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Cain on Linnaeus: The Scientist-Historian as Unanalysed Entity

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Zoologist A. J. Cain began historical research on Linnaeus in 1956 in connection with his dissatisfaction over the standard taxonomic hierarchy and the rules of binomial nomenclature. His famous 1958 paper 'Logic and Memory in Linnaeus's System of Taxonomy' argues that Linnaeus was following Aristotle's method of logical division without appreciating that it properly applies only to 'analysed entities' such as geometric figures whose essential nature is already fully known. The essence of living things being unanalysed, there is no basis on which to choose the right characters to define a genus nor on which to differentiate species. Yet Cain's understanding of Aristotle, which depended on a 1916 text by H. W. B. Joseph, was fatally flawed. In the 1990s Cain devoted himself to further historical study and softened his verdict on Linnaeus, praising his empiricism. The idea that Linnaeus was applying an ancient and inappropriate method cries out for fresh study and revision. © 2001 Elsevier Science Ltd. All rights reserved.

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The category to which the following belongs is not the history of systematics but the history of the history of systematics. I have just one small tale to tell, but I suspect there are similar tales scattered about unnoticed in the history of other sciences, and it seems to me they are worth uncovering. For several years now I have been thinking about the phenomenon of scientists who write about history (Winsor, 1994). Some people have suggested to me that the percentage of systematists who turn their hand to history may be higher than of scientists in other fields who do the like. Whether this is true I do not know, but if so, that might only make systematics an especially good location from which to explore questions about the significance of scientists' understanding of history.

Historians of systematics of whatever training are awfully scarce. We could ill afford to lose one and I regret the loss of Arthur Cain, who died in 1999. Were

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he alive, he would certainly have found outrageous errors of fact and judgement in what I write here. I entreat you, dear reader, to keep an eye out for such errors and then do as Cain would have done: either in a sternly worded private communication, or in print, with clear reasons, set me straight.

My interest in scientists' interest in the history of their science was stimulated by what I found when I began to look into the early days of the 'new systematics', in particular at the infant Systematics Association responsible for the book of that title. I was surprised to find among the cast of characters several systematists whose names were already familiar to me from their historical writings, such as Ramsbottom (1938), Sprague (1950) and Turrill (1957). My focus though, was John Gilmour and his several attempts to reform systematics (Winsor, 1995, 2000). Systematics in the 1930s was going through a period of turmoil, its perennial problem of being swamped by material collected in the traditional way being compounded by new kinds of data and new ways to analyze data, including microscopic views of chromosomes, serological measures of protein diversity, degrees of intersterility from breeding experiments, environmental effects, and statistics applied to variation. I traced how Gilmour's interest in shaping his colleagues' views stimulated his interest in philosophy, but I also noticed that he became interested in the history of systematics for the same reason (Gilmour, 1951). Among his papers I found a reprint of F. A. Bather's 1927 address 'Biological Classification, Past and Future' (Bather, 1927) with Gilmour's marginal annotations. In a letter to Cain in 1957 Gilmour says he has been reading Plato and Aristotle and 'turning over in my mind the possibility of trying to have a go at [producing] a history of biological taxonomy from the Greeks onwards!' (10 February 1957, Cain Papers).

Gilmour's letter was a response to one from Cain asking for comments on the manuscript of an iconoclastic paper which Cain wrote in 1956. This was the 'celebrated' (Atran, 1990, p. 296) study, 'Logic and Memory in Linnaeus's System of Taxonomy', which impressed and puzzled me in my graduate school days in the 1960s. Gilmour, Director of Cambridge University's Botanic Garden, and Cain's senior by 16 years, told Cain 'it is a most excellent and valuable contribution to the history of taxonomic theory', which reassured Cain (27 February 1957, Cain Papers). 'I am absolutely delighted that you like it', he replied. 'I thought I was on to something good, but I didn't trust myself—there might have been some previous work doing it all and better, or I might have made some fearsome blip in an unfamiliar field. Since both [Arvid H.] Uggla and you approved it, I think I shall go ahead and send it to the Linnean' (8 March 1957, Cain Papers). The Linnean Society published it in 1958.

The chief motivation which drew Cain to this topic is plain. He was concerned, in general, with the present and future of taxonomy, and he turned to history for help in fostering change, as he declared to his readers at the outset.

