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## NOMENCLATURE AND HOMOLOGY OF GENITALIA IN INSECTS

« There is nothing in taxonomic biology so hard to eradicate as a dead idea embalmed in traditional nomenclature ». When SNODGRASS wrote this (1957 p. 1) he meant it as an introduction to « an attempt to show that the parts of the male genital apparatus in all the principal orders of insects can be uniformly interpreted and named as here proposed, if the known facts of their development are given priority over theoretical generalizations ».

When the present author in 1956 edited and compiled the first edition of « Taxonomist's Glossary of Genitalia in Insects » he wrote in the preface: « It has not been my intention to lay down rules as to which terms should be used, and by no means should the book be used for solving nomenclature problems!... Nor, in fact is it necessary to use the same term for the same structure in all groups; it is not in the least intended that the present work should force a uniform terminology on taxonomists ». Now in preparing a second edition of this book to appear in 1970 I still stick to those words and in the following I shall try to give arguments for this viewpoint. Actually almost every scientist in advancing a theory of the origin of the genitalia in insects has expressed a wish to 'force' the 'theoretically true' nomenclature on taxonomists. So I shall begin with a survey of such theories.

## TWENTYFIVE YEARS' MORPHOLOGICAL THEORIES

After the milestone in insect morphology marked by SNODGRASS in his 'Principles' in 1935 and the further elaboration of the origins of the male external genitalia in his papers of 1936, 1937 and 1941, the next new general thoughts were brought forward by MICHENER in 1944 (1).

MICHENER begins with a short survey of the female ovipositor which he, as Snodgrass, regards as derived from gonopods. The valvifers are the 'gonocoxites', which word he substitutes for coxopodites « since each is only a part of the coxopodite, the other part being the gonapophysis » (p. 336). The gonapophyses are regarded as endites (same page) and so can hardly be called part of the coxopodites. It is true that gonocoxopodites is a long and inconvenient term, but coxite is misleading (SNODGRASS 1957 p. 5); anyway it has gained ground and should not be replaced by the longer term. The third valvulae he derives from the styli of the ninth gonopod (which SNODGRASS does not, see 1935 p. 612), and so he terms them gonostyli « in orders other than the orthopteroid groups » (p. 337).

(1) Though QADRI in 1940 and other authors also formed general ideas based on their developmental studies on selected groups.



In the male MICHENER supposes a close homology with the female and so compares the base of the male 'claspers' with the second valvifers and calls them gonocoxites, the apex (rarely many-segmented) with the third valvulae and calls them gonostyli, and some structures called 'penis valves' with the second valvulae, though with doubt. As with almost all morphologists he begins by describing the male gonapophyses in *Machilis* and homologizes - with doubt - those of the IX. segment with the penis valves and again with the parameres of Coleoptera which thus would be of appendicular origin. Lateral of the penis valves, structures are found in most Pterygota, parts of the coxopodites, which are the claspettes of Diptera, the volsellae of Hymenoptera. Though beginning with *Machilis* his viewpoint is that of a hymenopterist as can be seen from the universal use of the word volsella for structures variously named by specialists in different groups. Since such other names are rarely used by MICHENER a clear understanding of the homologization is difficult to obtain and the rough sketches do not offer much help. In its essentials, however, his theory is that penis (or phallus) alone is of non-appendicular origin, all other structures being derivable from appendages of the ninth abdominal segment. In proving the homologies of these structures throughout the insectan class he attempts «to use, for each structure, a name at least not misleading as to homology and neither used for so many different structures as to be meaningless (as is paramere), nor indicative of a shape not at all constant for the structure involved (as is squama)».

In 1956 (1958), however, MICHENER realized his error. «Today it must be admitted that the interordinal homologies among males are not certainly established and that the male-female homologies are also uncertain in Pterygota». «It is obvious under these circumstances that parts of the 1944 terminology were... premature and that a relatively noncommittal terminology would be preferable». And as conclusion: «It is recommended that morphologically non-committal terms of broad application be established in preference to terms of even broader significance based on questionable homologies» (p. 585).

