The animal itself, on account of its minute size, is seldom seen; and the uninitiated, when first troubled with it, are often alarmed at the symptoms and at a loss to account for them. Fortunately, these little plagues never attach to persons in such immense numbers as do sometimes young or so-called "seed" ticks; but I have known cases where, with irritation and consequent scratching, the flesh had the appearance of being covered with ulcers; and in some localities, where these pests most abound, sulphur is often sprinkled, during "jigger" season, in foot-gear as a protection.

Sulphur-ointment is the best remedy against the effects of either of these mites, though when that cannot be obtained, saleratus water, and salt water will partially allay the irritation.

The normal food of either must, apparently, consist of the juices of plants, and the love of blood proves ruinous to those individuals who get a chance to indulge it. For unlike the true chigoe the female of which deposits eggs in the wound she makes, these harvest bugs have no object of the kind, and, when not killed at the hands of those they torment, they soon die — victims to their sanguinary appetite.

ON THE GENETIC RELATIONS OF THE CETACEANS AND THE METHODS INVOLVED IN DISCOVERY.

BY THEODORE GILL, M.D., PH.D.

In a "Synopsis of the Primary Subdivisions of the Cetaceans," published in 1871,* I ventured some remarks on the apparent genetic relations of the Cetaceans, and observed that "between the Carnivores and the Cetaceans of the present age, the gap does indeed appear to be very great, but it is bridged over, to a very considerable extent, by the Zeuglodonts of the Tertiary epoch, ... and from the Zeuglodont stem have probably descended, in different directions, the toothed and whalebone whales; while the former, in some features, such as the general form of the skull, the teeth, etc., appear to deviate less from ordinary mammals; the latter, in other respects, but especially in the development of

* Proceedings and Communications Essex Inst., vol. vi, pp. 121–126.

the olfactory organ and of the nasal bones, depart less than they from the typical forms. It would therefore seem probable that the *Denticete* (Toothed whales) have become differentiated, as now recognized, little or not at all in advance of the *Mysticete* (Whalebone whales), or in other words that the latter are not offshoots from the former, but both from one original stock."

Dr. Brandt of St. Petersburg, to whom we are indebted for so many valuable memoirs in various departments of zoology, in a recent memoir on the classification of the Balænoidea* (or Mysticete), has misunderstood the tenor of these remarks, and supposing that I meant that the Balænoids (or Mysticete) and Delphinoids (or Denticete) were differentiated and developed from the Zeuglodonts in the Tertiary epoch, has expressed his dissent therefrom.

Such an interpretation illustrates the difficulty of expression so that there shall be no ambiguity. In view of my real sentiments, the interpretation in question struck me with astonishment on the first perusal, and at the same time appealed to my sense of the ludicrous. In season and perhaps out of season, in arguments with friends, and in public discourses, I have insisted upon the inadequacy of the palæontological record, and the absolute necessity, in view of our knowledge of the radical differences between the various types of animals, of extending the phylum of the various existing stocks into a most remote but necessarily indefinite past. I have even incurred the censure of geologists for insisting that the mammals, for example, must have been developed in a far earlier epoch than we have palæontological evidence of, and that even the palæozoic might not be too recent for their birth. The absurdity of the idea, that the specialized Denticetes and Mysticetes of the Tertiary epoch could have originated in that epoch and from tertiary Zeuglodonts, is such that it never occurred to me that it could be entertained by any scientific evolutionist, much less attributed to me. The remark that the gap between the Feræ and Cete is bridged over by the Zeuglodonts of the Tertiary epoch, and that from the Zeuglodont stem have descended the recent whales, certainly does not legitimately convey that idea, although, after consideration of the passage, I must confess that one unacquainted with any of my other writings might not be entirely in-

*BRANDT (Johann Friedrich). Ueber eine neue Classification der Bartenwale (Balaenoidea) mit Berücksichtigung der untergegangenen Gattungen derselben. . . . <Bulletin de l'Académie Impériale des Sciences de St.-Pétersbourg, t. 17, pp. 113–124, 1872; *also* < Mélanges Biologiques tirés du Bulletin. . . . . t. 8, pp. 317–333.

excusable for wresting such an interpretation therefrom, especially if my reference to their systematic places of the extinct typical Cetaceans was overlooked.

Methods involved in discovery. — In dealing with genetic problems, there are facts and inferences from facts to be considered.

