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No. 4



In search of haplochromines in Egypt - 1. Ismailia

A little known Pseudocrenilabrus species from East Africa

Maylandia or Metriaclima – the case for Metriaclima

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Editorial

In this issue of **eggspots** we not only continue our report on a trip to Egypt, but also present for the first time, in words and pictures, a *Haplochromis* species caught there and compare it with other North African species. Additional articles deal with new information on the phylogeny of cichlids in general, the description of a new *Haplochromis* species, the discussion of a taxonomic problem (which luckily doesn't affect us greatly), and the sad demise of an excellent cichlid association. We also devote several pages to a little-known *Pseudocrenilabrus* species from Tanzania, and finally we have a response to the *Maylandia* article in a previous issue of **eggspots**, in which the arguments of the *Metriaclima* supporters are explained in detail. The issue is completed by Eggspots Elsewhere and a short book review.

This issue also represents an experiment, in that we are allowing readers to simply download the journal from the Internet with payment on a voluntary basis. I would therefore like to take this opportunity to request all readers to make a contribution to the not inconsiderable costs involved in producing each issue. You will find details on the final page.

In this regard, I hope you will read this fourth issue with great interest!

Erwin Schraml

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Cover photo: *Haplochromis* sp. "Ismailia"

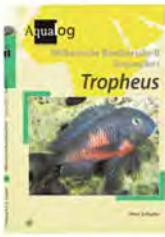


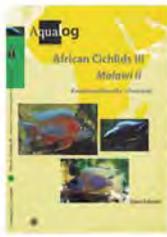


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In search of haplochromines in Egypt

- 1. Ismailia

by ERWIN SCHRAML

The story behind my trip to Egypt with ANDREAS DUNZ has already appeared in an article in the previous issue of **eggspots** (DUNZ & SCHRAML, 2010). Ismailia was the goal of the first stage of our expedition. This town can be reached without problem from Cairo in a few hours via a well-constructed road - at least once you have finally managed to escape the horrendous traffic chaos of the capital. Ismailia was mainly of interest to my travelling companion because it is the type locality for two little known tilapiine cichlids, *Oreochromis* and *Tilapia ismailiaensis*. Both were discovered in the Ismailia Canal, which runs for 128 kilometres from Cairo to Ismailia and there enters Lake Timsah, which is nowadays connected to the Suez Canal. The precursor of the Ismailia Canal was the so-called Bubastis Canal, which was constructed in ancient times (610-595 BC). There have even been suggestions (SCHÖRNER 2000) that a canal was constructed even earlier, at the time of the Pharaoh Sesostri I (1956 to 1910 BC). These historical details demonstrate that although the Ismailia Canal is an artificial waterway, it has perhaps been in existence as an aquatic habitat for almost 4000 years and hence it is quite conceivable that its own distinct species may have evolved there during this long period of time, even if they are probably closely related to species from the Nile. The more so in that the canal has probably periodically become silted up with sand in places and hence cut off from the Nile. In addition, prior to the construction of the Aswan Dam the Nile inundation probably regularly



A "side-arm" of the Ismailia Canal, about six kilometres from the town of Ismailia.

reached Lake Timsah via the Wadi Tumilat, which is again an additional indication of the likelihood of a distinct fauna in the Lake Timsah Tinsah region.

Andreas had previously measured the type specimens of the two tilapiines from the region at the Natural History Museum in London and studied their descriptions. The differences from the better-known Nilotic species were not very significant, and so we



Left: The Ismailia Canal is slightly higher than Lake Timsah where it enters the lake. Sluice-gates prevent salty water from flowing back into the canal, whose water is used for the irrigation of fields cultivated for agriculture.

Below: an African sardine (*Chelaethiops bibie*) at the water's surface, picking out food that had landed there.

were very interested in whether live specimens might perhaps exhibit greater differences. Unfortunately we were unable to catch any tilapiine fishes ourselves in the entire area around, Ismailia, and the few that we found in the catches of the fishermen looked just like normal *Oreochromis niloticus*. The only interesting non-cichlid that we saw was an African sardine (*Chelaethiops bibie*),



which was searching for food at the water's surface almost at the end of the Ismailia Canal. Immediately after the last sluice there were anglers busy catching fishes in the sea water. A glance into their catch buckets revealed that in the main they were capturing relatively small wrasses. Were these intended as bait for larger fishes? Communication with the anglers, as with other passers-by, was difficult, as the majority of the people here, outside the tourist regions, spoke only Arabic, but we did not.

The Ismailia Canal is completely concreted in the area of the town, and mostly with very high embankments. The water level lies so far below that it would be difficult to try and fish from the bank. Because the bottom was almost entirely bare of cover we decided not to bother. So initially the only place where we were successful in catching

Everywhere in Egypt we encountered North American faunal elements:

Gambusia (right: a male *G. affinis*)

and crayfishes (below: *Procambarus clarkii*, here a juvenile).

Bottom: Hopefully at least the shrimps that were also to be found almost everywhere are “true Egyptians”.



fishes was a side channel, around six kilometres outside the town, where we captured the North American *Gambusia* that we encountered practically everywhere in Egypt and which had probably been introduced a long time ago to combat mosquitoes. Other bycatches included shrimps and, to

our surprise, crayfishes as well. They may well have been another North American species, *Procambarus clarkii*, which we also frequently encountered in waters in Egypt that we visited subsequently.

But - to my great pleasure in particular - we also caught a number of haplochromines. The current list on FishBase doesn't include any described species of these cichlids from Egypt at

all. But in his book on the fishes of the Nile BOULENGER records a species that he terms *Haplochromis bloyeti*, and if I remember correctly this name and that of *Haplochromis loati* were until recently still included on the FishBase list for Egypt. And as already mentioned in the earlier article on the Egyptian Mouthbrooder (*Pseudocrenilabrus multicolor*), the literature does, however, contain indications regarding as yet undescribed species from Lake Maryut, from the Nile in the area of Luxor, and from the Fayoum region. And now also from the vicinity of Ismailia.





The specimen in the upper photo has an unmistakably longer snout than the one below. Note also the stripe pattern.



From the same site as the specimen above, a *Haplochromis* with a rounder head profile. The cuvette photos were taken immediately after capture.

The easiest place to find these fishes was beneath marginal plants trailing in the water or among truly submersed vegetation. It turned out that we were able to distinguish two different phenotypes among the fishes that we caught, specifically some with a long and some with a shorter and rounder snout. Immediately after capture all the specimens we caught had a vertical stripe pattern on the sides of the body, but this was hardly visible at all later on. In the aquarium they practically always exhibited a horizontal pattern consisting of a mid-lateral band and a subdorsal lateral band. The facial mask includes the



Hemichromis letourneuxi from a subsidiary canal in the vicinity of Ismailia.

entire spectrum of markings: nostril stripe, interorbital stripe, supraorbital stripe, nape band, and lacrymal stripe, and there is even a hint of a preopercular vertical stripe visible.

While I had the more pleasant job of photographing the specimens we had captured, Andreas had to ready for preservation those that we couldn't take back with us alive. He was also responsible for the DNA samples and writing up details of, for example, the collecting location. This division of labour meant that we always got things done quickly and were able to cope with a good deal of work.

As already described in the last issue of **eggspots**, our trip to the Ismailia Canal included a visit to the Birket Abu Jumal. On the way back to Cairo we stopped at yet another side-arm of a canal which produced additional *Haplochromis* and *Hemichromis letourneuxi*; we had already caught the latter in the Abu Jumal, but here they were somewhat paler in colour.

On the return journey we also investigated a place, around 50 kilometres from Ismailia, that appears on the satellite map as a lake-like widening of a flowing water. It lay right next to the road in the middle of nowhere, and appeared to consist of an artificial arrangement

with several pools. But to our disappointment we discovered that it was no more than some kind of effluent disposal system and the artificial pools were perhaps settling beds.

To sum up this first day: while we might not have found the tilapiine fishes we were looking for, instead we had discovered what was probably a new *Haplochromis* species and a new location for *Pseudocrenilabrus multicolor* and *Hemichromis letourneuxi*.



Male *Haplochromis* sp. "Ismailia" courting a female (in the background).

Aquarium observations on the *Haplochromis* from Ismailia:

Although *Haplochromis* sp. "Ismailia" is only a comparatively small species, territorial males are exceptionally aggressive towards not only rivals but also females that are unwilling to spawn. I was, however, able to establish that in winter (I wasn't able to determine the precise months but it was probably at least from December to February) all courtship activity came to a halt. During this period females and even males were in harmony with one another, without any aggressive encounters at all. The rest of the time the male was constantly busy displaying the moment a female came near, or actively searching for a female to court. At the peak of courtship arousal all the bars on the sides of the male's body disappear. By contrast, the females are invariably pale grey to cream-coloured all over.

A relatively shallow spawning pit is constructed by the male, who then entices ripe females into this depression. Spawning takes place in the manner typical of all haplochromines, in a head to tail position with the pair circling. The females brood for only around two weeks and harbour up to around 50 fry in their mouths - a relatively small number in comparison to other *Haplochromis*. The young are taken back into the mouth at any disturbance,



Two females of *Haplochromis* sp. "Ismailia".



During the phase of highest arousal the lateral stripes disappear completely in males of *Haplochromis* sp. "Ismailia".

though if the latter is too great then it may happen that the female takes flight before collecting up the fry. It is then sensible to separate the mother from the young. The further rearing of the fry is problem-free and possible using crumbled flake as food.

References:

- DUNZ, A. & SCHRAML, E. (2010): Fresh blood from Egypt - *Pseudocrenilabrus multicolor* imported again after many years. *eggspots*, 3: 5-16.
SCHÖRNER, H. (2000): Künstliche Schifffahrtskanäle in der Antike. Der sogenannte antike Suez-Kanal, *Skyllis*, 3 (1): 28-43.

A comparison of *Haplochromis* sp. "Ismailia" with other North African species

by ERWIN SCHRAML

To date only two haplochromine species have been described from northern Africa and the Levant. One is *Haplochromis desfontainesi* from Tunisia and Algeria, the type species of the genus *Astatotilapia*, and the other *Haplochromis flavijosephi* from the Jordan drainage, which is at the same time the only species of the group whose distribution lies outside of the African continent.

When PELLEGRIN (1904) erected the genus *Astatotilapia* to accommodate African species which he saw as standing between the genera *Tilapia* and *Paratilapia*, he chose as type species *Sparus desfontainii*¹⁾. He regarded as characteristic *inter alia* the change from bicuspid to unicuspid teeth on attaining adulthood that gives the genus its name (*astatos* = variable).

GREENWOOD (1979), who revalidated *Astatotilapia* (after REGAN (1922) had treated it as a synonym of *Haplochromis*), spends two pages describing the characteristics of taxa that he assigns to this genus and in addition provides a page-long diagnosis, but his characterisation is so broad that no clear picture emerges. He himself even expresses doubts (GREENWOOD 1980) as to whether the genus, as defined by him, is monophyletic. MEYER *et al.* (1991) and MEYER (1993) confirm on the basis of molecular studies that several lineages have been lumped together with no justification. In addition LIPPITSCH (1993) found uniform scalation characters in fluvatile *Astatotilapia*, but that these differed from those of the lacustrine species, for which reason only a distant relationship can be assumed at best.

