

## Reply to the “Response to ‘The Debate over Classification of *Archaeopteryx* as a Bird’”

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### Abstract

This paper aims to address some issues raised by McLain et al. (2023) in an article titled “Response to ‘The Debate over Classification of *Archaeopteryx* as a Bird.’” Their reply presents many logical fallacies, misrepresentations, and decontextualization of my arguments. Most of the issues raised by them would not be found if my arguments had been read and understood in the context they were written.

The main point is to demonstrate the undoubted influence of an evolutionary worldview on the supposed feathered dinosaur topic. I also urge for caution when discussing this subject.

**Keywords:** Response, reply, feathered dinosaurs, evolutionary worldview

First, I would like to thank Dr. McLain and colleagues for allowing me to clarify my paper’s main points and for opening more opportunities for discussion.

McLain et al. (2023), in their response to “The Debate over Classification of *Archaeopteryx* as a Bird” (Haynes 2022), stated the presence of some issues with my assumptions, methods, and conclusions. In this reply to McLain et al. (2023), I would like to clarify some of those issues.

However, before that, it is necessary to point out that their response presents misrepresentations, decontextualization, and logical fallacies. A few examples are listed below, followed by a brief explanation.

### Logical Fallacies and Misrepresentations

1. “Haynes disparages those who change the definitions for taxonomic groups...”

Based on the Oxford Languages definition, to disparage means to “regard or represent as being of little worth.” Other synonyms for this word are belittle, denigrate, deprecate, depreciate, downgrade, disdain, ridicule, mock, and defame. Most of those verbs, synonymous with disparage, have a pejorative connotation and tone that does not agree with my paper’s arguments nor my approach to them or about them. Therefore, that constitutes a strawman fallacy. That is, they misrepresented me to defeat my argument.

2. “Haynes sees cladistics as anti-biblical and Linnaean taxonomy as biblical because the former is based in evolution/common ancestry while the latter is based upon concepts of created kinds. However, this is an overly simplistic view.”

I never stated, suggested, or implied that Linnaean taxonomy was biblical. So, when they write that “this is an overly simplistic view,” it represents a strawman fallacy. My goal is not to state, suggest,

or imply which method is more biblical than others. So, that assertion is incorrect. It demonstrates, maybe unintentionally, a way of misrepresenting my proposition, trying to make it easier to defeat it.

3. “In a recent publication, Gabriela Haynes (2022) argued that *Archaeopteryx* should be considered a bird rather than a dinosaur, that birds and dinosaurs are obviously distinct, that statistical baraminology is too influenced by evolutionary thinking to be of use to creationists, and that scientists should only use Linnaean taxonomy in classifying organisms.”

I never said, implied, or suggested that only Linnaean taxonomy should be used. My point was not to indicate which type of classification should be used but to point out that an evolutionary worldview influences cladistics. When McLain et al. (2023) state that, they are misrepresenting my arguments and views. I aimed to demonstrate to the readers that an evolutionary worldview has influenced cladistics.

4. “Haynes argues that the ‘presence of feathers has been the key to classifying an animal as a bird based on the classical, conventional, and traditional taxonomy developed by Linnaeus in the eighteenth century.’”

That is not my argument or conclusion. It is from one citation, that of Gauthier and de Queiroz (2001). I cited it in my paper on page 288.

5. “It appears, then, that Haynes assigns a number of dinosaurs to birds: *Microraptor*, *Sinornithosaurus*, *Anchiornis*, and *Caudipteryx*.”

McLain et al. (2023) are begging the question with this claim. It is not indisputable that those genera are dinosaurs. Other scientists argue they are not dinosaurs but are birds. Also, I cannot assign any genera, because other scientists have already done that. Then, based on my understanding, I agree with some of those scientists’ assignments.

## Decontextualization

1. “Haynes argues that the ‘presence of feathers has been the key to classifying an animal as a bird based on the classical, conventional, and traditional taxonomy developed by Linnaeus in the eighteenth century.’” “Later she states, ‘following the classical, traditional Linnean classification and reasoning, it is concluded that there is no reason for *Archaeopteryx* to be anything other than a bird.’”

I listed five reasons, “in a biblical worldview, as well as on logical grounds, the anatomical features of the skeleton and skull, the presence of feathers (which is a key diagnostic for bird identification), and following the classical, traditional Linnean classification and reasoning, it is concluded that there is no reason for *Archaeopteryx* to be anything other than a bird” (Haynes 2022). The Linnaean classification is the last one on the list. I am not arguing that the Linnaean classification should be the ultimate way to classify *Archaeopteryx* or even the only way for scientists to classify organisms. I never said, suggested, or implied that in my paper or elsewhere. Mentioning only the last part of my conclusion decontextualizes and misrepresents my arguments. Also, for decades, most secular and creation researchers have considered *Archaeopteryx* a bird. So, the idea that it is a bird is not a novelty I invented.

2. “Haynes states that ‘if the feather is defined as filaments, then they appear in basal coelurosaurs such as *Sinosauropteryx*’ (Haynes 2022, 293).”

The whole paragraph explains my point. I do not agree or state that feathers are defined as filaments. The paragraph (below), where this phrase is, argues about how those terms and definitions are arbitrary.

“The main point here is that those definitions are arbitrary and ever-changing. They will depend on which phylogenetic hypotheses were made, what they were based on, and the definition used. Another example of that is related to the word “feather.” If the feather is defined as filaments, then they appear in basal coelurosaurs such as *Sinosauropteryx*. If a feather is defined as pennaceous, *then* it appears in maniraptorans. *The definition of feather changes everything*, whether more like Prum (1999), who proposed and hypothesized that a feather evolved through a series of stages, or more like the classical and traditional way we all understand what a feather is and has been found in the fossil record, complete and functional.” (Emphasis added).

McLain et al. (2023) decontextualized my arguments and misunderstood my points.

3. “It is surprising, then, that Haynes disparages those who change the definitions for taxonomic groups throughout her article while simultaneously acknowledging that ‘in all scientific fields and endeavors, scientists do not understand everything, which is why there is a need to keep researching...scientists also have fallible and finite minds in this fallen world as they try to understand the creatures created by the Creator’s perfect, infinite, and creative mind’” (Haynes 2022, 287–288). “We agree with these points, and it is precisely because our knowledge is incomplete, requires frequent updates, and is prone to errors and corrections, that adjustments and updates to biological definitions are necessary and welcome features of good scientific work.”

This whole paragraph was taken out of context. In my paper, this citation: “in all scientific fields and endeavors, scientists do not understand everything, which is why there is a need to keep researching...scientists also have fallible and finite minds in this fallen world as they try to understand the creatures created by the Creator’s perfect, infinite, and creative mind” (287–288) is talking about *features* that we might not fully understand. It is not talking about *changes in definitions*. The problem is not changing or updating definitions but doing it to fit an evolutionary worldview.

## General Comments

McLain et al. (2023) state that I am unfamiliar with cladistics and statistical baraminology. However, disagreement does not necessarily mean unfamiliarity with the topics. Based on the questionable evidence and the undoubted influence of evolutionary ideas on the subject of supposed feathered dinosaurs, I disagree with the conclusions of McLain et al. (2023) regarding this topic. That, in no way, demonstrates unfamiliarity, only a disagreement. Those are not necessarily related. They also say I confuse descriptive and comparative anatomy with classification methods, but do not present when or where I do that.

The traditional assignment of some dinosaurs “sporting feathers,” as mentioned by McLain et al. (2023), has been long questioned. Not only their classification as dinosaurs has been questioned, but also whether some of them present feathers. Some scientists assign those animals to birds instead and consider what they call feathers to be fibers.

McLain et al. (2023) further state that the “discussions of... feathers are, at times, imprecise and inconsistent with the current state of vertebrate anatomy and taxonomy.” The current state of vertebrate anatomy and taxonomy, concerning definitions and terms, has been influenced by

evolutionary ideas. That is the reality, so we all need to be cautious.

It was probably done unwittingly, but many examples of the "multiple issues" they raised with my "assumptions, methodologies, and conclusions" are the results of them mistaking and misunderstanding my arguments.

### Linnaean Classification?

In my paper (Haynes 2022), I cite Amorim (2002): "The Linnaean classification was based on similarities in creatures, but in contrast, Darwin's classification is based on their supposed ancestry." And I follow that quote by stating, "One can see that the data never changed because the animals and their features have been the same. Instead, the lenses used to interpret them changed." Both the paragraph before and the one where this citation is inserted explain the context. To understand a sentence, it is necessary to analyze the context where it occurs.

McLain et al. (2023) write four paragraphs disputing my arguments on Linnaean classification. However, my arguments would be better understood if they had read them in their context.

Never in my paper was it mentioned, implied, or suggested that:

1. Linnaean classification is the correct one.
2. Linnaean classification should be the only one used for classifying organisms.
3. Linnaean classification is biblical.
4. Linnaeus was correct in everything he did.
5. Linnaeus was or was not influenced.

On the same matter, McLain et al. (2023) state, "However, Linnaeus' entry for Aves in the groundbreaking tenth edition of *Systema Naturae* noted that a defining feature of birds is that they are edentulate (lacking teeth; Linné 1806, IV). Based on this, *Archaeopteryx* and numerous other fossil avians cannot be birds because they possess teeth (for example, *Bohaiornis*, *Ichthyornis*, *Hesperornis*). For Haynes to argue that *Archaeopteryx* is a bird requires that she, too, must modify or reject Linnaeus' definition, a practice that she heavily criticizes."

On page 12 of his original book in Latin, Linnaeus (1758) used these features to define Aves.

#### II. AVES.

*Cor biloculare, biauratum; Sanguine calido, rubro. Pulmones respirantes reciproce.*

*Maxilla incumbentes, nuds, exfertse, edentulae.*

*Penis fubintrans absque tcroto oviparas crulta calcarea.*

*Senfus: Lingua, Nares, Oculi, Aures absque auriculis.*

*Tegmenta: Pennae incumbents\*, imbricats.*

*Fulcra: Pedes duo. Alae duae\*.*

\*(Emphasis mine)

McLain et al. (2023) did not mention the presence of feathers as a feature that Linnaeus used to define Aves. *Archaeopteryx* specimens have feathers. So, using the same line of argumentation as McLain et al. (2023), *Archaeopteryx* should be a bird for presenting feathers.

McLain et al. (2023) also state:

1. "Haynes sees cladistics as anti-biblical and Linnaean taxonomy as biblical, because the former is based in evolution/common ancestry while the latter is based upon concepts of created kinds."
2. "It is therefore quite clear that employing Linnaean systematics does not guarantee that one's methods or results are biblical."
3. "In a recent publication, Gabriela Haynes (2022) argued that *Archaeopteryx* should be considered a bird rather than a dinosaur, that birds and dinosaurs are obviously distinct, that statistical baraminology is too influenced by evolutionary thinking to be of use to creationists, and that scientists should only use Linnaean taxonomy in classifying organisms."

Amorim's (2002) citation and the context of the paragraph where it is inserted and the paragraph before it demonstrate one of my main points: the definitions have been changed to fit an evolutionary worldview. I argue that Linnaeus's classification is based on characteristics and similarities, not ancestry, with the latter being the basis of cladistics.

McLain et al. (2023) raise some issues, regarding my use of the Linnaean classification, that mainly rely on misinterpretation, decontextualization, and incorrect reading of my arguments. The citation, its context, and the paragraph where it is cited explain precisely what I mean. McLain et al.'s (2023) four paragraphs discussing my arguments are, therefore, irrelevant, unnecessary, misread, misinterpreted, and misrepresent my arguments, taking them out of context. The issues they raised would be easily solved if my sentences were interpreted along with their contexts.

They say, "We submit that it is entirely reasonable and necessary to change and update the definitions and contents of taxonomic groups as we learn more about nature." I never suggested, implied, or said we could not update or change definitions. I suggested we all be cautious to watch for definitions and terms being changed based on facts, not because of evolutionary ideas.

### A Matter of Definition and Cladistics

McLain et al. (2023) use the word theropod, not making it clear which definition they were applying. They did not explain if they were using that term based on the 1881 definition (which groups all known dinosaurs from the Triassic and the carnivorous

dinosaurs from the Jurassic and Cretaceous) or the 1986 definition that groups birds and all saurischians via cladistics. This fact makes logical analysis of their arguments difficult and confusing to address. However, based on their writing, I assume they are using the 1986 definition of the word theropod. So, it is necessary to point out that is one of the main points of my paper. Terms and definitions must be clear, especially the term theropod, which has been influenced by evolutionary ideas and has had its definition changed.

Hendrickx, Hartman, and Mateus (2015) demonstrate what I argue above in this paragraph.

The adoption in the early 1980s of phylogenetic methodology developed by German entomologist Willi Hennig (1950) in the beginning of the second half of the 20th century, was a major step in the history of theropod systematics, and the results of those cladistic analyses radically changed prevailing views on theropod phylogeny. Thulborn (1984) was the first to investigate theropod interrelationships through a cladistic approach by addressing the systematics of *Archaeopteryx* and other stem-group birds.

Gauthier's (1986) work on saurischian interrelationships was the first to outline the current phylogenetic classification of non-avian theropods.

It is followed by:

Gauthier (1986) recovered Theropoda as a well-supported clade divided into Ceratosauria and Tetanurae, and provided the modern phylogenetic definition of theropods as birds and all saurischians closer to birds than to sauropodomorphs.

Since the pioneering work of Gauthier (1986), the availability of parsimony-based phylogenetic software has enabled a large number of authors to investigate theropod interrelationships via cladistic analysis, resulting in major revisions to theropod systematics.

As noted, the definition of the word theropod has changed, and the basis for that change is phylogeny, which relies on the evolutionary "history" of organisms. As creationists, we all need/should be cautious when new definitions embrace the scientific terms we usually use. We have seen this happening in many fields in our society. A secular worldview has changed many definitions. Then, a secular view is slowly imprinted.

McLain et al. (2023) using this definition is problematic, because "Definitions are ontological statements about the existence of entities that result from the relationships of common ancestry among their parts; descriptions and diagnoses are epistemological statements about how we recognize the parts of those entities" (de Queiroz and Gauthier 1990). That is the worldview behind definitions, descriptions and diagnoses.

So, based on what has been demonstrated above, I agree that "taxonomic definitions are precise and accurate given the state of knowledge at the time." But I also understand that the "state of knowledge at the time" has been influenced by an evolutionary worldview. That can be demonstrated by a statement from Padian and May (1993), citing other authors, wrote, "Systematics is rapidly moving from the Linnean 'phone book' approach to the phylogenetic system (de Queiroz and Gauthier, 1992), which invests taxonomy with firm adherence to evolutionary relationships." This phylogenetic system is carried out with a load of bias that we need to account for, being able to separate what is fact and what are evolutionary-biased terms and definitions.

### Cladistics

One of the issues McLain et al. (2023) raised is my arguments about baraminological analyses. So, I understand all of us should ponder/consider some points.

1. Characters used in baraminological analyses are selected by secular scientists with an evolutionary bias. They choose the characters, their weight, state, coding, definition, meaning, and importance for the analysis proposed and its purpose. Sanders and Cserhati (2022) state: "However, with minimal exception, creation scientists never select the characters. The characters come preselected from the evolutionary community in their datasets, mostly accumulated for phylogenetic purposes. Therefore, it is crucial to know how they select their characters and what a character means to them." They go on to cite Wiley (1981): "Characters are chosen to fit the purpose of the study at hand" which leads to "different character types often produce discordant phylogenies (Ridley, 1986)." Many examples of what Sanders and Cserhati (2022) mention can be seen in many scientific papers.
2. What a character is and its meaning in an evolutionary worldview makes the data evolutionary-biased. Sanders and Cserhati (2022) write: "Characters are defined as either evolutionarily stable or volatile, depending on whether they change with time or not and as either ancestral or derived depending on which is assumed to have appeared first." This assumption is biased. This assumption comes from an evolutionary worldview.
3. Sanders and Cserhati (2022) state: "As an example of some of the problems with character sampling, an analysis of character selection across 512 phylogenetic studies discovered that systematists were usually very vague when it came to why they selected traits." Sanders and Cserhati (2022)

mention about that study "taxonomists were selecting characters differently and assuming that everyone else was making selections the same way they were (Poe and Wiens 2000)."

4. Sanders and Cserhati (2022) also point out another problem: "One issue is how to assign discrete values to continuous character values, such as length. A larger issue is that numeric characters do not distinguish between traits which are diagnostic and those that are not. For example, the presence of mammary glands is diagnostic of a mammal, yet has the same weight as a non-diagnostic trait, such as teeth attached to the palate, which occurs in both reptiles and fish. This means traits which are truly unique to an organism or group of organisms, and thus could indicate discontinuity, can easily be lost in an unweighted system." Even when using diagnostic features, those are also embedded with evolutionary bias.

Langer et al. (2020) state: "Owen's conception was developed in the pre-Darwinian era and was burdened further by there being so few extinct dinosaurs known in the mid-nineteenth century. Nevertheless, pelvic and hind-limb modifications thought to reflect acquisition of an upright striding bipedal stance and gait (e.g., Gauthier et al. 2011) figured prominently in the diagnosis of Dinosauria from Owen's time to the dawn of the Hennigian (phylogenetic) era (e.g., Bakker and Galton 1974; Gauthier 1986)."

McLain et al. (2023) also say, "Haynes is correct to point out that cladistics was developed as a purely evolutionary taxonomic system that assumes universal common descent. However, it is an overreach to claim that, due to its origins in evolutionary studies, cladistics is always and invariably unbiblical."

After what has been demonstrated above, I understand it is not "an overreach." It is just the reality of things. An evolutionary influence is in the cladistics' origin and in a lot related to the system, the terms, definitions, diagnosis, descriptions, data, interpretation, etc. The idea that similarity equals ancestry permeates all the conclusions drawn by the studies that build the data used on, through, and for cladistics. In the secular interpretation, it is essential to note that this common ancestry is the universal common ancestry. And all that is imported into baraminological studies through the data.

McLain et al. (2023) also mention a paper by Jeanson (2022). However, Jeanson uses phylogenetic trees within humans, that is, the same species, also living creatures, not fossils. So, their example is irrelevant to the case. Jeanson is also not using two different groups (birds and dinosaurs), as McLain and his colleagues use when performing baraminological studies.

McLain et al. (2023) say that I bring "strong accusations, yet, Haynes does not provide a single example of erroneous observations, coding errors, misrepresentations, or evolution-influenced biases in any published morphological dataset." I wrote at least five paragraphs with citations (Haynes 2022 289–292) and two diagrams demonstrating the wrong conclusions from the published morphological dataset. I also provide in this paper an appendix with copious citations from two secular papers that show the authors admitting "erroneous observations, coding errors, misrepresentations," and demonstrating "evolution-influenced biases." McLain et al.'s (2023) belief in the accuracy of the datasets, namely, "the morphological datasets which tabulate the physical character states of these taxa are both accurate and robust," seems to differ from the assessment of those secular authors.

McLain et al. (2023) also raised some issues about my arguments regarding cladistics. They say: "After all, cladistics is simply a method of hierarchically arranging organisms (individuals, species, genera, and so on) by plotting them on trees based on the proportions of how *similar* their characteristics are" (emphasis mine). Concerning their statement, I find the wording they use for their explanation of what cladistics is, appealing. Here, I would like to break it apart and make some observations. I also provide some secular citations that seem to invalidate the statement made by McLain et al. (2023).

