had previously been called Zwischenwirbelbein by Von Meyer in *Sphenosaurus*, and, long before, Egerton, in 1836, had proposed the phrase 'subvertebral wedge-bone' for the same element in the ichthyosaurs. It may be of interest to observe that Marsh, as early as 1878 (*Amer. Journ. Sci.*, May), correctly recognized his 'intercentral bones' in the so-called hypapophyses of the Mosasaurs, though Boulenger, as late as 1891, denied their identity. Hypapophysis is yet frequently used for the intercentrum of the atlas, following Owen, and 'hypocentrum,' 'basiventral bone,' etc., are frequent and superfluous synonyms of intercentrum.

There is yet another anatomical term which bids fair to become confused in its application —splenial. Owen proposed the term ('Archetype and Homologies,' p. 15) in place of the Cuverian 'opercular,' a term inadmissible because of its double use in the fishes, for the splint-like element on the inner side of the mandible, and figured as typical of the mandible in the crocodile and ostrich. Baur, correctly, I believe, recognizing that the so-called splenial of the turtle is not morphologically identical with the splenial in the crocodile and lizard, but rather a dermal element separated from the articular, gave to it (improperly, I think) the name of angular, while the real angular he called the splenial, and for the real splenial he proposed the new name 'presplenial.' Lambe, recently, in his description of the mandibular elements in *Dryptosaurus*, retains the names previously used in the turtles, but calls the most anterior element, sometimes also present in the turtles, the presplenial. But, this is inadmissible. There can be little if any doubt but that the presplenial of *Dryptosaurus* and the testudinates is morphologically identical with the real splenial of the crocodiles and the lizards, and it must receive the same name. If we call it the presplenial, then Baur's arbitrary change of the angular must also be accepted, otherwise the crocodile, to whose mandible the name splenial was originally applied, is juggled out of a splenial entirely!

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THE ORIGIN OF FEMALE AND WORKER ANTS FROM THE EGGS OF PARTHENOGENETIC WORKERS.

Dzierzon's celebrated theory, according to which the unfertilized eggs of the honey-bee give rise to males, or drones, whereas fertilized eggs develop into females (queens or workers), has not only become one of the established tenets of apiculturists, but has also been expanded by theorists to include other social insects, such as the ants and social wasps. Nor is this expansion merely the result of a tempting analogy. Forel* and Lubbock† long ago showed that the eggs of parthenogenetic worker ants may develop into males, and more recently similar observations have been made by Miss Fielde.‡ These facts certainly confirm the Dzierzon theory and appear to justify its extension to the ants.

The further question, however, as to whether the unfertilized eggs of bees and ants may not, under certain conditions, give rise to workers, is still unanswered.§ In other words, the observation of a number of cases in which males developed from unfertilized eggs, is not in itself sufficient to preclude the possibility of the development of females or workers from such eggs under other circumstances. We know that this possibility is realized in the autumn broods of plant-lice, water-fleas, etc. That it may also be realized in ants is shown by the following observations made independently by three different observers and here quoted as a basis of suggestion for future experimental work. It is, perhaps, timely to stress these observations, for theorizing on sex determination is much in vogue and is being indulged in by some who seem to derive their facts from any but the original sources. That some of these observations have been 'snowed under'—*todtgeschwiegen*, as the Germans say—is not a matter of surprise when we consider the blinding

* 'Les Fourmis de la Suisse,' 1874, pp. 328, 329.

† 'Ants, Bees and Wasps,' London, 1888, pp. 36–40.

‡ 'A Study of an Ant,' *Proceed. Acad. Nat. Sci. Phila.*, July, 1901, p. 439.

§ See also Pérez, 'Mémoire sur le Ponte de L'Abeille Reine et la Théorie de Dzierzon,' *Ann. Sc. Nat.*, 6 ser., Tome VII., Art. 18, 1878, pp. 1–22.

effects of a brilliant theory like that of Dzierzon, backed by the weighty argumentation of a von Siebold, and the way it flatters our ineradicable tendency to formulate, conceptualize and schematize in advance of all exhaustive study of nature's processes.*

I find the following observations on a fungus-raising ant, the 'Sauba' (*Atta cephalotes*) of Trinidad, recorded by Tanner:†

My 'B' nest had neither queen nor male when it was set up on the 4th July; a few larvæ and pupæ were put into the nest at starting. The last of these became an ant on the 14th August, 41 days after capture.

The first eggs were seen 19 days after the capture, viz., on the 23d July. Very many small, medium-sized and large ants were matured from these eggs before its [the nest's] destruction on the 6th November, in periods of from 57 days for the smallest to 74 days for the larger ones. On the 20th October a male was matured, on the 3d November there were 25 males. On the 2d November a queen was matured, and another on the 5th, three days later, and their period was about 84 days. Thus, there are about 10 days for the egg, as a larva it varies from 27 days for the smallest workers, 44 days for the ordinary workers and 54 for males or queens and 20 days for the pupa stage. * * *

It is, therefore, as far as this experiment goes, conclusive, that workers, taken as these were from a nest which had been living in community with males, do lay eggs, and that from them they can produce males and queens.