> [F]ew have commented at all on the theoretical considerations that influenced [Linnaeus].

> They should be examined for two reasons. In the first place there is no better method for scientists of one period to bring to light their own unconscious, or at least undiscussed, presuppositions (which may insidiously undermine all their work) than to study their own subject in a different period. And secondly, when the writings of an earlier author have apparently been taken as the basis of subsequent work, constant scrutiny is necessary to prevent his presuppositions becoming fossilized, so to speak, in the subject. (Cain, 1958, p. 144)

Pure history (the Rankean ideal, the past for its own sake) was not Cain's goal; he sought enlightenment from the parts of the past most relevant to the present. This had been Bather's approach as well, and it is important for us to notice that there is nothing wrong with it. Without this motive, indeed, a study of the past is in danger of slipping into barren antiquarianism. Sad to say, when systematics later on failed to develop in the direction Cain wished, he would become more pessimistic about history's usefulness (Cain, 1981, p. 151).

Besides this general interest, Cain had a more particular agenda. What he wanted was no secret from his fellow biologists, for he had spoken and published on it already, but in his exposition in 'Logic and Memory' he saved it for the end, as if it were part of the conclusion of his research. He wanted the rules of nomenclature to be revised to allow species that were intermediate between genera, or whose relations were not yet known, to be named outside of the binomial system. The Linnaean rules, formally adopted by several international conventions as recently as 1953, stipulated that the name of a species consisted of two words, one of which was the name of a genus (Melville, 1995).

In 1958 Arthur James Cain was a very impressive figure in English zoology. Returning to Oxford from military service after World War II, he had finished his graduate studies in zoology in time to be hired in 1949 as University Demonstrator in Animal Taxonomy, a newly created position (reportedly the product of successful lobbying by the Systematics Association). His doctoral research, under John Baker, was on the chemistry of lipids within cellular structures, so Cain's next step was to seek training in taxonomy. Ernst Mayr became his mentor, inviting Cain to the American Museum of Natural History for 6 months in 1950. Under Mayr's eye he reviewed the taxonomy of a group of Australian parrots and a group of Malaysian pigeons (Cain, 1954, 1955). He returned to Oxford and taught taxonomy, and was elected a fellow of the Linnean Society. He became an active member of the Systematics Association, serving as its Zoological Secretary. From 1954 he was Curator of the Zoological Collections of the Oxford University Museum.

Cain's broad-ranging research was bold and original. Beginning in 1950 he undertook with his student Philip Sheppard a series of studies challenging the assumption that highly variable colours must be non-adaptative. They demonstrated that selection on the land-snail *Cepaea nemoralis* by a predator varied sufficiently, seasonally and locally, to account for the polymorphism (Vernon, 1985, p. 85, citing Cain and Sheppard, 1950, 1952; Cain, 1951a,b). Cain continued this research for several decades (citations in Cain, 1988). In 1954 Cain began another collabor-

ation, with Geoffrey A. Harrison, a mature student whose background in physical anthropology, with its tradition of skull measurements, meshed with Cain's concern about the reliance on intuition common among taxonomists. For several years they worked at how to make taxonomic comparison more rigorous. Together they published two papers, 'An analysis of the taxonomist's judgement of affinity' and 'Phyletic weighting' (Cain and Harrison, 1958, 1960). These papers, along with the independent work of P. Sneath, R. Sokal and C. D. Michener, are regarded as part of the foundation of 'numerical taxonomy' or 'phenetics', but as that field developed, Cain soon fell out of sympathy with it (Vernon, 1985, 1988).

Cain had left New York enthusiastic about the modern evolutionary synthesis and about Mayr's version of the new systematics; his 1954 short introductory text *Animal Species and their Evolution* was 'a summary of [Mayr's] lecture course' (Mayr, personal communication). He did, however, venture some improvements. Following the example of Huxley's 'cline' (with ecoclines, geoclines and chronoclines) and Gilmour's 'deme' (with ecodeme, gamodemes and topodemes), Cain (1954) proposed that the various kinds of species could be called agamospecies, morphospecies, paleospecies and biospecies. In 1953 Cain travelled to the Pacific, collecting birds and making ecological observations in the British Solomon Islands (Cain and Galbraith, 1956). He kept in touch with Mayr over the years, and they remained on cordial terms.