1. "After all"—This brings the idea that cladistics is essentially (beyond everything or most of all) a system of just organizing organisms. That is not precisely the proper definition and use of cladistics, nor the system's intention and purpose.
  - a. "In fact, a model of phylogenesis connecting species materially opposes not only to a creationist view of the world, in the sense of the literal interpretation of Genesis..." (Amorim 2002)
  - b. "By proposing the existence of a phylogeny that connects species, we move from models in which species are watertight entities (immutable and disconnected from each other) to a model in which not only species but also their characteristics are historically connected." (Amorim 2002)
  - c. "A phylogenetic relationship of varying degree exists between all living species, irrespective of whether we know of it or not. The aim of research on phylogenetic systematics is to discover the appropriate degrees of phylogenetic relationship within a given group of organisms." (Hennig 1965)
  - d. "Since the advent of the theory of evolution, one of the tasks of biology has been to investigate

- the phylogenetic relationship between species. This task is especially important because all of the differences which exist between species, whether in morphology, physiology, or ecology, in ways of behavior, or even in geographical distribution, have evolved, like the species themselves, in the course of phylogenesis. The present-day multiplicity of species and the structure of the differences between them, first becomes intelligible when it is recognized that the differences have evolved in the course of phylogenesis; in other words, when the phylogenetic relationship of the species is understood.” (Hennig 1965)
- e. “‘Phylogenetic relationship’ is thus a relative concept. It is pointless (since it is self-evident) to say, as is often said, that a species or species group is ‘phylogenetically related’ to another. The question is rather one of knowing whether a species or species-group is more or less closely related to another than to a third. The measurement of the degree of phylogenetic relationship is, as the definition of the concept shows, ‘recency of common ancestry’ [Bigelow (1 [1965])].” (Hennig 1965)
  - f. “The idea of evolution, as we understand it today, has certain important consequences. One is that any two species must have at least one common ancestor species. Of any three extant species, two have a common ancestor that is not common to the third—unless all three originated simultaneously. If we apply this reasoning to all species, we get a picture of a huge sequence of divisions that fragmented from the first ancestral species—the ancestor of all living beings to the species that exist today (assuming that life on Earth only appeared once). To the set of this history of ancestry among all species, we call, generically, PHYLOGENY. Secondly, we call the diagram that represents this history a phylogeny. Perhaps it is worth emphasizing that there is one and only one history of relationships between species.” (Amorim 2002)
2. “Cladistics is simply a method”
    - a. “The method of phylogenetic systematics, as that part of biological science whose aim is to investigate the degree of phylogenetic relationship between species and to express this in the system which it has designed, thus has the following basis: that morphological resemblance between species cannot be considered simply as a criterion of phylogenetic relationship, but that this concept should be divided into the concepts of symplesiomorphy, convergence, and synapomorphy, and that only the last-named category of resemblance can be used to establish states of relationship.” (Hennig 1965)
    - b. “Phylogenetic systematics is not theory-neutral because its deep structure embodies evolutionary assumptions.” They go on to state, “Phylogenetic systematics was based on the assumption that evolution had occurred, and was responsible for systematic relationships.” (Brooks and Wiley 1985)
    - c. “I use the term ‘cladistics’ throughout. As Mayr (1985) correctly points out, ‘phylogenetic’ refers to the totality of the evolutionary history of a group,...”. And “Cladistics has also emerged as the best method to provide basic systematic and phylogenetic data and hypotheses for a host of further studies, be they in structural adaptation, in structure evolution, in speciation or in biogeography.” (Linder 1988)
  3. “Hierarchically arranging”
    - a. “Such arrangement of monophyletic groups of animals according to their degree of phylogenetic relationship is called, in the narrower sense, a phylogenetic system of the group in question. Such a system belongs to the type called a ‘hierarchical’ system. Since ‘system’ in the wider sense means every arrangement of elements according to a given principle, the phylogeny tree, too, can be termed a phylogenetic system. Phylogeny diagrams and arrangement of the names of monophyletic groups in a hierarchical sequence are merely different but closely comparable forms of presentation whose content is the same. Therefore, everything which can be said about the methods of phylogenetic systematics (see below) applies irrespective of whether the results sought by the use of these methods are expressed only as a phylogeny tree or, as a phylogenetic system in the narrower sense, in a hierarchically arranged list of the names of monophyletic groups.” “...in a hierarchical system, each group formation relates to a ‘beginner,’ which is linked in ‘one-many relations’ with all of the members of that group and only those [Gregg (3) (1954)].” “But, in a phylogenetic system, the ‘beginner’ to which each group formation relates is a real reproductive community which has at some time in the past really existed as the ancestral species of the group in question, independently of the mind which conceives it, and which is linked by genealogical connections with the other members of the group and only with these.” “It was therefore inevitable that the classification of organisms according to their morphological resemblance, in association with certain features of their ontogenetic development and their geographical distribution, would sooner

- or later lead to the discovery of their successive degrees of phylogenetic relationship and thus to the theory of evolution." (Hennig 1965).
- b. "When coining the word clade from 'cladogenesis...taken over directly from Rensch,' Huxley intended to denote 'delimitable monophyletic units' (96, p.454) or, more precisely, 'monophyletic units whatever magnitude' (97, p.27)." "The noun cladist was introduced in 1965 'in line with the terminology developed by Rensch (1960), Huxley (1958) and Cain & Harrison (1960)' (116, p.167). Although none of these authors referred to Hennig, in coining the word cladism Mayr typifies the cladist as one who adheres to that 'phylogenetic school' of which Hennig is the 'most articulate spokesman' (118, p.167)." "Darwin, who had 'two distinct objects in view' (38, p.61), carried out two independent revolutions. His biological revolution proposed a mechanism (natural selection) to account for the amount of difference between organisms. His taxonomic revolution, founded on the inference of descent with modification, advocated genealogical taxonomic arrangements freed from categorical and typological thinking. Darwin repeatedly distinguished ranking, which implies amount of difference, from arrangement, which implies descent. He recalled (37a, pp.457–58) that even in the absence of a known mechanism, consideration of taxonomic arrangement alone would have directed him to the concept of descent." "Darwin and Hennig drew the same major logical deductions from the fundamental statement: (a) in order to be stable, the arrangement of taxa must reflect their history, i.e. their unique genealogy and (b) the hierarchically nested taxa, if genealogical, are monophyletic 'individuals.'" "In my opinion, 'the word synapomorphy expresses the full spirit of a true genealogical and Hennigian procedure' (44). Logically, synapomorphic clustering is equivalent to the single-link method advocated in phenetics (49, 103). Biologically, it corresponds to the proven fact descent with modification. Genealogically, it affords the only conceivable way of building strictly monophyletic arrangements—i.e., in which the taxa are homogeneous and complete. Consequently, the monophyletic taxa resulting from the chained clustering of synapomorphies are redefined not by attributes (similarity) but by members (genealogy); this is a requisite of the Hennigian fundamental statement and of an R-method (see below)" (Dupuis 1984).
- c. "Therefore the inclusion of even only certain genetic criteria in biological systematics necessarily resulted in the realization that there are in fact genetic relationships between all organisms. The theory of descent, that is the perception that the existing diversity of life on the earth arose historically from an earlier simpler condition, and that the semaphoronts—the elements of all systematic efforts in biology—must be regarded among other things as members of a community of descent, is thus derivable from biological systematics (not 'morphological systematics'). This is, in fact, its most important result to date. The historical relations between the theory of descent and biological systematics are of two kinds. On the one hand, different semaphoronts can be connected with one another within the life cycle of an individual by directly observable genetic relations. On the other hand, different semaphoronts can enter into relationships with one another for the purpose of producing common, but under certain circumstances clearly different, offspring. Since these and other important facts group themselves around the concept of the biological 'species,' it is accurate up to a point to say that the theory of descent is a result of a more profound study of the species category (Hertwig 1914). The statement by Dingler (1929) that 'the remarkable phenomenon that the genetic idea was conceived so late in human thought, especially that it does not appear in Greek natural philosophy, is related to the absence of a clearly formulated species concept' is probably correct in principle. This study could become fruitful in the sense of the theory of descent, however, only if another historically important fact is taken into account. This is the fact that the closer and broader relationships of morphological similarity among all the species can often be best expressed if the species are gathered into a hierarchic system of groups. It is doubtless evident from the observation that an 'organism' can change in a relatively short time from one form in which it resembles certain organisms into another form, dissimilar to its earlier condition, that makes it more similar to other organisms, and from the further observation that the offspring of two different individuals may be dissimilar to their parents, to conclude that even greater changes in form may possibly take place in the course of periods of time so vast that they cannot be surveyed. Only a small, though decisive, further step is needed to conclude that the graduated differences in form that are expressed in the

hierarchic system of species originated by similar processes of change lying farther back in time or requiring longer periods of time. Thus one has the most essential declaration of the theory of descent. In comprehensive accounts of the theory of descent a great number of 'proofs' of its correctness are generally given (paleontological, embryological, zoogeographic, and others). All these proofs are undoubtedly significant, but it must be pointed out that they gain this significance only through their relation to the hierarchic system and the similarity relationships (which need not be morphological in the narrow sense) of the semaphoronts that are expressed in this system. The fact that semaphoronts (-complexes) which, because of their similarity: relationships are included in the same group, may prove to be connected by other entirely different relationships (zoogeographic, for example) that were not taken into account in the original compilation calls for an explanation. The explanation is then provided by assuming common descent, by inferring that community of similarity=community of descent)." (Hennig 1966)

d. "That evolution produces a hierarchy has always been understood (cf. Lamarck, 1914; Darwin, 1859; Hennig, 1966)..." (Wiley and Brooks 1982)

#### 4. "Similar"

a. "Our knowledge about the order underlying biological diversity is clarified when we are able to discern, among the similarities of a set of taxa, which are symplesiomorphic, which are homoplastic and which are synapomorphic. Quadrillions and quadrillions of false assumptions about kinship relationships between species or groups of species disappear every time we formulate a hypothesis of monophyly, allowing an increase in our knowledge about order in biological diversity. In turn, this knowledge helps the understanding of phylogeny at smaller levels and the understanding of problems of primary homology and evolution of the group's characters themselves." (Amorim 2002)

b. "'Convergence' and 'polyphyletic groups' are concepts which presuppose acceptance of the theory of evolution." (Hennig 1965)

c. "When evolutionists look at traits that are similar, they assume that the traits are descended from a common ancestor unless there is reason not to (Brooks and McLennan, 1991). This assumption is not always true. Homologous characters can appear in unrelated organisms, which the evolutionists recognize,

and account for by appealing to convergent evolution. However, the first assumption when presented with a similar set of characters is that they are the result of ancestry, not convergence." (Sanders and Cserhati 2022)

#### 5. "Characteristics"

a. "It is a consequence of the theory of evolution that the differences between various organisms must have arisen through changes of characters in the course of a historical process." (Hennig 1965)

b. "The supposition that two or more species are more closely related to one another than to any other species, and that, together they form a monophyletic group, can only be confirmed by demonstrating their common possession of derivative character ('synapomorphy'). When such characters have been demonstrated, then the supposition has been confirmed that they have been inherited from an ancestral species common only to the species showing these characters." (Hennig 1965)

c. "Subject to these conditions, the value of fossil finds lies in enabling one to interpret character agreements in recent species when this cannot be done solely from a knowledge of these recent forms. There are, in the recent fauna, monophyletic groups which agree in certainly derivative (apomorph) characters with other diverse groups which are just as surely monophyletic. Some of these agreements must therefore rest on convergence. But it is often impossible to decide with certainty which of these agreements are based on convergence and which are to be considered as a true synapomorphy. The possibility of decision in such cases depends on a knowledge of the sequence in which the characters in question evolved." (Hennig 1965)

Character or characteristics are seen through an evolutionary sequence. They are not only a straightforward description of an anatomical feature, as McLain et al. (2023) seem to believe. A diagnostic characteristic of an organism in the phylogenetics/cladistics era passes through agreements if that feature is based on convergence or synapomorphy. That alone is biased in many ways. One of them is demonstrated on 4b.

d. "Our decision to present the results of the analysis of unordered character matrices, which are always more parsimonious for these data, is based on our concern that ordering transformation series within a character limits the number of alternative character state transformations and presupposes a specified



- history of evolution for a particular character. It could be argued that because a transformation series results from genetic modification in ancestral-descendant populations, the ends of a transformation series must pass through intermediates, so that an ordering of character state transformations emerges inevitably from the evolutionary process." (Sevim-Erol et al. 2023)
- e. "We need to remember that all character states are defined by the researcher and have no fundamental genetic basis." (Sevim-Erol et al. 2023)
  - f. "The only figure in the book *The Origin of Species*, by Charles Darwin (1859), is a phylogenetic diagram (Fig. 6.1). It is interesting to note, however, that, despite being revolutionary, the implications of the concept of a phylogeny for understanding biological diversity took more than a hundred years to be understood more broadly. In the phylogenetic evolutionary model, the characteristics of each species are not seen as belonging to it—unlike the idealistic Platonic model. The characteristics that a species presents are simply the result of the inheritance, with or without modifications, of homologous characteristics that existed in its ancestral species and the ancestors of its ancestors, until the beginning of life. Everything is connected." (Amorim 2002)
  - g. "In the phylogenetic model (B), current species are descendants of ancestral species and the characteristics of current species are copies, modified or not, of characteristics that existed in these ancestral species." (Amorim 2002)
  - h. "the most central concept behind the idea of phylogeny is that of ancestry between species. That is, there is only a phylogeny because there are ancestral species connecting other species. In this sense, talking about the ancestry of species and talking about phylogeny is the same thing." (Amorim 2002)
  - i. Sanders and Cserhati (2022) discuss character bias and cite Winsor (1994) "Everyone carries a bias and applies that bias to character selection." They also mention Sanderson (1995), "Often certain characters are deliberately excluded from the dataset for a variety of reasons, which has the potential to bias the data."—Even if cladistics is simply a method of arranging similarities and characteristics (characters), which I profoundly disagree with, that very nature of a character would be problematic enough.
  - j. "Characters change during evolution." (Linder 1988)
6. "by plotting them on trees based on the proportions of how similar their characteristics are."
    - a. "Cladograms therefore contain a large amount of implicit information. In the cladogram of Figure 6.14, for example, there are proposals for monophyletic groups at various levels, supported by assumptions about synapomorphies. As one can see, implicit information on monophylies and synapomorphies are present in the cladogram. The same is true for plesiomorphic information. They do that because they assume that 'the fact that there is a single phylogeny causes all characters to emerge in the context of this genealogical history.'" (Amorim 2002)
    - b. "Cladograms contain statements-hypotheses (see Chapter 12)—about the phylogenetic history of a given group. And these hypotheses explain the evolution of the group's diversity." (Amorim 2002)

McLain et al.'s (2023) understanding of cladistics seems inaccurate based on what is demonstrated above. Cladistics is a way of showing phylogenetic relationships "evidenced" by synapomorphies through cladograms. If McLain et al. (2023) see cladistics as they stated, that alone is problematic enough in every single way.

When cladistics is applied, the whole vocabulary carries an evolutionary bias. Phylogeny, synapomorphy, sister-taxon, out-groups, derived, basal, etc., are examples. Also, that bias is present in the definitions resulting from that system. So, how can someone use that vocabulary and definitions and escape from their meaning, purpose, and intention? Using them can be seen as appropriating a system loaded with evolutionary bias and applying it in the creation science field. Doing that in a creationist argumentation does not seem consistent or even correct.

Cladistics analyses aim to assess relationships among clades. That explicit information can be taken from any cladistic study and article. That alone is problematic since their goal is to demonstrate evolutionary relationships. Langer and Benton (2006) provided one example of that: "A new cladistic analysis of the early dinosaur radiation was performed to assess the relationships among the three major clades (Ornithischia, Sauropodomorpha and Theropoda) and to define the phylogenetic position of the basal members of the group."

McLain et al. (2023) state: "We also recognize that these datasets are reliable, which is why they are used in statistical baraminology to evaluate fossil groups." However, including one piece of information plus using a different method can completely change the results, conclusions, and interpretations. One example of that is Lee and Worthy (2011), who write: "A recent

study [3] presented the most compelling evidence to date challenging this long-held assumption: addition of an intriguing new dinosaur fossil (*Xiaotingia*) to one of the most comprehensive phylogenetic analyses of theropods removes *Archaeopteryx* from birds and places it with deinonychosaurs. Thus, *Archaeopteryx* becomes just one of a plethora of advanced bird-like dinosaurs [2–6], no more closely related to living birds than (for instance) *Velociraptor*. This phylogeny has important implications for the evolution of many features in early birds, such as the morphology of the skull and the flight apparatus [3,6]. However, it was acknowledged that the new phylogeny required further investigation, owing to weak support (Bremer support of 2 and bootstrap less than 50%; [3]). Also, as with most morphological studies, only parsimony (cladistic) methods were employed.”

McLain et al. (2023) go against my arguments of an evolutionary bias present in the data. Then, they imply that I argue for intentional bias. First, I never implied, suggested, or said that the bias was intentional but an influence of an evolutionary worldview. That does not necessarily require intentional action. They also say that “minor disagreements among specialists” affect “a small portion of the dataset.” My point is, how small is small enough? How can they measure that? Again, those disagreements, sometimes, are not as little as they imply/say. Some of the results are not only different but opposite. Also, are the authors stating that datasets cannot (possibly) be misinterpreted, misidentified, or insufficient? The authors seem to be very confident about datasets, a confidence that is not shared, for example, by Nesbitt (2011), who states: “Unfortunately, recent authors did not provide detailed character descriptions or rationale for scoring strategies as did Sereno (1991), Juul (1994), and Bennett (1996). This led to heavily recycled characters, sometimes compounding scoring errors from the original matrices.”

It is also essential to understand that bias is one potential source of error, not the only one. I never implied, said, or suggested it was the only one. Choosing which organism to include, which features to compare, which parts of those features to compare, and deciding how to analyze the data and how much of the data will be analyzed are other sources of potential error (see Engler 2020).

McLain et al. (2023) say that there are “robust baraminological studies which frequently include substantial efforts to evaluate the morphological datasets that inform them...” If they are reliable, why are there “substantial efforts to evaluate the morphological datasets that inform them (for example, Clausen and McLain 2021; Wood 2016)”? Also, one of those papers was written by McLain and Clausen in 2021. That paper is only an abstract. I

do not think an abstract should be called robust and substantial, because it is only a summary and thus does not provide all the necessary details.

McLain et al. (2023) also say “We must resist calls to disregard valid, empirical data merely because they were collected and published by evolutionists. These sorts of statements move beyond reasonable caution into unrestrained and debilitating skepticism.” Again, this is another example of something I never said, implied, or suggested. My call is for caution, considering the possibility (which I demonstrated in my paper) of biases and evolutionary assumptions influencing those data. Also, my call is for a critical evaluation of the data to ensure its validity. Neither I nor Answers in Genesis have ever called for such a thing. My call is to consider the evolutionary bias in the data set and its influence on the interpretations and conclusions drawn from it. That is, at least, to be discerning about that. No harm can be brought from doing that.

McLain et al. (2023) stated: “Taxonomic identification and character determination are objective and empirical data with minimal bias.” The authors seem to be sure about this claim. However, my question is, how minimal is minimal enough? How do they know it is minimal? How do we measure that? How do you distinguish between what is minimal and what is major? Who does this measurement? Based on what are those measurements are done? Which references are used for those measurements? As the authors admit, there is bias. So, how do we know the impact of this bias on the data? What was impacted, and how much was affected? They follow with “taxonomic definitions are precise and accurate given the state of knowledge at the time.” It does not seem precise and accurate if it has a bias (as the authors admitted), and we cannot measure how “minimal” that bias is.

