

irrelevant consideration; and I have never been able to understand the stress laid upon it by acute thinkers. It is because the triangle is as far as I can perceive isosceles, that I intuit it to be as far as I can perceive equal-angled.

It has, I believe, been already explicitly recognized by certain logicians that a "symbolically" proved conclusion need not give any actual information about "real things." Indeed some go further; but I do not know that any have gone so far as to say that it would not give any information about ideas—although perhaps this may be the logical conclusion.

Cambridge, November 10. E. E. CONSTANCE JONES.

Ice Crystals.

YOUR correspondent, C. M. Irvine (vol. xlvii. p. 31) will find letters on this subject in *NATURE*, vol. xxxi. pp. 5, 81, 193, 264, 480, and in vol. xxxiii. pp. 461, 486.

Prof. (?) McGee's letter at p. 480, of vol. xxxi., gives a list of communications on the same subject in earlier volumes.

B. WOODD SMITH.

The Late Prof. Tennant on Magic Mirrors.

SEVERAL scientific friends tell me that the late Prof. Tennant, the well-known mineralogist, published some twenty or twenty-five years ago a small pamphlet on Magic Mirrors. Failing to find a copy even in the library of King's College, I invite the readers of *NATURE* to assist me to discover one.

SILVANUS P. THOMPSON.

City and Guilds Technical College, Finsbury,
November 15.

On a Supposed Law of Metazoan Development.

UNDER the title of "The Relations of *Larvæ* to Adult Forms," I recently read a paper before Section D at the Edinburgh meeting of the British Association. The subject dealt with was of so extensive a nature, and the time available was so limited, that I fear much that was said must have appeared vague and ill-founded, if not entirely incomprehensible. The material of the essay had, indeed, been prepared with the intention of devoting at least an hour to its delivery: as it happened, I found myself under the necessity of cutting out whole passages of my notes whilst speaking.

The few lines of the report in *NATURE* (vol. xlv. p. 404), convey a very inadequate idea of what I aimed at proving in the paper, and hence I am tempted to offer a fuller account to the readers of this journal.

The subject of the essay furnishes a problem which must interest every embryologist, even though he should reject the conclusions to which observation and reflection have led me.

In working out the complete paper so many novel and confirmatory points have been met with, so much of importance in the writings of the older embryologists, and more especially in the memoirs of Johannes Müller on the Echinoderm larvæ, has been unearthed, that an extension of the original plan of the work has been rendered necessary.

My conclusions, moreover, are so much in conflict with prevailing doctrines that any haste in producing the full argument would be unpardonable, although a preliminary sketch by way of clearing the ground may be justifiable. On a subsequent occasion an attempt would be made to show how the researches of recent years had, with a few notable exceptions (such as the work of R. S. Bergh, J. Kennel, and N. Kleinenberg), tended away from rather than in the direction of a recognition of the fundamental fact of an alternation of generations as underlying Metazoan development, in that they had been concerned, for example, with unnecessary attempts at homologizing the "mesoderm" and its mode of formation throughout the animal kingdom.

If the facts in support of my case should not be as complete as the published researches of the last thirty years on the ontogeny of very many animals might lead one to anticipate, the circumstance would have an obvious explanation.

With the death of Johannes Müller—a man whose brilliance as an embryologist was only surpassed by his greatness as an anatomist—there closed one chapter, and that one of the finest, in the history of comparative embryology. What influence the publication of "The Origin of Species" had upon the subsequent progress of the science is too well known to need further expatiation here. The pernicious search after pedigrees,

initiated by Haeckel, led to an era of activity during which every fact with no apparent bearings on phylogeny was ignored. As a consequence the work of Müller on the Echinoderm larvæ and the essay of Steenstrup on "Alternation of Generations" became more or less mere curiosities in the history of the science. With little exception embryological speculation of the past thirty years has been naught else than a pursuit of will-o'-the-wisps.

It behoves us now to revert to the path along which Johannes Müller laboured.

My own embryological conclusions, like those of contemporaries, have not hitherto been influenced by the embryological works of Müller; for it was not until after my paper had been read that a first study of the Echinoderm memoirs convinced me how nearly he had anticipated what follows.

Before passing to the subject, one further remark may be permissible. Owing to lack of time when reading the paper, no opportunity offered itself for pointing out the analogy which obtains between the suggested mode of Metazoan development and the accepted fact of an alternation of generations in the life-histories of all plants above the lowest Thallophytes. Furthermore nothing was said about the mode of formation of the "mesoderm" in certain cases as one or more outgrowths of the endoderm; although the writer was fully alive to the explanation which from his standpoint could be offered. This and other questions of like character would receive consideration in the complete paper, in which it would be demonstrated that such things and processes need be neither "palingenetic" nor "cenogenetic," but that the analogy of the formation of imaginal discs in *Insecta*, or in the *Pilidium* of the Nemertine, ought to suffice to account for them. As an instance, the formation of the mesoblastic somites in Amphioxus as evaginations of the endoderm may be only a mode in which certain parts of the adult are in that particular case laid down upon the larva.

And now, after this digression, to return to the question under consideration. Two modes of development have long been distinguished, viz., *larval* with *metamorphosis* and *fetal* and *direct*. Cases are known in which there subsists no homology between the larva and the adult, and even such in which the larva (*Bipinnaria asterigera*) is said to exist apart for a time after it has given rise to the Echinoderm. In many such, moreover, the sole larval organ carried over to the adult is the alimentary tract, all other organs of the larva, such as nervous system, sense organs, locomotor and excretory organs, mouth and anus, &c., being replaced by new formations in the adult. The new organs are thus not homologous with those of the larva; indeed, neither as a whole nor in its parts is the larva the homologue of the adult form; but the latter arises upon the former by a mode of asexual generation.

The birth of the Nemertine on the *Pilidium*, and that of the Echinoderm upon the *Pluteus*, or upon the *Bipinnaria asterigera*, may be cited as examples, and the question may now be asked, What becomes of the larva when (a) food-yolk is more or less abundantly acquired, and (b) when uterine development is initiated? Does the larva really disappear? Anticipating the sequel, it is asserted that the larva never vanishes from the development, but is always present in more or less disguised form. In all cases the adult or imago would appear to arise upon it just as is so obviously the case in the examples previously cited.

In the complete paper the modifications of the process throughout the Metazoa would be considered; in this place generalities alone can be dealt with. If the larva be laden with food-yolk it becomes transformed into a more or less obvious *blastoderm*, upon which the *imago* or mature form takes its origin. Certain of the larval organs—such as those of locomotion—may then disappear, but others, such as the larval excretory and nervous mechanisms (e.g., *Hirudinea*, according to Bergh's researches, *Ichthyopsida* from my own work) would persist. Considerations of space do not permit me to enter fully into details regarding Molluscan development. The published work on this group furnishes one with useful material in support of my case; and the group is an interesting one in connection with this question of the relation of the larva to a blastoderm. In the *Mollusca* one can readily find all gradations from cases in which the adult is gradually substituted for a pelagic larva (*Patella*), through those in which the larva is somewhat burdened with food-yolk (*Buccinum*), to others, finally, in which there is a large yolk-sac and a blastoderm, on which the adult form arises (*Cephalopoda*). Incidentally I may remark that it was the study of some *Buccinum*