# THE LITOPTERNA—A LESSON IN TAXONOMY: THE STRANGE STORY OF THE SOUTH AMERICA 'FALSE' HORSES

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**Abstract**

*The supposed evolution of the horse was confronted by contradictory fossil evidence from South America. The solution in 1910 was to banish the contradictions into an obscure separate order of mammals. This paper illustrates the strange and wonderful contortions of taxonomists to remain mindlessly loyal when the theory of evolution fails them.*

*Editor's note—(The reader may wish to consult previous Quarterly articles on the subject of horse evolution: Cousins, F. W. 1971. A note on the unsatisfactory nature of the horse series of fossils as evidence for evolution. CRSQ 8:99-108; Davidheiser, B. 1975. Horse brain, cow brain. CRSQ 12:88-9.*

## A Familiar Story

In the fall of 1985 the Government of Canada opened the new world-class Tyrrell Museum of Palaeontology, said to be the largest fossil museum in the world, in the heart of one of the great dinosaur fossil beds of the world at Drumheller, Alberta. And there in the great museum, surrounded by a fabulous dinosaur collection, is the centerpiece of the 'proof' for evolution—the horse series from little *Eohippus* to modern *Equus* in five easy stages—the century-old illustration endlessly copied and recopied billions of times in four generations of textbooks, and just as endlessly in other media for the education and edification of the masses.

The horse series is the claim to fame of Yale palaeontologist Othniel C. Marsh who discovered fossil bones of the horse and of the *Hyracotherium* (incorrectly and commonly called *Eohippus)* in Wyoming and Nebraska. His reconstruction and arrangement of these fossils are still on display at Yale University and have been copied with some variation in the sequence in museums everywhere (Taylor, 1984, p. 152). It has become axiomatic that the horse evolved in the midwest of the United States, and if fossil *Equus* is found in Africa, or anywhere else, it migrated there from Nebraska or thereabouts.

Fix (1984, p. 164), who wrote a devastating critique of so-called human fossils, echoes countless writers in the field when he says that you can throw out any 'evidence' you like about evolution, but "we still have the evidence pertaining to the horse."

This paper focuses on some very interesting fossil discoveries in South America which both Darwin and the great Argentinian palaeontologist, F. Ameghino, placed among the horses (Scott, 1910, p. 551). In 1910, however, Scott (Cifelli, 1983, p. 28) removed the fossils from any connection with the horses and placed them in a far-removed and separate order, the Litopterna. This decision on classification, supposedly highly controversial and very much open to debate, has remained a closed question up to the present, as we shall see in the following discussion. Storer (1979, p. 804-5), places the Litopterna in Order 22 and the Perissodactyla, which include the horses, in Order 33. With creatures such as the aardvark, the elephant, and sea cow placed in the intervening orders, one gets the message that the Litopterna (odd-toed, hoofed mammals) and the Perissodactyla (odd-toed, hoofed mammals) are about as remote in structure from one another as one can get.

In the process of examining the decision to remove the Litopterna from the order containing the horses, we shall gain a rather illuminating lesson into the basics of taxonomy—the elements that go into the sometimes agonizing decision about placing a woefully incomplete or puzzling fossil into this or that category. Although the creationist may sharply and categorically disagree with the evolutionary assumptions of a palaeontologist, this paper in no way reflects on the fascinating but often excruciatingly difficult work of that field of study. Collecting and restoring fossils is not evolution. The interpretation of the evidence, as we see especially in the case of horse and human fossils, often is.

For the benefit of the reader with little or no background on taxonomy in zoology, the following somewhat oversimplified points should be noted as we examine some aspects of the horse story. The animal kingdom is divided into 22 phyla of which the last one, Chordata (notochord), includes the horses. Of the four great classes of Chordata (amphibians, reptiles, birds, and mammals), the latter includes the horses. Mammals then are divided into above 30 orders which classify about 4,400 living species and numerous fossil forms. Orders are quite broad, for example, the Primate order includes every kind in all the world of lemur, monkey, ape, gorilla, and even man, following evolutionary thinking. In this paper I focus on two of the orders: the extinct Litopterna and the Perissodactyla. The latter is the more familiar one, and includes a remarkable span of creatures, such as the horses, asses, zebras, tapirs, and rhinoceroses. Bizarre extinct creatures are also included. By definition, the horse is more closely related to any other creature within this order, such as the rhinoceros than to any other animal in any other order (Storer, 1979, passim).

The key to classification is phylogeny or the supposed evolutionary history of a group, such as small to large, more primitive to more modern structure, and the like. Some of the beliefs about evolutionary development are examined below. For a long time the five-toed *Phenacodus* from the Order of Condylarthra was thought to be the ancestor of the horse. The bases on which it was disqualified are interesting and vulnerable: It was too large for the supposed sequence including *Eohippus;* it was too well developed, that is, it was not 'primitive' enough; its existence in time did

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not allow for evolving into *Eohippus. Phenacodus* was replaced by *Hyracotherium (Eohippus)* as the ancestral horse. This creature was removed from still another order, the Hyracoidea, or the hyraxes and coneys. For more than a century, this animal has been placed at the beginning of horse evolution (Scheele, 1955, p. 27).

**Litopterna Placement into a Separate Order**

That the litopterns present a problem in classification is unquestioned. Made up of two very different kinds of creatures, one group seemed very camel-like while the other is sometimes described as out-horsing the horse. The Argentinian palaeontologist, F. Ameghino with his brother, Carlos, are described by Scott as providing services to palaeontology that are quite inestimable. They discovered the formations, the unique South American fossils, and arranged them in chronological order. Ameghino, who first distinguished and named the order of Litopterna, concluded that the Litopterna belonged with the Perissodactyla, that is, with the horses. Even Charles Darwin, when he discovered the camel-like, least horse-like *Macrauchenia* in the Pampas, declared unequivocally that it belonged to the order now called the Perissodactyla. What more would he have said if he had found the horse-like kind (Scott, 1910, p. 114-5)?

It is small wonder, then, that Scott (1910, p. 551) observes that this order has been the subject of much debate because of supposed convergence.

Convergence, we note, is a device in taxonomy which declares that though two animals may closely resemble one another (for example, the horse and the litopterns), their ancestors did not. Therefore, the likenesses are merely coincidental and may safely be ignored. This device, of course, innocent of any supporting evidence, has gotten many a taxonomist out of many a sticky wicket. As we shall see, little interest is shown today in taking a second look at how the litopterns are classified. Simpson (1961, p. 87) confronts the striking similarity between horse and litoptern in this manner:

> In spite of the various collocations of the typologists, horses are not so convergent toward any other living group that a modern taxonomist would be likely to mistake their homologies. There is, however, an extinct South American ungulate, *Thoatherium,* between which and horses there is strong convergence, notably in the fully one-toed feet. The discoverer of *Thoatherium,* Florentino Ameghino, an evolutionary taxonomist capable in his time (1845-1911), concluded that the resemblances are homologous and that litopterns and horses had a common ancestry closer than that, for instance, between horses and rhinoceroses.

**How Are Litopterns Described?**
**They are very like a horse!**

It is interesting and instructive to see how palaeontologists describe the litoptems. The statements below are edited so as to focus only on the expressions of various authors on how the horse and the litopterns were alike.

An important scholar and authority, Scott (1910, p. 551-68, 738), makes these statements:

a. The teeth and skeleton of the modern horses are extremely characteristic and unlike those of any other family, except for one group of the south American Litopterna.
b. The upper molars have cusps as in several families of perissodactyls; the incisors are arranged as in the horses.
c. The orbits, shifted behind the teeth, as in the later and more advanced horses.
d. The feet tridactyl as in the perissodactyls.
e. These extraordinary animals, remarkable for the many resemblances to the horses, are well proportioned agile and graceful creatures and resembled in appearance their very distant relatives of the northern hemisphere. It is in just this likeness to relatives which are both structurally and geographically so distant, that the wonderful features consist.
f. The shape of the hoofs and the whole appearance of the feet are most surprisingly like that of the three-toed horses.
g. The extraordinarily interesting (Litoptern) with completely monodactyl feet.
h. Limb bones, especially the femur, have a decided resemblance to those of *Mesohippus,* a three-toed horse. The most remarkable feature of the skeleton is the structure of the single-toed feet, which are more completely monodactyl than in any other known mammal.
i. Limb-bones, approximating much more closely to the proportions seen in the horses.
j. The head would seem to have had some resemblance to that of a small horse.
k. Most extreme of all is the much debated case of one family of the Litopterna, the genera of which resemble the three-toed and one-toed horses of the North American Tertiary in marvelous fashion.

*Thoatherium* from Argentina is the most completely monodactyl mammal known, surpassing even the modern horses in the complete reduction of the splint-bones.

Earlier in this century *(Encyclopedia Britannica.* 1910. 11th edition. 16:791; 17:526), the litopterns are described thus:
a. The Litopterna, exclusively in South America, are perhaps more nearly akin to the Perissodactyla (that is, to the horse).
b. The Litopterna show a curious parallelism to the equine line; the feet are very like those of *Hipparion* (a horse).

