

Temminck’s celebrated monographs on pigeons and gallinaceous birds, together with both editions of the *Manuel d’ornithologie*, had given him the authoritative status of a Cuvier or a Latham. Swainson, for example, ranked Temminck amongst the most celebrated ornithologists of his time: “Of general systems [...], the most popular and esteemed are those of Illiger, Cuvier, Vieillot and Temminck.”¹⁴ He thought that Temminck’s *Histoire naturelle des pigeons et gallinacés* and the *Manuel d’ornithologie* were both excellent, exemplary works. *Manuel of the Ornithology of Europe*.¹⁵ Neville Wood, in an annotated bibliography on ornithology wrote about the *Manuel*: “Temminck is the first Ornithologist since the time of Linnaeus, who promulgated a system worth attending to. In simplicity it almost rivals that of the illustrious Swede, and is infinitely its superior in exactitude and preciseness.”¹⁶

The *Manuel* rapidly became a standard work for other ornithologists and set the basis for others to build on, thanks to his careful comparative studies, extensive investigation and patient and rigorous method of work. In recognition of Temminck’s authority, he was awarded an honorary doctorate by the University in Jena, in 1828 (his first one had been awarded to him by the University of Groningen, in 1819). After its success, Temminck kept working on his system, preparing a second edition of the *Manuel*. It took him twenty years to complete this new edition, which was finished in 1840. Hugh Strickland wrote about the “long expected supplements” to the *Manuel*: “Although the author hesitates too much in adopting the generic groups of modern science, and does not sufficiently value the law of priority in nomenclature, yet the exactness of his descriptions and the general soundness of his criticisms will long render his work a valuable hand-book of European ornithology.”¹⁷

¹³ Lorraine Daston and Peter Louis Galison, *Objectivity* (New York: Zone Books, 2015), 58.

¹⁴ Swainson, *Natural History and Classification of Birds*, vol. 1, 200.

¹⁵ Swainson, *Natural History and Classification of Birds*, vol. 1, 205-06, 08.

¹⁶ Neville Wood, *The Ornithologist’s Text-Book: Being Reviews of Ornithological Works, with an Appendix, Containing Discussions on Various Topics of Interest* (London: John W. Parker, 1836).

¹⁷ Strickland, “Report on the Progress,” 180.

Temminck appreciated some of the criticisms he received, as long as he thought them to be constructive, and he used them to improve his *Manuel*. In the second edition, Temminck thanked those who helped him realize his mistakes and shortcomings and wrote: "Concerning science, a work without criticism is like a meal without salt."¹⁸ Indeed, Heinrich Boie's comments on Temminck's work had brought both men into contact and laid the basis for their friendship, and Temminck even secured Boie a place as a curator in the Leiden museum. Other criticisms, however, like those of Vieillot and Vigors, he could not accept, let alone be grateful for. He considered them to be personal attacks on his judgment and knowledge. Remarkably, although Vigors criticized Temminck's genera, Swainson praised them: "[Temminck's] merits in the arrangement of his generic groups, and the high finish he has bestowed upon them, have given to his system a prevalence and popularity above all others which have appeared since the days of Linnaeus; next to whom, as an ornithologist, he assuredly ranks."¹⁹ The first livraisons of his *Monographies de mammalogie* (1827–1842) were also celebrated for the accuracy and care put into the descriptions and the nomenclature.²⁰ Temminck's meticulousness was greatly appreciated by his fellow zoologists. In 1826, a review of his *Monographies* read:

Whatever falls from the pen of so sedulous an enquirer, is well worthy of attention, founded as his facts universally are on the most patient and laborious investigation. That such has been the case on the present occasion, is proved, by the references to the various collections of Europe, nearly the whole of which have been visited in the progress of his work. Nor are the descriptions founded only on living or on set-up specimens; to trace them with more accuracy and to obtain a more ample view of their frequent variations, he has also recourse to the warehouses of furriers in all the principal commercial towns, without a continual examination of which, he repeats again and again, no certainty can exist with respect to the species of *Felis*.²¹

Another reviewer was equally impressed with the first three installments of the *Monographies*, particularly with Temminck's treatment of the genus *Didelphis* (the large American opossums): "The synonymy of these species, which has hitherto been extremely confused, has been unravelled by M. Temminck with his usual sagacity and

¹⁸ Temminck, "Introduction troisième partie," *Manuel d'ornithologie*, 2 ed., vol. 3, lxii.

¹⁹ Swainson, *Treatise on the Geography*, 165.

²⁰ "Analytical Notices of Books"; Pierre August Joseph Drapiez, "Manuel d'ornithologie, ou tableau systématique des oiseaux qui se trouvent en Europe, par C. J. Temminck, 2me édition (1)," *Annales Générales des Sciences Physiques* 6 (1820).

²¹ "Analytical Notices of Books," 534.

assiduity.”²² But things changed, especially in the 1840s. The last installments of the *Nouveau recueil* were received with less enthusiasm.²³ William Swainson, who had praised Temminck's earlier works, was rather less impressed with this one:

In the mechanical part of its execution this is a very beautiful work [...]; but the drawing is not faithful, nor is the colouring generally natural: the figures are stiff and formal, and they are all put into nearly the same attitudes. The descriptions of the birds are meagre, and for the most part relate to the mere colour of the plumage. Occasionally, however, more extended remarks are introduced on certain genera, [...] which are very valuable; but the total absence of synonyms, specific characters, and scientific descriptions of the form, structure, or habits of the birds themselves, renders this work far inferior to what it might have been, and what the scientific world expected, from the reputation of its authors.²⁴

The same happened with the monographs on mammals. Temminck was re-describing species already known and that diminished his authority. Charles Bonaparte's criticism of the monograph of the genus of bats *Vespertilio* reflects these changes in prominence taking place in systematics: “to correct the errors of eminent writers is the most effectual means to advance science, while the great veneration which is entertained for Temminck, and his *ex cathedrâ* tone, may in some cases prove fatal to truth.”²⁵

The fact is that Temminck's works on mammals were not as much applauded as his earlier works on birds. John Edward Gray, the Keeper of Zoology at the British Museum of Natural History between 1840 and 1874 (and the elder brother of zoologist George Robert Gray, author of *Genera of Birds*) was particularly harsh. He published a revision of the group of civet cats using specimens kept in the British Museum in 1864, where he summarized his ideas about Temminck in this rather harsh paragraph:

M. Temminck was an eminent ornithologist, and has studied some groups of Mammalia, perhaps not with so much success. He was an amiable naturalist, but has carried his political anglophobia (so well seen in his ‘Essay on the Dutch Colonial Possessions’) into his zoological studies. This blinded him to the labours of the zoologists of this country, the richness of our collection, and thus rendered his observations in regard to their work not worthy of attention, as they otherwise might have been. It is to be observed that he never had a regular

²² “Monographies de Mammalogie, ou descriptions de quelques genres de Mammiferes dont les especes ont eté observées dans les differens Musées de l'Europe. Par J. C. Temminck. Livraisons 1-3. pp. 72. pl. viii.” *The Zoological Journal* 1, no. 4 (1824): 575.

²³ See, for example “Bibliographical Notices,” 267.

²⁴ Swainson, *Natural History and Classification of Birds*, vol. 1, 221.

²⁵ Bonaparte, “Observations of the State of Zoology,” 21.

scientific training, never attempted to form scientific specific characters, and is rather to be regarded as a patron and amateur than as a scientific zoologist. He was the first in his country, as the late Earl of Derby was in this.²⁶

But if Temminck's lack of academic training did not prevent him from becoming "an eminent ornithologist," we should be looking elsewhere for reasons to explain the inferior quality of Temminck's studies of mammals.

Whether Temminck's anglophobia—a disorder he most certainly suffered from—biased him to the point of deliberately ignoring the work of British zoologists is difficult to prove, but it may very well have been the case. He seemed to disregard a large portion of the zoological work done by British naturalists, perhaps in part because of his antipathy towards England. These feelings had been ignited by Raffles' role during the Napoleonic wars in the conquest of Java, and made worse afterwards by Raffles' criticisms of Dutch rule in the East Indies in his *History of Java*. When Temminck took up the task of re-writing the same history for his *Coup-d'oeil* (the book Gray referred to) he gave a very different account than Raffles about nearly every single episode—before, during and after the period of British rule.²⁷ Temminck claimed throughout the work that his account was based solely on the archives of the Dutch colonial government and therefore, truthful.²⁸ His anti-British feelings may also have grown because of his views on the administration of the British Museum (as we have seen, Temminck was baffled by the composition of its Board of Trustees) and the fact that English naturalists published exclusively in English, while French and German were the dominant scientific languages on the continent.²⁹

Britain made a rather late entry into the field of systematics, which had been dominated by continental Europe until the 1830s. This changed the course of ornithology as well as Temminck's authoritative status. As the rest of Europe was recovering from the Napoleonic wars and other political and economic difficulties, natural history flourished in Britain. By the 1840s, the research and collection in the British Museum of Natural History were challenging both the Muséum National

²⁶ Gray, "Revision of the Genera and Species of Viverrine animals," 505. Gray was referring here to Edward Smith Stanley, thirteenth earl of Derby and Lord Stanley (1775–1851), politician, naturalist, president of the Linnean Society and Trustee of the British Museum.

²⁷ Temminck, *Coup-d'oeil général*, vol. 1, chap. 1.

²⁸ See, for example, Temminck, "Avant-propos," *Coup-d'oeil général*, vol. 1, xiv; or Temminck, *Coup-d'oeil général*, vol. 1, 45, 106.

²⁹ For a study of the evolution of scientific language, see Michael D. Gordin, *Scientific Babel: the Language of Science from the Fall of Latin to the Rise of English* (London: Profile Books, 2017).

d'Histoire Naturelle in Paris and 's Rijks Museum in Leiden.³⁰ As the British Museum grew, English naturalists caught up with the French, the Germans and the Dutch, a change that was especially felt in Leiden. By then, Temminck was struggling for government funds, particularly for the Natuurkundige Commissie. He had to spend much of his time on administrative tasks for the museum and around 1840 he had practically abandoned ornithology. He produced the three volumes of the *Coup-d'oeil général sur les possessions néerlandaises dans l'Inde Archipélagique*, published between 1846 and 1849, partly as a cry for help to the Dutch government. In almost every page there is a passage on the importance of continuing the exploration of the colonies, both for society and for the natural sciences. These were meager times for 's Rijks Museum.