1) PELLEGRIN assumed, however, that this species was described by LACÉPÈDE as a *Labrus* and not, as was actually the case, in the genus *Sparus*, although he does also list the correct combination, as it had been used in GERVAIS (1869). Presumably he hadn't actually read the work of LACÉPÈDE and was simply adopting the combinations mentioned in BOULENGER (1899). An indication of this is that both authors for the first time use the species name *desfontainesi* rather than *desfontainii*, but cite the latter as the original spelling. GERVAIS (1869) and SAUVAGE (1877) both previously used *desfontainii*, and this was undoubtedly known to both BOULENGER and PELLEGRIN. For unknown reasons (perhaps only a printer's error?) we find both spellings in LACÉPÈDE, on p. 54 as *Sparus Desfontainii* and p. 160 as *Sparus desfontaines* (explicitly cited as the scientific nomen representing the similar-sounding French name). After BOULENGER (1899) *desfontainesi* was predominantly used in the literature, but following the publication of CLOFFA IV (1991) *desfontainii* resurfaced, although GREENWOOD (1979 - in a footnote) had already explained that BOULENGER corrected the name because he assumed that LACÉPÈDE wished to use this spelling as the name was chosen to honour a M. Desfontaines. The ICZN allows/requires correction of a name only if "32.5.1. If there is in the original publication itself, without recourse to any external source of information, clear evidence of an inadvertent error, such as a lapsus calami or a copyist's or printer's error, it must be corrected. Incorrect transliteration or latinization, or use of an inappropriate connecting vowel, are not to be considered inadvertent errors." Because LACÉPÈDE used both spellings and in addition states that the species was to be dedicated to "citoyen Desfontaines" (Citizen Desfontaines), who had discovered the fish in Tunisia, a spelling or printer's error appears likely. Hence in the event that if, for formal reasons, BOULENGER (1899) cannot be regarded as the First Reviser (article 24.2 of the Code) , I assert that '*desfontainesi*' should nevertheless be accepted as the correct spelling of the species name.



Threatening male *Haplochromis desfontainesi*.



Female *Haplochromis desfontainesi*.

On the other hand, other authors recognise morphological indicators that speak against any splitting into separate genera related to *Astatotilapia*. Thus WITTE & WITTE-MAAS (1987) had previously discovered, from material used by GREENWOOD & GEE (1969) for their descriptions, that the transition between *Yssichromis* and *Astatotilapia* is fluid. SNOEKS (1994) found intermediate species from Lake Kivu such that there was no longer any clear separation between *Astatotilapia* and *Gaurochromis*.

All in all, *Astatotilapia* is an example of why authors such as VAN OIJEN (1996) have recommended that all haplochromine species from the Lake Victoria basin should for the time being be left in the genus *Haplochromis*. This recommendation has long since been adopted for other haplochromines (with exceptions such as the so-called Malawi *Haplochromis*) by CLOFFA and well-known online databanks such as the Catalog of Fishes and FishBase. However, recently the name *Astatotilapia* has again been used in these online databanks. Because, in the absence of a newer and more precise diagnosis for this genus, it is difficult to determine what actually constitutes *Astatotilapia* and what does not, I would like to make a personal plea for the continued use of *Haplochromis* for the time being. In the subsequent course of this article I will, nevertheless, use the genus names that the authors in question have regarded as correct in the works cited. Even if I had decided to do otherwise, a certain amount of confusion in this regard would still have been inevitable.

A recent genetic, albeit only mitochondrial, study by GENNER & HAESLER (2010) finds that from a phylogenetic viewpoint *A. desfontainesi* belongs to the modern haplochromines of the mainly East-African/Nilotic lineage. It is thus relatively similar to *A. flavijosephi* from the Levant, but also to *A. burtoni* from Lake Tanganyika. On this basis it belongs to a relatively old and apparently monophyletic lineage.

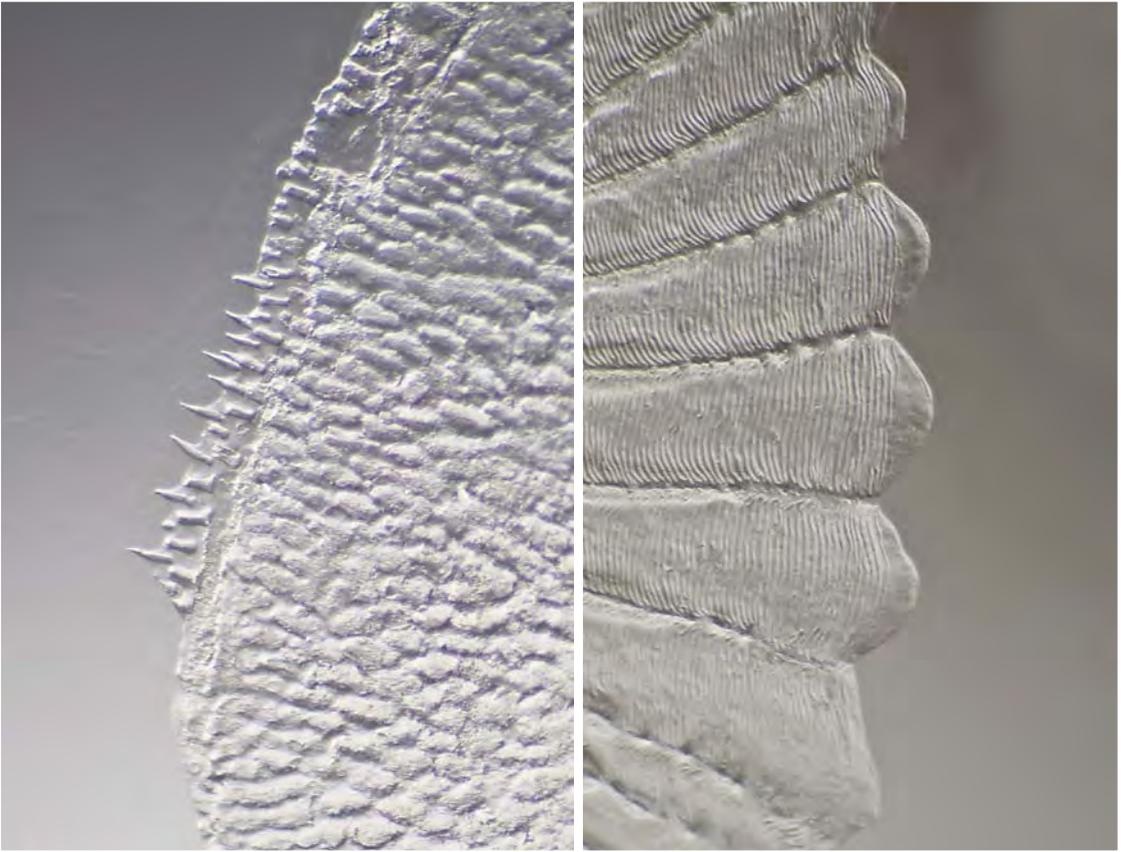
LIPPITSCH (1990) describes the structure of a flank scale in *A. burtoni* (as type 10), a typical *Astatotilapia*. According to her granulation is visible over the entire exposed part and totally obscures the ring structure. It consists of blunt grains or tubercles which are often wart-like. Ctenii are visible only at the very edge, if at all, and are rather small.

Haplochromis desfontainesi

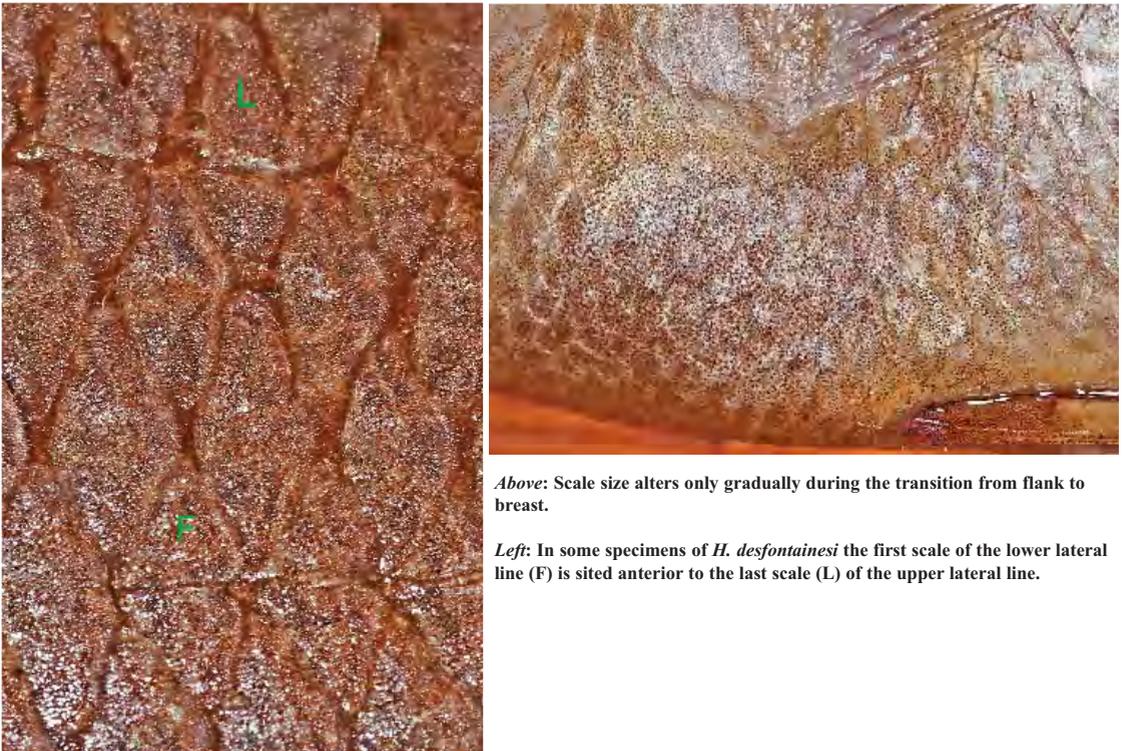
Haplochromis desfontainesi is known only from Tunisia and Algeria. It is regarded as a relict in permanent waters in the basin of the former Chott



Right: Flank scale of *H. desfontainesi*. As LIPPITSCH describes for type 10, the circuli on the exposed part of the scale are totally obscured by granulation and ctenii are present only at the extreme edge.



Details of a flank scale of *H. desfontainesi*: *left* the small number of ctenii and *right* the radii.



Above: Scale size alters only gradually during the transition from flank to breast.

Left: In some specimens of *H. desfontainesi* the first scale of the lower lateral line (F) is sited anterior to the last scale (L) of the upper lateral line.



