

# Considering affinity: an ethereal conversation (part two of three)

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**In 1840 Hugh Strickland published a diagram showing the relationships of genera of birds in the kingfisher family. Three years later he applied this mapping idea to genera of birds of prey and songbirds, creating a large wall chart that he displayed to colleagues but never published. Both of his diagrams featured a scale of degrees of affinity. The meaning of taxonomic affinity was something Darwin thought about deeply. Details in the chart undermine Strickland's claim that his method was purely inductive.**

*(We resume our eavesdropping on a conversation between Hugh Strickland, killed by a train in 1853 at the age of 42, and his near contemporary Charles Darwin, who has joined him in the afterlife in 1882. Finally free of life's cares, they can enjoy a relaxed discussion of a concept in which both men are deeply interested, the relationship that taxonomists, then and now, call affinity.)*

**STRICKLAND:** As you rightly say, the chief idea of this big chart of mine is that all birds are linked to one another by a relationship we call affinity. I did my best to ignore analogies, which I consider accidental or merely adaptive.

**DARWIN:** And you defined affinity as similarity based on essential characters.<sup>1</sup>

**STRICKLAND:** Exactly.

**DARWIN:** And you defined those essential characters by appealing to naturalists' conviction that groups in the natural system, once correctly identified, are real.

**STRICKLAND:** Most naturalists were ready to admit that the resemblances connecting living things form neither a linear chain, nor a continuous mass, but fall into groups separated by gaps. The groups to which we give a name, whether at the class level like birds, 'Aves,' or at the family level like kingfishers, 'Alcedinidae,' are entities that exist in nature. Of course I knew that there were still some naturalists

who asserted that since taxonomic categories are patently man-made things, the groups they name cannot be real, but I sensed that opinion on that was shifting.

**DARWIN:** I am in perfect sympathy with your feeling, though I fear we are trespassing into a realm of very old philosophical debate.<sup>2</sup>

**STRICKLAND:** I did not worry overmuch about this. The best zoologists and botanists were united in their determination to replace artificial classifications, of which any number can be constructed, with the natural system, which is but one, even though our knowledge of the natural system is still imperfect. And my reading of Whewell's volumes confirmed my impression.

**DARWIN:** Clearly this vision underlies your powerful analogy of mapping, for nothing feels more real than the solidity of land after one has spent time at sea. Your hope was to lay out on paper the families of birds much like a mariner who charts reefs and islands.

**STRICKLAND:** Well said. That view of the natural system was the foundation of my argument. Westwood's attempt to redefine essential character was a backhanded way of saying the natural system may include analogies. Even lapsed quinquarians, like your friend Waterhouse, still held onto Macleay's claim that analogies provide a second dimension of relationship, independent of affinities, which lie parallel to them and form a grander, more complex natural system.

My insistence that the term "essential character" must be strictly limited to those groups properly belonging to the natural system was fundamentally sound, even though I struggled to define it. I was sure of that, and I still am. Obviously I knew better than to say the

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<sup>1</sup> Although the dialogue is imaginary, all of the facts and ideas mentioned are based on historical evidence. Relevant quotations from the writings of Strickland and Darwin, as well as references to other primary and secondary sources, are given in the Supplementary data, and on my website, [www.maryp Winsor.com](http://www.maryp Winsor.com).

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<sup>2</sup> Whether a taxon (a group of organisms named in a classification) has real existence or is only a man-made abstraction is a question as old as philosophy. Many commentators have placed Darwin in the latter camp (as a nominalist), but I agree with David Stamos (*Darwin and the Nature of Species*, State University of New York Press, 2007) that he was a realist. Indeed I am sure that this was a central element in his concept of evolution (as I say in my article "Darwin and Taxonomy" (*The Cambridge Encyclopedia of Darwin and Evolutionary Thought*, ed. Michael Ruse, pp. 72–79, Cambridge University Press, 2013).

natural system may only use essential characters and then to define essential characters as the ones revealing the natural system. How quickly the attempt to define words gets one into difficulty! I was convinced the living world was created according to some deep rationale, giving it a subtle design far more interesting than the mere adaptations celebrated in Reverend Paley's *Natural Theology*, but it was hard to express so profound an idea. I was so pleased when I hit upon an apt metaphor that I remember it exactly. Those groups that all taxonomists agree are natural, like Mammalia, Insecta, Coleoptera, I called "real apartments in the edifice of the Divine Architect".

DARWIN: That broke you out of the vicious circle of definition, I suppose. At least you didn't make the mistake of defining essential character as a feature belonging to the creature's essence. Then if I had pressed you, your classical education would lead you to Aristotle's definition of "essence" as that-which-makes-it-be-what-it-is.

STRICKLAND: Thank you, yes, I was not quite that silly. Of course I understood that my allusion to the thoughts of the Creator could not function as a scientific definition of the natural system. Rather, I appealed to the consensus of our fellow naturalists, the majority of whom, I think, shared my conviction, as I know you did, that we were discovering rather than inventing natural groups.

