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Taxonomic notes on the tribe Aphnaeini and genus *Thestor* Hübner (Lepidoptera: Lycaenidae) arising from D'Abrera (2009)

by

A. Heath

alan.heath3@virgin.net

and

E. L. Pringle

epringle@eastcape.net

ABSTRACT

In response to taxonomic changes proposed in D'Abrera (2009), *Chryсорitis zeuxo cottrelli* (Dickson, 1975) stat. rev. and *C. swanepoeli hyperion* (Dickson, 1975) stat. rev. are reinstated as a valid subspecies. Various instances where D'Abrera (2009) has changed, failed to mention or overlooked extant taxonomy in the tribe Aphnaeini and the miletine genus *Thestor* Hübner, [1819] are noted and discussed.

INTRODUCTION

In his final volume of *Butterflies of the Afrotropical Region* (Part III, devoted to Lycaenidae and Riodinidae), D'Abrera (2009) introduced some taxonomic changes *inter alia* in *Thestor* Hübner, [1819] and in the tribe Aphnaeini. He also overlooked or failed to mention certain taxonomic changes made in these two groups that were proposed in Heath (1997, 2001), Heath *et al.* (2002) and Heath & Pringle (2004, 2007). In this paper we revisit the taxa we have reviewed over the past 14 years that have been affected by D'Abrera's treatment. The distributional data in D'Abrera (2009) also contain numerous errors but these have not been addressed here. Lastly, readers of his books need to be aware that this author does not subscribe to the practice of using parenthetical notation to indicate subsequent generic combinations.

APHNAEINI

***APHNAEUS* Hübner, [1819]**

Type species *Papilio orcas* Drury, 1782, by subsequent designation (Scudder, 1875).

= *Paraphnaeus* Thierry-Mieg, 1904, type species *Aphnaeus hutchinsonii* Trimen (synonymized by Stempffer, 1954: 516).

The monotypic genus *Paraphnaeus* was erected by Thierry-Mieg (1904) for *hutchinsonii* based solely on the more rounded shape of its underside spots. Stempffer (1954: 516) synonymized *Paraphnaeus* with *Aphnaeus*, again placing *hutchinsonii* in *Aphnaeus*, but later Stempffer (1967: 154) treated *Paraphnaeus* as a distinct genus based on two minor characters, namely "...the shape of the penis [aedeagus] differs slightly ..." and a difference in the lower fultura [juxta]. Heath (1997: 26) treated *Paraphnaeus* as a junior subjective synonym of *Aphnaeus* and provided reasons for doing so based on their morphological similarities, but D'Abrera (2009) apparently chose to not to follow either Heath (1997) or the synonymy proposed by Stempffer (1954).

The characters listed by Thierry-Mieg (1904) and Stempffer (1967) are in our opinion very minor, and not even unique to *hutchinsonii*. Further discussion on these characters can be found in Heath (1997: 27), also the male genitalia of the respective type species are illustrated in Figs 48-49, 71-73, 121-122 and 161-163. From these genitalia drawings it can be seen that the aedeagus of the two type species, contrary to what Stempffer (1967) stated, are of exactly the same type. Hence there being no consistent differences to support *Paraphnaeus* as a distinct genus or even subgenus, we treat it as a junior subjective synonym of *Aphnaeus*, as Stempffer (1954) did initially. As D'Abrera (2009) did not formally reinstate *Paraphnaeus*, the original synonymy stands.

CHRYSORITIS Butler, 1897

Type species *Zeritis oreas* Trimen, 1891, by original designation.

= *Poecilmitis* Butler, 1899, type species *Zeritis lycegenes* Trimen, 1874, by original designation (synonymized by Heath, 1997: 16).

= *Bowkeria* Quickelberge, 1972, type species *Zeritis phosphor* Trimen, 1864 by monotypy (synonymized by Heath, 1997: 16).

= *Oxychaeta* Tite & Dickson, 1973, type species *Phasis dicksoni* Gabriel, 1947 by original designation (synonymized by Heath, 1997: 16).

D'Abrera (2009) did not revoke the taxonomic changes in Heath (1997) concerning the status of this genus. Nevertheless, there are some discrepancies in his publication that require addressing.

C. pan lysander (Pennington, 1962)

= *C. atlantica* (Dickson, 1966) (synonymized by Heath, 2001: 89).

= *C. atalantica* [sic] Dickson, 1966 *sensu* D'Abrera, 2009: 728 (misspelling; misidentification).

In D'Abrera (2009: 726), under the entry '*pan lysander* Pennington, 1962', the taxon *atlantica* Dickson, 1966 is correctly acknowledged as having already been synonymized, but on page 728 a taxon '*Chrysoritis atalantica* Dickson, 1966' is listed as a distinct species. The illustration is of *C. pan lysander*. This is clearly a case of both a misspelling and misidentification.

C. zonaria coetzeri Dickson & Wykeham, 1994

Under the entry for '*Chrysoritis zonarius* Riley, 1938', D'Abrera (2009: 728) proposes that *coetzeri* should not be treated as a "race". He states, "The population from Nieuwoudtville has been (unjustifiably in my opinion) described as the race *coetzeri* Dickson & Wykeham, 1994." As this author did not formally synonymize this taxon, it retains its status as a subspecies. Two additional populations of this subspecies have been discovered recently in the coastal region of Namaqualand, over 200 km north-west of its type locality (see Heath *et al.*, 2008: 135). Material from these populations is consistent in retaining the characters originally described for this taxon, thereby providing further support for its subspecific status.

C. zeuxo cottrelli Dickson, 1975 **stat. rev.**

Under the entry '*Chrysoritis zeuxo* Linnaeus, 1764', D'Abrera (2009) synonymized *cottrelli*, previously known as the eastern subspecies of *C. zeuxo*. It is proposed that *C. zeuxo cottrelli* stat. rev. is here reinstated as a distinct subspecies. The easternmost populations of this species, distributed over a considerable area between Knysna in the Western Cape and the Cockscomb Mountains in the Eastern Cape, are consistently more reddish on the upperside, darker on the underside, and have comparatively reduced black markings. These differences support the status of this subspecies. There is a substantial gap between the ranges of the two subspecies of *C. zeuxo*, noticeably along the coast between Gourits River and Knysna, and inland between Sevenweekspoort and the Kammanassie Mountains.