Mayr handled with great skill, in his seminal 1942 book and ever after, the delicate task of elevating the status of systematics, by making it central to the evolutionary synthesis, while at the same time pointing out that systematics was plagued by careless practices and small-minded practioners. A key part of Mayr's strategy was to focus at the level of species. As for the nested sets of higher categories that gather species into successively larger groups, it had long been obvious that these groups could not neatly correlate with evolutionary history; as knowledge of phylogeny increased, discrimination of categories should become ever more difficult because gaps would get filled in. Yet Mayr endorsed the continued use of the traditional categories—genus, family, order, class and kingdom—with sub- or super-families and so forth inserted as needed. He claimed that taxonomists had the good sense to handle the obvious degree of arbitrariness in their choices of how large or small to make each group, and he credited them with knowing that all groups ought to be monophyletic, while warning that excessive splitting would bring disrepute upon systematics (Mayr, 1942, p. 289).

Those familiar with Mayr's later historical writings, especially his 1982 book *The Growth of Biological Thought*, may be surprised to learn that 40 years earlier, his *Systematics and the Origin of Species* mentioned the past very little. Its few and scattered historical statements were nevertheless enough to convey the sense that the development of systematics was generally progressive.

> Aristotle, almost 2,300 years ago, was the first to realize that the most practical system of organisms is based on the degree of similarity of their morphological or anatomical

> characters. This has eventually become known as the 'natural' system. (Mayr, 1942, p. 275)

Mayr was firm in his message that the 'old-fashioned' Linnaean species concept, still widely used by taxonomists, must enlarge to take account of the many polytypic species. But as the founder of taxonomy, Linnaeus escaped censure. His work was portrayed as a solid foundation for subsequent progress.

> It was Linnaeus's principal service to biology that he established a set of rules by which to play the taxonomic game. Every species was, according to him, the product of a separate act of creation and therefore clearly separated from all other species. Groups of similar species were united in genera, and consequently each species was given two names, one to designate the species and one the genus. This is what we understand by binary and binomial nomenclature. It is a system which is intimately connected with a static and strictly morphological species concept. (pp. 108–109)

> Linnaeus did a great service to taxonomy when he invented a definite terminology for the systematic categories and showed that they could be arranged in a graded hierarchy: species, genus, order, and class. (p. 102)

Grouping species into genera and other higher categories has been the quiet achievement of hundreds of hardworking taxonomists.

In their 1953 textbook Mayr and his co-authors E. Gordon Linsley and Robert L. Usinger began with an historical survey, including two pictures—portraits of Darwin and Linnaeus, with the title pages of their landmark works. They speak of

> the great Swedish naturalist Linnaeus (1707–1778), whose contributions were so influential on subsequent students that, with much justification, he has been called the father of taxonomy. In the tenth edition of his great work *Systema naturae* (1758) (Fig. 1), the binominal system of nomenclature was for the first time consistently applied to animals, and this work became the foundation of systematic zoology . . . The methods of Linnaeus were by no means wholly original, but his eminently practical system was quickly adopted, expanded, and elaborated . . . It dominated taxonomy for the next century, and most of the essentials of the Linnaean method are still components of modern taxonomy (Mayr, Linsley and Usinger, 1953, pp. 6–7).

Mayr did admit that the great enemy of the biological species concept, typology, which he traced back to Plato's *eidos* (Mayr, 1959, p. 2), was common in Linnaeus's time. But when identifying the twentieth-century entomologist Karl Jordan as a clear proponent of the biological species, Mayr also claimed that all good local naturalists, including John Ray in the seventeenth century and Linnaeus in the eighteenth, 'had arrived at this concept empirically' (Mayr, 1955, p. 52).

In 1955 Cain was beginning to drift away from Mayr's influence. Mayr thought that Cain had agreed to contribute to an important reference work, a revision of Peters' *Checklist of the Birds of the World*, which Mayr was helping to edit, but Cain ducked the task, rationalizing his avoidance as a matter of principle: 'I heartily dislike the idea of a check-list', he wrote Mayr, the 'obligate use of binomials merely conceals the fact [that relationships are unknown] by putting every species into *some* genus, whether or not one knows where it should go' (10 May 1956, Cain

Papers). Mayr replied, 'I think your attitude towards a checklist is too idealistic. No one expects it to be something final and permanent . . . Indeed the mere presentation of a list seems to have a certain heuristic value and stimulates people to try for a more natural arrangement' (4 June 1956, Cain Papers). It was no good, Cain dropped out of the project.

