The final publication is available at Springer via http://dx.doi.org/10.1007/s10739-015-9419-2

On the origins of the quinarian system of classification

Aaron Novick

Abstract

William Sharp Macleay developed the quinarian system of classification in his *Horæ Entomologicæ*, published in two parts in 1819 and 1821. For two decades, the quinarian system was widely discussed in Britain and influenced such naturalists as Charles Darwin, Richard Owen, and Thomas Huxley. This paper offers the first detailed account of Macleay's development of the quinarian system. Macleay developed his system under the shaping influence of two pressures: (1) the insistence by followers of Linnaeus on developing artificial systems at the expense of the natural system and (2) the apparent tension between the continuity of organic nature and the failure of linear classification schemes (which continuity seemed to require). Against what he perceived as dogmatic indolence on the part of the Linnaeans, Macleay developed a philosophy of science in which hypotheses that exceeded the available evidence should be proposed and subjected to severe tests. He also developed a novel comparative anatomical methodology, the method of variation, to aid in his search for the natural system. Using this method, he developed an intricate system that showed how organic nature could be continuous without being linear. A failure to appreciate these facets of Macleay's thought has led to several misunderstandings of him and his work, most notably that he was an idealist. These misunderstandings are here rebutted.

1. Introduction

In 1819 and 1821, the entomologist William Sharp Macleay published his *Horæ Entomologicæ*, in which he proposed a novel arrangement of the Linnaean genus *Scarabeus* (scarab beetles; Part I, 1819), then extended the principles of this arrangement to the entire animal kingdom (Part II, 1821). Macleay's principles, dubbed the "quinarian system," were intricate: they grouped all animals into nested circles of five subgroups (fig. 1; fig. 2). To modern eyes, the system's apparently geometrical regularity makes it seem ludicrous (de Beer 1963, p. 13; Ghiselin 1969, p. 104; Blaisdell 1992, p. 24). Macleay's contemporaries, however, considered the merits and flaws of his theory seriously. To name only the most famous case, Charles Darwin in the late 1830s engaged with the quinarian system extensively in his notebooks, going as far as trying to show how certain aspects of it were compatible with and could be explained by his evolutionary theory (Barrett et al. 2008; Ospovat 1981, pp. 101-113[1]).

Historians are increasingly catching up to Macleay's contemporaries, and now mostly recognize the quinarian system as an important attempt to ascertain the structure of the natural system – the order amid the diversity of nature (Ospovat 1981; Di Gregorio 1982). It is now recognized, for instance, that Macleay's distinction between two kinds of resemblance among organisms (affinity and analogy, see §2 below) was influential at the time, e.g. on Owen's homology/analogy distinction (Ospovat 1981) and remains pertinent to contemporary systematics (Williams and Ebach 2008, ch. 9). Despite growing appreciation of Macleay's importance, however, the quinarian system remains poorly understood. In particular, no studies have yet examined how Macleay developed his system. Existing studies have focused either on the quinarian system's influence on scientists such as Charles Darwin (Ospovat 1981; Di Gregorio 1996) and Thomas Huxley (Winsor 1976) or on its relation to the politics of British science at the time (Desmond 1985; McOuat 1996). Illuminating as these studies all are, existing views about the origins of the quinarian system remain at best superficial, at worst incorrect.

This paper offers the first detailed account of Macleay's motivations in developing the quinarian system. Macleay's overarching aim was to discover the natural system of classification, an aim widely shared by his contemporaries. Macleay's system took on its distinctive character as a result of two key pressures. First, Macleay sought to break the stranglehold of the British Linnaeans, whose indolence and dogmatism, he believed, had led to unscientific and even obscurantist contentment with artificial systems. In responding to them, he developed both a novel philosophy of science (in which hypotheses should be proposed in advance of the available evidence, then subjected to severe tests) and a novel comparative anatomical method (the method of variation). Second, he wished to reconcile the continuity of organic nature with the failure of linear classification schemes. He used his distinction between affinity and analogy to develop a system that preserved continuity while avoiding the absurdities characteristic of linear systems. With this understanding in place, the errors in thinking of Macleay's system as motivated by religious (Ospovat 1981) or idealistic (Rehbock 1983) commitments may be seen.

The structure of the paper is as follows. After an overview of the core features of the quinarian system (§2), I detail Macleay's opposition to the Linnaeans (§3). In the subsequent two sections, I show how this led Macleay to develop a novel philosophy of science that can explain his willingness to generalize his system (§4), and to develop

[1] All further references to Ospovat's book concern this page range.

his method of variation, by which he discovered the central tenets of his system (§5). I then show that Macleay's system was also shaped by a theoretical problem: the tension between continuity and non-linearity (§6). This raises a problem concerning the place of analogies in Macleay's system, which I address (§7). I contest two common but mistaken views about Macleay's work (§§8-9), then conclude by considering Hugh Strickland's criticisms of the quinarian system (§10).

2. Overview of the quinarian system

Macleay was the first son of Alexander Macleay, who was the secretary of the Linnean Society of London, and who had an extensive collection of insects, in particular scarab beetles.[2] The younger Macleay took an interest in entomology, and devoted his first work (*Horæ Entomologicæ*, Part I) to the study of the Linnaean genus *Scarabeus*. In arranging this group, he discovered complex patterns that he then generalized to the entire animal kingdom. The generalization of these patterns became known as the quinarian system.

Macleay's system sparked much discussion among British naturalists, finding adherents (Vigors 1825; Swainson 1827), cautious admirers (Charles Darwin, see Barrett et al. 2008; Kirby 1825; Wood 1836; Jenyns 1835), and fierce detractors ([Fleming] 1829; James Rennie, see Montagu and Rennie 1831; Blyth 1835, 1836; Rylands 1836; Strickland 1840, 1841, 1845, 1846). It also prompted William Swainson (in the 1830s; Swainson 1834, 1835; Swainson and Richardson 1831) and Edward Newman (1832) to develop distinct but related systems. Caution is required when referring to "the quinarian system": Swainson is generally considered a quinarian, but the system he promoted in the 1830s is quite different from Macleay's. To conflate the two is a serious error. When I speak of "the quinarian system" in what follows, I refer only to Macleay's version.

Macleay's system first and foremost arranged all organisms into nested groups each of which contained five subgroups (fig. 1). These five subgroups, in turn, contained five further subgroups (fig. 2), and so on for every rank in the classification. These groups of five consisted of circular chains of affinity (similarity, roughly, but see below). Circles were formed as follows: Given a group with subgroups A through E, those subgroups could be arranged in a linear chain of affinity (A has affinity to B, B to C, C to D, D to E). Then, the end of this chain looped around (E has affinity to A), closing the circle. The circular nature of groups also occurred at every rank.

Macleay did not restrict relations of affinity within circles. Instead, the gaps between groups were bridged by

[2] For biographical information on Macleay, see Fletcher (1920); Swainston (1985); Holland (1996).

what Macleay called osculant groups. Osculant groups had affinities to both of the two major groups they connected, e.g. cephalopods had affinities to both mollusks and vertebrates (fig. 1). Once again, these also occurred at every rank (fig. 2).

Thus far I have spoken only of affinity, but Macleay in fact included in his system two kinds of resemblance between organisms: affinity and analogy. Here one must carefully separate, as Macleay (1819/1821, pp. 363-64) did, the *test* of affinity and analogy (by which Macleay determined the status of a particular resemblance) from the *nature* of affinity and analogy (which depended on their place in his system). The test was as follows: given a resemblance between two groups, see whether it forms part of a continuous, gradual transition. If it does, it is an affinity. If it is an isolated resemblance, it is an analogy. Organisms were thus related by affinity if they resembled one another in many particulars, while they were related by analogy if they resembled each other only in a few particulars against a backdrop of overall dissimilarity.

The nature of affinity and of analogy, by contrast, had to do with their place in Macleay's system. Macleay believed that, given two groups (say, Ametabola and Mandibulata from fig. 2), the subgroups of these that occupied corresponding positions would be found to bear some relationship of analogy. Thus, for instance, the subgroups Thysanura (in Ametabola) and Orthoptera (in Mandibulata) should bear a relation of analogy, as indeed they do (fig. 3). Two circles (of the same rank) lined up in parallel would thus show relations of analogy at each corresponding point (see also Macleay 1819/1821, p. 367). This marked the difference in the nature of affinity and analogy: affinities came in circles, while analogies showed this parallelism.

Finally, Macleay (1819/1821, pp. 273-74) argued that all groups are characterized by a particular type, which is the perfection of the plan on which the members of that group are built. The empirical criterion of perfection was "complication of mechanism" (Macleay 1819/1821, p. 205). Within each circle of affinity, two groups were "normal" (i.e. most perfect; in fig. 1, Vertebrata and Annulosa), while three were "aberrant" (i.e. least perfect; in fig. 1, Mollusca, Acrita, and Radiata).

The quinarian system can thus be seen to consist of six major components:

- The affinity/analogy distinction
- Circular chains of affinity
- Parallelism of analogies
- Five subgroups per group

- Inosculation between contiguous circles
- Two normal and three aberrant subgroups per group

These do not exhaust all the regularities that Macleay believed he had discovered (see Macleay 1819/1821, p. 223), but they are the regularities in whose truth Macleay was most confident and that he discussed at the greatest length, and thus are properly considered the core of the quinarian system.

Macleay did not substantially change his system after 1821: these six components were still the core of his quinarian system in his final paper on the subject (Macleay 1841). Macleay's later papers concerning the quinarian system primarily either applied the system to different groups (Macleay 1825b, 1825c, 1833, 1838, 1840a, 1840b, 1841) or defended it from objections (Macleay 1829a, 1829b, 1830a). One paper (Macleay 1825a) is of special note, however. On March 2, 1822, a fire at the booksellers' (Samuel Bagster) destroyed most copies of *Horæ Entomologicæ* (Reider 1841). Macleay thus took the opportunity, in a paper ostensibly devoted to discussing the relations between his system and that of the mycologist Elias Fries, to summarize at length the major elements of his system. While the paper does not introduce any modifications to the core components of the quinarian system, it does include some arguments in their support that cannot be found in *Horæ Entomologicæ*.

