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8 May 2007 (last update **16 October 2010**), updates 27 Oct. 2007 with Appendix on Cameron & du Toit 2007: "Winning by a Neck…" pp .62-78; 5 Oct. 2008 some language corrections and a brief comment on Brown et al. 2007: "Extensive population genetic structure in the giraffe" on p. 79.

The Evolution of the Long-Necked Giraffe

(*Giraffa camelopardalis* L.)

What do we really know?

(Part 2)

As for **Part 1**, see http://www.weloennig.de/Giraffe.pdf

Some Questions, Facts and Quotations to Supplement Part 1

Repetitio est mater studiorum –Repetition is the best teacher (literally: the mother of studies)

Summary

Introduction: the story which is commonly taught in high schools about the evolution of the long-necked giraffe by natural selection (*feeding-competition*-hypothesis) fails to explain, among other things, the size differences between males and females. Giraffe cows are up to 1.5 meters shorter than the giraffe bulls, not to mention the offspring. The wide migration range of the giraffe and the low heights of the most common plants in their diet likewise argue against the dominant selection hypothesis. Now to the main points: 1) The fossil „links“, which according to the theory should appear successively and replace each other, usually exist simultaneously for long periods of time. 2) Evolutionary derivations based on similarities rely on circular reasoning (to refer once more to Kuhn's statement) 3) The giraffe has eight cervical vertebrae. Although the 8th vertebra displays almost all the characteristics of a neck vertebra, as an exception to the rule the first rib pair is attached there. 4) The origin of the long-necked giraffe by a macromutation is, due to the many synorganized structures, extremely improbable. 5) Sexual selection also lacks a mutational basis and, what is more, is frequently in conflict with natural selection („head clubbing“ is probably „a consequence of a long neck and not a cause“; see also Michell et al. 2009). 6) In contrast to the thus-far proposed naturalistic hypotheses, the intelligent design theory is basically testable. 7) The long-necked giraffes possibly all belong to the same basic type inasmuch as 8) a gradual evolution from the short-necked to the long-necked giraffe is ruled out by the duplication of a neck vertebra and the loss of a thoracic vertebra. 9) Chance mutations are principally not sufficient to explain the origin of the long-necked giraffe. 10) The intelligent design theory offers an adequate and satisfying solution to the problems and points to numerous „old“ and new research projects. 11) Mitchell and Skinner present a good analysis of the selectionist problem; however, their phylogenetic hypotheses presuppose the correctness of the synthetic evolutionary theory, and their claims of „*intermediate forms*“ are unproven.

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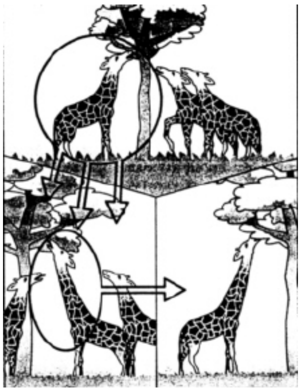
Introduction to Part 2

Is the Darwinian Theory as taught in high schools in harmony with (1) the sexual dimorphism, (2) the body size of the young or (3) migration range as well as (4) the heights of the plants in the giraffe's diet?

When one does a *Google* search on „Giraffe“ and „Evolution“, the first result listed (thus the most frequently visited site)* briefly presents the theories of Lamark and Darwin on giraffe evolution. The authors are Marzena Franek, Anne-Kathrin Johannsmeier, Mara Jung, Susana Santos and Anne-Kristin Schwarz from the Gymnasium Meschede (2001). Lamarck's theory is said to be refuted by the fact that „acquired characteristics are not inherited.“ Darwin's theory is presented as the correct one:

> „In one generation of giraffes there is, by chance, an animal whose neck is longer than those of the other animals. This one survives, since it has a clear advantage in reaching higher leaves. This animal has sufficient nutrition to survive and multiply. In following generations several giraffes with longer necks arise, who have inherited the trait. Over many generations, longer necked giraffes continually made their way in life, and so today's form developed.“

The following figure serves to illustrate the thesis in the textbook *Evolution, Materialien für die Sekundarstufe II, Biologie*, 1999, p. 15 by Peter Hoff, Wolfgang Miram and Andreas Paul (Schroedel-Verlag, Hannover):



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*Repeatedly checked, last on 2 March 2007.

One of the most noble and important goals of school education should consist of helping young people learn to be critical thinkers, and to give them the ability to make reasoned judgements.

Considering this question in connection with giraffe origins, one should cite, above all, the decisive fact that the giraffe cows are, on average, at least ***a full meter shorter*** than giraffe bulls, not to mention the ***much shorter offspring***.

„The normal heights at birth oscillates between 170 and 190 cm.“ – I. Krumbiegel 1971, p. 61. „The tallest giraffe, from Kenya and undoubtedly a male, measured 5.88 meters…the largest female, from northern Kalahari, measured 5.17 meters…“ – Dagg and Foster 1982, p. 71; also among *captive giraffes* we find a difference of some 1.5 m (according to Fig. 6-2 of the same work, likewise p. 71). Since on the next page the authors estimate the average difference at some 1m, this estimate may be somewhat too cautious.

If the mothers, in competition with the fathers, do not have anything to browse, they cannot nurse their offspring anymore (the young animals „may suck for up to two years, but they supplement the milk with solids at about one month. Perhaps they need relatively little milk because of the high nutritional value of the acacia tips they eat.“ – Dagg and Foster 1982, p. 138; when almost grown, they are 3 ½ to 4 years old – Sherr 1997, p. 70). Although the young animals themselves begin to graze after only a few weeks, neither they nor their mothers would have a chance to survive under the conditions assumed above. According to this figure, only the one mutant animal would survive, and thus the population would die out instead of further evolving and becoming taller (C. Pincher already presented this problem in a *Nature* Article of 1949 and other researchers did so independently of him). Doesn’t such an „ugly fact“ – as Huxley once expressed it – indeed call into question the entire Darwinian explanation of giraffe evolution? (“The great tragedy of Science – the slaying of a beautiful hypothesis by an ugly fact" Huxley 1870, but there are further “ugly facts”—see below.)

Why then is such a minor but decisive fact, which could easily be conveyed in a biology lesson, consistently left out of almost all textbooks and school instructions? Could it be that many evolutionary theorists prefer to impart evolution as a fact rather than to teach critical thinking?

James Perloff comments the question of the origin of the giraffe as follows (2003, pp. 54/55, colored boldface in the text, here and in the following quotes, are mine):

> “Did giraffes really develop long necks because they lived around high vegetation, causing the extinction of shorter-necked giraffes? How then did young giraffes survive? **Isn't it more likely that, facing such an environment, giraffes would have simply migrated to where food was more accessible?** Colin Patterson of the British Museum of Natural History noted:
>
> > It is easy enough to make up stories of how one form gave rise to another, and to find reasons why the stages should be favoured by natural selection. But such stories are not part of science, for there is no way of putting them to the test.
>
> Gould et al. wrote in *Paleobiology:*
>
> > Paleontologists (and evolutionary biologists in general) are famous for their facility in devising plausible stories; but they often forget that plausible stories need not be true.
>
> And I again quote France's Pierre-Paul Grassé:
>
> > Today, our duty is to destroy the myth of evolution, considered as a simple, understood, and explained phenomenon which keeps rapidly unfolding before us. Biologists must be encouraged to think about the

> weaknesses of the interpretations and extrapolations that theoreticians put forward or lay down as established truths. The deceit is sometimes unconscious, but not always, since some people… purposely overlook reality and refuse to acknowledge the inadequacies and the falsity of their beliefs.

While evolutionists can think up logical-sounding reasons for why natural selection produced certain things, many phenomena resist such rationalization. Canadian biologist Ludwig Bertalanffy told a Symposium:

> "I, for one, in spite of all the benefits drawn from genetics and the mathematical theory of selection, am still at a loss to understand why it is of selective advantage for the eels of Comacchio to travel perilously to the Sargasso sea, or why *Ascaris* has to migrate all around the host's body instead of comfortably settling in the intestine where it belongs; or what was the survival value of a multiple stomach for a cow when a horse, also vegetarian and of comparable size, does very well with a simple stomach; or why certain insects had to develop those admirable mimicries and protective colorations when the common cabbage butterfly is far more abundant with its conspicuous white wings. One cannot reject these and innumerable similar questions as incompetent; if the selectionist explanation works quite well in some cases, a selectionist explanation cannot be refused in others.
> In current theory, a speculative "may have been" or "must have been" (expressions occurring innumerable times in selectionist literature) is accepted in lieu of an explanation which cannot be provided. … in my opinion, there is no scintilla of scientific proof that evolution in the sense of progression from less to more complicated organisms had anything to do with better adaptation, selective advantage or production of larger offspring.""

Regarding the question *"Isn't it more likely that, facing such an environment, giraffes would have simply migrated to where food was more accessible"* the following facts on the migration and abundance of plant species in the giraffe's diet should be considered:

Y. le Pendu and I. Ciofolo (1999, p. 341):

> "The last population of giraffes in west Africa lives in Niger in an unprotected Sahelian region inhabited by farmers and herders. The spatial behaviour of each individual of the population (n = 63) was studied by direct observation during 15 months. Two-thirds of the population were resident in the tiger bush in the rainy season and in the nearby area of Harikanassou, a sandy agricultural region, in the dry season. Rainy season and dry season home ranges were mutually exclusive and individual home ranges were overlapping when considering one season (rainy season: 84%; dry season: 67%). **The mean size of the seasonal home ranges of these resident giraffes during the dry season (90.7 km^2) was twice the mean size during the rainy season (46.6 km^2).** A third of the population moved **80 to 200 km in three directions, and two giraffes from an isolated group from Mall moved 300 km along the Niger River.** Long distance movements of such length have never been reported before [see, however, below], and several explanations are proposed: previous distribution, social transmission, hydrographic network and food availability, poaching events. The giraffes in Niger do not avoid rural communities; indeed, they live in densely populated regions. Furthermore, their movements, synchronized with human activities in these regions, are representative of life conditions in the Sahel. "

J. T. du Toit (1990, p. 301):

> "Home range data were collected concurrently from four syntopic browsing ruminant species in a conserved savannah ecosystem. **Mean home range areas were: giraffe (*Giraffa camelopardalis*) 282 km^2**; kudu (*Tragelaphus strepsiceros*) 21.9 km^2; impala (*Aepyceros melampus*) 5.81 km^2; steenbok (*Raphicerus campestris*) 0.62 km^2. "

L.E. Caister, W.M. Shields and A. Gosser (2003, p. 201):

> "Niger is host of the last free-roaming herd of *G. c. peralta* (*Giraffa camelopardalis peralta*). We examined the foraging preferences of these giraffe in their **dry-season habitats**, with the goal of preserving the herd in the regions that they currently inhabit. The current dry-season habitat comprises two distinct vegetation zones. In both of these zones the giraffe must exist alongside the people of this region. The giraffes exhibit a sexual segregation in their dry-season habitat selection and forage choices. **The females show a strong preference for the intermediate zone (IM) when lactating.** The males and pregnant females show a preference for the Dallol Bosso (DB). Nursing cows exhibit an avoidance of tannins. Bulls and non-nursing cows prefer high protein and high fat forage, while subadults show a strong preference for high protein and carbohydrate contents and moderate tannin levels. ***Combretum glutinosum* is the preferred species for adults of both sexes in the IM.** Males and females have strong preferences for both ***Acacia nilotica* and *Acacia seyal*** in the DB. Sub-adults of both sexes strongly prefer ***Prosopis africana*** in the IM. **Unlike females, males retain their preference for *A. nilotica* when in the IM."**

D.M. Parker, R.T.F. Bernard, S.A. Colvin (2003, p. 245):

> "Giraffe are extralimital in the Eastern Cape Province, South Africa, where recent local introductions have persisted despite limited research into their impact on the indigenous flora. The diet of 15 giraffe at the Shamwari Game Reserve was recorded by direct observation during summer (March/April) and winter (July/August) 2001, quantifying diet by frequency of occurrence (individual records scored and expressed as a percentage of the total). Preference indices were also calculated. Habitat use was measured by the number of hours giraffe fed in different habitats. **The diet comprised of 14 plant species, the most important species being *Rhus longispina* (47.9%), *Acacia karroo* (25.7%) and *Euclea undulata* (17.6%).** Importance of *R. longispina*, *A. karroo* and *Tarchonanthus camphoratus* fluctuated seasonally. *Rhus longispina* was more important in winter with a corresponding decrease in feeding on *A. karroo*. *Tarchonanthus camphoratus* was only consumed during summer. *Acacia karroo* thickets (previously disturbed areas) were utilized most (summer 12 h; winter 9 h), with alternative habitats utilized more often in winter than in summer. We suggest that the seasonal fluctuation in the importance of *R. longispina* and *A. karroo* reflects the deciduous nature of *A. karroo*."

Rhus longispina, which, in the difficult dry season, is one of the most important nutrient sources for the giraffe, making up 47,9% of its diet as cited above, grows on average only to a height of 3 m, *Acacia Karroo* or „Karroo thorn" shows an average height of 4,41 m*, other bushes or trees of less (or no) importance in the diet seem to be taller (B. *Acacia mellifera*).

Y. le Pendu and I. Ciofolo (2002, p. 183):

> "The remaining West African giraffes (*Giraffa camelopardalis*) are found in Niger (62 individuals in January 1998). Their feeding behaviour was studied by direct observation during two periods of 6 and 12 months. The giraffe's diet is diverse: **at least 45 plant species were eaten**, depending on spatial arrangement and a given plant's stage of growth. Time spent browsing during the dry season was twice that devoted to browsing during the rainy season **(46 and 23 % respectively**). Time spent feeding on a plant was correlated with the total time spent feeding on this species. Giraffe browsed at a level which domestic animals cannot reach usually, **between two and four metres for females and juveniles and between four and five metres for adult males.** The total browsing time of a species was not correlated with its occurrence in the field [so giraffes are selective; note by W.-E.L]. The small number of giraffes, the diversity of their diet and the lack of competition with domestic animals indicate a weak impact of the giraffe on the vegetation and the possibility for the population to increase in this area. Giraffe are located in an area with a strong human presence and they feed on species used daily by the rural communities. This brings to light the close link existing between communities living in the same environment. The acknowledgement of that link requires the consideration of ecological factors in their relationship with regional economic expansion programs."

This report shows very clearly that – instead of a merciless struggle for nutrient resources that would lead to the demise of all smaller individuals and to the exclusive survival of the tallest animals – the resources are well shared: species survival by cooperation rather than brutal selection.

D. M. Parker (2004, p. 39):

> "Giraffe typically select more than 20 plant species in their diet (Leuthold & Leuthold, 1972; Hall-Martin, 1974b; van Aarde & Skinner, 1975; Sauer *et al.*, 1977; Sauer *et al.*, 1982). This is ascribed to the fact that **giraffe are capable of traversing large distances within their home ranges where they encounter and use a wider variety of vegetation types than other browsers** (Skinner & Smithers, 1990). In addition, due to their inherent need to consume large quantities of forage to sustain their metabolic and reproductive requirements (Bell, 1971; Pellew, 1984a) giraffe have less time to be selective [[?] perhaps in the dry season? Note by W.-E.L.] and consequently include a wide diversity of plant species in their diet (Innis, 1958). The results for the present study conform to such a finding with more than twenty species being consumed at each site. However, the number of species consumed was greater at Kariega (37) than the other two sites (22 and 23 respectively). The small size of Kariega provides a likely explanation for such a difference, as being confined into such a small area at a relatively high density (there are similar numbers of giraffe as at Shamwari, but in a smaller area) forces the animals to feed on a greater number of species. Although, the giraffe at all sites consumed a large variety of species, the majority (60-90%) of the diet comprised two or three species, the most important of which was *Acacia karroo*."

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*Maximum height 8,70 m; However, the species can grow substantially larger in regions with lots of precipitation.

In this context we may be reminded of the observation of Simmons and Scheepers (1996, p. 771):

> "A classic example of extreme morphological adaptation to the environment is the neck of the giraffe (*Giraffa camelopardalis*), a trait that most biologists since Darwin have attributed to competition with other mammalian browsers. However, in searching for present-day evidence for the maintenance of the long neck, **we find that during the dry season (when feeding competition should be most intense) giraffes generally feed from low shrubs, not tall trees**; females spend over 50% of their time feeding with their necks horizontal; both sexes feed faster and most often with their necks bent; and other sympatric browsers show little foraging height partitioning. **Each result suggests that long necks did not evolve specifically for feeding at higher levels**. Isometric scaling of neck-to-leg ratios from the okapi *Okapia johnstoni* indicates that giraffe neck length has increased proportionately more than leg length – an unexpected and physiologically costly method of gaining height. **We thus find little critical support for the Darwinian feeding competition idea**" (for comments on their counter-hypothesis of sexual selection, see below; see also Mitchell et al. 2009).

Numerous further details are discussed by the authors on pages 775-777, 781/782 and 784 of their work; see for example, also the points which are quoted in Note(1) toward the end of our paper.

Result: *Giraffes do not remain in a definite, narrowly bounded region and stretch their necks ever higher until all leaves are consumed, and all smaller giraffes – cows, calves and teenagers – have died out, but rather often migrate over long distances; they are thus „capable of traversing large distances within their home ranges where they encounter and use a wider variety of vegetation types than other browsers"* (see Parker above).

As the migrations of numerous smaller animal species shows, there is no reason to assume that the supposed ancestors of the long-necked giraffes should have manifested a fundamentally different behaviour.

This omission of inappropriate biological facts – inappropriate at least for the educational goal of teaching evolutionary theory as an absolute fact – is found not only at the high school level, but (as suggested above) also at the level of scientific publications (cf. the numerous examples in part 1 at http://www.weloennig.de/Giraffe_Erwiderung.1a.pdf).

As promised in the first part of the paper, we will now continue with the discussion of examples and further scientific details, which place Darwinism (more precisely, the synthetic evolutionary theory) in question:

1. Many species and genera of the Giraffidae lived contemporaneously with the supposed ancestors and thus often co-existed for millions of years with their „more evolved" descendants

One point regarding the origins of the giraffe that for our consideration seems to be

Table 1: Extracted from the data of Mikael Fortelius(2) regarding the deer-like and giraffe genera, which Hunt and/or Mitchell and Skinner consider as intermediate links (*Palaeomeryx* and *Climacoceras* according to the original work of Hamilton 1978a and b)(2a), *G. priscilla* according to Basu 2004(2a1)).

Deer-like Ungulates	Maximum Age	Minimum Age
Family Palaeomerycidae		
Genus ***Palaeomeryx***		
Palaeomeryx spec.	15 Mill. Years	?
Family Climacoceratidae		
Genus ***Climacoceras***		
Climacoceras africanus	13.8 Mill. Years	?
Climacoceras gentryi	13.8 Mill. Years	?
Family Canthumerycidae(2b)		
Genus ***Canthumeryx***		
Canthumeryx sirtensis	22.8 Mill. Years	11.2 Mill. Years
Canthumeryx indet.	18 Mill. Years	15.2 Mill. Years
Genus ***Injanatherium***		
Injanatherium arabicum	15.2 Mill. Years	12.5 Mill. Years
Injanatherium hazimi	9 Mill. Years	8.2 Mill. Years
Short-necked Giraffes		
Subfamily Palaeotraginae		
Genus ***Giraffokeryx***		
Giraffokeryx cf. punjabiensis	17.2 Mill. Years	5.3 Mill. Years
Genus ***Palaeotragus***		
Palaeotragus lavocati	12.5 Mill. Years	11.2 Mill. Years
Palaeotragus rouenii	11.2 Mill. Years	9 Mill. Years
Palaeotragus germaini	14.7 Mill. Years	7.1 Mill. Years
Palaeotragus expectans	12.9 Mill. Years	?
Palaeotragus pawlowae	9 Mill. Years	7.1 Mill. Years
Palaeotragus coelophrys	13.6 Mill. Years	7.1 Mill. Years
Palaeotragus primaevus	**18** Mill. Years	11.2 Mill. Years
Palaeotragus indet.	11.2 Mill. Years	**1.76** Mill. Years
Genus ***Samotherium***		
Samotherium africanum	**14.6** Mill. years	**3.4** Mill. Years
(And many other species of Samotherium as well as numerous further genera of the short-necked giraffes)		
Long-necked Giraffes		
Genus ***Bohlinia***		
Bohlinia attica	**11.2** Mill. Years	**5.3** Mill. Years
Genus ***Giraffa***		
Giraffa attica	9 Mill. Years	7.2 Mill. Years
Giraffa jumae	**7.1 (12)** Mill. Years	**0.01** Mill. Years
Giraffa camelopardalis	3.56 Mill. Years	Present
Giraffa stillei	5.3 Mill. Years	2.4 Mill. Years
Giraffa gracilis	3.56 Mill. Years	2.6 Mill. Years
Giraffa pygmaea	5.3 Mill. Years	2.6 Mill. Years
Giraffa pomeli	3.56 Mill. Years	1.7 Mill. Years
Giraffa priscilla	**12** Mill. Years	?
Giraffa undet.	**9** Mill. Years	3. 56 Mill. Years
Giraffa spec.	7.1 Mill. Years	**0.01 Mill. Years**

of special importance, and which is frequently ignored in evolutionary discourses, is the fact that several of the species and genera which in the evolutionary schemes of

Kathleen Hunt and many other authors appear ***successively*** co-existed ***simultaneously***(2b1). In the first part of this work we have already presented several facts that we now want to supplement. Remember (see Part 1, pp. 11 ff.) for example the often-cited presentation of Hunt:

> "Giraffes: Branched off from the deer just after *Eumeryx*. The first giraffids were *Climacoceras* (very earliest Miocene) and then *Canthumeryx* (also very early Miocene), then *Paleomeryx* (early Miocene), then *Palaeotragus* (early Miocene) a short-necked giraffid complete with short skin-covered horns. From here the giraffe lineage goes through *Samotherium* (late Miocene), another short-necked giraffe, and then split into *Okapia* (one species is still alive, the okapi, essentially a living Miocene short-necked giraffe), and *Giraffa* (Pliocene), the modern long-necked giraffe."

Similarly Mitchel and Skinner 2003, p. 51, write:

> "The Canthumerycids gave rise to the okapi and giraffes via the intermediate forms of *Giraffokeryx, Palaeotragus* sp. (of which the okapi is the extant form), *Samotherium* sp. and *Bohlinia* sp. all of which are extinct."

Starck on the other hand already points to some difficulties when he writes (cf. D. Starck cited in Part 1, p. 14):

> "An older form, † *Zarafa* (= † *Canthumeryx*) belongs to the early Miocene in North Africa. In the late-Miocene Giraffidae († *Palaeotragus*, † *Giraffokeryx*) appear in Eurasia. Along with these short-necked forms, the long-necked giraffes appear at **more or less the same time** as Savanna dwellers. († *Honanotherium* in Africa, Eurasia). In the late Tertiary another family line of Giraffidae appears in Eurasia and Africa, the Sivatheriidae with † *Helladotherium*, † *Sivatherium* among others. These were animals with heavy, cow-like body forms, and with branched, antler-like ossicones, which survived into the Pleistociene" (Starck 1995, p. 999).

We now add the so far known geological facts in the quote from Hunt, and Mitchell and Skinner. Let's first turn to Hunt (further details in the first part):

> "Giraffes: Branched off from the deer just after *Eumeryx*. The first giraffids were *Climacoceras* (very earliest Miocene [wrong, Middle Miocene, **13.8 million years – ?**]) and then *Canthumeryx* (also very early Miocene [**22.8 – 11.2 million** years before present]), then *Paleomeryx* (early Miocene [probably Middle Miocene, **15 million years - ?**]), then *Palaeotragus* (early Miocene [**18 – 1.76 million years** before present]) a short-necked giraffid complete with short skin-covered horns. From here the giraffe lineage goes through *Samotherium* (late Miocene [wrong, Middle Miocene, **14.6 – 3.4 million** years before present), another short-necked giraffe, and then split into *Okapia* (one species is still alive, the okapi, essentially a living Miocene short-necked giraffe [so a **living fossil** covering most of the time, **18 million years to present**]), and *Giraffa* (Pliocene [wrong, Middle Miocene for *Giraffa*, **12 million years to present**, and at the border to Middle Miocene for *Bohlinia*, **11.2 – 5.3 million** years before present, the genus being as large as *Giraffa*), the modern long-necked giraffe."

According to Hunt then the order is: (1) *Canthumeryx*, (2a) *Palaeomeryx* (for Mitchell and Skinner *Giraffokeryx* is second (2b) and *Palaeomeryx* is missing), (3) *Palaeotragus*, (4) *Samotherium*, (5) *Giraffa* (according to Mitchell and Skinner *Bohlinia* is fifth, and then comes *Giraffa*).

And now the time additions for the quote from Mitchell and Skinner 2003, p. 51:

> "The Canthumerycids [**22.8 – 11.2 million** years before present] gave rise to the okapi and giraffes via the intermediate forms of *Giraffokeryx* [**17.2 – 5.3 million** years before present], *Palaeotragus* sp. (of which the okapi is the extant form, [**18 million years to present**]), *Samotherium* sp. [Middle Miocene, **14.6 – 3.4 million** years before present] and *Bohlinia* sp. [**11.2 – 5.3 million** years before present, the genus being as large as *Giraffa*] all of which are extinct."

The order according to Mitchell and Skinner is thus: (1) *Canthumeryx*, (2a) *Giraffokeryx* (according to Hunt *Palaeomeryx* (2b)), (3) *Palaeotragus*, (4) *Samotherium*, (5) *Bohlinia* (Hunt places *Giraffa* directly after *Samotherium*) and (6)

Giraffa. *Okapia* is number 7 in this sequence. According to Hunt, it has descended from *Samotherium* but according to Mitchell and Skinner the okapi is “the extant form“ of *Palaeotragus*.

In order to elucidate the temporal „overlapping“ of forms that in most evolutionary treatises solely appear successively, I list for each genus the time period in which it co-existed with other genera. The reader should be aware that the *present* maximal dates are presented. I would hardly be surprised if further paleontological research would extend the overlapping further, in extreme cases even so far that the majority of the genera would have co-existed from the very beginning (of their family). (That many dates in the following presentation are redundant is to be expected.)

(1) ***Canthumeryx*** (22.8 – 11.2 million years before present), “the earliest and most primitive Giraffidae” (Geraads, 1986, p. 465), thus lived according to the current, still incomplete, dates (minimum dates) ***contemporanously*** with ***Giraffokeryx*** (17.2 – 5.3 million years before present) about **6 million** years, with ***Palaeomeryx*** *an unknown period of time, with* ***Palaeotragus*** (18 – 1.76 million years before present), contemporariously for about **7 million** years, with ***Samotherium*** (14.6 – 3.4 million years before present) some **3 million** years and it could have even met the almost 6 m tall ***Giraffa*** as well as ***Bohlinia*** (unless their different habitats prevented this).

(2a) ***Giraffokeryx*** (17.2 – 5.3 million years before present) lived ***simultaneously*** with ***Canthumeryx*** (**22.8 – 11.2 million** years before present) for about **6 million** years, with ***Palaeomeryx*** an unknown period of time, with ***Palaeotragus*** (18 – 1,76 million years before present) for some **12 million** years, with ***Samotherium*** (14.6 – 3.4 million years before present) simultaneously some **10 million** years, with ***Bohlinea*** (11.2 – 5.3 million years before present) **6 million** years, and with ***Giraffa*** (12 million years to present) simultaneously **7 million** years..

(2b) ***Palaeomeryx*** lived contermporanously with ***Canthumeryx***, ***Giraffokeryx***, ***Palaeotragus***, *and* ***Samotherium*** (***Palaeomeryx*** finds are dated from about 15 Million years ago, earlier finds seem to be uncertain).

(3) ***Palaeotragus*** (18 – 1.76 million years before present) libed ***simultaneously*** with ***Canthumeryx*** (22.8 – 11.2 million years before present) for about **7 million** years, with ***Giraffokeryx*** (17 – 5.3 million years before present) **12 million** years, with ***Palaeomeryx*** an unknown period of time, with ***Samotherium*** (14.6 – 3.4 million years before present) simultaneously some **11 million** years, with *Bohlinea* (11.2 – 5.3 million years before present) contemporanously **6 million** years and with *Giraffa* (12 million years to present) for **10 million** years.

(4) ***Samotherium*** (14.6 – 3.4 million years before present) lived ***simultaneously*** with ***Canthumeryx*** (22.8 – 11.2 million years before present) more than **3 million** years, with ***Giraffokeryx*** (17.2 – 5.3 million years before present) **9 million** years, with ***Palaeotragus*** (18 – 1.76 million years before present) some **11 million** years, with ***Palaeomeryx*** possibly an unknown period of time, with ***Bohlinea*** (11.2 – 5.3 million years before present) simultaneously **6 million** years and with ***Giraffa*** (12 million years to present) **8 million** years.

(5) ***Bohlinia*** (11.2 – 5.3 million years before present) possibly lived contemporaneously with ***Canthumeryx*** (22.8 – 11.2 million years before present) an unknown period of time, with ***Giraffokeryx*** (17.2 – 5.3 million years before present) ***simultaneously*** **6 million** years, with ***Palaeomeryx*** there is no known overlap, with ***Palaeotragus*** (18 – 1.76 million years before present) likewise some **6 million years**, with ***Samotherium*** (14.6 – 3.4 million years before present) again about **6 million** years, with ***Giraffa*** (12 million years to present) simultaneously **6 million** years.

(6) ***Giraffa*** (12 million years to present) lived ***simultaneously*** with ***Canthumeryx*** (22.8 – 11.2 million years before present) some **1 million** years, with ***Giraffokeryx*** (17.2 – 5.3 million years before present) **7 million** years, with ***Palaeotragus*** (18 – 1.76 million years before present) about **10 million** years, with ***Samotherium*** (14.6 – 3.4 million years before present) simultaneouly some **8 million** years, with ***Bohlinea*** (11.2 – 5.3 million years before present) contemporanously **6 million** years. (So far no overlapping with *Palaeomeryx*, but the dates for *Palaeomeryx* are still very incomplete.)

(7) ***Okapia*** is, according to Hunt, a descendent from ***Samotherium***, but according to Mitchell and Skinner ***Okapia*** is „the extant form“ of ***Palaeotragus*** (that is 18 million years – to present). In the latter case, okapi-like forms lived ***simultaneously*** with ***Canthumeryx*** (22.8 – 11.2 million years before present) for about **7 million** years, with ***Giraffokeryx*** (17.2 – 5.2 million years before present) **12 million** years, with ***Palaeomeryx*** an unknown period of time, with ***Samotherium*** (14.6 – 3.4 million years before present) simultaneously some **11 million** years, with ***Bohlinea*** (11.2 – 5.3 million years before present) simultaneously **6 million** years and with ***Giraffa*** (12 million years to present) simultaneously **12 million** years.

In the following, the temporal overlap of the genera are presented graphically. We begin, in the figure, with (7) *Okapia* and proceed in reverse order from the above list [(6), (5),(4),(3),(2),(1)] and add *Climacoceras*. The greatest morphological gaps exists between the long-necked giraffes (*Giraffa*, *Bohlinia*) and the short-necked giraffes (*Samotherium*, *Palaeotragus*, *Giraffokeryx*) and between the short-necked giraffes and *Palaeomeryx* (Superfamily Cervoidea) as well as the antelope *Canthumeryx* and the genus *Climacoceras*, but which does not fit chronologically.

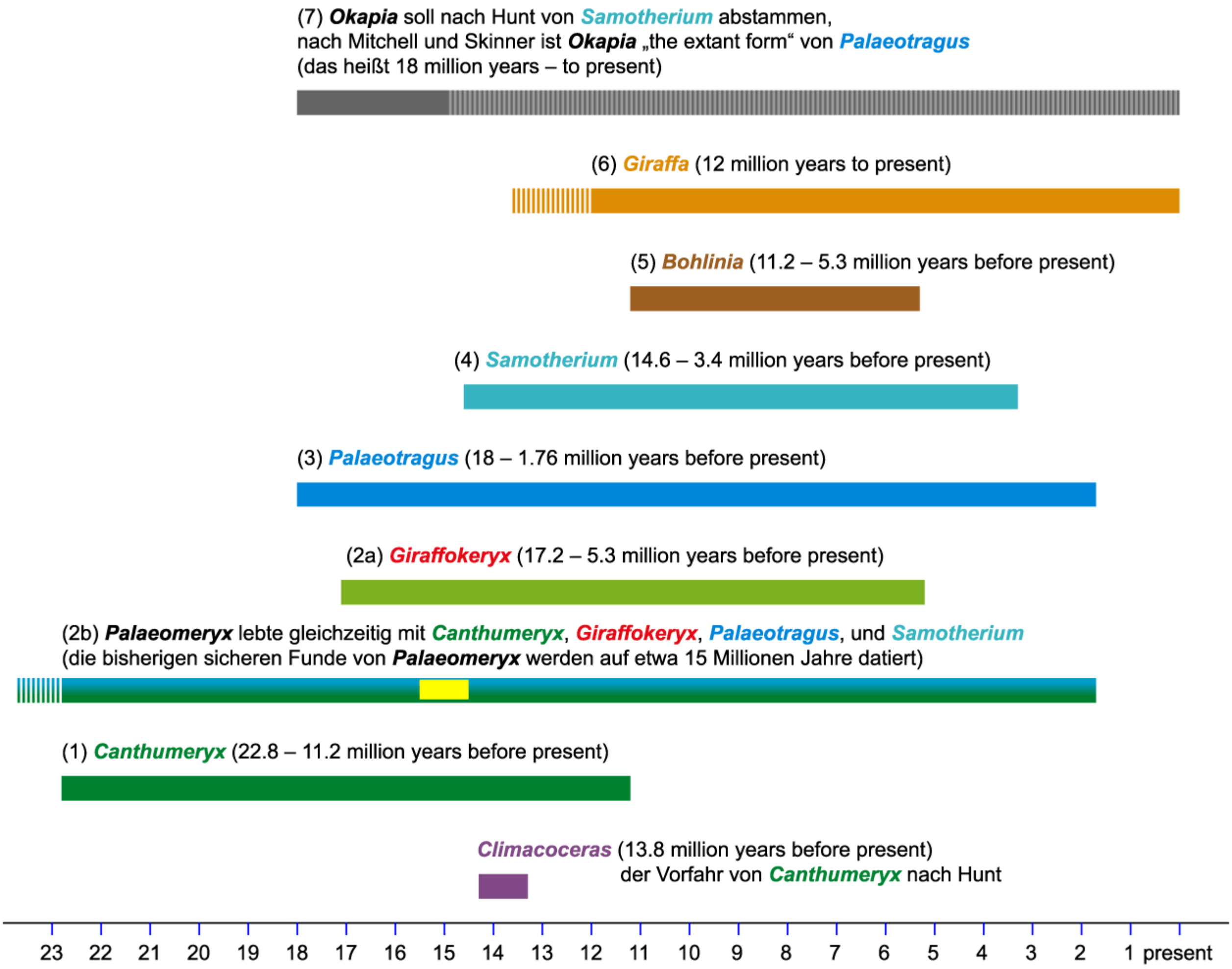


Fig. 1: Temporal overlap of the short-necked giraffes and deer which are considered possible ancestors of the long-necked giraffes. For questions of synonyms and species boundaries within the long- and short-necked griaffes, see the discussion below. *Giraffa jumae* was first dated at 12 million years ago (see details below), for the conservative dating of *G. priscilla* at 12 million years, see the Notes(2a1).

Such co-existence and completely unexpected stability of genera over millions of years is in many cases as if *Homo sapiens* today still co-existed on earth with his presumed ancestors from the Australopithecines (see further details at http://www.weloennig.de/mendel20.htm). Gradual morphological transitional series between the forms are lacking.

2. By evolutionary presuppositions a line of descent can almost always be postulated from a large variety of forms

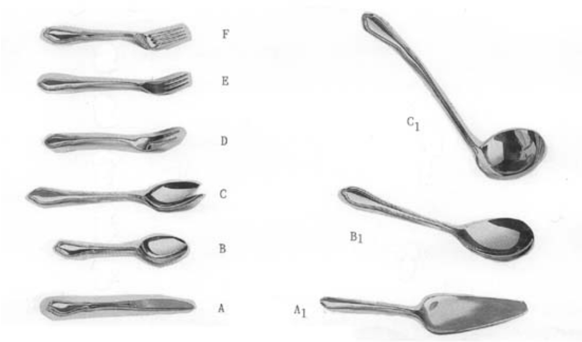
„Already in Darwin's day Galton warned of such erroneous constructions when he pointed out, for example, that firearms and chinaware can be ordered in a continuous series, and that it is necessary to take care in dealing with the same phenomenon in biology“ (H. Nilsson).

In this context we should remember Kuhn's basic statement:

> "The similarity of organic forms was explained by evolution, and evolution in turn was proven by the grades of similarities. That here one has fallen victim to circular reasoning was hardly noticed; the very point that one set out to prove, namely that similarity was based on evolution, was simply assumed, and then the different degrees in the gradation of the (typical) similarities, were used as evidence for the truth of the idea of evolution. Albert Fleischmann has repeately pointed out the lack of logic in the above thought process. The same idea, according to him, was used interchangibly as assertion and as evidence.
>
> However, similarity can also be the result of a plan, and ...morphologists such as Louis Agassiz, one of the greatest morphologists that ever lived, attributed the similarity of forms of organisms to the creation plan, not to evolution."

The fact that a morphological series is not necessarily proof of a line of descent, is further illustrated by the following morphological flatware or cutlery series (see also http://www.weloennig.de/AuIIMoIII.html):

Derivation of the fork from the knife, through the spoon, and the special evolution of the soup ladle from the cake slicer. One may note especially the stepwise perfection in the fork development from the 2-pronged meat fork (D) through the 3-pronged kitchen fork (E) to the 4-pronged dining fork (F). The salad server is the intermediate link between spoon (B) and meat fork (D) (mosaic evolution!). One only needs to assume that everything is derived from primitive knives.



Just to the right, as a second example, we see a number of different cross-country vehicles, which may be interpreted as an evolutionary series.

Here the objection is raised that tools and automobiles can, of course, not reproduce. Or stated another way (cf. Lönnig 1993, p. 538-540, see also http://www.weloennig.de/AesIV4.html#Intelligent at the close of the quotation):

„Sometimes the objection is raised, that the cybernetic systems created by humans cannot reproduce. **This completely ignores the fact that mitosis and meiosis themselves represent enormously complex cybernetic systems, whose successful function demands the most precisely coordinated interaction of hundreds of genes.** The fact that synorganised interactions of a large number of physiological and anatomical structures is required for reproduction in the more complex organisms will only be mentioned in passing.

Regarding mitosis, J. R. Broach 1986, p. 3 (Cell **44**, 3 - 4) remarks

> Segregation of a complete set of chromosomes to each daughter cell prior to cell division is a **mechanistically complex but extremely faithful process**. It requires the **precise assembly of several intricate structures**, including mitotic chromosomes and the spindle apparatus, and an exact dynamic interplay of these structures. The result is as beautiful to observe as it is difficult to fathom at the molecular level. Despite this complexity mitosis proceeds with high fidelity; the frequency at which a cell fails to transmit one of the complement chromosomes is, in yeast, less than once per 10^5 cell divisions.

See also D. M. Glover (1989): Mitosis in Drosophila. J. Cell Sci. **92**, 137-146

In regard to the topic of heterosis I have briefly touched on the question of the origin of meiosis in my dissertation (1980, p. 123):

> Regarding the question "What was the initial advantage of diploidy, and why is it almost the only condition present among all phyla of Metazoa?" G.. L. Stebbins 1977, p. 394, answers:
>
> > "The most plausible answer to this question is that ***the first diploid organism possessed marked heterosis or hybrid vigor***."
>
> This point is discussed in connection with the question of the "costs of meiosis", especially the objection of G.C. Williams (1975). If Stebbin's opinion were correct, the first diploid organisms must have already shown such a strong heterosis, that they had overcompensated the initial "50 per cent cost of meiosis" (G.C. Williams 1975). Incidentally it should be remarked that this would mean that all diploid organisms including humans would owe their existence to heterosis [hybrid vigour]. Experimental evidence for this hypothesis is lacking. However, the more difficult problem appears to lie in the origin of meiosis itself: Tinkle commented 1970, S. 97: "...the process of meiosis, with all its details, had to start in ***one generation***, else it would fail of its purpose and extinction would be the case. It is folly to visualize meiosis being built up by accidental changes."
>
> According to Gottschalk 1973, 1978c, S. 39, in at least *Pisum* 58 genes are known with specific control functions in meiosis, and over a hundred genes are probably responsible for the precise functioning of meiosis. How a entire chain of genes, each with a specific nucleotid sequence, should have evolved, a chain that only had a function as a system of integrated, complex interactions – i.e. at the „end“ of an assumed gradual evolution – is a difficult problem for the synthetic theory of evolution. Even if we suppose that the first meiosis was „simpler“ than in the thus-far investigated examples and that the genes that are now responsible for meiosis had other functions, this would raise more questions than it would answer.

For the topic of sexuality, including mitosis and meiosis, there exists an entire genre of literature. To discuss it in detail would require a book.

I would only like to state here, that despite decade-long, intensive efforts to find a solution of the question in terms of neo-Darwinian evolution, the recognition of the complexity of the events has only increased.

In a review of several more recent papers on this question M. Bulmer 1988, p. 214 (Why do they do it? *Nature* **332**) remarks:

> Sex is the big problem in evolutionary biology, the one we should all like to solve. Sexual reproduction has two clear disadvantages. First, recombination, its main consequence, breaks up coadapted gene complexes, which must be a bad thing in a constant environment. Second, there is the two-fold cost of sex.
>
> ...Felsenstein is cynical: This year the sex crisis seems to have returned ... Has a new source of data or a new kind of experiment been discovered, that will help us to solve the controversies? ...No...Biologists will once again all become convinced that they know the answer, but once again there will be no unanimity as to what the answer turned out to be.