As facts, the Zeuglodonts are less aberrant in structure and more related to the ordinary quadrupeds than are the existing Cetaceans, and they are not living, and their remains have only been found (or at least identified) in the Tertiary epoch.

As other facts, the Cetaceans of the present epoch share with the Zeuglodonts the special features which differentiate them as Cetaceans from other mammals, and superadd other specialized characteristics.

As facts, then, the Zeuglodonts (only yet known from tertiary beds) bridge over the gap between the Carnivores (or normal quadrupeds) and the existing Cetaceans, that is, they are more like the former than are the latter.

As inferences from these facts, it seems most probable that the known Zeuglodonts represent a stock relatively near the original stem or line of descent, and comparatively little differentiated (in at least the jaws, teeth, olfactory apparatus, members, etc.) from the generalized cetacean progenitors of the Denticetes and Mysticetes. Whether the restricted characters which might be applied to all the known Zeuglodonts could be extended to those atavan forms is questionable, but that the latter had the jaws, nasal apertures and teeth attributed to the suborder in my article is, I think, a perfectly legitimate inference from the facts and, therefore, it may with confidence be said that the Denticetes and the Mysticetes have originated from the generalized Zeuglodont stem (not Zeuglodonts) thus understood.

But when they originated is entirely another question, and for the solution of which we have no data. They — or one, or the other of them — may have become differentiated in the Cretaceous, or the Jurassic, or a still earlier age. I should probably in the main agree with Dr. Brandt, however remote he might place the date of origin * and at least would have no direct evidence to

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* Especially as Dr. Brandt concedes that the Sirenians may have originated little before the Miocene (perhaps before the Eocene), with the Halitheriids as witnesses of the high degree of specialization as Sirenians which the Miocene forms had already attained. Generum Sireniorum, ab initio verisimiliter e formis inferioribus, species vel genera quædam Sireniorum, non alia animalia heterogenea (*Pachyaermata*), sensim

sustain an opinion one way or the other. It seems very safe, however, in view of the relations of the extinct faunas of that epoch to those of our own, to assume that it could not have been as late as the Cretaceous epoch.

On so-called intermediate forms. — Dr. Brandt, in connection with the subject in question, has taught us how the genealogical record should and should not be sought. "The hypothesis of the derivation from earlier, older forms," says he, "can only be proved with certainty directly from palæontology, and in no wise from so-called intermediate forms, which may have also originated independently, neither can it be, by means of analogy, indirectly deduced from isolated facts in the history of development."*

Here again, I am happy to find that on the whole I have not been entertaining very different views from the eminent master, and I accept the dictum (which I have often urged myself) that the genealogical line can only be proved (in its details) by reference to the actual forms, and that many so-called "intermediate forms" are themselves derivatives from the same common progenitors (at different removes) as the more specialized types.

But if it is really meant that the so-called intermediate forms do in no wise indicate the line and mode of descent of the more specialized types, I must for the first time differ, and differ decidedly, from my eminent critic. Do the Prosimians afford *no* hint as to how the Simians have originated? *None*, the Hipparions, the Anchitheriids, and the Palæotheriids for the Horses? *None*, the Oreodonts and the Anoplotheres, for the Ruminants? *None*, the Marsupials and Monotremes for the mammals? *None*, the Dinosaurians for the Birds? *None*, the Dipnoans for the Batrachians? *None*, the Marsipobranchiates and the Leptocardians for the Fishes? But why enumerate more of the hosts that crowd upon the memory for almost equal recognition? If such intermediate forms really give no clews or hints as to how more specialized and aberrant forms may have originated and developed, then indeed are facts in biology almost as barren and inconsequential

sensimque procreandi potentiam possidentibus, exorturum, origines itaque ante periodum miocænam (imo forsan adeo eocænam) transponendæ esse videntur. — Brandt, Symb. Siren., 1868, p. 371.

* Die Annahme der Abstammung von frühern, ältern Formen kann nur direct auf paläontologischem Wege mit Bestimmtheit nachgewiesen, keineswegs aus sogenannten Mittelformen, die auch selbst-ständige sein können, oder aus vereinzelten, der Entwickelungsgeschichte entlehnten Thatsachen auf dem Wege der Analogie indirekt abgeleitet werden. — Brandt, *op. cit.*, 332.

for the evolutionist as for the believer in patterns and special creations.