3. Against the Linnaeans

It is generally recognized that Macleay was part of the search for the natural system (Ospovat 1981; Di Gregorio 1982), but it has not yet been appreciated that the particular manner in which Macleay went about searching for the quinarian system was shaped by his considerable distaste for the Linnaean school in British natural history. In opposing what he perceived as dogmatism and indolence on their part, he developed both a philosophy of science and a comparative anatomical method that are essential for understanding the intricate content of the quinarian system. In this section, I lay out Macleay's gripes with the Linnaeans; the following two sections characterize his philosophy of science and his method of variation, respectively.

The place to begin is a paper by William Roscoe, a British botanist who upheld the Linnaean "artificial" system against the challenge posed by Antoine Laurent de Jussieu's rival "natural" method (Roscoe 1815). Since James E. Smith bought Linnaeus' collections and founded the Linnean Society of London, British natural history had been dominated by Linnaeans, but the introduction of Jussieu's natural method in the early 19^{th} century challenged this dominance (Scharf 2009, pp. 102-03). Roscoe responded to that challenge, and in doing so he served as one of

Macleay's primary targets in *Horæ Entomologicæ*'s polemical preface.

Roscoe's main aim was to show that Jussieu's purportedly natural method in fact created an artificial arrangement, and that, *qua* artificial system, it was substantially inferior to Linnaeus' (because much more difficult to use). However, Roscoe also wrote at great length about the relative aims of natural and artificial systems, arguing that natural historians should focus on improving Linnaeus' artificial system, rather than search for the natural system.

As regards artificial and natural systems, Roscoe defended two principal claims. First, Roscoe argued that natural and artificial systems must be kept independent. The natural system should "unit[e] such [groups] as are most nearly allied, and separat[e] such as have no inherent affinity to each other," to which end it must consider "those primitive and secret alliances by which nature has bound them together." An artificial system, by contrast, should make "some plain and obvious distinction… decisive of the character" (Roscoe 1815, p. 76). This reliance on different types of evidence had important ramifications: the easy access to the traits on which an artificial system was based made such systems readily communicable, while "the knowledge of a natural system is chiefly confined to the author" due to its use of esoteric, hidden alliances (Roscoe 1815, p. 77). For this reason, further, no natural system could ever supplant Linnaeus' artificial system.

The reliance of the natural system on such "deep" characters led to Roscoe's second major claim, which was that the natural system was unattainable, if not in principle then at least in 1810 (when the paper was first read). He wrote (Roscoe 1815, pp. 50-51):

> Her vegetable productions are so numerous, their characteristics often so difficult to ascertain, they are related to each other by so many ties, that it is vain to expect that we shall ever be able clearly to define them, and accurately to seize upon the true distinctions; so as to combine the whole in the precise order in which they were primarily disposed by her hand.

Because of this inaccessibility of the natural system, Roscoe recommended continuing to develop Linnaeus' artificial system, quoting approvingly Linnaeus's comparison of those who rejected artificial systems for the mere fragments of the natural system to "those who, having a convenient and well roofed house, overturn it, in order to build one in the place of it of which they are unable to finish the roof" (Roscoe 1815, p. 61; see Larson 1967, p. 319 for its source in Linnaeus). For Roscoe, it was still not time to search for the natural system, and this was shown by the failure of Jussieu's purportedly natural method.

This sentiment attracted Macleay's ire. Though he cited Roscoe only once, apparently with approval, the preface to *Horæ Entomologicæ* responds at length to Roscoe's views. Take, for instance, the context of Macleay's (1819/1821, pp. xiii-xiv) citation of Roscoe:

> And it will still less be credited, that some of those even who can properly distinguish [natural and artificial systems], either covertly insinuate or openly assert that we ought to rest contented in our ignorance, and to cease our inquiries after those affinities, from the study of which, says a learned writer, "the science acquires new dignity; and instead of being conversant merely with exterior forms and nominal distinctions, becomes acquainted with the laws and operations of nature."

Roscoe clearly fell into Macleay's category of naturalists who can distinguish natural from artificial systems, yet who nonetheless "openly assert" that naturalists should pursue artificial systems.

At many other points, too, Macleay implicitly countered Roscoe. Roscoe (1815, pp. 56-62) spent several pages arguing against attempts to show that Linnaeus himself believed his system would one day be supplanted by the natural system, and condemned Jussieu for seeking to overthrow Linnaeus. Macleay, in the first paragraph of his preface, was careful to insist that he did not seek to overthrow past work. He granted, moreover, that Linnaeus himself condemned the search for the natural system. However, Macleay argued, referring to the same Linnaeus quote that Roscoe had used, that this opinion was "unfortunate for the celebrated man with whom it originated" (Macleay 1819/1821, p. xv). The impossibility of discovering the natural system, MacLeay contended, could be supported in only two ways: by establishment of "the Epicurean doctrine" (which, in denying that creation had any plan, denied the existence of the natural system), or by showing that the plan of creation, though real, was inaccessible to human inquiry (Roscoe's claim). Neither claim, however "rest[ed] on a foundation more solid than bare assertion" (Macleay 1819/1821, p. xix).

Macleay further challenged Roscoe's claim that developing artificial and natural systems were independent endeavors. Following the trope of likening artificial systems to dictionaries, Macleay (1819/1821, p. xii) argued that "an artificial system is a dry unmeaning collection of names, unless it be made subservient to the discovery of the natural one," just as a dictionary is but "an useless assemblage of words" without rules of grammar and syntax to give the language structure. An artificial system could be useful, but only if it led to the discovery of the natural system by which it gained its meaning.

Thus Macleay was committed to the search for the natural system, and so stood in direct opposition to Roscoe. Macleay was not just opposed to Roscoe, however: he thought Roscoe's views were devastating for the health of science. Macleay (1819/1821, p. xxi) did not mince words:

> The truth is, that, like the religion of Mahomet, the Linnæan system has given rise in some parts of Europe to an unfortunate species of self-content, a barbarous state of semi-civilization, which is so far worse than absolute ignorance, that the existence of it seems to preclude every attempt at further improvement.

The followers of Linnaeus, in Macleay's eyes, had abandoned the cause of science, and had done so in a particularly dogmatic manner.[3] Mere elaboration of artificial systems was not science: it left the so-called "*practical* botanist" no different "from the village herbalist or culler of simples" (Macleay 1819/1821, p. xvii, emphasis in original). The advancement of a truly scientific natural history thus required overcoming the Linnaeans.

Macleay's commitment to searching for the natural system left him with two problems, however. First, he needed some means of overcoming the indolence he believed the Linnaeans had imposed on British natural history. Second, he had to admit "that every naturalist who has hitherto proposed a natural system, has thereby only deceived himself and others with an illusive structure" (Macleay 1819/1821, p. xviii). If Macleay was going to discover the natural system, he needed to avoid the methodological pitfalls that had led to the "indisputable" failure of all previous attempts.

4. Macleay's philosophy of science

Macleay's response to the first problem, the indolence characteristic of British natural history in the early 1800s, was to develop a novel philosophy of science that he believed would stimulate critical inquiry and so break the Linnaean stranglehold. The primary locus in which Macleay developed his philosophical views, in addition to the preface of Part I of *Horæ Entomologicæ*, was the introduction to Part II. These views concerned the nature of the evidence supporting Macleay's system and the appropriate manner by which the system could be tested.

To get a handle on Macleay's philosophy of science, the best place to start is with consideration of the structure of *Horæ Entomologicæ*. Why did Macleay structure the work in two parts, one of which is a self-described "mono-

[3] MacLeay not alone in this view: cf. Lindley 1830, Strickland 1845, Jenyns 1835, Montagu and Rennie 1831, and [Luxford] 1850. For a contemporary historical treatment, cf. Endersby 2005.

graph" on *Scarabeus*, the other a sweeping overview of the entire animal kingdom (Macleay 1819/1821, p. v)?[4] The key to this division lies in Macleay's understanding of analytic and synthetic methods in natural history.

Analysis, for Macleay, was the arrangement of groups according to their natural affinities, achieved via comparative anatomical study. The paradigmatic genre for presenting the results of analysis was the monograph. By contrast, synthesis was synoptic: it involved using "principles of knowledge […] already acquired by analysis" to show how a given group could plausibly be arranged to fit those principles (Macleay 1819/1821, pp. 418-19).[5] Part I of *Horæ Entomologicæ* is an analytic monograph on *Scarabeus*; Part II is synthetic (Macleay 1819/1821, pp. 167, 459-60).

Because synthesis applied principles discovered by analysis, analysis had epistemic priority. Macleay insisted that he discovered the quinarian system through analysis.[6] In explaining why he published the analytic prior to the synthetic portion of *Horæ Entomologicæ*, he cited his wish "to let the public, as nearly as possible, arrive at the same conclusions by the same order and means that I had used myself" (Macleay 1819/1821, p. 163; cf. Macleay 1825a, p. 49). That is, Macleay discovered via the analysis of *Scarabeus* that certain principles governed the arrangement of that group. The synthetic work in *Horæ Entomologicæ* Part II did not modify the content of these principles, but merely showed how they could be more generally applied. In treating analysis as the means by which he discovered the principles of the quinarian system, it is probable that Macleay faithfully represented his process of discovery. At least, no known evidence suggests otherwise.

Analysis was thus the primary engine of classification: only by the detailed study of particular groups could

---

[4] In fact, *Horæ Entomologicæ* as published is incomplete. The two parts are labeled "Part I" and "Part II" of "Vol. I" – no second volume ever appeared. It seems that Macleay abandoned the work after most copies were burned. An indication of the content that might have been contained in later parts/volumes would have contained may be found in Macleay (1830b), which "belongs to the Third Part of the "*Horæ Entomologicæ*" entitled "*An Analytic Essay on the Development of Annulose Forms*," and which consists in a description of the parts of insects and the manner in which they vary. It is unclear whether this would have been Part III of Volume I, or whether it would have been part of the mysterious second volume.

[5] Note that Macleay's use of the terms 'analysis' and 'synthesis' is not the same as that discussed in Stevens (1994). It is much closer to that described in Appel (1987, pp. 46-47).