DARWIN: At the time you were making your maps, early in the 1840s, I had already become convinced that the diversity of organic form is the product of descent with modification, so that taxonomic groups, at least the ones accurately recognised by competent workers, indicate historical kinship. This idea, transformation, unfortunately does not supply any new basis for making judgements of affinity beyond the criteria skilled taxonomists were already using. Thus your appeal to the natural system as a means of defining essential characters strikes me as reasonable, in a practical sense.

STRICKLAND: You may be amused to learn that since my arrival in this blessed realm, I have learned that a somewhat different idea of essential characters existed in the 18th century.

DARWIN: Gracious, I am all ears.

STRICKLAND: I had the great pleasure of meeting, quite by chance hereabouts, our distinguished predecessor, the very founder of our enterprise it's fair to say, Carl Linnaeus.

I wasn't so clever as to think of seeking him out, but I'm not surprised he's here, for he was deeply pious, in spite of that streak of conceit that marred his character. I think you will be as surprised as I was to learn that he had constructed an eminently practical definition of essential character.

DARWIN: This is most extraordinary. Now I realize that I was so wrapped up in our conversation in this peaceful, secluded bower that I gave no thought to the fact there must be thousands – or is it millions – of other spirits here. My greatest joy will be to reunite with my daughter Annie, whose death caused such pain to her mother and me. Yet I find myself wonderfully free of any anxiety, knowing I have before me all eternity to hold her hand, so I am quite content for now that you and I should pick up the thread of our conversation. By all means go on, and tell me, what did Linnaeus say to you about essences?

STRICKLAND: In his time on Earth, he was quite uninterested in philosophical distinctions, although now he can often be found walking arm in arm with the king of philosophers, old Aristotle. They seem to enjoy endless jokes, though I had imagined them both to be rather humourless fellows.

In his day, Linnaeus did wonder what made a plant what it was, but he wrote nothing at all about the essences of philosophers. In his aphorisms, however, he mentioned something called the *character essentialis* of a plant. All his writings were in Latin, of course, but his language was not quite the classical Latin I learned as a boy, so he and I spent some time talking over these words. After a while we decided that his term *character* does not correspond to what you and I call a taxonomic character, the individual features of a plant or animal. Its closest equivalent in English would be the character you are asked to give a maid who leaves your service.

DARWIN: Oh, one of those exasperating English words used ambiguously. Your servant is a woman of fine character, so you give her a letter which we also call a 'character.'

STRICKLAND: Exactly so, and your document will contain points of information, such as her honesty, her cheerfulness, her promptness, all those features that together make up her character. In botany, Linnaeus called such features or characteristics *notae*, notes, things like the shape of a plant's leaves or details of its

flower. He taught that botanists could define a genus in one of three different ways, which I think really boils down to how many *notae* they use. He named these three sorts of definition the *character naturalis*, *character essentialis*, and *character factitius*.<sup>3</sup>

DARWIN: "Factitius" meaning artificial, man-made?

STRICKLAND: Quite so.

DARWIN: So, tell me, was his *character essentialis* like your essential character?

STRICKLAND: In one sense, it was close, but no, not exactly, that was what so impressed me. He made no mention of how God may have thought about His creation. The real existence of natural genera and species was certainly an article of faith for Linnaeus, but when it came to defining a genus, his approach was comparative. He always stayed solidly within the practical realm, referring only to how expert botanists work and how they communicate with anyone else who wants to identify a plant.<sup>4</sup>

DARWIN: I think I recall that old William Kirby, who had been a botanist before taking up entomology, and was devoted to Linnaeus, defined new insect species by sorting features into those three lists, *character factitius*, *naturalis*, and *essentialis*.<sup>5</sup> But few other taxonomists did that.

One Linnaean aphorism I vividly remember, because I quoted it twice in my species book: the characters do not give the genus, the genus gives the characters.<sup>6</sup> Such a curious thing! Which of his three was that about?

STRICKLAND: The aphorism is only paradoxical because he left off the adjectives. He meant that the essential character, the short list of *notae* used in some good recent handbook, is not what creates the genus, nor is it a definition in the logician's sense, because if a new plant is discovered which does not fit that definition but obviously belongs to the genus, the expert who writes the next monograph will have

to consult the natural character (the entire list of *notae*) to figure out how to rewrite the essential character. It really points to the fact that taxonomy is a work in progress, subject to revision as new specimens come into the hands of experts.

DARWIN: So the *character naturalis* contains every imaginable taxonomic character, all the *notae*?

STRICKLAND: Yes, and I suppose that must be a very long list indeed, existing *in potentia* rather than already written down anywhere. If there is anything like an essence to a living thing, it must reside there, I suppose. But Linnaeus said to me with feeling that he used to teach his students not to think they could isolate somehow the features that underlie the plant's essence, instead they should work simply by comparing any new specimen to the ones already described.

DARWIN: I hope you did not cause any distress to that great man by telling him how far later generations strayed from his teaching. Cuvier pretended he could analyze animal physiology and decide which organ systems are so important that they determine the subsidiary ones, which sounded eminently logical until humble taxonomists found that every rule has exceptions.

