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Ronald Brady and the cladists

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Abstract

Ronald Brady was the first philosopher to defend pattern cladistics as an independent scientific field. That independence was achieved through the decoupling of biological systematics from phylogenetics—that is, inferred evolutionary processes (e.g. character transformation). Brady saw parallels between biological systematics and Wolfgang von Goethe's Morphology, an empirical scientific field that incorporates human observation and perception to discover coherent morphological structures. Goethe's Morphology and pre-Darwinian systematics were independent from evolutionary narratives, a tradition that continued into the 20th Century through the work of biologists such as Agnes Arber. Most importantly, Brady provided the philosophical and historical foundations to an independent systematics by demonstrating the links between phenomenology, Goethe's Morphology and comparative biology.

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Introduction

There is still need for [a] dissenting voice, a devil's advocate, a skeptical whistle-blower (Macbeth, 1978, p. iv)

A rift occurred among cladists during the early 1980s, generally through an unwitting miscommunication about methodology. The prevailing view at the time was one derived by Brundin (1966) through his reading of Hennig (1950). Hierarchical biological classifications were based on synapomorphies and natural taxa were based on monophyly. Although the discovery of monophyly was evident in practice (i.e. taxa that are more closely related to each other than they are to any other taxon), how monophyly was defined differed (e.g. a taxon that included its most recent ancestor; see Vanderlaan et al., 2013). Unfortunately, this problem between identifying and explaining monophyly was largely left unresolved, leaving cladists to use the theory of process (e.g. unobservable and undiscoverable entities and mechanisms) to define the

practice of discovering monophyly (e.g. observable and discoverable patterns). The result was heralded as the triumph of process over pattern (see Hull, 1988). Those that had questioned the need for process to discover patterns led philosophers of biology to identify a subset of cladists they termed “pattern” or “transformed” cladists (Beatty, 1982). Although unobserved and undiscoverable processes were not necessary for discovering patterns, they were deemed useful for explaining patterns post discovery, an important point that was unfortunately lost on a majority of philosophers, such as Elliot Sober and David Hull (Hull, 1988; Sober, 1988). The pattern–process miscommunication was immediately used as a rallying cry by certain cladists to decry pattern or transformed cladism as “theory neutral,” “anti-evolutionary” and even “creationist”. Cladists identifying with the pattern approach were themselves victims of misquotes by creationists, further accelerating the anti-pattern approach within biology and beyond (Dawkins, 1986; Ridley, 1986; Hull, 1988).

A native from Yonkers, New York, Ronald Harold Brady (1937–2003) was one of those few dissenting

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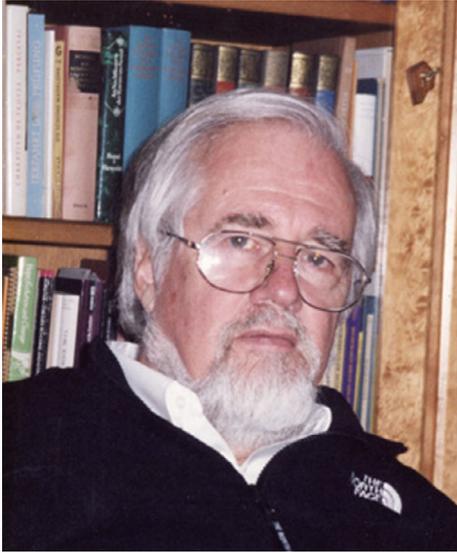


Fig. 1. Ronald Harold Brady (1937–2003). Used with permission from Nature Institute, Ghent, NY. Source: <http://natureinstitute.org/txt/rb/index.htm>

voices, a devil’s advocate, a skeptical whistle-blower within the wider cladistic community during the 1980s and 1990s (Fig. 1).¹ Unlike the pattern cladists who took him under their collective wing, Brady was a philosopher who did not toe the neo-Darwinian line that was common among his philosophical contemporaries, such as David Hull, Michael Ruse, Michael Ghiselin and Elliot Sober. Rather, Brady was concerned with critiquing the Modern Synthesis (i.e. neo-Darwinism) from a purely pragmatic perspective, which he derived from his study of Goethe’s approach to science. Brady was by no means the first, nor the last, to do this within comparative biology.