They also say, “These charges are impossible to square with the 25-year history of statistical baraminology.” However, the length of time of a discipline has no bearing on whether that discipline has been contributing to quality science. That even seems like an Appeal to Authority Fallacy. They follow it with, “First, baraminological studies differ sharply from cladistics, in that they 1) do not assume universal common ancestry, 2) utilize different computational methods, 3) identify both similarities and differences among taxa, and 4) are not tethered to tree-based representations of relationships.” Yet, the data, which are imported from the literature, are influenced by all of those points and assume the universal common ancestry. And even if they differ in the following ways cited by McLain et al. (2023), that does not mean (logically) they are immune from evolutionary assumptions. Because phylogenetic

systematics is not theory-neutral, it is embedded with evolutionary assumptions. That happens because "Phylogenetic analysis is based on the basic principle of evolution, i.e., descent with modification" (Grandcolas et al. 2001).

Platnick (1985) states, "But I know of no cladist so naive as to believe that there are any scientific statements whatsoever that are theory-free." Sæther (1986) also brings up an interesting point, "It is necessary to have knowledge of the nature of the different possible natural processes and try to explain these before undertaking estimations of patterns of kinship. The persistent intuitive and subjective element in phylogenetics was stressed by Hennig. Objectivity is a myth."

### About Dinosauria

McLain et al. (2023) bring a definition or perhaps an explanation for Dinosauria, and I would like to make some observations about that.

The authors write:

"Dinosauria is an archosaurian group characterized by a variety of skeletal traits possessed by its members. These include, but are not limited to (Nesbitt 2011):

1. Epiphyses present on the postaxial anterior cervical vertebrae;
2. A deltopectoral crest located 30% or further down the length of the humerus;
3. The radius is shorter than 80% of the humerus length;
4. A perforated acetabulum;
5. The fourth trochanter is a sharp flange.

Nesbitt (2011) identifies a total of 12 dinosaur-unique traits and thirteen additional traits that may also qualify (p.210). Beyond these, there are other traits that are characteristic of dinosaurs but are also shared with similar non-dinosaurian groups (for example, silesaurids), or are nearly, but not completely, found among all dinosaurian taxa. For example, dinosaurs typically have three or more fused sacral vertebrae, stand with their limbs directly under their bodies, and possess an astragalus that extends over the distal anterior surface of the tibia. All of the above anatomical features, plus additional ones, are the means by which paleontologists can identify whether an organism is or is not a dinosaur. The features observed that further define specific dinosaur groups, genera, and species are similarly (and increasingly) precise. In some cases, a taxon may lack some of these features but is still considered a dinosaur on the basis of possessing many of the other traits. This is not unusual in biology. For instance, all biologists consider snakes to be in Tetrapoda even though they have no legs. Rather, the myriad of reptilian traits that snakes possess supersedes

their limbless condition. In contrast to the rigorous anatomical details noted above, Haynes' approach to definitional issues is muddled."

This issue raised by the authors is confusing to me for the reasons listed below.

1. When the authors describe Dinosauria as an archosaurian group, I understand that the definition of the term archosaurian they are using needs to be explained because the definition of Archosauria was changed by Gauthier in 1986. That change has been applied since then.

Gauthier (1986) writes, "Archosauria (n. comb.) is redefined to encompass all the descendants of the most recent common ancestor of crocodiles and birds." Gauthier (1986) explains the reason for this new definition. "Defining Archosauria in this way provides a more fruitful perspective from which to examine archosaur phylogeny." Gauthier (1986) creates a new combination which stands for (n. comb.). In this new combination, he gathers birds and reptiles, and his reason is based on an evolutionary perspective of phylogeny.

Nesbitt (2011) follows Gauthier and Padian's (1985) and Gauthier's (1986) definition. Nesbitt writes that in the first three lines of the introduction of the paper McLain et al. (2023) cite. So, suppose the authors are using the understanding of Archosauria followed by Nesbitt (2011), who also follows Gauthier and Padian (1985) and Gauthier (1986). Is it thus legitimate to use an evolutionary-based understanding of Archosauria in a creationist discussion? The other point is that if they are using an understanding of Archosauria different from Nesbitt (2011), then using the features for Dinosauria from Nesbitt (2011) is logically inconsistent because Archosauria and Dinosauria are seen as phylogenetically related by that author. In that case, McLain et al. (2023) would be using the same term and the Archosauria grouping idea offered by Nesbitt (2011) but applying different meanings to them. That would fall into a fallacy type called equivocation.

Brusatte et al. (2010a) about Archosauria state, "Crown group Archosauria, which includes birds, dinosaurs, crocodylomorphs, and several extinct Mesozoic groups, is a primary division of the vertebrate tree of life." The evolutionary bias is present, and it is the background from where Brusatte et al. (2010a) perform their analysis.

The third possibility is that McLain et al. (2023) are using this term and grouping, unaware of the changes influenced by evolutionary bias. It was because of cladistics that the Archosaur understanding changed. Nesbitt (2011) writes, "In the 1980s, cladistic methods reshaped our understanding of basal archosauriform relationships. The works of Gauthier (1984), Benton (1985), Benton and Clark (1988), and Gauthier,

Kluge, and Rowe (1988) showed the following: (1) crocodylians and avians are each others' closest extant relatives, and they shared a common ancestor at some point in the Triassic...".

Thus, any of the three possibilities for using Nesbitt's (2011) features for Dinosauria is problematic.

2. The term Dinosauria also changed. Those changes were influenced by evolutionary bias. About Dinosauria, Nesbitt (2011) writes:

"Original definition: All descendants of the most recent common ancestor of birds and *Triceratops* (Padian and May, 1993). Revised definition: Node: The least inclusive clade containing *Triceratops horridus* (Marsh, 1889) and *Passer domesticus* Linnaeus, 1758 (sensu Sereno, 2005). TEMPORAL RANGE: Late Triassic (?Carnian-early Norian *Pisanosaurus mertii* Casamiquela, 1967) to Recent (*Passer domesticus* Linnaeus, 1758)."

If McLain et al. (2023) disagree with this definition of Dinosauria expressed by Nesbitt (2011), why are they using the features Nesbitt (2011) describes to diagnose this group? That is logically inconsistent. If the authors agree, why are they using the evolutionary-based approach of Nesbitt (2011) in a creationist discussion? Furthermore, if they are unaware of Nesbitt's definition, they should not use, or at least be careful using, Nesbitt's (2011) diagnostic features. In any way, using Nesbitt's (2011) features for Dinosauria is problematic.

3. McLain et al. (2023) cite Nesbitt (2011) about the skeletal traits that characterize Dinosauria. Nesbitt (2011) explains the group Dinosauria and writes:

The monophyly [sic] of Dinosauria is well supported in accordance with nearly all previous numerical phylogenetic analyses (Gauthier, 1986; Benton and Clark, 1988; Juul, 1994; Benton, 1999; 2004; Langer and Benton, 2006; Irmis et al., 2007a). I tested the monophyly of Dinosauria in the broadest context yet constructed and including most "thecodonts" as proposed by various authors arguing for a diphyletic or polyphyletic origination of the three major lineages of Dinosauria (e.g., theropods evolved from pseudosuchians, whereas sauropodomorphs evolved from proterosuchians [Thulborn, 1975]).

Nesbitt (2011) goes on and says: "Dinosauria is diagnosed by 12 unambiguous synapomorphies, many of which were found previously (see below)." As we can see in this statement from Nesbitt (2011), what characterizes Dinosauria is based on phylogenetic analyses and the idea that evolution happened. I wonder if McLain et al. (2023) know the origin of the features they mentioned and listed, following Nesbitt (2011). If they do not know, they should be careful using them. If they are aware, why

are they using features based on evolutionary ideas in a creationist discussion? If they aim to use those features with a different approach (from a creationist perspective), that is impossible because it is logically inconsistent.

Langer et al. (2020) write: "Owen's conception was developed in the pre-Darwinian era and was burdened further by there being so few extinct dinosaurs known in the mid-nineteenth century. Nevertheless, pelvic and hind-limb modifications thought to reflect acquisition of an upright striding bipedal stance and gait (e.g., Gauthier et al., 2011) figured prominently in the diagnosis of Dinosauria from Owen's time to the dawn of the Hennigian (phylogenetic) era (e.g., Bakker and Galton, 1974; Gauthier, 1986). This collection of apomorphies remains diagnostic of Dinosauria relative to the last ancestor it shared with *Alligator mississippiensis* (Archosauria, this volume). Nevertheless, these distinctly bird-like 'dinosaurian' synapomorphies likely originated before the divergence amongst Ornithischia, Theropoda, and Sauropodomorpha (=Dinosauria as defined here). A growing understanding of species diversity and anatomical disparity of Triassic dinosaurs and their close kin reveals that many of these traditional 'dinosaurian' apomorphies did indeed evolve earlier on the avian stem (e.g., Novas, 1996; Sereno, 1999; Langer and Benton, 2006; Ezcurra, 2006; Irmis et al., 2007b; Nesbitt, 2011; Cabreira et al., 2016; Nesbitt et al., 2017). Several authors have reviewed the synapomorphies of Dinosauria (e.g., Brusatte et al., 2010b; Langer et al., 2010; Nesbitt et al., 2010; Nesbitt, 2011; Nesbitt et al., 2012; Cabreira et al., 2016; Baron et al., 2017). They indicate that the precise diagnosis of the clade turns on whether it contains silesaurids (Dzik, 2003; Langer and Ferigolo, 2013; Niedźwiedzki et al., 2014; Cabreira et al., 2016) or not."

Benton (2004) writes: "Cladistics as applied to dinosaurs and their close relatives began in the early 1980s...". Bakker and Galton (1974) state, "Traditionally dinosaurs are classified as two or three separate, independent groups of reptiles in the Subclass Archosauria. However, evidence from bone histology, locomotor dynamics, and predator/prey ratios strongly suggest that dinosaurs were endotherms with high aerobic exercise metabolism, physiologically much more like birds and cursorial mammals than any living reptiles. Recently Ostrom has argued forcefully that birds are direct descendants of dinosaurs and inherited high exercise metabolism from dinosaurs. Here we present evidence that dinosaurs are a single, monophyletic group, and that the key advancements of endothermy and high exercise metabolism are justification for removing dinosaurs from the Reptilia and placing them with birds in a new class, the Dinosauria." Bakker and

Galton (1974) go on and write, "we believe that Ostrom has shown that the similarities between *Archaeopteryx* and small theropods are so detailed and comprehensive that the immediate ancestor of birds must have been a saurischian dinosaur." "Therefore, we propose the erection of a Class Dinosauria, to include as subclasses the Saurischia, Aves and Ornithischia." Bakker and Galton (1974) also write another justification for this change in the classification, "This new classification, we believe, reflects more faithfully the major evolutionary steps."

Novas (1996) states on monophyly: "Dinosaur monophyly has been corroborated on the basis of the apomorphies shared by saurischians and ornithischians, and Dinosauria properly defined on common ancestry (Gauthier, 1986; Novas, 1992, 1994)." The idea of monophyly is related to evolutionary ideas, "We identify interbreeding (resulting in populations) and evolutionary descent (resulting in monophyletic groups) as two processes of interest to phylogenetic systematists, and explore the relations between the systems resulting from these processes" (de Queiroz and Donoghue 1988).

Novas (1996) also states: "According to the available evidence, the early evolution of the Ornithodira manifested a sustained improvement in locomotor capabilities. Bipedal and digitigrade postures were acquired by the ancestral ornithodiran species (Gauthier, 1986) and inherited by dinosauiromorphs, including Dinosauria ancestrally. Seven of ten unequivocal synapomorphies listed for the Ornithodira (Serenó, 1991) pertain to the hindlimb. Dinosauiromorphs inherited such adaptations, evolving transformations in the pelvic and hindlimb skeleton also related to locomotion: on the basis of recent studies (Serenó, 1991; Serenó and Arcucci, 1994), 70% of the apomorphies diagnostic of Dinosauiromorpha are features of the tarsus and pes. Novas (1996) says "Most of the synapomorphies diagnostic of Ornithodira, Dinosauiromorpha, Dinosauriformes, and Dinosauria are involved with hindlimb anatomy. Thus, it is reasonable to conclude that the early evolution of Ornithodira was characterized by improved locomotor capabilities (e.g., erect hindlimbs, bipedality, digitigrady). Although new discoveries may demonstrate that other portions of the skeleton changed extensively during the early evolution of ornithodirans, it is clear that locomotor improvement was sustained. Modifications in the pelvic girdle and hindlimb anatomy were more profound than in the scapular girdle and forelimbs. Only pterosaurs and theropods manifested extensive transformations in the latter skeletal elements (Gauthier, 1986; Serenó, 1991; Novas, 1994)."

The taxon search website presents, "Dinosaurian monophyly was reestablished in the cladistic era with birds as descendants"<sup>1</sup>

Cladistics is not only a way of grouping, as McLain et al. (2023) see it. It was responsible for reestablishing the idea of "birds as dinosaurs' descendants."

4. McLain et al. (2023) mention "twelve dinosaur-unique traits".

However, those traits do not belong to dinosaurs (as this term has been traditionally understood). McLain et al. (2023) use the features that belong to Dinosauria, defined by Nesbitt (2011) as a group that comprises birds and dinosaurs (as demonstrated above), and say those traits are "dinosaur-unique traits."

There are many problems with these authors' statement. First, it is incorrect because the definition applied to Dinosauria by Nesbitt (2011), who lists the 12 traits cited by McLain et al. (2023), differs from that of Richard Owen from 1842. So, I do not understand why McLain et al. (2023) use those two terms interchangeably. If the authors are unaware of the changes that Dinosauria has undergone, they should be more careful using terms and listing traits related to that term. If the authors are aware, I wonder why they are using this term interchangeably, because Dinosauria has been applied to a group that gathers dinosaurs and birds, not only dinosaurs (as it was by Owen's definition). If the authors know how Dinosauria has been used lately, how can they differentiate which features are unique for dinosaurs and which are unique for birds? How can they know the ones Nesbitt (2011) listed only belong to dinosaurs? Nesbitt (2011) does not make that difference when listing the features. Are they calling birds dinosaurs? Are they agreeing with the premise of the term Dinosauria used by Nesbitt (2011)? If not, it is logically inconsistent to use the features and disregard the definition and meaning they are based on.

McLain et al. (2023) are using the traits from Nesbitt (2011) for Dinosauria as a group that includes dinosaurs and birds, and they are inconsistently and equivocatingly calling them dinosaurs. The other possibility is that McLain et al. (2023) explicitly use the cladistics definition for Dinosauria. That term is defined through evolutionary means and based on evolutionary assumptions. This raises one question: why would creation scientists use that definition with clear evolutionary bias in a creationist discussion?

5. One of the features McLain et al. (2023) mention as diagnostic for Dinosauria (citing Nesbitt 2011) is the Epipophyses presence on the anterior cervical, which is cited by Gauthier (1986) as an apomorphy (an evolutionary-based term) shared supporting

<sup>1</sup> [http://taxonsearch.uchicago.edu/?tax\\_id=2&exe=display&ke=key&sort=none](http://taxonsearch.uchicago.edu/?tax_id=2&exe=display&ke=key&sort=none)

the monophyly of Saurischian. Gauthier states that in the same paragraph, and he also says that Saurischia is within Dinosauria.

Gauthier (1986) defines Saurischia as combining “Saurischia Seeley, 1887 plus Aves Linne, 1758.” He does that based on a hypothesis, “Sauropodomorpha and Theropoda (including birds) are *hypothesized* (emphasis mine) to be sister-groups within Saurischia, and Ornithischia is considered to be the sister-group of Saurischia within Dinosauria.” He states, “Saurischia is here defined to include birds and all dinosaurs that are closer to birds than they are to Ornithischia.” And to make the evolutionary bias even more straightforward, he states in his conclusions: “Accordingly, Saurischia is herein defined ostensibly to include birds and all dinosaurs that share a more recent common ancestor with birds than they do with Ornithischia.” That bias is also demonstrated in Holtz and Osmólska (2004) in their chapter on Saurischia in the book *The Dinosauria*, who state: “Application of cladistic methodology by Gauthier (1986) brought about a significant rearrangement of phylogenetic inferences that ameliorated the confusion of the past.” They follow and write: “Gauthier (1986) was the first to formally define Theropoda as ‘birds and all saurischians that are closer to birds than they are to sauropodomorphs’”.

Langer and Benton (2006), on characters and Dinosauria, state, “The majority of the characters discussed below are modified from previous studies of early dinosaur *evolution* (emphasis mine), which are quoted accordingly.” The characters are counted regarding evolutionary ideas. One example is one of the features McLain et al. (2023) mention, a perforated acetabulum. Langer and Benton (2006) write: “Since the suggestion of a monophyletic Dinosauria, the open acetabulum has been used to diagnose the group (Bakker & Galton 1974; Bonaparte 1975). More recently, the term semi perforate acetabulum has been introduced to define the plesiomorphic condition for the group, given that various authors (Gauthier & Padian 1985; Novas 1996; Benton 1999; Benton et al. 2000; Carrano 2000; Fraser et al. 2002) noted that some basal dinosaurs lack a fully opened acetabulum. Accordingly, Gauthier (1986) has proposed that the full opening of the acetabulum occurred independently several times in dinosaur evolution.” More on the perforated acetabulum is also mentioned by Nesbitt (2011), “Rauisuchia and various subgroups have been grouped based on only a few potential synapomorphies (e.g., additional sacral vertebrae, rugose ridge on ilium, perforate acetabulum) and the fact they did not easily fit into Dinosauria, Aetosauria, Phytosauria, or Crocodylomorpha.” Novas (1992) mentions the

perforated acetabulum as a synapomorphic feature for Dinosauria. He says that the conclusion was “Based on recent studies on dinosaur phylogeny (Gauthier 1986; Brinkman and Sues 1987; Novas 1989a, 1989b; Sereno and Novas 1990), and as a result of the present analysis...”

Along with the other features mentioned above (epipophyses presence on the anterior cervical and perforated acetabulum), fourth trochanter, and the other features McLain et al. (2023) cite as “rigorous anatomical details,” from Nesbitt (2011), there are a lot of evolutionary ideas present.

That is the context of the “12 dinosaur-unique traits” mentioned by McLain et al. (2023) based on Nesbitt (2011).

Dinosauria is diagnosed by 12 unambiguous synapomorphies, many of which were found previously (see below). As a consequence, the placement of silesaurids, especially *Silesaurus*, is well supported outside Dinosauria. The following paragraphs describe the unambiguous and ambiguous synapomorphies for Dinosauria in a comparative context with previous results. Unambiguous synapomorphies found here and previously: supratemporal fossa present anterior to the supratemporal fenestra (144-1) (Gauthier 1986; Novas 1996; Sereno 1999); *epipophyses present in postaxial anterior cervical vertebrae (186-0)* (Novas, 1996; Langer and Benton 2006); apex of *deltpectoral crest situated at a point corresponding to more than 30% down the length of the humerus (230-1)* (Bakker and Galton 1974; Novas 1996; Sereno 1999; Fraser et al. 2002; Langer and Benton 2006; Ezcurra 2006; Irmis et al. 2007a); *radius shorter than 80% of humerus length (241-1)* (Irmis et al. 2007a); *fourth trochanter a sharp flange (316-1)* (similar to a character cited by Bakker and Galton 1974); fourth trochanter asymmetrical, with distal margin forming a steeper angle to the shaft (317-1) (found in ACCTRAN in Langer and Benton, 2006; Ezcurra, 2006); proximal articular facet for fibula of the astragalus and calcaneum occupies less than 0.3 of the transverse width of the element (362-1) (Langer and Benton 2006).” (Emphasis mine to highlight the features McLain et al. 2023 mentioned)

As one can see, all the features mentioned by McLain et al. (2023) are based on the idea of a relationship among different groups and the underlying idea that they evolved. Therefore, it is a considerable challenge and almost impossible even to discuss the presence of those features since they are related to evolutionary ideas.