Tanner's observations go to show that the eggs of *Atta cephalotes* workers may give rise to ants of all three sexual forms, that is, males, females and workers of the different castes so remarkably developed in these large fungus-raising ants. The implication in the last quoted paragraph, that the production of all these forms depended on the fertile workers having come from a colony containing males, may be gratuitous (*vide infra*).

More important observations on this subject have been recently made by H. Reichenbach, a very conscientious worker.*

I quote his results in full:

In the spring of 1899 I placed in an empty artificial nest of the Janet pattern eleven workers of *Lasius niger* L., more for the purpose of showing my pupils the commonest of our ants, than for the purpose of conducting definite observations. I fed them with invert sugar and hashed meal-worms. Even after a few days I noticed several packets of eggs which had been laid by the workers. This was nothing new to me, and I expected that to happen which had happened in my other colonies, namely, that the larvæ hatching from such eggs would succumb to the cannibalism of the ants. At most I supposed that I might obtain males, since it has long been known that males arise from unfertilized eggs laid by workers, as in the case of the honey-bee and the social wasps.

But to my astonishment, the larvæ pupated and produced *typical workers*, which agreed with their progenitrices even in size. A few days later they had acquired their mature coloration and began to take part diligently in the labors of the colony.

Thus it is possible that workers may develop from unfertilized eggs laid by workers.

A little later the number of egg-packets increased, and towards the end of June the number of workers had risen to over a hundred, and a number of larvæ and pupæ were being busily carried about, assorted, fed and licked; the ants' appetite was excellent, the glass manger was found licked clean every morning; pupa-cases,

* Absence of critical caution in accepting the Dzierzon theory is seen, for example, in works like Castle's 'Heredity of Sex,' when the author makes the following apodictic statement (p. 191): 'That the spermatozoon also bears sex is manifest in the case of animals like the honey-bee, for the egg of the bee, if unfertilized, invariably develops into a male, but if fertilized into a female. Professor T. H. Morgan, in his recent work, 'Evolution and Adaptation,' pp. 424, 425, makes a similar statement: 'In the honey-bee all the fertilized eggs produce females and the unfertilized eggs males'; although he proceeds to cite the conditions in an insect of the same natural order as the bees and ants, namely the currant-fly (*Nematus ribesii*), which may, under certain conditions, produce both males and females from parthenogenetic eggs.

† *Œcodoma cephalotes*. Second paper. Trinidad Field Naturalists' Club, Vol. I., No. 5, December, 1892, pp. 123–127.

* 'Ueber Parthenogenese bei Ameisen und andere Beobachtungen an Ameisenkolonien in künstlichen Nestern,' *Biol. Centralbl.*, 22. Bd., 1902, pp. 461–465.

remains of meal-worms, etc., were very neatly piled up in a particular corner of the middle chamber;—in brief, the life and activity of the ants were perfectly normal, notwithstanding the rather peculiar provenience of most of the inmates of the nest.

During the normal course of colonial life the following occurrences were noticed:

During the last week of August, as it were on the very day, when in the gardens and streets of Frankfurth, winged males and females of *Lasius niger* creep about as weary relicts of the nuptial flights, about a dozen fine, shining males hatched in my colony. When they had taken on their adult coloration, they sought the illumined chamber and walked about nimbly. Had it been possible for them to escape, they would certainly have joined in the nuptial flight of the mass of their species out-of-doors.

The males lived only a few weeks; most of them met with an accidental death through becoming glued down with their wings.

The colony passed the winter in good condition, and in the spring of 1900 a rapid increase again took place from eggs laid by the workers. On the 1st of August I was able to announce to our natural history society that the nest again contained 300 workers and two to three dozen males. This year, also, the appearance of the males coincided with the swarming time out-of-doors.

During the year 1901 the same events were repeated, with the difference that the number of individuals had fallen off; still there were a few males towards the end of July. By the spring of 1902 only about twenty workers survived; larvæ were still being reared, but towards the end of April, for some unknown reason, the whole colony became extinct.

Worthy of note, therefore, is the coincidence, three times in succession, in the appearance of males at the typical time of swarming for our neighborhood. From this we must conclude that the conditions in my colony did not depend on degenerative or similar causes. On the contrary, this decided periodicity points to normal processes, which probably also occur in wild colonies, whose workers, in all likelihood, take part in producing males. Of course, these conditions require further investigation.

He who takes for granted the completeness of our knowledge of propagation in ants, more particularly of mating and fertilization, will regard all the workers of my *Lasius* colony as having developed from unfertilized eggs. But the question arises, whether, after the males made their appearance, some kind of copulation could not take place within the nest, or whether, in fact, some of the eleven workers that founded the colony were not fertilized. Many will deny this with indignation and horror; but one is becoming accustomed to surprises, especially in sexual phenomena. Moreover, fertilization always occurs normally within the nest in the case of *Anergates atratulus* Schenck, which exhibits strict in-and-in breeding. Forel also opens up this question ('Les Fourmis de la Suisse,' p. 401). At any rate, a careful anatomical and microscopic analysis of the ovipositing workers, which are perhaps to be regarded as ergatogynous females, and their eggs, is in every respect important, and this alone would give value to the above observations.