STRICKLAND: Distress to Linnaeus? Assuredly not, for in life, Cuvier spoke of him with admiration, and hereabouts they are known to be great friends. And why do you say pretended? I agreed with Cuvier, and so did Whewell, in my expectation that physiological principles would some day allow us to deduce which characters are important. All right, I must admit, that happy day kept receding into the distant future.

DARWIN: I paid careful attention to how naturalists do their work, and I believe it's safe to say that they do not care, in practice, about the physiological value of the characters they use. According to my view of descent with modification, nature pays no heed to the logical structure Cuvier envisioned for it. No wonder it eluded you ornithologists.

STRICKLAND: May I perhaps mention, without bragging, that recently both Cuvier and Linnaeus told me that they liked my definition of "essential character"?

DARWIN: What a feather in your cap. And did you show them your chart?

STRICKLAND: I did. Cuvier was merely polite, but Linnaeus gave it his sincere approval. He said he had himself suggested, long

<sup>3</sup> These three categories of character were largely forgotten for most of the 19th and 20th centuries, but new interest in the term "essential" was created from a variety of sources in the late 20th century, reviewed by Staffan Müller-Wille, "Making sense of essentialism," *Critical Quarterly* 53 no. 4 (2011): 61–67. My attention to Linnaeus's language stems from the clear exposition of Arthur J. Cain, "The *Methodus* of Linnaeus," *Archives of Natural History* 19 no. 2 (1992): 231–250, pp. 233–234, n. 4.

<sup>4</sup> Our understanding of Linnaeus was thoroughly revised by Staffan Müller-Wille, *Botanik und Weltweiter Handel* (Berlin: Verlag für Wissenschaft und Bildung, 1999); his key findings are summarized in his "Collection and collation: Theory and practice in Linnaean botany," *Studies in History and Philosophy of Biological and Biomedical Sciences* 38 (2007): 541–562.

<sup>5</sup> William Kirby, "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 no.1 (1823): 93–110.

<sup>6</sup> Charles Darwin, *On the Origin of Species* (London 1859), p. 413, p. 417.





measured by degrees on a thermometer, is likewise continuous. Degrees of kinship are different, they go stepwise; between the second and third there is no two-and-a-half degree of consanguinity. University degrees are the same; after earning one's bachelor of arts, one may proceed to the M.A., or not, but there is no half-a-degree in academics.

DARWIN: Quite so.

STRICKLAND: I don't see how this is relevant.

DARWIN: Didn't you define your degrees of affinity according to our familiar taxonomic categories, such as class, order, family?

STRICKLAND: It's a bit disconcerting to be told that's how I defined my degrees. I thought of it the other way around, that the degree of remoteness of affinity was what caused me to jump up to a higher category.

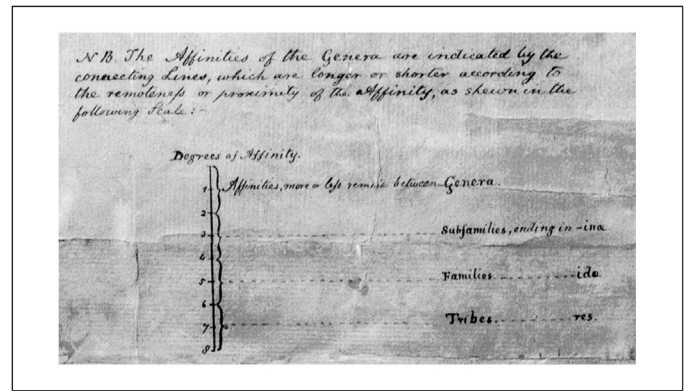
DARWIN: But all those taxonomic categories, surely they are a stepwise function?

STRICKLAND: Oh, no, certainly not. No, I won't accept that. Sensible naturalists all understand that the size of the gaps separating groups is not an absolute, but variable. I am sure your friend Waterhouse, when he catalogued the collections of the Zoological Society, did not think that the difference between two families of insects was the same as the difference between two families of mammals. Because of the variability of those distances, I carefully made my scale of degrees of affinity continuous. See here, on my big chart, I wrote "The Affinities of the Genera are indicated by the connecting Lines, which are longer or shorter, according to the remoteness or proximity of the Affinity, as shewn on the following Scale," and then the scale says that one or two degrees meant "Affinities, more or less remote, between genera," three or four degrees meant "Affinities, more or less remote, between sub-families." I even flagged it with the warning "N.B.," so that no one could overlook the point that gaps may be of any size (Fig. 2).

DARWIN: I can see that you did. And you're sure this is an exact copy of the one you unrolled at the Cork meeting in 1843?

STRICKLAND: On this point I am quite sure. I put a lot of thought into that scale, since as you kindly noted, it was original to me. I wanted to make my chart as much like a surveyor's map as possible, I say again, to convince our colleagues to return to the inductive method. I was weary of quinar-ians' fantasies.