But I cannot believe that Dr. Brandt really means what he says: my familiarity with his previous works and train of thought forbids such a belief and I cannot doubt till I shall be authoritatively undeceived, that his words simply involve a too energetic expression of dissent from those (if there be such) who would believe that all so-called intermediate forms are exactly those in the line of descent from the more primitive to the more specialized ones. If this only is meant, I still find myself in agreement with Dr. Brandt, and admit that so-called intermediate forms do not necessarily prove the line of descent, but (if rightly so called) they do furnish all ranges of indication from a vague hint to absolute proof, according as they be more or less generalized, and more or less allied to those extinct forms in the regular line of descent, and by which can alone be demonstrated with certainty, according to Dr. Brandt, the lineage of any form. But how will Dr. Brandt avail himself of palæontology and identify and recognize, when found, those ancestral types? How approach it otherwise than by the same methods by which the "generalized" and "intermediate" characters are recognized? The great difficulty, indeed, consists in the identification of the forms in the direct line of descent; and the exact identification is practically impossible, but it may be sooner or later sufficiently approximated to give us tolerably satisfactory ideas as to the origin and successive differentiation of various types. And that end will be attained by the recognition of forms as successively intermediate as to structure and time of development, and thus it will be exactly by intermediate forms (and not the less so because revealed by palæontology) that the lineage will be proved!

Toxonomic values of characters. — Dr. Brandt further contends that the teeth, the olfactory organs, and the nasal bones have no determinative value.* And yet he gives the suppression of the teeth and the coördinate development of whalebone as the sole distinctive characters of the whalebone whales. Therefore, it is evident that he thinks that the teeth do furnish distinctive characters. He recalls the familiar facts that in early youth all Ceta-

*"Auf die Zähne kann kein entscheidendes Gewicht gelegt werden .. Dem Geruchsorgan, oder den Nasenbeinen vermag ich gleichfalls keinen Werth bei der Verleitung der Abstammung beizulegen."— BRANDT, *op. cit.*, 332.

ceans have teeth, while on the other hand, not only the whalebone whales, but also many Delphinoids, in old age, are wholly toothless, while others have only one or two teeth. And still he uses the want of teeth in the whalebone whales as a distinctive character. And thus I find myself still on the same platform with Dr. Brandt as to practice although he appears to differ theoretically.

The coördination of the want of teeth with other characters in the whalebone whales is invariable for the known forms, and may therefore be used as a diagnostic character. The want or presence of teeth *per se* is a character of little importance and of extremely varying significance. In the Rhytinids, for example, the want of teeth is only of family value; in the walruses, the hypertrophy of the canines and concomitant atrophy or suppression of the incisors are also only of family value; in the Artiodactyle Ungulates the want of (upper) incisors indicates less than subordinal distinction for one group (Ruminants) and in another case (Phacochœrids) scarcely specific distinction! But when the teeth *are* developed, their structure and relations do afford hints, and most suggestive ones, and the significance of similarity is more than in ratio to the continuing agreement of teeth of increasingly complicated structure.

As to the jaws and the teeth, as well as other parts, they are, it seems to me, as matter of fact, more similar in the Zeuglodonts to those of ordinary mammals than are those of the Denticetes or the Mysticetes, and they are at the same time coördinated with other characters less aberrant; in other words, they are in all essential respects more similar to the ordinary mammals than are the existing Cetaceans and, therefore, to use the favorite expression of Dr. Brandt, *ubi plurima nitent*, they are, inferentially, more nearly allied to and less divergent from the ancestral stem. If, however, it is denied* that they are more similar, I will only reply that I prefer to rely upon the evidence of my senses, and

* Nicht blos die Schädel der *Bartenwale*, sondern auch die der *Delphiniden* erscheinen nach meiner Ansicht im Vergleich mit den Schädeln der Landsäugethiere auf eigenthümliche Weise ziemlich *gleich anomal* und bilden zwei für den Aufenthalt im hohen Meere geeignete und dazu durch Naturgesetz bestimmte, selbstständige Schädeltypen, denen sich als dritter *gleichwerthiger*, zu den *Phocaceen* hinneigender Schädeltypus, der der *Zeuglodonten* anschliesst.—BRANDT, *op. cit.*, 331.

