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Suppressing Synonymy with a Homonym: The Emergence of the Nomenclatural Type Concept in Nineteenth Century Natural History

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Abstract. 'Type' in biology is a polysemous term. In a landmark article, Paul Farber (*Journal of the History of Biology* 9(1): 93–119, 1976) argued that this deceptively plain term had acquired three different meanings in early nineteenth century natural history alone. 'Type' was used in relation to three distinct *type concepts*, each of them associated with a different set of practices. Important as Farber's analysis has been for the historiography of natural history, his account conceals an important dimension of early nineteenth century 'type talk.' Farber's taxonomy of type concepts passes over the fact that certain uses of 'type' began to take on a new meaning in this period. At the closing of the eighteenth century, terms like 'type specimen,' 'type species,' and 'type genus' were universally recognized as referring to typical, model members of their encompassing taxa. But in the course of the nineteenth century, the same terms were co-opted for a different purpose. As part of an effort to drive out nomenclatural synonymy – the confusing state of a taxon being known to different people by different names – these terms started to signify the fixed and potentially atypical name-bearing elements of taxa. A new type concept was born: the *nomenclatural type*. In this article, I retrace this perplexing nineteenth century shift in meaning of 'type.' I uncover the nomenclatural disorder that the new nomenclatural type concept dissolved, and expose the conceptual confusion it left in its tracks. What emerges is an account of how synonymy was suppressed through the coinage of a homonym.

Keywords: history of taxonomic nomenclature, type concept, Method of Type, type method, type specimen, codes of nomenclature, Carolus Linnaeus, William Whewell, John Obadiah Westwood, Hugh Edwin Strickland, John Edward Gray, Paul Farber

Introduction

Nineteenth century natural history was rife with talk of 'types' in various guises. In a landmark article from 1976, historian of biology Paul Farber set out to disentangle the web of connotations of this deceptively plain term. He pointed out that 'type' in this period "was not a simple notion. Rather, it was a constellation of concepts that zoologists employed in different specialties, assigned to different levels of organization, and interpreted in different ways" (Farber, 1976, p. 93). Farber argued that the distinct usages of 'type' pointed towards three different 'type concepts': the *morphological type concept*, the *classification type concept*, and the *collection type concept*.

Farber's analysis imbued some much-needed historical data into the diatribe against 'typological thinking' that had been launched by Ernst Mayr and his fellow Modern Synthesis architects.[1] Farber rightly emphasized that speaking of 'typological thinking' as a monolithic and backward category "does violence to the historical record and confuses contemporary debates rather than clarifies them" (Farber, 1978, p. 91). He showed that pre-Darwinian type-talk was not hung up on Platonic idealism or Aristotelian essentialism, but rather involved a spectrum of flourishing empirical and theoretical endeavors.

Farber's 1976 paper has deservedly attained the status of a classic in the history and philosophy of biology. His account of types has formed the backbone of many studies of late eighteenth and nineteenth century taxonomy and morphology, and continues to be viewed as a conceptual

[1] E.g. Dobzhansky (1967), Mayr (1959, 1976), and Simpson (1961). It is generally thought that typology/population dichotomy was a purely rhetorical ploy, which Mayr conjured up from thin air in the early 1950s (e.g. Chung, 2003; Winsor, 2006). Elsewhere, I show that the actual origination history of the typology/population dichotomy is considerably more complicated than this (Witteveen, under review[a]). It originated with independent (and disciplined) arguments by Dobzhansky and Simpson against distinct notions of type, used in different methodological, conceptual, and theoretical contexts. Mayr later swept up these individually meaningful type/population contrasts and recombined them into a rather obscure argument against typological thinking.

springboard for improving our grip on nineteenth century natural history.[2]

It is nonetheless high time to revisit Farber's account of types, for some things that he writes are prima facie puzzling. Take, for example, the claim that his tripartite taxonomy of type concepts should not be taken "[to] correspond to nineteenth-nineteenth-century definitions of the word 'type,'" since the word was generally "used in a very loose manner" (Farber, 1976, p. 93). What, then, did Farber attempt to capture with his taxonomy of type concepts? Obviously, he was not anachronistically *imposing* his own categorization of type concepts on history. Instead, Farber intended to articulate distinct interpretations that were *implicit* in widespread nineteenth uses of the word, as part of distinct constellations of theory, practice and belief.

This is illustrated by an observation Farber makes about the confused practices of some naturalists, when viewed against the background of type concepts that were recognized by their contemporaries. He notes that in the context of handling so-called 'type specimens,' some naturalists were "confusing the collection type-concept with the morphological or classification type-concept" (Farber, 1976, p. 107).

However, this same observation also suggests that there is something amiss with Farber's own classification of type concepts. For, when Farber specifies how certain naturalists confused the collection type concept with other type concepts, he gives an account of the collection type concept that is at odds with his analysis from earlier in his paper. In consequence, it appears that Farber is of two minds about what the collection type concept *really* amounted to, and which practices it warranted.

I will show that this internal tension in Farber's account originates in an assumption that turns out to be problematic. Farber's account is built on the hidden assumption that early nineteenth century uses of 'type' had *stabilized* around an array of meanings that correspond to his three type concepts. What Farber hereby overlooks, is that certain uses of 'type' underwent a radical change in meaning in this period. At the closing of the eighteenth century, terms such as 'type specimen,' 'type species,' and 'type genus' had referred to typical members of their respective taxa. They were taxon elements that could serve as models for their respective taxa in the practice of classification. Yet by the end of the nineteenth century, these same terms had lost their connotation to anything 'typical'; they now

[2] E.g. Amundson (2005), Camardi (2001), Eigen (1997), Gassó Miracle (2008), Lyons (1999), Stevens (1984, 1994), Varma (2009), and Winsor (2003).

referred to the fixed (and potentially aberrant), name-bearing elements of those taxa. The *nomenclatural type concept* had emerged.

To recognize the nomenclatural type concept for what it is, is to realize that there is a story to be told about how it originated.[3] This intriguing history forms the heart of this paper. I will show that the emergence of the nomenclatural type concept was at once virtually inevitable and entirely incidental. It was virtually inevitable, because the only way taxonomy could continue to be pursued in the radically changing context of the nineteenth century was by tying names to fixed name-bearing elements. Only this method held the promise of calling to a halt the rapid increase in synonymy – the pervasively confusing state of one taxon being known to different people by different names. On the other hand, it was entirely incidental that these name-bearing elements would become known as 'types.' Moreover, this choice of terminology invited confusion: references to 'the type of a taxon' became ambiguous between a taxon's typical element and its name-bearing element. In an effort to drive out synonymy, 'type' was being turned into a confusing homonym.

Farber's Taxonomy of Type Concepts

Before addressing the problem with Farber's taxonomy of type concepts, I will give a brief overview of the type concepts he distinguishes. The most familiar type concept he discusses is the *morphological type*. It was defined as an abstract plan, schema, or blueprint of a (taxonomic) group of organisms that picks out its defining characters: "The morphological type … was a plan of organization that in principle consisted of essential elements" (Farber, 1976, p. 107). Farber attributes the first exposition of this type concept in modern history to the comparative anatomist Louis-Jean-Marie Daubenton (1716–1800) "whose morphological descriptions of quadrupeds marked the beginning of the modern

[3] As far as I am aware, Lorraine Daston is the only one who has gestured to important parts of this history (Daston, 2004; Daston and Galison, 2007, p. 109ff.), albeit without clearly articulating the shift in type concepts. Instead, she reads the history of the type specimen notion through the lens of a larger historical–epistemological framework (Daston and Galison, 2007). I do not have the space here to discuss Daston's argument in any detail, but argue elsewhere that her approach is problematic (Witteveen, under review[b]). The constraints of her historiographical framework cause her to overlook several key nineteenth century developments, and leads her to misconstrue the notion of a type specimen that was being hammered out.

science of comparative anatomy'' (Farber, 1976, p. 100).[4] As the long-time collaborator of Georges-Louis Leclerc de Buffon (1707–1788), Daubenton provided anatomical descriptions of morphological types for his *Histoire Naturelle* (Buffon, 1749), by listing sets of exclusive and essential morphological characters as plans on which individual species had been constructed.

Through Buffon's works, the morphological type concept started to spread, and soon became applied to other levels of the taxonomic hierarchy. Johann Wolfgang von Goethe (1749–1832) applied the morphological type concept to the entire kingdom of plants through his notion of the *Urpflanze* (Lenoir, 1978; Nisbet, 1967).[5] Another example of the deployment of the morphological type concept at a higher taxonomic level was Georges Cuvier's (1769–1832) division of life into four *embranchements*, based on four fundamentally different kinds of nervous systems.

Farber notes that, over time, different interpretations of the morphological type were developed. Cuvier followed Daubenton in working with a functional conception of morphological types, while others, such as Étienne Geoffroy Saint-Hilaire (1772–1844), argued that meaningful plans had to be based on structural correspondences. Hence, the famous debate between Cuvier and Geoffroy can be understood as a controversy over the *nature* of morphological types – that morphological types *existed* was not at issue (Appel, 1987). A similar debate over the nature of types and the empirical basis on which they should be discerned arose in the nineteenth century, when the importance of embryological evidence in the description of morphological types became a topic of heated debate (Lyons, 1999; Trienes, 1989). As Lenoir (1978) has pointed out, the metaphysical status of morphological types also allowed for flexibility of interpretation: whereas Kant and Goethe considered morphological plans to be 'regulative types' the later *Naturphilosophen* gave them a realist interpretation, turning them into the 'constitutive types' that dominated much of nineteenth century morphology and embryology.[6]

The second type concept Farber distinguishes is the *classification type*. A classification type is an exemplary member of a taxonomic group that can be used to determine the group's boundaries by comparing and contrasting it with other potential members of that group.

[4] Others have traced the morphological type concept all the way back to antiquity (Hammen, 1981; Toepfer, 2011).

[5] Though, as Riegner (2013) has recently emphasized, Goethe emphasized the dynamic, developmental aspects of morphological types.

[6] For more on the diverse historical interpretations of morphological types, see Levit and Meister (2006), Nyhart (1995), Richards (2002), and Rupke (2009), among many others.

Farber argues that this was the meaning of such notions as 'type species' and 'type genus' in the late eighteenth and early nineteenth century. Cuvier, for example, used classification types at the genus-level when he wrote: ''Not being able to assign to each family an equivalent and exclusive character, we will limit ourselves at this time to indicating families by names derived from the most well-known genus of each family; the genus which one can consider as the type and from which it is easiest to form an idea of the family'' (Cuvier and Valenciennes, 1828, vol. I, p. 571; cited in Farber, 1976, p. 103). Buffon similarly made use of classification types at the species-level when he used the European flycatcher as a model of comparison in assigning twenty-four other species to the same genus (Buffon, 1778, IV, pp. 517–518).

Farber notes that in the early nineteenth century ''the use of the classification type-concept increased dramatically and became explicit rather than implicit'' (Farber, 1976, pp. 94–95). That is, taxonomists increasingly started using phrases like 'type (of the) species' and 'type (of the) genus' to refer to model members of taxonomic groups. This terminology was popularized by William Whewell (1794–1866), who named the practice of assigning and using classification types the 'Method of Type':

> Natural Groups are best described, not by any definition which marks their boundaries, but by a Type which marks their centre. The Type of any natural group is an example which possesses in a marked degree all the leading characters of the class … The type-species of every genus, the type-genus of every family, is, then, one which possesses all the characters and properties of the genus in a marked and prominent manner.
>
> Whewell (1840, pp. xxxii, 477)

Classification types were often used as an inductive means of narrowing in on the description of a morphological type.[7] Peter Stevens has argued that this use of classification types was especially prominent in botany, as plants were often found to be insufficiently ''morphologically coherent'' to allow for direct application of the morphological type concept (Stevens, 1984, p. 169).

As with the morphological type concept, the classification type concept was interpreted differently by different taxonomists. Some argued that there were definite, typical, representative members of each class in nature. This attitude was central to the quinary theory of classification of William John

[7] It is not clear whether this was also Whewell's view. Compare Snyder (2006, p. 157ff.) and McOuat (2009, p. 221).