But other factors also influenced the quality of Temminck's works. In the next sections, we will look at these more attentively, but the fact that discoveries were increasingly being published in specialized journals and magazines—many of them, British—instead of in books may account for Temminck's failure to incorporate the work of other naturalists in his own publications. Besides Temminck's physical decline in the 1840s, other factors that may have influenced his *Monographies de Mammalogie* include his habit of re-describing species he felt were not properly characterized and renaming species when making changes in the taxonomic arrangements. In addition, the preoccupations at the museum and the delay of the arrival of specimens and manuscripts from the Natuurkundige Commissie may explain his slow pace in publishing. We cannot exclude that pride may also have been at play. It seems that Temminck chose to publish manuscripts that were already outdated, despite the fact that he was probably aware of the fact. If so, he pushed on in order to claim authorship for the new species, assuming that his descriptions and classification were more accurate. As a result, not surprisingly, he provoked the animosity of his peers.

Temminck was caught in the middle of a transition in which his own authority was first widely recognized but then challenged later on. He could not grasp this. The confrontations with his peers pushed him to formulate his ideas on classification and nomenclature more clearly in the second edition of the *Manuel* (especially in the third volume of 1835, where he reacted to Vigors' article of 1824) than he had done in his previous ornithological works from 1815 to 1830, with the sole exception of his booklet *Observations sur la classification méthodique* of 1817. He was forced to address directly the issue of methodology, to define his notions of sections, genera and species very clearly, as well as to explain how he applied these categories to the classification of birds. In

³⁰ Camille Limoges, "The Development of the Muséum d'Histoire Naturelle of Paris, c. 1800–1914," in *The Organization of Science and Technology in France 1808–1914*, ed. Robert Fox and George Weisz (Cambridge Univ. Press, 2008); McOuat, "Cataloguing Power."

1846, at age 68, he wrote to his colleague and friend Martin Heinrich Karl Lichtenstein about his disappointments:

The new era is getting beyond me, and however much one reads one feels that it is impossible to make progress, and one seems to oneself quite stupid and ignorant in the new jargon of systematics and nomenclature, or at least too weak to swim against the stream, and through a sort of disgust frightened away from even attempting any sort of opposition. There is something revolutionary in every contemporary activity; we shall not live to see the settling of this ferment, but I cannot say that I cherish any great expectations of the final result, and I rejoice therefore that I have lived in a time when there were still scientific authorities and when everything proceeded more tidily.³¹

Indeed, with the decline of the scientific authorities things became complicated: the number of books, journals, societies and collections to process increased rapidly. Temminck, it appears, could not keep up with the faster pace of other naturalists. Around 1840, Temminck lost both his political influence in the Dutch government and his authority in ornithology. Naturalists were now quick to dispute the great experts of their time and they had a vehicle for their praise and criticism: the new journals and magazines of natural history. The time of the monarchs, as Strickland put it, was over.

The field, universities and museums

Traditionally, historians of natural history have noted the split between field naturalists and naturalists working from cabinets and libraries.³² However, most of the discussions among historians revolve around the *space* where naturalists were working. But it was the *object* of their efforts, as well as their personal knowledge and circumstances that played a central role in defining the differences between field- and collection-based natural history. What is more, the role of each naturalist evolved with time, whether in the cabinet or in the field, which requires a closer look at each kind of naturalist. And when that is done, the distinctions become less clear-cut than previously suggested.

Temminck replied to his critics through his writings: essays and letters. There are no records, it seems, of Temminck participating in debates in the academies nor in the meetings of the many societies of which he was a member. Temminck's natural habitat

³¹ Temminck to Hinrich Lichtenstein, 27 July 1846, Museum für Naturkunde Berlin Archives; quoted and translated in Stresemann, *Ornithology from Aristotle to the Present*, 150-51.

³² For example, Dorinda Outram, "New spaces in Natural History," in *Cultures of Natural History*, ed. Nicholas Jardine, James A. Secord, and Emma C. Spary, (Cambridge: Cambridge University Press, 1996); Stevens, *Development of Biological Systematics*; Lynn L. Merrill, *The Romance of Victorian Natural History* (New York, Oxford: Oxford University Press, 1989).

was the natural history collection. Furthermore, he never did other field work than going on shooting expeditions near home in search of birds for his collection and his kitchen. He never visited the Dutch colonies nor any other tropical region where he could have admired his beloved pigeons and fowl, alive and kicking. The exact reason for Temminck's apparently unadventurous disposition is unclear. Perhaps he did not feel physically up to it, perhaps the death of so many members of the *Natuurkundige Commissie* had scared him. In addition, the directorship of the Leiden museum took up all of his time and, in all probability, much of his energy too. But he did not complain about this lack of field experience. More interestingly, he never did make it a priority for his own research. His daily practice consisted in methodically and fastidiously comparing as many specimens as he could lay his hands on, that is, stuffed and mounted specimens, instead of observing and recording living ones. As we shall see, comparing large series of specimens was far more appealing and rewarding for the systematist than a trip to the tropics, however exciting, which partly explains Temminck's desk-bound years.

The fact that he had never visited the Dutch East Indies seemed to Temminck an advantage rather than a shortcoming. Temminck spent years on the completion of his *Coup-d'oeil général sur les possessions Néerlandaises dans l'Inde archipélagique* (1846–1849), the three volumes summarizing all that was known to him at that time of the Dutch East Indian colonies. Temminck admitted that not having visited the islands himself made his account drier and less vivid than if he had been there to see it all himself. But no-one could ever explore all of the archipelago islands and learn all there is to know about them. A work like this could therefore never be written from first-hand experience anyway (that is, from the field), but only by assembling information from different sources and collections.³³ General surveys must inevitably be produced from home. The very first sentence in Temminck's *Coup-d'oeil* is, curiously, a quote from Cuvier's *Règne Animal*. Referring to the system of arranging animals from bigger to smaller groups by a process of induction, Cuvier wrote: "The method, once mastered, can be apply with endless benefits to fields unrelated to natural history. Any discussion that assumes a classification of facts, any research that requires a division of the subject matter, is done applying the same rules."³⁴ Temminck followed Cuvier's method for his systematic works, delimiting natural units, detecting gaps in continuity and carrying out comparative work, which required collections and libraries of considerable size and

³³ Temminck, "Avant-propos," *Coup-d'oeil général*, vol. 1, xxi-xxii.

³⁴ Georges Cuvier, "Préface," in *Le Règne Animal Distribué d'après son Organisation, pour Servir de Base à l'Histoire Naturelle des Animaux et d'Introduction à l'Anatomie Comparée*, vol. 1 (Paris: Déterville, Imprimerie de A. Belin, 1817), xix.

scope. For his encyclopedic work on the Dutch colony, Temminck relied on the same recipe, and *comparison* was its chief ingredient.

Professor Caspar G. C. Reinwardt, who had worked for almost two decades in the Dutch East Indies, had a very different view of the subject. Reinwardt fiercely contended that the field naturalist, not the cabinet naturalist, was able to arrive at an understanding of nature. He stressed the unity and connectedness of all natural phenomena, and therefore, he believed that only by working in the field could one ever come to understand nature's organization.³⁵ Cabinets were nothing more than collections of inert objects taken away from their original context. But Temminck was convinced that systematics would lead science to the natural system, and that this knowledge would help the natural sciences in general to understand nature's organization. This was his first concern, and he repeatedly stated so. What Reinwardt conceived as a lesser endeavor, describing and classifying objects in a museum, was for Temminck proper scientific practice, a practice that could not be carried out in the field: the collection cabinet was the place to work on systematics. Its methods were based on comparison and ranking, something impossible to do without enough specimens covering morphological variation through a wide geographic range. This is not to say that Temminck held fieldwork in contempt, quite the opposite. But Temminck believed that the goals of the naturalist in the field were simply different from his and he was certain that classification could not be carried out in the field. Temminck was not alone in this.

The first members of the *Natuurkundige Commissie*, Heinrich Kuhl and Johan Conrad van Hasselt, once they were feverishly working in Java, were mostly collecting specimens and information with the hope of finishing their systematic work when they returned to the Netherlands. Unfortunately, this never happened, as both died much sooner than anticipated: Kuhl after only nine months in Java, and Van Hasselt following him two years later.³⁶ Both of them, together with their companions Gerrit van Raalten, a taxidermist, and the artist Gerrit Laurens Keultjes, amassed an astonishing amount of specimens, field notes and drawings. Three years after they had first set foot on Java, only Van Raalten survived. The results of Kuhl and Van Hasselt's efforts were never published, with the exception of extracts from their letters to Temminck, Professor Van Swinderen from Groningen and Daniel Jacob van Ewijk (the civil servant in the department of Education, Art and Science at the Ministry of Home Affairs who had

³⁵ Reinwardt, *Redevoering van C. G. C. Reinwardt*.