C. swanepoeli hyperion (Dickson, 1975) **stat. rev.**

Under the entry to '*Chrysoritis swanepoeli* Dickson, 1965', D'Abrera (2009) synonymized *hyperion* with *C. swanepoeli* considering it only a form.

We propose that *C. swanepoeli hyperion* (Dickson, 1975) stat. rev. is reinstated as a high altitude subspecies. Apart from a difference in wing markings, it is univoltine, whilst the low altitude nominate subspecies is bivoltine. Their respective emergence times are not concurrent, although occasional stragglers may possibly occur; hence any gene flow would be severely restricted.

C. chrysaor (Trimen, 1864)

= *C. lycia* (Riley, 1938) (synonymized by Heath, 2001: 87).

Heath (2001) synonymized *C. lycia* (Riley, 1938) with *C. chrysaor*, having found the characters defining *lycia* were neither unique nor consistent; at best it is a form of *C. chrysaor*. An extensive discussion on this point can be found in Heath & Pringle (2007: 19-20). Although D'Abrera (2009: 725) treats *lycia* as a distinct species, without reference to the earlier synonymy, he makes no formal change, hence the earlier synonymy stands.

TRIMENIA Tite & Dickson, 1973

Type species: *Zeritis wallengrenii* Trimen, 1887, by original designation.

= *Argyrocupha* Tite & Dickson, 1973, type species *Cigaritis malagrida* Wallengren, 1857, by original designation (synonymized by Heath, 1997: 19).

The distinction made between the two genera by Tite & Dickson (1973) was based on minor differences in their venation. These differences were found to be unstable and hence were rejected as suitable characters distinguishing *Argyrocupha* from *Trimenia* (Heath, 1997: 19).

This synonymy was supported by the uniform morphology of the respective type species, as illustrated in Heath (1997), in particular the male genitalia (Figs 32-33, 57-58 and 107-108) and wing patterns (Plates 2 & 3). Also they both possess an anal tuft of specialized setae at the tip of the female abdomen. Their obligate ant-association, egg and larval stages are also very similar (Heath & Claassens, 2003: 11). Further discussion supporting this synonymy is given in Heath (1997: 19-20). Although D'Abrera (2009) does not refer to this synonymy, he made no formal change, hence the synonymy remains valid.

CIGARITIS Donzel, 1847

Type species: *Cigaritis zohra* Donzel, 1847, by subsequent designation (Scudder, 1875).

= *Apharitis* Riley, 1925, type species *Polyommatus epargyros* Eversmann, 1854,

by original designation (synonymized with *Spindasis* Wallengren by Heath, 1997: 22).

= *Spindasis* Wallengren, 1857, type species *Spindasis masilikazi* Wallengren, 1857 (jun. subj. syn. of *Aphnaeus natalensis* Westwood, 1851) by monotypy (synonymized with *Cigaritis* by Heath *et al.*, 2002: 90). Note: Heath (1997) synonymized *Apharitis* with *Spindasis* and later Heath *et al.* (2002) synonymized *Spindasis* (and hence *Apharitis*) with *Cigaritis*.

The morphology of species in this genus is quite uniform throughout. The male genitalia are fairly distinctive, having an uncus composed of two lobes deeply divided medially, a substantial saccus, a stout base to the aedeagus and subtriangular valves joined by a membrane dorsally. The labial palps are fairly long, the forewing has 11 veins, there is a delicate tail on the hind wing at vein 1A+2A, and another shorter tail (sometimes stunted or absent) at vein CuA2. The wing pattern is characterized by the underside markings that consist of continuous or fragmented bars outlined in a darker colour on a pale background. These bars are also present, or partially so, on the upper side, particularly the forewing.

In erecting the genus *Apharitis*, Riley (1925) stated that it differed only in coloration; this is not a valid generic character in our opinion. Further discussion on these synonymies is given in Heath *et al.* (2002: 90) and Heath (1997: 22-23), where the male genitalia are illustrated in Figs 35-39, 60-64, 110-114, 140, and 149-152.

D'Abrera (2009: 702) did not refer to these synonymies but quotes Larsen as saying that he would prefer to keep the three genera separate "as a valid reflection of evolutionary history". We respect Torben Larsen for his knowledge and his contribution to lepidopterology but our view is that keeping similar groups together in one monophyletic genus serves that purpose better, and that structural similarities between groups of species in such genera should be acknowledged in taxonomy, not hidden under different genus names. Acknowledging groupings based on minor sub-generic characteristics such as behaviour or wing colour, etc., is better achieved by means of informally named 'species groups'. Based on all the morphological characters studied, we find the species within *Cigaritis* form a homogeneous group and can find no valid apomorphic characters distinguishing *Apharitis* or *Spindasis* from *Cigaritis*. As D'Abrera (2009) made no formal change to the taxonomy, the earlier synonymies stand.

CHLOROSELAS Butler, 1885

Type species: *Chloroselas esmeralda* Butler, 1886, by original designation.

= *Desmolycaena* Trimen, 1898, type species *Desmolycaena mazoensis* Trimen, 1898, by original designation (synonymized by Heath, 1997: 12).

Trimen (1898) erected the genus *Desmolycaena* based upon a character in the forewing venation, but this has since been shown to be unstable in both *Desmolycaena* (see Stempffer, 1967: 164) and *Chloroselas* (see Riley, 1932: 150 and Stempffer, 1967: 162) and hence not a valid character for defining a genus. Further discussion on this is given in Heath (1997: 13), and the male genitalia are illustrated in Figs 12-15, 55-56, 74-75 and 128-131. We are not aware of any valid derived characters distinguishing *Desmolycaena* from *Chloroselas* and despite D'Abrera (2009) not following the earlier synonymy, he did not introduce any formal change, hence the synonymy remains valid.

MILETINAE

THESTOR Hübner, [1819]

Thestor petra tempe Pennington, 1963

T. tempe Pennington, 1963 was synonymized with *T. petra* Pennington, 1963 and treated as a subspecies of the latter by Heath & Pringle (2004: 115), there apparently being no apomorphic characters distinguishing *tempe* from *petra*. Although D'Abrera (2009) treats *tempe* as a distinct species, without referring to the earlier synonymy, he makes no formal change, hence the earlier synonymy stands.