A decade before Brady, English botanist Agnes Arber (1879–1960; Schmid, 2001) attempted to show how comparative biology is independent of speculative processes (Arber, 1950). Pure morphology was for Arber the visual and conceptual interpretation of the perceived, rather than the conceptual prediction of the unperceived (Arber, 1964, p.125): “Arber felt that biological thinking suffered a sort of imprisonment by

being materialized, by turning from the actual relations of form which were the morphologist’s first concern to the physical theory of descent pinned upon them. She pointed out that this tendency was so powerful that it became impossible, within Darwinian modes of thought, to understand the idealistic concepts which had under written the phylogenetic ones [...]” (Brady, 1972, pp.179–180). For Arber, the “purely comparative aspect of the sciences of plant and animal form was emphasized in the days of Goethe and A.P. de Candolle, but fell somewhat into abeyance in the post-Darwinian period, when the passion for tracing phylogenies was at its height” (Arber, 1964, p.34). Her comment was apposite in 1964 as well as in 1972 when Brady was completing his doctoral dissertation.

Arber’s work provided the impetus for Brady’s sojourn into systematics. After all, why would a philosopher venture into the world of biological classification and evolution? The latter will be explained below. The former, however, may be due to Brady’s encounter during his undergraduate days as a chemistry major at the University of California, Berkeley with an unnamed botanist,

I later spoke to a morphologist at Berkeley about my interest in Goethe’s attempt to approach science by keeping to direct experience. The morphologist responded: “You are interested in this approach because you are a nature appreciator, while I am a productive scientist.” I left his office feeling very deflated. Again a representative of science had put his finger on my immaturity. (Brady in Maier et al., 2006, p.12)

Brady’s encounter had led him to question why aesthetic appreciation (“nature appreciation”), fully grounded in experience, disqualified him from science. For Brady there were two meanings of “experience”: one that was direct perception based on detailed observation, the other being a subjective point-of-view. Rather than continue with science, Brady turned to philosophy, an area where he “proposed a [PhD] dissertation on the crossover between scientific observation and aesthetic experience in Goethe’s science” (Brady in Maier et al., 2006, p.14). In his thesis, *Towards a Common Morphology for Aesthetics and Natural Science* (Brady, 1972 [unpubl.]) Brady refers to Arber’s work possibly because she had the same idea concerning the “crossover between scientific observation and aesthetic experience in Goethe’s science” (Brady in Maier et al., 2006, p.14). Moreover, Arber saw that in “the Darwinian reorientation of biology, however, the attention of most botanists was diverted from pure morphology to the use of form data in support of speculations about evolution” (Arber, 1950, p.63). Perhaps this also was a key influence in Brady’s later work, namely to separate out the thing to be explained (i.e. the *explanandum*), from the

¹A complete bibliography of Brady can be found in Amrine (2017, pp. 117–118), with the exception of Brady et al. (1990). Online versions of Brady’s work and a short biography can be found on the “Ronald Brady Archive” on the Nature Institute website: <http://natureinstitute.org/txt/rb/> The only known published obituary of Brady is in the *Ramapo Magazine* (Summer 2004): “Dr. Brady began his career at Ramapo College in 1972 as a professor of philosophy for the School of American and International Studies and taught courses in philosophy, literature and science. During his 31 years at Ramapo College, he presented thirty papers at major conferences all over the world and published 15 papers on perception and Goethe’s way of science” (Anonymous 2004, p.5).