DARWIN: Well and good, old chap, but I am sorry to insist that the fact that the gaps we observe between natural groups do vary,



**Figure 2.** Detail from Hugh Strickland's 1843 wall chart. It reads "N[ote] B[ene] The Affinities of the Genera are indicated by the connecting Lines, which are longer or shorter according to the remoteness or proximity of the Affinity, as shewn in the following Scale: - Degrees of Affinity. Affinities, more or less remote between Genera...." Compare this to the scale in Fig. 1. Image courtesy of the Cambridge University Museum of Zoology.

although interesting in itself, does not affect my claim that taxonomic categories are a stepwise function. Think of a staircase built by some drunken carpenter, in which one riser is the proper height of seven inches but other steps are six and a half or eight inches; you wouldn't pay the lout until he fixed them, but his staircase would, nevertheless, be a set of steps. Between two groups in taxonomy there must be a gap. This is demonstrated every time a naturalist lumps together two species that were based upon specimens collected from distant localities, after their differences are obliterated by specimens from the land between them, bearing intermediate features. The same thing goes on with the higher categories, where transitional forms can change our judgement of how to group things.

STRICKLAND: Getting your opinion on this chart, into which I put so much labour, is becoming more complicated than I expected.

DARWIN: I certainly beg your forgiveness, because in an important sense, the distinction between continuous and stepwise functions is a false dichotomy. A stairway would become a ramp if we made the risers small enough.<sup>11</sup> It is my experience that this situation arises around the species level much more often than at

<sup>11</sup> The essence of calculus is to make the steps of analysis so small that they become a smooth function. When Darwin and Strickland were undergraduates, Cambridge, but not Oxford, was beginning to expose undergraduates to this idea (I am indebted to Adrian Rice for this information). I have no evidence that either Darwin or Strickland knew anything of calculus, nor do I claim that exposure to it would have given rise to these thoughts about taxonomic affinity. Peter Stevens's invaluable historical study *The Development of Biological Systematics* (New York: Columbia University Press, 1994) shows that whether affinity was continuous or discontinuous was a central issue for early taxonomists. There is no doubt that Darwin gave considerable thought to the question; he proposed that evolution is continuous and that taxonomic groups are the product of gaps produced by two factors: divergence and extinction.

higher levels. By your sensible decision to use the genus as your lowest rank, you have avoided it.

STRICKLAND: I recall now that the distinction between varieties and species fascinated you when we were working on the nomenclature committee. And didn't you tell me a while ago that you at last did write a whole book on the subject?

DARWIN: Indeed I did. It is at the species level that the relationship of kinship enters the picture most clearly. I believe, as firmly as I believe anything, that species in the same genus are cousins, quite literally.

STRICKLAND: Oh, my, oh my. Well, I can understand how you could form that idea, since nearly all our fellow naturalists thought of varieties within a species as descendants of a common ancestor, the original pair created by God, just as cousins are descendants of the same grandparent. It does not stretch credulity, once one decides that species are not firmly fixed, to extend that plasticity to the genus. Yet I am surprised at you, because I had the distinct impression that you and I were in full agreement that species, things quite distinct and separated by gaps, do exist in nature.

DARWIN: Absolutely, and I still believe that's true for most species, though not for all, and only if we are speaking of plants and animals alive today.

STRICKLAND: I thought Lyell had pretty well scotched Lamarck's theory of transmutation. Anyway, anatomists like Cuvier and Owen made the idea even more unlikely by showing that there are distinct types between which no intermediates are conceivable.

DARWIN: I do not consider our lack of imagination to be evidence.

STRICKLAND: This is a delicate and difficult topic, but since I was limiting my attention to living birds, except for a few extinct forms like the dodo, speculation about the past is surely irrelevant, and so, may we return to my chart?

DARWIN: With pleasure. I do apologize for throwing us off the track, especially because I believe that the variable size of the gaps separating the higher ranks has nothing whatsoever to do with varieties at the species level. Furthermore, the fact that organisms are found to resemble each other in descending degrees is to me a fact of capital importance, which, I may add, the colours on your chart most beautifully display.

STRICKLAND: You mean that sincerely, you aren't just trying to flatter me?

DARWIN: With all my heart. I am convinced that our great familiarity with this ubiquitous practice in zoology and botany, the subordination of group under group, has blinded us to its deep significance. Careful, best secure that corner with a stone, the paper wants to curl back up. I am struck by how you use several methods of indicating the boundaries of the subfamilies, families, and tribes. You have simple black lines surrounding the subfamilies, dashed lines around your families, and bright red lines around your tribes. I admire the way you have washed in areas of colour for the subfamilies. Furthermore, you have written the names of those three kinds of group in three distinct typefaces and sizes. You have invented effective and imaginative ways to display most vividly the entire, irregular, taxonomic structure of a considerable portion of the class of birds (Fig. 3).<sup>12</sup>

STRICKLAND: What a pleasure it is to have your endorsement, my dear fellow.

DARWIN: You were wise, I say again, to choose genera as your starting point. You couldn't have fitted the names of every species of bird on a chart of practical size.

STRICKLAND: Quite so.

DARWIN: I notice that you made a slight change in how you mark your genera. On your 1840 map of kingfishers, the name of each genus is enclosed in an oval, its length apparently determined simply by the number of letters in its name, but here I see generic names enclosed in ovals of a standard size (Fig. 4).