Thestor protumnus terblanchei Henning & Henning, 1993

Thestor terblanchei was synonymized with *T. protumnus* (Linnaeus, 1764) and treated as a subspecies of the latter by Heath & Pringle (2004: 106). Listed under *protumnus aridus* van Son, 1941, D'Abrera (2009: 693) considers that *terblanchei* "...hardly warrants specific or even racial recognition". Yet on the following page, D'Abrera (2009: 694) lists it as a distinct species. This is clearly an oversight and confusing; but since no formal taxonomic change is proposed, the taxonomic status quo as per Heath & Pringle (2004) remains valid.

Thestor rossouwi Dickson, 1971

= *Thestor swanepoeli* Pennington, 1971 (synonymized with *T. rossouwi* Dickson, 1971 by Heath & Pringle, 2004: 112). D'Abrera (2009) does not mention this synonymy and treats *swanepoeli* as a valid species. Since no formal taxonomic

change is proposed, the earlier synonymy established in Heath & Pringle (2004) stands.

Thestor pictus van Son, 1941

Thestor montanus pictus van Son, 1941 was raised to full species as *T. pictus* by Heath (1994: 146). Under '*Thestor montanus* van Son, 1941', D'Abrera (2009) treats *pictus* as its subspecies, without referring to the earlier taxonomy, yet proposing no formal change either, hence its specific status as per Heath (1994) stands.

Thestor dicksoni malagas Dickson & Wykeham, 1994

Thestor malagas Dickson & Wykeham, 1994 was synonymized with *T. dicksoni* Riley, 1954 and treated as one of its subspecies by Heath & Pringle (2004: 108) as no diagnostic characters distinguishing between the two taxa could be found. D'Abrera (2009) does not refer to this synonymy and treats *malagas* as a distinct species. Since no formal taxonomic change was proposed, the earlier status proposed in Heath & Pringle (2004) remains valid.

Thestor calviniae Riley, 1954

T. dicksoni calviniae Riley, 1954 was raised to full species as *T. calviniae* by Heath & Pringle (2004: 109). Under '*T. dicksoni* Riley, 1954' D'Abrera (2009) treats *calviniae* as its subspecies, without referring to the earlier taxonomy, but making no formal change either; hence its specific status as per Heath & Pringle (2004) stands.

Thestor brachycerus dukei van Son, 1951

Heath & Pringle (2004: 116) synonymized *T. dukei* with *T. brachycerus* (Trimen, 1883) on morphological grounds. The male genitalia and other structures of the two taxa were found to be indistinguishable. There are small but constant differences in the wing markings that were considered to justify subspecific status, yet D'Abrera (2009) treats them as distinct species, without referring to the former synonymy.

The DNA work done at Harvard on the subfamily Miletinae has inferred that *T. dukei* and *T. brachycerus* might indeed be separate species (Mathew, 2003; Kaliszewska *et al.*, submitted), although the phylogeny on which this is based is poorly supported statistically. D'Abrera (2009) introduces no formal taxonomic change, hence the earlier synonymy as per Heath & Pringle (2004) remains valid

for the time being. Further molecular work is needed to establish the true status of these taxa.

T. yildizae Koçak, 1983 (= *T. obscurus* van Son, 1941) **stat. rev.**

Koçak (1983) considered *Thestor obscurus* van Son, 1941 a junior primary homonym of *Thestor nogelii* var. *obscura* Rühl, 1893 and proposed *Thestor yildizae* Koçak, 1983 as a replacement name.

Oberprieler (1989, 1990) commented in detail on this issue and concluded that Koçak (*loc. cit.*) had acted in compliance with the provisions of the International Code of Zoological Nomenclature, then in force. The attempt by D'Abrera (2009: 692) to reverse Koçak's decision and declare *Thestor yildizae* Koçak, 1983 a *nomen nudum* is invalid.

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A molecular study of the genus *Colotis* Hübner (Pieridae) – implications for South African butterfly names

Torben B. Larsen

Jacobys alle 2, Frederiksberg, DK 1806, Denmark

Background

Some time back I was contacted by Vazrick Nazari who wanted my collaboration on a molecular study of the genus *Colotis* Hübner, [1819], a large genus of the Pieridae which has representatives in India, across Arabia, to all of Africa, and then across to Madagascar. He also proposed to include other genera that have in various ways been associated in terms such as the “*Colotis* group”. I was interested in this project. My first experience with the genus was on the Delhi Ridge in India when I was just seven years old. We called *Colotis etrida* an “orange tip” but even then I remember being sure that they were not really close to the orange tip of my native Denmark (*Anthocharis cardamines* (Linné, 1758)).

The results of the analysis (Nazari *et al.*, 2011), which by its nature is quite technical, demand a number of changes to the taxonomy and nomenclature relevant to the South African members of the group that I list below, with revised names and combinations emphasized in **bold**. I do not like changes in well-known butterfly names more than anyone else so I believed it to be a good idea to summarize them in *Metamorphosis* in a simplified manner. There is obviously much more detailed information and discussion in the main paper. Note that authors' names are bracketed if the taxon was not originally described in its present genus. I shall be happy to send a copy of the paper to anyone really interested in the subject.

The genus *Teracolus*

At the genus level it became necessary to resurrect the genus *Teracolus* Swainson, [1833] for three species presently treated as *Colotis*. They cluster with the genus *Pinacopteryx* Wallengren, 1857 and are well separated from the true *Colotis*, with the genera *Eronia* Hübner, [1823] and the Oriental *Ixias* Hübner, [1819] in between. These three species should henceforth be referred to as ***Teracolus eris*** (Klug, 1829), ***Teracolus subfasciatus*** Swainson, 1833, and ***Teracolus agoye*** (Wallengren, 1857). They are characterized by their pointed forewings and rather

special larvae. To have retained them within the concept of *Colotis* would have meant including also *Pinacopteryx*, *Eronia*, and *Ixias* in the genus, the name of which would then become *Ixias*, having a two-page priority in Hübner's *Verzeichniss bekannter Schmettlinge* [sic].

