desirable. Such a classification will be attempted in this note.

Let us consider first the sources of affinity (*i.e.*, of attraction for other atoms or groups) in an atom or group of atoms. The most common of these are the following:

(a) An unpaired electron in the valence shell of an atom.

(b) A positive atomic kernel (H, Na, Cu, etc.) not surrounded by electronpairs.

(c) An electro-negative atom, or more specifically a lone electronpair (a pair of electrons not acting as a bond between atoms) in the valence shell of a negative atom.

(d) Double and triple bonds and similar structures (such as three- or four-membered rings) in which one (or more) of the bonding electronpairs is not near the line joining the centers of the two atoms it holds together.

The actual magnitude of the attraction between two structures will of course depend not only on the kind or kinds of affinity regions possessed by each but also on what we might call the "degree of affinity" or the "degree of unsaturation," which will vary widely for different substances. Thus we should expect an "acid" hydrogen atom (class b) to have a greater affinity than a hydrogen atom in a paraffine hydrocarbon for an oxygen atom in another molecule (class c).

Structures of type (a), because of their strong affinity for similar structures, rarely exist at ordinary temperatures.[3] Amorphous carbon and the fresh surfaces of some metals, however, probably constitute exceptions to this generalization. Adsorption by these substances we may assume to be largely of the (ab) (ad) and perhaps (ac) types, according to the nature of the substances adsorbed.

From crystal structure and other evidence,[4] we know that (b) and (c) type structures mutually attract each other, often quite strongly. From organic chemistry there is considerable evidence[5] that two (d) structures attract each other, an addition product (often existing only momentarily) being formed. Attractions of these types—(bc) and (dd) —we might assume to be important in adsorption processes. We might also expect (bd) and perhaps also (cd) adsorption.

Adsorption is very likely often a mixture of the above types. By properly choosing the substances studied, however, it may be possible to study separately the characteristics of the different kinds.

[3] *Cf.*, Lewis, "Valence and the Structure of Atoms and Molecules" (Chemical Catalog Co., New York, 1923); Chapter VI. Huggins, *Phys. Rev.*, March, 1926.

[4] *Cf.*, Huggins, *J. Phys. Chem.*, *26*, 601 (1922).

[5] Huggins, *J. Am. Chem. Soc.*, *44*, 1607 (1922).

The relation between the foregoing method of classification and the division into "polar" and "apolar" adsorption is only partly obvious. (bd) adsorption is certainly "polar," while (dd) adsorption is probably to be identified with "apolar" adsorption. The other types are more difficult to classify. Perhaps it will be better not to try, but rather to frankly admit that adsorption is of more than two kinds.

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WHO DISCOVERED VITAMINES?

WHILE it might be irrelevant for humanity who is the discoverer of vitamines, so long as they became known, nevertheless it is a question of general interest for the public and of personal interest to numerous workers in this field.

While the most important work on this subject was done in the years 1911–1912, it suddenly became known in 1919[1] that Sir Frederick G. Hopkins, of Cambridge University, was credited as its discoverer. The same opinion is shared by certain English, American, French and German investigators, to quote only a recent article by Drummond, Channon and Coward.[2] Attribution of the discovery to Hopkins was particularly surprising to me, as I have worked in the years from 1910 to 1915 in London, chiefly at the Lister Institute, on the same subject and never heard Sir Frederick quoted or regarded as the discoverer of vitamines. It is only since I left England in 1915 that these rumors began slowly to penetrate to me. Unless the English investigators possess in their hands some additional experimental evidence, beyond a lecture by Hopkins (which remained unknown to every worker up to 1919) in 1906 and two experimental papers in 1912, in justice to other pioneer workers in the vitamine field he should not be regarded as their discoverer. In fact, his experimental paper was presented so late (1912) that it exerted a relatively small influence on the development of the whole subject. His paper came many years after the researches of Bunge and his school, Forster and others and even later than the work of Eijkman, Grijns, Stepp, Schaumann and myself, and therefore remained unknown to all these workers.

What are the facts? In 1906 Sir Frederick undertook a series of famous experiments on the importance of certain aminoacids in foods, particularly tryptophane, then recently discovered by him. He apparently noticed then that even on adding tryptophane to tryptophane-deficient diet, the animals im-

[1] Report Medical Research Committee, No. 38, 1919.