explanation (i.e. the *explanans*; see Hempel and Oppenheim, 1948, p.152). For example, an empirically discovered pattern is an *explanandum* whereas an explanation of why that pattern has come into existence is part of the *explanans* (see Wilkins and Ebach, 2014). Brady (1985), paraphrasing comments made earlier by Remane (1952), commented that “He [Remane] has simply noticed that the definition of a pattern is interchangeable with the criteria used to recognize it (since those ‘criteria’ are actually the very relations that constitute the pattern), and he has avoided the error expressed above—i.e., that our explanation of empirical condition can define the condition” (Brady, 1985, p.117). If so, then the explanation of the empirical condition can define the condition. In other words, if “we fail to distinguish empirical problem from explanatory hypothesis [...] we have no independent evidence with which to test, or support, that hypothesis. By making our explanation into the definition of the condition to be explained, we express not scientific hypothesis but belief” (Brady, 1985, p.117). Brady uses the example of synapomorphy: “If we purify these definitions of explanation, comprehending within them simply the relations within the data that they are to define, then a character is *plesiomorphic* (more accurately *symplesiomorphic*) when it defines a group larger than and containing the group to be characterized, and *apomorphic* (synapomorphic) when it defines the group to be characterized [...] If we choose to add that such a character is, by our theory, primitive to the group (shared by ancestral forms), and that the synapomorphies proper to that group are uniquely derived in it, we begin to *explain* the distribution of characters by hypothesizing a historical process by which this distribution was formed. This account is, of course, the best explanation we have, but just because it is an explanation [*explanans*] we must not confuse it with a definition of the empirical conditions [*explanandum*]” (Brady, 1985, pp.117–118, original emphasis, *explanans* and *explanandum* added by us; also see Brady, 1994a,b, for a detailed history).

The theoretical confusion between the *explanandum* and *explanans* was, of course, discussed before Brady (e.g. Hempel and Oppenheim, 1948; for a history see Wilkins and Ebach, 2014). Brady’s main contribution was to carry on the tradition of aesthetic appreciation in the natural sciences, in the manner of Goethe’s empiricism, and bring it to a new generation of biologists in the late 20th Century, notably the cladists. Goethe’s empiricism, enshrined in Idealistic Morphology (e.g. Naef, 1919), had already been tainted by its association with German idealism and dismissed by early 20th Century biologists and philosophers of biology (Williams and Ebach, 2007).

Brady’s study of Goethe’s empiricism

Brady was a relative outsider to biology compared to his contemporary philosophical colleagues at larger universities, such as David Hull and Elliot Sober. He taught at the School of American and International Studies at Ramapo College, a liberal arts college in Mahwah, New Jersey² between 1972 and 2003, after finishing his PhD³ in philosophy at the State University New York, Buffalo (SUNY) on Goethe’s way of science [Brady, 1972 (unpubl.)].

With the exception of Arber, and German botanist Wilhelm Troll (1897–1978; Weberling, 1999), much of Goethe’s scientific method was critiqued by philosophers and historians of science (see Seamon and Zajonc 1998). For many philosophers, Goethe’s science did not fall neatly into Aristotelian or neo-Platonic tradition, and many placed it within phenomenology (e.g. Heinemann, 1934). Many historians, not impressed with Goethe’s “Theory of Colours [*Zu Farbenlehre*]” dismissed it as amateurish and flawed (see Wolff, 1953). Brady, however, noted Goethe’s unique strain of scientific enquiry—viewing nature and morphology from an aesthetic viewpoint—which has taken historians and philosophers of science a while to rediscover (e.g. Steigerwald, 2002). In his thesis, Brady noted that Goethe and Schiller’s own brand of aesthetics “is not to be found in the histories, nor in the comments of later philosophers. Indeed, the whole thing seems to have slipped out of history altogether, and matters advance today as if it never was” (Brady, 1972, p.2). Yet, even by Brady’s own admission, Goethe’s aesthetics were “easily and usually misread [and] only a few have troubled to ask where his many insights are actually coming from. (I was fortunate to come upon two such men, and had I not read Rudolf Steiner and Ernst Cassirer on Goethe’s epistemological position some years ago, I would never have realized that his work possessed something I very much desired)” (Brady, 1972, p.4). For Brady, art critics and philosophers had done little to penetrate aesthetic appearances, and in order to do so “[...] we need a study of form as experienced, or appearances seen or heard; in short, a *morphology* capable of discovering the coherent structure with appearances *qua* appearances. It was just such a

²Some of the courses Brady taught at Ramapo College were: Introduction to Literature, Readings in Poetry, Introduction to Philosophy, Introduction to American Studies, Readings in the Humanities, English Romantic Poets, Philosophy of Beauty, Ancient Philosophy, Plato and, Philosophy of Science (Edward Saiff pers. comm., 2017). Brady also was an Affiliate Researcher at The Nature Institute and also a member of SENSRI, in Saratoga Springs, New York later in his life (<http://natureinstitute.org/txt/rb/index.htm> accessed August 8, 2018).