While living in Botswana I undertook an expedition to study the two subspecies of *Teracolus a. agoye* and *T. a. bowkeri* Trimen, 1883 and concluded in a paper in *Metamorphosis*: “I had set up the transects specifically in order to prove a preconceived notion, namely that two distinct species were involved. I ended up convincing myself (my reader as well, I hope) of the opposite. I have no regrets, however, because that is what the scientific approach is all about” (Larsen, 1992). The genetics show that we are indeed dealing with two clearly differentiated subspecies.

The genera *Eronia* and *Afrodryas*

We did not get a stable position for *Eronia leda* (Boisduval, 1847) in the different analytical processes involved; sometimes it clustered with *Nepheronia* Butler, 1870, sometimes with *Colotis* (had we used more genes, the answer would have been clearer). What it never clustered with was *Eronia cleodora* Hübner, [1823] and its Malagasy subspecies (which we raised to species rank because of wide genetic divergence). We therefore decided to place it in the genus *Afrodryas* Stoneham, 1957 of which it is the type – and only – species. It is best now referred to as *Afrodryas leda* (Boisduval, 1847); it never really seemed to belong with *E. cleodora*.

In the past I used the combination *Madais fausta* for this rather unusual butterfly, which straddles the Somali zone, Arabia, parts of the Middle East, and the Arabian subcontinent. My main reasons were its curious geographic range, the wing shape, and the presence of a prominent forewing androconial patch that differs from all others. This genus is not at all supported by the analysis, but clusters among the other “salmon” species, and I stand corrected by the hard genetic facts. It should revert to *Colotis fausta* (Olivier, 1807).

The genus *Colotis*

All other *Colotis* fell comfortably into a monophyletic group, with a number of clusters that more or less match usual taxonomic practice. The species-complex surrounding *C. aurora* (Cramer, [1780]) proved the most difficult. It was originally referred to as *C. eucharis* (Fabricius, 1775), which is in invalid homonym that was replaced with *C. aurora*. Both have peninsular India as their

type locality. However, the genetic distance between the Indian and Arabian/African populations of *C. aurora* is too large to consider them members of the same species. The oldest name for the African species is *Colotis evarne* (Klug, 1829). This is the northern representative of the species, not extending much below the Equator, but found all the way from Arabia to Dakar. Despite much local and seasonal variation, there is little genetic variation within this vast area; numerous other names are available, all of doubtful subspecific value. Following a prompt by Hecq (1975; 1994), I treated the rather similar *C. dissociatus* as a distinct dry zone species in contrast to the larger, bright yellow *C. auxo* from the coastal areas of the RSA. Our genetic review showed this to be too closely related to *C. auxo* to be considered a distinct species, but that the distance was sufficient to justify separation at the subspecies level. These two should henceforth be referred to as *C. auxo auxo* (Lucas, 1852) and *C. auxo dissociatus* (Butler, 1879). We were surprised to find that the large very bright yellow species that was known as *C. auxo* from eastern Kenya and northeastern Tanzania actually belonged to a third valid species, *C. incretus* (Butler, 1881).

The *Colotis danae* complex also provided some real surprises. I had actually expected that *C. danae* (Fabricius, 1775) was a species distinct from the African, but the genetic distance was too small for that. A large number of specimens sequenced from between Arabia and Senegal were very similar despite much local and seasonal variation. The oldest name of an African subspecies is *C. danae eupompe* (Klug, 1829) but there may be space for some additional subspecies, though on purely morphological grounds rather than genetic. But the South African *C. danae annae* did qualify for species rank. Not only that, but it was genetically almost identical with that of *C. hildebrandti* further north. This species with its beautiful golden tip at first sight differs strongly from *annae*, but the difference is just “scale-deep”. This is a structural colour that is probably produced by a slight difference in the structure of the red scales of *annae*. There are really no other morphological differences. We therefore unite the two as *C. annae annae* (Wallengren, 1857) and *C. annae hildebrandti* (Staudinger, 1884). In Kenya the latter co-exists with *C. danae eupompe*.

Finally, there is the question of *Colotis amata/calais*. In Larsen (2005) I used the combination *C. amata amata* (Fabricius, 1775) for the West African populations, which look very like the Indian, but accepted *C. amata calais* (Cramer, [1775]) for the South African populations. These are actually genetically very divergent from their Indian counterparts ($6.6 \pm 0.4\%$) and morphologically distinct. We did not examine the *amata* populations from Arabia to Senegal, which may prove to have some intermediate status. However, on the basis of the data available to us we reinstate all non-Indian *amata* to *C. calais* and recognize the subspecies *C. calais williamsi* (Namibia and Angola) and *C. calais crowleyi* (Madagascar). This really

deserves further study; it is not completely impossible that the populations north of the Equator actually do pertain to the Indian *C. amata*.

Changes in species not relevant to South Africa

Among species not found in South Africa our only real surprises were in the *Colotis phisadia*-group. Its two white taxa have long been treated as *C. v. vestalis* in Asia with *C. vestalis castalis* (Staudinger, 1885) in the Horn of Africa and the Kenya coast. Larsen (1983) thought their absence from the Arabian Peninsula was distinctly odd. It turned out that *C. castalis* is quite distinct from *C. vestalis* and that the latter is the Indian subspecies of *C. phisadia*, which has salmon forewings. The correct terminology is thus ***C. castalis*** (Staudinger, 1885) and ***C. phisadia vestalis*** (Butler, 1876), respectively. The joker in this pack had been the salmon-coloured *C. phisadia protractus* in the very dry zones of northwestern India and southern Pakistan, which turned out to be genetically very different and should be upgraded to species level as ***C. protractus*** (Butler, 1876). It is one of those cases where you say to yourself: “Oh, yes ... why on earth did I never think of that!”

The Malagasy situation was also interesting. At the genus level *Gideona lucasi* (Grandidier, 1867) was found to be a sister taxon to *Pinacopteryx* + *Teracolus* as already suggested by Braby *et al.* (2006). ***Nepheronia pauliani*** Bernardi, 1858 and ***Pinacopteryx mabiliei*** (Aurivillius, 1898) were raised to species level from subspecies of *N. buquetii* (Boisduval, 1836) and *P. eriphia*, respectively. *Colotis manahari* (Ward, 1870) fitted well into the framework, but is obviously quite special. These species must all have had a long history of isolation on Madagascar, while species such as *Colotis evanthe* (Boisduval, 1836) and *C. calais crowleyi* (Sharpe, 1898) seem to be more recently established on the island.

