Wolf-Ekkehard Lönnig

The Evolution of the Long-Necked Giraffe

(*Giraffa camelopardalis* L.)

What Do We Really Know?

Testing the Theories of Gradualism, Macromutation, and Intelligent Design

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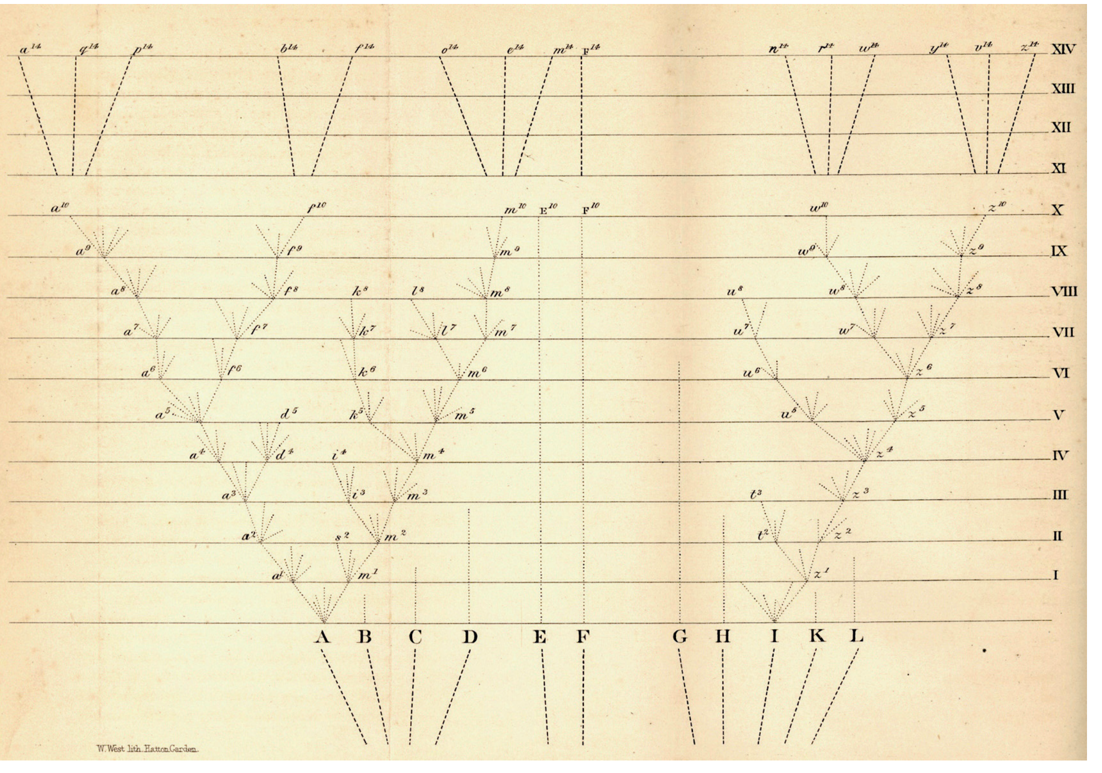
A Scientific Treatise

Wissenschaftliches Sachbuch

The Evolution of the Long-Necked Giraffe



Darwin's only figure in the *Origin*.* Concerning our topic we may apply this diagram to the evolution of the long-necked giraffes from a short-necked form. Thus, we could designate a short-necked giraffe for A and postulate **some 6,000 (punctuated equilibrium) to 1,000,000 generations (gradualism)** leading to the long-necked extant giraffes a^{14}, but q^{14} and p^{14} may designate long-necked giraffes, which died out recently like *Giraffa jumae* (for the numbers of generations, see the discussion in the present publication on p. 129). **Facts or hypotheses?**

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*In the *Origin* (1859, 6th edition 1872 with slight variations), Darwin explained his diagram at length (about 8 pages). To quote a few points (emphasis added):

"Let A to L represent the species of a genus large in its own country; these species are supposed to resemble each other in unequal degrees, as is so generally the case in nature, and as is represented in the diagram by the letters standing at unequal distances.
[...] The intervals between the horizontal lines in the diagram, may represent each a thousand generations; **but it would have been better if each had represented ten thousand generations**. [In 1872 abbreviated to "each a thousand **or more** generations."]
[...] After ten thousand generations, species (A) is supposed to have produced three forms, a^{10}, f^{10}, and m^{10}, which, from having diverged in character during the successive generations, will have come to differ largely, but perhaps unequally, from each other and from their common parent. [...] By continuing the same process for a greater number of generations (as shown in the diagram in a condensed and simplified manner), we get eight species, marked by the letters between a^{14} and m^{14}, all descended from (A). Thus, as I believe, **species are multiplied and genera are formed**.
[...] After fourteen thousand generations, six new species, marked by the letters n^{14} to z^{14}, are supposed to have been produced.
[...] In the diagram, each horizontal line has hitherto been supposed to represent a thousand generations, **but each may represent a million or hundred million generations**, and likewise a section of the successive strata of the earth's crust including extinct remains.
[...] I see no reason to limit the process of modification, as now explained, to the formation of genera alone. If, in our diagram, we suppose the amount of change represented by each successive group of diverging dotted lines to be very great, the forms marked a^{14} to p^{14}, those marked b^{14} and f^{14}, and those marked o^{14} to m^{14}, will form three very distinct genera. We shall also have two very distinct genera descended from (I); and as these latter two genera, both from continued divergence of character and from inheritance from a different parent, will differ widely from the three genera descended from (A), **the two little groups of genera will form two distinct families, or even orders**, according to the amount of divergent modification supposed to be represented in the diagram. And the two new families, or orders, will have descended from two species of the original genus; and these two species are supposed to have descended from one species of a still more ancient and unknown genus. (For the full context, see: http://darwin-online.org.uk/content/frameset?viewtype=side&itemID=F373&pageseq=133)

Evolution: Not a Fact According to Darwin Himself

The renowned German botanist Robert Caspary interviewed Darwin on the 27th of May 1866* on the question whether he thought that evolution would be either a fact or a hypothesis. This is what Caspary reported about that interview:

"It was important for me to hear, from his own mouth, if he considered his doctrine of evolution of species to be hypothesis or fact. I asked him if he considered that he had ever found a species anywhere for which it could be established through facts that it was derived from another by changes. – No! he answered very definitively. – Thus you yourself consider that your doctrine of evolution of species is a hypothesis. – O yes! was his decisive answer."**

Interview with Darwin by Robert Caspary 1866/1882, p. 778.

Botanische Zeitung 40: see http://www.biodiversitylibrary.org/item/104941#page/439/mode/1up

"Caspary, Johann Xaver Robert (Robert) (1818-1887), German botanist. Director, Bonn herbarium, 1856. Professor of botany and director of the botanic gardens at the University of Königsberg from 1858. Specialised in water plants." Burkhard, F. et al., eds. 2004, p. 502. Concerning Caspary's paleontological work, his zoological studies as well as the "Casparian strips" etc., see http://de.wikipedia.org/wiki/Robert_Caspary

Here is the original German text from *Botanische Zeitung*:

bringen. Es war mir wichtig, aus seinem eigenen Munde zu hören, wie er seine Lehre über die Abänderung der Arten auffasste, ob als Hypothese oder Thatsache. Ich fragte ihn: ob er meine, irgendwo eine Art gefunden zu haben, für die es durch Thatsachen festgestellt sei, dass sie aus einer anderen durch Abänderung hervorgegangen sei. — Nein! antwortete er sehr bestimmt. — Also halten Sie selbst Ihre Lehre von der Abänderung der Arten für eine Hypothese — Ja wohl! (O yes!) lautete die entschiedene Antwort.

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* "... Hooker was called upon to act as intermediary between Darwin and the German botanist Robert Caspary, who wished to visit Down in May: 'ask him by all means to come & sleep here, if he has spare time, but at same time tell him the truth how little exertion I can stand. I should like very much to see him, though I dread all exertion' (letter to J. D. Hooker, [12 May 1866]). Darwin's interest in Caspary's research on graft hybrids and self-pollinating waterlilies prevailed over considerations of health in this case."
http://www.darwinproject.ac.uk/correspondence-volume-14
http://www.darwinproject.ac.uk/entry-5026#back-mark-5026.f6

See (also) The Correspondence of Charles Darwin, Vol 14, 1866.
Cambridge University Press 2004
Burkhardt, F. et al., eds.

**As translated by Professor Granville Sewell, University of Texas, El Paso.

Wolf-Ekkehard Lönnig (Ph.D. Genetics, University of Bonn) is a transposon and mutation geneticist who has spent more than 30 years of experimental research at the Institute of Genetics of the University of Bonn (7 years) and the Max-Planck-Institute of Plant Breeding Research (altogether over 25 years, now retired). He has given talks and seminars at several Universities on the *Origin of Species – Evolution and Intelligent Design* and written several books on the topic. Concerning some more details, see his *Curriculum Vitae* http://www.weloennig.de/CurriculumVitae.pdf and *List of Publications* http://www.weloennig.de/literatur1a.html. His long-standing interest in the *Giraffe, Evolution* and *Design* has resulted in the present book on that topic.

Endorsements for the Book:

● "The evolution of the long-necked giraffe from short-necked ones is usually portrayed as one of the best cases for the truth of evolution we ever had. However, the author of the present book, the distinguished biologist Dr. Wolf-Ekkehard Lönnig, has written a careful treatise on the extensive evolutionary problems usually not mentioned in the long-neck curriculum – enormous problems related to its special anatomy, the missing links of paleontology, its sexual dimorphism, genetics and physiology etc.

For me as a researcher working on the nerve and synapse transmission in acute experiments, the part on the recurrent laryngeal nerves was especially revealing. Reading this very informative and comprehensive study I asked myself: Are Dawkins and other evolutionists really right when they speak of "a ridiculous detour" these multifunctional nerves are said to take, contrary to their own evolutionary predictions that selection is "gradually eliminating all imperfections"? The reader is invited to carefully test for himself what I understand to be many strong scientific arguments for intelligent design and against evolution recently approved to be "an unguided, unplanned process of random variation and natural selection" by 38 Nobel Prize winners. It is worth mentioning that the great majority of these variations are mutations (and recombinations), which however caused, are random with respect to quality, and that means they are usually bad because there are more ways of getting worse than of getting better, as stated by Dawkins himself. I am sure the book will help many to reconsider current principles of the evolutionary theory often presented to us as granted and its soft spots as the giraffe long neck, which is used everywhere in textbooks from elementary schools to university texts."

František Vyskočil, D.Sc. [= Ph.D.], Dr.h.c., Professor of Physiology and Neurobiology, Prague, Czech Republic (member of the Royal Physiological Society London and Cambridge, author or co-author of some 450 mostly peer-reviewed publications)

● "The neck of the giraffe has been an evolutionary puzzle ever since Lamarck. Reviewing in depth the modern biological literature, Dr. Loennig shows Darwinian theory has also failed to account for this wonder of life, and he instead proposes that it was intelligently designed. This book will benefit any person who wants to know the true status of our knowledge of the origin of this creature."

Michael J. Behe, Professor of Biological Sciences, Lehigh University, USA (author of the books *Darwin's Black Box* and *The Edge of Evolution* and many peer-reviewed papers)

● "Since Lamarck, several theories on the evolution of the giraffes's long neck have been put forward. The author provides a comprehensive analysis and critique on the current theories explaining why they are scientifically unsatisfying and examines whether the concept of ID might contribute to the debate. This book appeals to open minded (biology) scientists to form a new framework for nondogmatic research in evolutionary biology."

Joseph Merregaert, em. Professor of Molecular Biology, University of Antwerp, Belgium (author or co-author of 61 peer-reviewed papers on molecular biology up to 2010, see: http://anet.ua.ac.be/acadbib/uae/03003)

● "Darwin's story of how the giraffe got its long neck is perhaps the most popular and widely-told story of evolution. It is popular because it seems plausible: giraffes with slightly longer necks enjoyed a slight selective advantage in reaching the higher leaves of trees, and so over the ages these slight neck elongations accumulated, resulting in the modern giraffe. However, biologist and geneticist W.-E. Lönnig has written a detailed, thoroughly-researched study, "The Evolution of the Long-Necked Giraffe", which shows that almost everything about this popular story is either false or unsubstantiated. In Part I Lönnig shows that there is no fossil evidence to support the idea of a gradual elongation of the neck from the giraffe's okapi-like ancestors, and that the elongation required much more than simple quantitative changes: new features were required, for example, to handle the much higher blood pressure required by the long neck. In Part II, Lönnig looks at many other details of this widely-told story and finds them also not supported by the facts. He discusses the alternative of intelligent design, and answers the charge that it is not falsifiable, and in fact concludes:

"…the scientific data that are available to date on the question of the origin of the giraffe make both gradual as well as saltational evolution by mutations and natural selection so extremely improbable that in any other area of life such improbability would strongly motivate us to look for a feasible alternative. For biologists committed to a materialistic world view, however, an alternative is simply not considered. For them, any valid and even the most convincing objections against the synthetic theory of evolution are nothing but open problems that must be solved in the future entirely within the confines of the theory. … This essential unfalsifiability, by the way, places today's evolutionary theory outside of science, one of whose defining characteristics is that theories can only be considered scientific if they are falsifiable, and can list clear criteria by which they could potentially be falsified."

I have found many other mathematicians, engineers and physicists who, like me, find the idea that the struggle for survival could explain everything in the living world to be manifestly preposterous. Yet, most are reluctant to criticize this idea openly, because they see entire libraries full of scientific books and journals supporting the idea, and they wonder, is it really possible to write so much in support of an idea that is false? For these people I recommend reading every page of this 133-page work; when they are finished, they will understand that, yes, it is possible."

Granville Sewell, Ph.D., Professor of Mathematics, University of Texas El Paso (author of books like *The Numerical Solution of Ordinary and Partial Differential Equations* Sec. Ed. John Wiley & Sons, and *In the Beginning*; see his list of publications in his CV: http://www.math.utep.edu/Faculty/sewell/)

● "According to Darwin's theory of evolution, the giraffe's long neck formed from shorter ones by "numerous, successive, slight modifications." In this thoroughly researched study, Dr. Lönnig shows conclusively that the evidence does not support Darwin's theory on this point."

Jonathan Wells, Ph.D., Molecular and Cell Biology, Discovery Institute, Seattle, USA (trained as an embryologist, author of books like *Icons of Evolution*, *The Myth of Junk DNA*, and co-author of *The Design of Life* as well as of peer-reviewed papers in Biosystems, Development, PNAS.

● "Wolf-Ekkehard Lönnig has written a devastating critique of the standard Darwinian account of the evolution of the neck of the giraffe. This scholarly very carefully researched book is certainly the best I have read on the subject. It shows that beyond any doubt the extension of the giraffe's neck cannot be plausibly accounted for via a series of small adaptive steps nor could it have come about suddenly via a macromutation unless the reorganization of the anatomy and physiology of a presumed ancestral 'short necked giraffe' was intelligently directed. This monograph should be required reading for all biology high school pupils and would go a long way to countering the simplistic and uncritical claims of the Darwinian establishment."

Michael Denton, M.D., Medical Genetics (author of *Evolution – A Theory in Crisis* and *Nature's Destiny*. More about the author and his research, see http://en.wikipedia.org/wiki/Michael_Denton)

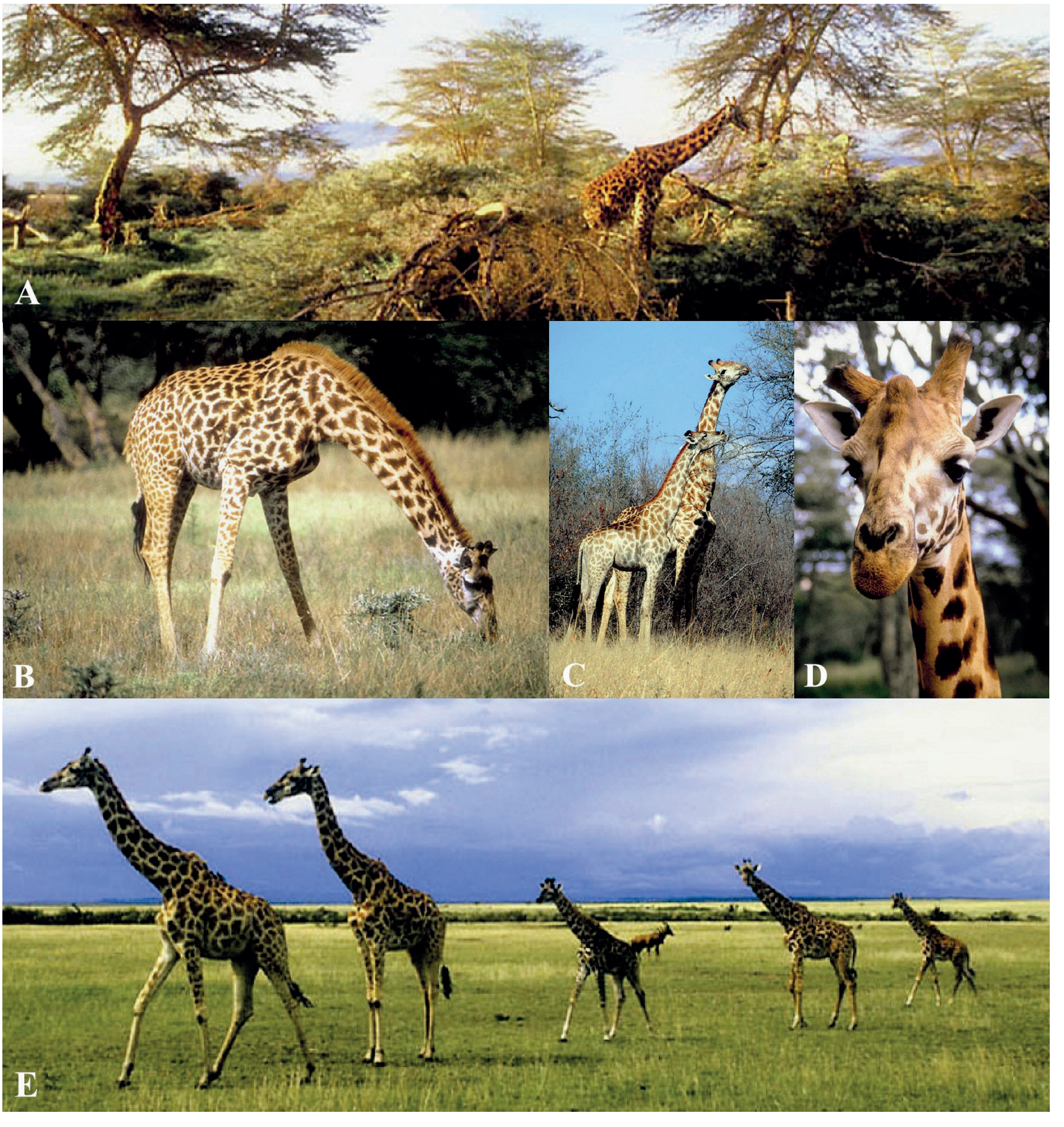

Figure A: Showing giraffe surrounded by bushes and trees of different height – the normal or most common situation in its environment (no stretching to the last leaf to survive, with the females and young ones perishing first). If only the larger bulls survived – what about reproduction of the population and survival of the species?

Figure B: Giraffe eating plants from the ground – so stretching downwards – although there seems to be much forage of different heights in the background (for a survey of the number of different plant species the giraffe eats, see pp. 42-44 below, see also the photos on pp. 109 and 114).

Figure C: Giraffes of different height (probably a juvenile and an adult one): Cooperation instead of brutal selection that would kill off the females and the younger ones first (see p. 44).

Figure D: Portrait (see a comment on the beauty of the giraffe's eyes on p. 87).

Figure E: Again giraffes of different heights: In case of a dearth, a population has to survive – not least including the females and juveniles – otherwise the population would die out.

All photographs from BUHL: 350,000 Power Clips

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(Part 1)

Giraffe, maximum values: life expectancy 34 years, height 5.80m [5.88], weight 1200 kg, speed 52 km/hr, [and general data:] ruminant, dental formula 0033/3133 (like the chamois), 66 heartbeats/minute, blood pressure in mm Hg: **systole 340, diastole 230** (average), age of sexual maturity: 6-7 years, gestation period 431-465 days (data so far according to Rainer Flindt 2000), 8 neck vertebrae (!), not 7 as reported in almost all textbooks (Nikos Solounias 1999, 2000), chromosome number 2n=30 (okapi 2n=44, 45 46).

"No data from giraffes then [in Darwin's time] existed to support one theory of causes over another, and none exist now."
"…ancestral species are relatively short necked, and the spotty evidence gives no insight into how the long-necked modern species arose."
"The standard story, in fact, is both fatuous and unsupported."

Stephen Jay Gould

Summary

In the following article the assertions of three supporters of the synthetic theory concerning the evolution of the long-necked giraffe will be discussed: the statements of Ulrich Kutschera, Richard Dawkins and Kathleen Hunt.

1. Ulrich Kutschera made the following statement regarding the origin of the giraffe, on 29 November 2005 in 3SAT (a German TV channel): "...the evolution of the long-necked giraffe can be reconstructed from fossils." According to today's best giraffe researchers, *almost all fossil links that could show us the gradual evolution of the long-necked giraffe from the short-necked giraffe are missing*, apart from the insufficiently answered question of causes. Some paleontologists postulate a "neck elongation macromutation" to explain the origin of the long-necked giraffe.

2. Richard Dawkins likewise considers – in a striking exception to his usual theoretical framework – the origin of the long-necked giraffe through a macromutation. *This exception would, of course, be entirely superfluous if the assumed gradual evolution of the long-necked giraffe could really be reconstructed from fossils*, especially since he much prefers the gradualist view. Dawkins draws the okapi, in relation to the giraffe, nearly twice as large as it really is. In this way, the problem of its evolution (the gap between the two forms) appears only about half as large. One may well ask if this technique is really useful in the search for truth.

3. Kathleen Hunt however, in her often-cited work *Transitional Vertebrate Fossils FAQ,* leaves no doubt that the origin of the giraffe is clearly and completely solved by the

> synthetic theory (gradual evolution by mutations, recombination and selection). When one looks at her reasoning more closely, however, one encounters numerous holes and problems and *the fossil evidence for the gradual evolution of the long-necked giraffe is — as expected — almost completely lacking.* A detailed analysis of her work shows, therefore, that the strong impression that one receives on a first reading concerning the continuous evolution of the giraffe stands in stark contrast to the current paleological facts.

The data so far obtained show that there are many suggestive but untestable evolutionary hypotheses on this topic and that we really know nothing about the evolution of the long-necked giraffes. Moreover, a close examination of the evidence reveals several deep problems for any of the current hypotheses explaining the origin of these species exclusively by mutations, recombination and selection.

1a. Ulrich Kutschera on the Evolution of the Giraffe

On the evolution of the giraffe, Ulrich Kutschera asserted in the German TV-3SAT-science programme *Nano*, 19 November 2005[1: p. 21], reacting to a clip from the film by Fritz Poppenberg *Is the Bible right after all?* – in which the origin of the long-necked giraffe is presented as a problem for the synthetic theory of evolution – the following points (my emphasis according to the oral TV-statement):

> "We know 20-million-year-old fossils, fossil giraffes, short-necked forms, from which the long-necked giraffes inhabiting the savannah, as well as the short-necked giraffes which inhabit the forest, have evolved. That is, *the evolution of the long-necked giraffe can be reconstructed from fossils.* We are dealing with a false statement in this film."

Before and after the "false statement", Kutschera made a short pause for stronger emphasis (however, a clarification of the question as to the origin of synorganization (co-adaptation) of the giraffe's organ systems and why the bull giraffes are generally more than 1 m taller than the cows, was not offered.)

Let us look more closely at the currently known facts, and let the reader decide, based on these facts, who has – according to the current state of knowledge – actually made unproven assertions in this matter. Regarding the fully inappropriate concept of the "false statement" ("consciously false statement (punishable)" – Wahrig) – see the detailed Note[1a1: p. 21]. (The first part of this text is in several points taken from the document http://www.weloennig.de/Giraffe.html, though expanded and modified).

In comparison to the long-necked giraffe, Petzsch remarked about the okapi (Urania/Rowohlt: Säugetiere Bd. 3, 1974, p. 412): "Completely different, the appearence of the short-necked, or forest giraffe, is more similar to the horse, cow or antilope." The okapi has a height of 150-170 cm, the Giraffe 390-450 cm (cow) and 450-580 cm (bull).

According to the theory of additive typogenesis (G. Heberer) by many small steps of adaptive character and, as Mayr says, by mutations with "*slight or even invisible effects on the phenotype*", numerous intermediate forms must be postulated just *for the height difference* between *Okapia* (or rather, a postulated *Okapia*-like ancestor) and *Giraffa*. "Macroevolution (evolution between species) is composed of **numerous**

small microevolutionary steps (additive typogenesis)" – Kutschera 2001, p. 250. Or: "**Uncountable successive small microevolutionary steps** have led to large changes in the body forms of organisms in the course of millions of years (**macroevolution, concept of additive typogenesis**)" – Kutschera 2006, p. 204 (my boldface).

Darwin had already postulated "infinitesimally small inherited variations", "steps not greater than those separating fine varieties" and "insensibly fine steps" for evolution, "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" (for further details see page 22, Note[1a2: p. 22]).

Ulrich Kutschera (2006, pp. 34/35) speaks of "the phylogenetic development of the body form of the African long-necked giraffe according to the principle of Darwin/Wallace of natural selection" as follows:

> "Starting from the short-necked giraffe, which is found in the fossil record (for example, okapi-like forms such as *Palaeaotragus*, about 20 million years old), Darwin (and Wallace) proposed the following scenario: The original short-necked forms comprised large, variable populations. Under the selection pressure of droughts and leaf shortages, those variations with longer necks and forelegs survived and reproduced preferentially. In this way, over the course of generations, these large mammals have arisen, being adapted to their special environment (DARWIN 1859/1872 and 1871). More recent research has shown that sexual selection has also played a role: male giraffes with especially long necks are dominant and mate with more fertile females than their shorter-necked competitors. In accordance with this naturalistic model, the long-necked varieties have gradually established themselves over thousands of generations throughout the African giraffe population."