³His thesis was written in 1971 at the Goetheanum in Dornach, Switzerland (Maier et al., 2006, pp.162–163).

method that Goethe developed, and for it coined the name *Morphology*. This is his empirical base” (Brady, 1972, p.4, original emphasis).

Goethe’s science poses a problem for philosophy. In order to penetrate aesthetic appearances you need to study “form as experienced,” meaning that Morphology has a practical component to it, one that cannot simply be discussed on paper, and one that requires the active perception of an observer. Yet, by not practicing morphology, that is, observing and comparing organisms and their parts, how then is it possible to justify it theoretically? One way would be to construct a narrative about what one thinks may be occurring (sensu Hanson, 1965). How can you view something without some sort of theory or process in mind, such as a species concept, or an evolutionary mechanism? Brady noted that Goethe questioned whether science must be Kantian, that is, whether synthetic universals are “logically prior to that of the nature of science” (Brady, 1972, p.68). How then, asks Brady, are we able to “conceive of a scientific method which could question it [?] In my bibliographical searches I could not find one article which put forward the view that the validity of Goethe’s aesthetics was dependent upon the validity of his morphological studies; such is the strength of established thought” (Brady, 1972, p.68). “Morphology, as Goethe developed it, is the study of the structure of appearances through direct inspection of phenomenal appearances. It is, therefore, an independent science in itself [...]” (Brady, 1972, p.300), that is, independent of metaphysical musings, such as hidden process and synthetic universals, and *dependent* on form and the observation of form (e.g. aesthetic appreciation).⁴ Although Brady believed that this point was missed by both historians and philosophers of science, it was not lost on practitioners of morphology, namely the comparative biologists of the past (e.g. Owen, Naef, Arber, etc.). But what of today’s comparative biologists?

Theoretical issues and “pattern cladistics”

Another influence on Brady was retired lawyer Norman Macbeth (1910–1989), who published “articles

⁴Brady cites Swiss comparative biologist Adolf Naef “‘Since it defined the natural system of organisms idealistic morphology is not only the pre-condition for the introduction of phylogenetic in the history of the science but is still the logical basis for the same. (After all, we cannot search for things which are no longer in existence without any previous suppositions)’ [Naef, 1919, p.33, translation in Brady, 1972]. Ernst Cassirer, writing in 1940, called Naef’s presentation a clear expression of ‘not scare’ the originality and methodological justification of idealistic morphology [Cassirer, 1950, p.145]. The present writer can see no reason why this judgement should not hold true today” (Brady, 1972, pp.178–179).

questioning Darwinism and Neo-Darwinism with a piece in the *Yale Review*” (Brady et al., 1990, p.523, see Macbeth, 1967). In 1971, Macbeth published *Darwin Retried: An appeal to reason* (Macbeth, 1971)⁵, a book that pointed out that traditional Darwinism is dead and many people, particularly lay-people, were unaware of this and unaware of the rise of Neo-Darwinism. Macbeth was unknown to the scientific community and wrote his book in comparative isolation from professionals. In fact, it was after his book was published that Macbeth made contact with the American Museum of Natural History. There he attended the monthly meetings of the Systematics Discussion Group, eventually contributing a talk as well as publishing in *Systematic Zoology* (e.g. Macbeth, 1975, 1979, 1980). Macbeth became a research associate in the Ichthyology Department at the American Natural History Museum (ANHM), as well as a Visiting Professor at Ramapo College in New Jersey, where he “savoured lecturing to students at the College on evolutionary topics” (Brady et al., 1990, p.525).

Macbeth’s influence on Brady’s work is noted in *Natural selection and the criteria by which a theory is judged*, where Brady acknowledges Macbeth “for bringing my attention to the problem in the first place, and for many conversations on the subject” (Brady, 1979, p.621) and later in *Dogma and Doubt*: “Norman Macbeth, whose book and many conversations on this subject made it irresistible” (Brady, 1982a, p.96, it being the status of “natural selection”); Donn Rosen and Norman Platnick, both cladists at the AMNH, were also acknowledged for comments and suggestions. In fact, Brady’s association with the Museum was due to Macbeth, who was probably initially contacted by ichthyologist Donn Rosen (Platnick, pers. comm. 2017).