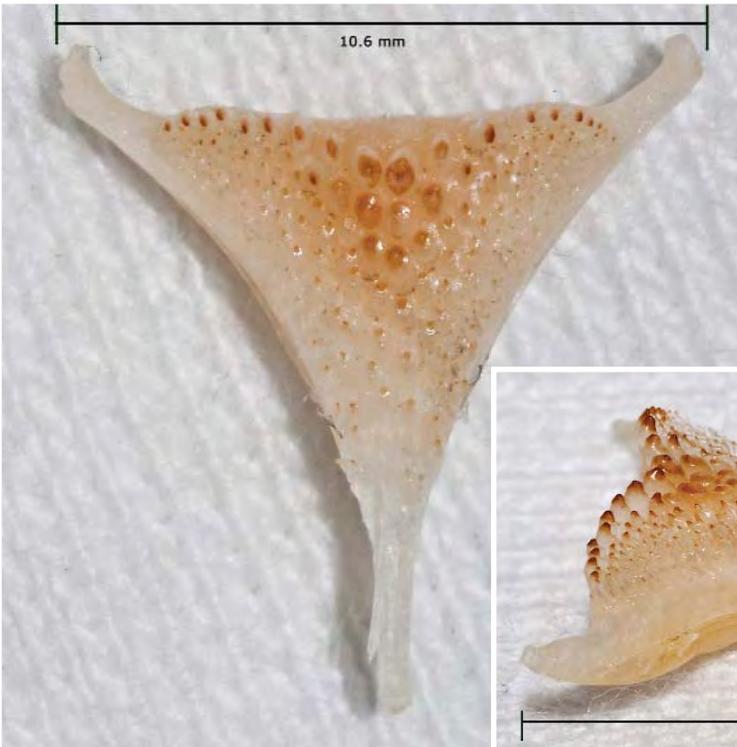
Above: The lower-jaw teeth are largely unicuspid in adult males of *Haplochromis desfontainesi*. The large photo shows the right-hand side (front to the right), the small photo two teeth on the left-hand side.

paleolake and originates from an epoch that probably came to an end with the last ice age. Back then the Sahara was also moister and it is known from Stone Age paintings that there were formerly also large animals there that today are known only from the savannas of East Africa. In 2006 I myself brought back a number of juveniles from Tozeur (SCHRAMML 2007a, b), but despite an exhaustive search was unable to find any other sites for the species in Tunisia. Hence it is categorised as seriously endangered (EN) in the IUCN Red List. However, JÖRG FREYHOF (pers. comm.) has subsequently been able to rediscover the species in Lala as well.

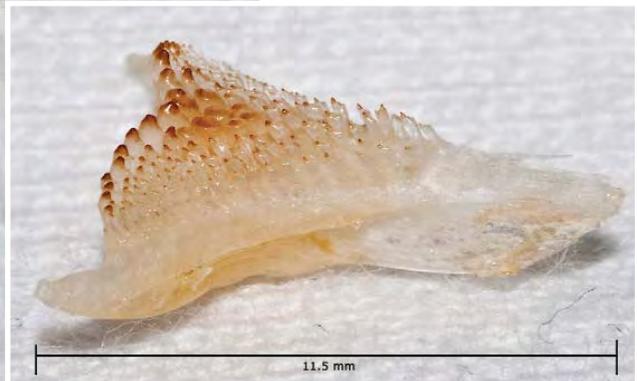


Anterior teeth of a male *H. desfontainesi*, in part with unicuspid teeth that look as if they were originally bicuspid, but the minor cusp has been lost.

Maximum length in males can be up to 15 centimetres. The taxon has a relatively large number of lateral-line scales (total for both parts) for a fluviatile species (according to GREENWOOD (1979) 31-33 vs. 28-30 in other *Astatotilapia*; a count of more than 30 is generally found only in lacustrine haplochromines (GREENWOOD 1980)). I have established that in specimens of *H. desfontainesi* with a high number of scales in the lateral line the count for the median series of scales is lower. In one case the number of pored scales was 32, but that for the median series only 29. In many haplochromines the two values are identical. The reasons for the difference are on the one hand that the species is high-backed and hence the upward curvature results in more scales being included in the upper lateral line, and on the other that the lower



Left and below: Pharyngeal dentition of *H. desfontainesi*: the large submolariform teeth in the upper centre are distinctive and typical, and their arrangement is regarded as characteristic for the genus *Astatotilapia*.



section doesn't continue from a point posterior to that at which the upper terminates, as in many other haplochromines, but the two parts overlap.

The size difference between the scales on the breast and flanks is not marked and takes place gradually, just as GREENWOOD (1979) also describes as typical for *Astatotilapia*. As can be seen from the photos showing flank scales, *H. desfontainesi* corresponds precisely to the type 10 of LIPPITSCH. GREENWOOD'S statement that in *Astatotilapia* the ctenii are generally strong and extend around the greater part of the scale's free margin does not apply in the case of the type species, where they are rather small and cover only a small part of the scale margin.

In the large specimens I studied (only males were available) the jaw dentition was predominantly unicuspid, although it was possible to discern individual variation. In some specimens the teeth are comparatively robust and the crowns rounded and pointed, and slightly incurved. In other individuals they are noticeably more slender and look as if they may actually be bicuspid teeth whose minor cusp has been partially worn away.



First gill arch of *H. desfontainesi*.

The pharyngeal dentition resembles that of *H. flavijosephi*, of which GREENWOOD (1979) writes that it constitutes an exceptional case in *Astatotilapia*, as only this species develops somewhat enlarged teeth with submolariform crowns in the central series.

The gill rakers correspond to expectations for *Astatotilapia*, following GREENWOOD (1979) in form



Male *H. flavijosephi* in courtship coloration.



Ripe female *H. flavijosephi*.

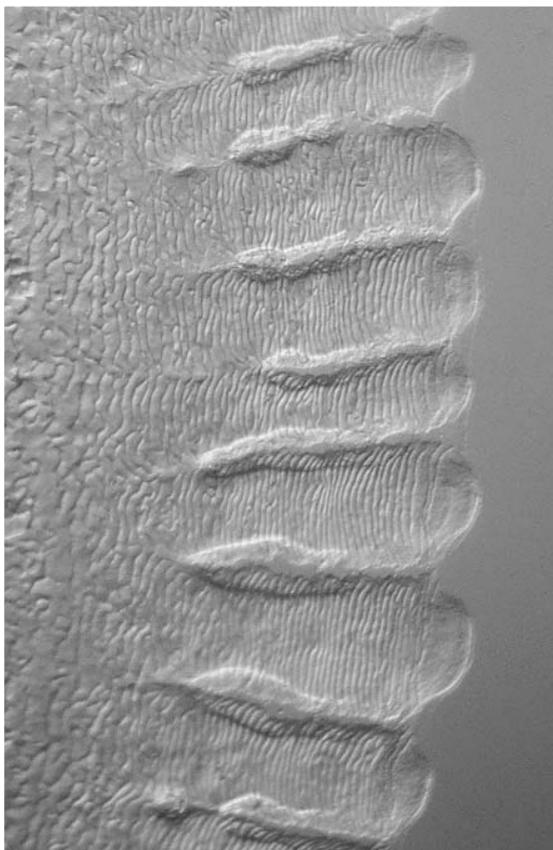
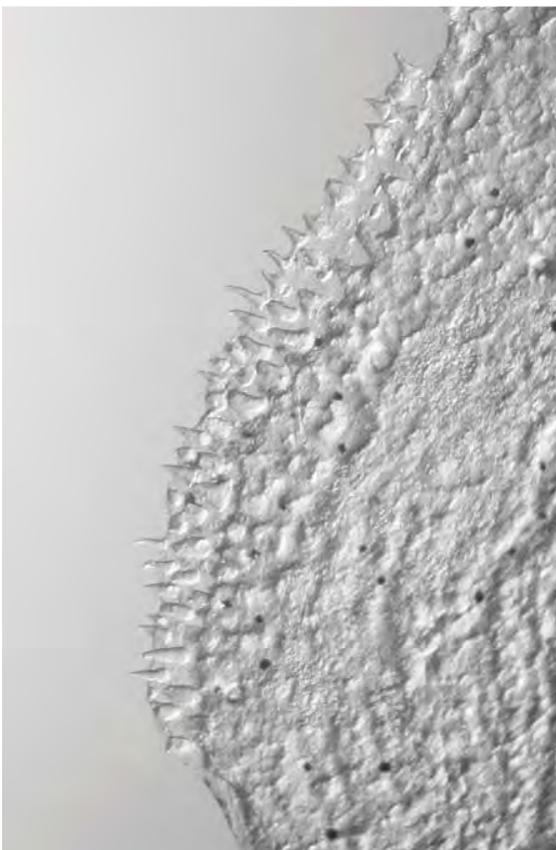


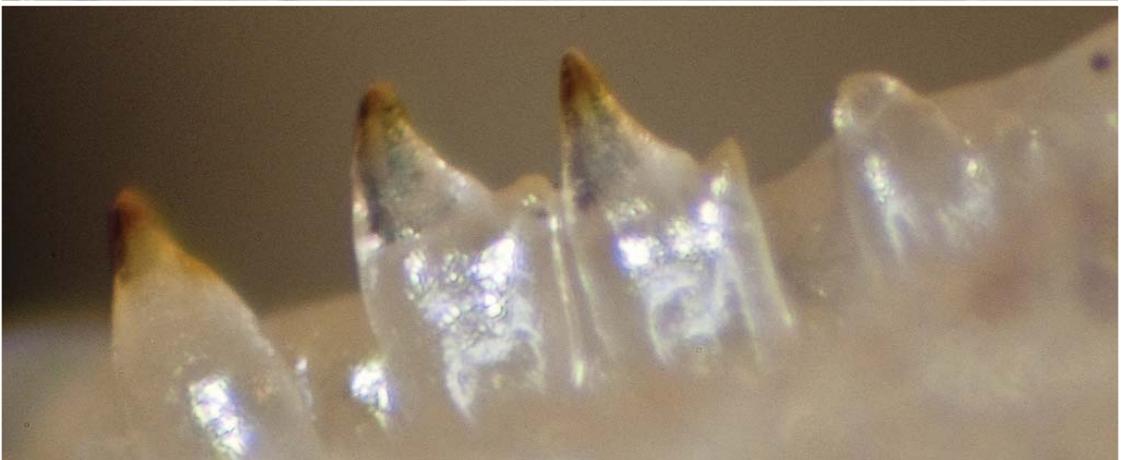
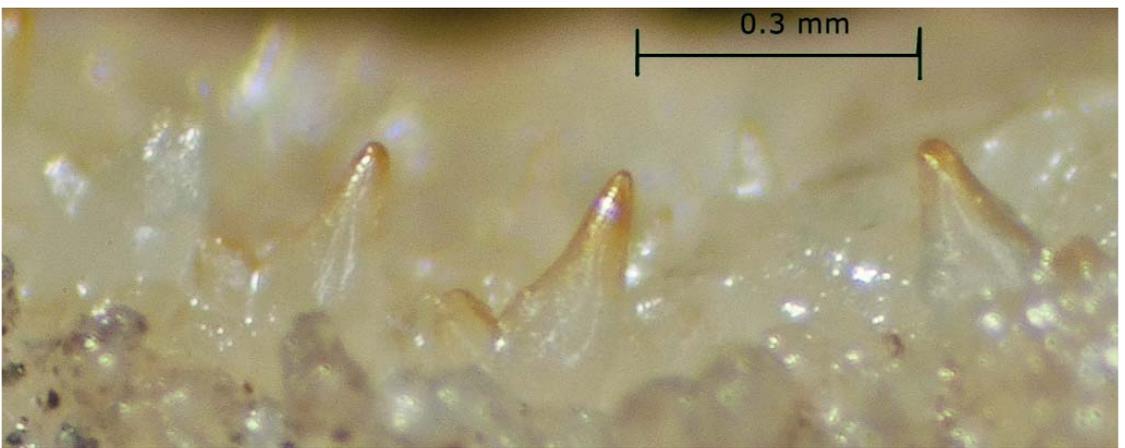
Above: First gill arch of *H. flavijosephi* with large gill rakers.



Right: Flank scale of *H. flavijosephi*. The short but broad radii (so-called secondary radii) are striking.