It is also worth mentioning that *Colotis elgonensis* (Sharpe, 1891) is found in a number of subspecies in the high mountains of eastern and central Africa. The genetic distance between these populations is very small, though it is usually possible to tell the origin of an unlabelled specimen by small morphological features so that they can continue to be considered subspecies. Only ssp. *glauningi* (Schultze, 1909) from the Cameroun Mountains – very far from any other population – differs more substantially, but not sufficiently for it to be accorded species rank.

Finally the strange – but gorgeous – little *Calopieris eulimene* (Klug, 1829) from the Horn of Africa and the extreme west of Arabia seems to be unrelated to any definition of the *Colotis*-group of genera; I never thought it belonged in Colotini since encountering it during the two hottest butterflying days of my life – in a wadi west of Omdurman in the Sudan and another south of Hodeida on the Yemen coast.

Concluding remarks

Molecular phylogeny is a powerful tool for puzzling out the relationships within a difficult genus, but it is not a perfect tool. As you will see if you read the paper there are potential outstanding issues ... loose ends. They may be partly due to insufficient sampling. Two species that I know as clearly distinct in Kenya, *Colotis vesta* (Reiche, 1850) and *C. aurigineus* (Butler, 1883) coalesce south of Kenya, which I simply do not understand. There are some indications that each of the species *C. pallene* (Hopffer, 1855), *C. euipe* (Linné, 1758), and *C. chrysonome* (Klug, 1832) might include a hidden species. We did not have sufficient material to reach a final conclusion.

However, we did establish that *Colotis evanthe* (Boisduval, 1836) from Madagascar and *C. evanthides* (Holland, 1896) from the Aldabra Atoll did not constitute the Lemurian link postulated by Cogan & Hutson (1971), I believe based on a hypothesis first proposed by Bernardi. The Indian component, *C. etrida* (Boisduval, 1836), in that proposed link clusters with *C. ephyia* (Klug, 1829) from Yemen and the Somali zone, while *C. evanthe* stands squarely grouped with *C. antevippe* and other widely distributed African species.

Finally, I must congratulate the lead author, Vazrick Nazari, for steering his motley team of collaborators through many pitfalls and rapids along the way to completion of a paper that we all believe better defines the relationships within the genus *Colotis*.

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Note on *Mimacraea gelinia* (Oberthür, 1893), with the description of a new subspecies (Lepidoptera: Lycaenidae)

Michel Libert* and Steve C. Collins**

*8 rue Henry Barbet, 76 000 – Rouen, France; michelibert@free.fr

** ABRI, PO Box 14308, Nairobi, Kenya; scollins@iconnect.co.ke

Summary

A new subspecies of *M. gelinia* from southern Tanzania, *M. gelinia georgia* ssp. nov., and the female of *M. gelinia nguru* Kielland are described.

Mimacraea gelinia was described after a male from the Usambaras, in NE Tanzania. Almost one century later, Kielland (1986) described the subspecies *nguru*, based on a single male collected in the eponymous mountains. In the revision of the genus *Mimacraea* (Libert, 2000: 51), four males of *M. gelinia* collected in the Uzungwa mountains (southern Tanzania) were tentatively attributed to this subspecies, but the author underlined several differences between these males and the holotype of *M. g. nguru*.

Since the revision was published, more specimens of *M. gelinia* have been collected by ABRI¹ collectors in both the Nguru and Uzungwa mountains. These new specimens allow better characterization of subspecies *nguru*, especially thanks to the discovery of the female, and show that the Uzungwa males represent a distinct subspecies, which is described here as *M. gelinia georgia*² ssp. nov.; the female of this subspecies is still unknown.

All three subspecies are clearly orophilic; many examples of such allopatric subspeciation are known in Tanzania (see fig. 1. page 21, map showing the distribution of the three subspecies).

Depositories of material

ABRI – African Butterfly Research Institute, Nairobi, Kenya

BMNH – The Natural History Museum, London, U.K.

¹– African Butterfly Research Institute, in Nairobi.

²– The name *georgia* can be found in a checklist of the Butterflies of the Udzungwa National Park, together with an illustration of the butterfly, but the authors (C. Congdon and I. Bampton) did not designate an holotype and did not really describe the subspecies; the name *georgia* must therefore be considered as a manuscript one. Besides, this list can be found on the internet, but has not been published elsewhere.

Mimacraea gelinia nguru Kielland, 1986 (plate 1, figs 3 & 4)

Mimacraea gelinia nguru Kielland, 1986. – New and little known butterflies from Tanzania (Lepidoptera: Lycaenidae and Riodinidae). *Lambillonea* **86**(11-12): 148 (male, illustrated; also in Kielland, 1990); neallotype designated below.

Holotype: male, Maskati, Nguru mts, 1700 m, Tanzania, 21-III-1984 (I. Kielland) (BMNH) [examined].

Neallotype: female, Nguru mts, Tanzania, iii.2003 (ABRI coll.), Nairobi.

Material examined

Only the male holotype was known when the revision by Libert (2000) was published; there are now 18 males and 10 females in ABRI, all collected in Tanzania, in two close localities of the Nguru mountains (Maskati and Mkombola).

Description

Forewing length: males, 26 to 33 mm ($n = 18$), females, 27 to 30 mm ($n = 10$). The recently collected males slightly differ from the holotype: on the forewings, the orange discal area is more extended (but remains smaller than in *M. g. gelinia*); as a consequence, the white spot in space 2 is closer to this orange area. While this spot is well separated from the orange area in the holotype and a few other males, a narrow orange «bridge» forms a junction between the orange area and the white spot in most of the other male specimens. Most males also have a minute but distinct black notch at the end of the cell (this character is better seen on the underside), but it is always much smaller than in the two other subspecies.