Swainson (1789–1855). Together with other quinary theorists such as William Sharp Macleay (1792–1865) and Nicholas Aylward Vigors (1785–1840), Swainson believed that animal taxa needed to be arranged in nested 'circles of five.' He argued that each circle had a representative typical group embodying the highest degree of organization, from which the other four groups of the same rank departed in various ways. Swainson's classification type concept thus was "a complex notion predicated on the principles of his system of classification" (Farber, 1976, p. 115).[8] Whewell, on the other hand, conceived of classification types as being independent of any particular taxonomic theory. He emphasized the role of classification types as heuristic devices, stressing that "we cannot say of any one genus that it *must* be the type of the family, or of any species that it *must* be the type of the genus" (Whewell, 1840, p. 477).

Finally – and this will become important in a moment – Farber notes that "the classification type-concept also doubled as a name carrier in nomenclature. The model species was used to determine the genus name, so that if at a later date the genus were split or rearranged, the group that contained the type species was given the original name" (Farber, 1976, p. 95).

The third and final type concept Farber distinguishes, the *collection type* concept, covers the concrete 'type specimens' stored in the collections of dried plants known as 'herbaria.' Farber notes that, functionally speaking, the collection type concept was "[s]imilar to the classification type-concept … the collection type concept served as a model and name carrier" (*ibid.*, p. 97). He nevertheless considers type specimens to form a separate class of types, because of their tangible nature. Unlike type species or type genera, type specimens could be picked up, preserved, stored, labeled, displayed and exchanged. The collection type concept was therefore wound up with taxidermic techniques, networks of exchange, and natural history collections (Farber, 1977, 1980).

Types in Tension

Farber's overall taxonomy of type concepts is summarized in Table 1. On first inspection, it appears elegant and parsimonious. It seems to capture some high-level distinctions in early nineteenth century uses of the word 'type,' which historians are well-advised to keep apart. However, when we give Farber's taxonomy a closer look, an internal tension appears that

[8] For a different, late nineteenth century use of classification types in a quinarian context, see Coggon (2002).

Table 1. Farber's taxonomy of type concepts, as outlined in the first half of Farber (1976)

type concept	**morphological type**	**classification type**	**collection type**
definition	An abstract representation of the essential features of a taxon	A taxon that serves as *name-bearer* and as *classificatory model* for its encompassing taxon.	A specimen that serves as *name-bearer* and as *classificatory model* for its encompassing taxon.
example / instantiation	the vertebrate archetype	type species; type genus	type specimen

raises questions about the overall coherence of his account. This tension surfaces when, based on the distinctions he has drawn, Farber criticizes how some nineteenth century naturalists deployed the collection type concept.

> Potentially, the main problem of interpretation with the collection type-concept had to do with considering the type-specimen truely [*sic*] typical of the species. There was little to recommend such a practice. The first description of a species was often made from an incomplete knowledge of the full range of variation within the species. If one argued that a type-specimen should be replaced by a more typical specimen when additional knowledge of the species was obtained, the entire value of the concept of the collection type-concept [*sic*] as a name carrier and reference for the original description would be lost …. [N]aturalists who wished to treat type-specimens as typical specimens, were confusing the collection type-concept with the morphological or classification type-concept.
>
> Farber (1976, pp. 105–106)

There is something odd about the criticism Farber voices here. In the previous section we saw that Farber characterized type specimens (i.e. collection types) as well as type species and type genera (i.e. classification types) as having served essentially the same roles at different hierarchical levels (see Table 1). He argued that all these 'type elements' served both as classificatory models and as name-carriers for their encompassing taxa. Yet, in the quotation above Farber backpedals on defining type specimens as classificatory models, by stating that their "entire value" lay in the role of being *fixed* bearers of taxon names. Treating a collection type as a model that could be replaced with a more typical exemplar is now portrayed as having been illegitimate and illogical.

Farber thus leaves us with two conflicting accounts of what the distinction between collection and classification types consisted in. At first he portrays the collection type concept as a functionally similar but pragmatically separable kind of classification type concept – collection types as *tangible* classification types, – but later he suggests that collection types and classification types were functionally at odds. Which of these accounts is the correct one?

Neither is. Instead, the tension in Farber's account signals a problem with his overall approach towards uncovering nineteenth century type concepts. Farber assumes that the terms 'type specimen,' 'type species,' and 'type genus' had stable (if implicit) meanings *across* the period ranging from the late-eighteenth to the mid-nineteenth century. In reality, however, these terms started to take on a new meaning in the early nineteenth century. At the end of the eighteenth century, everyone understood 'type of a taxon' to refer to a typical element that could serve as a model member for exploring the limits of its encompassing taxon. By the end of the nineteenth century, 'type of a taxon' had come to refer to the fixed (and potentially atypical) name-bearer of a taxon name.

To account for the nineteenth century change in meaning of a 'type element,' we need to modify Farber's taxonomy by setting it in motion. We need to recognize that extant type *terms* became associated with an entirely novel type *concept*. A term like 'type species' evolved from signifying a classification type towards betokening a *nomenclatural type*. The same goes for terms like 'type specimen' and 'type genus.' Table 2 shows how Farber's taxonomy needs to be modified in this light. A new category of nomenclatural types is recognized at the same level as that of classification types. Each of these two categories covers the meaning of 'type specimen,' 'type species,' and 'type genus' for a different period.

Using this revised taxonomy, we can start to make sense of Farber's allegation that certain nineteenth taxonomists were confusing type concepts. The point Farber was getting at, is that when taxonomists started using the term 'type specimen' to refer to a fixed name-bearing specimen (i.e. a nomenclatural type specimen), this implied that such a

Table 2. A revised taxonomy of type concepts, which overcomes the tension in Farber's account

Type concept	Morphological type	Classification type	Nomenclatural type
Definition	An abstract representation of the essential features of a taxon	A taxon or taxon-element that serves as *classificatory model* for of its encompassing taxon. A 'typical' representative.	A taxon or taxon-element that serves as name-bearer for the name of its encompassing taxon.
Example/ instantiation	The vertebrate archetype	Type specimen; type species; type genus	Type specimen; type species; type genus

specimen should not be replaced with a more typical exemplar at a later time. Doing so amounted to treating nomenclatural types as if they were classification types. This confusion between type concepts also manifested itself in the use of a nomenclatural type specimen as a model for the classification of other specimens. Sometimes this would have worked – a nomenclatural type specimen *could* be genuinely exemplary – but there was no principled reason to always expect this to be the case. Since nomenclatural type specimens were often designated upon the discovery of a small sample of material from a (putatively) new species, it was quite likely that they would be rather aberrant members of their species in the light of later discoveries.

Apart from allowing us to improve Farber's account, the revised taxonomy of type concepts points to an important new explanatory project. It raises the question how and why terms like 'type specimen' and 'type species' shifted meaning in the course of the nineteenth century. How could these terms become detached from connotations of typicality, and gain the new meaning of 'fixed name-bearer'? What developments drove this change? To answer these questions we need to delve into late-eighteenth century theory and practice of naming and classifying, and follow their development as we reach the maelstrom of early nineteenth century taxonomy.

From Method of Type to Type Method

Linnaeus and the Method of Type

Linnaeus (1707–1778) never wrote about types. Still, he clearly falls in the category of those who used the classification type concept 'implicitly,' as Farber rightly notes.[9] Starting from a set of carefully selected 'chief species' (*prima species*) Linnaeus made one-by-one comparisons with other known species in order to gradually determine the boundaries between genera. If the characters of a newfound species matched those of the chief species sufficiently well, the new species was added to the genus, and all characters by which the two species differed were cancelled from the description of the genus.[10] By iterating this proce-

[9] "One can find similar implicit classification type-concepts in the writings of other major eighteenth-century writers, such as Carl von Linné" (Farber, 1976, p. 94).

[10] Many commentators have noted that, in practice, Linnaeus frequently failed to remove the dissenting characters from his description, because of a lack of time (Müller-Wille, 2007; Winsor, 2003). This has enabled later authors to retrace which species he used a *prima specie* (Pennell, 1930).

dure, Linnaeus could provide increasingly accurate descriptions of the 'natural character' of a genus (Linnaeus, 1751, §193; Müller-Wille, 2006, 2007; Stearn, 1957, p. 37).

Linnaeus referred to this practice as the 'method of collation' (*collatio specierum*). In the *Flora Lapponica*, for example, he remarked that it could not be said with certainty that the genus *Andromeda* was distinct from the genus *Erica* until he would have ''collated flowers of more species from both genera'' (Linnaeus, 1737b, p. 126). Many later commentators have recognized this practice as an application of Whewell's *Method of Type*.[11] The botanist Henry Knute Svenson (1897–1986) stated that from a Linnaean viewpoint ''we may think of genera as broadening concentric circles such as the rings formed by pebbles thrown into water: the initial impact representing the type species of the genus, and the resulting concentric rings the accretion of species through historical usage'' (Svenson, 1945, p. 291). The American botanist Francis Whittier Pennell (1886–1952) remarked in a similar vein that in the *Genera Plantarum* ''each [generic description] was prepared from a species carefully selected as typical'' (Pennell, 1933, p. 38).[12]

Linnaeus emphasized that only the most seasoned taxonomists could be trusted in their selection of classification types (Linnaeus, 1751, p. 193). Only the expert possessed the requisite tacit knowledge for selecting a truly typical species for each genus. Another Linnaean recommendation for the selection of classification type species was that they be species with putative medicinal properties.[13] This is illustrated by the fact that a large number of the species Linnaeus used as classification types for the fifth edition of *Genera Plantarum* (Linnaeus, 1754) are species that he had included in his recently published *Species Plantarum* (Linnaeus, 1753) with the epithet '*officinales*' – a term denoting exemplary medicinal value with regard to some property

[11] Whewell mentioned Linnaeus, Adanson and de Candolle (the elder) as examples of taxonomists who had ''practically applied'' and ''theoretically enunciated'' what he regarded as the ''sound maxims of classificatory science,'' which obviously included the Method of Type (Whewell, 1840, p. 463).

[12] Also see Pennell (1930, 1939). Many other taxonomists and historians have similarly noticed Linnaeus' deployment of the Method of Type (e.g. Hitchcock and Green, 1929; Stearn, 1959, 1960; Winsor, 2003).

[13] Officinal species served an important role in the larger economic context in which Linnaeus's classificatory endeavors were situated. Ultimately, his generic classifications served the practical goal of presenting potential substitutes for imported products, so as to reduce dependence on trade, which he perceived as essentially parasitic on the domestic economy (Koerner, 1999).

common to the genus (Pennell, 1930; Hövel, 1999).[14] Hence, later taxonomists rightly observed that "the type of each genus of Linnaeus as stated by him is 'the best known European or officinal species' it contains," (Jordan, 1901, p. 501) and that "among Linnaean genera … such names as *communis*, *vulgaris*, *typicus*, and *officinalis* would seem to point out typical species" (Hitchcock, 1925, p. 131).

Meanwhile, Linnaeus was aware that even classifications made by experts were fallible. He realized that future taxonomists, with more knowledge about more species at their disposal, would likely judge that some genera were really composites, and needed to be split. Moreover, Linnaeus perspicaciously observed that this inherent fickleness of classifications could have serious consequences for the stability of taxonomic nomenclature. A name that was once established could all too easily get lost in a sequence of splitting events. Or worse, different taxonomists might start using the same original name to refer to different segments of a divided taxon. Nomenclatural chaos would ensue. In the *Fundamenta Botanica*, his first collection of ideas about reform in botanical taxonomy, Linnaeus offered a solution to these problems by means of proposing a procedural rule. Aphorism 246 of that work reads:

> If an established genus has to be split up into several, according to the Law of Nature and Art, then the name that formerly belonged to the *whole* should be kept to denote the plant that is most *vulgar* and *officinal*.[15]
>
> Linnaeus (1736, §246)

With this aphorism, Linnaeus suggested that the assignment of taxonomic names be made dependent on (expert) taxonomic judgment. Any taxonomist who would be able to ascertain the typical—i.e. vulgar and officinal—species of a genus would also be able to apply its name correctly when splitting the genus. In other words, aphorism 246 made the assignment of genus names dependent on deployment of the Method of Type. Classification types would *double* as name-bearers.