In 1950 DUPUIS set forward a theory on the male external genitalia taking the ontogenetical development of the structures into consideration. He mentions SNODGRASS' distinction (1935) between phallic organs «which are immediately concerned with the function of coition» and periphallic organs «that have for the most part a grasping or clasping role» (SNODGRASS 1935 p. 586). He discards this distinction based on «le critère du fonctionnement», but uses similar words in a similar, but morphological sense: Euphallic organs are the structures placed ventrally between segment IX and X in a genital chamber (poche génitale) and connected through the ductus ejaculatorius to the mesodermal genital apparatus. Pseudophallic organs have not this connection. And his conclusion is that the pseudophallic organs are the appendages of the IX. (or the IX. and VIII.) segment, the euphallic those of the X. segment, which is proved by the 'morphogénèse' of the structures (p. 33).

DUPUIS does not propose any change of terms on the basis of this theory. In 1952, however, he gives a short survey of the theory and concludes, that the parameres should never be called gonapophyses, and that the many terms for them (harpagones, claspers etc. etc.) should be abandoned in favour of paramere. In 1957 he even gives a sort of code for the nomenclature to which I shall revert.



In a paper from 1950 with the all embracing title: « Origin and evolution of genitalia of insects » GUSTAFSON of the Ferris school gives his reasons for the statement that « the genital appendages of the insects are not modified pedal appendages nor fragments nor secondary outgrowths from the basal segment (coxite) of the leg » (p. 39). Because of this result he proposes that « the term gonopod be dropped from further usage » (p. 39), and « since the parameres of the male and the gonapophyses of the female are considered to be homologous structures ... these structures, of both sexes, be designated with the same term - gonapophyses » (p. 39). Also the terms gonocoxites and stylus should be preferred to « the large series of terms » variously applied to these structures. Consequently, since the male and female gonapophyses « may be assumed to be completely homologous ... a theory regarding the origin of one will serve equally well for the other and these homologous structures in both sexes may and should be designated by the same term » (p. 40).

The reasons for this conclusion - given with an almost Ferrisian verbosity - are in short that the gonapophyses in machiloids are supposed to be serially homologous with the eversible sacs of the preceding segments in some genera of machiloids, and that these eversible sacs are sternal structures of the same origin as the ampullae at the end of the vasa deferentia, later evolving into the gonapophyses. Since in some genera of machiloids even two pairs of eversible sacs are present on abd. II-V the innermost one is supposed to be « employed in the formation of the penis, the outer to form the gonapophyses » (p. 51). The origin of musculature of the eversible sacs « is usually on the sternum or the laterosternites and is only on the coxites when the coxites have fused with the sternum » (p. 40). This is the only reason for regarding the eversible sacs as of « sternal, not pedal » origin, and it is very likely wrong. Already J. TH. OUDEMANS (1887) clearly depicts the musculature of the eversible sacs as originating in the coxopodites, SNODGRASS (1935) writes it clearly in the text, and GUSTAFSON gives no figures showing the opposite. He also variously states the gonapophyses to articulate now to the gonocoxites (p. 47 left, and 54), now to the sternite (p. 47 right), so that no conclusion can be drawn from this. It must be noted also that the muscles to the eversible sac and the stylus cross, whereas those of the gonapophyses and the stylus are parallel (GUSTAFSON fig. 23). It may be, nevertheless, that the eversible sacs and the gonapophyses are serially homologous, but in that case both must belong to abdominal appendages, as endites on the coxopodites. See, however, below the viewpoints of MATSUDA 1958. More thorough embryological and morphological investigations are needed to solve this question - and no change of terms should result.

In 1956 I published the « Taxonomist's Glossary » the aim of which was to help taxonomists through the jungle of names, but not to give any morphological theories nor attempts at standardizing the nomenclature.

1957 saw quite an explosion of ideas on the morphology and evolution of the external genitalia, in part arriving at entirely contradictory results.

