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ORIGINAL PAPER

Of elephants and errors: naming and identity in Linnaean taxonomy

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Abstract What is it to make an error in the identification of a named taxonomic group? In this article we argue that the conditions for being in error about the identity of taxonomic groups through their names have a history, and that the possibility of committing such errors is contingent on the regime of institutions and conventions governing taxonomy and nomenclature at any given point in time. More specifically, we claim that taxonomists today can be in error about the identity of taxonomic groups in a way that Carl Linnaeus (1707–1778), who is routinely cited as the "founder" of modern taxonomy and nomenclature, simply could not be. Starting from a remarkable recent study into Linnaeus's naming of *Elephas maximus* that led to the (putative) discovery of a (putative) nomenclatural error by him, we reconsider what it could mean to discover that Linnaeus misidentified a biological taxon in applying his taxon names. Through a further case study in Linnaean botany, we show that his practices of (re)applying names in taxonomic revisions reveal a take on determining "which taxon is which" that is strikingly different from that of contemporary taxonomists. Linnaeus, we argue, adopted a practice-based, hands-on concept of taxa as "nominal spaces" that could continue to represent the same taxon even if all its former members had been reallocated to other taxa.

Keywords Taxonomy · Nomenclature · Classification · Linnaeus · Error · Identity

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1 Introduction

What is it to make an error in the identification of a named taxonomic group? In this article we argue that the conditions for being in error about identifying taxonomic groups through their names have a history, and that the meaning and possibility of committing such errors are contingent on the regime of institutions and conventions governing taxonomy and nomenclature at any given point in time. Taxonomists today, we claim more specifically, can be in error about the identity of taxonomic groups in a way that Carl Linnaeus (1707–1778), who is routinely cited as the "founder" of modern taxonomy and nomenclature, simply could not be, since he did not operate under the conceptual and procedural regime that governs present-day taxonomy.

The question of what it means to be in error about taxonomic identity was prompted for us by a research effort from a few years ago that attracted a lot of media attention. In November 2013, *Nature*, *The New York Times*, and other (science) news media reported with much fanfare on a publication which showed that the elephant specimen to which Linnaeus referred in the tenth edition of *Systema naturae* (1758) under the name of "Elephas maximus" (commonly known as the Asian elephant), was in fact a specimen of *Loxodonta africana* (the African bush elephant).[1] In other words, Linnaeus had been wrong about the taxonomic identity of the specimen that served as his basis for naming and describing the Asian elephant species. He had named the Asian elephants using an African exemplar. This seemed to be a remarkable discovery indeed. An error committed by the very founder of modern taxonomy had gone largely unnoticed for 250 years and was finally exposed and corrected by means of molecular sequencing in combination with detailed scrutiny of historical materials, carried out by an interdisciplinary team of experts (Callaway 2013; Nuwer 2013; Nyström 2013).

But what, exactly, was the nature of Linnaeus's error or mistake? And how did molecular research contribute to uncovering and resolving it? We begin by arguing that, on closer inspection, it becomes questionable that the molecular research helped to establish a new fact about misidentification that had any scientific or nomenclatural relevance beyond the evidence that was already available. We argue that while the molecular evidence further established the provenance of a historically interesting elephant *specimen,* this piece of knowledge did not contribute to the store of long-known historical and morphological evidence about Linnaeus classification of elephant *species* in a way that suddenly permitted (or even demanded) nomenclatural action. Next, we turn to the news reports of the research. Interestingly, these present a more sweeping and radical narrative about the implications of the molecular study for the naming and identity of elephant species. Whereas the original research offered new evidence of a *classificatory* error about a specimen that we agree Linnaeus made—but which we argue has little to no scientific or nomenclatural relevance since it simply reflected the state of knowledge at

[1] Here and elsewhere in this article, we differentiate between references to names of taxa and references to (concepts of) taxa by using quotation marks and italics, respectively.

Linnaeus's time—the (science) news media turned the error into a *nomenclatural* one about elephant species. We argue that this is a kind of error that Linnaeus could not have *possibly* made. The news reports hold Linnaeus accountable for an error on the basis of practices and standards of naming biological taxa—practices that affect how one establishes and tracks the identity of taxa in the face of taxonomic revisions—that he did not subscribe to and could not have been familiar with.

However, despite being false, we think that the accusation of Linnaeus can nevertheless serve an instrumental role in raising unaddressed philosophical questions about nomenclatural practice and the identity of taxa. For, if Linnaeus did not subscribe to the contemporary methods and standards to adjudicate on the nominal identity and non-identity of biological taxa, what other practices did he follow and how do we make sense of these? In the second part of this paper (from Sect. 5 onwards), we answer this question by turning to an additional case study in Linnaean taxonomy. We trace Linnaeus's revisions of the plant genera *Erinus* and *Buchnera* to demonstrate that his practices of naming and (re)classifying embody conditions for establishing and tracking taxonomic identity that differ markedly from those that taxonomists use today. We conclude from this case that the very possibility of being wrong in any stricter sense about the identity established by using a particular name to designate a taxonomic group is a post-Linnaean phenomenon.

2 An elephant in a jar…

We begin by taking a closer look at the research that formed the basis for the study of Linnaeus and elephants that prompted our interest. For a start, it is worth noting that the research team behind the publication that attracted such an unusual amount of media attention was exceptionally diverse. Besides including protein chemists and ancient-DNA experts, it drew on the expertise of a mammal taxonomist, an art historian, and a historian of science, among others.

Their story starts at the Swedish Museum of Natural History in Stockholm, whose collections hold an elephant foetus that Linnaeus mentioned when he coined the name "Elephas maximus" in the tenth edition of the *Systema naturae* (Linnaeus 1758, vol. i, p. 33). The Swedish King Adolf Fredrik and Queen Lovisa Ulrika had acquired "the little miniature elephant" in 1752 through an auction sale of the collection of the Dutch apothecary Albertus Seba (1665–1736). Linnaeus had probably seen this specimen when he was tasked between 1751 and 1754 with cataloguing the collections of the royal couple. And perhaps that would have been a reunion, since it is possible that Linnaeus first saw the elephant on one of his visits to Seba's cabinet of natural curiosities during his stay in Holland between 1735 and 1738. We know that the elephant foetus was already in Seba's possession by then, since it featured prominently on a plate in the first volume of his *Thesaurus*, from 1734 (see Fig. 1).[2]

[2] Linnaeus later stated in his account of the King's collection that this specimen "was the very same as the one delineated by Seba" (Linnaeus 1764a, p. 6). Curiously, a similar remark is missing in the entry for the elephant in the first edition of this catalogue, which appeared in Swedish and Latin in 1754 and only mentions a few elephant teeth in the King's collection (cf. Linnaeus 1754, p. 11). That Linnaeus had heard of the preserved foetus shortly after its arrival in Stockholm is certain from a letter he sent from

Since the foetus had been stored in a glass jar with ethanol for over 250 years, it had degraded considerably by the time the researchers opened the jar for inspection. Most of its internal organs had vanished. Fortunately, a radiograph of the foetus showed that some of its bones and internal structure was still present and intact. This permitted the team to sample small pieces of cartilaginous rib and throat, which they could subject to ancient DNA (aDNA) and protein analyses using the latest high-throughput sequencing techniques. A comparison with known sequences from several specimens of *Loxodonta africana* and *Elephas maximus* showed that at all relevant diagnostic sites the peptides clearly corresponded to those of *Loxodonta africana*, the African (bush) elephant (Cappellini et al. 2014).

This discovery about Seba's specimen was seen as significant in the light of the rules of taxonomic naming and reference that contemporary zoologists subscribe to. The International Code of Zoological Nomenclature (henceforth: the Code) arbitrarily fixes the starting date of zoological nomenclature to 1 January, 1758, on which the tenth edition of Linnaeus's *Systema naturae* is deemed to have been published (ICZN 1999, Art. 3.1). Through a complex web of definitions and rules, the Code details relations between specimens, species names, and species taxa that are supposed to hold since that date. One important aspect of this is the "typification" of names. Ideally, a species name that was introduced on or after January 1, 1758, is anchored in a particular specimen—its "name-bearing type"—by the author who introduced the name. Henceforth, the name will refer, through this particular specimen, to the species taxon that includes it, "no matter how the boundaries of [the species] taxon may vary in the opinion of zoologists" (ICZN 1999, Art 61.1). In the absence of an explicit designation of a specimen as the name-bearing type by the author of the name, all of the specimens that he or she can be taken to have used as a basis for introducing the name are considered to be name-bearing "syntypes" (ICZN 1999, Art. 72.1). Since Linnaeus, when coining the name "Elephas maximus," cited the plate from Seba's *Thesaurus* that showed the elephant foetus that later became part of the royal collections in Stockholm, one can infer that this specimen belongs to the syntypes for this name (Linnaeus 1758, p. 33; cf. Seba 1734–1765, Vol 1, pl. cxi, Fig. 1).

This, however, is in tension with the findings of Cappellini et al. (2014). Instead of being a member of the species that we recognize as *Elephas maximus* today, the elephant foetus clearly belongs to *Loxodonta africana*. In other words, it turns out that the syntype material linked to the name "Elephas maximus" was composite: it included specimens from what we currently take to be different species, even different genera (Cappellini et al. 2014, p. 230). The Code describes what can be done to avoid such confusions by retrospectively selecting a single specimen—a so-called

Footnote 2 (continued)
Uppsala to his friend Abraham Bäck in Stockholm on May 18, 1753: "I am delighted from the bottom of my heart that the little miniature elephant has safely arrived. If it cost a lot, it will taste well. He is surely as curious as a diamond" (Linnaeus to Bäck, 18 May 1753, The Linnaean Correspondence, L1584, URL = http://urn.kb.se/resolve?urn=urn:nbn:se:alvin:portal:record-225072 accessed 21 Aug 2020; translations, if not indicated otherwise, are our own). Independent evidence that the specimen indeed came from Seba's collection is provided by Boeseman (1970, p. 182) and Cappellini et al. (2014, Suppl. 2).

Fig. 1 Plate CXI from the first volume of Seba's *Locupletissimi rerum naturalium thesauri*, Amsterdam: Apud Janssonio-Waesbergios, & J. Wetstenium, & Gul. Smith, 1734. The title of the accompanying description of Fig. 1 on this plate reads "Fœtus Elephantis Africani ineditus" and mentions that the specimen was still in possession of the Dutch West India Company, which stored it in a jar with alcohol. Image courtesy of Biodiversity Heritage Library, http://biodiversitylibrary.org/page/41047802

"lectotype"—from among the syntypes and designating it as the singular name-bearer for the species name (ICZN 1999, Art. 74.1). Since a lectotype is a single specimen rather than a (syntypic) series of specimens, it excludes the possibility of future confusions about the identity of the species that may arise from composite type material.[3] Cappellini et al. followed this recommendation of the Code. After an "[e]xhaustive examination of references cited by Linnaeus (1758)," they selected a lectotype from the syntypes cited by Linnaeus, keeping in mind that the Code recommends that the choice "should give great weight to accepted usage" of names (ICZN 1999, Recommendation 74A; cf. Cappellini et al. 2014, p. 230). That is, in order not to upset past and current uses of the name "Elephas maximus" for the Asian elephant species, they needed to be sure that the lectotype-to-be was indisputably an exemplar of this species.

[3] That is, unless the specimen that is selected as lectotype turns out not to have been a single specimen, but a mix of material from different specimens. We will ignore these and many other complexities of naming that are considered in great detail by the Code but are not pertinent to the cases we discuss.

At this point, we enter a detective story. Cappellini et al. noted that one of the other references that appeared in Linnaeus's description of *Elephas maximus* was to a work by John Ray (1627–1705): the *Synopsis Methodica Animalium Quadrupedum et Serpentini Generis* (Linnaeus 1758, p. 33; cf. Ray 1693, pp. 131–142).[4] In July 1664, Ray and his travel companion Philip Skippon had travelled to Florence on a grand tour through western Europe. In his account of the tour, Ray mentioned an elephant specimen he had inspected in Florence, which he later described in detail in *Synopsis* (Ray 1673, p. 334, 1693, pp. 132–134). Ray's description corresponds to an independent account of the same specimen by Skippon (1732, pp. 638–639). Ray's and Skippon's detailed remarks about the specimen's morphology and anatomy—which included references to its wooden replacement-ribs and toes—enabled Cappellini et al. to match the description to a specimen that is still on display at the Natural History Museum of the University of Florence (for a detailed analysis and translations from Ray's Latin account, see Supplement 4 of Cappellini et al. 2014).