The pattern of the females is very similar, except that the discal areas are yellow instead of orange, an important difference to the nominate subspecies, the females of which are almost identical to males. In addition, the yellow area is smaller than in males, which results in 1) its complete separation from the white spots in spaces 2 and 3 of forewing, and 2) a wider black margin on the hind wings. The black spots on the underside of the hind wings can be seen through the wings.

Mimacraea gelinia georgia ssp. nov. (plate 1, fig. 2)

= *Mimacraea gelinia nguru* Kielland, 1986, s. Libert, 2000, *partim* (pl. VI).

The new subspecies is named in honour of Mr Harrison Ford for his great support to wildlife conservation in the Eastern Arc Mountains of Tanzania; the butterfly is given the name of his daughter.

Twelve males have been found at the type-locality, above Sanje; the female of this subspecies is still unknown.

Holotype: male, Uzungwa mountains, 1130 m, Tanzania, 12.iii.2000 (ABRI coll.); ABRI, Nairobi.

Description (males)

Forewing length: 31 to 35 mm ($n = 12$).

As in subspecies *nguru*, the white on recto is brighter, and the orange is more red than in the nominate subspecies, which results in a greater contrast.

The apical white band on the forewings is similar to that in *M. g. nguru*, more developed than in *M. g. gelinia* (the spots being almost twice as large). The white spots in spaces 2 and 3 are also larger than in *M. g. gelinia*, but smaller than in *M. g. nguru* (about 1,5 and 2 mm instead of 3 and 4 mm, respectively); the white is not as pure as in the apical spots: it is slightly yellowish, but less so than in *M. g. gelinia*.

The orange discal area of the forewings is larger than in *M. g. nguru*, more or less as in *M. g. gelinia*. In space 1, the black margin is wider than in the two other subspecies. In spaces 2 and 3, the extension of the orange discal area is variable, and so is, consequently, the distance between the orange and the white spots: next to the orange area in three males, slightly separated from it in the others (but connected as in *M. g. nguru* in two specimens); there is even some orange in the white spots of two or three males. At the base of space 4, the orange is as extended as in *M. g. gelinia*, about twice as much as in *M. g. nguru*; sometimes, there is even some orange in space 5, just beyond the cell; at the end of the cell, the black notch is also as in *M. g. gelinia*, and deeper black than in *M. g. nguru*.

On the underside of the hind wings, the red intraneural markings are similar to *M. g. nguru*, and much wider than in *M. g. gelinia*.

Discussion

It can be seen that the new subspecies is phenotypically somewhat intermediate between subspecies *gelinia* and *nguru*, whereas, geographically speaking, subspecies *nguru* is found between *g. gelinia* and *g. georgia*. There is no doubt that three different taxa are involved; their geographical distribution is also shown in the map. It is sad to report that the site in the Nguru Mtns (Mkombola) no longer exists, as the forest has been felled for farmland. Despite several further visits over recent years no further specimens have been observed.

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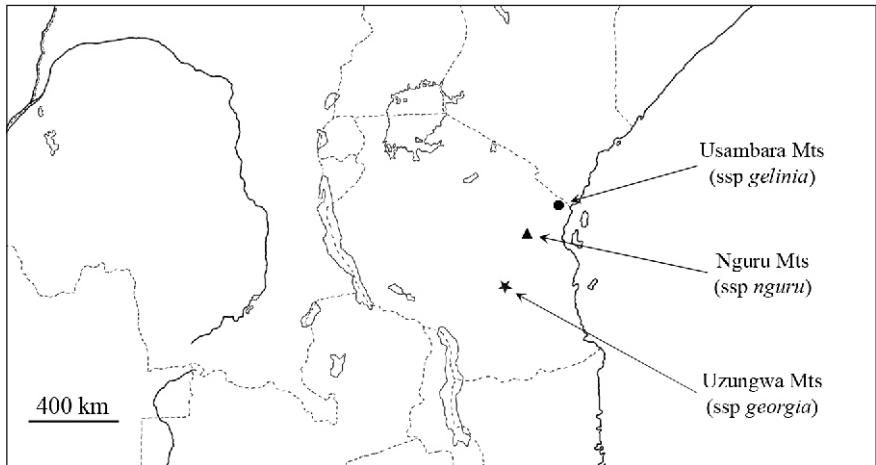


Fig. 1 . Map showing the distribution of the three subspecies

Confirmation of the presence of *Coeliades lorenzo* in the Mpumalanga Lowveld

I. C. Sharp

P.O. Box 146, Hoedspruit, 1380

E-mail: galago23@gmail.com

The Red-tab Policeman (*Coeliades keithloa*) has previously been considered as being represented by two separate subspecies in southern Africa, *C. k. keithloa* and *C. k. lorenzo* (Pringle *et al.*, 1994: 309). Woodhall (2005), however, treated *C. lorenzo* as a distinct species based on differences in male genitalia and specimens bred from the distinctively different larvae.

The distribution of the two 'sub-species' has been unclear according to Pringle *et al.* (*loc. cit.*), as was the taxonomic identity of the populations found in the former Transvaal province. Other than visual sightings in the Kruger National Park (KNP), the Wolkberg and Blyderivierspoort Nature Reserve, no other evidence was available to separate or confirm the 'sub-species' occurring in what is now the Mpumalanga and Limpopo provinces' lowveld region.

Woodhall (2005) describes the distribution of *C. lorenzo* as from northern KwaZulu-Natal north into Mozambique. The distribution of *C. k. keithloa* is more widespread, ranging from as far south as the Eastern Cape Province (Port Elizabeth), through the coastal areas of KwaZulu-Natal Province, into the lowveld of the Mpumalanga Province and up to the Soutpansberg of the Limpopo Province.

The author has since noted and/or recorded some sightings of what is thought to have been *C. keithloa* in the Mpumalanga Province, the first of which was at Orpen Gate of the KNP where an individual was seen at dusk but took flight before it could be photographed. The second sighting occurred in the forests of the Mariepskop Mountain where a single individual was seen and photographed (SABCA VM No. 16264). These sightings concur with those described for *C. keithloa* in Pringle *et al.* (*loc. cit.*), i.e. Satara in the KNP and the Blyderivierspoort Nature Reserve.

A discovery was however made that indicates that *C. lorenzo* occurs in the Mpumalanga lowveld. The following account of events leading to the discovery will provide a more complete picture.