³⁶ The history of the *Natuurkundige Commissie* is still awaiting to be written, but substantial information about it can be found in Greshoff, "Kuhl en Van Hasselt"; Klaver, *Inseparable Friends*; Sirks, "Indisch natuuronderzoek"; Veth, "Overzicht van hetgeen."

played a decisive role in changing the instructions given to the Natuurkundige Commissie in the 1830s).³⁷

Initially, Kuhl and Van Hasselt had ambitious plans. They wanted to write a treatise on the fauna of Java, using the material collected during their time in the field and the Leiden collection, including specimens collected by Reinwardt.³⁸ They were confident of their knowledge and so they started carrying out part of the systematic work while still on Java. Kuhl and Van Hasselt wrote to Temminck: "Hardly a day goes by without making a new Genus or designating a new Species, and we still collect a great deal, which we cannot examine now and which we intend to research once we are in Europe."³⁹ Their zoological notes consist mostly of descriptions of the species they collected, with their scientific name, references (if known), localities, observations on behavior, description of external morphology and extensive anatomical notes. Very often, the scientific name given to the animal was followed by a *Mihi*, or a *Nobis*, the Latin equivalents of "mine" or "ours," which, placed after a species name, indicates that the author of the manuscript is describing and naming the species for the first time. Kuhl and Van Hasselt were sure that they were describing species new to science. This seems quite a bold assumption, taking into account how isolated they were (news could take months to reach them) and that they had very limited access to scientific literature. Indeed, many of the species they described as new were simultaneously or shortly afterwards described and published elsewhere. They relied, however, on their superb knowledge of the fauna and flora of Java, gathered while in the Netherlands, Berlin, London and Paris. Their proficiency was even more impressive given their ages: both were then in their early twenties. They had no other books than those they had brought with them, and a self-made atlas of the animals and plants known at that time, a compilation of plates from "the best natural history writers" representing "an almost complete *Systema Natura* in plates."⁴⁰ Van Hasselt's library, for example, consisted only of Raffles' *History of Java* (1817), Sonnerat's *Voyage aux Indes orientales et à la Chine* (1782)

³⁷ The letters were published in several issues of the *Annales Générales des Sciences Physiques* from Brussels (1821); *Algemeene Konst- en Letterbode* from Haarlem (1821–1824), *Isis oder Encyclopädische Zeitung von Oken* (1822), and in the *Bulletin des sciences naturelles et de géologie* from Paris (1824). A list of these letters can be found in Klaver, *Inseparable Friends*, 57–63.

³⁸ Greshoff, "Kuhl en Van Hasselt," 66; Klaver, *Inseparable Friends*, 29; Veth, "Overzicht van hetgeen," 28.

³⁹ Heinrich Kuhl and Johan C. van Hasselt to Temminck, 18 July 1821; published in Heinrich Kuhl and Johan C. van Hasselt, "Pjihorjavor, aan den voet van den Pangerango, 18 Julij 1821," *Algemeene Konst- en Letterbode* 1, no. 7 (1822).

⁴⁰ As indicated in the will of Johan Conrad van Hasselt, 2 October 1823, Naturalis Biodiversity Center Archives, Archive Natuurkundige Commissie, MMNAT01_AF_NNM001001046_008. The atlas is not in the Naturalis archives. It is possible that it remained in Bogor, or that it has been lost. My thanks to Dr. Andreas Weber from the Department of Science, Technology and Policy Studies at the University of Twente, the Netherlands, for bringing this interesting piece of the archives to my attention.

and the first volume of Johannes van den Bosch's *Nederlandsche bezittingen in Azië, Amerika en Afrika* (1818).⁴¹ Hardly comprehensive, and not very helpful for their intended zoological treatise.

Temminck's opinion was that Kuhl and Van Hasselt's plans, the description and classification of Java's fauna, should be postponed until they had returned to the museum, an environment far better furnished to carry out the comparative work that was—and still is—the basic practice of systematics. After Kuhl's untimely death (three days short of his twenty-fifth birthday), Temminck wrote to the minister of Public Education, National Industry and the Colonies: "To make good field notes, to collect everything and to take the utmost care possible for the collections, as often as possible dispatch shipments to the Motherland, this is the primary instruction for a naturalist in the tropical regions."⁴²

Temminck had seen how Kuhl was trying to do it all at the same time and in doing so, he left things unfinished, his field notes were hastily written, almost stenographically, and badly preserved specimens were lost. It was just impossible to use the same specimen for anatomical observations, describe it as a new species, make drawings and then prepare it to be shipped and preserved in a cabinet for systematic study. Kuhl had found it difficult, it seems, to focus on only one thing at a time, which made him, in Temminck's eyes, a bad collector: "I do not want to deny it and the results show it, that Kuhl was not the right man to enlist for the expansion of a museum collection."⁴³ This was not meant to belittle Kuhl. On the contrary, Temminck thought very highly of him. But Kuhl could not restrain his insatiable curiosity to investigate *everything* and at the same *time* in the same *space*, that is, while in the field, and Temminck lamented that "he could have done far more useful work in Europe, but he considered such a job to be too limited, however broad it may seem to others."⁴⁴

The only way to fulfill Kuhl's potential would have been, according to Temminck, for him to have settled in Bogor, surrounding himself with all the facilities and instruments he needed, recruiting an army of helpers and assistants, where he could have become the head of a research center. But even then, Kuhl would have needed a network of correspondents in Europe to whom he could send his notes and drawings for comparison with the material in the European cabinets.⁴⁵ But Kuhl's situation was

⁴¹ Will of Van Hasselt, 2 October 1823, Naturalis Biodiversity Archives, Archive Natuurkundige Commissie, MMNAT01_AF_NNM001001046_007.

⁴² Temminck to the minister of Public Education, National Industry and the Colonies, 21 March 1822; quoted in Veth, "Overzicht van hetgeen," 29.

⁴³ Temminck to the minister of Public Education, National Industry and the Colonies, 21 March 1822.

⁴⁴ Temminck to the minister of Public Education, National Industry and the Colonies, 21 March 1822.

⁴⁵ Temminck to the minister of Public Education, National Industry and the Colonies, 21 March 1822.

infinitely more precarious, and the scene Temminck sketched, which sounds like a local natural history museum, was at that time far from attainable.⁴⁶ Incidentally, a few years later, in 1835, the General Governor and the Bataviaasch Genootschap der Kunsten en Wetenschappen (Batavian Society of Arts and Sciences) were lobbying for the foundation of a natural history museum in Java. Temminck fiercely opposed the idea, in the first place because the climate would practically guarantee that the specimens would rapidly decay, but it is not inconceivable that Temminck, already having economic difficulties to run the Leiden museum, was not keen on having to compete for money and specimens with a second museum, and one so far from his reach. The Batavian Society finally set up the Museum Zoologicum Bogoriense in 1894.⁴⁷

The members of the Natuurkundige Commission were appointed by the Dutch government and viewed by Temminck mainly as naturalist explorers, having as one of their main tasks observing and collecting minerals, animals and plants—at least, until the colonial government changed its policies after 1830. But Temminck also saw their time in the field as a stepping stone towards a career in systematics once they were back at home. He encouraged any man who aspired to become a member of the Commission to be skilled in this particular discipline. The way to achieve this was, seemingly, to spend considerable amounts of time working with natural history collections.

Kuhl for example, had been sent to London and Paris for additional training, after completing his education in the Netherlands first as a student of Van Swinderen and later spending some months with Temminck helping to identify his specimens during the winter of 1818–1819.⁴⁸ The training he received was focused on a two-step approach: the first one consisted in observing, identifying, collecting, preserving and shipping animals and plants for further study, and enriching the Leiden collections. The second step required the comparison and arrangement of the specimens using collections and literature as a reference, which would eventually result in publications in systematics. While the first step was carried out in the field, the second required a natural history cabinet and an extensive library. The young members of the Commission were actually expected to be able to excel in both, at least until it became painfully clear that their life-expectancy was exceptionally short in the tropics.

The contrasts in goals and methods between field work and cabinet study had already been the source of tensions in Paris decades before the Natuurkundige

⁴⁶ See also Gijzen, “'s Rijks museum”; Klaver, *Inseparable Friends*.

⁴⁷ Veth, “Overzicht van hetgeen,” 88; see also Hans Groot, *Van Batavia naar Weltevreden; Het Bataviaasch Genootschap van Kunsten en Wetenschappen, 1778–1867* (Leiden: Brill, 2009).

⁴⁸ Klaver, *Inseparable Friends*, 13.

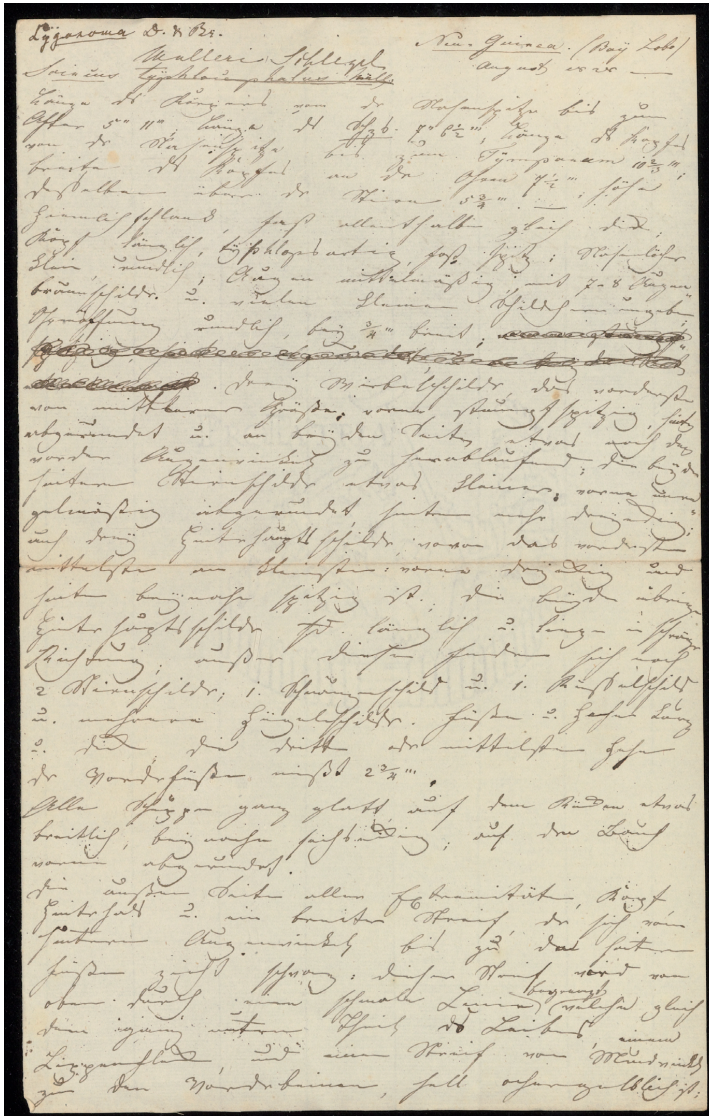


FIGURE 6.1. An example of Heinrich Kuhl's fieldnotes.