Current authors also expressed themselves thus about the litopterna:
a. The litopterna have notably horse-like fore and hind limbs (Peyer, 1968, p. 317).
b. The *Macrauchenids* of the Litopterna, whose record with regard to high crowned teeth, almost exactly parallels that of the horses (Keast, 1972, p. 283).
c. A three-toed form of the litopterns, *Diadiaphorus* (Figure l), lived alongside *Thoatherium,* which seems more horselike than any true horse, for it was single-toed with splints more reduced than those of modern equids (Romer, 1966, p. 260).

Finally, Colbert made these observations (1967, p. 343, 549):
a. The *proterotheres* were the “horses” among the South American ungulates. They never became very

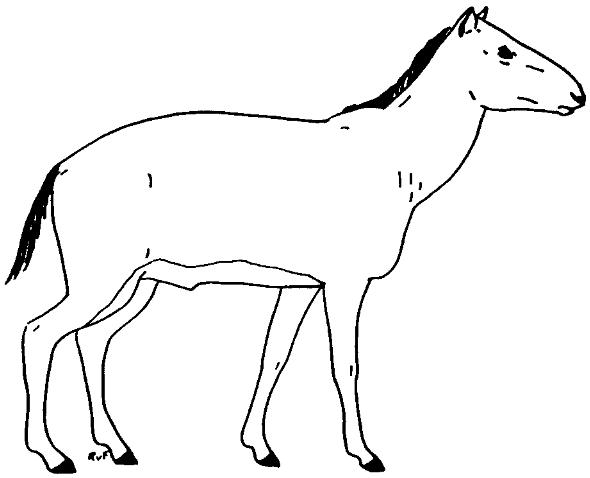

**Figure 1. A three-toed South American "false horse," *Diadiaphorus*. In the restoration the tail of the animal was left relatively hairless to make it look less horse-like. Here the tail hair is partially "restored"—a possibility just as likely. (Redrawn by Ruth von Fange from Scott, 1962).**

large, but some of them evolved in ways that were remarkably similar to horses, especially in the adaptations of the feet for running.
b. In a way the litopterns are easy for us to comprehend for they are more directly comparable to the hoofed mammals with which we are familiar. To put it another way, there were close parallelisms that make the litopterns seem to us like reasonably orthodox hoofed mammals.
c. In these Litopterna the skull is just as in the horses; the incisor teeth, the cheek teeth parallel to some degree the horses of the same age, and a molarization of premolars as in the horses.
d. The hind feet were especially horse-like; the ankle bone was similar to that of horses.
e. It seems reasonable to suppose, therefore, that the habits and the mode of life of these litopterna were similar to those of middle Tertiary horses in North America.

### How Are Litopterns Described? They are very unlike a horse!

After reading all of the above, we are quite nonplussed to find that things are not at all as they seem. The same palaeontologists show another side of the coin:
a. It is totally different from them (the horses) in dentition. The dentition and the rest (except limbs) of the skeleton show, however, that instead of kinship, parallel evolution took place (Peyer, 1968, pp. 274-5).
b. The feet of three-toed forms were strikingly (if only superficially) similar to those of horses (Keast, 1972, p. 270).
c. *Thoatherium* is said to be more horselike than true horses. This pseudohorse was, however, comparatively unprogressive in other respects, for the cheek teeth were low crowned, and the carpus was poorly adapted for monodactyl running (Romer, 1966, p. 260).

We leave the final comments to Scott (1962), the authority who "removed" the Litopterna from the Perissodactyls and placed them into a separate order:
a. The horse-like Litopterna are a remarkable instance of convergence, for with all their resemblances, they were not remotely related to the true horses, for they were not even perissodactyls (p. 257).
b. But as already suggested, the resemblances to horses are superficial, while the differences are fundamental (p. 562).
c. It must again be emphasized, however, that such likenesses to horses are superficial; the fundamental and significant characteristics are more primitive than in the most ancient of known perissodactyls (p. 566).
d. The members of this family (the Litopterna) are remarkable for the many and deceptive resemblances to the horses which they display (p. 561).

### The Literature on the Litopterna

If indeed the litopterns are a most remarkable case of convergence, that is, the animals are very similar, but their ancestors were not, one would suppose that such a case would attract widespread attention in the scholarly world and a large number of studies. Such is far from the case, however. An extensive computer search of a number of scientific data bases brought forth very little—three journal titles in English and four in Spanish—but none of these were of any substance in dealing with theoretical issues supposedly involved. There does not appear to be any book in English or Spanish devoted entirely to the litopterns—a major surprise, nor does there seem to be a book about the horse and its apparent twin, the litopterns.

The litopterns are touched on in some expected places. Martin (1967) treats Pleistocene extinctions. This Yale University publication promised slight to moderate coverage of South America. Litopterna is misspelled as Liptoterna several times in the text, and only the more camel-like varieties are briefly noted. The author failed to list the order in the index. Similarly, Peyer's (1968) authoritative reference on animal dentition carries the same misspelling of Litopterna, and at least one of the brief references to this order is omitted from the index.

It seems more than passing strange that there is a great curtain of silence around this problem described to be of great significance for the philosophy of evolution, full of far-reaching consequences, and supposedly the subject of much debate.

### A Lesson in Taxonomy

On the surface it would appear that a good deal of soul-searching by leading experts went into the decision to remove the litopterns from among the horses. It is important to let palaeontologists describe how decisions are made about difficult classification problems. Certainly the average reader at this point would not have a clear picture on why the separation took place.

Just how does the taxonomist do the extraordinarily difficult task of classifying animals when part of the animal resembles one kind of creature while other features are much like a completely different kind? Our first insight comes from Simpson (1961) in the introduction to his pioneering book on animal taxonomy, where he quotes A. J. Cain:

> Is it not extraordinary that young taxonomists are trained like performing monkeys, almost wholly by imitation, and that in only the rarest cases are they given any instruction in taxonomic theory?

The following material illustrates the state of such theory as it applies to the horse. It will quickly become apparent that we are dealing with quite fragile beliefs used in an *ad hoc* manner with respect to the horse at least, rather than demonstrable principles, and that one belief may contradict another, thus forcing an arbitrary choice in the direction chosen by the taxonomist. Here are some of the beliefs of the taxonomist:

a. The splitters versus lumpers beliefs. The first problem that should be mentioned is the almost legendary one of the lumpers versus the splitters. Lumpers focus on the remarkable degree of variety found within species and are sparing about identifying "new" species. Splitters tend to magnify minor differences among specimens and clutter the field with an inordinate number of new species. Most of the outraged comments on the subject appear to come from the lumpers, and no satisfactory solution is in sight, especially with the fossil record which often must deal with fragmentary remains. One of the best examples of the work of the splitters is the human fossil record where every new fragment is hailed before the media as a sensational new breakthrough, and of course a new species. Fix (1984, p. xxv) has observed that such news conferences tend to be held just before new funding is requested from foundations and other donors.

b. The embryo belief. It is believed that embryos represent or repeat ancestral adult structure, thus showing the evolutionary stages through which a species went (Simpson, 1961, p. 87). It is well known that this strange belief, known as recapitulation theory*, still persists in high places among evolutionists, however, even though leading evolutionists have discarded the idea long ago.

c. The subjectivity belief. Simpson (1961, p. 119) states that it is virtually impossible to be completely nonarbitrary for any taxonomy other than species, and frequently also for species, but he declares that there is nothing wrong with being arbitrary in the practice of an art, including the art of classification.

d. The evolutionary taxonomy belief. How does one decide which resemblances among animals count and which ones do not? According to Simpson (1961, p. 85), the only way is by the use of evolutionary taxonomy. The taxonomist creates a canvas of who the ancestor was, and the changes that must have occurred through the ages for the modern form to evolve. Whatever fits this totally speculative blueprint is taken as a homology—a resemblance that counts. For reasons not made clear, some creatures apparently suffer from arrested development. Thus the rhinoceros, and still more the tapir, are more 'primitive' with respect to their toe development, or closer to the supposed ancestor than the horse. If little or no change is evident between modern forms and ancient fossils, such occurrences are tagged as living fossils—an undefined evasion of the issue.

e. The anatomy belief. In proportions and general appearance, the three families have little in common (tapirs, rhinoceroses, horses); but so like in anatomy are they that placing all in the same order is obviously the only course to take (Scott, 1962, p, 395).

f. The teeth versus skeleton belief. The classification adopted will depend upon the relative importance given to the teeth, on the one hand, and the skeleton, on the other. For example, we are told that the dentition of the Litopterna shows that these extinct mammals of the South American Tertiary cannot be related to the horses, despite notably horse-like fore and hind limbs (Scott, 1962, p. 397).

g. The tooth belief. Taxonomists believe mammals originally had 44 teeth but through specialization over a long period of time, the number was reduced. Along that line of thinking Branson (1952, p. 451) suggests that man also fits this pattern when he says that man has 32 teeth and is somewhat specialized in that respect.

