

In pure glucose solutions tested at constant volume with fixed amounts of phenylhydrazine hydrochloride and sodium acetate, the time required for the precipitation of osazone varies with the amount of glucose present and is nearly constant for any given dilution. Pure solutions of fructose show similar variations with concentration, but always yield a precipitate of osazone in about one third the time required by the same amount of glucose. Invert-sugar reacts almost as readily as fructose. Maltose retards precipitation of glucosazone, interfering much more seriously with glucose than with fructose. Lactose interferes in a similar manner and to a greater degree than maltose.

Some Derivatives of Citronellal: F. D. DODGE.

The paper is a continuation of the author's previous work on citronellal (*American Chemical Journal*, XI., XII.). The preparation and properties of the so-called citronellal-phosphonic acid and a number of its salts are described.

The decomposition of the sodium salt by heat yielded a secondary alcohol, apparently identical with the iso-pulegol of Tiemann. This reaction, together with the general properties of the acid indicates that it is really a derivative of iso-pulegol, and should properly be called iso-pulegol-phosphonic, analogous to the phosphonic acids of the aromatic series.

The formation of this acid may be utilized for the identification of citronellal, when the latter is present in large amount, and even small quantities of the acid can be detected by means of the characteristic silver salt. The author was unable, however, to confirm the alleged occurrence of citronellal in oil of lemon by means of this reaction.

F. H. POUGH,
Secretary.

THE SOCIETY OF GEOHYDROLOGISTS, WASHINGTON.

A SPECIAL meeting of the society was held on January 29 for the purpose of discussing the significance of the term 'artesian' and of adopting definitions covering its use. As a result of the discussion the following definitions were provisionally adopted, subject to such changes in wording as may be necessary:

DEFINITIONS OF 'ARTESIAN.'

Artesian Principle.—The artesian principle, which may be considered as identical with what is often known as the hydrostatic principle, is defined as the principle in virtue of which water confined in the materials of the earth's crust tends to rise to the level of the water surface at the highest point from which pressure is transmitted. Gas as an agent in causing the water to rise is expressly excluded from the definition.

Artesian Pressure.—Artesian pressure is defined as the pressure exhibited by water confined in the earth's crust at a level lower than its static head.

Artesian Water.—Artesian water is defined as that portion of the underground water which is under artesian pressure and will rise if encountered by a well or other passage affording an outlet.

Artesian System.—An artesian system is any combination of geologic structures, such as basins, joints, faults, etc., in which waters are confined under artesian pressure.

Artesian Basin.—An artesian basin is defined as a basin of porous bedded rock in which, as a result of the synclinal structure, the water is confined under artesian pressure.

Artesian Slope.—An artesian slope is defined as a monoclinical slope of bedded rocks in which water is confined beneath relatively impervious covers owing to the obstruction to its downward passage by the pinching out of the porous beds, by their change from a pervious to an impervious character, by internal friction, or by dikes or other obstructions.

Artesian Area.—An artesian area is an area underlain by water under artesian pressure.

Artesian Well.—An artesian well is any well in which the water rises under artesian pressure when encountered.

M. L. FULLER,
Secretary.

DISCUSSION AND CORRESPONDENCE.

THE KELEP EXCUSED.

IF Dr. Cook will revert to my paper published in *SCIENCE*, Vol. XX., 1904, pp. 766-768, he will notice that I did not promise to keep silence till the Greek Kalends, but merely expressed my willingness to wait till that date for the extermination of the cotton boll-weevil by the keleps he had introduced from Guatemala. And I am still waiting. I did, indeed, promise to let the insect rest, as I supposed

that Dr. Cook would confine himself to its economic aspect, a subject on which I had nothing further to say. But he has seen fit to make his observations on the kelep the basis for certain general statements which, if true, would go far towards revolutionizing our knowledge of the social insects. Under the circumstances I craved the privilege of the 'positively last appearance' accorded to some of the members of other professions than my own. Nor have I had occasion to regret this, for may there not be a distinct gain to science in Dr. Cook's admission of some of his errors and his promise to be more careful in the future? In his latest article there are still a few matters which do not seem to me to be fairly stated, and if I again ask for a little space in SCIENCE, it is not for the purpose of continuing my 'scolding' and, perhaps, too drastic criticism, but merely for the sake of setting my previous remarks in a proper light. Dr. Cook says:

If one were to generalize on this series of entomological episodes the deduction would be that adequate ignorance of literature is a necessary qualification for learning the habits of a new insect like the kelep, for at each important step the investigation has been met by Professor Wheeler's *non possumus*. Last year he was quite as unable to believe that the keleps would kill boll-weevils as he is to credit now their failure to regurgitate nectar. After surviving so many of these literary dangers it is only natural that one become a little reckless, and venture even to hope that in the course of another year the additional facts, at present so objectionable, will receive due credence, having now become a part of 'literature of the subject.'

There can be little doubt that scientific investigation is often impeded rather than furthered by too much attention to the 'literature of the subject.' Many a piece of zoological research may be perverted from the outset by an incessant appeal to what has been written, for reliance on a knowledge of the literature, especially in entomology, may not only clog the free movements of the investigator, but may lead him to waste much valuable time in the blind bypaths of his science. Investigation and publication are, however, two very different matters. One may investigate a

thousand things, experience all the thrills of first discovery in every one of them and still never care to inflict one's results on one's fellow beings. But whenever one does decide to publish, it is necessary to reckon with the great 'paper memory of mankind,' the conserved experience of other workers who have loved and investigated the same things. It then becomes a duty to study the 'literature of the subject,' if only for the purpose of bringing the new work into intelligible, organic relation with the old. Failure to do this may be justly interpreted as carelessness, sloth, ignorance or conceit.