In summary, McLain et al. (2023) seem to commend the features cited in Nesbitt (2011). However, the “rigorous anatomical details” are not

simply anatomical. They are counted as diagnostic and that relies on an evolutionary-based worldview of interpretation as demonstrated above. With that said, there are two problems I would like to raise. First is the problem of seeing those features from Nesbitt (2011) as anatomical only and not accounting for why they are used as diagnostic. As one can note, they are not simply characteristics or diagnostic (as will be shown in the definition topic); they became diagnostic because of the evolutionary-based worldview. Second is the problem of using and commending those features, secular data, evolutionary-influenced definitions, and terms without accounting for their evolutionary-based worldview. Doing that, as it seems done by McLain et al. (2023), makes inescapable the conclusion that birds are dinosaurs, or at least incoherent and inconsistent, to avoid this conclusion. That is the logical consequence. Gauthier's last statement (37) in his 1986 paper is, "Birds are living dinosaurs, and as such, they have extended the preeminence of Dinosauria among terrestrial vertebrates from the late Triassic to the present day."

6. Nesbitt (2011) has his evolutionary assumptions presented in the paper's title and the first paragraph of the paper's abstract.

Title: "The Early Evolution of Archosaurs: Relationships and the Origin of Major Clades."

Abstract: "Archosaurs have a nearly 250 million year record that originated shortly after the Permian-Triassic extinction event and is continued today by two extant clades, the crocodylians and the avians. The two extant lineages exemplify two bauplan extremes among a diverse and complex evolutionary history, but little is known about the common ancestor of these lineages."

Nesbitt (2011) also states, "This study investigates the evolutionary relationships of basal archosauriforms and places disparate clades of Triassic archosaurs into a comprehensive analysis." Those assumptions will, consequently, lead to evolutionary-based conclusions, as demonstrated.

In summary, the article cited and commended by McLain et al. (2023) shows that the author (Nesbitt, 2011)

1. assumes evolution,
2. is using cladistics in its very nature,
3. assumes common ancestry,
4. assumes relationships between different groups, and
5. his analyses aim to investigate evolutionary relationships between different groups to prove evolutionary relationships.

So, based on that, a persistent question remains: why would creation scientists use all of that in a creationist discussion?

## Definitions

In his chapter in the book *The Dinosauria*, Michael Benton (2004) writes on the influence of cladistics on definitions and diagnoses, and what a character means in that context. He states on page 7: "There have also been important developments in the definition and diagnoses of taxa. Until 1990 most cladistics equated taxa with their characters (e.g., Aves is diagnosed as those organisms that possess primary flight feathers and wings). Since then, a clear distinction has been proposed between the definitions and diagnoses of clades (de Queiroz & Gauthier 1992, 1994). Clades are diagnosed by characters that evolve at or immediately prior to their origin but do not make individual clades 'what they are.' Instead, individual clades are named on the basis of their membership. Named taxa are then fixed on the basis of their ancestry."

De Queiroz and Gauthier (1990) state about definitions and diagnoses: "Definitions are ontological statements about the existence of entities that result from relationships of common ancestry among their parts; descriptions and diagnoses are epistemological statements about how we recognize the parts of those entities." That is, the diagnoses recognize ancestry. Diagnosis commonly relies on including or excluding other groups hypothesized as evolutionary-related.

With that said, how can one separate what is what when you have synapomorphies (evolutionary biased) diagnosing groups that evolutionists see as deeply past-connected? How can McLain et al. (2023) separate and differentiate what characterizes a bird and what characterizes a dinosaur in the dataset from secular scientists? This question is pertinent because addressing any feature-related issue without a clear definition of terms is hard or even impossible. As McLain et al. (2023) question *Archaeopteryx* and state: "It is not evolution that informs us whether or not *Archaeopteryx* has teeth in its maxilla, the fossils adjudicate this question." Thus, instead of using an obvious feature like teeth, which nobody, including myself, would question, those authors should demonstrate why and how the more controversial features are empirically scored. However, regarding the datasets, how can the authors explain how to empirically separate different kinds of evolutionary datasets since birds have been included within Dinosauria?

Holtz and Osmólska (2004) state: "The origin of birds lies within theropod dinosaurs, meaning that theropods include birds in addition to the traditionally understood theropod dinosaurs." Gauthier (1986) states: "Theropoda is defined ostensibly to include birds and all saurischians that are closer to birds than they are to sauropodomorphs." Gauthier (1986) combines the definition of Theropod from Marsh

(1881) plus Aves Linne (1758). Theropoda's definition and diagnosis reflect the evolutionary thinking and understanding that birds and dinosaurs are connected. So now, how can one separate and differentiate in diagnosing the features that should characterize birds and those that should describe dinosaurs?

Turner, Makovicky, and Norell (2012) write: "Gauthier (1986) applied a stem-based definition to Theropoda, which consists of the last common ancestor of birds (Aves) and all descendents [sic] closer to birds than to sauropodomorph dinosaurs. This definition combined the traditional usage of Marsh's (1881) Theropoda with the realization that Aves (Linnaeus, 1758) was deeply nested within Theropoda as originally proposed by Thomas Huxley (1868)."

Gauthier (1986) also creates a new combination for Coelurosauria. He states: "As defined here, Coelurosauria includes birds and all other theropods that are closer to birds than they are to Carnosauria." The Maniraptoran term and definition were also created by Gauthier in 1986. It was defined based on and influenced by an evolutionary worldview. That said, when McLain et al. (2023) use the terms Maniraptora/maniraptoran, theropod, and Coelurosauria/coelurosaur, my question is, are they following the logical consequence of their use? Do they agree with the premise from which those terms were created, used/applied? If they disagree, that makes their use of those terms inconsistent, and, for that reason, it should be inapplicable in the arguments those authors raised.

It is necessary to understand that "the current state of vertebrate anatomy and taxonomy" is embodied with evolutionary bias. We all need to be aware and discerning about this issue. And, as McLain et al. (2023) write, my "discussions of dinosaurs, birds and feathers are, at times, imprecise and inconsistent" with the actual state, I can do nothing but agree with them. My arguments do not follow the "current state." Because I do not accept the premise from which the "current state" has been built, I do not take or follow the evolutionary bias behind it. Benton (2004), in his chapter in *The Dinosauria* book, writes: "Both before 1990 and since, names have been applied to nodes in cladograms according to the whim of the systematics."

### On Using the Brusatte et al. (2014) Dataset

McLain et al. (2023) compare *Archaeopteryx* to *Falcarius*, *Deinonychus*, and *Confuciusornis*.

"The situation of *Archaeopteryx* and maniraptoran dinosaurs is unquestionably different, in that numerous anatomical traits seen in the specimens of *Archaeopteryx* show clear homology to theropod

dinosaurs. Returning to the random selection of 20 characters from Table 1 above, we have included the character states for two ~100-kg maniraptoran theropods (*Falcarius*, a 4–5m long therizinosaur, and *Deinonychus*, a 3.4m long dromaeosaurid), *Archaeopteryx*, and the crow-sized Cretaceous bird *Confuciusornis*. These taxa are chosen for anatomical comparison to *Archaeopteryx* because their taxonomic status is uncontroversial (two decidedly terrestrial dinosaurs and one volant bird) and the substantially complete nature of their skeletal remains maximizes the number of verifiable character states available for comparison."

They write four paragraphs arguing this comparison demonstrates that "These animals show a striking degree of skeletal similarity...". However, I would like to point out some observations on this matter:

1. The Brusatte et al. (2014) dataset presents 853 characters. They use 20 characters for this comparison.
2. No explanation is given as to why they use an amount of 20 characters.
3. They assume the classification of *Deinonychus* as a dinosaur. This classification is not "uncontroversial" by any means, as they state.
4. They assume that dromaeosaurids are dinosaurs.
5. The definition of maniraptoran is embedded in an evolutionary worldview, as demonstrated in this paper.
6. The definition of the term theropod is embedded in an evolutionary worldview, as demonstrated in this paper.
7. They assume *Archaeopteryx* is a dinosaur to argue *Archaeopteryx* is a dinosaur, that is, circular reasoning.

Therefore, their arguments regarding this topic become irrelevant, confusing and impossible to address.

### Feathers

McLain et al. (2023) say that the definition of feathers and dinosaurs is central to the debate. They also cite Xu and Guo's (2009) definition of "modern" feathers. However, that definition does not apply to most of the types Xu and Guo describe, "but other morphotypes such as morphotypes 1–5 are difficult to refer to a known modern feather category." There are eight of them; five do not fit in the category.

Xu and Guo (2009) try to explain why "They could be primitive feathers different from any modern feathers. Alternatively, this could be due to the fact that the available specimens do not preserve enough details, or we are unable to observe some preserved morphologies. Nevertheless, the distribution of these feather morphotypes across a dinosaurian phylogeny,



in combination with recent advances in neontological study on archosaurian integuments, reveals an evolutionary sequence of these structures and their possible adaptive context (Fig. 5)." It is important to point out that they are using the changed definitions for dinosaurian and archosaurian.

Xu and Guo (2009) go on to write: "Five major events are *inferred* (emphasis mine) to have occurred sequentially early in feather evolution before the origin of the Aves, and they are: 1) appearance of filamentous and tubular morphology..." "We believe, however, that the defining features of modern feathers might have evolved in an incremental manner rather than in a sudden way." "Among these defining features, tubular nature and filamentous morphology represent the earliest ones appearing in feather evolution and mark the origin of feathers as indicated by both paleontological and neontological data (Harris et al., 2002; Xu et al., 2009)." "Feathers are thus *here* (emphasis mine) defined as integumentary structures that are tubular and filamentous in morphology. Follicle, hierarchical branches, and planar form are inferred to have evolved later in feather evolution. *Under such a definition* (emphasis mine), eight morphotypes of feathers are identified in nonavian dinosaurs". The reason Xu and Guo write that is mentioned earlier in the same paragraph: "In an evolutionary context, these features have probably evolved in an incremental manner rather than simultaneously, as also indicated by developmental data." This developmental data, Xu and Guo mention, is an evo-devo idea proposed by Prum (1999).

Lin, Li, and Chuong (2020), in their chapter in the book *The Evolution of Feathers: From Their Origin to the Present* state: "Key events in the evolution of feathers have been reconstructed by linkages of the extant structures of feathers and the developmental process. So, according to the analysis of this kind of linkages, a five-staged feather evolution-development (evo-devo) model was proposed by Prum in 1999."

The idea of filaments is based on an evolutionary model for feather origin, as demonstrated by the citations from Xu and Guo (2009), the paper mentioned by McLain et al. (2023). Yes, I agree definitions are necessary. But about this matter, I do not understand why these authors cited Xu and Guo's (2009) definition of extant feathers but did not mention the general definition of feathers used by Xu and Guo (2009), which is based on an evolutionary context, as shown above. The paper these authors mention (Xu and Guo 2009) is titled "The Origin and Early Evolution of Feathers: Insights from Recent Paleontological and Neontological Data." It states in its abstract: "We believe, however, that the defining features of modern feathers might have *evolved*

(emphasis mine) in an incremental manner rather than in a sudden way. Consequently, an evolutionary model characteristic of both transformation and innovation is more acceptable for feather evolution."

Also, it is quite disturbing that McLain et al. (2023) in their response to my paper also mention McLain, Petrone, and Speights (2018) for detailing "the various types of feathers found in fossil birds and dinosaurs." The types they describe are based on the definition of Xu and Guo (2009), which carries an evolutionary-based view, as demonstrated above. Xu and Guo (2009) say, "complex modern feathers *must* (emphasis mine) have been evolved from simpler structures and their morphological and developmental complexity probably has been increased incrementally in feather evolution..."

Furthermore, what McLain et al. (2023) are doing is, again, begging the question. It is not indisputable that most of the genera McLain, Petrone, and Speights (2018) mentioned are dinosaurs, nor the structures they mention as feathers are actually feathers. Interestingly, one of the papers cited by McLain, Petrone, and Speights (2018) was written by one colleague and laboratory co-worker of mine, who really and profoundly disparages creationists.

McLain et al. (2023) state: "First, Haynes claims that 'bird' is defined as those organisms 'having actual modern-looking feathers' (Haynes 2022, 293). What exactly is meant by this is unclear." They are right in mentioning that, because it is fair that we clarify the definitions of the terms we use. However, it is interesting and surprising that they requested me to define a feature known for so long (feather). Yet, they do not demand from themselves the same rigor in defining terms that have undergone many changes like maniraptoran, theropod, coelurosaur, Archosauria, and Dinosauria. They grant the definition of those terms and claim that my approach to the definition is "muddled." They also claim the term I used "modern-looking feathers" is "unclear," even though they used the definition of a modern feather by Xu and Guo (2009) and mentioned the distinct forms of feathers known among extant birds that "qualify" as "modern-looking feathers." So, they seem to have understood what a modern-looking feather is.

McLain et al. (2023) say, at least three times, that I grant or state the presence of filaments in dinosaurs. However, that is not the case at all. The whole point of my paper is the very opposite of that. I never said, implied, or suggested that. Those authors' conclusion that I granted the presence of feathers or "feather-like" structures in "theropod dinosaurs" or coelurosaurs results from a misreading and misunderstanding of my statements taken out of context.

And it is because of their misunderstandings they also say that my definition of filaments “would demand reclassifying substantial numbers of theropod dinosaurs, including the Tyrannosauroidae, Compsognathidae, and others, as birds.” I am not demanding anything. It is not my place to demand or reclassify anything. I consider this whole argument raised by these authors as a strawman fallacy, because they first take my arguments out of context and then misrepresent them.

They raise some questions about whether a filament is homologous to feathers. Based on the information about filaments like, “Patches of long filaments, reminiscent of structures present in theropods and thought to be the evolutionary precursors of feathers...” (Godefroit et al. 2020), who also state: “Even if these filaments can be confirmed as representing epidermal structures, it is still unclear whether these simple monofilaments are part of the evolutionary lineage of feathers, or represent independent evolution of projecting epidermal appendages (Witmer 2009; Barret et al. 2015)”. And Xu (2020) writes: “Most of these filamentous integumental structures have been identified as early feathers, but their relationships to various modern feather morphologies are not well known because many of their morphological details are often poorly preserved or have yet to be recovered (Barrett et al. 2015; Xu and Guo 2009).”

It means that when a fossil is found with filaments, they are coded as having feather-like structures or the so-called protofeathers, or even feathers. And that happens because “In general, it is accepted now that the first feathers are simple, filamentous structures” Xu (2020). One example is the *Yutyranus huali*, which holds the title of a gigantic feathered dinosaur and has in its name the word feathers in Mandarin (Yu) (Xu et al. 2012). Yet only filaments were found.

One thing needs to be clear: my main goal and purpose in this discussion (regarding my paper) is not assigning animals and their classification. My paper points out that the debate over feathered dinosaurs should be more carefully conducted because of the evolutionary influences. That is biased, and that bias is embedded in the coding. The point is, a filament is not even related to a feather, so how can it be or not be homologous? Homology is not the point. To answer whether it is homologous, I need to grant a filament is related to a feather on some level, which I do not grant or agree with. This idea comes from an evolutionary context. I do not consider filaments are homologous to feathers because of the change in the definition of feathers demonstrated here in this paper, and the mentioned citations.

Regarding the issue raised by the McLain et al. (2023) about what a bird is, I say that what a bird

is should not be defined by evolutionary means. I would say that defining it is not the only important objective, but also explaining where this definition comes from, because an evolutionary worldview could have permeated the origin of that definition.

These authors beg the question when they say, “Second, if only pennaceous feathers qualify as sufficiently “modern-looking,” then Haynes’ mention of their presence in maniraptoran dinosaurs (Haynes 2022, 293) would require these dinosaurs to be birds.” Again, it is not indisputable that many of those animals are dinosaurs.

These authors change the point here: “Maniraptora non-controversially includes the Alvarezsauridae, Therizinosauria, Oviraptorosauria, Troodontidae, Dromaeosauridae, and Scansoriopterygidae. Clear pennaceous feathers have been found in each of these groups save the Alvarezsauridae and Therizinosauria, whose members display more tuft-like feathers.” The question is not whether those groups are part of Maniraptora but whether Maniraptora includes birds. Because if it includes birds, there is no problem for Oviraptorosauria, Troodontidae, Dromaeosauridae, and perhaps Scansoriopterygidae to present feathers. Some scientists hold that those groups are represented by birds (Feduccia 2020).

McLain et al. (2023) say: “As Haynes notes in her paper, there are numerous definitions of Aves and Avialae. Nonetheless, vertebrate paleontologists typically define Aves as inclusive of all living birds, while the term Avialae is applied beginning with or near *Archaeopteryx* and is inclusive of the wide array of non-modern fossil bird groups plus modern birds [see Cau (2018) for a detailed discussion]. While we are not advocating for a phylogenetic definition (as they are evolutionary in their construction), it is important to recognize that neither Aves nor Avialae are defined solely on the basis of possessing feathers because (1) feathers are known in numerous non-modern bird fossils classified outside of Aves; and (2) feathers are known among numerous dinosaurian taxa classified outside of Avialae.”

Even though McLain et al. (2023) say they are not advocating for a phylogenetic definition (“as they are evolutionary in their construction”), they are using and commending them. So, if the terms are evolutionary in their construction, these authors should note that evolutionary ideas define them. Gauthier and de Queiroz (2001) write regarding Aves: “We then propose a taxonomy that resolves current nomenclatural ambiguity by using a different name for each of the clades that everyone wants to talk about—namely, those composed of bird-line archosaurs, the feathered dinosaurs, the flying dinosaurs, and the crown dinosaurs instead of referring to all of them as ‘Aves.’” Gauthier and de Queiroz (2001) follow and

write: "Thus, 'Aves' has been used for (1) the clade of feathered dinosaurs (e.g., Paul 1988; Lee and Spencer 1997), (2) the clade of flying dinosaurs (e.g., Ji and Ji 2001), (3) the clade stemming from the most recent common ancestor of *Archaeopteryx* and crown dinosaurs, hereafter referred to as the 'Archaeopteryx node' (e.g., Padian and Chiappe 1998), and (4) the clade of crown dinosaurs (e.g., Perle et al. 1993). The name has occasionally been used in other ways: Patterson (1993), for example, applied 'Aves' to the entire clade of bird-line Archosauria, while Thulborn (1975) applied it to only part of that clade, namely, Theropoda."

Gauthier and de Queiroz also write this about Avialae: "The name 'Avialae' was defined originally by Gauthier (1986:36) as referring to 'Ornithurae plus all extinct maniraptorans that are closer to Ornithurae than they are to Deinonychosauria,' which is a stem-based definition. However, unlike the case for definitions proposed for names of clades that emerged from his analysis, Gauthier (1986) did not always provide explicit definitions for his terminal taxa (e.g., Carnosauria) or use them consistently when he did. Thus, although Gauthier (1986) defined 'Avialae' as the name of a stem-based clade, he also used it as a name for the *Archaeopteryx* node (for example, when listing the synapomorphies [1986:12]) and as an apomorphy-based name (when referring to avialans as 'winged theropods' [1986:12])." Gauthier (1986) writes: "In a formal sense, however, 'birds' and Aves will not be synonymous. The 'winged theropods' included in Avialae possess the following synapomorphies distinguishing them from other Theropoda."

The term Aves has been applied through an evolutionary context in an evolutionary context and became not synonymous with birds because of an evolutionary context. Avialae is a new term created and defined by Gauthier (1986) based, also, on an evolutionary context.

Interestingly, McLain et al. (2023) can note a phylogenetic definition and evolutionary construction in Avialae and perhaps in Aves' term application. Still, they seem not to have noted that the same happens to the terms Archosauria, Dinosauria, Coelurosauria, Theropod, and Maniraptora.