Since Kutschera himself offers no naturalistic alternative to this example, but only adds the hypothesis of sexual selection[1b: p. 22] to the gradual evolution over thousands of generations, and as he refers approvingly to the thesis of additive typogenesis in various places in his work (see for example the citations above), one is not unjustified in assuming that he favors this explanation, in agreement with his TV-3SAT-statement [1c: p. 22].

The question of selection pressure and sexual selection, mentioned in the above citation, will be more closely considered in the second part of this paper. (Supplement 9 May 2010: See, especially, **Mitchell et al. (2009): Sexual selection is not the origin of long necks in giraffes.**) Concerning the inquiry of to what extent Darwin was prone to a Lamarkian interpretation in his considerations, see http://www.weloennig.de/Giraffe.html and *Part 2* below.

So, how many intermediate forms should a hypothesis of gradual evolution lead us to expect?

If we estimate *only one intermediate form for each centimeter* and if we take into account the variations within each species, we may conclude that there were, say, about **200 missing intermediate forms** (assuming only 2 m difference between "small giraffes" and large okapis). Since G. G. Simpson, one of the most renowned proponents and pioneers of the synthetic theory of evolution in paleontology, estimates a growth rate in horse teeth of about one millimeter per million years, and assumes that even this millimeter is gradually bridged by numerous intermediate forms (cf. Artbegriff 1993, p. 448), one can ask, to what extent this estimate could also be applied to the growth rate of the length of neck vertebrae and other bones.

Using such calculations, there are even more intermediate forms required: *According to the theory of gradual evolution* **at least 1000 intermediate links are missing between the okapioid ancestor and *Giraffa,* conservatively estimated!**

Yet, if one applies Simpson's considerations to the growth rate of the 7 (8) neck vertebrae, etc. – more literally, i.e. with numerous links per millimeter – on can even postulate 10,000 or more transitional links (similarly Badlangana et al. 2009, see the **details on p. 129**).

However, *this still does not take into consideration the many other anatomical, physiological and ethological differences between Giraffa and Okapia*, so that according to the theory of additive typogenesis **numerous further links** in other characters must be postulated between an okapi-like ancestor and the giraffe.

For every one of these links, on the one hand, literally thousands of components (in *rough numbers* some 25,000 protein-coding genes and due to alternative splicing 90,000 proteins, 200 joints, 300 bones associated with 1,000 ligaments and 4,000 tendons, 700 muscles, 100 billion neurons constituting the nervous system, 100,000 km of blood vessels etc.) must remain so fine-tuned with each other that a functional and survivable organism is always guaranteed. On the other hand, every one of these almost unnoticable steps that is supposed to improve adaptation, must 'fit' into the existing framework, that is, be able to be fully integrated into the existing synorganized structures. We are expected to assume that, in this manner, by the addition of thousands upon thousands of small steps, new species, genera, families, etc., even new body plans could arise. And all of this, it is believed, happened by random mutations (non-directional by definition), independently of each other and at numerous different genetic loci! I have discussed the improbability of such a process in detail in my work on the eye (2nd edition 1989 – internet-edition 2003: http://www.weloennig.de/AuIn.html; see also Wittlich 1991/2002: http://www.weloennig.de/NeoD.html as well as my contribution of 2006: http://www.weloennig.de/ShortVersionofMutationsLawof_2006.pdf, and Lönnig 2007, 2010). ***The result of these investigations is that the theory of additive typogenesis does not function, neither mathematically nor experimentally.***

Incidentally, the okapi already shows nicely the phenomenon of co-adaptation (synorganization). In the okapi not only the neck is somewhat lengthened, but also the legs, and all the anatomical and physiological features are fine-tuned to work harmoniously together.

When we now move to the paleontology of the giraffe and investigate Kutschera's above-cited claims, as well as his thesis of additive typogenesis, let me state that for this discussion that I accept all time stipulations as "given" and investigate the weak points and contradictions of the synthetic theory, essentially depending on mutations, recombination and selection, on this assumption. A critical scientific treatment of the time-question lies beyond the scope of the present work.

1b. On the Paleontology of the Giraffe

"Several distinct forms have been preserved as fossils, though most are still not very similar to the two modern representatives of the family" (Cox et al. 1989, p. 280). According to Carroll, long-necked giraffes first appear in the Middle Miocene era (Carroll 1993, p. 629; see also the discussion below on K. Hunt).

There are, however, many evolutionary statements that leave the impression that we already know the whole story: "The family of Giraffidae, which today is represented by only 2 genera (1 species each) in sub-Saharan Africa, arose from primitive, antlerless deer in the Miocene era" (Siewing 1985, p 553/554); Storch and Welsch claim 1991, p. 673 likewise, that giraffes "derive from primitive deer" (see also their edition of 2003). In Herder/Spektrum Biologielexikon (1994, Vol. 4, p. 67, also in the edition of 2001) the giraffe is perhaps more cautiously spoken of as an even-toed ungulate "which presumably developed in the Early Miocene from deer-like hoofed animals (Palaeomerycidae)" or more clearly with the words of a Spanish researcher "***Probably*** the giraffe family evolved from the *Climacoceras*;...". Similarly, Mitchell and Skinner (2003) write, "These ancestors [of the modern giraffes] ***appear*** to have arisen from the gelocid ancestral assemblage of 20-25 Mya via the family Palaeomerycidae" (my boldface, in the following quotations as well). After the introductory remark **“The origin, phylogeny, and evolution of modern giraffes (*Giraffa camelopardalis*) is obscure”**, they present, however, several questionable evolutionary hypotheses, which I will examine in the second part of this work.

The fact is, in any case, that *no continuous series of fossil links leads to the Giraffa or Okapia.* "The giraffe and the okapi of the Congo rain forest are considered as sister groups***, the origins of which are still not known***" (Devillers and Chaline 1993, p. 247). Similarly Dietrich Starck, the leading German evolutionary anatomist of his time, remarks 1995, p. 999: **"The ancestry of Giraffidae is disputed."**

Wesson (1991, pp. 238/239) agrees with these statements about giraffe fossils, as follows (as ever, my boldface):

> "The evolving giraffe line left no middling branches on the way, and **there is nothing, living or fossil, between the moderate neck of the okapi and the greatly elongated giraffe**. The several varieties of giraffe are all about the same height. There are a number of fossil giraffids with more or less the shape of the okapi; it would seem that one of them rather suddenly took off and grew to the practical limits of a giraffe."

But what scientific evidence is there for the claim that one of these varieties rather suddenly – or according to synthetic evolutionary theory, very gradually – took a new path that led to the lofty giraffe height? I will come back to this question below and in *Part 2*.

I have written to a number of paleontologists who are most familiar with mammal paleontology asking them the following question: "Is there a series of intermediate fossil forms between the short-necked (like *Okapia*) and long-necked giraffes (*Giraffa*)?" None of these evolutionary biologists was able to answer 'yes', although no doubt they would gladly have done so, if such links existed – not to mention that, in this case, the intermediate fossil forms would be published in every evolutionary textbook.

Dr. X, a paleontologist and evolutionary biologist, who, according to his own statement has carefully studied and documented the fossil neck vertebrae of the Giraffidae, but would like to remain anonymous ("I am sure you understand how delicate this point is”), answered this question in an e-mail to me on March 3, 2006, as follows:

"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**."

Dr. X is thus in agreement with Wesson, Devillers, Chaline, Starck and in general with those evolutionary biologists who have to date commented on this matter, but who have refrained from making firm but completely unproven statements about fossil links. (See also Dr. Y and Dr. Z, p. 18 of this article, last paragraph, and the supplement from April 23, and May 1, 2006, Note [1d: p. 22] and [3: p. 29].)

The assertion of Charles Devillers (1914-1999) and Jean Chaline (1937-), however, that the oldest giraffes were the largest, is contested by Dr. X ("incorrect"). I have so far not been able to check the evidence on which Devillers and Chaline have based their following statement: "The oldest fossils attributed to the genus *Giraffa* date from the end of the upper Miocene in east Africa, some 10 million years ago. They are assigned to the species *Giraffa jumae*, which was larger than the largest present giraffe (*G.[c]amelopardalis*)". "...the palaeontological record shows that in the oldest deposits, the giraffe was represented by **specimens which exceeded in size even the largest current giraffes**. This is in contradiction to what we might expect from theoretical considerations on evolutionary trends, such as an apparent general increase in size. The evolution of the giraffe therefore appears to represent a particular case" (Devillers and Chaline 1993, p. 247 and p. 207).

Under the assumption that these authors, both respected biologists with numerous publications - Devillers for example has co-authored with Grassé (Grassé, Pierre-P, and Charles Devillers, 1965, Zoologie. Vol. 2: Vertébrés, 1129 pp., Masson et Cie, Paris 1965; or Charles Devillers and P. Clairambault: Précis de zoologie: vertébrés, tome I: Anatomie comparée, Masson 1976, 2nd edition) and Chaline is one of the more important vertebrate paleontologists of our time (http://fr.wikipedia.org/wiki/Jean_Chaline), - have not simply invented this claim, I will leave this contradictory statement at that for now and will examine some points later (see ***Part 2*, p. 96**: Churcher and other authors).

Supporters of the synthetic theory of evolution will probably object that the fossil material here is still much too fragmentary. *The sudden appearence of new forms is however also confirmed in the best-preserved animal groups*. The paleontologist Oskar Kuhn from the University of Munich remarked on this question already in 1965, p. 5 (similarly 1981 pp. 53/54; further documentation of mine 1993/2003, pp. 314 -324, and 1998/2003; italics and spacing by Kuhn):

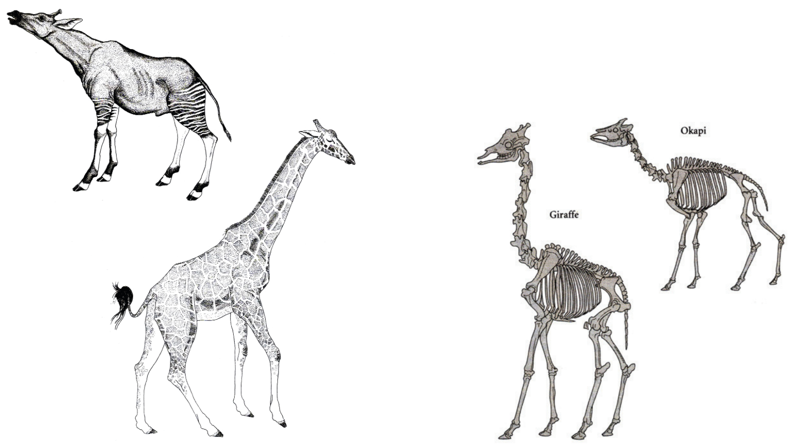
"The prejudice that the phylogenetic history of life could only be an accumulation of the smallest variational steps and that a more complete knowledge of the paleontological documents would prove [the assumed] gradual evolution, is deeply rooted and widely accepted. But the paleontological facts have long spoken *against this prejudice*! Especially German paleontologists such as B e u r l e n, D a c q u é and S c h i n d e w o l f 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*".

Moreover, it should be remarked that the paleological material in the case of the giraffe is likewise by no means as incomplete as is generally assumed. In fact, Mikael Fortelius, Professor of Evolutionary Palaeontology in Helsinki, provided a fossil list for the Giraffidae of some 62 pages, with more than 500 findings in hundreds of locations (partly from http://www.helsinki.fi/science/now/) and this list is still by no means complete. It is also noteworthy that numerous genera and species of this family are only known from fossils (see discussion on Hunt below).

The interested reader can find several further interesting points about the giraffe (up to the present time) at http://en.wikipedia.org/wiki/Giraffa_camelopardalis (the comments on *Taxonomy and Evolution* should, however, be corrected in agreement with the facts and arguments presented below).

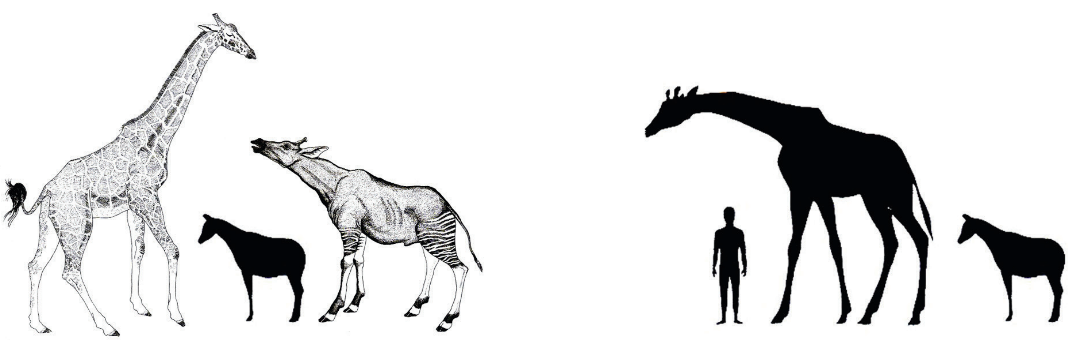
2. Richard Dawkins on the Evolution of the Giraffe

Dawkin's book CLIMBING MOUNT IMPROBABLE, original drawings by Lalla Ward, Viking, Published by the Penguin Group (1996), contains a discussion on the origins of the giraffe (pp. 91-93), which includes the following illustration (p. 92, shown on the left and the figure on the right from Dawkins THE GREATEST SHOW ON EARTH 2009/2010, p. 295; both figures strongly scaled down):



Left: Figure 3-3 of Dawkins/Ward (1996): “Steps to a long neck. *Okapia johnstoni*, which may be similar to an ancestor of modern giraffe, with giraffe, *Giraffa camelopardalis*.” Right: Comparison of the skeletons of *Giraffa* and *Okapia* according to Dawkins 2010. Note the false relative sizes!

In the book ANIMALS OF OUR WORLD (1988), Bertelsmann Lexikothek, however, the true relative sizes are shown as follows (p. 512, the **silhouettes** on the right side, of man, giraffe and okapi):



On the left side I have placed Dawkin's illustration of 1996 for comparison, but with the okapi placed on the same level as the giraffe (cf. Dawkins illustration above). In between, I have repeated the drawing of the okapi with its real relative size shown (silhouette).

From Dawkins' portrayal one gets the impression that the step from okapi to long-necked giraffe is slight, and the text reinforces this impression. The placement of the okapi above the giraffe in Dawkin's books also makes it appear larger than if it were placed on the same level as the long-necked giraffe.

If an intelligent design proponent used such methods – what objections would be raised for example by the "AG Evolutionsbiologie" or AK Evolutionsbiologie, two groups of German evolutionary biologists? (German: AG: Arbeitsgemeinschaft: team, study group, AK: Arbeitskreis, study group as well.)

Here are some excerpts from Dawkins' text (p. 91) on the evolution of the giraffe, with comments from me (as for the topic of the recurrent laryngeal nerves see below p. 30):

> "Giraffes have evolved from an ancestor rather like a modern okapi (Figure 3.3)."

Here Dawkins offers as fact a hypothesis which still needs to be scientifically investigated. This method is not scientifically admissible, otherwise one could interchange all possible hypotheses with facts (an example: "It could be a case of bird flu", or "It is a case of bird flu" – an important difference![2: see p. 28]). Even if "conceivable", there is still a categorical difference between a hypothesis and a scientifically proven fact. For example, it is also conceivable (though not in accord with the intentions of Dawkins), that the okapi arose "from an ancestor like a modern giraffe".

As noted above, the two figures of Dawkins (1996 and 2009/2010) present the relative sizes unrealistically: The real okapi is substantially smaller in comparison to the giraffe than that presented by Dawkins to support its evolutionary derivation. Dawkins continues:

> "The most conspicuous change is the elongation of the neck. Could this have come about in a single, large mutation? I hasten to say that I am sure it didn't."

Thus Dawkins believes also in this case in his idea of gradual evolution! In the next sentence, however, he qualifies this:

> "But that is another matter from saying that it couldn't."

OK! In the following sentences, Dawkins develops a sort of macromutation theory on the origin of the giraffe, although he is sure that this theory is not correct (did the elongation of the neck come about by a single large mutation? "...I am sure it didn't"). He simplifies the biological problems to a degree that is tolerable for evolutionary theory, but not realistic with regard to the biological facts (italics by Dawkins):

> "A Boeing 747 mutation like a brand-new complex eye - complete with iris diaphragm and refocusable lens, springing from nothing, like Pallas Athene from the brow of Zeus — that can *never* happen, not in a billion billion years. But, like the stretching of the DC8, the giraffe's neck could have sprung out in a single mutational step (though I bet it didn't). What is the difference? It isn't that the neck is noticeably less complicated than the eye. For all I know it may be more complicated. No, what matters is the complexity of the *difference* between the earlier neck and the later one. This difference is slight, at least when compared with the difference between no eye and a modern eye. The giraffe's neck has the same complicated arrangement of parts as the okapi (and presumably as the giraffe's own short-necked ancestor). There is the same sequence of seven [eight in *Giraffa* — note by W.-E. L.] vertebrae, each with its associated blood vessels, nerves, ligaments and blocks of muscle. The difference is that each vertebra is a lot longer, and all its associated parts are stretched or spaced out in proportion."

Only in the fantasy world of evolutionary theory are things as simple as that. In the world of biological realities, on the other hand, things are different:

"For rumination, semi-solid food [pulp, mash] must be forced over 3 m high from the reticulum stomach to the mouth!" (Bertelsmann Lexikon der Tiere 1992, p. 259.) For this, the giraffe is equipped with a *special muscular esophagus*. "The uniform circulation of blood to the different body parts makes several adaptations of the heart, arterial and venous systems necessary" (Marcon and Mongini: Die Grosse Encyclopedie der Tierwelt 1988, p. 303). To avoid bloodlessness by the movement of the head from drinking water at ground level to – seconds later – 5 m height, this animal is equipped with appropriate *muscular arteries*. Furthermore, it has a *complicated system of valves in the veins*, as well as a "*wundernetz*", a *rete mirabile, of blood-storing arteries at the brain base.* Also, the lengths, powers/strengths and functions of the skeletal, muscle and nervous systems, etc. must be precisely in tune with each other, if the animal is to be capable of survival.

Davis and Kenyon summarize the main points as follows (1993, p. 13):

> "When standing upright, its blood pressure must be extremely high to force blood up its long neck; this in turn requires a very strong heart. But when the giraffe lowers its head to eat or drink, the blood rushes down and could produce such high pressure in the head that the blood vessels would burst. To counter this effect, the giraffe is equipped with a coordinated system of blood pressure controls. Pressure sensors along the neck's arteries monitor the blood pressure and activate contraction of the artery walls (along with other mechanisms) to counter the increase in pressure."

McGowan lists additional details (1991, pp.101/103):

> "The blood leaving the giraffe's heart has to do more than just reach the level of the head, it has to be at a high enough pressure to pass through all the fine vessels, the capillaries, that supply the brain and other organs. To achieve this the blood leaves the heart at a pressure of 200-300 mm Hg [260-350 mm Hg according to Starck 1995, p. 206[2a, see p. 28]], which is probably the highest blood pressure of any living animal (Warren, 1974; Hargens et al., 1987). A giraffe's blood pressure is so high that it would probably rupture the blood vessels of any other animal, but two mechanisms appear to prevent this. First, the arterial walls are much thicker than in other animals. Second, the fluid that bathes the cells of the body is maintained at a high pressure; this is largely achieved by the thick skin, which is tightly stretched over the body and which functions like the anti-gravity suit worn by pilots of fast aircraft.
>
> ...Another problem posed by the possession of a long neck is the large volume of air in the trachea, the tube that connects the back of the throat with the lungs. This air is unavailable for respiration and the space it occupies is consequently referred to as the dead space. The dead space has a volume of about five pints (2,5 l) in the giraffe. Since this air has to be moved each time the animal breathes, the rate of ventilation has to be increased to compensate for the reduced air flow. 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."

Correspondingly efficient and "big lungs" have the task of balancing respiration "through a 10 feet long tube; many muscles, tendons, and bones had to be modified harmoniuosly" (Wesson 1991, p. 226) (for full quotation see Note 2b, p. 28).

Davis and Kenyon summarize the problems of the giraffe for the synthetic evolutionary theory as follows (1993, p. 13, my italics):

> "In short, the giraffe represents *not a mere collection of individual traits but a package of interrelated adaptations.* It is put together according to an overall design that integrates all parts into a single pattern. Where did such an adaptational package come from?
>
> According to Darwinian theory, the giraffe evolved to its present form by the accumulation of individual, random changes preserved by natural selection. *But it is difficult to explain how a random process could offer to natural selection an integrated package of adaptations, even over time.* Random mutations might adequately explain change in a relatively isolated trait, such as color. But major changes, like the macroevolution of the giraffe from some other animal, would require an extensive suite of coordinated adaptations."

All of these questions are completely ignored by Dawkins, and he continues:

> "The point is that you may only have to change one thing in the developing embryo in order to quadruple the length of the neck. **Say you just have to change the rate at which the vertebral primordia grow, and everything else follows."**

"... and everything else follows": Can one, in view of the above details, describe this conclusion perhaps as purely wishful thinking? And such and/or further wishful thinking on evolution passes today as science that must scarcely be questioned, or not at all. – Richard Dawkins continues:

> "But in order to make an eye develop from bare skin you have to change, not one rate but hundreds (see Chapter 5). If an okapi mutated to produce a giraffe's neck it would be a Stretched DC8 macro-mutation, not a 747 macro-mutation. It is therefore a possibility which need not be totally ruled out. Nothing new is added, in the way of complication. The fuselage is elongated, with all that entails, but it is a stretching of existing complexity, not an introduction of new complexity."

"Nothing new is added, in the way of complication" – this claim is simply false (see details above). His subsequent comparison with the different numbers of vertebrae in snakes seems inappropriate, since the unique problems of the giraffe, cited above in some detail, cannot applied here (however, possibly others could be found in snakes).

3a. Kathleen Hunt on the Evolution of the Giraffe

When one examines the assertions of zoologist Kathleen Hunt on one of the most frequently cited internet pages regarding the origin of the giraffe, *Transitional Vertebrate Fossils FAQ*, one immediately gets the impression that all questions and problems on the origin of the giraffe are completely resolved within the context of the synthetic theory of evolution – like the statements of Kutschera quoted above. It should be observed that this site, because of its seemingly stringent scientific level and way of reasoning, has perhaps convinced more readers of the correctness of the theory of evolution than many other internet sites. On the evolution of the giraffe, Hunt writes (1999):

> "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." (http://www.talkorigins.org/faqs/faq-transitional/part2c.html).

When we now look more closely at her exposition and examine the reasoning behind the individual statements, we should be aware of the following problem: we have to start from the current state of knowledge, which cannot be considered settled, since we do not know if and which further developments and findings may lead to revisions in certain questions.

But we obviously cannot start from fossil finds that perhaps some day will be discovered and described, applying the motto: "Faith is the substance of fossils hoped for, the evidence of links unseen" (according to A. Lunn[2c: p. 29]). Besides, it is possible that further fossil finds may even deepen the mystery of the giraffe ancestry – a possibility that most evolutionary theorists deem highly unlikely (unjustifiedly, as many paleontological examples show).

> "Giraffes: Branched off from the deer just after *Eumeryx*."

This statement is not supported by any fossil finds. Thus, we might ask, if and from where K. Hunt and many other authors, who make similar and often even stronger assertions and apparently completely certain deductions, can know these things so definitively. In this connection we should further ask, what these first deer looked like and when they appeared. "The first deer emerged more than 30 million years ago in the Oligocene era, in Asia. The early deer *Eumeryx* had as yet no antlers on his long and primitive skull. The male animals had dagger-like eye teeth in the upper jaw, like today's water chevrotain" – Ernst Probst in: http://www.fortunecity.de/lindenpark/wittgenstein/30/RekordederUrzeit.html

In view of the complete lack of fossil evidence for the derivation of the giraffes from *Eumeryx*-relatives, one can justifiably ask whether such antlerless deer, with daggerlike eye-teeth, really have evolved by mutation, recombination and selection into giraffes. As for deer themselves, one may further ask: was does "emerge" mean? Where do these deer come from? Further, a transitional series leading to the *Prodremotherium* from the late Eocene is also lacking. Evolutionary claims are not supported, neither for the ancestry of the deer nor for the giraffe, by "very fine-grained sequences documenting the actual speciation events" (in accordance with Hunt's *Introduction*). Of such transitions, she further says:

> "These species-to-species transitions are unmistakable when they are found. Throughout successive strata you see the population averages of teeth, feet, vertebrae, etc., changing from what is typical of the first species to what is typical of the next species."

In agreement with this statement, the English zoologist Douglas Dewar wrote already decades ago (1957, p. 35):

> "If the evolution theory be true, the record should exhibit the following features:

> I. Every class, order, family or genus would make its appearance in the form of a single species and exhibit no diversity until it has been in existence for a long time.
>
> II. The flora and fauna at any given geological horizon would differ but slightly from those immediately above and below except on the rare occasions when the local climate suddenly changed if the sea flowed over the land, or the sea had retreated.
>
> III. It should be possible to arrange chronological series of fossils showing, step by step, the origin of many of the classes and smaller groups of animals and plants. By means of these fossil series **it should be possible to draw up a pedigree accurately tracing the descent of most of the species now living from groups shown by the fossils to have been living in the Cambrian period.**
>
> IV. **The earliest fossils of each new group would be difficult to distinguish from those of the group from which it evolved, and the distinguishing features of the new group would be poorly developed**, e.g. the wings of birds or bats. "

And precisely these criteria are not fulfilled here. Otherwise we could follow the evolution of the long-necked giraffe, and the giraffes in general, back to their origins. Whether at least her description of the "general lineage" can be applied to the giraffes, will be discussed later.

Let us first look for descriptions of unmistakable "species-to-species transitions" in the giraffe's evolution (transitions which according to Hunt appear especially frequently in *Part 2* of her work):

> "The first giraffids were *Climacoceras* (very earliest Miocene)..."

The New Shorter Oxford Dictionary defines "giraffids" as follows: "...of or pertaining to, any animal of, the artiodactyl family Giraffidae, comprising the giraffe, okapi, and related extinct forms." *Webster* says about *Giraffa*: "...comprising the giraffes which together with the okapis and extinct related forms constitute a family and sometimes a superfamily of the Artiodactyla."

I would only like to remark here that *Climacoceras* does <u>not</u> belong to the Giraffidae family. This genus should rather be placed in its own family, Climacoceratidae (Hamilton 1978). Both families, however, belong to the superfamily Giraffoidea.