[6] Fletcher (1920), Winsor (1976), and Rehbock (1983) follow Macleay on this point.

their true arrangement be discovered. What, then, was the purpose of synthesis, which takes up the majority of *Horæ Entomologicæ* Part II? Near the beginning of the introduction, Macleay (Macleay 1819/1821, p. 162) presented an objection that had been raised against his view: "it has been objected that in the ardor of discovery I have advanced my principles too far, and have argued from a solitary and singular fact to the existence of a circular disposition throughout nature." Macleay (Macleay 1819/1821, p. 164) granted the charge: "In the actual state of natural science it is presumptuous no doubt to assert positively that the general distribution of organized matter is in circles."

Macleay's use of synthesis in Part II was aimed to combat, in a preliminary way, this objection. His goal was to provide an overview of the "general character" of the classification of animals, showing that they could plausibly be arranged according to quinarian principles (Macleay 1819/1821, p. 166). Thus, for instance, Macleay's discussion of Lamarck's twin series (Macleay 1819/1821, pp. 329-334) is meant to show the coincidence of Lamarck's arrangement of Animalia (slightly modified), which Lamarck proposed without a thought of quinarian principles, with Macleay's deliberately quinarian arrangement of Animalia. Macleay thus showed the consistency of Lamarck's work with the principles Macleay had discovered in *Scarabeus*, specifically the principles that all natural groups form circular chains of affinity and that all natural groups have five subgroups. Successful synthetic extension of these principles did not amount to definitive proof of their generality, however: "This part of my work therefore differs from the former in as much as it takes more the character of an hypothesis, and as such deserves more suspicion" (Macleay 1819/1821, p. 167). The ultimate arbiter of the truth of Macleay's synthetic claims was analysis: only analysis could "ascertain the truth with accuracy" (Macleay 1819/1821, p. 167).

Still, this does not fully explain Macleay's use of synthesis, for one might have objected that only the analytical method was legitimate and that synthesis had no place in natural history (see §10 below). Macleay, however, had an explicit defense of his use of synthesis, and this defense was tied to his opposition to the Linnaeans. Macleay believed it worthwhile to use the synthetic method because "the will to criticize produces investigation, and […] investigation must always tend to the development of truth" (Macleay 1819/1821, p. 166). So long as a proposed theory is not dogmatically accepted (as Macleay believed Linnaeus' dictates were received), it will advance science even if it is false. In Macleay's eyes, it was not false theories that threatened science; rather, it was contentment with existing, unsatisfactory states of scientific development. In a clear reference to the Linnaeans, Macleay (1819/1821, p. 166) wrote: "The great enemy to the progress of Natural History has hitherto been indolence, or, at least, the disposition to rest satisfied with the actual state of a science which till very lately has been wholly illusory."

Macleay thus believed that his willingness to propound the fully general form of his system was an important means of overcoming the Linnaeans. It would do so by stimulating criticism. Macleay thus insisted that his system should not be accepted before it had "been tried by several and severe tests" (Macleay 1819/1821, p. 166), and that synthesis was inadequate to provide such tests. A "severe" test, for Macleay, was an attempt to find, by analysis, some fact that contradicted the principles of the system, i.e. a group of organisms that could not be arranged into five subgroups, that did not admit of a circular arrangement, that did not show parallel chains of analogies, etc. Macleay twice justified his decision to publish his theory on the grounds that no phenomena "have yet been found to contradict it" (Macleay 1819/1821, p. 167; see also p. 345). In publishing his theory, Macleay was attempting to reorient analytical work from mere fiddling with Linnaeus' artificial groupings to genuine attempts to refute the quinarian system. Either such work would lead to the establishment of the quinarian system, or to the proposal of a more accurate system; either way, Linnaean dogmatism would come to an end.

Macleay thus developed a philosophy of science emphasizing the use of analysis to discover principles of arrangement, the use of synthesis to generalize these principles, and the subjection of these generalized principles to severe analytic tests. The purpose was to stimulate the search for the natural system, which required eradicating Linnaean indolence. Macleay, moreover, believed he had succeeded. In 1830, he wrote: "I myself was one of the first, I acknowledge, to declare just war against [the old Linnæan School of England]; but they are now at the last gasp. A few days more, and their existence will be a matter of history" (Macleay 1830a, p. 35).

5. The method of variation

Macleay's insistence on the use of analysis as the means to discover the principles of natural arrangement raised a further problem. Though he opposed the indolence of the Linnaeans, he granted their "indisputable" charge that all existing "natural" systems were "like the castle in a fairy tale, [that] falls to pieces on being tried by the talisman of truth" (Macleay 1819/1821, p. xviii). Macleay thus needed to provide some reason to think he would succeed where others had failed. He traced past errors to the methods of analysis used and developed a new method to rectify these errors. Macleay did not name it, but described it as tracing the "method of variation" of parts of organisms (Macleay 1819/1821, p. 454; Macleay 1827, p. 66). It may thus be reasonably dubbed the "method of variation."

One of the topics addressed by Roscoe (1815) was the relative merit of what were called (by Cuvier, followed by Macleay) "systems" and "methods," though Roscoe did not use those terms. A system used a single character to classify organisms, while a method used multiple characters.[7] Systems were easier to use, but methods promised to get closer to natural affinities. Because he believed that Jussieu's "natural" method in fact created an artificial arrangement, Roscoe could easily favor Linnaeus's sexual system. Jussieu's method, which involved "a separate mode of arrangement" for each of the three departments of the vegetable kingdom, was less simple to use than that of Linnaeus, who "applie[d] his method indiscriminately to the whole" (Roscoe 1815, p. 65).

Macleay believed that both systems and methods were incapable of producing anything but artificial systems: "a system or method entirely founded on *distinctions* must be artificial, whether such distinctions be drawn from the consideration of one or of one hundred parts" (Macleay 1819/1821, p. ix). The trouble lay in looking for distinctions that would cleanly separate groups, rather than in looking for affinities that would connect species together. In doing so, naturalists created rules of classification that, Macleay feared, violated the natural order. He cautioned, "we should not allow ourselves to be dazzled by the seeming simplicity of rules, so far as to overlook the cases where these rules interfere with the evident order of nature" (Macleay 1819/1821, p. 6).

Rather than seek to create rules of division, Macleay sought to trace the gradual variations of nature. He started from the principle, which he called the "maxim of variation," that parts that are shared between organisms may in some cases diminish or even disappear, while in other cases they may "undergo an extraordinary development" (Macleay 1830b, p. 151). This principle was to be the foundation for the study of the natural system: the naturalist should arrange organisms according to these gradual variations. For instance, while a method founded on divisions might take the absence of a part to mark a division between groups, by the method of variation it may simply mark the extreme end of a continuum within a group.

Macleay (1830b, p. 152n) further distinguished between "the comparison of organs" and "the comparison of animals." The naturalist was involved with the latter. While comparing animals of course required comparing their organs, the study of single organs could not provide principles of arrangement to be followed rigidly. Such comparisons must be seen in the context of the entire organism: in this context, what looked like an affinity when considering one organ might prove to be an analogy, on the grounds that the *other* organs in the species under comparison varied drastically. This was indeed Macleay's main complaint about methods founded on distinctions:

---

[7] Macleay adopted this characterization of the system/method distinction from Cuvier. For Cuvier, the distinction was important because systems could be used to create linear arrangements, while methods could not (see §6 below).

they invariably mistook analogies for affinities.

In *Horæ Entomologicæ* Part I, Macleay applied these methodological considerations to the arrangement of *Scarabeus*. In the first chapter of that work, Macleay offered an extended argument for his decision to focus on the mouthparts of his subjects, specifically the mentum and maxillae. From this discussion emerged several criteria by which to judge the usefulness of parts for classification. First, they should "less liable to variation, when viewed in a general manner," i.e. they should be assimilable to "one general plan of construction" (Macleay 1819/1821, p. 2). At the same time, they should be "subject in detail to all those endless changes of form" that result from adaptation to diverse modes of life (Macleay 1819/1821, p. 2; see also Macleay 1830b, p. 157).

Macleay illustrated this by drawing up a table (which was "to be considered with reference to the Coleopterous insects only") that showed, for a variety of parts, which ones were known to vary, in either form or number, both across sexes and species (fig. 4; Macleay 1819/1821, p. 4). From this table it can be seen that three parts (mentum, oculi, maxillae) are not sexually dimorphic (in either form or number) and occur in constant numbers across all species of Coleoptera. Thus they satisfy the first criterion. Their variability in form across species then provides the relevant "changes of form" required by the second criterion. Moreover, though Macleay relied primarily on the mentum and especially the maxillae in *Horæ Entomologicæ* Part I, he was explicit that "our system when completed will express the variation of all organs" (Macleay 1830a, p. 32).

6. Reconciling continuity and linearity

Though Macleay discovered his principles by the analytic method employed in *Horæ Entomologicæ* Part I and not the synthetic method employed in Part II, his discovery was not purely inductive, in the sense of eschewing all abstract, theoretical concerns. The understanding, presented above, of Macleay's system as emerging from his attempt to arrange *Scarabeus* is thus incomplete if we do not further recognize that Macleay's arrangement of *Scarabeus* was shaped by his attempt to resolve a theoretical puzzle. Macleay firmly believed that the natural system was both continuous (as Linnaeus wrote, "nature makes no leaps"; Freer 2005, pp. 40, 49, 243) and non-linear, in senses to be explained below. However, in prior systems, linearity and continuity were often accepted or rejected together. Macleay developed his circular system as, in part, a way of showing how a system could be continuous without also being linear.[8]

Macleay explicitly described his work as providing a resolution to the conflict between those who favored continuous, linear systems (such as Charles Bonnet) and those who preferred discontinuous, non-linear systems (such as Georges Cuvier):

> Bonnet fancied that, if affinities were continuous, the series must therefore be simple, and some modern naturalists finding by experience the series not to be simple, therefore supposed the affinities could not be continuous. […] It does not however appear that either of these inferences has been very philosophically drawn; for there is a certain rule in natural history which originates solely in observation, and which, if properly followed up, will infallibly induce us to grant to Bonnet the truth of his proposition, that affinities are continuous, and yet to agree with his opponents that the series of natural beings is not simple. This rule is, that *Relations of Analogy must be carefully distinguished from Relations of Affinity*. (Macleay 1825a, pp. 48-49)

In this important passage, Macleay presented his understanding of the theoretical context informing his work. In the work of prior naturalists, the continuity of nature appeared to require a linear ("simple") arrangement. But experience showed that no such arrangement could work. Nature could not be arranged in a line. Given the presupposition that continuity required linearity, this meant that nature must be discontinuous, and thus Cuvier treated his four *embranchements* (Appel 1980). Macleay believed that his distinction between affinity and analogy and the quinarian system he founded thereon gave him the means to reject this presupposition, and thereby harmonize continuity and non-linearity.