Rosen and fellow ichthyologist Gareth Nelson, both at the AMNH, were at the time leading an assault on palaeontologists because of their assumptions concerning the discovery of actual evolutionary lineages and of actual ancestors (Williams and Ebach, 2004). Brady’s involvement, particularly his critique of natural selection, was timely at best. US historian John Beatty identified a subset of cladists who were termed “pattern cladists” (Beatty, 1982, p.25). Beatty decried the notion of an “evolutionary neutral brand of cladistics” that “is at odds with evolutionary thinking” and it is worth “considering whether the supposed methodological and empirical achievements are worth the price of the conceptual confusions paid”—namely the notion that those unwilling to discard evolutionary theorizing will somehow find themselves at a quandary when doing pattern cladistics (Beatty, 1982, p. 33). This critique may have resonated

⁵There were two further reprints, with the 1978 reprint a short forward (Macbeth, 1974, 1978).

with Brady, given that his earlier criticism of natural selection in *Systematic Zoology* resolved the “conceptual confusion,” such that “the actual discovery of the patterns of nature may not necessitate a theory of their mechanism” (Brady, 1979, p.620). Brady saw Beatty’s complaint extending to all cladists: if any cladist refused “to interpret their results according to current [evolutionary] theory,” then that “would not be injurious to the pursuit of science” (Brady, 1982b, p.290). True, Brady (1979) was critical of the pervading 1970s understanding of evolutionary theory; however, either Beatty had overlooked Brady’s critique or had decided to ignore it. One simply has to look at Brady’s contribution to the second Willi Hennig Society (WHS) meeting in Ann Arbor, Michigan on 1–4 October 1981⁶, entitled “Parsimony, hierarchy, and biological implications.” Brady’s message was simple: hierarchical patterns were not merely points of view as “too many workers from too many different theoretical persuasions have recovered it” (Brady, 1983, p.59). In this sense, “homologies *are* hierarchical” rather than transformational, something reached via observation rather than speculation. The connection between Brady and the cladists is therefore two-fold: through the anti-Neo-Darwinian viewpoint held by Macbeth and through the Goethean/Husserlian paradigm of observation as empirical.

Defending “pattern cladistics”

The mis-labelling of pattern and transformed cladistics as “theory-free” and “anti-evolutionary” most likely first occurred at the third meeting of the WHS at the University of Maryland, 20–22 November 1982. Brady gave a presentation entitled “Description, Explanation, and Tests: Interrelation Between Observation and Theory” and Platnick gave a presentation entitled “Philosophy and the Transformation of Cladistics (Revisited)”⁷ (Stevens, 1983). Both defended the position “that pattern recognition should be carried out independently of covering process theories” (Stevens, 1983, p.287). Here one of the first claims that pattern cladistics was “theory neutral” and “anti-evolutionary” was made, notably by US philosopher Marjorie Greene, leading others (Kluge pers. comm. in Stevens, 1983) to point out that “transformed cladistics and evolutionary theory will become separated” (Stevens, 1983, p. 288). Stevens’ conclusion that pattern cladistics was a rehash of idealistic morphology, something that “Hennig (1966) had made sharp criticisms of [...] so the wheel has turned full circle” (Stevens, 1983,

p.287; see also Hull, 1988, p.263). Hull considered Brady to be “partial to Goethe,” and “extremely critical of Darwinian versions of evolutionary theory [urging] scientists to make their classifications as independent of process theories as possible” (Hull, 1988, p.375). Hull goes on to infer that Brady acknowledges (“does not deny”) the existence of pattern cladism—after all, Hull posed the question: “Did pattern cladists exist, or were they figments of overheated imaginations?” (Hull, 1988, p.263). Brady may have provided an answer—pattern cladism was a thing and it did have practitioners, all of whom were cladists. Brady’s defense of Pattern Cladistics and the decoupling of systematics from phylogenetics was his most significant contribution⁸ as it may have endeared the term to those who identified as pattern cladistics (sensu Platnick, 1979; Brady, 1982a,1982b, 1985; see also Ebach et al., 2008)⁹. The terms cladistics and pattern cladistics were first used by detractors and adopted by those who were accused of practicing so-called “theory neutral” and “anti-evolutionary” systematics. The problem, of course, is that the definitions stayed associated with the label “pattern cladistics” leading cladists to unwittingly take the position of philosophers such as Greene and Hull when accusing patterns cladists of being “anti-evolutionary”¹⁰ and “theory-neutral” (see Williams and Ebach 2007 for a history). For example, Carpenter (1987), in a tongue-and-cheek article published in *Cladistics*, presented a “Cladistics of Cladists.”¹¹ The article was a response to an earlier piece

⁸“Ronald H. Brady formulated what at the time was considered by many to constitute the historical and philosophical foundation of pattern cladism and its independence from evolutionary theory” (Rieppel, 2014, p.127).