Neither in Benton's *The Fossil Record 2* (1993, pp. 756,758/759) is *Climacoceras* placed into the giraffe family nor by McKenna and Bell (1997/2000). Carroll 1988/1993 even assigns this genus to the deer family Palaeomerycidae – that is, a good bit further away outside the Giraffoidea (see also Thenius 1970/2000). In none of the newer sources known to me is the genus placed in the Giraffidae family.

If Hunt, by "giraffids", refers to the superfamily (which by the way would seem to be an unusual use of the term in English), then one may include *Climacoceras*. However, this choice of wording leaves the less-informed reader with the impression of a closer kinship to the giraffe family than exists in reality.

Incidentally, a horizontal evolution of special features from one family to another seems difficult to accept because of the the problem of *heterobathmy*. In addition there are some serious time problems, that we will address in detail later.

Climacoceras (about 100 kg and 1.5 m tall) according to:
http://www.girafamania.com.br/primitiva/girafideo_chifre.jpg

Climacoceras according to http://caramitten.deviantart.com/art/Climacoceras-177067025 (left) and http://blog-imgs-37.fc2.com/r/o/m/romanticbeast/climacoceras.jpg (right) [2c1, 2c2, see p. 29]

Furthermore, according to Stucky and McKenna (see Benton) the assignment of *Climacoceras* to the "very earliest Miocene" is false and correct is ***Middle Miocene*** (see also McKenna and Bell 1997/2000, p. 432). Carroll, on the other hand, only stipulates "Miocene".

In the original work by Hamilton (1978), the species *C. africanus and C. gentryi* were dated approximately 14 (13.8) million years back, that is the Middle Miocene (Miocene: begins 23.03 million years ago, ends 5.33 million years ago; **Middle Miocene: 16.3 to 10.4 million years ago**[2d, p. 29] – see Harland et al. 1990, Kearey 1993).

If the date of 13.8 million years is correct, the closest short-necked giraffe, *Canthumeryx,* dated by Hunt in the Early Miocene, is older than the Climacoceratidae, from which these giraffes supposedly evolved. In this case the children would have existed before the parents. Carroll (1988/1993, p. 629) puts the first fossil evidence for the genus *Giraffa* into the Middle Miocene. This is corroborated by fossils of *Giraffa priscilla* from the Middle Miocene of Ramnagar, India (Basu 2004, see Note [2a1] in the second part of the paper, p. 89). *Bohlinia* has a thus far maximum calculated age of 11.2 million years (see below). In this case, *Climacoceras* and the long-necked giraffe would geologically appear much closer together, leaving hardly enough time for a gradual evolution through thousands of intermediate stages.

Thenius remarks in Grzimeks Tierleben (1970/2000, p. 255):

> "...the giraffes were once a wide ranging family abundant in forms of even-toed ungulates. They evolved relatively late – presumably little less than 25 million years ago in the Early Miocene – from a group of deer-like (with respect to teeth) hoofed animals, to which the European genera *Lagomeryx, Procervulus* and *Climacoceras,* among others, belong. The Lagomerycides (Lagomerycidae family) had forked, branched, or stalked and branched flat-spread, bony skull protrusions, reminiscent of deer antlers, but which no doubt were permanently covered with skin, and could not be regenerated [exchanged]."

Note that Thenius also assigns *Climacoceras* to the Lagomerycidae. Yet the assumption that *Climacoceras* first emerged in the Early Miocene is clearly incorrect. Apart from the unproven claims regarding evolutionary derivations, most authors agree, however, that the short-necked giraffes appeared in the Early Miocene. "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 more or less at the same time**, as Savanna dwellers. († *Honanotherium* in Africa, Eurasia). In the Neogene another line of descent of the Giraffidae appears in Eurasia and Africa, the Sivatheriidae with † *Helladotherium*, and † *Sivatherium* among others. These were animals with heavy, cow-like body forms, and with branched, antler-like ossicones, which survived into the Pleistocene" (Starck 1995, p. 999). We have already noted above that the same author points out that "the ancestry of the Giraffidae is disputed". The reasons for this should now have become clearer. Starck is thus in agreement with all the other critical giraffe researchers, at least in principle.

To summarize: with respect to *Climacoceras* it should be stressed that a series of transitional forms from early antlerless deer (such as *Eumeryx*) to *Climacoceras* with its bony skull protrusions ("branched, antler-like ossicones") is completely lacking, and that according to current dating *Climacoceras* arose several million years too late to be considered a possible ancestor of *Canthumeryx* (the earliest genus unanimously assigned to the Giraffidae [see, however, below and *Part 2*]). But even if the assignment of *Climacoceras* to the "very earliest Miocene" were correct, this genus would still not be older than *Canthumeryx* and thus could hardly be its ancestor: even in this case the time would still not be sufficient for a gradual series of transitional forms from one genus to the other in a continuous evolutionary process over millions of years.

Neither the claim, put forth as fact, that *Climacoceras* arose from early antlerless deer, nor the idea, also presented as fact, that this genus is the starting point for further giraffe evolution, can in any way scientifically be firmly established.

> "...and then *Canthumeryx* (also very early Miocene),..."

The oldest dating of a specimen of *Canthumeryx sirtensis* lies between 18 and 22.8 million years ago (according to the dating of Mikael Fortelius). If one dates the beginning of the Miocene at 23.03 million years, K. Hunt's assignment of *Canthumeryx* to the "very early Miocene" is correct, but then this genus would be at least 8 million years older than the "forerunner" genus *Climacoceras.* (If one wants to be very critical, one could argue that the average estimate of 20.4 million years would be in the Miocene, but not "very early" Miocene.)

So far I did not find good illustrations of *Canthumeryx* (see, however, *Part 2*).

"....then *Paleomeryx* (early Miocene),..." [more accurately, *Palaeomeryx*]

In the recent technical literature, the deer *Palaeomeryx* is unanimously placed in Palaeomerycidae (a group which – as already mentioned above – lies outside Giraffoidea), being a family to which Carroll and Thenius have also assigned *Climacoceras*. These so-called "oldest relatives of the giraffe" (as claimed by the following internet source, in agreement with Hunt) are dated to be **15 million years** old and cannot fill the role claimed for them for chronological (compare Note [(2a)] in *Part 2* of the paper) and morphological reasons, though the rest of the exposition may be correct:

"These animals, called *Palaeomeryx* had somewhat the same size as today's red deer. It is evident from skeleton remnants from China, that male specimens of *Palaeomeryx* had bony protrusions on the skull. *Palaeomeryx* inhabited the forest, and ate leaves" (http://fossilien-news.blog.de/?tag=Palaeomeryx).

So let me emphasize that according to the best sources known to me, *Palaeomeryx* first arose in the Middle Miocene (and not "early Miocene"), thus later than *Canthumeryx* and would in this respect fit chronologically, – except only that they do not belong to this family and superfamily at all. But even if *Palaeomeryx* could be assigned to the giraffes, this genus, 15 million years old, is still some 1.2 million years older than *Climacoceras* (13.8 million years), which again leads us to the above-mentioned time problem of the evolutionary derivations according to Hunt.

Recent deer, similar to the *Palaeomeryx*, according to http://eo.wikipedia.org/wiki/Cervedoj

It hardly needs to be mentioned, that the postulated "species-to-species transitions" are again completely absent, otherwise we would certainly not have the above mentioned chronological and further difficulties; remember please Hunt's words:

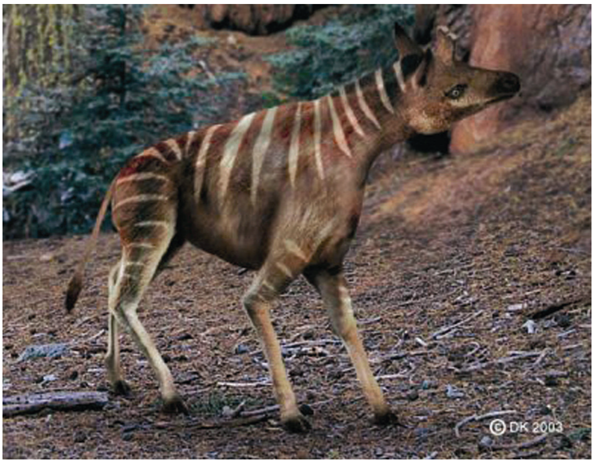
> "These species-to-species transitions are unmistakable when they are found. Throughout successive strata you see the population averages of teeth, feet, vertebrae, etc., changing from what is typical of the first species to what is typical of the next species."

Hunt calculates something less than 1 million years for "species-to-species transitions". Transitional series between genera would correspondingly require several times as many years.

In place of *Palaeomeryx,* in the recent literature a genus called *Propalaeomeryx* is frequently mentioned, which unlike *Palaeomeryx* is assigned to the family Giraffidae. However, this "Pro" has nothing to do with an evolutionary first step to *Palaeomeryx*, since the latter belongs to the Palaeomerycidae and the former to Giraffidae. Regarding *Propalaeomeryx* McKenna and Bell remark (1997/2000, p. 432): "Proposed as a provisional name" by Lydekker 1883, pp. 173-174. Further hints: "[Including † *Progiraffa* Pilgrim, 1908: 148,155.]". This "Pro" in *Progiraffa* has likewise nothing to do with a link to *Giraffa*, since *Progiraffa* is "an uncertain large cervoid" [thus, a deer] (Berry et al. 2005), maximum age 18 million years.

> "...then *Palaeotragus* (early Miocene) a short-necked giraffid complete with short skin-covered horns."

Palaeotragus is, to be sure, dated to be maximally 18 million years old (occurring in the Early Miocene), but again there is no known series of links to any forerunners, and this genus is, according to the current finds, also several million years older than the presumed ancestor *Climacoceras,* which is incorrectly arranged by Hunt as to the time of its first appearance as well as morphology and evolution.



Palaeotragus, according to http://37.img.v4.skyrock.net/377/alexis-loulou-59700/pics/2713733672_small_1.jpg

A similar illustration can be found in Metcalf 2004, p. 37.

Further, Metcalf conveys the idea by his text and illustrations, that *Helladotherium* was a forerunner of *Palaeotragus*. The former, however, first appears in the Late Miocene, and thus from time considerations alone cannot be considered an ancestor of the latter. In addition, *Helladotherium* belongs to the Sivatheriinae, the above-mentioned animals with "heavy, cow-like body forms and with branched, antler-like skull ossicones, that survived into the Pleistocene".

The reconstruction of *Palaeotragus* looks somehow disproportionate as to its anatomy and is possibly built in part on evolutionary assumptions (yet the neck is in any case as short as it should be according to the fossils found).

Further, Kathleen Hunt writes about the next short-necked giraffe:

> "...From here the giraffe lineage goes through *Samotherium* (late Miocene), another short-necked giraffe,.."

Samotherium according to: http://site.sinodino.cc/_HeZhang/2003pic09.jpg

None of the other authors so far known to me places *Samotherium* (maximum 14.6 million years for this genus) into the "late Miocene", but rather into the ***Middle Miocene***. The time between *Palaeotragus* and *Samotherium* is then some 3.4 million years, again relatively short for a gradual evolution in the sense of Darwin and the synthetic theory of evolution. Once again a transitional series is missing, and in addition, up to now we have ***nothing but short-necked giraffes***.

The wording: "From here the giraffe lineage goes through *Samotherium*..." implies – even according to cladistic evolutionary assumptions – the unrealistic idea that the above-mentioned genera represent the "giraffe lineage". Already in 1978, Hamilton pointed out that in all these cases we are dealing only with "sister-groups": "The giraffines are identified as the sister-group of the *Palaeotragus* group using lengthening of the limbs and neck as a synapomorphy" (p. 220), and on p. 219 we read some similar arguments on the evolutionary relationships of these forms: "...*Canthumeryx* is identified as the sister-group of the giraffids and *Climacoceras* is the sister-group of *Canthumeryx* plus the giraffids."

What are "sister-groups"? According to evolutionary assumptions, they are defined as follows: "...sister groups are the two monophyletic groups produced by a single dichotomy; each is the other's nearest relative; sister species-groups" (Lincoln et al.: A Dictionary of Ecology, Evolution and Systematics). As repeatedly mentioned above, **the line itself with its numerously assumed *speciation events* has not been documented. Rather, according to Hamilton and many other authors, we know more or less only the tips of the twigs of the assumed evolutionary tree in the form of *sister-groups*.**

The giraffe lineage therefore does not go "through *Samotherium*", but rather, even according to evolutionary presumptions, **past *Samotherium*** .

> "...and then split into *Okapia* (one species is still alive, the okapi, essentially a living Miocene short-necked giraffe),..."

The above sources place *Okapia* in the early Pleistocene. *Samotherium* however, according to current dating, lived 14.6 to 3.4 million years ago. The transitional series is missing, as in the afore-mentioned cases. And the okapi, "essentially a living Miocene short-necked giraffe" could – according to this assertion – be classified as a living fossil (basic form essentially unchanged for some 15 million years; on the topic of living fossils, cf. http://www.weloennig.de/mendel20.htm; see also Janis 1984 and the further points in the second part of the paper).

> "...split into *Okapia* ...and *Giraffa* (Pliocene), the modern long-necked giraffe."

The long-necked giraffes first appear *not* in the Pliocene, but rather with *Bohlinia attica* (maximum 11.2 million years ago) and *Giraffa priscilla* (about 12 million years ago) already in the Middle Miocene. The end of the Middle Miocene is dated at 10.4 million years ago according to Harland et al. (1990) and Kearey (1993), thus the oldest estimates for *Bohlinia* and *Giraffa* reach back into the Middle Miocene. So far *both genera appear in the fossil record without a series of transitional stages with their very impressive heights of almost 6 meters.* Since the genus *Giraffa*, with an age of some 12 million years, is placed into the Middle Miocene, it can in any case be considered a living fossil.

Now at this point, where the most thrilling part for our basic question begins, i.e. at the point, where the gradual evolution of the long-necked giraffe is asserted to have been documented by intermediate fossil forms ("...the evolution of the long-necked giraffe can be reconstructed from fossils" – see Kutschera above), ***we no longer hear anything about the fossil evidence*****,** but only the assertion that this evolution has taken place ("...split into *Okapia* ...and *Giraffa*"). If, however, Kathleen Hunt could produce the fossil evidence for a gradual evolution, then, given her desire to show the public that all fundamental questions and problems on the origin of the long-necked giraffes have been completely solved in accord with the synthetic theory of evolution, so that only the ignoramuses and/or religious fanatics could doubt this fact, then surely she would have laid it out in detail. However, she does not present the evidence, because such 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, see p. 29] 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 primitive than those of *Giraffa* (when the term "primitive" is used, in my experience caution and further investigations 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 evolution of the long-necked giraffe *Bohlinia* 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 forms are unknown (because clearly: if we already had them, there would be no reason to search for them – neither in Africa nor in Eurasia).

The majority of the corrections concerning Hunt's statements are based on data that were already known at the beginning of the 90s of the previous century – thus she (like Kutschera) has not done careful and critical research, but rather made statements designed to provide impressive rhetorical support for the synthetic theory of evolution, yet incorrect in the essential points.

Thus, we come full circle back to the first part of our exposition: The assertion, made before an audience of altogether some 1 million viewers by Ulrich Kutschera that the difficulties for the synthetic theory of evolution presented in Fritz Poppenberg's film were "false statements" (see Kutschera above), is shown to be itself incorrect by the above data.

3b. General lineages

If the evidence for "species-to-species-transitions" for the giraffe is so completely lacking (although such cases should, according to her words, appear especially frequently in Part 2 of her work, in which the giraffe is also treated) – could not, at least, her second main assertion be correct, i. e. that evidence exists for a "general lineage", confirming the evolution of the Giraffidae indirectly? So, let us look more closely at her assertions on the matter of the "general lineage":

> "This is a *sequence of similar genera or families*, linking an older group to a very different younger group."

However, this could just mean a purely morphological derivation, which cannot necessarily be identified with a series of evolutionary stages (Dacqué, Kuhn, Troll). She continues:

> "Each step in the sequence consists of some fossils that represent a certain genus or family, and the whole sequence often covers a span of tens of millions of years."

Since the fossil evidence for Giraffidae stretches back some 23 million years, this assertion could be correct in principle. Interpreting the existing fossil genera as "steps" in a genetic-evolutionary sequence, however, runs into the above-discussed time and anatomical difficulties (see further points below). Hunt further defines:

> "A lineage like this shows obvious morphological intermediates for every major structural change, and the fossils occur roughly (but often not exactly) in the expected order."

The evidence of "obvious morphological intermediates for ***every major structural change***" does not exist for Giraffidae, neither within the short-necked giraffes nor for the decisive step to the long-necked giraffes, nor within the long-necked giraffes. And one would have to be unrealistically benevolent if one wants to claim that, in the sense of evolutionary connections, the fossils in this family appear "roughly (but often not exactly) in the expected order".

> "Usually there are still gaps between each of the groups - few or none of the speciation events are preserved."

Gaps exist between all the genera of the Giraffidae, and not a single one of the numerous postulated "speciation events" has been preserved (granted that they ever occurred).

> "Sometimes the individual specimens are not thought to be *directly* ancestral to the next-youngest fossils (i.e., they may be "cousins" or "uncles" rather than "parents")."

This can be said of *all* fossil and living Giraffidae genera and species.

> "However, they are assumed to be closely related to the actual ancestor, since they have intermediate morphology compared to the next-oldest and next-youngest "links"."

As a rule, not even the expected "intermediate morphology" is present. "...they are *assumed* to be closely related to the *actual ancestor*...": In both cases we are dealing with *assumptions*, for the "actual ancestor" as well as for the evolutionary "cousins or uncles". None of these assumptions is scientifically stringent.

> "The major point of these general lineages is that animals with intermediate morphology existed at the appropriate times,…"

Both the "intermediate morphology" as well as evidence of links "at the appropriate times" are missing.

> "...and thus that the transitions from the proposed ancestors are fully plausible."

This would not be the case, even if all the criteria were fulfilled, cf. http://www.weloennig.de/mendel13.htm and the following chapter, as well as: http://www.weloennig.de/mendel14.htm and also http://www.weloennig.de/AesWesen.html and the ensuing chapter.

In this connection, we should remember Kuhn's basic statement concerning the relationship between morphology and evolution:

> "The similarity of forms was explained by evolution, and evolution in turn was proven by the various grades of similarities. It was hardly noticed that here one has fallen victim to circular reasoning; 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 a creation plan, not to evolution."

It would perhaps be "fully plausible" only if there were no alternative to the evolutionary interpretation by mutation, recombination and selection. That is however, not the case (see in *Part 2* the exposition on ID).

Kathleen Hunt continues:

> "General lineages are known for almost all modern groups of vertebrates, and make up the bulk of this FAQ."

In this case, the Giraffidae family would be an exception to this rule of "general lineages". According to my knowledge, however, the giraffes conform to a rule, which has first been established for the classification of the higher systematic categories, and which according to current knowledge also holds true for the origin of the genera of the giraffes (cf. http://www.weloennig.de/AesIV5.SysDis.html, thus the statement of Steinmann about the more or less closed series of evolutionary sequences within lower systematic categories should likewise be carefully examined for any concrete case).

If, however, the general lineages for almost all modern groups of vertebrates are as uncertain as in the case of the giraffes, then we are dealing only with suggestive evolutionary interpretations in most other groups as well, yet without solid scientific proof.

Notes

(1) The program was, according to the statement of a MPG employee, replayed several times the following morning. Upon my question, the TV management informed me that the science program *Nano* has an average of a half million viewers, and similarly for the reruns.

(1a1) Upon further reflection I have come to the conviction that the term "Falschaussage" (false statement) used by U. Kutschera is completely out-of-place here. According to all dictionaries and encyclopaedias available to me, this is a precise *legal* term, which is defined as follows (Brockhaus, Band 7, 1988, p. 86, further points there): "**Falschaussage, uneidliche [not under oath] Falschaussage, falsche uneidliche Aussage**, the intentional false statement of a witness or expert, not under oath, in a courtroom or other place where examinations of witnesses or experts take place (for example, parliamentary investigation committees). "Falschaussage" will be punished by three months to five years imprisonment (§ 153 StGB)." What Kutschera here apparently intends is the criminalization of opinions deviating from his own view of things, as evidenced by the following citations and commentaries made by him:

On page 159 of his book STREITPUNKT EVOLUTION ("Controversies of Evolution") Kutschera cites an article by Professor Werner Gitt, agreeing with the comments of the Jenaer biologist W. Bergmann as follows (boldface again from me):

> "It should be further mentioned that the exposition of this author on the topic of "Animal and Plant Life" is factually incorrect and conveys a completely out-dated picture of the physiology of organisms: The concept of "metabolic energy" seems to be fully unknown to the author. The biologist Prof. W. Bergmann (Jena) sent me this journal with the following comments on the article by the engineer W. Gitt: 'Such journals with pseudo-scientific assertions were distributed at the Bible exhibition in Jena. This is irresponsible "dumming down" of the public, which must be **penalized and forbidden**. One can only say, adapting a quote by Prof. H. Küng about Pope John Paul II, that with such writings, Christianity remains a middle-age galley of minors." **There is nothing to add to these appropriate comments**."

If – as U. Kutschera says – "there is nothing to add to these appropriate comments", that means that the article should be penalized and forbidden – rather than discussed and factually refuted. For a work to be penalized and forbidden, it must first be criminalized, and this he attempts to do with regard to the topic of giraffe evolution, with the legal idea of the "Falschaussage", – it only remains to be asked, who should be the judge in this trial, though one can well imagine.

I cannot tell whether Kutschera's judgement on the article by Gitt is justified or not, since I have not as yet seen Gitt's comments. Anyhow, Kutschera himself has not offered any factual refutation. If Kutschera's claims about Gitt's article are as unfounded as his statements on giraffe evolution, then extra skepticism is appropriate. In any case, according to my understanding, anyone who – instead of arguing publicly, factually and scientifically – wants to **penalize and forbid**, **has ventured outside the framework of the Constitution** not only of the FRG, but of all countries which are in agreement with the Universal Declaration of Human Rights.

(1a2; Note added 7 November 2008.) Thus, Darwin had provided the basic idea of continuous evolution some 150 years ago by postulating "*innumerable* slight variations", "*extremely* slight variations" and "*infinitesimally* small inherited variations" (he also spoke of "infinitesimally small changes", "infinitesimally slight variations" and "slow degrees") and hence, as likewise quoted in part above, imagined "steps not greater than those separating fine varieties","insensibly fine steps" and "insensibly fine gradations", "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" or "the transition [between species] could, according to my theory, be effected only by ***numberless small gradations***" (emphasis added, see http://darwin-online.org.uk/).

(1b) The suggestion by R. E. Simmons and L. Scheepers of sexual selection was, however, not offered as a supplement to Darwin's explanation (feeding competition), but rather as an *alternative*. In the abstract of their article "Winning by a neck: Sexual selection in the evolution of giraffe" (American Naturalist 148 : 771-786, 1996) they say, among other things:

> "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**. [Here follow their arguments for sexual selection, which I do not want to address until the second part.]
>
> ...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" (my boldface).

(1c) The TV-3SAT-remark should also be understood in connection with the presentation of giraffe evolution by Dr. Ragnar Kühne (Berlin Zoo) in Fritz Poppenberg's Film. There Kühne defends the gradual evolution in connection with the selection theory. Poppenberg follows with a technical criticism, and Kutschera is now more or less defending Kühne.

(1d) Supplement from 23 April 2006 and 1 May 2006: Since I want to keep my readers as correct 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. 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 (2011).

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 excludes by definition a gradual evolution of this step – by whatever method the giraffes were created.

Note of 9 October 2008 (last modified 16 November 2008): Ever since the present article appeared online, some evolutionists seem to have been eagerly looking for "missing links" or transitional forms and recently they claimed to have found one (see, for example, http://www.conservapedia.com/Giraffe and Note below*). ***If true, it would show how extraordinarily***

fruitful the present article has been for scientific research. However, there is strong reason to doubt that the neck of this so far **un**published fossil specimen "is a perfect intermediate between the short-neck ancestors and their long-neck descendants". For the time being, the main reason is that some of long-necked forms are most probably ***older*** than this fossil "link" (a candidate fossil link should come at least from the ***Middle Miocene***, and not be described "from the late Miocene and early Pliocene"). Remember, please, that – as stated on page 13 – according to Carroll (1988/1993, p. 629) **the first fossil evidence for the genus *Giraffa* is from the Middle Miocene.** And this is corroborated by fossils of ***Giraffa priscilla*** **from the Middle Miocene of Ramnagar**, India (Basu 2004, see Note [(2a1)] in the second part of the paper). Thus, the fossil with its 'perfectly intermediate neck' cannot be in the assumed phylogenetic lineage leading to the long-necked giraffes.

Also, both long-necked giraffes and the species with its 'perfectly intermediate neck' lived contemporaneously for millions of years like many other presumed ancestors of the giraffe with some intermediary features (see the figure on page 48 in *Part 2*).

Another question could be: Does the fossil whose neck is thought to be a "perfect intermediate..." (see above) have 7 or 8 cervical vertebrae?**

Moreover, except for the assertion concerning the neck just quoted, a description of the other parts of the unpublished fossil animal is not known to me; yet a mosaic-like combination of the neck with uniquely derived (autapomorphic, 'new-featured') characters not fitting into the presumed giraffe line may exclude it from the long-necked giraffe's ancestry *per se* (as is usually the case with "missing links" or "transitional forms"). Hence, this question has to be carefully investigated too.

As for possibilities and predictions of 2006 concerning intermediate forms mentioned in the present paper ("2 or 3 further mosaic forms with some intermediary features" in the 'right'geological strata, but **no continuous series in Darwin's sense** and "as mosaics they will **not** unequivocally be "connecting any of the fossil taxa to *Giraffa*""), see here pp. 22 and 23, and *Part 2*, pp. 44-49, 62-63, 66, 71-86. Considering the facts and arguments presented on these pages, there is, in principle, ***nothing new*** with another relatively small adult giraffe-like animal, which is, geologically speaking, ***younger*** than the long-necked giraffes (see, for instance, the pygmy-giraffes mentioned above and in *Part 2* of the paper, pp. 45, 62, 72, 92 and, perhaps in part, also the zoo giraffes referred to in *Part 2* as well (p. 122), not to speak of the females and young ones). However, if the fossil find with the intermediate neck ***were older*** than the long-necked giraffes, then it could be a good candidate for my prediction of "2 or 3 further mosaic forms with some intermediary features" – here especially the (7 or 8) shorter neck vertebrae – in the 'right'geological strata, granted that it would be an adult male animal, or at least the sexual dimorphism could be taken into account, and that the factor 'modification' could be neglected.