This immediately raises three questions. What was continuity, and what was the connection between continuity and linearity, in natural history prior to Macleay? What did Macleay mean by saying nature was continuous, and

---

[8] This view of Macleay's reasons for favoring circles should replace the view that Macleay got the idea of circles by modifying Lamarck's twin series (Desmond 1985; McOuat 1996; Coggon 2002; Williams and Ebach 2008). Though not strictly incompatible with the view I defend, the claim of a Lamarckian origin for Macleay's circles overlooks the synthetic nature of *Horæ Entomologicæ* Part II, in which Macleay applied principles discovered through analysis. Macleay's discussion of Lamarck served as independent confirmation of Macleay's *arrangement* of Animalia, not as inspiration for the *principles* of his system (Macleay 1819/1821, pp. 328-334). Ospovat (1981) and Panchen (1992, pp. 23-25) have recognized that reconciling continuity and linearity were important for Macleay.

what were his reasons for accepting that nature was both continuous and non-linear? Lastly, how did the quinarian system show continuity and non-linearity to be compatible? I take these up in turn.

Continuity, for Bonnet, was a fundamental organizing principle of his entire thought, not just his natural history, and he saw himself as giving Leibnizian metaphysics an empirical basis (Anderson 1982; Rieppel 1988). In natural history specifically, the way that the law of continuity manifested itself was via a linear chain of organisms, proceeding from the simplest to the glory of Man. In fact Bonnet's chain extended well beyond just organisms, in both directions, including both inanimate matter and celestial beings (Appel 1980). Cuvier, by contrast, argued strenuously against both continuity and linearity. Against linearity, Cuvier argued that the only way to produce a linear series was by considering only a single organ; different choices of organ would lead to different, incompatible series (Russell 1916, p. 39; Appel 1980). Against continuity, Cuvier argued that the functional integration among the parts of an organism did not allow intermediates between his four *embranchements*: each *embranchement* thus marked a group cleanly separated from all the others.

It does not appear that Macleay was quite right in saying that linearity and continuity were inextricably linked in the work of prior naturalists. Buffon, for instance, had defended a continuous but non-linear system (Coleman 1964, pp. 22-23), and, as Macleay himself noted, Lamarck's twin series (see fn8 above) introduced some branching into the scale of nature. Even more to the point, Cuvier's objections to continuity and linearity were independent: he did not infer from the failure of linearity to the failure of continuity. Even if Macleay exaggerated the connection, however, he was still touching on a real problem, for Cuvier had strong arguments against continuity. For Macleay, who accepted Cuvier's arguments against linearity (see fn9 below), this meant he had to show how linearity could be rejected without rejecting continuity as well.

The first thing to note is that Macleay's understanding of continuity differed from that of Bonnet. For Macleay, continuity in space and time was distinct from continuity in form. Though he admitted both as forms of a single "law," he insisted that they were nonetheless different kinds of continuity. In continuity of space and time, "an interval is impossible; and their continuity depends on this impossibility." To move from one spatial location to another, for instance, an object must pass through every point along some path between them: no points may be skipped over. "On the other hand, continuity in gradation of structure depends on the existence of intervals; but requires, in order that the gradation be more distinct, that these intervals be extremely small and numerous." If two structures were identical, then one would "produce no effect in continuing the chain of structure" (Macleay 1830a,

p. 22); thus, there must be some difference between them, and this difference, however slight, would constitute an interval.

Because of this difference, the continuity of space and time did not guarantee the continuity of organic structure (henceforth just "continuity"), and Macleay sought to establish continuity on independent empirical grounds (but see §10 below). Macleay's unwavering adherence to continuity was made possible by the time in which he lived, a time in which collectors were discovering foreign species at a high rate. Thus could Macleay (1830a, p. 19) write, "True it is, that chasms occur; but now, thanks to our collectors, those chasms are comparatively trifling, and moreover are every day filling up." In addition to this, he took himself and other naturalists to have shown that "continuity manifestly holds good in these particular parts of the Creation, which have been carefully examined," and, if so, "it may hold good in all" (Macleay 1830a, p. 22).

At the same time, however, Macleay knew that a linear system could not be maintained. Here, too, Macleay's reasons were empirical.[9] In *Horæ Entomologicæ*, Macleay illustrated the failure of continuity by considering an attempt to arrange the five subgroups of Animalia, in particular Annulosa, Mollusca, and Vertebrata. Any adequate arrangement of these groups, he argued, must respect several facts. First, Mollusca possess an affinity to Vertebrata through the Cephalopoda. Second, Annulosa possess an affinity to Vertebrata through cyclostomous fishes. Third, Annulosa are superior to Mollusca. If Mollusca were to be placed above Annulosa, then the lowest mollusks, "whose existence is little better than vegetative," would have to be placed above the highest insects, e.g. "the bee, which astonishes us by its industry and social qualities" (Macleay 1819/1821, pp. 204, 206). These three conditions "will be completely violated if we account [the chain of nature] to be a regular line or ladder, commencing with the *Infusoria* and terminating in Man" (Macleay 1819/1821, p. 206).[10]

[9] Macleay was also clearly influenced by Cuvier on this point, for he echoed Cuvier's argument that a linear series could be produced only by considering a *single* organ, and that different choices of organs would yield different series (Macleay 1819/1821, pp. 333-34; cf. Russell 1916, p. 39).

[10] Macleay is correct that no linear arrangement "terminating in man" could satisfy these conditions. However, a linear arrangement that placed Annulosa above Vertebrata and Vertebrata above Mollusca would respect all three conditions. Macleay did not consider this solution to the problem he raised, presumably because nobody defended such a system. Macleay's three conditions in effect contained a suppressed fourth condition: no group could be placed above Vertebrata. Interestingly, Macleay (1819/1821, p. 340) argued, later in *Horæ Entomologicæ*, that

Thus continuity and non-linearity were, for Macleay, both empirically established truths. Macleay presented his system as harmonizing these two truths. The crux was the affinity/analogy distinction, as the block quote at the start of this section attests. One of the major features sustaining linear systems was their use of analogical resemblances to connect groups. In other words, analogies were mistaken for affinities. By drawing a general distinction between the two kinds of resemblance, Macleay undermined empirical considerations undergirding linear arrangements. At the same time, he gave analogies a place in the natural system, but a secondary one: *after* organisms were arranged according to their affinities, regular patterns of analogy could be seen.

The affinity/analogy distinction played heavily into Macleay's circles, and the circles provided the specific pattern that was simultaneously continuous and non-linear. Affinity captured the relation between subgroups within a group, while analogy captured relations between the subgroups of different groups (Macleay even referred to analogy as "lateral affinity" at one point; 1819/1821, p. 32). Of course, to preserve continuity, there must be some cross-group relations of affinity (e.g. the Dynastidæ–Anoplognathidæ relation in fig. 5). These cross-group affinities illustrate Macleay's point that analogies and affinities themselves graded into one another (recall the tests of affinity and analogy). They were not quite as strong as the affinities between contiguous subgroups within a group (for they passed through osculant groups), but they were stronger than the other cross-group analogies. They were thus intermediate between the two.

The circles themselves were simply linear chains of affinity, but with one important additional property: the ends of the lines themselves bore affinity to one another. This important innovation allowed Macleay to preserve the benefits of discontinuous systems without having to accept discontinuity itself. Cuvier's *embranchements* were distinct groups; so too were Macleay's circles. It was unambiguous which groups were inside and which groups outside a given circle. Whereas a linear, continuous system did not admit of obvious breakpoints in the chain (for these would indicate discontinuity), Macleay's circles allowed a clear division between where one group ended and another began. At the same time, by placing osculant groups between his circles, continuity was preserved. One could move, via unbroken chains of affinity, from any one group to any other (of the same rank) no matter how distantly placed in the system. Thus, for instance, the Medusida (a subgroup of Radiata; fig. 1) could be connected to

---

Vertebrata and Annulosa "cannot very logically be compared" (with respect to perfection) because they were built on distinct plans. Thus, in the quinarian system, Annulosa were not ranked *below* Vertebrata.

Pisces by travelling through Stellerida, Echinida, Cirripeda, Crustacea, Ametabola, and Annelida – all connected by affinity.

Macleay's inosculating circles also fed into his notion of types. Peter Stevens (1994, p. 9) has plausibly suggested that most naturalists who invoked types tended to reject continuity. For Macleay, however, types helped to preserve continuity. Types defined the perfection of the tendencies evinced within a particular circle, and thus were connected with the two normal groups, those most distinct from contiguous circles. Aberrant groups represented deviations from the type. Recalling that perfection, for Macleay, was (or was evidenced by) complication of mechanism, this meant that aberrant groups tended to be simpler, and thus could more easily transition into another group, than the more specialized normal groups. Importantly, deviations from one type could be deviations *in the direction of* another type (more properly, in the direction of certain deviations from that other type). As the specializations characterizing the type were lost, the simpler deviations could approach one another, creating a bridge between the two groups. Such mutual deviations are, moreover, non-linear. In a linear system (of increasing perfection), the most perfect subgroup of one group would have to bear affinity to the *least* perfect subgroup of the next higher group. The connection of aberrant subgroups to aberrant subgroups, by contrast, ensured a non-linear structure.

We have seen two advantages of circles: they made a place for both affinities and analogies without conflating the two, and they preserved the distinctness of groups and types without sacrificing continuity. There was a third: they were not beset by the empirical problems troubling linear systems. Macleay's discussion of the difficulties faced in arranging Annulosa, Mollusca, and Vertebrata did not merely expose the poverty of linear systems. It further served as a triumph of the circular system: "Now these conditions will all be fulfilled if the chain of nature be viewed as returning into itself" (Macleay 1819/1821, p. 206).