An aDNA analysis of this specimen showed that it was very likely an Asian elephant, and this was confirmed from another fascinating angle. It had previously been suspected that the Florentine skeleton belonged to a well-known itinerant elephant that had been described as performing tricks in front of large audiences all over Europe. In 1633, this three-year old elephant had been shipped from Ceylon (Sri Lanka) to the Netherlands by the Dutch East India Company at the request of *stadtholder* Frederik Henrik. It changed ownership a few times before the young elephant was bought by a certain Cornelis van Groenevelt in 1637, who named it "Hansken." Van Groenevelt reportedly taught Hansen some 36 different tricks, including how to use her trunk to pick up a coin, draw a sword, and shoot a rifle. He made handsome money by taking Hansken on travels across Europe, where she performed for large audiences at fairs and entertained the rich and famous at private parties. Among the many who were enthralled by this large, mysterious land animal was Rembrandt van Rijn, who depicted Hansken in several charcoal sketches. The Italian draughtsman Stefano della Bella also drew Hansken several times, including on the day she died from an infection, in Florence in 1655 (Cappellini et al. 2014, Suppl. 7).

The correspondences between the reported life histories and anatomical features of Hansken and the Florence specimen are striking. Both were reported to have been born in 1630, to have died in 1655, and to have weighed around 6660 kilograms. In addition, an analysis of the molar wear of the Florence specimen confirmed that it must have been around 25 years old when it died. Finally, Skippon mentioned in his travel diary that the elephant, whose remains he had seen on display in Florence, had reportedly been able to draw a sword with its trunk (Skippon 1732, p. 638). Cappellini et al. concluded that there is little room for doubt: it is overwhelmingly likely (a) that Hansken and the Florence specimen are one and the same historical individual, (b) that it is an Asian elephant specimen from Ceylon (which Linnaeus stated was the original home of elephants), and (c) that Linnaeus, by citing Ray, referred to this

[4] Linnaeus's reference states page 123, probably an error.

specimen, among others, when coining the name "Elephas maximus" in 1758.[5] It was therefore especially suitable to serve as a lectotype for this name (Cappellini et al. 2014, p. 230; see also Gentry et al. 2014).

3 …and the elephant in the room

It can hardly surprise that *Nature* and other media picked up on the research described in the previous section. Here was a compelling story about an iconic animal that combined state-of-the art molecular science with research into natural history and even art history. However, its obvious attractions as an interdisciplinary detective story notwithstanding, it is worth considering what exactly made the research of Cappellini et al. scientifically significant. What sort of error did their examination of the elephant foetus reveal, and what were its implications? This question, we suggest, points to another "elephant in the room": the question whether a genuine discovery was made by Cappellini et al. and whether a live issue was addressed by it.

To start with, we note that Cappellini et al. present their molecular research as a means to "*further* resolve" (our emphasis) the identity of the Seba specimen. It had indeed already been presumed for a considerable time that this specimen was of African provenance. In fact, as Cappellini et al. acknowledge, Seba himself mentioned that the "unborn foetus" was of an "African elephant" in the description accompanying the plate (Seba 1734–1765, Vol 1, p. 175: *Foetus Elephantis Africani ineditus*). The prospectus of Seba's auction sale from 1752 also described the foetus as "a particularly beautiful and rare unborn elephant from Africa" (reproduced in Boeseman 1970, pl. 2: *Een extra fraaije ongemeene ongeboren Oliphant uit Africa*).[6] Finally, Cappellini et al. point out that nineteenth- and early twentieth-century curators at the Natural History Museum in Stockholm "believed that, although it was labelled *Elephas maximus*, the specimen was identifiable as an African elephant" (Cappellini et al. 2014, p. 223; cf. Lönnberg 1905, p. 323). The true identity of Seba's elephant seems to have been part of the institutional folklore of the Natural History Museum in Stockholm, which is confirmed by what the lead

[5] Further evidence has been acquired since Cappellini et al. published their study. Roscam Abbing (2016, p. 118) has uncovered the receipt of the sale of the skin and skeleton of the elephant to the Florentian museum where it was first displayed. The receipt clearly mentions "capitano Cornelio Vangroenpelt" as the former owner.

[6] It is therefore quite likely that Linnaeus was fully aware that the specimen was from Africa. It is interesting in this regard to observe that in 1754 Linnaeus used the name *Elephas indicus* (Linnaeus 1754, p. 11), referring to the description from John Ray and a set of teeth from an Indian elephant in the King's collections. It is possible that Linnaeus changed the specific epithet to *maximus* in *Systema naturae* (1758) after he had seen the Seba specimen and had heard (or read) that it came from Africa. *Systema naturae* includes additional references to descriptions of what clearly were African elephants which may also have motivated Linnaeus to change the name. Another possibility, mentioned by Richard Lydekker (1916), is that when Linnaeus changed the name he had specimens of a Bengal subspecies of the Asian elephant in mind, which had been imported to Sri Lanka. The males of this subspecies have large tusks, as opposed to the "insignificant" tusks of elephants that were native to Sri Lanka (p. 82).

taxonomist on the team, Anthea Gentry, told *Nature*: "Questions about the fetus still lingered" when she began cataloguing material from the royal collections in the late 1990s (Callaway 2013). Although the final paper downplayed this aspect by noting that "morphological comparison of foetal organisms can be challenging" (Cappellini et al. 2014, p. 227), Seba's specimen seems to have been readily recognizable as an African bush elephant to mammalogists. Thus, when Gentry showed a photograph of the Seba specimen to her husband, a mammal palaeontologist, he immediately confirmed her doubts by stating categorically: "It's an African elephant" (Callaway 2013).

Although none of this contradicts Cappellini et al.'s claim that molecular data could be used to further strengthen the evidence of the African origins of this particular elephant *specimen*, it does raise the question whether this additional evidence was at all decisive in resolving a question about the naming and identity of an elephant *species*. That is to say, it is not evident that molecular evidence about the provenance of Seba's specimen helped to establish that Linnaeus's conception of *Elephas maximus* was problematic in a way that permitted, facilitated, or demanded nomenclatural action. Interestingly, the historical research that Cappellini et al. themselves carried out supports this.

For a start, their historical research on Linnaeus's description of *Elephas maximus* shows that several of the other syntypes from Linnaeus's description were clearly African elephants. Conrad Gessner (1516–1565), Ulisse Aldrovandi (1522–1605), and John Jonston (1603–1675) all had given elaborate accounts (relying on both ancient and contemporary sources) of elephants that live and breed in Africa, from Ethiopia to Libya and Mauretania, and to the inlands of (what is now) Tanzania.[7] Illustrations in Gessner and Aldrovandi, moreover, clearly show African elephants (Gessner 1551, p. 410; Aldrovandi 1616, p. 465; see Cappellini et al. 2014, S. 1 for reproductions). Hence, even if the Seba specimen had been lost, it would still be clear that Linnaeus had a conception of the species he named *Elephas maximus* that has to be considered composite by current standards.

Secondly, the composite nature of Linnaeus conception of *Elephas maximus* is hardly any news to mammal taxonomists. In 1942, in a historical résumé of the nomenclature of extant elephants, Henry Fairfield Osborn already concluded that Linnaeus must have thought of the Asian and African elephants as a single species. Pondering over the question why Linnaeus in 1758 introduced the name "Elephas maximus" as a substitute for the name he had used until then, "Elephas indicus," Osborn asked: "Is not the explanation found in his belief that the Indian and the

[7] Some of these might have been Asian elephants that had been brought to Africa, but likely not all of them. Gessner, who provided the most extended description, noted important differences in size and form and hinted at the existence of two "species" (Gessner 1551, p. 411: *duo eorum genera sunt*). The name change Linnaeus introduced in 1758, from *E. indicus* to *E. maximus*, was perhaps also due to his reading of Gessner, who quoted Pliny as stating that the elephant "is the largest terrestrial animal" (Gessner 1551, p. 412: *Terrestrium (inquit Plinius) maximum animal est elephas*). In *Systema naturae*, Linnaeus wrote that the elepant is the "largest quadruped" (Linnaeus 1758, p. 33: *Maximum quadrupes*).

African elephant were of the same species?" (Osborn 1942, vol. ii, p. 1310). He contacted Einar Lönnberg, head of the Vertebrate Department at the Swedish Royal Museum of Natural History, who replied that Linnaeus indeed referred to both Asian and African elephants in his description from 1758. Notably, Lönnberg already mentioned that Linnaeus's reference to the elephant specimen from Seba's collection concerned "an African Elephant, probably from West Africa". Osborn also contacted Charles Davies Sherborn—"the greatest living authority on generic and specific names,"—who similarly confirmed his hunch: "Linnaeus clearly spatchcocks the whole lot into one species both for Asia and Africa and considers that there is only one Elephant. And as that beast has been more or less of a domesticated animal since before Alexander the Great, I don't think Linnaeus was far wrong in such a guess." Osborn concluded accordingly that when Linnaeus introduced the name "Elephas maximus," he clearly "had both the African and the Indian elephant in mind, apparently in the belief that they constituted a single created species" (Osborn 1942, vol. ii, pp. 1309–1311).

Finally, as both Lönnberg and Sherborn pointed out, it is quite clear that Linnaeus must have thought that *Elephas maximus* originated in India or Sri Lanka. Lönnberg adds to this that the reason why Linnaeus penned "Habitat in Zeylonæ" in his entry for *Elephas maximus* "is probably that he quotes Rajus [Ray] in the first rank" (Osborn 1942, vol. ii, p. 1310), whom he had earlier cited in his account of *Elephas indicus* (Linnaeus 1754, p. 11). Osborn follows Lönnberg and Sherborn in claiming that "[t]here is little doubt" that the species that was foremost on Linnaeus's mind in describing *Elephas maximus* "was a domesticated elephant from the island of Ceylon". This, then, inclines him to the "technical opinion that Linnaeus's *type*, both of his 1754 description of *Elephas indicus* and of his 1758 description of *Elephas maximus*, was the Ceylon animal."[8]

Cappellini et al. might counter that even if various geographical and historical sources had already been sufficient for earlier taxonomists to establish that the Linnaean syntypes were a mixture of African and Asian elephant specimens, the molecular research nevertheless helped to resolve which of the specimens Linnaeus listed belonged to which species. By showing with high confidence that the Seba specimen was an African specimen, the molecular results thus made clear that it could not be elected as the lectotype for the name "Elephas maximus." But again, we argue it is not at all clear that the molecular study provided any additional grounds for this, over and above those based on the geographic and morphological evidence. In order to exclude a syntype as a candidate for lectotypification of "Elephas maximus," it is not required to establish incontrovertibly that it is not an Asian specimen. Instead, it calls for making the comparative case that other specimens are *better* candidates for lectotypification. In case of the Seba specimen, available morphological

[8] Osborn (1942, vol. ii, p. 1323). The reason why Osborn refers to his impression about Linnaeus's type as a "technical opinion" is that he understood full well that Linnaeus did not actually *designate* types, in the modern sense, as anchors for names. In a footnote on the same page, he adds: "In this early stage of zoology no one dreamed of selecting any particular specimen and designating it as the type" (Osborn 1942, vol. ii, p. 1323, n. 1).

and geographical information had already made it abundantly clear that it was an unsuitable candidate for lectotypification compared to the Florence specimen (i.e. Hansken). The molecular data did not help to "further resolve" this choice, which had already been clearly defended by Sherborn and Osborn without recourse to such data.

It is not surprising that several taxonomists responded to the Cappellini et al. study with mixed feelings. On the one hand, it did taxonomy a service by drawing public attention to a field that is often struggling for recognition. On the other hand, it presented a skewed image of how to deal with nomenclatural issues:

> In our opinion, this paper sends a wrong message to taxonomists and to the biological community as a whole. This message is that "problems", that had remained unsolved for centuries because taxonomists were only relying on morphology, can now be solved thanks to modern molecular techniques, and that this will at last salvage taxonomy from its old-fashioned techniques and thinking, to propel it into modernity… In the end, the main purpose of the paper by Cappellini et al. (2014) seems to have been to make the promotion of modern molecular techniques more than to solve a "phantom" nomenclatural problem. (Dubois et al. 2014, p. 58)

It is perhaps a bit too brusque to characterize Cappellini et al.'s act of lectotypification as a solution to a "phantom problem," merely because there has largely been agreement among mammalogists about the naming and classification of Asian and African elephants. By selecting a lectotype for a species name that had not yet been properly "anchored" in a single specimen, latent ambiguity about the application of "Elephas maximus" that might have become relevant in a future state of taxonomic research was removed once and for all. That said, the critics did have a point in taking issue with the presentation of molecular evidence as being key to identifying and resolving nomenclatural issues. Concerning the Florence specimen that ended up being selected as lectotype, Cappellini et al. themselves even further downplayed the value of their molecular research in their response to Dubois et al. Since the molecular data did not establish that this specimen came from Sri Lanka, Cappellini et al. dropped molecular considerations altogether, noting in their response that "[i]t is as certain as anything can be *from the written records of the past* that the elephant in the Natural History Museum of the University of Florence, now the *Elephas maximus* lectotype, came from Sri Lanka" (Gentry et al. 2014, p. 210; emphasis added).