[2] *Biochemical Journal*, 19, 1047, 1925.

proved for a while, but died, however, later. He refers vaguely in his New York City lecture to an evidence, which at best must have been very inconclusive at this early period,[3] that some hitherto unknown food elements must be present in a complete dietary. He refers to this in 1906[4] as follows:

> But further no animal can live upon a mixture of pure protein, fat and carbohydrate, and even when the necessary inorganic material is carefully supplied, the animal still can not flourish. The animal body is adjusted to live either upon plant tissue or other animals and these contain countless substances other than the proteins, carbohydrates and fats. Physiological evolution, I believe, has made some of these well nigh as essential as are the basal constituents of diet; lecithin for instance, has been repeatedly shown to have a marked influence upon nutrition, and this just happens to be something familiar, and a substance that happens to have been tried. The field is almost unexplored, only it is certain that there are many minor factors in all diets of which the body takes account. In diseases such as rickets, and particularly scurvy, we have had for long years knowledge of the dietetic factor, but though we know how to benefit these conditions empirically, the real errors in the diet are to this day quite obscure. They are, however, certainly of the kind which comprises these minimal quantitative factors that I am considering. Scurvy and rickets are conditions so severe that they force themselves upon our attention, but many other nutritive errors affect the health of individuals to a degree most important to themselves, and some of them depend upon unsuspected dietetic factors.

If we analyze this statement we must admit that Hopkins showed unusual perspicacity at this early time. On the other hand, he showed no evidence that he knew to what class of substances these mysterious agents could be referred. His mention of lecithin, for instance, makes him attribute a particular rôle to already known substances that has been undoubtedly misleading. If we compare this statement of Hopkins of 1906 with the statement of Bunge of 1891, *viz.*, "Mice can live well under these conditions when receiving suitable foods (milk), but as the above experiments demonstrate that they were unable to live on proteins, fats, carbohydrates, salts and water, it follows that other substances indispensable for nutrition must be present in milk besides casein, fat, lactose and salts," we must admit that Hopkins did not advance the question much since the work of Bunge.

As regards my own rôle in the vitamine field the only claims I can put forward are: (1) the recognition of the existence of several vitamines; (2) the right conception about the importance of vitamines for nutrition; (3) the first chemical study of vitamine B (1911), which unfortunately for the problem has not been improved on yet; (4) general stimulation of researches in this field through expressed ideas, experimental and summarizing work.

We come to the conclusion, therefore, that the discovery of vitamines can not be attributed to a single man. Among the pioneer workers in this field can be named: Bunge, Röhmann, Stepp, Eijkman, Schaumann, Suzuki and others. And the most that one can concede to Hopkins is that he was one of the pioneers. His distinguished services in the field of biochemistry and physiology (discovery of tryptophane, the chemistry of the muscle, the discovery of glutathion) together with his charming personality have made him, even without the title of discoverer of vitamines, one of the leaders in the biochemical world.

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[3] *J. Ind. Eng. Chem.*, 14, 64, 1922.

[4] *Analyst*, 31, 395, 1906.

CITATIONS OF SCIENTIFIC LITERATURE

MAY I make a comment and ask a question with reference to the recent notes on citations of scientific literature that have been appearing in SCIENCE?

Furfey (February 26, 1926, pp. 231 f.) makes many excellent comments. To his remarks upon the use of *"op. cit."* I should like to add the comment that, much as uniformity is to be desired, clarity is even more important. There is something to be said for footnotes, since they allow the author to add important but casual information and content where a parenthesis or a parenthetical digression would break the main thought. Where references are to be given in footnotes, then it becomes obvious that they should be immediately available. For an author or editor to insist on uniformity with respect to *"op. cit."* means that often the most careful scrutiny of many preceding pages must be undertaken to find references. Plainly in such a case the reference ought to be repeated. On the other hand, the page which makes numerous references to the same articles should certainly not have the reference repeated upon it. There might be some rule, like a rule to repeat the reference every four pages and to use *"op. cit."* otherwise, but in general it seems to me better to let good judgment prevail over reason, and to decide in Ms. when the precise reference can easily be found and when it will be lost among others. My plea here is against arbitrary uniformity by authors or editors.

My other question concerns the place of the date in a citation. Leffmann's (February 26, 1926, p. 231) and one of Merrill's (November 6, 1925, p. 420) instances place the date separately from the volume and pages. It seems to me to be much better for the date