Bulmer himself is, to be sure, more optimistic, but neither can answer the questions in terms of the synthetic theory of evolution. The neo-Darwinian authors do not consider the possibility that there may be more involved in this question than a simple gap of biological knowledge: a gap in the theory itself (cf. p.596).“

(More detailed references in the original work.)

3. Number of neck vertebrae: why it is so hard to count to eight in the giraffe's neck.

To the question, how many neck vertebrae the giraffe (*Giraffa camelopardalis*) displays, the answer given is „seven" in almost all textbooks, commentaries and debates to date (consistent with the number of neck vertebrae in almost all other mammals).

However, one of the best giraffe specialists of the world, Nikos Solounias, comes to a different conclusion. After thorough anatomical (including ontogenetic) studies he comes to the conclusion that the giraffe has eight neck vertebrae (*The remarkable anatomy of the giraffe's neck*, Journal of Zoology **247**: 257–268, 1999). If that is correct, then the question naturally arises, why all anatomists previously studying this question counted only seven.

The answer is perhaps immediately understood with the aid of the following illustrations: http://www.nature-wildlife.com/girskel.htm and http://www.nature-wildlife.com/babygir.jpg

So it appears that the giraffe has only seven neck vertebrae. How, then, is it possible to come to a different conclusion? In his above-cited paper Solounias argues as follows:

> "Mammalian cervical vertebrae 6 and 7 and thoracic vertebra 1 possess many distinguishing characteristics. In the giraffe, bone morphology, muscle origins and insertions, as well as the location of the brachial plexus (described as many osteological and some soft tissue characters) are identical to those in other mammals **but are all displaced posteriorly by one vertebra.**"

Thus, the question would be answered, if there were not two strong exceptions to this rule. Solounias continues:

> „There are two exceptions to these observations: the pre-sacral vertebral count is unchanged when compared with that of the okapi and C7 supports the first rib."

The connection of the ribs to the vertebrae is easy to detect by an attentive observer (see figures in the links above) and the vertebra on which the first rib pair is attached – *together with several further important characteristics (most, however, not so easily determined)* – is identified as the first thoracic vertebra (thorax vertebra). In addition, since "the pre-sacral vertebral count is unchanged when compared with that of the okapi" one would thus in comparison with the only still living (all well as all the extinct) short-necked giraffe(s), expect one additional vertebra. This is, however, not the case. Solounias comments on this question, among other topics, as follows (1999, p. 265, emphasis and numbering are mine):

> „**The adult giraffe V8** [that is, the 8th vertebra counting "down" from the skull] is very similar to the okapi C7 [the 7th neck vertebra of the Okapi], and **is completely unlike a typical T1** [a first thoracic vertebra] except for the presence of a rib. **V8 is unlike a T1** possessing [1] a long vertebral body, [2] a highly convex anterior articular facet, [3] a ridge on the pars interarticularis of the dorsal lamina, [4] an anteriorly inclined and spinous process, and [5] a thin flat pillar, **as in a C7** (Fig. 2, V8). The posterior articular facets are [6] not situated inferior to the spinous process but laterally **as in a C7**. Even the transverse process [7] protrudes as in a typical C7 despite the presence of a rib. [8] In the giraffe V8, **the rib does not affect the shape of the transverse process**, which still resembles that of a C7. **The first rib attaches in a totally unusual way on V8**. In typical vertebrae the rib head meets a facet that is confluent with the anterior articular surface of the

> vertebral body. In the giraffe, [9] the articular facet of the first rib is isolated and well posterior to the anterior articular surface of the vertebral body of V8 (Fig. 2, V8, 46)."

Thus far, the similarities between the 8th vertebra of the giraffe and the 7th neck vertebra of the Okapi. Then follow references to the differences:

> "Two characters distinguish the giraffe V8 from a typical C7: (a) the presence of a rib (Fig. 2, V8 bottom row), and (b) the posterior articular facets are positioned slightly more closely than the anterior. In this respect V8 is unlike a typical C7 and reminiscent of T1."

The giraffe thus shows in the 8th vertebra an astonishing combination of characteristics, the majority (9 characteristics) typical of a neck vertebra and 2 additional characteristics of a typical thoracic vertebra. Now Solounias has also gone to the trouble of making a study of the development of the neck vertebra from the young giraffe to the adult. He discovered the following astonishing facts (p. 265):

> "Cervicals of giraffe juveniles are important in this study because their bones have not been subjected to extreme elongation. Thus, **the shape of the juvenile V8 of the giraffe is identical to that of an adult or juvenile C7 of the okapi** (Fig. 3, V8 *vs* C7). This is especially true for the width of the posterior articular facets of V8 which are constructed as in a normal C7. Other juvenile ruminants with long and short necks also possess juvenile T1s with structures similar to those in adults. I have observed a series of giraffe specimens of different ages and have determined that during growth there is an allometric change as the posterior articular facets of V8 grow much less apart (vertebral width) than the anterior ones. This differential growth alters slightly the shape of V8 which begins as identical to a C7 and with age changes to one which is slightly narrower posteriorly, thus tending towards a T1 morphology."

That is, the form of the 8th vertebra „begins as identical to a C7“ (like a typical 7th neck vertebra of the Okapi) and only later becomes similar to a thoracic vertebra in characteristic (b) ("the posterior articular facets are positioned slightly more closely than the anterior"). As an aside it should be mentioned that the so-called biogenetic law is stood on its head by this characteristic (as in so many other known cases): ontogenetically the first differences appear early (according to the „rule“, they should be „added“ only at the end of the development), which, however, in the wake of further development to an adult animal, become in some respects similar to the first thoracic vertebra of the Okapi (and to most other mammals). Thus, the typical difference which should become more pronounced with time becomes increasingly less pronounced or masked.(2c)

Hence of the 11 anatomical characteristics of the 8th giraffe neck vertebra which could identify it as the a thoracic vertebra, there remains, for practical purposes, only the attachment of the ribs, which however is different in comparison with the other mammals („The first rib attaches in a totally unusual way on V8“, see details above).

Solounias continues:

> "Accessory articular facets occur between C7 and T1 in a few okapi individuals (Lankester, 1908). In the giraffe, the **accessory facets** are always present but are located one vertebra posteriorly, as expected. They **occur between V8 and V9**. This occurrence is in agreement with the current proposal that V8 is homologous to C7. **V9 of the giraffe is identical to a typical T1 and unlike any T2**. Thus, V9 possesses the long massive pillar with a well-defined posterior ridge as in typical T1s (Fig. 4, first grey region). The anterior articular facets are located laterally on the pillars and face medially as in typical T1s. Similarly, the pillars and articular facets of V10 of the giraffe correspond to that of a typical T2. Thus, the anterior articular facets are located for the first time medially on the laminae as in all T2s (Fig. 3, V10)."

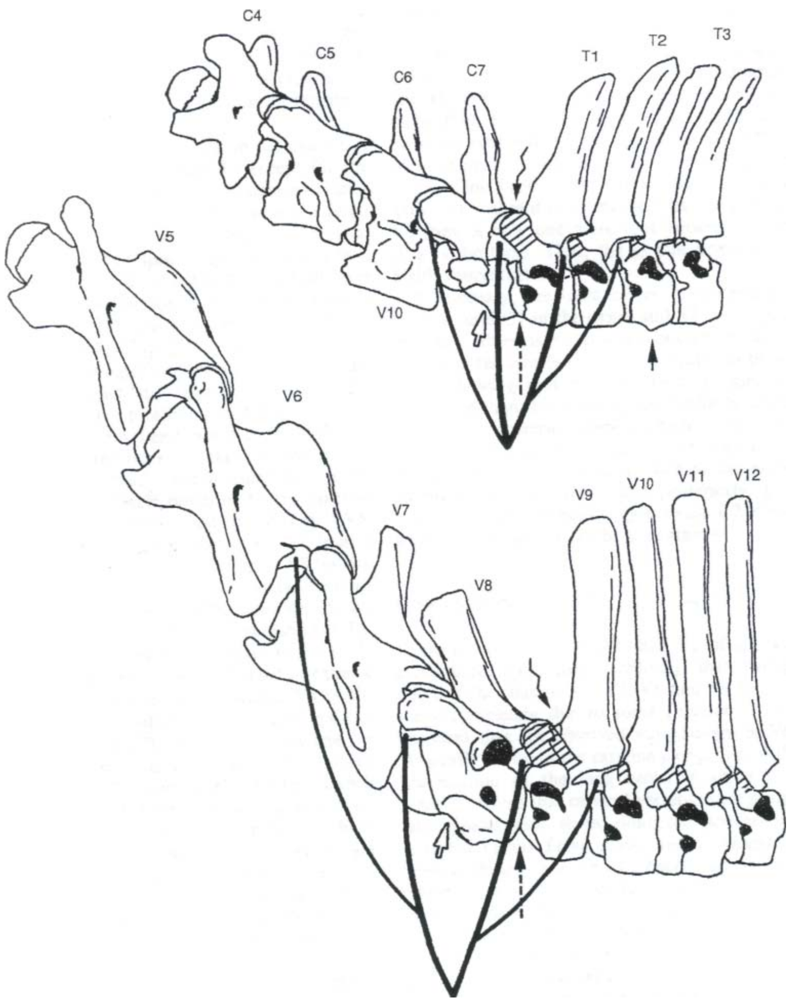


Fig. 4. Comparison of several cervical and thoracic vertebrae of *Okapia* (above) and *Giraffa* (below). From Solounias (1999, p. 264) with the following detailed clarification: "Lateral views of vertebrae and schematic location of the brachial plexi. Upper row: C4 through T4 of the okapi. Bottom row: V5 through V12 of the giraffe. The symbol V is used for the giraffe vertebrae instead of the traditional C or T terminology (cervical or thoracic respectively). For example, C3 is V3, C7 is V7, and T1 is V8. Wavy black arrow shows location of accessory articular facets *sensu* Lankester (1908: figs 64-71). Hollow arrow shows steep inclination of vertebral body of C7 and V8. This inclination is characteristic of the C7. Thus V8 resembles a C7 and not a T1. Thin dotted arrow shows the first vertebra with a flattened anterior articulation of the vertebral body and is taken here to represent the true T1. Solid black arrow shows major region of insertion of thoracic longus colli muscles. Dark regions show articular surfaces for the heads of the ribs and tubercles. Hatching shows thoracic pillars. The pillar of T1 is large and inclined. The basic nerves of the brachial plexi form around C7 and V8. In the okapi there is no anterior branch between C5 and C6 in the brachial plexus. In the giraffe the anterior branch is between V6 and V7 and is small. In the giraffe there is only one posterior branch. Thus, in the okapi the brachial plexus is simplified anteriorly and in the giraffe it is simplified both anteriorly and posteriorly."

For further details confirming the author's identification of the 8th neck vertebra in the giraffe through the position of the brachialplexus (plexus brachialis), please see the original work („In summary, the basic nerves of the brachial plexi form around C7 in the okapi and V8 in the giraffe").

Solounias concludes from his identification of the 8th neck vertebra of *Giraffa camelopardalis* that one thoracic vertebra is deleted (p. 266):

> "It would be ideal if the giraffe had an extra vertebra or rib in terms of total number but it does not (using the okapi as a standard). Both the giraffe and the okapi have a total of 26 pre-sacral vertebrae and 14 pairs of ribs. There is no apparent difference in the number of thoracics - defined as those which possess a rib - or lumbars. I

> have not observed sacralized lumbars or sacrals where an extra vertebra would hide. Thus, the **giraffe V8, although entirely a C7 in morphology, eliminates one thoracic vertebra in the thorax by taking its place.** In terms of the first rib and of total number, V8 is the first thoracic. In terms of morphology however, V8 is a C7. Apparently morphogenetic blending of vertebrae occurs at the cervicothoracic junction."

This conclusion fits very well with the relatively short torso of the giraffe. Lankester, however, suggested in 1908 that the 8th neck vertebra should be considered as only „cervicalized". To this, Solounias replies (p. 265):

> "I consider it unlikely, that owing to the detail of the change, V6, V7, V8 and V9 have changed shape completely due to some function. It might be proposed that the observed morphology of V6-V8 in the giraffe is due to the extreme elongation of the neck. **Examination of the long necks in other mammals, however, shows that cervical vertebrae are morphologically typical with seven elongated vertebrae.** I have examined Hamas *Lama glama* and *L. vicugna*, camels *Camelus dromedarius* and *C. bactrianus*, including the extinct camelid *Aepycamelus*, mohor gazelles *Gazella dama*, dibatags *Ammodorcas clarkei*, gerenuks *Litocranius walleri*, the litoptern *Macrauchenia*, as well as the extinct giraffids *Samotherium* and *Palaeotragus* (Godina, 1979). Thus, length alone may not have been a directing force in the observed specializations of the giraffe neck.
>
> The junction of the neck with the thorax (the cervicothoracic junction) has always been based on two characters that are coupled in mammals: the occurrence of the first rib and the location of a brachial plexus centred on C7 (Burke *et al*, 1995; Griffin & Gillett, 1996). In the examples of lost vertebrae no dispute can be posed. In the sloth *Bradypus* where there are nine cervicals, the cervicothoracic junction is still typical in terms of the first rib and the brachial plexus. **The giraffe is truly unusual in that the brachial plexus centres around V8, the same vertebra which bears the first rib.** It is proposed here that V8 is homologous with the C7 of other mammals.

Although 99.99% of all mammal species possess exactly seven neck vertebrae, the author emphasizes that this number can, in principle, vary, and he mentions the ensuing examples (pp. 257 and 266):

> "It is well known that mammals typically possess seven cervical vertebrae. This number is stable from mouse to whale in contrast to the necks of reptiles and birds. There are few exceptions to the number of seven cervical vertebrae in mammals. The sloth *Choloepus* has a variable number of either six or seven cervical vertebrae. The manatee *Trichechus* has six and **the sloth *Bradypus* has nine cervicals** (Filier, 1986; Nowak, 1991). In contrast to the stability of the cervical vertebrae **in mammals, the number of thoracic and lumbar vertebrae is variable** (Filier, 1986; Burke *et al.*, 1995)."
>
> "*Bradypus* is the only mammal that has nine cervicals and demonstrates that it is possible for the giraffe to have eight, although in the giraffe the first rib located on V8 masks its cervical nature. At present it is not clear how or where exactly a vertebra is added in the neck of the giraffe. What is almost certain is that an insertion has taken place between C2 and C6."

Based on his many anatomical arguments, we accept Solounias' interpretation that the giraffe possesses a very unique 8th neck vertebra among the mammals, and that one thoracic vertebra has been eliminated. The number of neck vertebrae is thus eight and not seven.

All evolutionary attempts to explain why even the giraffe has only seven vertebrae are thus highly doubtful, to say the least.

Two short examples:

> "The long neck of the giraffe contains only the seven vertebrae typical of most mammals. This is an excellent example of how the evolutionary process tends to modify existing structures, rather than creating new ones" (Donald J. Tosaw Jr., 2002).

Tosaw's comments seem to me to be a very nice illustration of „evolutionary storytelling": Basically, one can always find a „story" which spectacularly confirms the theory, even when the basis, the description of the facts, turns out to be unsupported or even completely false.

Conway Morris (2003, pp. 239/240) offers a somewhat different explanation attempt, but likewise under the supposition that the long-necked giraffe has only 7 neck vertebrae:

> "Why, for example, do practically all mammals have a fixed number of neck (cervical) vertebrae? In giraffes and moles, for example, the lengths of the respective necks could hardly be more different, but in both the number of cervical vertebrae is seven. In contrast, in the other vertebrates this total is much more variable. All things being equal, it would be more 'sensible' for the giraffe to multiply the number of neck vertebrae, rather than being 'forced' to elongate each of the seven it has. Why then the constraint? An intriguing suggestion, made by Frietson Galis is that in the mammals **a presumably fortuitous coupling has arisen from the involvement of key developmental genes** (especially *Hox* genes) in both the laying down of the axial skeleton, including of course the cervical vertebrae, and the process of cell proliferation.
>
> If, owing to some developmental abnormality, the patterning of the axial skeleton is upset so, too, there is a tendency to develop childhood cancers. These are examples of uncontrolled cell proliferations, which in this case originate in the developing embryo. **For mammals, departure from seven spells lethality**. Moreover, in mammals some cancers may owe their initiation to the production of highly reactive molecules known as free radicals). In the mammals, at least, the free radicals are an unavoidable by-product of an active metabolism. It may be no coincidence that the few exceptions to the rule of seven in neck vertebrae are in the **metabolically sluggish animals**, such as the torpid sloth. In this sense the rule of seven in mammalian necks is a good example of stabilizing selection, and may be the 'price' to pay in ensuring the effective development of very complex organisms. Such a constraint has, therefore, its costs, but when we see the diversity of mammals it seems that a restriction to seven cervical vertebra in animals as diverse as bats and camels has **been more than offset in other respects**."

Now, giraffes clearly do not belong to the group of "metabolically sluggish animals". On the contrary: "A resting giraffe takes about twenty breaths per minute, compared with our twelve and an elephant's ten; this is a ***very high respiration rate*** for such a large animal" (cf. McGowan in the first part of this work, cited on p. 9).

The further "explanations" ("a presumably fortuitous coupling has arisen from the involvement of key developmental genes (especially Hox genes)… and the process of cell proliferation" and "For mammals, departure from seven spells lethality") lead immediately and naturally to the question, why then *Choloepus* shows a variable number of six or seven neck vertebrae, *Trichechus* ***six*** and *Bradypus* even ***nine*** neck vertebrae. And additionally, why the number of neck vertebrae in reptiles and birds can vary, even strongly? Moreover, what selective advantage should this ***loss of variation potential***, this presumed accidental linkage with key developmental genes, have had, when the decoupled condition had already proven its merit in reptiles, the assumed ancestors of mammals, for millions of years before?

The additional explanation ("…but when we see the diversity of mammals it seems that a restriction to seven cervical vertebra in animals as diverse as bats and camels has been more than offset in other respects") is not convincing either. If there is any consensus among evolutionary biologists at all, it is that evolution cannot anticipate the future:

> „Evolution is not anticipatory; structures do not evolve because they might later prove useful. The selective advantage represented by evolutionary adaptability seems far too remote to ensure the maintenance, let alone to direct the formation, of DNA sequences and/or enzymatic machinery involved" (Doolittle and Sapienza).

Or the principle in the words of R. Dawkins: "Short-term benefit has always been the only thing that counts in evolution; long-term benefit has never counted. It has never been possible for something to evolve in spite of being bad for the immediate short-term good of the individual." (And Dawkins adds that in this respect man has a special place in Nature, since he can see beyond this short term usefulness). And one may continue: even if we could justifiably assume, that certain disadvantages could possibly be short- or long-term (weakly disadvantageous alleles, accumulation of junk DNA, degeneration in several species, genera, and families. - cf. Artbegriff pp. 403 ff.), ***it is still not possible that evolution could have anticipated the long-term welfare and future development of species and genera producing a wealth of complex genetic information [or even single chance couplings or linkages] that were simply superfluous [or even disadvantageous] on a short-term timescale.*** Otherwise this would have meant the formation of a wealth of genetic information [or a fundamental linkage] *initially* without any selective advantage, and *short-term* without any morphological function [or even a disadvantageous function].

It seems rather bold to attribute the **diversity of mammals** to a linkage of the genetic program for the number of neck vertebrae with other vital developmental programs, so that any deviation in the number of neck vertebrae would be lethal for the mutant. The diversity of reptiles and birds, including extinct forms, is also very impressive. Apparently a variable, or even strongly variable, number of neck vertebrae has been advantageous for these classes (not to mention that the number of thoracic and lumbar vertebrae is also variable in mammals).

It could just as easily be argued that the variety of mammals is possible not because of, but ***despite*** the (almost) constant number (seven) of neck vertebrae. One may ask, however, if the diversity of this animal class would not have been even greater, if the number of neck vertebrae could vary strongly as in the cases for reptiles and birds.

The explanation of the constancy of the number of neck vertebrae by natural selection of linked genes is thus not convincing. However, the question is whether this phenomenon could perhaps have a deeper significance, in the sense of typology (idealistic morphology, cf. the work of the botanist Wilhelm Troll on these questions.)

4. The question of causes (I): Again, the question of macromutations – possibilities and limitations

The naïvete with which Dawkins discusses the possibility of the origin of the long-necked giraffe by a macromutation (although he believes in a gradual evolution through many small steps; see the detailed discussion in Part 1 of our work http://www.weloennig.de/Giraffe_Erwiderung.1a.pdf) shows that he has very little understanding of the deep biological problems associated with this question (the highly complex anatomical constitution of the 8th neck vertebra should, from what has been said above, be added to the other characteristics) and should perhaps be fit into the category of a „materialistic miracle belief“.

Schützenberger http://www.arn.org/docs/odesign/od172/schutz172.htm answered the question „In what sense are you employing the word 'miracle'?” in the example of the supposed origin of the elephant trunk through a macromutation as follows (the reader should apply the principles of the argument also to the origin of the giraffe's neck):

> “A miracle is an event that should appear impossible to a Darwinian in view of its ultra-cosmological improbability within the framework of his own theory. Now speaking of **macromutations**, let me observe that to generate a proper elephant, **it will not suffice suddenly to endow it with a full-grown trunk**. As the trunk is being organized, a different but complementary system – the **cerebellum** – **must be modified** in order to establish a place for the ensemble of wiring that the elephant will require to use his trunk. These **macromutations must be coordinated by a system of genes in embryogenesis**. If one considers the history of evolution, we must postulate thousands of miracles; miracles, in fact, without end. No more than the gradualists, the saltationists are unable to provide an account of those miracles. The second category of miracles are directional, offering instruction to the great evolutionary progressions and trends in the elaboration of the nervous system, of course, but the internalization of the reproductive process as well, and the appearance of bone, the emergence of ears, the enrichment of various functional relationships, and so on. **Each is a series of miracles, whose accumulation has the effect of increasing the complexity and efficiency of various organisms**. From this point of view, the notion of bricolage [tinkering], introduced by Francois Jacob, involves a fine turn of phrase, but one concealing an utter absence of explanation.”

Already more than 40 years ago, in a *Nature* contribution, Brownlee quoted Graham Cannon’s words: "It is this idea of co-ordinated variation that is, to my mind, the central core of the whole problem of evolution."

In the first part of this work we have already discussed in detail that it is not sufficient to simply elongate, in a single step, the neck vertebrae of a short-necked giraffe to those of the long-necked giraffe (and *Giraffa camelopardalis* is 'finished'), but rather that numerous characters must be changed in a coordinated way (here again arises the synorganization (coadaptation) problem that is so difficult to explain for both the gradualist *and* the saltationist), a problem which includes, among many other tasks, the need for an entire series of precisely tuned mutations to give rise to the many interdependent anatomical structures just for the origin and development of the 8th neck vertebra. We summarize the special vertebra structure of the long-necked giraffe according to Solounias 1999, p. 260 as follows (illustrations and their numbers are here omitted; the reader should check the original work):

> "V6 has no ventral lamina unlike a true C6; transverse process does not protrude unlike the true C6 cervicals of other ruminants; V6 does not possess the first foramen transversarium (V7 has an additional foramen transversarium with the vertebral artery passing through it); V7 has a normal ventral tubercle unlike a true C7; the transverse process of C7 extends laterally; the transverse process of V7 does not extend laterally unlike a true C7; in the giraffe, the facet for the attachment of the first rib (cranial costal fovea) is unlike any other mammal's as it forms an isolated island on the vertebral body. The traditional facet is part of the anterior articulating surface of the vertebral body (centrum)."

Concerning the theory of a stepwise origin of the giraffe's neck Burkhard Müller asks (2000, p. 114), if a small increase could really mean the difference between life and death of a giraffe. Assuming this were true (we ignore here the above mentioned problem of sex-dimorphism), then there quickly arises a further problem:

> „But as soon as this small innovation has spread to a large portion of the population, many or nearly all of the giraffes consume a few more leaves, and with that the neck elongation sinks back into irrelevancy. The more successful a mutation was, the faster it spreads, and the fewer additional resources are available to the individual organisms, and the less useful it becomes: a too-well known secret 'tip'."

Again let us clarify the difficulty of the assumption of a macromutation, with the following words of Burkhard Müller – a summary (so to speak) of the main points from the first part of our work:

> „There is yet another problem in this elongating giraffe neck. It is not just a ladder, to which one simply throws on another rung (and even with ladders, there are stability problems). Many structures have to change to make it longer! The neck vertebrae must grow, of course, but not only they but also the skin, the muscles, all nerves, arteries and veins, sinews. Do they really all sit together on the same scales, so that one only needs to assign a higher value? And even if the entire system could be stretched in unison, without even suffering the small distortions of a thermostat that consists of two metals, which with uniform temperature variations stretch quite differently: that is still not sufficient, the *entire* skeleton must change, so that the animal remains in harmony with itself, there must be a counterweight, or it will fall on its nose; the heart must strengthen to transport the blood to 6 meter heights, and the neck arteries must be equipped with a special valve system, which impedes backflow of the blood pumped to the neck. Even if the rest of the changes could be written off as simple quantitative increases, the new valve system is an ingenious invention, a new quality, that could never be dismissed as „more of the same!"
>
> In short, it is not sufficient, that *one* mutation takes place. Practically every alteration of the form of an organism must be extended to *all* affected individual systems of the body, or what is produced is not the superior tree-crown grazer of the forest savanna, but rather a front-heavy defective monster that constantly looses its consciousness and balance.
>
> Let us never forget that mutations must have the character of an accident to fit into Darwin's scheme. Any driver would laugh at the idea that his vehicle could be improved through an accident. But that an accident could simultaneously improve the aerodynamics *and* the motor power *and* the tire performance *and* the transmission, that would be assigned to the realm of fairytales and dreams.
>
> When an alteration of an organism is to be advantageous, simply everything much change."(3)

Regarding these comments and quotations on the origin of the long-necked giraffe, it seems to be strongly significant that numerous authors – usually independently of each other – have arrived at the same basic conclusions.(3a)

Now concerning the potentials of macromutations, these are mostly limited to losses of gene functions with corresponding effects on the phenotypes (cf. Lönnig in detail 2002: http://www.weloennig.de/Artbegriff.html, among others, the chapter *Degeneration im Organismenreich* http://www.weloennig.de/AesV1.1.Dege.html as well as further works on the theme *Mutationen: Das Gesetz der rekurrenten Variation* http://www.weloennig.de/Gesetz_Rekurrente_Variation.html ; see also Sanford 2005).

5. The question of causes (II): Further hypotheses on the origin of the long-necked giraffe: sexual selection

Before we turn to the attempted explanation of Simmons and Scheepers of 1996 regarding the giraffe, we would like to make a couple of remarks on the general topic of sexual selection, as a background for the discussion of the interpretation of these authors.

Schmidt (1985, p. 198) mentions some difficulties regarding this topic as follows:

> „In sexual selection the choice of the sex partner is apparently determined by an inborn behavior program. **In most cases it stands in definite opposition to natural selection**. This is illustrated clearly by the birds of paradise. Let us assume, for example, that a female, due to a highly unusual mutation – for which there is not the slightest evidence – has obtained a special preference for bright colored males with long decorative feathers. For the species as a whole, there is no recognizable selection advantage for this mutation. **On the contrary: conspicuously colored males preferencially fall victim to their enemies.** ...The long tail feathers reduce the ability to fly and are also a hinderance in the search for food. One should assume, according to the principle of natural selection, that behavior mutations that lead to sexual selection with a disadvantage for the species as a whole, would be soon eliminated. It can, in the case of the bird of paradise as well as the Irish Giant Deer, be passed on, not in accord with, **but only against natural selection**. There must therefore be a factor that is stronger than Darwinian selection.“

The author assumes this factor to be an "endogenous orthogenetic developmental tendency", and he further remarks:

> „That selection cannot be the decisive factor for the long decorative feathers of the birds of paradise, peacocks and diamond pheasants, and so forth, follows from the fact that we find this in only relatively few bird species, at least to this degree.“

Similarly, Endler 1986, p. 11 remarks:

> “ ...sexual selection may sometimes be disadvantageous, or opposed by other components of natural selection (Darwin 1871; Ghiselin 1974; Wade and Arnold 1980).”

Reinhard Eichelbeck comments on the question of sexual selection as follows (1999, p. 202/203):

> „For Darwin »sexual selection« had two aspects. The first dealt with the struggle of the male for possession of the female animal. Here he was of the opinion, that »the struggle is possibly the most violent between males of polygamous animals, and they often seem to be equipped with special weapons«.
>
> In any case, these »weapons«, as we know, for most animals are so constructed that they serve to avoid injuries rather than to inflict ones – various horns and antlers, for example. Rutting fights are in many, **perhaps even in most cases, ritualistic show fights**.
>
> And what kind of **a battle is it, where the hummingbirds are armed with beauty and blackbirds with song?** Even Darwin realized, that for example, with birds »the competition often has a peaceful character«, and thus he preferred the second aspect of »sexual selection« in which the female animals of some species prefer magnificent, handsome males, or those who are especially good at dancing, singing, performing somersaults, or building artistically decorated nests.
>
> In Australia and New Guinea there are several species of so-called catbirds/**bowerbirds [Ptilonorhynchidae]**. For their mating ritual, they build small huts, which they decorate artistically with all sorts of objects, with stones, fruits, feathers, snail shells, and recently with pieces of glass and bottle tops. One species decorates its huts with flowers that are changed daily, another paints them with fruit pulp using for this purpose a piece of bark as a spachel. When scientists changed around their decorations while the birds were absent, the birds restored the original order when they returned. The artist knows what he wants. Then he entices the hen he had

> chosen into his love nest and courts her until she belongs to him – or maybe not. After all, the ladies have their own artistic taste.
>
> There are so many bizarre mating customs among birds that one could write a book about it. There are aesthetic orgies, in view of which only the **most dusty academic could arrive at the idea that everything in Nature is about survival and maximizing reproduction**. The motto is not only »make love, not war«, but also »make art, not sex«. With the immense effort that the foreplay costs, there does not remain much time for reproduction. But apparently everything is allowed -- »natural selection« closes one, if not both, eyes. Especially with the artistic feather costumes that some birds wear, and which **not only hinder flying, but also running –** and all this only because the ladies want it like this?
>
> »I see no reason to doubt«, wrote Darwin, »that female birds, by preferring the most musical and handsomest males, during thousands of generations, could produce a remarkable effect.«
>
> In **crows, however, which have similar voice organs to those of the nightengale, though seemingly not**. Or should the female crows have a preference for cawing black-coated males?
>
> Against the assumption that the artistic pattern of birds or insects have arisen through gradual accumulation of small variations and the special tastes of the females, there are indeed a couple of objections. One problem is the so-called »rejection reaction« among animals that live in groups. When an animal distinguishes himself from the others to a certain degree, he is chased away or even killed.

Then Eichelbeck describes some drastic examples and concludes that *conspicuous* changes may be rejected or even be fatal, "On the other hand changes that [according to human measures] are not conspicuous do not attract attention [in the animal kingdom either] and thus cannot have a significant effect" (p. 204; for further evidence with impressive examples – color patterns in butterflies, behavior of North American sage grouse – the reader is referred to the original work).

Tentative result: The concept of sexual selection by mutation is questionable in many areas of biological research.

After this background information, we would like to turn now to the comments of Robert Simmons and Lue Scheepers (1996) on the topic of sexual selection among giraffes.

As already in the first part of our work, and above on p. 5 again cited, they reject in their contribution *Winning by a Neck: Sexual Selection in the Evolution of the Giraffe* (The American Naturalist 148, 771-786) the widely accepted hypothesis of natural selection (*Darwinian feeding competition*) in favor of sexual selection.

They reason for the sexual selection thesis in the case of the giraffes as follows (p. 771):

> “We suggest a novel alternative: increased neck length has a sexually selected origin. Males fight for dominance and access to females in a unique way: by clubbing opponents with well-armored heads on long necks. Injury and death during intrasexual combat is not uncommon(3b), and larger-necked males are dominant and gain the greatest access to estrous females. Males' necks and skulls are not only larger and more armored than those of females' (which do not fight), but they also continue growing with age. Larger males also exhibit positive allometry, a prediction of sexually selected characters, investing relatively more in massive necks than smaller males. Despite being larger, males also incur higher predation costs than females. We conclude that sexual selection has been overlooked as a possible explanation for the giraffe's long neck, and on present evidence it provides a better explanation than one of natural selection via feeding competition.”

Craig Holdredge comments on this opinion in the year 2003:

> “…Simmons and Scheepers (1996) proposed that sexual selection has caused the lengthening and enlarging of the neck in males. These scientists place their ideas in relation to known facts and point out shortcomings in relation to larger contexts — a happy contrast to the other hypotheses we've discussed. They describe how male giraffes fight by clubbing opponents with their large, massive heads; the neck plays the role of a muscular handle. The largest (longest-necked) males are dominant among other male giraffes and mate more frequently. Since long-necked males mate more frequently, selection works in favor of long necks. This would also help explain why males have not only absolutely longer, but proportionately heavier heads than females.

> This hypothesis seems consistent with the difference between male and female giraffes. At least it gives a picture of how the longer neck of males can be maintained in evolution. **But it doesn't tell us anything about the origin of neck lengthening in giraffes *per se*** — the neck has to reach a length of one or two meters to be used as a weapon for clubbing. **How did it get that long in the first place**? Moreover, the female giraffe is left out of the explanation, and Simmons and Scheepers can only speculate that female neck lengthening somehow followed that of males. In the end, **the authors admit that neck lengthening could have had other causes and that head clubbing is a consequence of a long neck and not a cause**."

For further discussion of the original work of Simmons and Scheepers see below, under point 11a (the mechanism question); see also Mitchell et al. 2009.

6. The question of causes (III): Is Intelligent Design testable and falsifiable?

After about 200 years of fruitless evolutionary speculations (beginning with Lamark in 1809), and also several thousand years of similar African evolutionary legends; see point 11 below, it is no longer comprehensible why the intelligent design hypothesis (ID) should, for the question of the origin of the living world, continue to be ruled out on principle. The main objection, that ID is not scientifically testable, has long been refuted, so that we can limit ourselves to responding to the basic points of this objection in the following paragraphs. First we take up one of the main questions, according to Dembski:

"Isn't it at least conceivable that there could be ***good positive reasons*** for thinking biological systems are in fact designed? (Dembski 1999, p. 126, emphasis in the text is mine)."

A candidate for ID should show as many as possible of the following nine characteristics (the question of ID for the origin of a biological system can thus be scientifically investigated, and objectively be considered according to specific criteria). Summary of Dembski and later Behe according to Lönnig 2004:

> "1. High probabilistic complexity (e.g., a combination lock with ten billion possible combinations has less probability to be opened by just a few chance trials than one with only 64,000).
>
> 2. Conditionally independent patterns (e.g. in coin tossing all the billions of the possible sequences of a series of say flipping a fair coin 100 times are equally unlikely (about 1 in 10^{30}). However, if a certain series is *specified before* (or *independently of) the event* and the event is found to be identical with the series, the inference to ID is already practiced in everyday life).
>
> 3. The probabilistic resources have to be low compared to the probabilistic complexity (refers to the number of opportunities for an event to occur, e.g. with ten billion possibilities one will open a combination lock with 64,000 possible combinations about 156,250 times; *vice versa,* however, with 64,000 accidental combinations, the probability to open the combination lock with 10 billion possible combinations is only 1 in 156,250 serial trials).
>
> 4. Low specificational complexity (not to be confused with specified complexity): although pure chaos has a high probabilistic complexity, it displays no meaningful patterns and thus is uninteresting. "Rather, it's at the edge of chaos, neatly ensconced between order and chaos, that interesting things happen. That's where specified complexity sits".
>
> 5. Universal probability bound of 1 in 10^{150} - the most conservative of several others (Borel: 1 in 10^{50}, National Research Council: 1 in 10^{94}, Loyd: 1 in 10^{120}.
>
> "For something to exhibit specified complexity therefore means that it matches a conditionally independent pattern (i.e., specification) of low specificational complexity, but where the event corresponding to that pattern

> has a probability less than the universal probability bound and therefore high probabilistic complexity" For instance, regarding the origin of the bacterial flagellum, Dembski calculated a probability of 10^{234}."
>
> In addition the following questions belong here: (6.) "irreducible complexity" (Behe 1996, 2006) (3c) and last not least the similarities respectively between organisms and machines on the (7.) bionic, (8.) cybernetic and (9.) informations theoretic levels. On the question of the scientific details and the tasks in connection with these nine points, please see the contributions of Behe, Berlinski, Dembski, Lönnig, Meis, Meyer, Rammerstorfer, Wells, Wittlich and numerous other authors that are mentioned in the refenece list. Also, the ensueing questions belong to the bsaic problems: To what extent do mutations and selection explain the origin of new biological species and forms? What exactly are the boundaries where the origin of new specified genetic information requires intelligent programming because random mutations (‚chance mutations') no longer have explanatory value?

By these criteria the intelligent-design-hypothesis is in principle testable and also potentially falsifiable. In the section *"Old" and completely new research projects for the ID-theory* I will come back (see below) to some points, which deal with the use of ID for the origin of the long-necked giraffe.

7. Species concepts and basic types

The question of interbreeding of the living genera *Giraffa* and *Okapia* appears to be already answered by their chromosome numbers (Giraffe 2n=30 and Okapi 2n=44, 45, 46). Due to the large difference in the chromosome numbers, even a viable F_1 seems to be very improbable. Also, there are no known hybrids (cf. Gray 1971). To what extent the numerous extinct genera and species belonged to the same basic type is naturally no longer possible to determine by interbreeding programs. According to the current status of paleontological research, there could be a deviding line between long-necked and short-necked giraffes, so that all long-necked giraffes (that is, all *Bohlinia*- and *Giraffa* species) with their numerous special features in distinction to the short-necked giraffes, belong to a single basic type, but not necessarily so the entire range of the morphologically and anatomically very different short-necked giraffes.

Churcher remarked on the long-necked giraffes (1976, p. 529):

> "Unfortunately the variation in size and morphological characters of modern *G. camelopardalis* is such as to render any conclusions on the limits of variability of the extinct *Giraffa* populations inconclusive. It is not inconceivable that the *G. gracilis* and *G. jumae* specimens represent the lesser and greater limits of size and morphological variations of a single population, the modern descendants of which we call *G. camelopardalis*" (see also Harris (3c1)).

Many of these questions require a more precise morphological and anatomical investigation, to the extent that this problem can be decided by such methods. For more about species concepts and basic types in general, see Scherer 1993, Junker and Scherer 2006, and Lönnig 2002. Concerning the "species" of the genus *Giraffa*, see below and Note (3d).

8. Supplementary question: In view of the duplication of a neck vertebra, is a continuous series of intermediate forms possible at all?

The problem in the design of the long-necked giraffe is not only the duplication of a neck vertebra, but also the elimination of a thoracic vertebra (see details above). How one could imagine such a process through "infinitesimally small inherited variations", "steps not greater than those separating fine varieties" and "insensibly fine steps" ("for natural selection can act only by taking advantage of slight successive variations; she can never take a leap, but must advance by the shortest and slowest steps" etc. — all quotes again from Darwin, see Part 1 of the paper, p. 3 and more on p. 22) is not comprehensible for me (or according to the synthetic evolutionary theory, by mutations with "*slight or even invisible effects on the phenotype*" — Mayr). But even under the sacrosanct presuppositions of a purely natural evolution, a continuous development in the sense of Darwin or the synthetic evolutionary theory is clearly ruled out. In the following I would like to recall again my Note (1d) in part 1 of this work (if this text is fresh in the mind of the reader, he is invited to skip directly to the next subtopic):

Since I want to keep my readers informed as correctly and up-to-date as possible, I feel obliged to add the following points to the discussion on the origin of the long-necked giraffes: On 21 April 2006, Dr. X partially retracted his statement [namely: **"They [the fossil cervical vertebrae] are all short except of those of *Bohlinia attica* from Pikermi (Miocene of Greece) and *Giraffa*. *Bohlinia* is just as long as *Giraffa* and certainly not an intermediate.** There are differences in the short vertebrae of the various species. These vertebrae are a few and not connecting any of the fossil taxa to *Giraffa*. The okapi is not related in any way to any of the fossils and there are no fossil okapis." And a couple of hours later: "The variation in the short-necked extinct forms is interesting **but not leading to long necks**"]. However, the facts – if there are any – on which this retraction was based, and which would support a view partially in opposition to his clear and unequivocal previous statements as well as those of the other giraffe specialists quoted above, are not known to me. (Such fully new facts must therefore have been discovered in the last couple of weeks, yet I have heard nothing of this. His *hypothesis* is, that the neck vertebrae were first lengthened stepwise, and then a quantum mutation produced the duplication of a cervical vertebra.) Therefore I sent him the following questions (22 April 2006) concerning his statement "I have intermediates with partially elongated necks but they are unpublished":

> "If you really have intermediates (How many? Really a continuous series leading to the long-necked giraffes? What does "partially elongated" exactly mean? Are the intermediates really "intermediate" in the strict sense of the term?), which are relevant for the origin of the long-necked giraffes and which are occurring in the expected, i.e. "correct" geological formations (taking also into account the sexual dimorphism of the species and excluding juvenile stages and the later pygmy giraffes etc.), bridging in a gradual/continuous fashion of small steps in Darwin's sense the enormous gap between the short-necked and long-necked giraffes, I can only advise you to publish these results as a *Nature* or *Science* paper as soon as possible. And if you have, in fact, unequivocal proofs, I can only add that I, for my part, will follow the evidence wherever it leads. So drop all secondary things and publish it as rapidly as you can."

He replied, but did not answer these questions, neither does he intend to publish his findings this year. So at present I have no reasons to doubt that his original clear statements as quoted in the main text of the article were essentially correct and that Gould's verdict quoted on page 1 of the present article in accord with the answers of the other giraffe specialists, is still up-to-date.