4 Identity and error

As one might expect, the emphasis on the purported relevance of cutting-edge molecular techniques became amplified in the many news reports of the study. The subtitle of the *Nature* news feature read: "Molecular sleuths crack 300-year-old mystery over the identity of the Asian elephant type specimen" (Callaway 2013). But several news reports—again with *Nature* in the lead—also added another twist to the story that went beyond the conclusions from the scientific study.

We have seen that it has long been recognized that Linnaeus made a *classificatory* error by grouping two species into one. This error has nomenclatural consequences in the context of the type-based naming system taxonomists today rely on, but is not a *nomenclatural* error in itself. *Nature*'s Ewen Callaway, however, suggested that Linnaeus himself had made such an error, and that this error was enabled by his use of the type method:

> Linnaeus's system hinged on the concept of types—individuals that serve as the archetypes for a species, in much the same way that a platinum–iridium cylinder outside Paris defines the kilogram. And because Linnaeus was the one who came up with this system, which is still used by scientists today, he got to pick the type specimens … [Seba's] fetal elephant, the size of a well-fed cat, became the elephant's type specimen, included in *Systema naturae*. (Callaway 2013)

Callaway suggests that, against this background, Cappellini et al. raised the question "Could the pickled fetus he used as its archetype actually have been a different species?" which they answered with a resounding "yes" (Callaway 2013). This is a problematic interpretation of the study and its implications, for two reasons. First, as already indicated, and as also not implied by Cappellini et al., it is incorrect that Linnaeus was familiar with the type method as taxonomists use it today, not to say that he introduced it. He did not anchor the application of the name "Elephas maximus" to a single specimen, which would thereby be stipulated to belong to *Elephas maximus*. Instead, we have seen that he cited a series of sources, which in turn were based on a series of specimens, without selecting any of these as the privileged name-bearer for the species name. Second, even *if* Linnaeus had complied with the type method, it would have been impossible for later taxonomists to discover that the type belongs to another species than the one for which it serves as name-bearer. After all, by virtue of designating a specimen as the type of a species name one is naming the species to which the specimen *in fact* belongs, regardless of one's fallible knowledge about the location and circumscription of the named species (see Witteveen 2015). Hence, if Linnaeus were to have designated Seba's foetal elephant as the type for the name "Elephas maximus," he could not have possibly been wrong about it being a member of the species *Elephas maximus*. Instead, we would have to say that later taxonomists, in using this name for the Asian elephants, failed to recognize that the name actually referred to the African species.

In short, Callaway both misattributes the type method to Linnaeus and misinterprets the consequences that using this method would have had for taxonomic identities. At the same time, these common misperceptions raise new questions of historical and philosophical interest. For, if Linnaeus did not use the type method to apply names to taxa and establish their identity in the face of revisions, how else did he do this? In other words, what can Linnaeus's own practices of naming and classifying tell us about his thinking about taxonomic identity? And what, if anything, would it have meant for him to be wrong about such identities? In the remainder of this paper we show that his practices of naming, combined with his central position in a network of exchange, isolated him from

the very possibility of making the kinds of errors about naming and identity that taxonomists can commit today, while exposing him to other kinds of errors.

5 Linnaeus's paper tools: labels and containers

If one approaches Linnaeus's classifications of plants and animals with the eyes of a modern taxonomist, there are more surprises to be encountered than the occasional conflation—or "spatchcocking," as Sherborn put it—of distinct species, as in the case of the African and Asian elephant. "Elephas" as a generic name entered Linnaeus's classifications of the animal kingdom from early on, and initially, Linnaeus seems to have been unsure whether to include the rhinoceros in *Elephas* or not. In a first manuscript outline of the animal kingdom, entitled "Pan europæus" and probably compiled in 1734 when Linnaeus stayed in the mining town of Falun, Linnaeus listed *Rhinoceros* as a distinct genus below *Elephas*, but included it later on in the same manuscript under the genus *Elephas* as a second species, named "Elephas naso cornigero" (Elephant with horn-bearing nose; see Fig. 2). Linnaeus's ambivalence translated into print a year later, in his famous *Systema naturae*: by placing a question mark before "Rhinoceros", listed under the name "Elephas" Linnaeus expressed doubt whether the animal actually belonged under this genus (Linnaeus 1735, unpag.). In subsequent editions, up to the fifth published in 1747, he then appears to have made up his mind, listing "Elephas naso cornigero" unambiguously under "Elephas" (Linnaeus 1740a, p. 40, 1740b, p. 46, 1744, p. 69, 1747, p. 48). It should be noted that, at the time, Linnaeus was not alone among European naturalists with this taxonomic judgement. In a letter to Linnaeus written in December 1743, for example, the Dutch naturalist Jan Frederik Gronovius told his friend how a female exemplar of *Elephas naso cornigero* had been paraded through the streets of Leyden.[9]

Yet this consensus, probably reached by his own publications, did not keep Linnaeus from changing his mind. In the sixth edition of *Systema naturae*, published in 1748, he recognized *Rhinoceros* as a distinct genus, listed together with *Elephas* under the order *Jumenta* (derived from *jumentum*, Latin for "beasts of burden"). The genus now included two named species within its bounds, the "rhinoceros with one cone-shaped horn" and the "rhinoceros with two wedge-shaped horns" (Linnaeus 1748, p. 11: *Rhinoceros cornu unico conico* and *Rhinoceros cornibus duobus cuneiformibus*).[10] A manuscript based on this edition, and apparently put together in preparation of the tenth edition, makes the separation palpable: the descriptions it contains of the two genera occupy the recto and verso side of a paper slip.[11] The

[9] Johan Frederik Gronovius to Linnaeus, Dec 12, 1743, The Linnaean Correspondence, L0518, URL = http://urn.kb.se/resolve?urn=urn:nbn:se:alvin:portal:record-223445, accessed 23 Aug 2020: "Conspiciendus datur in hac urbe Elephas naso cornigero sive Rhinoceros, animal ferox et horrendum; sexus feminei est."

[10] Linnaeus's second, two-horned species was probably based on a fake specimen of *Rhinoceros unicornis*; see Rookmaaker (2005, p. 369).

[11] Carl Linnaeus, "Jumenta", Linnaean manuscripts, GB-110/LM/LP/ZOO/2/1/1/5, f. 4v, Linnean Society of London, Library and Archives.

taxonomic gap separating the two genera would widen even further with subsequent print editions of *Systema naturae*: While *Elephas* remained positioned in the second order of the mammalia (now called "Bruta" instead of "Jumenta"), the tenth edition moved *Rhinoceros* to the fourth order (named *Glires*, Latin for dormice, and indeed including mice and other rodents alongside the rhino), and the twelfth to the fifth order (called "Bellua", and also including the horse, the pig and the hippopotamus; Linnaeus 1758, vol. 1, pp. 33, 56, 1766–1768, vol. 1, pp. 48, 104).

The ease with which Linnaeus first allocated the rhino within the genus *Elephas*, then accorded it the status of a separate genus, and finally moved it further and further away from its original neighbour, illustrates an important aspect of his self-styled "reform" of natural history. Rather than postponing the publication of a tentative classification until it could be confirmed by collecting and investigating further evidence, Linnaeus opted for rapid publication followed by frequent new editions to update and correct previous ones on the basis of new information he received from correspondents and travelling students (Dietz 2012, 2017; Müller-Wille and Scharf 2012). This strategy of serial and collaborative publication had obvious drawbacks that we see manifested in the correspondence with Gronovius about the rhinoceros: it limited the shelf life of any given name or classification and could create disparities in the usage of taxon names by different naturalists over time. On the other hand, it meant that one never needed to wait long for the latest and most reliable information to become publicly available. But most importantly, the expectation that a published work would in time be followed by an amended and expanded edition amounted to an open invitation to his readers to become involved in the Linnaean project by sharing their own taxonomic information. Linnaeus actively solicited expert colleagues to point out errors of identification and classification, allowing him to resolve these in future editions. As Bettina Dietz has pointed out, this resulted in Linnaeus becoming a hub in an extensive network of correspondents with whom he effectively "co-produced" his works (Dietz 2012). Apart from facilitating the resolution of classificatory mishaps, the networked nature of Linnaean taxonomy also offered a means to detect and resolve nomenclatural problems. Dietz (2019) describes how plant identification was aided by the publication of lists of synonyms, which constituted "networked names" that were constantly corrected and revised based on the input from the botanical community. The iterative and collaborative nature of publication thus shaped a key epistemic feature of eighteenth-century taxonomy (botany in particular).

This social-epistemic architecture in turn relied on a material architecture for channelling and ordering the flow of information about changes and additions that would eventually need to be incorporated into new editions. In order to stem the information overload that he had helped to create, Linnaeus relied on a range paper tools and technologies. Numbered lists, interleaved copies of his own works, filing systems, and index cards were among the devices he introduced to facilitate the storage, retrieval, and reorganisation of information in the process of revision. Recent studies of Linnaeus's deployment of these paper tools have yielded a much-improved understanding of his day-to-day epistemic practices of information management (Charmantier and Müller-Wille 2014; Müller-Wille and Charmantier 2012). Here, however, we will argue that in addition to providing insight into the

a

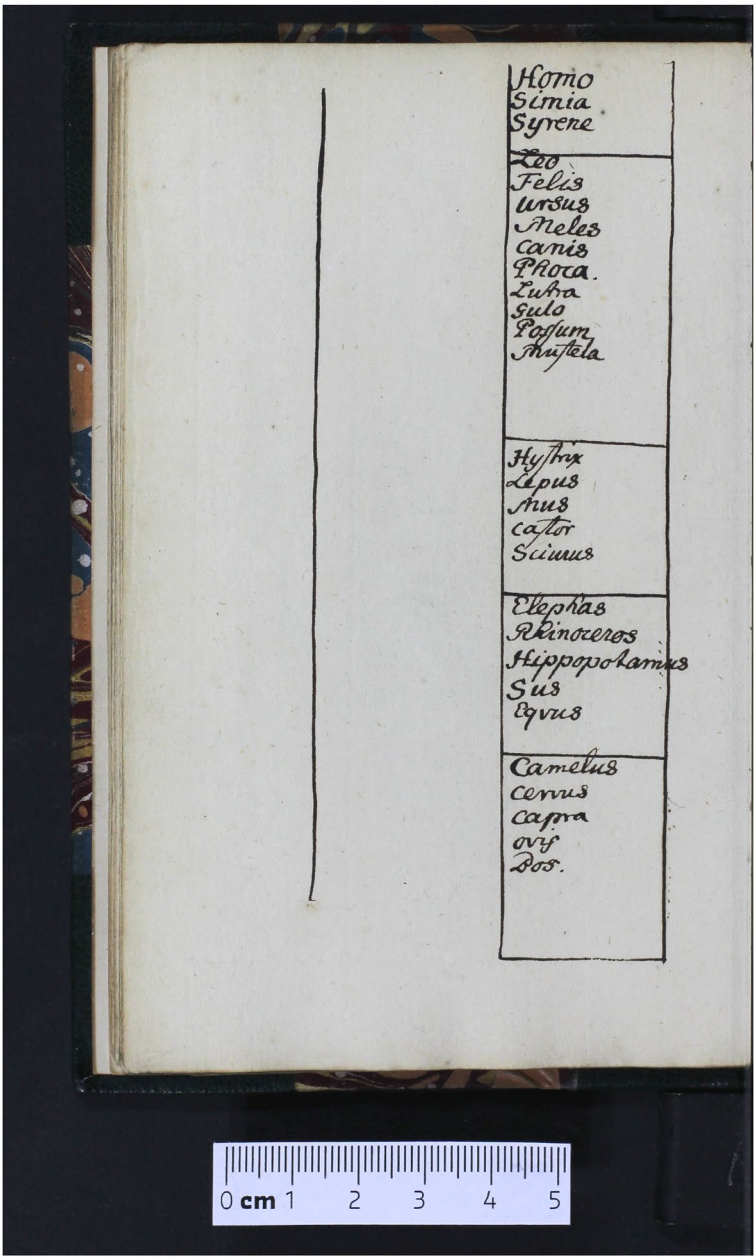


Fig. 2 Two pages listing the rhinoceros from Carl Linnaeus, "Pan europæus", 1734, Linnaean manuscripts, GB-110/LM/LP/ZOO/3/6, p. 14 and 73. **a** Tabular listing of animal genera (URL = http://linnean-online.org/159935/). **b** Page with species entries for the genus Elephas, on which "Rhinoceros" is listed as a synonym of the second species named "Elephas naso cornigero", with a reference to a plate in John Jonston's *Historia naturalis de quadrupedibus* (Amsterdam 1657) (URL = http://linnean-online.org/159994/). With kind permission of the Linnean Society of London, Library and Archives, www.linnean.org