STRICKLAND: I very much appreciate your paying such close attention. When I thought it over, I realised that if the ovals really represent genera, they should be large or small depending on how many species they contain. That averages about five, for birds, but the number ranges from several score down to monotypic genera like *Steatornis*, of which we know but one species, *Steatornis caripensis*, the oilbird of Venezuela.

DARWIN: I see your point, and I appreciate your solution. To vary the ovals thus would have cost a great deal of extra trouble, not just because the numbers differ so much, but because ornithologists never quite agree about which species are valid and which ones should be lumped together. You opted to make your ovals all the same size to avoid endless quibbles.

<sup>12</sup> Systematists nowadays describe this structure as a hierarchy, but Victorians naturalists did not use this word.



**Figure 3.** Detail from Hugh Strickland's 1843 wall chart. The red letters, now greatly faded, name the tribe "Accipitres" (birds of prey), the black dashed lines encircle families like "Falconidae," the subfamilies are washed in various colours to make the clusters into noticeable units. Image courtesy of the Cambridge University Museum of Zoology, which granted permission to digitally remove the blue lines enclosing orders and the blue letters spelling the order, Insectores. Strickland added those when he enlarged the chart in 1844.

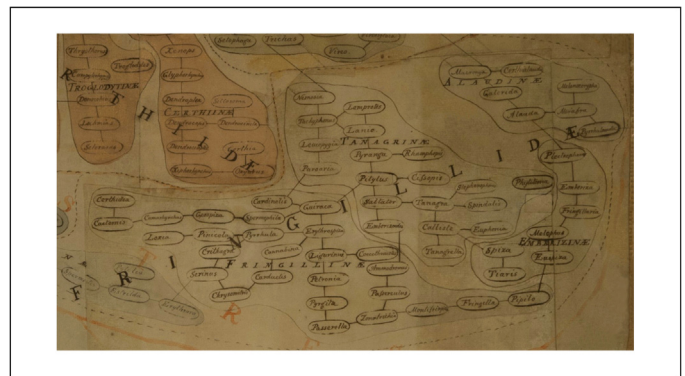
**STRICKLAND:** Exactly so. In fact by calling my ovals "cartouches" I tried to signal that they don't represent the genus itself, but only enclose its name, just as a hieroglyph only gives the name of a pharaoh, not an image of the man.

**DARWIN:** I once tried myself, in a feeble way, to put on paper a representation of how the natural groups might have come about, but I quickly gave it up. Two dimensions are hopelessly inadequate to capture the countless affinities that make up the natural system.

**STRICKLAND:** I quite agree. That was obviously the problem with the old notion that nature forms a great chain of being. Each link in a chain is connected to exactly two others, forming a simple series, whereas everyone knows nature is more complex than that. A genus may be affined to one other genus, or to two, or to several.

**DARWIN:** In your "True method" paper, you went so far as to suggest that the ramifications of affinities might be so complicated that they could not be represented on paper, perhaps not even in three dimensions.

**STRICKLAND:** Yes, and I worried a bit about that, because if that were so, then it would be impossible to visualize the affinities; they could only be captured by a mathematical formula, well beyond my powers. I imagined that the lines of affinities might



**Figure 4.** Detail from Hugh Strickland's 1843 wall chart. In his finch family Fringillidae Strickland has subfamilies Fringillinae (true finches), Tanagrinae (tanagers), Emberizinae (buntings), and Alaudinae (larks). Each genus name is in a "cartouche" of uniform size, whether enclosing the seven letters of Pitylus or the fourteen letters of Coccothraustes, a change from his 1840 map (Fig. 2). I have erased what Strickland added in 1844, a wide blue line enclosing the order and blue letters spelling "Insectores". Image courtesy of the Cambridge University Museum of Zoology.

escape up off the paper into the third dimension; in that case, their ramifications could be represented by a tree. That gave me the idea that a museum could display a family of birds by placing a stuffed example of each genus perched on the branches of an appropriately constructed tree.

**DARWIN:** It would be a pretty object, but a misleading one, in my view.

**STRICKLAND:** Misleading? In what way?

**DARWIN:** I have serious reservations about the meaning of affinity. However, please let's set my worries aside for now, and first concentrate on the many wonderful details in the chart you displayed at Cork. It really was a *tour de force*.

Looking now at the lines of affinity, I see something curious. You were a sworn enemy of quarianism, yet there are several places where I can trace a path from one genus to another and before long arrive back at the first. Isn't such a circuit what Macleay and company meant by a circle? Here's two in the Fringillinae, the finches (Fig. 4).

**STRICKLAND:** Some sequences of affinity do naturally loop around, which is doubtless what misled those naturalists into expecting all affinities to do the same, but as you well know, the quarians' circles came in packets of five, while the ones I found had no fixed number. Also, each circle in a quarian system is tightly bound to others by analogies, which form structural crosspieces.

**DARWIN:** Quite so. The form of life occupying the third position of one circle must resemble the form of life in the third position of four other circles.