During a visit to a lodge in the Timbavati Private Nature Reserve on 04 February

2010, together with Vaughan Jessnitz, photographs of butterflies around the lodge were being taken for the SABCA project. At one point I was called by Vaughan to look at a 'policeman' butterfly that was ovipositing on a shrub. A number of photographs were taken for submission to the SABCA Virtual Museum. The butterfly was identified as *Coeliades anchises anchises* and this was confirmed (VMNo. 11454) by the experts of the VM.

On that day no consideration was given to collecting of the eggs for breeding purposes. This turned out to be a source of regret especially as at a later stage a variety of butterfly eggs and larvae were collected to obtain photographs of the different life cycle stages.

The larval food plant was identified as *Triaspis glaucophylla* from the photographs of *C. a. anchises* and from later photographs of the flowers (plate no. 3 fig. 2).

On subsequent visits to the lodge attempts were made to once again locate the *T. glaucophylla* bush but it had been destroyed by foraging animals. By pure chance a larger plant was found scrabbling in a raisin bush only 10 m from the site of the original specimen. It was only on the third visit to this bush that two 'policeman' larvae were spotted.

It was an overcast day and this had been the trump card! As soon as the cloud cover broke and the sun came shining through, the larva rushed off into his hideaway of leaves expertly woven together.

Its escape was too late and the specimen was collected to document the life cycle and obviously the expected adult *C. a. anchises*. The upkeep of the larva was difficult in that the food plant was not readily available in the area where I lived. A local botanist was consulted and showed us some locations where sufficient food could be found to ensure the completion of the larval phase. A beautiful white chrysalis formed (plate no. 2 fig. 2) and the expectation was growing.

What a surprise when from out of the chrysalis pops a 'red-tab' policeman (plate no. 3 fig. 1)!

The books were quickly drawn closer and consulted to check if *T. glaucophylla* was listed as a larval food plant for *C. k. keithloa*. It was not, but something else was also strange as pointed out by my wife (see plate no. 4 fig. 2 for a comparison of *C. keithloa* & *C. lorenzo* larvae): the colour of the larva collected (plate no. 2 fig. 1) did not match that of the photograph depicted in *What's that butterfly* (Woodhall, 2008). A photograph of a larva taken in the Krantzklouf Nature

Reserve in February 2008 and submitted to SABCA (VM No. 1622) was different, too. This was also identified as *C. k. keithloa*.

To quell the curiosity the series of photographs was forwarded to Andre Coetzer for comment. His response indicated that, other than genitalia dissection, the only way to positively separate the two taxa was to find the larva. The photograph of the larva was evidence enough to positively identify the butterfly as *C. lorenzo*, resulting in an extension of its known range and *T. glaucophylla* being identified as a new larval food plant for the species.

It appears therefore that according to the events above, *Coeliades lorenzo* is alive and well, living in the Mpumalanga Lowveld utilizing the same larval food plant as *Coeliades anchises anchises*!

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Climatic Vicariance and the Kalahari Basin- drivers of recent speciation events?

Moths `n things #5

Hermann Staude

hermann@busmark.co.za

It has been some time since the last 'Moths 'n things' appeared in *Metamorphosis*, so it is probably appropriate that I offer a short précis on what the series is all about. Philosophising about things that concern us lepidopterists is an activity that is normally reserved for late night conversation around camp fires rounding off perfect field days. These discussions are usually very interesting but seldom culminate in anything as tangible as the written word. This is a pity as such thought processes could lead to focussed scientific research, possibly answering those questions tumbling around our inquisitive minds. The Moths 'n things articles are not formal, structured 'scientific articles' but are intended rather, as hypotheses that may perhaps trigger good science. The series is not meant to be a one-man show but, apart from Colin Congdon's *Insects rule, OK!*, contributions from members have not been forthcoming - so one soldier on.

The Kalahari or the Kalahari Basin or the Kalahari Desert, call it what you will, extends westwards from its primary location in south central Africa, spanning the north western parts of South Africa, most of Botswana and stretching north into Angola. Some even say that the Kalahari sands can be traced right up to the Congolese forests. To the south it meets up with the dry Nama- and Succulent Karoo Biomes. Experts tell us that the system probably has its origins in the forming of Gondwanaland many million years ago. The amalgamation of continents apparently caused this huge inland depression and it is said that most of the southern part used to form a great lake during wet climatic conditions. More importantly it is said to have been, quite recently, a huge sand desert that effectively divided the west from the east. Large parts of the North West Province and the south western Free State of South Africa have a thin layer of sand covering fertile soil, said to have blown in from this sand desert.

Much has been written about the basin in popular literature, but references to any science actually supporting this literature are hard to find. What is important for the purpose of this article is that the Kalahari Basin appears to have been the catalyst for the recent formation of ecological refugia to its west, the formation of which seems to have started a speciation process by isolating populations of various life forms.

Classic vicariance is a process of speciation whereby populations of the same species are separated by the development of geographic barriers, such as the formation of mountains or the drifting apart of continents or the formation of rivers too wide to cross, etc. This is said to prevent gene flow between the isolated populations and over time separate species are formed. One major criticism of this hypothesis is that such barriers are usually extremely slow in forming and it is difficult to imagine that this could be the major cause of speciation culminating in the amazing biodiversity we see today. I happen to agree that such isolating mechanisms seem to be too slow and too infrequent to be a major factor in speciation.

There is little doubt in my mind, however, that **climatic vicariance** in the sense of geographic isolation caused by **changing climate, rather than changing topography**, may be a major driving force of speciation in terrestrial species. I am amazed that nowhere in the literature do I see the importance of climate change as a driving force in vicariance, being mentioned.

Repeated changes in climate are shrinking ecosystems into refugia where organisms already adapted to these conditions may persist. This has been repeating itself numerous times all over the world as prevailing climates fluctuate over the millennia.

Ecosystems, and the range of species they harbour, shrink into isolated pockets (refugia) where favourable conditions remain embedded in the otherwise hostile, more general climatic environment. Conversely, these pockets amalgamate into bigger units as they grow when the prevailing climate again becomes more favourable to them.