Linnaeus reiterated this aphorism in the *Critica Botanica*, warning that "inextricable confusion would arise" if taxonomists would be allowed to "choose indifferently" which part of a genus to apply the

[14] An officina was a building, often adjacent to a herbal garden, were medicaments were prepared from plant extracts (Pearn, 2010).

[15] "Si genus receptum, secundum jus naturae & artis, in plura dirimi debet, tum nomen antea *commune* manebit *vulgatissimae* & *officinali* plantae."

original name to (Linnaeus 1737a, p. 99). Linnaeus's aphorism reached later generations of taxonomists through his widely read *Philosophia Botanica* (Linnaeus, 1751). His student Johan Christian Fabricius (1745–1808), for example, included aphorism 246 almost verbatim in his influential *Philosophia Entomologica*[16] (Fabricius, 1778, §30). In the early nineteenth century, John Lindley's (1799–1865) widely read *An Introduction to Botany* made mention of the "Linnaean canon" that was recognized for providing stability to taxonomic nomenclature: "If an old genus is divided into several new ones, the old name will remain with the species that is best known" (Lindley, 1832, p. 456). One would hardly suspect that soon enough, taxonomist would reject this principle because of its *contribution* to nomenclatural chaos.

Names, Meaning, and Typicality

To understand why and how aphorism 246 and its later incarnations came under threat, we will need to make an excursion into some general developments in early nineteenth century taxonomy. As the Napoleonic Wars drew to close, overseas surveys and expeditions started to bring home masses of new specimens from previously unexplored areas, driving a rapid expansion of botanical and zoological collections in Britain and on the continent. Naturalists who sifted through the materials that were brought home soon discovered numerous new species, genera, and even entire families.[17] Joining the sudden increase in specimens needing to be named and classified was an increase in taxonomists who wished to be involved in the naming and classifying. A new branch of self-fashioned British provincial radicals started practicing taxonomy as a pastime, and colonial collectors began to name species on their own (McOuat, 1996; Secord, 1994; Endersby, 2008).

The metropolitan establishment harbored more than a few reservations about the involvement of these new classes of amateur taxonomists in these endeavors. Imperial botanists objected that colonial collectors tended to raise mere varieties to the status of species, and that they did so mainly to derive prestige from introducing new names. To the imperial experts these new names useless, redundant, and a cause of

[16] "Si genus receptum, secundum leges naturae et artis, in plura dirimatur, tum nomen antea commune *vulgatissimo* insecto manebit."

[17] Whewell mentioned that "Linnaeus knew approximately 10,000 species of plants; a few decades later this number had already swelled to 60,000" (Whewell, 1840, p. 489). By the mid-1830s this number had risen sharply, as it was reported that "new species are joined to those known to Linnaeus, in the proportion of at least 100 to 1" (Westwood, 1836, p. 562).

confusion. Joseph Dalton Hooker (1817–1911), soon to become assistant director of the Royal Botanical Gardens at Kew, lamented the ''chaos of synonymy which has been accumulated by the thoughtless aspirants to the questionable honour of being the first to name a species'' (Hooker, 1853, xiv).[18]

From the side of provincial radicals, the contributions to the 'chaos of synonymy' were compounded by another factor. Radical reformists argued that many taxon names that had once been 'expressive' of typical and distinguishing features of taxa seemed to be rather 'off' in the light of recent discoveries. Hence, a good number of Linnaean and colloquial names needed to be replaced with more meaningful names.

Even more than the careless splitting of taxa, these arguments about meaning were anathema to the conservative metropolitan establishment.[19] Apparently, the radicals had deeply misunderstood what purpose names served. As Linnaeus had already made clear, a good name was simply a memorable one. The ulterior value of names was entirely constituted in them being good mnemonic devices. Names should therefore not be too long, too similar to others, or be mixtures of Latin and Greek words.[20] Ideally, a name would capture the distinguishing features of a taxon, but neither Linnaeus nor his followers considered it essential for a name to wear its 'meaning' on its sleeve. ''Names have the same value on the marketplace of botany as coins have in public affairs, which are daily accepted as certain values by others, without metallurgical examination'' (Linnaeus, 1737a, §284). Not the most *expressive*, but the *first* (well-formed) name given to a taxon should be counted as the rightful name for that taxon: ''priority in time confers precedence'' (Linnaeus, 1737a, §243).

The controversy between radicals and conservatives flamed up properly over an article written by the anonymous S. D. W., who argued that the recent discovery of a white specimen of the species 'coalhood' was good reason to replace its name: ''With regard to the Scientific Name of the Coalhood, I have ventured to suggest *Denisirostra atricapilla*, as being more definite and expressive than the name of Lin-

[18] Hooker's collaborator George Bentham (1800–1884) similarly proclaimed that ''[i]t is only second rate botanists who pride themselves on the number of names, good or bad, to which their initials can be attached'' (Bentham, 1878, p. 190).

[19] For a broader and more detailed treatment of the controversies that ensued, see McOuat (1996).

[20] Generally speaking, only the improper *form* of an extant name (e.g. not being in Latin, or being barbarian) would be a reason for Linnaeus to introduce a new name: ''If it is decided that none of the synonyms is really suitable for the plant, then necessity compels us to make up a new one'' (Linnaeus, 1737a, p. 258; cited in Dayrat, 2010, p. 189).

naeus, *Loxia Pyrrhula*'' (S.D.W., 1834, p. 593). S. D. W.'s article raised the hackles of Hugh Edwin Strickland (1811–1853), a conservative, Oxford-educated naturalist. Strickland quickly drafted a reply in which he strongly objected to the idea of substituting more 'expressive' names for accepted ones, ''a practice which appears to me highly detrimental to the progress of natural history'' (Strickland, 1835, p. 36). He continued by teaching S. D. W. and his allies some Linnaean philosophy of names: ''[I]n order that the object of the specific name may be duly performed, it is essential that a name be universally adopted, and, therefore, never, or very rarely, altered. But it is not, I think, essential that the meaning of the name should precisely designate the species; or, indeed, that it should have any meaning at all'' (Strickland, 1835, p. 38). With Linnaeus, Strickland also held that once a name was assigned to a species, it should be retained as its unique and memorable marker. Only a little reflection was needed to realize that S. D. W.'s alternative of 'updating' species names in the light of new knowledge would be unworkable:

> Can S. D. W., for instance, expect that the whole republic of science will take the trouble of relabelling their cabinets, altering their catalogues, or making notes in their works of reference, because an anonymous writer fancies that he can improve *Pyrrhula vulgaris* by changing it to *Densirostra atricapilla*? Again, if some adopt the alteration, a large number will not: and hence it is that we rarely find the same species labelled alike in two different museums. In short, if this practice be once given way to, there will soon be an end of all nomenclature, and, through it, of all science.
>
> Strickland (1835, pp. 38–39)

Strickland's message was clear: ''[T]he evil of changing a name … is much greater than any advantage supposed to result from substituting a term which is 'more appropriate.'" Reformists like S. D. W. who pressed for 'better' names failed to appreciate that taxon names, like proper names, are ''arbitrary signs adopted to represent real things or conceptions'' (Strickland, 1835, p. 37; italics in original). There was, in other words, a ''complete parallel'' between species names and names of men: ''The first discoverer of a species may be regarded as its parent or godfather; who bestows on it any name he thinks fit, and publishes it to the scientific world in some standard work, as in a parish register'' (Strickland, 1835, p. 39).

In concluding his response to S. D. W., Strickland briefly noted that his argument was not restricted to species names, but equally applied to ''the proper names of genera, or of larger groups, where such groups are

retained unaltered.'' And if a group *was* altered, Linnaeus's aphorism 246 needed to be followed: ''Where an old genus is divided into several new ones, new appellations must, of course, be found for them; but, even then, the original name should be retained for that group which is the most typical of the whole'' (Strickland, 1835, p. 39). It sounded almost like Strickland reading out the *Philosophia Botanica* to a new generation of taxonomists.

In spite of Strickland's efforts, the radical reformists were not swayed easily. When Strickland returned to the topic of nomenclature two years after his initial bout of criticism, he noted to his dismay that ''the lovers of confusion have been hard at work,'' and that ''specific names are as variable as the London fashions'' (Strickland, 1837b, pp. 127, 128). Once again, Strickland reminded the reformists that ''[t]he meaning of a name is … a point of less importance than its universality; and, when the latter object has been once gained, would never sacrifice it to the former'' (Strickland, 1837b, p. 129). Yet by now he realized that repeating this lesson over and over would not suffice. The 'curse of Babel' posed by the proliferation of synonyms required a more pro-active effort on behalf of ''the true friends of science to counteract this evil tendency'' (Strickland, 1837b, pp. 127–128). It had become high time to compile a set of clear and authoritative nomenclatural rules, to be accepted and followed by the entire taxonomic community. The *Philosophia Botanica* was in need of an update, and Strickland took the lead in getting the job done.

Just one month later, Strickland published a list of 22 provisional rules of zoological nomenclature, which he compiled from the writings of contemporary naturalists. Many of the rules he listed had ancestors in Linnaean aphorisms.[21] Rule 4, for example, articulated the 'priority principle' of aphorism 243: ''The first name given to a group or species should be perpetually retained.'' Rule 9 read: ''It is *desirable*, but not *essential*, that a name should have an etymological meaning.'' Yet the list was clearly provisional and incomplete. It did not, for instance, include the equivalent of Linnaeus's aphorism 246 that Strickland had mentioned in his response to S. D. W. two years earlier. However, Strickland did include another rule that tied the assignment of names to 'typical' elements: ''Rule 18: The names of families and subfamilies should be derived from the most typical genus in them'' (Strickland, 1837c, p. 175).

[21] Strickland later noted in correspondence that in his efforts to formulate nomenclatural rules he proceeded by taking Linnaeus' *Philosophia Botanica* and adapting it to the demands of nineteenth century taxonomy (Strickland to Bonaparte, 30 May 1844, SA F-170).

Strickland's adoption of this rule is interesting, since it amounted to a partial departure from the purely nominalist spirit he had embraced earlier. After all, Rule 18 effectively stated that a family name should in some sense *reflect* the family's content. Strickland indeed adapted this rule from the writings of the reform-minded William Swainson (1789–1855), who had argued for the improvement of certain names. ''Before we impose a name upon a group which has never been characterised, we should carefully analyse it; without which we shall run no small risk of not discovering the typical character of the whole and consequently apply a false name'' (Swainson, 1836, p. 235). As an example, Swainson mentioned that ''[the genus] *Muscicapa* being more typical than *Todus*, the family to which both belong should be called the *Muscicapidae*.''[22] In Swainson's view it would be ''comparatively trivial'' to carry through changes like these for existing families. ''While the whole science is undergoing a revision and correction, it may be as well to make these and every other necessary change of nomenclature at the same time'' (Swainson, 1836, p. 235).

Strickland's inclusion of Swainson's rule suggests that he was not trying to push his nominalist philosophy at all costs and at all taxonomic ranks. Since, ultimately, Strickland was seeking a way to stabilize names, he may have been swayed by Swainson's claim that this rule was ''universally acted upon in Britain'' (Swainson, 1836, p. 235). In any case, Strickland soon started criticizing naturalists who failed to follow the ''very convenient rule now generally adopted by naturalists, that the name of a family should be compounded of the name of the most typical or best known genus in it'' (Strickland, 1837a, p. 605).

Yet, 'convenient' as this rule may have appeared, many taxonomists would soon protest that it was far from clear how to apply it. What were they supposed to understand by a 'typical' genus? typical of what, and to whom? Assigning names on the basis of typicality judgments was far more problematic than Strickland realized, and threatened to undermine his aim of stabilizing names.

[22] Swainson carried this change through in a later monograph, but not without remarking that the name 'Todus' had meanwhile been determined to have been misapplied, in such a way the correct name of the family might actually be 'Todidae' after all! He nevertheless refrained from adopting the chain of nomenclatural changes that followed from all this, since ''these alterations would lead to so much confusion, that we have not ventured upon, much less do we recommend, their adoption'' (Swainson, 1838).

Whose Type?