h. The toe beliefs. The number of digits on each foot of those belonging to the order of Perissodactyla is usually odd, one or three, but may be four, as in the front foot of the tapir. See Figure 2. No five-toed perissodactyl has yet been found (Scott, 1962, p. 395). There is the belief in a principle that many animals originally possessed five digits—a primitive stage—and that through evolutionary processes the number was reduced, for example, to a perfect one in the case of the horses. Thus we have Branson (1952, p. 451) reflecting this belief when he states that man is primitive in respect to number of toes. He does not discuss man's fingers. With respect to the horses, the first toe is already 'missing' when the story begins with *Eohippus*, and the fourth toe of the front feet was only a temporary stage between five and three (Branson, 1952, p. 456). Thus the tapir with four toes on the front feet today and the other orders of animals with the same pattern may be expected to drop off the fourth toes momentarily. Simpson (1961, p. 86) extends this belief by stating that the common ancestor of horses, tapirs, and rhinoceroses must have had four toes on front feet and three on hind feet, and he uses *Eohippus* and tapirs as his proof.

i. Time belief. There is a belief that *Hipparion* (three-toed horse) became extinct three million years ago and was replaced about two million years ago by Equus, a true horse. Johanson (1981, pp. 176-7) relates with some relish that his rivals, the Leakeys, are in trouble trying to establish a date of three million years for a 'human' fossil find when it was found with two-million year old *Equus* teeth. "You can't turn a three-million year old *Hipparion* into a two-million year old *Equus* just because you want to." Curiously, this belief and the famous pig studies of Basil Cooke supersede any K-Ar dates in Africa in establishing dates for the so-called human fossils found there.

j. Uniformitarian belief. Wright (1980, p. 827) comments on a gradualistic view of evolutionary change, for example, that in the lineage of horses from *Eohippus* to a modern form, the average change in size was .0001 inch per thousand years.

k. Environment beliefs. South American ungulates (the litopterns) illustrate very nicely the close correlation between animals and their environments, and indicates how similar environmental conditions will lead, by genetic processes, to the evolution of remarkably similar animals, quite unrelated except through their very remote ancestors (Colbert, 1967, p. 338).

*Editor's note: Previous Quarterly articles on the recapitulation theory that may be of interest to readers—Rusch, Sr., W. H. 1969. Ontogeny recapitulates phylogeny. *CRSQ* 6:27-34; Wolfrom, G. W. 1975. Perpetuation of the recapitulation myth. *CRSQ* 11:198-201; Lammerts W. E. 1988. Article review. *CRSQ* 25:147

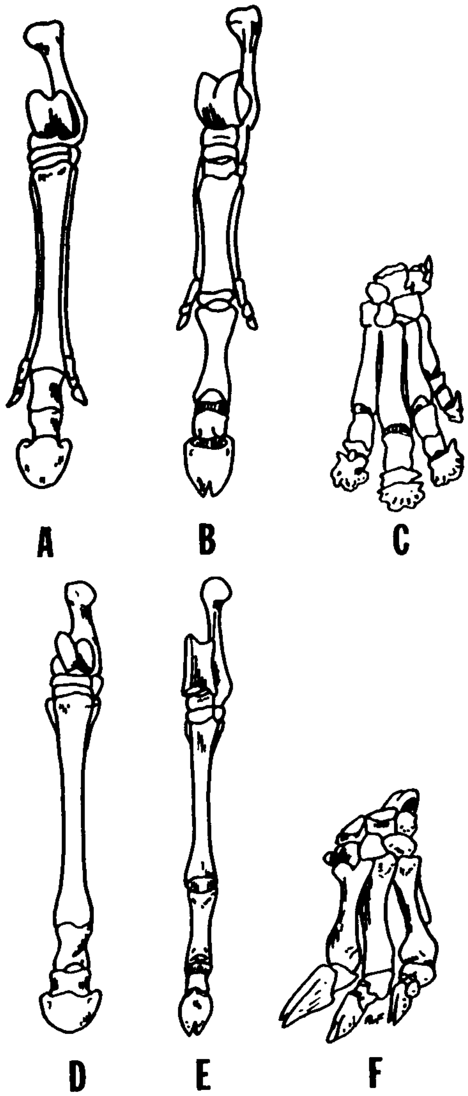


**Figure 2. Which two feet do not belong to the Order of Perissodactyla that includes the horses? Answer: B and E are litopterns, supposedly not remotely related to the horses. All the rest are from the same Order of Perissodactyla: A and D are horses; C is a tapir; and F is a "clawed" horse, the chalicothere. (Redrawn by Ruth von Fange from Colbert, 1967; and Scott, 1962).**

1. Geography beliefs. Environment beliefs are offset by beliefs related to geography. With or without evidence, taxonomists invoke land bridges, mountain or other barriers, drowned lands forming an island, such as South America at times, in order to attempt to explain the fossil record. Palaeontologists believe they have located the point of origin where a type of animal evolved, and that similar creatures found in other parts of the world migrated from the place of origin. There is considerable aversion to suggesting two or more evolutions of the same species, despite the environment belief noted above. We are familiar with such declarations, for example, that man evolved in East Africa, the horse originated in Nebraska and/or Wyoming, and the litopterns originated in South America. Such statements are of course based on where the fossils have been found along with estimates of their age. Scott (1962, p. 255, 406) observed that geography is difficult to apply because of the uncertainty concerning the manner in which the evolutionary process operates. While it is easy to announce that many genera and families of mammals travelled in both directions across the Bering land bridge, Scott speaks of unexplained facts and the difficulty to understand absence of crucial fossils to support such beliefs. The above by no means exhausts the list of beliefs which taxonomists use for explanation and classification. What is missing from these principles is any logical, scientific method of choosing among them. Taxonomists sometimes acknowledge the hazards of their profession, although such uncertainty is seldom alluded to in texts and popular literature on the subject. Scott (1962, p. 254) rightly says that ancestor charting:

> is the most inexact of the sciences, because it has such a large subjective element and depends so much upon the judgment of the individual naturalist. It is for this reason that palaeontologists differ so often and so radically in the answers which they give to phylogenetic questions, for their fundamental preconceptions are so irreconcilable, that one regards as quite impossible what another believes to be usual and normal.

It seems appropriate to give several examples of the kinds of difficulties which taxonomists have identified in their work.

a. Simpson (1961, p. 84): Horses and rhinoceroses are evidently related in some degree. On the other hand horses differ from rhinoceroses quite strikingly in having only one toe and in numerous other respects, such as thinner and more hairy skins. In those respects and some other characteristics rhinoceroses resemble hippopotamuses more than they do horses.

b. Scott (1962, p. 431): There is a problem of expected but missing fossils. Tapirs lived in the Pleistocene along the Pacific and in the eastern forests, but apparently not on the Great Plains. There is not a half-complete skeleton between the Eocene and Pleistocene, yet they must have been plentiful to persist to the present.

c. Scott (1962, p. 114) points up the difficulty of relating fossils from South America to other regions. For example, he notes that no Paleocene deposits, marine or continental, have been found in any part of South America, and that most of the tertiary formations in South America are little more than names. Only one formation, the Patagonian, is thought to relate directly with a formation in the northern hemisphere.

d. Dentition—tooth structure—has received much comment. Peyer (1968, p. 317-8) observed that the significance of tooth characteristics in determining relations among animals has been differently interpreted in the course of time. Many assignments of groupings based on dentitional features have proved to be untenable. Peyer then gives examples where animals with similar teeth are placed in different orders, while some with different dentition go into the same order. Limbs too may follow the same pattern in classification. He cautions that the obviously great significance of odontological features should not lead to their one-sided over-evaluation. It should always be remembered that dentitional characteristics represent only one aspect of an animal's structure. And Scott (1962, p. 397) chimes in that too exclusive a dependence upon the dentition has more than once led to unfortunate error.

**How Does Scott Dispose of the Unwanted Litopterna?**

As we observed earlier, Scott (1910) describes the classification of the Litopterna as a problem of great significance for the philosophy of evolution and that his solution has far-reaching consequences. Thus it comes as a surprise to find his pivotal monograph in storage in one of the great research libraries of the world, the University of Michigan, and to note that apparently no one had ever opened it or checked it out during the 77-year period it lay in the library after it was received as a gift from Princeton University. Further, the writer assigned to write the monograph died, and the actual author, Scott, devoted all his time in the La Plata Museum in Argentina studying other groups of Santa Cruz fossils. Finally, he states that his analysis was done without any detailed stratigraphical knowledge of the formations or any record of where the specimens were located in the strata. Such comments are hardly the foundation for such a pivotal work.

Scott (1910, pp. 5-8) devotes only several pages explaining why he concluded that the Litopterna should be placed into a separate order, and he lists some differences of dentition and skeletal remains. One could ask why he does not do the same with the tapir, rhinoceros, and other forms in the Perissodactyla order which differ far more radically. He acknowledges that other taxonomists differ:

> It is not surprising that students of the Litopterna should have reached opposite conclusions regarding the systematic position of the group, for this is merely another case of the oft recurring problem, as to how far certain resemblances are offset by differences of structure. The answer to this question is largely conditioned by the opinion which is held concerning the manner in which the evolutionary process acts, and how generally similar structures are acquired in unrelated, or distantly related groups.