And, of course, an absolutely ingenious and prolific mind having generated, and sustaining, the laws of physics (as, for example, also many nobel laureates of science have inferred for the origin of the universe: http://www.weloennig.de/Nobelpreistraeger.pdf), has the potential to create as many mosaic forms with some intermediary characters as are imaginable within functional limits, front-loaded or otherwise, but hardly so by "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 as quoted on p. 3 and p. 22 above in agreement with the basic assumptions of modern neo-Darwinism ("Macroevolution ... is composed of numerous small microevolutionary steps (additive typogenesis)" or of "uncountable successive small microevolutionary steps...." – see the details above).

So this is what the synthetic theory really needs to prove its case for the giraffidae: many continuous series in Darwin's sense, not isolated genera with some intermediary features appearing as late as or later than the long-necked giraffes and living contemporaneously with them for millions of years.

The reason or basis for the absence of such continuous series may consist in the functional limits due to the law of correlation (Cuvier) on almost all biological levels, and to the related law of recurrent variation concerning mutagenesis (http://www.weloennig.de/Loennig-Long-Version-of-Law-of-Recurrent-Variation.pdf) corroborating Cuvier's insights. He defined the law of correlation as follows:

> "Every organized being constitutes a whole, a single and complete system, whose parts mutually correspond and concur by their reciprocal reaction to the same definitive end. None of these parts can be changed without affecting the others; and consequently each taken separately indicates and gives all the rest."
> http://aleph0.clarku.edu/huxley/comm/ScPr/Falc.html (See the French original text below.***)

Living beings are, in fact, highly integrated, functional systems (all parts being correlated with limited space or tolerance concerning functional variation), which permits microevolution generating intermediate forms to a certain extent, but precludes infinite transformations. The law of correlation can be illustrated by Pierre Paul Grassé's remark on the eye as follows:

> "In 1860 Darwin considered only the eye, but today he would have to take into consideration all the cerebral connections of the organ. The retina is indirectly connected to the striated zone of the occipital lobe of the cerebral hemispheres: Specialized neurons correspond to each one of its parts – perhaps even to each one of its photoreceptor cells. The connection between the fibers of the optic nerve and the neurons of the occipital lobe in the geniculite body is absolutely perfect."

As to the eye, see please http://www.weloennig.de/AuIn.html. We have seen on pp. 9 and 10 above, that the law of correlation is also relevant for the long-necked giraffes as coadaptation/synorganization.

Every **intermediate macroevolutionary step** would thus necessitate the coordinated change of many genes and physiological and anatomical functions. How much faith is required to believe that random ('micro'-)mutations could really afford this task? What about intelligent design to implement such or similar steps?

Another point: Prof. W. R. Thompson made the following instructive comment on intermediates in his introduction to Darwin's *Origin of Species* on the geographic level, properly applying this insight also to paleontology (1967, p. xix):

> "As the range of our collections extends, so we invariably enrich our representation of various groups, and this **necessarily and inevitably entails the appearance of intermediates between the forms in the collection** from the restricted area in which we started. The recognition of this fact, with respect to the collections of organisms existing here and now, **does not necessarily commit us to any particular view of the origin of species**; and the same thing is true of the collection of fossil material."

Morphologic space within families like the giraffidae is not infinite and thus unavoidably entails the existence of at least some 'intermediates' (more exactly, 'mosaic forms') in any family with a plethora of genera and species, whatever their cause of origin. To a certain extent this appears to be true also for some higher taxonomic entities. Yet, as Thompson aptly stated on p. xvi of his introduction:

> "On the Darwinian theory, evolution is essentially undirected, being the result of natural selection, acting on small fortuitous variations. The argument specifically implies that **nothing is exempt from this evolutionary process**. Therefore, **the last thing we would expect on Darwinian principles is the persistence of a few common fundamental structural plans** [the phyla and within them the many equally well defined subordinate groups]. Yet, this is what we find."[o]

Hence, a general assertion of a "perfect intermediate"[v] for the neck of the giraffe to prove Darwin's idea of evolution by "insensibly fine steps" etc. without the indispensable scientific discussion of the details and objections mentioned above, may be quite useful for propagandistic purposes on the false premise that only a mindless process could be responsible for its origin[x], but is *definitely insufficient and unqualified on the scientific level*. Let us hope that an unbiased, profound and critical scientific report on the fossil find will follow soon.

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*Donald Prothero: *What missing link?* New Scientist, 27 February/1 March 2008, pp. 35-41. On page 35 we read: "Darwin's 1859 prediction that transitional forms would be found was quickly confirmed." Yet, Prothero qualifies the term "transitional form" as follows: "A transitional form need not to be a perfect halfway house directly linking one group of organisms to another. It merely needs to record aspects of evolutionary change that occurred as one lineage split from another". Well, by this rather imprecise definition in combination with unprovable evolutionary presuppositions almost anything can serve as a transitional form (to perhaps overstate the problem somewhat).

However, according to the same author, the situation seems to be somewhat different in the case of the giraffe, for he answers the question "**How did the giraffe get its long neck?**" with the ensuing sentences (p. 40): "This question has puzzled biologists as far back as the early 18th century naturalist Jean-Baptiste Lamarck, who famously – and wrongly – speculated that the giraffe's ancestors had stretched their necks in search of food and passed this "acquired characteristic" onto their offspring."

Here Prothero omits to mention that Darwin speculated in a similar way as follows (*Origin of Species*, 1872/1967, pp. 24/25):

> "**Changed habits produce an inherited effect** as in the period of the flowering of plants when transported from one climate to another. **With animals the increased use or disuse of parts has had a more marked influence.** The great and inherited development of the udders in cows and goats in countries where they are habitually milked, in comparison with these organs in other countries, is probably another instance of the effect of use. Not one of our domestic animals can be named which has not in some country drooping ears; and the view which has been suggested that **the drooping is due to the disuse of the muscles of the ear**, from animals being seldom alarmed, **seems probable**."

And concerning the origin of the giraffe, Darwin combined natural selection with "the inherited effects of the increased use of parts" (p. 202):

> "...natural selection will preserve and thus separate all the superior individuals, allowing them to intercross, and will destroy all the inferior individuals. By this process long continued, which exactly corresponds with what I have called unconscious selection by man, *combined no doubt* **in a most important manner with the inherited effects of the increased use of parts**, it seems to me almost certain that an ordinary hoofed quadruped might be **converted into a giraffe.**"

For a direct comparison let's have a look at Lamarck ideas (quoted according to Gould 2002, p. 188:

> "It is interesting to observe **the result of habit** in the peculiar shape and size of the giraffe […] known to live […] in places where the soil is nearly always arid and barren, so that it is obliged to browse on the leaves of trees and to make **constant efforts to reach them**. From **this habit long maintained** in all its race, it has resulted that the animal's fore-legs have become longer than its hind-legs, and that its neck is lengthened to such a degree that the giraffe […] attains a height of six meters (1809, p. 122)."

Prothero continues: "The giraffe fossil record is fairly good, with a wide variety of species known from the Miocene. These sported a range of weirdly shaped horns, but all had short necks rather like that of the only other living species of giraffid, the okapi. Only in the late Miocene do we see the fossils of long-necked giraffes. Like modern giraffes, they have an extra vertebra in the neck - recruited from the back - and lengthened neck vertebrae.

Until recently, there was no fossil evidence **linking the long-necked giraffes to their short-necked relatives.** But as my book went to press, news emerged that Nikos Solounias of the New York Institute of Technology had described [but not yet published] a fossil giraffe from the late Miocene and early Pliocene. **Its neck is a perfect intermediate between the short-neck ancestors and their long-neck descendants**" (emphasis added).

Thus, Prothero's message clearly is: Now we have, indeed, fossil evidence (although unpublished so far) **linking** the long-necked giraffes to their short-necked relatives. If the neck were a "perfect intermediate" ("a perfect halfway house", which may be doubted for the reasons given above) – what about all the other features of the animal? (See the facts and arguments concerning coadaptation/synorganization listed on pp. 4, 9, and 10.)

Also, Prothero's assertion that "A transitional form … merely needs to record aspects of evolutionary change that occurred as one lineage split from another" presupposes much of the neo-Darwinian worldview of continuous evolution and is at odds with, for example, T. H. Huxley's drawing of a hypothetical intermediate link between dinosaurs and birds, displaying an entire range of intermediate characters.

**If, however, V8 (see *Part 2*, p. 53) displayed further intermediate features, Lankester's hypothesis that this neck vertebra was only a "cervicalized" thoracic would be reinforced.

***"Tout être organisé forme un ensemble, un système unique et clos, dont les parties se correspondent mutuellement, et concourent à la même action définitive par une réaction réciproque. Aucune de ces parties ne peut changer sans que les autres changent aussi; et par conséquent chacune d'elles, prise séparément, indique et donne toutes les autres" (Cuvier 1825): http://records.viu.ca/~johnstoi/cuvier/cuvier-f12.htm. There are several English translations. This one is also fine: "Every organized being forms a whole, a unique and closed system, in which all the parts correspond mutually, and contribute to the same definitive action by a reciprocal reaction. None of its parts can change without the others changing too; and consequently each of them, taken separately, indicates and gives all the others." http://www.ansp.org/museum/jefferson/otherPages/cuvier_revolutions.php

Similarly the botanist Antoine-Laurent de Jussieu stated (1789): "C'est dans cette dépendance mutuelle des fonctions, et ce secours qu'elles se prêtent réciproquement, que sont fondées les lois qui déterminent les rapports de leurs organes, et qui sont d'une nécessité égale à celle des lois métaphysiques ou mathématiques: car il est évident que l'harmonie convenable entre les organes qui agissent les uns sur les autres, est une condition nécessaire de l'existence de l'être auquel ils appartiennent, et que si une de ses fonctions étoit modifiée d'une manière incompatible avec les modifications des autres, cet être ne pourroit pas exister" (quoted according to evolutionist Jean-Pierre Gasca (2006): Cent ans après Marey: Aspects de la morphologie fontionnelle aujourd'hui, Comptes Rendus Palevol **5**, 489-498). **Any scientist who has ever systematically worked with mutants will immediately be able to give a range of examples corroborating this verdict.**

[o] See also http://www.weloennig.de/AesVIII2.html and the following chapter, and this paper, *Part 2*, p. 95/96.

[v]As implied by the text above, this would also be true for a general assertion concerning **several** of such "intermediate" genera. What Darwinism needs to prove its case for the giraffidae and other families are 'unmistakable species-to-species transitions' etc. (see above pp. 11, 15/16, 19).

[x]For example, in his book *The Great Chain of Being* Arthur Lovejoy (1936/1964) has carefully documented the fact that for about 2,000 years any newly discovered intermediate link (real or imagined) was viewed to be another powerful proof for the truth of the entirely static Platonic world view ("the immutable essences of things", Lovejoy p. 34) for many philosophers and naturalists alike. And "the safest general characterization of the European philosophical tradition is that it consist in a series of footnotes to Plato" – Whitehead according to Lovejoy, p. 24.

Lovejoy pp. 50/51 on Plato's myths, whose implications were taken seriously even by high-ranking intellectuals like Gottfried Wilhelm Leibniz: "To the ... question – **How many kinds of temporal and imperfect beings must this world contain?** – the answer follows the same dialectic: ***all* possible kinds**. The "best soul" could begrudge existence to nothing that could conceivably possess it, and "desired that all things should be as like himself as they could be." "All things" here could consistently mean for Plato nothing less than the sensible counterparts of every one of the Ideas; and, as Parmenides in the dialogue bearing his name (I3oc, e) reminds the young Socrates, there are in the World of Ideas the essences of all manner of things, even things paltry or ridiculous or disgusting. In the *Timaeus,* it is true, Plato speaks chiefly of "living things" or "animals"; but with respect to these, at least, he insists upon the necessarily complete translation of all the ideal possibilities into actuality. It must not, he says, "be thought that the world was made in the likeness of any Idea that is merely partial; for nothing incomplete is beautiful. We must suppose rather that it is the perfect image of the whole of which all animals – both individuals and species – are parts. For the pattern of the universe contains within itself the intelligible forms of all beings just as this world comprehends us and all other visible creatures. For the Deity, wishing to make this world like the fairest and most perfect of intelligible beings, **framed one visible living being containing within itself all other living beings of like nature**," that is temporal and sensible. … It is because the created universe is an exhaustive replica of the World of Ideas that Plato argues that there can be only one creation; it includes the copies "of all other intelligible creatures," and therefore there is, so to say, nothing left over in the model after which a second world might be fashioned. So, in the form of a myth, the story of the successive creation of things is told. After all the grades of immortal beings have been generated, the Demiurgus notes that mortals still remain uncreated. This will not do; if it lack even these the universe will be faulty, "**since it will not contain all sorts of living creatures, as it must do if it is to be complete**." In order, then, that "the Whole may be really All," the Creator [in distinct contrast to Genesis 1 und 2, note also the offer for everlasting life to the first human pair; – for further differences see http://en.wikipedia.org/wiki/Timaios] deputed to the lesser divinities who had already been brought into being the task of producing mortal creatures after their kinds. And thus "the universe was filled completely with living beings, mortal and immortal," and thereby became "a sensible God, which is the image of the intelligible – the greatest, the best, the fairest, the most perfect." In short, Plato's Demiurgus acted literally upon the principle in which common speech is wont to express the temper not only of universal tolerance but of comprehensive approbation of diversity that **it takes all kinds to make a world**."

The following exposition of Lovejoy (pp. 231-233) on the application of Plato's ideas in science reads to a large extent like the program of modern evolutionary biology:

"Even for those biologists [of the eighteenth century] who did not explicitly reject the belief in natural species, the principle of continuity was not barren of significant consequences. It set naturalists to **looking for forms which would fill up the apparently "missing links" in the chain**. Critics of the biological form of this assumption attacked it largely on the ground that many links which the hypothesis required *were* missing. But **the more accepted view was that these gaps are only apparent**; they were due, as Leibniz had declared, "only to the incompleteness of the knowledge of nature then attained, or to the minute size of many of the — presumably lower — members of the series. The metaphysical assumption thus furnished a program for scientific research. It was therefore highly stimulating to the work of the zoologist and the botanist, and especially to that of the microscopist, in the eighteenth century. Every discovery of a new form could be regarded, not as the disclosure of an additional unrelated fact in nature, but as a step towards the completion of a systematic structure of which the general plan was known in advance, an additional bit of empirical evidence of the truth of the generally accepted and cherished scheme of things. Thus the theory of the Chain of Being, purely speculative and traditional though it was, had upon natural history in this period an effect somewhat similar to that which the table of the elements and their atomic weights has had upon chemical research in the past half-century. The general program of the Royal Society, wrote its first historian (1667), in an interesting passage in which Platonistic and Baconian motives are conjoined, was **to discover unknown facts of nature in order to range them properly in their places in the Chain of Being**, and at the same time to make this knowledge useful to man.

Such is the dependence amongst all the orders of creatures; the animate, the sensitive, the rational, the natural, the artificial; that the apprehension of one of them, is a good step towards the understanding of the rest. And this is the highest pitch of humane reason: **to follow all the links of this chain**, till all their secrets are open to our minds; and their works advanc'd or imitated by our hands. This is truly to command the world; to rank all the varieties and degrees of things so orderly upon one another; that standing on the top of them, we may perfectly behold all that are below, and make them all serviceable to the quiet and peace and plenty of Man's life. And to this happiness there can be nothing else added: but that we make a second advantage of this rising ground, thereby to look the nearer into heaven…[12]

The *Encyclopedie* in the middle of the eighteenth century also, though in a less devout tone, dwelt upon this as the program of the advancement of knowledge: Since "**everything in nature is linked together**," since "beings are connected with one another by a chain of which we perceive some parts as continuous, though in the greater number of points the continuity escapes us," the "art of the philosopher consists in adding new links to the separated parts, in order to reduce the distance between them as much as possible. But we must not flatter ourselves that gaps will not still remain in many places." It was, in the eyes of the eighteenth century, a great moment in the history of science when Trembley in 1739 rediscovered the fresh-water polyp ***Hydra*** (it had already been observed by Leeuwenhoek), this creature being at once hailed as the **long-sought missing link between plants and animals** – for which Aristotle's vague zoophytes were no longer considered quite sufficient. This and similar discoveries in turn served to strengthen the faith in plenitude and continuity as *a priori* rational laws of nature; and the greater credit, it was sornetimes remarked, was due to those who, not having seen, yet had believed in these principles. The chief glory, said a German popularizer of science, *à propos* of Trembley's work, is that "of the German Plato [Leibniz], who did not live to know of the actual observation" of this organism, "yet through his just confidence in the fundamental principles which he had learned from nature herself, had predicted it before his death."

The quest of organisms not yet actually observed which would fill these lacunae was prosecuted with especial zeal at two points in the scale: near the bottom of it, and in the interval between man and the higher apes. "Nature," remarked Bonnet, "seems to make a great leap in passing from the vegetable to the fossil [i. e., rock]; there are no bonds, no links known to us, which unite the vegetable and the mineral kingdoms. But shall we judge of the chain of beings by our present knowledge? **Because we discover some interruptions, some gaps in it here and there, shall we conclude that these gap's are real?** …The gap that we find between the vegetable and the mineral will apparently some day be filled up. There was a similar gap between the animal and the vegetable; the polyp has come to fill it and to demonstrate the **admirable gradation there is between all beings**."

But the program of discovering the hitherto unobserved links in the chain played a part of especial importance in the beginnings of the science of anthropology."

Now, the creationist assumption that there are <u>no</u> mosaic forms with some intermediate characters is as false as the evolutionary and Platonic views of the (living) world that there are <u>only</u> intermediates. The gaps at least between the higher systematic categories are real, but in many cases the distances are definitely not as large as once assumed by many creation scientists and on the genetic level also by almost all evolutionists (see the topic "genetic conservation" in http://www.weloennig.de/DynamicGenomes.html). Evidently, there was (and is) much more elegant simplicity, unity and order in complexity as well as an unfathomable abundance of thoughts in the ingenious and prolific mind of the Designer than humans have imagined or can ever envisage (Psalm 139: 17-18).

<u>End of note of 9 October 2008 (last modified 16 November 2008)</u>.

(2) "However, bird flu actually exists. Concerning evolution, on the other hand, one is looking for a black cat in a dark room, where, in reality, there is no cat at all, yet one continually yells: I have found it." – Remarks of Dr. Werner Gieffers.

(2a, from p. 9) Dietrich Starck 1995, p. 206: "...in giraffes the blood pressure in arteries near the heart is very high (systolic 260-350 mm Hg), in the brain arteries however it is more or less the same level as in short-necked hoofed animals (130 mm Hg). The high pressure in the cartoid (heart) arteries is necessary in order to overcome the large hydrostatic differences in the standing animal (3 m neck length). The drop of pressure in the brain blood vessels is achieved by the *rete mirabile* in the cartoid arteries, which serves as a protection mechanism for the brain."

(2b, from p. 9) Wesson 1991, p. 226: "...an important part of the adaptation of the giraffe would have been protogiraffes' copying one another in stretching toward higher leaves, and this would promote the selective process favoring longer-necked mutants. This still leaves a lot for natural selection to explain. The protogiraffe had not only to lengthen neck vertebrae (fixed at seven in mammals [but with some exceptions, including the giraffe with its 8 neck vertebrae; my note]) but to make many concurrent modifications: the head, difficult to sustain atop the long neck, became relatively smaller; the circulatory system had to develop pressure to send blood higher; valves were needed to prevent overpressure when the animal lowered its head to drink; big lungs were necessary to compensate for breathing through a tube 10 feet long; many muscles, tendons, and bones had to be modified harmoniously; the forelegs were lengthened with corresponding restructuring of the frame; and many reflexes had to be reshaped. All these things had to be accomplished in step, and they must have been done rapidly because no record has been found of most of the transition. That it could all have come about by synchronized random mutations strains the definition of random. The most critical question, however, is how the original impetus to giraffeness – and a million other adaptations – got started and acquired sufficient utility to have selective value (John and Miklos 1988, 236)."

For additional examples clarifying Wessons "most critical question" see Markus Rammerstorfer http://members.aon.at/evolution/gererk.html

As to further remarkable features of the long-necked giraffe, R. Peachey quotes Lynn Hofland as follows:

> "Equally marvellous is the fact the blood does not pool in the legs, and a giraffe does not bleed profusely if cut on the leg. The secret lies in an extremely tough skin and an inner fascia [fibrous connective tissue] that prevents blood pooling. This skin combination has been studied extensively by NASA scientists in their development of gravity-suits for astronauts. Equally helpful to prevent profuse bleeding is that all arteries and veins in the giraffe's legs are very internal. The capillaries that reach the surface are extremely small, and the red blood cells are about one-third the size of their human counterparts, making capillary passage possible. It quickly becomes apparent that these unique facets of the giraffe are all interactive and interdependent with its long neck. But there's more. The smaller red blood cells allow for more surface area and a higher and faster absorption of oxygen into the blood. This helps to retain adequate oxygen to all extremities, including the head."

(2c, from p. 11) The Bible: according to Hebrews 11:1, ironically modified by Lunn. The King James Version of 1611 translates: "Now faith is the substance of things hoped for, the evidence of things unseen." However, some modern translations render the original text and its much deeper and evidence-based meaning far more accurately, for example: "Faith is the **assured** expectation of things hoped for, the **evident demonstration of realities** though not beheld" (NW). In the present book I use Lunn's version here only in its ironical sense, which is, however, the sense in which uninformed evolutionists usually misunderstand it.

(2c1) und (2c2, from p. 13): Retrieved Sept. 12, 2011; for unknown reasons some of the original Spanish internet sites or links of 2006 cannot be opened any more. This is also true for the Spanish researcher quoted on page 5 of the present paper, for which I intended to set a link in the list of references in the second part on the evolution of the long-necked giraffe (the original quotation read: "Probablemente la família de los girafídeos evolucionó de los *Climacoceras*;...").

(2d, still p. 13) Boundaries for the Middle Miocene according to Hardland et al. (1990) und Kearey (ed.) (1993). Kearey differs slightly from these data setting the limits at 16,2 and also 10,4 million years respectively (p. 401, Fig. M14 Miocene). However, Robert A. Rohde's numbers for the Middle Miocene are 15,97 and 11,608 million years (see http://www.stratigraphy.org/geowhen/stages/Miocene.html, last update 2005). Yet, these numbers may again not be the last word in this matter. Nevertheless this recent redating may also raise the maximum age for *Bohlinia* – a question which needs further investigation. If the dates presented by Rohde for the boundaries of the Middle Miocene were correct as well as the maximum age given so far for *Bohlinia*, this genus would approach the Middle Miocene but not be represented there.

(3, from p. 18) Regarding *Bohlinia,* see the citation on page 5 of the present article as well as Hamilton (1978, p. 212): "...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 writes (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 vraisemblance homologues (ossicônes)."

The recurrent laryngeal nerve (Supplement 26 August 2010 and 29 September 2010): Much ado has been made in recent years by evolutionsts like Richard Dawkins, Jerry Coyne, Neil Shubin, Matt Ridley and many others about the *Nervus laryngeus recurrens* as a "proof" or at least indisputable evidence of the giraffe's evolution from fish (in a gradualist scenario over millions of links, of course): Markus Rammerstorfer has written a (scientifically detailed and convincing) synoptic critique on this old and, in fact, already long disproved evolutionary interpretation of the course of this nerve in 2004 (see Rammerstorfer http://members.liwest.at/rammerstorfer/NLrecurrens.pdf). There are several main points which I would like to mention here:

1. As to the evolutionary scientists just mentioned: A totally nonsensical and relictual misdesign would be a severe contradiction in their own neo-Darwinian (or synthetic evolutionary) world view. Biologist and Nobel laureate Francois Jacob described this view on the genetic level as follows: "The genetic message, the programme of the present-day organism ... resembles a text without an author, that a proof-reader has been correcting for more than two billion years, ***continually improving, refining and completing it, gradually eliminating all imperfections***." The result in the Giraffe? Jerry Coyne: "One of nature's ***worst designs*** is shown by the recurrent laryngeal nerve of mammals. Running *from the brain to the larynx*, this nerve helps us to speak and swallow. The curious thing is that ***it is much longer than it needs to be***" (quoted according to Paul Nelson 2009). And: "…it *extends down the neck to the chest*…and then runs back up the neck to the larynx. In a giraffe, that means a 20-foot length of nerve where 1 foot would have done" (Jim Holt in the *New York Times*, 20 February 2005: http://www.nytimes.com/2005/02/20/magazine/20WWLN.html). "Obviously a ridiculous detour! No engineer would ever make a mistake like that!" – Dawkins 2010 (see below) (All italics above mine.)

Apart from the facts that the nerve *neither runs from the brain to the larynx nor extends down from the neck to the chest* ("On the *right* side it arises from the vagus nerve in front of the first part of the subclavian artery;..." "On the *left* side, it arises from the vagus nerve on the left of the arch of the aorta..." – Gray's Anatomy 1980, p. 1080; further details (also) in the editions of 2005, pp. 448, 644, and of 2008, pp. 459, 588/589), the question arises: ***why did natural selection not get rid of this "worst design" and improve it during the millions of generations and mutations from fish to the giraffe onwards?*** Would such mutations really be impossible?

2. The fact is that even in humans in 0.3 to 1% of the population the *right* recurrent laryngeal nerve is indeed shortened and the route abbreviated in connection with a retromorphosis of the forth aortic arch. ("An unusual anomaly … is the so-called 'non-recurrent' laryngeal nerve. In this condition, which has a frequency of between 0.3 – 1%, only the right side is affected and it is always associated with an abnormal growth of the right subclavian artery from the aortic arch on the left side" – Gray's Anatomy 2005, p. 644.; see also Uludag et al. 2009 http://casereports.bmj.com/content/2009/bcr.10.2008.1107.full; the extremely rare cases (0.004% to 0.04%) on the left side appear to be always associated with *situs inversus*, thus still "the right side"). Nevertheless, even in this condition its branches still innervate the upper esophagus and trachea (but to a limited extent?). Although this variation generally seems to be without severe health problems, it *can* have catastrophic consequences for the persons so affected: problems in deglutition (difficulties in swallowing) and respiratory difficulties (troubles in breathing) (see Rammerstorfer 2004; moreover "**dysphagia (**if the **pharyngeal and oesophageal branches** of nonrecurrent or recurrent inferior laryngeal nerve are injured)" – Yang et al, 2009: http://journals.cambridge.org/action/displayAbstract?fromPage=online&aid=5868576).

