

causes the total operative pressure of the solution to be that of only the solvent present in it. Now, I have shown that if we take a solution such as that of propyl alcohol in water, and place it in a semipermeable vessel surrounded by water, the latter will pass through towards the solution, *ergo*, the vessel is permeable to water but impermeable to the alcohol; but if the same vessel with its same contents is surrounded by propyl alcohol, it is the alcohol that passes through towards the solution, *ergo*, the vessel is permeable to the alcohol, but not to the water, and must, therefore, at the same time, be both permeable and impermeable to each substance; which is absurd. The obvious conclusion to draw from this experiment is, that it is the solution, and not either of the substances separately, to which the membrane is impermeable, and this is just what we should anticipate on the hydrate theory, the molecules of hydrates being necessarily larger than those of their constituents.

As to ionic dissociation: When one molecule of sulphuric acid is added to 100 molecules of water, if no change occurs, we shall have a total of 101 molecules, entities or acting units, whatever they may be called; if hydrates are formed, we shall have less than 101 acting units; and if dissociation occurs, we shall have more than 101. Relying on the depression of the freezing-point, the dissociationists maintain that this last is the case, and that something between 101 and 103 acting units are present; but, if this method is really to be trusted, it proves too much, for by measuring the depression of the freezing-point of a large bulk of acetic acid produced by adding to it bodily the $100\text{H}_2\text{O} + \text{H}_2\text{SO}_4$, we find that this mixture contains very considerably less than 100 acting units, instead of more.

The explicit evidence afforded by such experiments surely calls for some comment on the part of those whose theories seem to be negated by it, and their silence on the subject is so significant that it is surprising that it should not have attracted more attention than it has.

The strong evidence of the existence of compounds such as hydrates in solutions, both concentrated and weak, is inconsistent with that perfect freedom of molecules and atoms postulated by the dissociationists, but the discussion of this evidence cannot be attempted here.

The hydrate theory did not lead, and probably never would have led to an explanation of that peculiar behaviour of electrolytes which simulates dissociation, but, as I showed some years ago (*Ber. deutsch. chem. Gesell.*, and, in outline, in an article on Solutions in "Watts' Chemical Dictionary"), the theory is perfectly consistent with the observed facts, and, further, affords an explanation of them, which is free from the serious objections attaching to the dissociation theory.

Bath, December 23, 1896. SPENCER PICKERING.

Some Neural Descriptive Terms.

IN a recent circular asking the opinion of experts as to the prevailing and preferred usage of anatomic and neurologic terms, on behalf of the projected Dictionary of Philosophy and Psychology, Dr. C. L. Herrick mentions certain terms and principles which have been either proposed or adopted by me.

But for the request to "respond as early as possible," I should suggest that replies be either delayed or regarded as provisional until after the appearance of my paper, "Neural Terms, International and National" (*Journal of Comparative Neurology*, vi. pp. 216-340, December 1896), wherein the general subject is discussed at length, and in parallel columns are given the neuronyms adopted by the Anatomische Gesellschaft in 1895, and those now preferred by me. But for the remoteness of Dr. Herrick's present address the following comments would be submitted to him first.

3 (b). For the part now called by the Gesellschaft "Substantia perforata lateralis," I formerly proposed *præperforata*, but since 1889 have employed *præcricbrum*.

4 (e). *Metencephalon*, as employed in the last three editions of "Quain," and adopted by me in 1881, designates the last definitive encephalic segment, *i.e.* between the cerebellar segment (our epecephalon) and the myelon or spinal cord. As given in the circular, it has two other usages, *viz.* either for the cerebellar segment alone (His), or for both regions (some authors). The encephalic segments will form the subject of a paper at the coming meeting of the Association of American Anatomists.

(g). *Metencele* is doubtless a misprint for *metacole*. The Latin (international) forms are *metacolia* and *mesocolia*; the national English forms *metacele* and *mesocole*.

(j). As to *Neuron* (proposed by me in 1884 as a mononym for *axis cerebro-spinalis*) see "Reference Handbook," ix. 100, and *Proceedings Assoc. Amer. Anat.*, 1895, 44-45. Indirect endorsement of it is contained in such compounds as *neuromere*, *neuenteric*, &c. In like manner *myelencephalon* (for either the entire cerebro-spinal axis or for the last encephalic segment) embodies indirect endorsement of *myelon* for *medulla spinalis*.

As to *cephalic* and *caudal*, *cephalad* and *caudad*, during an experience of sixteen years no actual instance of misapprehension has been observed. But since they evidently are not acceptable to some, might not the increasing employment of *præ* and *post* in composition with the force of adjectives justify taking these prepositions as the bases of adjectives, *viz.* *præalis*, *postalis*; Eng., *præal* and *postal*; adverbs, *præad* and *postad*? As mere vocables the last two are no more objectionable than *quoad*.

Classic precedents for the derivation of adjectives from prepositions or adverbs are *contrarius*, *extraneus*, *proprius*, *crastinus*, *pristinus*, *interior*, *superius*, and *ἀνώτερος*.

Ithaca, N.Y., December 19, 1896. BURT G. WILDER.

Measurement of Crabs.

IN his letter of December 3, Mr. Cunningham suggests that if the young crabs be compared so that the frontal ratio is taken as the standard of comparison between the respective groups of the two years, the difference will be one of carapace length only, and this may be due to variation in the food supply. But he does not explain why the effect of the supposed variation in the food supply should be confined to the one dimension of carapace length, and not extend to the frontal ratio also. There is no evidence to lead us to suppose that the change of frontal ratio is a more accurate criterion of development than carapace length; but whichever be taken as a basis for comparison, the result is a change of shape in the carapace as between the two years.

I have, however, to make a correction as to the *adults*, two groups of which, belonging to different years, I compared in my paper as being equal and comparable. I had reason for thinking that they were equal; but I regret to say that a more accurate investigation of them, to which I was led by Mr. Cunningham's criticisms, shows me that they were not so, and that the inequality was not a natural one, but was due to an unconscious selection in the process of collecting them; and therefore these two groups are not comparable, and must be eliminated from a consideration of the question.

December 28, 1896.

H. THOMPSON.

Marriage of the Dead.

MARCO POLO narrates of the Tartar tribes thus:—"They have another notable custom, which is this. If any man have a daughter who dies before marriage, and another man have had a son also die before marriage, the parents of the two arrange a grand wedding between the dead lad and lass. And marry them they do, making a regular contract! And when the contract papers are made out they put them in the fire in order that the parties in the other world may know the fact, and so look on each other as man and wife. And the parents thenceforward consider themselves sib to each other just as if their children had lived and married. Whatever may be agreed on between the parties as dowry, those who have to pay it cause to be painted on pieces of paper, and then put these in the fire, saying that in that way the dead person will get all the real articles in the other world" (Yule, "Book of Ser Marco Polo," 2nd ed., vol. i. pp. 259-260). On this narration of Polo, the late Colonel Yule, quoting the authors of later date, remarks that "this is a Chinese custom, though no doubt we may trust Marco for its being a Tartar one also" (p. 260).

As it is not well known whether or not there is a record of this strange custom earlier than the beginning of the dynasty of Yuen, I was in doubt whether it was originally common to the Chinese and Tartars until I lately came across the following passage in "Tsoh-mung-luh" (Brit. Mus. copy, 15297, a 1, fol. 11-12), which would seem to decide the question—"In the North there is this custom. When a youth and a girl of marriageable ages die before marriage, their families appoint a match-maker to negotiate their nuptials, whom they call 'Kwei-mei' (*i.e.* 'Match-maker of Ghosts'). Either family hands