As we shall see, it is no secret why Scott's conclusion was quickly and generally accepted without objection or even discussion at least in print.

**There Are Problems and Questions**

From the foregoing discussion, it seems reasonable to offer some comment about the horse and the litoptern—some of which has already been said by others in other contexts:

(1) The horse story is still based on old uniformitarian principles now repudiated by leading evolutionists, but if this outworn concept is replaced by punctuated equilibrium, which operates in such a way as to leave no fossils, then in terms of paleontology the new thinking rests on negative or missing evidence, which Darwin and many other evolutionists declared worthless (Fix, 1984, p. 169). The litopterns were removed from the order because they upset a now repudiated view of horse evolution.

(2) There is a troubling dishonesty in the way the showpiece of evolution is displayed in museums and in the texts. Many prominent authorities in evolution have declared the horse series to be fictional. No attempt is made to sort fact from speculation, and contradictory evidence is ignored (Fix, 1984, p. 189).

(3) Hapgood (1970, p. 280-91) is one of numerous authors who has documented the obvious fact that the great fossil beds of North and South America, Siberia, Africa, India, and elsewhere, are the result of catastrophic events, much more recent than conventional thinking permits, yet interpretation invariably follows the old uniformitarian path of slow gradual change over many millions of years. Darwin observed in South America that extinct animal bones, when heated in the flame of a spirit-lamp, exhaled a very strong animal odor and even burned. Yet he could not draw the obvious conclusion, because in his thinking there would be no time for present-day species to evolve. Note how catastrophism is treated in Alaska by Hopkins (1967, pp. 266-7). The author states that unfortunately most of the Alaskan Pleistocene vertebrate material now in museums have no known stratigraphic context. Speculations about age are therefore based entirely upon the morphology of the skeletal material and upon assumptions of probable trends with time of changes in such critical dimensions as horn width and tooth size. We may add that such guesses are built on now discarded uniformitarian notions.

(4) While granting the great difficulties inherent in developing a taxonomy for animals, the arbitrary and subjective path taken in orders such as those including the horse and the litopterns should be fully confessed. It is easy to document such excesses where the grossest differences are shrugged off as irrelevant, while the most minute differences are taken as crucial. A perissodactyl may have claws—the *Chalicothere*— but the litoptern described as more horselike than the horse "is not even a perissodactyl" when among other things it has a smooth spot on the heel.

(5) One kiss of death for the litopterns in South America was that the three-toed form lived after the one-toed form became extinct, according to interpretations of the time zones in which the fossils were found. This would have by itself made the assumed horse sequence untenable.

(6) Contradictory evidence is routinely discarded to make life possible for speculative animal family trees. As Scheele observes (1955, pp. 62, 80), it takes an expert to decide if horse bones are of recent vintage or 10,000 years old. In Africa, bones are collected which can scarcely be distinguished from fossil forms that are known from late Pliocene times, that is, up to at least several million years old according to the conventional time scale. Skeletal material has always been endangered, that is, destroyed, if it looks too modern. In many ways we can see that evolution is a theory that prohibits thinking rather than stimulates it.

(7) An isolated island is invoked for South America at times in an attempt to explain horse distribution or lack of it. Yet during the same periods other animals such as the giant sloth and the armored glyptodont—both extremely slow-moving creatures—apparently travelled freely between the two continents. The horse people do not talk with the sloth people. The possibility of land bridges between South America and Africa, much more recent than the conventional geological time table would allow, ought to be re-examined. This concept was argued by F. Ameghino who was the

discoverer and namer of the litopterns among his many other achievements in palaeontology (Cifelli, 1983, p. 42).

(8) It seems reasonable to conclude that if the horse story were done again from scratch by evolutionists today with no preconceptions other than their commitment to evolution, it would be very different than the fraudulent illustration with which we are confronted *ad nauseam.* Despite the tapir, which no one wants to deal with seriously, little *Hyracotherium* would not be misnamed *Eohippus* and it would be placed with the hyraxes and conies where it has always belonged. The litopterns would be placed with the horses where they belong, but of course another equally speculative horse ancestry would have been created by imaginative evolutionists, innocent of any substance. The creationist can see relationship among creatures, but the evolutionist can see relationship only in terms of supposed ancestry.

(9) The gross subjectivity of taxonomy is well illustrated by the horse/litoptern story and the many contradictory statements of taxonomists about the skeletal parts, ancestral forms, and the like. The statements given on why the litopterns are not horses are vague and unconvincing. It borders on the incredible that an authority would accuse the litopterns of practicing deception for looking so horselike. If the reasons are so obvious that litopterns are not horses, it ought to be possible to communicate this clearly in the professional literature. A careful examination of reconstructions and illustrations of the fossils is interesting. *Eohippus,* closely related to the hyrax or coney, and which bears no resemblance at all to the horse, is made to appear precisely like a miniature horse, while the litoptern branches particularly horselike are made to appear more like antelopes. Tails are made to look quite hairless, when structurally they are no different from that of the horse.

In this paper I have dealt with only a tiny aspect of the horse story. Much more remains to be sifted. But when Ruse (1982, p. 311) says, "It would be nice to see the Creationist take on the question of the horse, which is one of the best documented cases of evolutionary change," I suggest that Ruse ought to begin reading the evaluations of the horse written by his own colleagues. In this all too brief examination of the Litopterna and the horse, we have found nothing to contradict Cain. See Simpson's quote of A. J. Cain mentioned previously (p. 186-7).

In her book, *Out of Africa,* Dineson (1963, p. 121) gives a remarkable description of the behavior of natives in her area.

> . . . I learned that the effect of a piece of news was many times magnified when it was imparted in writing . . . but if a mistake was made in writing, which was often the case, as the Scribes were ignorant people, they would insist on construing it into some sense, they might wonder over it and discuss it, but they would believe the most absurd things rather than find fault with the written word.

Despite the fictional nature of horse evolution, believers cannot let go of the idea because every text and countless other materials endlessly and reassuringly repeat the fiction.

## References

Branson, E. *et al* 1952. Introduction to geology. Third edition. McGraw-Hill. New York.
Cifelli, R. 1983. The origin and affinities of the South American condylarthra and early tertiary litopterna (Mammalia). American Museum Novitates. Number 2772:1-49.
Colbert, E. 1967. Evolution of the vertebrates. Science edition. Wiley. New York.
Dinesen, Isak 1963. Out of Africa. Time. New York.
Fix, W. 1984. The bone peddlers. Macmillan. New York.
Hapgood, C. 1970. The path of the pole. Chilton. Philadelphia.
Hopkins, D. (editor). 1967. The Bering land bridge. Stanford University Press. Palo Alto, CA.
Johanson, D. and M. Edey 1981. Lucy: The beginnings of human kind. Warner Books. New York.
Keast, A., *et al.* (eds) 1972. Evolution, mammals, and southern continents. State University of New York. Albany.
Martin, P. and H. Wright (editors) 1967. Pleistocene extinctions: the search for a cause. Yale University Press. New Haven, CT.
Peyer, B. 1968. Comparative odontology. University of Chicago Press. Chicago.
Romer, S. 1966. Vertebrate paleontology. Third edition. University of Chicago Press. Chicago.
Ruse, M. 1982. Darwinism defended: a guide to the evolution controversies. Addison-Wesley. Reading, MA.
Scheele, W. 1955. The first mammals. World. Cleveland.
Scott, W. 1962. A history of land mammals in the western hemisphere. Revised edition. Hafner. New York.
________ 1910. Reports of the Princeton University expeditions to Patagonia 1896-1899. Volume VII, Part I Litopterna of the Santa Cruz beds. Princeton University Press. Princeton, NJ.
Simpson, G. 1961. Principles of animal taxonomy. Columbia University Press. New York.
Storer. T.. et al. 1979. General zoology. Sixth edition McGraw-Hill. New York.
Taylor, I. 1984. In the minds of men. TFE Publishing. Toronto.
Wright, S. 1980. Genes and organismic selection. *Evolution* 34:825-43.

## MINISYMPOSIUM ON THE SPEED OF LIGHT—PART IV

# THE ATOMIC CONSTANTS IN LIGHT OF CRITICISM

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**Abstract**

*Criticisms to the arguments offered to support a recent decay in the speed of light are answered. The hypothesis is still a viable model for any young-earth discussion.*

### Introduction

The proposal that the speed of light, c, has undergone a decay with time has drawn a variety of comments. After the critiques were formulated, a significant development has occurred. Apparently independently of our work, a Soviet scientist, V. S. Troitskii of the Radiophysical Research Institute in Gorky, U.S.S.R., has published an article (1987) in *Astrophysics and Space Science* 139:389-411 entitled

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"Physical Constants and Evolution of the Universe" coming to essentially the same conclusions as we did in our Report.** Among them was that c decay (hereafter designated CDK), and the associated variation in a number of atomic constants, explains such cosmic phenomena as the red-shift, the isotropy of the microwave background, the superluminal jets, and other superluminal observations. His treatment also set the CDK postulate on a sound footing with regard to General and Special Relativity.