This passage is *apropos* of my remarks respecting the intermediate character of the Zeuglodonts quoted in the introduction to this article. The only comment I shall venture shall be in the form of a question. If the Zeuglodonts incline towards the Phocacea in their skull, why are they not to that extent (less their own deviation from the direct lineage) intermediate between the recent whales and the Phocacea?

even if the facts do not appeal to the senses of another in like manner, still do I prefer to trust to my own.

Inferences respecting genetic relations.—The question having been raised as to the comparative degrees of differentiation of the cetaceous types, it may be well to pursue it further.

Zeuglodonts. As already observed, the Zeuglodonts, in the form and structure of the jaws, the character of the teeth (molars double-rooted in part), the presence of the typical (Edneabilian) number of teeth in the intermaxillary bones, the more or less anterior position of the nostrils, the contour of the skull and general relations of its constituent elements, and in fact almost all the known parts of their organization, differ much less from the ordinary mammals than do any of the existing Cetaceans. They are therefore the most generalized or the least specialized Cetaceans known ; these are simple facts which appeal to the senses. As inferences, the forms so distinguished represent, better than any other Cetaceans, the primitive ones from which they, as well as the latter, have descended. None of the known Zeuglodonts can, indeed, be the progenitors of the modern Cetaceans, since types closely related to the latter are associated with them in tertiary strata, and the known Zeuglodonts may have become much differentiated (possibly even more than the modern Cetaceans), in some minor points, from the primitive forms, but that they are, as a whole and in all essential features, more like (and therefore more allied to) those ancestral types can scarcely be doubted, *me judice*. Therefore those Zeuglodonts may appropriately be regarded as the nearest known representatives of the Protocetacean types, as quasi-intermediate forms between the quadruped mammals and the more specialized Cetaceans, and in a genealogical system must be represented as the nearest of kin to the prototypes of the order.

But even the few forms of Zeuglodonts known differ in degrees of differentiation from the normal mammals, and must be so represented, the *Basilosauriids* representing a more generalized and the *Cynoreids* a more specialized type.

Mysticetes. It seems more probable that the agreement of the Mysticetes and Denticetes in the attenuated intermaxillaries, the anterior nostrils, pectoral members, etc., should be the result of inheritance than of independent assumption, and therefore that they have developed from forms thus differentiated from the primitive Zeuglodont stem.

As to the forms most generalized, serious doubts may be entertained. The Denticetes have almost universally been considered as entitled to that rank, and if the form of the jaws and the teeth are alone considered, such would seem to be undoubtedly the correct view. But in other respects (such *e. g.* as the relations of the bones around the calvarium, the frontals, the posterior portion of the maxillaries, the development of the lachrymal, the less atrophy of the pelvis, the rudimentary hind limbs) the Mysticetes appear to me to be the most generalized, and, although the evidence may be vague and inconclusive, I may be permitted, till contrary evidence supervenes, to represent such apparent probability in a genealogical system. Of the two families (*Balænopteridæ* and *Balænidæ*) known, the Balænids appear to have superadded to the Mysticete type the most specialized feature and most generalized characters, such, for example, as the orbital prolongations of the frontal bones, the reduced coronoid processes of the lower jaw, etc.

Denticetes. Respecting the families of Denticetes the evidence is also vague, but hints are furnished by various structural characters. These may be illusive, but in default of evidence to the contrary, and until superseded, may be followed. It may be that other parts would furnish conflicting testimony, that there may be an unusual persistence of primitive characters in some regions, while in some others the structure has been much modified, and it is even not impossible that there may have been a reversion to ancestral characteristics in certain parts, but until such deviations are proved, it seems most in accord with sound philosophy to take provisionally, and in default of other, the *prima facie* evidence offered. With these remarks, the succession of the various families of Denticetes may be sought.

In the first place, two forms present themselves, each of which presents claims for the nearest representation of the ancestral line —the Iniids and the Ziphiids. The Iniids, and their near relatives the Platanistids, offer in their comparatively long neck and free vertebræ testimony in favor of such title, while the Ziphiids, in the development and continued independence of the lachrymal bones, produce theirs. And it seems very much more credible that these characters should have been inherited without fault than that they should have been the result of reversion after once having been lost, especially as there appear to be no offsets to such