Those authors beg the question when they state: "This brings us to a curious situation. Because of the clear evidence of feathers on certain dinosaur fossils, a number of young-earth creationist authors have advocated for reclassifying these taxa as birds." It is not indisputable that some of those fossils are dinosaurs, and some have feathers.

About their comment on Dr. David Menton, I find it unnecessary to mention his name for any reason, just to make a case.

McLain et al. (2023) are correct in pointing out that I did not express a definition of feathers. I do not eschew a morphological definition of feathers. I clarify that a definition based on an evolutionary context should not be used in a creationist discussion. And I emphasize the reasons for that in this paper.

### Pygostyle

McLain et al. (2023) raise an issue with the pygostyle. They state that my claim about *Archaeopteryx* having a pygostyle is mistaken by definition. First, I mention it only as *acting as a* pygostyle.

One necessary point to demonstrate is that the idea regarding a pygostyle is also related to some evolutionary ideas. Wang and O'Connor (2017) state: "The transformation from a long reptilian tail to a shortened tail ending in a pygostyle and accompanied by aerodynamic fanning rectrices is one of the most remarkable adaptations of early avian evolution." So, caution needs to be applied when analyzing this structure.

Wang and O'Connor (2017) also say: "whereas the pygostyle in more primitive birds does not appear morphologically capable of supporting the rectricial bulbs and musculature necessary to control an aerodynamic fan-shaped tail." So, it seems like it is not necessary that the pygostyle has to anchor tail feathers, as McLain et al. (2023) mention. Also, pygostyle-like structures have been mentioned in a group considered to be birds, Oviraptorosauria (Barsbold et al. 2000a, 2000b), which McLain et al. (2023) seem to consider to be dinosaurs.

Wang and O'Connor (2017) say about this genus: "In the very primitive *Caudipteryx* the distal five caudal vertebrae articulate tightly through the presence of well-developed prezygopophyses and the last three of these are ankylosed but remain unfused (IVPPV 22606). These vertebrae lack neural spines and transverse processes, which are present in the proximal caudal vertebrae. Tail feathers are preserved in at least three primitive oviraptorosaurs: *Protarchaeopteryx*, *Caudipteryx* and *Similicaudipteryx*. The holotype of *Caudipteryx* has an estimated eleven pairs of tail feathers attached to the caudalmost five or six vertebrae, which form the incipient pygostyle." So, it seems like the possibility of an unfused or an "incipient" pygostyle is reasonable.

Rashid et al. (2018) also mention unfused, and partially fused pygostyles. Those expressions, used along with the term pygostyle, make the definition not strictly call for bone fusion. Wang and O'Connor (2017) mention *Epidipteryx* and say it has a rod-like structure, unfused, and they call it a pygostyle. It seems like the bone fusion can be incomplete and still be considered a pygostyle. Wang and O'Connor

(2017) mention that this situation suggests that “the pygostyle fully co-ossifies late during ontogeny.” Also, there is a juvenile species, *Zhongornis haoae*, that was considered to be a short-tailed bird lacking a pygostyle (Gao et al. 2008).

Persons et al. (2014) state: “*Caudipteryx* also had a caudal feather-fan (Ji et al. 1998), but lacks a pygostyle; nevertheless, the last five vertebrae appear to be tightly integrated into an inflexible unit (personal observation).” Interestingly, some authors agree that *Caudipteryx* had a pygostyle, as cited by McLain et al. (2023), and others say it did not.

Rashid et al. (2018) write: “In conclusion, this study reveals several aspects of avian tails that impact the interpretation of Mesozoic specimens. From extant bird tails, we observe that pygostyle formation is a post hatch phenomenon, and vertebral morphology changes dramatically during ontogeny.” And that, “post-hatching pygostyle formation is a conserved feature in modern birds, and the same appears to be the case in Mesozoic birds” (Knoll et al. 2018). “The contributions of these bone fusions and tail truncation to a more streamlined, fortified skeleton are thought to improve fight dynamics and stability” (Heers and Dial 2012). There is also the possibility that “pygostyle formation timeframe is better correlated with behavior, developmental mode, or associated genetic group” (Rashid et al. 2018).

As demonstrated above, the understanding of pygostyle seems broader than McLain et al. (2023) mention. So, despite McLain et al.’s (2023) claim that I have “either misunderstood the nature of *Archaeopteryx*’s tail,” “misunderstood the term pygostyle,” or “is redefining pygostyle as the opposite of its meaning,” I have done none of those. I also never mentioned that Wellnhofer (2009) referred to *Archaeopteryx* as having a pygostyle. The absence of bone fusion in *Archaeopteryx* has not been a problem for the scientists who have always considered it to be a bird (Lee and Worthy 2011; Padian 2004).

McLain et al. (2023) state: “Furthermore, nowhere in Wellnhofer (2009) is the list of eight features presented by Haynes, and the order of Wellnhofer descriptive writing is altogether different from the list provided by Haynes.” Those authors have misread, again, the paragraph where I talk about this topic. Here is the part of my paper from where they raised this issue:

“Following is the list of features that *Archaeopteryx* has and which are interpreted as shared with dinosaurs. Of course, it is impossible to discuss all of them since that is not the scope of this paper. However, this brief explanation presents principles from which those characteristics can be seen, analyzed, and understood. The list from Wellnhofer (2009) includes the

presence of:

- (1) teeth in its jaws,
- (2) three clawed digits,
- (3) a long bony tail (pygostyle),
- (4) gastralia,
- (5) tetradactylous palatine (Mayr, Pohl, and Peters 2005),
- (6) a hyperextendable claw on the second toe,
- (7) a reduced fifth toe, and
- (8) interdental plates.”

From that, one can understand I am not listing all the features that belong to *Archaeopteryx*. As written in my paper, I mention “features that *Archaeopteryx* has and which are interpreted as shared with dinosaurs.” Nowhere in my paper do I say they are all of *Archaeopteryx*’s features. I even say, “It is impossible to discuss all of them.” I mentioned the list from pages 123–131 of Wellnhofer (2009). He presents the features of *Archaeopteryx* itself, not comparing them with anything. My point with that paragraph is to “present principles from which those characteristics can be seen, analyzed, and understood.”

I believe that unintentionally, McLain et al. (2023) keep misreading and misunderstanding what is written. They only mention the part where Wellnhofer compares *Archaeopteryx* with theropods (162–166). On page 162, Wellnhofer writes about T. H. Huxley, who pointed out that “birds and dinosaurs were closely related...”. Wellnhofer says that John Ostrom “revitalized the theropod hypothesis...” He also mentions that “Using the subsequently developed cladistic analysis of characters, Gauthier (1986)...” supported Ostrom’s hypothesis. Gauthier’s idea was “further supported by later phylogenetic analyses (Holtz 1994, Currie & Padian 1997).” Wellnhofer says: “This means that the genealogy of both groups [Avialae and Dromaeosauridae] goes back to a common ancestor and together they form a monophyletic group. As yet, no particular fossil has been identified as the taxon in this role, but clearly, the ancestor of the birds was a dinosaur within the group of the Maniraptora (Chiappe 2009).” Wellnhofer also states: “In the course of their evolution in a bird-like direction, theropods experienced a progressive...” (mentioning the features that changed throughout time in those groups). When mentioning some characteristics, he says, “*Archaeopteryx* retained it as an inheritance from its ancestors [mentioning in the sentence theropods and archosaurs]. The papers Wellnhofer cites were written by himself (Elzanowski and Wellnhofer 1996; Wellnhofer 1993). He goes on to say: “In the course of theropod evolution toward *Archaeopteryx*...” [then he mentions another feature]. He concludes (page 163), citing (Padian and Chiappe [sic] 1998), “These structures of *Archaeopteryx* can only be interpreted

as inherited from dinosaurian ancestors evolving towards birds; *they do not make sense* as characters that have developed exclusively with regards to the mode of life of modern birds" (emphasis mine).

The place where Wellnhofer (2009) compares *Archaeopteryx* with theropods (the one mentioned by McLain et al. 2023), page 163, starts by stating, "In his fundamental work, 'Archaeopteryx and the Origin of Birds,' Ostrom (1976) assembled the arguments for the theropod hypothesis of the origin of birds from the materials available at that time."

From the background presented above, we can see that Wellnhofer (2009) seems to agree with the following:

1. T.H. Huxley's idea that "birds and dinosaurs were closely related".
2. John Ostrom brought back the theropod hypothesis.
3. Gauthier's work on cladistics in 1986 supported Ostrom's idea.
4. Gauthier's idea was further supported by phylogenetic analyses.
5. The genealogy of Avialae and Dromaeosauridae goes back to a common ancestor and together they form a monophyletic group.
6. The ancestor of the birds was a dinosaur within the group of the Maniraptora.
7. Theropods were in the course of evolution in a bird-like direction.
8. *Archaeopteryx* retained features of its ancestors [theropods or archosaurs].
9. *Archaeopteryx* was in the course of the theropod evolution.
10. The structures of *Archaeopteryx* only have one interpretation, an inheritance of dinosaurian ancestors, and those structures only make sense that way.

It is with this worldview that Wellnhofer (2009) compares *Archaeopteryx* with theropods. He was not neutral. His biases, influenced by evolutionary ideas, shaped his interpretation of the features of *Archaeopteryx* and made him understand them as related. His comparisons should be seen in the context of his thoughts and worldview. Without that, it is impossible to discern what is a fact and what is an interpretation of the facts. Besides the background of an idea, another problem is the definition of the word theropod. Wellnhofer seems to be using the definition of Gauthier (1986), which includes birds in the theropod group. So, he can merely be comparing birds with birds. After all, other scientists do understand that most of the Maniraptoran group are, in fact, birds (Feduccia 2020).

Lastly, two more observations about *Archaeopteryx*. First, I am not saying *Archaeopteryx* looks like the birds we have today. Second, *Archaeopteryx* being a

bird is not something I conjured up. Many secularists have reached this conclusion for decades. Lee and Worthy (2011) state: "The status of *Archaeopteryx* as a primitive (stem, basal) bird has been almost universally accepted since its discovery over 150 years ago."

### Skeletal Pneumaticity

When arguing against pneumaticity, I intended to bring up the background of the idea that relates PSP (postcranial skeletal pneumaticity) to bird-like air sacs. Such pneumaticity presents other possibilities to explain its presence in different groups other than birds. Also, explaining that, we need to consider the possibility of not necessarily relating PSP to bird-like air sacs.

Here is the context where my argument is. "The presence of air-filled cavities (pneumaticity) in the post-cranial vertebrae of many dinosaurs has been known since around 1870. Pneumaticity in the postcranial area is also described in pterosaurs. This feature has been considered functionally related to a specialization to reduce weight (Benson et al. 2012; O'Connor 2004, 2009). Robert T. Bakker was mentored by John Ostrom, who revived the idea of the relationship between birds and dinosaurs after studying *Deinonychus*. Bakker's articles gave support to the notion that dinosaurs were warm-blooded. In 1972, Bakker *proposed* (emphasis mine) that the cavities in the vertebrae called postcranial skeletal pneumaticity (PSP) found in dinosaurs were for air sacs. He said dinosaurs 'probably had an avian-type lung with unidirectional flow' (Bakker 1972)."

McLain et al. (2023) seem not to accept that this "inference" they have in their citations relies on the background I presented. And that PSP function can also have explanations other than bird-like air sacs.

Also, I do not cite O'Connor (2006). It is Schachner, Hutchinson, and Farmer (2013) that do that, presenting an argument against the relation between PSP and lung flow.

The presence of PSP is not a problem but relating that with bird-like air sacs is.

McLain et al. (2023) say: "Haynes makes the argument that skeletal pneumatization in dinosaurs should not be taken as evidence for the presence of bird-like air sacs, and that the only reason scientists have argued this point is "to support the theory of the relationship between birds and dinosaurs (Haynes 2022, 294)."

Here is what I said (Haynes 2022):

So, based on what has been presented about Bakker's evolutionary-based proposal in 1972, Schachner, Hutchinson, and Farmer (2013), and Butler, Barrett, and Gower (2012), we can better understand the background of the pneumaticity and air sacs related topics regarding their presence in dinosaurs. It is

necessary to consider all that and discern that the consequence of this idea aims to support the theory of the relationship between birds and dinosaurs.

I never said what McLain et al. (2023) claim I said, as demonstrated above with the citation of the paragraph where this line is taken out of context. My argument was the necessity of considering the background from where the idea of relating PSP and bird-like air sacs, and that the consequence of this idea has one goal in secular literature. It aims to support the theory of the relationship between birds and dinosaurs.

Also, I am not arguing against the presence of PSP in groups other than birds. I am arguing against the inference that the presence of PSP is related to bird-like air sacs. In addition, I used not only the background of this idea as an argument against PSP and its relationship with bird-like air sacs, but I also used other papers that demonstrate challenges to this idea.

### Miscellaneous Paleontological Issues

McLain et al. (2023) raised some issues and said: “In addition to the above issues, there are some minor errors in Haynes (2022) that are nonetheless troubling to see in a paper on vertebrate paleontology. Haynes refers to *Majungasaurus* as a sauropod (the enormous, long-necked dinosaurs) when it is in fact a theropod (the bipedal, carnivorous dinosaurs). Haynes labels *Sapeornis* an enantiornithine in fig. 3, but it does not belong to this group (it is, instead, an omnivoroptyerygid—an extinct bird group outside of Pygostylia).” These authors are correct, and I thank them for pointing out these errors. They resulted from mistakes in editing and moving words around in the text. Fig. 3 also had an error that resulted from moving the words around other figures and names.

McLain et al. (2023) also say: “Haynes also wrote that Heilmann propagated a view that helped to shape the idea of a relationship between *Archaeopteryx* and dinosaurs when, in fact, Heilmann argued in his 1926 book that birds were not descended from dinosaurs because dinosaurs lacked clavicles (Heilmann’s clavicle argument would later be disproved when clavicles were discovered on dinosaurs).” Yes, I wrote that statement, but I also cited its origin. It comes from Wellnhofer (2009). Also, Padian (2004), in his chapter in the book *The Dinosauria*, states that Heilmann’s book “had an enormous influence...”

“Finally, Haynes states that the arched-back neck seen in *Archaeopteryx* specimens has been demonstrated to happen in hypersaline water. However, experimental research on this phenomenon, called the opisthotonic death posture, has indicated it occurs in cool freshwater (Cutler et al. 2011), and other researchers previously could not replicate

the posture in saltwater (Faux and Padian 2007). This topic is in dire need of further experimental research from creationists and would be an excellent taphonomy project.” Wellnhofer (2009) says that about the arched-back neck and hypersaline water, not me. I only cited it as a reasonable possibility. I did not state it. Also, fresh or salty, it was in the water. That was the point I wanted to make.

### Conclusion

McLain et al. (2023) state: “As creationists, we must be willing to evaluate the data from nature on its own terms, regardless of whether a discovery is made by a creationist or an evolutionist.” It is not because an evolutionist made it. I never said, implied, or suggested that. My words were of caution, for the need to consider the possibility of bias and the impact of it in the data and its interpretation. No one at Answers in Genesis has suggested that observational science by evolutionists should not be evaluated on its terms. This debate concerns worldviews and the influence of evolutionary ideas permeating creationist research.

One thing needs to be clear about my paper: I did not mean to define, redefine, assign, or reassign anything. I can only agree or disagree with the premises, methods, data, worldview, and reasoning of these assignments. I point out what seems more reasonable based on how a logical and biblical worldview allows me to interpret them.

The main point of my paper is to show that there have been changes in the definitions of terms because of the influence of evolutionary ideas. Those influences should be considered when dealing with secular definitions, data, and interpretations.

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## Appendix

This appendix has a list of citations from Nesbitt (2011) and Turner, Makovicky, and Norell (2012). Their papers were chosen randomly. Those citations present some of the problems regarding cladistics, its data and datasets, namely:

1. Questionable data, taxa, scoring, characters;
2. Agreements and disagreements on interpretation of features, data and scores;
3. Tentative and putative scoring;
4. Circular reasoning;
5. Interpretation on scoring, taxa, features;
6. Characters scored but in need of revision;
7. Questions on taphonomic effects on the features;
8. Different opinions on some genera;
9. Change of terms from different authors;
10. Admission of errors and mistakes;
11. Evolutionary bias in the dataset;
12. Difficulties to score some characters;
13. Character needs to represent synapomorphy;
14. Subjectiveness wording;
15. Wrong conclusions based on wrong scoring;
16. Evolutionary hypothesis influencing scoring;
17. Different results found;
18. Terms and definitions vague and hard to be defined;
19. Unclear information;

20. Problems on scoring;
21. Ambiguity on terms;
22. Different descriptions in different scoring;
23. Conclusions influencing scoring;
24. Different judgments in scoring;
25. Problems with scoring strategies;
26. Different scores;
27. Possibility of mistakes being repeated;
28. Hypothesis of evolutionary relationships are constructed;
29. Assumptions;
30. Sampling and evolutionary bias;
31. Characters and evolutionary bias;
32. Intentions on dataset.

**Quotes from Nesbitt (2011).** [The cited references are listed in the original paper.]

Similarly, *character states that appear synapomorphic with theropods* (for example, deep depression on the posterior portion of the neural arches of the cervicals, "four" sacral vertebrae) are also *interpreted as convergent* because they are not found in the taxa basal to *Silesaurus* and basal to Theropoda.

However, the ilia of *Revueltosaurus* are nearly vertical (0°–20°), thus indicating that the condition in aetosaurs *may be convergent* with that of paracrocodylomorphs.

An ectopterygoid recess was cited as a theropod synapomorphy (Gauthier 1986), and as a character uniting *Eoraptor* with theropods (Sereno 1999; Langer and Benton 2006). However, *I disagree* with Sereno (1999) and Langer and Benton (2006) *about the scoring* of *Eoraptor* and basal dinosaurs. The ectopterygoid articulates with the dorsal surface of the lateral flange of the pterygoid in dinosaurs (character 84). As a result, the ventral surface of the ectopterygoid has a slight depression for the articulation with the pterygoid. In *Allosaurus* and other tetanurans, there is a distinct recess (possibly pneumatic) and a depression for the pterygoid. The ectopterygoids of *Plateosaurus* (AMNH FR 6810), *Eoraptor* (PVSJ 512), *Liliensternus* (MB R. 2175; Rauhut 2003: fig. 19B), and *Coelophysis* (AMNH FR 7239) possess only a facet for the pterygoid and lack a distinct recess. Yates (2003) described a deep fossa in the ectopterygoid of the basal sauropodomorph *Pantyraco*. *This depression is only the articular face with the pterygoid, not a distinct fossa as in Allosaurus*. Furthermore, any slight disarticulation of the ectopterygoid from the pterygoid may look like a distinct recess; *I urge caution when scoring this character*.

Langer and Benton (2006) extensively discussed the possible homologies of additional sacral vertebrae other than the primordial two in Dinosauria and close relatives. Nevertheless, Langer and Benton (2006) only suggested that sacral vertebrae are added anterior to or posterior to the two primordial sacra, and they always *assumed* the primordial sacral vertebrae are adjacent. *Here, I argue that the primordial sacra do not always have to be adjacent and an additional sacral vertebra is present between primordial one and two in a number of archosaurs*.

Langer and Benton (2006) stated that *a ventral recess is present in Sphenosuchus. However, this slight depression is very different from that of theropods. Therefore, it is scored as (0)*.

Fifty-seven of the 87 total taxa were scored in person whereas the remaining taxa were scored from detailed drawings, notes, and photographs. I recorded all character scores and, *therefore, any mistake is my own and not the result of following others' previous scores*. In this study, *it was apparent that scoring errors were compiled in the most recent basal archosaur phylogenies*. Hence, I scored every character during this study *to prevent the repetition of errors of previous scores*.