One conclusion by Troitskii was that the initial value of c was of the order of 10,000 million times its current value. This was based on observations of the microwave background. Depending on which of the three most favored decay curves was followed, our Report suggested initial c values ranging from 10 million up to 100,000 million times c now if red-shift, microwave and radiometric data were to accord with theory. We are in substantial agreement with Troitskii on this point.

Troitskii indicated that it may take 10 years of observation of the constants to finally prove the point. However, he seemed unaware of the wealth of data accumulated over the last 300 years, that is included in our Report, which substantially confirms the CDK thesis. These data have been criticized by Gerald Aardsma, D. Russell Humphreys, Roy Holt and Robert H. Brown in *CRSQ*. This reply addresses the major issues they have raised point by point.

## AARDSMA'S CRITIQUE *(CRSQ* 25:36-40)

### Unusual Behavior of c

In criticizing the Report, Aardsma suggested that it was remarkable that experimental proof of the CDK theory has eluded us by just 30 years. However, as de Bray (1931) stated:

> If the velocity of light is constant, how is it that, INVARIABLY, (his emphasis) new determinations give values which are lower than the last one obtained. . . There are twenty-two coincidences in favour of a decrease of the velocity of light, while there is not a single one against it . . .

This comment reveals that the very idea of CDK came from the experimental data itself, not any theoretical source.

Today, measurements of c by laser techniques have been locked into the atomic time-frame and so, as any changes in c move lock-step with the atomic system of measurement, no variation will be noted. However, the measurements of some associated constants are not so constrained, and since about 1975 have shown a three standard deviation change in the direction of increasing c (Cohen and Taylor, 1986; Petley *et al.*, 1987). If this trend is sustained, it would appear that CDK reached a minimum around 1975-1980 and is slowly increasing again.

### Bevington Quote

The use of the Bevington (1969, p. 1) quote is misleading unless it is read beside his clarification. As it stands in the critique, it implies a gradual, one-sided asymptotic approach of the measured values of a physical quantity to their 'correct' value. However, Bevington (p. 10) makes it plain that some measurements of the quantity will be too large and others too small. They will approximate a normal distribution, with some measured values above, and others below, the true datum. That datum is then approached asymptotically from both above and below. The suggestion that the one-sided decay in c is merely the result of the normal behavior of physical measurement does not conform to Bevington or statistical theory.

### CDK Registered by 16 Methods

Aardsma states that ". . . it is highly unlikely that 16 different experimental methods would all accidentally and independently conform to the same mathematical equation describing c decay, if c was constant." I agree! That was the purpose of the analysis in the Report. Each of the 16 methods used to measure c did in fact register a statistical decay. Furthermore, in the many instances where the same equipment was used later, a lower value for c resulted. Generally, aberration measurements obtained at the Pulkova and Flower Observatories, as well as those conducted by the International Latitude Service each individually registered a decay, as did the whole suite of aberration measurements. As Aardsma states, this would be highly unlikely if c were constant. Coincidence fades as a possibility when confirmatory trends appear in 475 measurements of 11 other atomic quantities by 25 methods, as tabulated in the Report.

### Uncertainties in Decay Rates

Aardsma points out that for all 163 c data, an uncertainty in the decay rate of ±100 km/s per year would not be very 'convincing,' whereas ±1 km/s per year would. He then gives the decay for all 163 c data as 38 ±8 km/s per year. However, if the least squares procedure is followed (Bevington, 1969, pp. 104-5) the uncertainty is even smaller than that. I have corrected the Roemer point to read 292000 km/s for reasons outlined below. When this is done, all 163 c data yield a decay rate of 28.59 ±0.0016 km/s per year. Analyses of data from each of the 16 methods of c measurement give similarly 'convincing' results.

### The Data and Subjective Bias

It was suggested that subjective bias and misapplied analysis may yield unwarranted conclusions. He prefers the data to be analyzed as a whole. Yet Aardsma felt some difficulty with obviously aberrant values such as Cassini's. One way of overcoming that difficulty is illustrated by the following procedure.

a. 163 data, with uncertainties up to ±23,000 km/s:
Decay = 28.59 ±0.0016 km/s per year, r –0.26
b. 134 data, with uncertainties less than 1000 km/s:
Decay = 8.46 ±0.0018 km/s per year, r –0.60
c. 120 data, with uncertainties less than 300 km/s:
Decay = 5.06 ±0.0023 km/s per year, r –0.47
d. 64 data, with uncertainties less than 100 km/s:
Decay = 7.34 ±0.0032 km/s per year, r –0.61

The low values of the correlation coefficient, r, reflect the large numbers of points involved. When the confidence in the decay trend is calculated for those r values, they are at 99.97% or higher. In each case the mean value of the data is significantly above c with

**The Report refers to *'The Atomic Constants, Light and Time'* by Trevor Norman and Barry Setterfield.

good confidence intervals. These results indicate the general nature of the decay trend, whether or not the aberrant values and various error margins are included. This surely overcomes any objection on the basis of subjective bias.

### Seven Statistical Tests

The center-piece of Aardsma's critiques is his weighted analysis of the data. This was claimed to be necessary because of the wide range of uncertainties in the measurements. However, as shown above, it is possible to treat the data with respect to error bars without weighting.

I have used standard statistical procedures to analyze the c and other data. Firstly, analysis of data means broadly indicated CDK. Data means were usually significantly above c now for each method and for all 163 points. Newcomb in 1886 reported that the 'best' c value in 1740 was about 1% higher than that pertaining in 1880. Birge in 1941 conceded that the average c value obtained in the 1880's was in turn 100 km/s higher than that in 1940. These statements of observational fact confirm our data means analysis. Secondly, median analysis of all 163 points indicated that the hypothesis that the median value was equal to c now could be rejected at the 97% confidence level. Regardless of measurement error or date of observation the distribution of c values was significantly skewed. Thirdly, the Spearman-Rank test indicated that there was strong correlation with the date of observation for all 163 data as well as for data analyzed according to the 16 methods of observation.

Confident that these three non-parametric tests indicate CDK, we then applied the parametric tests. The least-squares linear fit and Students t-distribution were used as outlined in the Report with concordant CDK results. Analysis of residuals indicated a non-linear decay. Residuals reduced from 22,000 for an assumption of a constant c, to under 2000 for a curve fit (Malcolm, 1982). A final parametric test of the mean square successive difference performed on successive data or each third datum, produced high confidence intervals for CDK with time. Therefore, although the conclusions from Aardsma's weighted analysis differs, it can be unequivocally stated that seven major statistical tests all favor CDK. Trace the reason why the weighted analysis used by Aardsma deviates from the united testimony of the other tests.

### Weighting Procedures

Weighting may be done by a variety of criteria. One method is to weight data according to the number of observations in determining each point. When this is done for all 163 points, a decay of 11.26 ±0.000095 km/s per year results with r = -0.276 and a confidence in the decay correlation of 99.97%. Another method combines the number of observations with the inverse of the uncertainty (error bars). On this basis, all 163 data yield a decay of 1.12 ±0.00026 km/s per year with r = -0.284 and a confidence in the decay correlation above 99%.

### Inverse Square Error Weighting and r

The criterion used by Aardsma for weighting is the inverse square of the uncertainty (error bars). For all 163 data, this gives a value for the correlation coefficient r = -0.00375 with a low confidence interval. At best this is about l/73 of the value for r by the above methods. All the above methods give reasonably concordant values for r. However, weighting by the inverse square of the error produces a straight line that plainly does not fit the data. If it did fit, r would have been significantly higher and the confidence interval would indicate a better correlation as they are readily obtainable by other methods. On that basis alone, this form of weighting is inappropriate for the data set.

This method will show a decay trend if it is linear. Data with small error bars, that count highly in the analysis, would still follow a line with the same slope as earlier data with large error bars. Here it does not matter that the earlier points are virtually ignored by the analysis. The inverse square weighting still puts a straight line through all data and a good value of r will result.

### Non-Linear Decay and Weighting

To see what this weighting procedure has done to the CDK data, take the Table 11 data with the best 57 values in the Report. From about 1972 onward, the data had a mean error of about ±0.004 km/s, while the mean error from 1783 to 1927 was ±82 km/s. The inverse square weighting means that data from 1783 to 1927 were each counted 0.00015 times, while the post 1972 values were counted 62,500 times each. In other words, data prior to 1927 were essentially excluded from the analysis. All the analysis is doing is finding the decay trend post 1972. It is for this reason that Aardsma's weighted analysis is at such variance with the results from other statistical methods.

Furthermore, his method can be shown to be generally inappropriate to apply to any non-linear function, as the form of CDK clearly is. A consideration of data conforming to a physical law $y = x^2$ illustrates this well in the range x = -10 to x = 0. Allow initial measurements with large uncertainties near x = -10 to increase in precision towards x = 0 as instrumentation becomes more sophisticated with time. The inverse square error weighting technique applied to this data set will result in the conclusion that there is little discernible change in the measured quantity as only the values near x = 0 count at all. In fact, the quantity has changed considerably. The situation is the same as with CDK.