If mutations for such a short cut are possible and regularly appearing even in humans (not to mention some other non-shorter-route variations), – according to the law of recurrent variation (see Lönnig 2005: http://www.weloennig.de/Loennig-Long-Version-of-Law-of-Recurrent-Variation.pdf, 2006: http://www.weloennig.de/ShortVersionofMutationsLawof_2006.pdf), they must have occurred already millions of times in all mammal species and other vertebrates taken together from the Silurian (or Jurassic respectively) onwards. ***And this must also be true for any other*** (at least residually) ***functionally possible shorter variations of the right as well as of the left recurrent laryngeal nerve***. Inference: **All these 'short-cut mutations' were regularly counter-selected due to at least some disadvantageous and unfavourable effects on the phenotype of the so affected individuals** (including any such mutants in the giraffes). Hence, they never had a chance to permeate and dominate a population except for the above mentioned very small minority of the (right) 'non-recurrent' laryngeal nerve, which is perhaps already accounted for by the genetic load ("The embryological nature of such a nervous anatomical variation results originally from a vascular disorder, named **arteria lusoria** in which the fourth right aortic arch is abnormally absorbed, being therefore unable to drag the right recurrent laryngeal nerve down when the heart descends and the neck elongates during embryonic development." Defechereux et al. 2000: http://www.ncbi.nlm.nih.gov/pubmed/10925715). Thus, even from a neo-Darwinian point of view, important additional functions of the *Nervus laryngeus recurrens* should be postulated and looked for, not to mention the topic of embryological functions and constraints.

3. However, just to refer to one possible substantial function of the Nervus laryngeus recurrens sinister during embryogenesis: "The vagus nerve in the stage 16 embryo is very large in relation to the aortic arch system. The recurrent laryngeal nerve has a greater proportion of connective tissue than other nerves, making it more resistant to stretch. It has been suggested that ***tension applied by the left recurrent laryngeal nerve as it wraps around the ductus arteriosus could provide a means of support that would permit the ductus to develop as a muscular artery***, rather than an elastic artery" – Gray's Anatomy, 39th edition 2005, p. 1053.

4. Yet, implicit in the ideas and often also in the outright statements of many modern evolutionists like the ones mentioned above is the assumption that the only function of the *Nervus laryngeus recurrens sinister* (and *dexter*) is innervating the larynx and nothing else. Well, is it asking too much to state that they should really know better? In my copy of the 36th edition of Gray's Anatomy we read (1980, p. 1081, similarly also in the 40th edition of 2008, pp. 459, 588/589):

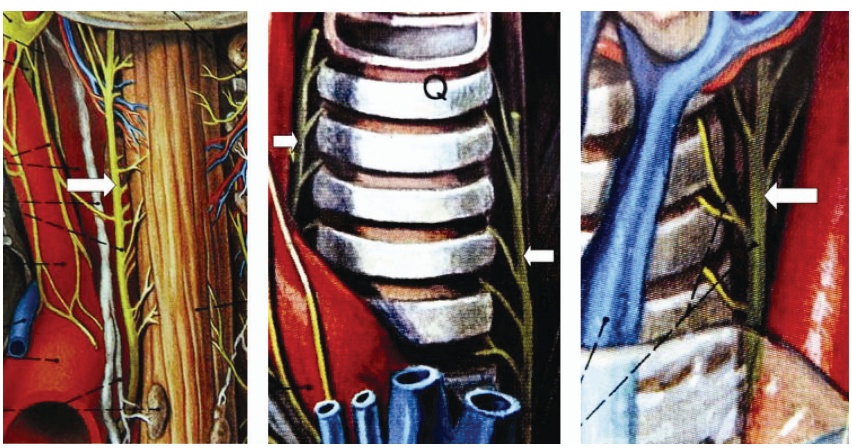
> "As the recurrent laryngial nerve curves around the subclavian artery or the arch of aorta, it gives ***several cardiac filaments to the deep part of the cardiac plexus***. As it ascends in the neck ***it gives off branches***, more numerous on the left than on the right side, ***to the mucous membrane and muscular coat of the oesophagus***; ***branches to the mucous membrane and muscular fibers of the trachea*** and some filaments to the inferior constrictor [Constrictor pharyngis inferior]."

Likewise Rauber/Kopsch 1988, Vol. 4, p. 179, *Anatomie des Menschen*: "Äste des N. laryngeus recurrens ziehen zum ***Plexus cardiacus*** und zu ***Nachbarorganen*** [adjacent organs]**.**" On p. 178 the authors of this *Anatomy* also mention in Fig. 2.88: "***Rr.*** [Rami, branches] ***tracheales und oesophagei des*** [of the] ***N. laryngeus recurrens***." – The mean value of the number of the branches of Nervus laryngeus recurrens sinister **innervating the trachea und esophagus** is **17,7** and for the Nervus laryngeus recurrens dexter is **10,5** ("Zweige des N. recurrens ziehen als Rr. cardiaci aus dem Recurrensbogen abwärts zum Plexus cardiacus – als *Rr. tracheales* und *esophagei* zu oberen Abschnitten von Luft- und Speiseröhre, als N. laryngeus inferior durch den Unterrand des M. constrictor pharyngis inferior in den Pharynx. An der linken Seite gehen 17,7 (4-29) Rr. tracheales et esophagei ab, an der rechten 10,5 (3-16)" – Lang 1985, p. 503; italics by the author(s)).

I have also checked several other detailed textbooks on human anatomy like *Sobotta –Atlas der Anatomie des Menschen*: they are all in agreement. Some also show clear figures on the topic. *Pschyrembel* – Germany's most widely circulated and consulted medical dictionary (262 editions) – additionally mentions "Rr. bronchiales".

To innervate the ***esophagus and trachea*** of the giraffe ***and also reach its heart***, **the recurrent laryngeal nerve needs to be, indeed, very long**. So, today's evolutionary explanations (as is also true for many other so-called rudimentary routes and organs) are not only often in contradiction to their own premises but also tend to stop looking for (and thus hinder scientific research concerning) further important morphological and physiological functions yet to be discovered. In contrast, the theory of intelligent design regularly predicts further functions (also) in these cases and thus is scientifically much more fruitful and fertile than the neo-Darwinian exegesis (i.e. the interpretations by the synthetic theory).

To sum up: The *Nervus laryngeus recurrens* innervates not only the larynx, but also the esophagus and the trachea and moreover "gives several cardiac filaments to the deep part of the cardiac plexus" etc. (the latter not shown below, but see quotations above). It need not be stressed here that all mammals – in spite of substantial synorganized genera-specific differences – basically share the same *Bauplan* ("this infinite diversity in unity" – Agassiz) proving the same ingenious mind behind it all.



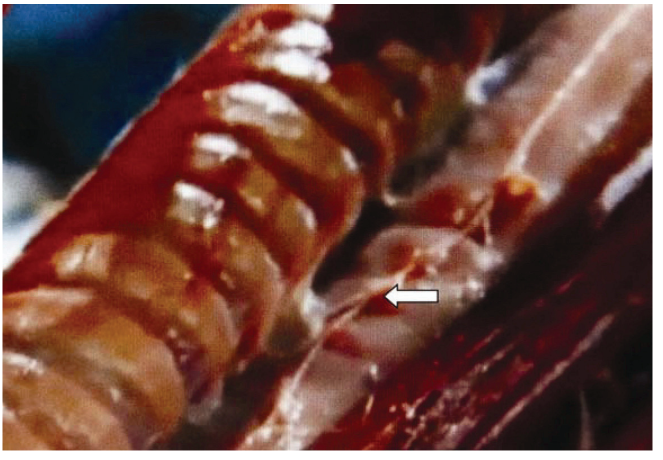
Left: Detail from a figure ed. by W. Platzer (enlarged, contrast reinforced, arrow added): In yellow beside the esophagus (see arrow): *Nervus laryngeus recurrens sinister* running parallel to the oesophagus on left hand side with many branches innervating it (dorsal view).[(1)]

Middle: Detail from a figure ed. by W. Platzer (enlarged, contrast reinforced, arrows added): Now on the right because of front view: *Nervus laryngeus recurrens sinister* and on the left *Nervus laryngeus recurrens dexter* (arrows) sending branches to the trachea.[(2)]

Right: Detail from a figure ed. by W. Platzer (enlarged, contrast reinforced, arrow added): Again on the right (arrow) because of front view: *Nervus laryngeus recurrens sinister* (as in the middle Figure, but more strongly enlarged), sending branches to the trachea.[(3)]

Fig. (1), (2) and (3): All three figures (details) from Werner Platzer (editor) (1987): *Pernkopf Anatomie, Atlas der topographischen and* *angewandten Anatomie des Menschen*. Herausgegeben von W. Platzer. 3., neubearbeitete und erweiterte Auflage. Copyright Urban & Schwarzenberg, München – Wien – Baltimore. Fig. (1): Detail from *Das Mediastinum von dorsal*, 2. Band. Brust, Bauch und Extremitäten, p. 83, Abb. 79. – Fig. 2: Detail from *Die prae- und paravertebralen Gebilde nach Entfernung des Eingeweidetraktes in der Ansicht von vorne*, 1. Band. Kopf und Hals, p. 344, Abb. 396, drawn by K. Endtresser 1951. – Fig. (3): Detail from *Topic der Pleuralkuppeln und des Halseingeweidetraktes in der Ansicht von vorne*, 1. Band. Kopf und Hals, p. 333, Abb. 388, drawn by F. Batke 1951.

As to the giraffe, direct evidence for more functions of the laryngeal nerve than just innervating the larynx and nothing else, was quite unintentionally provided by R. Dawkins and J. S. Reidenberg on YouTube (17 March 2010, but first shown on British TV in 2009, Channel 4) in their contribution *Laryngeal Nerve of the Giraffe Proves Evolution* (http://www.youtube.com/watch?v=0cH2bkZfHw4) showing directly some of the branches of the N. laryngeus recurrens innervating the oesophagus and the trachea (see 2:09):



The Nervus laryngeus recurrens obviously displaying some of the branches innervating the oesophagus and trachea in *Giraffa camelopardalis*. Photo of detail from the YouTube video of Dawkins (2010) *Laryngeal Nerve of the Giraffe Proves Evolution*: http://www.youtube.com/watch?v=0cH2bkZfHw4: 2:07/2:09 (arrow added; study, please, especially carefully the sequence of the pictures from 2:07 to 2:11).

Note, please, how Dawkins at 0:28 and later the anatomist Joy S. Reidenberg are unwarrantedly ***equating the vagus nerve with the laryngeal nerve*** in the video**.** Dr. Reidenberg in her explanations starting at 1:17 first says correctly about the N. laryngeus recurrens: "…It actually **starts out not as a separate nerve, but as a branch coming off of a bigger nerve called the vagus nerve** and this [*the vagus*] is going to keep running all the way down the body, so you can see it again over here all the way down the neck, on both sides. … And this [*the vagus*] is going to wrap around the great vessels coming out of the heart. … **So here is the vagus going down and here is the vagus continuing**. And **right over here**, there is a **branch**, right there [namely ***the N. laryngeus recurrens*** *very near the great vessels coming out of the heart*]. So it's looping and coming back, doing a U-turn all the way down here [*at that point she seems to start equating the laryngeal with the vagus nerve*]. So it [*actually the **vagus**, not the laryngeal nerve*] has travelled that entire distance to make a U-turn [*and now concerning **its new branch**, the laryngeal nerve:*] to go all the way back again.* And so we can follow it back up again. So we follow this branch. And if we look we see it again over here. Here it is. Like that [2:07; see above]. And here you see it going up, this is the voice box, the larynx. …also coordinating breathing and swallowing in this area [*yet, not only in this area*!]. So this is a very important nerve. Interestingly, where it [*the laryngeal nerve*] ends is pretty close to where it started" [*wrong; it really started **near the vessels coming out of the heart** – see above*]. Reidenberg continues: "It started here coming out of the brain [*totally wrong; this is where the **vagus nerve** started*]. It really needs to go about two inches. But it [*the vagus nerve really*] went all the way down and it [*the laryngeal nerve*] came all the way back." Dawkins: "It is a beautiful example of historical legacy as opposed to design." And then Joy Reidenberg again: "This is not an intelligent design. An intelligent design would be to go from here to here."

Following that, an intelligent point was raised by Mark Evans, the veterinary surgeon and presenter of the film *Inside Nature's Giants: The Giraffe*, which was first shown at full length (48Mins) on Monday 9pm, 20 July 2009, on Channel 4 (a UK public-service television broadcaster): "It does kind of beg the question, even in an animal that might have been many millions of years ago with its head down here: why the route 'round the blood vessels, ***unless there's a reason they were there to enervate something else***." This implicit question ("to enervate something else") was unjustifiably denied by Dawkins answering: "Well that was in earlier ancestors, then it was the most direct route. In fish." Etc. – followed by the typically inconsistent neo-Darwinian explanation (evolution 'continually improving, refining and completing the genetic message, eliminating all imperfections' (see above), yet stretching the laryngeal nerve for absolutely no functional reasons almost endlessly instead of ever finding a short cut etc.).

To repeat: the vagus and not the laryngeal nerve has travelled all the distance and it is **its entirely new branch, the laryngeal nerve (not the vagus) that goes all "the way back" innervating with many branches the heart, the laryx and the oesopahgus on its way*]. [Comments in brackets and footnote added by W-EL].

So is the recurrent laryngeal nerve really an "Obviously a ridiculous detour" etc. as Dawkins stated in the TV show 2009 and YouTube video 2010?

Wilhelm Ellenberger and Herrmann Baum sum up the multiple functions of that nerve in their *Handbook of Comparative Anatomy of Domestic Animals* as follows (only in German 1974/1991, p. 954, italics by the authors):

> "Der N. recurrens führt die Hauptmasse der Vagusfasern für das Herz (HIRT 1934) und gibt sie vor Austritt aus der Brusthöhle an den *Plexus cardiacus* (s. unten und Abb. 1409). Er gibt außerdem Zweige an den in der präkardialen Mittelfellspalte zwischen Trachea und den großen Blutgefäßen gelegenen *Plexus trachealis caud.* Und steht mit dem Ggl. cervicale caud. des N. sympathicus in Verbindung. Nach seinem Austritt aus der Brusthöhle gibt der N. recurrens im Halsbereiche jederseits Zweige ab, die einen *Plexus trachealis cran*. bilden und *Rami oesophagici* und *Rami tracheales* an Muskulatur und Schleimhaut von Speise- und Luftröhre schicken. Im Kehlkopfbereich verbinden sich dünne Zweige von ihm mit solchen des N. laryngicus cran. (siehe dort)."

For me, personally, it is really impressive, how evolutionists like Dawkins, Coyne, Reidenberg and other 'intellectually fulfilled atheists' inform the public on such scientific questions in contrast to the facts cited above.

May I suggest that an unbiased scientific anatomical examination of the laryngeal nerve of the giraffe would have – as far as possible – included attention to and dissection of all the branches of the nerve, including the queries for the "several cardiac filaments to the deep part of the cardiac plexus", the many "branches, more numerous on the left than on the right side, to the mucous membrane and muscular coat of the oesophagus" as well as the "branches to the mucous membrane and muscular fibers of the trachea" and perhaps even the "Rr. bronchiales" (Pschyrembel). So, when the opportunity arises, let's do such a more comprehensive dissection of that nerve all over again – and add, perhaps, the research question on an irreducibly complex core system concerning the route and function of that nerve.

This seems to be all the more important since some of the observations by Sir Richard Owen made on the dissection of three young giraffes – two of them 3 years old and one about 4 years of age – seem to deviate from those of Dr. Reidenberg. Although the great anatomist Owen also made some mistakes in his work on other organisms (mistakes, which especially Thomas H. Huxley liked to stress), Owen's findings on the giraffe should not be dismissed too easily. He writes (1841, pp. 231/232, italics his, bold added as also the comment in brackets):

> "From the remarkable length of the neck of the *Giraffe* the condition of the recurrent nerves became naturally a subject of interest: these nerves are readily distinguishable at the superior third of the *trachea,* but when sought for at their origin **it is not easy to detect them or to obtain satisfactory proof of their existence** [this comment seems to be in disagreement with what Dr. Reidenberg demonstrated by her dissection – she had no problems to detect it/them from the very beginning; also Owen's following observations seem to disagree with those of Reidenberg's to a certain extent]. Each nerve is not due, as in the short-necked *Mammalia,* to a single branch given off from the *nervus vagus,* which winds round the great vessels, and is continued of uniform diameter throughout their recurrent course, but it is formed by the reunion of **several small filaments derived from the *nervus vagus* at different parts of its course**.
>
> The following is **the result of a careful dissection of the left recurrent nerve**. The *nervus vagus* as it passes down in front of the arch of the *aorta* sends off **four small branches**, which bend round the arch of the *aorta* on the left side of the *ductus arteriosus;* the **two small branches** on the left side pass to the *oesophagus* and are lost in the oesophageal *plexus;* **the remaining two branches** continue their recurrent course, and ascend upon the side of the *trachea,* **giving off filaments which communicate with branches from the neighbouring oesophageal nerves**: these recurrent filaments also receive twigs from the oesophageal nerves, and thus increase in size, and **ultimately coalesce into a single nerve of a flattened form**, which enters the *larynx* above the cricoid cartilage and behind the margin of the thyroid cartilage." – (Similarly Owen 1868, p. 160.)

Nevertheless, Owen's observations of filaments, which are given off by the recurrent nerve(s) are obviously in agreement with what Joy S. Reidenberg found, yet failed to mention and draw attention to explicitly (see above).

I have to admit that – the more deeply I am delving into the harmonious complexity of biological systems – the more elegant and functionally relevant the entire systems appear to me, even down to 'pernickety detail' (to use one of Dawkins' expressions), including the *Nervus laryngeus recurrens sinister* and the *Nervus laryngeus recurrens dexter* with their many branches and functions also in the giraffe and their correspondingly appropriate lengths.

Incidentally, Graham Mitchell's slip of the tongue or perhaps better his formulation from his innermost feelings in connection with his investigations of the giraffe's lungs and mechanism of respiration appears to be rather revealing (even if meant only figuratively): "It couldn't have been more beautifully designed … [after a little pause] … evolved" [laughter]. See this captivating dissection and investigation of the giraffe's lung here: http://channel.nationalgeographic.com/episode/inside-the-giraffe-4308/Photos#tab-Videos/07902_00

"Design should not be overlooked simply because it's so obvious" – Michael J. Behe 2005. May I repeat in this context that even from a neo-Darwinian perspective it would be very strange to assume that only the laryngeal nerve(s) could be "more beautifully designed" in contrast to all the rest which already is (see Francois Jacob above).

As to further discussions, including the quotation above of Jerry Coyne according to Nelson, see Paul Nelson (2009): Jerry, PZ, Ron, faitheism, Templeton, Bloggingheads, and all that — some follow-up comments.

Notes added in proof

(29 September 2010 and 19 October 2010)

a) The recurrent laryngeal nerves and most probably also some of their many branches usually missed/overlooked by leading neo-Darwinian biologists today, have been known *for more than 1800 years* now. See, for instance, E. L. Kaplan, G. I. Salti, M. Roncella, N. Fulton, and M. Kadowaki (2009): *History of the Recurrent Laryngeal Nerve: From Galen to Lahey* http://www.springerlink.com/content/13340521q5723532/fulltext.pdf.

> "…it was Galen [ca. 129 to about 217 A. D.] who first described the recurrent laryngeal nerves in detail in the second century A. D." "He dissected these nerves in many animals – even swans, cranes, and ostriches because of their long necks…" "Because of Galens fame and the spread of his teachings, the recurrent laryngeal nerve was discussed by many surgeons and anatomists thereafter." – Kaplan et al. 2009, pp. 387, 389, 390.

The keen observer Claudius Galenos [Galen] – having discovered, concentrating on and meticulously dissecting the recurrent laryngeal nerves of many different species of mammals and birds[1] – must necessarily also have seen at least some of the their branches leading to other organs as well. Yet, in agreement with Lord Acton's verdict that "The worst use of theory is to make men insensible to fact", not only many of today's neo-Darwinians but also Galen himself missed the altogether some thirty branches of the RLNs due to his own peculiar 'pulley-theory' (see again http://www.springerlink.com/content/13340521q5723532/fulltext.pdf). Margaret Tallmadge May comments in her translation of *Galen on the Usefulness of Parts of the Body* (1968, p. 371, footnote 62) on his assertion that "both [recurrent] nerves pass upward to the head of the rough artery [the trachea] without giving off even the smallest branch to any muscle…": "*As Daremberg (in Galen [1854], I, 508]) intimates,* ***Galen is being ridden by his own theory here****. The recurrent nerve does, of course, give off various branches as it ascends*."

However, accepting the fact of the many branches given off by the recurrent laryngeal nerves innervating several other organs as well would have completely disproved Galen's own 'pulley-theory'[2] as it currently refutes the "ridiculous detour"-hypothesis of Dawkins and many other neo-Darwinians.

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[1] See some points written by Galen in the English translation of *On Anatomical Procedures, The later Books,* Translated by Duckworth (1962) under http://books.google.de/books?hl=de&lr=&id=P508AAAAIAAJ&oi, pp. 81-87 and especially pp. 203 ff.

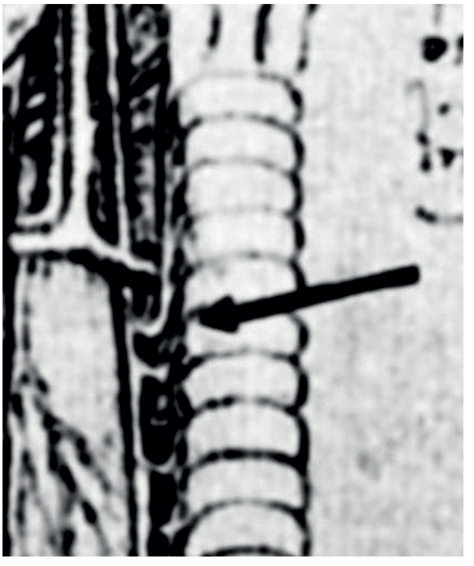
[2] There are, however, several hints that he saw more then his theory allowed: "And when it [the Nervus laryngeus recurrens dexter] is extending upward after the turn, Nature stretches out to it from the sixth pair **the handlike outgrowth** which binds it to the large nerve and makes both its turn and its ascend safe. The portions of the nerve on the two sides of the turn are supported on both the right and left **by the outgrowths** [*rami cardiaci inferiores*? Comment by M. T. May] of the sixth pair which it makes to the parts of that region" (May: *Galen on the Usefulness of Parts of the Body* 1968, II, p. 694). "When immediately the after the turn these [recurrent] nerves are mounting straight upward, **the large nerve extends to them an outgrowth**, as if reaching out a hand, and by means of this it draws and pulls them up" (May I, pp. 370/371). Margaret T. May comments in her footnote 61 to *The Seventh Book of Galen* (I, pp. 370/371):

> "The large nerve mentioned here is certainly the vagus itself; for in chapter 4 of Book XVI he mentions this helping hand extending to the recurrent nerve again and says that it comes from the "sixth" pair. Since no mention is made of it in *De nervorum dissectione* and no further light is ever shed on it either here or in *De anat. admin.,* XIV (Galen [1906, II, *189;* 1962, 207]), where it is described once more, I have been unable to determine what may have misled Galen. Neither Daremberg (in Galen [1854, I, 507]) nor Simon (in Galen [1906, II, *344])* has a satisfactory explanation. The former suggests "the superior cardiac nerves, or perhaps the anastomotic branch"; the latter says that it may be "**certain connecting twigs** which Galen had seen at the point of reflection, going from the recurrent to the vagus." I cannot find these connecting twigs described elsewhere. Dr. Charles GOSS, however, tells me that "the vagus in the neck of a pig in a recent atlas is labelled vagosympathetic trunk. This gives ample opportunity for communicating fibers." Cf. Ellenberger and Baum (1926, *874).*"

So, whatever Galen meant in detail by the "the handlike outgrowth which binds it to the large nerve" etc. – he must have seen "**certain connecting twigs**" going out from and to the recurrent nerves. But perhaps also a word of caution: Of the extant codices of the work of Galen, the *codex Urbinas* "dating from the tenth or eleventh century, is the oldest and also the best of the lot" – May 1968, I, p. 8. Nevertheless on p. 362 she argues as follows:

> "The following description of the discovery of the recurrent laryngeal nerves and their function is a classic. In his splendid article, "Galen's Discovery and Promulgation of the Function of the Recurrent Laryngeal Nerve," Walsh (1926, *183)* says that he has no doubt that it embodies the actual lecture given by Galen and taken down stenographically on the occasion when he demonstrated publicly the structure of the larynx, the muscles moving it, and their innervation. As for the importance of the discovery, Walsh *(ibid.,* 7751) says, "This discovery established for all time that the brain is the organ of thought, and represented one of the most important additions to anatomy and physiology, being probably as great as the discovery of the circulation of the blood.""

Interestingly, additional branches of the right recurrent laryngeal nerve to the trachea were indeed noted and drawn by **Leonardo da Vinci in 1503**, see the following detail from Fig. 3 of Kaplan et al. 2009, p. 388:



b) According to Dietrich Starck – one of the leading German evolutionary anatomists of the 20th century – **the recurrent laryngeal nerves are missing in the suborder Tylopoda** (family Camelidae with camels, lamas and vicugnas), see Starck 1978, p. 237. However, Hans Joachim Müller, who published the results of his careful dissections on *Camelus bactrianus* and *Lama huanacus* [*guanicoe*] in 1962[3], found that – although in fact, the innerveration of the larynx by the Nervus laryngeus inferior [the part of the RLN proximal to the larynx] is exceptional[4] in these animals – there still is a **ramus recurrens sinister**, which arises from the vagus nerve near the heart and 'curves around the arch of aorta' in order **to ascend** at the latero-dorsal (and during further development at the more dorsal) part of the trachea, **but does *not* innervate the larynx**. Müller writes (p. 161):

> "Beim Überkreuzen der Aorta verlassen mehrere Äste den Nervus vagus und ziehen zum Herzen und zum Lungenhilus. Einer der Äste ("***Ramus recurrens sinister***") umschlingt den Aortenbogen und **steigt rückläufig am latero-dorsalen Rand der Trachea auf**. Im weiteren Verlauf liegt er mehr auf der Dorsalseite der Trachea, verbindet sich **mit entsprechend rückläufigen Ästen des rechten Nervus vagus** zu einem Nervenkomplex und anastomosiert schließlich mit dem absteigenden Ramus descendens n. vagi."