7. The place of analogies in the quinarian system

Thus can be seen how Macleay's system showed that continuity and non-linearity were not incompatible, and how various elements of Macleay's system fed into resolving this theoretical difficulty. Nonetheless, it may be asked at this point why analogies needed a place in the system at all. It was affinities that connected distinct circles, and thus that were important for continuity. The reconciliation of continuity with non-linearity did not require that analogies be important at all, let alone require that they run in parallel. Moreover, it was the parallelism of analogies that guaranteed, for Macleay, the existence of a determinate number of subgroups per group. Two of the most conspicuous elements of Macleay's system thus were not required to reconcile continuity and non-linearity. Why, then, were they part of his system?

There is little textual evidence that clearly indicates why Macleay thought analogies required a place in his system. They first appear in Macleay's discussion of his arrangement of *Scarabeus*: "It will next be remarked, that the families in one of these columns have each a striking similarity of general form to the corresponding families in the other column" (Macleay 1819/1821, p. 27). It is possible that this one instance of regular pattern set Macleay to searching for it everywhere. As Macleay's contemporaries argued, stray resemblances between distant groups were not terribly difficult to find, so, once he set about looking for them, he was likely to find them.[11] But other explanations are possible, too.

However Macleay arrived at the parallelism of analogies, once it was in place, the existence of a determinate number followed. Macleay's argument on this point is set out in his paper, "Remarks on the Identity of certain general Laws which have been lately observed to regulate the natural Distribution of Insects and Fungi" (1825a, pp. 55-56). The crux of the argument is that perfect parallelism requires an equal number of groups, for otherwise at least one group will be left without a partner. Since analogies can be traced (at various removes) between all corresponding subgroups at a given rank, there must be a determinate number for each rank. This can be seen by comparing figures 2 and 3. Take Branchiopoda, Vermes, Hymenoptera, Diptera, and Acaridea, which all occupy corresponding positions within their respective circles (fig. 2). As figure 3 then shows, Vermes has an analogy to Hymenoptera, Hymenoptera to Diptera, Diptera to Acaridea, Acaridea to Branchiopoda, and Branchiopoda back to Vermes.

There is a hole in this reasoning. It only shows that all subgroups of Annulosa must have the same number of sub-subgroups. But what about the other circles of Animalia: do the sub-subgroups of Annulosa have analogies to sub-subgroups of Vertebrata? This was required to ensure that there is a determinate number for *all* groups at a given rank. But Macleay never explicitly claimed that such analogies exist: neither in the abstract nor by presenting

[11] Note, however, that Macleay was not as opportunistic as Swainson in finding analogies. The analogies Macleay presented between groups usually followed a theme: those between Saprophaga and Thalerophaga all concerned the form of the body and the mouthparts (Macleay 1819/1821, pp. 25, 27), and those between the Mandibulata and Haustellata all concerned metamorphosis (p. 367). They were not arbitrarily drawn from just anywhere.

such analogies. We must assume that he would have answered affirmatively if asked whether there were such analogies, but he was never asked and never answered.

With the existence of a determinate number at a given rank established, it remained to show that the same number existed at all ranks. Macleay (1825a, p. 55), citing the authority of Elias Fries (1821), claimed, “class bears the same relation to class which order does to order.” But this merely asserted the fact and did not defend it. However, Macleay did show that some analogies existed between groups of different ranks. For instance, the subgroups of Mandibulata (1819/1821, p. 433) bore relations of analogy to the subgroups of Neuroptera (p. 440), itself a subgroup of Mandibulata. Likewise, Macleay (1819/1821, p. 223) argued that, in the five subgroups of the Acrita, “Nature, so far from forgetting order, has, at the commencement of her work, in these imperfect animals given us a sketch of the five different forms which she intended afterwards to adopt for the whole animal kingdom,” another instance of analogies between groups at different ranks. Macleay did not point out such cases nearly as frequently as cases of within-rank analogies, but their existence did provide some support to Macleay’s claim.

Having arrived at the end of the presentation of the motivations underlying Macleay’s development of his system, we may recapitulate. Macleay saw the Linnaean contentment with artificial systems as an obstacle to the development of a truly scientific natural history. To remove this obstacle, Macleay developed a philosophy of science in which bold hypotheses were to be proposed as synthetic generalizations of principles discovered by analysis, then subjected to severe analytic tests. Macleay further developed a novel method of analysis, the method of variation, that he believed was adequate to discover natural affinities. The application of this method to *Scarabeus* led to his discovery of the principles constitutive of the quinarian system. These principles were also shaped by Macleay’s twin commitment to continuity and non-linearity, as they showed how these two commitments could be made compatible. Macleay’s views on the parallelism of analogies and the existence of a determinate number in nature were less essential to this reconciliation, and were supported by problematic arguments. They were not, however, wholly disconnected from the other aspects of his system.

8. Macleay’s system was not especially religious

With the above understanding of how Macleay arrived at the various elements of his system in place, we may turn to evaluating two mistaken views often found in the secondary literature on Macleay. The first is that the quinarian system was an especially religious way of arranging the denizens of the organic world. The basic idea behind this view is that Macleay's faith gave him "confidence in order and plan," and that this explains why Macleay settled on such an intricate and regular system (Ospovat 1981, p. 106; see also Rehbock 1983, p. 26). While Macleay certainly interpreted his system as (an approximation of) the plan followed by God in the creation, I contend that this fact provides little help in understanding why Macleay's system was so different from the others of his time, for two key reasons.

First, the interpretation of the natural system as the plan of creation was simply too widespread at this time to explain why Macleay's views diverged from those of his contemporaries. It was the default interpretation, and it encompassed views that varied dramatically in the degree of order they tolerated. Hugh Strickland (1840, 1841), for instance, who ranked among Macleay's most stringent critics and who argued that there was *no* regularity in the natural system (see §10 below), equally well accepted that the natural system was the plan of creation. Simply interpreting the natural system in this way thus did not commit one to any particular view about its orderliness, and thus Macleay's religious interpretation of his work cannot explain the stark differences between his highly regular system and Strickland's irregular "maps."

Second, Macleay believed that explicitly religious arguments had no place in natural history. In the introduction to *Horæ Entomologicæ* Part II, Macleay discussed the objections that had been raised against his system. Among these was the following: "I have been told that the idea of a chain of beings returning into itself militates against those notions of an ascending scale in nature, which […] are inculcated by revelation" (Macleay 1819/1821, p. 162). Macleay (1819/1821, p. 165) then grumbled that this objection "does not depend on experiment." That this made the objection irrelevant, in Macleay's eyes, may be seen by his comment on the subsequent page: "In the present happy state of science, which is founded solely on observation and experiment…" (Macleay 1819/1821, p. 166). An objection not dependent on experiment was no real objection at all.

In keeping with this mistrust of religious reasoning in natural history, Macleay on numerous occasions railed against others for their use of religious arguments. For instance, he condemned those who chose, on biblical grounds, to base their system on the number seven: "The number seven might also, perhaps, for obvious reasons, occur to the mind, were it allowable in natural history to ground any reasoning except upon facts of organization" (Macleay 1825a, p. 57). In his vituperative response to John Fleming, who had invoked biblical authority for his dichotomous system, Macleay complained that "it is really surprising how this eleventh chapter of Leviticus is shoveled into our faces by various writers" and that "Clergymen […] have too great a propensity to silence all inquiry with a text" (Macleay 1830a, p. 5). Macleay (1830a, p. 6) further wrote: "No one but a madman, a fanatic, or an interested knave, can pretend to tell us that [the Bible] is an encyclopædia of science" (see also Macleay's letter to W.B. Clarke, July 4, 1842, in Branagan and Vallance 2008). Finally, late in his life, Macleay read Darwin's *Origin*. Though no transmutationist himself, Macleay expressed the hope that Darwin's views would not be silenced by religious criticism (Holland 1996).

On these matters, Macleay was no hypocrite: nowhere in his work did Macleay seek to establish any particular feature of the quinarian system on religious grounds. The closest he came to doing so was in the following passage, which was part of Macleay's account of the affinity/analogy distinction:

> We shall thus have two very different relations, which must have required an almost infinite degree of design before they could have been made exactly to harmonize with each other. When, therefore, two such parallel series can be shown in nature to have each their general change of form gradual, or, in other words, their relations of affinity uninterrupted by any thing known—when moreover the corresponding points in these two series agree in some one or two remarkable circumstances, there is every probability of our arrangement being correct. It is quite inconceivable that the utmost human ingenuity could make these two kinds of relation tally with each other, had they not been so designed in the creation. (Macleay 1819/1821, p. 363)

Macleay here argued that if analysis led to an arrangement in which there were (A) unbroken circular chains of affinity and (B) analogies running in parallel across these circles, such an arrangement was probably correct, on the grounds that it is highly unlikely that such an arrangement could be created by mere human ingenuity. This takes for granted the view that the natural system was the plan of creation, but no more. Once again, Macleay's use of religious argumentation here runs no deeper than what we can find in Strickland's work, for Strickland (1840, p. 224) opposed the divine beauty of Macleay's analogies with the view that such regular analogies were "derogatory to the Creative Power." Strickland was there arguing that only affinities, not analogies, were reflective of divine intent. The problem was to determine which resemblances deserved consideration in the search for the natural system, and it was discussed against the shared background of viewing the natural system as tracing the divine plan of creation. Beyond this background, Macleay's system found no especially religious confirmation.

Nonetheless, as Ospovat notes, Macleay did claim that his system found "some of its very best supports" in the doctrines of revealed religion (Macleay 1819/1821, p. 165). Why did he say this? I propose that this is best understand as an exasperated response to criticisms of his system as in conflict with Christian doctrine. We have already seen that Macleay had encountered such criticism regarding Part I of *Horæ Entomologicæ*, and that he was frustrated by the criticism on account of its being non-empirical. With this in mind, a fuller presentation of the quote is instructive: "The object, therefore, of the present Essay is to show in a general but very rapid manner, […] that so far is this plan from militating against the doctrines of revealed religion, that it will be found to depend on these as some of its very best supports" (Macleay 1819/1821, p. 165). However, in keeping with his aversion to religious argumentation, Macleay did not return, later in the work, to these doctrines in order to show in detail how they formed the "very best supports" of his system. The claim is thus best viewed as a tossed off response to an aggravating criticism, not as illuminating the motivations underlying Macleay's development of his system.