Is a monophyletic Silesauridae incompatible with the results of Ezcurra (2006)? Ezcurra (2006) *hypothesized that Eucoelophysis is more closely related to Dinosauria than Silesaurus*. He cited three unambiguous synapomorphies supporting *Eucoelophysis*+Dinosauria including: proximal end of the pubis with acetabular depression poorly

developed or absent (214-1); pubic shaft nearly straight (217-1); and femoral trochanteric shelf absent (239-0). The first two characters are scored from the pubis of *Eucoelophysis*. Nesbitt et al. (2007) questioned the inclusion of the pubis originally assigned to *Eucoelophysis* to the holotype because the preservation is different from the rest of the holotype, the pubis was not found articulated to the other material, and there were other taxa mixed in with the holotype of *Eucoelophysis*. Therefore, the pubis originally assigned to *Eucoelophysis* cannot be shown to belong to the rest of the holotype with any confidence. The first two characters listed by Ezcurra (2006) must be scored as “?” for *Eucoelophysis*. *The last character, femoral trochanteric shelf absent, is difficult to interpret because it was homoplastic in Ezcurra's (2006) analysis, and a femoral trochanteric shelf is absent in smaller specimens of Silesaurus* (Nesbitt et al. 2007). *Therefore, there were no concrete character states separating Eucoelophysis from Silesaurus in Ezcurra's (2006) analysis. When Ezcurra's (2006) character scores were modified and rerun, Eucoelophysis and Silesaurus formed a weakly supported monophyletic clade*.

UFRGS 0156-T is a very large skull (88cm) that was assigned to *Prestosuchus chiniquensis* by Barberena (1978) and more recently by Azevedo (1991). *Parrish (1993) separated UFRGS 0156-T from P. chiniquensis in his phylogenetic analysis and found that no character scores separated the two. Thus, he combined the two in his final hypothesis*. I separate *P. chiniquensis* and UFRGS 0156-T as terminal taxa. *P. chiniquensis* and UFRGS 0156-T are both from a similar stratigraphic position near the bottom of the Santa Maria sequence. *Parrish (1993) scored characters of the calcaneum and pes of UFRGS 0156-T, but these elements are absent in UFRGS 0156-T*.

*The studies of Ezcurra (2006) and Nesbitt et al. (2007) agreed for the most part*. However, because the hind limbs were the only elements of *Eucoelophysis* that were definitely associated (within a multitaxic quarry), Nesbitt et al. (2007) *considered* the hind limbs and metatarsals the only definite material pertaining to the holotype of *Eucoelophysis*. *Nesbitt et al. (2007) hypothesized that the pubis does not go to the hind limbs, whereas Ezcurra (2006) scored the pubis as part of Eucoelophysis in his data matrix*. If the pubis character scores of *Eucoelophysis* are removed, *Eucoelophysis*, *Silesaurus*, and Dinosauria form a polytomy in Ezcurra's (2006) matrix.

In non-crocodyliform crocodylomorphs, the basiptyergoid processes are enlarged relative to the plesiomorphic condition. This character is equivalent to “basiptyergoid processes simple, without large cavity (0) or greatly expanded, with large cavity

(1)" of Clark et al. (2000). *Clark et al. (2000) scored all crocodylomorphs except Pseudhesperosuchus as having state (1). Crocodyliiforms do not have basiptyergoid processes, and so they are scored as inapplicable.*

Gower (2002) used the presence of state (1) to suggest a close relationship of aetosaurs and crocodylomorphs to the exclusion of rauisuchians (*Batrachotomus* was the only "rauisuchian" taxon scored for this character). Poor preservation, absence of access within the metotic foramen, and poor preservation in this delicate region hampers the scoring of this character in most taxa. *Even though this character requires extraordinary preservation to score, the presence of state (1) in crocodylian-line archosaurs is a potential synapomorphy (Gower and Walker 2002). Therefore, I retain this character.*

This character can be scored from the orientation of the opisthotic (Gower 2002). *This character was used by (Gower 2002) to hypothesize a sister-group relationship between aetosaurs and crocodylomorphs. After examining the braincase of Stagonolepis (MCZD 4-2), I cannot score the taxon as (1) as it appears the braincase is slightly compressed. The orientation of the descending process of the opisthotic (mediolaterally) is like that of other non-crocodylian-line archosaurs. Therefore, the foramen cannot be oriented laterally. In Sphenosuchus (SAM 3014) and Alligator, the perilymphatic foramen faces laterally.*

Gower (2002) presented this character to unite a subset of pseudosuchians including *Postosuchus kirkpatricki*, *Batrachotomus*, and extant crocodylians (for example, *Crocodylus*). As described by Gower (2002), this opening may be a discrete passage for the posterior cerebral/cephalic vein. Furthermore, *this character is difficult to score because the location of the foramen requires exceptional preservation. A groove in Sphenosuchus (SAM 3014) is located in the same position as the foramen in Postosuchus kirkpatricki and Batrachotomus (Gower 2002), and these features may be homologous. However, Gower (2002) scored Sphenosuchus as absent. Here, I suggest the condition in Dibothrosuchus (IVPP V 7907) is not clear even though it was scored as (0) by Gower (2002).*

This character cannot be scored in taxa where the prootic and epiotic/supraoccipital have coossified. This is the case in *Plateosaurus* (AMNH FR 6810). *Even though Arizonasaurus was scored as (0) by Gower and Nesbitt (2006), a reexamination of braincases (MSM P4590, P4647) indicates that the suture between the prootic and epiotic/supraoccipital cannot be discerned.*

Langer and Benton's (2006) description of this character is detailed and complete; therefore, little can be added. *I agree with their interpretations and score sauropodomorphs as (1).*

A lateral mandibular fenestra is present in nearly all archosauriforms plesiomorphically as indicated in the analysis by Juul (1994). A lateral mandibular fenestra has been reported to be small or absent in *Proterosuchus* (Charig and Reig 1970; Cruickshank 1972). Welman and Flemming (1993), confirmed by Juul (1994), and Welman (1998), showed that the well-preserved specimens of *Proterosuchus* have a small lateral mandibular fenestra. However, given the small size of the opening, the presence of this character in *Proterosuchus* deserves more discussion. The small fenestra forms at the junction of the dentary, angular, and surangular in *Proterosuchus* (RC 96, TM 201; Welman 1998). Here, the mandibular elements do not have a distinct concave region forming an edge as in *Erythrosuchus*+Archosauria. However, though there are differences, the lateral mandibular fenestra occupies the same area and is composed of the same elements in both *Proterosuchus* and other archosauriforms. The small gap may be a consequence of the slight disarticulation of the mandibular elements, but a lateral mandibular fenestra is clearly present in QR 1484 (listed as NMC 3014 in fig. 3 of Welman 1998). *Therefore, Proterosuchus is scored as having a lateral mandibular fenestra. The presence or absence of a lateral mandibular fenestra is difficult to determine in isolated mandibular elements of taxa near the base of Archosauriformes. It is unclear whether Vancleavea has a lateral mandibular fenestra. If present, it is very small like that of Proterosuchus.*

In its original formulation, Gauthier (1986) used this character to diagnose Dinosauria and focused on those elements the supratemporal fossa was present on. The character is rewritten here in order to test the homology of the extension of the supratemporal fossa anterior to the supratemporal fenestra regardless of which element is anterior to the supratemporal fenestra. Furthermore, in the original form, Gauthier (1986) scored the entire supratemporal fossa as extensive or not extensive. *In this vague wording, the presence of the supratemporal fossa in different regions cannot be evaluated. Additionally, it is not clear what constitutes extensive in character state (0) versus state (1).*

*This character may be hard to score given the erratic distribution of the element. Currently, palpebrals are known in suchians and have not been found in the many well preserved skulls of phytosaurs or the many well-preserved skulls of Euparkeria or proterochampsians.*

*This above character is rewritten to incorporate Sereno's (1991a) character "septomaxilla, present or absent." The term septomaxilla for the structure in phytosaurs is abandoned because it assumes homology among the structure of phytosaurs and other amniotes*

with *septomaxillae* (Stocker 2008). The phylogenetic position of phytosaurs previously found (Benton and Clark 1988; Gauthier 1984; Juul 1994; Parrish 1993; Sereno 1991a) within archosauriforms suggests that the “septomaxilla” of nonarchosauriform amniotes and the “septomaxilla” of phytosaurs are not homologous. Phytosaurs, *Prolacerta* (UCMP 37151), and *Proterosuchus* are scored as (1).

Character state (1) is clearly present in *Postosuchus kirkpatricki* (TTU-P 9002) and *Tikisuchus* (ISI 305) but absent in aetosaurs, *Batrachotomus* (Gower 2002), *Saurosuchus* (Alcober 2000), and *Arizonasaurus* (MSM P4590). Parrish (1993) used this character to unite *Batrachotomus*, *Gracilisuchus*, *Postosuchus*, and *Dibothrosuchus*. Gower (2002) clearly showed that *Batrachotomus* lacks state (1) but explicitly stated that state (1) is not present in any crocodylomorph. Here, I disagree with Gower (2002) and hypothesize that the elongated parabasisphenoid of *Sphenosuchus* (SAM 3014) and *Dibothrosuchus* (IVPP V 7907) is homologous with that of *Postosuchus kirkpatricki* (TTU-P 9002) and *Tikisuchus* (ISI 305). These taxa all share a blind trough that is anteroventrally elongated in an identical way. The parabasisphenoid of *Gracilisuchus* (MCZ 4117), although elongated, is not ventrally elongated; therefore, it is scored as (0).

The orientation of the ilium has been used as a character since Bonaparte’s (1981, 1984) comparison of archosaur pelvises. Benton and Clark (1988) first used state (1) to unite aetosaurs and rauisuchians. However, Gower (2000) suggested that the scattered distribution of the character has made the homology of the character unclear.

The potential that *Silesaurus* may represent the sister taxon of Dinosauria renewed the spark in basal dinosaur studies because *Silesaurus* is nearly complete and well preserved; *Silesaurus* can help with character optimization at the base of Dinosauria, and it can help resolve the topology of contentious taxa. For example, the phylogenetic positions of controversial taxa (for example, *Herrerasaurus*, *Eoraptor*) are directly related to the diagnosis of Dinosauria (see Langer and Benton 2006) and character optimizations at the base of Dinosauria. These two problems are directly related to the outgroup of Dinosauria.

The presence of a predentary was long cited as a synapomorphy of Ornithischia (for example, Butler 2005; Butler et al. 2007; 2008b; Gauthier 1986; Norman 1984; Sereno 1986, 1999; ). The predentary is a separate, single ossification that lies anteriorly between the dentaries in ornithischians. Ferigolo and Langer (2007) argued that the edentulous “beak” of *Sacisaurus* and *Silesaurus* is homologous with the predentary of ornithischians. The authors

hypothesized that the predentary originated from the paired dentaries of archosaurs and cite examples of an independent origination of the structure on the anterior portion of the dentary in extinct birds and teleosts. *Sacisaurus* and *Silesaurus* do have an anterior dentary that is predentary-like, have an anteriorly tapering tip, and anterior portion of the dentary is edentulous. In *Sacisaurus* and *Silesaurus*, the anterior portion of the dentary is not a separate ossification separated from the rest of the dentary by a suture. The suture is not present on the exposed medial surfaces (Ferigolo and Langer 2007: fig. 3I). The suture reported by Ferigolo and Langer (2006) in MCN PV10061 does not extend to the ventral margin. This also is the case in MCN PV10042. In the largest specimen, MCN PV10041 (holotype), there is no trace of the suture (S.J.N., personal obs.). Instead, there is a foramen at the ventral extent of the hypothesized suture in the other dentaries. The preceding evidence suggests that there is no suture between the anterior portion of the dentary and the rest of the dentary. Therefore, *Silesaurus* and *Sacisaurus* are not scored as having a predentary. Furthermore, aetosaurs also have a similar anterior end of the dentary and it is clear that the tapering anterior end is composed solely of the dentary (Long and Murry 1995; Parker 2007).

Crocodylomorphs lack a distinct gap between the process and the glenoid. The dorsomedially projecting process is more elongated in crocodylomorphs relative to other taxa scored as (1). The process is hypothesized to be homologous between taxa scored as (1) and (2).

In non-archosaurian archosauriforms, the medial side of the articular lacks a foramen. The foramen is present posteromedial to the glenoid in *Euparkeria* (Ewer 1965), *Revueltosaurus* (PEFO 34561), and paracrocodylomorphs. In *Hesperosuchus “agilis”* (CM 29894), *Dromicosuchus* (UNC 15574), *Sphenosuchus* (SAM 3014), *Postosuchus kirkpatricki* (TTU-P 9000), *Postosuchus alisonae* (UNC 15575), *Batrachotomus* (SMNS 80260), and *Polonosuchus silesiacus* (ZPAL Ab III/563), the foramen has a large diameter relative to that of *Arizonasaurus* (MSM P4590), *Revueltosaurus* (PEFO 34561), and phytosaurs (USNM 18313). In basal crocodylomorphs, the foramen was termed the foramen aerum by Walker (1990), thus implying its homology with the pneumatic opening in crocodyliforms. However, as discussed by Gower (1999), the foramen in basal crocodylomorphs is not pneumatic. Therefore, the term foramen aerum should be abandoned for the structure in basal crocodylomorphs.

Sereno (1991a) cited this character as a synapomorphy of *Riojasuchus* and *Ornithosuchus* and remarked that phytosaurs have character state (1), but did not score it.

Parrish (1993) scored both *Leptosuchus* and *Prestosuchus* as having heterodont dentition. Additionally, Sereno et al. (1993) listed heterodont dentition as an autapomorphy of *Eoraptor*. Phytosaurs have markedly heterodont teeth (see Hungerbühler 2000). However, as noted by Hungerbühler (2000), the subjective term "heterodonty" to describe a set of teeth is *highly ambiguous*. Here, homodonty describes the general dentition of carnivorous teeth (recurved, serrated) of most archosauriforms and the herbivorous teeth of sauropodomorphs, ornithischians, aetosaurs, and *Revueltosaurus*. All these taxa have teeth that are generally similar. In contrast, the teeth of phytosaurs differ significantly depending on position (see Hungerbühler 2000). The two character states are general bins to separate phytosaurs (1) from other basal archosaurs (0). *Eoraptor* is scored as (0); the difference in the teeth lies in the direction and number of serrations per 5 mm, but the general form of the teeth is very similar.

Tooth serrations are absent in the two non-archosauriform archosauriforms (*Prolacerta*, *Mesosuchus*) used here. Within Archosauriformes, tooth serrations are present in nearly all clades *ancestrally*.

The tooth implantation of basal archosauriforms was discussed in great detail (Romer 1956; Hughe, 1963; Charig and Sues 1976; Gauthier, Kluge, and Rowe 1988; Benton and Clark 1988). *The terms thecodont and subthecodont were confused in the literature in reference to basal archosaur dentition, and both terms were used interchangeably to describe the same taxon and condition. Gauthier, Kluge, and Rowe (1988) first used the depth of the tooth sockets to score this character for basal archosauriforms. However, as explained by Juul (1994), the depth of the socket is difficult to determine and compare. The confusion of thecodont versus subthecodont dentition may be related to different authors' interpretations (Bennett 1996). However, there is a clear difference between the dentition of Prolacerta (BP/1/2675) and Proterosuchus (BP/1/3773), and Erythrosuchus+Archosauria. Here, ideas associated with thecodont versus subthecodont are abandoned. Instead, differences of how the base of each tooth attaches to the tooth-bearing element are explored.*

*The small size and the apparent absence of ossification of some of the intercentra in the column of Euparkeria may suggest that closely related taxa that have been scored as lacking intercentra, may indeed have very small intercentra. Euparkeria is scored as polymorphic for this character. Benton and Clark (1988) used the absence of intercentra to support the clade proterochampsians+Euparkeria+Archosauria (the crown group).*

Langer and Benton (2006) use a similar character (states reversed) to describe the cervical ribs of dinosauriforms. However, the plesiomorphic state of Langer and Benton (2006), short and directed posteroventrally, is not equivalent to state (0) employed here. *Nearly all the taxa scored in Langer and Benton (2006) as (0) and (1) would be scored as (0) here.* As described by Langer and Benton (2006), the cervical ribs of ornithischians are short and are scored as (1). Juul (1994) *incorrectly* states that *Lagerpeton* has slender cervical ribs; the cervical region of *Lagerpeton* is unknown.

The neural spines of the dorsal vertebrae of *Euparkeria* (SAM 6047B), phytosaurs (for example, *Smilosuchus*, USNM 18313), *Riojasuchus* (PVL 3827), *Revueltosaurus* (PEFO 34561), aetosaurs (for example, *Longosuchus*, TMM 31185–98), *Saurosuchus* (PVSJ 32), *Batrachotomus* (Gower and Schoch 2009), and *Fasolasuchus* (Bonaparte 1981) expand laterally at the distal end and form a flat surface. This morphology is periodically referred to as a spine table. In *Herrerasaurus* (Novas 1994), *Eoraptor* (PVSJ 512), and *Heterodontosaurus* (SAM-K-1332) the distal end of the neural spines of the dorsal vertebrae expand, but do not form a flat dorsal surface. *Here, the condition in dinosaurs is not considered homologous to that of state (1), but is instead scored as (2).*

*Given this disparity, only taxa with relatively complete tails can be scored as (0) whereas taxa with any caudal vertebrae with any form of anterior laminar projections are scored as (1).*

The ratio of the length of the forelimb to that of the hind limb has been used repeatedly in most phylogenetic analyses of basal archosaurs. Gauthier (1984) first used this character as an ornithodiran synapomorphy, but stated that avian-line archosaurs have a forelimb–hind limb ratio of 0.5. *Gauthier (1984) considered pterosaurs as having a ratio less than 0.5 and thus excluded the elongated manus whereas Sereno (1991a) scored pterosaurs as having a ratio greater than 0.5. Benton (1999) later changed the ratio to greater or less than 0.55. None of the authors ever defined what was measured in both the hind limb and the forelimb. It was assumed by later authors that the humerus to the distal end of the longest ungual was measured for the forelimb and the femur to the ungual of the longest pedal digit for the hind limb. However, many of the taxa that were scored lack manus material.*

Interclavicle: (0) present; (1) absent (fig. 30) (Benton 1999; Gauthier 1986; Juul 1994; Sereno 1991a). The interclavicle is present in archosauriforms plesiomorphically (Sereno 1991a) and persists through Pseudosuchia. In Pterosauria, an interclavicle appears to be present

in young individuals of *Eudimorphodon* (MCSNB 8950), but fuse to the pectoral elements in larger individuals (Wild 1993). A distinct interclavicle is not present in all other pterosaurs. Ornithischians and saurischians lack an interclavicle. *However, the pectoral girdles in the successive sister taxa to Dinosauria* (Silesaurus, Marasuchus, Lagerpeton) *do not have the pectoral region completely preserved. As a result, the optimization of this character within Dinosauromorpha is not clear.*

*The condition in Batrachotomus is not known because all the anterior portions of the coracoids are broken and reconstructed (contra Parrish 1993).*

In most basal archosauriforms, the proximal portions of the metacarpals overlap each other. In this configuration, the contacting surfaces of the metacarpals are imbricated laterally where metacarpal I lies on the anterior/dorsal surface of metacarpal II. Clark et al. (2000), followed by later studies using the same dataset (Clark et al. 2004), scored CM29894, Saltoposuchus, and Dibothrosuchus as having abutting metacarpals. *However, in these taxa, the metacarpals are imbricated as in Alligator.*

The distal carpals are proportionate to the size of the proximal portion of its metacarpal. Langer and Benton (2006) argue that the enlarged carpal 1 of sauropodomorphs and theropods is homologous. Sauropodomorphs (for example, *Massospondylus* BP/1/4934) do have an enlarged carpal 1, but this is proportionate with the enlargement of metacarpal I relative to basal members of Dinosauria. In theropods (*Coelophysis*, AMNH FR unnumbered and *Allosaurus* DINO 11541), the carpal capping metacarpal one also caps metacarpal II. This is not the case in sauropodomorphs were [sic] a fully formed carpal 2 lies on top of metacarpal II. *Here, I suggest that only theropods can be scored as (1). Langer and Benton's (2006) scoring of this character should not be a eusaurichian synapomorphy and, as a result, this character would not be a character excluding Herrerasaurus from Eusaurischia.*

Nevertheless, Sereno's (1999) character states and scorings are retained here even though this character needs further revision.