One problem with Strickland's talk of 'types' and 'typicality' was raised in an exchange with the Irish naturalist William Ogilby (1808–1873). When Strickland criticized Ogilby for not following Rule 18, the latter responded that he was "at loss to imagine" what this rule meant: "We hear continually of the *type* of such or such a genus, and of *typical* species, *typical* groups, and *typical* genera. Now if the word type be merely synonymous with *example*, I see no objection to it, but on the contrary have employed it in this signification myself: but it is notoriously employed by others in a very different sense, and one to which I confess I can attach no definite meaning" (Ogilby, 1838a, pp. 281–2 82).[23]

The 'very different sense' Ogilby referred to, was that of quinarians like Swainson. As noted earlier (Section "Farber's Taxonomy of Type Concepts"), the quinary theorists' understanding of types or typical elements was rooted in a particular theory of classification, on which types were the most perfected members in a 'grand system' of nested circles of taxa. Like all other conservatives, Ogilby was strongly opposed to this speculative classificatory scheme. When he learned that Strickland took his Rule 18 straight from Swainson, it must have appeared to him that Strickland was importing quinary elements into his nomenclatural rules.

Quick to recognize Ogilby's worries, Strickland responded by agreeing that "the quinary theorists attach to the word *type*, a deeper and more mysterious meaning, but this is not the only one of their doctrines to which I do not subscribe."[24] In reality, Strickland's understanding of a 'type' was "precisely the same as Mr. Ogilby's … By 'the most typical genus' of a family, I mean that genus which seems to afford the best sample of the characters on which the family is based, with the least tendency to diverge into other families." The same counted for the genus level, where the taxonomist selects "that species which affords the fairest sample of the whole, [which] he calls a *type*" (Strickland, 1838a, pp. 330–331).

[23] Ogilby also protested against the idea of formulating nomenclatural rules in general. He argued that they "make no part of zoology" and that there was no need to strictly apply "these scientific thumb-screws, these verbal crucibles" (Ogilby, 1838b, p. 150). Strickland, of course, replied that rules of some form of regulation would be necessary to get all naturalists to speak the same language (Strickland, 1838b).

[24] At the British Association for the Advancement of Science meeting in Glasgow in 1840, Hugh Strickland also mentioned that "[a]ll systems, circular, quinary, dichotomous, etc. are not natural, but artificial and only of use in arranging museums" (see McOuat, 1996, p. 503, n.140).

Strickland's response to Ogilby evinces that he did not intend to make taxonomic nomenclature dependent on a particular taxonomic *theory*, let alone on quinary theory. On the other hand, his response does affirm that he considered the proper assignment of names to be dependent on informed taxonomic *judgment*. To determine the correct name of a genus or family, a taxonomist would have to be able to identify its typical species or genus, respectively.

Yet this led to a further problem, raised by Charles Thorold Wood (1777–1852) – "the most vociferous of the nomenclature radicals" (McOuat, 1996, p. 498). Wood pointed out that "it frequently happens, that naturalists cannot agree on the type of the family: thus, Selby pronounces the genus *Sílvia* to be the type of its family; Swainson says, on the other hand, *Régulus*; and Blyth, rejecting both, adopts *Ficédula* as the typical genus … and so on throughout zoology" (Wood, 1836, p. 340).[25] Moreover, Wood noted that aphorism 246 and its cognates were vulnerable to the same problem:

> Mr. Strickland observes, "Where an old genus is divided into several new ones, new appellations must, of course, be found for them; but, even then, the original name should be retained for that group which is most typical for the whole." This latter proposition sounds well in theory, but will be found, on many occasions, impracticable.
>
> Wood (1836, p. 340)

Interestingly, the same line of criticism was voiced by a prominent naturalist on the very opposite end of the radical-conservative spectrum. John Obadiah Westwood (1805–1893) – "arguably the most vociferous critic of nomenclature radicalism" (McOuat, 1996, p. 518) – illustrated the problems with Swainson's rule by highlighting that Swainson himself sometimes "considered the wide geographical range of a form as indicating typicality," but at other times "opposes this principle, by considering the preeminently typical form to … exist in a group of confined geographic range" (Westwood, 1836, p. 563).

A decade earlier, Westwood had already diagnosed the root problem with nomenclatural rules that depended on judgments about typicality. The issue was not only that different measures of typicality could be used, or even that judgments on a given measure depended critically on subjective factors. The deeper problem was that even perfectly aligned

[25] In *The Ornithological Guide*, Wood gave more examples of families for which "there is difference of opinion with regard to which is the typical genus" (Wood, 1835, p. 75).

typicality judgments made *at any time* would be liable to change *over time*, because of our evolving taxonomic knowledge. "Indeed, until the contents of any particular family are clearly ascertained, the supposed typical group will be continually subject to variation as new forms are discovered, and hence, … the family name will necessarily be subject to similar variation" (Westwood, 1828, p. 4).[26]

Westwood realized that the same lesson applied to any nomenclatural rule that hinged on taxonomic judgment. Hence, like Wood, Westwood also leveled his criticism at rules that descended from aphorism 246:

> Fabricius, in his *Philosophia Entomologica* (p. 114) lays down the following rule, "Si genus receptum secundum leges naturae et artis, in plura dirimatur, tum nomen antea commune *vulgatissimo* insecta manebit." I do not know any method so likely to create confusion and uncertainty as that contained in the above rule, since it is next to impossible that every Entomologist would select the same particular insect, and consider it as the most common in the family to which it belongs.
>
> Westwood (1828, p. 5)

Wood and Westwood's criticisms put pressure on the Linnaean strategy of using classification types to adjudicate on the application of names. Both men realized that co-opting the Method of Type for nomenclatural purposes would increase synonymy rather than reduce it.

Yet, as much as they agreed in their diagnosis of the problem, they thoroughly disagreed about how to solve it. Wood took the radical position that nomenclatural rules should be abandoned wholesale. The establishment should stop trying to regulate nomenclature. Instead, usage by taxonomists over time should be the standard for determining the correctness of a taxon name. In his own work, Wood therefore often gave "more than one name to the same family," leaving his readership to determine which of the names was "the best" (Wood, 1835, p. 74).

Westwood could not disagree more. He strongly favored the institution of authoritative nomenclatural rules, and concluded that a *different* set of rules would be needed to drive out synonymy. As Westwood saw it, the problem with nomenclatural rules based on typicality judgments was that they went against the grain of a principle that

[26] As Hooker put it some years later: "The type of a group often turns out (on extended knowledge of that group) to be the most aberrant form in it" (Hooker to Darwin, 5 April 1844; DAR Letter 745). [I use 'DAR' to refer to Darwin's correspondence, collected in Burkhardt et al. (1985–).]

should be at the heart of any set of rules: the principle of priority.

The principle of priority stabilizes naming by making it a matter of *ostension*, isolating it from matters of *taxonomic judgment*. If the valid name of a taxon is simply that which it has been baptized with by its first describer, no future change in taxonomic judgment or opinion can unsettle it. Westwood observed that Linnaeus's aphorism 246 and Strickland's Rule 18 were at odds with this separation between naming and casting taxonomic judgments. These rules opened a backdoor through which fickle and subjective taxonomic judgments could reenter the stage of objective nomenclature. This backdoor would need to be shut by formulating a new principle about how to apply names to split genera.

The solution Westwood came up with was simple but ingenious. He proposed that, apart from fixing the first name given to taxon as its valid name, that name would have be *permanently anchored* to the part of the taxon that was regarded as typical *by the name-giver*. In other words, Westwood proposed to extend the principle of priority from names to name-bearers.

If Westwood's proposal were to be followed, names would be prevented from going adrift in the inevitable merger and splitting of genera. Changing judgments about typicality would no longer impact the assignment of genus names, as names would 'track' the ostensively specified name-bearing species. The very species which the first describer of a genus at first used as his classification type would count as the *fixed point* for application of the genus name.

How to call this notion of a fixed, name-bearing taxon element? Apparently without giving the question much thought, Westwood started referring to it as the taxon's "original type," "real type," "true type," or simply as its "type." We thus read that a genus name should never be "wrested from its true type," at the risk of upsetting nomenclatural stability (Westwood, 1837b, p. 172). And in concluding his article, Westwood summarized his use of 'type' by stating:

> [I]t must be evident that the author, in constructing [a] genus, must have especially have had in view the typical species, which, indeed, often furnishes the generic name; that, by removing such type, the creation is dissolved, the tie which held the remaining discordant materials together is broken; and that, after the removal of that type, it would be as improper as practically injurious, to speak of the remainder as constituting such or such a genus of the author by whom the name was proposed.
>
> Westwood (1837b, p. 173)

On the one hand, this choice of terms seemed straightforward. After all, Westwood's proposal was to fix the *typical* element from the originally named sample as the taxon's name-bearer. But on the other hand, Westwood's move was bound to invite confusion. On Westwood's usage, a genus' 'type' could be a species that had been judged typical by its namer, but that had become an atypical – or even wholly aberrant – member of its genus after taxonomic revision. Moreover, if an author had not expressly stated what he considered to be the typical species at the time of introducing the name, Westwood suggested that the name be tied to the first species listed under the genus name.[27] Hence, the types Westwood was speaking of did not need to coincide with any classification type ever used by anyone. To apprehend that this was not a *reductio ad absurdum* of the term 'type,' readers would need to realize that Westwood was not speaking of types as subjectively selectable model members, but rather as name-bearers fixed by ostension. Westwood was speaking of 'type' under a different concept: the *nomenclatural type concept*.

The Strickland Code

Following his publication of 22 provisional rules, Strickland sought the support of the British Association for the Advancement of Science (BAAS) as an authoritative platform for supporting the formulation of rules of nomenclature.[28] At the 1841 BAAS meeting in Plymouth, Strickland tried to rally support for setting up a committee charged with this task, but he failed to get a motion passed. Undeterred, he set out to draft a 15-page pamphlet himself, entitled *Proposed Plan for Rendering the Nomenclature of Zoology Uniform and Permanent* (Strickland, 1841b), which he circulated widely among naturalists at home and abroad. In the preamble, Strickland expressed his desire "to mature the plan as much as possible, by obtaining the opinions of eminent zoologists in various countries; and the proposers of the measure will therefore feel grateful for any remarks or criticisms … " (Strickland, 1841b, p.1).

Meanwhile, a new request to the BAAS Council was approved, allowing Strickland to set up a committee to prepare a proposal for the

[27] "[W]here an author does not state the particular species which he regards as the type of his genus, we are bound to suppose that he would place it at the head of his genus" (Westwood, 1837b, p. 170).

[28] Rookmaaker (2011) provides a detailed discussion of Strickland's efforts from 1841 to 1843. A detailed calendar and index of his scientific correspondence can be found in Rookmaaker (2010).

next BAAS meeting, in 1842.[29] Strickland quickly formed a committee with 16 eminent scientists, including Owen and Darwin, and also got Ogilby and Westwood on board.[30] Based on their input, Strickland composed and distributed an updated set of rules in the spring of 1842, entitled *Proposed Report of the Committee on Zoological Nomenclature. For the use of the members of the Committee* (Strickland, 1842). These rules were presented by Strickland at the June 1842 BAAS meeting in Manchester. In early 1843 they were published in the Society's Proceedings[31] under the title *Series of Propositions for Rendering the Nomenclature of Zoology Uniform and Permanent* (Strickland, 1843). Before long, these rules would become known as the 'Strickland Rules' or the 'Strickland Code.'[32]

On a cursory reading of the Strickland Code, it seems that the committee had held fast to Strickland's earlier ideas about naming and typicality, in spite of counting Westwood among its members. The third rule, for example reads as follows:

> §3. A generic name, when once established should never be cancelled in any subsequent subdivision of the group, but retained in a

[29] Strickland Correspondence, University Museum of Zoology, Cambridge, N-095.

[30] The other members were Bell, Broderip, Jenyns, Phillips, Richardson, Shuckard, Smith, Waterhouse, Westwood, and Yarrell. Rookmaaker (2011) presents a useful table, listing which of the several meetings were attended by whom. McOuat (1996) provides an appendix with short biographies of all members, except for Whewell, who only joined for one of the meetings, and who did not want to be considered an official member. Whewell reported to Strickland that all he had to say on the subject he had already said, in the first volume of his *Philosophy of the Inductive Sciences* (Whewell to Strickland, 1841; Strickland Correspondence, N-096).