In May 1957 ANKER NIELSEN gave his viewpoints « on the evolution of the genitalia in male insects ». Being a trichopterist he worked from the Trichoptera, as MICHENER did from the Hymenoptera and GUSTAFSON (and originally SNODGRASS) from the Thysanura, and he concludes that « the musculature gives evidence that the gonopods really are the limbs of segment IX, and that the phallus is composed of parts of these limbs, or that it contains parts of the limbs » (p. 30). This view he broadens to all male Endopterygota, comparing



e.g. the coleopterous parameres with the trichopterous gonopods, not with the trichopterous parameres (titillators). In dealing with Machilidae he argues that « it seems justified to regard the thysanuran phallus as arisen by fusion of the limbs of segment X » (p. 39) and so the final conclusion (p. 54) is that

« The phallus in the Thysanura is formed by the limbs of segment X. In the Pterygota the phallic structures are formed by the gonapophyses of segment IX and the limbs of segment X. ...In the class Insecta a copulatory organ, a phallus, has evolved independently several times. The exopterygotan and the endopterygotan aedeagus thus are not strictly homologous ».

« The term parameres ought to be reserved for structures which may be considered as homologous with (a part of) the gonapophyses in the Thysanura ».

The last phrase and a warning against 'endophallus', by the way, are the only nomenclatural considerations in the paper.

In August 1957 SCUDDER in « Nature » published a preliminary note on the insectan ovipositor. He starts from the small triangular piece in Lepismatidae which SNODGRASS (1935 p. 610) thought was connected to the second pair of gonapophyses, but which is actually part of the first gonapophysis and articulates with the ninth tergum and the second gonocoxa. This triangular piece he terms gonangulum, finding it in all pterygotan ovipositors. He also points out that the 'third valvulae' of SNODGRASS are not the styli as MICHENER thought (but what SNODGRASS knew they were not, 1935 p. 612), and so he substitutes MICHENER's gonostylus with the term gonoplac.

In December 1957 SNODGRASS published his important paper on the male external genitalia, a revised interpretation he calls it and so it was. He bases his theory on the ontogenetical development. The male external organs are in all insect orders « derived from a pair of small ectodermal outgrowths, which appear on the nymph or larva, and may be termed the primary phallic lobes ». In Thysanura these lobes unite to form the penis, in Ephemeroptera they form the two penes, in all other orders they divide each into two secondary lobes, the mesal mesomeres and the lateral parameres. The mesomeres in orders other than Orthoptera unite to form the aedeagus, the parameres develop into the various claspers. These claspers are of the same non-appendicular origin in all orders except Ephemeroptera, where they originate from the ninth segment legs. The term paramere, coined originally by VERHOEFF for « the lateral genital lobes » of Coleoptera should be retained so as not to deny « a true claimant his right because of impostors ». The parameres in higher orders may be secondarily divided into a proximal basimere and a distal telomere (= harpago).

Ontogenetically all these claspers develop from the primary phallic lobes and not as limbs of the abdominal segments, as the coxal plates and styli do in Thysanura, Ephemeroptera and Grylloblattodea. When such limb buds appear during embryology in the other orders they later disappear and there is no proof that the primary phallic lobes are a re-appearance of these limb buds. The male gonapophyses of Thysanura (Machiloidea) are endites of the ninth (and eighth) appendages, but have no connection, in development or in morphology, with the penis derived from the primary phallic lobes.

In many other arthropods the penes are on the limb bases, in some crustaceans and diplopods on the sternum between the legs, and it is « a logical deduction, therefore, that the paired phallic rudiments of modern insects represent a pair of primitive penes » (p. 8). « We may then correlate the organs of sperm emission in all the arthropods, whether they are penes on the leg bases, paired

or single penes on the venter of the genital segment, or the variously developed phallic organs of most insects » (p. 8-9). In other words, the male intromittent organ with claspers etc. is an independent development without appendicular origin in all arthropods.

In the sections following this introduction he attempts to show that « the male genital apparatus in all principal orders of insects can be uniformly interpreted and named », namely « from the known facts of their development ».