After making his observations with commendable enthusiasm and in great freedom of spirit, Dr. Cook failed in his full duty to other investigators—*hinc illæ lacrymæ*. That even now he does not seem to be fully aware of his omissions is obvious from the following considerations:

1. His grounds for concluding that no nuptial flight occurs in the kelep have little value, because it is known that some species of ants, like certain species of *Camponotus* and *Prenolepis*, which celebrate this flight at regular intervals, nevertheless retain males and winged females in their nests during the whole or several months of the year. Moreover, copulation within the nest has been observed in species like our common tent-building ant (*Cremastogaster lineolata*), which has a typical nuptial flight. Miss Fielde has even photographed a number of mating males and females of this species in one of her glass formicaries. Hence there is nothing in Mr. McLachlan's observations on artificial nests of the kelep to demonstrate the absence of a nuptial flight. Like all similar negatives, this would, in fact, be extremely difficult to prove.

2. Dr. Cook's remarks on *Leptogenys* are incomprehensible to me. The queens of *L. elongata* are, indeed, little more than egg-laying workers, as he would have noticed had he read my account of these insects. No one has ever been able to find a winged queen of any of the numerous species of this tropicopolitan genus or of its subgenus *Lobopelta*, although egg-laying workers, similar to those which I found functioning as queens in *L.*

elongata of Texas, have been seen in an Indian species.

3. It is not I, but Dr. Cook, who has been studying the habits of the kelep. I have been trying to interpret his observations in the light of what I have seen in other ants. He claims that I 'was quite unable to believe that the kelep would kill boll-weevils.' Reference to my previous papers will show that this is an unfair and purely gratuitous statement. I have never doubted his observations on this point, but merely the ability of the ants to keep at the good work of killing the pests with sufficient concentration and perseverance to make them a considerable factor in the extermination of the boll-weevil.

4. Dr. Cook endeavors to show that I am wrong in inferring that the keleps can ingurgitate and regurgitate liquid food. He says that they "persist in going about with large, round drops of nectar on their bills. They regularly carry it into their nests in this way, and feed it to their friends and families without having once swallowed it, or spewed it up again. This incredible conduct is very easy of observation," etc. If this observation were beyond suspicion, I should be the last to reject it as a proof of Dr. Cook's contention, for the very reason that it agrees so well with the many primitive habits I have detected in the Ponerinæ that have come under my own observation. But I still have serious doubts on this subject, not because Dr. Cook's statement conflicts with anything in the 'literature of the subject'—on the contrary, it confirms my own statements on *Cerapachys*—but because I have seen large camponotine ants carrying drops of liquid on their mouth parts when they had ingurgitated as much food as they could hold in their crops. Is Dr. Cook sure that none of the liquid is drawn into the crop of the kelep and that this is not regurgitated to members of the family after the hanging drop has been disposed of? Remarkably concise observations would be required to make sure of this point, and as soon as Dr. Cook can produce these I shall be only too glad to accept them.

5. Dr. Cook's remarks on the phylogeny and classifications of the ponerine ants are thrown

off in a haphazard, hit-or-miss fashion not at all reassuring to those who can appreciate the long and serious study devoted to these subjects by men like Gustav Mayr, Roger, Forel, Emery, Ernest André and others. Tracing phylogenies is at best a very dubious and highly speculative performance, but it may be said that the phylogenies in question have not only been traced, contrary to Dr. Cook's assertion, but they have been so conscientiously traced that there is practical unanimity on the subject among myrmecologists. The ants of the higher subfamilies (Myrmicinae, Dolichoderinae and Camponotinae) have been derived from the Ponerinae, and it has long been known that this primitive subfamily embraces more disparate groups of genera than are to be found in any of the subfamilies of recent development. This is, of course, quite in harmony with what is known of many other archaic groups of animals and plants.

WILLIAM MORTON WHEELER.

SPECIAL ARTICLES.

RAMBUR AND THE NATURE OF SPECIES.

IN a volume entitled 'Histoire Naturelle des Insectes,' published in 1842, by Dr. M. P. Rambur, prior to Darwin, Wallace and Wagner, there are certain very sage reflections on the nature of genera and species. My attention has been called to this work by Mr. For-dyce Grinnell. I append an extract which is worth reading even at the present day. I place the sentences referring to the effect of varied environment on species-forming in italics.

Dr. Rambur says:

Il ne faut pas se faire illusion, la classification n'est pas la science, n'est pas l'histoire naturelle, elle n'est qu'un moyen factice pour arriver à la connaissance des différents êtres qui se trouvent dans la nature. Certes c'est un progrès heureux de l'avoir basée sur des rapports plus ou moins naturels (quoique quelquefois insuffisants); mais la science est surtout la connaissance de l'être qu'on appelle espèce, l'histoire naturelle est cette connaissance, et celle des rapports nombreux d'organisation et de mœurs que les espèces présentent entre elles. Il ne faut donc pas reculer devant le mot *espèce*, il faut chercher à le comprendre; toute la science est là; c'est s'en écarter