**STRICKLAND:** The very idea of it fair turns my stomach.



- DARWIN: Never fear, I understand that your chart excludes any resemblance that could be called an analogy. Limiting yourself to affinities was one of your chief principles. And you certainly have no paired circuits.
- STRICKLAND: My overriding principle, I still insist, was to remind naturalists of the inductive method.
- DARWIN: I am curious about your method. Did you begin with a large pile of birds from your collection, or did you just write each genus name on a little card? Wallace recommended punching out a stack of little paper circles using the punch one uses to make gun wadding. One could then move the names around seeking the best arrangement, putting similar ones closer together than dissimilar ones.
- STRICKLAND: What a clever idea. And he was a collector in the East Indies, you say? Well, to begin with, you must understand I was very familiar with nearly every kind of bird.
- DARWIN: It must have been helpful that Gray had published his definitive list.<sup>13</sup>
- STRICKLAND: A work I knew intimately, but my first-hand knowledge was my main advantage. As a boy I had learned how to shoot, skin, and mount birds. Would you believe I even made the cases in which they were displayed, having learned some carpentry and glazing? Luckily for me, our house had plenty of space and when my father saw I was serious, he paid for whatever I needed. From my 1835 excursion to Turkey with Mr. Hamilton, I brought back over a hundred specimens.
- DARWIN: But you never again went abroad, I believe?
- STRICKLAND: Much to my regret. From my youth I had burned to see the world, perhaps in the navy, but duty to my family denied me any such great adventure as you enjoyed. The death of my older brother and younger sister in 1836 obliged me to return to England and reside with my parents in Worcestershire. Enlarging my bird collection with specimens from every corner of the globe allowed me to experience the world's variety, vicariously.
- DARWIN: So many country houses were inhabited by gentlemen, and ladies too, who delighted in assembling collections of natural objects, whether shells, insects, fossils, even seaweed. The popularity of those amusements provided a living to publishers of magazines and books, as well as to collectors like Wallace.
- STRICKLAND: I obtained over a thousand birds from far-flung places from my cousin Nathaniel, and many more from a variety of dealers.<sup>14</sup> By 1843 I had something like 3,000 specimens.<sup>15</sup> It was very nearly a complete collection, that is, I had at least one specimen representing every genus on Earth.<sup>16</sup>
- DARWIN: The number is stunning; didn't your house resemble an aviary?
- STRICKLAND: No, because by then I had recognized the wisdom of Swainson's advice that specimens for scientific study should be left unmounted. Instead of being stuffed and made life-like, my birds were just skins, preserved with arsenic soap, of course, which was adequate since most taxonomic characters came from feet, feathers and beak. Arranged in drawers, these specimens fit easily into one room.
- DARWIN: Clearly your great collection was essential to your making the chart.
- STRICKLAND: Sometimes quite literally. I could spread out on a large table all the specimens in some sub-family or family and move them around, like tin soldiers in a war-game, until I was satisfied with the arrangement.<sup>17</sup>
- DARWIN: Most remarkable. What a store of energy you had, eh?
- STRICKLAND: I detect that you are making an effort to be tactful.
- DARWIN: By no means, I am quite sincere in my admiration, for I was only too glad to dispose of all my *Beagle* specimens, and it filled me with awe to think of the hours of work men like Gould, Waterhouse, and Hooker were putting into the preservation, recording, and storage of specimens, without which natural history would not exist. My appreciation multiplied when I took up the taxonomy of one group myself, the Cirripedia. I never allow the name of a serious taxonomist to be disrespected in my presence. What you are detecting in my tone, I suppose, is that I doubt your philosophical claim, that your method of tracing affinities was pure induction. When I was at college I greatly admired Herschel's book, and Whewell also made clear that framing hypotheses is

<sup>14</sup> Osbert Salvin, *A Catalogue of the Collection of Birds Formed by the Late Hugh Edwin Strickland, M.A.* (Cambridge University Press, 1882), p. v.

<sup>15</sup> Leendert C. Rookmaaker, *Calendar of the Scientific Correspondence of Hugh Edwin Strickland in the University Museum of Zoology, Cambridge* (Cambridge: University Museum of Zoology, 2010), p. 33.

<sup>16</sup> Paul Farber, in *The Emergence of Ornithology as a Scientific Discipline: 1760–1850* (Dordrecht: Reidel, 1982), points out (p. 109) that although the 1844 edition of Gray's *List* contains 2,400 names of genera, Gray accepted only 815 of those as valid, judging the others to be synonyms. Strickland's 1843 chart named 490 genera of passerines and birds of prey, which comprise roughly half of all birds.

<sup>17</sup> I am imagining, without direct evidence, how Strickland probably worked.

<sup>13</sup> The first edition of George Robert Gray's definitive *List of the Genera of Birds* appeared in 1840, and Strickland went through it line by line.



not a bad thing. The inductive process, as I understand it, does not operate without hypotheses.

STRICKLAND: Wherever do you see any hypotheses entering into my method?

DARWIN: I see several, but will start with the simplest. You said it yourself, and I could see it on your chart. You had in your hands the specimens, but you had in your mind the groups long familiar to ornithologists. Look, here at the top, there's the tribe Accipitres, the hawks, falcons, vultures, and owls. You already knew which genera belonged there, and into which families they were commonly divided. Can it be a coincidence that you began with the group that was the first in Linnaeus's six groups of birds?