The apparent religiosity of Macleay's system is thus a reflection of generic beliefs about the natural system and is not a feature that sets the quinarian system apart from its rivals. Macleay was rather opposed to than supportive of the use of religious reasoning in natural history.

9. Macleay's system was not idealistic

The most pervasive myth surrounding the quinarian system is that it was in some sense idealistic (or mystical, or numerological, or transcendental).[12] This is almost always taken to reflect negatively on Macleay's work, and it is likely that some, if not all, of the neglect of his work is due to the tendency to accept Macleay's supposed idealism as a sufficient explanation of the intricacy of his system. Macleay, however, was not an idealist, a mystic, or a numerologist.[13] It is noteworthy that Macleay's contemporaries, with one possible exception (discussed below), did *not* consider his system idealistic. The view appears to be restricted to contemporary historiography.

Those who consider Macleay an idealist generally either trace his idealism to Pythagorean mathematical

[12] Mystical: de Beer (1963, p. 13). Numerological: Ghiselin (1969, p. 104). Transcendental: Bowler (1976, p. 55). For the charge of Macleay as an idealist, see fn14 below.

[13] Recall that Macleay and Swainson offered distinct versions of the quinarian system. Swainson, who linked the number three to the Christian trinity, might be said to have been something of a numerologist (I am less certain he can justly be called an idealist).

idealism or to the German *Naturphilosophie* movement.[14] The former view emphasizes the role that the fixed number five plays in the quinarian system; the latter view takes the system to be an outgrowth of the work of the *Naturphilosophen*, who were heavily influenced by the German idealistic tradition in philosophy that emerged in the wake of Kant. I shall address these views in turn.

The view that Macleay's system falls into the Pythagorean tradition of mathematical idealism does not turn on any claim of direct influence: rather, it views such idealism as inherent to the importance Macleay placed on the number five and on the geometrical figure of the circle. In fact, however, Macleay explicitly cautioned against such understandings of his system, on both counts. He insisted that there was nothing special about treating chains of affinity as *circular*. The image of the circle merely represented, in a convenient fashion, the fact that the first and last groups in a chain of affinity bore an affinity to one another, i.e. that chains returned into themselves. "This property of a distribution […] for convenience only we have considered circular" (Macleay 1819/1821, p. 319) and "might be represented by any curve, such a circle or ellipse, having this property" (Macleay 1819/1821, p. 163). For that reason, Macleay was frustrated by criticisms he received (presumably in person) that attacked his circles and not the affinities they represented: "it was useless to deny generally the truth of the circle, while certain affinities were unmolested of which it was only used as a symbol" (Macleay 1819/1821, p. 164). Already in 1821, Macleay was defending himself against "geometrical" interpretations of his circles.

Likewise for the number five: it had no more than empirical significance for Macleay. We have seen above (§7) how Macleay argued for the existence of a determinate number throughout nature: it was required by the parallelism of analogies. This argument was problematic, but offering a sketchy argument is not idealism. Once this argument was in place, Macleay (1825a, p. 56) argued, "it only remains for the naturalist to discover from observation what this number is," and the only criterion to assess a given number was its ability to create a natural arrangement. As noted above (§8), Macleay (1825a, p. 57) explicitly condemned those who chose the number seven on account of its "obvious" significance. It is also worth noting that Macleay (1825a, p. 62) dismissed the "doctrine of *quintessence*" (found in the works of authors such as Plutarch, Linnaeus, and Thomas Browne) as "speculations, often unintelligible and always vague."

---

[14] Pythagorean idealism: Stresemann (1975), Mayr (1982), di Gregorio (1982), Rehbock (1983). *Naturphilosophie*: Stresemann (1975), McOuat (1996), Endersby (2005). Stevens (1984) and Blaisdell (1982) consider Macleay an idealist but do not tie him explicitly to either tradition.

Macleay was thus not a Pythagorean idealist. What about his debts to *Naturphilosophie*? Here there are two questions. First, was Macleay directly influenced by their work? Second, even if he was not influenced by their work, did he nonetheless share ties to the German philosophical tradition that informed their systems? Both questions are to be answered in the negative.

As Rehbock (1983, pp. 27-28) correctly notes, Macleay's "writings show no obvious manifestations of the Naturphilosophie tradition." Macleay's primary influences were not German but French, no doubt due to the years he spent in France (1814-1818), during which he conducted the research that eventuated in *Horæ Entomologicæ*. Citations of the work of *Naturphilosophen* are rare in Macleay's writings. In *Horæ Entomologicæ*, Macleay (1819/1821, pp. 314, 317) cited only Johann Baptist von Spix, and then only for certain details of the anatomy and habits of the Radiata. In later works (Macleay 1825a; Macleay 1833, pp. 8-12), Macleay cited two further *Naturphilosophen*, Elias Magnus Fries and Gotthelf Fischer von Waldheim. In both cases, however, Macleay explicitly stated that he encountered their work only *after* writing *Horæ Entomologicæ*, and he cited them as independent discoverers of systems similar to his own. That same paper from 1825 also contains a citation of Lorenz Oken, but only to criticize him for running into the difficulties that attend having two determinate numbers (namely, the disruption of the parallelism of analogies). There is no reason to think that any of these four *Naturphilosophen* especially influenced Macleay; indeed, there is good reason to think they did not.

Even without such direct influence, Macleay might still have accepted some of the same idealistic tenets that can be found in the work of the *Naturphilosophen*. Here we may compare Macleay's system to the mycological system of Fries. Fries developed his system roughly simultaneously with Macleay: his major work expounding it appeared in 1821 (Fries 1821). Macleay, in 1822, wrote a paper that served both to lay out the main elements of his system and to compare his system with that independently expounded by Fries (Macleay 1825a). As the title indicates, Macleay saw substantial "identity" between the laws of the two systems. In particular, Macleay highlighted three key similarities between the two systems: both distinguished between affinity and analogy and both involved circular chains of affinity with a definite number of subgroups.[15]

Comparing the two systems is instructive because Fries explicitly offered an idealistic interpretation of his system, in the sense that he saw it as pointing to an eternal order of ideas underlying the diversity of nature.

---

[15] In fact, Fries thought the determinate number was four, and Macleay had to argue at some length that Fries' four subgroups were resolvable into five.

Macleay, by contrast, saw the quinarian system as doing no more than capturing patterns within this diversity. In other words, while Macleay and Fries offered quite similar arrangements, they interpreted these arrangements rather differently.

In the introduction to his *Systema Orbis Vegetabilis*, Fries laid out the philosophical basis of his system (Lindley 1826).[16] One aspect of this basis was Fries' distinction between artificial and natural systems. Like Macleay, Fries compared artificial systems to "indexes of nature" that "have reference only to names" (Lindley 1826, p. 84). However, when it came to the natural system, Fries distinguished several types, characterized by their methodologies. Of these, *Philosophical systems* were the "superior" type. These "do not depend upon individual productions which are subject to continual variation, but upon eternal and unchangeable ideas" (Lindley 1826, p. 85). These systems rested "upon the laws of logic," which are "eternal and immutable, and established by Nature herself" and could be deduced from first principles (though Fries did to note that a selection of false principles would lead to the deduction of a "naturally false" but "logically true" system).

For Macleay, by contrast, the natural system consisted in "*all* the phænomena of natural history" (Macleay 1819/1821, p. x). That is, the natural system consisted in the arrangement of all organisms according to their affinities. There might be patterns within such an arrangement, but these patterns did not point to and could not be deduced from underlying ideas. Indeed, for Macleay (1819/1821, p. x), the order was reversed. Patterns were to be deduced from the phenomena:

> In natural philosophy a system has usually indeed been considered as synonymous with an hypothesis; but the two ideas expressed by these words have of late been very properly distinguished by observing that though a mere fiction or hypothesis may explain phænomena, yet a system is a certainty that must be deduced from these.

This quote shows that Macleay took the quinarian system *not* to be a hypothesis that could explain the patterns in organic nature in virtue of its being deeper than those patterns. Rather, Macleay took the quinarian system to be nothing other than the patterns themselves, which could be "deduced" from the phenomena. That is, once a group of organisms was arranged according to its affinities and found to be circular (and to have five subgroups, etc.), there was nothing more to say about that instance. The circle did not explain why the series returned into itself: it just *was* that property of the distribution, as we saw earlier in this section. The patterns of the quinarian system were just the

[16] The paper by Lindley primarily consists of a translation of the introduction to Fries (1825) into English.

repetition of such distributional properties throughout the organic world.

Eternal, immutable ideas, by contrast, cannot be *deduced* from transient, mutable nature, even if they may be inferred therefrom. This point also had implications Macleay's and Fries' respective methodologies, which followed from their opposed views on the nature of the natural system: Fries proceeded from ideas to classification, Macleay from classification to regularities. Fries, in other words, did not seek to discover his principles of arrangement by analysis. Importantly, Macleay called the results of his synthetic work a "hypothesis" in the above sense (see §4 above).

Macleay's regularities were just that: regularities. These regularities were the result of God's plan of creation, but Macleay gave them no further interpretation in terms of underlying ideas. In this regard, he was once again in agreement with Strickland, who viewed the natural system as "an accumulation of facts" (Strickland 1841, p.185). Despite their methodological and empirical disagreements (see §10 below), Strickland and Macleay were in fact in basic agreement as to the nature of the natural system. If Strickland is not considered an idealist, and he is not, why should Macleay be treated differently?

It is interesting that this discrepancy in the self-interpretation of Fries and Macleay did not dampen Macleay's assessment of the "identity" between their two systems. In fact, if anything, it made Macleay *more* enthusiastic. For the most part, Macleay simply ignored these aspects of Fries, with one telling exception. In his argument that Fries' four groups could be resolved into five, Macleay noted triumphantly that though Fries had deduced the number four from logical considerations, empirical facts forced Fries to admit that one of these four groups was further resolvable into two, making five total. Macleay took this as especial confirmation that the empirical facts supported his view over Fries' view, and that the deduction of the number four (which found its source in Fries' idealism) could be safely ignored.