Commissie had been conceived. In 1807, Georges Cuvier wrote a review of Humboldt's fieldwork report, making a very clear distinction between the "field naturalist" and the "sedentary naturalist." Cuvier noted that the field naturalist was able to see the wonders of nature in full, observing the living beings in their own environment, but these observations were forcibly brief in time and confined to the place where the *voyageur* was at that particular moment: "He [the field naturalist] is thus deprived of the possibility of comparing each being with those like it, of rigorously describing its

characteristics, and is often deprived even of books which would tell him who had seen the same thing before him." But on the other hand:

A thousand things escape him [the sedentary naturalist] about the habits and customs of living things which would have struck him if he had been on the spot. Yet these drawbacks have also their corresponding compensations. If the sedentary naturalist does not see nature in action, he can yet survey all the products spread before him. He can compare them with each other as often as is necessary to reach reliable conclusions. He chooses his own problems; he can examine them at his leisure. He can bring together the relevant facts from anywhere he needs to. The traveller can only travel one road; it is only really in one's study (*cabinet*) that one can roam freely through the universe, and for that, a different sort of courage is needed, courage which comes from unlimited devotion to the truth, courage which does not allow its possessor to leave a subject until, by observation, by a wide range of knowledge, and connected thought, he has illuminated it with every ray of light possible in a given state of knowledge.⁴⁹

Cuvier's main point was that "usually, there is as much difference between the style and ideas of the field naturalist, and those of the sedentary naturalists, as there is between their talents and qualities," while arguing that it was the museum scientist who led natural history towards the truth. In passing, he made the case that the true hero is not the valiant *voyageur* (much admired by the general public), but the tenacious and knowledgeable sedentary naturalist.⁵⁰ Cuvier's review of 1807 seemed to be specially aimed at discrediting Humboldt's work and to claim that it was his own work that would lead to the truth. Perhaps the mutual antipathy of these two authoritative figures and the Muséum politics played an important role in Cuvier's review.⁵¹ Humboldt had a very different view of the subject: he believed that it was his all-encompassing work, multidisciplinary in a way, that would lead man to understand nature.

Dorinda Outram has referred to this particular passage. She focuses her discussion of the excerpt on the meaning of space for the understanding of natural history, mostly on the *distance* between the sedentary naturalist and his objects of study. She stresses the

⁴⁹ Georges Cuvier, "Analyse d'un ouvrage, de M. Humboldt, intitulé: Tableaux de la nature ou considérations sur les deserts, sur la physionomie des végétaux, et sur les cataractes de l'Orenoque," in *Fonds Cuvier 3159* (Paris: Library of the Institut de France, 1807); quoted and translated by Outram, *Georges Cuvier*, 62-63. See also Outram, "New spaces in Natural History," 259-61. However, I believe that the correct translation for *cabinet* should be "natural history cabinet" or "collection," as the term *cabinet* was generally used as short for *cabinet d'histoire naturelle*.

⁵⁰ Cuvier, "Analyse d'un ouvrage, de M. Humboldt"; quoted and translated by Outram, *Georges Cuvier*, 62-63.

⁵¹ See, for example, Coleman, *Georges Cuvier, Zoologist*; Outram, *Georges Cuvier*.

influence of different experiences of space on two types of naturalist (the field naturalist and the sedentary one), leading to different practices of natural history. She writes: "The field naturalist is seen as highly responsive, engaging with each passing incident in the natural world around him, erecting few or no defences against the passage of rapid, immediate impressions into his inner world. The sedentary naturalist, on the other hand, is seen as preoccupied with both physical and psychic distance, and with the belief that out of distance comes truth. In doing this, Cuvier is engaging natural history with a new cultural value."⁵² However, there is an additional and crucial difference between the Humboldt-style *naturaliste-voyageur* and Cuvier: they were occupied with different subjects, which required different methods. When Outram writes "Muséum work in natural history needed close physical control involved in the preparation of delicate specimens" to illustrate the need of the museum naturalist for order and a "guarded inner space," she is referring more to the conditions for practicing systematics and comparative anatomy, than to the differences between field and museum work. These disciplines do, indeed, require physical control over the specimens, but phytogeography or meteorology, for example, do not. The distinction is an important one, especially if conclusions are drawn for the history of natural history: the tensions between Cuvier and Humboldt, or Reinwardt and Temminck, do not only relate to the *space* in which they were working, or their experience of an inner and an outer world, but also to the *disciplines* on which these naturalists were focusing.

Systematics and its counterpart disciplines, anatomy and physiology, relied on *comparison*, hence Cuvier's and Temminck's insistence on the necessity of working not in the field, and not in the colonies, but in a cabinet of natural history in Europe. Humboldt's methods were different because, simply put, he was practicing a different discipline of natural history altogether. His work was mainly a geographical and physical study of nature, based not only on observation but mostly on measurements, and these could only be made in the field. There were different kinds of field naturalists, as there were different kinds of sedentary naturalists, as a consequence of the breaking up of natural history into several disciplines. Kuhl and Van Hasselt, for example, were in the field with systematics as their main goal. They were busy with discovering, describing and naming new species, which were then arranged in the natural system. This was clearly not Humboldt's main concern. But when he did turn his attention to new species, Humboldt's field notes were remarkably similar to those made by Kuhl

⁵² Outram, "New spaces in Natural History," 262.

books. Temminck asked Humboldt for information one could only gain in the field, and Humboldt asked Temminck for facts only he had access to in large collections. In 1822, Temminck sent Humboldt a plate by Nicolas Huet of the Andean condor, intended for the *Nouveau recueil de planches coloriées*. Huet's watercolor had probably been made in the Kaiserliche Hof-Naturalienkabinett in Vienna.⁵⁴ Well aware of the limitations of describing stuffed specimens, Temminck asked the German naturalist for feedback on the accuracy of the plate, as Humboldt had seen the bird during his travels. Humboldt had described the bird in 1811.⁵⁵ The Andean condor's head was a tricky thing to draw from museum specimens, as it has skin folds that inevitably change form and color after the process of stuffing, and Temminck therefore asked the one man who he knew had seen the condor alive, and had drawings of it made in the field.⁵⁶ In a footnote to his description of the condor, Temminck reminded the reader of the problems one encountered studying stuffed animals, and the need of field observations to come to the aid of the sedentary naturalist:

It is necessary to point out to those who compare our plate with that published by M. von Humboldt, that the drawing made by this scholar and retouched by Barrabant pictures the head and neck drawn from a living individual; in the plate attached here, these same parts were drawn from a stuffed one. Before publishing it I submitted my engraving to M. van Humboldt; this scholar finds that the occiput is too raised, the back is too arched and the transverse wrinkles of the neck are not at all visible; all these defects are the result of a deficient preparation of the skin in the mounted individual.⁵⁷

Temminck and Laugier decided not to alter the original design by Huet, but included Humboldt's comments in their description, in a footnote. Not satisfied with this, Temminck wrote an *Addition à l'article du Catharte Condor* with the description and a plate of the female Andean condor, and a detail of the head of the male. This time, the plate was drawn after a living bird from the "Ménagerie du Jardin du Roi."⁵⁸ The

⁵⁴ Raat, "Humboldt and Temminck."

⁵⁵ Alexander von Humboldt and Aimé Jacques Alexandre Bonpland, "Essai sur l'histoire naturelle du Condor, ou du Vultur Gryphus de Linné," in *Recueil d'observations de zoologie et d'anatomie comparée, faites dans l'Océan Atlantique, dans l'intérieur du Nouveau Continent et dans la Mer du Sud, pendant les années 1799, 1800, 1801, 1802 et 1803*, vol. 1 (Paris: F. Schoell et G. Dufour, 1811), 26-30.

⁵⁶ Humboldt to Temminck, [February-June 1822], Naturalis Biodiversity Center Archives, Archive C. J. Temminck, NAT_ARC_TEM_01613-15.

⁵⁷ Temminck and Laugier de Chartrouse, "Catharte Condor. *Cathartes gryphus* Temm.," in *Nouveau recueil de planches coloriées d'oiseaux*, vol. 1, 35, pl. 133 (livraison 23). For the dates of publication of each livraison, see Dickinson, "'Nouveau recueil de planches coloriées' of Temminck & Laugier."

⁵⁸ Temminck and Laugier de Chartrouse, "Addition à l'article du Catharte Condor," in *Nouveau recueil de planches coloriées d'oiseaux*, vol. 1, 40, pl. 494 (livraison 83).

differences between the drawing based on the Vienna specimen and the one made after a living specimen are striking.

Occasionally, Humboldt also asked Temminck for advice, appealing to his extensive knowledge of which kinds of animals were to be found where—the kind of knowledge one gathers from natural history collections. After all, Temminck did formulate a law on the geographical distribution of animals. Accordingly Humboldt approached Temminck while preparing the second volume of *Cosmos*, asking for examples of the same kind of animals inhabiting separate regions:

The second volume of the *Cosmos*, which will appear in a few months, will include the subject of zoological geography, the analogies and the contrasts between the great continental landmasses; the islands and neighboring continents; the forms that replace each other in America and Asia; the species of mammals and birds at great distances that we can certainly assume to be identical. Is there a bird that is known to be present everywhere, are there identical birds in the tropics of two continents, e.g., Africa and America? Are there identical mammals and birds in the northern and southern temperate regions? Ignorant as I am, I would like to be precise in the few pages I will dedicate to animal Geography. The sources I need are not easily accessible to me. I would like to have a few spicy examples for these questions. What you so easily can provide, is solid gold.⁵⁹

Temminck was happy to help, and attached to his letter some notes on the matter. He also planned to send Humboldt the first volume of the *Coup-d'oeil général sur les possessions néerlandaises dans l'Inde Archipélagique*. Unfortunately, Temminck's notes seem to be missing, and Humboldt never published this zoological essay.⁶⁰

Just as the process of stuffing animals influenced their description and hence, their classification, the preparation of plants for the herbaria also had major consequences for systematics. Botanical specimens are particularly susceptible to physical change after preparation: dried plants are glued to sheets of paper, losing in the process their original form and color. Often, botanical specimens are missing important features like flowers, fruits or roots. What might seem an insurmountable stumbling block for botanists was actually incorporated in their classification systems from the beginning, and the main tool for botanical systematics was, as it was for zoology, comparison. This is why botanists like Joseph Hooker also contended, as Temminck and Cuvier did, that the

⁵⁹ Humboldt to Temminck, 16 August 1846, Naturalis Biodiversity Center Archives, NAT_ARC_TEM_01616-17.