But let's assume for a moment that there once existed say 2 or 3 further mosaic forms with some intermediary features: Would that prove the synthetic theory to be the correct answer to the question of the origin of the long-necked giraffes? As the quotation of Kuhn shows (see p. 20 above) that would be circular reasoning as long as the problem of the causes of such similarities and differences have not been scientifically clarified (just *assuming* mutations and selection is not enough). In 1990 and 1991, I wrote:

> Since roughly half of the extant genera of mammals have also been detected as fossils (details see http://www.weloennig.de/NeoB.Ana4.html), one might – as a realistic starting point to solve the question of how many genera have existed at all – double the number of the fossil forms found. Thus, there does not seem to exist a larger arithmetical problem to come to the conclusion that by also doubling the intermediate fossil genera so far found (which represent in reality most often mosaics) one cannot bridge the huge gaps between the extant and fossil plant and animal taxa.

However, from this calculation is seems also clear that in many plant and animal groups further mosaic forms (but not genuine intermediates) will most probably be found, which will nevertheless – on evolutionary presuppositions – be interpreted as connecting links. Since the quality of the fossil record is often different for different groups (practically perfect concerning the genera in many of the cases mentioned by Kuhn above, but in other groups imperfect), it is not easy to make definite extrapolations for the giraffes. My impression is, however, that with about 30 fossil genera already found (only *Giraffa* and *Okapia* still extant), the number still to be discovered might be rather low (generously calculated perhaps a dozen further genera may be detected by future research). As to the origin of the long-necked giraffes one may dare to make the following predictions on the basis that at least about half of the giraffe genera have been detected so far:

> (a) **A gradual series of intermediates** in Darwin's sense (as quoted above on page 3) **has never existed and hence will never be found**.
>
> (b) Considering *Samotherium* and *Palaeotragus*, which belong to those genera which appear to display (to use

the words of Dr. X) "some differences in the short vertebrae", a few further such mosaics might be discovered. As mosaics they will **not** unequivocally be "connecting any of the fossil taxa [so far known] to *Giraffa*". Nevertheless gradualists would as triumphantly as ever proclaim them to be new proofs of their assumptions (thus indicating that hardly any had been detected before).

c) The duplication of a cervical vertebra [a loss of one thoracic vertebra] excludes by definition a gradual evolution of [such] step[s] – by whatever method the giraffes were created.

9. The question of chance (résumé)

The detailed, numerous, precise, interdependent anatomical and physiological special characteristics mentioned above – this supercomplex synorganization(3) – (specific construction of the vertebrae, the heart, the blood circulation, the skin, muscles, nerves etc.) are, in my opinion, sufficient to rule out random mutations and selection as the primary cause of the origin of the long-necked giraffe.

Klaus Wittlich and other authors have raised the question of chance on the genetic level and answered it (cf. for example: *On the probability of the chance appearance of functional DNA-chains* http://www.weloennig.de/NeoD.html and *Frequent objections to the probability calculations* http://www.weloennig.de/NeoD2.html as well as *The eye: probability on the molecular biology level* http://www.weloennig.de/AuIWa.html. (Further, see the detailed discussion of objections by Frieder Meis: http://www.intelligentdesigner.de/, especially his contribution: *Defense of the probability calculations, part 1* and with a different URL address, *Part 2* (http://www.intelligentdesigner.de/Wahrscheinlichkeit2.html).

Several authors have also devoted time to this question on the anatomical level (cf. http://www.weloennig.de/AuIZu.htm). On both levels, it is especially interesting to notice the question of correlation.

Finally, in this connection the contributions of Prof. Granville Sewell (Mathematics Department, University of Texas El Paso) *A Mathematician's View of Evolution* should be mentioned (The Mathematical Intelligencer Vol. **22**, 5-7): http://www.math.utep.edu/Faculty/sewell/articles/mathint.html and *A Second Look at the Second Law* http://www.math.utep.edu/Faculty/sewell/articles/article.html as well as the book by Paul Erbrich (1988): *Zufall – Eine naturwissenschaftlich-philosophische Untersuchung* und Lee Spetner (1997): *Not by Chance! Shattering the Modern Theory of Evolution*.

10. "Old" and completely new research projects as deduced from the ID-theory.

Now that the question whether the ID-theory is testable and falsifiable can be answered positively (see details above) and the questions of species concepts and basic types have been mentioned as well as some pointers given to detailed contributions and discussions about probability estimates on the molecular and anatomical levels (see the links just above), we now want to turn to some „old" and new research projects, which can be further investigated by the ID-theory:

1. Paleontological research should be boosted under the ID-viewpoint: paleontological research in Europe and Asia of extinct giraffe species should move forward, considering, among other things, the issue of the postulated morphological-anatomical appearance without transitions, of the basic types and subtypes of the

family Giraffidae.

> At this point the testability and potential falsifiability of ID is again clearly revealed. For this issue, an important step to falsify ID would be obtained when, against all expectations, a continuous series in Darwin's sense from short-necked to long-necked giraffes could be proven to have existed (how that could work for the duplication and a loss of a vertebra, is however unimaginable for me). The ID-theory would, on the other hand, be further confirmed if, by additional fossil material and anatomical investigations, **the boundaries of species and sub-species were shown to be even sharper** (for a first judgement on this expectation, let us remember the statement of Kuhn in the first part of this work, p 6: "Especially German paleontologists such as Beurlen, Dacqué and Schindewolf have emphatically pointed out that in many animal groups such a rich, even *overwhelming amount of fossil material exists* (foraminifers, corals, brachiopods, bryozoans, cephalopods, ostracods, trilobites etc.), that the gaps between the types and subtypes *must be viewed as real*"

2. The genomes of the okapia and giraffa genera should be completely sequenced, systematically compared, and the differences determined: some fully new DNA-sequences as well as numerous modified sequences can be expected. **Research should focus on** *the gene functions and sequences for the numerous anatomical and physiological peculiarities of the long-necked giraffe as* for example (a) the duplication of a neck vertebra, as well as the many related specific anatomical structures discussed above by Solounias; further points could be (cf. Part 1, pp. 9/10 and 24/25): (b) the especially muscular esophagus (ruminator), (c) the various adaptations of the heart, (d) the muscular arteries, (e) the complicated system of valves, (f) the special structures of the *rete mirabile* (system of blood-storing arteries at the brain base), (g) the „coordinated system of blood pressure controls“ (for, among other things, the enormously high blood pressure), and it should again be kept in mind: (h) „The capillaries that reach the surface are extremely small, and (i) the red blood cells are about one-third the size of their human counterparts, making capillary passage possible“; (j) the precisely coordinated lengths, strengths and functionality of the skeletal, muscular and nervous systems; (k) the efficient „large lungs“ (l) „the thick skin, which is tightly stretched over the body and which functions like the anti-gravity suit worn by pilots of fast aircraft“. For the significance of the nonetheless expected high degree of similar and identical DNA and protein sequences, please see the contribution *Do molecular similarities refute Mendel's idea of constant species? – The example of humans and chimpanzees*: http://www.weloennig.de/mendel22.htm

3. What are the limits of accidental genetic alterations in giraffes (microevolution), where the construction of genetic information requires intelligent programming because undirected mutations ('chance mutations') no longer have explanatory value? (Except for DNA-sequencing and cell culture investigations, here we are forced to stick to theoretical research becausee a mutation program with several million giraffes including segregating M_2-Populations – as we can do and have done in a rather uncomplicated way with annual plants – is to my understanding not tenable with giraffes for ethical reasons (animal suffering, not to mention the financial question). In connection with the issue of random or "chance mutations", several other points arise, namely:

4. The question of new „irreducibly complex systems“ (in comparison to the short-necked giraffes) should be investigated thoroughly on the anatomical, physiological and genetic level.

5. Likewise the question of „specified complexity“ should be thoroughly

researched on both levels (probabilistic complexity, conditionally independent pattern for gene functions, gene cascades, organs and organ systems).

6. The question of similar or identical systems in the long-necked giraffe compared to other known (or as yet unknown) bionic and cybernetic structures and functions in engineering (it is very probable that we can still learn a lot from the giraffe's anatomical and physiological constructions). For an accurate understanding of this issue and its significance for the ID-theory, see, for example, the details in *Origin of the Eye*: http://www.weloennig.de/AuIEnt.html.

7. Research into the question of similar or identical systems discovered (or to be discovered) in giraffes on the information theory level (cf. Stephen Meyer on the topic *Intelligent Design: The Origin of Biological Information and the higher Taxonomic Categories* (2004) http://www.discovery.org/scripts/viewDB/index.php?command=view&id=2177).

8. The question, to what extent DNA functions can explain ontogenesis (what are the explanatory limitations of gene functions and gene sequences?). Which structures of the cytoplasm are involved? (cf. on this issue the contribution *Lamprechts Konzept der intra- und interspezifischen Gene* at http://www.weloennig.de/AesIV3.Lam.html and also *Weitere Hinweise auf ein plasmatisches Genregulationssystem* at http://www.weloennig.de/AesIV3.Hi.html .

9. Studies on the modification, epigenesis and spontaneous mutations in long-necked giraffes compared to okapis.

10. Population size and Haldane’s Dilemma for long and short-necked giraffes.

11. Genetic basis of behaviour (ethology) in the long and short-necked giraffes.

12. Further investigation of the selectionist explanations, including the hypothesis of sexual selection.

For all these questions and research topics, the ID hypothesis on the origins of the long-necked giraffe can be directly or indirectly investigated and potentially falsified or further confirmed: Regarding point (1) see above. (2) Confirmation of ID-theory in case of the discovery of new gene functions and sequences, and in connection with this, by evidence of (3) limitations in the generation of new functional or specifically altered DNA by „chance mutations“, (4) again through evidence of new „irreducibly complex systems“, (5) of „specified complexity“, (6) the discovery and decoding of further complex cybernetic systems, relevant for biotechnology, (7) reinforcement of the evidence for the identity of the necessary information in the construction of the (giraffe) organism and in technical systems, and its creation by intelligence, (8) the discovery of interspecific genes (in the nuclei), which cooperate with complex information systems of the cytoplasm, including further cell structures (such as membranes, organelles, centriols), that work together in ontogenesis, and evidence of (9) differences in the potentials and limits of modifications (phenotypes) as well as epigenetic factors in the living giraffe genera not explicable by chance mutations, (10) confirmation of Haldane’s dilemma in the giraffes, and (11) by evidence for ethological programs inexplicable by mutations (perhaps similar to the origin of the genetic programs for bird migration, which appears to be inexplicable by chance mutations, cf. for example, Schmidt 1986), (12) further evidence for the improbability of the selectionist hypothesis in both forms („natural and sexual selection“) concerning the origin of the giraffe.

If eventually all these research projects falsified the ID-theory, then it would have to be excluded from the scientific question on the origin of the long-necked giraffe. The fact is, however, that to date the research results have confirmed the theory in many essential issues (so that the theory has already shown its scientific value) and that numerous additional confirmations by further research programs in the above sense can be expected (regarding ID-theory, see further the works of Behe (1996, 2004, 2006), Dembski (1998, 2002, 2004), Junker (2005), Junker and Scherer (2006), Lönnig (1989, 1993, 2004), Meyer (2004), Rammerstorfer (2006).)

11. Mitchell and Skinner

> “This general tendency to eliminate, by means of unverifiable speculations, the limits of the categories Nature presents to us, is the inheritance of biology from *The Origin of Species*. To establish the continuity required by theory, historical arguments are invoked, even though historical evidence is lacking. Thus are engendered those fragile towers of hypothesis based on hypothesis, where fact and fiction intermingle in an inextricable confusion.”
>
> W. R. Thompson, F.R.S. (1967, p. XXIV): Introduction to Charles Darwin *The Origin of Species*.

G. Mitchell and J. D. Skinner, in their contribution *On the origin, evolution and phylogeny of giraffes Giraffa camelopardalis* (2003), start with the stated goal of justifying Darwinian gradualism for the origin of the long-necked giraffe. From the beginning, factual criticism and alternatives to gradualism are dismissed as „folklore tales“. In their introduction, for example, they write (p. 51):

> „One of the more enduring **folklore tales** about modern giraffes is that they defy Darwinian “long continued” gradualistic evolution, appearing in the African Pleistocene as if they had no ancestors, having been created by an act of God as a monument to biological structural engineering. In Lankester’s (1908) words, “It’s altogether exceptional, novel, and specialised.”

First, I would like to make the following remark: Whoever, after a detailed study of the peculiarities of the giraffe, does not understand that it really is an animal species that is “altogether exceptional, novel, and specialised“ is someone to whom Lord Acton’s words may apply: “The worst use of theory is to make men insensible to fact.“ Incidentally, it should be mentioned that E. R. Lancester was a devout Darwinist, that he belonged to the best giraffe specialists of the world, and that he performed lasting pioneer work in this research area (cf. Lancester 1901, 1907, 1908).

Speaking of “folklore tales“, I would like to bring to the reader’s attention the following facts, from Simmons and Scheepers (1996, p. 771):

> “Darwin (1871) **and many African folk legends before him** (e.g., Greaves 1988) proposed a simple but powerful explanation for the large and elongated shape. Long necks allowed giraffe to outreach presumed competitors, particularly during dry-season bottlenecks when leaves become scarce; thus, interspecific competition could provide a selective pressure driving necks (and bodies) upward. So appealing is this hypothesis that students of giraffe behavior and evolutionary biologist alike accept it implicitly [references].“

a) The question of the mechanisms: selection fails

Since Mitchell and Skinner represent the viewpoint of a *Darwinian long continued evolution* and *from the beginning completely rule out* any form of *intelligent design* for the origin of the long-necked giraffe, it will be very informative to know by what evolutionary mechanism they intend to explain the giraffe's origin (in parenthesis it should be remarked, that they also reject the punctuated equilibrium hypothesis of Gould and Eldredge).

So let us first look more closely at their quite detailed discussion of the problem of selection (p. 68/69) and let us keep in mind the question, whether the authors can present a convincing mechanism that would justify their certainty in ruling out ID for the origin of the long-necked giraffe, as they claim to do with their above-quoted words (emphasis in the text is again mine, the tables will not be reproduced here):

> "If the anatomical substrate for increased height can be analysed, **the advantages that it might confer are less obvious.** The cost of a long neck and limbs in terms of the many physiological adaptations needed to support them is high (e.g. Mitchell & Hattingh, 1993; Mitchell & Skinner, 1993). Moreover the nutritional demands to support giraffe skeletal growth seem also to be high (Mitchell & Skinner, 2003). Giraffe reach their adult height of 4-5 m in 4-5 years (Dagg & Foster, 1976). During this time total body calcium increases about 10-fold from 2850 g to 26 000 g (Table 2). This increase means that daily calcium absorption over the five-year period must average about 20 g (for comparison a human weighing 1/10 of a giraffe has a daily calcium requirement of 1/40). This quantity can only be obtained by almost complete dependence on legume browse, especially *Acacia* trees (Table 3) (Dougall *et al.*, 1964)."

The authors then address the objection of Pincher (already discussed above in detail) to the hypothesis that the long-necked giraffe arose by competition over nutrient resources:

> "While dependence on leguminous browse seems essential, the idea that tallness enables **exploitation of food sources** that are beyond the reach of competitors such as bovids, **is unlikely to be true**. Pincher (1949) made one of the first objections to this hypothesis. He indicated that a **Darwinian dearth** severe, long-lasting enough, and/or frequent enough for natural selection to operate to produce a long neck, would cause the recurrent wastage of young giraffes, and **would thus lead to extinction of the species rather than its evolution.** Secondly, Pincher noted that the same dearths would have encouraged selection of other ungulates with long necks, and yet only giraffes achieved this distinction. Thirdly, males are on average a metre or more taller than females, which in turn are taller than their young. **Dearths would place less tall members of the species at a permanent disadvantage, and extinction would be inevitable.** His preferred explanation, following Colbert (1938), was that there had to be concomitant elongation of the neck as a response to increasing limb length, if giraffes were to be able to reach ground water. **Quite why an increase in leg length might have been advantageous, he did not discuss."**

Brownlee, on the other hand, postulates a thermoregulatory advantage for increasing body size:

> "Brownlee (1963) also concluded that preferential access to nutrients could not be the evolutionary stimulus for a long neck, and suggested that their shape conferred a thermoregulatory advantage usable by "young or old, male or female continuously and not merely in times of drought". Brownlee was referring to the fact that metabolic mass increases at a rate related to the cube of body dimensions while body surface area increases as the square of the dimensions. Thus long slender shapes increase surface area for heat loss without proportionately adding volume and metabolic mass. In addition, such a shape also enables giraffes to "achieve that size and tallness which confers greater ability to evade, or defend against, predators and to reach a source of food otherwise unavailable to them"."

In this case one should again ask the question, why selection favored only the long-necked giraffe and why many other animal genera have not shot up in height together with the giraffe. And, why did the giraffe cows not become as tall as the bulls? Mitchell and Skinner do not discuss these questions, but surprisingly return to the feeding-advantage-hypothesis and contrary to their previous discussions assert:

> "Nevertheless the persistent idea that giraffe height evolved because it confers a selective feeding advantage has some justification."

And it seems even more surprising that after this sentence the authors, instead of substantiating their assertion, call it further into question with many additional good arguments and facts in their following discussion (pp. 68/69):

> For example, du Toit (1990) compared the preferred feeding heights of giraffes to those of a potential competitor, kudus *(Tragelaphus strepsiceros).* He found that, at least in the Vegetation type of the central Kruger National Park (Tshokwane region), giraffes tended to feed at heights of 1.7 to 3.7 m with a preferred neck angle (with respect to the forelegs) of 90-135° (Figure 20). Giraffe bulls generally fed at a higher level than cows and the preferred neck angle of bulls was greater than 90° while that of cows was below 90°. **Kudus**, on the other hand, had a height preference of around a metre but a range of up to 2.0 m, and a preferred neck angle of 45-90°. They **are thus competitive with female (and young) if not male giraffes**. Young & Isbell (1991) concluded that preferred feeding height is shoulder height i.e. 60% of maximum height and **far below maximum possible feeding heigh**t. Feeding height varied according to the gender composition of groups. Females in female groups fed at 1.5 m, females in male groups at 2.5 m, and males in male groups at 3.0 m. At best therefore a long neck may confer intermittent advantage. In another study Leuthold & Leuthold (1972) found that in a different habitat (Tsavo National Park, Kenya), giraffes spend about half their feeding time browsing below a height of 2.0 m. In the Serengeti, giraffes spend almost all their feeding time browsing low *Grewia* bushes (Pellew, 1984). The question then is, if a height of 3.0 m is adequate to avoid nutrient competition why do giraffes grow to heights of 5 m? Dagg & Foster (1976) suggest the reason that when giraffes were evolving there were a number of high level browsers, including Sivatheres, competing for browse. This hypothesis is weak however because for **many millions of years small giraffes were coeval with Sivatheres** and larger giraffes and would not have been able to compete with them for nutrients.

Concerning this point see also the table on page 7 and the figure on page 10 above as well as the text on pp. 7-10: Small giraffes were not only many millions of years coeval with Sivatheres but also coeval with larger giraffes. The authors continue:

> The underlying theme of these studies is that current utility mirrors selective pressures. Although this is an unsubstantiated idea (Gould, 1996) it implies that in the evolutionary history of giraffes the tendency to elongate will have been produced by competition for preferred browse with the tallest winning. The implicit assumption is that browse abundance at the lower levels was insufficient for all competitors - which as shown above is not true given that young vulnerable giraffes then must compete maximally. The idea that a unique advantage for adults is an advantage for the species generally is an additional and questionable corollary. The studies also raise the obvious problems of how young giraffes and young trees ever grow into adults if there is competition for preferred browse and for browse at low height. The only reasonable answer to this paradox is that the volume of low level browse is far greater than is that of high level browse, and is abundant enough to provide browse for small as well as large giraffes, other browsers and allow for growth of the browse itself. In other words **the presumptions of historical unavailability of browse and of browse bottlenecks as the selective pressures for neck and limb elongation, are highly doubtful and probably false**."

After the summarizing statement that all the hypotheses on the origin of the long-necked giraffe in the Darwinian sense by competition over nutrient resources (which were assumed to be disappearing into greater and greater heights), are "***highly doubtful and probably false***", Mitchell and Skinner turn to the hypothesis of Simmons and Scheepers on sexual selection (p. 69):

> "As the feeding hypothesis is not robust another suggestion, analysed in depth by Simmons & Scheepers (1996), is that the alternative main driver of natural selection, sexual advantage, may be the reason for the long neck. In support of this idea is the relatively greater elongation of the neck vertebrae compared to thoracic and lumbar vertebrae. The frequently observed use of the neck as a weapon by males when defending a female in oestrus (Coe, 1967), and the dominance of large males over younger smaller ones in the competition for females (Pratt & Anderson, 1982) is additional evidence. If this is the case there will be sexual selection for a long neck, especially in males. Presumably if this is an autosomal mechanism, a consequence is that **females would be genetically linked to the trait although having little need for it**."

We have already heard above that the whole concept of sexual selection as an explanation for the *origin* of the many impressive examples of sex dimorphism (from guppies to peacocks) by mutation and selection is in many areas highly questionable (though not necessarily as an explanation for the *maintenance* of the phenomena by stabilizing sexual selection). We have further established that this hypothesis cannot offer us any concrete answers for the origin of the long-necked giraffe. (“*But it doesn't tell us anything about the origin of neck lengthening in giraffes per se…*” “*How did it get that long in the first place?*” … “*In the end, the authors admit that neck lengthening could have had other causes and that head clubbing is a consequence of a long neck and not a cause*” – Holdredge, see above).

Simmons and Scheepers themselves write on this question (pp. 783/784):

> “If one accepts that necks may be present-day sexual traits, it is still arguable that giraffe necks are exaptations, not adaptations (sensu Gould and Vrba 1982). That is, elongated necks were primarily a response to other selection pressures and once lengthened could no longer be used in head-to-head combat. We do not reject this hypothesis because it is a parsimonious explanation for the switch from head butting (as in okapi) to head clubbing seen in giraffe, as necks became too long to wrestle with. **That is, slightly elongated necks were not likely to have evolved just for clubbing but were increasingly effective once longer necks arose.** Likewise, we cannot claim that longer legs did not allow other advantages, since most ancestral giraffids exhibited long legs. Long legs may have evolved for reasons such as antipredator responses (i.e., defence by kicking) or long-distance travel. Correlated responses with increasing body size must be considered in each case, and the okapi's long legs may be a clue to the long legs of extant and ancestral giraffe.”

In any case, regarding the question of the origin of the long-necked giraffe the authors limit their views to the selectionist explanation exclusively: If the origin cannot be ascribed to sexual selection nor *directly* to natural selection, then the latter must have been responsible *at least indirectly*, i. e. as a sort of a side effect to "other selection pressures" – exaptation. However, these other selection pressures are not elaborated and the just-so leg stories appears to be doubtful, too. (Why, then, are the necks of okapis still short? Of course, another just-so story may help.) Also, as far as sexual selection is concerned, we can establish the following: Since the basis for the origin of sexual dimorphism by selection of random mutations is not sufficient, very probably cause and effect are being confused by this hypothesis.

Mitchell and Skinner conclude that none of the hypotheses thus far proposed is convincing (p. 69):

> “**None of these ideas provide a definitive explanation for the evolution of a long neck, a conclusion at odds with its uniqueness.** Other examples of neck/limb elongation in camels *Camelus dromedarius*, Hamas *Lama glama*, gerenuks *Litocranius walleri*, and ostriches *Struthio camelus* are rare and are not as dramatic as the giraffe, and do not seem to be associated exclusively with feeding. If a long neck had some general utility or advantage then its evolution, as in the case of flight, would have initiated an impressive radiation of forms and not the rather meagre array that exists and that the palaeontological evidence suggests. But even this conclusion is worrying because **if a long neck has no utility then why has it survived?** The costs are high in terms of the many physiological adaptations needed to support it and it seems to require dependence on protein and calcium rich browse.

Subsequently the authors add to the discussion some considerations from Brownlee:

> “Thus another suggestion, first mooted by Brownlee (1963) is that a long neck has survived because it has allowed evasion of predation: the good vision and height give giraffes an advantage over other animals by improving their vigilance. Dagg & Foster (1976) indicate that adult giraffes move to improve their view of a predator rather than try and rely on camouflage. Moreover their large size makes them a formidable physical opponent. As a result, although always vulnerable, giraffes are rarely killed by predators. Pienaar (1969)

> noted that fewer than 2.0% of all kills in the Kruger National Park were giraffes and that lions, *Panthera leo,* were the main predator. In one sense this ratio is not surprising as giraffes generally constitute about 2.0% of a fauna (Bourliere, 1963). However if they were an easy source of food presumably they would form a higher proportion of lion kills.”

This explanation may also fail due to several justifiable objections: (1) Why should *good vision and height* only be of decisive selective advantage for the few long-necked giraffes developed over thousands of intermediate states and not for numerous other animal genera, too? (2) The entire camouflage question is debated(4). (3) The next hypothesis of Brownlee ("formidable physical opponent") contradicts the fact, that giraffe bulls are killed by lions almost twice as often as the smaller giraffe cows (Simmons and Scheepers p. 782, according to Pienaar 1969).

We can thus essentially agree with the authors in their critical evaluation of the different selection hypotheses: “*None of these ideas provide a definitive explanation for the evolution of a long neck,…*” (see also Mitchell et al. 2009). Another question is, by the way, whether this conclusion is really “at odds with its uniqueness” and whether the conclusion is worrisome at all (“...is worrying because if a long neck has no utility then why has it survived?”). This view presupposes the foundation of Darwinian utility as the only correct one. If, on the other hand, one views Nature as ingenious artwork that cannot be reduced to the question of utility alone, these problems disappear (as to Darwinian utility, see among others, Wilhem Troll 1984, p. 74(4) and the work of Goebel and Uexküll). In connection with the subtopic *Coat colour patterns* and as a general conclusion (p. 71) the authors attempt to salvage the situations with a quotation from Darwin, which was already used by Pincher in his *Nature* article of 1949: „The preservation of each species can rarely be determined by any one advantage, but by the union of all, great and small“. This statement is, of course, so general that it can give us no concrete information on the question of the origin of the long-necked giraffe based on selection.

With regard to the mechanism question, we can reformulate the above quoted words of Mitchell and Skinner as follows: “*One of the more enduring folklore tales about modern giraffes is that they prove Darwinian “long continued” gradualistic evolution by natural selection*”.

According to their own analysis Mitchell and Skinner cannot offer a conclusive selectionist explanation (the word „mutation“, incidentally, does not appear in their work). Thus, a convincing evolutionary *mechanism* for the origin of the long-necked giraffe is lacking, and they confirm, contrary to their goals, the statement of Gould: „*No data from giraffes then existed to support one theory of causes over another, and none exist now*.” With what justification – one may well ask – do the authors rule out *a priori* intelligent design for the origin of *Giraffa camelopardalis*? Could the answer perhaps be found in their philosophical loyalty to naturalism?

Further, how do the authors know, in the absence of a convincing evolutionary mechanism, that the origin of the „modern giraffe“ rests on *gradual evolution* in the Darwinian sense (*Darwinian “long continued” gradualistic evolution*)? These questions lead us to the next subtopic, the evolutionary tree problem.

b) The problem of the phylogenetic tree

In spite of some principal objections and notes, I would first like to express my respect to the authors for their discussion of the question of natural selection: their research was thorough and critical, and most open problems have been clearly mentioned and often exhaustively discussed.

In sharp contrast to that part of their work, unfortunately numerous statements about the evolutionary lineage of the long-necked giraffe and about supposed intermediate links will, upon close examination, be shown to be *uncertain, speculative and in essential points even false,* inasmuch as their assertions are for the most part presented as certain statements of facts.

If the results of the discussion of the problem of selection stands in contrast to their declared goal and clear claim of eliminating an intelligent cause in the origin of the long-necked giraffe by the Darwinian mechanism, the reader should judge for himself whether their treatment of the problem of the giraffe's evolutionary lineage illustrates fully the words of Thompson, quoted on page 28 on the 'elimination of the limits Nature presents to us by means of unverifiable speculation', and „to establish the continuity required by theory, historical arguments are invoked, even though historical evidence is lacking“ etc..

Let us look more closely at the main statements of the authors:

b$_1$) *Bohlinia* as „intermediate form“

In contrast to Simmons and Scheepers 1996, p. 772 (“Modern Giraffes radiated… from a large, morphologically similar species, *Giraffa jumae,…*” – in turn derived from *Palaeotragus* [p. 776]), Mitchell and Skinner assert: *Bohlinia attica* “can be regarded as the immediate ancestor of giraffes” … “It gave rise over the next few million years to a relatively rapid adaptive radiation, and emergence of the genus *Giraffa*” (p. 60). In antithesis to Simmons and Scheepers, they assign *G. jumae* to a side branch (Fig. 16, p. 64) and with Harris (1976) they further assert that *Bohlinia* was smaller than the „early“ African *Giraffa gracilis*. We should remember, as already cited in the first part of this work, that according to one of the best contemporary giraffe researchers, who, according to his own statement, has studied and documented in detail all the giraffe neck vertebrae found so far that “***Bohlinia* is just as long as *Giraffa* and certainly not an intermediate**.“ In Note 3 of the first part (p. 25) we further stated:

> …Hamilton (1978, p. 212) [commented]: "...Post-cranial material of *B. attica* is figured by Gaudry (1862-7) and the synonymy between Gaudry's species *Camelopardalis attica* and *B. attica* is indicated by Bohlin (1926, p. 123). This species has limb bones that are as long and slender as those of *Giraffa*. *Bohlinia* is more advanced than *Honanotherium* in features of the ossicones and is therefore identified as the **sister-genus** of *Giraffa*.” Denis Geraads wrote (1986, p. 474): “*Giraffa* (y compris les espèces fossiles) et *Bohlinia* possèdent quelques caractères crâniens communs (Bohlin 1926); l’allongement et les proportions des membres sont très semblable (Geraads 1979). Les deux genre sont manifestement très voisins et leur appendices crâniens selon toute vrai semblance homologues (ossicônes).”

As for the „early“ *Giraffa gracilis* it should be remarked that according to the latest dating *G. gracilis* and *G. camelopardalis* are equally old (maximum 3,56 million years) and that relative to the latter, the even larger *G. jumae*[4a)] is at least twice as old

(7,1 or perhaps even approximately 12 million years). The relatively smaller giraffes such as *G. gracilis* (3,56 million years) and *G. pygmaea* (5,3 million years) thus appear later than the larger giraffes (*Bohlinia attica* and *G. jumae* – maximum 11,2 and 7,1 [or perhaps even 12] million years). Hence, the smaller giraffes, according to current dating, can not be considered as intermediates for the larger ones (unless one assumes that children can appear before the parents). As for *G. pygmaea*, the situation perhaps is reminiscent of similar phenomena for *Homo sapiens*: pygmies, only slightly more than 1 meter tall, appear later than the larger races and are likewise not possible intermediate ancestors for the taller populations of their species.(4b)

I don't quite comprehend why Mitchell and Skinner insinuate that Francis Hitching proposes the Darwinian evolutionary idea of "infinitesimally small inherited variations", "steps not greater than those separating fine varieties" and "insensibly fine steps" ("for natural selection can act only by taking advantage of slight successive variations; she can never take a leap, but must advance by the shortest and slowest steps", see Darwin above) in connection with *Bohlinia*, and then claim, that Hitching has erred in this point (p. 60):

> "The evolutionary experiment that *Bohlinia* inherited from *P. germaini/S. africanum* was evidently successful, and had clearly not required Hitching's "series of accumulated modifications over thousands of generations" (Hitching, 1982)."

The thesis of gradual evolution is, of course, not an invention of Francis Hitching, but rather an integral component of the Darwinian theory, as well as of the present synthetic theory of evolution ("…metaphysical uniformitarianism is part and parcel of pure neo-Darwinism, and one of its severe weaknesses" – S. N. Salthe; see further related points at http://www.weloennig.de/AesV3.Konti.html). Even if *Bohlinia* were an "intermediate form" in the sense of Mitchell and Skinner, between *Giraffa camelopardalis* and *P. germaini/S. africanum*, then it would only represent ***one*** of the hundreds and perhaps thousands of intermediate forms required by the theory, links which are assumed to have continuously filled the morphological-anatomical and physiological gaps between the distinct forms of the past and present (on the number of required intermediate links, see the exposition in Part 1 of this paper http://www.weloennig.de/Giraffe_Erwiderung.1a.pdf pp. 2-4).

Regarding the time line, let us recall point (5) above on page 9 of the current work (*Many species and genera of Giraffidae lived contemporaneously with their supposed ancestors and thus often co-existed for millions of years with their „more evolved" descendants*):

(5) ***Bohlinia*** (11.2 – 5.3 million years before present) possibly lived contemporaneously with ***Canthumeryx*** (22.8 – 11.2 million years before present) an unknown period of time, with ***Giraffokeryx*** (17.2 – 5.3 million years before present) ***simultaneously* 6 million** years, with ***Palaeomeryx*** there is no known overlap, with ***Palaeotragus*** (18 – 1.76 million years before present) likewise some **6 million years**, with ***Samotherium*** (14.6 – 3.4 million years before present) again about **6 million** years, with ***Giraffa*** (12 million years to present) simultaneously **6 million** years.

Given such a time overlap, the supposed derivation is doubtful or improbable, inasmuch as *Giraffa* is, according to the present knowledge, older than *Bohlinia*.

The phylogenetic proof of Mitchell and Skinner rests principally on similarity arguments, which according to Kuhn involves circular reasoning (as already mentioned). They further assert (p. 60):

> "The pre-eminent status of *Bohlinia* as an intermediate form between its palaeotragine-samothere ancestors and *Giraffa* can be judged from its many common traits with *Giraffa*, particularly their long legs and neck, similar ossicones, the characteristic bilobed lower canine, and selenodont rugose molars."

Jonathan Wells (2006, p. 21) offers the following critical arguments on this method (the reader is invited to apply these considerations about whales again to the question of the origin of the long-necked giraffe):

> "Even in the case of living things, which *do* show descent with modification within existing species, fossils cannot be used to establish ancestor-descendant relationships. Imagine finding two human skeletons in your back yard, one about thirty years older than the other. Was the older individual the parent of the younger? Without written genealogical records and identifying marks it is impossible to answer the question. And in this case we're dealing with two skeletons from the same species that are only a generation apart.
>
> So even if we had a fossil [record] representing every generation and every imaginable intermediate between land mammals and whales—if there were *no missing links whatsoever,* it would still be impossible *in principle* to establish ancestor-descendant relationships. At most, we could say that between land mammals and whales there are many intermediate steps; we could not conclude from the fossil record alone that any one step was descended from the one before it.
>
> In 1978, fossil expert Gareth Nelson, of the American Museum of Natural History in New York, wrote: "The idea that one can go to the fossil record and expect to empirically recover an ancestor-descendant sequence, be it of species, genera, families, or whatever, has been, and continues to be, a pernicious illusion.""

What, then, do we really know? In this regard we should again keep in mind, that even the hard parts of fossil material are frequently fragmentary and that generally the soft parts are not fossilized at all. But even for genera with many well preserved fossils there can be problems, although we – as emphasized in Part 1 – don't want to underestimate the value of fossil material for the origin of species. Churcher describes one such problematic case as follows (1978, p. 514/515):

> „*Palaeotragus primaevus* is known from some 243 specimens, including 25 dental rows, 83 isolated teeth, and 60 teeth, and 60 postcranial elements from the Fort Ternan volcanic beds. There is thus a comparatively numerous sample of bones of this animal on which to base a description. **Unfortunately the skull is not known and the absence of ossicones can only be inferred**, since the only possible ossicones preserved in the deposits are larger than recorded for *Palaeotragus* and match best those given for *Samotherium* (Bohlin 1926)."

However, how can one be sure that the *ossicones* could not belong to *Palaeotragus primaevus*? – In addition, certain genera such as *Palaeotragus* consist of polyphyletic groups according to the views of Hamilton and others. Yet, other forms, which are presently considered to be different species, may really belong to just one species. Hamilton comments this problem as follows (1978, p. 166):

> "The Palaeotraginae is shown to be an invalid **polyphyletic** grouping and the genus *Palaeotragus* is also shown to be **polyphyletic**. *Palaeotragus microdon* is **probably synonymous** with *Palaeotragus rouenii* and the three species *Palaeotragus rouenii (P. microdon), Palaeotra.gus coelophrys* and *Palaeotragus quadricornis* are retained in the genus *Palaeotragus.* It is suggested that *'Palaeotragus' expectans* and *'Palaeotragus' decipiens* are closely related to *Samotherium. Palaeotragus primaevus* is **probably synonymous** with *Palaeotragus tungurensis* and this species is closely related to the giraffines."

Considering the arguments and points just mentioned, how certain are assertions such as the following ones from the work of Mitchell and Skinner? "*Georgiomeryx* was a direct descendent of *Canthumeryx…*" (p. 59); "Samotheres… follow *Palaeotragus* chronologically [and thus co-existed for some 10 million years with *Palaeotragus*, note added by W.-E. L.], and this together with their features, is convincing evidence of an ancestor-descendent relationship" (p. 59; see further points below); *Giraffokeryx* "has all the attributes of a giraffe ancestor and occupies the right evolutionary position” (p. 58); “…*Giraffokeryx* [is] an ancestral species, to *Giraffa*” (p. 59); “The earliest giraffine ancestor is *Canthumeryx sirtensis*“ (p. 57); “The Palaeomerycinae were the origin of the Giraffidae” (p. 56). “From the gelocid genetic pool came all of modern artiodactyl ruminants…” (p. 55); “The family of fossil artiodactyls that arose out of the Leptomerycidae and showed these characters was the Gelocidae” (p. 54) etc.

In the first part of this paper (p. 12 ff.) we have already discussed in detail that the expected “*very fine-grained sequences documenting the actual speciation events*” are generally lacking and that neither additional evolutionary criteria are fulfilled for the giraffes as referred to by Hunt and Dewar (not to mention that even in the contrary case, *ancestor-descendant relationships* cannot be proven with certainty, although a continual transitions series between all genera of giraffes would, of course, fit much better with the gradualist idea than the currently observed discontinuous appearance of basic genera and species).

For the reader not familiar with the details, however, Mitchell and Skinner leave the impression as if all essential questions have already been solved in terms of Darwinian gradualism. Whether this misconception should be characterized, with Nelson, as a "pernicious illusion", depends perhaps on the reader. (Many Darwinists will rather welcome such an illusion. Yet, in any case such methods are not useful in the search for truth.)

In my view, rather than providing the promised scientific evidence, the authors presuppose a *Darwinian “long continued” gradualistic evolution* as certain fact, and then, using appropriately selected data and interpretations, try to convey as convincing a Darwinist scenario as possible. Thus the decisive open questions of giraffe evolution and the *limits of the categories Nature presents to us* are eliminated in the pursuit of the goals of the authors *by means of unverifiable speculations* (including the evolution of „*pseudo*genera“) – entirely in the sense of Thompson’s further characterization of the method, namely: „…to establish the continuity required by theory, historical arguments are invoked, even though historical evidence is lacking“.

The unfortunate task of analyzing all strengths and weaknesses of their paper on the phylogentic question, as we carried out in detail for the author's scientifically exact and accurate analysis of the selection hypotheses, would require a long exposition (with, among other things, numerous further reproductions from the first part of our giraffe article).

We will limit the analysis to the main points in the following text.

b$_2$) *Samotherium* as an intermediate link to *Bohlinia*

According to Mitchell and Skinner *Samotherium africanum* should be „a logical antecedent of the giraffe lineage”:

> “*S[amotherium] africanum* fossils have been recovered from Algeria, Tunisia, and Egypt, and possibly Kenya (Churcher 1970). Its giraffe-like features and chronological age make it a logical antecedent of the Giraffe lineage.”

This is perhaps correct in the sense of the so-called „idealistic morphology“ (Dacque, Kuhn, Troll), but not in the sense of a gradualistic Darwinian evolution that Mitchell and Skinner wish to prove to the reader in their paper. For they completely overlook, to a certain extent even cover up (as previously in Kathleen Hunt’s case) the decisive height difference between the short-necked giraffe *Samotherium africanum* and the long-necked giraffe *Bohlinia attica*: „[*S. africanum*] had forelegs about 33 cm shorter than those of the extant giraffe and a **neck described as „normal length**“ (Colbert 1938, p. 48)“ – Quotation from Simmons and Scheepers 1996, p. 780 (see also Note (5)).

This difference is still unmistakable, when one, like Mitchell and Skinner, depicts *Bohlinia* smaller than it really is (see above) and draws the neck of *S. africanum* longer than corresponds to reality, or, like Dawkins, represents Okapi almost twice as large as it is (see Part 1) in order to „minimize“ the relative difference to the long-necked giraffe – all more than doubtful scientific methods to prove a *Darwinian gradualistic evolution* („We show… that a history of intermediate forms does exist“ – Mitchell and Skinner p. 51).

In this connection it is also perhaps revealing that many authors reduce the difference of from 1 to 1.5 meters between giraffe bulls and cows to only „a few inches“ (Pincher 1949 – however, I am not sure however whether that was the intention) and that the largest thus-far found giraffe species (*Giraffa jumae*), which chronologically does not fit the theory at all, seems to have been revised, from an original dating of 12 million years for the oldest finds (Simmons and Scheepers 1996, p. 772 and 777 with reference to other autors(5a)) to a 5 million year younger date.