That Reichenbach's supposition of a fertilization of the workers by their male progeny in his nest is unnecessary, is shown by the following observations kindly sent me by Mrs. A. B. Comstock, and published with her consent:

About the middle of August I colonized some ants of the species *Lasius niger* L. var. *americanus* Emery, in a glass nest in my room for the purpose of giving my pupils in nature study an opportunity for observing the habits of ants. I found this species common under the stones on a dry side hill, and I brought in, with the workers, pupæ and larvæ of two sizes and some eggs still unhatched. My prisoners soon put their nest in order and placed the pupæ in two separate heaps, and separated the larvæ into two groups according to size, and also placed the eggs by themselves. After a day or two the eggs hatched and these young larvæ were kept in a group away from the others. A few days later more eggs appeared. I at once looked for the queen but found none. No one ant in my colony was any larger than her sisters, and I was mystified as to the source of these eggs. However, they continued to appear; and there have been reared in this nest up to date at least three complete broods. We naturally expected that the eggs which were evidently laid by workers would produce males as is the case with bees. But this theory was wrong, for *all the eggs laid by the workers in this nest have developed into workers.* I have never been able to observe the actual process of egg laying. I am rather inclined to believe that the eggs were usually produced during the night. There was nothing in actions or appearance that enabled me to distinguish the egg-laying indi-

viduals from their sisters. I have noticed that when eggs were being produced a large number of the ants were crowded together in one corner of the nest, and only a few seemed to be on duty as nurses. Whether this segregation has to do with the egg laying or not I do not know.

In this case no males have as yet made their appearance. So accomplished an entomologist as Mrs. Comstock could not have overlooked either these or a queen in her colony, especially as the latter sex in *Lasius* is very much larger and more conspicuous than the worker.

While the observations above quoted are by no means final, they are, nevertheless, of sufficient value to call a halt to all speculation based on the Dzierzon theory formulated in the usual text-book style. As thus expressed this theory can at most be valid for the honey-bee only. The probability that worker ants can really produce other workers or even queens parthenogenetically is of ominous import, not only to some current views on sex determination, but also to many fine-spun theories of instinct and organic development. It has been generally admitted that worker insects have their own specific instincts (a proposition not strictly true, as I have endeavored to show,* since the instincts of the queen ant include all or nearly all the important worker instincts), and that these insects are smitten with such complete sterility as to be absolutely incapable of transmitting their inherited or acquired psychical or physical characteristics. Hence, it is urged, we can explain the existence of these worker traits only by resorting to a natural selection among the queens as bearers of characters which they do not themselves exhibit or exercise. Hence the additional sets of ids, etc., hypostasized in the germ-plasma of the queens. Or, if we have an innate repugnance to natural selection, we are requested to fall back on something like orthogenesis, some Aristotelian principle of perfectibility or Naegelian 'Vervollkommnungsprincip.' But after reveling in this tenuous atmosphere of hypothesis, which I would be the last to deprecate, since it is the only free playground of the living and struggling scientific imagination, are we not now bound to return to the cold facts and the drudgery of experiment and observation, if only to gain strength for another flight?

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* 'The Compound and Mixed Nests of American Ants,' *Am. Naturalist*, 1901, p. 798.

QUOTATIONS.

THE CARNEGIE INSTITUTION.

THE trustees of the Carnegie Institution held their second annual meeting at Washington on December 9. Nothing that has become known in regard to this meeting will tend to allay the anxiety with which men of science are watching the administration of this great trust. It is reported that Dr. Gilman presented a letter to the trustees announcing his intention to resign the presidency at the close of next year. The institution will consequently drift along for another year, and its immediate future will in large measure depend on the president then chosen. There is no reason to doubt the ultimate outcome, and even the present conditions are only what might have been expected. Special creations are no longer regarded as feasible. The reply may be called to mind of the little boy, who, on being asked who made him, said 'God made me one foot big, and I growed the rest.' A new foundation such as Mr. Carnegie's can only gradually become a true organism adjusted to the environment.

Mr. Carnegie's original plan of establishing a research university at Washington was comparatively plain sailing. The trustees are now divided as to policy, some wishing to establish certain laboratories at Washington, and others perferring to distribute subsidies throughout the country. The latter plan has been adopted; it has the obvious advantage of not committing the institution as to the future. No special objection can be made to the way the subsidies have been allotted. It is quite certain, for example, that the Harvard, Lick, Yerkes, Dudley and Princeton observatories can spend to advantage any money that may be entrusted to them. Almost any grant for research made to men of science of established reputation will bear fruit a hundredfold.