Spongelidæ from the Chalinidæ? Indeed, the most simple procedure is to adopt the Keratosa as an independent group; but it should not be overlooked that this procedure is nothing more than the concession to our natural wish to have for the groups we establish the sharpest possible diagnoses.

I should like to summarise my conclusions. We have seen that the subdivision of the Keratosa into two orders is inadmissible; we have seen that their subdivision directly into families gives also as results families of a very ambiguous nature. After the above deduction we can but say that all this is very comprehensible, *since the whole group is nothing more than a simple family.* Of course, as is the case with the subtype of *Acrania*, a high systematic subdivision can be represented by a simple family; and, on the other hand, as is the case with, *e.g.*, Terebellidæ, a family can be subdivided into numerous subfamilies, these latter consisting again of generic unities. This latter measure finds, however, its application in exceptionally rare cases, and only then when it is really necessitated by the richness of the forms as well as by the richness of systematic characters, and, on the other hand, by higher phylogenetic considerations, while the immediate purpose of my foregoing discussion consisted precisely in the attempt to prove that it is due exactly to the erroneous opinion that Keratosa forms a systematically high subdivision, that naturalists split them into orders, suborders, and families. Of course, it cannot be denied that certain genera established in them are more closely connected one with another than with the remaining representatives of the group. This would be, however, only of consequence if all the genera in question were homogeneous, while in reality some of them are undoubted genera, the others perhaps but species. This is the gist of the matter, and I think that the only natural reconciliation of all these contradictions can be obtained by rendering our genera equivalent one to another, which can be realised by enlarging the idea of genus, *e.g.*, by uniting forms, distinguished as *Hippospongia*, *Euspongia*, &c., in the single genus *Spongia*, which would be, on the whole, thoroughly equivalent to the genus *Ianthella* or *Darwinella*. But if the species constituting the conjectural genera *Hippospongia* or *Stelospongos* are yet undoubted species? I answer, prove that they are so, and in that case subdivide the genus *Spongia* into corresponding *subgenera*. As is well known, these latter systematic unities are out of use; I regard, however, their introduction in systematic practice to be equally profitable for systematic purposes in general, as well as with respect to the special case of classifying the Keratosa in a tolerably natural manner. I opened my "criticism of the genera" with a comparison of different opinions as to the value of generic distinctions, and we have seen that in this respect diametrically opposite ideas have been expressed by different naturalists. The word "diametrically" just used alludes to the impossibility of their thorough reconciliation; the introduction of subgenera in zoological calculations would reconcile them at least so far as this is possible, and again it is obvious that sooner or later this reconciliation must be realised, since neither the opinions of Nägeli