Finally, that Macleay was no idealist may be seen by considering chapter two of *Horæ Entomologicæ* Part II, for Macleay therein laid out his metaphysics. He claimed that only the following types of things exist: one primary cause (God), many secondary causes (e.g. human souls), matter, space, and time (Macleay 1819/1821, p. 179). Immutable, eternal, underlying ideas appeared nowhere in this chapter. The closest Macleay came to mentioning them was in footnote in which he dismissed as unworthy of notice the claim that ideas exist as immediate objects of perception, on the grounds that "if we admit the existence of ideas as immediate objects of perception, it follows that there is nothing else in the universe but ideas" (Macleay 1819/1821, p. 180). He used the same reasoning to dismiss

Leibniz's monads.

The view that Macleay was an idealist is simply false. He explicitly endorsed a non-idealistic metaphysics, he argued against overly mathematical interpretations of his circles and against attaching any importance (besides truth) to the number five, and he was not influenced by the genuinely idealistic tradition of *Naturphilosophie*. Nor can the view be rescued by looking to the criticisms raised against Macleay in his own time. For Macleay's contemporaries, his system was *a priori*, absurd, atheistic, empirically disastrous, and many other vicious things, but it was conspicuously *not* idealistic.

The only potential exception to this rule is a critique of the quinarian system written by James Rennie for his introduction to Montagu's *Ornithological Dictionary of British Birds*. There, Rennie claimed, in passing, that Macleay's doctrine of types was "a shoot from Plato's wild theory of pre-existent ideas" (Montague and Rennie 1831, p. xxxviii). This was not Rennie's major criticism (he was more concerned to show that Macleay's system was atheistic), and it is not indicative of any general tendency to regard Macleay's views as idealistic. It is, moreover, a "wild" misreading of Macleay. Macleay was quite clear that his types were either real or abstract. In groups of lower rank, the type could be an actually existing species. In groups of higher rank, however, perfections were dispersed in nature: different parts were perfected in different groups. In that case, the type was "a sort of ideal being, uniting in itself all the various perfections of organization which may in reality be dispersed throughout the group" (Macleay 1819/1821, pp. 273-74, see also p. 405). By "ideal," Macleay merely meant that the type was more perfect than any actual organism, and he denied any real existence to these abstract unions of dispersed perfections: "it may eventually be necessary to describe that imaginary being, the Annulose type of form."

Macleay was no idealist.

10. Hugh Strickland and the demise of the quinarian system

Though Macleay spoke of subjecting his theory to "severe tests," in practice he insulated his theory from criticism by making it very difficult to actually find facts that contradicted it. In this way he became a dogmatic defender of his own system. Hugh Strickland picked up on this dogmatism and developed the arguments that ended the quinarian system's popularity.[17]

The first way in which Macleay insulated his system came in response to an obvious objection. Many groups,

[17] For Strickland's role, see Wallace (1855); Ospovat (1981); McOuat (1996).

on the basis of the species then known, could not be divided into five natural subgroups, e.g. Mollusca (fig. 1, missing groups are labeled ***). Macleay (1819/1821, pp. 368-69) suggested that missing groups either had gone extinct or were yet to be discovered, and made predictions as to what would fill the gaps. For instance, to fill the hiatus between Reptilia and Cephalopoda, "some Chelonian reptile must be found destitute of feet" (Macleay 1819/1821, p. 261). However, Macleay made it nearly impossible to falsify this prediction: one could not claim that intervening forms did not exist until "the bosom of the deep, and the vast tracts of land which remain, to the peculiar disgrace of England, still unexplored by the naturalist, shall have delivered up their treasures to the eye of science" (Macleay 1819/1821, pp. 258-59).

John Fleming (1829, pp. 324-35) took up this prediction in his critique of Macleay, deeming it "desperate" and accusing Macleay of violating his own doctrine of affinity and analogy. Macleay, in his response, insulated his theory in a second way. Instead of relying on the *test* of affinity and analogy, Macleay invoked the *nature* of the distinction (which, recall, assumed the truth of major aspects of the quinarian system). Relations that do not show parallelism, he argued, *cannot* be analogies. There was no parallelism in the case at hand, so the Reptilia-Cephalopoda relations "cannot be relations of analogy, and therefore must be relations of affinity" (Macleay 1830a, p. 30). This defense merely presupposed that the quinarian system was correct.

Thus Macleay became dogmatic.[18] Just as Macleay had reacted against the dogmatism of the Linnaeans, the geologist and ornithologist Hugh Strickland reacted against Macleay's dogmatism. Where Macleay sought to reconcile continuity with non-linearity, Strickland offered powerful arguments against continuity. Where Macleay reacted against Linnaean dogmatism by promoting a bold theory, Strickland reacted against quinarian dogmatism by eschewing theorizing altogether.

Strickland's most powerful argument against the quinarian system drew on his experience as a geologist, as well as on his functionalism. Strickland (1840) claimed that all modifications of animal form were closely tied to conditions of existence. Because of this, irregularity and discontinuity in the geological and climatic features of the earth ought to be reflected in the variation of animal forms. Thus, this variation should, *a priori*, be expected to be

[18] The claim that Macleay did not fully live up to his epistemological strictures should not be assimilated to the claim that Macleay's philosophical views were purely rhetorical. Macleay genuinely attempted to fulfill them; he simply did not always succeed.

irregular and discontinuous (Strickland 1841, p. 187; Strickland 1845).[19] Though chains of affinity might in some cases be circular, this could not be expected to recur regularly.

Given that no regularity could be expected to obtain in the natural system, Strickland proposed a cautious, inductive methodology for natural history. In Macleay's terminology, he rejected any role for synthesis, arguing that "the natural system is an accumulation of facts which are to be arrived at only by a slow inductive process" (Strickland 1841, pp. 185-86). He bolstered his argument for a purely analytic methodology by noting how quinarians forced facts to fit their theory, both by assuming missing groups to be either extinct or unknown and by changing the ranks of groups to equalize their numbers (Strickland 1841, p. 186). He realized, that is, that Macleay's predictions did not make possible genuine tests, because Macleay insulated them from failure. Strickland accordingly rejected any role for prediction in classification (Strickland 1841, p. 189; Strickland 1845, p. 177).

Strickland raised his objections in print and at the 1840 and 1843 meetings of the British Association for the Advancement of Science (BAAS). By 1843, no major quinarians remained in England: Macleay moved to Australia in 1838, Swainson moved to New Zealand in 1840, and, also in 1840, Vigors died. Vigors, before his death, responded to Strickland at the 1840 BAAS meeting. This was the only response any quinarian offered to Strickland, and it was placatory: one should not assume the quinarian system was the natural system, it was merely pragmatic, it could coexist with other systems – in short, *it was only one artificial system among many* ([Anonymous] 1843). Had Macleay, who though his system "useless" if not natural (Macleay 1819/1821, p. 322), heard Vigors' response, it would have struck him as capitulation. Yet it was all that was on offer in 1840, and, by 1843, even that was gone. As a candidate in the search for the natural system, the quinarian system was finished.

[19] A similar argument appears in Blyth (1835).

Acknowledgments

I owe thanks to Michal De-Medonsa for beautifully recreating Macleay's original figures and to James Lennox, Aleta Quinn, Elay Shech, Nora Boyd, David Colaço, Laura Novick, and two anonymous reviewers for *Journal of the History of Biology* for comments on drafts.

Figure captions

Fig. 1. Macleay's arrangement of Animalia (Macleay 1819/1821, p. 318). This and all subsequent figures were redrawn from the originals by Michal De-Medonsa.

Fig. 2. Macleay's arrangement of Annulosa (Macleay 1819/1821, p. 390).

Fig. 3. Analogies among the Annulosa (Macleay 1819/1821, p. 395).

Fig. 4. Variation of diverse organs within Coleoptera (Macleay 1819/1821, p. 4).

Fig. 5. Macleay's original quinarian arrangement of *Scarabeus* (Macleay 1819/1821, p. 29).

All images were created in Photoshop.

References

Anderson, Lorin. 1982. *Charles Bonnet and the Order of the Known*. Dordrecht: D. Reidel Publishing Company.

[Anonymous]. 1843. [Report of BAAS meeting]. *Athenaeum* 829 (16 September):851.

Appel, Toby. 1980. "Henri de Blainville and the Animal Series: A nineteenth-century chain of being." *Journal of the History of Biology* 13 (2):291-319.

– – – –. 1987. *The Cuvier-Geoffroy Debate: French biology in the decades before Darwin*. Oxford: Oxford University Press.

Barrett, Paul H., Peter J. Gautrey, Sandra Herbert, David Kohn and Sydney Smith, eds. *Charles Darwin's Notebooks: 1836-1844*. Cambridge: Cambridge University Press.

Blaisdell, Muriel L. 1982. "Natural Theology and Nature's Disguises." *Journal of the History of Biology* 15 (2):163-189.

– – – –. 1992. *Darwinism and its Data: The adaptive coloration of animals*. New York: Garland Publishing.

Blyth, Edward. 1835. "Observations on the Marked Seasonal and Other External Changes Which Regularly Take Place in Birds." *The Magazine of Natural History* 8:40-53.

– – – –. 1836. "Further remarks on affinities of the feathered race and on the nature of specific distinctions." *The Magazine of Natural History* 9:505-514.

Bowler, Peter J. 1976. *Fossils and Progress: Paleontology and the Idea of Progressive Evolution in the Nineteenth Century*. New York: Science History Publications.

Branagan, D.F. and T.G. Vallance. 2008. "Some Unpublished Correspondence of the Rev. W.B. Clarke." *Journal & Proceedings of the Royal Society of New South Wales* 141:1-31.

Coggon, Jennifer. 2002. "Quinarianism after Darwin's 'Origin': The circular system of William Hincks." *Journal of the History of Biology* 35 (1):5-42.

Coleman, William. 1964. *Georges Cuvier, zoologist: A study in the history of evolution theory*. Cambridge, MA: Harvard University Press.

De Beer, Gavin. 1963. *Charles Darwin: evolution by natural selection*. Garden City, NY: Doubleday and Company.

Desmond, Adrian. 1985. "The Making of Institutional Zoology in London 1822-1836: Part 1." *History of Science* 23 (60):153-185.