In January 1958 MATSUDA's general interpretation appeared based upon his study of *Machilis* and *Agulla* (Raphidioidea) published in July 1957. His basic argument is that the gonapophyses are sternal outgrowths and not of appendicular origin and he finds among other things support for this viewpoint in HEYMONS' observation in *Lepisma* (1897) that « the gonapophysis is of quite independent origin from the segmental limb, and arises from the primary sternum » (MATSUDA 1957 p. 56). The musculature of the gonapophyses in *Machilis* and *Agulla* arising from the sternum, not from the coxopodite, confirms this idea and moreover proves that the gonapophyses and the eversible sacs are not homologous (1957 p. 53). The female external genitalia arise from a pair of gonapophyses of the eighth segment and two pairs of gonapophyses of the ninth. In the male he stresses the bipartition of what Snodgrass called the primary phallic lobes, and even a second division of the lateral pair in Hymenoptera giving rise to inner and outer claspers = penis valve and clasper, and together = the parameres. He thus puts forward the same viewpoint on the male genitalia as does SNODGRASS, and as to the female he points out the likelihood that they, too, have an origin independent of the limbs; « an entirely different derivation for these two sorts of structures must be thought unlikely » (1958 p. 90).

Much discussion on the external genitalia, morphologically and ontogenetically, has been based on investigations on Orthoptera. On this point MATSUDA (1958 p. 92) says that « the commonly or subconsciously accepted impression among entomologists that orthopteroid insects are standard insects possessing all primitive structures among pterygote forms is no longer tenable ». SNODGRASS (1957 p. 23) said: « The Orthoptera are genitalic individualists ».

In 1959 STYS gave what he called a « reinterpretation of the theory on the origin of the pterygote ovipositor ». He accepts the idea of the homology of the thysanuran gonapophyses with the eversible sacs (SILVESTRI 1905 and GUSTAFSON 1950) and their primitively sternal position, but thinks (p. 77-78) that when the coxopodites during evolution approached each other, displacing the original sternum, they « provoked the displacement of the vesiculae from their primitive sternal position to the anteromedian corners of coxopodites ». In this way the gonapophyses, later evolving into the first and second valvulae, are of non-appendicular origin though they are carried by the first and second valvifers which are the coxopodites and thus of appendicular origin. The third valvulae are « a subdivided portion of the 2nd valvifer » (p. 81). For these he prefers the name styloid, proposing « more accurately » gonostyloid.

In 1961 and 1964 SCUDDER elaborated his idea put forward preliminarily in 1957. The main point is still the gonangulum of which it is said (1961a p. 268) in *Lepisma* that « when the insect is about seven millimetres in length, the anterodorsal corner of the second gonocoxa 'separates' from the rest of the second gonocoxa and connects with the first gonapophysis, tergum IX and the main part of the second gonocoxa, attaining the condition seen in the adult insect ». In *Acheta* (ibid. p. 270) « the gonangulum develops from the sternal region of



the ninth segment. It later develops an articulation with the ninth tergum and second gonocoxa ». A review (1961b) of the insect orders with ovipositor shows the presence of this gonangulum in different stages, fused with tergum IX, with first gonocoxa, or free, and first gonocoxa reduced or absent. So the evolution of the pterygote ovipositor should be based on conditions in Lepismatoidea, not Machiloidea (\*). « The gonangulum is homologous with the anterodorsal corner of the limb base of the ninth segment ». The third valvulae for which he coined the term gonoplac, is a « posterior process of the second gonocoxa », « homologous with the gonocoxal process present in some Thysanura ».