⁶⁰ Raat, "Humboldt and Temminck," 30. There are no copies of Temminck's notes on biogeography in the Naturalis Biodiversity Center Archives.



FIGURE 6.4. Plates of the Andean condor from Humboldt and Bonpland's "Essai sur l'histoire naturelle du Condor." In *Recueil d'observations de zoologie et d'anatomie comparée, faites dans l'Océan Atlantique*, plates VIII ad IX (1811).



FIGURE 6.5. The Andean condor as illustrated in Temminck and Meiffren Laugier's *Nouveau recueil de planches coloriées d'oiseaux*, vol. 1, Pl. Col. 133 and 494 (1838). The detail of the head was made from a living specimen, while the plate on the left was made from a stuffed specimen.

place for systematics was the herbarium, in the metropolis. Hooker particularly disliked the proliferation of genera and species. He pointed out how local botanists were prone to describe as a separate species what actually was, in Hooker's view, a variety:

the local botanist looks closer, perceives sooner, and often appreciates better, inconspicuous organs and characters, which are overlooked or too hastily dismissed by the botanist occupied with those higher branches of the science, which demand a wider range of observation and broader views of specialities; and there is no doubt but that the truth can only be arrived at through their joint labours; for a good observer is one thing, and the knowledge and experience required to make use of facts for purposes of generalization, another: minute differences however, when long dwelt upon, become magnified and assume undue value, and the general botanist must always receive with distrust the conclusions deduced from a few species of a large genus, or from a few specimens of a widely distributed plant.⁶¹

For him, local botanists lacked an overview of the whole range of species and varieties of the plants they were studying. Furthermore, using living specimens instead of dried ones was a source of misinterpretation for the colonial botanist. It led him to focus on minute and unimportant details. Using as an example the classification of a particular fern from New Zealand, Hooker wrote: "In this (and in many similar cases) he [the colonial botanist] must bear in mind that I have examined many hundreds of specimens of the plant, gathered in all parts of the south temperate hemisphere, and have found, after a most laborious comparison, that I could not define its characters with sufficient comprehensiveness from study of its New Zealand phases alone."⁶² The classification systems of local floras evidenced the tension between the field naturalist and the herbarium botanist; or, as Jim Endersby puts it, between "the empire's center and periphery."⁶³

The differences in approach and subjects between field and collection naturalists were further complicated by the fact that *field naturalist* was not a single, unequivocal category. Not all of them were out there doing the same thing. Temminck had built a network of collectors scattered all over the Dutch colonies, trading posts, consulates and embassies. Some were fellow naturalists, some were hunting aficionados, and others learned to collect on the job. Temminck's network was a heterogeneous mix of people. Some could be rightfully called field naturalists, doing specific research in the field.

⁶¹ Joseph Dalton Hooker, "Introductory Essay," in *The Botany of the Antarctic Voyage of H.M. Discovery Ships Erebus and Terror in the Years 1839–1843 under the Command of Captain Sir James Clark Ross. II, Flora Novae-Zelandiae*, vol. 2 (London: Reeve Brothers, 1853), xiii-xiv.

⁶² Hooker, "Introductory Essay," *Flora Novae-Zelandiae*, vol. 2, xiii-xiv; Endersby, *Imperial Nature*, 155.

⁶³ Endersby, *Imperial Nature*, 154.

Others were engaged dilettantes with considerable knowledge, while some were only collecting for Temminck. To guide all of them, Temminck wrote a manual with precise instructions on how to kill, preserve and send every kind of animal.⁶⁴

Different people were exploring and collecting nature in quite different ways, which is partly a reflection of the fact that natural history was breaking up into separate disciplines, and partly the result of the way the European expansion took place and how each country tackled the exploration of its colonies. This last point is a complicated, multifaceted aspect of the history of natural history that, at least for the Netherlands, needs to be investigated more in-depth.⁶⁵ In addition the distinctions between various kinds of field naturalists are blurred not only because of their disparate activities and interests, but also because, as they worked, if they survived long enough in the tropics to learn and develop, their knowledge, goals and methods also grew and developed, and the meaning of terms like collector, field naturalist, colonial naturalist or *naturaliste-voyageur* was constantly changing. Therefore, any comparison between the field naturalist and the sedentary naturalist requires specification and should take into account that the goals, subjects and methods of all of them varied according to their field of expertise, experience and relation to colonial and imperial governments. When it came to systematics, the cabinet naturalist was the one with the upper hand in influencing the discipline. He had access to large collections and enjoyed the bird's-eye view needed for the classification of the huge diversity of nature. Stevens notes that "[t]here was indeed such a split [between field naturalists and the closet naturalists], but during the first seventy years of the nineteenth century it was closet naturalists who occupied most of the paid positions at universities, herbaria, and academies, and it was their approach that prevailed."⁶⁶

The place to practice systematics was therefore the natural history collection, not the field. In the Netherlands, the collections suitable for systematics were not to be found in the universities, which were specifically designed for teaching and practicing comparative anatomy and physiology. Systematics was practiced in 's Rijks Museum in Leiden and other natural history cabinets. Each of these natural history collections contained specimens prepared and ordered in such a specific way as to be useful to its intended users: anatomical preparations for the professors, stuffed specimens for the systematists. This was, however, not as clear-cut as Temminck had desired, as 's Rijks

⁶⁴ Temminck, *Voorschrift*.

⁶⁵ See, for example, Van Berkel, Van Helden, and Palm, *History of Science in the Netherlands*; Boomgaard, "The Making and Unmaking of Tropical Science: Dutch Research on Indonesia, 1600–2000"; Goss, *The Floracrats*; Honig and Verdoorn, *Science and Scientists in the Netherlands Indies*; Weber, "Hybrid Ambitions."

⁶⁶ Stevens, *Development of Biological Systematics*, 207.

Museum was also supposed to provide the Leiden University with specimens for teachers and students. The University of Groningen had a large, systematically ordered collection very suitable for taxonomy. So much so, that men like Kuhl had been trained in Groningen, under Van Swinderen's care. Even so, natural history collections in the Netherlands were, generally speaking, divided into systematic collections, with Leiden at the forefront, and the academic collections for anatomy and physiology, like the one at Leiden University. Hieke Huistra has demonstrated how the way of using anatomical collections—a hands-on approach where preparations were meant to be handled—was similar in Western Europe, even though there were local differences.⁶⁷ Whether the differences between university and museum collections were the result of the universities' tradition of teaching natural history as applied to medicine, a situation particular to the Netherlands, or a consequence of the authority of specific key figures like professor Brugmans or Temminck, needs further exploring.

In all events, in other countries the situation could be very different from the Dutch one. The Muséum National d'Histoire Naturelle in Paris, for one, was unlike the Leiden museum in almost every possible respect. Between 1800 and 1850, the Muséum was an open, public space, with zoological and botanical gardens, combining academic teaching and research, a place where comparative anatomy and systematics shared avenues and offices, and where different views of classification were exposed in exhibition galleries and hotly debated in public lectures.⁶⁸ By 1832 the scientific staff of the Parisian museum was made up of titular professors occupying thirteen different chairs. Many of them were also simultaneously employed in various remunerated posts in different institutions, mostly academic institutes and Écoles. This made the museum researchers visible and very present in the educational system. They were also involved in the Academy of Sciences and allowed to enjoy several salaries (*cumul*).⁶⁹ Besides the obvious financial advantage of multi-tasking, the professors had access to a larger public and to a greater number of colleagues and societies. None of this was possible in Leiden, where Temminck's views dominated the research, the galleries, and access to the collections for the general public as well as for academics and students. The Leiden museum was isolated by the divorce between the Universities and the museum, especially from students and scientific communities. It evolved on its own, and its core business was, and has remained for almost two hundred years, systematics.

⁶⁷ Huistra, *Afterlife of the Leiden Anatomical Collections*, 159.

⁶⁸ Outram, "New spaces in Natural History," 249-65; Limoges, "The Development of the Muséum d'Histoire Naturelle of Paris, c. 1800–1914."

⁶⁹ Limoges, "The Development of the Muséum d'Histoire Naturelle of Paris, c. 1800–1914," 215.

The situation in Leiden was similar to that in other European museums, like those in Vienna, Berlin and London, which had also been focusing on systematics since their beginnings. The question of the relation between universities and natural history museums is a very interesting one to explore. It would help to discern how systematics, its evolution and its status were related to institutional politics within and without the museums. In Germany, the Zoologische Museum was part of the University of Berlin, founded in 1810. Karl Illiger was its first director, and after his death in 1813, he was succeeded by Hinrich Lichtenstein. Both men worked on systematics, and both corresponded and exchanged specimens with Temminck during their respective directorships. The Berlin museum was also an academic collection, tied to the university's curriculum, as well as a public institution.⁷⁰

In Victorian Britain, the situation was slightly different. While men of independent means put their personal fortunes at the service of science, paid scientists were regarded as having a lower social status.⁷¹ Hugh Strickland was the epitome of gentlemanly science. While Strickland remained in close association with the University of Oxford, and his collection was bequeathed to the Museum of Zoology of the University of Cambridge after his tragic death (he was hit by a train while examining geological strata along the railway, at the age of forty-two).⁷² Strickland's work on systematics had considerable influence during the first half of the nineteenth century. He worked mostly in his private collection, while he had access to other British natural history collections as well. Strickland benefitted from corresponding with a huge network of peers, but he was not employed in a museum or a university. For the botanist Joseph Hooker, however, gentlemanly science was not an option, and after being rejected for the Chair of Botany at the University of Edinburgh in 1845, he decided not to further pursue a university post. At the university, botany was considered a minor department of the medical profession, not a discipline in its own. Hooker therefore declined a chair at Glasgow University (the same chair his father William Hooker had held) and instead accepted a job as botanist for the Geological Survey of Great Britain in 1846. After almost a decade of travelling and publishing, he finally became assistant-director of the Royal Botanic Gardens in Kew, and its director in 1865. Thus Hooker's place as a systematist

⁷⁰ T. G. Ahrens, "The Ornithological Collections of the Berlin Museum," *Auk* 42 (1925): 241-45; Lynn K. Nyhart, *Modern nature: the rise of the biological perspective in Germany* (Chicago: University of Chicago Press, 2009), 46; Erwin Stresemann, "Die Entwicklung der Vogelsammlung des Berliner Museums unter Illiger und Lichtenstein," *Journal für Ornithologie* 70 (1922): 498.