As a character measuring the orientation of an element, *taphonomic effects should be carefully considered.*

The taxon is tentatively scored as (1) given that the pubis is incomplete and a relative size of the pubis compared to the size of the obturator foramen cannot be fully assessed.

Wu and Chatterjee (1993) scored *Hesperosuchus*, *Saltoposuchus*, *Protosuchus*, and *Alligator* as having a bulge, but considered it different than the condition in *Postosuchus*. They never tested the homology of the conditions in *Postosuchus* and crocodylomorphs.

Furthermore, *there is no evidence for either Protosuchus or Alligator having a knob at the distal end of the pubis.*

They observe a slight bulge in *Plateosaurus*, *Eoraptor*, and *Saturnalia* and score this expansion as homologous to that of *Herrerasaurus* and neotheropods whereas other workers do not consider these taxa to have any expansion (for example, Gauthier 1986; Rauhut 2003).

Therefore, the divergent morphology of the expansion in *Herrerasaurus* should be considered an autapomorphy of the taxon, but the presence of the distal expansion *should be considered homologous to that of neotheropods when scored.*

Juul (1994) warned that *taphonomic distortion of femora may affect the scoring of taxa.* Therefore, only well-preserved femora are scored here. *New specimens and a reevaluation of well-preserved basal archosauriform femora have allowed more taxa to be scored accurately.*

Therefore, the presence of a sharp ridge or a rounded ridge may be ontogenetically developed. *Nonetheless, the character is still used and taxa showing both states (1) and (2) are scored as polymorphic.*

Langer and Benton (2006) scored ornithischians and theropods as (0), and *Herrerasaurus* (PVSJ 373) and sauropodomorphs as (1). *I agree with their scoring here.*

*I agree with Sereno (1991a) in his thorough discussion of the character and most scoring decisions except for phytosaurs.*

*Sereno (1991a), followed by others* (for example, Juul 1994), scored phytosaurs as having a state similar to (1) here (5 flexed). *Parrish (1993) disagreed with Sereno (1991a) and concluded that phytosaurs have a concave surface. My observations of the tibial facet of the astragalus of Smilosuchus (USNM 18313) and Pseudopalatus (UCMP 27235) agree with the observations of Parrish (1993) and I score phytosaurs as concave. Further, the distal surface of the tibia of phytosaurs is convex in phytosaurs. That said, I have observed a specimen of a phytosaur (TMM 31100-466, possibly Paleorhinus or Angistorhinus) from the Otis Chalk quarries that has a weakly flexed tibial facet (see discussion below).*

In *Euparkeria*, the extent of the posterior deflection of the tuber has been debated. Gower (1996) suggested that the tuber is nearly lateral whereas Sereno (1991a) and Parrish (1993) claimed that the orientation is near 45°. Articulation of the ankle of *Euparkeria* (UMCZ T692) indicates that the orientation of the tuber must have been posteriorly at least 20°, and this is supported by articulated examples (SAM 5867). *The exact range cannot be determined but here it is scored as (1).*



Hence, ornithosuchids are scored as (0) because I cannot separate the condition in *Riojasuchus* (PVL 3827) from that of *Euparkeria* (UMCZ T692) (contra Sereno 1991a). This character is not present in any specimen of *Euparkeria*. In criticism of Sereno (1991a), Parrish (1993) reported that this character was present in *Euparkeria* and unknown in proterochampsians. However, *Parrish (1993) was mistaken in his interpretation of Sereno's (1991) character.*

In nearly all archosauriforms, five phalanges are present on pedal digit IV. In the crocodylomorphs, *Terrestriusuchus* (BMNH R 7557), *Protosuchus* (AMNH FR 3024), and *Alligator* there are four or fewer phalanges on pedal digit IV. *The only known specimen of Pseudhesperosuchus (PVL 3830) was scored as (1) by Parrish (1993); however, the pes of Pseudhesperosuchus is unknown.*

Sereno (1991a) discusses this character in depth and concluded that all dinosauromorphs have state (1) and that all other archosauriforms are scored as (0). *His conclusions are followed here except for a few exceptions.*

*Parrish (1993) scored Gracilisuchus as having a tapering metatarsal V; however, metatarsal V clearly does not taper and bears a fully developed first phalanx in specimen PVL 4597.*

The osteoderms of *Ticinosuchus* (PIZ T2817), *Saurosuchus* (PVSJ 32), and *Prestosuchus* (UFRGS 156-T, UFRGS 0152-T) *seem to be flat ventrally even though they have been scored as (1) by Benton and Walker (2002).*

Basal archosaurs lie at the critical junction between phylogenetic datasets examining crocodylomorph relationships (for example, dataset of Clark et al. 2000; Pol et al. 2009), basal dinosaur relationships (for example, Langer and Benton 2006), and those of non-archosaurian archosauriforms (for example, Gower and Sitnikova 1997). As a result, the phylogenetic positions of the outgroup or taxa at the base of the tree in studies examining crocodylomorphs and basal dinosaurs are usually based on *a priori assumptions* and the relationships are not tested in a broad context. Some authors (for example, Benton and Clark 1998; Juul 1994; Sereno 1991a) have attempted to include many basal archosaur groups and were greeted with some success, but with poor support for many of the relationships. Therefore, *the dataset presented here fuses the datasets of basal archosaur (for example, Juul 1994), non-archosaurian archosauriforms (for example, Gower and Sennikov 1997), basal crocodylomorphs (for example, Clark et al. 2004), and basal dinosaurs (Langer and Benton 2006).*

The inclusion of characters from the various basal archosaur studies designed to support the relationships of less inclusive clades (for example,

Dinosauria) required the addition and modification of the character states and modification of the characters themselves. *I relied on the phylogenetic analyses of Bennett (1996), Benton (1999, 2004), Benton and Clark (1988), Clark et al. (2000, 2004), Gauthier (1984, 1986), Gower (2002), Gower and Sennikov (1996, 1997), Irmis et al. (2007a), Juul (1994), Langer and Benton (2006), Novas (1996), Parrish (1993), Rauhut (2003), and Sereno (1991a) as a base of characters. Other sources were also used and cited where appropriate. As a consequence of the large number of datasets spanning hundreds of taxa, I have revised many previously used characters and have excluded others. Moreover, I have added nearly 100 new characters. In the end, the dataset includes 412 discrete characters sampled from all over the skeleton.*

Of the hundreds of previously reported basal archosauriform characters available from the literature, most of the characters were incorporated into my analysis. *Modification of the characters ranges from accepting the original formulation (for example, postfrontal: [0] present or [1] absent) to a complete revision (for example, rewritten, divided into two or more characters, combined with other characters).* In most cases, revisions were completed to incorporate a greater diversity of morphologies whereas clarity and specificity were added to others.

The much-welcomed revisions of basal archosaur characters previous to 1993 by Juul (1994) and the assessment of Sereno (1991a) by Parrish (1993) are generally accepted here unless noted in the character descriptions. However, since these two revisions, some characters were not included here for four main reasons: (1) *the character was just listed with no, little, or vague explanations;* (2) *problems with interpretation of the morphology;* (3) *characters describing complex shapes of cranial openings;* and (4) *characters focusing on features that are related to taphonomy instead of actual morphology.* Examples of these four categories are shown below: (1) *Characters just listed with no, little, or vague explanations: Fibula nontapering and calcaneum unreduced (0), or thin, tapered fibula and reduced calcaneum (Juul 1994: character 49).*

*The formulation of these kinds of characters are problematic because (1) it uses vague terms such as nontapered and reduced and (2) it combines the morphology of the fibula and calcaneum together even though they are possibly two discrete changes.* The vague terms, in this situation, cannot be assessed for taxa not included in the analysis of Juul (1994). The author's intentions for this character cannot be understood; why some archosaurs are scored as (1) whereas other are scored as (0) is not explained.

Other examples of characters listed with no, little, or vague explanations: Parasphenoid rostum rodlike (0) or a dorsoventrally expanded wedge (1) (Parrish 1993: character 28); Anterior parts of lateral sides of the dorsal vertebrae smoothly convex (0) or constricted (1) (Parrish 1993: character 19); (2) Problems with interpretation of the morphology: Metacarpal IV and V bases (0) lie more or less in the same plane as the inner metacarpals or (1) lie on palmar surfaces of manual digits III and IV respectively (Gauthier 1986); As far as I have observed, metacarpals always lie in an arc and never in a single plane (for example, *Alligator* versus *Herrerasaurus*, PVSJ 373). Moreover, I have not observed any cases where the base of metacarpal IV lies on the palmar surface of metacarpal III and the base of metacarpal V lies on the palmar surface of metacarpal IV. Therefore, this character is excluded.

Other examples of problematic characters describing complex shapes of cranial openings: Intramandibular joint: absent or poorly developed (0), well developed (1) (Juul 1994: character 73); Jugal-lacrimal articular relation: (0) lacrimal laterally overlaps jugal or (1) jugal laterally overlaps lacrimal (Sereno and Novas 1994: character 16). (3) Characters describing complex shapes of cranial openings: Lower temporal fenestra shape: nontriangular (0), or triangular and reduced in size (1) (Benton and Clark 1988: character two supporting Suchia). The complex morphology of skull openings is nearly always the product of two or more skull elements and therefore, the morphology of the particular elements can be turned into characters. This better represents statements of homology. In the example above from Benton and Clark (1988), modifications in either the squamosal or the quadratojugal can make a triangular lower temporal fenestra.

Other examples of a problematic characters [sic] describing complex shapes of cranial openings: Antorbital fenestra shape elliptical or circular (0), triangular, and with elongate narrow anterior point (Benton and Walker 2002: character 38). (4) Characters focusing on features that are related to taphonomy instead of actual morphology: Prominent rim encircling the distal end of the calcaneum tuber absent (0) or present (1) (Parrish 1993: character 33). The prominent rim described by Parrish (1993) is a consequence of the transition from compact bone to unfinished bone on the distal end of the calcaneum. This transition occurs in all nonarchosaurian archosauriforms as well as most crocodylian-line archosaurs examined by me. However, the “prominent rim” is much more noticeable in well-preserved specimens compared to specimens that are not well prepared, incomplete, or poorly preserved. For example, Parrish (1993) scored *Prestosuchus* as

(0) even though a clear “rim” transition from compact to a more spongy bone is present (Parrish 1993: fig. 3B).

Another example of a similar problematic character: Two prominent ridges on the anterior face of the distal end of the fibula absent (0) or present (1) (Parrish 1993: character 34).

The new characters derive from all over the skeleton. Many of the new characters stem from parts of the skeleton poorly sampled before. For example, I sampled characters from the ulna and radius whereas ulna and radius characters were previously used only among dinosaurs. The elements of the posterior portion of the skull have also been sparsely represented in previous basal archosaur datasets. Furthermore, I converted a number of observations (for example, Nesbitt 2005a) into phylogenetic characters. This includes observations of the femur and the osteoderms. Gower (2002) described 27 basal archosauriform characters from the braincase, a region that had only a handful of characters previously. Moreover, I added 20 more braincase characters (new and from other datasets). Thus, braincase characters consist of 11% of the total number of characters, a tenfold increase. This has been a direct result of the work published in numerous papers by David Gower and the increase of well-prepared specimens and newly discovered taxa. Characters originally used exclusively for theropods also support clades such as Crocodylomorpha—these characters are essentially new for basal archosaurs. For example, Rauhut (2003; character 29) uses the height of the lacrimal stretching from the bottom of the orbit to the top as a theropod character; the same character state is also a character basal crocodylomorphs. Essentially, many of the new characters stem from the discovery of new taxa, which, in turn, calls for a critical reevaluation of previously examined taxa.

The support for phytosaurs as pseudosuchians in previous analyses was weak and relied mainly on ankle character states, characters that sometimes cannot be scored in basal avian-line archosaurs.

The position of *Gracilisuchus* near *Postosuchus* and Crocodylomorpha obtained by Parrish (1993) and Juul (1994) is supported by a number of incorrectly scored characters. These characters include the mis-scoring of the morphology of the fifth metatarsal of *Gracilisuchus*; the fifth metatarsal is long and possesses at least one phalanx (PVL 4597), whereas the fifth metatarsal was scored as tapered and lacking any phalanges by Parrish (1993) and Juul (1994). *Gracilisuchus* does not have a dorsoventrally aligned groove on the distal end of the calcaneum tuber whereas Juul (1994) scored the groove as present.

My results place *Gracilisuchus* in an unresolved polytomy with *Revueltosaurus* +

Aetosauria, *Turfanosuchus*, and *Ticinosuchus* + *Paracrocodylomorpha* at the base of *Suchia*. *Gracilisuchus* is found in a variety of positions among basal suchians (fig. 54). This analysis confirms that *Gracilisuchus* is more closely related to *Crocodylomorpha* than phytosaurs are related to *Crocodylomorpha* and *Gracilisuchus*, and is at the base of *Suchia*. *If Turfanosuchus is removed from my analysis Gracilisuchus becomes the sister taxon to Ornithosuchidae* (fig. 54). And it is followed by... The phylogenetic position of *Gracilisuchus* at the base of *Suchia* is very important for resolving the relationships of pseudosuchians.

Even though a nearly complete skeleton of *Ticinosuchus* is known, only 49% of my characters could be scored for this taxon. *The phylogenetic position of Ticinosuchus as the sister-taxon to Paracrocodylomorpha generally agrees with the topology found in previous analyses (for example, Parrish 1993). However, it takes only two steps to place Ticinosuchus within Paracrocodylomorpha.*

*Prestosuchus* was one of the first "rauisuchians" to be described. Huene (1942) named two species of the taxon, *P. chiniquensis* and *P. loricatus*, from material collected from the same region in the Santa Maria Formation. Here, I scored only the more complete holotype of *P. chiniquensis*. Furthermore, Barberena (1978) assigned a complete skull and nearly complete presacral vertebral column (UFRGS 0156-T) to *P. chiniquensis*. *Parrish (1993) recognized that the holotype of P. chiniquensis and UFRGS 0156-T may not necessarily represent the same taxon and scored them as separate terminal taxa.*

Ferigolo and Langer (2007) and Dzik and Sulej (2007) hypothesized a close relationship between *Silesaurus* and ornithischians and cited potential synapomorphies between the two taxa. Here, the placement of *Silesaurus* as more closely related to ornithischians that [sic] to any other taxon requires an addition of a minimum of 11 steps to the tree. Furthermore, nearly all of the classic dinosaur synapomorphies (for example, open acetabulum, see below) become homoplasies among the three major dinosaur lineages. However, all the characters cited that do not occur in a variety of dinosauriforms (for example, increasing the number of sacral vertebrae) are restricted to the dentition and mandible characters. The acquisition of ornithischian-like dentition and a "beak" evolved by the Middle Triassic (for example, *Asilisaurus kongwe*), and these character states were retained through much of the Triassic.

The resolution of the sister taxon of Archosauria remains controversial. Both proterochampsians (Benton 1999, 2004; Juul 1994; Parrish 1993; Sereno 1991a) and *Euparkeria* (Benton and Clark

1988) were found as the sister taxon to Archosauria. However, *Proterochampsia* was always scored as a suprageneric taxon, and it is not clear which proterochampsian taxa were scored.

Benton and Clark (1988) were the first to use the presence of a slitlike fenestra/foramen between the maxilla and premaxilla to diagnose the clade *Rauisuchidae* in a phylogenetic context. Parrish (1993) and other basal archosaur workers also termed the opening of the *subnarial foramen* in their character lists. The differences in the shape of the gap between the maxilla and the premaxilla, *the scoring inconsistencies* among the various authors, and the scant distribution of an opening between the maxilla and premaxilla in various archosaurs (see Juul 1994; Gower 2000) led Gower (2000) to question the validity of this character as a synapomorphy of a clade of "rauisuchians."

The palatal processes of the maxillae of crocodylians meet at the midline and expand posteriorly to form an extensive secondary palate (Brochu 2003). Parrish (1993) scored basal crocodylomorphs as having a "secondary palate." Scoring a "secondary palate" in basal archosaurs is difficult given the range of morphologies between that of basal crocodylomorphs and those of crocodylians. Therefore, the ambiguous term "secondary palate" is abandoned for this character state and the actual morphology is described.

Slight disarticulation of the exoccipitals can hinder scoring of this character.

I disagree with Gower (2002) for scoring *Batrachotomus* as (0). The condition in *Batrachotomus* is unknown because the external foramina for passage of the abducens nerve cannot be located with confidence.

Here, I do not agree with all the scoring of this character by Langer and Benton (2006). They score nearly all saurischians as having state (1).

Here, I follow the conclusions and scorings of Langer and Benton (2006).

The presence of a crest dorsal to the supraacetabular crest (5 rim) has been repeatedly cited as a character uniting various suchian taxa (see review of Gower 2000) especially taxa traditionally regarded as rauisuchians. The various descriptors (buttress, swelling, supraacetabular crest, rugose ridge) of this feature have led to confusion because they (1) are never described using specific taxa, (2) are vague and later authors have confused the terms when scoring matrices, and (3) only one of them incorporated a wide range of variation.

Langer and Benton (2006) recently reviewed the location of the lateral condyle relative to the medial condyle of the tibia in basal dinosaurs. *For the most part, I agree with their conclusions and scorings.*

The existence of a clade of basal dinosauromorphs outside Dinosauria significantly *changes the view of* early dinosauromorph *evolution*.

Character states that *I view as either too simplistic or representing several changes* are divided into two or more characters.

Among suchians, *scoring of this character has been inconsistent in the literature*.

This character is *difficult to score*, and the *scoring strategy* implemented by Parrish (1993) *could not be reproduced*.

Osteoderm sculpture present (Benton 1999). The *scoring* of this character *cannot be reconciled* with the scoring strategy of Benton (1999) and previous authors (Parrish 1993).

Movable joint between the maxilla and the premaxilla (Benton and Clark 1988; Juul 1994). I did not use this character in my phylogenetic analysis. The definition of *a movable joint is unclear*, and I *could not reproduce the scoring strategy* of either Benton and Clark (1988) or Juul (1994). Moreover, the character state *describes an inferred function rather than morphology*.

However, *I disagree with the scoring* of perilymphatic foramen as posterolateral in *Stagonolepis*. I did not see any difference in the orientation of the ventral process of the opisthotic of *Stagonolepis* and that of *Postosuchus*, *Arizonasaurus*, phytosaurs, *Turfanosuchus*, *Euparkeria*, or avian-line archosaurs (see above also).

*Scoring differences between* this analysis and Gower (2002) and Gower and Walker (2002) in this character prevent it from uniting aetosaurs and crocodylomorphs.

Unfortunately, recent authors *did not provide detailed character descriptions or rationale for scoring strategies* as did Sereno (1991a), Juul (1994), and Bennett (1996). *This led to heavily recycled characters, sometimes compounding scoring errors from the original matrices*. Few, if any, new characters have been added to these analyses. For example, Benton's (1999) character list consisted only of characters previously used in the literature. Benton (2004) and Nesbitt and Norell (2006) added taxa to Benton (1999), *yet the relationships of pseudosuchians changed easily when new taxa and characters were added* (see fig. 2).