⁹Carpenter (1987, p.218) sarcastically “accused” Brady of leading Nelson and Platnick “down the primrose path” of pattern cladistics. Sarcasm aside, this is clearly not the case. Rather it was Brady who defended the term “pattern cladistics,” which was ascribed to an existing practice attributed to Nelson and Platnick. Carpenter (1987) clearly admits to the existence of a form of pattern cladistics “by whatever name” well before Brady (1983) defended the term: “Van Valen, 1978, referred to it as ‘New York cladism’; Cartmill, 1981, as ‘neocladistic’; Hill and Crane, 1982, as ‘methodological’ cladists [...]” (Carpenter, 1987, p.364). In addition, Dupuis (1984, p.14) called transformed cladists “Nelsonians” citing Platnick (1979) as well as “numerous other advocates,” such as (Forey et al., 1982) and Patterson (1980, 1982).

¹⁰Examples include: “... which even those anti-evolutionary pattern cladists (you know who you are) would accept” (Carpenter, 1986, p.188).

¹¹Ebach et al. (2008) conducted their own cladistics of cladists using “a ‘short-hand’ approach to identify synapomorphies rather than considering the distribution of binary characters” (Ebach et al., 2008, p. -154). Ebach et al. (2008, fig. 1) did find that pattern cladists shared a single diagnostic characteristic (synapomorphy), namely “Homology as sameness, All taxa and their parts as individual gestalten, Monophyly as bildung,” contrary to Carpenter (1987), who claimed that there were no single characters to support some schools of thought.

⁶Brady’s talk is listed for “Sunday 4 October 1981 The principle of parsimony in systematics” (Platnick and Funk 1983, p. 214).

⁷“Philosophy and the Transformation of Cladistics (Revisited)” was eventually published in the first issue of *Cladistics: Journal of Willi Hennig Society* (Platnick, 1985). The authors are unsure whether Brady’s contribution was published under a different title.

by bryologist Brent Mishler, who was a “naive graduate student [and] teaching fellow for Professor John Beatty (of pattern cladistics fame, 1982) in a course called ‘The Darwinian Revolution’” (Mishler, 1987, p.55). Mishler wondered if pattern cladistics existed at all: “Is there a split within cladistics at present? I used to think so, when I thought in terms of ideological systems as phenetic clusters. But now I think that there is currently no split; perhaps the best evidence to offer is that we all attend meetings together” (Mishler, 1987, p.59). Carpenter (1987) was well aware of the split between pattern and phylogenetic cladistics¹² and responded by constructing a data matrix based on characters resembling in-house jokes within the Willi Hennig Society. The idea was to group practicing cladists, historians and philosophers of science into “taxa” (e.g. philosophers, Holy Rollers, Phylogenetic cladists) and use sets of “behavioral traits” that reflected a conflict within the WHS as well as between cladists and pheneticists: “8. Popperian-Beatty (1982) and Charig (1982) have both considered adherence to Popper’s philosophy of science as a fundamental attribute of ‘pattern’ cladistics, but in fact ‘phylogenetic’ cladists claim to follow these views as well (Wiley, 1975), whereas Patterson (1978, 1982) explicitly does not [...] 10. Cliques-Compatibility analysis versus parsimony [...] Be that as it may, ‘pattern’ cladists and clique freaks have been likened to one another” [...] 11. Vicariance-Interest in vicariance biogeography is largely congruent with characters like synapomorphy and monophyly. Considering that congruent biogeographic patterns are among the best evidence that evolution has in fact occurred, it seems inconsistent with the characterization of “pattern” cladists as antievolutionary that Nelson and Platnick (1981) have been so interested in their investigation” (Carpenter, 1987, p. 369). The result of the analysis revealed that “all cladists are a group on every tree, so this group may be a natural one. But there are neither ‘phylogenetic’ nor ‘pattern’ cladists on any tree [...] Undoubtedly the reason that none of the rumored ‘schools’ of cladists are revealed by these analyses is because single or few characters formed the basis for them. Systematists would do better to stop describing hypothetical groups, but since they probably won’t, I would suggest some others that might be sought (or christen them?). This is because some subgroups of cladists ought to exist, and these groups (pattern and phylogenetic) are basically boring” (Carpenter, 1987, pp.371–372).