Concerning the question of the existence of a series of transitional forms between *Samotherium* and *Bohlinia* I refer again to the discussions from the first part of this paper. Kathleen Hunt was quoted there with the assertion that the giraffe lineage goes through *Samotherium* („another short-necked giraffe“) and then branched off to *Okapia* and *Giraffa*. At precisely this point one would expect the chain of evidence – the finely graded series of intermediate forms – for the gradual evolution of the long-necked giraffe. However, we had to state:

> [Hunt] however does not produce the evidence, because a transitional series does not exist.
>
> Recently this last point was confirmed by a fervent defender of evolutionary theory, we will call him Dr. Y, by answering my question „Is there a series of intermediate fossil forms between *Samotherium africanum* and *Bohlinia*?“(3) clearly in the negative (“There is not an intermediate that I am aware of“). Another biologist – likewise a giraffe expert (Dr. Z) – said, to be sure, that the skull and teeth of *Bohlinia* are more primative than those of *Giraffa* (when the term „primitive“ is used, in my experience caution and further investigation are advisable), but he added: “...but it is true that the post-cranials are about as long as those of the living giraffe.” This author questioned the derivation from *S. africanum* and from his following statement: “The ancestors of *B. attica* should rather be sought in Eurasia...” it is easy to conclude that the assumed series of evolutionary ancestors and transitional series are unknown (because clearly: if we had them, we no longer need to search from them – neither in Africa nor in Eurasia).

Regarding the chronology, let us recall point (4) above:

(4) ***Samotherium*** (14.6 – 3.4 million years before present) lived ***simultaneously*** with ***Canthumeryx*** (22.8 – 11.2 million years before present) more than **3 million** years, with ***Giraffokeryx*** (17.2 – 5.3 million years before present) **9 million** years, with ***Palaeotragus*** (18 – 1.76 million years before present) some **11 million** years, with ***Palaeomeryx*** possibly an unknown period of time, with ***Bohlinea*** (11.2 – 5.3 million years before present) simultaneously **6 million** years and with ***Giraffa*** (12 million years to present) **8 million** years.

b3) *Samotherium* – where did it come from?

As previously mentioned, Simmons and Scheepers trace the long-necked giraffe back to *Palaeotragus*, but *Samotherium* is not listed at all. Several authors however tend to run the postulated giraffe lineage through *Samotherium* and trace this genus back to *Palaeotragus*. The question of a link between *Palaeotragus* and *Samotherium africanum* is addressed by Mitchell and Skinner as follows (p. 59):

> "Eurasian samotheres did not have the morphology that suggests they were the ancestors of *Giraffa*, and in any case do not seem to have left any descendants. On the other hand *S. africanum* did have the morphology, but the origin of *S. africanum* is less clear than is the origin of the Eurasian samotheres.
>
> A possible intermediate form between the palaeotragines and the African samotheres is *Helladotherium*, which was first described by Forsyth Major and Lydekker (1891) from fossils found in Greece and in the Siwalik. A cave painting (Joleaud, 1937) of *Helladotherium* (Figure 12B) which makes it look like a large hornless *Giraffokeryx* or okapi, makes this conclusion plausible."

Yet, according to Metcalf (2004) *Helladotherium* was **a forerunner** of *Palaeotragus* (cf. Part 1 of this work, p. 17). On page 60, however, Mitchell and Skinner reject the derivation of *Helladotherium* and write:

> „**A more likely origin of *S. africanum* is *P. germaini***. Harris (1987b) noted that the skeleton *of P.germaini* had the same dimensions as that of *S. africanum* and differed only in that *S. africanum* had larger ossicones. Therefore, he concluded, that ***P. germaini* was *S. africanum* or at least an antecedent** to it. *S. africanum* fossils have been recovered from Algeria, Tunisia, and Egypt, and possibly Kenya (Churcher 1970). Its giraffe-like features and chronological age make it a logical antecedent of the *Giraffa* lineage."

Geraads emphasizes (1986, p. 474) the fragmentary nature of the preserved *P. germaini* fossils.

If *P. germaini* belongs to the same species as *S. africanum* and if only the "ossicones" of *S. africanum* were somewhat larger (a similar variation exists within today's okapis and giraffes: Northern giraffes, for example, have „a larger frontal ossicone" than southern giraffes and today's giraffe species are able to crossbreed – Krumbiegel 1971, pp. 38, 64 ff., Gray 1971), then **the names suggest an evolution, that did not really exist** („only the names have evolved" – H. Nilsson) and the above quoted statement ("Samotheres… follow *Palaeotragus* chronologically, and this together with their features, is convincing evidence of an ancestor-descendent relationship" (p. 59)) may at least not be a fundamental problem for the relatives of the same species, although the above quoted objections of Wells and Nelson would not be off the table for this concrete case.

Additionally it has to be pointed out that, if the identification is correct, a (presumed) transitional species (*Samotherium africanum*) would have to be eliminated from the postulated evolutionary series – and with this the authors would be further removed from their goal, namely the proof of transitional forms („a history of intermediate forms does exist").

It has to be emphasized that with *Samotherium/Palaeotragus* and the genera to be discussed, we are talking only about short-necked giraffes, and I would like to stress again that ***to date the expected continuous series between short and long-necked giraffes is entirely missing***. What is the situation, however, with regard to continuous series within the short-necked giraffes?

Let us look more closely at *Palaeotragus*. Mitchell and Skinner write (p. 58/59):

> "*Palaeotragus* sp. were **medium sized giraffids having limbs and neck slightly elongated** [like *Okapia*; note by W.-E.L.], usually with a single pair of horns that were sexually dimorphic. Their skull was elongated and broadened, especially between the horns (Forsyth Major, 1891), but did not contain the sinuses so characteristic of later *Giraffa*. They ranged from East Africa (Churcher, 1970) to Mongolia (Colbert, 1936b), immense distances apart.
>
> Churcher (1970) described the earliest palaeotragine from fossils recovered from the Fort Ternan (and also Muruorot and Rusinga), a deposit dated at 14 Mya (Retallack *et al.*, 1990), and named it *Palaeotragus primaevus*. At Fort Ternan this species was so common that it could be described from 243 specimens. It had gracile long limbs, and we can conclude it was a powerful runner and leaper. Its dental formula (Churcher, 1970) was:
>
> 0/3 C 0/1 P 3/3 M 3/3 = 32.
>
> which is the same as *Giraffa* [***and Okapia* and "the same as that of cervids, bovids, and pronghorn antelope"-** Dagg and Foster, p. 176; note by W.-E.L.]. The lower canine was bilobed. Its teeth were however primitive being slim, not broadened, and brachydont. It depended almost completely on browse for food and water (Cerling *et al,* 1991, 1997). The shape of its muzzle was similar to okapi and giraffes (Solounias & Moelleken, 1993), and its teeth show microwear patterns of pits and scratches, which are determined by food, similar to those found in modern giraffes (Cerling *et al*, 1997). Churcher (1970), following the assumptions of the time, regarded *P. primaevus* as an offshoot of the Asian palaeotragine stock that had reached Africa by migrating across the Suez isthmus as sea levels fell between 23 and 16 Mya (Figure 4). Both Hamilton (1978) and Gentry (1994) regarded ***P. primaevus* as being close to or identical to *Giraffokeryx punjabiensis***, and this linkage provides the continuum between *Giraffokeryx,* which was becoming extinct, and the palaeotragine assemblage that filled the niche created."

Above we have stated that according to Harris, Mitchell and Skinner *Samotherium africanum* together with *Palaeotragus germaini* probably belong to the same species, which means that species-separating characteristics are not yet known [see previous page]). How then is this assertion compatible with their statement (p. 59): „Sinuses were absent in *Palaeotragus* and therefore in the Samotheres represent an evolved and developed feature"? – The authors do not, however, speak of a smooth transitional series between these characteristics.

P. primaevus is again said to be "close to or identical to *Giraffokeryx punjabiensis"* – thus it appears that only the differences between *Palaeotragus germaini* and *P. primaevus* remain to be clarified. Mitchell and Skinner remark about the two species, p. 59:

> "In Africa two *Palaeotragus* sp. are thought to have existed: *P. primaevus* and *P. germaini*. ***P. germaini,*** a paleotragine first described by Arambourg (1959) and known from Moroccan, Algerian and Tunisian fossil deposits of the late Miocene and therefore later than *P. primaevus (Giraffokeryx)*, **was of large size** and resembled *Giraffa* in its elongate neck and limbs. The evolutionary line of these species **could be** *Canthumeryx > Injanatherium > Giraffokeryx/P. primaevus > P. germaini."*

A more detailed comparison between *Giraffokeryx/Palaeotragus primaevus* and *P. germaini* is not provided. We only learn that *P. germaini* was „of large size" and the following clause contains a fundamentally false assertion („…resembled *Giraffa* in its elongated neck and limbs" – as if the species were a transitional form to the long-necked giraffe). Yet, according to the statement of the authors themselves, it only connects *Giraffokeryx/P. primaevus* and *Samotherium africanum* ("A more likely origin of *S. africanum* is *P. germaini*" – assuming *P. germaini* is not identical to *S. africanum* and thus does not belong to the same species). *In all these cases, however, we are clearly dealing only with short-necked giraffes.* „*P. germaini* is a moderate sized giraffid of the late Miocene (Arambourg 1959, Churcher 1979)" – Tsujikawa 2005, p. 37(5b) (see also Solounias 2007, p. 258).

In the text these authors time and again use ***suggestive*** allusions and phylogenetic interpretations in the sense of their gradualist views, and to be sure, with interpretations that go far beyond the facts and in part are even contrary to the phylogenetic scheme of the authors themselves. They could have more correctly said: „…resembled ***Okapia*** in its elongate neck and limbs much more than *Giraffa*“.

Now, with Churcher, we have already established that the species *Palaeotragus primaevus* is not yet completely known. Recall please that:

> “**Unfortunately the skull is not known and the absence of ossicones can only be inferred**, since the only possible ossicones preserved in the deposits are larger than recorded for *Palaeotragus* and match best those given for *Samotherium* (Bohlin 1926).”

Palaeotragus germaini is not completely known either (Churcher p. 516). Can one really, with such gaps in our knowledge, establish a gradual evolution between the different groups of the short-necked giraffes?

Interestingly Churcher (78, p. 528) offers an evolutionary tree that differs in several points strongly from the reconstruction of Mitchell and Skinner:

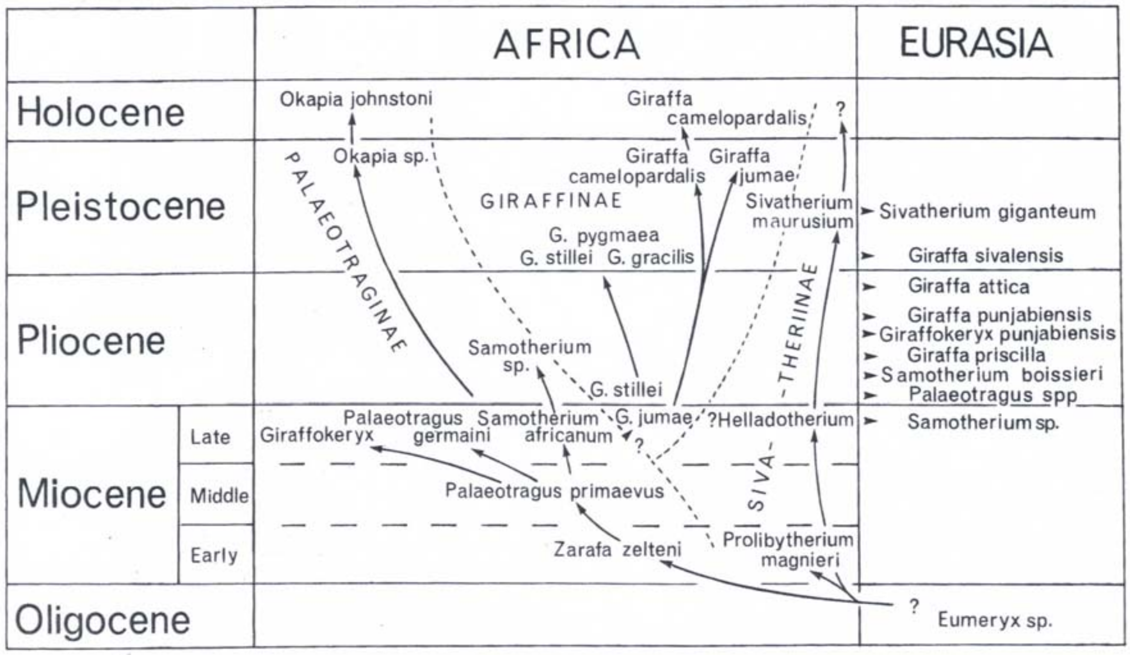
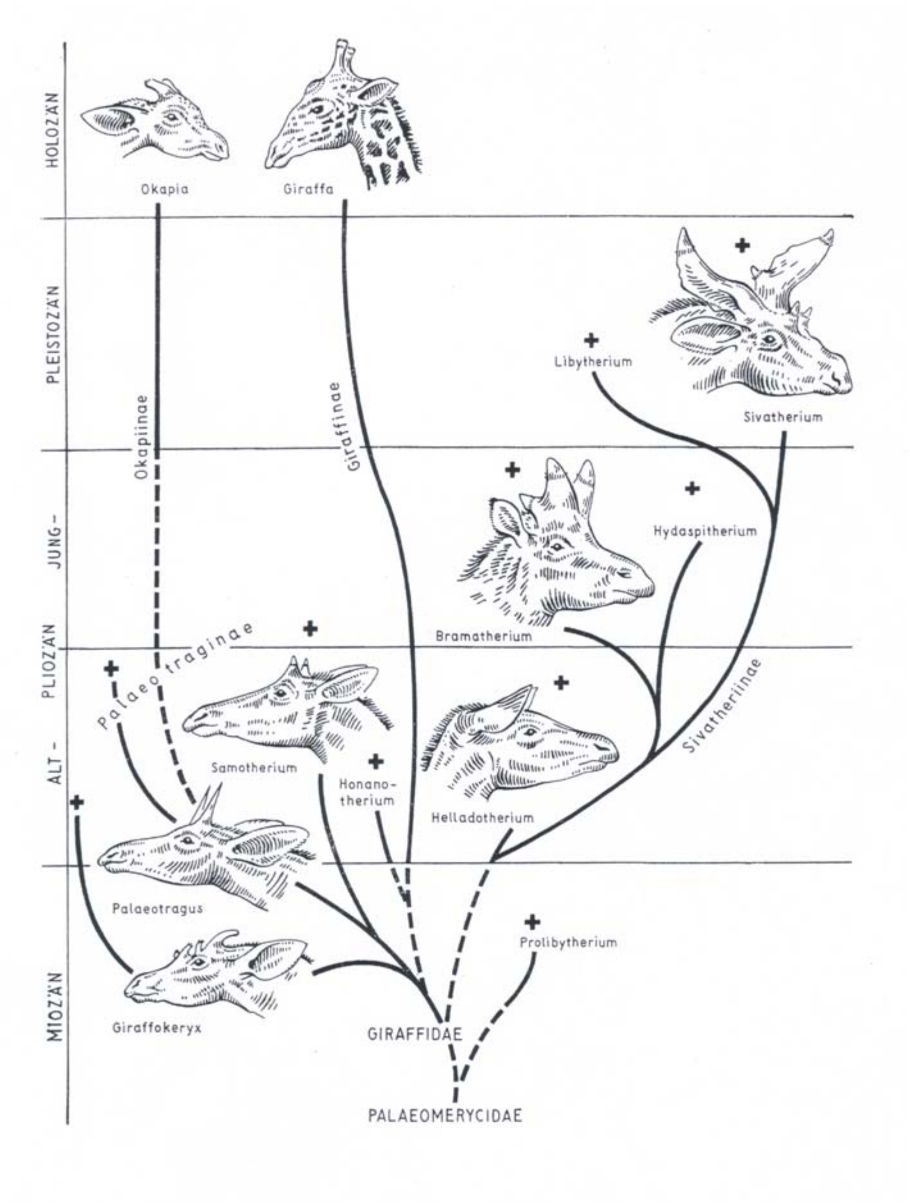


Figure 25.9 Relationships of the African Giraffidae, revised on the basis of an original Miocene radiation from which Europe and Asia were subsequently colonized. Arrows suggest possible lineages but not necessarily direct descent; dotted lines separate subfamilies; carets indicate possible migrations from northern Africa into Eurasia; and question marks indicate putative origins or occurrences.

According to the likewise hypothetical phylogeny of Churcher, *Giraffokeryx* and *Palaeotragus germaini* do not lie on the line that could have led to the long-necked giraffes and the connection to *Samotherium africanum* is uncertain. According to Thenius (next figure) *Palaeotragus* and *Samotherium* lie entirely on assumed side branches. The largest giraffe species, *Giraffa jumae*, is placed by Churcher next to *Samotherium africanum* as its possible nearest relative, which again highlights the huge jump between short and long-necked giraffes.

By the way, Solounias et al. do not accept the hypothesis that a Middle Miocene radiation in Africa was the starting point of the Eurasian populations (1998, p. 438): "We propose that many modern African savanna dwelling large animals originated not from forest dwelling African Middle Miocene relatives, but rather from taxa of the Pikermian Biome."

The evolutionary tree of Thenius differs from the representation of Mitchell and Skinner as well as from that of Churcher (although the latter resembles in several points that of Thenius 1972, p. 250):



Even though some new finds have been made in the interim, the existing ambiguity on the question of the origin regarding the short and long-necked giraffes (see also Part 1 of this work) shows beyond any doubt that the proof of gradual evolution through "*very fine-grained sequences documenting the actual speciation events*" so far does not exist (not to mention that – as emphasized above – even if such evidence existed, it would not solve the fundamental problems cited above by Kuhn, Wells and Nelson).

Simmons and Scheepers distinguish *two evolutionary lines*, and *Samotherium* does not lie on the line that would lead to Giraffa. They intrepret the hypothetical lines again exclusively from a selectionist viewpoint (1996, pp. 776/777):

> "Among fossil giraffids two evolutionary lines are apparent. Among Pliocene Sivatheriinae, evolution favored massive oxlike animals with long robust anterior limbs to support great weight and more elaborate deerlike horns or ossicones (Harris 1974, 1976). Deep pits in the horns for the attachment of large neck muscles were also obvious (Foster and Dagg 1972; Churcher 1976), **but necks were unelongated**. This is characteristic of most Sivatheriinae giraffids (Singer and Bone 1960; Churcher 1976; Harris 1976). These and other examples indicate that **the largest fossil giraffid *(Samotherium),* with a leg length 83% that of *Giraffa camelopardalis* (Colbert 1938), did not exhibit parallel increases in neck length**. Instead, selection appeared to favor heavier bodies, large, heavy necks, and changes in horny growths on the skull. Such traits are typical of sexually selected armaments among extant mammals (Geist 1966; Clutton-Brock 1982). **The other evolutionary trajectory** was from savannah-dwelling **okapi-like animals** *(Palaeotragus primaevus* and *Palaeotragus stillii)* that were agile and fast with relatively long legs and necks."

So the authors do not consider *Samotherium africanum* as an ancestral species (in contrast also to the phylogenetic tree of Devillers and Chaline 1993, p. 208, and other authors). Unlike Mitchell and Skinner as well as Churcher, Hamilton places *Giraffokeryx* within the Sivatheriinae (1978, p. 166):

> "With slight changes the subfamilies Sivatheriinae and Giraffinae are valid monophyletic groups. *Hydaspitherium* is synonymized with *Bramatherium* and the **Sivatheriinae** includes the genera ***Giraffokeryx***, *Birgerbohlinia, Bramatherium* and *Sivatherium* while the Giraffinae includes the genera *Honanotherium, Bohlinia* and *Giraffa* and the species *'Palaeotragus' tungurensis (P. primaevus).*"

And he justifies his view on placing *Giraffokeryx* in the Sivatheriinae subfamily as follows (p. 219):

> „This group [the Sivatheriinae] is characterized by the **large ossicones which are unlike those found in any other giraffid**. Features of the metapodials, neck and possibly the P_4 suggest that the *Samotherium* and *Palaeotragus* groups and the giraffines are closely related and the sivatheres are identified as the sister-group of these giraffids. *Giraffokeryx* is the only other giraffid which may be identified with the sivatheres. The synapomorphy linking this genus with the sivatheres is the presence of **two pairs of well developed ossicones**. The *Bramatherium* species were shown to have an apomorphy of the ossicones in which the anterior pair were large and the posterior pair small. The *Sivatherium* species have the apomorphy of large posterior ossicones and smaller anterior ones. The condition in *Giraffokeryx* with both pairs of ossicones approximately the same size may be identified as plesiomorphic for the sivathere group. Pilgrim (1941, p. 147) indicated the development of some complication of the ossicones in *Giraffokeryx.* Identification of *Giraffokeryx* as a sivathere would not conflict with any of the evidence presented by the dentition: indeed the P_3 and P_4 of BMM 30224 are surprisingly similar to those of *Giraffokeryx.*"

Thus we have already three different opinions on the evolution and systematics of *Giraffokeryx*: 1. Mitchell and Skinner: („Both Hamilton (1978) and Gentry (1994) regarded *P. primaevus* as being close to or identical to *Giraffokeryx punjabiensis"* and *Giraffokeryx* "has all the attributes of a giraffe ancestor and occupies the right evolutionary position.") "…***Giraffokeryx* [is] an ancestral species, to *Giraffa***"; 2. Thenius and Churcher: Giraffokeryx is **an extinct side branch** of the Palaeotraginae and 3. Hamilton: ***Giraffokeryx* does not belong to the Palaeotraginae,** but rather to the Sivatheriinae and thus **cannot even be considered as an ancestor of the giraffes.**

If one had "*very fine-grained sequences documenting the actual speciation events*", that is, data which would allow a gradualist interpretation in the neo-Darwinian sense, such astonishing contradictions would not be possible.

Incidentally one might ask why Mitchell and Skinner choose to refer to Hamilton. The latter remarks (p. 186):

> "Aguirre & Leakey (1974, pp. 225-226) record the presence of *Giraffokeryx* sp. nov. from Ngorora and figure two specimens which they describe briefly. These specimens agree closely with *Palaeotragus primaevus* from Ngorora and Fort Ternan and I suggest that they are **incorrectly identified with *Giraffokeryx***. Aguirre & Leakey do not refer to Churcher's (1970) description of the Fort Ternan giraffes and it is possible that they were not aware of its publication. Figures from Aguirre and Leakey are referred to where relevant in the following description."

This quotation is followed by a detailed description of the specimens. If I understand Hamilton correctly, they point to a misinterpretation of Aguirre and Leakey who have identified certain *Palaeotragus-primaevus*-finds incorrectly with *Giraffokeryx* and not because *P. primaevus* is „close to or identical to *Giraffokeryx*". However, Gentry (1994, p. 135) corroborates the view of Mitchell and Skinner (for the details, see Note 5c).

Geraads takes Giraffokeryx as a separate genus and comments on the origins questions as follows (1986, p. 476):

> „La trichotomie Sivatheriini/Giraffokeryx/Giraffini, la position de *Palaeotragus*, la définition précise des Giraffini, sont quelques-uns des **problemes non resolus**."

Anyway, either *Palaeotragus primaevus* and *Giraffokeryx* are so closely related that one cannot rule out that they belong to the same species, and in this case, too, only the names have evolved (and the gap to the nearest relatives among the short-necked giraffes naturally becomes wider) or they, in fact, belong to different genera without a continuous transitional series connecting them. Evidence for a gradual evolution connecting the larger groups within the short-neck giraffes in either case is nonexistant.

If the identification of *Palaeotragus primaevus* with *Giraffokeryx* is correct, another link (namely, either *P. primaevus* or *Giraffokeryx*) has to be eliminated from the postulated evolutionary series and the authors again take an additional important step farther away from their goal, namely the proof of an transitional series in Darwin's sense („a history of intermediate forms does exist"). The hypothetical evolutionary series for the short-necked giraffes *Canthumeryx -> Injanatherium -> Giraffokeryx ->P. primaevus -> P. germaini ->S. africanum* would be reduced to *Canthumeryx -> Injanatherium -> P. primaevus -> P. germaini* .

b$_4$) *Canthumeryx* and *Injanatherium*

Canthumeryx according to Mitchell and Skinner (Figure 10. A.), from Churcher 1978.

Regarding *Canthumeryx* Mitchell and Skinner remark, among other things (pp. 57/58):

> "*Canthumeryx* was a medium sized, slender antelope about the same size as a fallow deer *Dama dama* (Hamilton, 1973, 1978). Crucially it had the characteristic bilobed giraffoid lower canines. Hamilton (1978) further suggested that the utility of this feature was that it facilitated stripping of foliage from browse. Its

> limb length can be calculated to have been about 85-100 cm long, and its shoulder height would therefore have been about 1.5 m. It had unbranched (simple) horns that projected sharply laterally and lay almost horizontally from a position above its orbits (Figure 10A). Its skull was wide and had large occipital condyles (which articulate with the first (atlas) vertebra), but the **atlas was not elongated** having a length to width ratio of 1.03 *cf.* that of a giraffe of 1.17. Like **its gelocid ancestor** it seems to have been very similar to a lightly built, medium sized, slender-limbed, but in this case, a not very agile **gazelle**."

The assertion about the genetical derivation of this antelope from the Gelocidae („its gelocid ancestor"), offered as fact, rests once more on the not-stringent proof due to morphological similarities, and faces anew the problems described above by Kuhn, Wells and Nelson. In the current state of affairs, it belongs to the realm of faith statements. This is equally true of the following claim about *Georgiomeryxs* as a direct descendent of *Canthumeryx*. Again, according to Mitchell and Skinner (p. 58):

> "Related and later species have been discovered throughout the middle east, in Iraq and Saudi Arabia and Greece, and these species existed over a period 18-15 Mya. The species that are **similar** to *Canthumeryx* are *Injanatherium,* which flourished in the mid-Miocene in Saudi Arabia and in the late Miocene in Iraq (Morales *et al,* 1987), and *Georgiomeryx* from Greece (De Bonis *et al.*, 1997). *Georgiomeryx* was a direct descendant of *Canthumeryx,* had flattened supra-orbital horns, and its fossils have been dated to 15.16 to 16.03 Mya (De Bonis *et al,* 1997). ***Injanatherium,* significantly, had two pairs of horns** and its later age and distribution of its fossils suggest that it occupied a more easterly, Asian, part of the central southern European biome, while *Georgiomeryx* had migrated more westwards".

At this point one may raise the question concerning the existence of a continuous transitional series from the two-horned to the four-horned species. To my knowledge there is not yet any find that would support such a derivation.

> „While *Canthumeryx* and its relations clearly are at the base of the *Giraffa* line, they existed 10 to 15 My before the first appearance of *Giraffa* and **clearly did not have a giraffe-like shape**. They also appear to have become extinct towards the early middle Miocene about 14 or 15 Mya. The 7 to 8 My gap between them and the appearance of the first undoubted giraffes has to be filled, therefore, by some or other ancestor. It is filled first by *Giraffokeryx"* (Mitchell and Skinner p. 58).

Giraffokeryx seems to fit chronologically – where, however, is the evidence of a continuous morphological transitional series between the gazelle *Canthumeryx* and the short-necked giraffe *Giraffokeryx*? What about the origin of the decisive new characters such as the *ossicones*?(6)

> „It [*Giraffokeryx*] was a medium sized member of the Giraffidae distinguished by two pairs of horn cores (ossicones)" (see the corresponding figure)…" "The horns differ in that cervid antlers are deciduous while those of giraffids and bovids are not. They differ also in their anatomical origins. Cervid antlers and bovid horns are an outgrowth of bone base while **giraffe horns develop from an epithelial cartilaginous growth point** (Lankester, 1907), which subsequently ossifies and fuses with the skull. This difference in origin of giraffid horns is captured in the name "ossicone" (Lankester, 1907)." – Mitchell and Skinner pp. 58 and 55/56.

The following figure illustrates some of the phylogenetic questions:

On the right side, diagrams C and D show reconstructions of *Giraffokeryx* according to Colbert, Savage and Lang. The representation of the neck is exaggerated by Colbert (1935), as the correction to a representation of a shorter neck by Savage and Lang (1996) shows – although even this neck length may be somewhat exaggerated. Otherwise *Giraffokeryx* has already a longer neck than the supposed *„intermediate forms"* of the *Palaeotragus*- and *Samotherium*-species.

Between the gazelles *Canthumeryx* and *Injanatherium* respectively and the short-necked giraffe *Giraffokeryx* (=*Palaeotragus primaevus?*) exists a **gigantic morphological-anatomical gap,** which may come close qualitatively to the gap between short-necked and long-necked giraffes. Once more we note the tendency to cover up decisive evolutionary questions with diversionary tactics and with seemingly certain chronological dates ("The 7 to 8 My gap between them [*Canthumeryx* and *Injanatherium*] and the appearance of the first undoubted giraffes has to be filled, therefore, by some or other ancestor. It is filled first by *Giraffokeryx*").

Diagrams C and D: different reconstructions of *Giraffokeryx punjabensis*: C from Colbert (1935) and D from Savage and Lang (1996) – both from Mitchell and Skinner, p. 58. Left: Diagrams of the sub-species *reticulata* (top left), *angolensis* (top right) and *tippelskirchi* (right front) of *Giraffa camelopardalis* and to the far left below, in comparison *Okapia johnstoni* from Grzimek's Tierleben, Vol. 13, p. 261.

There exists a general tendency of numerous authors and artists for all reconstructions of species that could have anything to do with the giraffe, to represent the neck longer than it really is. Even on the "medium-sized slender antelope" *Canthumeryx*, reproduced on page 43, a longer neck is indicated than it really had. Examining the original paper of Colbert (1935) on *Giraffokeryx* one has to realize that among the fossil material he dealt with there were no vertebrae. The longer neck in Colbert's figure was not based on new evidence.

b$_5$) *Climacoceras*

Regarding *Climacoceras* Mitchell and Skinner remark, among other things (p. 57):

> „ Maclnnes called it the "fossil deer" of Africa saying it was the **size of a roe deer, *Capreolus capreolus***. … …although having features that indicate their closeness to giraffes **they are not on the lineage that leads to**

modern giraffes. It is more likely that *Climacoceras* gave rise to a sister group of *Giraffa*, the Sivatheriinae. Sivatheres were as big as elephants, *Loxodonta africana*, massive and heavily built, short-legged, short-necked, with large and ornamented horns (Figure 9C, D).

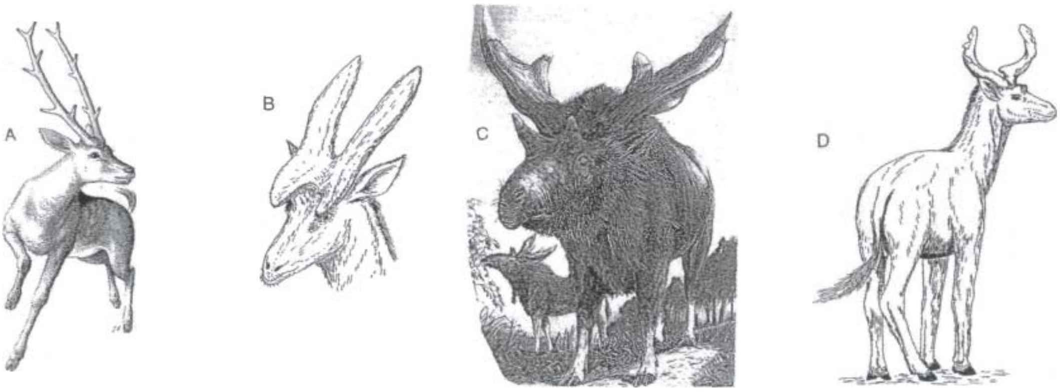


Figure 9 of Mitchell and Skinner 2003, p. 57: “Reconstructions of Sivathere species.
A. *Climacoceras* from Hendey (1982); B. *Prolibytherium magnieri* from Churcher (1978);
C. *Sivatherium giganteum* from Savage & Long (1986); D. *Sivatherium maurusium* from Churcher (1978).”

As we have already established in the first part of our giraffe paper, a continuous transitional series from the presumed ancestors among the *Cervidae/Palaeomerycidae* to *Climacoceras* is lacking, as well as from *Climacoceras* to the Sivatheriinae. The wording "it is more likely" shows only that we know nothing concrete, but under evolutionary presuppositions can assume phantastically many things. The assertion: "The Palaeomerycinae were the origin of the Giraffidae" (p.56) is once more a statement of faith in the sense of Lunn: "Faith is the substance of fossils hoped for, the evidence of links unseen." Proof is lacking.

Summary of the evolutionary hypotheses of Mitchell and Skinner: In the introduction of the discussion of the paper by G. Mitchell and J. D. Skinner *On the origin, evolution and phylogeny of giraffes Giraffa camelopardalis* (2003) we have mentioned that the authors start with the declared goal to justify Darwinian gradualism for the origin of the long-necked giraffe, and that critical thinking and alternatives to gradualism are treated from the beginning as „folklore tales“.

However, after the detailed discussion of the problem of selection we have come to the conclusion that the authors (according to their own thorough analysis, for which we have expressed our respect for the writers) not only were not able to offer any convincing selectionist hypothesis for the origin of the long-necked giraffe, but they have even offered numerous arguments and facts contradicting all the selectionist explanations proposed thus far. A conclusive *mechanism* for the appearance of the long-necked giraffe is thus far completely unknown.

Moreover, the authors have promised to deliver evidence for the case of *Giraffa camelopardalis* „that a history of intermediate forms“ does indeed exist. However, in our analysis we had to conclude that (1) neither the long-necked giraffe *Bohlinia attica* (2) nor the short-necked giraffes *Samotherium, Palaeotragus* and *Giraffokeryx* can be considered to be „intermediate forms“, (3) that determining the exact boundaries of several species of these genera is problematic due to insufficient fossil material or to questions of synonymy, (4) that the authors apparently have correctly perceived Gentry's comment when they identify *Palaeotragus primaevus* with *Giraffokeryx* but seem to have misunderstood Harris and (5) that if their identification in this case as well as that of *Samotherium africanum* with *Palaeotragus germaini* is correct, they are left with **two** (of the five to six genera considered by them as possible) transitional forms **fewer** than before.

Due to the lack of transitional series and the other unsolved problems listed above, the various experts offer *several hypotheses which completely contradict each other not only regarding the evolutionary derivation of the long-necked giraffe* but also regarding such derivations within the short-necked giraffes. And finally we had to conclude once more that the gap between the short-necked giraffes and their postulated ancestors from the Canthumerycidae is likewise not bridged by a continuous series of intermediate links, not to mention the origin of the Canthumerycidae itself..

The method practiced by the authors in this part of their paper – entirely in contrast to their exact analysis of the selectionist deductions – to cover up most of the decisive problems of evolution, as well as their attempt to support their gradualist view by ***suggestive*** allusions and evolutionary presuppositions etc., instead of clearly conveying the relevant scientific problems, is not helpful to detect the truth on these questions. Their following statement may be also characterized as a illusion (p. 65): „Throughout the giraffid fossil record there is clear evidence of progressive limb and neck elongation.“(7) The fact, however, is that a continuous transitional series is lacking, not only between the short-necked giraffes and the antelopes (their supposed ancestors) but also within the large group of short-necked giraffes themselves, and between the short and long-necked giraffes.

The homologous similarities themselves, which we notice between both fossil and living genera of Giraffidae, can very well be understood in the sense of the so-called idealistic morphology (Linné, Cuvier, Agassiz, Dacqué, Kuhn, Troll, Vogel and many others).

Now we can quote once more the words of Mitchell and Skinner in the altered form not only on the selectionist explanations but *also on the phylogenetic derivation* of the long-necked giraffes: ***“One of the more enduring folklore tales about modern giraffes is that they prove Darwinian “long continued” gradualistic evolution by natural selection” –*** which anew may remind us especially of the „many African folk legends before him [Darwin]“ .

12. Concluding remarks

In the first part of the paper we have come to the conclusion that the assertions on the evolution of the long-necked giraffes by Ulrich Kutschera, Richard Dawkins and Kathleen Hunt do not have a scientific basis. This is also true for macroevolutionary propositions of Mitchell and Skinner and others, which have been discussed in the second part. Although an absolute negative proof is nearly or completely infeasible, nevertheless the scientific data that are available to date on the question of the origin of the giraffe make a gradual development by mutation and selection so extremely improbable that in any other area of life such improbability would force us to look for a feasible alternative.

Yet biologists committed to a materialistic world view will simply not consider an alternative. For them, even the most stringent objections against the synthetic evolutionary theory are nothing but open problems that will be solved entirely within the boundaries of their theory. This is still true even when the trend is clearly running against them, that is, when the problems for the theory become greater and greater with new scientific data. This essential unfalsifiablity, by the way, places today's evolutionary theory outside of science, one of whose defining characteristics is that theories can only be considered to be scientific if they are falsifiable, and when they set forth criteria by which they can potentially be falsified.(8)

For the intelligent-design-theory (ID), on the other hand, not only have potential falsification criteria been presented (see above and http://www.weloennig.de/NeoC.html and also http://www.weloennig.de/NeoVorKl.html and http://www.weloennig.de/Popper.html), but it also offers numerous further positive research possibilities (see for the giraffes the research program described also above as well as http://www.weloennig.de/DynamicGenomes.pdf). Furthermore, the ID-theory is in full agreement with the known biological facts – from genetics (cf., for example http://www.weloennig.de/Gesetz_Rekurrente_Variation.html) to paleontology (http://www.weloennig.de/AesIV5.SysDis.html) and makes numerous biological *predictions* on questions which the synthetic evolutionary theory in principle cannot answer – see the comparison of the synthetic evolutionary theory with the ID-Theory: http://www.weloennig.de/IntelligentDesign.html .

In this connection it should be clear that *on the scientific level* the two present articles on the evolution of the long-necked giraffe are only a beginning (even if one, on a personal level, may consider the basic questions to be completely solved): What we need is an international research group that goes on to critically evaluate the question of the origin of the long-necked giraffe on the paleontological, anatomical, physiological, ethological and genetic levels without a dogmatic commitment to a neo-Darwinian worldview, and which includes the ID-question *sine ira et studio*. In this way one may predict that many of the questions discussed above will be further corroborated and confirmed in agreement with the intelligent design theory, but in some areas perhaps in a way that we could never before have suspected („…*the universe is not only queerer than we suppose, but queerer than we can suppose*“ – Haldane, similarly Eddington), yet I would like to add: „…*not only queerer but also often harbouring a more ingenious design than we can suppose*). But this only adds to the attraction of a nondogmatic research.

Finally, with regard to an aesthetic treatment of today's giraffes, I would like to repeat an observation of Lynn Sherr, which deals with, among other things, the beauty of *Giraffa* (1997, p, 55):

> "[I]t is the aesthetic of the eye that appeals to us above all – its “bewitching softness,“ in the words of one converted hunter. I have gotten lost in a giraffe eye, too, mesmerized by the high gloss and sympathetic expression beneath those long, straight lashes. "There is nothing to compare with its beauty throughout the animal creation," wrote Sir Samuel Baker, who got to know giraffes after helping discover the source of the Nile.
>
> A zoo curator I know, a bachelor, confessed to me with absolutely no embarrassment, “The day I find a woman with eyes as beautiful, I’ll get married.""

It goes without saying, that this animal species must also be treated with care, in the sense of a modern and compassionate understanding of Nature. Regarding the treatment, see Note (9).

13. Acknowledgements

I would like to thank Professor Granville Sewell, Mathematics Department of the University of Texas El Paso, for the English translation of Part 2 of this giraffe paper. Mr. Roland Slowik prepared the figure showing the simultaneity of the genera. Dr. Wolfgang Engelhardt (physicist, Munich) gave me the German translation of the book of G. R. Taylor *The Great Evolutionary Mystery* as a present. Last (and of course) not least, I thank the One without whom there would be no giraffes (Revelation 4:11 – for several nobel laureates with a similar attitude, see http://www.weloennig.de/Nobelpreistraeger.pdf).

13a. Notes

(1) (From page 6): A couple of points should be mentioned (p. 775):

> “In the Serengeti, giraffes spend almost all of the dry-season feeding from low Grewia bushes, while only in the wet season do they turn to tall *Acacia tortilis* trees, when new leaves are both proteinaceous and plentiful (Pcllew 1984a) and no competition is expected. This behavior is contrary to the prediction that giraffe should use their feeding height advantage at times of food scarcity. Neither are giraffe exploiting better-quality (higher-protein) foods at such times since dry-season scarcity of leaves coincides with the lowest protein levels in *Acacia* leaves (Sauer et al. 1982). Similarly, in the Tsavo National Park, about 50% of all browsing is **below 2 m (less than half the height of both sexes)** and thus within reach of potential competitors such as gerenuk *Litocranius walleri* and lesser kudu *Tragelaphus imberbis* (Leuthold and Leuthold 1972). During the dry season, 37% of the browse taken by giraffe was below 2 m. Giraffe were not avoiding interspecific competition by selecting different food plants (the third prediction): considerable (unquantified) overlap was apparent between giraffe and sympatric browsers in Tsavo (Leuthold and Leuthold 1972). Only in South Africa were giraffe found to allocate 90% of their time to feeding above the average feeding height of browsers such as kudu *Traxelphus strepsiceros* (1,0 m) and impala *Aepyceros melampus* (ca. 0.3 m; du Toit 1990), but lower than their long necks allow (5-6 m). In each study both sexes frequently fed at or below shoulder height (ca. 3.1 m and 2.8 m for adult males and females; L. Scheepers, unpublished data). For example, female giraffe spent over 50% of the time feeding with their necks at or below shoulder height in both South Africa (du Toit 1990) and Kenya (Young and Isbell 1991), contrary lo the second prediction. So common is this behavior in females in eastern Africa that it is used as a field guide to sex individuals at a distance (Sinclair and Norton-Griffiths 1979; Pellew 1984a). However, low feeding heights are not restricted to females: males also regularly feed below or at shoulder height in Kenya, and only dominant bulls regularly fed at 5.0 m or more in both South and Eastern Africa (du Toit 1990; Youn and Isbell 1991).”