Cain's few brief mentions of the sweep of history in his 1954 book had agreed with Mayr's. Explaining the superiority of using many features, which gives a 'natural' classification, rather than one feature, which produces an 'artificial' one, Cain credited the 'great' taxonomist Linnaeus with understanding the difference (Cain, 1954, p. 18). In 1955, though, at a symposium on the genus category held under the aegis of the Systematics Association, he hinted at a disaffection with Linnaeus. Cain called for finding a workable compromise between the continuous branching tree of evolution and the higher categories, which implied that Mayr was wrong to teach that the system was already working. Cain's paper included several paragraphs on Linnaeus which did portray him as a half-way mark to the present, because Cain accepted the evidence assembled by botanists (Ramsbottom, 1938; Svenson, 1945; Bartlett, 1940) showing that Linnaeus had appreciated species as true-breeding units and also had recognized their mutability, and that he had striven for a natural classification, based on all features. But because evolutionists should realize that no discrete natural entities corresponding to higher groups exist, Cain concluded this paper with the warning, 'In view of the differences between Linnean and present-day taxonomy, it is worth while considering whether the latter can still be said to be Linnean' (Cain, 1956, p. 109).

The sources cited in Cain's paper from this symposium (Cain, 1956) included only one publication of Linnaeus himself (in a recent English translation: Linnaeus, 1938), the 1737 *Critica Botanica*, which set forth practical rules for choosing generic and specific names. Two other cited items tell us that Cain was casting about looking for insight on the genus. One was an article on the medieval concept of species by former zoologist Alistair Crombie (1951), who came to Oxford to teach history of science in 1953. The other was by virologist Norman Wingate Pirie, who in 1952 used the new *British Journal for the Philosophy of Science* as a soap-box to complain about the imposition of the rules of botanical and zoological classification onto the classification of microbes. Although this, its main subject, was probably what brought Pirie's article to Cain's attention, he may have also noticed Pirie's complaint about molecular biologists. They import notions of chemical purity which are appropriate only for small molecules, Pirie says, accusing his colleagues of harboring 'an unrecognised piece of idealism', 'pure Platonism'. They ought to be interested in the real population of macromolecules, including their individual differences, he says, but they focus on the ideal, thereby running into the 'difficulty inherent in Platonism, recognised or unrecognised, when it leaves the examination of simple concepts like circles and dodecahedra and starts to meddle with complex systems' (Pirie, 1952, p. 275).

In addition to Pirie and Crombie, there was one more source Cain had looked at by the time he wrote 'Logic and Memory' which must have contributed to his abandoning Mayr's view of Linnaeus. The great plant physiologist Julius von Sachs had produced a major survey of the history of botany in 1875. He had emphasized the distinction between medieval scholasticism, based on writings of Aristotle, an attitude relying on words and concepts that is concerned to defend an existing scheme as dogma rather than to correct and alter it, and the modern scientific spirit, which is critical and experimental. Sachs claimed that

> in his inner nature [Linnaeus] was a schoolman, and that in a higher degree than even Cesalpino himself, who should rather be called an Aristotelian in the strict sense of the word. But to say that Linnaeus' mode of thought is thoroughly scholastic is virtually saying that he was not an investigator of nature in the modern meaning of the word; we might point to the fact that Linnaeus never made a single important discovery throwing light on the nature of the vegetable world. (von Sachs, 1890, p. 85)

> Linnaeus was in fact a dangerous guide for weak minds, for his curious logic, among the worst to be met with in the scholastic writers, was combined with the most brilliant powers of description. (p. 89)

While seeing Linnaeus's affirmation of the fixity of species as a positive step (p. 99), Sachs was confident that this dogma was

> properly a conclusion from scholasticism, and ultimately from the Platonic doctrine of ideas, and was therefore assumed as self-evident before the time of Linnaeus, who only gave it a more distinct and conscious expression . . . The strength of the dogma lies in its relation to the platonico-scholastic philosophy, which the systematists followed, more or less consciously, up to quite recent times. (p. 100n)

(This harsh view of the founder of botanical taxonomy had doubtless been a stimulus to botanists who investigated his proto-evolutionism, including Greene (1909), Svenson (1945), and Ramsbottom (1938), but that is another story.)