¹²“Hull had a separate category for quantitative cladistics which was orthogonal to the other two; apparently the pattern/ phylogenetic dichotomy was insufficient to classify cladists. But of course Beatty (1982) was the one who really made the distinction notorious. He basically followed Hull in splitting cladists into pattern and phylogenetic, but added the accusation that the former were antievolutionary” (Carpenter, 1987, p.55).

Carpenter’s “Cladistics of Cladists” was perhaps the first time any cladist took the pattern/phylogenetic split seriously, regardless of the poor characters (intended more to entertain than inform) and the dismissal of the “pattern and phylogenetic” divide as “boring.” Not surprisingly, Brady never again broached the topic of pattern cladism as the debate drifted towards a battle of methods where the subtler theoretical nuances of pattern and process became obscure to the new generation of cladists (see Williams and Ebach, 2007). What, then, became of Brady’s main message, namely the “historical and philosophical foundation of pattern cladism and its independence from evolutionary theory” (Rieppel, 2014, p.127)? By the 1990s the discussion of the independence of systematics had shifted from the pages of systematics journals, such as *Systematic Zoology*¹³ (renamed *Systematic Biology* from 1992) and *Cladistics* to philosophical journals (e.g. Vergara-Silva, 2009; Winther, 2009; Pearson, 2010; Sterner and Lidgard, 2018) and symposia resulting in edited books (e.g. Grande and Rieppel, 1994; Scotland et al., 1994; Hamilton, 2013; Williams and Knapp, 2010; Williams et al., 2016).

On the independence of systematics

Brady’s call for an independent systematics drew the attention of cladists working at the Natural History Museum, London (called the British Museum (Natural History) until 1992; herein NHM), namely Colin Patterson, Peter Forey, Christopher Humphries, David Williams, Darrell Siebert and Robert Scotland. Brady had already interacted with the NHM cladists, possibly during the second and third WHS meetings in Michigan (1981) and Maryland (1982). Brady was invited to present “Pattern Description, Process Explanation, and the History of the Morphological Sciences” at the Systematics Association “Models in Phylogeny Reconstruction” meeting held at the NHM in August in 1993. This was to be Brady’s last piece on systematic theory¹⁴ (Brady, 1994a) and a combination of comparative biology and his ideas on observation and perception (*sensu* Brady, 1977, 1981, 1984), a topic he discussed seven years later as an invited paper titled “Perception and Hypotheses of Perception” at a

¹³No papers on pattern cladistics were published after Brady (1982b) in *Systematic [Zoology] Biology*.

¹⁴Brady also presented at the “Systematics and Process” symposium at the Field Museum in Chicago in 1992, which resulted in a chapter (Brady, 1994b).

conference sponsored by the Natural History Museum, London in April, 2000.¹⁵

Brady (1994a, p. 13) tackled the criticism that an independent systematics lacked explanatory power (e.g. Hull, 1973). Brady goes on to suggest that many who take this stance see observation as theory-laden and are “therefore never innocent of our explanations” (Brady, 1994a, 1994b, p.14). Although observation may be theory-laden, Brady argues it is not necessarily laden with explanatory power. Hanson (1965) noted that observation is a physical and conceptual act that is reported in a theory-laden language, which Brady notes is a “very different point.” Explanation, rather, is something that comes after observation, rather than before it, regardless of whether seeing is theory-laden or not. Brady’s main point and one that few philosophers have picked up on, is that the replication of perception, namely, the ability for an observer to replicate the original perception, “is a central goal of morphological description.” Brady continues, “since perception is the combination of intention and sensation, if either component varies the resultant perception must vary as well. On the other hand, explanation varies independently of perception, and several explanatory hypotheses may be advanced to explain the same observation” (Brady, 1994a, p.17). Descriptions are followed by explanations and although both are conceptual, “the concepts of the former cannot be changed without producing a new observation, while the latter set may vary freely against a fixed set of observations” (Brady, 1994a, p.17). Brady’s message, like that of Goethe’s *Morphology*, was “easily and usually misread [and] only a few have troubled to ask where his many insights are actually coming from.”