Such climatic refugia persist because an undulating topography of mountains, valleys, plateaus and basins, from the tropics to the poles, allows for virtually every possible past climatic condition that has ever existed on this earth to be still present somewhere, each set of climatic conditions harbouring organisms uniquely adapted to it.

The phenomenon of pockets of surviving ecosystems adapted to different climatic conditions is often underestimated. It is, in my opinion, very important to the better understanding of the diversity of life surrounding us. We need to be constantly aware of these ecosystems, each one adapted to its own set of unique climatic conditions.

One so often hears of the notion that species gradually adapt to changing climate as it happens. Those species that manage to do so survive and those that don't, do

not. This is then applied to the argument that climate change induced by humans (such as global warming) is too rapid for species to adapt and that, by extension, global mass extinction of species is imminent. Natural selection is simply too slow to react that quickly. This only applies if species already adapted to the new circumstances do not already exist currently in refugia or if currently dominant climatic conditions were to disappear completely. The latter may be true at higher latitudes where many ecosystems have been wiped out by historic climatic fluctuations, such as the series of recent ice ages, so extreme that most climatic refugia were simply wiped out. But in Africa we do have this surviving diversity of ecosystems. I think that as the climate warms, ecosystems already adapted to the new circumstances will expand their ranges and those ecosystems dominant today will shrink into refugia where today's prevailing climate will remain. If we want to be prepared for future climate change then I think we should identify and ensure protection of as many of these ecological refugia as possible.

Classic vicariance processes such as continental drift separate large healthy populations which may each number, perhaps, millions of individuals. Genetic drift ensures that mutations are mostly eliminated in large populations and the populations remain genetically relatively stable. This means that isolated large populations of the same species probably remain genetically unchanged for a very long time, preventing speciation.

This is, however, not so in the case of climatic vicariance because ecosystems are broken up into small refugia and population numbers of individual species drop drastically. Ecosystems are not homogeneous entities but rather assemblages of interacting and often interdependent species with similar climatic tolerance levels.

So, if climatic conditions become increasingly adverse, these pockets become smaller and increasingly poor in species and numbers of individuals.

It is during these conditions that rapid extinction may occur....or rapid speciation. The chances that a species becomes locally extinct grow increasingly greater as the number of individuals in an isolated population drops. The chances that a beneficial mutation occurring in one individual manifests itself in the entire population also becomes increasingly greater as the number of individuals drops. This allows for rapid adaptation and eventual speciation to take place. There is something poetic about this: **The same mechanism (low numbers) that drives extinction also drives the emergence of new species!**

Once the size of a population reaches a critically small number three things can happen to it. It can either become extinct (the most probable scenario); or it can

adapt to the changing circumstance, as described above, and again grow its numbers possibly becoming a new species in the process; or circumstances again turn favourable as before and numbers increase again with members of the population again interbreeding with other populations. Mark Williams has been advocating the importance of dispersal as a speciation mechanism for years. In this case you have the ultimate low population number, in the case of all sexually reproducing taxa one individual gravid female. It follows that the vast majority of such dispersal events will be unsuccessful but in the very few cases where the dispersal is successful, new species will likely emerge. Low populations allowing mutations to rapidly express themselves are still the driving mechanism of speciation – as much here as in the process associated with climatic vicariance.

But what has all of this got to do with the Kalahari?

The Kalahari Basin has been changing its dress every time the climate has changed, from a lush inland sea to an extreme sandy desert. This has forced a multitude of species in and out of refugia. These conditions seem not to have lasted long enough for the speciation process to have been completed in all cases, but rapid adaptations seem to have taken place. What we see now are lots of 'almost species' in a basin that is at present in a transition phase between lake and desert.

Examples in butterflies are the many *Colotis* species where close relatives have either been described as subspecies or distinct species. In Geometridae we have *Lhommeia biskraria* and *L. subapicata*; *Chiasmia brongusaria* subspecies; *Chiasmia multistrigata* subspecies; *Conchylia sesquifascia* and *Conchylia canescens*, etc. Among trees we have *Acacia erioloba* and *A. haematoxylon* and all their hybrids.....and there are numerous other examples.

The point I'm making - in the spirit of moths 'n things – is this: here we are sitting on a great natural experimental laboratory; here we can see populations where isolation mechanisms seem to have disappeared and perhaps reappeared prior to the completion of the speciation process; here we perhaps have a chance to support or refute some of the philosophical hypotheses, such as the one above, with real empirical science. Torben Larsen did some work on *Colotis agoye* and asked some questions about *Papilio demodocus* but was never in Botswana long enough to do it justice. Given the new tools at our disposal, such as genetic sequencing, would someone please do some work in the KALAHARI!

Footnote

In this article, I do not elaborate on nor explain, for the sake of brevity, concepts

such as 'genetic drift', 'rapid adaptation' the effect of mutations on smaller populations nor how a change in one trigger protein can turn on genetic switches which can effect major morphological changes in an organism. These can be found in the literature dealing with genetics and embryology and are generally accepted.

Thanks to all those fellow lepidopterists with whom I have been fortunate to engage in conversation on these matters and who have influenced my thinking in many ways. I refrain from thanking anyone individually as I may be embarrassed by leaving out someone or perhaps someone may be embarrassed to be associated with my musings above, this is entirely my own fault. Many thanks, though, to John Joannou who critically evaluated the text, sorted out some of my dodgy syntax and generally helped to make this article more readable.

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Fig. 1. *Mimacraea gelinia* ♂ (Ambangulu, Tanzania), R & V



● Fig. 2. *M. g. georgia*, HT ♂, R & V



Fig. 3. *M. g. nguru* ♂ (Maskati, Nguru Mts, Tanzania), R & V



● Fig. 4. *M. g. nguru*, neAT ♀, R & V



Fig. 1. *Coeliades lorenzo* larva



Fig. 2. *Coeliades lorenzo* pupa



Fig. 1. *Coeliades lorenzo*, adult



Fig. 2. *Triaspis glaucophylla*, larval foodplant of *Coeliades Lorenzo*



Fig. 1. *Coeliades keithloa* ♀, ex Hawaan em. 1 August 2008: S. Woodhall



Fig. 2. *Coeliades keithloa* (left) and *C. lorenzo* (right): A. Coetzer