### Radioactive Intensities

On the approach adopted in our Report, radiation has an effective mass, m, which, for conservation of energy to hold, is proportional to l/G. Consequently, radiation momenta, mc, are proportional to l/c. Thus radiation has an intrinsically lower momentum when c is higher. This is one major point.

Assume that a light or heat source emits one photon per second. Place a receptor of one square centimeter area in the path of the beam. The momentum absorbed per square centimeter per second by the receptor is the radiation pressure, W (French, 1959, p. 41). It is apparent that W is proportional to l/c in a CDK scenario. However, W also expresses the energy density of radiation (French, p. 40). Thus with higher c, emitted photons have a lower energy density because of their lower momentum. If c were 10 times greater in the past, a photon's energy density would be 1/10th of one today. It would require 10 photons to give the same energy density as one photon today. In the case of radioactive decay, 10 disintegrations then would be required to give the equivalent of one disintegration today.

If radiation is considered as a wave form, the maximum amplitude, E, of a wave is related to W by $W = \epsilon E^2/8\pi$ (French, p. 40). When c is higher, W is smaller, proportional to 1/c. Thus the wave amplitudes are smaller, reflecting the lower energy density or intrinsic momentum of the wave. A simple interference experiment shows what happens when waves interfere destructively, so that the resultant amplitude is zero. At that point no light is seen: the intensity and wave energy is zero. When the resultant amplitude is a maximum, the intensity and wave energy is at a maximum. Consequently, when c is high and the wave amplitude and energy density is low, the resultant intensity will also be low. Ten waves would have to pass in one second to give the same intensity as one wave today if c were 10 times greater then. Radioactivity would therefore be less dangerous when c was higher.

### What Happens to the Kinetic Energy?

Far from being 'unnecessarily abstruse' the above argument is straightforward. However, Aardsma appears to have a problem relating to the kinetic energy involved in radioactive decay and similar processes. He states that:

> If the decay rate was 10 times higher, the heat production in the earth would also be 10 times higher . . . These conclusions are only dependent on the conservation of energy: the details of energy transport are irrelevant.

In the CDK scenario, kinetic energy is indeed conserved. What happens to it?

In the first place a distinction must be made between the intensity of radiation and its kinetic energy. The photoelectric effect illustrates this well. A very low intensity photon beam, barely discernible, still has sufficient kinetic energy to propel electrons from the metal and activate the circuit. With a higher value for c, each photon or wave has an intrinsically lower intensity as outlined above. Its kinetic energy, however, remains invariant with CDK. One photon at higher c still has the same kinetic energy needed to activate a photoelectric circuit.

There are two points that have eluded Aardsma. When c is 10 times higher and that photoelectron is ejected from the circuit, the electron velocity is also 10 times greater. When an alpha particle, whose kinetic energy is constant for all c, is stopped in the atomic lattice, the heat radiation produced (a form of light) also travels 10 times faster, but its intensity is 10 times lower. The constancy of the kinetic energy thereby ensures that all velocities are proportional to c, even though intensities are proportional to 1/c. Ten alpha particles are thereby needed to produce the same radiation intensity as one alpha particle today, but the alpha particle and the radiation each have a velocity of 10 times today's value.

### Thermal Conductivity and CDK

There is no heat build-up under these circumstances because of the second point that has apparently eluded Aardsma. Refer to equation 42 (p. 57) of our Report where the thermal conductivity, K, of a substance is proportional to c. Consequently, when c is 10 times higher, and radiation velocities are 10 times greater the heat conduction rate also increases by a factor of 10. In other words there is no heat trap. When all this is coupled with the lower intensities, the temperature rise from the decay of 10 radioactive atoms will be the same as the temperature increase from one radioactive atom today. Far from being 'an enormous problem,' higher values of c seem to be a distinct advantage.

### Radiocarbon

The most accurate curve for the historical period discussed by the critique in this section is the $\text{cosec}^2$ form. The other two forms presented in the Report give better results with distant astronomical phenomena at earlier times. It has been shown (Setterfield, 1986) that, from 1 AD to the present, theory and data agree well. Futhermore, the period covered by the Dead Sea scrolls has a variation due to CDK of 178 years which is well covered by the dating error ±200 years quoted by Aardsma.

Taking the era prior to Sesostris III near 1900 BC, Pearson *et al.* (1986) give a radiocarbon age of 3579 years before present (BP). Using the $\text{cosec}^2$ form in the Report we get 5765 years BP. This is from the C-14 observations by 0.39 half-lives (if the Libby standard of 5568 years is accepted). This contrasts with 4000 half-lives suggested in the critique.

However, why is there a discrepancy with CDK theory? For theory to come into accord with observation, living systems must have been in equilibrium with an atmosphere and oceans containing 1.31 times the C-14 available today. The reason is not hard to find. About 1900 BC, the above equation suggests that c had about three times its current value. From the astronomical side of CDK theory, this suggests that the solar neutron flux (which produces C-14 in our atmosphere) was about three times its present level. The C-14/C-12 ratio would thus be systematically higher than now. The greater preponderance of C-14 would result in the observed discrepancy. The extreme results proposed in the critique are shown to be erroneous when all CDK factors are taken into account.

### Conclusion

Dr. Aardsma's conclusion that there is a lack of positive, historical experimental evidence for CDK is itself negated by a quote from Dorsey (1944). Despite being opposed to CDK he was forced to concede that:

> As is well known to those acquainted with the several determinations of the velocity of light, the definitive values successively reported . . . have, in general, decreased monotonously from Cornu's 300.4 megameters per second in 1874 to Anderson's 299.776 in 1940 . . .

In fact, even Dorsey's reworking of the original data left c values generally above those currently prevailing. In view of this and all the facts outlined above, it would seem that the CDK proposal rests on a far more solid foundation than Aardsma claims.

HUMPHREYS' CRITIQUE (CRSQ 25:40-5)

**The Roemer and Cassini Values**

I am grateful that Humphreys has clarified the situation with Goldstein and the Roemer value. I have noted the new value of 292,000 km/s above. The latest published results by Goldstein (1975) indicate a mean residual of 31.5 seconds which is an uncertainty of ±18,000 k/s over the earth orbit radius. This more than cancels any proposed CDK. However, when analysis is done with this new value, there is no substantial difference to our conclusions. For all 163 data a decay of 28.59 ±0.0016 km/s per year results. If the Roemer point or the Cassini point are omitted individually, the remaining 162 values still give a decay. If both are omitted the decay for 161 values is 6.59 ±0.0018 km/s per year.

Cassini's value provides further ammunition for Humphreys criticism. He states that "If an analysis like Goldstein's were done on Cassini's original data, a much more accurate value might be recovered." I agree, and in fact an analysis has been done by Delambre. It contained about 100 individual observations, including all the Roemer/Picard and Cassini data, from 1667 up to 1809 for a mean date of 1738. His treatment gave a c value to an accuracy of 0.1 seconds by this method. The result of 303,320 ±65 km/s was regarded as definitive at the time of publication. This should allay any concerns about these early values. Our Report is just the tip of the iceberg. A much more extensive document is partly written in which each experiment is discussed with diagrams, as Humphreys suggested.

**'The Missing Coefficient'**

Humphreys makes some capital out of the omission of the correlation coefficient for the 63 aberration values. He correctly notes that the value of r = -0.409 which he considers ". . . a rather low value. Is it just coincidence that the single unreported coefficient is the one least supportive of the authors' hypothesis?" This contention is demolished when one considers that the significance of the value of r is also determined by the number of points, in this case 63. If there were only six points, he would be justified in his criticism. The test of a good r value comes from its confidence interval. If there were six points r = -0.409 has confidence of 78.9%. For 63 points the confidence is 99.95% that the decay trend of 4.83 ±0.0038 km/s per year is significant.

**'A Misleading Zero'?**

In the Report it is noted at the bottom of Table 3 that the aberration constant is disputed. There is a systematic error that involves different techniques in Europe to those at Washington and is enhanced by the large number of twilight observations at Pulkova. The details are too lengthy to discuss here. The Washington aberration constant is now accepted as definitive resulting in systematically low c values from the European observations. When this error is corrected for, many values of c in Table 3 increase by about 230 km/s, with some higher. This correction overcomes the perceived problem with Figure I. The zero takes account of this systematic error and is thus not misleading, nor is the decay trend spurious, and the vast majority of values in Table 3 are then above c.

The aberration values are very useful. The majority of Russian observations were done on the same instrument with the same errors. They display the decay trend well, which cannot be due to instrumental effects nor error reduction. The same comments apply to the results obtained from the Flower Observatory as well as the International Latitude Service. All give decay rates above 4 km/s per year. Far from being 'misleading,' the aberration values only serve to confirm the decay hypothesis.

**'Bias in the Analysis'?**

Humphreys calls upon systematic error to account for the trend downwards with time in the c observations. That explanation fades when it is realized that 16 different methods of c measurement all had to favor the same bias instead of the opposite one—an increase. The explanation recedes into oblivion when it is realized that a further 25 methods used to measure an additional 11 physical quantities also had to have a bias that favored CDK. That makes 41 methods acting in a united fashion. How much coincidence am I expected to accept? Contrary to suggestion in this paragraph, appropriate weighted fits have been done. The most consistent results are weights according to the number of observations per experiment.