The fact that the *ramus recurrens sinister* does *not* innervate the larynx in the Camelidae, but still takes the ascendent course of the normal recurrent laryngeal nerve of all the other mammal families (so much so that J. J. Willemse thought he had even found a *normal* Nervus recurrens in a young camel[5]), yet to eventually anastomose with corresponding recurrent branches of the right vagus to take part in the formation of a special network of nerves, *also implies **important and indispensible functions of that route***. As for similar observations on the *ramus recurrens dexter*, see footnote below[6]. To discover or deepen our understanding of these necessary and probably further vital functions will be a task of

3 *Beobachtungen an Nerven und Muskeln des Halses der Tylopden*; Zeitschrift für Anatomie und Entwicklungsgeschichte **123**: 155-173

4 "Seit etwa 60 Jahren [in the interim more than 100 years] ist bekannt, daß der Nervus laryngeus inferior [the part of the recurrent laryngeal nerve near the larynx] beim Lama (v. Schumacher 1902] und beim Kamel (Lesbre 1903) einen eigentümlichen Verlauf nimmt. Seine Fasern gelangen auf direktem Wege über einen absteigenden Ast des Nervus vagus zu den inneren Kehlkopfmuskeln." Außerdem fehlt bei dem Tylopoden der periphere Nervus accessorius.

5 "Die Feststellung von Willemse (1958), daß bei einem jungen Kamel ein normaler Nervus recurrens vorhanden war, dürfte wohl nur im Hinblick auf die topographischen Beziehungen dieses Nerven getroffen worden sein." – Müller, p. 167.

6 As to the *Ramus recurrens dexter*, Müller notes p. 162: "Der rechte Nervus vagus gelangt nach Trennung vom Truncus sympaticus ventral der Arteria subclavia in den Thorax, wo er die Trachea zum Lungenhilus begleitet (Abb. 7). Noch vor Passieren der Arteria subclavia verläßt ihn ein kleiner Ast, der, neben ihm verlaufend, ventral die Arteria subclavia kreuzt, um dann auf der Rückseite **rückläufig zum Truncus sympathicus aufzusteigen**. **Caudal der Arteria subclavia** gehen mehrere Nervenzweige vom Nervus vagus ab und beteiligen sich an der Bildung des beschriebenen Nervenplexus auf der Dorsalseite der Trachea. Es läßt sich ein etwas stärkerer Strang durch das Geflecht verfolgen, der sich in den Ramus descendens n. vagi der rechten Seite fortsetzt (= ***Ramus recurrens dexter***) (Abb. 7)."

future research.[7]

c) I have now checked two additional (and again several further) research papers, which clearly imply that the last dissections of the giraffe **did *not* take place in 1838** (as stated by Mark Evans on public TV in England; see the link above), but were performed shortly before 1916, 1932, and 1958 and also between at least 1981 and 2001. (It could, perhaps, be a special task for historians of biology to find out whether further dissections and anatomical studies of the giraffe have taken place between 1838 and 2009, and especially to what extent such studies were relevant for the routes and functions of the RLNs.)

H. A. Vermeulen (1916): *The vagus area in camelopardalis giraffe*. Proc. Kon. Ned. Akad. Wet. **18**: 647-670. (Proceedings of the Koninklijke Nederlandse Akademie van Wetenschappen.)

He introduces his work on the giraffe as follows (1916, p. 647): "I […] found several remarkable relations, particularly of vagus and accessorius nuclei of Camelidae which roused in me the desire to examine what the circumstances might be in the giraffe. I was able to to examine one part only of the central nervous system of this class of animal, and was enabled to do so by the courtesy of Dr. C. U. ARIENS KAPPERS, Director of the Central Institute of Brain Research, at Amsterdam, who kindly placed **part of the material at my disposal. This consisted of the brain stem and a piece of the first cervical segment of one specimen, and the first and second segment of another specimen**. In the latter preparation the nervi accessorii Willisii could be seen perfectly intact in their usual course between the roots of the two first cervical nerves, so that in this respect the giraffe differs here at least, from the Camelida." However, Vermeulen could not dissect and investigate the laryngeal nerve itself of the giraffe. He only writes on p. 665: "…one might conclude, judging from the strong development of the nucleus at this place [the nucleus ambiguus spinally from the calamus] in the giraffe, that **the nervus recurrens, even in this animal in spite of its long neck, well deserves its name,** in which case the highly exceptional conditions of this nerve in Camelidae have wrongly been connected by LESBRE with the unusually long neck of these animals."

J. J. Willemse (1958): *The innervation of the muscles of the trapezius-complex in Giraffe, Okapi, Camel and Llama*. Arch. Néerl. Zool. **12**: 532-536. (Archives Néerlandaises de Zoologie.)

Willemse 1958, p. 533 and p. 535: "ZUCKERMAN and KISS (1932) made an attempt to obtain certainty about the spinal accessory nerve of the giraffe. […] **The dissection of two giraffes, carried out by Zuckerman and Kiss themselves**, indicate that the muscles of the trapezius-complex were supplied, as in other Ungulates, by branches from the spinal accessory and from cervical nerves.

The dissection of a giraffe at out own laboratory gave results which resembled those of ZUCKERMAN and KISS very much. […] Some twenty years ago anatomists showed that in the giraffe a n. accessorius is present, but the nerve is lacking in camels and llamas. Recent investigations are in accordance with these facts." – However, unfortunately no new information on the laryngeal nerves of the giraffe is given in this paper.

For some further dissections and anatomical studies of the giraffe, see the papers by Kimani and his co-workers (1981, 1983, 1987, 1991), Solounias 1999,

[7] I earnestly hope without doing harm or being cruel to the respective animals. There are now many alternatives to animal experiments: http://www.vivisectioninfo.org/humane_research.html (I do not, of course, subscribe to everything these people say or do). We must, nevertheless, for many scientific and further reasons assign different values to humans and animals, but definitely without being incompassionate to either of them.

Concerning dissections: If an animal – like a mammal or bird – has died, but was not killed for studying its anatomy, it appears to be fully okay to me. On the other hand, I remember well the *Zoologische Praktika*, where we, i. e. the students, had the task to dissect fish, frogs and rats and that we were admonished to do our best especially because the animals had to die for these studies. My impression was that the lecturers (understandably) were not all too happy about killing these creatures. Although being fascinated by anatomical studies (I even taught [theoretical] human anatomy for nurses for a while), I later focussed on plant genetics for my further research to avoid killing or doing harm to sensitive animals myself (but there were also additional reasons for this choice). For a more differentiated comment on animal pain, including insects, see http://www.weloennig.de/JoachimVetter.pdf.

A word on Galen's vivisections: I am of the opinion that they were cruel. In this context one may also ask: What about Darwin and vivisection? Rod Preece has stated (2003): "In the first major ethical issue that arose after the publication of Darwin's The Descent of Man – legislation to restrict vivisection – Darwin and Huxley stood on the side of more or less unrestricted vivisection while many major explicitly Christian voices from Cardinal Manning to Lord Chief Justice Coleridge to the Earl of Shaftesbury – demanded the most severe restrictions, in many cases abolition." http://muse.jhu.edu/login?uri=/journals/journal_of_the_history_of_ideas/v064/64.3preece.pdf. See also: http://darwin-online.org.uk/EditorialIntroductions/Freeman_LetteronVivisection.html and http://darwin-online.org.uk/pdf/1881_Vivisection_F1793_001.pdf (as to the latter link: It seems that Darwin could also be very compassionate to animals as shown by the quotation of T. W. Moffett). However, in the second edition (1874 and 1882), Darwin added "…unless the operation was fully justified by an increase of our knowledge, …"

and Sasaki et al. (2001) in the references in *Part 2* for the present paper http://www.weloennig.de/GiraffaSecondPartEnglish.pdf.

d) The verdict of Nobel laureate Francois Jacob quoted above that natural selection has been correcting the genetic message "for more than two billion years, *continually improving, refining and completing it, gradually eliminating all imperfections*" is not an isolated case but describes, in principle, an important and constitutive part of the general state of mind of neo-Darwinian biologists, which can be traced back to Darwin himself. The latter states – just to quote a few examples:

> "As natural selection acts solely by the preservation of profitable modifications, each new form will tend in a fully-stocked country to take the place of, and finally to ***exterminate, its own less improved parent-form and other less-favoured forms with which it comes into competition***. Thus extinction and natural selection go hand in hand."

Or: "…old forms will be supplanted by new and improved forms." And on the evolution of the eye that natural selection is:

> "intently watching each slight alteration" … "carefully preserving each which…in any way or in any degree tends to produce a distincter image." And "We must suppose each new state of the instrument to be multiplied by the million; each to be preserved until a better one is produced, and then the old ones to be all destroyed."
> And: "In living bodies, variation will cause the slight alterations, generation will multiply them almost infinitely, and **natural selection will pick out with unerring skill each improvement**."

In the same manner and context of eye-evolution (including necessarily the entire innervation and corresponding parts of the brain in complex animals), Salvini-Plawen and Mayr regularly speak of "evolutive improvement" (p. 247), "eye perfection", "gradually improved types of eyes", "grades in eye perfection", "the principle of gradual perfectioning from very simple beginnings", "regular series of ever more perfect eyes" (1977, pp. 248 – 255; see please http://www.weloennig.de/AuIINeAb.html).

Applying this kind of reasoning to the recurrent laryngeal nerve leads us directly into the contradiction in the neo-Darwinian world view pointed out above, to wit, that the "**unerring skill**" of natural selection – that exterminates every "less improved parent-form and other less-favoured forms", which picks out and preserves "each improvement…", which is believed to also have produced 'regular series of ever more perfect nerves' and which is, above all, "gradually eliminating all imperfections" – results in "one of nature's worst designs", the "ridiculous detour" etc., of the recurrent laryngeal nerve.

If I understand anything at all, the testable scientific theory of an intelligent origin of life in all its basic and often also irreducibly specialized forms is the superior explanation.

For further aspects on the laryngeal nerves, see Casey Luskins' post (15 Oct. 2010) *Direct Innervation of the Larynx Demanded by Intelligent Design Critics Does exist* (http://www.evolutionnews.org/2010/10/direct_innervation_of_the_lary039211.html#more), explicating the role of the superior laryngeal nerves (**SLN**s) innervating the larynx directly from the brain, especially their co-operation with and complementation of the recurrent laryngeal nerves (**RLN**s). In his post of October 16, 2010 on the topic of *Medical Considerations for the Intelligent Design of the Recurrent Laryngeal Nerve* (http://www.evolutionnews.org/2010/10/medical_considerations_for_the039221.html#more), he sums the former point up as follows:

> "There is dual-innervation of the larynx from the SLN and RLN, and in fact *the SLN innervates the larynx directly from the brain.* The direct innervation of the larynx via the superior laryngeal SLN shows the laryngeal innervations in fact follows the very design demanded by ID critics like Jerry Coyne and Richard Dawkins. Various medical conditions encountered when either the SLN or RLN are damaged point to special functions for each nerve, indicating that the RLN has a specific laryngeal function when everything is functioning properly. This segregation may be necessary to achieve this function, and the redundancy seems to preserve some level of functionality if one nerve gets damaged. This dual-innervation seems like rational design principle."

For a separate version of the text on the laryngeal nerve of the giraffe, see please http://www.weloennig.de/LaryngealNerve.pdf

The following topics and questions will be addressed in *Part 2*. Due to many other time-consuming tasks, however, I will probably come back to this topic only in a few months:

1) Many Giraffidae species and genera appear in the fossil record practically simultaneously and the assumed ancestors co-exist millions of years with their "more evolved" offspring (illustration)
2) Using evolutionary assumptions, one can almost always postulate a line of descent out of a large variety of forms.
3) Neck vertebrae: Why is it so difficult to count to eight, in the giraffe neck?
4) The question of causes (1): Macromutations – Possibilities and limitations
5) The question of causes (2): Further hypotheses on the origins of the long-necked giraffe.
6) The question of causes (3): Is Intelligent Design verifiable and falsifiable?
7) Species concepts and basic types
8) With regard to a duplication of a neck vertebra: could there ever be a continuous transitional series of fossils?
9) The question of chance
10) "Old" and entirely new research topics by the ID-theory.
11) Mitchell and Skinner
12) Conclusions
13) Acknowledgement
14) References

The German article was translated into English mainly by Granville Sewell, Professor of Mathematics, the University of Texas at El Paso, yet the responsibility for any mistakes in words and grammar and especially of the contents of the text rests entirely with W.-E.L.

Wolf-Ekkehard Lönnig

The Evolution of the Long-Necked Giraffe (*Giraffa camelopardalis* L.)

What do we really know? (Part 2)

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 Mitchell 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.

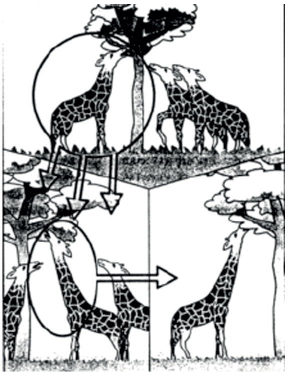
Contents of Part 2

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):



*Repeatedly checked, last on 14 September 2011.

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, 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 juveniles – 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 incompatible 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*).

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 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. 10/11 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 Mitchell 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) ***contemporaneously*** 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), contemporaneously 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 contemporaneously with ***Canthumeryx, Giraffokeryx, Palaeotragus,*** *a*nd ***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) contemporaneously **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) simultaneously 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 – 5.3 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) 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.

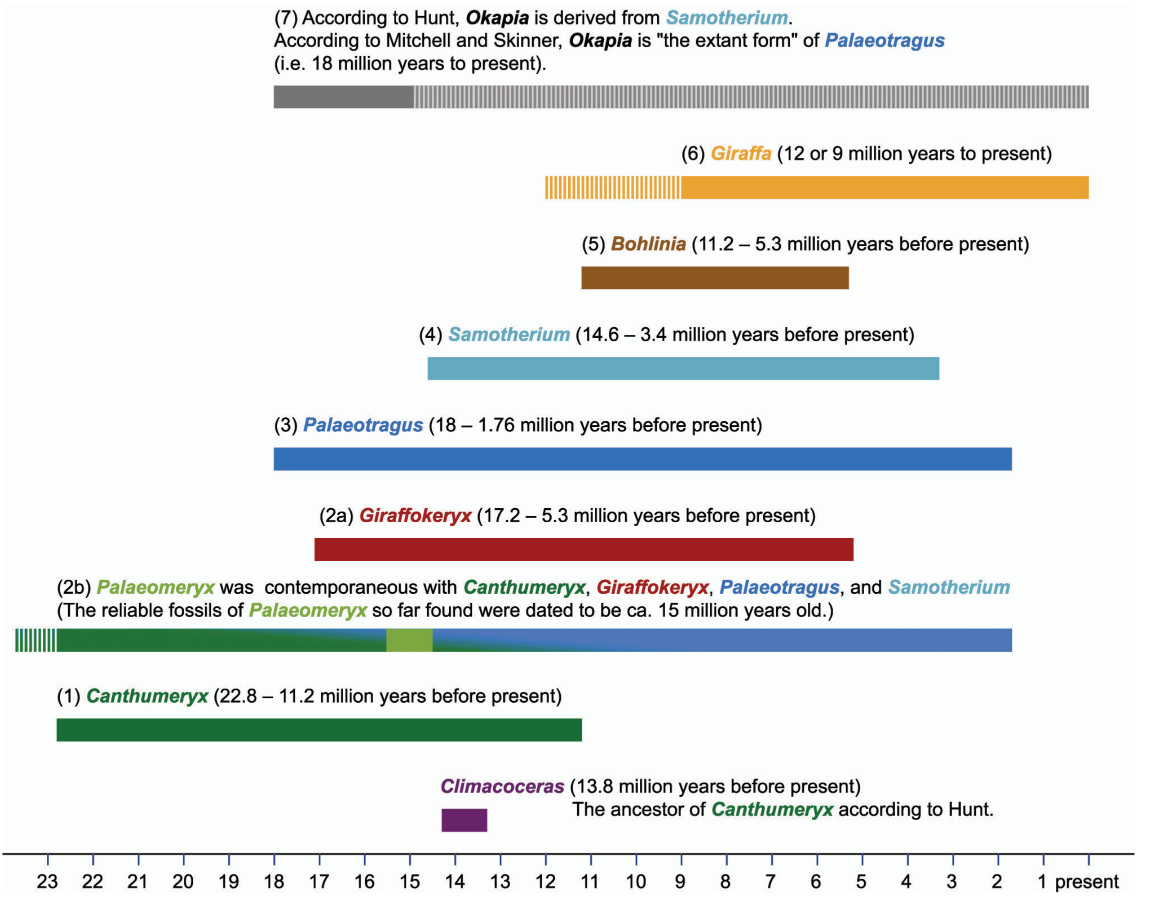


Fig. 1: Temporal overlap of the short-necked giraffes and deer which are considered possible ancestors of the long-necked giraffes. For questions of synonyms and species boundaries within the long- and short-necked giraffes, see the discussion below. *Giraffa jumae* was first dated at 12 million years 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

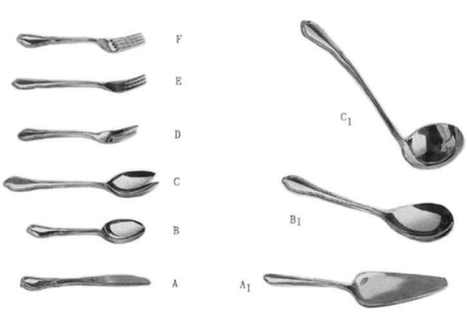
"Already in Darwin's day Galton warned of such erroneous constructions when he pointed out, for example, that firearms and chinaware can be ordered in a continuous series, and that it is necessary to take care in dealing with the same phenomenon in biology" (H. Nilsson).

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

> "The similarity of forms was explained by evolution, and evolution in turn was proven by the various grades of similarities. It was hardly noticed that here one has fallen victim to circular reasoning; 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 themselves. **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 synorganized 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 (the "extremely faithful process" represents a truth permanently reinforced to this very day).

Concerning 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.

See also Roughgarden et al. 2006, Roughgarden 2009. 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 (as 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 two 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 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)."

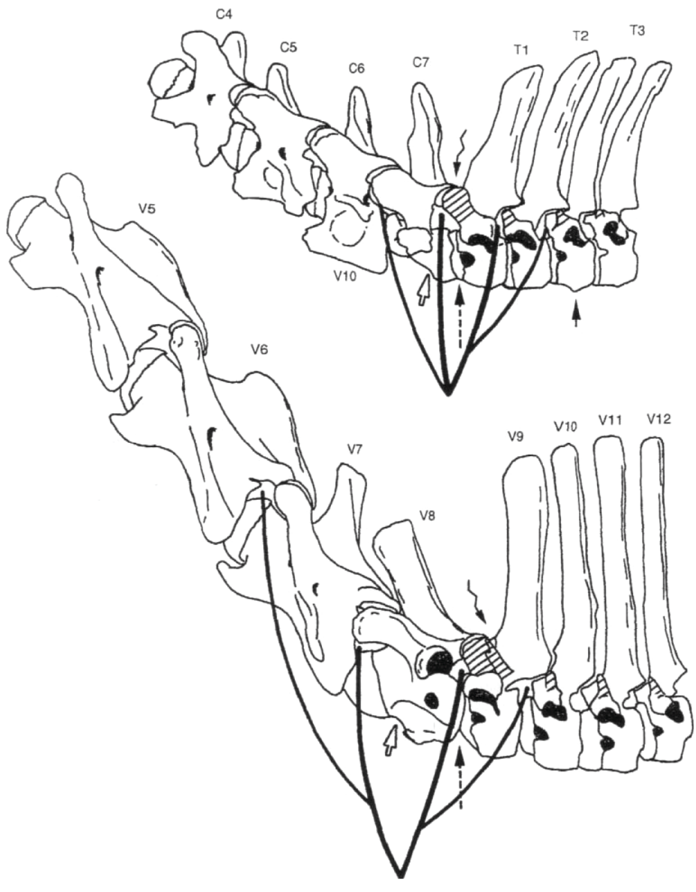


Fig. 4. Comparison of several cervical and thoracic vertebrae of *Okapia* (above) and *Giraffa* (below). From Solounias (1999, p. 264) with the following detailed clarification: "Lateral views of vertebrae and schematic location of the brachial plexi. Upper row: C4 through T4 of the okapi. Bottom row: 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 99.9% 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 may 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 the present book) 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 (one may apply the principles of the argument also to the origin of the giraffe's neck as follows):

> "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 [or giraffe], **it will not suffice suddenly to endow it with a full-grown trunk** [or full grown neck respectively]. As the trunk [or neck] 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 [or giraffe] will require to use his trunk [or neck respectively]. 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 [or other features like the extraordinary long tongue in the giraffe], 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 best-kept sekret."

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: p. 90]

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: p. 91]

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; Lönnig 2007, 2010, 2011).

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 behaviour 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 coloured males with long decorative feathers. For the species as a whole, there is no recognizable selection advantage for this mutation. **On the contrary: conspicuously coloured males preferentially fall victim to their enemies.** ...The long tail feathers reduce the ability to fly and are also a hindrance in the search for food. One should assume, according to the principle of natural selection, that behaviour 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 spatula. 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 nightingale, 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 – colour patterns in butterflies, behaviour 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. 44 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 favour 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:p.92], 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 more than 200 years of fruitless evolutionary speculations (beginning with Lamarck 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 Councel: 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:: p. 93) 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 ensuing questions belong to the basic 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, of course, no longer possible to determine by interbreeding programs. According to the current status of paleontological research, there could be a dividing 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 "species" of the genus *Giraffa*, see below and Note (3d: p.92).

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. [Supplement Oct. 2011: So far this statement is still correct.] 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. Nevertheless, I would not be too much astonished, if – under neo-Darwinian pressure – some people would now be talking the reverse of what they had definitely stated before.

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: p. 90] – (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 causes 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: *Defence 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 B e u r l e n, D a c q u é and S c h i n d e w o l f 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 *Okapia* and *Giraffa* 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 oesophagus (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 because 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, 2010, 2011), Meyer (2004, 2009), Rammerstorfer (2006, 2010).)

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 the above quoted words of Lord Acton's may apply: "The worst use of theory is to make men insensible to fact." Incidentally, it should be mentioned that E. R. Lankester was a devout Darwinist(see 3e, p. 94), that he belonged to the best giraffe specialists of the world, and that he performed lasting pioneer work in this research area (cf. Lankester 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: 'all-powerful' 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 favoured 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 45 and the figure on page 48 above as well as the text on pp. 45-48: 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 stories appear to be doubtful anyway. (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[3f, p. 94]. 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 [3f, see pp. 94/5]. (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: *Sexual selection is not the origin of long necks in giraffes*). 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: p. 70] 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 66 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], – these two points being in agreement with Devillers and Chaline 1993, p. 208, similarly also Churcher: see below p. 78), 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 and other authors, 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. 29), I 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 to the size of *Bohlinia*, see also Arambourg quoted below on p. 117.]

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: p. 96)

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 pp. 2-4 and Badlangana et al. 2009, quoted below on p. 129).

Regarding the time line, let us recall point (5) above on pp. 47/48 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 also 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. (And one may add that the entire family tree is said to be rooted in shrews (*Sorex*-like forms), which in turn are eventually derived from fish.)

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 phylogenetic 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" (Dacquè, 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) below, p. 96).

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 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 authors[5a, see p. 98]) 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*?" 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 primitive 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). [See also Badlangana et at. 2009 as quoted on p. 129.]

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. 16). 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, p. 98] (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 and overall appearance much more than *Giraffa*."* And there is absolutely no scientific proof that *P. germaini* and/or *Samotherium* were ancestors of *G. camelopardalis*, yet actually strong evidence against it (heterobathmies, time overlaps).

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; see also the note on Badlangana 2009 on p. 116 ["only a single C6 [neck] vertebra" known] and pp. 128 and 131). Can one really, with such gaps in our knowledge, establish a gradual evolution between the different groups of the short-necked giraffes?