In 1964 SCUDDER discusses the problem of whether the gonapophyses of the ovipositor may still be regarded as derived from limb rudiments though the development shows them to have a sternal origin. And so he elaborates on the possibility of organs being present, then suppressed, and yet be present, but latent, and to develop again if conditions are favourable. This thought, which has also been taken into consideration in phylogenetical speculations, is here defended for the ontogeny of the individual. The conclusion, since he adheres to GUSTAFSON's suggestion of a homology between gonapophyses and eversible sacs and even that of the last mentioned organs with the genitalic ampullae (not known from non-genitalic segments), is the following (p. 414): « Whereas it is possible that the gonapophyses have been derived from genitalic ampullae, the rest of the ovipositor has probably been developed from the primary segmental appendages of the eighth and ninth segments. The original abdominal limbs have provided a source of competent tissue which, through subtle changes in selection, has evolved along many pathways, to form pleuropodia, pseudoplacenta, prolegs and parts of the genitalia ». Though « the limb anlagen have lost their competence for full development with time ». The same thought was set forward also by MICHENER (1944 p. 338): « so that lobes formed later in ontogeny, even though seemingly in slightly different positions, may nevertheless be redeveloping appendages ».

The motivation for these speculations is a desire to prove « that the insect ovipositor is most probably not a radically new structure, but has developed from a previous organ system ». This « fear of the new » is not new. MICHENER (1944 p. 338) writes: « that the copulatory organs are new structures (probably of phallic origin) seems far less probable than that they are derived from pre-existing structures ». In dealing with this phrase SNODGRASS (1957 p. 7-9) gives his comparison of penis-structures without appendicular origin throughout the arthropod phylum (see above). GUSTAFSON (1950 p. 39 and 56) claims with joy: « No completely new structures have arisen, all variations from the basic pattern occurring by modification of existing appendages and sclerites or by loss ». Both, again, seem to have recourse to the « morphological principles » by FERRIS as stated e.g. in 1942 (p. 26: « Not until the evidence is conclusive should it be assumed that any structure could not have been derived from some preceding structure and consequently must represent an entirely new development »). This always reminds me on the old preformation theory; but since man with all his organs cannot be present in an amoeba it seems to me futile to discuss

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(\*) E.G. BECKER (1925, reprinted 1966, p. 254 fig. 106) pictures a sclerite on coxopodite 8 in *Praemachiloides* which he calls subcoxa and which according to SHAROV (1966 p. 199) might be gonangulum.



at what time during evolution organs are permitted to emerge *de novo*, and at what time this is not allowed. Also the appendages of arthropods or Articulata must have an origin.

In 1966 SHAROV deals with the matter (p. 198-210) ascribing an appendicular origin to every one of the genitalic structures. He accepts the homology of gonapophyses and eversible sacs (protrusible vesicles) which he regards as *epipodites* of the appendages, which have « moved to the inside of the telopodite » (p. 185). Since he adheres to Hansen's theory of a crustacean origin of the insects he regards the « coxopodites » as *precoxae* and insists on using the term *precoxa* for *gonocoxa* or *coxopodites*. These viewpoints are completely identical with SILVESTRI's (1905 p. 797): « che le appendici genitali si debbono considerare omologhe alle vescicole retrattili delle subcoxe e ritenendo queste omologhe a coxopoditi, le vescicole retrattili e le appendici genitali si possono considerare come epipoditi ». *Gonangulum* is « vestiges of the sternum of the ninth segment ».

Also in the male all structures are of appendicular origin, the aedeagus belonging to the tenth segment. The classpettes, volsellae « and so on » he compares with the gonapophyses in Machilidae and the grasping appendages with « the precoxal plates and the styli sitting on them in Thysanura ».

In 1969, finally, EDWARD L. SMITH proposed a theory to which I have had access in manuscript, partly for my « Taxonomist's Glossary ». It proceeds from an appendicular origin for all external genitalia of both sexes. Each gonocoxite of segment VIII and IX carries two appendages, gonopods; the lateral is the gonostylus, apparently the telopodite, the medial one the gonapophysis, an endite or (less likely) an exite. Each gonocoxite is a composite of subcoxal, coxal and sternal elements. All these elements are primitively identical in both sexes. The appendages are segmented, the first segment called *radix*, and this only is retained in male pterygote gonapophyses IX and in the gonostyli of both sexes of most adult insects. Male pterygote insects have lost the appendages of segment VIII, and parts of the gonapophyses IX have fused mesally to form an intromittent tube out of the appendages - the phallus or aedeagus which thus is derived as part of the gonapophyses and so of appendicular origin. The gonostyli in the male develop into clasping organs. A long series of new names is coined on basis of this idea and used in preference to the older established ones.