Cain also looked at the chief publications of Linnaeus; he knew enough Latin to decide that the translation of *Critica Botanica* was 'dreadful' (Vernon, 1985, p. 64). Undoubtedly Cain would have identified his own reading of Linnaeus as the major source of his changed view. In 'Logic and Memory' he cited also Linnaeus's *Classes Plantarum*, *Species Plantarum*, *Systema Naturae*, *Methodus Plantarum*, *Genera Plantarum* and *Philosophia Botanica*. He told Gilmour he had been 'looking at what Linnaeus really said, and had had something of a surprise' (3 December 1956, Cain Papers). In fact he found two surprises. One was that Linnaeus expected botanists to be able to recognize all known genera and remember their names; but we need not pursue this issue of 'Memory' further here. The other Cain thought remarkable. Some of the terms Linnaeus used (*genus*, *species*, *definitio generis*, *differentia essentialis*, *divisiones*, *fundamentum*) belonged to the language of formal logic, a subject tracing back to Aristotle. (This point had been noted by Swedish scholar Arvid H. Uggla, as Cain reports—Cain, 1958, p. 144—but whether Cain

was led to it by his remark or had noticed it independently is not clear.[1]) Following up this intriguing clue, Cain decided he needed to understand the principles of Aristotelian logic. These he found explained to his satisfaction in a 1916 textbook, *An Introduction to Logic*, by Oxford tutor H. W. B. Joseph.

At this point I must remind you that what I am undertaking here is not the history of systematics but the history of the history of systematics. If I were equipped to write a history of systematics (which I am not, for my Latin is very slight and my Swedish nonexistent) I would need to know what the eighteenth-century naturalist Linnaeus was thinking and doing, as nearly as limited evidence would allow it to be reconstructed. I would include not just the man himself (in his soul, as it were), but also what his contemporaries thought Linnaeus was saying and doing. Any professionally trained historian will recognize how dangerous a short-cut the busy zoologist Cain was taking. We know that Linnaeus cannot have read Joseph. How can we be sure that the 'Aristotelian' logic familiar in the eighteenth century was the one Joseph described in the twentieth? Cain was probably lulled by Joseph's assurance that he had gone 'back largely to [logic's] source in Aristotle . . . The terminology of Logic owes more to Aristotle than to anyone else . . .' (Joseph, 1916, pp. viii–ix). If so, Cain made the mistake of ignoring Joseph's remark that '[i]n the course of centuries, the tradition [of logic] has become divergent, and often corrupt' (p. viii). Joseph cannot set out for us the version of formal logic known to Linnaeus (although the coincidence of terms assures us there will be some resemblance). What we can learn from Joseph, however, is what logic Cain was reading in 1956.

Dipping into Joseph's book, and into Linnaeus, and back to Joseph, Cain began to see Linnaeus in a whole new light. Before long he arrived at the conviction that one of the 'theoretical considerations' influencing Linnaeus was 'the general principles of all classification laid down by Aristotle in his *Logic*'.[2] That these principles influenced Linnaeus is important, Cain believed, because they 'may still be a powerful source of unwarranted bias in modern classifications' (Cain, 1958, p. 162).

Cain presents his readers with substantial quotes from Joseph, and his own summary. In Cain's words, 'According to Aristotelian logic, the genus should not be regarded merely as a collection of species. The genus and the differentia taken together are the definition of the species, the statement of its essence . . .' (Cain, 1958, p. 145). This makes good sense when dealing with entities whose essential properties we perfectly understand, such as geometric forms. 'Mathematics, and

[1]Arvid Uggla was working in Uppsala editing Linnaeus's correspondence. Cain refers to a comment Uggla sent to zoologist Karl P. Schmidt to assist in a translation of Linnaeus's 'Methodus' which Schmidt published in 1952 (Schmidt, 1952). In connection with Linnaeus's term 'differentia specificans', Uggla had written to Schmidt, 'I think Linnaeus has in mind the old rule of logic: "Definitio fit per genus proximum et differentiam specificam" '.

[2]The italics are Cain's, but in error, for there is no Aristotelian book with this title; his logical doctrines are contained in *Topics* and *Analytics*.

especially geometry, studied by both Plato and Aristotle, is the most obvious example of a subject-matter suitable for classification by genus and differentia' (Cain, 1958, p. 145). Using 'triangle' as a *genus*, we can neatly and exhaustively divide all possible instances into three *species* depending on whether all three sides are the same length (equilateral), two are the same length (isosceles) or none are the same length (scalene). Here the proportion of the sides is the *fundamentum divisionis*.