[31] Several political and bureaucratic hurdles needed to be taken before publication in the authoritative main section of the BAAS *Report* could happen, since not everyone agreed that the BAAS should back these rules. The strongest opponent was John Edward Gray, whom we'll meet later on. He argued that, "the rules be not adopted until they have been compared with Linnaeus's 'Philosophia Botanica,' Fabricius's 'Philosophia Entomologica,' Tiliger's 'Prodromus,' and De Candolle's 'Theorie Elementaire,' and that when they are not in conformity with the laws proposed by these authors, which have been accepted by all recognized systematic naturalists, the reasons for the proposed alterations should be given in detail." (Gray, 1864b, p. 85). Another Gray, the American botanist Asa Gray, agreed: "We recommend that … the *Philosophia Botanica* of Linnaeus … be reprinted, with indications of the rules which in the lapse of time have become inoperative, or were from the first over nice" (Gray, 1864a, p. 278).

[32] For a discussion of the Strickland Code in the Victorian societal context, see Ritvo (1997). McOuat (1996) offers an excellent in-depth discussion of the Code's broader political and philosophical motives and implications (see Section "Under the Radar of Nomenclatural Codes").

> restricted sense for one of the constituent portions.
>
> *[Generic names to be retained for the typical portion of the old genus.]*
>
> Strickland (1843, p. 110)

The bracketed part suggests that Strickland's committee had held fast to Linnaeus's aphorism 246: a genus name should track the genus' typical features. However, the next rule introduced an important proviso:

> When a genus is subdivided into other genera, the original name should be retained for that portion of it which exhibits in the greatest degree its essential characters as at *first* defined. Authors frequently indicate this by selecting some one species as a fixed point of reference, which they term the "type of the genus." … We submit therefore that
>
> §4. The generic name should always be retained for that portion of the original genus which was considered typical by the author.
>
> Strickland (1843, pp. 110–111; italics in original)

This article testifies that Westwood's voice had been heard after all. With "typical portion of a taxon" being specified as the portion that was considered typical *by the namer*, it was clear that the Code did not refer to types under the classification type concept. The 'type of a genus' being described as "a fixed point of reference" left no doubt that 'type' in the Code referred to the nomenclatural type concept.

The Code further anchored this new stance about naming and types in several other articles. In §G, for example, it was recommended that for newly described genera "one species should be invariably selected as a type or standard of reference" (Strickland, 1843, p. 121).

It is not clear whether Westwood (or any of his followers)[33] directly influenced Strickland to adopt the nomenclatural type concept. In any

[33] Shuckard, who also served on Strickland's committee, was one of those early followers. He agreed on the "fixed principle" that whenever a genus is name and described "the type [should] be at the same time exhibited, which would insure justice being done to the original describer, by the necessity for retaining his generic name to the type, whatsoever might subsequently become of its congeners, upon the occurrence of new views, or the introduction of new creatures" (Shuckard, 1837b, p. 250). Westwood and Shuckard nevertheless become embroiled in a short but bitter dispute about what should be done if the author of a genus had not clearly indicated what he considered to be the typical species (Westwood, 1836, 1837a; Shuckard, 1837b, 1837a).

case, it looks like Strickland already grasped its usefulness before he composed his committee. The *Proposed Plan* from September 1841 already included the exact same formulation of the relevant rules. Moreover, in February of that year Strickland already made a remark in print which suggests that he had abandoned his earlier rule. In an essay review of George Robert Gray's (1808–1872) *Genera of Birds,* Strickland no longer recommended that the name of a genus that was being split should be retained for the most typical segment of the original, but instead stated that "an author, in restricting an existing genus, ought always to retain the original name for that part of the old genus which was considered as typical by its author" (Strickland, 1841a, p. 419; italics mine). Textually speaking, the change was minute. Conceptually speaking, it marked a major shift.

'Type of a taxon' could now mean different things, depending on the context. It could either be understood in the traditional sense of "an element which is judged typical of a taxon," which is to say, as the taxon's *classification type* – the cornerstone of Whewell's *Method of Type*. Alternatively, 'type of taxon' could be understood as "the fixed element that anchors the name of the taxon," the taxon's *nomenclatural type*, and the organizing concept of what would in due time become known as the *type method*.

It was easy to confuse these two notions of 'type,' and to confuse the practices they were implied in. Followers of the Strickland Code would have to respect the designated nomenclatural types when referring to genera that had already been named, but the Code did not require taxonomists to use *these* types as classificatory models – classification types – for further study of those genera. The Code being a set of rules about *nomenclature*, it made no provisions about what counted as sound taxonomic judgment.

Interestingly, it seems that not even Strickland fully grasped these implications, since he later wrote:

> We may obtain a great amount of fixity, in the position at least, if not in the extent of our groups, by invariably selecting a type, to be permanently referred to as a *standard of comparison*. Every family, for instance, should have its type-subfamily, every subfamily its type-genus, and every genus its type-species. [These types are] examples or illustrations selected for convenience to serve as *permanent fixed points* in our groups, whatever be the extent which we may give to their boundaries."
>
> Strickland (1844, p. 219; italics added)

Here Strickland can be seen to muddle the notion of a nomenclatural type with that of a classification type. His reference to ''standards of comparison'' suggests that he is talking about classification types. Yet, a classification type is not a ''permanent fixed point'' in a taxonomic group, but is rather supposed to track the typical center of a group, if it is to be a representative member of that group. A taxon element that once served as a good classification type may not do so any longer, because it is judged to be atypical in the light of new taxonomic knowledge. The only elements that need to be fixed are nomenclatural types. And because they need to be fixed, they tend not to make for good, durable standards of comparison.

Although Strickland seemingly failed to grasp this subtle distinction, he understood that the conception of types as fixed name-bearing devices was a new, post-Linnaean idea. This much is evidenced by an interesting exchange on nomenclature between him and Charles Darwin in the late 1840s. In a discussion about the intricacies of the principle of priority, Strickland explained to Darwin the need

> to trace out the original *type-species*, on which, like the lineal descendant of an ancient family, the original estates privileges & *title* must be for ever entailed. That is the theory, but it happens unluckily that a type-species is a modern idea – Linnæus & his immediate successors considered all the species in a genus as free and equal, and no more thought of making one species the ''type'' than a Yankee would think of making his eldest son the head of the family.
>
> Strickland to Darwin, 15 February 1849; DAR Letter 1226

To make sure that Darwin had understood him, Strickland emphasized in the final paragraph of his letter that ''by *type-species* I only mean a conventional distinction, referring only to *words*, not to *things*, and like human titles, only used as a matter of convenience.''

The Nomenclatural Type

Word of the Strickland Code spread quickly. Translations into Italian and in French appeared almost immediately (Bonaparte, 1843; Guérin-Méneville, 1843; Anonymous, 1844), foreign societies recommended their members to follow the Code (Dana, 1846), and extensive reviews appeared in scholarly journals (Gould, 1843). Some taxonomists were quick to grasp the need and purpose of the new 'type method' it codified. The Danish conchologist Otto Morch (1828–1878), for example,

wrote: "Linnaeus directs that, if a genus must be divided, the most common species shall preserve the old name. This course can scarcely, in the present day, be considered as very scientific. The author who establishes a genus alone has the right to decide which species he wishes to be regarded as the type, and to interpret the meaning of his generic name" (Morch, 1858, p. 136). Most taxonomists, however, failed to notice that the Strickland Code attached an entirely novel meaning to talk of 'types.'

Exemplary in this regard is the response from Louis Agassiz, who compared Linnaeus's nomenclatural aphorisms with the Strickland Code in the introduction of his *Nomenclator Zoologicus*, a massive work listing all generic names used from Linnaeus to his day. In evaluating §3 of the Strickland Code, Agassiz remarked "this rule does not depart from Linnaeus's §246,"[34] and thus overlooked that Strickland was actively departing from this Linnaean canon (Agassiz, 1842–1847, p. xx).[35]

There were also those who did notice Strickland's departure from Linnaeus, but who were hesitant to follow in his footsteps. This is especially clear in the first botanical code of nomenclature, drafted by Alphonse de Candolle (1806–1893) – son of Augustin Pyramus – on request of the 1867 International Botanical Congress, and modeled on the Strickland Code. De Candolle wrote:

> According to Linnaeus, the name of the genus should rest with the most common species and that which is officinal (*vulgatissimae et officinali*), an equivocal rule in all those cases where one species is most common and another officinal. Subsequent authors tend to say that, the name should stay with the oldest known species, those forming the original type, etc., but it is impossible not to take into account the relative number of species.[36]
>
> Candolle (1867, p. 60)

Hence, if one part of a divided genus contained many more species than the other part(s), de Candolle argued that the largest part should inherit

[34] "Haec lex a §246 Linnaei non discrepat."

[35] Even more confusingly, Agassiz continued by stating about §4 and §5: "Hae duae leges necessariae factae sunt, ex quo antiqua genera in infinitum dividi coepta sunt."

[36] "D'après Linné, le nom du genre divisé doit rester à l'espèce la plus commune et à celle qui est officinale (*vulgatissimae et officinali*), expression équivoque dans tous les cas où il y a une espèce très-commune et une autre officinale. Les auteurs subséquents disent, en général, qu'il faut laisser le nom aux espèces le plus anciennement connues, à celles formant le type ancien, etc., mais il est impossible de ne pas tenir compte du nombre relatif des espèces."

the original name, even if another part contained the 'original type' (Candolle, 1867, §54). This demonstrates that although de Candolle did understand a 'type' to be a fixed element, he did not want to always privilege it as a taxon's name-bearer.[37]

Gradually, however, the nomenclatural type concept found its way into many of the other, specialized codes of nomenclature that sprouted in the second half of the nineteenth. The *Kiesenwetter Code of Entomological Nomenclature* (Kiesenwetter, 1858), the *Report on Nomenclature in Zoology and Botany* commissioned by the AAAS (Dall, 1877), the *American Ornithologists Union Code* (Coues et al., 1886), and the French Zoological Society's *Règles Applicables a la Nomenclature des Êtres Organisés* (Blanchard, 1881, 1889): all of them included rules that (explicitly or implicitly) concerned the fixation of names by means of designated nomenclatural types.[38] The wording of these codes generally made clear that the 'types' at issue were not to be regarded as typical from a present point of view, and some emphasized this by speaking of the 'type ancien,' 'type originaire,' 'original type,' or the 'type of the primitive genus.' Yet most codes just stuck with the simple 'type,' 'Typus,' or 'typical form' – terms that could easily be mistaken to refer to classification types.

The confusion over the meaning of 'type' endured in the late nineteenth century. In a discussion on nomenclature in entomology, the Dutch taxonomist Pieter Snellen reported that he "completely rejected the system of generic types [i.e. type species]," since he did not think that any species of a genus could be singled out as more typical than another (Snellen in Durrant, 1898, p. 312). Another entomologist immediately pointed out that Snellen was confusing two senses of 'type': "A genus as a systematic idea has no type … from a practical and *nomenclatorial* point of view, however, the name must always be connected with a certain species" (Aurivillius in Durrant, 1898, p. 312).

Around the same time, the geologist Francis Bather warned of the "danger that needs constant guarding against, namely, the employment

[37] Here de Candolle was essentially following his father, who had similarly remarked "Si un botaniste reconnaît qu'un genre doit être divisé en plusieurs, il doit conserver l'ancien nom, ou au groupe le plus nombreux en espèces, ou à celui dans lequel se trouve l'espèce qui l'a primitivement reçu" (Candolle, 1813, pp. 236–237; Candolle, 1819, p. 266). It is noteworthy that the elder de Candolle *did* accept the nomenclatural type concept in the context of species names (see Section "Affixing Labels, Fixing Names").

[38] For general discussions of the development of nomenclatural codes (mostly focused on 'priority'), see Linsley and Usinger (1959), Melville (1995), Knapp et al. (2004), and especially Nicolson (1991) and Dayrat (2010).

of a common word in a restricted or altered technical sense. The man in the street knows the meaning of 'type' and 'typical,' but the meaning of those terms to the zoologists something quite different … [Types] are often aberrant forms, i.e., are *not* typical in the ordinary English sense'' (Bather, 1897, p. 844). Bather was even willing to completely constrain the meaning of 'type' to that of 'nomenclatural type.' He argued that, with regard to classification, ''the word type has been stolen from us,'' and needed to be replaced with something like 'norm' (Bather, 1897, p. 844). On the European continent it was similarly noted that ''The difference between the historical and the natural type – i.e., between the taxon element that happened to be described first and the taxon's actual centre – is often completely overlooked'' (Kobelt, 1904, p. 147).[39]

Only in the twentieth century did some revised codes of taxonomic nomenclature start to make explicit reference to 'nomenclatorial types' or 'nomenclatural types.'[40] By the 1920s, a popular textbook on systematic botany could open with the warning: ''A nomenclatural type must not be confused with a biological type. The latter is a representative of the group to which it belongs; the former determines the application of a name'' (Hitchcock, 1925, p. 129).