(2) (From page 7): The dates for the genera listed in the table (according to the document sent to me in early 2006 by M. Fortelius from his paleontological data bank; see part 1 of this giraffe paper) are usually derived from the dating of numerous finds. So, for example, there is an entire series of dated specimens for *Giraffokeryx*. The highest datings lie between 17.2 and 15.2 million years, the lowest between 7.1

and 5.3 million years. In the history of paleontology it has happened thousands of times that due to further research, the dates for the life span of certain forms had to be extended in both directions (first and last appearances) – up to those forms now known as living fossils. Based on the frequencies one can speak here of a general tendency. *Regarding the Giraffidae and their morphological relatives, it goes without saying that the dates for species and genera listed in the table are not the final word.* Given this tendency to expand, I have listed the highest and lowest values that are currently available as maximum and minimum ages of the respective genera and species. This is also practiced in the renowned reference book of M. J. Benton (1993) *The Fossil Record 2* for all fossil groups. As for *Palaeotragus indet.*: although for several specimens the exact species determination was "indeterminable" (indeterminata), the genus could probably be identified, so that I have also included the youngest finds. It is to be expected that with increasing numbers of finds and data quantity, the currently known life spans of several genera will be further increased, so that the present maximum dates will be shown to have still been too low and the minimum dates too high .

With regard to *Giraffa jumae*, the oldest dating of about 12 million years ago is not mentioned by Fortelius. A special investigation is probably needed to accurately clarify how and why in this case a re-dating from at least 12 million years down to 7.1 million years has occurred. In this connection it may be instructive that several cases of chronologically inconvenient fossils (inconvenient from an evolutionary point of view) have illegitimately been made younger – a typical example is *Baragwanathia longifolia*, which belongs to the lycopods. This complex group of plants was not expected to appear in the Upper Silurian and occurred thus much too early according to evolutionary expectations. So after re-dating, it was moved to the Lower Devonian („made younger“), but then, based on further data, was finally dated back to the Upper Silurian (cf. Nilsson 1953, White 1990, Kotyk et al. 2002).

(2a) The time specifications for *Palaeomeryx* are contradictory. McKenna and Bell (1997/2000, p. 423) give, for this genus, the following dates: *E.-M. Mioc.; Eu. M. Mioc.; As* (E., early; M., middle), and they list *Bedenomeryx* and *Sinomeryx* with the genus *Palaeomeryx*. According to Jehenne (1988) *Bedenomeryx* is „**un nouveau genre** de ruminant primitif de **l'Oligocéne supérieur et du Miocène inférieur d'Europe**“. Further, in other references (that I could not yet check) two species of *Palaeomeryx* (*P. oweni und sivalensis*) are dated into the **Pliocene**. That would – if the datings and identifications are correct – considerably widen the time frame for this genus into both directions. (*Sinomeryx* also has yet to be checked.)

Hamilton 1978b, p. 498, writes about *Palaeomeryx*: “…middle-upper Miocene; Europe. ?lower Miocene; Africa.” And he comments on the African finds as follows:

> “Palaeomerycids were recorded from Africa by Whitworth (1958), who established the species *Palaeo-meryx africanus* to accommodate a small ruminant from Songhor, Koru, Moruorot, and Rusinga. Ginsburg and Heintz (1966) suggested that this species should be removed from the genus *Palaeomeryx*. They based their suggestion on interpretation of features of the premolars, particularly the presence in *"Palaeomeryx" africanus* of a P_1 and the primitive condition of the other anterior premolars. Ginsburg and Heintz suggested that this species should be placed in a new genus, *Kenyameryx*. I have argued (Hamilton 1973a) that *Palaeomeryx africanus* and *Walangania gracilis* (Whitworth 1958) are synonymous and that the species resulting from this synonymy, *Walangania africanus*, **is probably a bovid**. *Walangania africanus* is described and discussed by

> Gentry in this volume. Whitworth also described several isolated cheek teeth, which he identified as *"?Palaeomeryx sp."* In my description of the ruminants from Gebel Zelten (Hamilton 1973a) I identified the Palaeomerycidae as a family of the Giraffoidea and described a new genus, *Canthurneryx*, which I placed in the family. I also suggested that the genus *Palaeomeryx* was represented in the Gebel Zelten fauna by two molar fragments (BM M-26691 and BU-20112). A pair of ossicones (BM M-26690) was identified as Palaeomerycidae indet. In my discussion of the Palaeomerycidae I suggested that the "Oligocene genera which lack ossicones" should be removed from the Palaeomerycidae and that the African genus *Propalaeoryx* and the Iberian genus *Triceromeryx* should be included in the family. This left the Palaeomerycidae with the genera listed below:
>
> Palaeomerycidae
>
> *Climacoceras* Maclnnes 1936, middle-upper Miocene;
> Africa
> *Canthumeryx* Hamilton 1973, lower Miocene; Africa *Heterocemas*[3] Young 1937, upper Miocene; Asia
> *Palaeomeryx* Von Meyer 1834, middle-upper Miocene;
> Europe. **?lower Miocene; Africa**"

If both the early and the above-mentioned late appearances are correct, the dates of the simultaneous genera listed above should correspondingly be corrected.

(2a1) (Also from page 7): P. K. Basu lists (2004, p. 110) *Giraffa priscilla* „from the upper interval of the Lower Siwalik, Ramnagar" (Jammu, Sub-Himalaya, India). "The Ramnagar fauna represents the Chinji mammalian fauna (**Middle Miocene**) of the Potwar Plateau, Pakistan" (p. 105). For the Lower Siwaliks, Colbert (1935b, p. 9) has listed "*Giraffa priscilla* Matthew" as belonging to the group "Giraffinae, **Large Giraffids** with a moderately brachycephalic skull", besides *Giraffa camelopardalis* and several other species, and added the following remark: "Lower Siwaliks, Lower Pliocene." The specification "Lower Pliocene" is clearly obsolete in the interim. THE PALEOBIOLOGY DATABASE (2004) remarks on Ramnagar: "Key time interval: **Early/Lower Miocene – Middle Miocene**" and "**Age range interval: 23.03-11.61 m.y. ago**" and adds below the fossil finds of Basu. Basu himself leaves open the question of a more accurate dating (p. 116). Kollmann mentions (1999, p. 63) that the find of *Anthracotherium cf. bugtiense* provides the evidence of an early **oligocene** vertebrate fauna in the Lower Siwaliks (lower part) in Pakistan – the time frame for the Lower Siwaliks is thus greater than previously assumed. We work in the present paper with a conservative estimate of some 12 million years for *Giraffa priscilla*. The Serravallian (upper middle Miocene) was recently given a time frame of from 13.6 - 11.608 million years before the present (cf. Note (2d) in Part 1).

(2b) (Still page 7): As already mentioned in the first part of this work (pp 14-15), the majority of researchers include *Canthumeryx* (and thus also *Injanatherium*) in the short-necked giraffes. However, Hamilton 1978, p. 178 has removed these forms out of the Giraffidae family and placed them in their own family: Canthumerycidae („New Family"). He puts this family together with the Climacoceratidae and the Giraffidae in the *Superfamily* of Giraffoidea. As quoted on page 43, Mitchell and Skinner call *Canthumeryx* "a medium sized, slender antelope about the same size as a fallow deer *Dama dama*." In order to emphasize the later-discussed independence and differences of the genera of this family to those of the Giraffidae, I have listed them in Table 1 provisionally under the „deer-like hooved animals" *sensu lato* together with the Palaeomerycidae and Climacoceratidae. As noted on page 13 of the first part, *Climacoceras* is counted by Carroll 1988/1993 among the deer family Palaeomerycidae (the Palaeomerycidae family belongs to the *Superfamily* Cervoidea according to McKenna and Bell).

(2b1) (Supplement to page 8): E. Ray Lankester has (1891) illustrated the difficulty of the evolutionary view here by the following example: *"A little reflection suffices to show that any given living form, such as the gorilla, cannot possibly be the ancestral form from which man was derived, since ex hypothesi* ***that ancestral form underwent modification and development, and in so doing ceased to exist.****"* As to this problem, see further http://www.weloennig.de/mendel20.htm

(2c) (From page 14): Some authors however point out to the fact that at birth the neck of the giraffe calf is proportionally shorter than of the adult animal and they interpret this fact in a phylogenetic sense (Krumbiegel, p. 60):

> „The newborn is, as is usual for hooved animals, stilt-legged, that is, disproportionally long legged. Nevertheless, the legs are rather clumpsy and broad-hooved, with already strong prominent ankles. The neck, as a **new phylogenetic acquisition** is on the contrary, **still short**. This shortness is still more evident in the embryo (Fig. 37).“

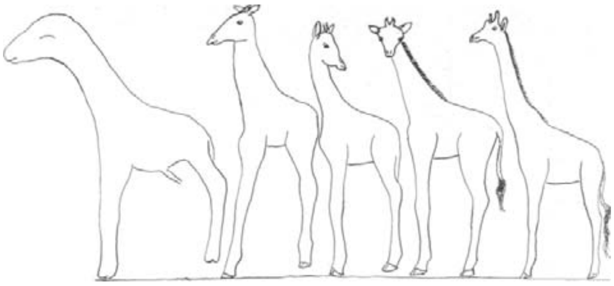


Fig. 37 from Krumbiegel 1971, p. 61: „Differences in body proportions during development. From left to right:: Embryos of approximately 50 cm tall.according to Krumbiegel 1955, preserved specimen of the Museum of Natural History, Berlin. At the ages of 24 hours, 32 days, 89 days....and full grown.“

As to the phylogenetic interpretation one may ask whether one should have expected an embryo with the *exact proportions of an adult animal* almost in the sense of a preformation theory. This is, however, very improbable for functional reasons alone. The specification „after 24 hours“ should be checked. In any case, the neck seems to be **astonishingly long** already in early ontogenetic stages as compared to the trunk. According to the “biogenetic law” such relative proportions should be expected only **very much later** in ontogenesis; regarding the dispute on this controversial „law“, a dispute continuing until this very day, see the informative paper of Markus Rammerstorfer: http://rammerstorfer-markus.batcave.net/ArtofCrHaekRekFinal.pdf as well as the textbook study of Casey Luskin: http://www.evolutionnews.org/2007/03/the_truth_about_haeckels_embry.html

(3) (From pages 19 and 25): Dagg and Foster (1976/1982) bring to our attention that many questions about the synorganized peculiarities of the long-necked giraffe are still open. The topic of the *Vascular system* is introduced (p. 166) with the words: „This system is the only one in which extensive physiological experiments have so far been carried out.” They provide the following details, among others (pp. 168/169):

> “As in most ruminants, the blood reaches the brain from the heart via the common carotids and the external carotids. The two latter vessels divide just before each reaches the brain into many small vessels forming a tight network that is called the *rete mirabile,* a structure that is present near the brains of many if not all ungulates. The vessels of the giraffe's *rete* have **elastic walls** which can accommodate excess blood when the head is lowered so that the brain is not flooded. As a further safeguard for the brain while the giraffe is in this position, **a connection between the carotid artery and the vertebral artery** drains off a portion of the blood even before it reaches this network. The walls of the *rete mirabile* vessels are also **elastic** enough to retain sufficient blood when the head is raised so that the brain's supply is not depleted momentarily until the system has adjusted to the pressure changes (Lawrence and Rewell, 1948).
>
> ….Several other anatomical factors help the giraffe adapt to its normal blood pressure – probably the highest present in any animal – and to sudden changes in that pressure. These factors include the **extensive presence of valves** in the vessels, the **structure and histology** of the vessels, and their **arrangement**. All of the large

> veins, the splenic, the renal, the saphenous, the brachial, the axial, and the inferior vena cava, have valves which counteract the effects of gravity, preventing excess backflow in the blood returning to the heart from the long legs (Amoroso *et al.*, 1947). Even the **jugular veins** have valves which prevent a backflow of blood to the brain when the animal leans down to drink. These pocketlike cusps may be present singly or in groups. Five tricuspid valves are present on the thick walls of the jugular vein, and tricuspid, bicuspid, and simple cusps are found in the brachial and axillary veins. The **tributaries** emptying into the jugular veins **also have valves** which are able to withstand high pressures in the jugular vein even if there are negative pressures in the tributaries themselves (Coetz and Budtz-Olsen, 1955). In an experiment carried out on a preserved length of giraffe axillary vein complete with its serried valves, the valvular system enabled the vein to withstand pressures up to 200 mm Hg, a value far above that which would occur naturally there (Amoroso *et al.*, 1947).
>
> The **structure of the blood vessels** also assists in regulating the circulatory system. The vessels in the legs, especially the veins, are very thick with tiny lumens. By contrast the jugular vein is also large, but the lumen diameter measures over 2.5 cm even at the base of the head. This vein is relatively collapsed when the head of the giraffe is upright, but when the head is down, it acts as a large reservoir that keeps the excess blood from flooding into the brain. Histologically, the aorta, pulmonary artery, and common carotid, as in the long-necked ostrich, consist mainly of elastic tissue in the well-developed middle layer of the vessel, with only a few scattered muscle fibers. The muscle fibers increase in prominence towards the head in the carotid (Franklin and Haynes, 1927). The entire wall of the aorta is 1.5 cm thick, that of the pulmonary 0.75 cm thick. **In the limbs, the histology of the vessels is reversed**. Here there is little elastic tissue and a thick layer of smooth muscle, largely situated in the huge *tunica media.* These leg vessels must withstand high hydrostatic pressures, which explains the necessity for their extensive muscularity.

Previously the authors, among several other points, report the ensuing facts concerning the muscular system (I have already called attention to some of these points in the first part of this paper). We read, from Dagg and Foster p. 166:

> “Rothschild and Neuville (1911) studied the omotrachelian muscle, which, in short-necked mammals, usually extends from the acromion of the scapula to the atlas. In the camel, whose neck is curved, this muscle is inserted at the fifth or sixth neck vertebra. **In the giraffe this muscle extends to the sixth or seventh cervical**. They also noted the often close correlation between muscle masses and whorls, feathering, and crests in the hair above these masses.
>
> Finally Joly and Lavocat (1843) commented particularly on the **absence of skin muscles in the giraffe**. Instead the **body is enveloped in a strong aponeurosis of fibrous sheet**, fastened loosely to the skin and often confused with the yellow fibrous fat layer. The giraffe is thus less able to dislodge insects and other pests by shaking its coat than are other animals.”

The long-necked giraffe displays very unusual structures and phenomena elsewhere, too (Dagg und Foster pp. 164 und 191): „The karyotype of the giraffe is similar to those of bovids, especially the sitatunga (Koulischer *et al.*, 1971).“ – One would really have expected a special similarity of the karyotype with those of the assumed deer relatives. But even more astonishing seems to me the following point: „Although it seems unlikely that **pronghorn and giraffe** could have evolved together to any extent, given their distribution, Beintema *et al.* (1979) have, in fact, found that the primary structure of their pancreatic ribonuclease is similar, indicating a close relationship. Using this criterion, both should be placed with the bovids rather than the cervids.”

(3a) (From page 19): Another author, Gordon Rattrey Taylor, comments on the question of the origin of the long-necked giraffe in his book *The Mystery of Evolution* as follows (1980, pp. 205/206):

> „While an adaptation of this kind [the giant clam *Tridacna gigas*] is hard enough to explain in terms of natural selection, the case of … the Giraffe, which calls for a **whole series of interlocked changes**, is probably even tougher. No one gave much thought to the giraffe's problems until World War II, when the difficulties which pilots of fighter aircraft experience under severe accelerational forces caused biologists to look around to see how animals cope with a reduced blood supply to the brain.
>
> Nineteenth-century observers assumed that the giraffe had only to develop a longer neck and legs to be able to reach the leaves which other animals could not. But in fact such growth created severe problems. The giraffe had to pump blood up about eight feet to its head. The solution it reached was to have a heart which beats faster than average and a high blood pressure. When the giraffe puts his head down to drink, he suffers a

> rush of blood to the head, so a special pressure-reducing mechanism, the *Rete mirabile,* [or "wondernet", of finely branched arteries, which is also present in other hooved animals (artiodactyla), cf. for example Futuma et al 2007, the long-necked giraffe however shows peculiarities, see Dagg and Foster above] had to be provided to deal with this. However, much more intractable are the problems of breathing through an eight-foot tube. If a man tried to do so, he would die - not from lack of oxygen so much as poisoning by his own carbon dioxide. For the tube would fill with his expired, deoxygenated breath, and he would keep reinhaling it.
>
> Furthermore, one study group found that the blood in a giraffe's legs would be under such pressure that it would force its way out of the capillaries. How was this being prevented? It turned out that the intercellular spaces are filled with fluid, also under pressure - which in turn necessitates the giraffe having a strong, impermeable skin. To all these changes one could add the need for new postural reflexes and for new strategies of escape from predators. It is evident that the giraffe's long neck necessitated **not just one mutation but many - and these perfectly coordinated.**"

(3b) (From page 21): However, this is not the rule („…injury from sparring is rare" – Dagg and Foster, p. 126).

(3c) (From page 23): Behe defines the concept of "irreducible complexity" (1996/2006, p. 39) as follows:

> "By *irreducibly complex* I mean a single system composed of several well-matched, interacting parts that contribute to the basic function, wherein the removal of any one of the parts causes the system to effectively cease functioning."

Concerning the following objection that is raised almost stereotypically against his test criterion for accessing the possibility of a gradual evolution, Behe comments in the ensueing paragraphs (2006, pp. 260/261):

> „Miller redefined irreducible complexity to mean that none of the component parts of an IC system could have its own function separate from the system. …In Miller's thinking, if he could point out that, say a piece of a moustrap could be used as a paperweight … then an "individual part" could serve a "function", "irreducible complexity" would vanish by definitional edict, and all good Darwinists could breathe easier once more. Yet there is no reason that individual components of an irreducibly complex system could not be used for separate roles, and I never wrote that they couldn't. Rather, for an IC system I wrote that "the removal of any one of the parts causes the system to effectively cease functioning" – *system*, not *parts*."
>
> „…In a more technical vein, Miller excitedly announced that some components of IC biochemical systems I discuss have other roles in the cell, such as the ciliary proteins tubulin and dynein. But I myself pointed that out when I first wrote Darwin's Black Box ten years ago"

(3c1) (From page 23): Harris writes 1976a, p. 315: (3c1):

> "Five giraffine taxa have been recorded from the early Pleistocene of Africa: *G. jumae, G. camelopardalis, G. gracilis, G. stillei* and *G. pygmaeus.* Cranial and postcranial characters appear both to separate and to support the acceptance of *G. jumae, G. gracilis* and *G. camelopardalis* as valid species although the presence of *G. camelopardalis* in the early Pleistocene has not yet been satisfactorily demonstrated. Giraffine teeth are remarkably uniform in morphology and tooth size is the only distinguishing dental character. On this basis *Giraffa pygmaeus* from East Rudolf and Olduvai would appear to be substantiated as a valid taxon. It is likely that *G. stillei* from Laetolil may be very closely related to *G. gracilis.* (A sixth species of *Giraffa,* intermediate in size between *G. gracilis* and *G. pygmaeus* sp. nov. is now known from the Pliocene of Ethiopia and from the Lake Baringo region of Kenya. This species is associated with *G. jumae* and *Sivatherium maurusium.* Its relationship, if any, to *G. gracilis, G. stillei* or *G. pygmaeus* sp. nov. is not yet determined.)
>
> The presence of so many giraffine species at this point in time in Africa needs some explanation. Perhaps it may be attributed at least partly to explosive evolution of the Giraffinae on reaching sub-Saharan Africa for the first time at the end of the Neogene. **Alternatively it is possible that giraffine taxa are more variable in their characteristics than has been accepted here and that African species of *Giraffa* are fewer in number than those listed above.** This premise, however, requires further and more complete material before it can he substantiated one way or another."

(3d) (From page 23): All the „species" of the extant genus *Giraffa* can crossbreed. Gray, in her work *Mammalian Hybrids* (1971, pp. 148/149) lists the following examples:

„Family GIRAFFIDAE [Giraffes]

Giraffa Brisson

478. *Giraffa camelopardalis angolensis* Lydekker [Angola Giraffe]
 x *Giraffa camelopardalis tippelskirchi* Matschie [Masai or Kilimanjaro Giraffe]
 A hybrid was born in Berlin Zoo in 1962.
 International Zoo Yearbook 1963.

479. *Giraffa camelopardalis antiquorum* Jardine [Kordofan Giraffe]
 x *Giraffa camelopardalis camelopardalis* Linnaeus [Nubian Giraffe]
 Hybridization occurred at Fort Worth, U.S.A., in 1962.
 International Zoo Yearbook 1963.

480. *Giraffa camelopardalis camelopardalis* Linnaeus [Nubian Giraffe]
 x *Giraffa camelopardalis antiquorum* Jardine [Kordofan Giraffe]
 See No. 479. x *Giraffa camelopardalis reticulata* De Winton [Reticulated Giraffe]
 Hybrids (at least one a female) have been born in zoos in Vienna (Austria) and Honolulu (U.S.A.).
 International Zoo Yearbook 1967, 19680, 1970.

481. *Giraffa camelopardalis cottoni* Lydekker [Cotton's Giraffe]
 x *Giraffa camelopardalis reliculata* De Winton [Reticulated Giraffe]
 A hybrid was born at Whipsnade Park (Great Britain) in 1961.
 International Zoo Tearbook 1962; Matthews, L. H. 1961, 1963.

482. *Giraffa camelopardalis reticulata* De Winton [Reticulated Giraffe]
 x *Giraffa camelopardalis camelopardalis* Linnaeus [Nubian Giraffe]
 See No. 480.
 x *Giraffa camelopardalis cottoni* Lydekker [Cotton's Giraffe]
 See No. 481.
 x *Giraffa camelopardalis rothschildi* Lydekker [Baringo Giraffe]
 A stillborn hybrid was produced in San Diego Zoological Garden in the I940's.
 Dolan, J. M. 1971.
 x *Giraffa camelopardalis tippelskirchi* Matschie [Masai or Kilimanjaro Giraffe]
 Male hybrids were born at Dudley (Great Britain) in 1967 and 1969, and also at Sacramento (U.S.A.) in 1968.
 International Zoo Yearbook 1969, 1970, 1971.

483. *Giraffa camelopardalis rothschildi* Lydekker [Baringo Giraffe]
 x *Giraffa camelopardalis reticulata* De Winton [Reticulated Giraffe] See No. 482.

484. *Giraffa camelopardalis tippelskirchi* Matschie [Masai or Kilimanjaro Giraffe]
 x *Giraffa camelopardalis angolensis* Lydekker [Angola Giraffe]
 See No. 478.
 x *Giraffa camelopardalis reticulata* De Winton [Reticulated Giraffe] See No. 482.”

“Hybrids of the giraffe also occur between different subspecies in the wild in border areas and hybrids [of subspecies] are also known among other cloven-hooved animals (Ruxton [and] Schwarz [1929])“ – See Krumbiegel p. 64, who continues with a list of examples, too. However, in contrast to these authors, Brown et al. (2007) suggest that there are at least 6 *Giraffa* species (if not many more): see my objections in the brief note in the references p. 79.

(3e) (From page 29, Lankester): Richard Milner mentions (1999, p. 90) regarding E. Ray Lankester among other things: „From his teens onward, he was a dedicated evolutionist" and further on the same page: "According to his biographer, Joe Lester, Lankester "remained Huyley's most faithful disciple"" on the socio-political as well as the biological level. Milner, however, qualifies this as follows (p. 93): „Unlike Huxley, Lankester was a doctrinaire materialist who thought science would ultimately explain everything about nature and human nature. With massive government support, it could banish ignorance, replace religion, and provide the foundation for a prosperous, moral, and just society. Only through obeying the laws of science, he wrote, could England hope to save her people from "degradation" and "degeneration."" – For our discussion on the giraffe, this comment seems to show that even a „dedicated evolutionist" and „doctrinaire materialist" was able to understand clearly that *Giraffa* is a genus which is „altogether exceptional, novel, and specialised". Incidentally, Milner's following comment (p. 90) distinctly reveals that in evolutionary questions there is often much more at stake than factual biology:

> "Lankester adopted not only Huxley's teaching techniques, but his **evangelical zeal** for spreading the **gospel of science and evolutionary biology** (Fig. 3). As Huxley put it: "Lankester...is helping me as Demonstrator in a course of instruction in Biology which I am giving to Schoolmasters - with a view of **converting them into scientific missionaries** to convert the Christian Heathen of these islands **to the true faith**.""

The comment on Fig. 3 reads: "Caricature of E. Ray Lankester published by *Vanity Fair* on June 12, 1905, when he was director of the British Museum (*Natural History*). The cartoon's legend states, "His religion is the worship of all sorts of winged and finny freaks."" (This reminds me of Romans 1:23.)

Supplement: On March 14, 2007 I was able to check Lankester's original work of 1908. Here is the quotation of Mitchell and Skinner in context (pp. 326/327): „There are a number of interesting details to be observed and discussed in regard to these **minor processes of the vertebrae** in different groups of mammals. My purpose is not now to enter on that subject, but merely to show briefly what is the value of the difference between Okapi and Giraffe in regard to the **inferior transverse process of the cervical region** – when the chief facts as to this structure in other mammals are taken into view. **Clearly enough it is Giraffe which is altogether exceptional, novel and specialised, not archaic or atavistic.** Giraffe has not even the great plate-like inferior transverse process on its 6th cervicals, which is obvious and prominent in such widely separate forms as the Hedgehog, the Carnivora, and the commoner Ungulata." This context qualifies, of course, Lankester's statement on *Giraffa.*

(3e) (From page 29): Cf. the detailed description of the problems by Dagg and Foster (pp. 66-68), which they introduce as follows: „Different writers disagree violently on the effect of the giraffe's coloring as a protection to it from its enemies, mainly lion and man." And after detailled discussion of the different viewpoints, they conclude (p. 68): "Which if any of the theories is correct can only be speculated." So we would like to point out that neither in the question of camouflage is there any convincing selectionist answer.

(4) (From page 32): Wilhelm Troll 1984, pp. 73-75:

"The explanation of homologies simply through common descent is thus no longer tenable. Nor is the so-called „law of Conditions of Existence“, that DARWIN even wanted to place above the "law of the Unity of Type".

[Quotation from Darwin]: „The expression ‚conditions of existence' is fully embraced by the principle of natural selection. For natural selection acts by either now adapting the varying parts of each being to its organic and inorganic conditions of life; or by having adapted them during past periods of time, the adaptations being aided in many cases by the increased use or disuse of parts, being affected by the direct action of external conditions of life, and subjected in all cases to the several laws of growth and variation. Hence, in fact, the law of the Conditions of Existence is the higher law, as it includes, through the inheritance of former variations and adaptations, that of Unity of Type" (116).

Ergo: Darwin eliminates the ideational [non-material] nature of the [biological basic] type, which is completely independent of the exterrnal world. According to him the „Unity of Type“, was due to common descent as well as an adaptation of the organism to the environment, and thus to be understood entirely as an effect of the environment, which D. H. SCOTT (117) states even more concisely when he directly says, „All the characters which the morphologist has to compare are, or have been, adaptive.“ **By this, Darwinism reveals itself to be a teleological system**, for which it doesn’t matter if problems of organic forms are viewed by final causes, that is, causes which, so to speak, preconstructed the organs for suitability, or a mechanism which constructs suitable structures. In any case, it appears to be really grotesque that Darwin in the 14th chapter of his main work rejects the consideration of final causes, which for him are identical with creationism (118), by the words: „Nothing can be more hopeless than to attempt to explain this similarity of pattern in members of the same class, by utility or by the doctrine of final causes", **while, in fact, his entire system is built on the point of view of utility, and is directly described by NÄGELI (110) as „doctrine of utility“.** In fact, teleology was inserted all the more into biology under the influence of Darwin’s work (120), yet a kind of teleological view of nature, to be sure, that is as far away from the classical idea of teleology as Darwinism is from "Natura", of the "Physis", which lives by creative powers.

As previously stressed, selection theory knows only the external or ecological usefulness, which to be sure cannot be strictly separated from the constitutive or inner usefulness [or suitability], but is nevertheless of subordinate significance as compared to the latter. This is shown by the low resistence of the relevant phenomena to a critical [non-darwinian] examination. There is hardly a single case, for which one could not say with Goebel (121): "So it is [constituted], but it could also be different.""

For the reader who is able to read German, we repeat the paragraphs just quoted from the famous botanist Wilhelm Troll also in the original language:

„Die Erklärung der Homologien bloß aus der Gemeinsamkeit der Abstammung ist also nicht mehr haltbar. Ebensowenig aber das sogenannte „Gesetz von den Daseinsbedingungen“ (law of Conditions of Existence), das DARWIN sogar über das „Gesetz von der Einheit des Typus“ (law of the Unity of Type“) gestellt wissen wollte.

[Zitat Darwin]: „Der Ausdruck ‚Daseinsbedingungen' wird durch das Prinzip der natürlichen Zuchtwahl voll umfaßt. Denn die natürliche Zuchtwahl wirkt entweder dadurch, daß sie die veränderlichen Teile jedes Wesens seinen organischen und anorganischen Lebensbedingungen jetzt anpaßt oder während früherer Zeiten angepaßt hat, wobei die Anpassungen in vielen Fällen durch den zunehmenden Gebrauch oder Nichtgebrauch einzelner Teile unterstützt, durch die unmittelbare Einwirkung der äußeren Lebensbedingungen beeinflußt werden und in allen Fällen den verschiedenen Gesetzen des Wachstums und der Abänderung unterworfen sind. Daher ist in der Tat das Gesetz von den Daseinsbedingungen das höhere Gesetz, da es vermittelst der Vererbung früherer Veränderungen und Anpassungen das der Einheit des Typus einschließt" (116).

Ergo: DARWIN eliminiert die aller Äußerlichkeit entzogene ideenhafte Natur des Typus. Nach ihm ist das Phänomen der „Einheit des Typus", über die Gemeinsamkeit der Abstammung hinaus, eine Anpassungserscheinung der Organismen an die Umwelt und somit durchaus als Wirkung der Umwelt zu verstehen, was D. H. SCOTT (117) noch prägnanter ausspricht, wenn er geradewegs sagt: „All the characters which the morphologist has to compare are, or have been, adaptive.“ **Der Darwinismus erklärt sich damit selbst als teleologisches System**, wobei es schon gleichgültig ist, ob die Probleme der organischen Gestalt nach Endursachen, d. h. die Zweckmäßigkeit der Organe gleichsam vorkonstruierenden Ursachen, oder nach einem Mechanismus beurteilt werden, der zweckmäßige

> Strukturen schafft. Jedenfalls nimmt es sich geradezu grotesk aus, wenn DARWIN im 14. Kapitel seines Hauptwerkes eine Betrachtung nach Endursachen, die für ihn identisch mit der Schöpfungstheorie ist (118), mit den Worten ablehnt: „Nothing can be more hopeless than to attempt to explain this similarity of pattern in members of the same class, by utility or by the doctrine of final causes", **wo doch sein ganzes System auf dem Nützlichkeitsgesichtspunkt aufgebaut und von NÄGELI (110) geradezu als „Nützlichkeitslehre" bezeichnet wurde.** Tatsächlich zog unter dem Einflüsse der Werke DARWINS die Teleologie erst recht in die Biologie ein (120), freilich eine Art der teleologischen Naturauffassung, die vom klassischen Teleologiebegriff ebenso weit entfernt ist wie der Darwinismus von der „Natura", der „Physis", die im Schaffen lebt.
>
> Wie schon früher betont wurde, kennt die Selektionstheorie nur die äußere oder ökologische Zweckmäßigkeit, die sich zwar von der konstitutiven oder inneren nicht streng scheiden läßt, ihr gegenüber aber dennoch von untergeordneter Bedeutung ist. Das zeigt namentlich die geringe Widerstandskraft der einschlägigen Erscheinungen gegen die kritische Prüfung. Gibt es doch kaum einen derartigen Fall, bei welchem man nicht mit GOEBEL (121) sagen könnte: „Es geht so, aber es ginge auch anders.““

(4a) (From page 33, *Giraffa jumae*): Churcher 1978, pp. 518/519: „*Giraffa jumae* is generally **more massive** than the largest recorded speciemens of *G. camelopardalis*…” Further, Churcher mentions the following points:

> "Harris (1976b) has described a tibia, metatarsal, and astragalus of G. *jumae* from East Turkana and listed measurements taken for these bones and those of a scapula, metacarpals, and femora of the Rawe type specimen. The lengths of these bones, when available, **fall within or above the range of G. *camelopardalis***, while the dimensions of the proximal and distal epiphyses appear to be proportionately smaller. Some minor differences are noted between the articular surfaces in the fossil and modern giraffe bones.
>
> Undescribed limb bones referable to *G. jumae* were recovered at Kanapoi, Kenya in 1966 by B. Patterson. These include portions of the major elements of a left forelimb, an almost complete right tibia, and the proximal third of a left radioulna. Where dimensional comparisons can be made, **these limb bones are as large, if not larger,** than those of male *G. camelopardalis* and the tibial morphology compares well with Harris's description of the *G. jumae* tibia from East Turkana (M. L. Richardson, pers. comm.). Along with the material assigned to *G. cf jumae* from Langebaanweg, and undescribed G. cf *jumae* from late Miocene sediments in the Baringo Basin, Kenya (Pickford 1975), the Kanapoi post-cranial specimens confirm the very early occurrence of undoubted *Giraffa* in Africa."

(4b) (From page 34): By phenomena such as dwarfism (which occurs not only in humans but also in numerous animal groups), it is clear that absolute size can **secondarily** lead to "transitional forms". Dwarfism or nanisms, however, does not change the overall design or body plan of an animal species. Also, the potentials and limits of modifications belong to the research topics, which have to be especially investigated further. Dagg and Foster point out (p. 72) that giraffes in **captivity seldom grow to more than 5 m** height („…probably because of the artificial diets and unusual climates“ – cf. also the study of Franz-Odendaal 2004). But no rational zoologist would consider these smaller giraffes (especially the cows, not to mention the juvenile animals) as *intermediate forms* in an evolutionary sense. See further the discussion on page 2 of the current work.

(5) (From page 37, quote from Colbert): The original quotation in its context reads as follows (Colbert 1938, p. 48):

„Several authors have divided the family Giraffidae into subfamilies, the more recent attempts along this line having been made by Bohlin (1927), Arambourg and Piveteau (1929), Matthew (1929), and Colbert (1935). The different taxonomic schemes of these authors may be compared as follows

BOHLIN	ARAMBOURG AND PIVETEAU	MATTHEW, COLBERT
Giraffidae	Giraffidae	Giraffidae
Palaeotraginae	Palaeotraginae	Palaeotraginae
Giraffinae	Giraffinae	Giraffinae
Okapiinae	Okapinae *[sic]*	Sivatheriinae
Sivatheriinae	Helladotheriinae	
(Progiraffinae)		

Whatever plan is used for the division of the family Giraffidae, the following characteristic types are recognizable.

1. The generally primitive, **medium-sized giraffes**, characterized by limbs and neck of **approximately normal length**, and in most cases by a single pair of supraorbital, frontal, spike-like horns. These are the palaeotragines, and include such genera as *Palaeotragus, Samotherium, Giraffokeryx* and possibly *Okapia*. Bohlin separates this last genus on the basis of certain characters in the skull and dentition, placing it in a subfamily by itself.

2. The large giraffes with greatly elongated legs and neck, a highly specialized skull, and horns that are simple, truncated spikes variously located on the skull roof. Usually there is a dominant pair over the fronto-parietal suture. Most characteristic of this group is, of course, the modern *Giraffa;* other genera referred to it are *Orasius* [=*Bohlinia* according to McKenna und Bell 1997/2000, p. 433; see the details below] and *Honanotherium* ["L. Mioc. and/or Plioc.; As." – McKenna and Bell;]."

3. The gigantic, ox-like giraffes, with short legs and neck, and with heavy broad skull surmounted by highly developed horns. Usually there are two pairs of these horns, on the frontals and on the parietals. In this group are such genera as *Sivatherium, Bramatherium, Hydaspitherium* and *Helladotherium*."

In addition, it should be noted that *Honanotherium* was also a long-necked giraffe and not an „intermediate form", as is sometimes incorrectly claimed and correspondingly depicted graphically. The main points about *Honanotherium* are summarized by Hamilton as follows (1978, p. 212):

> "Colbert (1935a,b), Matthew (1929, p. 546) and Bohlin (1926) grouped *Orasius* and *Honanotherium* as giraffines. This was followed by Simpson (1945) except that following Matthew's (1929, p. 546) suggestion he used the name *Bohlinia* instead of '*Orasius*'. Crusafont-Pairó (1952, p. 188) groups *Giraffa, Honanotherium* and his new genus *Decennatherium* in the Giraffinae but places *Bohlinia* with *Okapia* in the Okapiinae.
>
> Schlosser (1903, p. 103) states that **skeletal elements *of Honanotherium schlosseri* agree closely with *Giraffa camelopardalis*.** Bohlin (1926, p. 102, fig. 148; pl. 10, figs l, 2) shows that the ossicones *of Honanotherium* were supraorbitally positioned and therefore the genus is plesiomorphic when compared with G. *camelopardalis.* However Bohlin. (1926, p. 102, fig. 148) indicates that the ossicones were relatiyely massive which suggests relation with either the sivatheres or giraffines. Relation with the giraffines is more likely because **the post-cranial skeletons of *Honanotherium* and *Giraffa* are very similar**. Bohlin (1926, p. 102) mentions the development of sinuses in the frontal and parietal regions.
>
> ***Honanotherium sivalenase* (syn. *Camelopardalis sivalensis* Falconer and Cautley 1843) is a large long-limbed giraffid** (Lydekker 1883; Pilgrim 1911) but its skull is not known and detailed relations cannot be established. Matthew (1929, p. 549) disagrees with Bohlin's transfer of this species to *Honanotherium* and suggests closer affinities with *Bohlinia* or *Giraffa.* In this situation, the species is best retained as '*Giraffinae indet.* under its usually accepted name of *G. sivalensis.* Reasons for using the generic name *Bohlinia* as a synonym *of Orasius* are discussed by Matthew (1929, p. 546). A synonym list for *Bohlinia attica* is given by Bohlin (1926, p. 123), who describes an almost complete skull (Bohlin 1926, p. 123, fig. 195) from Pikermi. Bohlin (1926, p. 125) suggests that the ossicones of this species are shifted posteriorly and towards the mid-line of the skull. Post-cranial

> material of *B. attica* is figured by Gaudry (1862-7) and the synonymy between Gaudry's species *Camelopardalis attica* and *B. attica* is indicated by Bohlin (1926, p. 123). **This species has limb bones that are as long and slender as those *of Giraffa.*** This coupled with features of the skull suggests close relation between this species and *Giraffa. Bohlinia* is more advanced than *Honanotherium* in features of the ossicones and is therefore identified as the sister-genus of *Giraffa.*"

(5a) (From page 37) Simmons and Scheepers, p. 772 und 777:

"Modern Giraffes radiated on Africcan savannas about 1 million (M) yr ago, from a large, morphologically similar species, *Giraffa jumae*, which had existed unchanged **for at least 12 M yr** (Churcher 1976; Harris 1976)" p. 772. "Fossil evidence suggests that a large species (*Giraffa jumae*), differing from modern giraffe only in its more flattened ossicones (Churcher 1976; Harris 1976), arose from this stock **at least 12 M yr ago**."

(5b) (From page 39): In 1959 Arambourg strongly exaggerated the similarities between *Palaeotragus germaini* and *Giraffa*. Churcher 1979, pp. 6/7 comments: „Arambourg (1959) described *P. germaini* as a large giraffid with elongate neck and legs, and with a forelimb slightly longer than the hind. … He considered that *P. germaini* exhibited **a parallel evolution separate from *Giraffa* or *Samotherium***, and its lineage would thus be **separate from those of the Giraffinae or Sivatheriinae** and would represent the more progressive and larger Palaeotraginae (Churcher 1978, Fig. 9). Yet, Churcher then takes into consideration (p.7): "However, the characters of the molar teeth also place the taxon within the genus *Palaeotragus* rather than any other genus of the Giraffinae." The correct description was apparently first given by Harris 1987 ("Harris (1987b) noted that the skeleton *of P. germaini* had the same dimensions as that of *S. africanum* and differed only in that *S. africanum* had larger ossicones" – see the quote from Mitchell and Skinner above). As an urgently needed argument for a transitional form, however, the **obsolete old interpretation** is again offered („***P. germaini***…was of large size and **resembled *Giraffa* in its elongate neck and limbs**"). Haeckels „biogenetic principle" is presently being used in a similar fashion (cf. Rammerstorfer 2005, Luskin 2007).

Supplement: In the original work Arambourg tries to stress both the similarities and the differences between the Palaeotraginae and the Giraffinae as follows (1959, p. 113):

> „Les *Palaeotraginae* diffèrent des *Giraffinae* essentiellement par leur structure cranienne, leurs longs ossicones surorbitaires, ainsi que **la moindre élongation de leurs membres et de leur cou**, et surtout par une disproportion moins grande entre leurs membres antérieur et posteriéur, ce dernier étant toujours plus court chez *Giraffa* que le membre antérieur, tandis que, chez les *Palaeotraginae* — de meme que chez *Okapi* — la disposition est inverse. Enfin, chez *Giraffa*, l'humérus, ainsi que le fémur, sont relativement très courts, et le radius sensiblement plus long que le tibia. Il en est de même, mais à un degré moins accentué, chez *Okapi* et chez les *Palaeotraginae* (cfr. FRAIPONT, 1907, p. 89; BOHLIN, 1926, tableau p. 97; voir aussi tableau ci-apres).
>
> Cette structure des membres, **jointe a l'elongation considerable du cou**, sont, a mon avis, avec celles du crane, **les caracteristiques essentielles du genre *Giraffa***."

However, Arambourg additionally postulates a large but still unknown *Palaeotragus* species, when he writes in a footnote on the same page:

> „Je persiste donc a penser que les dents *d'Orasius* sont celles d'un **grand *Palaeotragus*** — dont les membres sont encore inconnus — et que seul, le crane décrit par BOHLIN doit appartenir a *Giraffa (Bohlinia) attica;* les dents de cette dernière espèce seraient celles, provenant de Pikermi, que j'ai décrites et figurèes *(loc. cit.*, fig. 7), ainsi que celles décrites par WAGNER (1861) sous le nom de *G. vetusta.*"

For this idea „d'un grand *Palaeotragus* — dont les membres sont encore inconnus" there is, however, no confirming evidence known to me.