When we move from mathematics to a consideration of living things, however, we quickly learn that such logical procedures no longer apply. We have no knowledge of the essence of things, so can only guess at which features are appropriate to a definition. Without *a priori* knowledge, we must work empirically, and in these circumstances, logical division is impossible. Aristotelian division requires us to use as differentia characters derived from the essence, but we do not know the essence of natural things. Cain invents contrasting terms to underline the point. 'Where logical division is possible, we can have a *taxonomy of analysed entities*; where not, only a *taxonomy of unanalysed entities* is possible, and the best example of it is indeed biological taxonomy' (Cain, 1958, p. 146).

Cain connected Linnaeus's rules of definition to Aristotelian definition *per genus et differentiam* (*sive differentias*), but overlooked Joseph's decription of it as the reverse of division, and the warning that 'in actual practice our thought moves in both directions at once' (Joseph, 1916, p. 116). Joseph added parenthetically, in small print,

> Just as the *theory* of Definition, with its sharp distinction of essence and property, breaks down amidst the complexity and variety of concrete things, so it is with the theory of Division . . . [It works in mathematics but] in other sciences for the most part we must wait upon experience. (p. 133)

In fact, Cain's assumption that this was a modern insight was mistaken; Aristotle understood it thoroughly (Bolton, 1987).

Cain's evidence that Linnaeus was following the principles of Logical Division, applying *fundamenta divisionum* from *a priori* beliefs about living things, consists chiefly in Linnaeus's use of the sexual organs of plants as the characters defining his classes and orders '*because* the stamens, pistil, etc. subserve the extremely important function of genuine sexual reproduction' (Cain, 1958, p. 148). Cain also reported various facts that seem inconsistent with his thesis. Sexual characters do not dominate Linnaeus's animal classification. He called his sexual system for plants artificial and longed for natural groups to replace it. He 'found himself unable, because of the empirical facts, to prescribe any set principles on which species could be differentiated . . .' (p. 148). His actual practice was certainly not a model of Logical Division, but Cain insisted that this was always the ideal at which Linnaeus, oblivious to the fact that the method could not be expected to work beyond the realm of mathematics, had aimed.

In Cain's experience, there were a 'depressing' number of twentieth-century

taxonomists who needed schooling in elementary philosophy. Citing his own earlier complaint, he said that the taxonomist too often would foolishly struggle 'to "define" groups which cannot in fact be defined although they are clearly natural' (Cain, 1954), or worse, 'to mutilate his natural groups until each is definable' (Cain, 1958, p. 150). That is, they would try to find a few salient characters (such as would be handy in a key) to characterize a genus (or sub-genus) which is actually constituted by a cluster of resemblances, none of which is present in them all. It was these second-rate zoologists and not the dead Swede whom Cain was out to reform.

Excited by his discovery that Linnaeus was aware of the forms of Aristotelian division, Cain slipped into a common historical fallacy: he assumed linear development. Joseph may have led him astray, for the Oxford logician mentioned several times that the theory of evolution was responsible for destroying all hope of defining organisms by their essences.

> For a long time the doctrine of the fixity of species, supported as well by the authority of Aristotle and of Genesis . . . encouraged men to hope that there was a stable character common to all members of a species . . . But now that the theory of organic evolution has reduced the distinction between varietal and specific difference to one of degree, the task of settling what is the essence of a species becomes theoretically impossible. (Joseph, 1916, pp. 95–96)

Cain quoted Joseph's statement that

> 'the problem of distinguishing between essence and property with regard to organic kinds may be declared insoluable. *If species were fixed*: if there were in each a certain nucleus of characters, that must belong to the members . . . then this nucleus would form the essence of the kind. But such is not the case . . . There may be deviation from the type, to a greater or lesser degree, in endless directions . . .' (Joseph, 1916, pp. 102–103; quoted by Cain, 1958, pp. 146; emphasis mine)

Cain apparently assumed—he certainly allowed his readers to imagine—that before Darwin the inapplicability of division by essences to unanalysed entities cannot have been widely recognized. This was a serious mistake. The revolution against the medieval teachers of Aristotle had succeeded so well that the school logic text most widely used in the eighteenth century declared:

> Knowing that there are genera, species, differences, properties, and accidents is not very useful . . . although nothing is more important in science than classifying and defining well, we need say no more about it here, because it depends much more on our knowledge of the subject matter being discussed than on the rules of logic. (Arnauld and Nicole, 1996, pp. 44, 128)

What logic the young Linnaeus was made to read, I do not know, nor did Cain.[3]

After quoting Joseph's 'If species were fixed', Cain immediately laid before his

[3]Philosopher Mary Tiles of the University of Hawaii tells me that there was a wide variety of views on definition and logic in the eighteenth century. She also points out that scholastic logical division derives from Plato and Plotinus, via Boethius, not from Aristotle.

readers a grand overview of the history of systematics, effectively a master narrative, essential overview, or backbone story.

> The relevance of these quotations from logicians for the whole history of biological taxonomy from Aristotle to the present day can hardly be over-estimated. They epitomize the most important change in taxonomic theory that has occurred, namely the gradual abandoning of attempts to set up classifications on *a priori* principles agreeable to the rules of logic and some particular theory, and the partial substitution of an empirical attitude. This substitution was not complete when the theory of evolution arrived to provide a new theoretical approach to the problem of classifying organisms, the full implications of which have still not been completely thought out. (Cain, 1958, p. 147)

The image of 'whole history' conjured up by the words 'change', 'gradual' and 'complete' doubtless stayed in the memory of many readers who only half understood, or soon forgot, the details of Cain's argument.

Cain repeated simplified versions of 'Logic and Memory', shorn of its subtleties and qualifications, on several occasions over the next few years (Cain, 1959a,b, 1962a,b, 1963).

> Linnaeus's principles of classification are based on the theory of Logical Division worked out by Aristotle, which held the field until the beginning of this century (later in some quarters) as the only good way of arranging anything . . . It [Linnaeus's classification] failed because it was essentially deductive but was applied indiscriminately to situations where only inductive treatment was possible . . . (Cain, 1962a, pp. 2–3)

Cain's perception that Linnaeus was an essentialist nicely complemented the work he was doing at the time with Harrison. They wanted to make taxonomic decisions transparent and testable by breaking them down into particular differences which could be counted up objectively, free of hypothetical genealogies. Following where Gilmour had led, Cain began to argue that phylogeny was an extra and unnecessary burden for taxonomy, and that using a large number of characters was all the word 'natural' should mean. To have standard taxonomic practices identified with an ancient and discredited philosopher laid a fine background for the claim that modern taxonomy should wash itself clean of the remaining taint of essentialism (Cain, 1962a,b, 1963; Cain and Harrison, 1958, 1960).

In 1964 Cain left Oxford for the University of Manchester; four years later he moved to the University of Liverpool. He was disappointed that his work on *Cepaea* had not stimulated other studies of selection, and bitter that systematists had been lured away from his reasoned recommendations by such foolish schemes as numerical taxonomy and cladistics. He disdained spinners of grand theories, the 'insufficiently based theoretians' (meaning S. J. Gould) who 'pour out an insubstantial froth of pontifications that mislead badly young and unexperienced minds' (Cain, 1988, p. 185). He became Emeritus Professor in 1989 and turned his attention back to Linnaeus and his predecessors (Cain, 1993, 1994b, 1995, 1999). History is a subject that requires, he declared, 'all the scientific virtues, accuracy,

industry, objectivity, and a flair for detective work' (Cain, 1981, p. 151), virtues which Cain loved to exercise and whose absence in others exasperated him.

Research on Linnaeus had blossomed in the meanwhile, providing a greatly enriched picture of Linnaeus's way of working and influence on colleagues and students (Broberg, 1985; Eriksson, 1983; Gustaffson, 1985; von Hofsten, 1958; Koerner, 1999; Larson, 1971; Lindroth, 1983; Stafleu, 1971; Stearn, 1971; Stevens and Cullen, 1990; Winsor, 1976). The thesis of 'Logic and Memory' was taken as established fact by almost everyone, and fully expounded in survey texts (Simpson, 1961; Mayr, 1982; Panchen, 1992). One voice of dissent was that of anthropologist Scott Atran, who described Aristotle as a philosopher of eminent common sense.