Interestingly Churcher (1978, p. 528) offers an evolutionary tree that differs in several points strongly from the reconstruction of Mitchell and Skinner:

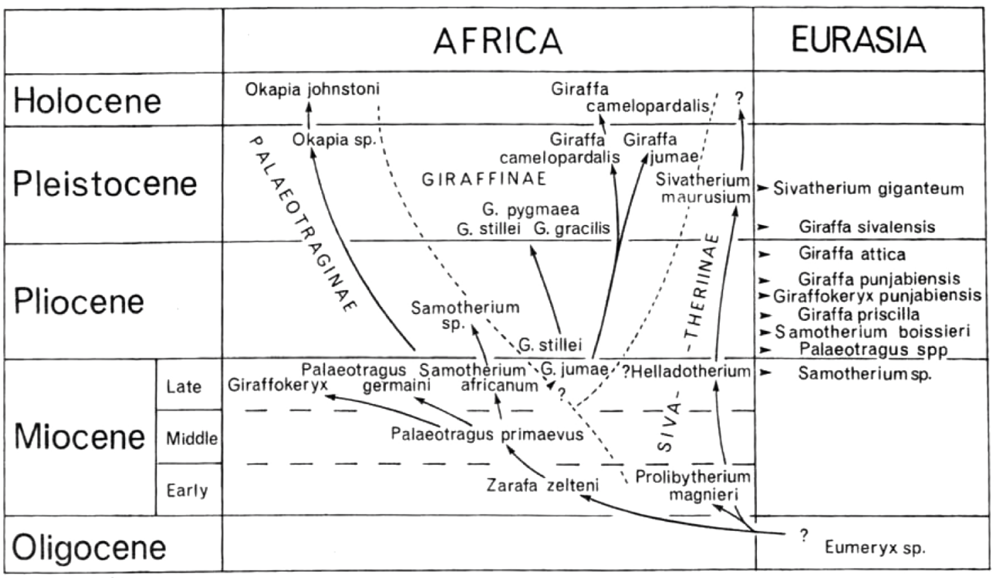


Figure 25.9 Relationships of the African Giraffidae, revised on the basis of an original Miocene radiation from which Europe and Asia were subsequently colonized. Arrows suggest possible lineages but not necessarily direct descent; dotted lines separate subfamilies; carets indicate possible migrations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

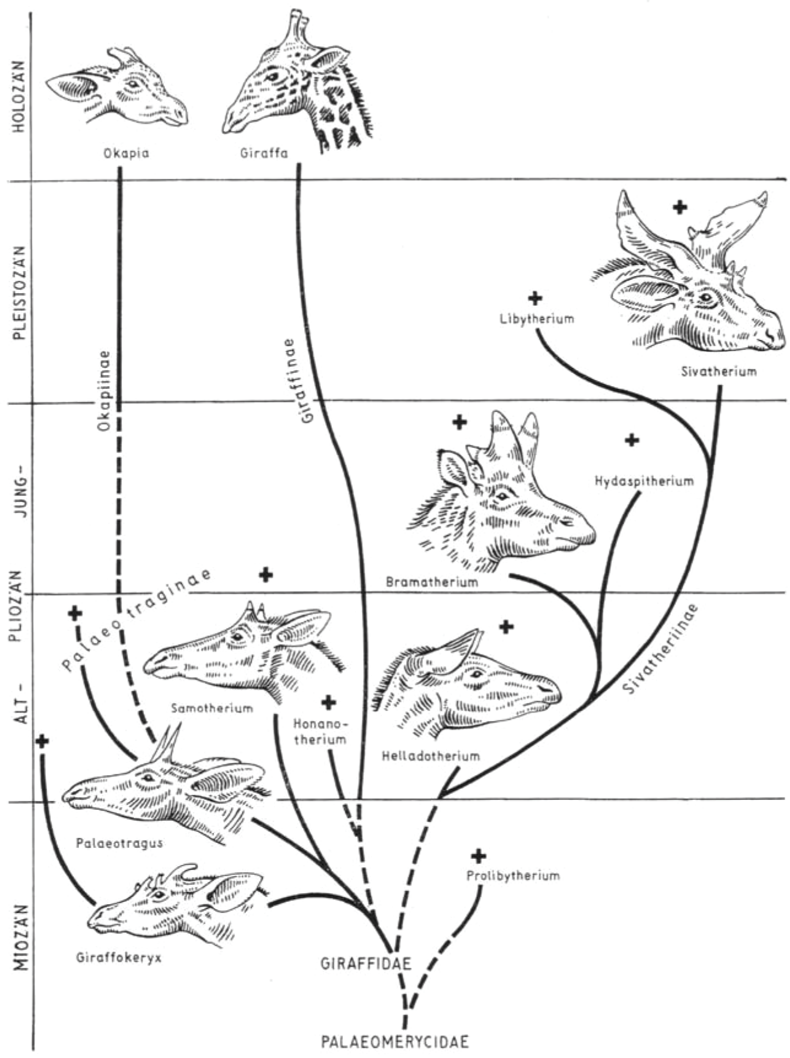
According to the likewise hypothetical phylogeny of Churcher, *Giraffokeryx* and *Palaeotragus germaini* do not lie on the line that could have led to the long-necked giraffes and the connection to *Samotherium africanum* is uncertain. According to Thenius (next figure) *Palaeotragus* and *Samotherium* 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

*In fact, especially due to its long **legs** [see p. 80 for the similar *Samotherium*], several authors call *P. germaini* "a 3 m tall okapi", which seems to be comparable in size to several Sivatheres in contrast to the 6 m tall *G. camelopardalis*; incidentally: even if one replaced Dawkins' okapi [see p. 7] with *P. germaini*, Dawkins' 'okapi' would still be too large.

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 may 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 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, p. 99).

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 non-existent.

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).

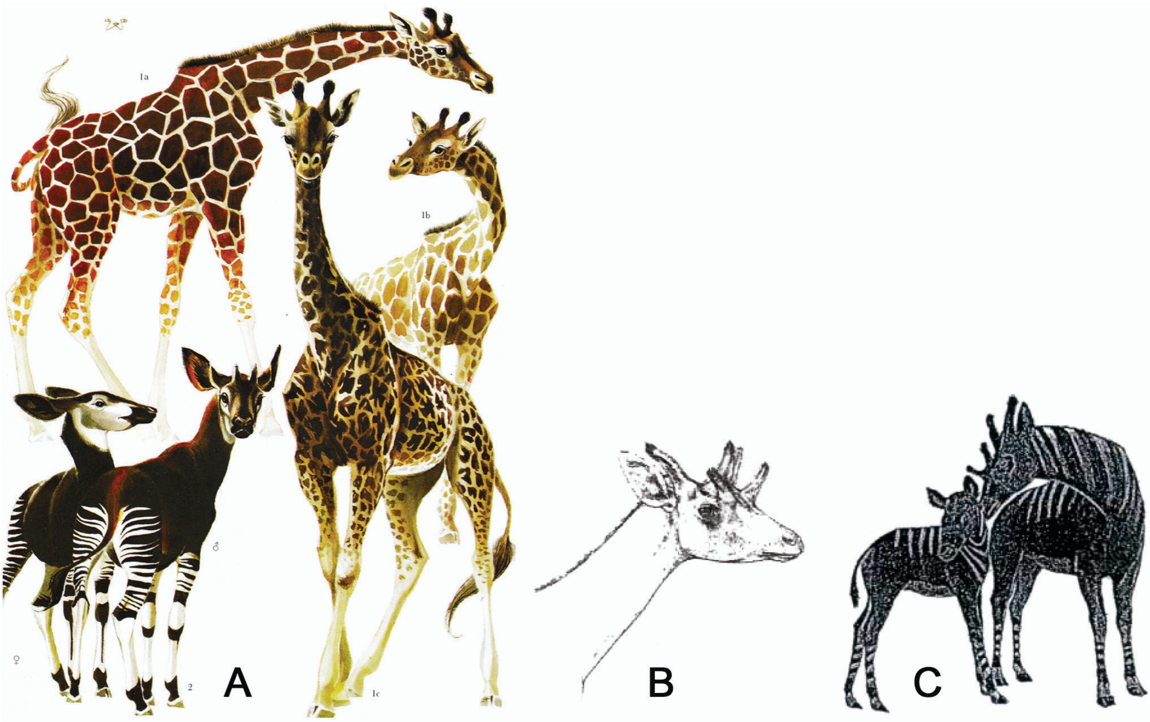
Giraffokeryx seems to fit chronologically – where, however, is the evidence of a continuous morphological transitional series between the gazelle *Canthumeryx* and the short-necked giraffe *Giraffokeryx*? What about the origin of the decisive new characters such as the *ossicones*?[6: p. 99]

"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 B and C: different reconstructions of *Giraffokeryx punjabensis*; height only about 1.6 m: B from Colbert (1935) and C from Savage and Lang (1996) – both from Mitchell and Skinner, p. 58. Left (A): 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 (1970/1979; strongly scaled down).

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 81, 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).

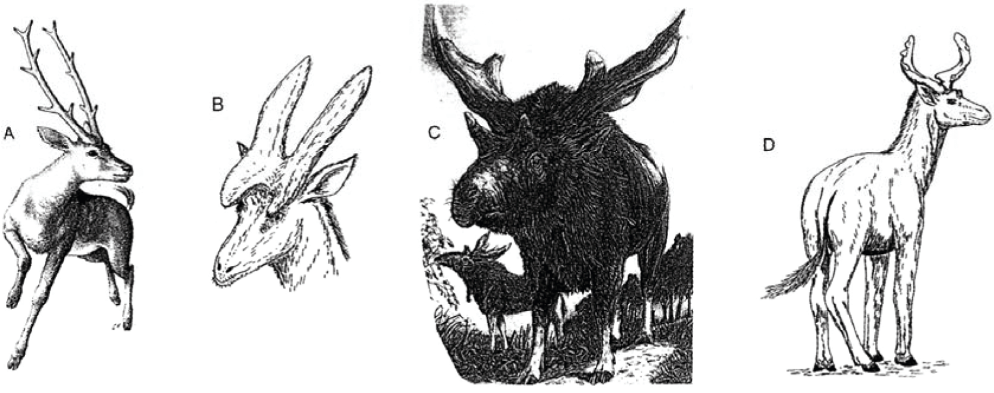


Figure 9 of Mitchell and Skinner 2003, p. 57: "Reconstructions of Sivathere species. A. *Climacoceras* from Hendey (1982); B. *Prolibytherium magnieri* from Churcher (1978); C. *Sivatherium giganteum* from Savage & Long (1986); D. *Sivatherium maurusium* from Churcher (1978)."

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, see p. 99] 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: p. 99]

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 non-dogmatic research.

Finally, with regard to perhaps an aesthetic appraisal 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: p. 100).

13. Acknowledgements

I would like to thank Professor Granville Sewell, Mathematics Department of the University of Texas El Paso, for the English translation of both parts of this giraffe paper. Mr. Roland Slowik, Dietzenbach (FRG), 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. Horst Steinke, Dekatur, IN, USA, made some important final grammatical corrections. 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 44): 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 45): 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) 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 *Palaeomeryx 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 MacInnes 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 45): 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 45): 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 46): 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 52): 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 hoofed animals, stilt-legged, that is, disproportionally long legged. Nevertheless, the legs are rather clumpsy and broad-hoofed, 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)."

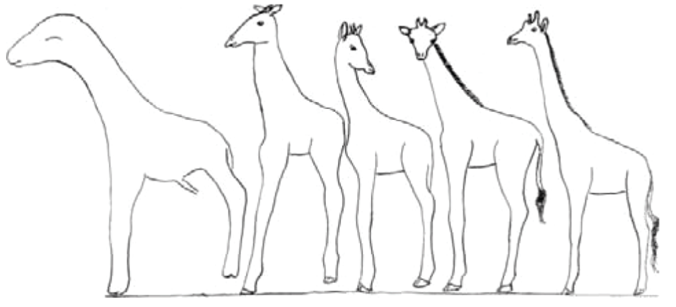


Fig. 37 from Krumbiegel 1971, p. 61: "Differences in body proportions during development. From left to right:: Embryos of approximately 50 cm tall according to K r u m b i e g el 1955, preserved specimen of the Museum of Natural History, Berlin. At the ages of 24 hours, 32 days, 89 days....and full grown."

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 57 and 63): 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 57): 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 hoofed 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 59): However, this is not the rule ("…injury from sparring is rare" – Dagg and Foster, p. 126).

(3c) (From page 61): 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 ensuing 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 mousetrap 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 61): Harris writes 1976a, p. 315:

"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 61): All the "species" of the extant genus *Giraffa* can cross-breed. 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-hoofed animals (R u x t o n [and] S c h w a r z [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 66, 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.*

(3f) (From page 69): 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 detailed 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 70): 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 external 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 resistance 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 gan-**

zes 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 pages 6 and 71, *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."

On p. 6, I quoted Devillers and Chaline as follows: "...the palaeontological record shows that in the oldest deposits, the giraffe was represented by specimens which exceeded in size even the largest current giraffes. This is in contradiction to what we might expect from theoretical considerations on evolutionary trends, such as an apparent general increase in size. The evolution of the giraffe therefore appears to represent a particular case" (Devillers and Chaline 1993, p. 247 and p. 207). This was denied by Dr. X. However, according to Badlangana et al. 2009, pp. 739/740 (Tables 3 and 4) the neck vertebrae C5 and C7 are definitely larger in the fossils than in their extant giraffe, especially C7: Present giraffe **192** mm, fossil *Giraffa* spec. KNM-ER 3205: **255** mm.

(4b) (From page 72): 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 nanism, 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 pp. 23-28 of the current work.

(5) (From page 75, 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 Giraffinae Okapiinae Sivatheriinae (Progiraffinae)	Palaeotraginae Giraffinae Okapinae *[sic]* Helladotheriinae	Palaeotraginae Giraffinae Sivatheriinae

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 relatively 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 75) Simmons and Scheepers, p. 772 und 777:

"Modern Giraffes radiated on African 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 77): 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 contradiction to the fossil finds. (See also the comments on p. 116.)

(5c) (Supplement to page 81): 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 horns 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 82): 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 Sivatheriinae, 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 49 of the present work.)

(8) (From page 86): 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 87): 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) or that of Avise (1999) that "natural selection comes close to omnipotence" (similarly Exley 2009). However, **the article does not address any of the key problems** discussed at length in our two parts 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. 44-48, 61-62, 71-85, 88-89, 92, 96-99).

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. 58-60, 67-70, 100).

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. 51-56 above), no word on synorganization or coadaptation (*Part 1*, pp. 4, 8-10, 23-24, *Part 2*, pp. 56-58, 64, 90-92, 103, 104, 107, 108, 113, 115). No word on the points addressed on p. 64 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 oesophagus (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. 56-58, 63-65).

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. 67 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."

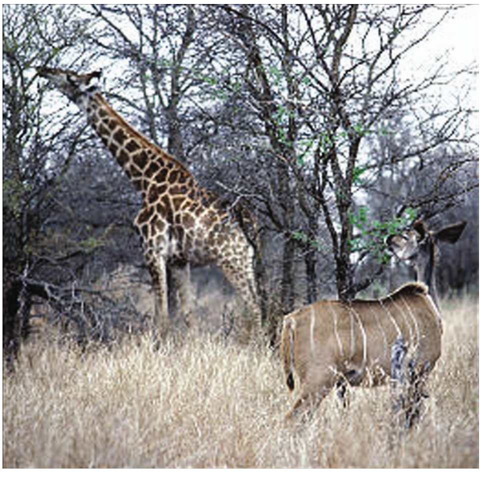
(That is, under normal conditions including dry seasons, yet not extreme "Darwinian dearths") – What did they do to solve the problem posed for such normal conditions?

"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:

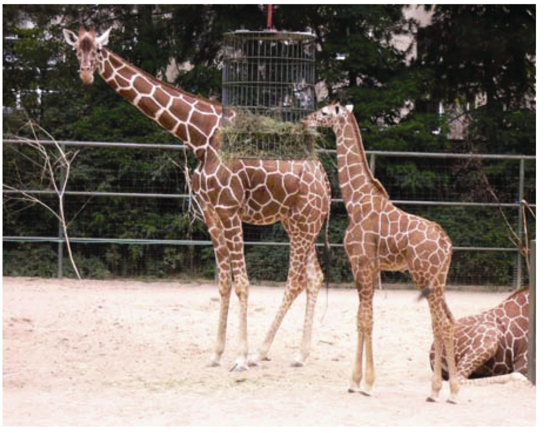
"We conclude that giraffes preferentially browse at high levels in the canopy to avoid competition with smaller browsers."

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. 44 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."

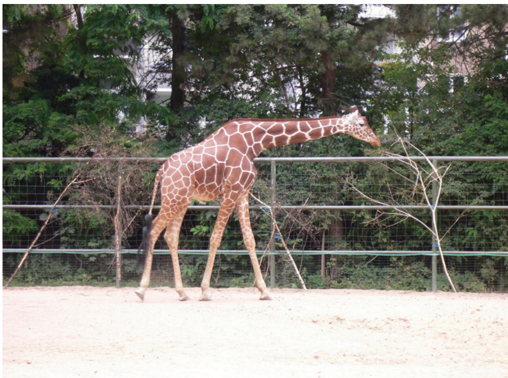
Concerning the Giraffe's behaviour during those dry seasons, see also the long quotation on p. 87 of the present paper, where Simmons and Scheepers mention the following observations (to repeat in abbreviated form):

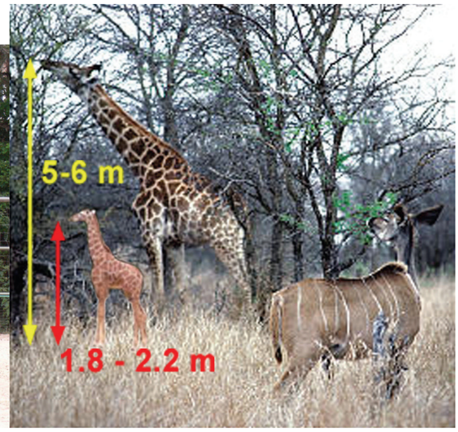
(1) In the Serengeti "giraffes spend almost all of the dry-season feeding from low Grewia bushes" ("…contrary to the prediction that giraffe should use their feeding height advantage at times of food scarcity.") (2) Concerning *all* browsing, about 50% is below 2 m in the Tsavo National Park "within 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:

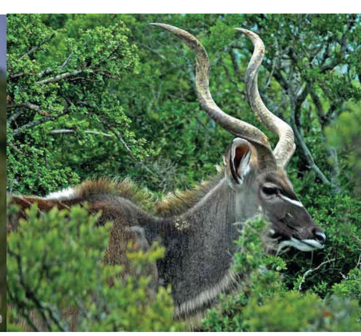
> "In each study both sexes frequently fed at or below shoulder height (ca. 3.1 m and 2.8 m for adult males and females; L. Scheepers, unpublished data). For example, female giraffe spent over 50% of the time feeding with their necks at or below shoulder height in both South Africa (du Toit 1990) and 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. 87 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. 102 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:

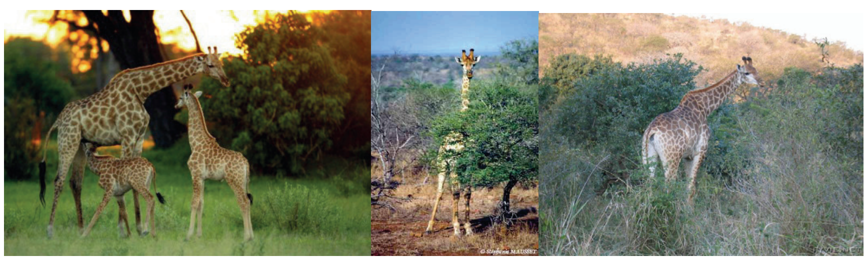


See references for the photographs at the end of the appendix under (1), (2), and (3).

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. 42 and 43 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. 41 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, 1 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.

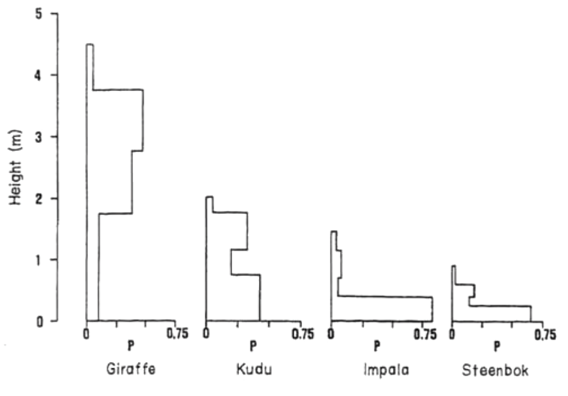


Figure 1 from du Toit for the central region of KNP (1990, p. 58): 'Proportions (P) of feeding time allocated to height classes, which correspond to the four neck angle classes [45°, 90°, 135° and 180° respectively] for each browser species, calculated over the complete seasonal cycle.'

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: 1 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).

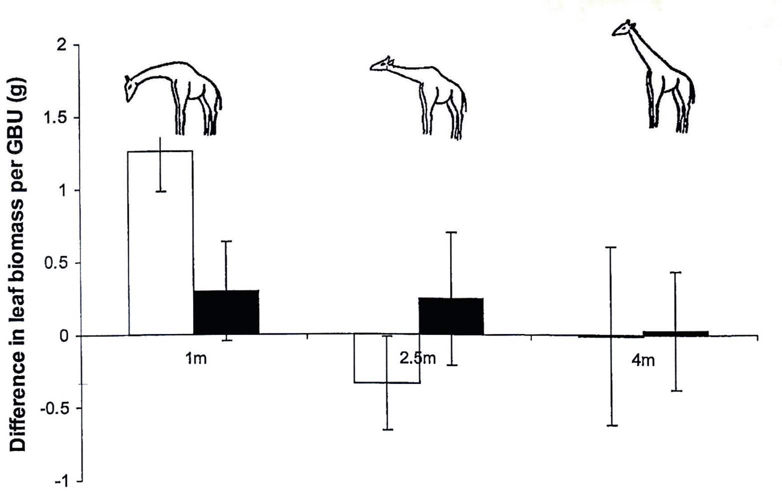


Figure 1 of Cameron and du Toit (2007, p 132): "Difference in leaf biomass per giraffe browse unit *(GBU)* between prefencing and two growing seasons after the erection of exclosures around experimental trees. Open bars are excluded trees; filled bars are control trees. Schematic giraffes indicate the 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:

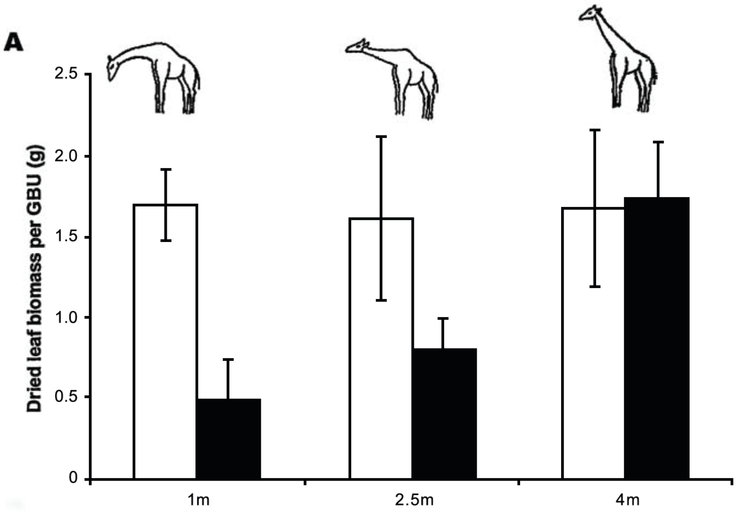


Figure 2A of Cameron and du Toit: "Leaf biomass per giraffe browsing unit (*GBU; A*)". "Open bars are again excluded trees; filled bars the control trees" and – as in Figure 1 – the "schematic giraffes indicate the posture of an adult female when browsing at each height".

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).

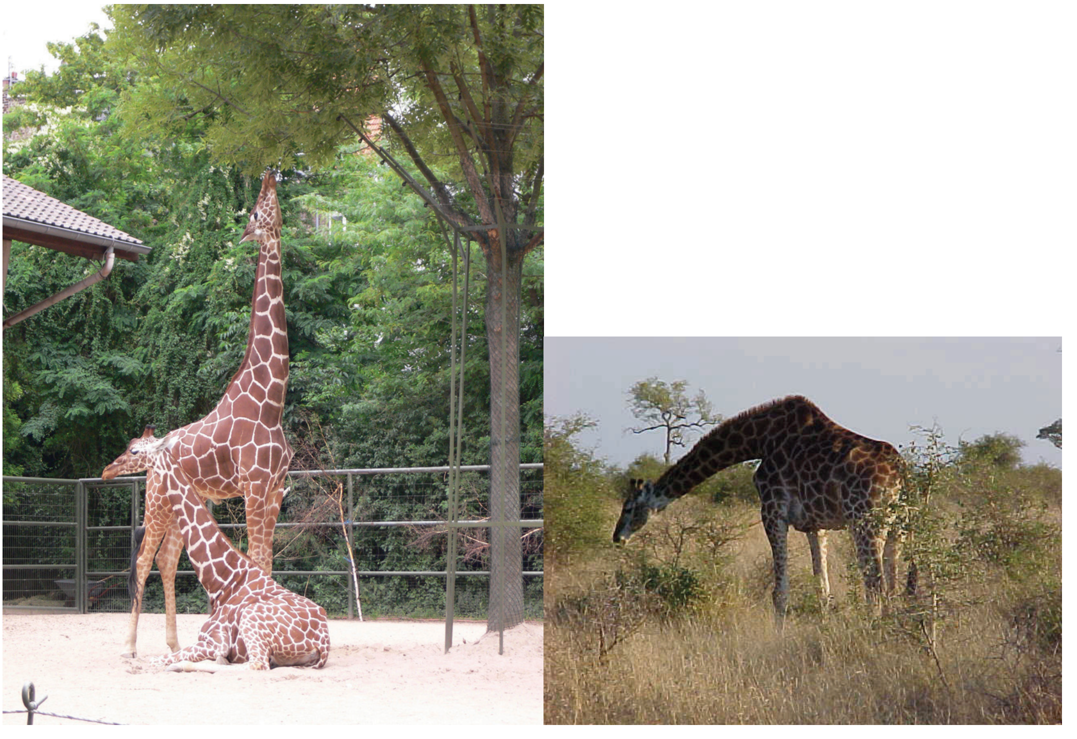
Yet probably the more important question continues to be: ***to what extent do the giraffes themselves contribute to the depletion*** found by Cameron and du Toit? The figures of the authors reproduced above clearly show that female giraffes do browse at a height of 1 m.

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 I did receive an answer (1 Oct. 2011).

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. 42-44 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. 42-44). 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 efficiency 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. 43 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 pp. 108 and 110/111 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, 2010, 2011). 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 (although such grazing is not so easy for giraffes, they do it).
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 Lankester'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 (retrieved 2007):

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. 86 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. 63-66.

(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. 54 above). And, of course, neither would it account for all the other synorganized giraffe features enumerated on p. 64 and repeated on p. 101 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-five 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

Note: 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 boldface, also the publishers, when they are listed first. 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.

Avise, J. C. (1999) as quoted by E. J. Larson and L. Witham (1999): Scientists and Religion in America. Scientific American **281**: 78-83.

Axe, D. D. (2000): Extreme functional sensitivity to conservative amino acid changes on enzyme exteriors. Journal of Molecular Biology **301**: 585-595.

Axe, D. D. (2004): Estimating the prevalence of protein sequences adopting functional enzyme folds. Journal of Molecular Biology **341**: 1295-1315.

Axe, D. D. (2010): The case against a Darwinian origin of protein folds. Bio-Complexity **1**: 1-12. http://bio-complexity.org/ojs/index.php/main/article/view/BIO-C.2010.1/BIO-C.2010.1

Badlangana, N.L., Adams, J.W. and P.R. Manger (2009): The giraffe (*Giraffa camelopardalis*) cervical vertebral column: *a heuristic example in understanding evolutionary process?* Zool. J. Linnean Soc. **155**: 736–757.