**'A Suppressed Explanation'?**

Humphreys statements as well as those quoted from Dorsey are largely negated by actual history. It is also negated by the aberration series of measurements run over 200 years on essentially the same equipment with unchanged accuracy which recorded the decay. Furthermore, around the early part of this century, c values were successively dropping with each measurement. This can hardly be attributed to subjective bias and preconceived notions as to what results should be obtained. If Michelson had preconceived notions as to what he should find as the 'correct' value, then why did his data run 299,910 km/s in 1879.5, then drop to 299,853 km/s in 1882.8, followed by another plunge to 299,802 km/s in 1924.6 and final decay to 299,798 km/s in 1926.5? Dorsey's explanation is not sufficient.

At the same time as Michelson was working on his experiment in 1882, so was Newcomb independently. Furthermore, unknown to either of them, Nyren was assessing final results from a different method and concluded in 1883 that c was 299,850 km/s. Newcomb obtained 299,860 km/s in 1882.7 and Michelson had 299,853 km/s in 1882.8. The mean of these three values, 299,854 km/s lies above c now by 61.8 km/s even though the standard deviation of the three values is only ±5 km/s.

If the quote from Dorsey is valid in attributing CDK to ". . . (1) the observer's exaggerated opinion of the accuracy of his own work, . . ." then how is it that a standard deviation of only ±5 km/s results when three independent experimenters work at the same time and get essentially the same value (significantly above c now) by two different methods? Furthermore, if Dorsey's next statement that the trend was the result of ". . . (2) his (the observer's) inability to avoid being influenced in some measure by his preconceived opinion as to what he should find, . . ." how is it that neither knew of the other's result until some significant time later? These quoted statements by Dorsey (1944) are neither in accord with history, nor is the example that is derived from them.

**'General Remarks'**

Humphreys appears irked by the fact that several references were repeated three times and states here that:

> There are many other such redundancies, which tend to give the reader a mistaken impression that nearly 400 books and articles back up the monograph, instead of much fewer.

Note that reference 360 contains no less than 44 references under one heading to answer his inuendo. The only point of substance that remains is that concerning the rotating mass clock. This is discussed in the reply to Holt.

## HOLT'S CRITIQUE (CRSQ 25:84-8.)

This falls naturally into two sections. The first deals with perceived rotation rates of macroscopic bodies under CDK, the second addresses the issue of measurement of pulsar periods.

**Rotational Rates of Macroscopic Objects Under CDK**

Both Holt and Humphreys had problems with this issue as it was not amplified sufficiently in our Report. Humphreys also found the terminology used in the Report unhelpful. I will clarify the concept in as simple language as possible.

A. Energy Conservation

To conserve energy in the $E = mc^2$, the rest-masses of atomic particles are proportional to $1/c^2$. Measurement of electron rest-masses are not in disagreement with this contention as Table 14, p. 32 of our Report shows. Notice that the heading IV (B) in the Report was Atomic Rest-Masses and (C) was The Atom and Planck's Constant. It is in this latter section that the conservation of kinetic energy is mentioned that Holt seizes. Note that the discussion was about atomic phenomena, not macroscopic or astronomical phenomena.

Therein lies the difference. Einstein's equation as applied to chemistry and nuclear physics is essentially dealing with mass conversion to energy at the atomic level, even when larger quantities are involved. Even in the sun, where a significant mass is changed into energy, the conversion occurs at the atomic particle level. The prime application of this equation is therefore to the atomic environment and associated light photons. As a consequence, atomic rest-masses that vary as $1/c^2$ require atomic particle velocities that vary as c to maintain kinetic energy.

B. Atomic Rest-Masses

Apart from Einstein's equation, the reason atomic particles vary in rest-mass compared with macroscopic objects is found in their electromagnetic nature which is typical of the whole atomic environment. The classical equation describing electron and proton rest-masses is given by Barnes (1986, pp. 23, 24, 27, 33) as

$$m = \mu q^2/6 \pi r \qquad (1)$$

where q is the electric charge, r is the particle radius and μ is the permeability of free space. We note in our Report that q and r are invariant under CDK. Furthermore, from equation [1] on pp. 29, 30, it is shown that μ is proportional to $1/c^2$. From (1) above, when we measure varying atomic particle rest-masses we are essentially measuring the changing permeability of space. This is the only time-dependent term on the right hand side of equation (1). The $1/c^2$ variation in m is directly traceable to this cause. The permeability controls the way the atom behaves electromagnetically. As a consequence, since the rest-masses of atomic particles are electromagnetic in character, it should be no surprise that they vary with free space permeability.

In order to conserve energy within this electromagnetic environment of the atom with increasing μ as c decays, it becomes apparent that the electromagnetic mass increase must be coupled with a slower orbital speed for electrons and other particles. Consequently, the atomic clock ticks more slowly with CDK, and all atomic processes are slower, seen dynamically.

C. Consistency within each System—Variation without

However, no change is registered within the locked-in atomic system. Light always travels the same distance in one atomic second, rest-masses remain invariant, and radioactive decay rates are constant. It is only as we view the system from outside, in dynamical time, that any variation is noted. Conversely, if exterior phenomena are viewed from the atomic viewpoint, a change in behavior would be noted with time. Thus, orbital times for the earth or moon appear to be changing when measured by the slowing atomic clock when in fact they are dynamically invariant. A similar situation occurs for dynamical phenomena, such as Holt's rotating masses and Humphreys' rotating mass clock. Dynamically, we note no change in orbit or rotation times of objects in the solar system. Therefore, the macroscopic system governed by the dynamical clock is consistent within itself, just as the atomic system is. Only cross-comparisons show inconsistencies.

D. Where Two Systems Meet

Essentially we have two systems whereby mass is measured, the atomic and the dynamic. In the electromagnetic system of the atom, mass is determined in nuclear reactions from the equation $E = mc^2$, where m behaves in accord with (1). This is sometimes called the 'Q-value mass' (Dicke, 1960). On the other hand, in the dynamical system, mass is measured on a macroscopic scale. This is often referred to as inertial mass.

The question then arises: what happens when atomic masses measured by dynamical or inertial means, are compared with the same atomic masses measured by nuclear means within the atomic system? Historically, the calibration standard for both systems was the hydrogen atom with its single proton and electron. The atomic and dynamical masses (or Q-value and inertial masses) for the components of hydrogen would thus be expected to agree both by definition and calibration.

E. A Measured Discrepancy between the Two Systems

As the number of atomic particles increases for more complex atoms, a discrepancy proportional to μ should be noted. A single particle, as in (1), has a Q-value mass or atomic mass proportional to $(1 \cdot \mu)$. Multiple systems with n particles in the nucleus should be discrepant by an $(n \cdot \mu)$ factor. We can therefore predict that the Q-value mass should show a progressive increase with nucleon number against the dynamical or inertial mass. In fact, this is exactly the case. Dicke (1960, Figure 1) has called attention to this discrepancy which was verified for elements up to atomic number 21, scandium. The data support our equation

$$\Delta\,(m - M) = K(n - 1)\,\mu \qquad (2)$$

if M is the dynamical or inertial mass, m is the Q-value mass in the atomic system, both expressed in electron volts, n is the nucleon number, and $\mu$ is free-space permeability. Then equation 2 becomes:

$$m - kM\mu = K(n - 1)\mu \qquad (3)$$

If it is desired to retain an electrical theory of gravity based on the atomic environment, as Barnes (1986) does, a 'gravitational permeability term, $\mu*$, is required to overcome the discrepancy between the two systems and maintain the observed dynamical consistency on a large scale. To this end, the Report places $M = m / \mu*$, where $\mu*$ is the coupling factor or gravitational permeability which follows from equation (3).

## Measured Rotation Rates of Pulsars

A. Conceptual Error

With all of Holt's derivation in this section, he has made a conceptual error which negates his analysis. Consider a pulsar-to-earth scenario in which the emitted speed of light, $c_e$, is 10 times the received speed of light, $c_r$. The distance, L, between the two initial pulses, as the pulsar goes through one rotation, remains constant as they travel from pulsar to earth. Pulsar periods are taken to be invariant dynamically with CDK.

For the observer on earth using dynamical time, these initial two pulses will take 10 times as long to pass with the slower light-speed. As a result, the dynamical period for the pulsar will be registered as 10 times the actual dynamical period at the time of emission. However, all pulsar observations have been made using an atomic clock. This clock has been the astronomical standard since 1967, and the first pulsar (CP 1919) was discovered in 1968 (Audouze and Israel, 1985).

Now the atomic clock moves lock-step with the speed of light. In one atomic interval, light always travels the same distance. Consequently, if light were to cross a fixed distance, L, the atomic clock will always register the same period for the crossing time no matter what value c takes. When light travels 10 faster than now, the atomic clock would also tick 10 faster than now. Therefore, if we take the atomic interval needed for the distance L between the two pulses to pass the earthbound observer at the speed of light, it will always be the same as the atomic interval measured at the pulsar at the moment of emission.