> A major source of error in the interpetation of natural history is thus owing to a misleading analysis of Aristotle's theory of Logical Division that has been unduly influenced by the idealism of some of the Oxford scholars, most notably H. W. B. Joseph (1916). Such analyses tend to maintain that Aristotle accepts the parallelism between the division of geometical forms and those of biological kinds *as a matter of observed fact* . . . But everything in Aristotle's biological works indicates this is not so. (Atran, 1990, p. 87)

Atran knew this not just from his own study of Aristotle but from reading classicists, including Balme (1987) and Lennox (1987).

> For my part, I have so far failed to find any natural historian of significance who ever adhered to the strict version of essentialism so often attributed to Aristotle. Nor is any weaker version of the doctrine that has indiscriminately been imputed to Cesalpino, Ray, Tournefort, Linnaeus, A. -L. Jussieu and Cuvier likely to bear up under closer analysis. (p. 84)

Although Cain found no occasion to cite Atran, he did become acquainted with Aristotle the biologist. In response to the claim by Philip Sloan (1972), disputed by Mayr (1982, p. 163), that the English naturalist John Ray may have undergone 'a philosophical revolution in his attitude to accidental and essential characters of plants, as a result of reading the philosopher John Locke' (Cain, 1996, p. 343), Cain read Ray and the texts Ray read, including Aristotle's biological works. It pleased him to find that there was no need for Ray to wait for Locke to explain that essences were unknown to mortals, leaving us to do the best we can with constant characters; Ray knew it already, and so had Aristotle.

> . . . Ray could think himself to be a faithful follower of Aristotle, in (i) referring to constant differential characterics as (essential) accidents, (ii) taking a collection of such accidents as the definition of a species or higher-ranking group, and (iii) separating as 'merely' accidental or varietal effects of (e.g.) soil, drought, disease in producing modifications in individual plants, and indeed animals. (Cain, 1996, p. 356)

In other articles Cain assembled evidence that Linnaeus had admired Descartes and been interested in the Rosicrucians.

The Liverpool Cain could see that the picture of Linnaeus painted by the Oxford Cain was not being reinforced by all this new information.

> It might be said by a philosopher that in various papers I have made Linnaeus out to be a sort of intellectual caddis-worm, going about in a case of incongruous bits and pieces of philosophy from Aristotelianism [Cain, 1958, 1992], Hermeticism [Cain, 1992], and now Cartesianism. But the bits and pieces are complementary or overlapping, and bound together by the toughest silk, experience [Cain, 1994a, p. 33].

How lovely to have this new metaphor, complementing Bacon's ant (dull collector), spider (spinning a web of her own substance), and bee (distilling sweet nourishment from pollen she laboriously collects) (Bacon, 1994, p. 105). Like Cain, I have long admired the underwater larva of the caddis fly, who spins a rugged tube that protects his tender flesh. His 'case' is wonderfully camouflaged because he covers its surface with sticks or stones chosen from his home stream. (If you balk at the the word 'chosen', read up on caddis flies, for they do exercise preference.) And I certainly agree that the priority Linnaeus always gave to experience—the large degree of empiricism in his method—was above all what earned him the respect of his fellow naturalists.

I will leave the humble insects, though, and turn to a larger and not humble organism for an analogy suitable to the development of scientific knowledge. Still in the realm of metaphor, let us consider the means of visual perception in *Homo sapiens*. Experimenters are showing that what happens when a person has the experience of sight is constituted of interpretation as well as data. By itself, this may not seem remarkable, until you learn that these two are interwoven in the depths of the very nature of seeing. Neither is prior, for a system that consists of data and interpretation mixed together evolved as a unit. I expect the history of science to express something similar; whether science investigates the motion of the planets or the diversity of living things, acquiring an understanding of nature must consist of induction and deduction intertwined. An historical narrative like Cain's, that traces a progressive change in scientific method moving from the *a priori* to the empirical, cannot be true. Individuals and local fashion may favour one or the other, but overall they must co-exist in every age.

The Aristotelian Linnaeus of Cain's Oxford days has been repeated endlessly, but the caddis-worm Linnaeus of the mature Cain has attracted no notice. It is time this imbalance were corrected. Indeed, if further research into Linnaeus's understanding of philosophy fails to support Cain's 1958 claims, the big picture may need revision. Portraying the history of systematics as the saga of an ancient, inappropriate method finally loosening its grip might, after all, turn out to be a fiction.

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