To stress this point again: If *Palaeotragus germaini* were larger than *Samotherium* and furthermore had a longer neck (cf. the figure of *Samotherium* in the first part of this work on page 17), then not only the evolutionary series (*Giraffokeryx* -> *Palaeotragus* -> *Samotherium* etc.), but also the identification of the two forms as claimed by several authors (see above), would stand in fundamental contradition to the fossil finds.

(5c) (Supplement to page 43): Gentry comments on this question in (1994, p. 135) as follows:

> „*Giraffokeryx* **and giraffids wrongly referred to *Palaeotragus*** in middle Miocene faunas have advanced over *Canthumeryx* in such features as higher crowned cheek teeth, upper molars with less of a basal pillar and lingual cingulum, labial wall of metacone more upright on upper molars, lower molars with less prominent metastylids in earlier wear and smaller basal pillars, frequent metaconid-paraconid fusion on P/4, deciduous P/3 wider posteriorly, and the front lobe of dP/4 more fully crescentic. *G. punjabiensis* is rather completely known from the Siwaliks prior to c.9.0Ma (Colbert 1935) and has an additional anterior pair of homs in front of the orbits. Its posterior or main pair of horns are longer than in *Canthumeryx*, but remain so much expanded at the base that their insertion extends behind the orbits. The P/4 transverse metaconid crest from the protoconid is weakening, but the entoconid mostly continues its old link with the labial side of the tooth (weakening at Pasalar). ***"Palaeotragus" primaevus* Churcher 1970 from the Fort Ternan middle Miocene, is close to *G. punjabiensis*,** but its upper molars seem to have more bulky styles and a less upright labial wall of the metacone than at Pasalar. The limbs are very long and narrow. A cast in London of a horn KNM 3119 (=FT1961.711) looks as if it would have been inserted very divergently and would have had lessening divergence towards the tips. This horn was part of the hypodigm of *Samotherium africanum* Churcher (1970:73) for which the holotype was another very similar horn from Fort Ternan. **It need not be regarded as a species additional to *"P." primaevus.*** *”*

(6) (From page 44): After Geraads (1986) and Janis (1986) had disputed the existence of *ossicones* for fossil giraffes in general, Solounias (1988) states in the following assessment of a special study (among other things, p. 845): “I agree with Geraads (1986) and Janis (1986) that the Climacoceridae and Triceromerycidae probably had „horns“ that were outgrowths of the frontals whatever their direction of growth might have been. I present evidence that Giraffidae such as Siivatheriinae, Palaeotraginae (which includes only *P. rouenii* (=*microdon*) and *P. coelophrys* (=*quadricornis*)), and Samotheriinae possessed true ossicones).” A series of transitional forms that would connect the two forms is as yet unknown.

(7) (From page 47): This statement could *not* only refer to the size differences between (most) antelopes and the short-necked giraffes as well as between the short-necked and long-necked giraffes because these large differences *still exist*, as is well-known. The real question is about the evidence for continuous (gradual) evolution. (See also the large differences in the Jeep-Family on page 10 of the present work.)

(8) (From page 48): One of many examples of the essential unfalsifiability of evolutionary doctrine is provided for us by Daniel Dennett in the context of the question "why do giraffes have long necks?" (1995, pp. 102/103):

> “There is one answer that could in principle be “read off” the total Tree of Life, if we had it to look at: Each giraffe has a neck of the length it has because its parents had necks of the lengths they had, and so forth back through the generations. If you check them off one by one, you will see that the long neck of each living giraffe has been traced back through long-necked ancestors all the way back... to ancestors who didn't even have necks. So that's how come giraffes have long necks. End of explanation. (And if that doesn't satisfy you, note that you will be even less satisfied if the answer throws in all the details about the individual developmental and nutritional history of each giraffe in the lineage.)”

This discussion on the question of the origin of the long-necked giraffe could almost be used as a textbook example for a *petitio principii* („A thesis is offered as proof for a thesis that is, to be sure, not obviously false, but which needs a proof itself“ - http://www.phillex.de/petitio.htm). Dennett simply presupposes as fact the „total Tree of Life“ in terms of a gradual evolution by mutation and selection. He does not consider falsification criteria for his evolutionary worldview. However, the entire chain of evidence for his view is lacking – from the origin of life, to the Cambrian explosion, to the question of the origin of complex genetic information, and also the origin of synorganized structures and *irreducible complexity* by random mutations and selection, etc. etc..

Incidentally, Dennet’s answer can also be included in the category of *science stoppers*: if further scientific questions and research on the origin of the long-necked giraffe will only lead you to „be even less satisfied if the answer throws in all the details about the individual developmental and nutritional history of each giraffe in the lineage“, then the best we can do is probably to abolish such investigations. For who wants to become „less satisfied“ by scientific research? Nevertheless, Dennett, contrary to his intentions, as well as such persons as Kutschera, Dawkins, Hunt, he himself and many others, probably become „less satisfied“ with their basic convictions if they carefully studied papers as for example the present one with its many details on the evolutionary problems on the origins of the long-necked giraffe (and that is just a beginning). For some neo-Darwinian authors their frustration can even so strong that they turn to intolerance. Behe comments on this point (1996/2006, pp. 250/251, a quotation which I have also referred to in another paper):

> “Intolerance does not arise when I think that I have found the truth. Rather it comes about only when I think that, because I have found it, everyone else should agree with me. **Richard Dawkins** has written that anyone who denies evolution is either "ignorant, stupid or insane (or wicked - but I'd rather not consider that.)" **It isn't a big step from calling someone wicked to taking forceful measures to put an end to their wickedness. John Maddox**, the editor of *Nature*, has written in his journal that "it may not be long before the practice of religion must be regarded as anti-science." In his recent book *Darwin's Dangerous Idea*, philosopher **Daniel Dennett** compares religious believers - 90 percent of the population [of the USA] - to wild animals who may have to be caged, and he says that parents should be prevented (presumably by coercion) from misinforming their children about the truth of evolution, which is so evident to him. **This is *not* a recipe for domestic tranquility**. *It is one thing to try to persuade someone by polemics; it is entirely different to propose to coerce those who disagree with you.* As the weight of scientific evidence shifts dramatical1y, this point should be kept prominently in mind. Richard Dawkins has said that Darwin made it possible to be an "intellectually fulfilled atheist." The failure of Darwin's theory on the molecular scale may cause him to feel **less fulfilled**, but no one should try to stop him from continuing his search” [note in square brackets and emphasis in the text are mine.]

(9) (From page 49): The brutality employed against giraffes not only by hunters but also some scientists (especially in the past) is beyond my understanding and is not justified by anything, including scientific research in pursuit of "material".

14. **Appendix** (22 and 27 October 2007)

A Note on the Paper by **Elissa Z. Cameron and Johan T. du Toit (2007): "Winning by a Neck: Tall Giraffes Avoid Competing with Shorter Browsers."** *The American Naturalist* ***169****: 130-135.*

The authors assert in their abstract (p. 130) that their findings provide "the first experimental support for the classic evolutionary hypothesis that vertical elongation of the giraffe body is an outcome of competition within the browsing ungulate guild."

Accordingly, the paper has been celebrated as the neo-Darwinian solution to the problems of the origin of the giraffe by natural selection in the popular press and elsewhere (for some examples see the links below) – as if all questions have now been answered in agreement with the dictum that "all of biology rests on the foundation of neo-Darwinism, drawing on the principles of population biology and molecular genetics" (G. T. Joyce in *Nature* **346**, p. 806, 1990). However, **the article does not address any of the key problems** discussed at length in our two papers (2006, 2007) on *The Evolution of Long-Necked Giraffe* (*Giraffa camelopardalis*) - *What do we really know?*

First to mention some details (not to criticize the authors Cameron and du Toit on the majority of the following points, but their readers and commentators who, in their enthusiasm for Darwin and natural selection, seem to have overlooked the fact that the writers did not speak about the following topics):

1. The paper by Cameron and du Toit does not address any of the problems presented by the fossil record (see Part 1 and several chapters and notes of Part 2 above, especially pp. 6-10, 23-24, 33-47, 50-51, 54, 58-61)

2. It does not address any of the problems that natural selection has to explain concerning the prominent sexual dimorphism of *Giraffa camelopardalis*, not to mention the special requirements of young animals (see summary and introduction above as well as pp. 20-22, 29-32, 62).

3. It does not address any of the anatomical or physiological questions and problems discussed in detail in our two papers. No word on the number of vertebrae (see pp. 13-18 above), no word on synorganization or coadaptation (Part 1, pp. 4, 8-10, 23-24, Part 2, pp. 18-20, 26, 52-54, 65, 66, 69, 70, 75, 77). No word on the points addressed on p. 26 of this paper (to repeat):

> (a) the duplication of a neck vertebra, as well as the many related specific anatomical structures discussed above by Solounias… (b) the especially muscular esophagus (ruminator), (c) the various adaptations of the heart, (d) the muscular arteries, (e) the complicated system of valves, (f) the special structures of the *rete mirabile* (system of blood-storing arteries at the brain base), (g) the „coordinated system of blood pressure controls“ (for, among other things, the enormously high blood pressure), … (h) „The capillaries that reach the surface are extremely small, and (i) the red blood cells are about one-third the size of their human counterparts, making capillary passage possible“; (j) the precisely coordinated lengths, strengths and functionality of the skeletal, muscular and nervous systems; (k) the efficient „large lungs“ (l) „the thick skin, which is tightly stretched over the body and which functions like the anti-gravity suit worn by pilots of fast aircraft“.

4. Moreover, the paper by Cameron and du Toit does not address any of the genetic questions, i.e. random 'macromutations' vs. an almost infinite number of accidental 'micromutations' (pp. 18-20, 25-27).

5. Apart from the missing question of sexual dimorphism, neither does the article address the essential problem of the theory of natural selection for the origin of the giraffes in general: i.e. the behaviour of the giraffe and "the survival of the fittest" ***under extreme food shortages***, especially with regard to the young animals again (remember Mitchell and Skinner quoted p. 29 of the present paper):

> “While dependence on leguminous browse seems essential, the idea that tallness enables **exploitation of**

> **food sources** that are beyond the reach of competitors such as bovids, **is unlikely to be true**. Pincher (1949) made one of the first objections to this hypothesis. He indicated that a **Darwinian dearth** severe, long-lasting enough, and/or frequent enough for natural selection to operate to produce a long neck, would cause the recurrent wastage of young giraffes, and **would thus lead to extinction of the species rather than its evolution**."

So, then, which problems do Cameron and du Toit actually address? We read on p. 130:

> "The problem was that no study had been designed to explicitly test whether giraffes achieve a foraging advantage by foraging above the reach of smaller browsers."

(That is, under normal conditions including dry seasons, yet not extreme "Darwinian dearths") – What did they do to solve the problem posed for such normal conditions?

> "We erected exclosures around individual *Acacia nigrescens* trees in the greater Kruger ecosystem, South Africa. After a complete growing season, we found no differences in leaf biomass per shoot across height zones in excluded trees but significant differences in control trees."

Their inference:

> "We conclude that giraffes preferentially browse at high levels in the canopy to avoid competition with smaller browsers."

Joe Bowman, staff writer of the *Deseret Morning News* (Salt Lake City), wrote a favourable comment on the work of Cameron and du Toit and published the following photograph (by du Toit) for illustration with the accompanying text as quoted below:

"Giraffes feeding efficiency is reduced at low heights" because of competition with smaller animals such as the kudu, a study finds.
(Johan du Toit)"
http://deseretnews.com/dn/view/1,1249,650224911,00.html

For the argument's sake let's first assume that the procedures, experiments and inferences of the paper by Cameron and du Toit are correct. Would this *prove* that the long-necked giraffe originated by selection of random mutations in a series of severe, long-lasting (and frequent enough) Darwinian dearths? Would it explain the prominent sexual dimorphism and (an almost preferential) survival of the young ones? Would it throw light on the question how all the complex anatomical and physiological synorganizations (as repeated under point 3.) just happened accidentally at the time when needed? Would it decide the question, whether an almost infinite number of naturally selected random ‘micromutations’ were the genetic cause or just one or a few accidental ‘macromutations’ (not to mention ID in this context)? The intelligent reader will give the correct answers.

Young giraffe getting forage at corresponding height in Cologne Zoo.
Picture by W.-E.L. (9 June 2007, 15.00; young giraffe born 8 March 2007)

Now, as to the dry seasons Simmons and Scheepers had noted (see p. 6 above):

> “…we find that during the dry season (when feeding competition should be most intense) giraffes **generally feed from low shrubs**, not tall trees; females spend over 50% of their time feeding with their necks horizontal; both sexes feed faster and most often with their necks bent; and other sympatric browsers show little foraging height partitioning. Each result suggests that long necks did not evolve specifically for feeding at higher levels.”

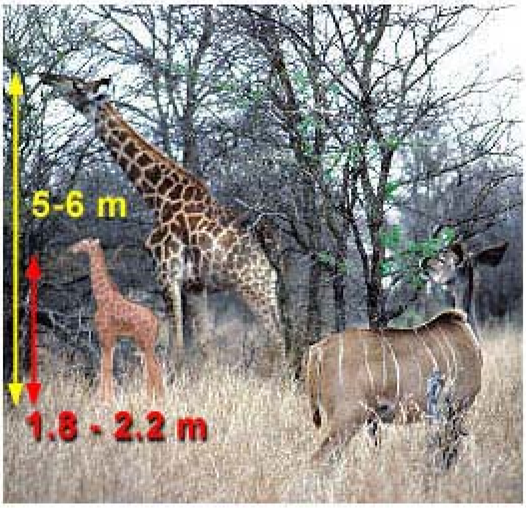
Concerning the Giraffe’s behaviour during those dry seasons, see also the long quotation on p. 49 of the present paper, where Simmons and Scheepers mention the following observations (to repeat in abbreviated form):

(1) In the Serengeti “giraffes spend almost all of the dry-season feeding from low Grewia bushes“ (“…contrary to the prediction that giraffe should use their feeding height advantage at times of food scarcity.”) (2) Concerning *all* browsing, about 50% is below 2 m in the Tsavo National Park “within reach of potential competitors such as gerenuk *Litocranius walleri* and lesser kudu *Tragelaphus imberbis* (Leuthold and Leuthold 1972).” And “during the dry season, 37% of the browse taken by giraffe was below 2 m.” (3) “Giraffe were not avoiding interspecific competition by selecting different food plants (the third prediction): considerable (unquantified) overlap was apparent between giraffe and sympatric browsers in Tsavo (Leuthold and Leuthold 1972).”

Interestingly, according to Simmons and Scheepers “it was only in South Africa were giraffe found to allocate 90% of their time to feeding above the average feeding height of browsers such as kudu *Traxelphus strepsiceros* (1,0 m) and impala *Aepyceros melampus* (ca. 0.3 m; du Toit 1990), but lower than their long necks allow (5-6 m).” – ***So Cameron’s and du Toit’s observations appear to be the exception from the rule mentioned by Simmons and Scheepers.*** But even in this case the following points have to be considered:

> “In each study both sexes frequently fed at or below shoulder height (ca. 3.1 m and 2.8 m for adult males and females; L. Scheepers, unpublished data). For example, female giraffe spent over 50% of the time feeding with their necks at or below shoulder height in both South Africa (du Toit 1990) and Kenya (Young and Isbell 1991), contrary to the second prediction.”… “…only dominant bulls regularly fed at 5.0 m or more in both South and Eastern Africa” (see p. 49 of the present paper).

Coming back to the figure of the Giraffe and the Kudu presented by Bowman/du Toit above, it could be interpreted to be a “dominant bull” feeding at 5.0 m or more. Now add to the picture a female (feeding with its neck at or below shoulder height, like on the left, photo by W.-E.L.) and being anyway 1 to 1.5 m shorter than the bull and projecting also a young one of about 2 m height into the figure (right), you’ll get about the following result:



Moreover, the hypothesis of “depletion of higher-quality plant parts by smaller browsers” (Cameron/du Toit, p. 131) appears to be doubtful already from a look at the original figure shown on p. 64 above: Are we really to assume that the entire range of higher-quality plant parts below the giraffe’s stretched-out neck and head (of the photograph on the right) has already been depleted by smaller browsers? But if so, why and how do the young ones and female giraffes keep on living? – Yet, if I interpret the photograph correctly, there is enough to browse for the smaller browsers as well as the larger ones and there is hardly any depletion of the lower plant layers, which would drive “giraffes to forage higher in the canopy, thereby supporting the competition hypothesis, paralleling results from the grazing guilt” (also p. 131). And looking at the following photographs (left and middle from South Africa, right from Namibia; see links to sources below) the depletion and competition hypothesis may appear even more unconvincing:

See references for the photographs at the end of the appendix under (1), (2), and (3).

See references for the photographs at the end of the appendix under (4), and (5).

Photographs of Kudus (above) and Giraffes (below) on this page were also taken from South Africa by different photographers (see again sources at the end of the appendix). Of course, one should check whether the plant species shown belong to diet of ca. 70 plant species of giraffes (in South Africa), Kudus and other browsers. Nevertheless, as long as there is a sufficient food supply even under dry conditions, one may doubt the depletion and competition hypothesis. Yet under a series of severe Darwinian dearths the young giraffes would be heavily affected, too.

It may also be worthwhile to remember in this connection the long distance movements of giraffes (for the details see pp. 4 und 5 above).

See references for the photographs at the end of the appendix under (6), (7), and (8).

As to the photographs of the giraffes above one may note that concerning the left one the words of Dagg and Foster for the young animals may apply: "…they supplement the milk with solids at about one month. Perhaps they need relatively little milk because of the high nutritional value of the acacia tips they eat" – see full quotation on p. 3 above). Anyway, the young animals would starve to death if they had to avoid "competition" with Kudus able to browse up to a height of 2.5 m. The photographs in the middle and on the right show examples where the giraffes are larger than the plants surrounding them in KNP (remember that in the Serengeti "giraffes spend almost all of the dry-season feeding from low Grewia bushes").

Let's return to the experiment of Cameron and du Toit. They explain their method as follows (2007, p. 131):

> "Fences excluding smaller browsers were built and maintained for a growing season at a savanna site in South Africa with an intact guild of indigenous browsing ungulates."…
>
> "We constructed fences around individual *A. nigrescens* trees in November 2001. Nine exclosure plots were created by selecting trees that were taller than 4 m and had branches throughout their height range. We constructed fences 2.2 m in height, l m from the outside canopy branches. Therefore, we excluded all small browsers and partially excluded larger browsers, except giraffes, who could freely forage at heights above 2.2 m. … Each excluded tree was paired with the nearest unfenced (control) tree within 10 m that met the same selection criteria (at least 4 m tall with branches at all potential foraging heights)."

So the authors ***excluded not only the smaller browsers from the trees but also the giraffes, the young ones as well as the adult female and male animals, from browsing below 2.2 m***.

For Kruger National Park (KNP), South Africa, du Toit and co-workers report (2006, p. 249) that “Giraffe browsing range was observed to be PH2 and PH3 (*c.* **1.7-5.1 m**)” for *Acacia nigrescens*. And, as can be concluded from Figure 1 of du Toit (1990, p. 58), even in KNP giraffe allocated ***more than 10%*** of feeding time ‘at the height ranges of kudu, impala, and steenbok’, that is ***below 1.7 m*** (or from the ground up to 1.7 m). Moreover, du Toit himself notes (p. 59) that “giraffe are also quite capable of feeding at lower levels though, so even this separation [between giraffe and the other browsers] is not always complete. For example, in Tsavo East National Park, Kenya, giraffe have been found to allocate about 50% of feeding time to browsing below a height of 2 m (Leitholt & Leuthold, 1972)”. Similar observations have been made by Ginnett and Demment (1997, 1999) in Mikumi National Park, Tanzania.

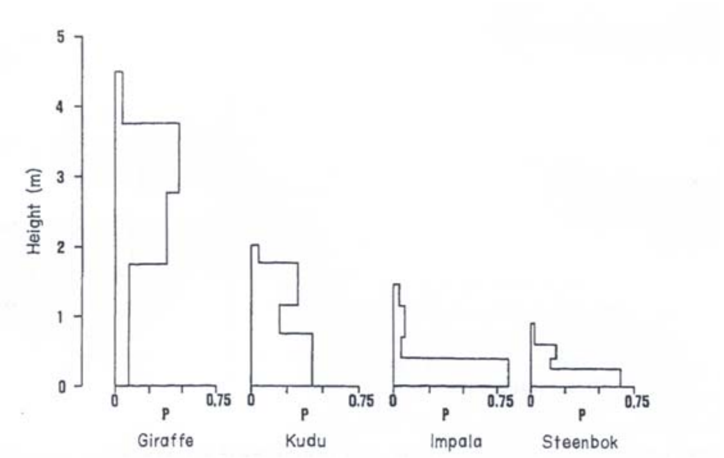


Figure 1 from du Toit for the central region of KNP (1990, p. 58): ‘Proportions (P) of feeding time allocated to height classes, which correspond to the four neck angle classes [45°, 90°, 135° and 180° respectively] for each browser species, calculated over the complete seasonal cycle.’

Also, the African savanna biome comprises more then 46 ungulate species. Though a majority of them are grazers and some are both, grazers and browsers, there are many more browsers than the 4 species mentioned above (by the way, the giraffe is grazing a bit, too, for example on the *Tribulus zeyheri*, an annual forb [belonging to the low-growing Zygophyllaceae], “which constitutes a moderately important forage source for giraffe during the wet season (**9% of its diet**)” in northwestern Namibia; Fennessy 2004, p. 207). For a photograph of the plant, see for example http://www.biologis.de/photo/botanik/fenster/art/bild_th/tribulus_zeyheri0040tt.html .

Thus, the findings of Cameron and du Toit from KNP can neither be generalized for all of parts of Africa where giraffes occur today nor can they be correct for their area of investigation as long as giraffes are fully excluded from browsing below 2.2 m, since usually giraffes may eat a not inconsiderable amount of plant material even below 1.7 m in more than 10% of their feeding time.

A few lines downstream of *Winning by a Neck* (2007, p. 131) Cameron and du Toit define the giraffe browse unit as follows:

> "For sampling forage availability, we used a previously defined giraffe browse unit (GBU). The GBU is equivalent to the average twig pruned or leaf stripped by a giraffe in a single bite, which for *A. nigrescens* is 144 mm long (Woolnough and du Toit 2001). The GBU thus incorporates the smallest bites of steenboks, impalas, and kudus and provides a measure of biomass return per bite, reflecting both foliage depletion and foraging efficiency."

And before this definition the authors write:

> "Experimental and control trees were sampled before fencing and again in the early dry season (July 2003) after two complete growing seasons. We defined three levels for sampling available browse: l m (available to steenboks, impalas, kudus, and giraffes), 2.5 m (available for kudus and giraffes), and 4 m (available only to giraffes). At each height level, we recorded the presence of recent browsing of shoot ends on a presence/absence basis for 10 randomly selected shoots around the canopy. This provided a proportional index of browsing intensity up and down the canopy (du Toit et al. 1990)."

I have to admit that I am not yet fully persuaded to accept the "10 randomly selected shoots around the canopy", for this "random selection" method is not convincingly explained.

> "We calculated the difference in leaf biomass from prefencing to postfencing two growing seasons later. Two experimental trees were excluded from the final analysis because of elephant damage."

So, apart from the difficulties just mentioned, eventually the authors had seven trees ("enclosure plots") for further evaluation – not too strong a basis for sweeping inferences on the origin of species with so many varying random factors all around (which control tree and enclosure plot is visited by how many individuals of which animal species of which gender how many times? – One can hardly assume that all seven cases were simply equal).

Now let's have a closer look at their results (p. 132):

> "Before fencing, our results confirm the findings of previous research. There was a significant difference in forage availability at the different heights, with less browse per GBU low in the tree and more at heights available only to giraffes (ANOVA, $F = 9.20$, df = 2,51, $P < .0005$)."

The difference in biomass per GBU was about 1 g (dry mass) less at 1 m in the controls and an inverse ½ g at 2.5 m according to their Figure 1 (however, the authors assert that the unexpected difference of <u>less</u> biomass in the experimental trees in the exclosures at 2.5 m as compared to the controls was statistically insignificant).

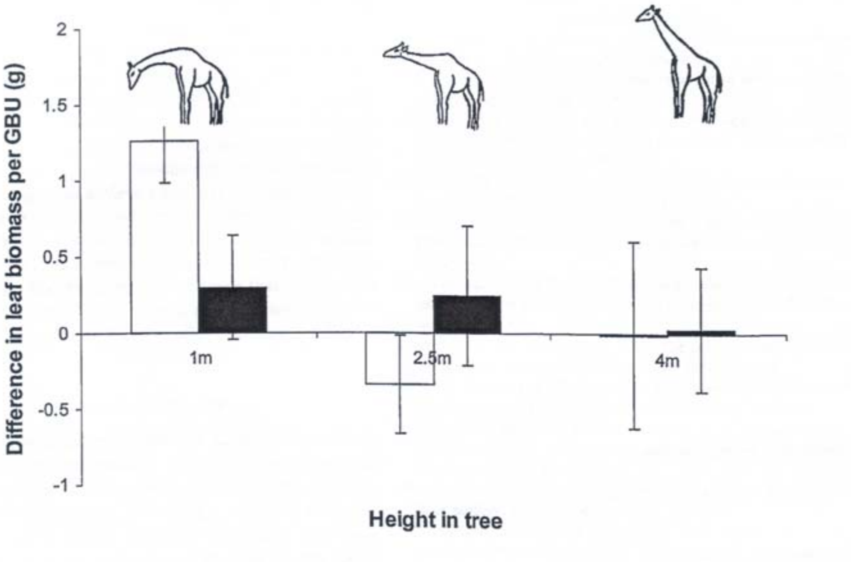


Figure 1 of Cameron and du Toit (2007): "Difference in leaf biomass per giraffe browse unit *(GBU)* between prefencing and two growing seasons after the erection of exclosures around experimental trees. Open bars are excluded trees; filled bars are control trees. Schematic giraffes indicate the posture of an adult female when browsing at each height."

Nevertheless, I have to admit that I have some problems to match their results for 2.5 m as shown in Figure 1 with those of the same height in Figure 2 A:

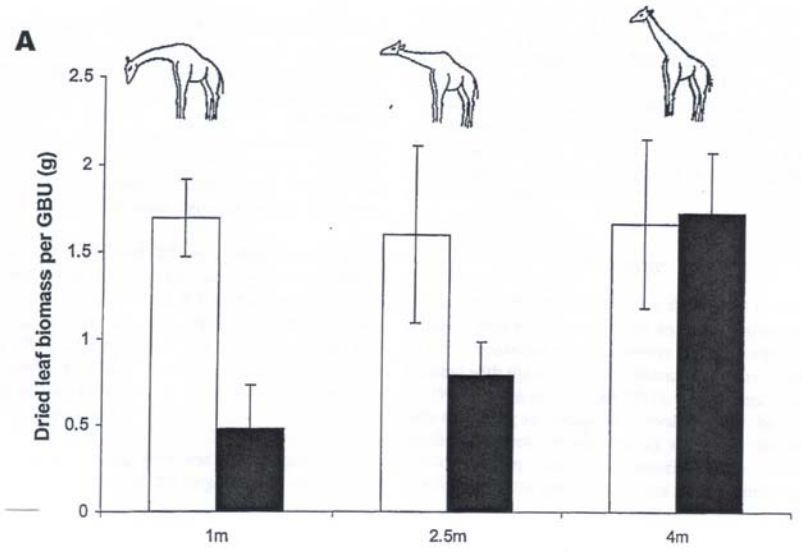


Figure 2A of Cameron and du Toit: Leaf biomass per giraffe browsing unit (GBU). Open bars are again excluded trees; filled bars the control trees and – as in Figure 1 – the "schematic giraffes indicate the posture of an adult female when browsing at each height".

Possibly this is simply an artifact of the non-significant statistical results.

The differences per GBU may perhaps be relevant at the brink of starvation (with the unfortunate young ones dying first). But is it really a question of life and death and the survival of the fittest in a normal situation with still enough forage all around? Also, it has to be considered that GBU varies strongly depending on the plant species browsed and that there are differences of bite size between the sexes ("because IWP [average within-patch dry-matter intake rate] is positively related to bite mass, males could increase IWP and thereby shorten feeding times by taking larger bites than females" and there was "significant variation across forage species" – Ginnett and Demment 1997, pp. 297/298 and 295).

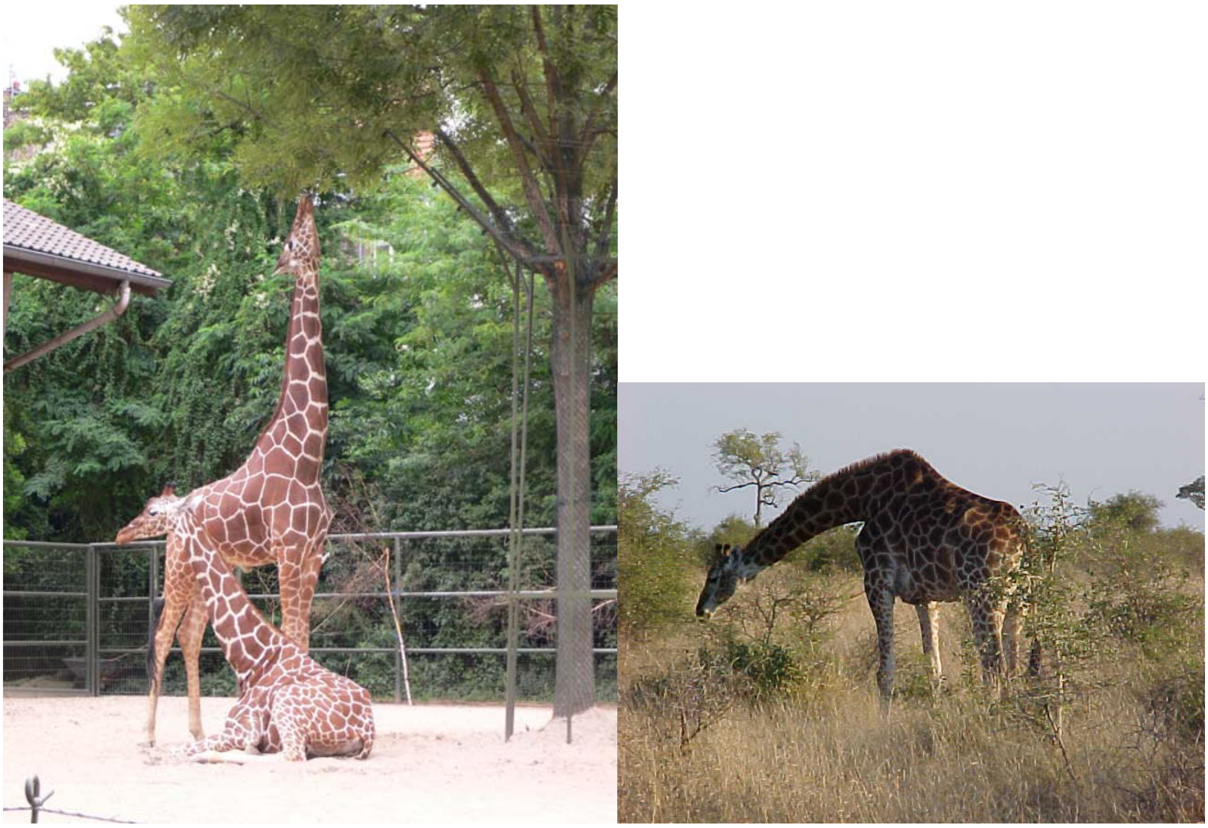
Yet probably the more important question continues to be: ***to what extent do the giraffes themselves contribute to the depletion*** found by Cameron and du Toit? The figures of the authors reproduced above clearly show that female giraffes do browse at a height of 1 m.

On p. 131 the authors had stated:

> "Giraffes gain a nutritional advantage by foraging above the height of the other species, as they receive more biomass per bite higher in the canopy (Woolnough and du Toit 2001)."

Prima facie this seems to be obvious. But again: granted that animals are the main cause for the depletion, the author's experiments cannot solve the question as to what extent exactly the giraffes themselves (especially the juvenile and the female ones) are responsible for the reduced biomass *at* the height of other species. *In fact, the experiment has even excluded the solution of this question.*

Yes, as the authors stated, giraffes "could freely forage at heights above 2.2 m", *but hardly below* – and this seems to be the decisive weakness of their method concerning feeding competition. *All* animals are excluded: steenboks, impalas, kudus and others, **and giraffes**. In order to forage below 2.2 m, the giraffes would have had to bend their necks down over the fences for food whilst the same high-quality food was just before their mouths and all around their heads without any bending over the fences at all. And, as expected, the animals did not display such a curious behaviour.



The tree on the right side of the left photograph is fenced around up to a height of ca. 5 m because giraffes also eat ‘everything’ below (photo by W.-E.L. 9 June 2007 at Cologne Zoo). Giraffe on the right from Kruger National Park according to http://www.satowns.co.za/Photo%20Library/kruger/Giraffe%205.jpg. On 3 October 2007 I asked Prof. Cameron for some photos or links to photos of their enclosures in KNP, South Africa. So far she did not answer.

Cameron and du Toit continue on p. 131:

> “This suggests that the depletion of higher-quality plant parts by smaller browsers drives giraffes to forage higher in the canopy, thereby supporting the competition hypothesis, paralleling results from the grazing guild (Illius and Gordon 1987; Murray and Illius 2000).”

As long as the giraffes themselves are debarred, this suggests hardly anything. Moreover, one could as well argue that the depletion of higher-quality plant parts also *drives the smaller browsers to forage perpetually higher and higher in the canopy transforming them into giraffe-like animals in the long run*.

One of the basic problems with natural selection, however, is that – to illustrate – it only acts like a sieve which selects (screens) tea leaves from a certain size onwards but, of course, ***sieves never create the tea leaves*** themselves (for a detailed discussion on the limits of natural selection, see http://www.weloennig.de/NaturalSelection.html.). Hence, it is necessary to clearly distinguish between selection and the rich but limited genetic potential for phenotypic variations of any species (the range of 'tea leaves', so to speak, that it can offer for survival to the sieve of natural selection). So for the smaller browsers this definitely means that phenotypic variation is limited too. Moreover, whatever 'selection pressure' may exist, one may safely predict it will never transform them into 6 m tall animals at all. And naturally this was true for the past as well.

Let's return to the authors' results (p. 132). After stating the initial condition as follows:

> "There was no significant difference between control and treatment trees at any height. The patterns for recent foraging were more ambiguous, with no significant difference in foraging with tree height and no difference between control and treatment trees."

– Cameron and du Toit continue to report:

> "There was a significant difference from pre- to post-fencing between control and excluded trees at l m (paired *t*-test, $t = 2.62$, $P = .03$). At 2.5 m, the pattern looked similar (fig. 2) but was not significant (paired *t*-test, $t = 1.30$, $P = .24$), and there was no difference at 4 m (paired *t*-test, $t = 0.07$, P = .95; fig. 1). In addition, there was a significant difference in forage availability at l m between excluded and control trees ($t = 3.60$, $P < .005$) but not at 2.5 m ($t = 1.48$, $P = .16$) or 4 m ($t = 0.10$, $P = .92$). After fencing, there was still a significant difference in forage availability by height for the control (unfenced) trees, with less forage available at l and 2.5 m and significantly more at 4 m (ANOVA, F = 5.54, df = 2,22, P < .01; fig. 2). However, there was no significant difference in forage availability for the excluded trees (ANOVA, $F = 0.01$, df = 2,22, P = .98)."

Thus, the result of their investigations was (not unexpected):

> "Our recently browsed shoot data confirmed that we had successfully excluded foragers at low foraging heights; no shoots were foraged on excluded trees at l m (fig. 2). Because fences were 2.2 m high, we reduced but did not eliminate foraging at 2.5 m."

So what does this prove concerning the evolution of the giraffe in the authors' view? First, they maintain:

> "Our study confirms that there are differences in browsing intensity with foraging height in an intact browsing guild…"

This seems to be almost self-evident and I tend to accept it. Yet apart from the many weak points already mentioned above, for this generalization the authors seem to have overlooked that they have investigated ***just 7 individuals (of thousands) of only 1 plant species out of about 100 different ones***, which are eaten by giraffes under different ecological and environmental conditions (see pp. 4 and 5 of the present paper and the further links below on South Africa). Moreover, in agreement with Ginnet and Demment quoted above, Woolnough and du Toit have shown in 2001 that the results can vary strongly for different plant species (p. 588):

> "For *A. nigrescens* we found that browsing intensity (% shoot ends freshly browsed) increased significantly across browsing heights as a function of leaf dry mass/GBU (P<0.005), although there was no significant relationship between browsing intensity and leaf biomass within any browsing height (Fig. 1). **No similar trends were found for *B. [Boscia] albitrunca*, however, probably because giraffes tend to leaf-strip the non-spinescent *B. albitrunca* shoots more frequently than they prune them.**"

And on p. 589 we read:

> "The generality of this pattern [browsing intensity increases with height] could vary depending on the composition of the browsing guild and the browse resources available. For example, **Ginnett and Demment (1997, 1999) found no significant variation in intake rate (g/min) across the 0- to 3-m feeding-height range for giraffes in Tanzania, but these were feeding mainly on trees that did not include *Acacia* species.**"

Moreover (p. 586):

> "From a pilot study of twigs recently browsed by giraffes, the mean GBU was **144 mm ... for *A. nigrescens* and 89.5 mm ... for *B. albitrunca*,** reflecting the different leaf and twig morphologies of the two species."

These observations corroborate the conclusion that one ***cannot*** simply generalize the results from *Acacia nigrescens* – important as it is – to all plant species eaten by the giraffes. Yet this is what the authors continually do.

Cameron and du Toit continue (2007, p. 132):

> "…and that browsing pressure across feeding heights is associated with the available leaf biomass per bite for browsers (Woolnough and du Toit 2001)."

One may doubt whether there is any "browsing *pressure*" at all as long as there is enough forage for all the browsers. Incidentally, in the late dry season in *A. nigrescens* as well as in *B. albitrunca* the lowest percentages of browsed shoot ends were found at a height of 0.5 m as compared to 1.5 and 2.5 m; Woolnough & du Toit 2001, table 2, p. 588 (an important point not mentioned in the paper of 2007). Thus, according to the feeding competition hypothesis, severe depletion could be completed first in one of the higher levels eventually resulting in competition for the rest of the forage at 0.5 m and below with perhaps correspondingly unexpected evolutionary consequences for giraffes and other browsers. – Only under extremely sore environmental conditions and food shortages (Darwin's series of severe dearths not addressed by the authors), one may postulate such a thing as "browsing pressure across feeding heights" (perhaps nothing left below 2.5 m – still available for Kudus – yet also leading to the starvation of the young giraffes, at least if all the giraffes stayed in that area; see, however, home range areas pp. 4 and 5). And obviously the giraffes themselves do not display much respect for this hypothesis either. Remember the key observations by Simmons and Scheepers from p. 65 above, "that during the dry season ... giraffes generally feed from low shrubs, not tall trees" etc. and that each result of their investigations "suggests that long necks did not evolve specifically for feeding at higher levels."

Besides, the ***mean feeding height of giraffe is ca. 2.7 m*** (du Toit 1990, p. 58). And Young and Isbell (1991) found "that giraffe feeding rates were greatest for both sexes at intermediate heights" (Ginnett and Demment 1999, p. 103). One would perhaps expect a higher figure of 4 m or even more from the feeding competition and selection hypothesis.

Cameron and du Toit go on as follows:

> "Consequently, giraffes gain a foraging advantage by browsing above the reach of smaller browsers."

Yet obviously they don't care too much for the ½ to 1 g difference per GBU found for the 7 trees of *A. nigrescens* – otherwise also their young ones and females would perpetually practice it as far as possible. But even if they did, this would also be fully compatible with the ID-hypothesis on the origin of the giraffe (see pp. 22 and 25-28). And it would prove nothing concerning evolution by the postulated random mutations and natural selection. – Incidentally, the hypothesis of an intelligent origin of species/families would, of course, not expect the design of an animal almost 6 m high with forage options on all levels just to limit its foraging say to 1 m above the ground. Rather, it would postulate and predict multiple options of behaviour and organismal reasons as well as ecological factors contributing to the welfare of our 'tall blondes' as constitutive elements of the synorganization of the entire ecological system of plants and animals. "Foraging in large herbivores can be viewed as a hierarchical process (Johnson 1980; Senft et al. 1987)" – Ginnett and Demment 1997, p. 292. Besides, there may be no necessary foraging advantage *in competition* with smaller browsers. Cameron and du Toit continue:

> "We additionally show that variation in leaf biomass per shoot across browsing heights diminishes significantly if the smaller browsers are experimentally excluded."

Once again: the authors have excluded ***not only the smaller browsers but the giraffes as well*** – possibly the weakest point in the entire experimental scheme.

> "Consequently, the pattern of variation in leaf biomass per GBU across feeding heights must be due to depletion of leaf biomass by selective browsing at low canopy levels,…"

There is neither a "consequently" nor a "must be". They have excluded the giraffes (especially the young and the female animals) from the outset of their experiment and *they simply postulate* that only the smaller browsers are responsible for the depletion found.

Also, the question may be raised *whether* and if so to what extent the trees themselves compensate for only being browsed at higher levels by perhaps producing more leaf biomass per GBU at the lower levels excluded from browsing ("...increased tolerance and resistance in heavily browsed trees is associated with important changes in tree branching, prickle spacing, shoot growth rates, shoot diameter and shoot number" - Fornara 2005, p. 80; "The higher number of shoots produced by heavily browsed trees suggests that the removal of apical dominance stimulates the growth of secondary shoot meristems" and "Our evidence is that browsing lawns increase the feeding efficieny of browsers through increased production of shoot mass all around the distinctly hedged canopies of browsed trees. This makes more food available to ungulate browsers such as giraffes, kudus, and impala, which often remove shoot ends and, hence, have a pruning effect (Pellew 1983, du Toit 1990). Leaf mass did also increase in regrowth shoots" – Fornara and du Toit 2007, pp. 204 and 207).