## HOMOLOGY AND NOMENCLATURE

From the foregoing it is evident that there are two, very antagonistic, main theories on the origin of the external genitalia. According to one they are of sternal origin, i.e. « new » buildings, according to the other they are of appendicular origin, transformed abdominal limbs. Let us recapitulate.

1944. MICHENER: ♂, ♀, appendicular origin except phallus, i.e. ♂ dual origin.

1950. DUPUIS: ♂, appendicular origin from segm. IX and X.

GUSTAFSON: ♂, ♀, sternal origin. « Gonopods » do not exist.

1957. ANKER NIELSEN: ♂, appendicular origin.

SCUDDER: ♀, appendicular origin.

SNODGRASS: ♂, sternal origin.

MATSUDA: ♂, ♀, sternal origin.



1959. STYS: ♀, dual origin.

1961. SCUDDER: ♀, appendicular origin.

1966. SHAROV: ♂, ♀, appendicular origin.

1969. SMITH: ♂, ♀, appendicular origin.

MICHENER, DUPUIS, GUSTAFSON, SCUDDER, SNODGRASS, STYS, SHAROV, and SMITH coin new terms and reject others in accordance with their theoretical views, especially SMITH in whose papers it can be difficult to find connection with the old terms except via his simultaneously published glossary for Hymenoptera.

Of course it is not my purpose to make an attempt at « solving the problem » which, as it is, to some degree is a question of belief. It seems to me that it is overlooked by many of the morphologists using the ontogenetical evidence that HEYMONS (1897 p. 628) showed in *Lepisma* and QADRI (1940 p. 168) in *Machilis* that the styli are developed in direct continuation of the embryological limb buds of segment VIII and IX whereas the gonapophyses develop later and independently. A salient point in all discussions is whether the gonapophyses are independent structures or parts of limbs. Another salient point is whether they are homologous with the eversible vesicles which GUSTAFSON (and before him SILVESTRI) set forward as a hypothesis without, however, giving any proof or even proof of probability, but which following him is often regarded as a fact (SHAROV says: « Silvestri convincingly showed the homology »!). Since the eversible sacs receive their muscles from the coxal part of the coxosternum they are often regarded as endites (by SHAROV as epipodites) which also make a homology with the gonapophyses doubtful. Even less proof is given for GUSTAFSON's assumption that the eversible sacs are remnants of genital ampullae supposed to be present originally on all abdominal segments.

Before discussing whether or not the limb buds can be present during stages of the ontogenetic development without manifesting themselves and later come in existence again, a thorough study of the development of the eversible sacs, gonapophyses and abdominal limbs in lepismatoids, and also in *Machilis* (with two pair of eversible sacs on some segments), is badly needed. This is important first of all for the female structures, but then also to decide whether the gonapophyses of the male *Machilis* actually develop independently of the phallus, thus not being homologous with the (true) parameres of Pterygota (developing from the divided primary phallic lobes). The weakness in SNODGRASS' idea of 1957 is that he does not take the male gonapophyses of *Machilis* into account, merely calling them « endite processes ».

Another difficulty for those trying to form an opinion for themselves is that the morphologists generally are content with giving schematic figures on which, naturally, they stress the points in their favour. Detailed drawings, from which the reader will get a clear idea of what the structures actually look like are badly needed. This just as a small 'cri de coeur'.