⁷¹ David Allen, "Amateurs and Professionals," in *The Cambridge History of Science. Vol. 6: The Modern Biological and Earth Sciences*, ed. Peter J. Bowler and John V. Pickstone (Cambridge: Cambridge University Press, 2009), 15; Endersby, *Imperial Nature*, 8-12.

⁷² William Jardine, red. *Memoirs of Hugh Edward Strickland. With a Selection from his Scientific Writings* (London: John Van Voorst, 1858); Rookmaaker, *Calendar of the Scientific Correspondence*.

was also outside the university, as a result of his personal circumstances, as well as of the universities' tradition of including botany in the medical sciences.⁷³

The conditions under which each of these men worked varied greatly and were determined by very different factors, from the status of their disciplines within the universities, to their personal fortunes, to the political situation in their respective countries.⁷⁴ The institutional history of each discipline is relevant, as whether or not systematics was being practiced and taught at the universities had direct consequences for higher education and the continuity of the field, in the education of students and the interaction between students and professors. Therefore, the institutional base of systematics matters. The divorce between the University of Leiden and 's Rijks Museum was not without consequences for Temminck. It also meant that, besides competing for governmental funding and collection specimens, Temminck was working from an isolated position within the Netherlands, as there were just a handful of systematists in the country. Most of them worked under Temminck in the Leiden museum. Temminck's network consisted mainly of fellow systematists, collectors and field naturalists, but he had little or no contact with students and professors in other disciplines, and no influence at all in higher education. It also seems that Temminck kept aloof from Dutch learned societies, like the *Hollandsche Maatschappij der Wetenschappen* in Haarlem. Perhaps his focus on systematics, quite exceptional in the Netherlands, was the main reason for this. Temminck was also spending a lot of time on the museum administration and struggling for resources, which was another consequence of the separation of the museum from the university.

During the last quarter of the nineteenth century, when the universities started enlarging their courses of comparative anatomy and physiology to accommodate the new experimental zoology, 's Rijks Museum van Natuurlijke Historie continued to carry out its systematic work, secure in its own tradition and independent from laws and resolutions on education and university research programs.⁷⁵ The Leiden museum was still in competition with universities and laboratories for funding and status, but the institution prevailed and set the basis for modern taxonomy, while fulfilling its role as a repository of specimens.

⁷³ Endersby, *Imperial Nature*, 11.

⁷⁴ An overview of these and other factors can be found in Bowler and Pickstone, *The Modern Biological and Earth Sciences*.

⁷⁵ Jacob van der Land, ed. *The History of Natural History in Leiden* (Leiden: Naturalis, 2001). For more information on museums as sites for science, see John V. Pickstone, "Museological Science? The Place of the Analytical/Comparative in Nineteenth-Century Science, Technology and Medicine," *History of Science* 32, no. 2 (1994); Sally Gregory Kohlstedt, "Museums: Revisiting Sites in the History of the Natural Sciences," *Journal of the History of Biology* 28, no. 1 (1995).

Temminck's podium

Reconstructing the whole intellectual, political, cultural and institutional landscape in which Temminck lived and worked would require an extensive examination of the Dutch as well as the European context, which would greatly surpass the scope of this chapter. However, a look at how Temminck communicated, to whom and from where may provide a sketch of his surroundings. The effect of his environment on his views and work may become apparent.

The most obvious feature of Temminck's landscape is 's Rijks Museum van Natuurlijke Historie in Leiden. The museum had been founded by king Willem I as part of his strategy of enhancing the scientific and political influence of the country, but just about a decade after its foundation, the museum was already struggling for funding. This had an enormous effect on Temminck's position, who was overwhelmed by administrative tasks, lobbying for funding, and trying to deal with the material pouring in from the Dutch East Indies. But even before the museum lost the support of the government, the king's strategy to nationalize science and uplift the museum was already backfiring, and resulted in the isolation of its scientists, rather than international prestige. In particular his idea that the results of Dutch science should be published in Dutch created a huge hurdle for both the Natuurkundige Commissie and Temminck.

The king's instructions for the Natuurkundige Commissie commanded that "no description of the objects observed or sent, nor observations of natural phenomena, will be published in a foreign language nor sent to foreign societies nor scholars, except with permission from the Minister [of Public Education, National Industry and Colonies] and after previous publication by one of the learned societies or journals of this Kingdom."⁷⁶ The king was determined to claim for the country any new discoveries made by the Commission, but the Dutch language was not one commonly understood outside the low countries. French was the scientific language of the time, so this decision included in article no. 3 of the royal decree did not help in promoting the Commission or the museum. If the Dutch discoveries could not be read, spread and shared outside the Netherlands because of the language barrier, how could the work of the members of the Commission and the museum staff be noted, let alone recognized? Confronted with this dilemma, Temminck decided to bypass the inconvenient article no. 3 whenever possible.

As thousands of specimens and no fewer pages of field notes and drawings arrived from the Indies, Temminck, short of staff and work piling up, had to decide how to deal with the material. One of the first things Temminck did was to publish extracts from

⁷⁶ Royal Decree no. 251, 29 April 1820, art. 3; Veth, "Overzicht van hetgeen," 22; Striekwold, Robbert J., "Naam maken: wat de dood van twee negentiende-eeuwse wetenschappers ons leert over de natuurhistorie," *Ex Tempore* 37, no. 3 (2018): 217-218.

Kuhl and Van Hasselt's letters in two journals, the *Annales Générales des Science Physiques*, published in Brussels, in French, and the *Algemeene Konst- en Letterbode*, which appeared in Haarlem, in Dutch. Many of the letters that were published in Dutch were later translated (and at times slightly edited) by Temminck and published in the Parisian *Bulletin des Sciences Naturelles et de Géologie*.⁷⁷ Disclosing their letters was an attempt to distribute Kuhl and Van Hasselt's discoveries as quickly as possible. Considering that these members of the Commission were not returning home to publish their findings themselves, Temminck decided to distribute the material among the experts that could publish them. He would deal himself with the mammals and the birds, and Heinrich Boie was in charge of the amphibians, while De Haan took care of the mollusks, together with the Baron André Étienne J. d'Audebert de Férussac—a French naturalist specialized in mollusks.⁷⁸ But there were no Dutch experts that could describe and classify the thousands of fishes and insects collected by Reinwardt, Kuhl and Van Hasselt between 1815 and 1821. Temminck proposed that Valenciennes and Cuvier worked on the fishes. For the insects, Temminck had the celebrated entomologist and engraver from Nürnberg in mind, Jacob Sturm.⁷⁹

Achille Valenciennes, from the Zoological Laboratory of Reptiles and Fishes of the Paris museum, was working with Cuvier on an ambitious project, a *Histoire Naturelle des Poissons* which, in the end, took up twenty-two volumes. Valenciennes visited the Leiden museum twice, in 1824 and in 1827, to study the collection of fishes, many of them still undescribed.⁸⁰ While in Leiden, Valenciennes made drawings of the specimens as well as copies of the original sketches from the Commission, and selected some specimens to be sent to Paris as a loan for further study (the return of this loan proved afterwards to be rather difficult and Schlegel had to go to Paris himself to retrieve the fishes).⁸¹ Naturally, Cuvier and Valenciennes published their results in French, against the explicit wishes of the Dutch government. Invoking the famous Article no. 3, Daniel Jacob van Ewijck, from the Ministry of Public Education, National Industry and

⁷⁷ A complete list of the published letters of Kuhl and Van Hasselt can be found in Klaver, *Inseparable Friends*, 57-63.

⁷⁸ Coenraad J. Temminck, Report to the Ministry of Public Education, National Industry and the Colonies, 3 December 1824, Naturalis Biodiversity Archives, Jaarverslagen.

⁷⁹ See Coenraad J. Temminck, Report to the Ministry of Public Education, National Industry and the Colonies, 3 December 1824, Naturalis Biodiversity Archives, Jaarverslagen; Gijzen, " 's Rijks museum," 40.

⁸⁰ T. R. Roberts, "The Freshwater Fishes of Java, as Observed by Kuhl and van Hasselt in 1820-23," *Zoologische Verhandelingen* 285, no. 1 (1915);

⁸¹ Gijzen, " 's Rijks museum," 224.

Colonies, reprimanded Temminck in no uncertain terms for this flagrant violation of the royal decree—three times.⁸²

Apparently unconcerned, Temminck went ahead with his original plans. Cuvier and Valenciennes described the fishes held in the Leiden museum. Temminck himself worked on the collection of mammals and published the *Monographies de mammalogie*, in French. The first volume appeared in 1827, in Paris, and the second volume appeared simultaneously in Leiden and in Paris in 1835. More than eight hundred species of birds were described and depicted in the *Nouveau recueil de planches coloriées d'oiseaux*. About two hundred of them had been collected by the members of the Natuurkundige Commissie.⁸³ Temminck being the author, it seems that the Ministry did not object to his works being published in French.