Type Specimens in Transition

Under the Radar of Nomenclatural Codes

Thus far, we have seen how the nomenclatural type concept emerged at the level of type *species* and type *genera*. But what about type *specimens*? Whewell never mentioned types below the species level in his discussions of the *Method of Type*, nor did the Strickland Code extend the new *type method* towards the naming of species. The notion of a 'type specimen' was indeed conspicuously absent from the vast majority of nineteenth century discussions about types. The first nomenclatural code to apply the type method below the level of species was the American Ornithologists Union Code (Coues et al., 1886). And it took until the

[39] ''Den Unterschied zwischen dem historischen und dem natürlichen Typus, d. h. zwischen der zufällig zuerst beschriebenen Form eines Kreises oder einer Formenkette und dem tatsächlichen Mittelpunkte derselben, wird meistens völlig übersehen.''

[40] E.g. ''The application of a name is determined by reference to its nomenclatorial type'' (Arthur et al., 1907, §4), and ''The application of names of taxonomic groups is determined by means of nomenclatural types. A nomenclatural type is that constituent element of a group to which the name of the group is permanently attached'' (Briquet, 1906, §8).

twentieth century before many other codes began to include articles on types *qua* specimens. How come?

The reasons for this 'delay' needs to be sought in a mix of practical, theoretical, political and institutional factors. Consider first the practical complications in applying the nomenclatural type concept below the species level. Because of their tangible, collectible nature, specimens could be displaced, lost in exchange, eaten by moths, perish in drawers, or get discolored through sunlight. Hence, before the invention of reliable taxidermic methods, it was practically impossible to permanently affix names to individual specimens. The very possibility of 'fixing' nomenclatural type specimens depended on technology.

On a theoretical plane, we ought to realize that the lion's share of nomenclatural issues generated by the splitting and merging of taxa took place above the species level. The boundaries of genera had been resculpted incessantly since the days of Linnaeus, and the new classifications that had cropped up – with Lamarck, Cuvier, and the quinarians – tended to leave species boundaries largely untouched.

Finally, the stability of species boundaries were actively policed by means of institutional politics. As Gordon McOuat has pointed out, naturalists in powerful positions in the British Empire used their influence to ban discussion of species circumscriptions in various ways (McOuat, 1996, 2001). One tactic, for which the Strickland Code set the example, was to actively exclude the topic of splitting species from the rules of nomenclature. ''For the Rules, species were just what competent (read: institutional, published, gentlemanly, conservative) naturalists said they were'' (McOuat, 1996, p. 512). Where species boundaries could be set, with institutional fiat, by a small group of experts, there was little reason to provide communal guidelines for the naming of divided species. The establishment did the naming for the rest.

Another tactic was mastered at the British Museum, where a zealous assistant (and later Keeper) of the Zoological Department, John Edward Gray (1800–1875), came up with a system of cataloguing that turned museum curators into the guardians of species limits. In the catalogues that were published under Gray's guidance, each species was assigned a separate leaf under which all its known specimens in the Museum were listed. Gray allowed that ''in any future time the leaves may be separated and bound in any other form,'' so as to fit new systems of classification (Parliamentary Papers, 1836, §2500, p.195). But the leaves themselves – the species – could not be fiddled with. Species thus became the museum's stable 'base units,' which outsiders could not split, merge, or rename. Through this ''little legerdemain of standard-

ization'' species were both 'made' and 'protected' at the Museum (McOuat, 2001, p. 4; also see Thomas, 2012). Again, there was no reason to anchor species names to nomenclatural types.

However, each of the dimensions I have touched on – practical, theoretical, and political-institutional – also had its flip-side. In the course of the nineteenth century, the fixation of nomenclatural type specimens became possible from a practical point of view, prestigious from an institutional point of view, and pressing from a theoretical point of view. Together, these changing parameters explain how the notion of a 'type specimen' went through the same transformation in meaning as 'type species' and 'type genus.' Once having signified a classification type, 'type specimen' came to refer to the nomenclatural type of a species.

Affixing Labels, Fixing Names

Linnaean taxonomic practice once again forms a natural starting point for tracing out the transformation at issue. As we saw earlier (Section ''Linnaeus and the Method of Type,'' Linnaeus used classification type *species* to set limits to genera, by means of 'collating' typical species with others. It should be noted, however, that indirectly this practice also relied on classification type *specimens*. The reason is that Linnaeus often used a typical specimen of a typical species as its proxy. In the *Hortus cliffortianus*, for example, Linnaeus mentioned that he had determined through ''collation with a specimen [received] from Sherard'' (Linnaeus, 1737c, p. 167; cited in Müller-Wille, 2003, p. 475) that a certain new species belonged to an already known genus. Hence, rather than using a composite image or description of a typical species a classificatory standard, Linnaeus typically used a typical specimen as its stand-in.[41]

To make this practice possible, a large repository of typical specimens was needed. Linnaeus therefore accumulated a large stock of specimens through collection and trade, and stored this material in several herbaria – large cabinets used for filing sheets with dried plants specimens on them.[42] Linnaeus's use of herbaria made them into more

[41] Classification type specimens played less of a direct role in determining the boundaries of species. Linnaean species were usually based on a synthesis of data derived from descriptions by predecessors, illustrations, experiments with living plants, and comparisons with typical specimens (Svenson, 1945; Clarke, 1894; Müller-Wille, 2001).

[42] When, after Linnaeus's death, his herbarium cabinets were sold to the Linnean Society of London, they reportedly contained some 14,000 dried typical specimens (Müller-Wille, 2006).

than mere repositories for specimens (Müller-Wille, 2006). By virtue of harboring one (or at most a few) typical specimens for each named species, the sheets in a herbarium implicitly marked the boundaries between species. To sustain and improve this function, Linnaeus often swapped damaged or decayed specimens for fresh exemplars, and sometimes substituted more typical specimens for ones that in the light of new knowledge appeared somewhat aberrant. As for the replaced specimens: ''Linnaeus sometimes gave [them] away when they had been replaced in his herbarium by better representation of the species concerned'' (Pennell, 1939, p. 380).

These practices demonstrate that Linnaeus's 'typical' specimens were nothing like our modern 'type' specimens. Linnaeus's inclination to replace original specimens with more typical exemplars at a later date made it possible for a species name to wander from one species to what, by the judgment of later taxonomists, was another species. Taxonomists who have studied Linnaeus's herbarium have indeed noticed that ''Linnaeus sometimes replaced an earlier gathering by a later conspecific with it in his opinion though not necessarily so, according to ours'' (Stearn to Jarvis, 1969; quoted in Jarvis 2007, p. 44).[43] It would therefore be a mistake to think of the specimens in Linnaeus's herbarium as nomenclatural types for the names he coined. ''Linnaeus never designated any specimens as [nomenclatural] type. Whether his description was based on one single or on several specimens, it cannot even be taken for granted that [these] were preserved in his collection'' (Lindroth, 1957; also see Kirby, 1892). It has even be said that those who mistake Linnaeus's herbarium specimens for nomenclatural type specimens ''seem to have lost their sense of proportion. Great as Linnaeus's opinion of his own work was – with abundant reason – it is foolish to imagine that he could ever have anticipated the importance that future botanists would place on his specimens'' (Svenson, 1945, p. 380).

The Linnaean practice of treating original specimens as substitutable exemplars was still common in the early decades of the nineteenth century. Museum curators and collectors showed little hesitation in replacing the specimen(s) on whose basis a species had first been named and described with other specimens ''whenever the old ones became faded or were damaged by insect pests'' (Mayr, 1969, p. 368). A museum curator who understood his job would promise visiting naturalists

[43] The same had already been noticed in the mid-nineteenth century: ''Linnaeus subsequently introduced into his herbarium … a totally different plant'' (Harvey and Sonder, 1865, p. 361).

to "certainly … in time exchange the bad [original] specimens for better ones" (Wiedemann, 1818; cited in Zimsen, 1954, p. 6).

Gradually, however, several prominent taxonomists started pointing to the nomenclatural ramifications of these practices. In the *Théorie élémentaire de la botanique*, Augustin-Pyramus de Candolle pointed out that

> One of the principle benefits that science draws from herbaria, is the fixity they give to nomenclature; with their aid, one can always ascertain which is the plant that served as the type for the descriptions of original authors, and thus avoid mistakes that can result, either from the accumulation of erroneous synonyms, or from defects or omissions in the descriptions.[44]
>
> Candolle (1819, p. 321)[45]

As a practical advice, Candolle recommended taxonomists to complement their descriptions of new species with a reference to a properly labeled "type de l'espèce," to be permanently stored in a collection (Candolle, 1819, p. 323). Candolle here clearly used 'type' in the sense of 'nomenclatural type,' the fixed name-bearer of a species name.

Candolle was aware that the nomenclatural need to 'fix' specimens as name-bearers had a practical downside. It impelled botanists to invest in drying, storing and labelling original specimens carefully, so as to preserve them for the long haul. De Candolle realized that "all these precautions are punctilious and may appear almost mechanical," but he maintained that "it is only through them that the prodigious number of plants, that we continue to discover, can be described and recognized without confusion" (Candolle, 1819, p. 323).

Although the preparation of plant specimens for long term storage was a tedious effort, it could be done. However, for most animal specimens no similarly reliable taxidermic methods were available. Birds, for example, would be damaged when stored in alcohol, and were likely to be eaten by insect pests when dried. That is what happened with the collection of Sir Hans Sloane (1660–1753), the founding col-

[44] L'une des principales utilités que la science retire des herbiers, est la fixité qu'ils donnent à la nomenclature; on peut toujours retrouver avec certitude, par leur secours, quelle est la plante même qui a servi de type pour lés descriptions des auteurs originaux, et éviter ainsi les erreurs qui peuvent résulter, soit de l'accumulation des synonymes erronés, soit des vices du des omissions des descriptions.

[45] It is worth noting that this paragraph was absent from the first edition of de Candolle's book (Candolle, 1813). This might indicate that he had only become aware of the need to 'fix' specimens as name-bearers in the late 1810s.

lection of the British Museum (Natural History). In the early twentieth century, it was reported that "not a single specimen of bird from the Sloane Collection now exists in the Museum. All have perished." Many other eighteenth century collections had similarly "fallen to pieces" (Sharpe, 1906, 79–80).[46] Only with the invention and spread of reliable taxidermic procedures around the 1830s could animal specimens be preserved for more than a few years (Farber, 1977; Johnson, 2005).[47]

And yet, there was still a gigantic obstacle ahead. To actually authenticate, isolate, label, preserve and store these specimens would require considerable resources. Realistically, only large museums and botanical gardens would have the people, facilities and funds to undertake the effort. But what could motivate them to do so?

Creating Value

To answer this question, we need to return to John Edward Gray and the British Museum. As I mentioned earlier, there is a sense in which his innovative system of cataloguing removed the need to preserve original specimens for use as fixed name-bearers. Using Gray's system, species names were anchored to individual pages, not to any of the specimens listed on those pages. Nevertheless, Gray realized that there were good other reasons to take special care of the large gatherings of original specimens in the Museum's collections. He was acutely aware that, along with the catalogues, these specimens could be recruited to increase the Museum's power and prestige.

Gray sketched his attitude towards original specimens in the dozens of catalogues that were published under his auspices from the early 1840s onwards.[48] In each of these catalogues Gray included a brief introduction in which he accentuated the "peculiarly fortunate" position of Museum as the recipient of many specimens on whose basis "the species to which they belong were originally described, or … in which they first received their names." Gray emphasized that these specimens were of special value, since "there can be no doubt of the specimens being ascertained representatives of the names they bear" (e.g. Gray,

[46] The same held for collections at other museums. By 1844, more than half the collection of Fabrician specimens of Coleoptera held in the Kiel Museum had been "sehr zerfressen" (Hagen in Zimsen, 1964).