The independence of systematics is a result not only of separating the explanation with the thing to be explained, but also acknowledging the observer—in this case the taxonomist/systematist—as part of the scientific process, making morphology both a descriptive science and a causal one (Brady, 1984). Consider archetype as an example of a descriptive science as well as a causal one—an archetype is not an ancestor—but neither is it an imaginary form, something that might guide the invention of evidence in its support. Instead, an archetype represents what we know about a morphological form, such as a forelimb and its many manifestations, such as in various people, cats and horses. The forelimb itself is an idea or an abstract

concept, but is easily recognizable and based on evidence found in both living and fossil forms. The archetype is an idea, one that is founded on evidence and experience though observation (see Ebach 2005) and not one from which new forms are created, but has the ability to discover new forms through comparison to existing forms (i.e. comparative biology). The concept of archetypes as ideas based on evidence and experience was not new to Brady’s work but has its origins in Goethean science (see Bortoft, 1996), and was an idea Brady borrowed. However, Brady was the first to attribute these existing ideas to cladistic concepts by engaging with cladists and cladistic literature (see Williams and Ebach, 2007). Unfortunately, these ideas were never allowed room to develop because most historians and philosophers of biology in the 1980s had a particular view of the scientific process (e.g. Beatty, 1982; Hull, 1988; Sober, 1988) and biologists had a particular view of the evolutionary process (e.g. Dawkins, 1986; Ridley, 1986).

Brady’s legacy

Brady had never abandoned his primary research objective that he started as an undergraduate in Berkeley: Goethe’s way of science (Maier et al., 2006; Rieppel, 2014, pp.127–131). He published in nonscientific journals and presented at nonscientific conferences, such as the symposium “Goethe as a Scientist” held at the University of California at Los Angeles and the California Institute of Technology between 12–13 April 1982; the Boston Colloquium for the Philosophy of Science and the Departments of Germanic Languages and History of Science at Harvard University, 3–4 December, 1982. Brady also presented work at the Proceedings of the Second Annual Camelford Conference on the “Implications of the Gaia Thesis” in Cornwall in 1988.

On 27 March 2003, Brady attended his last conference at the Goetheanum in Dornach, Switzerland, where he suddenly died “on the way to the auditorium where he was to give a talk” (Maier et al., 2006, p.9; Holdredge pers. comm. 2017; see also Rieppel, 2014, p.127). Some of Brady’s posthumous work was published in the edited volume *Being on Earth: Practice In Tending the Appearances* (Maier et al., 2006) and in *Truth and Science: Prelude to a Philosophy of Freedom* (Amrine, 2017). These works represent Brady’s Steinerian or Anthroposophist contributions, which are a result of his strong connection with the Nature Institute in New York, USA, and the Goetheanum (see Brady in Maier et al., 2006).

Brady’s contributions to systematics were few in number, but his call for an independent systematics based on Goethe’s aestheticism may provide a far

¹⁵The paper is listed in the contents (Section 1, Perspective 5) of a proposed volume titled “Nature’s Treasurehouses: The Roles of Natural History in Contemporary Society?” to be edited by Norman Macleod. The proposed book was never published; however, Brady’s chapter, retitled as “Perception: Connections between art and science” can be accessed from the Ronald H. Brady Archive at the Nature Institute: <http://natureinstitute.org/txt/rb/art/perception.htm>

greater legacy. In 1982, Norman Macbeth recounted an incident “in an Ivy League college” instigated by “a respectable man” who had torn out Brady’s (1979) paper from the December issue of *Systematic Zoology* (Brady, 1979). When confronted by his colleagues he replied “Well, of course I don’t believe in censorship in any form, but I just couldn’t bear the idea of my students reading that article” (Macbeth, 1982, pp.11–12). That censorship is still with us (e.g. Waters et al., 2013). Yet, 30 years later, Brady’s work is still relevant to a modern audience that is trying to understand systematics and its place in modern biology as an independent scientific discipline. Let us hope that Brady’s work is not forgotten.

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