Below: Magnified details of (*left*) the free edge of the flank scale with its relatively small ctenii, and (*right*) the part normally covered by the adjoining scale, showing the radii.







Above: The teeth have been exposed in this preserved specimen of *H. flavijosephi* (a male measuring 71.1 mm SL). Some of the teeth appear almost unicuspid, though this appears to be attributable more to reduction (wear?) of the minor cusp than to their being true unicuspid teeth.

Photos page 20: Teeth of a 55.7 mm SL male of *H. flavijosephi*: lower pharyngeal dentition (*top*); jaw teeth in anterior (*centre*) and left-hand lateral (*bottom*) view.

and number (eight rakers, some slender and moderately long, some shorter and blunt). It is noteworthy, however, that there is a rather delicate, well-pigmented, protective membrane covering the gill arches and rakers.

Haplochromis flavijosephi

Haplochromis flavijosephi is the only non-African haplochromine, and has apparently to date never been seen in the aquarium hobby. That, however, is only because there have been no reports on the species in the relevant journals and magazines. It is in fact already being kept in the aquarium in Europe. I myself have been able to obtain it via two different Internet contacts, with the fishes originating from two sites close together in the vicinity of the Nahal Ha'Kibbutzim in Israel.

The maximum size of males of this species is purportedly almost 13 centimetres (TL), but as a rule they attain only some seven centimetres in length. One of the specimens I studied had only 26 scales in the lateral line, fewer than GREENWOOD (1979) cites as normal for *Astatotilapia*. GREENWOOD (1979) gives a size criterion of 70 mm SL for the change from bicuspid to unicuspid teeth in the outer series of the jaws. Because *H. flavijosephi* only exceptionally attains a larger size, and I have never yet seen really large specimens of the species, I cannot provide any data on the tooth form of larger specimens. The specimens I studied all possessed bicuspid teeth, in which the anterior teeth in each case had the median cusp greatly protracted and the minor cusp was barely visible without pushing back the flesh of the gum in which it was partially embedded (swollen as a result of preservation?). The more lateral the teeth, the more they assume a subequilateral form. Among them was one tooth whose second cusp was so insignificant that the tooth gave the impression of being unicuspid. It may thus be that the minor cusp disappears (through wear?). The pharyngeal teeth are a real arsenal of different tools. In the centre, in the upper series, there are submolariform teeth, which become ever more delicate in form the closer they become to the edges of the bone. The flank scales are typical as given for *Astatotilapia* (see LIPPITSCH) (and in this respect, as already mentioned, she differs widely from GREENWOOD), ie there are very small ctenii and only on the outermost edge of the exposed part of the scale. The central ring structure of the scale is barely visible, as it is obscured by flattened granules and tubercles. The scalation on the breast is likewise typical, with only a gradual transition in size from the flank scales.

The gill rakers are, unlike in *H. desfontainesi*, relatively uniform and triangular.

Haplochromis sp. "Ismailia"

In the aquarium males of this species attain a length of barely eight centimetres (SL). On looking into the mouth the distinctive discoloration of the tooth pulp is immediately apparent through the highly transparent enamel. Adult males and females can again be distinguished by the different form of the tooth crowns in the outer series. Those of males are predominantly unicuspid, those of females bicuspid, and in the latter there is sometimes a flange visible on the longer cusp. The bicuspid teeth are more close-packed than the unicuspid.

There are two possible reasons for this sex-linked tooth form. Firstly, males and females may have different feeding habits. There is clear evidence for this in a work by SPATARU & GOPHEN (1985) on *Haplochromis flavijosephi*, according to which males with a length of 70 - 80 mm upwards feed largely on molluscs (probably snails by preference). In young males these constitute only 20-30% of gut contents. According to the same study, females feed exclusively on insect larvae (chironomids), amphipods, and oligochete worms. It would be interesting to



Jaw teeth of a male (*above*) and female (*below*) *H. sp. "Ismailia"* (in each case the anterior right-hand side). Striking features include not only the variable form of the tooth crowns but also the different intervals between the individual teeth and the dark coloration of the tooth pulp, possibly mineral in origin and not previously known from any other species.

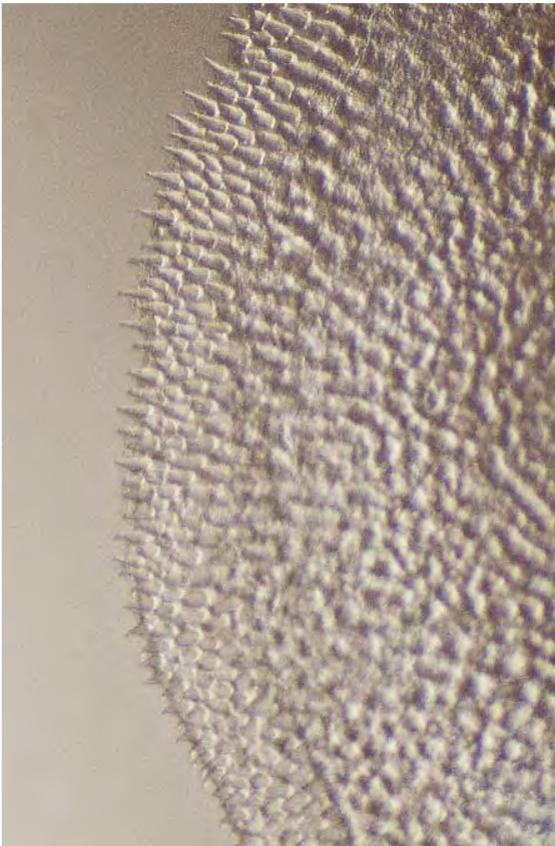


investigate whether the change in the jaw dentition requires the relevant food as a trigger, as has been established in the case of the pharyngeal dentition in *Astatoreochromis alluaudi* (HOOGERHOUD 1986). The other possible reason is at present no more than a hypothesis. In an earlier work (SCHRAML & TICHY 2010) I have surmised that males with unicuspid teeth are at an advantage during territorial battles, because they are better armed. But this remains a matter for more detailed investigation.

The flank scales in *H. sp. "Ismailia"* possess more series of ctenii on the free margin of each scale than in the preceding two species.

Right: Flank scale of Haplochromis sp. "Ismailia".

Below: Magnified details of flank scale showing: (left) the ctenii, which in H. sp. "Ismailia" are arranged around the entire free margin of the scale, and in more series and closer together than in H. desfontainesi and H. flavijosephi; and (right) the part normally covered by an adjacent scale, showing the radii. The very short tongue terminating the interradial zones is a striking feature.



The form of the granulation is again irregular and blunt and largely obscures the central ring structure, although it is to be found on only around a third of the scale as a whole. The tongues at the margins of the interradial areas are narrower, but on the other hand the radii extend to the centre of each scale. The number of scales in the lateral line is, at 29, within



Female *H. sp.* "Ismailia": mood-related pattern where horizontal stripes predominate (vertical striping weakly visible).

the expected range of less than 30 for fluviatile haplochromines (the fact that this is more than the 27 scales in the median series relates to the relatively large body depth). The head in this species is significantly broader than in *H. flavijosephi*, and males possess far fewer eggspots (which are also often relatively large) than in the other two species. All three species have the common feature that, depending on mood, they may exhibit the same melanin pattern, consisting of a median and a subdorsal stripe.

It is anticipated that a more precise description of characters will follow when other populations/species found in Egypt are compared.

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Cichlids (and other acanthopterygian fishes) phylogenetically reclassified

by KURT F. DREIMÄTZ

One of the German terms for cichlids is *Buntbarsche*, one of the few German words also known in the English-speaking world, at least among enthusiasts. The American Cichlid Association for instance, has named their journal *Buntbarsche Bulletin*.

Among many German enthusiasts there is not really any clear distinction between the terms *Barsch* (perch) and *Buntbarsch* (colourful perch) and when a club member speaks of “*meine Barsche*” (my perches) he doesn’t as a rule mean that he is actually keeping members of the family Percidae (perches) such as the European Perch (*Perca fluviatilis*), for example, but is in all probability referring to cichlids (family Cichlidae). But despite certain external similarities between the two (one is minded in particular of the South American genus *Cichla*), the two families aren’t in fact so dreadfully close at all in phylogenetic terms.

The Percidae (perches) belong to the suborder Percoidei (perches and their relatives) of the order Perciformes (perch-like fishes) in the superorder Acanthopterygii (spiny-finned fishes). Until recently the cichlids were included in the suborder Labroidei (wrasses and their allies), which, of course, again belongs to the order Perciformes (perch-like fishes). But according to a molecular genetic study published last year by LI and his colleagues, in which a new nuclear DNA marker (RNF213) was utilised, the phylogenetic history of the cichlids has to be rewritten and they belong to neither the Labroidei nor the Perciformes. According to this study the cichlids belong to a separate phylogenetic branch, together with the Atherinomorpha (smelts and their allies), which also includes the Cyprinodontiformes (toothcarps), for example the livebearing (Poeciliidae) and egg-laying toothcarps, the Mugiloidei (mulletts and their allies), the Plesiopidae (spiny basslets), the Gobiesocoidei (clingfishes and their allies),



LI *et al.* used two *Haplochromis* species for their genetic research. One of them was supposedly *H. nubilus*.

and the Pomacentridae (damselfishes). Hitherto just a single morphological characteristic has linked all these groups of fishes, namely their eggs. The special characteristic of these eggs is sticky threads (“adhesive chorionic filaments”) around the opening (micropyle) by which sperm enters.

LI *et al.* remove both the cichlids and the Pomacentridae from the Labroidei (wrasses and their allies), and, instead of erecting a new suborder for the new group, create a new order which they term the Stiassnyformes. Eggs such as are found in the above-mentioned groups are also known from other groups of fishes that LI *et al.* have not investigated but which they now consider to be possible members of their Stiassnyformes. These are the Pseudochromidae (dottybacks), Opisthognathidae (jawfishes), and Grammatidae (basslets). These and the Pholidichthyidae (convict blennies) had previously turned up as a phylogenetic lineage in an earlier study by SMITH & CRAIG (2007), but the latter authors did not cover all the spiny-finned fishes dealt with in the current study. However, the Gobioidae (gobies and their allies) and the Kurtidae (nurseryfishes) also possess such eggs, but according to the study by LI *et al.* belong to another phylogenetic lineage. On the other hand, the Embiotocidae (surf perches) and Mugiloidei (mulletts and their allies) do not have such eggs but have been found to be members of the Stiassnyformes on the basis of DNA markers. The authors do not offer any credible explanation for these anomalies. A further anomaly in their work is that it creates another new order, the Blenniiformes (blenny-like fishes), which is supposedly part of the Stiassnyformes. An order part of another order???

Overall the Acanthopterygii (spiny-finned fishes) have been subjected to a huge re-organisation. Additional new taxa created by LI *et al.* at higher levels include the Zeioigadiformes, Stromateoidei, Anabantiformes, Paratrachinoidei, Zoarciformes, Cottimorpha, Triglimorpha, Serraniformes, Carangimorpha, and Epigonoidei. It remains to be seen whether the results of the research are confirmed by other scientists or whether there are other possible interpretations. Specifically, obvious differences in phenotype, for example between toothcarps and cichlids, are in no way explained by the new grouping.