[47] Johnson (2005) mentions this as a reason for the "almost complete lack of types from between 1758 and 1815" in the 3-volume index of type specimens of birds in the BM(NH) (Warren, 1966; Warren and Harrison, 1971, 1973).

[48] An incomplete list of catalogues published under Gray's guidance is given in Thomas (1906, p. 66).

1843, p. vi; Gray, 1844, p. v). Gray's introduction was sometimes followed by a list of collections from which the Museum had acquired these 'type specimens,' 'types of the species,' 'original specimens' or 'authentic specimens,' as he variously called them.

Gray's commitment to preserving original specimens could already be heard some years earlier, when he appeared before a Parliamentary Committee that investigated the affairs at the BM in 1834. One of the many questions fired at Gray was when, in his opinion, the Museum ought to recognize a new species or genus which had been described in print. Gray answered:

> There is one test of the value of such divisions, the importance of which is universally admitted, while it is seldom that an opportunity is given to apply it; I mean the placing in the National Collection type specimens of the objects described, authenticated by their authors. Such specimens, with the names attached and so authenticated, would always remain open to future investigators, and would supply the deficiencies which occur even in the best figures and descriptions, and which, by rendering doubtful what has been done before, contribute much to embarrass science with repetitions of the same object under new and varying names.
>
> Parliamentary Papers (1835, §3345, p. 240)

The point about 'authentication' was essential. Not just *any* old specimen, but only those specimens which had been used in naming and describing species were of special value; even if they were not representative of the typical characters of the species.

The latter point was also driven home by zoologist Nicholas A. Vigors (1785–1840), when he appeared before the same Parliamentary Committee two years later. Vigors was asked whether "A very inferior specimen of the giraffe, for instance, would be valuable in a national collection, if it were the first specimen introduced into Europe for the last 15 centuries?" He answered that there would be "nothing scientific" about including it in the collection for mere historical reasons. "[B]ut if it was the first specimen that had been described by a particular zoologist, I should then, as a type of his description, and the very example from which he took his characters, preserve it as most sacred" (Parliamentary Papers, 1836, §1315, p. 111). Vigors clearly understood 'type' in the sense of an original specimen, and not in terms of a typical one.

The value that was attributed to these original type specimens is underscored by the changing attitude towards 'duplicates' that started

to take hold. In Linnaean times, the notion of a 'duplicate specimen' had applied symmetrically: two collected specimens of the same species counted as duplicates *of each other*, and any specimen in a set of duplicates could be traded with another museum. This changed when original specimens acquired a special status *qua* originals. A duplicate specimen became a duplicate *of* the original specimen.

The difference in value between originals and duplicates was illustrated by the remarks of anatomist John Flint South (1797–1882), yet another scientist who appeared before the Parliamentary Committee. In a back-and-forth about the value of specimens in the collection, South told the Committee that "it is useless load the Museum with four or five individuals of the same species." This prompted a question from a Committee member about whether he considered it important "to preserve the identical specimen first discovered as historical evidence … however inferior the original specimen?" South: "Certainly; and in such case I consider it right that it should be marked as the first specimen discovered, and by whom, and at what date" (Parliamentary Papers, 1836, §§1156, 1175ff.; pp. 99–100).

Meanwhile, Gray developed a strategy to dispose of duplicates that would simultaneously increase the value of the originals. He envisioned the BM as a hub for the 'authentication' of specimens in a national network of provincial museums and collections. The Museum's original specimens being the nomenclatural types, they could be used as reference standards for labeling the duplicates in other collections. Gray therefore proposed to the Trustees that the Museum to start selling "series of duplicates properly selected and named … to the different local institutions" (Gunther, 1980, p. 221).[49]

Not all duplicates could be given away, though. Even the BM needed duplicates to furnish its displays with, since original specimens were too valuable for that purpose. "Such specimens ought especially to be preserved in such a way as to be least liable to injury from exposure to light, dust, or other extraneous causes of deterioration; and this is best done by keeping them in a state least exposed to these destructive influences, instead of in the open cases of a public and necessarily strongly lighted gallery" (Gray, 1864b, p. 77).[50] This required a shift in

[49] McOuat (unpublished) has pointed out that this was part of Gray's more general program of middle-class reform. Gray wanted local institutions to compete on the 'open market' for the acquisition of duplicates – a market for which the BM set the standards through its nomenclatural type specimens.

[50] The logician William Stanley Jevons (1835–1882) later reiterated Gray's point, stating that "by far the largest part of the biological collections should be packed in draws, and only the most distinct and typical specimens exposed to view" (Jevons, 1883, p. 70).

mindset from that adopted "some forty years ago," when curators had thought it desirable to mount every specimen in the collections for public display. Buttressing the new attitude towards original specimens was a request for government funding Gray had made in the late 1850s, for the purpose of storing "the type specimens described by various authors, [as] they may be considered, in a scientific point of view, as invaluable; and if these specimens are not very shortly removed to a dryer place, they will be utterly destroyed" (Parliamentary Papers, 1858, p. 4).

"A Crime Against Science"

Gray's efforts at the BM helped to spread the idea that original specimens should be preserved, and should function as fixed anchors for species names.[51] Yet, much like in the case of type species and type genera, old terminology obscured the new philosophy of type specimens. Gray himself, for example, often alternated talk of 'type specimens' with talk of 'typical specimens.' In the mid–1850s, he reported with pride:

> The extent to which the description of the Museum's collection has been carried on has rendered it the greatest store house of typical specimens, from which very large numbers of species of animals of all classes have been described, so that the consultation of the collection has now become absolutely necessary to the naturalists of all parts of the world, who may be desirous of extending the domains of their science, and fairly knowing what has been before recorded and described.
> British Museum Annual Report, (1854); cited in Gunther (1975, p. 111)

Where Gray spoke of 'typical specimens' he was referring to the nomenclatural type specimens. Undoubtedly, many of these specimens possessed the typical characters of their species, and in this light his talk of 'typical specimens' was warranted. But it was also confusing and problematic, since even an indisputably atypical original specimen could still serve as a nomenclatural type specimen. As long as a nomenclatural

[51] A testimony to the importance of nomenclatural type specimens is a section on 'Types in the Collection' in an early twentieth century history of the zoological collections at the BM. It mentioned that "the value of type-specimens, and the index which their possession gives to the importance of a Museum, are now so universally recognized that a few lines may be devoted the richness of the British Museum in this respect" (Thomas, 1906, p. 64). The same work also singled out J. E. Gray as "the real maker of the collection" (p. 2).

type specimen could be placed reliably within the boundaries of one species only, it could do its job of naming that species.

Gray was not an exception in using ambiguous language. Many other naturalists similarly spoke of 'type' and 'typical' specimens interchangeably and with different meanings. Joseph Dalton Hooker noted about 'type' that "the word is often used in a vague and unphilosophical manner: in the too frequent sense of the term it denotes that individual of a species which was first cultivated, described, figured, or collected, or that form which is most abundant in the neighborhood of the writer" (Hooker, 1853, p. xvi).[52]

Hooker's friend Charles Darwin (1809–1882) shared his scruples about type talk. Having noted that "there is … so much vague in the meaning of 'typical forms,'[53] he turned to Hooker and George Waterhouse (1815–1898) for help. They replied by concurring that the terms 'type' and 'typical' were being used equivocally, to refer either to (an exemplar of) the most common form or to a most perfected one.[54] Adding to Darwin's confusion was a later letter from his botanist friend Asa Gray (1810–1818), who remarked that "Our choice of what to take as the typical forms very often is not free. We take, e.g. for one of them the particular form of which Linnaeus, say, happened to have a specimen sent him, and on which he established the species." More generally, Gray noted that "The form which first comes & is described & named … sticks as the type, tho, in fact it may be far from the most common form."[55] At this point Darwin seems to have thrown in the towel, writing to another correspondent of his that "with respect to Typical – I observe that Naturalists use it in two very different senses; hence I have almost entirely or entirely avoided its use."[56]

[52] Moreover, Hooker considered none of these uses to capture the meaning of a 'true type,' which (to him) referred to "the originally created form of any plant." About this form the naturalist could have "no clue whatever" since a plant's past typical state need not coincide with its present typical state. Hooker thus concluded that, theoretically speaking, "the type is a phantom" (Hooker, 1853, p. xvi). In a sense, Hooker herewith applied Wood's and Westwood's criticism of talk about "typicality" to the species level. (For more on Hooker on types, see Endersby (2008, p. 160ff.) and Stevens (1994, p. 150)).

[53] Darwin to Hooker, 31 March 1844; DAR Letter 744; DAR Letter 1934.

[54] Hooker to Darwin, 5 April 1844; DAR Letter 745; Waterhouse to Darwin, 26 april 1844; DAR Letter 748.

[55] Asa Gray to Darwin, early August 1856; DAR Letter 1934. A little later Gray repeated the same point in print: "Affixing of a name to a sufficient specimen in distributed collections … [will] more surely identify the genus or species than might a brief published description!" (Gray, 1864a, p. 279).

[56] Darwin to Woodward, 6 March 1860; DAR Letter 2724.

The foregoing shows that, by the mid-nineteenth century, talk of 'types' and 'typicality' invited as least as much confusion at the level of specimens as it did at the level of species and genera. Similarly, it was only in the final decades of the century that the distinction between classification and nomenclatural types was made more explicit. Alphonse de Candolle was among the first to state clearly that "the expression 'type' or 'typical sample' (*echantillon typique*) is … used incorrectly for the specimen described by its author. One should say 'authentic sample' (*echantillon authentique*). Sometimes the first sample described departs from the average, or the type of the species" (Candolle, 1880, pp. 51–52). In a discussion of Blanchard's *Règles* (see p. 22) at the 1889 International Congress of Zoology it was similarly remarked that "we should clarify the meaning that is assigned to the word 'type' or 'typus.' Currently, this word is used in very different senses in the everyday language of naturalist," one of them being "the specimens on which the first description of a species was based"[57] (Oberthür, 1889, p. 476).[58]

At the closing of the century, confusion over the meaning of the term 'type specimen' appears to have cleared. The generally recognized definition of a type specimen as a fixed name-bearing specimen was now clearly stated in journal articles and the prefaces of catalogues: "By a 'type' is meant the original specimen to which any generic or specific name was first assigned" (Hughes, 1891). "A 'TYPE SPECIMEN' is the specimen of an insect from which the original describer drew up the first description of a species; and it is often of great importance to settle disputed points of nomenclature … for if we are certain that we have the original specimen before us, no further dispute is possible" (Kirby, 1892, p. 244).

The value of original specimens qua nomenclatural types was similarly recognized clearly, in proclamations to the effect that "there can be but one type [which] no museum can afford to part with … Typical specimens are quite another matter, and the more distributed the better" (Lucas, 1897, p. 544). To treat a type specimen like any other specimen had come to be seen as irresponsible, if not outright criminal: "The exhibition in glass cases of type specimens of animals injured by light – as birds and mammals – indicates a disinterestedness amounting almost to criminal neglect" (Merriam, 1897, p. 732). It was equally "a crime

[57] "Actuellement ce même mot, dans le langage ordinaire des naturalistes, est appliqué dans des sens très différents … On dit aussi: 'la collection *X* contient beaucoup des *types*,' c'est-à-dire d'échantillons ayant servi à la premier description d'une espèce."

[58] For more on the discussion of Blanchard's *Règles*, see Dayrat (2010, pp. 213–214).

against science'' to remove the name tag from an original specimen and to attach it to another one. A kind of crime which ''in olden times little thought was bestowed on.'' Modern taxonomists, however, were expected to recognize ''the importance, I may almost say the sacredness, of the 'original label'" (Hartert, 1918, p. 5).[59]

Typicality Redux

In the early twentieth century, this understanding of type specimens also began to make its way into a number of new nomenclatural codes. The American Code of Botanical Nomenclature stated unambiguously that ''the nomenclatorial type of a species or subspecies is the specimen to which the describer originally applied the name in publication'' (Arthur et al., 1904, §14).[60] The First International Rules of Zoological Nomenclature included recommendations on the deposition of type specimens in museums (ICZN, 1905).