b

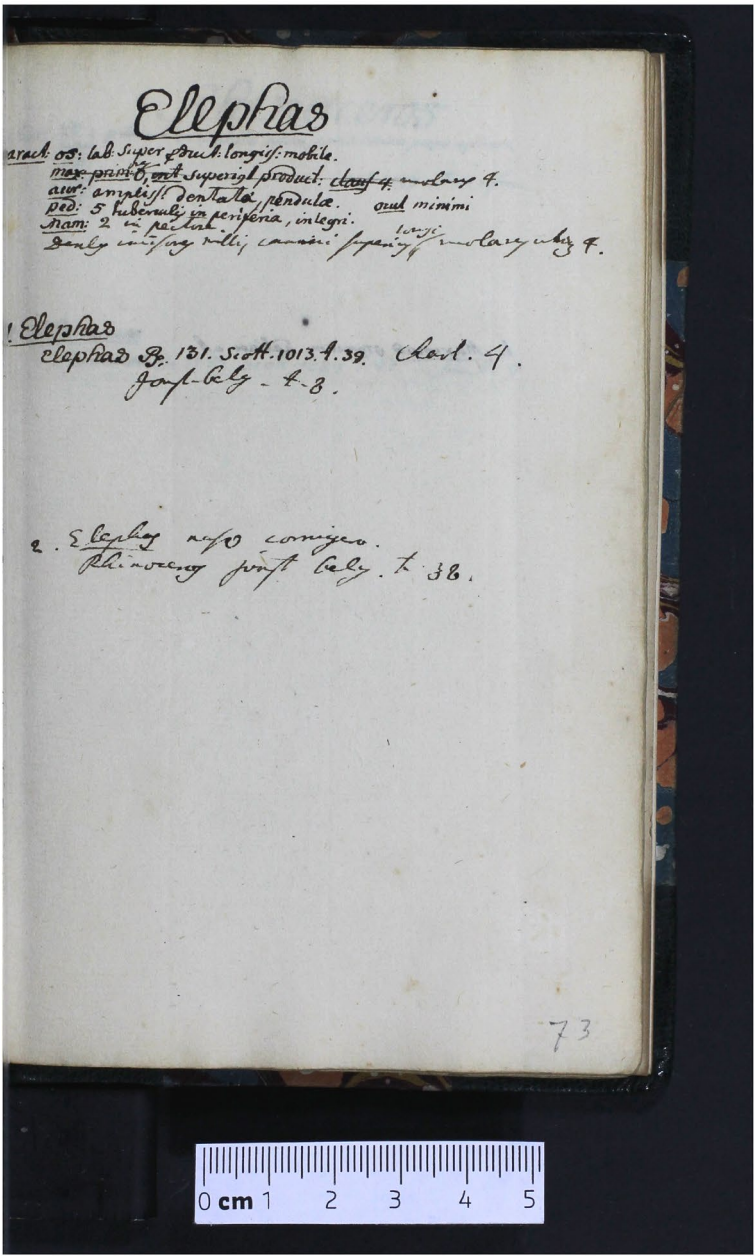


Fig. 2 (continued)

epistemic practices of updating classifications and resolving synonyms, Linnaeus's use of paper tools also provide insight into his *referential* practices. Indeed, we will suggest that Linnaeus's way of handling names on paper has consequences for their referential relation to things in nature. His treatment of names through his paper tools implies a tacit philosophy of reference that seems distinctly peculiar from the viewpoint of contemporary taxonomic practice.

In the remainder of this section we will first present a key paper tool—the "paper box"—that served Linnaeus's information management needs. Next, we will demonstrate the surprising consequences that using this tool could have for the relation between taxon names and their referents between different revised editions of taxonomic works. This will require us to leave elephants and rhinos behind for a while and turn to a less glamorous botanical example.

Linnaeus paper-based practices of revision demonstrate a treatment of genera and higher taxa as containers, or "paper boxes" in which species-level information could be stored. Whenever he began a new manuscript, he would typically pen a genus name at the top of a blank page or below a set of horizonal lines that served as a divider between genera. In the course of his work on the manuscript, he then gradually filled these blank spaces with species names and descriptions that he had collected from other botanical works or had received through correspondence, basically drawing up a list of synonyms as discussed by Dietz (2019). After publication—and in preparation for the inevitable next edition—he often used an interleaved copy of the published work as a means of creating additional blank spaces next to the formatted spaces of printed genera. Again, one can see Linnaeus use these spaces to drop new species into genera and move species between genera. Whenever the time was ripe for a new edition, he would aggregate his annotations and copy them into a new manuscript (see Müller-Wille and Charmantier 2012 for a more detailed discussion).

This practice of adding and removing species to and from boxes on paper—also exemplified by Linnaeus's treatment of the rhinoceros discussed above—resembles a filing system, much like Linnaeus's own herbarium. Rather than gluing his plant specimens into bound volumes, as was the custom, Linnaeus kept them on loose sheets that he collected into folders, which in turn were stored in size-adjustable compartments of a purpose-built cabinet (Müller-Wille 2006). Linnaeus filled his manuscripts as he filled his herbarium, and, by extension, his zoological collections: on a day-to-day, piecemeal basis, as he encountered relevant information through reading, correspondence, or specimens he received. And like the folders and specimen sheets in his herbarium, his manuscripts and printed works could be freely reorganized when changing taxonomic insights called for it.

This potential for limitless rearrangement and reorganisation of the contents of taxa has wider implications for the relation between names and their referents. In an information system like the one used by Linnaeus, names first and foremost serve the function of labelling "spaces" dedicated to containing information and do not by themselves contain such information. This point is best illustrated by turning to a particular example. The case of the genus *Buchnera*, which Linnaeus revised extensively several times, presents an example of the extreme consequences—from today's point of view—that his paper-based practices could have for the relation between a taxonomic concept (in this case a genus), its corresponding name, and

the elements that were grouped under them and to which they referred (in this case, species). It turns out in the case of *Buchnera*, that Linnaeus sometimes continued to use the same name despite the fact that *all* the elements originally included had been removed from the taxon and replaced by a different set of elements. The identity of the named taxonomic concept over time, that is, was not secured by tying its meaning to *any* particular set of elements, be they species, or by extension, specimens. Instead, a revised description of a genus counted as a description of the same genus solely by virtue of the sustained relation between the name (or label) and the "container" with which it had been associated in earlier classifications, be it a folder in his herbarium or a delimited space on paper in an interleaved copy of his own works. The relation between name and container, that is, was grounded in prior classificatory practice, and not in any preconceived "order of things."[12]

Buchnera started as an afterthought. Linnaeus introduced this genus name in 1737 in the appendix of *Hortus Cliffortianus*, a work that described and classified the plants in the collection of the Dutch banker (and Linnaeus's patron) George Clifford. Below the new genus name, he listed two descriptions of African species (Linnaeus 1737c, p. 501). His discovery of this new genus came too late to be included in the first edition of *Genera plantarum* (1737a) or in a supplement to this work that was printed in the same year, the *Corollarium Generum Plantarum* (1737b). However, in his personal interleaved copy of the latter publication Linnaeus did add a handwritten description of the generic characters of *Buchnera* (see Fig. 3).[13] In addition, Linnaeus inserted the name "Buchnera" into the list of genera, arranged by orders and classes according to the sexual system, that preceded the volume. He positioned it in the class of Didynamia, right below the genus *Erinus* (see Fig. 4).[14] These annotations were clearly intended to be added to a future edition of *Genera plantarum*. When the second edition of *Genera plantarum* appeared in 1742, it included *Buchnera* immediately below *Erinus*, with a generic description that was virtually identical to the one Linnaeus had penned in his annotated copy of the *Corollarium* (Linnaeus 1742, p. 302).

In *Genera plantarum* Linnaeus only provided descriptions of generic characters and did not include lists of species that he attributed to each genus. However, we can tell from annotations he made in other works that his judgment about which species belonged to *Buchnera* was changing in the background. In one of his personal copies of *Hortus Cliffortianus*, Linnaeus listed synonyms in the form of a genus name followed by a short diagnostic phrase of African plant species copied from the works of other botanists that he apparently considered to refer to the same species of *Buchnera*, and next to his handwritten description of the generic character in the

[12] This is a reference to the title of Foucault (1974). Foucault's otherwise perceptive analysis of the "classical" episteme in terms of two-place relationships between "words" and "things" misses the fact that the classificatory tableaus that Linnaeus and other eighteenth-century naturalists designed were constantly revised by moving elements around, and that the only stable relation in the process was the relation between names and classes, not names and objects classified (Müller-Wille 2015).

[13] Carl Linnaeus, *Corollarium Genera Plantarum,* Leiden: Wishoff, 1737, Linnean Society of London, Library and Archives, call no. BL.49B, URL = http://linnean-online.org/120005/, image 59.

[14] Carl Linnaeus, *Corollarium Genera Plantarum,* Leiden: Wishoff, 1737, Linnean Society of London, Library and Archives, call no. BL.49C, URL = http://linnean-online.org/120006/, image 23.

Corollarium, he scribbled down similar diagnostic names for two further Peruvian species.[15] When in 1746 he started preparing a first draft of *Species plantarum*, he aggregated these marginal annotations and included them on a single new manuscript page serving as the container for *Buchnera*, and now containing four species, two "African" (*Habitat in Aethiopia*), and two "Peruvian" (*Habitat in Peru*; see Fig. 5).[16]

It would take another seven years from this first draft to the publication of the first edition of the *Species plantarum* (1753). In these years Linnaeus continued to add new species, merging others, and moving several of them between genera. The result of this can be seen in the printed edition of *Species plantarum*: all of the species that he had grouped under *Buchnera* until 1746 were moved to the adjacent genus of *Erinus* (Linnaeus 1753, vol. 2, p. 630; see Fig. 6). Under this genus Linnaeus now included *Erinus alpinus*, *E. africanus*, *E. peruvianus*, and *E. laciniatus*. The first one of these he had already described as a species of *Erinus* in *Hortus Cliffortianus*, the others were the transfers from *Buchnera*, with the two African species from *Hortus Cliffortianus* merged into one, and *E. peruvianus* and *E. laciniatus* being identical to the two Peruvian species he had manually added to the *Corollarium*.[17]

Linnaeus's reallocation of all the original species from *Buchnera* to *Erinus* is perhaps anticipated by some marks in his personal copy of the sixth edition of *Systema naturae*, which appeared in 1748. In this copy he drew inclined strokes through the name and diagnosis of *Buchnera* and added a bracket connecting *Buchnera* to *Erinus*, perhaps indicating that he was planning to merge the two genera (Fig. 7).[18] However, merging the genera is not what he ended up doing, for in *Species plantarum* he still listed the genus *Buchnera*, but with two new species assigned to it: *B. americana* and *B. asiatica*. In other words, in between the first draft and the publication of the first edition of *Species Plantarum*, Linnaeus "emptied out" the contents of *Buchnera* as he had conceived of it until then, only to "refill" the genus with two new species.

From a contemporary point of view, these operations seem perplexing. A modern taxonomist would judge that when all species that were initially attributed to a first genus are reassigned to a second genus, the former genus has thereby been folded into the latter. Yet Linnaeus's classification of *Buchnera* and *Erinus* defies this logic. His continued recognition of *Buchnera* as a distinct genus, but now with a set of entirely different species included in it, bespeaks a conception of genera as labelled

[15] Carl Linnaeus, *Hortus cliffortianus*, Amsterdam: s. n., 1737, Linnean Society of London, Library and Archives, call no. BL.1186, URL = http://linnean-online.org/120153/, image 479; Carl Linnaeus, *Corollarium Genera Plantarum,* Leiden: Wishoff, 1737, Linnean Society of London, Library and Archives, call no. BL.49B, URL = http://linnean-online.org/120005/, image 58.

