genesis, formation, growth and classification of language, Mr. Stetson appealed to the latter to exercise greater care in announcing, and greater diligence in seeking for unanimity in their concepts.

He suggested that, in order to prevent the existing confusion among professional writers as well as among students, it was imperative that a line be drawn and a classification adopted which shall definitively separate the crude, fluctuating, undeveloped, and unrefined speech of a narrowly circumscribed region—*i. e.*, the 'dialectic stage'—from the comparatively fixed and highly developed inflected speech of an extensive area, or the 'cultivated stage.'

That patois and dialect should cease to be used as synchronal or equivalent terms, as in the history of language the former represents the destructive and the latter the constructive period.

That the use of 'dialect' as a relative term, by which the meaning of the 'dialect' and 'language' is made to depend upon the connection in which the terms are used should be abandoned, in the interest of clear thought and intelligible classification.

That writers should more particularly differentiate 'speech' from 'language,' and, in comparative philology, the study of the affinities of language, from linguistics, the study of the derivation of words.

That the classification and relations of dialect, language, patois, and jargon be more absolutely defined and rescued from their present confusion by some authoritative body.

That the aim of writers on comparative philology and experts in linguistics should be to more completely separate the conceptual and hypothetical from the practical and profitable, and thus prevent the needless waste of thought and effort.

"That more is to be learned from analogy and living speech, as Professor Sayce suggests, than from dead literature," or it may be added from the questions of origin and precedence.

In conclusion Mr. Stetson remarked that he did not wish to convey the impression that the absence of unanimity in concepts and confusion in terminology is peculiar to the writers on philology; he feared that they might be found in a greater or less degree in all philosophical inquiries.

He also expressed the opinion that students generally, in view of the prevailing contradictions, the dearth of recorded facts, and superabundance of hypotheses, are not inclined to accept without question the present claim of comparative philology as a science, and that while extremely valuable work has been and is being done,—especially in the division of linguistics, a study which has been practically born within our memory,—its essays and instruction are too frequently founded upon hypotheses "which furnish no perceptible evidence of truth or of value in their practical application."

J. H. McCormick,
Secretary.

DISCUSSION AND CORRESPONDENCE.

HOMOLOGIES OF THE WING VEINS OF HYMENOPTERA.

Very important investigations of the morphology of the venation of the wings of insects have recently been made by Professor Comstock in his 'Manual for the Study of Insects,' published in 1895, and more recently by Comstock and Needham, in a series of articles published in the *American Naturalist*, 1898–99, reissued as a pamphlet of 124 pages and 90 figures by the Comstock Publishing Company.

While I accept their principles, the application of them and a comparison of the figures lead me to a different conclusion with regard to homologies of the wing-veins of hymenoptera, which in connection with my studies of the bees it has been very important for me to work out. In the Manual vein M is regarded as three-branched, as in the diptera, but in the later articles this vein is regarded as four-branched.

In the first place I regard the wing of *Macroxyela* (Manual, p. 606, fig. 705) as a better example of the typical hymenopterous wing than the composite wing produced by a combination of the wing of *Macroxyela* and *Pamphilius* (*Am. Nat.*, 414; figs. 38–39). But the latter will illustrate my views.

My conclusions are: that the cross-vein m connects M_2 and $M_3 + Cu_1$, as in the wing of

Pantarbes (40) and *Rhamphomyia* (41), and that the cross-vein marked *m — cu* in *Leptis* (*Am. Nat.*, 32; 337, fig. 30) does not exist in the hymenoptera, but is obliterated by the coalescence of the above-mentioned veins; M_4 is Cu_1, and Cu_1 is Cu_2; the cross-vein marked *m — cu* is not homologous with the one so marked in *Leptis*, but belongs to the *arculus!* To account for the vein marked Cu_2 I should say that it is before the arculus and does not enter in the consideration of the ordinary cases. According to the authors, this vein does not occur in any of the hymenoptera, except *Pamphilius*.

If my view is correct, a large part of the peculiarities of the venation of hymenoptera is connected with the great lengthening of the arculus and the shifting of it from the base of the wing.

The only changes in the designation of the cells which my attempt at elucidation involves are: M_3 is Cu_1; Cu_1 is *2nd Cu*; *Cu* is *1st Cu*; *M* is homologous with the cell marked *1st M* in *Scenopinus* (*Am. Nat.*, 32; 339, fig. 36); M_4 is the same as the cell marked *2nd M* in *Scenopinus* and the cell marked *M* in *Rhamphomyia* (41).

CHARLES ROBERTSON.

NOTES ON INORGANIC CHEMISTRY.

A GOOD illustration of how much material there is in inorganic chemistry which needs reinvestigation, is found in the fact that there has been no general method of forming the sulfids of the rare earths, nor have any of the sulfids been obtained in a pure condition, with the possible exception of that of cerium. This gap has now been filled by Muthmann, of Munich, in conjunction with L. Stützel. They find that while the oxids are very slowly converted into sulfids when heated in a stream of hydrogen sulfid, the anhydrous sulfates are, under the same conditions, very readily converted quantitatively into the sulfids. The sulfids of cerium, lanthanum, neodymium and praseodymium were formed in this way, and their properties, physical and chemical, studied. They are fairly stable in the air at ordinary temperature, but are decomposed with evolution of hydrogen sulfid by warm water or dilute acids. They take fire readily on heating in the air, and when finely divided the cerium sulfid often proved pyrophoric. They burn to a mixture of oxid and sulfate. On heating in a current of dry hydrochloric acid, they are readily and quantitatively converted into the anhydrous chlorids, and on a small scale this is the best and easiest method of preparation of these chlorids. The study as a whole, which is published in the last *Berichte*, is a valuable contribution to the chemistry of the rare earths.

IN a series of experiments by A. Petterson, of Upsala, printed in the *Klinische Wochenschrift* (Berlin), the fact is established that in meat and fish preparations, containing 15% of salt for the purpose of preservation, a constant and luxuriant growth of microörganisms takes place. From this the conclusion is drawn that the special flavors, odors, consistencies, and colors of salt conserves are chiefly produced by various microörganisms.

THE subject of food preservatives is also discussed from a different standpoint by R. Kayser, of Nuremberg, in the *Zeitschrift für öffentliche Chemie*. In earlier times, the tendency of various foods to decomposition was counteracted by drying, smoking, pickling and the like. In some cases special processes were used, as the treating wine with sulfur, beer with hops, etc. At the present day, scientific progress has led to the use of low temperatures, of sterilization, and especially to the use of chemicals. In this last case the demand is made that the preservatives used shall not only be harmless in the quantities used, but inert to the human system even in vastly greater quantities than ever used in foods. This demand, it is pointed out, is unprecedented, for it is not complied with under old methods. Common salt, saltpeter and creosote are present in these and are not less injurious in quantity than the more recently used boric acid, borax, salicylic acid, benzoic acid, etc. There are no authentic instances on record of injury from the use of any of these in foods, while there are very many instances of injury from foods which, apparently good, were in reality decomposed (presence of ptomains, etc.). The whole subject needs to be treated in a more rational way.