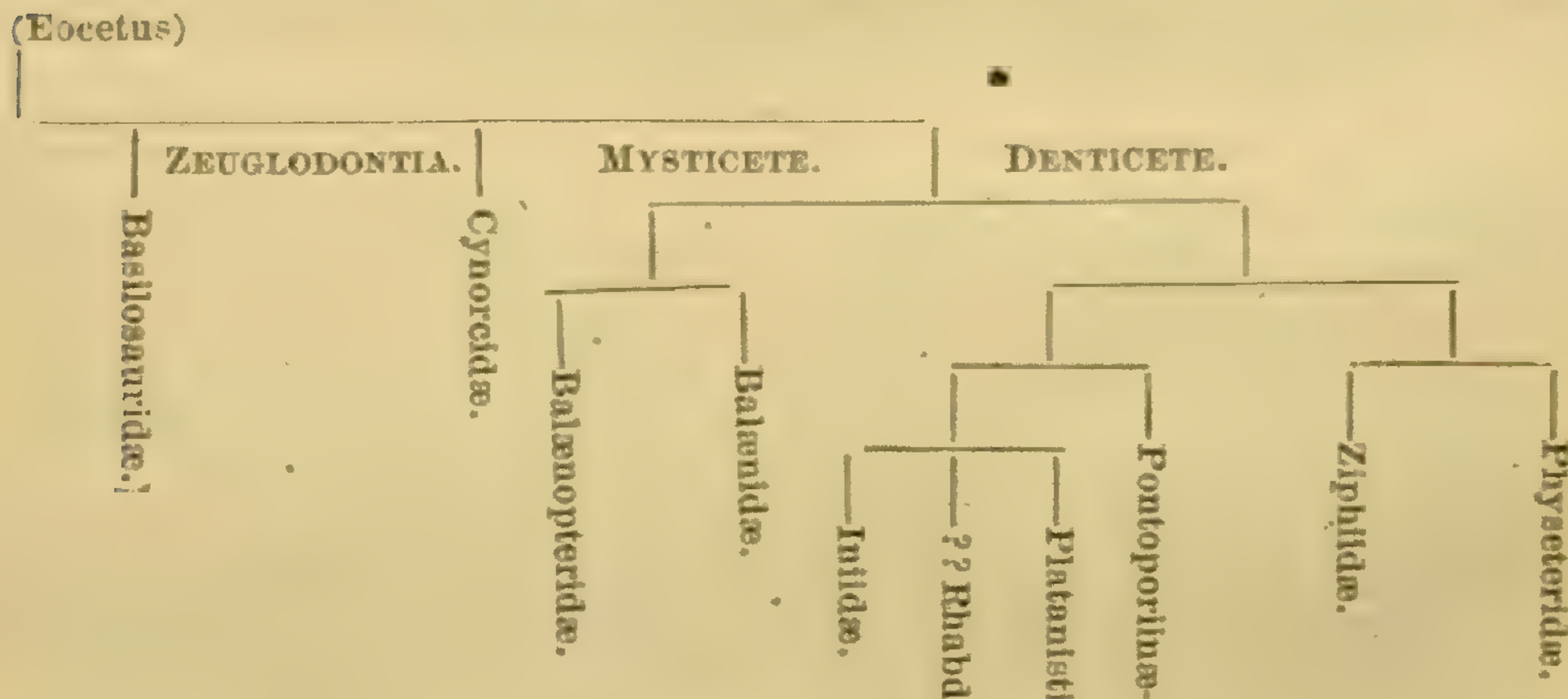
characters, and the rest of the organization is not in disaccord with those evidences of generalization.

On the whole, it appears to me that the long-necked Cetaceans represented by the living Iniids and Platanistids and in greater number by various forms in the Tertiary epoch are best entitled to the first rank. Whether of those, the Iniids or the Platanistids are the first is equally uncertain, but as the latter are certainly in some respects the most specialized, to the Iniids may be conceded the rank provisionally.

Probably, as more differentiated offshoots from the same secondary stem as the Iniids and the Platanistids, may be considered the Delphinids, of which the Pontoporiinæ doubtless represent the most generalized form.

Recommencing with the other secondary stem, apparently the Ziphiids represent the oldest rank, and the Physeterids are the results of an offshoot from the same lineage.