[16] Carl Linnaeus, "Species plantarum", Linnean Society of London, Library and Archives, Linnaean manuscripts, GB-110/LM/LP/BOT/3/4/1, URL = http://linnean-online.org/61340/, image 805.

[17] Linnaeus (1753, p. 630). The first edition of *Species plantarum* was the first work in which Linnaeus consistently applied "trivial names" (binomials) to each species. The trivial names were added in the margin, and did not otherwise affect Linnaeus's diagnostic descriptions and synonymies, which can easily be traced to the earlier works and manuscripts mentioned above.

[18] Carl Linnaeus, *Systema naturae*, 6th ed., Stockholm: Kiesewetter, 1748, Linnean Society of London, Library and Archives, call no. BL.10, URL = http://linnean-online.org/119963/, image 240.

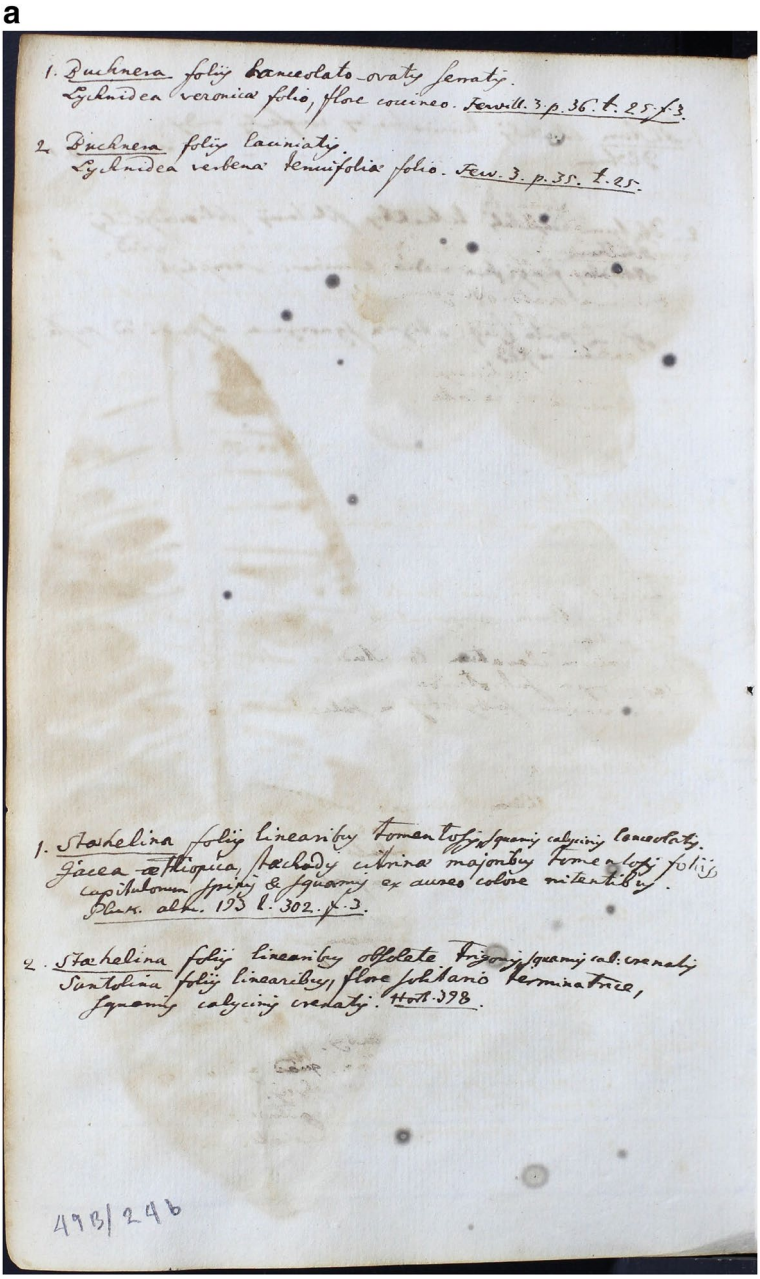


Fig. 3 Two entries for new species **a** placed against a handwritten description of the genus *Buchnera*) (URL = http://linnean-online.org/83811/) **b** from Carl Linnaeus's personal copy of *Corollarium Genera Plantarum,* Leiden: Wishoff, 1737 (URL = http://linnean-online.org/83812/). Linnean Society of London, Library and Archives, call no. BL.49B. With kind permission of the Linnean Society of London, Library and Archives, www.linnean.org

b

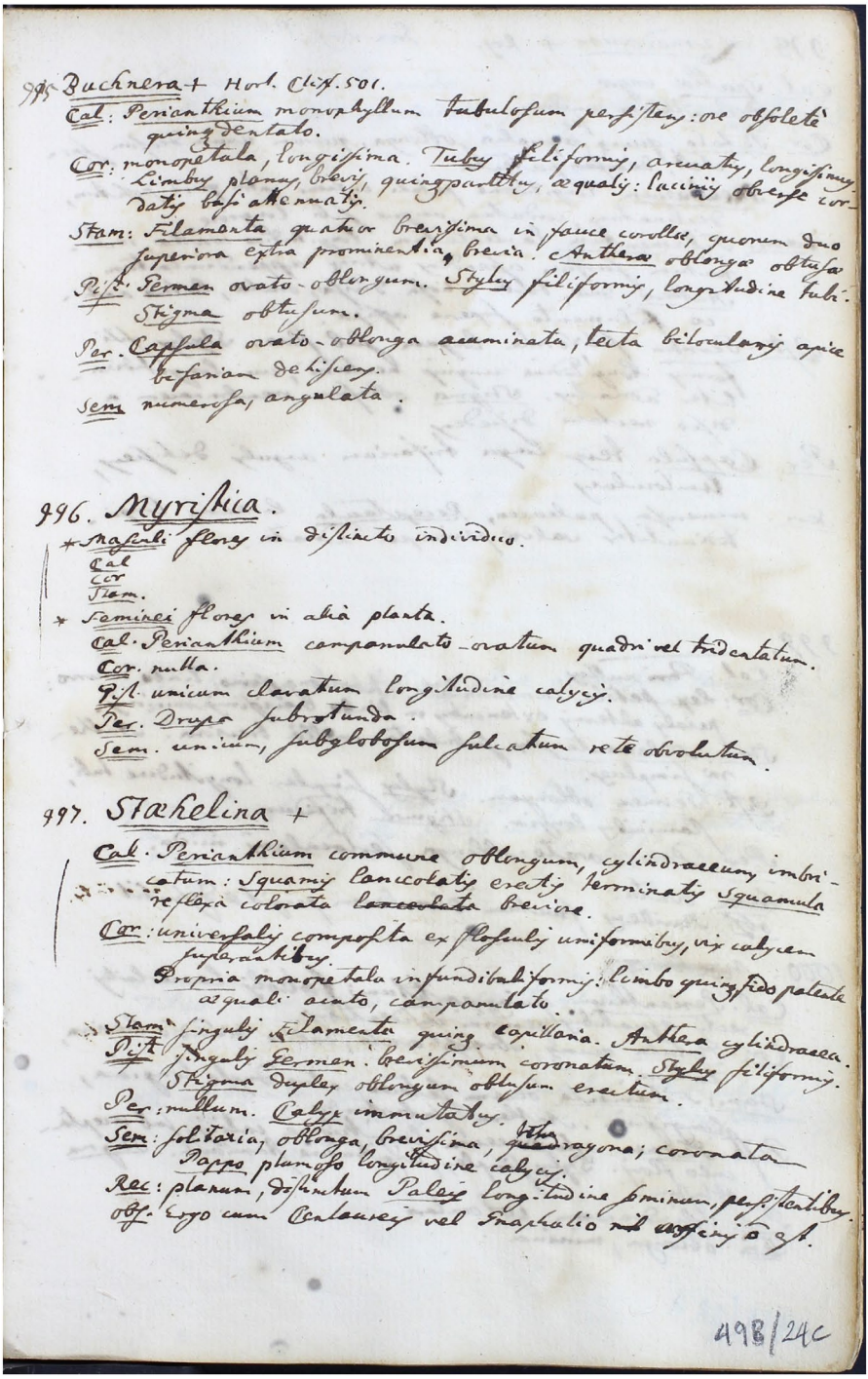


Fig. 3 (continued)

containers that can have an identity and existence that is independent from the species that are placed in them.

6 Linnaean metaphysics in action

As we mentioned earlier, we do not present the *Buchnera* case to argue that Linnaeus subscribed to an explicit philosophical view of how generic names and concepts related to their species contents, or, by extension, how species names and concepts related to specimens. Instead, we aim to suggest that Linnaeus's quotidian practices of taxonomic information management entailed a tacit view about identity criteria of taxa, the extreme consequences of which are illustrated by the *Buchnera* case. To borrow Daston's (2004) felicitous phrase, Linnaeus's treatment of genera exhibits a "metaphysics in action" that was shaped by his everyday practices and routines, rather than through deliberate and considered philosophical reflection. The act of writing a genus name at the top of a demarcated space on paper established that the same genus as described in an earlier work was being described again, even if (as in the case of *Buchnera* in 1753) none of the species-content from the previous edition appeared underneath. The same lesson can be drawn on a more general level from approaching Linnaeus's practices of revision by looking at the way in which he constructed his herbarium. By writing the genus name on the folder itself, and by allowing sheets to be moved between folders without constraint, Linnaeus in a practical, hands-on manner decoupled the identity of genera from that of their species. Since none of the specimen sheets essentially belonged to a folder, it could continue to represent the same genus even if all specimen sheets were reshuffled. In other words, the space that was marked by a genus name on paper represented the genus as a "nominal space", as we suggest to call it, a space, that is, which could be filled with species content, but emptied of it again on occasion.

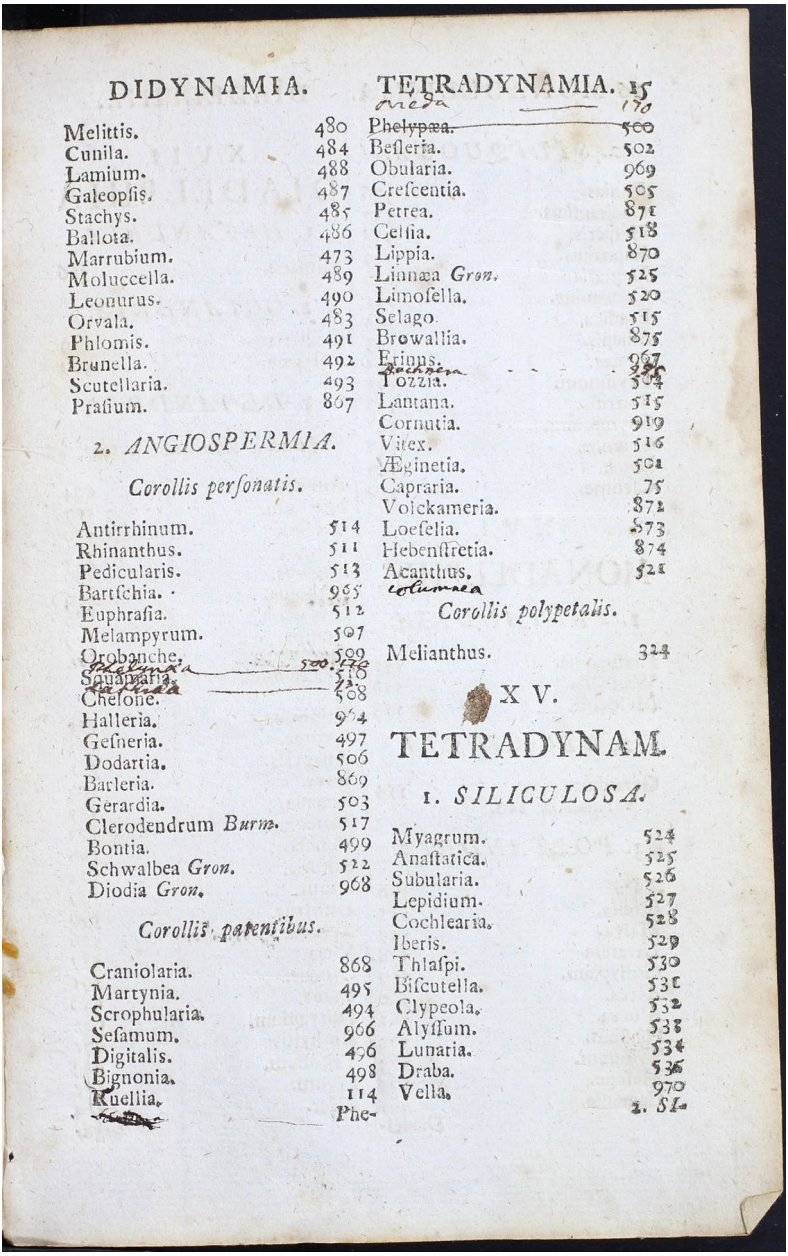
We suggest taking this idea of a nominal space quite literally. If the identity of a genus in the face of revisions was not constrained by any of the species it contained, but only by the space allotted to it within an arrangement that included other such spaces, then the identity of the genus was not fixed by essential reference to any of its contents, be it specimens, species, or diagnostic traits. Instead, it was solely defined by the position of a named paper space in relation to others such spaces. Thus, in the sixth edition of the *Genera plantarum*, published in 1764, Linnaeus included an entry for the genus *Cycas* which merely consisted in a name followed by a series of dashes and a casual note that Linnaeus had not yet received any information "about the character of this genus" (Linnaeus 1764b, p. 572). This epistemic problem was resolved more than a decade later, when he published a description of *Cycas* in the journal of the Royal Academy of Sciences of Paris (see Müller-Wille and Scharf 2012, pp. 30–31, for a more detailed account). What is remarkable, is that Linnaeus mentions this epistemic issue without even acknowledging the metaphysical wager from which it surfaces. By "describing" a genus in the absence of being able to tell exactly which species with which traits should be attributed to it, Linnaeus had created a generic nominal space in the purest sense: a genus in name

only. The fact that he confidently included a blank space (with dashes) in a published work makes for an excellent illustration of this conception of a genus as, at heart, a named space.