STRICKLAND: Of course not, but I would hardly call prior knowledge, arrived at by the efforts of our many predecessors, an hypothesis.

DARWIN: For myself, I consider a fact to be something that cannot be altered, and an hypothesis something that can. You know very well that group boundaries are a thing that taxonomists are not afraid to alter whenever new facts come in to disturb them.

STRICKLAND: I protest that you are once more posing a false dichotomy. A group whose naturalness the best ornithologists agree upon seems more like fact than supposition to me, because the progress in zoology in this century convinces me that we are discovering what is real. None of my lines were drawn arbitrarily. My rich collection enabled me to make careful assessments of every group boundary and line of affinity I drew.

DARWIN: I have no doubt of it. Hypotheses can become fact when a range of evidence, considered by several expert judges, proves them correct. Still, you must admit that there are some lines of affinity and some group boundaries on your chart that were being debated in 1843. Even I, though I'm no expert, can see several details that differ from what leading authorities came to believe in subsequent decades. For example, you have the brilliant little sunbirds of Africa (Nectariniidae) directly linked to the hummingbirds of the Americas (Trochilidae). You drew a line of affinity from the type genus *Nectarinia*. Ornithologists nowadays would say their similarities are merely the result of being adapted to similar nectar-feeding lifestyles. Likewise you have swifts, *Cypselus*, within

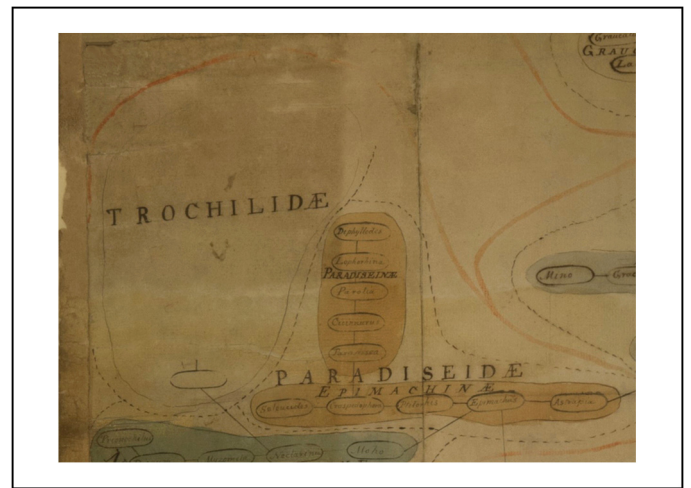


Figure 5. Detail from Hugh Strickland's wall chart. Above the birds of paradise, Paradiseidae, and the sunbirds, Nectariniidae, Strickland has sketched one empty cartouche and left a space for the hummingbirds, Trochilidae. One other family on his chart, the Psitticidae (parrots), is also vacant. Image courtesy of the Cambridge University Museum of Zoology.

the swallow family Hirundinidae (Fig. 5).<sup>18</sup>

STRICKLAND: You have me there. Soon after I finished my chart, the affinity I drew between *Hirundo* and *Cypselus*, agreed upon by most of us, was destroyed by a couple of foreign ornithologists because of anatomical differences. If other anatomists later moved the sunbirds far from hummingbirds, I must defer to their deeper investigation. So if I understand you rightly, your point is that some taxonomic groups are hypotheses, while others are fact?

DARWIN: Yes, and the challenge is always to know which is which, or I should rather say, to understand at which point along the continuous scale that connects solid fact to wild speculation a particular group lies.

STRICKLAND: The idea that a taxonomic group is an hypothesis is a novel way of looking at it.

DARWIN: Here's another hypothesis I see on your chart, near the top. Your arrangement postulates that the goatsucker family forms a link between the owls and the swallows.<sup>19</sup>

STRICKLAND: Well, most ornithologists would agree with that placement, yet I grant that

<sup>18</sup> Edward Blyth, in his "Outlines of a new arrangement of insessorial birds," *Magazine of Natural History* n.s. 2 (1838): 256–268, had already seen that swifts do not belong with swallows (p. 262), but the association remained standard until challenged by J. P. Müller and Cabanis, according to Charles G. Sibley and Jon E. Ahlquist in their classic *Phylogeny and Classification of Birds* (Yale University Press 1990), p. 392.

<sup>19</sup> The goatsucker (*Caprimulgus europaeus*), also called nightjar, was a bird familiar to most Europeans: "[Every Spring] the song of the cock from its singularity, attracts attention amid all rural sounds." Alfred Newton, *A Dictionary of Birds* (London: Adam and Charles Black, 1896), p. 639. Its North American cousin, the whip-poor-will (*Antrostomus vociferus*), has a similar lifestyle and a distinctive call. The oilbird is still classified as a relative, in the larger group Caprimulgiformes. The origin of the odd name "goatsucker" may be explained by its habit of lying on the ground among grazing animals, and darting up to feed on insects flying near them.

some of us worry sometimes whether those are really affined. It is not surprising that the nightjar's plumage resembles an owl's, for both of them rest in the daytime, so they have to blend in with bark and dry leaves. And their swift acrobatic flight, similar to the swallows, is surely an adaptation to the nightjar's mode of feeding, catching insects on the wing. Possibly we are being misled by resemblances that are only analogies.