I have thus endeavored to present my views, and I trust that the language I have employed may prevent me from being misunderstood to mean that any one of the known specialized forms is derived from another of the known specialized forms. I have simply essayed to indicate what now appear to me to be the proximate relations of the several forms, and respectively the more generalized of the approximated groups. The following table may more vividly convey my views; in each case, the left branch indicates the supposed most generalized and the quasi-oldest form:



I shall only add that I have no intense convictions of the correctness of this representation, and regard it as simply provisional and subject to the modifications which the accumulating testimony now being so rapidly wrested from the living and the dead may necessitate. I do believe, however, that it is not in opposition to the data which have up to the present time been collected and tabulated. The advantages of such tables, in bringing into synoptical form and impressing upon the mind the various degrees of relationship and subordination of the respective subdivisions of a group, appear to me to be equally obvious (although not equally pregnant with meaning), whether we are evolutionists or patternists.

Remarks on Dr. Brandt's classification.—A few words on nomenclature and on the subfamilies of Mysticetes may be advisable.

Dr. Brandt* implies censures, by an exclamation mark (!), on the name Mysticete, and the inference conveyed thereby, and by his language, would be that I was responsible for the introduction of the name. As to the name itself, I perfectly agree with Dr. Brandt that it is objectionable and I hesitated sometime before adopting it. It was, however, the first introduced (by Gray, in 864†) and for that reason and that alone, I have employed it. It seems strange that Dr. Brandt should have been ignorant of this previous introduction, as he has referred to Gray's works in his memoir. I adopt very many names that are objectionable to me, recognizing as I do the inexorable demands of priority,‡ nor do I consider it necessary to protest against every inapt or ungrammatical name thus adopted, or found in the works of others, such, for example, as *Kyphobalæna* and others adopted by Dr. Brandt. §

As to the subfamilies, Dr. Brandt has suppressed those admitted by myself and others among the Balænopterids adding, however,

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*Eine dritte, neueste, von Th. Gill vorgeschlagene, Classification der Bartenwale, die er *Mysticete* (!) nennt, etc.· Brandt, Mel. biol., viii, 317.

†Gray, Proc. Zool. Soc. 1864, p. 198. It is true that Brisson had before called the same group *Cetacea edentula*, and Wagner, *Cetacea edentata*, but neither of those names fulfilled the requisites of nomenclature.

‡Lest I may be here, too, misunderstood, I add that I simply recognize the rule of priority because of the advantage afforded as a basis for uniformity of nomenclature, and am not influenced in the slightest degree by any considerations of "honor" or "justice" to nomenclators.

§ *Cetotherium*, *Cetotheriopsis*, etc., are employed in the same memoir by Dr. Brandt.

two for extinct types, *Cetotheriinæ* and *Cetotheriopsinæ*. But while suppressing the subfamilies, he has retained the characters, the want of which induced me to frame one of them, in the diagnosis of the family itself. In other words, the subfamily Agapheliнæ was named for forms of Balænopterids distinguished by the absence of pectoral folds and of a dorsal fin, yet Dr. Brandt, while suppressing it as unworthy of subfamily distinction, considers the development of such folds and of a dorsal fin as family characters.* The development or not of the folds and fin is certainly not of family value and should therefore be eliminated from the definition of the family, as it misleads both as to the prevalence of the characters and their value, and at the same time diverts the identifier from the path. Whether the characters are of subfamily value is another question, and one which need not be discussed here.

In conclusion, it appears that I share the opinions of Dr. Brandt on most of the questions discussed, and I am happy to find that I can enroll myself under the banner of so able a leader; and I decidedly protest against being held responsible for views which I am as willing to oppose as he. As to the other points in which we appear to differ, I am fain to believe that it is due to the use of language more comprehensive than was meant by Dr. Brandt, and with the disposition to exercise that allowance for ambiguity which I would wish to have practised in respect to myself, prefer to surmise his real views from the general tenor of his works and thought, than to accept his exact phraseology.

REVIEWS AND BOOK NOTICES.

Archæological Collections in America.—The recent report† by Prof. J. Wyman on the specimens received by the Peabody Museum in Cambridge is a most instructive document, as it not only gives a list of the additions made to the Museum during the year but also contains much interesting information relating to

*Pectus et abdomen sulcis longitudinalibus exarata. Pinna dorsalis perfecta vel tuberculo repræsentata.—Brandt, Mel. Biol. viii, 326; see also p. 321.

† Fifth Annual Report of the Trustees of the Peabody Museum of American Archæology and Ethnology. Presented to the President and Fellows of Harvard College, May 15, 1872. 8vo pamphlet, pp. 35. Boston, 1872.