Ironically, Linnaeus's explicit metaphysics of genera was nonetheless realist; in a famous aphorism from *Philosophia botanica* (1751), which was approvingly quoted by Charles Darwin more than a century later, he claimed that "a character does not make a genus, but the genus makes the character" (Linnaeus 2003, p. 132). It is possible, however, to understand this metaphysical stance as a direct result from Linnaeus's handling of genera as "nominal spaces". Linnaeus's initial treatment of genera as labelled boxes on paper, as we saw, positively occluded questions about what it was precisely that made a genus the same genus. But the subsequent accumulation of successful operations on their species contents, literally filling in the details, would increasingly erase this appearance and let genera look like predetermined spaces in nature instead, inspiring Linnaeus's famous map-metaphor of the "natural system" (Linnaeus 2003, p. 40; for a detailed discussion, see Müller-Wille 2007). This also explains why Linnaeus's taxonomic practices could spread widely in the eighteenth century, despite the fact that most naturalists remained sceptical of taxonomic realism.

Notwithstanding their nature as "nominal spaces", it is therefore hardly surprising that the extreme effect that we encounter in the *Buchnera* case, of a genus cycling through *all* its species contents, is a rarity in Linnaeus's works. Continuity, to use a phrase of historian of systematics Peter F. Stevens, was established "in practice" (Stevens 1994, pp. 191–192). Had Linnaeus frequently applied genus names to entirely non-overlapping sets of species, he would have seriously destabilised the usage of such names. It is one thing to ask readers to update their usage of taxonomic names in the light of new taxonomic discoveries and subsequent revisions. It would have been quite another thing to ask readers to accept that genus names could freely float around between successive taxonomic works. Nevertheless, the fact that this phenomenon is rare—and perhaps even unique to the case of *Buchnera*—in the works of Linnaeus does not make it any less telling about the metaphysics implied by his taxonomic practice.

The problematic consequences of too many names being severed from their original designations is illustrated by the accumulation of *Buchnera*-like phenomena in the course of the nineteenth century (see Witteveen 2016, 2018). By the end of the century, the American botanist Orator F. Cook attributed this severance to the use of the "method of concepts" rather than the type method: "The method of naming the concepts was used by Linnaeus and his followers for over a century, but had to be abandoned on account of the confusion caused by *names slipping away* from their original application" (Cook 1916; italics added). Cook explained that the problem of names slipping away was a consequence of the treatment of the genus as "a negative concept, since it stands at the mercy of all comers, who may dismember at will and remove any of the species without apology" (Cook 1898a). This interpretation of Linnaean genera as negative concepts accords with our account of genera as nominal spaces. Cook, however, went on to suggest that this method was a holdover of medieval scholasticism or "philosophic idealism" (Cook 1898b). In contrast, we have argued that it was simply an emergent phenomenon: a byproduct of the



DIDYNAMIA.

Melittis. 480
Cunila. 484
Lamium. 488
Galeopsis. 487
Stachys. 485
Ballota. 486
Marrubium. 473
Moluccella. 489
Leonurus. 490
Orvala. 483
Phlomis. 491
Brunella. 492
Scutellaria. 493
Prasium. 867

2. *ANGIOSPERMIA.*

Corollis personatis.

Antirrhinum. 514
Rhinanthus. 511
Pedicularis. 513
Bartschia. 965
Euphrasia. 512
Melampyrum. 507
Orobanche. 509
Squamaria. 516
Chelone. 508
Halleria. 964
Gesneria. 497
Dodartia. 506
Barleria. 869
Gerardia. 503
Clerodendrum *Burm.* 517
Bontia. 499
Schwalbea *Gron.* 522
Diodia *Gron.* 968

Corollis patentibus.

Craniolaria. 868
Martynia. 495
Scrophularia. 494
Sesamum. 966
Digitalis. 496
Bignonia. 498
Ruellia. 114
Phe-

TETRADYNAMIA. 15

Phelypæa. 500
Besleria. 502
Obularia. 969
Crescentia. 505
Petrea. 871
Cellia. 518
Lippia. 870
Linnæa *Gron.* 525
Limosella. 520
Selago. 515
Browallia. 875
Erinus. 967
Buchnera
Tozzia. 504
Lantana. 515
Cornutia. 919
Vitex. 516
Æginetia. 501
Capraria. 75
Volckameria. 872
Loeselia. 873
Hebenstretia. 874
Acanthus. 521
Columnea

Corollis polypetalis.

Melianthus. 324

X V.

TETRADYNAM.

1. *SILICULOSA.*

Myagrum. 524
Anastatica. 525
Subularia. 526
Lepidium. 527
Cochlearia. 528
Iberis. 529
Thlaspi. 530
Biscutella. 531
Clypeola. 532
Alyssum. 533
Lunaria. 534
Draba. 536
Vella. 970
2. *Si-*

Fig. 4 Handwritten entry for *Buchnera* (below *Erinus* under the heading Tetradynamia) from Carl Linnaeus's personal copy of *Corollarium Genera Plantarum,* Leiden: Wishoff, 1737 (URL = http://linnean-online.org/83854/). Linnean Society of London, Library and Archives, call no. BL.49C. With kind permission of the Linnean Society of London, Library and Archives, www.linnean.org

hands-on practices of naming.[19] We nonetheless concur with Cook's claim that there is a fundamental difference between this Linnaean approach to naming and naming practices based on the type method. The two approaches entail different criteria of identity for taxa as they undergo revisions.

In light of this fundamental difference between the two approaches toward applying names to taxa, it is surprising that the (type-based) International Zoological Code of Nomenclature still incorporates a notion that appears remarkably similar to that of a nominal space. At various places the Code invokes the concept of a *nominal taxon*—"A concept of a taxon which is denoted by an available name"—as distinct from that of a *taxonomic taxon*—"A taxon (e.g. family, genus, species) including whatever nominal taxa and individuals a zoologist at any time considers it to contain in his or her endeavour to define the boundaries of a zoological taxon" (ICZN 1999, Glossary).[20] This distinction between nominal and taxonomic taxa is in tension with the Code's use of the type method. It suggests that we can name taxa (qua nominal taxa) regardless of any secure knowledge about their members (qua taxonomic taxa). Yet, the type method tells us that for each typified taxon we must know for sure that at least one element belongs to the taxon: its name-bearing type, without which the relation between name and taxon simply would not exist.

By including both types and nominal taxa in its conceptual repertoire, the Code thus risks presenting two incompatible views about the relation between names and (taxonomic) taxa. If we adopt the view of nominal taxa as being conceptually distinct from taxon members, it should be possible in principle to move types around between taxa.[21] Yet, the very idea of moving types between taxa in taxonomic revision is incompatible with the idea that types set the identity criteria for taxa. According to the type method, types cannot, strictly speaking, be "placed in" one taxon or another. Types determine identity conditions: it is by reference to types that we establish that a revised taxon is the same taxon (with a different circumscription) as before, and hence also is referred to by the same name. Thus, it becomes impossible to place a type in a different taxon; the type carries the identity of the taxon it names with it by necessity.

The contentious nature of the notion of a nominal taxon has not escaped the attention of zoological taxonomists. In the 1970s, the ICZN's Editorial Committee published a proposal to remove the notion of a nominal taxon from the next edition of the Code, on grounds that "[t]he concept is unnecessary to zoological

[19] That Linnaeus, in his taxonomic thinking, was influenced by Aristotelian or scholastic philosophy is an idea that goes back to Julius Sachs' *History of Botany* (1875), and was especially popularized by Ernst Mayr. For a critique of this idea, see Winsor (2006). Note that we do not take a stance on this question in this paper. As explained above, our analysis applies to the level of "metaphysics of action", even if it may very well help, as indicated, to make sense of some of the overt, and often confusing, metaphysical statements Linnaeus made.

[20] The ICZN also mentions a third notion, the *zoological taxon*, which is defined as "A natural taxon of animals (which may, or may not, have had a name applied to it)." Interestingly, this notion does not appear in the actual text of the Code, but leads a quiet life in the Glossary.

[21] Adding to the puzzlement about the notion of nominal taxa is the claim (made in the Glossary of the ICZN) that nominal taxa below the family level are "based on a name-bearing type". However, the idea of "basing" a nominal taxon on a particular taxon member is at odds with the definition of a nominal

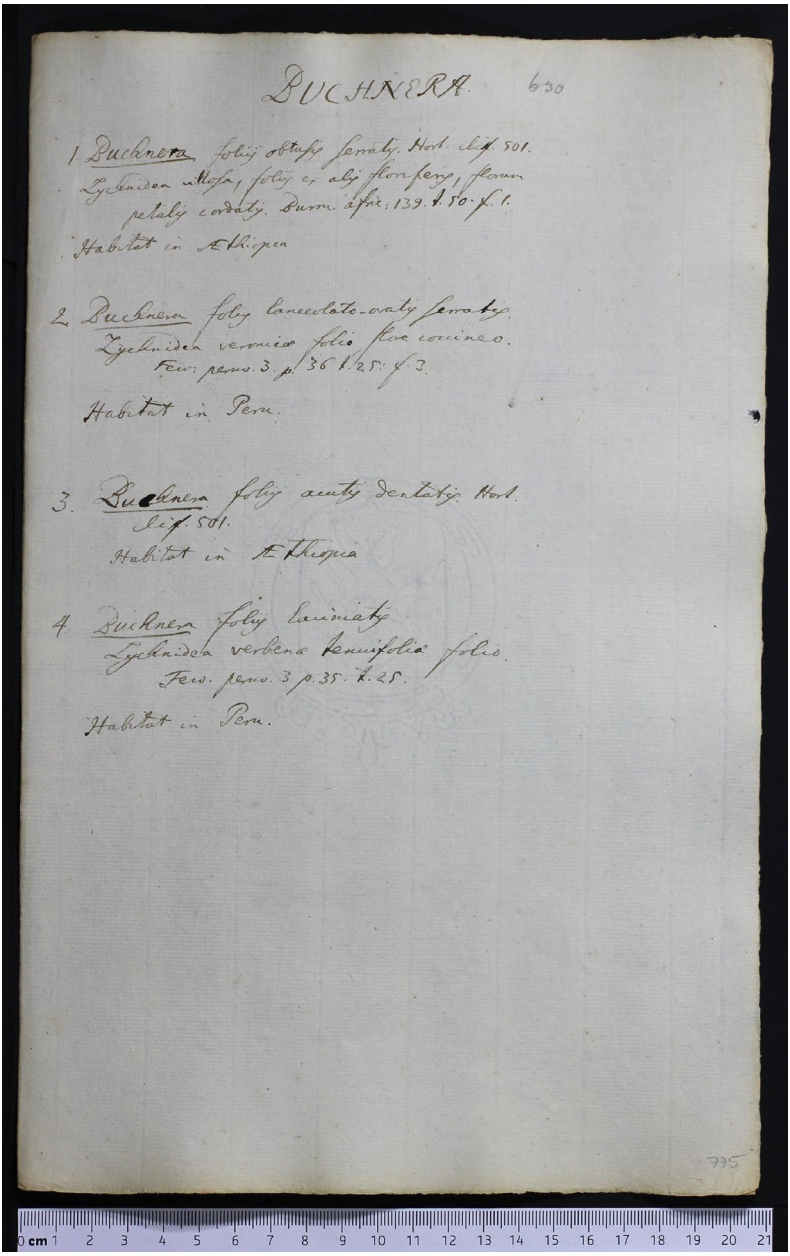


Fig. 5 Page listing four species under the genus *Buchnera* from Carl Linnaeus's manuscript "Species plantarum" (1746) (URL = http://linnean-online.org/60234/). Linnean Society of London, Library and Archives, GB-110/LM/LP/BOT/3/4/1. With kind permission of the Linnean Society of London, Library and Archives, www.linnean.org

nomenclature and its embodiment in the Code is a hindrance to comprehension by confusing its language" (Ride et al. 1979). By that time, other prominent taxonomists had already complained that the idea of a nominal taxon was at odds with the type-based foundation of the Zoological Code. They pointed out that other type-based codes, such as the Botanical Code, appear more logical and consistent since they simply speak of the relation between names, types, and taxa—without drawing a distinction between nominal and taxonomic taxa (Simpson 1961, pp. 40–41; Blackwelder 1967, p. 590).[22]

Nevertheless, the notion of a nominal taxon continues to be part of the language of the Code today. The proposal to remove the term was not passed. Although several zoological taxonomists recognized its potentially misleading connotations, they considered the risk of confusion too low to justify the effort of writing the term out of the Code (see the commentaries in Ride et al. 1979). The notion of a nominal taxon thus remains in the Code as something of an idle reminder of an approach to naming from earlier days, before the type method was adopted.