The *International Code for Zoological Nomenclature* does not regulate strictly the treatment of taxa above superfamily and lays down only very sketchy precepts, for example that the name must be uninominal (consisting of a single name). There is, however, no precept requiring that a subordinate taxon should be designated as the type of a superior taxon above the level of superfamily. There is also no requirement to indicate the intention to create a new name by an explicit additional phrase, as is expressly prescribed for a new species (n. sp.), new genus (n. gen.), and other taxa up to the level of superfamily. It would nevertheless be sensible to do so – but LI *et al.* have not.

As a rule it is customary at the higher levels of taxonomy to name a superior category after a well-known representative, which in this case might have meant the Cichlidiformes, for example. Not so in this case. LI *et al.* have named an entire order after a living ichthyologist, MELANIE L. J. STIASSNY, the Curator of the Department of Ichthyology at the American Museum of Natural History. What an honour!

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A new *Haplochromis* described from the Katonga

by FRANK SCHÄFER

Katonga is the name of a river in Uganda and more recently also that of a *Haplochromis* species to date found only in this water. The Katonga doesn't really deserve the name of river at all, as it is largely choked with papyrus and reeds. The swamp in which the Katonga rises, and which it drains into Lake Victoria to the east, is also drained by the Mpanga westwards to Lake George. The Katonga and Mpanga thus theoretically constitute a connection between Lake Victoria and lakes George and Edward. However, GREENWOOD (1973), wrote that the upper course of the Katonga was uninhabitable for all fishes except those that breathe air - and thus implied that this river did not permit any genetic exchange between Lake Victoria and lakes George and Edward.

During an expedition in 1998 HERBERT TICHY and ERWIN SCHRAML fished in the Katonga not far from its source and despite GREENWOOD's statement caught gill-breathing fishes there - small barbs (*Barbus* sp.), *Pseudocrenilabrus victoriae*, *Oreochromis* sp., and a *Haplochromis* species that could not be assigned to any previously described taxon. Because it was the only



The holotype of *H. katonga*, the only adult male of the species found to date, shortly after capture.

Haplochromis known from this river to date, they described the species as new, choosing the name of the river as the specific name and *Haplochromis* as the genus name. In so doing they followed the advice of VAN OIJEN, who has recommended choosing this generic designation for all haplochromine cichlids from the Lake Victoria basin for the time being. In fact the new species cannot be unequivocally assigned to any of the genera erected or revalidated by GREENWOOD, as it exhibits affinities with *Astatotilapia* as well as with *Enterochromis*.

The fishes were relatively uncommon at the collecting site near the hamlet of Kabagole. Despite an extensive search, during which large parts of a channel cleared for canoes were fished with a drag net, only nine specimens came into the hands of the describers, and only one of those an adult male.

Genetic studies have shown that this species is more closely related to species from Lake Victoria than to those from lakes Edward and George or other haplochromines. At less than seven centimetres (SL), the species remains relatively small. Because male and female cannot unequivocally be assigned to a single trophic group and no studies were undertaken



One of the females of *H. katonga* from the type series.

of the food consumed, it can only be surmised that these fishes are either an insect-eating species or that they feed on zooplankton. The heavy black deposits on the jaw teeth indicate a particular, as yet unclarified dietary preference. The bold red coloration on the anal and caudal fins, the iridescent greenish colour of the flanks, and the bluish-grey head and dorsum in males are very distinctive and would also make it an attractive aquarium fish. A pity that the species has to date not been imported alive.

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The gender of the genus *Haplochromis* and other *-chromis*

by MARY BAILEY and ERWIN SCHRAML

Anyone who has had anything to do with the subject of taxonomy will be aware that the grammatical gender of a genus name can have an effect on the species name, specifically when the latter takes the form of an adjective. Grammatical gender is a feature of most languages, especially those of Europe, and is even found in Old English (Anglo-Saxon), albeit not in modern English. The German reader of **eggspots** will not, however, derive any great benefit from this in terms of understanding, as the Latin-derived genders involved here are significantly different to the German.

As always where taxonomy is concerned, the Code of the International Commission on Zoological Nomenclature (ICZN) provides relevant precepts, and in this case in particular, helpful examples as well. Although these rules occupy several pages in the Code (and hence repeating them in full in this article would take too much space), the examples nevertheless cannot hope to explain everything relevant, and as a result authors without any special knowledge of Latin often make mistakes. The commonest is that they do not appreciate when a species name takes the form of a noun and when it consists of an adjective. Examples of this include names that end in *-taenia*, which means 'band' and hence is a substantive, while the ending *taeniatus* (*-ata*, *-atum*) signifies 'banded' and is, of course, an adjective. In fact the matter can be even more complicated, as names that end in, for example, *-urus* can be either a substantive or an



Chromis chromis (UW-Photo Adriatic Sea), the species from whose name the suffix commonly used for cichlid genera is derived.

adjective at the discretion of the author of the name, and it is common for such names to be incorrectly inflected as adjectives when they are actually substantives. For this reason the ICZN also advises that every new description should not only give an indication of etymology but also state the gender of a name in the case of a new genus, and what type of word the name is in the case of a new species.

You may well ask what all this has to do with cichlids whose genus names end in *-chromis*. Well, if you browse through ESCHMEYER's Catalog of Fishes for genera with such an ending, you will find that they are all designated as masculine. It is also laid down by the Code that the last part of a compound name determines its gender, and because all these compound genus names are formed by the simple addition of *-chromis* to the first part of the name, it is this suffix that determines the gender.

It is also important to realise that this suffix, in the taxonomic sense, is derived not from the Ancient Greek word *chroma* meaning 'colour', but from a marine fish with this name in Ancient Greek. And in fact for a long time various cichlids were assigned to the genus *Chromis* (if you search for the terms *Chromis* and Cichlidae in the Catalog of Fishes you will get a list of 84 relevant entries!), which still exists today but is now reserved for reef fishes of the family Pomacentridae (formerly known as the Chromidae). This is the reason why, for example, *Haplochromis* was originally erected as a subgenus of *Chromis* (for the species *Chromis (Haplochromis) obliquidens*).

The final part of the equation is that in 1986 the genus *Chromis* was designated as feminine by an official act of the ICZN (Opinion 1417). This means that in consequence a number of familiar names, for example *Chromis chrysurus* and *C. flavomaculatus*, are now *C. chrysur* and *C. flavomaculata* respectively.

From this one might conclude that in consequence all genus names that relate to *Chromis* and include this taxonomic suffix would likewise have to be changed, and assume that many African cichlid genera as well as various genera of marine fishes would also be affected. But don't worry! The ICZN, whose main objective is the stability of nomenclature, has put the brakes on the avalanche of name changes that would otherwise inevitably have followed. It is expressly stated in Opinion 1417 that "It is to be noted that this in no way defines the gender of generic names ending in *-chromis*". Phew! So we don't have to relearn everything after all!

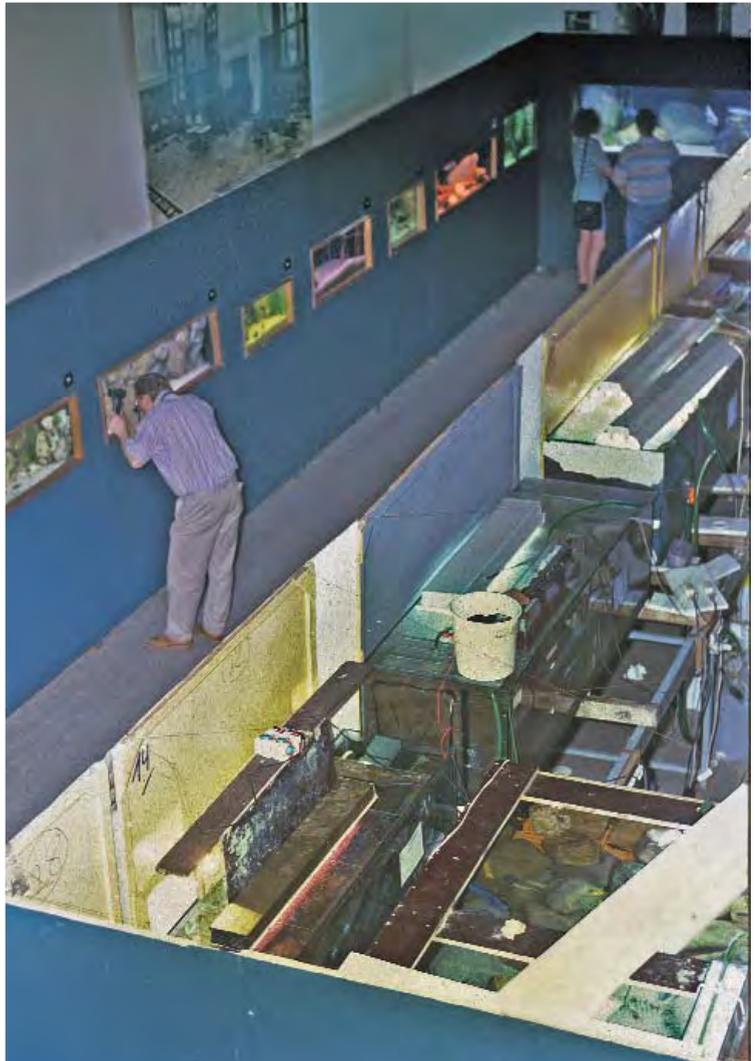
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The end of an era

by ERWIN SCHRAML

Dissolved through lack of interest! These few words, brief and to the point, and dire to the enthusiast's ear, mark the end of an era. Many aquarium clubs are battling against the constant loss of members. But even so it is a shock to hear that a national organisation has found itself obliged to fold. The more so in that the organisation in question is one whose dynamism has brought it fame beyond the borders of its own country. Yet until just a few years ago it regularly organised a major aquarium show in Antwerp. For many clubs and regional groups of other cichlid organisations the "Cichlidenshow" was a reason to visit the city, sometimes by the busload. The members of the Belgische Cichlidenliefhebber Vereniging (A. B. C. V. = Belgian Cichlid Association) put on this incomparable event, sometimes every other year, in a former cinema in the suburb of Borgerhout. The aquaria, some of them constructed of wood, were always attractively and often also innovatively decorated. The fishes, cichlids from all over the world, were impressive, and frequently spectacular and rare. Many an illustrious guest attended the exhibition, and noteworthy experts were always invited to be guests at the assemblies of the association. *Cichlidae*, the informative journal of the association, was independently published even in the early years. In its heyday the club had several hundred members, but only five turned up at the last member assembly, even though it was known that matters had reached a "life or death" situation. Given that sort of lack of interest, the "last of the Mohicans" had no choice but to dissolve the association on 22nd October 2009, after more than 35 years in existence.



View from the upper floor of the former cinema, showing some of the aquaria at the (9th) "Cichlidenshow" for which the Belgian Cichlid Association was famous far and wide.

A little known *Pseudocrenilabrus* species from East Africa

by RICO MORGENSTERN

It was a good ten years ago that I first saw an unfamiliar *Pseudocrenilabrus* species on sale in a Chemnitz pet shop as *Pseudocrenilabrus philander*. Interestingly there was just one male among the 20 or so individuals, and I bought it along with three females. These fishes exhibited certain similarities to both *P. philander* and *P. victoriae*, but I came to the conclusion that they must be another species. And initially I was unable to discover any more about them, so until further information became available all I could do was hope that they would grow well and breed. Unfortunately, however, this was not the case: two females died after only a few days, the remaining pair spawned several times and the eggs developed, but I was unable to rear any youngsters, as they proved very delicate. And finally an outbreak of the normally easy-to-treat *Ichthyophthirius* (or the treatment used?) led to the loss of the fishes. Enquiries at the shop where I had purchased the fishes failed to provide either information on their origins or the opportunity of obtaining more. I first found photos of the species in LAMBOJ (2004), where on page 221 a male and female of unknown provenance are depicted as *Pseudocrenilabrus philander*. A short time later I came across an interesting article by SEEGER (1996a), which not only pictured "my" fish but also gave its provenance: apparently it originated from the upper Ruaha drainage in Tanzania (see below).