However, just at about the time that the meaning of type specimens as nomenclatural types seemed to have been anchored in nomenclatural codes, the meaning of the term was strained once again. For, although most taxonomists agreed that type specimens in the role of name-bearers ''are of necessity unique'' (Holmes, 1896, p. 56), a minority began to use the term slightly more liberally.

The British, BM(NH)-based zoologist Oldfield Thomas (1858–1929) already noted in the late nineteenth century that ''The word 'type' itself when first introduced was meant to refer to the particular specimen (in the singular) originally described, but it soon was naturally applied to any individual of the original series, if more than one specimen was examined by the describer'' (Thomas, 1893, p. 241). Thomas was of the opinion that there was ''little cause for confusion'' in this extended use of the term. Yet he noted that it did get problematic when 'type' was given an even broader interpretation. Recently, authors had started using 'type specimen' to refer to ''any individual from the collection of

[59] The enduring value of types is also illustrated by the complete segregation and separate storage of type specimens that was being realized. When Britain declared war on Germany in September 1939, over 85% of type specimens at the BM(NH) had already been packed and readied for transport to the Zoological Museum at Tring (Warren, 1966, p. iv).

[60] However, the Americans failed to convince their European colleagues of the importance of this paragraph at the International Botanical Congress held in Vienna, in June 1905 (Arthur et al., 1907). Much to the chagrin of the Americans, the First International Rules of Botanical Nomenclature continued to be modelled on de Candolle's Code from 1867.

the original author, obtained no matter how much later,'' and to specimens collected from the same locality as the original. These uses of the term were ''certainly liable to give rise to inconvenience and confusion,'' and needed to be cleared up. To do so, he suggested to distinguish between five different kinds of type specimens:

> A *Type* is a single specimen either unaccompanied by others at the time of description, or else deliberately selected as such by the author out of a series.
>
> A *Co-type* is one of two or more specimens together forming the basis of a species, no *type* having been selected. No species would have both type and co-types, but either the former, or two or more of the latter.
>
> A *Para-type* is a specimen belonging to the original series, but not the type, in cases where the author has himself selected a type. It should, however, be one of the specimens mentioned or enumerated in the original description.
>
> A *Topo-type* is a specimen simply collected at the exact locality where the original type was obtained.
>
> A *Meta-type* is a specimen received from the original locality after the description has been published, but determined as belonging to his own species by the original describer himself.
>
> Thomas (1893, p. 242)

On Thomas' scheme, a type specimen was no longer simply a name-bearer; only one *kind* of type specimen now counted as a name-bearer. Type specimens of other kinds were in some sense 'typical' of their species. Little did Thomas realize that this amounted to opening Pandora's box once again. Thomas' attempt at clarification threatened to reintroduce the problem of marrying taxonomic naming to taxonomic judgment.

Soon, the pages of *Science* began to fill with suggestions for other kinds of type specimens that needed to be distinguished, and with other type-terms that needed to be added to Thomas' list. The American paleontologist Charles Schuchert (1858–1942), for example, suggested to rename Thomas 'type' into 'holotype,' and introduced 'plastotype,' as the term for an artificial specimen moulded directly from any of the originally figured or described specimens. He mentioned that the latter term could in turn be compounded into 'hypoplastotype' for a specimen molded from a non-original specimen. Schuchert had clearly let go of

the idea that type specimens were name-bearers above anything else, as he stated that "For a clear description of a new species a paleobotanist may require as many individuals as there are specimens selected for study, *all of which are regarded as types*" (Schuchert, 1897, p. 637; italics mine).

Schuchert's fellow countryman C. Hart Merriam (1855–1942) protested against the "apparently incurable form of mania" among naturalists who coined new type-terms. In an attempt to cure the disease by an appeal to reason, Merriam emphasized the 'old' lesson that "type specimens … should from the nature of the case be single, not multiple," since "in a considerable percentage of the cases where several specimens have been used as types, subsequent study has shown these specimens to belong to different species. Is not this fact alone an unanswerable objection to the existence of more than one type specimen of a species?" (Merriam, 1897, p. 732).

Schuchert could agree with Merriam in principle, but not in practice. He admitted that although "the practice of selecting a single example as the type … has its advantages, since all doubt is thus removed when a new species is later found to contain diverse elements," this advantage was trumped by the fact that virtually no species "living or extinct, can be defined from a single individual; hence a multiplicity of types is generally a necessity" (Schuchert, 1905, p. 8). By now, Schuchert had come to discriminate between no less than 18 kinds of type specimens (Schuchert and Buckman, 1905).

In the years that followed, the terminology of types exploded. In 1933, a list of no less than 233 (!) type-terms was published (Frizzell, 1933), which included entries such as

> Holoparalectotype – a specimen from the original material, later established as a paratype, that belongs to the sex described by the author.

Ironically, the author of the list mentioned that he had excluded "absurd terms" such as *para-adelfo-allopara-andro-lipo-mimo-paraedoeotype*; a 'hypothetical' term for a specimen "that was included in the original collection; was used as basis for the original description; that is of the same sex (male) as the allotype [a specimen with the opposite sex of the holotype]; shows the genitalia; is characteristically absent from certain faunas; and is analogous to certain unrelated forms in other countries" (Frizzell, 1933, p. 639).

When, in 1939 another list was published with 'only' 108 type-terms that were (supposedly) actually being used (Fernald, 1939), two prominent taxonomists stood up to reiterate Merriam's objections, aiming to end the avalanche of type terminology.

In Britain, the entomologist Carrington B. Williams (1889–1981) mocked Fernald's list by noting that he had found "no word in [it] for a photograph of a cast of an abnormal larva of a worker of a social hymenopteron, which has been compared with specimens from the same locality as the type. It should, I believe, be: "Photo-plasto-terato-nepi-ono-ergato-homotopo-type," but perhaps I am wrong" (Williams, 1940, pp. 623–624). More seriously, however, Williams made a plea to restrict use of 'type specimen' to purely nomenclatural ends.

> The object of the type is to eliminate from nomenclature the possibility of human error, the personal equation, and private opinion and this object is defeated if more than one type is designated. As soon as a second specimen appears opinion is brought in. If I designate 'cotypes' or 'paratypes,' or any of the hundred or so others listed by Fernald, I imply my belief that these specimens are co-specific with the type. My opinion may be sound or unsound, but it is an opinion … As long as the original single 'type' exists, such specimens as these can never be the final argument for nomenclature. Always in them is expressed the scientific opinion of the limits of a species and hence the possibility of uncertainty.
>
> Williams (1940, p. 622)

In the U.S., the same lessons were spelled out at great length by George G. Simpson (1902–1984), a rising star in paleontological taxonomy. Simpson diagnosed the problem with the expansive type terminology as stemming from the idea that "somehow there is a limited suite of specimens that really represent or give rise to the species, hence must be 'types,' while others, although they belong to the species, do not and hence are not 'types'" (Simpson, 1940, p. 422). Yet, he continued, this assumption was entirely unwarranted. A proper inference about the boundaries of a species "should be based on all the available specimens that are then considered as belonging to the species and on all of them equally." Whether a specimen had been described in the original publication, or had been collected at a later data was irrelevant. All specimens in the known sample should have equal weight in inferences about species limits. "[T]here is no mystic virtue in 'types,' as such, that makes them any better for comparison than would be any other member" (Simpson,

1940, p. 420). Hence, taxonomy could do entirely without the expansive terminology of non-nomenclatural type-terms, "an irregular framework – it cannot be called a system – that is approaching the fantastic" (Simpson, 1940, p. 421).

Simpson agreed with Williams that the only use for 'type' was that of signifying a name-bearing specimen. He therefore proposed to retain only those few compound type-terms that referred to name-bearing specimens. This meant that apart from 'type' (or 'holotype') there remained a use for such terms as 'lectotype' (a name-bearing specimen selected from an original sample from which no holotype has been selected by the first describer of the species) and 'neotype' (a substitute name-bearer for a lost lecto- or holotype).[61] All other type-terms could easily be discarded.

The efforts by Williams and Simpson paid off. Almost twenty years later, it was noted that "The whole scheme … of different kinds of types … collapsed like a house of cards in 1940 when George Simpson published his short but epoch-making paper on 'Types in modern taxonomy'" (Dunbar, 1959, p. 911; also see Romer, 1959, p. 919).[62] Around the same time, a small set of nomenclatural type-terms was also included in the major nomenclatural codes that are still in use today, such as the International Code of Zoological Nomenclature (Ride, 1999) and the International Code of Nomenclature for Algae, Fungi, and Plants (McNeill et al., 2012).

Conclusion

In Linnaeus's day and age there was little reason to anchor names to fixed taxon elements. Names and classifications were 'made' and disseminated from a few high seats, and were accepted on the authority of their authors. Their status quo was upset by the nineteenth century expansion of taxonomy, which saw a rapid increase in collected materials, techniques, and, above all, practitioners. Existing classifications were disrupted, and in the waves of change taxon names became afloat;

[61] The notion of a 'neotype' raises further philosophical issues. For discussion of these intricacies, see Simpson (1945), Haber (2012) and Witteveen (2015).

[62] Meanwhile, Simpson himself continued to correct confused (or careless) taxonomists who wrote things like "the closer the description [of the species] comes to fitting the holotype exactly, the better the picture one can obtain of the typical specimen of the species" (Shenefelt, 1959). Simpson response: "[T]ypes are not typical; description of a specimen does not describe or define a species; and the proper function of a type ('holotype') is solely that of name-bearing" (Simpson, 1960).

different authors started applying the same names to different taxa and different names to the same taxa.

In the introduction to his Code, Strickland captured what had happened, and what was at stake:

> The world of science is no longer a monarchy, obedient to the ordinances, however just, of an Aristotle or a Linnaeus. She has 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.
>
> Strickland (1843, p. 107)

Restoring regularity required regulation. And even though classifications could not be stabilized by decree, names could. Taxon names could be stabilized by anchoring them to taxon-elements, so that even if classifications would remain in a state of perpetual flux, each recognized taxon would have a definite designation.

To this day, the hierarchical anchoring of family names in genus-elements, genus names in species-elements, and species names in specimens, provides stability to naming in the face of ever-changing classifications. Following this method, any two taxonomists can agree on the correct name for any given taxon, regardless of their disagreements about its limits.

It is an artifact of history that this method has become known as the 'type method,' despite the fact that name-bearing 'types' are not required to be typical. It took some time for this message to sink in, but by the mid-twentieth century it could be said that "no [taxonomist] will fail to understand an author writing, e.g., 'It is unfortunate that the biological average of *Planta vulgaris* is poorly represented by the specimen in the original publication.' … It is manifest that the type-specimen and the biological type of the same aggregate may thus be at odds" (Croizat, 1953, p. 124).

However, to the non-expert the notion of a 'type specimen' continues to prompt unwarranted connotations of typicality. Even in news reports for a journal like *Science* we still read that "as new specimens … are found, they are deemed part of a known species, a new species, or even a new genus based on how closely they resemble the type specimen" (Pennisi, 2001, p. 2304).

In an enduring effort to stamp out this confusion, taxonomists have come up with various alternatives for 'type' that do not invite associations of typicality: 'standard element' (Green, 1925), 'testimonium'

(Dennler, 1939), 'index,' 'nominal element' (Williams, 1940), 'onomatophore' (Simpson, 1940), 'nomenifer' (Schopf, 1960), and 'onomyphoront' (Dubois, 2005). Ironically, taxonomy has thus been burdened with numerous *synonyms* for a notion that was invented to drive out synonymy.

And yet, none of these alternatives for 'type' have caught on.[63] All current nomenclatural codes and virtually all taxonomists continue to speak of 'types' when they talk of name-bearers. What counts for taxon names therefore also holds for the modern notion of a 'type': it does not carry its meaning on its sleeve. Therefore, it needs constant reminding that Whewell's Method of Type and the modern type method are similar in name, but not in nature. Whewell's types represented *nature*, our types represent *names*.

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[63] Some were not even intended to catch on. Immediately upon coining 'onomatophore,' Simpson (1940) already threw in the towel by saying: "As a matter of practical usage, however, it is evident that the word 'type' is so deeply rooted in our science that it is not desirable and probably not possible to uproot it" (p. 421).

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