Moreover, some browsers – including the juvenile and female **giraffes** – being barred from forage below 2.2 m on the excluded trees, may turn to the next control tree to combat their appetite all the more there.

> "…supporting the hypothesis that giraffe feeding efficiency is reduced at low heights as an outcome of competition with smaller guild members."

At present, this inference is as doubtful as the presuppositions. Moreover, it is in conflict with the observations by Simmons and Scheepers as quoted above as well as Ginnett and Demment (1997, 1999).

My impression is that Cameron and du Toit are trying to force the state of being of the giraffe and other browsers into the Procrustean bed of perpetual Darwinian evolution by natural selection, taking for granted that mutations have produced the genetic variation necessary to evolve all the animals now found; and du Toit has consistently tried to interpret his observations in terms of selection theory. Just to give another example (du Toit 1990, p. 60):

> "In East Africa too, giraffe bulls usually feed at full neck stretch while cows prefer feeding at body or knee height (Sinclair & Nortan-Griffiths, 1979; Pellew, 1983). Pellew (1984b), who used this difference in feeding posture as a means of sexing giraffe from a distance, proposed that it reduces competition between the sexes. In contrast, I suggest that it could in fact indicate the existence of such competition."

As far as I know there is no evidence for competition between the sexes (see also Ginnett and Demment 1999). Rather, 'the resources are well shared: species survival by cooperation rather than brutal selection' (see p. 5 of the present paper).

Moreover, "vertical zonation of browse quality in tree canopies" – as correct as the investigations and results concerning *A. nigrescens* may be ("giraffe feeding efficiency increases with height up the canopy", but not inevitably in other genera as well, see p. 70 and 72/73 above) – is simply ***translated*** into the language of competition, selection, and evolution without sufficient scientific evidence for adequate positive mutations and natural selection (see for example Behe 1996, 2007, Lönnig 2001, 2006, 2007). Instead, Darwinism is implicitly assumed to be true and the facts are interpreted according to this presupposition.

Thus, concerning evolution, Cameron & du Toit conclude their paper as follows (p. 134 last paragraph subdivided into several parts for the following discussion):

> "Despite popular acceptance that giraffes have long necks because of foraging competition during their evolution, no previous studies have experimentally investigated foraging competition between giraffes and smaller browsers."

Although the authors maintain that they have done this, ***they failed to experimentally investigate foraging competition between giraffes and smaller browsers*** by excluding not only the smaller browsers but also the giraffes from the outset for the lower forage levels.

> "Simmons and Scheepers (1996) argued that there was little evidence that giraffes forage high in the canopy because of competition and suggested sexual selection as an alternate hypothesis."

Simmons and Scheepers showed ***evidence*** to the contrary of competition.

> "However, Woolnough and du Toit (2001) showed that giraffes achieve a bite-size advantage by feeding higher in the tree,..."

– Which especially the young and female giraffes often cannot or do not care for or appreciate too much.

> "…and now we show that this is explained by the avoidance of competition with smaller browsers."

This is exactly what Cameron and du Toit fail to prove. However, their conclusion reads as follows:

> "While not resolving the controversy, our study provides the first experimental evidence that the giraffe's extremely elongated body form is naturally selected in response to competition from smaller browsing species."

So far the experimental evidence is deeply flawed. The title of the paper "Winning by a Neck: Tall Giraffes Avoid Competing with Shorter Browsers" is incorrect. It could perhaps be a truism like "Winning by a Neck? Tall Giraffes Cannot Display Any

Competition with Smaller Browsers ***when Forage is Excluded for All Browsers at Least Up to a Height of 2.2 m***." Moreover, to date it is doubtful whether there is any severe competition at all between various species of browsers at different tree heights.

Grazing giraffe. See references for the photographs at the end of the appendix under (9).

The only inference on which one may fully agree with the authors is that they have not resolved the controversy, the rest of their interpretations is hardly more than neo-Darwinian guesswork and story-telling.

As for the ID-hypothesis one may suggest the following scenario (also still in the beginning and to be extended and tested in detail, – as pointed out above and in clear contrast to the neo-Darwinian viewpoint, I think that on the scientific level further options like ID should be carefully investigated as well): Giraffes were 'designed' (front-loaded or otherwise) – according to their respective developmental stages and gender in correlation with different and varying environmental parameters and conditions – to browse from lower layers of vegetation upwards to about 6 m in height with a mean feeding height of about 2.7 m. For a lush vegetation with many different plant species on the menu of the giraffe, "depletion of higher-quality plant parts by smaller browsers" will hardly be a serious competition factor determining the behaviour of this 'altogether exceptional, novel, and specialised' animal (to apply Lancestor's words to the whole animal). And even in the dry seasons giraffes often do not behave as expected. Yet especially the bulls may take some advantage from 'the tendency of trees to allocate more leaf biomass to shoots high in the canopy' without any obligatory competition with other animals.*

The paper of Cameron and du Toit clearly *does not provide* what the authors promise in the abstract of their paper (2007, p. 130), namely "the first experimental support for the classic evolutionary hypothesis that vertical elongation of the giraffe body is an outcome of competition within the browsing ungulate guild." Further options like the intelligent origin of the giraffe should be carefully considered.

*(By modification of a sentence of Cameron and du Toit 2007, p. 131, which reads: "However, it is also possible that the tendency of trees to allocate less leaf biomass to shoots low in the canopy may explain this variation even in the absence of competition (Woolnough & du Toit 2001").

References for the popular press etc. and the photographs:

http://deseretnews.com/dn/view/1,1249,650224911,00.html
http://www.physorg.com/news86017365.html

http://www.sciencedaily.com/releases/2006/12/061223092600.htm
(1) http://www.jostimages.com/bilder/preview/000000016280/_(000000016280).jpg
(2) http://www.urlaub-suedafrika.de/UserFiles/Image/AddoNP3/Kudu_male.jpg
(3) http://www.namhunt.de/ger/jagdbilder/fotos/kudu.jpg
(4) http://torch.cs.dal.ca/~riordan/sa2007/SA3/MaleKudu.jpg
(5) http://reto.checkit.ch/SouthAfrica/Images/20010619_1043__Kudu.jpg
(6) http://african-safari-and-travel-advisor.com/images/young-african-wildlife-safari-2-young-giraffe-w-michael-poliza-b.jpg
(7) http://www.photos-voyages.com/afriquedusud/girafesecachant.jpg
(8) http://www.fyvie.net/photos/Travel/South%20Africa%20July%202004/slides/IMG_3413.JPG
(9) http://www.miamimetrozoo.com/assets/conservation/giraffe.jpg

"Up to 100 species of plants recorded for the giraffe's diet": http://spot.colorado.edu/~humphrey/fact%20sheets/giraffe/giraffe.htm
70 plant species in KNP: http://www.krugerpark.co.za/Kruger_National_Park_Wildlife-travel/images-of-kruger-the-antelope.html

Brief comments on some objections

As to the two papers on the origin of the long-necked giraffe, sometimes there seem to be some misunderstandings, which I will briefly address here (the basic problem causing these misconceptions probably is that nowadays there are many bloggers and commentators who are *writing much more than they read* – careful study appears to be hard for some people): (1) One blogger thought that I had "a low threshold for jumping to design". This person possibly did not read or understand the last paragraph of p. 48 above: "In this connection it should be clear that *on the scientific level* the two present articles on the evolution of the long-necked giraffe **are only a beginning** (even if one, on a personal level, may consider the basic questions to be completely solved)… " – Neither did he reflect the research projects necessary to corroborate or deny the ID-hypothesis for the giraffe on that scientific level as discussed on pp. 25-28.

(2) "Homeotic shifts" are assumed by other authors to explain, for instance, the number and specific architecture of the neck vertebrae of the giraffe. However, this does not explain why such a functionally favourable homeotic shift should have occurred almost exclusively in the long-necked giraffe out of thousands of other mammal species (see p. 16 above). And, of course, neither would it account for all the other synorganized giraffe features enumerated on p. 25 and repeated on p. 63 of the present paper. It would not even clear up the enormous length of the giraffe's neck vertebrae (for an elongation is not an inevitable by-product of a homeotic shift). At present, the assumption of an accidental homeotic mutation is nothing but a simplistic *ad hoc* explanation with hardly any contents at all. Perhaps I should add that I myself have experimentally worked on homeotic shifts for some twenty years now: regularly there are strong negative pleiotropic side effects so that the organisms thus affected have no chance at all for further evolution. In all the homeotic shifts I have experimentally gained and investigated so far, there was not even one case of which I could say that it was simply positive. To obtain a long-necked giraffe from an okapi-like animal, if only for the number and architecture of the neck vertebrae, much more is necessary than just a random homeotic shift.

So, what do we really know about "the evolution of the long-necked giraffe"? We know that there is an enormous morphological, anatomical and physiological distance between *Giraffa camelopardalis* and its nearest relative, the okapi. Also, a continuous series of connecting links between short-necked and long-necked giraffes is unknown so far. We also know that *Giraffa* "represents not a mere collection of individual traits but a package of interrelated adaptations" (Davis and Kenyon, see Part 1, p. 10) and that all these intricate parts are perfectly fine-tuned to each other and are integrated into an enormously complex "single pattern" of an impressive and beautiful animal species 'altogether exceptional, novel, and specialized'. – Further research should focus on the question, among others, whether *systems of irreducible and specified complexity* are involved in the origin of the long-necked giraffe (see again the research projects above). If so, then ID is *scientifically* the most likely explanation in this case, too. On the other hand, "the standard [neo-Darwinian] story, in fact, is both fatuous and unsupported" (Gould).

After some remarks on the origin of phyla, subphyla and classes, Michael J. Behe (2007, p. 199) answers the question whether design extends "even further into life, into the orders or even families of vertebrate classes? To such creatures as bats, whales, and giraffes?" as follows: "Because 'all of the structural characters of the edifice, from its overall form to minute aspects that determine its local functionalities…must be specified in the architect's blueprints' [Davidson], I would guess the answer is almost certainly yes. But at this point our reliable molecular data run out, so a reasonably firm answer will have to await further research. Given the pace of modern science, we shouldn't have to wait too long."

15. References for Part 1 and Part 2

Annotation: The references in the quotations themselves are not listed in the following catalogue of papers and books, the web-links only in isolated cases. The authors are given in blue boldface, the publishers, when they are listed first, in black boldface. The titles of English articles from journals and books are cited in small letters, for English book titles at least the nouns are capitalized. For the full titles of some journals, see the *NCMR Library List of Journal Titles abbreviations*: http://atlantis.ncmr.gr/abbreva.htm.

Several points on the peculiarities of the long-necked giraffe are further supplemented in the following reference list (mostly from abstracts).

Agustí, J., Cabrera, L., Garcés, M., Krijgsman, W., Oms, O. and J.M.Parés (2001): A calibrated mammal scale for the Neogene of Western Europe. State of the art. Earth-Science Reviews **52**: 247-260.

Arambourg, C. (1959): Vertébrés continentaux du Miocène Supérieur de L'Afrique du Nord. Publications du service de la carte géologique de L'Algerie (Nouvelle Série) Paléontologie, Mémoire No 4. Service de la carte géologique de L'Algérie, Alger.

Barry, J.C., Cote, S., MacLatchy, L., Lindsay, E.H., Kityo R. and A. Rahim Rajpar (2005): Oligocene and Early Miocene Ruminants (Mammalia, Artiodactyla) from Pakistan and Uganda. http://palaeo-electronica.org/2005_1/barry22/german.htm (Abstract) oder http://www.nhm.ac.uk/hosted_sites/pe/2005_1/barry22/barry22.pdf (ganzer Artikel).

Basu, P.K. (2004): Siwalik mammals of the Jammu Sub-Himalaya, India: an appraisal of their diversity and habitats. Quaternary International **117**: 105-118.

Behe, M.J. (1996/2006): Darwin's Black Box: The Biochemical Challenge to

Evolution. The Free Press, New York.

Behe, M.J. (2004): Irreducible complexity. Obstacle to Darwinian evolution. In: Dembski W.A., Ruse M. (eds.), Debating Design: From Darwin to DNA, 352-370. Cambridge University Press, Cambridge.

Behe, M.J. (2005): Design for living. The New York Times, 7 February 2005 (http://www.nytimes.com/2005/02/07/opinion/07behe.html).

Behe, M.J. (2007): The Edge of Evolution. The Search for the Limits of Darwinism, The Free Press, New York.

Benninghoff/Drenckhahn (2004): Anatomie. Makroskopische Anatomie, Histologie, Embryologie, Zellbiologie. Band 2: Herz-Kreislauf-System, Lymphatisches System, Endokrines System, Nervensystem, Sinnesorgabe, Haut. Herausgegeben von **D. Dernckhahn**. 16., völlig neu bearbeitete Auflage 2004. Elsevier GmbH, München.

Benninghoff/Drenckhahn (2008): Taschenbuch der Anatomie. D. Drenckhahn und J. Waschke (Herausgeber). Urban und Fischer, Elsevier, München. (P. 231 zum Thema Atemsystem: "I [Innervation]: parasympathisch und sympathisch innerviert, die Nervenfasern verlaufen mit Ästen des N. vagus (N. laryngeus recurrens, Rr. bronchiales) und des Truncus sympathicus, Rr. pulmonales.")

Benton, M.J. (1993): The Fossil Record 2. Chapman and Hall, London.

Bertelsmann Lexikothek (1988): Die Tiere unserer Welt (G. Bateman et al. 1988, Farb- und Strichzeichnungen von P. Barrett). Verlagsgruppe Bertelsmann GmbH, Gütersloh.

Bertelsmann Lexikon der Tiere (1992), herausgegeben vom Lexikon-Institut Bertelsmann, Gütersloh.

Bohlin, B. (1926): Die Familie Giraffidae, mit besonderer Berücksichtigung der fossilen Formen aus China. Palaeontologia Sinica, eds. V.K. Ting and W.H. Wong. Series C, Vol.IV. Fascicle 1: 1-178 (plus Verzeichnis der Lokale und 36 pp. Tafeln). Published by the Geological Survey of China, Peking 1926.

Brockhaus Enzyklopädie in 24 Bänden. Bd. 7, 19. Auflage, 1988. F.A.Brockhaus, Mannheim.

Brown, D.M., Brenneman, R.A., Koepfli, K.-P., Pollinger, J.P., Milá, B., Georgiadis, N.J., Louis, E.E. Jr., Grether, G.F., Jacobs, D.K. and R.K. Wayne (2007): Extensive population genetic structure in the giraffe. BMC Biol **5**: 57-(69?). http://www.biomedcentral.com/content/pdf/1741-7007-5-57.pdf

Brown et al. (pp. 63/64, if I counted correctly, - the page numbers on the PDF of the paper are “not for citation puposes”) suggest “that the giraffe might represent more than one species” and that their results and arguments “support viewing the giraffe as containing **multiple distinct species** rather than a single polymorphic form. Reciprocal monophyly in mtDNA sequences and nearly absolute partitioning in microsatellite data support **minimally six species**, corresponding to *Giraffa peralta, G. rothschildii, G. reticulata, G. tippelskirchi, G. giraffa, and G. angolensis*.” Also, “the Masai might constitute more than one species” and “additional taxa might be defined, pending analysis of the subspecies included in taxonomic schemes (Table 1) not sampled in our study design (e. g. *G. c. antiquorum* [10]). Finally **many of these species appear to include multiple distinct population units that are genetically differentiated**.”

However, if *every genetically (molecularly) differentiated population unit* were finally raised to the status of a species of its own, one might ask, among other things: How great, then, would the number of giraffe species eventually be? (The authors already detected “**at least 11 genetically distinct populations**” (p. 57).) Also, would this not mean that many species could be distinguished from each other only after thorough molecular investigations? (In several cases even members of the same phenotype of the giraffe would have to be assigned to different species. To which of these "species" would the giraffes of your nearest zoo belong to?). Moreover, applying the author's species concept to humans: How many ***species*** ("genetically distinct populations") could be discriminated – in more than one sense of the verb – among present human beings? For some mistakes of the past, see please http://www.weloennig.de/AesIIMe.html .

Yet, as we have seen in detail above (p. 55), most probably all the giraffe "species" can mate and produce fertile hybrids. And "forms which, in all characters, follow the Mendelian laws upon reciprocal crossings have to be viewed as varieties of the same species" (De Vries in agreement with almost all classical Mendelian geneticists up to the present; see http://www.weloennig.de/Artbegriff.html, 622 pp., especially http://www.weloennig.de/AesIV3.html). Thus, there is **only one species, *Giraffa camelopardalis,* with many subspecies** (and, interestingly, even with molecularly defined populations within these subspecies; Richard Goldschmidt would possibly call them "subsubspecies", see http://www.weloennig.de/AesIV2.A.3.Ka.html).

Let me supplement the list of p. 55 by also quoting the hybridizations mentioned by Ingo Krumbiegel 1971, p. 65 (according to Fig. 41; subspecies on the left here not always the female parent):

Southern subspecies *Giraffa camelopardalis tippelskirchi* X *Giraffa camelopardalis reticulata* (Northern subspecies)
Southern subspecies *Giraffa camelopardalis tippelskirchi* X *Giraffa camelopardalis camelopardalis* (Northern subsp.)
Southern subspecies *Giraffa camelopardalis tippelskirchi* X *Giraffa camelopardalis antiquorum* (Northern subspecies)
Southern subspecies *Giraffa camelopardalis tippelskirchi* X *Giraffa camelopardalis peralta* (Northern subspecies)
Southern subspecies *Giraffa camelopardalis capensis* X *Giraffa camelopardalis antiquorum* (Northern subspecies)
Northern subspecies *Giraffa camelopardalis rothschildi* X *Giraffa camelopardalis peralta* (Northern subspecies)
Northern subspecies *Giraffa camelopardalis congolensis* X *Giraffa camelopardalis peralta* (Northern subspecies)
Northern subspecies *Giraffa camelopardalis peralta* X *Giraffa camelopardalis reticulata* (Northern subspecies)

The main reason of the Brown et al. to split *Giraffa camelopardalis* into several species is the rather strong reproductive isolation which they seem to have found in the giraffe populations ***in the wild***: "…our results indicate that neighbouring subspecies as well as those that are geographically separated are essentially reproductively isolated, suggesting that some might represent distinct species rather than a single polytypic form" (p. 64).

The authors have to admit, however, that "hybridization in the wild has been reported for some subspecies (e.g., Masai and reticulated giraffes [2]" (p. 61) and that there are suggestions "that hybridization occurs frequently among giraffe subspecies" (p. 63), yet their data so far detected show only that such events seem to be quite rare (according to their microsatellite data in 3 of 381 sampled individuals).

Dagg and Foster write (1976/1982, p. 156 and p. 158): "The reticulated giraffe was regarded as a separate species until recently, although **many transitional individuals** between the reticulated and blotched giraffe have been recorded **both in captivity and in the wild** (see Krumbiegel, 1951)."…"The range of *G. c. rothschildi* is uncertain, as it is bounded on most sides by ranges of **neighboring races which intergrade with it**, and it has decreased greatly in recent years."

On the basis of such records, the question may be raised whether the molecuar basis and sample collection of Brown et al. were sufficient and specific enough to substantiate their far-reaching taxonomic inferences, even if only for a doubtful evolutionary species concept (these points could constitute the topic of a discussion of its own). Incidentally, I think that the authors should better speak of "***micro***evolutionary significant units" instead of "evolutionary significant units" (pertainig to the genetically differentiated populations).

Especially interesting in this connection are their calculations for the divergence times (p. 60):

> "Divergence times between the seven clades obtained from coalescence analysis [19] ranged from 0.13–0.37 million years (MY) between Masai and South African clades, to **0.54–1.62 MY** between the southern clade (Masai, Angolan and South African giraffes) and the northern clade (West African, Rothschild's and reticulated giraffes) (Table 2). Values for the northern giraffe grouping were intermediate, with West African and Rothschild's giraffes diverging about 0.16–0.46 MY ago, and the two splitting from reticulated giraffes about 0.18–0.54 MY ago. These dates argue for a mid to late Pleistocene radiation of giraffes."

Now, let us extrapolate from the values of up to 1.62 million years, as found for the microevolutionary divergence on the morphological, anatomical and physiological levels between the southern and the northern clade, to the time necessary for the evolution of the enormous differences between the long-necked and short-necked giraffes or even to all the (mega-)differences within the entire giraffe family. As a first educated guess I would say that we could possibly approach the Cambrian period some 544 million years ago. Of course, I am most certainly not the first author who thus concludes that there must be a **fundamental difference between micro- and macroevolution** (see, for example, the authors quoted by Junker 2006 http://www.genesisnet.info/pdfs/Mikroevolution_Makroevolution.pdf, and 2008 http://www.genesisnet.info/pdfs/Evo-Devo.pdf as well as Lönnig et al. 2007 http://www.weloennig.de/Dollo-1a.pdf .

Brownlee, A. (1963). Evolution of the giraffe. Nature **200**: 1022.

Caister, L.E., Shields, W.M. and A. Gosser (2003): Female tannin avoidance: a possible explanation for habitat and dietary segregation of giraffes (*Giraffa* *camelopardalis peralta*) in Niger. African Journal of Ecology **41**: 201-210.

Cameron, E.Z. and J. T. du Toit (2007): Winning by a neck: tall giraffes avoid competing with shorter browsers. The American Naturalist **169**: 130-135. (See comment in the **Appendix**, p. 62 ff.)

Carroll, R. L. (1988): Vertebrate Paleontology and Evolution. W. H. Freeman and Company, New York. (German edition 1993: Paläontologie und Evolution der Wirbeltiere. Thieme Verlag, Stuttgart.)

Churcher, C.S. (1978): Giraffidae. In: V.J. Maglio and H.B.S. Cooke (eds.): Evolution of African Mammals, 509-535. Harvard University Press, Cambridge (Mass.) and London.

Churcher, C.S. (1979): The large palaeotragine giraffid, *Palaeotragus germaini*, from late Miocene deposits of Lothgam Hill, Kenya. Breviora, Museum of Comparative Zoology, Cambridge, Mass., Number 453: 1-8.

(I must confess that I was surprised by the content of this work. I had expected more exact data on the size of *Palaeotragus germaini*, what if found was a precise description of a molar tooth, which provided the first evidence for the species in East Africa: "ABSTRACT. An isolated and slightly damaged left M^1 constitutes the first record of the species *P. germaini* in East Africa. This specimen matches in size and development of the buccal ribs and styles the upper molars of *P. germaini* from Oued el Hammam, Algeria."

And from the text, p. 1: "An isolated and damaged left upper molar, probably M^1 (KNM-LT 414, field no. 138/67K), was collected by Dr. V. J. Maglio from Lothagam Hill, Kehya, in 1967. The tooth was found at Lothagam-I near the base of Member B (Patterson *et al.* 1970), and is deposited in the collections of the International Louis Leakey Memorial Institute for African Prehistory, Kenya National Museums, Nairobi.")

Ciofolo I. and Y. Le Pendu (2002): The feeding behaviour of giraffe in Niger. Mammalia **66**: 183-194.

Colbert, E.H. (1935): Siwalik mammals in the American Museum of Natural History. Transactions of the American Philosophical Society **26**: 1-401. (Giraffidae: 323-385; nach Colberts Stammbaum p. 375 ist *Palaeotragus* ein Vorfahr von *Okapia* und die Langhalsgiraffen zweigen zeitlich schon vor *Palaeotragus* ab.)

Colbert, E.H. (1935b): Distributional and phylogenetic studies on Indian fossil mammals. V. American Museum Novitates, Number 800: 1-15. Published by The American Museum of Natural History, New York City.

Colbert, E.H. (1938): The relationships of the okapi. J. Mamm. **19**:47-64.

Conway-Morris S. (2003): Life's Solution: Inevitable Humans in a Lonely Universe Cambridge University Press, Cambridge.

Cox, B., Dixon, D., Gardiner, B., und R.J.G. Savage (1989): Dinosaurier und andere Tiere der Vorzeit. Mosaik-Verlag, München

Dagg, A.I. and J. B. Foster (1976/1982): The Giraffe. Its Biology, Behavior, and Ecology. Krieger Publishing Company, Malabar, Florida (Reprint 1982 with updated supplementary material.)

Davis, P. and H. Kenyon (1993): Of Pandas and People. The Central Question of Biological Origins. Second Edition. Haughton Publishing Company, Dallas, Texas.

Darwin, C. R. (1872/1967): On the Origin of the Species, John Murray, London. 1967: Everyman's Library No. 811, reprint of the sixth edition of 1872. (And 1859: On the Origin of Species by Means of Natural Selection or, The Preservation of Favoured Races in the Struggle for Life. Wordsworth Classics of World Literature 1998, Mackays of Chatham, Chatham, Kent.)

Darwin, C. R. - The Complete Work of Charles Darwin Online: http://darwin-online.org.uk/

Dawkins, R. (1996): Climbing Mount Improbable. Penguin Group, London.

Dawkins, R. (1997): Evolution: the dissent of Darwin - debate between zoologist Richard Dawkins and computer scientist Jaron Lanier. Psychology Today, Jan/Febr. 1997. http://www.findarticles.com/p/articles/mi_m1175/is_n1_v30/ai_19013604/pg_2

Dawkins, R. (2010)(See his comments on the Nervus laryngeus recurrens of the giraffe in the YouTube video): *Laryngeal Nerve of the Giraffe Proves Evolution*: http://www.youtube.com/watch?v=0cH2bkZfHw4

Defechereux, T., Alber, V., Alexandre, J., Bonnet, P., Hamoir, E. and M. Meurisse (2000): The inferior non recurrent laryngeal nerve: a major surgical risk during thyroidectomy. Acta Chirurgica Belgica **100**: 62-67: http://www.ncbi.nlm.nih.gov/pubmed/10925715

Dembski, W.A. (1998): The Design Inference: Eliminating Chance Through Small Probabilities, Cambridge University Press, Cambridge.

Dembski, W.A. (2002): No Free Lunch: Why Specified Complexity Cannot Be Purchased without Intelligence. Rowman and Littlefield, Lanham.

Dembski, W.A. (2004): The Design Revolution. InterVarsity Press, Downers Grove.

Dembski, W.A. and M. Ruse (eds.) (2004): Debating Design: From Darwin to DNA, Cambridge University Press, Cambridge.

Denis-Huot, C. und M. (2003): Faszination Savanne. Wildlife in Ostafrika. KnesebeckGmbH & Co. Verlags KG, München. (This book contains, among other things, a series of 21 very good giraffe photos. On page 41 the following is said of the long-necked giraffe: "They move gracefully amid **bushes and trees, and they [the giraffes] tower high above them,** and their gaze wanders into the distance." On the same page we read: „The form of the giraffe necessitates special physiological requirements in terms of blood circulation between heart and brain. When the neck is upright, the brain finds itself two to three meters above the heart. However, when the animal lowers his head, more blood flows to the brain. With rapid head movements, the change in blood pressure would lead to a loss of consciousness or even a stroke, **if the giraffe had not, in the course of time, adapted to these conditions**: An ingenious network of many additional blood vessels at the lower edge of the brain works like a sponge. These vessels consist of arteries, which control the blood flow to the brain by contraction and expansion and only allow the necessary quantity of blood flow through to the brain. The giant giraffe heart – it weighs more than ten kilograms and displays walls, which are more than eight centimeter thick – can transport more than 60 liters of blood per minute!" The adaptation hypothesis, taken literally, leads to the question, **how did the long-necked giraffe get along and survive all the time – until the adaptation was "ready".**

Prof. Thure von Uexküll of the University of Heidelberg comments on the idea of adaptation (1963, S. 235/237 and 237 in his book *Grundfragen der psychosomatischen Medizin*. Rowohlt, Reinbek bei Hamburg):

> „Behind the way in which the term 'adaptation' ... is used, there is a philosophy assuming that living beings initially found themselves in a world for which they were not equipped, and that the organisms had to adapt to their environment in the course of an enormous long evolutionary history. According to this conception, all the

> achievements and functions of living beings arose by adaptation. If one carries this idea to its ultimate consequence, then the living beings of the earliest times were not equipped to respond in any meaningful way to the environment. It is however, extremely improbable that animals, plants or even single celled organisms could remain alive and have time to complete their adaptations in an environment to which they were not adapted or accommodated at all. *A fish, that ends up on land, does not adapt to its new environment, but dies.* ...We know of no adaptation that leads from a condition of primary disorder to a condition of order" (italitcs are mine).

For the reader who can speak German, too, the above paragraphs are also given in the original language:

Das Buch enthält u.a. eine Serie von 21 sehr guten Giraffenfotos. Auf der Seite 41 heißt es zur Langhalsgiraffe: "Anmutig bewegt sie sich inmitten der **Büsche und Bäume, die sie weit überragt,** und lässt ihren Blick in die Ferne schweifen." Auf derselben Seite lesen wir unter anderem weiter: „Die Gestalt der Giraffe erfordert spezielle physiologische Gegebenheiten im Bereich des Blutkreislaufes zwischen Herz und Gehirn. Das Gehirn befindet sich bei aufgerichtetem Hals zwei bis drei Meter über dem Herzen. Senkt das Tier jedoch seinen Kopf, so fließt vermehrt Blut in das Gehirn. Bei schnellen Kopfbewegungen würde die Veränderung des Blutdrucks zu Bewusstlosigkeit oder gar zu einer Hirnblutung führen, **hätte sich die Giraffe nicht im Laufe der Zeit an diese Gegebenheit angepass**t: Ein ausgeklügeltes Netz aus zahlreichen zusätzlichen Blutgefäßen am unteren Rand des Gehirns wirkt wie ein Schwamm. Bei diesen Gefäßen handelt es sich um Arteriolen, die durch Zusammenziehen und Erweitern den Blutfluss in das Gehirn kontrollieren und nur die jeweils benötigte Menge an Blut hindurchfließen lassen. Das riesige Giraffenherz - es wiegt mehr als zehn Kilogramm und hat über acht Zentimeter dicke Wände - kann mehr als 60 Liter Blut pro Minute befördern!" Die Anpassungshypothese wörtlich genommen führt zur Frage, **wie denn die Langhalsgiraffe die ganze Zeit – bis die Anpassung „fertig" war – zurecht gekommen ist und überlebt hat**.

Dazu sei an Prof. Thure von Uexkülls Bemerkung zum Anpassungsbegriff erinnert (1963, S. 235/237 und 237, *Grundfragen der psychosomatischen Medizin*. Rowohlt, Reinbek bei Hamburg):

> „Hinter der Art und Weise, wie der Begriff 'Anpassung'...verwendet wird, steckt eine Philosophie, die von der Annahme ausgeht, die Lebewesen hätten sich zu Beginn in einer Welt befunden, für die sie nicht ausgerüstet waren und an die sie sich erst im Laufe einer unendlich langen Entwicklungsgeschichte hätten anpassen müssen. Nach dieser Vorstellung wären schließlich alle Leistungen und Reaktionen lebender Wesen durch Anpassung entstanden. Denkt man diese Vorstellung konsequent zu Ende, dann hätten die Lebewesen der ersten Zeiten noch nicht über Reaktionen verfügt, die in irgendeiner Weise sinnvolle Antworten auf die Außenwelt bedeuteten. Es ist aber außerordentlich unwahrscheinlich, daß Tiere, Pflanzen oder auch Einzeller in einer Umgebung, mit der sie nicht das Geringste anfangen können, am Leben bleiben und Zeit haben, Anpassungsleistungen zu vollziehen. *Ein Fisch, der aufs Land gerät, paßt sich der neuen Umgebung nicht an, sondern geht zugrunde.* ...Wir kennen keine Anpassung, die von einem Zustand primärer Unordnung zu einem Zustand der Ordnung führt" (kursiv von mir).

Dennet, D. (1995): Darwin's Dangerous Idea. The Penguin Press, London.

Devillers, C. and J. Chaline (1993): Evolution. An Evolving Theory. Springer-Verlag, Berlin.

Dewar, D. (1957): The Transformist Illusion. Dehoff Publications, Murfreesboro, Tennessee.

Dr. X (2006): Mehrere E-Mails an W.-E. L. vom 3. März 2006 bis 24. April 2006. (E-Mail 3 March 2006: "I have examined all fossil cervical vertebrae of Giraffidae. I have notes and pictures of them and one day I plan to write a study on them. There are all short except of those of *Bohlinia attica* from Pikermi (Miocene of Greece) and *Giraffa*. *Bohlinia* is just as long as Giraffa and certainly not an intermediate. There are differences in the short vertebrae of the various species. These vertebrae are a few and not connecting any of the fossil taxa to *Giraffa*.")

Dr. Y (2006): E-Mails an W.-E. L. vom 6. März und 3. April 2006.

Dr. Z (2006): E-Mail an W.-E. L. vom 11. April 2006.

Duckworth, W.L.H. (1968): Galen on Anatomical Procedures: The Later Books. Cambridge University Press, Cambridge, see http://books.google.de/books?hl=de&lr=&id=P508AAAAIAAJ&oi (also 2010, same title: Galen on Anatomical Procedures: The Later Books. Cambridge Library Collection, edited by M. C. Lyons and B. Towers).

Du Toit, J.T. (1990): Home range-body mass relations: a field study on African browsing ruminants. Oecologia **85**: 301-303.

Du Toit, J.T. (1990): Giraffe feeding on acacia flowers predation or pollination. African Journal of Ecology **28**: 63-68.

Du Toit, J.T. (2001): Vertical zonation of browse quality in tree canopies exposed to a size-structured guild of African browsing ungulates. Oecologia **129**: 585-590.

Eichelbeck, R. (1999): Das Darwin-Komplott. Aufstieg und Fall eines pseudowissenschaftlichen Weltbildes. Riemann Verlag, ein Unternehmen der Verlagsgruppe Bertelsmann, Gütersloh.

Ellenberger, W. and H. Baum (1974/1991): Handbuch der vergleichenden Anatomie der Haustiere. 18. Auflage, 4. Nachdruck. Springer-Verlag, Berlin.

Endler J.A. (1986): Natural selection in the wild. Princetown University Press, Princeton, New Jersey.

Endo, H. Yamagiwa, D., Fujisawa, M., Kimura, J., Kurohmaru, M., and Y. Hayashi (1997): Modified neck muscular system of the giraffe (*Giraffa camelopardalis*). Annals of Anatomy **179**: 481-485.
("The modified spinous processes provided the large attachment surface for the strong nuchal ligament and for the muscles of the axis and other cervical vertebrae, while the muscle tendons had their origin in the ventrocaudally-enlarged transverse process. It is concluded that the **modified muscles** with their expanded belly and tendon have the functions of occupying the interspace among long vertebrae, and also of supporting the head and neck by means of their wide attachment to the altered vertebral processes.")

Erbrich, P. (1988): Zufall – Eine naturwissenschaftlich-philosophische Untersuchung. Verlag W. Kohlhammer, Stuttgart. (This is an astute investigation of the question of chance, which unfortunately has practically been ignored in the discussion of ID until now. Erbrich is a philosopher, S J, Hochschule für Philosophie in München (a Jesuit college), he retired in summer 1996. The interested reader can download a discussion on the subject of chance from the internet at http://82.135.31.182/jahresbericht_94_95.pdf. To be sure, Erbrich accepts rather uncritically the questionable claim that evolutionary theory is justifiably seen by biologists as the „*unifying theory* of biology", as well as the so-called „fact" of evolution, etc. He then, however, beginning on page 2, investigates critically the neo-Darwinian mechanism of mutation and on page 3 starts the „criticism of chance".)

Fennessy, J. T. (2004): Ecology of the Desert-Dwelling Giraffe *Giraffa camelopardalis angolensis* in Northwestern Namibia. PhD Thesis, University of Sidney, Australia.

Fleming, P.A., Hofmeyr, S.D., Nicolson, S.W. and J.T. du Toit (2006): Are giraffes pollinators or flowers predators of *Acacia nigrescens* in Kruger National Park, South Africa. Journal of Tropical Ecology **22**: 247-253.

Fleming, P.A., Hofmeyr, S.D. and S.W. Nicolson (2007): Role of insects in the pollination of *Acacia nigrescens* (Fabaceae). South African Journal of Botany **73**: 49-55.

Flint, R. (2000): Biologie in Zahlen, 5. Auflage. Spektrum Akademischer Verlag, Heidelberg.

Fornara, D.A. (2005): Ungulate Browsing as an Ecosystem Process: Browser-Plant-Soil Interactions a Southern African Savanna. PhD Thesis. University of Pretoria.

Fornara, D.A. and J.T. du Toit (2007): Browsing lawns? Responses of *Acacia nigrescens* to ungulate browsing in an African savanna. Ecology **88**: 200-209.

Fortelius, M. (2006): http://www.helsinki.fi/science/now/

(Data sent on 1 March 2006 „Giraffidae from the 2003 release“ and on the next day - 2 March 2006, 3:58 PM - Mikael Fortelius was so friendly to send me „the latest version“ of the data as an attachment: filename=Now_20060302_giraff_public.txt).

Franek, M., Johannsmeier, A.-K., Jung, M., Santos, S. und A.-K. Schwarz (2001): Vergleich der Evolutionstheorien von Lamarck und Darwin http://www.gymnasium-meschede.de/projekte/romantik/evolution.htm

Franz-Odendaal, T.A. (2004): Enamel hypoplasia provides insights into early systemic stress in wild and captive giraffes (*Giraffa camelopardalis*). Journal Of Zoology **263**: 197-206 (Part 2).
(“Enamel hypoplasia, a developmental tooth defect, provides a permanent record of systemic stress during early life. The incidence and distribution of linear enamel hypoplasia has been used by anthropologists and palaeontologists to assess the health status of past populations but has not been applied by wildlife biologists studying extant animals. This study investigates enamel hypoplasia in 23 *Giraffa camelopardalis* skulls from wild and captive animals of various ages and sex to determine whether any systemic stress events are unique to life in captivity. **Results indicate that wild giraffes are relatively stress-free as they do not have linear defects.** Based on the distribution of linear defects in other giraffes, three key stress periods during the first 6 years of giraffe life were identified. The first stress event occurs during weaning, the second at about 3 years of age and the third, which is the least common, at 4-5 years of age. All three stress events were observed in both male and female giraffes. This study highlights the usefulness of assessing enamel hypoplasia in both wild and captive animals as well as the need for further research on tooth developmental timings in many wild ungulates. Some left right asymmetry was observed in the development of linear and non-linear defects, which has implications for the aetiology of these defects.” – I have to confess that I expected the opposite: almost complete freedom from stress under human care among *captive animals* (without threats from lions) and more stress *in the wild*. Perhaps this discovery explains, why so many zoo giraffes do not reach the same height as their relatives in the wild: “Captive giraffe are seldom over 5.0 meters tall, probably because of the artificial diets and unusual climates“ – Dagg and Foster, p. 72.)

Fukuta, K., Kudo, H., Sasaki, M., Kimura, J., bin Ismail, D. and H. Endo (2007): Absence of carotid rete mirabile in small tropical ruminants: implications for the evolution of the arterial system in artiodactyls. Journal of Anatomy **210**: 112–116.

Galenos, C. (About 165 and the following years A. D.): See W. L. H. Duckworth (1962) and M. T. May (1968).

Gentry, A.W. (1994): The Miocene differentiation of old world pecora (mammalia). Historical Biology **7**: 115-158.

Geraads, D. (1979): Les Giraffinae (Artiodactyla, Mammalia) du Miocène supérieur de la région de Thessalonique (Grèce). Bull. Mus. natn. Hist. nat., 4e ser., 1., section C, no **4**: 377-389.

Geraads, D. (1986): Remarques sur la systématique et la phylogénie des Giraffidae (Artiodactyla, Mammalia). Geobios **19**: 465-477.

Geraads, D. (1991): Derived features of ossicones. J. Mamm. 72: 213-214. (p. 213: „[A]n ossicone is a bone originally independent from those of the cranial roof, ossifying from a cartilaginous matrix.“)

Geraads, D., Spassov, N. and D. Kovachev (2004): Giraffidae (Artiodactyla, Mammalia) from the Late Miocene of Kalimantsi and Hadjidimovo, Southwestern Bulgaria. Geologica Balcanica **35**: 11-18.

Ginnett, T.F. and M.W. Demment (1997): Sex differences in giraffe foraging behavior at two spatial scales. Oecologia 110: 291-300.

Ginnett, T.F. and M.W. Demment (1999): Sexual segregation by Masai giraffes at two spatial scales. African Journal of Ecology 37: 93-106.

Gould, S. J. (1996): The Tallest Tale. Natural History. May 1996, 18-23 and 54-57.

Gray, A.P. (1971): Mammalian Hybrids. A Check-List with Bibliography. Commenwealth Agricultural Bureaux, Farnham Royal, Slough SL2 3BN, England.

Gray's Anatomy: 36th Edition (1980). Edited by **Peter L Williams & Roger Warwick**. Associate Editors: **Mary Dyson & Lawrence H. Bannister**. Churchill Livingstone, Edinburgh./ 39th Edition (2005): Editor-In-Chief Susan Standring. Churchill Livingstone and Elsevier./ 40th Edition (2008): Editor-In-Chief also Susan Standring. Churchill Livingstone and Elsevier. Edinburgh, London, New York, Oxford, Philadelphia, St Louis, Sydney, Toronto.

Gymnasium Meschede: http://www.gymnasium-meschede.de/projekte/romantik/evolution.htm

Hamilton, W.R. (1978a): Fossil giraffes from the Miocene of Africa and a revision of the phylogeny of the Giraffoidea. Philosophical Transactions of the Royal Society of London, B. Biological Sciences **283**:165-229.

Hamilton, W. R. (1978b): Cervidae and Palaeomerycidae. In: V.J. Maglio and H.B.S. Cooke (eds.): Evolution of African Mammals, 496-508. Harvard University Press, Cambridge (Mass.) and London.