Pseudocrenilabrus sp. "Ruaha", male in courtship coloration.

In February 2006 I finally again saw live specimens. Once again the females were in the majority, but at least there were several males this time. Nevertheless I bought just one pair, as I had learned in the course of time that in the case of *Pseudocrenilabrus* species and some other haplochromines it is unwise to keep a single male with a "harem" because of the aggression of the females among themselves. Because the weakest individual is then attacked by all conspecifics, the desired effect of spreading the aggression often becomes precisely the opposite.



Male *Pseudocrenilabrus* sp. "Ruaha" enticing the female to the spawning site.

By and large these fishes can be maintained like other *Pseudocrenilabrus*, but my initial fear that the species was somewhat more sensitive and delicate was to be confirmed. Initially, however, the fishes did quite well and spawned regularly, but it took four attempts before the female finally brooded to term. Unfortunately the fry took hardly any food and died fairly quickly. At the next attempt only bellysliders were produced - the reasons for this are unknown, but I had previously also had this problem with *P. multicolor*. Perhaps it related to the condition of the female, so I separated the fishes for a number of weeks.

When the female's genital papilla was clearly visible I put the male back with her, and he immediately began to court her. I was now able to observe the courtship and spawning behaviour in full detail. The male approached the female in an unusual way. He adopted a diagonally downward-pointing position, with all his fins erect. Apart from the rapidly fanning pectoral fins the fish looked as if he had been struck rigid as he slowly moved towards the female. When he reached her he began typical quivering behaviour, displaying in front of the female in a slightly curved position, dorsal fin slightly folded, anal fin spread, but not extended towards the female in the way with which I was familiar

from *P. victoriae* and *P. philander* and which can be seen even more strikingly in species with eggspots. The final stage was typical leading behaviour by the male, to guide the female in the direction of a shallow pit he had excavated previously, and which he repeatedly improved. The entire sequence was repeated numerous times until the female followed to the "nest". The spawning process resembled that of the other *Pseudocrenilabrus*, and will be described and discussed in more detail elsewhere.

After spawning I placed the female in a small rearing tank where she could incubate her brood undisturbed. After 12 days (at a constant temperature of 26 °C) the young were released for the first time, and this time were normally developed. There were around 40 of them. The fry were taken back into the mouth for another four days at night and when danger threatened, but the female then ceased brood care so I caught her out. The fry fed well right from the start, but took only *Artemia* nauplii and sieved pond foods; flake food was accepted only after a week. They grew well, and hence I hoped that this time I would succeed in rearing them. After six weeks they already measured a good two centimetres long and were already exhibiting some of the typical markings of the species. But then I spotted a number of individuals with noticeable respiratory distress, and shortly thereafter there were occasional corpses lying in the aquarium, then increasingly more, and after a week the whole brood was dead. I was filled with despair. The cause remains a puzzle, the water parameters were as they should be. And to cap it all the female had now also become unwell, she was no longer able to swim in the open water but merely "crawl" around on the bottom. Another breeding attempt was obviously out of the question.

I subsequently managed to obtain individuals of the species on two further occasions (most recently in summer 2008), but in each case they were already rather sickly at the time of purchase and died soon afterward, without producing any offspring. And that was the end of that chapter, but I think that there may still be aquarium strains in existence, as these fishes keep turning up and are also sometimes available simultaneously in a number of different shops.

Provenance

According to SEEGER (1996a), this fish originates from the Usangu Plain in the upper Ruaha drainage in Tanzania. More precise details of the location can be found in SEEGER (1996b: 716) (translated from German): "Finally, it turned out that the Kimani River is crossed by the road to Dar es Salaam around 95 kilometres east of Mbeya; however, there are no waterfalls there. But there may well be a fast-flowing stretch of river [...]. Nowadays a weir has been constructed there and part of the water diverted for irrigating rice crops. Trial collections demonstrated that not only does *Parakneria tansaniae* occur regularly at this spot, but also that numerous other fishes are to be caught there, such as various barbs, *Labeo cylindricus*, *Mesobola spinifer*, *Amphilius jacksonii*, *Leptoglanis rotundiceps*, *Chiloglanis deckenii*, *Haplochromis* sp., and *Pseudocrenilabrus* sp. A very large number of the species found here are rheophilic [...], even though the current is certainly not very turbulent (in the most literal sense of the word)".

SEEGER (pers. comm.) found the species only at this site, and I have so far been unable to find any other evidence regarding this or additional locations. There are no details known of how these fishes came to be in the trade. They might be linked with the specimens brought back by SEEGER, but it is also possible that they have been imported several times, as killifish enthusiasts have now found their way into the area. For unknown reasons,

enquiries as to the provenance of these fishes in various shops have not been answered with any great willingness, though in one case I did manage to elicit the information that they came from a Czech breeder or wholesaler.

Even though the precise origins of the fishes I kept thus remains unclear, I nevertheless believe that



Above: Spawning procedure is as in other *Pseudocrenilabrus*, here the T-position.



Left: Female with larvae in her mouth.

Below: For several days after first release of the brood, the fry are taken back into the female's mouth when danger threatens.

their very distinctive characteristics leave no doubt that they were the same form. I am of the opinion that they do not belong to *P. philander* in the broad sense. I am far more inclined to regard them as a well-defined and distinct species, for which I suggest using the provisional name of *Pseudocrenilabrus* sp. "Ruaha".



Characteristics

This is a rather slender, elongate fish, which in my experience attains a total length of around eight centimetres; females remain somewhat smaller. The upper head profile is slightly rounded, the mouth relatively small. The dorsal fin is rather low, and the soft-rayed part is pointed, at least in males. The first ventral-fin ray is no or only slightly longer than

the second, so that the form of ventral fins often appears almost rounded, especially in females.

The markings are very characteristic; they consist of a longitudinal band, around two scale-rows wide anteriorly, tapering to one scale-row wide on the caudal peduncle. The posterior part (from around the level of the spinous part of the anal fin) is continuous, while on the anterior body there are regular lighter areas, creating the impression of a row of spots. Depending on mood, these spots can in turn become part of a pattern of crossbands, most clearly seen in females during brood care. Dominant males almost always exhibit a dark (lachrymal) stripe on the snout.

Males have a brownish base coloration, paling to yellowish on the flanks and white on the underside. The scales on the upper half of the flank and on the caudal peduncle are metallic bluish green, those on the mid-flank golden, and those on the belly light blue. The lower lip is bright blue, a narrow stripe of the same colour extends along the lower margin of the cheek to the preoperculum, and there are a number of additional irregular metallic blue markings on the lower part of the operculum. The dorsal fin has a blackish margin, somewhat broader anteriorly and tapering posteriorly, followed by a narrow red band and then a striking metallic blue stripe extending right to the tip of the fin. There are hints of this in many members of the genus, but I know of no other form in which it is so clearly and evenly expressed. The proximal part of the dorsal fin exhibits a double row of deep red spots on the spinous portion, which is covered in fainter blue markings; in the soft-rayed part there are alternating metallic blue and reddish stripes or rows of dots running diagonally across the fin-rays on a background of transparent yellowish green, becoming colourless distally. The upper part of the caudal fin is similar, but much more faintly coloured, in particular the blue dots are barely visible. The lower half of the caudal fin is yellowish, at its base there are several large, irregular, metallic blue dots, and with increasing age vertical rows of dots extend posteriorly. The anal fin exhibits broad metallic blue stripes separated by only narrow yellowish interspaces, a grey anterior margin, and a small, irregular red spot at the tip. The ventral fins are reddish in colour apart from the anterior margin.

During courtship the coloration intensifies, the sides of the head and body are now bold golden yellow, a number of scales above and behind the pectoral fins have red centres, and the red on the fins, especially the ventral fins, becomes more intense. Interestingly the sooty black coloration seen on the throat and breast, as well as the on the ventral fins, in many other species is almost completely absent in this case.

The females resemble those of *P. victoriae* and some forms of *P. philander* in their coloration, but are can easily be distinguished on the basis of their typical markings. Traces of the striking dorsal-fin markings in males are also apparent in females, and the intensity of the blue head pattern that brightens up females during brood care also appears to be diagnostic.

Comparison with other species

So how is this species to be classified taxonomically? To date two members of the *Haplochromis* assemblage have been described from the Rufiji/Ruaha system, namely *Paratilapia kilossana* STEINDACHNER, 1914 and *P. vollmeringi* STEINDACHNER, 1914. The types are adult specimens; one syntype of *P. kilossana* originates from Kilosa (Wami system), the

second, as well as the holotype of *P. vollmeringi*, from Kidatu on the Ruaha. In the light of current knowledge the detailed descriptions and illustrations in STEINDACHNER (1916) leave little doubt that they are male and female of the same species, although REGAN (1922) synonymised *P. kilossana* with *Haplochromis bloyeti* and *P. vollmeringi* with *H. gigliolii*. He regarded the latter species as “perhaps not distinct from *H. moffati*” (= *Pseudocrenilabrus philander*). However the description of the holotype of *Hemichromis gigliolii* PFEFFER, 1896 from the Kingani River (nowadays the Rufu) corresponds to that of an *Astatotilapia* species widespread in the Tanzanian coastal basin, which according to SEEGER (1996c) should be assigned to *A. bloyeti* (SAUVAGE, 1883). The type locality is “Kandoa” (also Kondo; according to old maps this place was situated in the vicinity of what is now Kilosa, Wami drainage, and is not to be confused with the modern Kondo). *Ctenochromis strigigena* PFEFFER, 1893, described from Mbusini and Matamondo in the lower Wami drainage, has several times been confused with *Pseudocrenilabrus* species, and is today regarded by some authors as likewise a synonym of *A. bloyeti*.

From this we may assume that we are dealing with an undescribed species. As far as is known at present its distribution is isolated; the closest populations geographically are to be found in the Malagarasi (*Pseudocrenilabrus victoriae* SEEGER, 1990; for the status of the species see MORGENSTERN 2010), in the Wembere (*P. cf. victoriae*), in the Lake Rukwa system (only the upper Saisi drainage; *P. cf. philander*), as well as in the Lake Malawi basin (*P. philander* (WEBER, 1897)). It is noteworthy that this is the only *Pseudocrenilabrus* species found to date in an eastward-flowing system north of the Zambesi, if we discount *P. philander* from the Lake Chilwa/Chiuta basin (Ruvuma system) and a probably introduced population of *P. victoriae* in the Athi River (Kenya).

P. victoriae resembles *P. sp. “Ruaha”* as regards the metallic scale pattern, which extends practically all over the exposed part of each scale, above all in the ventral region and the anterior part of the flank, unlike in *P. philander* where it is limited to the outer margin of the scales. In addition, both forms exhibit only a small amount of black in the ventral fins. *P. victoriae* is, however, less slender and has noticeably pointed ventral fins in both sexes, and in adult males these may even be prolonged into fairly long threadlike extensions. The form of the mouth is similar in *P. victoriae*, but the mouth is somewhat smaller. In addition both lips are coloured bright blue, while otherwise there are no blue head markings present. There is at best only a hint of the blue band in the dorsal fin, but on the other hand the caudal fin is much more intensely and evenly spotted with blue. The anal fin is more striped rather than spotted, but the pattern is less dense in this case. Finally, the large, bright orange-red spot on the tip of the anal fin is particularly characteristic.