Be this, however, as it may, the question here is whether to change the terminology whenever a new idea sees the light of day. Of course every author will think his idea is the right one, but the survey of 25 years' hypotheses will show that another theory is always just around the corner. It is a characteristic distinction between the morphologist and the taxonomist that the latter sticks to the terms he has always used, and to me this seems to be the only sound attitude. The morphologists may prove that the claspers are parameres or gono-

Pods or transformed gonapophyses or whatever they can imagine, but the taxonomist will for taxonomical purposes want to know the term used for the structure by his predecessors; if he is morphologically minded (as he should be) he will be interested in knowing the true morphological value of the structure, but a continuous change of names will merely make his job more difficult.

A parallel has recently come to my knowledge: In 1961 G. VANDEBROEK gave a new and convincing interpretation of the tooth cusps in Mammalia and changed the nomenclature of these structures, which are extremely important to mammalian taxonomy, accordingly. A recent question to a specialist on whether the new nomenclature was followed received the answer: it was disregarded by everybody - irrespective of agreement.

But changes in morphological interpretation are not the only reasons for wishing a change of terms. Many authors want to change a term because it is « incorrect ». The « endotendons » and « hypotendons » of Siphonaptera as named by PEUS are not tendons (though they look like them), so SMIT changes them into *virga penis* and *apophysis*. The « triangular piece » in *Lepisma* used by SNODGRASS is not always triangular in other groups, so SCUDDER changes it into *gonangulum*. And so forth. This, too, is an unhappy procedure, even though the term *gonangulum* is a fine invention. One of the most important things in nomenclature is stability, one of the most charming things in morphology is a new hypothesis, so these two things should never be confused.

A stabilizing factor in nomenclature would be to use latin (greek) or latinized terms. The question of the meaning would then be minimized. Everybody will use the word *elaterium* without realizing that it means a drug against extreme constipation, everybody uses the word *juxta* without realizing that it is an adverb without substantial meaning. Only, the authors should consult a dictionary to find out how the words are inflected (in *Taxonomist's Glossary* I have given inflections for all Latin words in common use) or whether it is actually latin; too many words have had to be marked « not true Latin » in the *Glossary*.

Still a lot of terms of Latin or Greek origin are known mostly in their English dress; such words, however, are easily translated into German or Roman languages and easily understood. In some cases even the English, French or German term has been used unchanged in other languages. This, too, is stabilizing. By all means, however, a stabilizing should not be aimed at through nomenclatural rules like those used for the scientific names of the animals; DUPUIS in 1957 attempted to give a codex for such names, but to my feeling a free hand and a glossary is better than another nomenclatural codification.

## CONCLUSION AND SUMMARY

The external genitalia of insects have been and are « the delight of the taxonomist, the despair of the morphologist », as SNODGRASS said in 1957. Within the last 25 years ten authors have given morphological theories on the homology, most of them wanting to name the structures anew according to their own theory, which in every case is thought to be the only true one. Since these theories nevertheless are extremely different it is argued that the taxonomist should stick to the terms in common use for his group and no changes should be made for nomenclatural, morphological or semantic reasons. The difficulties may be overcome by glossaries like « *Taxonomist's Glossary* ». For new terms a Latin (Greek) or latinized form should be aimed at to avoid the translations of vernacular names.



## CONCLUSIONE E RIASSUNTO

*Nomenclatura e omologia dei genitali degli Insetti.*

I genitali degli insetti sono stati e sono «le delizie dei tassonomisti e il tormento dei morfologi», come disse SNODGRASS nel 1957. Negli ultimi 25 anni 10 autori hanno formulato teorie morfologiche sull'omologia, e la maggior parte di essi assegnano alle stesse strutture nomi nuovi in relazione alla propria teoria, che ogni volta è considerata come l'unica giusta. Poiché ciò nondimeno queste teorie sono estremamente differenti l'una dall'altra, si capisce come al tassonomista convenga fissarsi sulla terminologia in uso nel suo gruppo e non apportarvi alcun cambiamento né per ragioni nomenclaturali né per ragioni morfologiche o semantiche. Le difficoltà possono essere superate da un glossario tipo il «Taxonomist's Glossary». Per i nuovi termini si dovrebbe scegliere una forma latina (greca) o latinizzata onde poter evitare traslazioni di nomi vernacoli.

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