7 The origin of error

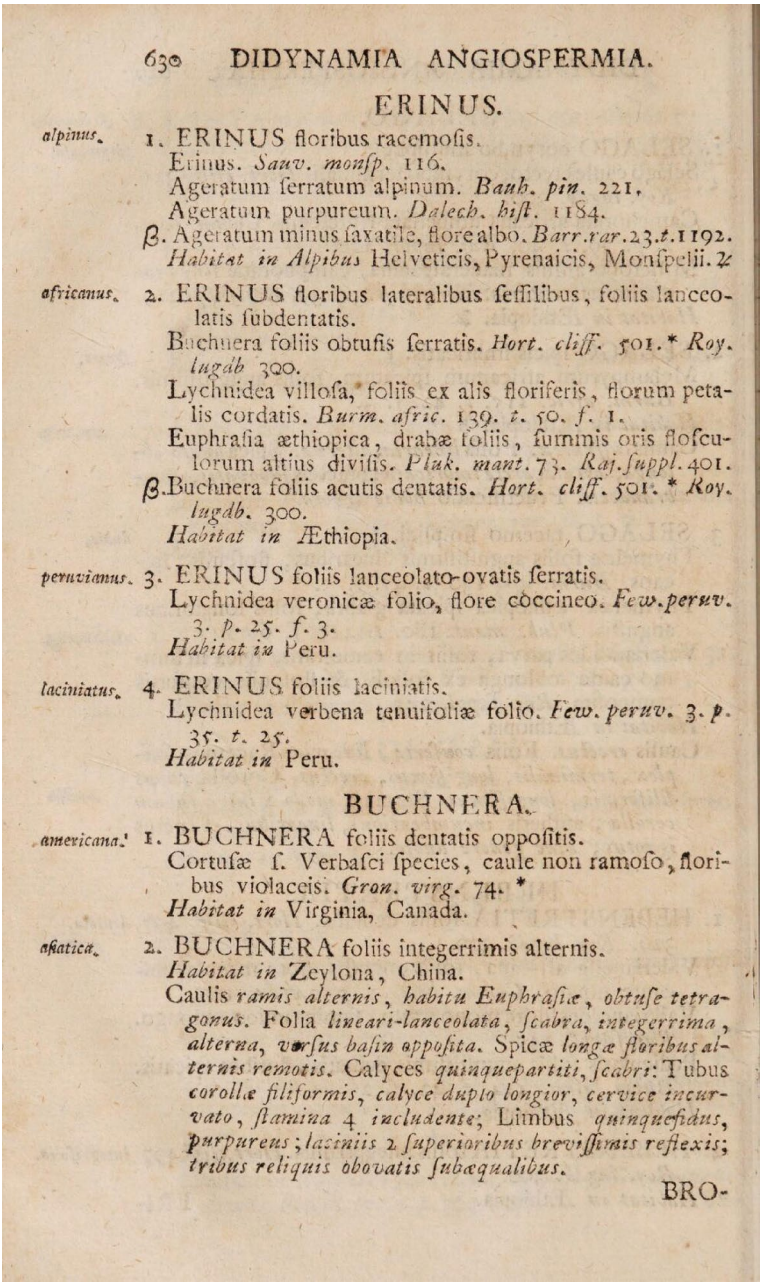
Having explored the differences between Linnaean and contemporary practices of applying names in some detail, we can now return to the question that we raised at the end of Sect. 4: What do Linnaeus's practices of applying names to taxa tell us about the possibility for him to be in error about the identity of taxa? Let us consider the answer by returning to the case of *Buchnera*. Could we say that Linnaeus was in error when, from 1753 onwards, he used the name "Buchnera" for a genus that shared no members with the genus for which he had introduced this name in 1737? In other words, was Linnaeus wrong to suggest in 1753 that he was referring to the same genus, in the light of the radical revisions he had made to its membership?

It should be clear by now that our answer is negative: Linnaeus cannot be said to have been in error when we judge him by the standards of naming of his day and age. By carving out nominal spaces on paper in his herbarium, his working notes and his printed works, Linnaeus established criteria for passing judgment on identity that allowed for the application of names to change in the way we saw in the case of *Buchnera*. Put differently, what may appear to us as the removal of the name "Buchnera" from the original genus does not need to be viewed as such from Linnaeus's point of view. From his perspective, "Buchnera" continued to designate the same *nominal* genus after 1753, in spite of losing the connection to all of its former content.

Linnaeus's practice-based perspective on the identity of taxa did not exist in a vacuum, though. It is important to consider that his approach to naming was legitimated

Footnote 21 (continued)
taxon as not having anything to do with taxon members, as is suggested by the distinction between nominal and taxonomic taxa.

[22] For instance, the International Code of Nomenclature for algae, fungi, and plants speaks of "types of names of taxa" (ICN 2018, Art. 7.1). Also see Witteveen (2015, p. 577) for more on the nature of this relation between names, name-bearers, and referents.



630 DIDYNAMIA ANGIOSPERMIA.

ERINUS.

alpinus. 1. ERINUS floribus racemosis.
Erinus. *Sauv. monsp.* 116.
Ageratum serratum alpinum. *Bauh. pin.* 221.
Ageratum purpureum. *Dalech. hist.* 1184.
β. Ageratum minus saxatile, flore albo. *Barr. rar.* 23. *t.* 1192.
Habitat in Alpibus Helveticis, Pyrenaicis, Monspelii. ♃

africanus. 2. ERINUS floribus lateralibus sessilibus, foliis lanceolatis subdentatis.
Buchnera foliis obtusis serratis. *Hort. cliff.* 301.* *Roy. lugdb* 300.
Lychnidea villosa, foliis ex alis floriferis, florum petalis cordatis. *Burm. afric.* 139. *t.* 50. *f.* 1.
Euphrasia æthiopica, drabæ foliis, summis oris flosculorum altius divisis. *Pluk. mant.* 73. *Raj. suppl.* 401.
β. Buchnera foliis acutis dentatis. *Hort. cliff.* 301.* *Roy. lugdb.* 300.
Habitat in Æthiopia.

peruvianus. 3. ERINUS foliis lanceolato-ovatis serratis.
Lychnidea veronicæ folio, flore coccineo. *Few. peruv.* 3. *p.* 25. *f.* 3.
Habitat in Peru.

laciniatus. 4. ERINUS foliis laciniatis.
Lychnidea verbena tenuifoliæ folio. *Few. peruv.* 3. *p.* 35. *t.* 25.
Habitat in Peru.

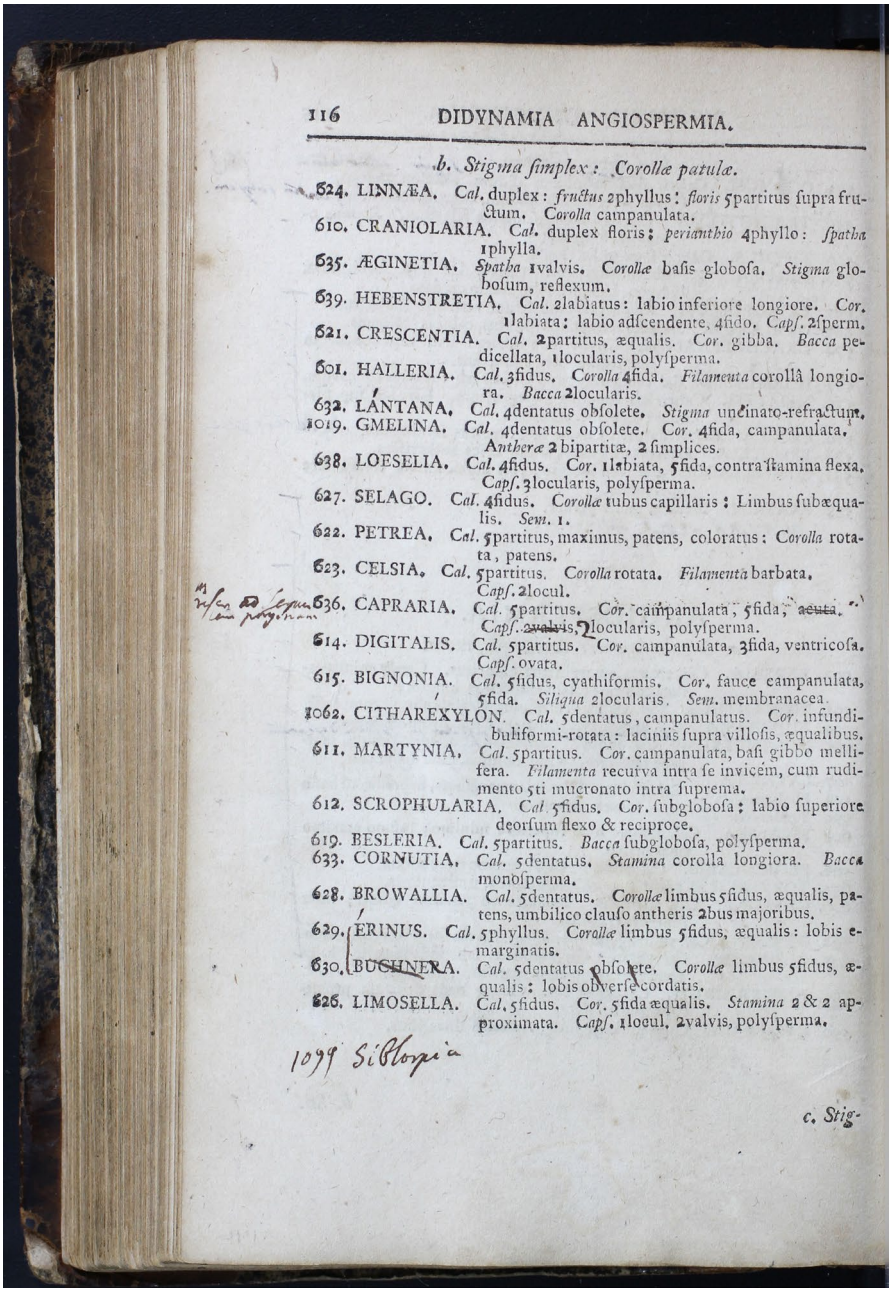
BUCHNERA.

americana. 1. BUCHNERA foliis dentatis oppositis.
Cortusæ f. Verbasci species, caule non ramoso, floribus violaceis. *Gron. virg.* 74. *
Habitat in Virginia, Canada.

asiatica. 2. BUCHNERA foliis integerrimis alternis.
Habitat in Zeylona, China.
Caulis *ramis alternis, habitu Euphrasiæ, obtuse tetragonus.* Folia *linearia-lanceolata, scabra, integerrima, alterna, versus basin oppositа.* Spicæ *longæ floribus alternis remotis.* Calyces *quinquepartiti, scabri:* Tubus *corollæ filiformis, calyce duplo longior, cervice incurvato, stamina* 4 *includente;* Limbus *quinquefidus, purpureus; laciniis* 2 *superioribus brevissimis reflexis; tribus reliquis obovatis subæqualibus.*

BRO-

Fig. 6 Species entries for *Erinus* and *Buchnera* in Carl Linnaeus, *Species plantarum*, Stockholm: Salvius, 1753, vol. 2. Image courtesy of the Wellcome Library: http://wellcomelibrary.org/player/b3053491, vol. 2, image 630



116 DIDYNAMIA ANGIOSPERMIA.

b. Stigma simplex: Corollæ patulæ.