Di Gregorio, Mario A. 1982. "In search of the natural system: Problems of zoological classification in Victorian

Britain.” *History and Philosophy of the Life Sciences* 4 (2):225-254.

– – – –. 1996. “The Uniqueness of Charles Darwin: His reading of W.S. MacLeay’s *Horae Entomologicae*.” *Historical Records of Australian Science* 11 (2): 103-117.

Endersby, Jim. 2005. “Classifying Sciences: Systematics and status in mid-Victorian natural history.” In *The Organisation of Knowledge in Victorian Britain*, ed. Martin Daunton. Oxford: Oxford University Press, pp. 61-87.

[Fleming, John.] 1829. “On Systems and Methods in Natural History. By J.E. Bicheno, Esq. 1829. (*Linn. Trans.*, xv, part 2.).” *Quarterly Review* 41:302-327.

Fletcher, J.J. 1920. “The Society’s Heritage from the MacLeays.” *Proceedings of the Linnaean Society of New South Wales* 45:567-635.

Freer, Stephen, trans. 2005. *Linnaeus’ Philosophia Botanica*. Oxford: Oxford University Press.

Fries, Elias Magnus. 1821. *Systema mycologicum : sistens fungorum ordines, genera et species, huc usque cognitas, quas ad normam methodi naturalis determinavit*. Lundæ: Ex Officina Berlingiana.

– – – –. 1825. *Systema Orbis Vegetabilis*. Lundæ: Typographia Academica.

Ghiselin, Michael T. 1969. *The Triumph of the Darwinian Method*. Berkeley: University of California Press.

Holland, Julian. 1996. “Diminishing Circles: W.S. MacLeay in Sydney, 1839-1865.” *Historical Records of Australian Science* 11 (2): 119-147.

Jenyns, Leonard. 1835. “Report on the Recent Progress and Present State of Zoology.” *Report of the British Association for the Advancement of Science* 4:143-251.

Kirby, William. 1825. “A description of some insects which appear to exemplify Mr. William S. MacLeay's doctrine of affinity and analogy.” *Transactions of the Linnean Society of London* 14:93-110.

Larson, James L. 1967. “Linnaeus and the Natural Method.” *Isis* 58 (3):304-320.

Lindley, John. 1826. “Some Account of the Spherical and Numerical System of Nature of M. Elias Fries.” *The Philosophical Magazine* (1st ser.) 68:81-91.

– – – –. 1830. *An introduction to the natural system of botany: or, A systematic view of the organisation, natural affinities, and geographical distribution, of the whole vegetable kingdom : together with the uses of the most important species in medicine, the arts, and rural or domestic economy*. London: Longman, Rees, Orme, Brown, and Green.

[Luxford, George]. 1850. “Natural Systems of Botany: review of Lindley’s *Vegetable Kingdom* and other works.” *Westminster Review* 54:38-65.

Macleay, William Sharp. 1819/1821. *Horae Entomologicae: or, Essays on the annulose animals*. London: S. Bagster.

––––. 1825a. “Remarks on the Identity of certain general Laws which have been lately observed to regulate the natural Distribution of Insects and Fungi.” *Transactions of the Linnean Society of London* 14 (1):46-68.

––––. 1825b. “Anatomical Observations on the Natural Group of Tunicata, with the Description of three Species collected in Fox Channel during the late Northern Expedition.” *Transactions of the Linnean Society of London* 14 (3):527-555.

––––. 1825c. *Annulosa javanica, or, An attempt to illustrate the natural affinities and analogies of the insects collected in Java by Thomas Horsfield and deposited by him in the museum of the Honourable East-India Company.* London: Kingsbury, Parbury, and Allen.

––––. 1827. “On the Structure of the Tarsus in the Tetramerous and Trimerous Coleoptera of the French Entomologists.” *Transactions of the Linnean Society of London* 15:63-73.

––––. 1829a. “A Reply to some Observations of M. Virey in the ‘Bulletin des Sciences Naturelles, 1825.’” *The Zoological Journal* 4:47-51.

––––. 1829b. “A letter to J.E. Bicheno in examination of his paper ‘On systems and methods’: in the Linnean Transactions.” *The Philosophical Magazine* (2nd ser.) 6:199-212.

––––. 1830a. *A Letter on the Dying Struggle of the Dichotomous System*. London: Richard Taylor.

––––. 1830b. “Explanation of the Comparative Anatomy of the Thorax in Winged Insects, with a Review of the present state of the Nomenclature of its parts.” *The Zoological Journal* 5:145-179.

––––. 1833. “Remarks on the Comparative Anatomy of certain Birds of Cuba, with a view to their respective Places in the System of Nature or to their Relations with other Animals.” *Transactions of the Linnean Society of London* 16 (1):1-46.

––––. 1838. *Illustrations of the Annulosa of South Africa : being a portion of the objects of natural history, chiefly collected during an expedition into the interior of South Africa, under the direction of Dr. Andrew Smith, in the years 1834, 1835, and 1836, fitted out by the Cape of Good Hope Association for Exploring Central Africa*. London: Smith, Elder.

– – – –. 1840a. “Observations on Trilobites, founded on a comparison of their structure with that of living Crustacea.” *Annals of Natural History; or, Magazine of Zoology, Botany, and Geology* 4:16-22.

– – – –. 1840b. “Note on the Annelida.” *Annals of Natural History* 4:385-388.

– – – –. 1841. “On the Natural Arrangement of Fishes.” *Calcutta Journal of Natural History* 2:263-275.

Mayr, Ernst. 1982. *The Growth of Biological Thought*. Cambridge: Harvard University Press.

McOuat, Gordon. 1996. “Species, rules and meaning: The politics of language and the ends of definitions in 19th century natural history.” *Studies in History and Philosophy of Science* 27 (4):473-519.

Montagu, George and James Rennie. 1831. *Ornithological dictionary of British birds*. London: Hurst, Chance and Co.

Newman, Edward. 1832. *Sphinx Vespiformis*. London: Frederick Westley and A.H. Davis.

Ospovat, Dov. 1981. *The Development of Darwin’s Theory: Natural History, Natural Theology, and Natural Selection, 1838-1859*. Cambridge: Cambridge University Press.

Panchen, Alec L. 1992. *Classification, Evolution, and the Nature of Biology*. Cambridge: Cambridge University Press.

Rehbock, Philip. 1983. *The Philosophical Naturalists*. Madison: University of Wisconsin Press.

Reider, William D. 1841. *The New Tablet of Memory; or, Recorder of Remarkable Events, Compiled, and Alphabetically Arranged from the Earliest Period to the Present Time*. London: John Clements.

Rieppel, Olivier. 1988. "The Reception of Leibniz’s Philosophy in the Writings of Charles Bonnet (1720-1793).” *Journal of the History of Biology* 21 (1):119-145.

Roscoe, William. 1815. “On Artificial and Natural Arrangements of Plants: and particularly on the Systems of Linnæus and Jussieu.” *Transactions of the Linnean Society of London* 11 (1):50-78.

Russell, E.S. 1916. *Form and Function: a contribution to the history of animal morphology*. London: J. Murray.

Rylands, Peter. 1836. “On the Quinary, or Natural, System of M’Leay, Swainson, Vigors, &c.” *The Magazine of Natural History* 9:175-182.

Scharf, Sara T. 2009. “Identification Keys, the ‘Natural Method,’ and the Development of Plant Identification Manuals.” *Journal of the History of Biology*, 42 (1):73-117.

Stevens, Peter F. 1984. “Haüy and A.–P. de Candolle: Crystallography, Botanical Systematics, and Comparative Morphology, 1780-1840.” *Journal of the History of Biology* 17 (1):49-82.

– – – –. 1994. *The Development of Biological Systematics: Antoine-Laurent de Jussieu, Nature, and the Natural System*. New York: Columbia University Press.

Stresemann, Erwin. 1975. *Ornithology: From Aristotle to the present*. Trans. Hans J. Epstein and Cathleen Epstein, Ed. G. William Cottrell. Cambridge: Harvard University Press.

Strickland, Hugh. 1840. "Observations upon the affinities and analogies of organized beings." *Magazine of Natural History* 4:219-226.

– – – –. 1841. "On the true method of discovering the natural system in zoology and botany." *Annals and Magazine of Natural History* 6:184-194.

– – – –. 1845. "On the progress and the present state of ornithology." *Report of the British Association for the Advancement of Science* 14:170-221.

– – – –. 1846. "On the structural relations of organized beings." *The Philosophical Magazine* (3rd ser.) 28:354-364.

Swainson, William. 1827. "A Sketch of the Natural Affinities of the Lepidoptera Diurna of Latreille." *The Philosophical Magazine* (2nd ser.) 1:180-188.

– – – –. 1834. *A Preliminary Discourse on the Study of Natural History.* London: Longman, Rees, Orme, Brown, Green & Longman.

– – – –. 1835. *A Treatise on the Geography and Classification of Animals*. London: Longman, Rees, Orme, Brown, Green & Longman.

– – – – and John Richardson. 1831. *Fauna boreali-americana, or, The zoology of the northern parts of British America : containing descriptions of the objects of natural history collected on the late northern land expeditions, under command of Captain Sir John Franklin, R.N., part second, The Birds.* London: J. Murray.

Swainston, A.Y. 1985. "William Sharp MacLeay, 1792-1865." *The Linnaean* 1 (5):11-18.

Vigors, Nicholas. 1825. "Observations on the Natural Affinities that connect the Orders and Families of Birds." *Transactions of the Linnean Society of London* 14 (3):395-517.

Wallace, Alfred Russell. 1855. "On the law which has regulated the introduction of new species." *Annals and Magazine of Natural History* (2nd ser.) 16:184-196.

Williams, David M. and Malte C. Ebach. 2008. *Foundations of Systematics and Biogeography*. New York: Springer. PDF File.

Winsor, Mary P. 1976. *Starfish, Jellyfish, and the Order of Life: Issues in Nineteenth Century Science*. New Haven:

Yale University Press.

Wood, Neville. 1836. *The ornithologist's text-book: being reviews of ornithological works: with an appendix, containing discussions on various topics of interest*. London: John W. Parker.