I regard the form from the Wembere/Kitangiri drainage in northern central Tanzania as rather similar, at least on the basis of the photo published by SEEGER (1990, 1996a). In particular the dorsal fin pattern is highly reminiscent of that of the fish portrayed here, but the blue submarginal band is not so clearly expressed. The fish in the photo exhibits a similar head and body form, but is not quite as slender. It resembles *P. victoriae* as regards the blue lips, the anal-fin pattern, and the long ventral fins.

It is difficult to make generalisations regarding differentiating characteristics when it comes to the multitude of different forms currently grouped together as *P. philander*. In this case reference must be made to the typical characters of *P. sp. “Ruaha”*, which are unknown from any other form, at least in this combination. *P. philander* usually has a more robust body form and a larger mouth, the dorsal band is absent or only weakly expressed,

the anal fin is spotted rather than striped, and the ventral fins in reproductively active males are as a rule at least partially black pigmented. In addition they are usually pointed and the first soft ray is prolonged into a short threadlike extension, but this doesn't apply in, for example, the form described as *Pseudocrenilabrus* sp. aff. *philander* from the upper Saisi drainage (Rukwa system) by SEEGERS (1996c); similar fishes are also found in the upper Chambeshi region and in southern affluents of Lake Tanganyika). This fish is, however, quite different in coloration and corresponds to other forms of *P. philander* as regards overall habitus.

The form of the ventral fins in the Ruaha fishes is, however, interesting for another reason, particularly in connection with the slender form. Specifically, rounded ventral fins, in which the second ray is somewhat longer than the first, and body form - in addition to scalation characters - were the reasons why GREENWOOD (1979, 1984) assigned *Orthochromis machadoi* (POLL, 1967) a rheophilic cichlid from the Cunene in Angola, to the genus *Orthochromis*, although in terms of colour characters, including the genus-typical anal-fin pattern, it corresponded closely to the genus *Pseudocrenilabrus*. In the original description POLL had already indicated a close relationship with *P. philander*, and according to him the morphological characteristics shared with the species then assigned to the genera *Orthochromis* and *Rheohaplochromis* were the result of convergent evolution. This appears thoroughly plausible, as many of these characters are also found in rheophilic cichlids from unrelated groups (including New World species). In addition, according to DNA studies (KOBLMÜLLER *et al.* 2008) *O. machadoi* should be assigned to the genus *Pseudocrenilabrus*. In terms of certain morphological characters (only in relation to the expression of the characters, a close relationship is unlikely by virtue of the widely separated distribution regions) *Pseudocrenilabrus* sp. "Ruaha" may occupy a position between *O. machadoi* and the rest of *Pseudocrenilabrus*, but this is something that requires more detailed study.

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Maylandia or Metriaclima - the case for Metriaclima

by MARY BAILEY

In an earlier issue of **eggspots** (no. 2) editor ERWIN SCHRAML (2009) eloquently put forward some arguments in the dispute regarding *Maylandia* and *Metriaclima*, but I think it is quite clear that his views accord with the proponents of the former. So here is the view from the other side of the fence.

To understand why *Maylandia* is a *nomen nudum*, and why this is important, it is necessary to consider the fundamental objective of taxonomy, which is, to put it as simply as possible, one name per taxon (genus, species, etc), so that anyone anywhere who encounters the scientific name of an animal will know precisely what animal is meant. The rules say (again put as simply as possible) that the earliest name used is the correct one ("Principle of Priority"), provided the name is valid, and the ICZN Code of Nomenclature lays down various further rules (termed "Articles") that define what makes a name valid or not.

As SCHRAML has stated, Article 13 is the critical rule in the case of *Maylandia*, but to date I have yet to find any pro-*Maylandia* author who has considered the fundamental purpose of that rule, or, indeed, actually understood its wording. Much of the difficulty seems to me to hinge on linguistic difficulties, as most of the pro-*Maylandia* protagonists do not speak English or French as their mother tongue (if at all), while the Code is laid down in both languages and MEYER & FOERSTER's paper was published in the latter. A short word on my own credentials here: English is my mother tongue and I have a degree in it, as well as working professionally as an editor for some years, making sure material - some of it scientific - says what its (often non-English) authors intend. I have also worked for almost 20 years as a translator of ichthyological and other biological French and German into English, so MEYER & FOERSTER's paper holds no linguistic mysteries for me. Among the *Maylandia* supporters many have little or no French, and only CONDÉ & GÉRY (1999) were native French speakers - and, perhaps significantly, they took refuge in a quite different argument to everyone else - but more of that later.

Differentiate and differentiate from

Article 13 requires that a name must be linked to a particular taxon - eg genus, subgenus, or species - by the provision of information, in words, that defines the taxon. The point of this is to allow other workers, perhaps with a pile of specimens to identify and classify, to determine whether any particular specimen belongs to that taxon. In order for this to be possible the description MUST provide an unequivocal list of characters that together define that taxon and no other. This is the meaning of the verb "differentiate" as used in Article 13 (in all three of the 2nd, 3rd, and 4th editions!). "Differentiate" used in this context

means “define”, a specialised usage, and is not the same as “differentiate from” – the commonplace, everyday usage which “*Maylandia*-ites” cite so regularly with regard to MEYER & FOERSTER’s alleged (see below) differentiation of *Maylandia* from *Pseudotropheus williamsi*. Differentiating taxon X **from** taxon Y tells us **only** how to tell that X is not Y; it **does not** tell us how to distinguish X from anything else (in this case other closely-related (ie mbuna) genera and subgenera), or how to determine whether any particular animal belongs to taxon X, which is what Article 13 and “differentiate” require.



A tacit cry for help? *Maylandia* or *Metriaclima* – after so many years a conclusive decision would seem called for.

To give a trivial example. You could differentiate me from the editor of **eggspots** quite easily as I am a lot smaller and don’t have a beard, but that probably applies to most of the women and a lot of the men on this planet, and it does not tell you a thing about who I am, what makes me MB rather than any other woman you might come across. But “female English writer on cichlids” would enable a lot of fishkeepers to make a fair stab at who you were talking about.

With me so far?

Unfortunately very few of the people who support *Maylandia*, even those whose mother tongue is English, seem to appreciate this semantic nicety, but I am in no doubt whatsoever that the authors of the Code DID, and that they intended “differentiate” to mean “define”, ie differentiate from ALL other similar taxa, not just one. The introduction to the Code is quite specific about its use of precise, unambiguous language to convey specific meanings. Moreover any other sense

of “differentiate” would render the rule pointless as it would not fulfil its purpose, ie to ensure that a name is valid only if it does its job in allowing others to identify specimens as belonging or not belonging to the taxon in question.

Maylandia or zebra complex?

SCHRAML has drawn attention to another relevant problem of interpretation, this time of the wording of MEYER & FOERSTER rather than the Code. To avoid readers having to refer back to his article I will again quote the relevant text (translated by myself and, I believe, accurately).

“Some other species of *Pseudotropheus*, s. lat., resemble *Ps. greshakei* and differ from *Ps. williamsi* by having less regular internal rows of teeth and, at least in adults, containing many unicuspid, small pharyngeal teeth which are very close-packed posteriorly, and the melanin pattern on the body forming vertical bars which are more or less visible. Such species include *Ps. zebra* BOULENGER, *Ps. aurora* BURGESS, *Ps. lombardoi* BURGESS and *Ps. livingstonii* BOULENGER. The last two species have, in females and non-territorial males, a well-defined pattern of six vertical bars on the body, five of which extend into the dorsal and which diminish to the point of disappearing on the lower part of the flanks. *Ps. zebra* and the many related forms have vertical bars, as in *Ps. greshakei*, but much bolder. We suggest that this *zebra* complex should be included in the subgenus *Maylandia*.”

Maylandia supporters assert that the characters given here constitute a description of *Maylandia*; SCHRAML cannot understand why supporters of *Metriaclima* refute this. I can only assume that the problem is again linguistic, as it is quite clear and totally unambiguous that the characters listed relate **not** to *Ps. greshakei* but to “Some other species”. These “other species” are subsequently listed in part and finally described as a “zebra complex”. This group **does not** include *Ps. greshakei*, it merely resembles that species, and that means nothing as a resemblance does not necessarily imply a close phylogenetic relationship, it can be the result of, for example, similar environmental pressures producing a similar result.

But, *Ps. greshakei* is the type species of *Maylandia*. As the characters listed do not belong to its type species, they cannot possibly constitute a description of *Maylandia*.

Again, *Maylandia* supporters argue that what the text means is “.....resemble *Ps. greshakei* and like it differ from *Ps. williamsi* by having.....”, in an attempt to include *Ps. greshakei* in the list. But the text does not say that, it is describing two separate and unrelated features of the “other species”. And it is what you say, not what you mean to say, that is critical in science. Were scientific texts open to individual reader interpretation then the result would be chaos, so the assumption **must** always be that what is said is what is intended.

Note that in any case this list of characters that *Maylandia* supporters point to as a “description” of the subgenus do not unequivocally define anything as required by Article 13; they mainly differentiate the *zebra* complex from *Ps. williamsi*,

which we have already seen is not a valid method of establishing what a taxon **is**, only a very limited what it is not.

Moreover the authors themselves are unsure which species should be included in the group they are “describing” and even go on to say that this depends on the weight assigned to one character.

“Qu'il faille aussi inclure *Ps. livingstonii* et *Ps. lombardoi*, depend de la valeur que l'on accorde au patron melanique.” (Whether or not *Ps. livingstonii* and *Ps. lombardoi* should be included depends on the importance that one assigns to the melanin pattern.)

Astute readers will note the paradox - *Maylandia* supporters assert that MEYER & FOERSTER have differentiated (defined) *Maylandia*, but the authors themselves state that their “definition” is not concrete, and what you include depends on how important you consider one of the “defining” characters. But it was **their** job to decide on the relevance of that character.

Ah, but *Maylandia* supporters might argue that the list of characters has only to “purport to differentiate” (= define) the taxon, that is, state what the authors believe is a valid definition at the time of writing. Well yes, but that is because new species and new information may come along later and require the taxon to be redefined (as has happened with *Metriaclima*, hence the 2006 redecription by KONINGS and STAUFFER broadening the scope of the genus in the light of additional knowledge). The Code cannot reasonably require that the description of a taxon **does** define it unequivocally, as a biologist can work only with the information currently available, which may not be complete. Hence biologists are merely required to provide what they believe to be a valid definition. But MEYER & FOERSTER’s “description” doesn’t do anything of the kind, as they themselves immediately question its validity on the basis of information that was available but which they hadn’t properly considered.

To sum up, MEYER & FOERSTER actually provide a very sketchy description of the “zebra complex”, not *Maylandia*, and aren’t sure what species actually belong in that complex. They actually exclude *Ps. greshakei* (the type of *Maylandia*) from the zebra complex; and to cap it all, they aren’t even sure if this complex does belong in *Maylandia*, they only suggest its inclusion, they don’t actually state it **is** to be included.