624. LINNÆA. *Cal.* duplex: *fructus* 2phyllus; *floris* 5partitus supra fructum. *Corolla* campanulata.
610. CRANIOLARIA. *Cal.* duplex floris; *perianthio* 4phyllo: *spatha* 1phylla.
635. ÆGINETIA. *Spatha* 1valvis. *Corollæ* basis globosa. *Stigma* globosum, reflexum.
639. HEBENSTRETIA. *Cal.* 2labiatus: labio inferiore longiore. *Cor.* 1labiata: labio adscendente, 4fido. *Capſ.* 2sperm.
621. CRESCENTIA. *Cal.* 2partitus, æqualis. *Cor.* gibba. *Bacca* pedicellata, 1locularis, polyſperma.
601. HALLERIA. *Cal.* 3fidus. *Corolla* 4fida. *Filamenta* corollâ longiora. *Bacca* 2locularis.
632. LANTANA. *Cal.* 4dentatus obſolete. *Stigma* uncinato-refractum.
1019. GMELINA. *Cal.* 4dentatus obſolete. *Cor.* 4fida, campanulata. *Antheræ* 2 bipartitæ, 2 ſimplices.
638. LOESELIA. *Cal.* 4fidus. *Cor.* 1labiata, 5fida, contra ſtamina flexa. *Capſ.* 3locularis, polyſperma.
627. SELAGO. *Cal.* 4fidus. *Corollæ* tubus capillaris: Limbus ſubæqualis. *Sem.* 1.
622. PETREA. *Cal.* 5partitus, maximus, patens, coloratus: *Corolla* rotata, patens.
623. CELSIA. *Cal.* 5partitus. *Corolla* rotata. *Filamenta* barbata. *Capſ.* 2locul.
636. CAPRARIA. *Cal.* 5partitus. *Cor.* campanulata, 5fida, acuta. *Capſ.* 2valvis, 2locularis, polyſperma.
614. DIGITALIS. *Cal.* 5partitus. *Cor.* campanulata, 3fida, ventricoſa. *Capſ.* ovata.
615. BIGNONIA. *Cal.* 5fidus, cyathiformis. *Cor.* fauce campanulata, 5fida. *Siliqua* 2locularis. *Sem.* membranacea.
1062. CITHAREXYLON. *Cal.* 5dentatus, campanulatus. *Cor.* infundibuliformi-rotata: laciniis ſupra villoſis, æqualibus.
611. MARTYNIA. *Cal.* 5partitus. *Cor.* campanulata, baſi gibbo mellifera. *Filamenta* recurva intra ſe invicem, cum rudimento 5ti mucronato intra ſuprema.
612. SCROPHULARIA. *Cal* 5fidus. *Cor.* ſubgloboſa: labio ſuperiore deorſum flexo & reciproce.
619. BESLERIA. *Cal.* 5partitus. *Bacca* ſubgloboſa, polyſperma.
633. CORNUTIA. *Cal.* 5dentatus. *Stamina* corolla longiora. *Bacca* monoſperma.
628. BROWALLIA. *Cal.* 5dentatus. *Corollæ* limbus 5fidus, æqualis, patens, umbilico clauſo antheris 2bus majoribus.
629. ERINUS. *Cal.* 5phyllus. *Corollæ* limbus 5fidus, æqualis: lobis emarginatis.
630. BUCHNERA. *Cal.* 5dentatus obſolete. *Corollæ* limbus 5fidus, æqualis: lobis obverſe cordatis.
626. LIMOSELLA. *Cal.* 5fidus. *Cor.* 5fida æqualis. *Stamina* 2 & 2 approximata. *Capſ.* 1locul. 2valvis, polyſperma.

1099 Siblopia

c. Stig-

Fig. 7 Entries for *Erinus* and *Buchnera* in a list of plant genera from Carl Linnaeus's own copy of *Systema naturae*, sixth edition, Stockholm: Kiesewetter, 1748 (URL = http://linnean-online.org/66029/). Linnean Society of London, Library and Archives, call no. BL.10. With kind permission of the Linnean Society of London, Library and Archives, www.linnean.org

and, in a way, validated by the position he occupied in the social context of eighteenth-century natural history. As we pointed out in Sect. 5, the innovative methods of information management that empowered Linnaeus's practices of revision, and the social position he acquired through these, were crucial to his success. Uppsala became a nexus in the networks of exchange among naturalists: a place where a never ceasing flow of specimens and descriptions from a worldwide network of collectors and correspondents were processed and organized into updated classifications based on Linnaeus's authoritative taxonomic judgments. With the application of names being part and parcel of classification, Linnaeus's names stood with his authority.

That authority in part also included respect for earlier authorities, and Linnaeus was aware of that. In *Critica botanica* (1737d) and in Chapter 7 of *Philosophia botanica* (1751) he set out rules governing the retention of old names and the introduction of new ones in great detail, amounting to a whole politics of naming that tried to balance established usage with nomenclatorial innovation (Schiebinger 2007). The discussion of naming sets out with a rule that nicely captures the spirit of what we suggest to call "nominal taxa". "Any plants that agree in genus", Linnaeus stipulated, "should be designated by the same generic name" (Linnaeus 2003, p. 170). Two further rules are particularly interesting for our purposes, since they indicate the conditions under which one could, after all, be wrong about taxonomic identity under the Linnaean nomenclatorial regime: "The generic name must be *the same* within the same genus" and "If one and the same generic name has been taken to designate different genera, it will have to be excluded from one place or the other" (Linnaeus 2003, p. 171; emphasis in the original). In other words, it is wrong to apply two different labels for the same nominal taxon; and it is equally wrong to apply the same label to two distinct nominal taxa.

We are not aware of any cases in which Linnaeus himself could be accused of such mislabelling, but it certainly is a possibility, given the sheer amount of material he was dealing with. Notably, however, mislabellings were the subject of a scathing attack that Jean-Baptiste Lamarck (1744–1829) launched against the so-called thirteenth edition of *Systema naturae*, that the German naturalist Johann Friedrich Gmelin (1748–1804) had edited 1789–1790. Gmelin, Lamarck claimed, had produced a work "full of the grossest errors", in particular, by presenting "an enormous quantity of double usages [*doubles emplois*], and even triple usages" (Lamarck 1792, p. 82). In an appendix to his review, Lamarck listed numerous instances where Gmelin had allegedly used different names for what was in fact "the same thing" or "plant". Interestingly, Lamarck suspected that these errors were due to the fact that Gmelin had relied on naturalists "living in diverse countries, and thus far away from each other", which inevitably led to "considerable discordance in their works" (Lamarck 1792, p. 81).

In the course of the nineteenth century, the topic of (mis-)applying existing taxon names in the face of new material became increasingly a subject of discussion and dissent. Several key changes to the information economy of natural history exposed the limits of Linnaeus's approach to applying names and made salient that its implicit philosophy of taxon identity was no longer tenable under the new conditions. First, together with the widespread adoption of Linnaeus's system of binomial naming, taxonomy became increasingly decentralized and less centred on key individuals such as Linnaeus. Instead of the previous authority of a few individuals,

several institutional hubs arose around which natural history revolved, such as the Museum d'Historie naturelle in Paris, at which Lamarck worked, but also the Linnaean Society or the British Museum in London. Second, these new institutional hubs increasingly relied on widening circles of collectors and travelling naturalists which led to a steep increase in the number and diversity of specimens to be classified (particularly following the end of the Napoleonic wars), and often necessitated sweeping revisions to earlier classifications. Genera and families frequently needed to be revised and were often split up to make room for newly recognized species (Farber 1982; Müller-Wille 2017).

As an immediate effect of this expansion, decentralization, and democratization of taxonomy, names became applied to taxa in different, incongruous ways. Different naturalists often published revisions without being aware of the work of others, or in open disagreement with the taxonomic judgment of others (see Witteveen 2016, 153ff). As names thus started to become applied in different, sometimes non-overlapping ways by different naturalists, confusion about the relation between names and the taxa they referred to became rampant. What appeared as the same genus with a changed circumscription in one revision would be presented as a different genus (with a different name) in another revision. Such divergent applications of names were bound to create confusion. Clearly, it was no longer sustainable and expedient to make the application of names a matter of personal judgment based on extensive, but subjective experience. The larger scale and more complex social organization of taxonomy called for new ways of settling on the identity of taxa in taxonomic revisions (McOuat 1996, 2001).

We have already seen that the solution to this problem was eventually found in the formulation of nomenclatural codes that incorporated the type method.[23] By making the application of the names a matter of following type specimens around, an objective procedure replaced the earlier subjective judgment in determining the application of names. It is important to note, though, that the adoption of an objective, interpersonal and rule-governed method of applying names to taxa as such does not entail a shift in stance on taxon identity. This is illustrated by the method of applying names in generic revisions from the first botanical code of nomenclature, de Candolle's *Lois de la Nomenclature Botanique* from 1867. It contained the rule that when a genus without a type species is divided into two or more genera, the name should be applied to that portion of the original genus that contains the most species (de Candolle 1867, Art. 54). While this rule eliminates individual judgment from the application of genus names in any given case, it still allows for the possibility that after a sequence of revisions a genus name has become dissociated from all of the species it originally applied to. Hence, the eventual adoption of the type method (by botanists and zoologists alike) did not just constitute a transition from a personal and subjective to a communally agreed upon, objective method of applying names. Qua objective method, it also offered new and disciplined criteria for establishing taxon identity against a backdrop of changing taxonomic judgments.

[23] The complex histories of the formation of the nomenclatural codes and their incorporation of the type method need not concern us here. For detailed studies see Daston (2004), Dayrat (2010), McOuat (1996), Nicolson (1991) and Witteveen (2016).

8 Conclusion: elephants and errors

We began this paper with the case of the discovery that an elephant foetus which Linnaeus's had used for naming and describing *Elephas maximus* turned out to be a specimen of *Loxodonta africana*. We used this case as a springboard for considering how Linnaeus kept track of the identity of taxonomic groups that underwent revisions. This led us to consider another case in Linnaean taxonomy, that of *Buchnera*, which illustrates the complex implications of his method of attaching taxon names to spaces on paper or folders in his herbarium, representing "nominal taxa". We showed that this practice of attaching names directly to the thinnest possible taxon concepts allowed for permutations to taxon contents that seem odd from a contemporary type-based perspective on names and identity. From a contemporary point of view, it seems as if Linnaean practice allowed for names to become dissociated from the taxa they referred to. But this only holds if we interpret Linnaean naming practice anachronistically, using naming standards that he and his contemporaries did not subscribe to. Judging Linnaeus by what were accepted practices of his day shows that there was a good rationale for naming taxa directly, as nominal taxa. In particular, this referential practice, and its associated implicit metaphysics, worked well with the co-production of taxonomic knowledge in eighteenth-century botany and zoology.

We conclude by returning to the case of *Elephas maximus*. Now that we have become acquainted with some important differences between Linnaeus's and contemporary practices of applying taxon names—and with their implications for the metaphysics of taxon identity—we can see more clearly that on a Linnaean approach it makes no sense to ask whether the name "Elephas maximus" was applied correctly or not. To illustrate this, we would like to consider two brief exercises in counterfactual history based on this case.

One possible scenario is that Linnaeus was aware that Asian and African elephants formed two distinct species. In this case, we would have to conclude that he simply mislabelled the "miniature elephant" from the King's collection and placed it in the wrong "box" labelled with the name for Asian elephants. This, as we have shown in detail, was not the case, and our modern investigators have admitted as much. But how would Linnaeus have applied the name "Elephas maximus" if he had eventually realized that he had initially applied this name to a composite of distinct elephant species, one from Africa and one from Asia? One possibility for him would have been to continue using the name for the Asian species, in line with the observation "habitat in Zeylonæ" from the original description. But it would have been equally possible for him to use the name for the African species. This was the only species of which he had inspected an actual specimen before drawing up the original description. Moreover, the epithet *maximus* would have nicely fitted the fact that the African elephant species is the largest land animal on earth. In any case, nothing would have forced Linnaeus to take one decision or the other. The nominal space labelled "*Elephas maximus*" in his manuscripts could have received the members of either species.

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Compliance with ethical standards

Conflict of interest The authors declare that they have no conflict of interest.

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