DARWIN: Much as Cuvier was misled by the resemblance of dugong to dolphin.

STRICKLAND: I feel pretty sure in this case my affinities are correct, but I take your point that in placing the groups where you see them, I was stating an hypothesis.

DARWIN: Not that there is the least thing wrong with that; I mean merely to dissent from your description of pure induction.

STRICKLAND: I still find this rather a subtle point.

DARWIN: Then let me point out the most striking evidence that your prior knowledge of groups controlled your operation. Here we see two important kinds of bird, the hummingbirds, family Trochilidae, and the parrots, family Psittacidae, named but consisting of empty spaces, no genus, no cartouche in either of them (Fig. 5).

STRICKLAND: Well, those are both such distinctive kinds of bird that a child cannot mistake them, and both families contain a great many genera, so when I realized I was running out of time I just indicated their location. It did no damage to my purpose. I never found time to fill them in, what with my marriage, and our move to Oxford.

DARWIN: Quite understandable, but don't you admit that those two families expose the procedure you followed in writing all the others? Forgive my impudence, but you did announce in the "True method" paper that you would discover natural groups by means of step-by-step accumulation.<sup>20</sup>

STRICKLAND: Feeling so perfectly at peace in this blissful place, I cannot quarrel with you, but I must confess to some surprise and amusement at your quibble. If the naming of categories, which was the dreary duty of all taxonomists, can be termed hypothesizing, then our endeavour would enjoy a more elevated status among the sciences.

DARWIN: And I would be the first to applaud that elevation. That was certainly how Whewell regarded the importance of naming

and classifying. I regret to say that since your premature departure, systematists are getting worse rather than better at convincing those who control the public purse that their work is of high value.

STRICKLAND: You spent many years of drudgery yourself, I know, to produce monumental monographs of barnacles. I would never have had the patience. In fact it's hard to imagine lavishing so much attention upon such small and unattractive creatures. I cannot imagine why the ocean would require more than a dozen or so species.

DARWIN: Sometimes I wondered myself. I did not set out to do taxonomy, but I became curious about one peculiar form, and then came across the puzzle of their sex lives. I hadn't really intended giving them so much time, but once word got out that I was looking at barnacles, every collector sent me whatever fragmentary specimens were sitting unidentified in his cabinet of shells. I am sure I could not have done as well as I did if I had to work under the conditions in which the employees of our national museum must work.

Now please tell me, when you had on your large table a dozen or two related specimens, each one standing for a genus, were they at first scattered randomly about?

STRICKLAND: Randomly? Let me think, how did I arrange them? In most families I already knew which was the type, and I put it in the middle.

DARWIN: Ah, yes, the type. I put my long-suffering correspondents Waterhouse and Hooker on the rack, demanding from them a coherent explanation of what that word signifies. They both confessed they could give it no rigorous definition, admitting that zoologists and botanist had vague and variable notions of that familiar concept. Sometimes the type was a species with distinctive characters, like the cat being the consummate carnivore, while at other times the type was something like the average, a central point of reference to which other forms are compared, perhaps an ideal that doesn't exist. Many times the type of a group was merely the most common or familiar form.

STRICKLAND: You may remember that I insisted that the idea of type was purely a human invention for our own convenience, and should not be mistaken for something existing in nature.

DARWIN: Such was obviously the case within the rules of nomenclature, where designating

<sup>20</sup> Strickland would have known about the first two volumes of Darwin's *Monograph of Cirripedia* of 1851, though not about the Royal Society medal these volumes earned Darwin a few months after Strickland's death, nor the two other volumes of 1854.

a type species for each genus, and a type genus for each family, was a good rule, delivering stability when an old genus or family was divided up. Instead of replacing one name with two new names, we would retain the old name and create only one new one. That wasn't the meaning of type I was concerned to clarify, it was the one Whewell referred to in his book on the inductive sciences.

STRICKLAND: I thought he did a brilliant job of explaining that naturalists were not in the business of merely defining words, which you can do by listing all the elements that go into each word's meaning, but that when they described a kind of organism they have in mind a comparison to other organisms similar to it. I certainly didn't get that idea from Whewell though; it was a concept we simply used without worrying too much about

defining it. Edward Blyth had a sound grasp of the issue, but his language was painfully obscure.

DARWIN: I quite agree. Yet back to the subject, which is your wonderful chart.

STRICKLAND: Besides my prior knowledge of the accepted groups of birds, to which I plead guilty, dare I ask whether you see any other nasty hypotheses in my chart?

*to be continued*

#### **Acknowledgements**

I am greatly indebted to many people who have encouraged my unorthodox exposition of issues in the history of systematics. In addition to those I acknowledged in Part One, I owe thanks to my friends Margaret Kingsland and Carolyn Lane.

#### **Appendix A. Supplementary data**

Supplementary material related to this article can be found, in the online version, at <http://dx.doi.org/10.1016/j.endeavour.2014.11.001>.