W.-E.L.: The authors invested a lot of work in this important paper presenting a wealth of notable data, which may be appreciated without following each of their conclusions and methods. To mention some not so convincing points: The entire reconstruction of the neck of *Palaeotragus germaini* (which is said to have "…resembled *Giraffa* in its elongated neck" – Mitchell and Skinner; see p. 77 above; and similarly Simmons and Altwegg 2010, p. 9: "Among subsequent ancestors, the medium-sized *Paleotragus germaini* and the large *Samotherium* sp. exhibited elongated necks relative to their total vertebral column (Badlangana et al., 2009). *Paleotragus*

germaini was apparently the first species to show elongation of modern giraffe proportions as reconstructed from fossil cervical vertebrae (Badlangana et al., 2009)".) rests ***on only one neck vertebra*** so far: "We found a **single measurement** for C6 [6th neck vertebra] of *P. germaini* (Arambourg, 1959; see Table 4)" – And: "We were only able to analyse **a single C6 vertebra** from *P. germaini* from the Oed el Hammam site in the current study" – Balangana et al. 2009, pp. 749 and 752 (bold added). The vertebral body length of that single C6 is given as 161 mm in contrast to the corresponding C6 vertebra of an extant giraffe (AZ121) of 232 mm, and intriguingly of a C7 of a fossil *Giraffa camelopardalis* (KNMER 3205) of **255 mm**. The corresponding C7 in the AZ121 is only 192 mm – the 7th neck vertebra being generally distinctly shorter than the 6th (see Tables 3 and 4 in Badlangana et al. 2009). Thus, comparing the 6th neck vertebrae of the two fossil species *P. germaini* and fossil *G. camelopardalis* (KNMER 3205), that of ***P. germaini*** **is at least about 100 mm (=10 cm) shorter than that of the giraffe**. The C6 of the okapi is about 70 mm, but the neck vertebrae of *Paleotragus primaevus* generally seem to be a few mm longer than that of the okapi. Incidentally, the C6 of the **camel** (*Camelus dromedarius*) was measured by Badlangana et al. (2009, p. 739) to be **156** mm – so about the size of that of *P. germaini* (neck vertebrae of the camel: C2: 217 mm, C3: 186 mm, C4: 180 mm, C5: 169 mm, C6: 156 mm C7: 122 mm). – According to the measurements of the authors just mentioned, in the **long-necked giraffe** "**half of th**e […] **vertebral column length is comprised of the cervical vertebrae**" (calf: 45%; three **older giraffe individuals**: **52-54%**) (p.741). Compare, please, these results with the neck of *Samotherium* shown above on p. 17 (representing a phenotype very similar to that of *Paleotragus germaini* but said to be larger) and note the enormous difference to *Giraffa* (although I have not yet found exact measurements of the thoracic, lumbar and sacral vertebrae of *Samotherium* (if measured), as an approximate estimate on the basis of that skeleton shown, the neck vertebrae constitute **only about one third or less of the entire vertebral column**). So, does *P. germaini* really show neck "elongation of modern giraffe proportions"? As the camel (*Camelus dromedarius*) and *Samotherium* may indicate, elongated neck vertebrae are not necessarily on the way to a 6 m towering *Giraffa camelopardalis*. "D'ailleurs les caractères primitifs de la denture de ce dernier [*P. germaini*], ainsi que **la moindre** [small, insignificant] **élongation de son cou** et de se membres montrent *qu'il ne sagit point d'une véritable Girafe, mais d'un type voisin de formes fossiles telles que Palaeotragus ou Samotherium* qui présentent des charactères et des proportions analogues" – Arambourg 1959, p. 113 in his original paper (thus ***correcting his earlier statement*** from the same page where he misleadingly said: "Le Giraffidé de l'Oued el Hammam rapelle, par sa taille élevée, ainsi que par l'élongation de son cou et de ses membres, le genre actual *Giraffa*." However, subsequently – when comparing *P. germaini* with *Giraffa attica* (=*Bohlinia attica*), the latter displaying the size of or possibly being even larger than those of the extant *Giraffa camelopardalis** – he correctly states: "Mais la taille de ***Giraffa attica*** est **plus élevée que celles de fossile oranais**; se membre sont aussi plus grêles, mais surtout les proportions de leurs diverses parties [*G. attica*] sont bien celles du genre *Giraffa* et **different entièrement de celles de notre fossile** [*P. germaini*]." // *See Arambourg p. 114, Fig. 48 and the Table on that page (not numbered): in the Table all values for *G. attica* (= *Bohlinia attica*) are greater than those for *G. camelopardalis* – always choosing the highest values of the latter (in mm: Humérus G. a. 500+:450 G. c., Fémur 510:480, Radius 800:700, Tibia 600:550, Métacarpe 710:620, Métatarse 690:630). // As for some more relevant and important points on *P. germaini* see below pp. 128/129.) Concerning *Honanotherium sivalense* (Upper Miocene), Bohlin writes in his original work (1926, pp. 121/122) "Ein 3. Halswirbel […] hat denselben Grad von Verlängerung erreicht wie bei den rezenten Giraffen […]" In English: "A **third neck vertebra** displays the **same length as that of extant giraffes**". Thus, as already pointed out earlier (see above pp. 45, 47, 48), in the Upper Miocene (and before) there existed giraffes of the same size as found today. Concerning ***Bohlinia attica*** (=*Orasius atticus*), Bohlin remarks in his original paper (1926, p. 132): "Die Verlängerung der Extremitätenknochen und auch die Beziehungen zwischen den Längen der langen Röhrenknochen **stimmen völlig mit *Giraffa* überein** […]; auch in einer Reihe von Details im Skelettbau folgt *Orasius* [=*Bohlinia*] *Giraffa*, z. B. Anordnung der Muskelansätze an der Scapula (?), die Gelenke zwischen Astragalus und Centerotarsale und zwischen Centerotarsale und Metatarsus."

Badlangana et al. comment on the contemporary evolutionary theories (2009, p. 755): "The current state of knowledge on both the fossil record and sources of influence on developing embryos **does not allow for either a gradualistic (microevolutionary) or punctuated interpretation** of the evolution of the elongated giraffe neck to be supported over the other…" (for the continued statement, see below p. 131, and further comments pp. 128/129). As for their statement (p. 736): "Darwin (1872) countered the Lamarckian view..." see p. 26 and "Solounias (1999) following the suggestion of Lankester (1908), has argued that the giraffes have eight cervical vertebrae,..." (p. 737), see the information given on pp. 26 and 54.

Badlangana, N.L., Adams, J.W. and P.R. Manger (2011): A comparative assessment of the size of the frontal air sinus in the giraffe (*Giraffa camelopardalis*). The Anatomical Record: Advances in Integrative Anatomy and Evolutionary Biology **94**: 931-940. ("Our results suggest that the volume of the frontal sinus in the giraffe is likely to be unique among artiodactyls,...")

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 (entire paper).

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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. 93), 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. 93 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 as well as 2011.

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

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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. 100 ff.)

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(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 I 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.")

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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.)

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

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

Dawkins, R. (1996): Climbing Mount Improbable. Penguin Group, London. (Concerning Fig. 3-3, see above p. 7, left, a commentator wrote in his mail to me 19 August 2011: "…please note that the POSTURE of the okapi is also distorted so as to increase the appearance of similarity. The okapi is shown stretching its head up, putting the neck into the maximum possible up-reach position, and also, the hind legs are shifted rearward, which has the effect of lowering the butt of the animal below the level of the shoulders, making the back tend to slope upward. This mimics the resting posture of the giraffe – but the okapi is NOT shown in its resting posture. The giraffe is shown in a normal standing posture, with head facing somewhat downward, thereby decreasing the height difference between the two figures.")

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

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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 centimetre 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. (According to the author's Fig. 16.1, p. 208, the rather short-necked *Okapia* is drived from *Samotherium africanum*, which they unrealistically depict as a fairly long-necked giraffe. Thus, in that case short-necked giraffes would have had long-necked ancestors.)

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).

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("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".)

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Fleischmann, A. (1903): Die Darwinsche Theorie. Verlag von Georg Thieme, Leipzig (Chapter 9 is critical assessment of Darwin's ideas on the evolution of the Giraffe; astonishingly [or not] several basic criticisms are still up-to-date.)

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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.

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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).

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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.

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(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.")

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("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.")

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("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.")

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("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.")

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("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. ")

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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.")

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(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.)

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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.)

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(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.)

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Prothero, D.R. (2007): Evolution: What the Fossils Say and Why It Matters. Columbia University Press, New York.
I first saw Prothero's comments on the giraffe from his book on 26 September 2011. Prothero, who – in contrast to Badlangana et al. 2009, p. 739 ("the giraffe has only seven cervical vertebrae") – fully accepts Solounias' understanding of the duplication of a neck vertebra (thus 8 neck vertebrae) implying also the loss of a thorax vertebra, triumphantly points out to "a classic transitional form (f. 14.15): a giraffe fossil with an intermediate-length neck, longer than that of the okapi and the other extinct forms but shorter than that of the living giraffes" (p. 316). See, please, my comments on that intermediary form on pp. 24-26. For more or less intermediary cervical vertebrae, Prothero could as well have quoted the long known *Palaeotragus germaini* (although with only a single C6 measurement) and many species of *Samotherium* (see discussion above pp. 76-78, 81, 85, 98/99 and 116/117). The "new"(?) fossil form seems to be in agreement with my scientifically based prediction (2006, see p. 24 above) of **"2 or 3 further mosaic forms with some intermediary characters**" yet to be found (see also Lönnig 2002 http://www.weloennig.de/NeoB.Ana4.html as well as 1990/1991 for the **predicted duplication to quadruplication** of the number of the mammal fossil forms so far discovered including 'intermediary' ones) – in the case of the giraffe especially if detected in the "right" geological strata. By the way, Prothero also stated in 2007(!) that "Nikos Solounias is currently publishing a description of that fossil form" – So far (Oct. 2011) this doesn't seem to have happened, but, of course it may need more time. Subsequently Prothero continues (still p. 316):

> "For so many years people have speculated about how giraffes got their long necks, and now we finally have the fossils to show exactly how it happened."

Well, from an undescribed fossil except a drawing of its probably intermediary long neck vertebrae – similar to those of *P. germaini* or *Samotherium africanum* or *S. sinense* – he now knows "exactly **how** it happened"?

Isn't it surprising that most other evolutionary biologists haven't received that good news yet so long hoped for? How does this so far undescribed fossil show that the species that once roamed the earth was the result of "an unguided, unplanned process of random variation and natural selection" – according to a definition of evolution endorsed by 38 Nobel Laureates in 2005? So what does this phenotype really tell us about its evolutionary genetics? How did its (unknown) DNA-sequences differing from its assumed but unidentified nearest ancestors and its new genes come about? (For the problems involved in these questions, see Axe 2000, 2004, 2010, Gauger et al. 2010, 2011, even for seemingly closely related enzymes). Above I have already quoted the evolutionary biologists Badlangana et al. (2009): "**The current state of knowledge on both the fossil record and sources of influence on developing embryos does not allow for either a gradualistic (microevolutionary) or punctuated interpretation of the evolution of the elongated giraffe neck to be supported over the other**…" (see p. 117 and below p. 131). Now, let's assume – in spite of all the unanswered questions and contradictions listed on pp. 23-26 – that the fossil form in question would indeed be "a perfect intermediate" in all its features (including a neck vertebra "half duplicated" and a thorax vertebra "half deleted" so to speak): Would that help solving the problem discussed by Badlangana et al. just quoted? The authors mention that "in the literature […] there is a tendency to argue towards the microevolutionary gradualistic occurrence, where slow, progressive elongation of the giraffe neck took place" and they discuss a microevolutionary scenario *vs*. a punctuated one in relation to the difference only between *Palaeotragus primaevus* and *P. germaini* (on the basis of "a single C6 vertebra", which is about 10 cm or more ***shorter*** than that of the fossil of *G. camelopardalis* referred to) and a gap of about 2 million years between the two species, but definitely not the modern giraffe, wrongly assuming that *P. germaini* would already display neck "elongation of modern giraffe proportions" (see the discussion above pp. 116/117)" as follows (2009, pp. 753/754, bold added):

"If such a microevolutionary scenario holds true, where a series of adaptive morphological changes occurred in response to climatic and vegetative variation during the Miocene, then individual cervical vertebral lengths and entire vertebral column lengths for fossil species in the Palaeotraginae should gradually adopt extant giraffe-like proportions. Over this 2-Myr period, based on a generation time of 5 years between birth and first parturition in extant female giraffes (Dagg & Foster, 1976), and a generation time of less than 3 years in extant okapi (Bodmer & Rabb, 1992), between **400 000 and 666 666 generations** of palaeotragines may have occurred. The lengthening of the cervical region between *P. primaevus* and *P. germaini* was in the range of 350—570 mm (… [method of calculation given]), thus requiring an average increase in CVLs [total cervical vertebrae lengths] of **between 0.72 and 1.19 µm per generation** to reach extant giraffe proportions in this time period."

Not the extant giraffe proportions, but only the difference between *Palaeotragus primaevus* and *P. germaini* (see the details above). ***Thus, are there really decisive selective advantages for the survival of giraffe populations of about 1 millionth of 1 meter or 1 thousandth of 1 mm higher in each generation? And that for about 500,000 or so generations each*** reaching 1 thousandth of 1 mm higher than their ancestors into the canopy of the last leaves during a dearth? (Not to mention the smaller females, juveniles and Haldane's dilemma.)

And now the punctuated scenario according to Badlangana et al. (2009, p. 754):

"With a generation time of 5 years between birth and first parturition among extant female giraffes (Dagg & Foster, 1976), and less than 3 years in extant okapi (Bodmer & Rabb, 1992), **between 2000 and 3333 generations** could occur in the 10 000 years allowed for in a punctuated event by Eldredge & Gould (1972). A punctuated event occurring over such a brief period of geological time could be essentially invisible in the fossil record. Given that we are most likely to be discussing an increase in total length of the cervical vertebrae of approximately 477 mm between *P. primaevus* and *P. germaini* (calculation presented above), an average increase of **143.1-238.5 µm per generation** would be sufficient in the time proposed for a punctuated event to acquire extant giraffe cervical proportions."

Again definitely not "extant giraffe cervical proportions". Even their "calculated TVL [total vertebral column length], giraffe regression" of Table 4, p. 740, shows a mean difference between the *G. camelopardalis* and *P. germaini* of 1059.8 mm, for the "calculated TVL ungulate regression" a differences 1456.8 mm, for the "calculated NVL [calculated normalized vertebral length], giraffe regression" 420.2 mm, and for the "calculated NVL, ungulate regression" 1033,1 mm. All these relations and computations, to emphasize this point again, are based on just a **single C6 – and this one is only "*probablement*" a C6** according to the original account of Arambourg 1959, p. 103, raising some doubts on their calculations in relation to it: for almost all values would have to be changed and the differences to *G. camelopardalis* described above *would become greater* and probably also more consistent with the rest of the *P. germaini* fossil material if it were a C3, C4 or C5; since so much depends on that unsure C6, I would like to suggest to examine Arambourg's original fossil again. Moreover, **it is definitely not correct to take an extant young giraffe, like ZA1253 of their Table 3, p. 739, of about 1-1.5 years old for comparisons with (the fossil) *P. germaini*** (p. 749: "…the length of the single C6 of *P. germaini* indicates that it is consistent with the sizes obtained from young adult extant *Giraffa*…" – well, is a giraffe of 1 to 1,5 years a'young "adult"'? **Age of sexual maturity: 6-7 years**, apart from the question whether it was a male or female, see the 1 to 1.5 m difference in height above pp. 39, 41, 67), for they also state on that page that "We assumed that ***all fossil specimens were derived from fully mature adults***…" (emphasis added). However, when comparing extant *young* giraffes (ZA1265 [0.5-1 year] and/or ZA1253) with different *adult* fossil or contemporary species, one could speak of cervical vertebrae sizes of several further mammals like *Camelus dromedarius, Lama glama, Tragelaphus strepsiceros* and *Kobus ellipsiprymnus* of their Table 1 to be consistent with "extant giraffe cervical proportions", especially if one compared a *female* young adult with them, not to speak of a comparison between the latter and a fully mature adult *male* of *P. germaini*. So the differences between males and females of *G. camelopardalis* (see systematic measurements by Harris 1976, p. 287, Table II) and others (possibly by DNA tests for fossils) would also be very important.

Additionally, may I also suggest that the authors should perhaps better (or at least also) have compared (the fossil) *P. germaini* with the fossil *Giraffa*, which latter seem to be larger than the extant giraffes, not least as to be deduced from the C7 (compare their Tables 3 and 4; only the C2 of AZ121 appears to be 5 mm longer than the C2 of the fossil KNM-ER 3205). Extant *Giraffa* (oldest animal): C2: 270 mm, C5: 240 mm, **C7: 192 mm**.

Fossil *Giraffa*: C2: 265 mm, C5: 256 mm, **C7: 255 mm**.

A comparison focussing particularly on that large C7 would have been especially captivating (unlike Badlangana et al. and Harris, I surmise so far that – due to its enormous difference to the C7 of the extant *G. camelopardalis* – there appears to be the possibility that it came from a different fossil individual, which perhaps a DNA-test could decide).

Back to the punctuated scenario: So there would be an increase of about 0,2 mm per generation and very similar questions like those for the microevolutionary scenario may be raised: Hence, ***are there actually decisive selective advantages for the survival of giraffe populations of about 0.2 mm higher in each generation? And that for about almost 3000 or so generations*** each reaching ca. 0.2 mm higher than their ancestors into the canopy of the last leaves during a dearth?

And now on this background of some **3,000 to 500,000 giraffe generations** (leading only to *P. germaini*) Prothero and Solounias seem to point to a so far somewhat doubtful "classic transitional form (f. 14.15): a giraffe fossil with an intermediate-length neck, longer than that of the okapi and the other extinct forms but shorter than that of the living giraffes" (p. 316) of probably **just 1 generation**, the neck of which fossil *prima facie* appears to be similar to the necks of *P. germaini* and the *Samotherium* species – with all the questions of pp. 23-26 left unanswered. And the number of the generations from something like *P. primaevus* via *P. germaini* and *Samotherium africanum* to *G. camelopardalis* would perhaps have to be doubled to about **6,000 to 1,000,000 generations** (ignoring all the possible time overlaps of p. 45, including that of *P. primaevus* and *P. germaini*). But now Prothero knows "exactly how it happened"? Moreover, I would like to remind the reader of the basic problem mentioned on pp. 76/77: If *Samotherium* is derived from *P. germaini* – why then are almost all the calculated values for *P. germaini* given by Badlangana et al. (2009, Table 4, p. 740) based on that one C6 – larger than the values determined for *Samotherium boisseri/sinense*?

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.)

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Roughgarden, J. (2009): The Genial Gene. Deconstructing Darwinian Selfishness. University Presses of California, Columbia.

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.

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("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.")

Sathar, F., Badlangana, N.L. and P.R. Manger (2010): Variations in the thickness and composition of the skin of the giraffe. The Anatomical Record **293**: 1615-1627.

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.

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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. 58 and 68 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.")

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

Simmons, R.E. and R. Altwegg (2010): Necks-for-sex or competing browsers? A critique of ideas on the evolution of giraffe. Journal of Zoology **282**: 6-12.
(From the Abstract: "The two main hypotheses are (1) long necks evolved through competition with other browsers allowing giraffe to feed above them ('competing browsers' hypothesis); or (2) the necks evolved for direct use in intra-sexual combat to gain access to oestrous females ('necks-for-sex' hypothesis." In contrast to his earlier hypothesis (see p. 22 above: sexual selection was **not offered as a supplement to Darwin's explanation (feeding competition), but rather as an *alternative***), Simmons now states together with his new co-author R. Altwegg: "We conclude that probably both mechanisms have contributed to the evolution and maintenance of the long neck, and their relative importance can be clarified further. […] Although both mechanisms can explain long necks in giraffes, no tests have attempted to distinguish between them as the origin or maintenance of the long neck. The two hypotheses may not be mutually exclusive, and **it may not be possible to differentiate between them if both have provided selective pressures to neck lengthening**. The question, therefore, should be what is the relative importance of the two mechanisms in explaining the origin and maintenance of the giraffe's long neck?"
– So these and most other important questions have not yet been solved within the doubtful evolutionary framework for more than 200 years now, and it even "may not be possible to differentiate between the hypotheses" mentioned by them (non-falsifiability). Nevertheless, ID is *a priori* and dogmatically excluded. However, in case the materialistic evolutionary framework is incorrect and the ID-theory has the correct answer, both evolutionary hypotheses would be wrong. As for design: "A fundamental facet of our rationality is our ability to discern the existence of other minds. […] In our world we perceive other minds through their physical effects. ***A theory which arbitrarily rules out mind as an explanation for certain physical effects has abandoned a facet of reason. Abandoning a facet of reason leads ultimately to irrationality***. […] Life reeks of design, it reeks of design" Michael J. Behe *vs.* Stephen Barr (7 April 2010): http://www.youtube.com/watch?v=knEY1wKODR0

Concerning a scientifically established decision between the microevolutionary and the punctuated scenarios, Badlangana, Adams and Manger conclude (2009, p. 755): "The current state of knowledge on both the fossil record and sources of influence on developing embryos **does not allow for either a gradualistic (microevolutionary) or punctuated interpretation** of the evolution of the elongated giraffe neck to be supported over the other,…" – So far the authors are entirely correct. However, their statement continued may be true in the sense of ID (see above pp. 63-66), which they probably did not have in mind: "…but does suggest many ways forward to resolve the manner in which the length of the giraffe neck was attained. Increased research of both the paleobiology and developmental biology of the giraffe, and other ungulate species, will ultimately resolve the questions surrounding the evolution of long necks in the giraffe." See also Badlangana et al. on *Paleotragus germaini* as quoted above on p. 116.

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 (see also above p. 129).

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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.")

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(The veterinarian vividly describes his difficulties during operations on giraffes due to their special blood circulation. 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.)

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(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.

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http://casereports.bmj.com/content/2009/bcr.10.2008.1107.full

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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.)

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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.")

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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.

Wolf-Ekkehard Lönnig (Ph.D. Genetics, University of Bonn) is a transposon and mutation geneticist who has spent more than 30 years of experimental research at the Institute of Genetics of the University of Bonn (7 years) and the Max-Planck-Institute of Plant Breeding Research (altogether over 25 years, retired). He has given talks and seminars at several Universities on the *Origin of Species – Evolution and Intelligent Design* and written several books on the topic. Concerning some more details, see his *Curriculum Vitae* http://www.weloennig.de/CurriculumVitae.pdf and *List of Publications* http://www.weloennig.de/literatur1a.html. His long-standing interest in the *Giraffe, Evolution* and *Design* has resulted in the present book on that topic.

Endorsements for the Book (abbreviated; ***for the full quotations, see pages VI and VII of this publication***):

- "[T]he author of the present book, the distinguished biologist Dr. Wolf-Ekkehard Lönnig, has written a careful treatise on the extensive evolutionary problems usually not mentioned in the long-neck curriculum - enormous problems related to its special anatomy, the missing links of paleontology, its sexual dimorphism, genetics and physiology etc.

  For me as a researcher working on the nerve and synapse transmission in acute experiments, the part on the recurrent laryngeal nerves was especially revealing. […] I am sure the book will help many to reconsider current principles of the evolutionary theory often presented to us as granted and its soft spots as the giraffe long neck, which is used everywhere in textbooks from elementary schools to university texts."

  František Vyskočil, D.Sc. [= Ph.D.], Dr.h.c., Professor of Physiology and Neurobiology, Prague, Czech Republic (member of the Royal Physiological Society London and Cambridge, author or co-author of some 450 mostly peer-reviewed publications)

- "Reviewing in depth the modern biological literature, Dr. Loennig shows Darwinian theory has […] failed to account for this wonder of life, and he instead proposes that it was intelligently designed. This book will benefit any person who wants to know the true status of our knowledge of the origin of this creature."

  Michael J. Behe, Professor of Biological Sciences, Lehigh University, USA (author of the books *Darwin's Black Box* and *The Edge of Evolution* and many peer-reviewed papers)

- "The author provides a comprehensive analysis and critique on the current theories explaining why they are scientifically unsatisfying and examines whether the concept of ID might contribute to the debate. This book appeals to open minded (biology) scientists to form a new framework for non-dogmatic research in evolutionary biology."

  Joseph Merregaert, em. Professor of Molecular Biology, University of Antwerp, Belgium (author or co-author of 61 peer-reviewed papers on molecular biology up to 2010, see: http://anet.ua.ac.be/acadbib/uae/03003)

- "According to Darwin's theory of evolution, the giraffe's long neck formed from shorter ones by "numerous, successive, slight modifications." In this thoroughly researched study, Dr. Lönnig shows conclusively that the evidence does not support Darwin's theory on this point."

  Jonathan Wells, Ph.D., Molecular and Cell Biology, Discovery Institute, Seattle, USA (trained as an embryologist; author of books like *Icons of Evolution*, *The Myth of Junk DNA*, and co-author of *The Design of Life* as well as of peer-reviewed papers in Biosystems, Development, PNAS.

- "Darwin's story of how the giraffe got its long neck is perhaps the most popular and widely-told story of evolution. It is popular because it seems plausible: […]. However, biologist and geneticist W.-E. Lönnig has written a detailed, thoroughly-researched study, "The Evolution of the Long-Necked Giraffe", which shows that almost everything about this popular story is either false or unsubstantiated."

  Granville Sewell, Professor of Mathematics, University of Texas El Paso (author of books like *The Numerical Solution of Ordinary and Partial Differential Equations* Sec. Ed. John Wiley & Sons, and *In the Beginning*; see his list of publications in his CV: http://www.math.utep.edu/Faculty/sewell/)

- "This scholarly very carefully researched book is certainly the best I have read on the subject. It shows that beyond any doubt the extension of the giraffe's neck cannot be plausibly accounted for via a series of small adaptive steps nor could it have come about suddenly via a macromutation unless the reorganization of the anatomy and physiology of a presumed ancestral 'short necked giraffe' was intelligently directed. This monograph should be required reading for all biology high school pupils and would go a long way to countering the simplistic and uncritical claims of the Darwinian establishment."

  Michael Denton, M.D., Medical Genetics (author of *Evolution – A Theory in Crisis* and *Nature's Destiny*. More about the author and his research, see http://en.wikipedia.org/wiki/Michael_Denton)