The other “evidence” for *Maylandia*

By now you are probably quite confused, which is not surprising as MEYER & FOERSTER’s work is itself totally confused, and it is essential that scientific work should be clear and unambiguous. But I hope that you also now understand why *Maylandia* does not satisfy Article 13 (wording and intention) and hence is unavailable, a *nomen nudum* with no differentiating characters, not even for the zebra complex.

Nevertheless I think it is necessary to also “poke holes” in some of the other “evidence” adduced by *Maylandia* supporters discussed by SCHRAML.

KULLANDER (1999): laudably - and rather over-emotionally - defends the Principle of Priority, which states that the oldest name is that which should be used, but omits to mention that the Principle of Priority applies only to names that satisfy the criteria of availability (for our purposes = validity) laid down by the Code. He adduces no arguments to defend the availability of *Maylandia*, and it is not enough simply to say a name is available in disputes like this. Even if you are a world-famous cichlid taxonomist you must back up your statements with arguments and facts. So strike KULLANDER from the debate.

CONDÉ & GÉRY (1999) assert that Article 13d of the 1985 edition of the Code applies, and hence *Maylandia* is valid as MEYER & FOERSTER described the subgenus and its type species in a single description. However, Article 13d applies only to monotypic genera (those containing one species, where there is no point in separate descriptions as the genus is based on just the one species). But we have another school of thought arguing that *Maylandia* is valid because MEYER & FOERSTER provide a description by listing a set of characters that apply to the *Pseudotropheus zebra* complex which also belongs to the new subgenus. If you subscribe to that view then this means that in writing their paper MEYER & FOERSTER were aware that *Maylandia* was not monotypic and so Article 13d cannot possibly apply. Moreover, as SCHRAML points out, the rule doesn't apply to subgenera (though why is a mystery, but the Code admits to not being perfect, so it could be an oversight). So strike CONDÉ & GÉRY!

KONINGS (2005) made some small errors in his translation of MEYER & FOERSTER. So? These were of no relevance whatsoever to his arguments. Perhaps the object of pointing out these errors was to discredit KONINGS' understanding of the paper in general? I can see no flaws in KONINGS' arguments based on his understanding of the paper.

The abstract to MEYER & FOERSTER's paper states that they are describing a species and subgenus. So? Abstracts are merely brief summaries designed to tell the reader what a paper is about, so he can decide whether to read it. They are of dubious taxonomic validity as they are often written by the publisher, and where they appear in more than one language they frequently do not say precisely the same thing. MEYER & FOERSTER's paper contains abstracts in English and German (but not in the language of the paper - French - as one would expect), and these two abstracts are so very different that they might be summarising two different papers! So, which abstract do we take as cogent and valid? The English one cited by SCHRAML, or the German one (MEYER & FOERSTER's mother tongue) which does not make the statement about the subgenus and species? Search me!

One argument not mentioned by SCHRAML, but which I've seen elsewhere, is that Dr ETHELWYNN TREWAVAS peer-reviewed the paper, as stated by its authors. That may be true, but it doesn't mean that she approved it, for all we know she may have said it was utter rubbish. Her own paper published in the same issue of the journal is, after all, to infinitely higher taxonomic standards and has not, as far as I know, been challenged. There is no obligation on authors and editors to take any notice of reviewer comments, though editors usually do and have the power to reject papers unless amended or justified by the author. I would mention that

peer review is supposed to be anonymous, and the fact that in this case it clearly was not suggests to me that the journal's peer-review policy was at best not as strict as desirable.

Retro-active or not?

SCHRAML asks whether the setting of a date from which a new rule applies is truly intended to be retro-active, ie affecting names previously regarded as valid under the previous Code. The answer is, quite simply, YES! The introduction to the 1985 Code makes this quite clear: "(6) The Code provides guidance for zoologists needing to establish new names, **and rules to determine whether any name, previously proposed, is available** and with what priority; **whether the name requires amendment** for its correct use....." (my bold type). From this it is quite clear that the Code is intended to be retro-active.

The latest (2004) edition of the Code caused some major retroactive changes to names, most notably the proper way to deal with unlauted u (ü) in names based on proper names, such that u or ue is now correct depending on a cut-off date, and names after that date need to be corrected where they do not conform. Even more far-reaching is the requirement by the ICZN for the dates of names to be the date when the paper appeared in print. In days of yore, papers, for example those of the Zoological Society of London, were frequently read to a meeting and only printed later, and the date of the reading was taken as the date of the paper. Not any more! It now has to be the ACTUAL date of publication, and this has caused huge confusion. For example, REGAN's revision of Lake Malawi cichlids, cited in much of the literature and a host of names as REGAN, 1921, is now REGAN, 1922.

SCHRAML has questioned the relationship between these retro-active rules and stability in nomenclature, one of the most important objectives of the Code. But in fact it's quite simple. Although the Code is designed to be absolutely explicit and unambiguous in its wording in order that the INTENTION of the rule is fulfilled, sometimes this objective is not achieved and clarification of a rule is required, by rewording, and where the old, inadequate rule has caused problems, then to put these right the new rule must be retro-active. Nowadays we write specific names with a lower case letter - *greshakei* - and hyphens aren't allowed, but it wasn't always thus, many names have been corrected retro-actively, even before the first edition of the Code was published in 1961. Was that wrong too?

We should, perhaps, consider that the fundamental date of 1st January 1758, from which modern taxonomy is taken to begin, was not selected at that time but much later, and the decision taken that - retroactively - all names dating from before that date would be disregarded.

Fall-out

There has been a certain amount of unfortunate "fall-out" linked to the dispute, in part a function of our electronic age. I have been told by a number of authors, who believe *Metriaclima* to be the correct name, that they have been obliged to use *Maylandia* in order to have their work published, as the editors of some

scientific journals and some peer-reviewers are stating that *Metriaclima* is invalid because Fishbase or some noted taxonomist has plumped for *Maylandia*.

One thing must be made clear - no organisation such as Fishbase, no institution, and no individual such as an editor or peer-reviewer has the right to pre-empt the role of the ICZN in ruling on nomenclatural disputes. I can only deplore such attempts to impose a personal, editorial, or institutional viewpoint on the freedom of every author to decide for him- or herself which name to follow until a ruling is made by the ICZN.

In this regard I cannot commend too highly the editorial policy of **eggspots** in allowing authors their rightful freedom of taxonomic expression.

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Period February - September 2010

Amazonas (<http://www.ms-verlag.de/AMAZONAS.121.0.html>)

6 (5), No. 31, September/October 2010:

- SZILLAT, K.: Arterhaltung im Wohnzimmer. (*Yssichromis piceatus*) Pp. 58-59.

AqualogNews (www.animalbook.de)

No. 94:

- SCHÄFER, F.: *Placidochromis* cf. *phenochilus* "Tanzania" - nicht nur schön, sondern auch friedlich. Pp. 24-25.

Aquaristik Aktuelle Süßwasserpraxis (http://www.aquaristik-online.de/index_aquaristik.html)

18 (5):

- STAECK, W.: Buntbarsche aus dem Viktoriasee. Oft attraktiv - doch selten gepflegt. Pp. 40-45.

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42 (1), No. 211, February/March 2010:

- SEIDEL, I.: Eine Zuchtfarm für Malawi-Buntbarsche in Florida. Pp. 12-16.

Buntbarsche Bulletin (Journal of the American Cichlid Association, www.cichlid.org/index.php?pageid=buntbarsche_bulletin)

No 256, February 2010

- STEEVES, G.: Species at Risk Highlights. ('*Haplochromis*' *thereuterion*, *Xystichromis* sp. 'Kyoga Flameback', '*Haplochromis*' sp. 'fine bar scraper'). P. 3.

No 258, June 2010

- STEEVES, G.: Species at Risk Highlights. (*Astatotilapia desfontainii*, *Lipochromis* sp. "Matumbi Hunter", *Prognathochromis perrieri*, *Pyxichromis orthostoma*). P. 3.

- STEEVES, G.: *Pyxichromis orthostoma*. P. 25.

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36 (2) 2010:

- VAN HEUSDEN, H.: Vijf *Orthochromis*-Soorten van Tanzania. Pp. 5-29.

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- KONINGS, A.: Sex determination and the OB pattern in Malawi cichlids. Pp. 6-13.

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- VAN HEUSDEN, H.: *Orthochromis* from Tanzania: Report of a Collecting Trip to the Malagarasi Basin - Part 1. Pp. 6-14.

- KONINGS, A.: The Largest Cichlid of Lake Malawi. Pp. 15-19.

19 (4) October 2010:

- KONINGS, A.: *Pseudotropheus elegans* rediscovered. Pp. 14-17.

- VAN HEUSDEN, H.: *Orthochromis* from Tanzania: Report of a Collecting Trip to the Malagarasi Basin - Part 2. Pp 20-27.

- ANDERSEN, T.: Observations on *Limnochromis staneri* POLL, 1949. Pp 6-12.

DATZ Die Aquarien- und Terrarienzeitschrift (Ulmer Verlag; ISSN 1616-3222; www.datz.de/)

63 (8) 2010:

- SCHRAML, E.: Der Kleine oder Vielfarbige Maulbrüter. Pp. 4-6 in Aquarien-Praxis.

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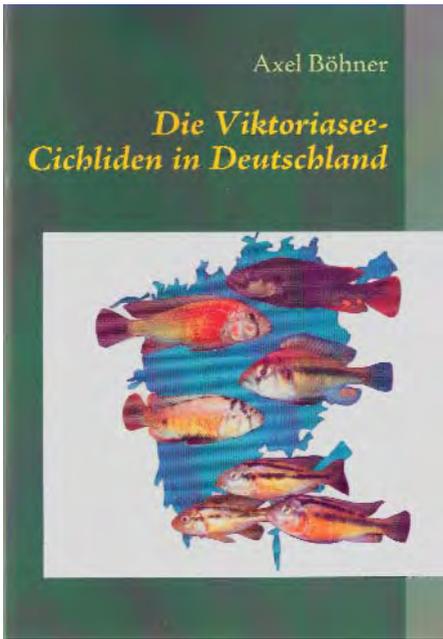
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Aug (8) 2010:

- KONINGS, A.: Ad Konings introduces the kambuzi cichlids of Lake Malawi. Pp. 48-50.



Book review: AXEL BÖHNER (2010): *Die Viktoriesee-Cichliden in Deutschland. Ein Leitfaden für die Pflege der Buntbarsche aus dem Viktoriesee. Books on Demand, Norderstedt. ISBN 978-3-8391-3286-9, 26.90 €.*

In the 146 pages of this book AXEL BÖHNER has managed to cover, in words and photos, almost all the cichlid species of the *Haplochromis* assemblage currently available in Germany, totalling more than 60 in number. For all of them the author provides a summary of distribution, size, coloration, the appropriate aquarium, feeding, social behaviour, breeding, and so on. When it comes to scientific names BÖHNER conforms with GREENWOOD's proposals, but at the same time he doesn't omit to include synonyms and trade names. The names used are also correct, with a few exceptions (for example *Astatotilapia nubila*). The species are all illustrated with colour photos. It is true that some are rather small (though several views of the species are generally given to compensate) and because of the paper quality the print standard isn't quite up to that usual for glossy magazines, but nevertheless the overall impression can be described as generally attractive. The book is rounded off by an introductory section discussing Lake Victoria and its problems, the habitats of the cichlids in the lake, and general details of maintenance and breeding, creating a successful reference work, written by an enthusiast for enthusiasts. Published in German only.

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