Gower (2002) found that the medial wall of the vestibule is completely ossified in nearly all suchians. Later, Gower and Nesbitt (2006) scored *Arizonasaurus* as (0). However, *after a careful inspection* of taxa with completely ossified medial walls of the vestibule, it is clear that the larger specimen of *Arizonasaurus* (MSM P4647) has a completely ossified medial wall. Therefore, *Arizonasaurus* is scored as (1) here.

The two reasons that Parrish (1993) discussed for dismissing Sereno's (1991a) character are welcomed, but oversimplified. The exceptions listed by Parrish (1993), as well as Sereno (1991a) in the original formulation can also be *interpreted* as autapomorphies.

A small spur of bone present on the posterior side of the calcaneum of *Marasuchus* was interpreted as homologous with the calcaneal tuber of suchians by Sereno (1991a), *and I follow this interpretation here*.

I have *interpreted* the elongated slit between the premaxilla and the maxilla *Saurosuchus* as a *taphonomic artifact* (see above).

The lateral ridge in taxa scored as (1) and (2) is present at the *same location* as the change of angle in taxa scored as (0).

The element that was identified as the pubes in the holotype (FMNH 357) is actually the ischium; therefore, the pubis is not represented in the holotype material. The element that was once identified at the pubis bears a large distal expansion (5 pubic boot), and *this expansion has greatly influenced the interpretation of its relationships* in older (Colbert 1961) and more recent (Weinbaum and Hungerbühler 2007) studies.

Many basal archosaur workers previously used this character without discussion. The original wording of the character "antorbital fenestra shape: elliptical or circular (0), triangular, and with elongate narrow anterior point" *is open to interpretation and difficult to score*.

As described by Gauthier (1986) and Rauhut (2003), metacarpal IV is either highly reduced or absent in tetanurans (see Xu, Zheng, and You 2009, *for a different interpretation*).

The following description corrects the *mistake* in Nesbitt et al. (2009a).

Note: The reconstruction of the midline contact between the ischia in *Silesaurus* is *incorrect* in the reconstruction of Dzik (2003, fig. 12E).

The two taxa overlap in only one element, the tibia. In retrospect, the tibia of *Lewisuchus* (UNLR 01; Romer 1972d, fig. 8, *incorrectly* identified as the femur) bears a strong cnemial crest like that of other dinosauriforms, specifically *Pseudolagosuchus*.

Furthermore, the explicit phylogeny allows the identification of homoplastic characters that may have been previously used to identify fragmentary fossils *incorrectly*.

I examined the skull region very carefully and have a few comments. Unfortunately, the blocks with skull elements appear to have been reassembled *incorrectly*. The skull must have been split longitudinally when collected, and a portion of the right side was placed incorrectly posterior to the left side of the skull (see fig. 9). This is apparent

because the specimens from the "Grenzbitumen" horizon were collected in pieces, then reassembled later (Furrer, personal comm.). The medial surface of the right maxilla is exposed laterally, and there is a large gap between the anterior and posterior portions of the skull. Once this *incorrectly placed piece is removed and the anterior and posterior portions of the skull are brought back together, the skull becomes much shorter* (see fig. 9).

This character was *incorrectly* identified as the posteroventral portion of the dentary laterally overlaps the anteroventral portion of the *surangular* in Nesbitt et al. (2009a).

Colbert (1952) *made a few errors in the identification of elements in his description*, but Walker (1970) corrected these mistakes. For example, the "pterygoid" (Colbert 1952: fig. 9) is actually a sacral rib from the first primordial sacral.

The postfrontal has been scored as absent in *Erpetosuchus* by both Benton and Walker (2002) and Olsen et al. (2000). However, the specimen scored (BMNH R3139) by Benton and Walker (2002) *is preserved as a mold; thus, sutures are extraordinarily difficult to discern*.

Therefore, the hypothesis that *Heptasuchus* represents the skull of *P. gracilis* is *rejected here*.

Gower (2002) *hypothesized* that this is intermediate between character state (0) and (1), and therefore ordered the character.

Sereno (1991a; character 20) *hypothesized* that the strongly mediolaterally compressed unguals in *Ornithosuchus* and *Riojasuchus* *represented a synapomorphy uniting the two taxa*.

Both Parrish (1993) and Juul (1994) *hypothesized* that the laterally projecting flange of the squamosal in *Gracilisuchus* is homologous with that of *Postosuchus kirkpatricki* and crocodylomorphs. I agreed with this potential homology and scored the conditions in *Gracilisuchus* and crocodylomorphs as homologous. *However, it is found as convergent in this analysis*.

Gower (2002) and Gower and Walker (2002) *hypothesized* that having a vertical ridge on the lateral side of the exoccipital and having both hypoglossal foramina posterior to this ridge united aetosaurs and crocodylomorphs exclusive of other clades of pseudosuchians. However, the addition of more archosaurian taxa, specifically avian-line archosaurs, *complicated* Gower's (2002) character definition and states (character 2 of Gower 2002). Therefore, *the character was rewritten* to incorporate the large range of morphology here.

Additionally, Nesbitt et al. (2007) *hypothesized* that *Pseudolagosuchus* from the Middle Triassic of Argentina and *Technosaurus* from the Late Triassic of Texas *may be close relatives to Silesaurus*.

The taxon is *tentatively* scored as (1) given that the pubis is incomplete and a relative size of the pubis compared to the size of the obturator foramen cannot be fully assessed.

Most workers cite an "offset femoral head" as unique dinosaurian character. However, it is *unclear* which features form the "offset femoral head." As I *understand*, the "offset femoral head" is an enlarged tuber on the proximal portion of the femur that articulates into the acetabulum. Others have also added a direction as a component to the "offset femoral head." *Here, I consider* the direction of the long axis of the femoral head a separate character (see below).

Langer and Benton (2006) argued that these character states are predatory features and *should* be viewed as convergences.

I *argue* that the two examples of a slit-like gap between the maxilla and premaxilla in both *Luperosuchus* (UNLR 4) and the holotype of *Saurosuchus galilei* (PVL 2062) *are taphonomic features* resulting from disarticulation and preservation rather than real morphology.

*Furthermore, it is difficult to argue* that the restricted number of tarsals, the size of the distal tarsals, and the shape of the proximal tarsals in pterosaurs would be convergent with those of dinosauromorphs *based on function alone* (Sereno 1991a).

One of the most surprising results of this study is *the inferred convergence* between both *Silesaurus* and ornithischians and *Silesaurus* and theropods.

This study *has largely been driven* by the discovery of new basal archosauriform taxa in the last 10 years and a *renewed interest in the early evolution of Archosauria*.

Although sampling is far from complete in both Sauropodomorpha and Theropoda, *the selected taxa are chosen because they were found to be basal in all phylogenetic analyses* of basal Dinosauria (for example, Langer and Benton 2006; Rauhut 2003; Yates 2007).

*The goal of this analysis* was to place a wide breadth of basal archosauriforms into a phylogenetic context.

**Quotes from Turner et al. (2012)** [The cited references are listed in the original paper.]

*Given this interpretation*, the oblique ridge serves a role similar to that of the "vertical ridge" present in tyrannosauroid ilium—namely the division of the preacetabular concavity from the postacetabular concavity. Here *I follow the conclusion* of Hutchinson (2001a) and do not consider the "vertical ridge" and the oblique ridge below the supratrochanteric process homologous.

Complicating matters is that the structure labeled as the maxillary fenestra in the line interpretation of the holotype skull is a fractured portion of the underlying right maxilla—not the structure present in the line drawing that corresponds to the maxillary fenestra in the authors' reconstruction of the skull.

This feature was previously interpreted as convergently shared between unenlagiine dromaeosaurids and troodontids but currently optimizes at Deinonychosauria.

This suggests that the correct interpretation of the *Coelurus* carpal in question is that it is a distal carpal I.

Reexamination of the holotype material for the present study resulted in 102 changes to the taxon's scoring for existing characters plus numerous additional observations (table 3). This added information is critical for testing whether *Adasaurus* is in fact a dromaeosaurine, a velociraptorine, or a stem taxon.

According to Witmer (1990), this structure may be an extension from the caudal tympanic recess, although it has been interpreted as the main part of the caudal tympanic recess by some previous authors.

The exact phylogenetic position of the recently described four-winged paravian *Anchiornis*, which has variably been interpreted as either an avialan or a troodontid (Xu, Zheng, and You 2009) has great significance for understanding the evolution of aerial locomotion in paravians and ultimately for the origin of avian flight.

Early analyses of these morphological modifications and/or coelurosaur phylogeny placed these changes at the avialan node (Jenkins 1993; Sereno 1997 1999), but it is now clear that they are paravian synapomorphies.

The character-scoring changes for *Adasaurus mongoliensis* resulted in repositioning of it as a derived velociraptorine as opposed to the dromaeosaurine position recovered in previous analyses (for example, Turner et al. 2007a, 2007b).

A reanalysis of the matrix combining changes in the scoring of this trait along with those in character 322 culminates in a strict consensus tree in which the position of Troodontidae, Avialae (without *Archaeopteryx*), Dromaeosauridae, and the Archaeopterygidae clade found by Xu et al. (2011) are fluid.

Additionally, it is unclear whether phalanx II-2 has an elongate flexor heel as would be expected for a dromaeosaurid of this size. It is our opinion that this taxon cannot be confidently referred to Dromaeosauridae and should be considered Deinonychosauria incertae sedis.

The position of *Saurornitholestes* was labile and therefore unclear whether it belonged to

Velociraptorine. Similar resolution and membership was found by Makovicky et al. (2005), Norell et al. (2006), and Turner et al. (2007a, 2007b).

For instance, with *Albertosaurus libratus* and *Dilong paradoxus* both showing state 0, it is unclear whether state 1 is autapomorphic for *Tyrannosaurus rex* or it diagnoses a more restricted clade of tyrannosaurids.

A few inconsistencies exist in the reconstruction of the skull. The quadratojugal was reconstructed as not contacting the squamosal. It was specified in the text, however, that it was unclear whether contact between these elements existed (Xu and Wu 2001: 1745).

Xu et al. (2010) provided eleven putative autapomorphies diagnosing *Linheraptor exquisitus*. This taxon is strikingly similar to the contemporaneous *Tsaagan mangas* from the Ukhaa Tolgod locality, Djadokhta Formation of Mongolia. The autapomorphies for *Linheraptor* were provided to distinguish it from *Tsaagan*. A review of these features reveals that they fail to differentiate these two taxa.

The last three putative synapomorphies listed by Liu et al. (2004) likewise are not sufficient to distinguish *S. haoiana* from *S. millenii*.

However, the robustness of this putative synapomorphy is highly suspect. Of the four taxa, only *Deinonychus* is scored as possessing character state 25-0—it is coded as 25-1 in *Achillobator* and as unknown (?) in *Dromaeosaurus* and *Utahraptor*. However, the known specimen of *Achillobator* lacks a dentary, so this character state cannot be considered anything other than uncertain (?). This leaves *Deinonychus* as the only taxon in the group to exhibit the putative synapomorphy. Therefore, it is equally parsimonious to interpret a straight dentary as autapomorphic for *Deinonychus*, with this conclusion the only one supported by the current data.

Therefore, moving the phylogenetic position of *Epidexipteryx* among paravian and the closely related oviraptorosaurs requires accepting only slightly less parsimonious topologies. The great similarity that exists among basal paravians, basal oviraptorosaurs, and *Epidexipteryx* leads us to caution that the precise phylogenetic position of *Epidexipteryx* requires additional work to understand the interesting, and highly derived, anatomy of this taxon as well as a better understanding of the character changes taking place near the split between oviraptorosaurs and paravians.

Colbert and Russell (1969) gave *Dromaeosaurus albertensis* a more thorough and complete description while Currie (1995), after reparation of the holotype and CT analysis, was able to add new

information on cranial and braincase morphology and improve upon the *incorrect reconstruction* of the skull by Colbert and Russell (1969).

The scoring of the latter *trait was based* on the reconstruction offered by Ostrom (1974), *but further examination* of MCZ 4371 indicates *the pubis was more reverted than in Ostrom's reconstruction*.

Because *early phylogenetic analyses* of coelurosaur relationships *considered* Dromaeosauridae at a supraspecific level (Holtz 1998; Sereno 1997, 1999), interrelationships among dromaeosaurids *were not considered, and so content of and/or monophyly of the Dromaeosaurinae/Velociraptorinae dichotomy were not tested*. Early analyses utilizing the versions of the TWiG matrix recovered little or no consensus on dromaeosaurid interrelationships (for example, Hwang et al. 2002; Makovicky et al. 2003; Norell et al. 2001) (fig. 3). Later analyses revealed that underlying structure existed within Dromaeosauridae by increasing character sampling (Makovicky et al. 2005) or looking at reduced strict and Adams consensus (Norell et al. 2006; Novas and Pol 2005; Turner et al. 2007a). These analyses showed that distinct Dromaeosaurinae and Velociraptorinae clades exist.

Currie (1995) considered *Saurornitholestes* as a close *Velociraptor* relative. Makovicky et al. (2003) and Novas and Pol (2005) found the phylogenetic position of *Saurornitholestes langstoni* to be very *labile* among dromaeosaurids more derived than *Sinornithosaurus* and *Microraptor*. Norell et al. (2006) and Turner et al. (2007a) found *Saurornitholestes* in an unresolved position but still more derived than microraptorines.

Review of the Xu et al. (2011) matrix reveals some character scorings that *we dispute*.

Although Xu et al. (2011) *describe and figure* a ventrolateral flange or heel on pedal phalanx II-2 of *Xiaotingia*, *they score it as absent* in their matrix.

Based on our own study of the relevant specimens, *we disagree* with these scorings and instead code *Xiaotingia*, *Mei*, *Sinovenator*, *Microraptor*, *Unenlagia*, and *Sinovenator* as having state 2.

Although we still discover *Archaeopteryx* as closer to deinonychosaurs than to Avialae, *this result draws into doubt* Xu et al.'s (2011) contention that their result was *predicated entirely on the inclusion of Xiaotingia*.

Another trait, whose scoring *we find contentious*, is their character 366 that describes the relative position of the postorbital process of the jugal along the length of that bone. Xu et al. (2011) score the process as close to the middle of the bone (state 0) in *Epidexipteryx*, which *we agree with, but they go on to consider* the basal avialans *Jeholornis*, *Sapeornis*, and *Confuciusornis* as well as the unenlagiine *Buitreraptor* as sharing this condition.

Overall, the results *conflict* less with those from the unweighted parsimony analysis than those found by model-based analysis, although a couple of taxa with *copious missing data* (*Hagryphus*, *Haplocheirus*) exhibited *anomalous affinities*.

Rauhut (2003) scored *Compsognathus longipes* as "0/1" because of *ambiguity*. Examination of MNHNCNJ 79 clarifies this scoring as state 0. State 1 was retained for future use do to bluntly squared anterior rami of jugals in basal theropods like *Coelophysis* and *Liliensternus liliensterni* (Rauhut 2003, 53).

A final *dataset* of 474 characters scored for 111 taxa *was used to address paravian evolution*. This *dataset* is important in that it *bridges a phylogenetic gap that had persisted between studies on birds and studies on all other coelurosaurs*.

We provide the most detailed and comprehensive *phylogenetic analysis* of paravians to date *in order to explore the phylogenetic history* of dromaeosaurid taxa.

Our study reviews and revises the membership of Dromaeosauridae and provides an *apomorphy-based diagnosis* for all valid taxa.

The general pattern of paravian relationships is explored within the broader context of Coelurosauria with *an emphasis on sampling basal avialans, because of their importance for character optimizations at the base of Paraves*.

Of the *currently 31* named dromaeosaurid species, we found *26 to be valid*.

...*Rahonavis ostromi*, which was *considered* a basal avialan *but later* shown to be a basal dromaeosaurid (Makovicky et al. 2005).

A number of similarities exist between *Epidendrosaurus* and *Epidexipteryx*; however, the juvenile nature of *Epidendrosaurus* material makes it difficult to determine *whether the differences between these taxa are ontogenetic or taxonomic*.

Previous approaches to paravian systematics have typically been split between nonavialan taxa or strictly avialans, and the *character sampling has been drawn along this line as well*.

The most recent incarnation of this argument was proposed by Mayr et al. (2005). These authors proposed that the foot of *Archaeopteryx* possesses "a hyperextensible second toe, as in Deinonychosauria..." (Mayr et al. 2005: 1485; also see Mayr et al. 2007). This claim was based on observation of the tenth described specimen of *Archaeopteryx* (WDC-CSG-100) as well as citations of Gauthier (1986), Elzanowski (2002), and Paul (2002). This observation, however, is problematic. Gauthier (1986a) does not say that the second digit of *Archaeopteryx* is hyperextensible, just that the distal articular surface of pedal phalanx II-1 was enlarged.

Contrary to Mayr et al. (2005 and 2007), Elzanowski said that *Archaeopteryx* lacked an enlarged articular surface and stressed that the digit might in fact be hyperflexive.

Scoring *Unenlagia* and *Neuquenraptor* as separate terminals results in a set of most parsimonious trees of the same length as the primary analysis. *Neuquenraptor* can occupy multiple placements across the base of Dromaeosauridae therefore collapsing the base of the clade in the strict consensus solution. However, Adam's consensus demonstrates that the monophyly and membership of Unenlagiinae is unchanged. Due to the paucity of material for *Neuquenraptor argentinus* we don't view this alternate analysis of dromaeosaurids as a strong indication of the phylogenetic position of *Neuquenraptor* or of the viability or validity of the contaxonomic status on these two taxa. Instead, this analysis should be viewed as exploratory and should only definitely illustrate that *Unenlagiinae* monophyly no longer rests on the assumed contaxonomic status of these two Gondwanan taxa. The ultimate resolution of this question depends on either the discovery of a large amount of additional material referable to one or both of these taxa, or in coelurosaur systematists converging on a consensus.

Previous approaches to the phylogenetic relationships of Paraves have had various levels of taxon sampling among the three major constituent clades. The two sampling regimes typically fall into two categories: (1) *analyses interested primarily in coelurosaurian relationships and the placement of birds within coelurosaurs* (for example, TWiG

iterations); and (2) analyses interested in the interrelationships of birds (Avialae) with only a few nonavialan outgroups (that is, Clarke 2004; Clarke et al. 2006; Zhou et al. 2008).

*We are inclined to interpret* (and think it is more readily borne out by the data) that this is instead a reflection of the overall morphological similarity of the basal members of each paravian clade (for example, compare *Mahakala* to IGM 100/1126 or *Archaeopteryx*).

Twenty-eight avialans and almost 200 morphological characters *relating to avialan relationships* were added.

Lee and Worthy (2011) recently reanalyzed the Xu et al. (2011) dataset using maximum likelihood and Bayesian optimality criteria. They recovered *Archaeopteryx* at the base of Avialae, and attributed the support for this position as coming from fewer, but less homoplastic characters than those recovered under maximum parsimony as supporting a position closer to Deinonychosauria. Other parts of their recovered topology conflict significantly with the parsimony results, however, a point Lee and Worthy (2011) did not comment on. Major discrepancies include paraphyly of Tyrannosauroidae and a more derived, but stratigraphically less congruent, position for *Epidexipteryx* and *Epidendrosaurus* among Avialae. *This leaves some question* as to whether the differences are due to opposing resolutions of conflicting characters resulting from different optimality criteria or uneven evolutionary rates, which in this case translate to uneven character sampling across the tree.