Temminck always chose to write his more extensive treatises in French, and only short articles were published in Dutch.⁸⁴ But then again, he later translated most of his Dutch articles into French and incorporated them in his more comprehensive publications. He did so with his essays on the Japanese fauna and with some articles on bats, later included in his *Monographies de mammalogie*. The French language was, after all, the predominant scientific language in Europe, and Temminck was not only decidedly Francophile, but first and foremost, seriously committed to reaching out to the international scientific community. Almost all Leiden curators published preferably in French as well. Boie had produced a manuscript on reptiles and amphibians (based mostly on Kuhl and Van Hasselt's notes and specimens) entitled *Erpétologie de Java* before leaving for the Indies himself as a member of the Natuurkundige Commissie.⁸⁵ Willem de Haan wrote several essays on insects, the *Mémoires sur les Métamorphoses des Coléoptères*, the first of which appeared in 1835.⁸⁶ Hermann Schlegel used German (for example, *Abbildungen neuer oder unvollständig bekannter Amphibien*, 1837–1844) and French for his scientific publications.⁸⁷ Temminck cannot possibly have been pleased

⁸² Van Ewijk to Temminck, 9 May 1825, 28 May 1825 and 24 December 1825; National Archives of the Netherlands, The Hague, Binnenlandse Zaken, nummer toegang 2.04.01, inventarisnummer 4859; Striekwold, "Naam maken," 217.

⁸³ Letter from Temminck to the minister of Public Education, National Industry and the Colonies, 24 November 1836; quoted in Veth, "Overzicht van hetgeen," 84.

⁸⁴ See Appendix II, Bibliography of Coenraad Jacob Temminck.

⁸⁵ Boie's manuscript was never published. It had been sent to Brussels for printing, but it was lost during the Belgian Revolution in 1830 (Holthuis, Rijksmuseum, 29). The manuscript, including the plates, was eventually somehow returned to the Leiden museum and is now kept in the Naturalis Biodiversity Center Archives.

⁸⁶ See also Vrolik, "Levensbericht van Wilhem de Haan."

⁸⁷ After the middle of the century, and especially after becoming Temminck's successor as director, Schlegel focussed on the Dutch fauna and wrote mostly in Dutch. Significantly, these works were not only

when, in 1839, the king commanded the publication of a comprehensive work on the natural history of the East Indian colonies, demanding that it be written in Dutch: the *Verhandelingen over de Natuurlijke Geschiedenis der Nederlandsche Overzeesche Bezittingen*.⁸⁸

Abroad, the long-expected *Verhandelingen* were met with a mixture of satisfaction and disappointment. The choice of language was criticized, and its limited scope lamented. Strickland remarked: "This superb work contains figures and descriptions of many new species from the remote islands of the Malay archipelago; and it is only to be regretted that so valuable a publication should be compiled in a language with which few men of science out of Holland are acquainted." In that same report, Strickland noted the work by Schlegel and lamented: "There is a paper by M. Schlegel on the supposed absence of nostrils in the genus *Sula*, in the *Tijdschrift voor Natuurlijke Geschiedenis*, 1839, of which, from being unacquainted with the Dutch language, I regret my inability to give a summary."⁸⁹ Obviously, it was not in the best interest of the Leiden museum and its scientists, nor of the Dutch government, to insist on publishing in Dutch.

But perhaps the greatest obstacle for Temminck in communicating with his peers abroad was the format in which his works appeared. In forty-seven years, he published (as author and editor) fourteen book titles, a total of twenty-six volumes, most of them monographs on ornithology and mammalogy. In contrast, during these four and a half decades, he wrote only sixteen journal articles, most of them in Dutch. His first articles appeared in French in the *Annales Générales des Sciences Physiques et de Géologie*, edited jointly by the Frenchmen Jean-Baptiste Bory de Saint-Vincent and Pierre Auguste Drapiez, and Jean-Baptiste van Mons, from the Académie royale des Sciences et Belles-Lettres in Brussels. After the Belgian Revolution, which had shaken him to his core, Temminck turned to the *Bijdragen tot de Natuurkundige Wetenschappen* and to Van der Hoeven and De Vriese's *Tijdschrift voor Natuurlijke Geschiedenis en Physiologie*.⁹⁰ Temminck clearly preferred to write extensive and detailed books, which he kept revising. He spent a very long time—thirty-five years, to be precise—perfecting and polishing just one title, the *Manuel d'ornithologie*. This single-mindedness was exactly

intended for fellow naturalists: Schlegel also wrote for students of natural history. See A. A. W. Hubrecht, "Hermann Schlegel," *De Gids* Jaargang 4 (1884); Schlegel, "Levensschets van Hermann Schlegel."

⁸⁸ Husson and Holthuis, "The dates of publication of 'Verhandelingen'"; Fransen, Holthuis, and Adema, "Type-catalogue of Decapod Crustacea."

⁸⁹ Strickland, "Report on the Progress," 188, 207.

⁹⁰ In 1831, Temminck had nothing but harsh words and contempt for Brussels: "The enrichment of the Academies and of that ungrateful Brussels, of those mutinous cities of Belgium, whose memorial pillars still stand despite the superstitious Vandalism, will do service to the civilization of a stupid and fanatic people; at last the works of natural history, through which the arts were cultivated, were without hesitation put under legal protection by their rebellious hands." In Coenraad Jacob Temminck, "Algemeen overzigt van de orde der Cheiroptera, en Monographie van de geslachten Harpyia en Cephalotes," *Bijdragen tot de natuurkundige wetenschappen* 6 (1831): 316, footnote.

what gained him a reputation as a thorough, comprehensive and meticulous worker. On the other hand, it kept him stuck with the same subject for decades. Despite his reputation, this preference turned against him as journals became more and more the favored place to publish, communicate and debate, especially after 1840.⁹¹ An anonymous British reviewer did not hesitate to refer to him as “the wary and tardigrade Dutchman” (sloths were at the time known as *Tardigrada*).⁹² Bonaparte, who had lamented the fact that the *Monographies de mammalogie* contained many double descriptions summed it up as follows:

The same naturalist [Temminck] has resumed, after a long interval, his very useful Monographs of Mammalia, among which the most anxiously expected was that intended to clear up the genus *Vespertilio*, more especially as the publication of his researches on these animals had been retarded more than twenty years. As long ago as 1830, I remarked in my *Osservazioni sulla seconda edizione del Regno Animale del Cuvier*, “I shall abstain from saying more on the Chiroptera (of America) to which I at one time gave my attention, but afterwards entrusted the specimens which I had collected to the learned Temminck, who is in a position to make a better use of them than I can do.” We may therefore conclude, that he has devoted very little time to this subject during these twenty years, for notwithstanding his immense materials, and the aid which he has received from every side, the work would have afforded us but little light if its defects had been less prominent.⁹³

Temminck’s sluggish pace meant that he was losing touch with developments in systematics, at least, for popular groups of birds and mammals. Charles Darwin worked for nearly eight years on barnacles, and his monograph was widely celebrated.⁹⁴ Besides Temminck’s preference for exhaustive and in-depth monographs, there might be other reasons for his absence from the domain of journals. The most obvious one is lack of time. He was painstakingly working on his books and managing a national museum, a task that was not without its challenges and accompanying headaches. On top of that, the ever-growing museum lacked, despite its towering size and prestige, its own journal.

⁹¹ In 1845, Strickland listed no fewer than forty-two journals of interest for ornithologists, indicating that there even were “many others.” (Strickland, “Report on the Progress,” 200).

⁹² “A Retrospect of the Literature of British Ornithology, from the Close of the 17th Century to the Present Time,” *The Analyst: a Quarterly Journal of Science, Literature, and the Fine Arts* 3, no. 3 (1836): 94.

⁹³ Bonaparte, “Observations of the State of Zoology,” 21-22.

⁹⁴ Charles Darwin, *A Monograph on the Sub-Class Cirripedia, with Figures of all the Species.*, 2 vols. (London: Ray society, 1851-1854).

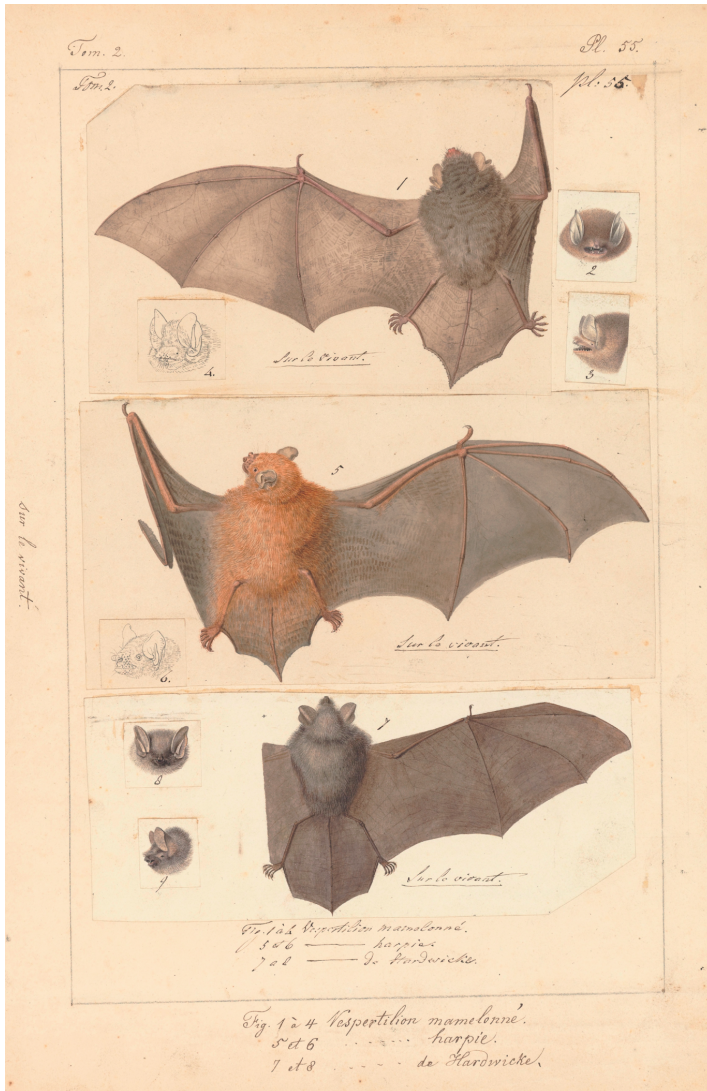


FIGURE 6.6. Preliminary design for plate no. 55 of of Temminck's *Monographies de mammalogie*. For his descriptions of new species of *Vespertilio* bats, Temminck used the field notes and drawings made by members of the *Natuurkundige Commissie*.