Atomic clocks at the point of reception of the nth pulse will always register the same pulsar period as atomic clocks actually at the pulsar when the nth pulse was emitted, irrespective of CDK. Consequently, no information about CDK can be gleaned from this part of the process. But there is an additional factor to consider as the pulsar period is largely invariant in dynamical time.

B. Pulsars Spin-Up Atomically with CDK

If c was 10 times faster initially, the distance L between the pulses would be 10 times greater than for pulses emitted now. This occurs since a light pulse would travel 10 times further before the next is emitted. The atomic clock thus registers 10 times the current period, even though the actual period in dynamical time was constant. With CDK the atomic period of a pulsar would lessen. There would be 10 ticks on the atomic clock at emission and reception for the initial pulses, and only one tick at emission and reception for the pulses emitted now. That is to say an apparent spin-up should occur with CDK as the pulsar period lessens atomically.

Some x-ray pulsars do in fact show a spin-up. The current explanation is that it occurs because of a transfer of material from a companion star. However, in several instances, no companion can be detected (Shaham, 1987, p. 35). Other x-ray pulsars show spin-up followed by a spin-down phase (White, 1988). Some, like PSR 1937 + 21(4), are almost stable with a virtually constant period atomically. The majority of pulsars show a consistent spin-down.

C. Further Complications

There is a natural tendency for pulsars to spin-down because of loss of energy through one form or another, even though the details of all processes are still poorly understood (White, 1988). Superimposed upon this spin-down, then, is a counter-trend, the spin-up due to CDK. As a result, what we observe in all instances is a net effect. In the case of PSR 1937 + 21, the two tendencies seem to virtually cancel. Some spin-up cases are complicated further by the accretion of material from a companion star which enhances their spin-up.

It is apparent that pulsars do not offer a 'clean' system whereby CDK may be checked. The above complications are compounded by the fact that we have only been observing pulsars for 20 years. This represents somewhat less than 20 years of CDK at the pulsars due to the slowing of light from distant sources. In the above example where c was initially 10 times c now, our 20 years of observations would represent two years of CDK at the source.

D. Results from CDK Curves

Pulsars all fall within a few kiloparsecs of our sun (Audouze and Israel, 1985). Assuming then a distance of 15,000 light years and a mean pulsar period of 0.7 seconds, some calculations can be done. The three most favored curves in the Report produce spin-up rates ranging from $1.44 \times 10^{-12}$ to $2.38 \times 10^{-12}$ s/s for this mean. Given the tentative nature of these curves, this result is of the same order of magnitude as the changes being observed. When any reasonable natural spin-down is subtracted from this value, a very realistic result is achieved. Holt's quandary disappears under these circumstances.

BROWN'S CRITIQUE (CRSQ 25:91-5)

### The Missing Factor

Brown made a glaring statistical error in his paper. He omitted a $\sqrt{n}$ term from his computations, where n is the sample size. The $\sqrt{n}$ factor must be employed, as we did in the Report. (See Sharpe, 1984; Spiegel, 1972; Anon, 1977). This error by Brown negates all his assertions against CDK. If he corrected his error, his results would accord with ours and support the CDK propositions.

### Summary

The only alternative to counter this observational evidence for CDK is to invoke unacceptable implications. This has been done by Aardsma, Humphreys, and Holt. One of the first of these, historically, was a supposed conflict with General Relativity. This was shown to be invalid by several authors. Troitskii (1987 reached a similar CDK conclusion to ours and set the CDK postulate on a sound footing with regard to General and Special Relativity.

The final issue raised against CDK that mainly quelled the debate for the last 45 years was the problem of atomic behavior (Birge, 1934). It took four years for the framework to be laid by Dirac (1937) that held the potential to overcome the objection. It was another 27 years before Kovalevsky (1965) stated the principle clearly. It was not until the advent of the atomic clock that experiment revealed that this clock could indeed behave in the way Birge had considered unacceptable (Van Flandern, 1984).

A wealth of theoretical, statistical, and observational evidence supports CDK. If any difficulty arises, the Birge example above suggests that the perceived problem should be examined in depth to resolve all matters rather than rejecting the CDK proposition.

### References


Anon. 1977. TI programmable 58/59: applied statistics Texas Instruments.

Audouze, J. and G. Israel, 1985. Cambridge atlas of astronomy, Cambridge University Press. New York p. 270.

Barnes, T. G. 1986. Space medium, the key to unified physics. Geo/Space Research Foundation, El Paso, TX.

Bevington, P. R. 1969. Data reduction and error analysis for the physical sciences. McGraw-Hill. New York.

Birge, Ft. T. 1934. The velocity of light. *Nature* 134:771-2.

________ 1941. The general physical constants. *Reports on Progress in Physics.* 8:90-101.

Cohen, E. R. and B. N. Taylor. 1986. The 1986 adjustment of the fundamental physical constants. Codata Bulletin No. 63 Pergamon Press. New York. pp. 8, 9, 20.

de Bray, M. E. J. G. 1931. The velocity of light. *Nature* 127:522.

Dicke, R. H. 1960. Eotvos experiment and general relativity. *American Journal of Physics* 28:344-7.

Dirac, P. A. M. 1937. The cosmological constants. *Nature* 139:323

Dorsey, N. E. 1944. The velocity of light. *Transactions* of *the American Philosophical Society* 34 (Part 1):1-110.

French, A. P. 1959. Principles of modern physics. John Wiley and Sons. New York.

Goldstein, S. J. 1975. On the secular change in the period of Io, 1668-1926. *Astronomical Journal* 80:7, 532-9.

Kovalevsky, J. 1965. Astronomical time. *Metrologia* 1:4, 169-80.

Malcolm, D. 1982. Private communication, August 23.

Newcomb, S. 1886. The velocity of light. *Nature* 34:32.

Pearson, G. W. *et al.* 1986. High precision C-14 measurement of Irish oaks *Radiocarbon* 28:2B, 911-34. (Values averaged over central date ±50 years from Table 2 A-G.)

Petley, B. W. *et al.* 1987. A measurement of Planck's constant. *Nature* 327:605-6.

Setterfield, B. 1986. Carbon-14 dating, tree-ring dating, and speed of light decay. *E. N. Technical Journal* 2:169-88.

Shaham, J. 1987. The oldest pulsars in the universe. *Scientific American* 256(2):34-40.

Sharp, V. F. 1984. How to solve statistical problems with your pocket calculator. TAB Books. Blue Ridge Summit, PA. pp. 100, 250.

Spiegel, M. R. 1972. Theory and problems of statistics—SI (metric) edition. Schaum's Outline Series. McGraw-Hill, New York. pp. 144, 188-9.

Troitskii, V. S. 1987. Physical constants and evolution of the universe. *Astrophysics and Space Science* 139:2, 389-411.

Van Flandern, T. C. 1984. Is the gravitational constant changing? in Taylor, B. N. and W. D. Phillips, (Editors). Precision measurement and fundamental constants II. National Bureau of Standards Special Publication 617. Washington, D. C.

Wesson, P. S. 1978. Cosmology and geophysics: Monographs on astronomical subjects No. 3. Adam Holger. Bristol.

White, N. E. 1988. Spin-up changes to spin-down. *Nature* 333:708.


# BOOK REVIEWS

*Proceedings of the First International Conference on Creationism.* Volumes I and II. 1986. Creation Science Fellowship. 362 Ashland Avenue, Pittsburgh, PA 15228. 193 and 253 pp. $35.00.

Reviewed by Edmond W. Holroyd III*

The International Conference on Creationism was held in Pittsburgh, August 4-9, 1986, with the theme, "the age of the earth." It was divided into three sessions: basic, educational, and technical, with many of the presentations running simultaneously. Volume I covers the basic and educational sessions. Volume II covers the technical sessions and some additional topics, including two of the evening addresses. The evening sessions and most of the technical papers have already been summarized (Holroyd, 1987). All sessions were taped and the 60 tapes are available at $5 each or $265 for the full set. The technical and evening sessions were video taped in addition and the 19 tapes are available at $35 each with discounts for multiple tapes up to $490 for the full set. The video tapes will often contain images of color slides, overhead transparencies, and a motion picture which are not printed in the Proceedings. All tapes will record the actual discussions at the ends of the technical sessions. Only some of the questions and comments by invited reviewers (not necessarily creationists) and by conference attendees are included after each technical paper in volume II. The author's responses printed there may be different and more refined than those recorded on tape.

Both volumes are arranged alphabetically by first author. In this review the papers of Volume I will be regrouped by subject. Robert E. Walsh reviews the correct rules for interpreting Biblical text. He then uses the rules to show that the Hebrew word for "day" in Genesis 1 can only be correctly interpreted as a solar day of 24 hours. Jerry Bergman discusses the importance of time in the discussion of origins. He reviews dating systems, the insufficiency of even 4.6 billion years or the evolutionary processes, and the Biblical understandings of time. Donald E. Chittick reviews the historical development of modern science. He points out how time and the age of the earth became important issues. Though there have been

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