Hargens, A.R. (1994): Developmental adaptations to gravity/cardiovascular adaptations to gravity in the giraffe. Life Sciences Division, NASA Ames Research Center (California). Quoted according to Lynn Hofland: http://www.creationism.org/articles/giraffes.htm. See also http://fr.wikipedia.org/wiki/Girafe#cite_note-2

Hargens, A.R., Pettersson, K. and R.W. Millard (2007): Giraffe cardiovascular adaptations to gravity. In: William C. Aird (ed.): Endothelian Biomedicine, 99-106. Cambridge University Press, Cambridge.

Harland, B., Armstrong, R.L., Cox, A.V., Craig, L.E., Smith, A.G. und D.G. Smith (1990): A Geologic Time Scale 1989. Cambridge University Press, Cambridge.

Harris, J.M. (1976a): Pleistocene Giraffidae (Mammalia, Artiodactyla) from East Rudolf, Kenya. Fossil Vertebrates of Africa **4**: 283-332 (zitiert nach Mitchell und Skinner).

Harris, J.M. (1976b): Pliocene Giraffoidea (Mammalia, Artiodactyla) from the Cape Province. Annals of the South African Museum **69**: 325-353. (Nicht identisch mit 1976b im Zitat von Churcher.)

Harris, J.M. (1987): Fossil Giraffidae from Sahabi, Libya. In: N.T. Boaz, A. El-

Arnauti, A.W. Gaziry, J. De Heinzelin and D.D. Boaz (eds.): Neogene Paleontology and Geology of Sahabi, 317-321. Alan R. Liss, Inc., New York.

Heberer, G. (1959): Theorie der additiven Typogenese. In: G. Heberer (Hrsg.): Die Evolution der Organismen. Gustav Fischer Verlag, Stuttgart.

Herder/Spektrum (1994): Lexikon der Biologie. Spektrum Akademischer Verlag, Heidelberg.

Hitching, F. (1982): The Neck of the Giraffe. Where Darwin Went Wrong. Ticknor and Fields, New Haven and New York.

Hoff, P., Miram, W. und A. Paul (1999): Evolution, Materialien für die Sekundarstufe II, Biologie. Schroedel-Verlag, Hannover.

Holdredge, C. (2003): The Giraffe’s Short Neck. http://www.natureinstitute.org/pub/ic/ic10/giraffe.htm

Holdredge, C. (2004): The Giraffe in its World. http://natureinstitute.org/pub/ic/ic12/giraffe.htm#

Holt, J. (2005): Unintelligent design. The New York Times Magazine, 20 February 2005: http://www.nytimes.com/2005/02/20/magazine/20WWLN.html?_r=1

Hunt, K. (1999): Transitional Vertebrate Fossils FAQ. http://www.talkorigins.org/faqs/faq-transitional/part2c.html

Jacob, F. (1973/1987): Zitat nach B. Lewin (1987): Gene III, p. 397; Oxford University Press, Oxford.

Janis, C.M. (1984): Tragulids as Living Fossils. In: N. Eldredge and S.M. Stanley: Living Fossils, 87-94. Springer-Verlag, New York.

(P. 93: “The okapi, the rare forest-living giraffid, appears based on morphological evidence to be a very primitive member of the giraffoid radiation, although the living genus lacks a fossil record (Hamilton 1978). Since chromosomal evidence indicates that giraffids are the most primitive of the Eupecora (Todd 1975), *Okapia johnstoni* may well represent a true relict example.”)

Janis, C.M. (1986): Evolution of horns and related structures in hoofed mammals. Discovery **19**: 8-17. (Zitiert nach Solounias und Moelleken, 1991.)

Jehenne, Y. (1988): *Bedenomeryx* un nouveau genre de ruminant primitif de l'Oligocéne supérieur et du Miocène inférieur d'Europe. Comptes Rendus de l'Académie des Sciences Serie II, **307**:1991-1996.

Junker, R. (2005): Intelligent Design: http://www.genesisnet.info/pdfs/Intelligent_Design.pdf

Junker, R. und S. Scherer (2006): Evolution – Ein kritisches Lehrbuch. 6. Auflage. Weyel Lehrmittelverlag, Gießen.

Kaplan, E. L., Salti, G. I., Roncella, M., Fulton, N. and M. Kadowaki (2009): History of the Recurrent Laryngeal Nerve: From Galen to Lahey http://www.springerlink.com/content/13340521q5723532/fulltext.pdf.

Kearey, P. (Editor-in-Chief) (1993): The Encyclopedia of Solid Earth Sciences. Blackwell Scientific Publications, Oxford.

Kimani, J.K. (1981): Subendothelial fibrillar laminae in the carotid arteries of the giraffe *Giraffa camelopardalis*. Cell and Tissue Research **219**: 441-443.
(“The endothelium in the carotid arteries of the giraffe [G. camelopardalis] lies on an extensive basement membrane complex that has a distinctly fibrillar component. Small electron-dense areas occur in the basal portion of the

endothelium and normally in apposition to the fibrillar laminae of the basement membrane. These zones apparently constitute hemidesmosomes where endothelial attachment may be most tenacious.”)

Kimani, J.K. (1983): The structural organization of the tunica intima in the carotid arteries of the giraffe *Giraffa camelopardalis*. African Journal of Ecology **21**: 309-316.

(“The structural organization of the tunica intima in the carotid arterial system of the giraffe (G. camelopardalis) was investigated at various levels from the root of the neck to the base of the skull. **A prominent elastic zone** was demonstrated in the bicarotid trunk and caudal carotid segment between the endothelium and the internal elastic lamina. **Branching and bifurcation areas show highly organized intimal thickenings both in the adult and fetal carotid arteries.** The structural features reported in this study constitute morphological adaptations of the tunica intima to elevated shearing forces resulting from the relatively high blood pressure of this animal.”)

Kimani, J.K. (1983): The structural organization of the carotid arterial system of the giraffe *Giraffa camelopardalis*. African Journal of Ecology **21**: 317-324.

(“Carotid arteries in the giraffe were removed and their structural organization studied with light microscopy. **The carotid artery has a largely muscular structure in the adult** except for the carotid baroreceptor area located at the carotid-occipital junction. The fetus carotid has an elastic structure in its caudal portion and a musculo-elastic structure in its cranial portion. **Apparently, the muscular structure of the carotid arterial system constitutes part of the structural mechanism by which this animal autoregulates its cranial circulation, particularly when it bends the neck down.** Pertinent to this suggestion is the fact that the elevated distal carotid pressure does not cause a significant increase in cerebral blood flow.”)

Kimani, J.K. (1987): Structural organization of the vertebral artery in the giraffe *Giraffa camelopardalis.* Anatomical Record **217**: 256-262.

(“It is suggested that the vertebral and carotid arteries though found in the neck have a different structural organization, which, in turn, may imply that the two blood vessels are subjected to different hemodynamic demands. The findings of this study therefore render further support to the statement that **the largely muscular structure of the common carotid artery in the giraffe may be related to the unique cerebral hemodynamics of this animal**, rather than being a common feature of the blood vessels situated in the neck.”)

Kimani, J.K. and J.M. Mungai (1983): Observations on the structure and innervation of the presumptive carotid sinus area in the giraffe *Giraffa camelopardalis*. Acta Anatomica **115**: 117-133.

(“The tunica media of the giraffe carotid artery is characterized by a predominantly muscular structure, except for a small area at the origins of the occipital and the vestigal internal carotid arteries. The latter has a preponderantly elastic structure and corresponds in extent to the parts of the carotid arterial wall innervated by a branch of the glossopharyngeal nerve. Sensory nerve terminals, characterized by an abundance of mitochondria, are found in the deeper parts of the adventia bordering the elastic zone. Apparently, the elastic zone at the cranial portion of the carotid arterial system in the giraffe constitutes the equivalent of a carotid sinus. **The preponderance of elastic tissue in this area may be a morphological adaptation of the arterial wall to a baroreceptor function.** Pertinent to this suggestion is the demonstration of a close structural relationship between the sensory nerve endings and the elastic fibers.”)

Kimani, J. K. and I.O. Opole (1991): The structural organization and adrenergic innervation of the carotid arterial system of the giraffe *Giraffa camelopardalis.* Anatomical Record **230**: 369-377.

(“The symapthetic innervation of the giraffe (**Giraffa** camelopardalis) carotid arterial system is described in this study using the sucrose-potassium phosphate-glyoxylic acid (SPG) method. The brachiocephalic and bicarotid trunks showed a paucity of sympathetic innervation. Smooth muscle nets observed in the outer layers of the tunica media in these arteries revealed a rich network of sympathetic nerve fibres. The common carotid artery showed numerous sympathetic nerve fibres particularly in the outer muscular zone of the tunica media. The internal maxillary, ramus anastomoticus, and arteria anastomotica also revealed a rich sympathetic innervation and a deep penetration of the nerve fibres into the tunica media. **It is suggested that the rich sympathetic innervation of the giraffe carotid arteries maintains a basal tonic state in the smooth muscle in the tunica media.** This, in turn, may enable the animal to maintain a relatively high rate of blood flow in the carotid arteries in diastole despite the pressure run-off. It is further suggested that the muscular structure and dense sympathetic innervation of the internal maxillary and its branches to the carotid rete mirabile provide the animal with an array of mechanisms to modulate its cranial circulation particularly when it bends its head to drink.”)

Kimani, J.K., Opole, I.O. and J.A.O’Ogeng (1991): Structure and sympathetic

innervation of the intracranial arteries in the giraffe *Giraffa camelopardalis.* Journal of Morphology **208**: 193-204.

("Fluorescence histochemistry discloses that the carotid rete mirabile in the giraffe has a poor sympathetic innervation. In contrast, the efferent artery of the rete (internal carotid artery) and the cerebral arteries show moderate sympathetic innervation. A certain degree of regional variability was noted in which the rostral arteries (anterior and middle cerebral) receive more sympathetic nerves than the caudal (posterior communicating and basilar) arteries. **The sympathetic nerves on the giraffe cerebral vessels may constitute part of a host of mechanisms by which regional blood flow to the brain is regulated.** Conversley, the paucity of sympathetic innervation of the carotid rete mirabile may indicate that this structure does not play an active role in vasoconstrictor responses during postural changes of the head.")

King James Version (1611) of: The Holy Bible containing the Hebrew and Greek Scriptures. Publishers: Watchtower Bible and Tract Society, Inc. Brooklyn, New York (ohne Jahresangabe).

Kollmann, M. (1999): Strukturelle und sedimentologische Prozesse der Entwicklung eines Foreland-Basins. Das Zinda-Pir-Antiklinorium und die Sulaiman Range, Pakistan. Dissertation. Göttingen.

Kotyk, M.E., Basinger, J.F., Gensel, P.G. and T.A. de Freitas (2002): Morphologically complex plant macrofossils from the Late Silurian of Arctic Canada. American Journal of Botany **89**:1004-1013. (The entire article is readable at: http://www.amjbot.org/cgi/content/full/89/6/1004)

Kuhn, O. (1951): Die Deszendenztheorie. 2. Auflage. Koesel-Verlag, München.

Kuhn, O. (1965): Die Abstammungslehre. Tatsachen und Deutungen. Verlag Oeben, Krailling bei München.

Kuhn, O. (1981): Die Evolution. Ergebnisse und Probleme. Verlag Gebr. Geiselberger, Altötting.

Krumbiegel, I. (1971): Die Giraffe. A. Ziemsen Verlag, Wittenberg.

Kutschera, U. (2001/2006): Evolutionsbiologie. Eine allgemeine Einführung. Paul Parey Verlag, Berlin (1. Auflage) und Eugen Ulmer Stuttgart (2.Auflage).

Kutschera, U. (2004): Streitpunkt Evolution. Darwinismus und Intelligentes Design. Lit Verlag, Münster.

Kutschera, U. (2005): Kommentar zum Film von Fritz Poppenberg *Hat Die Bibel doch recht?* in Nano/3SAT.

(29. 11. 2005 in 3SAT u.a. zum Ursprung der Giraffe. The station gives the following information, http://www.3sat.de/3sat.php?http://www.3sat.de/nano/bstuecke/86192/index.html but not the text itself of the interview. The quotation reproduced here is an exact account in accordance with a recording of the transmission.)

Lang, T. von and W. Wachsmuth (1985): Praktische Anatomie. Kopf, Teil A, Übergeordnete Systeme; von Johannes Lang in Zusammenarbeit mit H.-P. Jensen und F. Schröder. Springer-Verlag, Berlin, Heidelberg.

Lankester, E.R. (1891): Zitiert nach: http://www.nceas.ucsb.edu/~alroy/lefa/Lankester.html

Lankester, E.R. (1901): On Okapia johnstoni. Remarks on "Two skulls and the skin of the new mammal, the Okapi, discovered by Sir Harry Johnston, K.C.B., F.Z.S." etc., Proc. Zool. Soc. Lond. (**2**?): 279-281.

Lankester, E.R. (1907): The origin of the lateral horns of the giraffe in foetal life on the area of the parietal bones. Proc. Zool. Soc. Lond.: 100-115 (Reference according to

Colbert1935, and Mitchel and Skinner 2003).

Lankester, R. (1908): On certain points in the structure of the cervical vertebrae of the okapi and the giraffe. Proc. Zool. Soc. Lond. 1908: 325-334.

Leakey, L.S.B. (1970): Additional Information on the status of *Giraffa jumae* from East Africa. Fossil Vertebrates of Afrika **2**: 325-330 (plus 13 pages for plates).

Le Pendu Y. and I. Ciofolo (1999): Seasonal movements of giraffes in Niger. Journal Of Tropical Ecology **15**: 341-353.

Lönnig, W.-E. (1989/2002): Auge - widerlegt Zufalls-Evolution. 2. Auflage. Naturwissenschaftlicher Verlag Köln (Internet edition 2002).

Lönnig, W.-E. (1991): Kann der Neodarwinismus durch biologische Tatsachen widerlegt werden? Naturwissenschaftlicher Verlag, Köln. (Hier speziell: http://www.weloennig.de/NeoB.Ana4.html)

Lönnig, W.-E. (1993/2003): Artbegriff, Evolution und Schöpfung. 3. Auflage 622 pp.; Internet Edition 2003. Köln.

Lönnig, W.-E. (1995): Mutationen: Das Gesetz der rekurrenten Variation. In: J. Mey, R. Schmidt und S. Zibulla (eds.): Streitfall Evolution, 149-165. Universitas, Wissenschaftliche Verlagsgesellschaft, Stuttgart (Internet edition 2002).

Lönnig, W.-E. (2001): Natural selection. In: W.E. Craighead and C.B. Nemeroff (eds.): The Corsini Encyclopedia of Psychology and Behavioral Sciences (3rd ed), 1008-1016. John Wiley and Sons, New York, Vol. 3.

Lönnig, W.-E. (2004): Dynamic genomes, morphological stasis, and the origin of irreducible complexity. In: V. Parisi, V. De Fonzo and F. Aluffi-Pentini (eds): Dynamical Genetics, 101-119. Research Signpost, Trivandrum, India.

Lönnig, W.-E. (2005): Mutation breeding, evolution, and the law of recurrent variation (Recent Research Developments in Genetics and Breeding **2**: 45-70. (Detailed/itemized version) http://www.weloennig.de/Loennig-Long-Version-of-Law-of-Recurrent-Variation.pdf

Lönnig, W.-E. (2006): Mutations: the law of recurrent variation. In: J.A. Teixeira da Silva (ed.): Floriculture, Ornamental and Plant Biotechnology: Advances and Topical Issues, Vol. 1, 601-607. Global Science Books, London. [Peer reviewed condensed version: "All GBS Books and Journals are internationally peer-reviewed."])http://www.weloennig.de/ShortVersionofMutationsLawof_2006.pdf

Lönnig, W.-E. (2007): Biodiversity and Dollo's law: To what extent can the phenotypic differences between *Misopates orontium* and *Antirrhinum majus* be bridged by mutagenesis? Bioremediation, Biodiversity and Bioavailability **1**: 1-30.

Luskin, C. (2007): http://www.evolutionnews.org/2007/03/the_truth_about_haeckels_embry.html

Maniou, Z., Wallis, O.C. and M. Wallis (2004): Episodic molecular evolution of pituitary growth hormone in cetartiodactyla. Journal of Molecular Evolution **58**: 743-753.
(“The sequence of growth hormone (GH) is generally strongly conserved in mammals, but episodes of **rapid change**

occurred during the evolution of primates and artiodactyls [evolutionäre Interpretation der unerwarteten Unterschiede], when the rate of GH evolution apparently increased substantially. As a result the sequences of **higher primate and ruminant GHs differ markedly from sequences of other mammalian GHs**. In order to increase knowledge of GH evolution in Cetartiodactyla (Artiodactyla plus Cetacea) we have cloned and characterized GH genes from camel (*Camelus dromedarius*), hippopotamus (*Hippopotamus amphibius*), and giraffe (*Giraffa camelopardalis*), using genomic DNA and a polymerase chain reaction technique. As in other mammals, these GH genes comprise five exons and four introns. **Two very similar GH gene sequences (encoding identical proteins) were found in each of hippopotamus and giraffe.** The deduced sequence for the mature hippopotamus GH **is identical to that of dolphin**, in accord with current ideas of a close relationship between Cetacea and Hippopotamidae. The sequence of camel GH is identical to that reported previously for alpaca GH. **The sequence of giraffe GH is very similar to that of other ruminants but differs from that of nonruminant cetartiodactyls at about 18 residues.** The results demonstrate that the apparent burst of rapid evolution of GH occurred largely after the separation of the line leading to ruminants from other cetartiodactyls. “)

Marcon, E. und M. Mongini (1988): Die Grosse Enzyklopädie der Tierwelt. Neuer Kaiser-Verlag, Klagenfurt.

May, M. T. (1968): Galen on the Usefulness of Parts of the Body. Translated from the Greek with an Introduction and Commentary by Margaret Tallmadge May. Two Volumes. Cornell University Press, Ithaca, NewYork (802 pp.).

Mayr, E. (1967): Artbegriff und Evolution. Verlag Paul Parey, Hamburg.

Mayr, E. (1970): Populations, Species and Evolution. The Belknap Press of Harvard University Press, Cambridge, Mass.

McGowan, C. (1991): Dinosaurs, Spitfires, and Sea Dragons. Harvard University Press. Cambridge, Massachusetts.

McKenna, M.C. and S.K. Bell (1997/2000): Classification of mammals above the species level. Columbia University Press, New York. Paperback edition August 2, 2000.

Meis, F.: Verteidigung der Wahrscheinlichkeitsrechnung http://www.intelligentdesigner.de/ Teil 1 und mit spezieller Linkadresse Teil 2 (http://www.intelligentdesigner.de/Wahrscheinlichkeit2.html).

Metcalf, J. (Lektoratsleitung) (2004): Säugetiere. Dorling Kindersley, London. (P. 37 of *Helladotherium*: „In comparison to today's giraffes, this prehistoric animal had a short neck and short legs.“)

Meyer, S.C. (2004) The origin of biological information and the higher taxonomic categories. Proceedings of the Biological Society of Washington **117**: 213-239.

Milner, R. (1999): Huxley’s bulldog: the battles of E. Ray Lankester (1846-1929). The Anatomical Record (New Anat.) **257**: 90-95. (http://www3.interscience.wiley.com/cgi-bin/fulltext/68000678/HTMLSTART)

Milton, R. (2005): The Alternative Science Website: Kritik zu K. Hunt: http://www.alternativescience.com/talk.origins-transitions.htm

Mitchell, G. and J.D. Skinner (2003): On the origin, evolution and phylogeny of giraffes Giraffa camelopardalis. Trans. Roy. Soc. S. Africa 58: 51-73.

Mitchell, G., van Sittert, S.J. and J.D. Skinner (2009): Sexual selection is not the origin of long necks in giraffes. Journal of Zoology **278**: 281-286. (“The evolutionary origin of the long neck of giraffes is enigmatic. One theory (the 'sexual selection' theory) is that their shape evolved because males use their necks and heads to achieve sexual dominance. Support for this theory would be that males invest more in neck and head growth than do females. We have investigated this hypothesis in 17 male and 21 female giraffes with body masses ranging from juvenile to mature animals, by measuring head mass, neck mass, neck and leg length and the neck length to leg length ratio. We found no significant differences in any of these dimensions between males and females of the same mass, although mature males, whose body mass is significantly (50%) greater than that of mature females, do have significantly heavier (but not longer) necks and heavier heads than mature females. We conclude that morphological differences between males and females are minimal, that differences that do exist can be accounted for by the larger final mass of males and that sexual selection is not the origin of a long neck in giraffes.")

Müller, B. (2000): Das Glück der Tiere. Einspruch gegen die Evolutionstheorie. Alexander Fest Verlag, Berlin.
(Note: The work of Burkhard Müller *Das Glück der Tiere* shows contains a large number of critical points of the synthetic evolutionary theory that are very well thought-out and worth reading – as the quotation above on the topic of macromutations reveals. In some points, however, I cannot follow the author.)

Müller, H.J. (1962): Beobachtungen an Nerven und Muskeln des Halses der Tylopoden. Zeitschrift für Anatomie und Entwicklungsgeschichte **123**: 155-173. (Aus dem Dr. Senckenbergischen Anatomischen Institut der Universität Frankfurt a. M.; Direktor: Prof. Dr. F. Starck.)

Nelson, P. (2009): Jerry, PZ, Ron, faitheism, Templeton, Bloggingheads, and all that — some follow-up comments. (Quotation of Jerry Coyne's comment an the laryngeal nerve of the giraffe) http://www.uncommondescent.com/intelligent-design/jerry-pz-ron-faitheism-templeton-bloggingheads-and-all-that-some-follow-up-comments/

Nilsson, H. (1953): Synthetische Artbildung. Verlag CWK Gleerups, Lund.

Owen, R. (1841): Notes on the anatomy of the Nubian Giraffe. The Transactions of the Zoological Society of London **2**: 217-243.

Owen, R. (1868): On the Anatomy of Vertebrates. Vol. III. Mammals. Printed by Spottiswoods and Co., London.

Parker, D.M. (2004): The Feeding Biology and Potential Impact of Introduced Giraffe (*Giraffa camelopardalis*) in the Eastern Cape Province, South Africa [152 pp.]. Master of Science Thesis, Rhodes University. http://eprints.ru.ac.za/48/01/Dan-thesis.pdf

Parker, D.M., Bernard R.T.F. and S.A. Colvin (2003): The diet of a small group of extralimital giraffe. African Journal of Ecology **41**: 245-253.

Peachey, R. (2005): The giraffe: A favourite textbook illustration of evolutionary theories: Quotation of Lynn Hofland siehe: http://www.scq.ubc.ca/?p=158 (The article contains a number of precise considerations and arguments. However, I do not follow the creationist framework.)

Perloff, J. (2003): Tornado in a Junkyard. The Relentless Myth of Darwinism. Refuge Books, Burlington, Mass.
(James Perloff offers in his book *Tornado in a Junkyard* a number of very correct and clear critical points on the synthetic theory of evolution. Some of them I have quoted above. I do not share the author's creationist assumptions however.)

Pernkopf Anatomie: See W. Platzer below.

Petzsch, H. (1966/1974): Säugetiere Bd. 3. Das Urania Tierreich in 18 Bänden. Rowohlt Taschenbuchverlag, Reinbek bei Hamburg. (P. 413: „Compared to the long-necked giraffes, the **Okapi** is the **dwarf** among the giraffidae.")

Pienaar, U. De V. (1969): Predator-prey relationships among the larger mammals of the Kruger National Park. Koedoe **12**: 108-176 (zitiert nach Simmons and Scheepers).

Pincher, C. (1949): Evolution of the giraffe. Nature **164**: 29-30.

Platzer, W. (Ed.) (1987): Pernkopf Anatomie, Atlas der topographischen and angewandten Anatomie des Menschen. Herausgegeben von W. Platzer. 2 Bände. Urban & Schwarzenberg. München, Wien, Baltimore.

Poppenberg, F. (1998): Hat die Bibel doch recht? Der Evolutionstheorie fehlen die Beweise (English title: Is the Bible Right after All? The Theory of Evolution lacks Evidence). Drei Linden Filmproduktion, Berlin.

Preece, R. (2003): Darwinism, Christianity, and the Great Vivisection Debate. Journal of the History of Ideas **64**: 399-419 (http://courses.csusm.edu/hist460ae/humanevivi.pdf).

Probst, E. (o.J.) http://www.fortunecity.de/lindenpark/wittgenstein/30/RekordederUrzeit.html

Pschyrembel Klinisches Wörterbuch 2011. 262. Auflage, Redaktionsleitung Dr. Martina Bach. Verlag Walter de Gruyter, Berlin und New York. Quotation according the 255th edition 1986, p. 1156, bearbeitet von der Wörtrbuchredaktion unter der Leitung von Christoph Zink ("Rr. cardiaci inferiores, tracheales, oesophagei, pharyngei, bronchiales, N. laryngeus inf."); see also Benninghoff/Drenckhahn 2008, p. 231. (The new edition of Pschyrembel was abrreviated concerning this point and mentions only Rr. trachealis, oesophagei and phyryngei.)

Rammerstorfer, M.: http://rammerstorfer-markus.batcave.net/

Rammerstorfer, M.: (2004): Nervus laryngeus recurrens - suboptimal? http://members.liwest.at/rammerstorfer/NLrecurrens.pdf

Rammerstorfer, M. (2005): Die Kunst der Kritik nach dem Rezept der *AG Evolutionsbiologie*: http://rammerstorfer-markus.batcave.net/ArtofCrHaekRekFinal.pdf

Rammerstorfer, M. (2006): Nur eine Illusion? Biologie und Design. Tectum-Verlag, Marburg.

Rauber/Kopsch. Anatomie des Menschen. Lehrbuch und Atlas. Herausgegeben von **L. Leonhardt, B. Tillmann, G. Tönfury und K. Zilles**. Band I: Bewegungsapparat (Herausgegeben und bearbeitet von **B. Tillmann und G. Töndury** 1987); Band II: Innere Organe (**H. Leonhardt** 1987); Band III: Nervensystem und Sinnesorgane (**B. Krisch, S. Kubik, W. Lange, H. Leonhardt, P. Leuenberger, G. Töndury und K. Zilles** 1987); Band IV: Topographie der Organsysteme, Systematik der peripheren Leitungsbahnen (**H. Leonhardt, B. Tillmann und K. Zilles 1988**). Georg Thieme Verlag, Stuttgart und New York.

Sakall, S.E. (2006): http://www.sergiosakall.com.br/okapi/paleontologia.html Zu Giraffokeryx: “Nome Científico: ***Giraffokeryx punjabiensis***. Época: Plioceno. Local onde viveu: no leste da África. **Peso: Cerca de 150Kg. Tamanho: 1,6m de altura**.“ (The specifications should be checked; the portrayal of *Honanotherium* should be corrected.)

Salvini-Plawen, L. v. and E. Mayr (1977): On the evolution of photoreceptors and eyes. *In*: M.K. Hecht, W.C. Steere and B. Wallace (eds.): Evolutionary Biology. Vol. **10**, pp. 207 - 263. New York and London.

Sanford, J.C. (2005): Genetic Entropy and the Mystery of the Genome. Ivan Press, Lima, New York.

Sasaki, M., Endo, H., Kogiku, H., Kitamura, N., Yamada, J., Yamamoto, M., Arishima, K. and Y. Hayashi (2001): The structure of the masseter muscle in the giraffe (*Giraffa camelopardalis*). Anatomia Histologia Embryologia. Journal of Veterinary Medicine **30**: 313-319.
(“In the giraffe (*Giraffa camelopardalis*), the masseter muscle was divided into several layers. The superficial and more medial (second) tendinous sheets of the masseter muscle fused with each other at the dorso-caudal part and a fleshy portion was located between these tendinous sheets. In the rostral part, the most superficial tendinous sheet turned around as a compact tendon and attached to the facial crest (Crista facialis). The turned tendinous sheet, however, never fused with the second tendinous sheet and this layer of the masseter muscle, that originated from the facial crest with the turned sheet, was inserted into the mandible with its fleshy portion. In the cattle, goat, sheep and Sika deer, the rostral layer of the masseter muscle arises from the facial crest with its fleshy portion and is inserted into the tubercle on the mandible through the strong tendinous sheet. In this study, the takin also showed the same structure of the masseter muscle**. In the giraffe, however, the rostral layer inserted into the mandible through the strong tendinous sheet could not be distinguished, thus, there was no conspicuous tubercle on the mandible**. Moreover in the masseteric region of the skull, the giraffe was similar to the Sika deer in several ways. However, it is suggested that the giraffe exerts smaller forces on the cheek teeth than does the Sika deer because of its longer Margo interalveolaris.”)

Savage, R.J.G. and M.R. Long (1986): Mammal Evolution – An Illustrated Guide. British Museum (Natural History), London. (P. 228: „GIRAFFIDS The first pecorans appear in the early Miocene of Europe and Africa and are difficult to assign to family status, hence **the origin of the three main lineages (giraffes, deer and cattle) remains obscure.**”)

Scherer, S. (Hrsg.) (1993): Typen des Lebens. Pascal Verlag, Berlin.

Schmidt, F. (1985): Grundlagen der kybernetischen Evolution. Goecke und Evers, Krefeld.

Schmidt, F. (1988): Neodarwinistische oder kybernetische Evolution? Bericht über ein internationales Symposium vom 15.-17. July in Heidelberg. Universitätsdruckerei Heidelberg.

Schützenberger, M.-P. (1996): The miracles of Darwinism. http://www.arn.org/docs/odesign/od172/schutz172.htm

Senter, P. (2007): Necks for sex: sexual selection as an explanation for sauropod dinosaur neck. Journal of Zoology **271,** 45–53.
(The example of the interpretation of the origin of the long-necked giraffe by sexual selection according to Simmons and Scheepers seems to have gained some acceptance; however, the fundamental objections described above in detail appear to be also true for the work of Senter on dinosaurs (cf. among others pp. 22 and 32 above): “Abstract. The immensely long neck of a sauropod is one of the most familiar and striking of anatomical specializations among dinosaurs. Here, I use recently collected neontological and paleontological information to test the predictions of two competing hypotheses proposed to explain the significance of the long neck. **According to the traditional hypothesis, neck elongation in sauropods increased feeding height**, thereby reducing competition with contemporaries for food. According to the other hypothesis, which is advanced for the first time here, neck elongation in sauropods was driven by sexual selection. Available data match the predictions of the sexual selection hypothesis and contradict the predictions of the feeding competition hypothesis. It is therefore more plausible that increases in sauropod neck lengths were driven by sexual selection than by competition for foliage.”)

Sewell, G. (2000): A mathematician’s view of evolution. The Mathematical

Intelligencer **22**:5-7.

Sherr, L. (1997): Tall Blondes. A Book about Giraffes. Andrews McMeel Publishing, Kansas City.

Siewing, H. (1985): Lehrbuch der Zoologie. Bd. 2. Systematik. Gustav Fischer Verlag, Stuttgart.

Simmons, R.E. and L. Scheepers (1996): Winning a neck: sexual selection in the evolution of giraffe. Am. Nat. **148**: 771-778.

Simpson, G.G. (1944): Zitiert nach E. Mayr *Artbegriff und Evolution* (1967).
Ernst Mayr shows us the meaning of gradual evolution for the concrete paleontological case of the rate of increase of teeth length in horse evolution in 'Selection pressures in equilibrium' 1967, p. 193: "…actually the extent of its increase amounted to only some **1mm per million years** (Simpson 1944)." - cf also Lönnig 1993, p. 448.

Solounias, N. (1988): Prevalence of ossicones in Giraffidae (Artiodactyla, Mammalia). J. Mamm. **69**: 845-848.

Solounias, N. (1999): The remarkable anatomy of the giraffe's neck. J. Zool., Lond. **247**: 257-268.

Solounias, N. (2001): Giraffe. McGraw-Hill Yearbook of Science and Technology, 174-175.

Solounias, N. (2007): Family Giraffidae. *In*: D.R. Prothero and S.E. Foss (eds.): The Evolution of Artiodactyls, 257-277. The Johns Hopkins University Press. Baltimore, Maryland. (P. 258: "*Samotherium* and *Palaeotragus* are envisioned as similar to the okapi with short necks and simple ossicones.")

Solounias, N. and S.M.C. Moelleken (1991): Evidence for the presence of ossicones in *Giraffokeryx punjabensis* (Giraffidae, Mammalia). J. Mamm. 72: 215-217.

Solounias, N., Placvan, J.M., Quade, J. and L. Witmer (1998): The paleoecology of the Pikermian Biome and the savanna myth. *In*: J. Agusti, L. Rook and P. Andrews (eds.): Hominoid Evolution and Environmental Change during the Neogene of Europe, 436–453. Cambridge University Press, Cambridge.

Spetner, L. (1997): Not by Chance! Shattering the Modern Theory of Evolution. The Judaica Press, Brooklyn, N.Y.

Starck, D. (1978): Vergleichende Anatomie der Wirbeltiere auf evolutionsbiologischer Grundlage: Theoretische Grundlagen. Stammesgeschichte und Systematik unter Berücksichtigung der niederen Chordata. Bd. 1. Springer-Verlag, Berlin.

Starck, D. (1995): Lehrbuch der Speziellen Zoologie. Bd. II: Wirbeltiere. 5. Teil: Säugetiere. Gustav Fischer Verlag, Jena.

Storch, V. und U. Welsch (1991): Systematische Zoologie. 4. Auflage. Gustav Fischer Verlag, Stuttgart.

Stucky, J.D. and B.J. McKenna: (1993): Mammalia. In: M.J. Benton: The Fossil

Record 2, 739-771. Chapman and Hall, London.

Taylor, D. (1980): Zwei Meter Halschmerzen (die englische Originalausgabe erschien 1980 unter dem Titel *Going Wild, more Adventures of a Zoo Vet* im Verlag George Allen & Unwin, London). Bertelsmann, Güterloh.
(The veterinarian vividly describes his difficulties during operations on giraffes due to their special blood circulationl. After an unsuccessful operation and the death of one of the animals, he comments, after a detailed description of the case (p. 61): „I drove home desperate. Only a fool of a zoo veterinarian could fight death [in the giraffe]. Pigs, cats and cows are okay, but giraffes are very problematic.“ This is a backtranslation from the German edition and the wording will most probably be different in the English original book.)

Taylor, G.R. (1983): Das Geheimnis der Evolution. S. Fischer Verlag, Frankfurt am Main. (Die englische Originalauflage ist unter dem Titel *The Great Evolutionary Mystery* 1983 im Verlag Secker und Warburg in London erschienen.)

The New Shorter Oxford English Dictionary on Historical Principles, edited by Lesley Brown, 2 Vol. 1993. Oxford University Press, Oxford.

Thenius, E. (1972): Grundzüge der Verbreitungsgeschichte der Säugetiere. Eine historische Tiergeographie. Gustav Fischer Verlag, Stuttgart.
(P. 101: „In the early Pliocene the Giraffids (Fam. Giraffidae) were widespread in Africa and Europe, with the Okapi-like Palaeotraginae (***Palaeotragus*** [=“*Achtiaria*“], ***Samotherium***). They were **short-legged, short-necked giraffes** with simple paired skull protrusions.“)

Thenius, E. (1970/1979/2000): Stammesgeschichte zum 9. Kapitel (Giraffen) von Bd. 13, Säugetiere 4, 255-274 (p. 255) von Grzimeks Tierleben, Enzyklopädie des Tierreichs. Deutscher Taschenbuchverlag, München.

The Paleobiology Database: http://paleodb.org/cgi-bin/bridge.pl und http://paleodb.org/cgi-bin/bridge.pl?action=displayCollectionDetails&collection_no=11797

Thompson, W.-R. (1967): Introduction to Charles Darwin: The Origin of Species. Everyman's Library No. 811, reprint of the sixth edition of 1872.

Tosaw, D. J. Jr. (2002): Evolution happens.
http://www.evolutionhappens.net/giraffes1sdz2002.htm

Troll, W. (1984): Gestalt und Urbild. Gesammelte Aufsätze zu Grundfragen der organischen Morphologie. 3. Auflage mit einer Vorbemerkung und einem Nachwort herausgegeben von Lottlisa Behling. Böhlau Verlag, Köln und Wien.

Tsujikawa, H. (2005): The updated late Miocene large mammal fauna from Sambura Hills, Northern Kenya. African Study Monographs. Suppl. 32: 1-50.

Uludag, M., Isgor, A., Yetkin, G. and B. Citgez (2009): Anatomic variations of the non-recurrent inferior laryngeal nerve. BMJ Case Reports 2009; doi:10.1136/bcr.10.2008.1107 http://casereports.bmj.com/content/2009/bcr.10.2008.1107.full

Vermeesch, J.R., DeMeurichy, W., VandenBerghe, H., Marynen, P. and P. Petit (1996): Differences in the distribution and nature of the interstitial telomeric (TTAGGG)(n) sequences in the chromosomes of the Giraffidae, okapi (Okapia johnstoni), and giraffe (Giraffa camelopardalis): Evidence for ancestral telomeres at the okapi polymorphic rob(4;26) fusion site. Cytogenet. Cell Genet. **72**: 271-293 und

310-315.
(“Intrachromosomal telomeric sequences (TTAGGG)n were analyzed in the two members of the family Giraffidae, the giraffe and the okapi. The giraffe has a diploid chromosome number of **2n = 30,** whereas the okapi chromosome number varies from **2n = 46 to 2n = 45 and 2n = 44** due to a "recent" Robertsonian fusion event.”)

Vermeulen, H. A. (1916): The vagus area in camelopardalis giraffe. Proceedings of the Koninklijke Nederlandse Akademie van Wetenschappen **18**: 647-670.

Wahrig, G. (1968/1975): Deutsches Wörterbuch. Bertelsmann Lexikon-Verlag, Gütersloh.

Wells, J. (2006): The Politically Incorrect Guide to Darwin and Intelligent Design. Regnery Publishing, Inc., Washington, DC.

Wesson, R. (1991): Die unberechenbare Ordnung. Artemis & Winkler-Verlag, München.

White, M.E. (1990): The Flowering of Gondwana. Princeton University Press. Princeton, New Jersey.
(Pp. 67/68: “*Baragwanathia* is now recognised as being a Lycopod, derived from the Zosterophylls. It is similar in organisation and structure to living Lycopods, especially *Lycopodium squarrosum*. **Because of its high degree of specialisation, there has been heated controversy over the age of the beds in which it first occurs.** The sequence of strata is continuous from the Silurian to the Early Devonian. At Yea *Baragwanathia* occurs with Rhyniophytes and a Zosterophyll, and with Graptolites (which are Invertebrates). The Graptolite has been used to correlate the fossil horizon with the Ludlow Division of the Silurian in Wales and elsewhere in the world. Until recently, only poorly preserved examples of the Graptolite were found and the correlation was considered to be dubious. Recent evidence from the study of excellently preserved examples **confirms the Late Silurian age** for the Yea locality.

The very advanced appearance of *Baragwanathia* compared with that of the Zosterophylls from which it has evolved was the cause of the scepticism about the Late Silurian date. It is, however, becoming clear with the increasing volume of information on land-plant spores and fragments in pre-Late Silurian rocks that the vascular plants may indeed date back further than had been imagined. This greater age would render the degree of specialisation reached at such an early date less surprising.” – The derivation of *Baragwanathias* from Zosteropyllaceae is by the way unproven und ob die Graptoliten anfangs nur “poorly preserved” waren, wäre noch genauer zu untersuchen.)

Wikipedia (2007): Giraffe. http://en.wikipedia.org/wiki/Giraffa_camelopardalis

Willemse, J. J. (1958): The innervation of the muscles of the trapezius-complex in Giraffe, Okapi, Camel and Llama. Archives Néerlandaises de Zoologie **12**: 532-536.

Wittlich, K. (1991): Über die Wahrscheinlichkeit der zufälligen Entstehung brauchbarer DNA-Ketten. http://www.weloennig.de/NeoD.html

Wittlich, K. (1991): Häufige Einwände zur Wahrscheinlichkeitsrechnung. http://www.weloennig.de/NeoD2.html

Wood W.F. and P.J. Weldon (2002): The scent of the reticulated giraffe (*Giraffa camelopardalis reticulata*). Biochemical Systematics and Ecology **30**: 913-917.
(**The giraffe even displays extraordinary chemical compounds in its scent** (p. 913): **“**The giraffe (*Giraffa camelopardalis*) emits a scent that can be detected by humans over considerable distances. Dichloromethane extracts of hair samples from adult male and female reticulated giraffes (G. c. reticulata) were analysed by gas chromatography-mass spectrometry. Two highly odoriferous compounds, indole and 3-methylindole, identified in these extracts appear to be primarily responsible for the giraffe's strong scent. Other major compounds identified were octane, benzaldehyde, heptanal, octanal, nonanal, p-cresol, tetradecanoic acid, hexadecanoic acid, and **3,5-androstadien-17-one; the last compound has not previously been identified from a natural source.** These compounds may deter microorganisms or ectoparasitic arthropods. Most of these compounds are known to possess bacteriostatic or fungistatic properties against mammalian skin pathogens or other microorganisms. The levels of p-cresol in giraffe hair are sufficient to repel some ticks.”)

Woolnough, A.P. and J.T. du Toit (2001): Vertical zonation of browse quality in tree canopies exposed to a size-structured guild of African browsing ungulates. Oecologia **129**: 585-590.

Yang, Y.S., No, S.K., Choi, S.C. and K.H. Hong (2009): Case of coexisting, ipsilateral nonrecurrent and recurrent inferior laryngeal nerves. The Journal of Laryngology & Otology **123**: http://journals.cambridge.org/action/displayAbstract?fromPage=online&aid=5868576

Young, T.P. and L.A. Isbell (1991): Sex differences in giraffe feeding ecology: energetic and social constraints. Ethology **87**: 79-89.

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