The fact that the Leiden museum did not have its own journal was not because of lack of interest on Temminck's part. He had envisioned the *Rijksmuseum* to be modelled after the Paris *Muséum*, and such a vision included, quite naturally, a museum journal. Temminck had already picked a title, *Annales du Musée d'histoire naturelle des Pays-Bas*,

but it was never to be.⁹⁵ Oddly enough, the Dutch government did not support this idea, even if it provided scientific advertisement for the museum, and did provide financial aid. In fact, during Temminck's directorate, the only funding the museum received for scientific publications was in the form of the purchase of copies of books. For example, in the case of the *Fauna Japonica*, the government agreed to buy a number of copies as a means of financial support, but the publication was largely paid for by Siebold himself, regardless of the glaring fact that all five volumes were written by the Leiden museum's staff.⁹⁶

Most curators there were either publishing in journals or paying for their works privately, as Temminck did. Some received a small financial subsidy from the government. Schlegel even took on the adventure of publishing a work entitled *Abhandlungen aus dem Gebiete der Zoologie und Vergleichende Anatomie*.⁹⁷ The enterprise did not last long. Only two issues were published, one on cetaceans in 1841, and a second one on falcons in 1843. The exact reason for the discontinuation of the work is unknown, but as Schlegel continued to be very active and interested in this particular subject, Schlegel's running out of funds seems to be a plausible explanation. The Leiden museum did not edit its own journal until 1879, under the direction of Schlegel, and tellingly, in English, entitled *Notes from the Royal Zoological Museum of The Netherlands at Leyden*, or *Notes from the Leyden Museum* for short.

The paucity of articles in journals by Temminck was accompanied by another conspicuous fact: it seems that he did not give any public lectures, nor did he contribute in a noticeable way to the activities of the Dutch learned societies. He was a member of forty different societies and institutes, from the Imperial Society of Naturalists of Moscow to the Zoological Society in London and the American Philosophical Society in Philadelphia. To get a sense of how impressive this network is, note that Strickland's Report of 1844 listed thirty-four scientific societies that were in some way or another occupied with ornithology.⁹⁸ Temminck made use of some of these societies' platforms to publish a few articles and corresponded with an extensive network of naturalists from all around the globe. But his contributions in the form of articles or lectures were very limited. His focus remained fixed on the production of extensive, detailed

⁹⁵ Holthuis, *Rijksmuseum*, 69; Veth, "Overzicht van hetgeen," 79.

⁹⁶ Holthuis, *Rijksmuseum*, 39.

⁹⁷ Hermann Schlegel, *Abhandlungen aus dem Gebiete der Zoologie und vergleichenden Anatomie* (Leiden: A. Arnz & comp., 1841–1843). Holthuis (*Rijksmuseum*, 69) noted that Schlegel published his *Abhandlungen* "at his own risk," implying that he had no financial support from the government and that it was Schlegel's own project.

⁹⁸ Strickland, "Report on the Progress," 200.

monographs. In the Netherlands, where it had been relatively easy for Temminck to participate in meetings with his peers, it seems that he only did so very sporadically.

In October 1838, Temminck read one of his papers in the meeting of the First Class of the Koninklijk Instituut van Wetenschappen (the same institute that had rejected his application for membership thirty years before), and a second one in 1841.⁹⁹ And that was it. Both papers were published in Dutch journals, in 1838 and 1841 respectively, and were translated into French.¹⁰⁰ Interestingly, both papers dealt with the mammals of Japan and how their position in the classification systems supported Temminck's law of geographical distribution. This is remarkable, as it means that the only time Temminck chose to present something to the Koninklijk Instituut, it was not on systematics. He was introducing his only attempt to tackle a purely theoretical topic, a general law, which he based on the newly discovered Japanese mammals. A reason may be that morphological descriptions do not lend themselves to being read in front of an audience, and are better presented on paper, with illustrations, tables, and all kind of comparisons and references.

Systematics, it may seem, was not a suitable topic for the meetings of learned societies. This is not to say that there were never any presentations on zoological or botanical questions. To give just one example, during the thirteenth meeting of the First Class of the Koninklijk Instituut, the members presented essays on subjects as diverse as the form of the pelvis of the woolly mammoth, the structure of the carpal bones of the orangutan, how to extract pigments from the indigo plant, and the mammal fauna of Japan.¹⁰¹ Talks solely on systematics were rare, which may also have influenced the visibility of the field within the learned societies in the Netherlands. At all events, Temminck was not a particularly engaged member of any society. Whether this was a consequence of his specialization in technical, dry systematic research, a consequence of his dislike of an animated social life, or a combination of both, remains unclear. Remarkably, in his letters Temminck never discussed difficult issues nor did he share new discoveries with any of his correspondents. His network kept him informed of new publications; it served him mostly to exchange information and specimens, but it was not his podium for debate. But then again, Temminck was a reserved man. Perhaps he

⁹⁹ The First Class was concerned with the natural sciences as broadly as one can interpret the term, including mathematics, chemistry, astronomy, zoology and botany, anatomy and physiology, geology, medicine and pharmacy.

¹⁰⁰ Temminck, "Faune des Iles de la Sonde et de l'Empire du Japon," *Fauna Japonica*, vol. 1; "Zoogdieren van Japan"; "Over eenige geslachten van Zoogdieren, een deel der Fauna der Japan uitmakende," *Verslagen en mededeelingen uitgegeven door de vier Klassen van het Koninklijk Nederlandsche Instituut van Wetenschappen, Letterkunde en Schoone Kunsten* (1841); "Urotrichus."

¹⁰¹ Held on August 31, 1841 in Amsterdam, see *Verslagen en Mededeelingen, uitgegeven door de vier Klassen van het Koninklijk-Nederlandsch Instituut van Wetenschappen, Letterkunde en Schoone Kunsten*, vol. 4 (1841).

guarded his new discoveries more zealously after his bitter experiences with Vieillot, Boitard and Knip.

There is yet another issue that characterizes how Temminck communicated. He rarely explained the principles underlying his classifications, as if his views were somehow self-evident. As we have seen from the debates he was involved in, he only offered extended explanations when compelled to defend himself against direct criticisms, like those from Vieillot and Vigors. But then again, in Temminck's time, so did most naturalists. In particular, those dealing with zoological classification were not in the habit of explaining their views in extended chapters or articles.¹⁰² There are just a few exceptions to this, such as Hugh Strickland, who devoted essays to the species and genus concepts, helped define terms like *affinity* and *analogy*, and wrote on the methods to achieve a natural classification in ornithology.¹⁰³ Likewise, the botanist Joseph Hooker explained the classificatory principles on which his works were grounded, mostly in essays that preceded his floras.¹⁰⁴ In 1853, Hooker wrote on the need of explaining one's "general theoretical views on the origin, variation, and dispersion of species":

it is very necessary for those who set themselves up as systematists, to give their individual impressions upon these important and obscure subjects, the elucidation of which is one great object of their studies. Not only may a naturalist's views be supposed to represent the result of his accumulated experience, but his mode of treating his subject must in many cases be influenced by them, however much he may try to avoid it. For instance, it is natural to suppose that an observer who believes species to be arbitrary divisions of a genus, dependent on the naturalist's choice of characters, will adopt widely different conclusions as to their limits and origin, from one who regards them as distinct creation; and he who denies that a plant which grows spontaneously in England and New Zealand can have originated from one common parent, will reason differently on the subject of migration and dispersion from him who holds an opposite view. Now the actual amount of knowledge we possess on such subjects is so very limited, that few experienced

¹⁰² Mayr, "Illiger and the Biological Species Concept"; Farber, *Emergence of Ornithology*; Stevens, *Development of Biological Systematics*; Endersby, *Imperial Nature*.

¹⁰³ Illiger, "Einige Gedanken über die Begriffe"; Strickland, "Observations upon the Affinities and Analogies of Organized Beings"; Strickland, "On the True Method of Discovering the Natural System in Zoology and Botany."

¹⁰⁴ For example, Hooker, "Introductory Essay," *Flora Novae-Zelandiae*, vol. 2; *Flora Indica: being a Systematic Account of the Plants of British India, together with Observations on the Structure and Affinities of their Natural Orders and Genera* (London: W. Pamplin, 1855); *On the flora of Australia: its Origin, Affinities, and Distribution being an Introductory Essay to the Flora of Tasmania* (London: Lovell Reeve, 1859).

naturalists are inclined to pronounce positively upon them, whilst the majority offer no opinion at all.¹⁰⁵

Whether Temminck assumed his views were obvious once the reader looked at his classifications, or thought that he had explained them sufficiently, is hard to say. But his approach to general explanations (or rather, the lack thereof) was more the rule than the exception among naturalists. Carefully articulated premises for classification, such as the idea of continuity or the fixity of species, are rare. The theories of the naturalists from this period, with a few exceptions, have to be pieced together from their classification systems and the introductory sections of their works.¹⁰⁶ This is also true for the genus concept, the definition of *relation* between groups, or interpretations of geographical distribution of species. This failure to provide general explanations for natural diversity was intimately related with the general aversion towards any incursion into the realm of natural philosophy. Systematists were very weary of theories and hypothesis, and, as we will see in the next chapter, this would have consequences for the status of their discipline, especially after the 1840s.

Ultimately, Temminck's chosen means for communication, the languages and platforms he chose to publish in, the public he had in mind, his relation with the learned societies and his network of peers, all had their influence on how much and how often he communicated, his visibility, the width of his circle and his scientific authority.

¹⁰⁵ Hooker, "Introductory Essay," *Flora Novae-Zelandiae*, vol. 2, i-ii.

¹⁰⁶ See also for example, Mayr, "Illiger and the Biological Species Concept," 167; Stevens, *Development of Biological Systematics*, 133.

