Thus the family Ranunculaceae is represented only by a supposed *Thalictrum,* so doubtful that it was not given a specific name. Since then I have found buttercup seeds in the Florinant shales (Miocene), but of the many species of Ranunculaceae which must have been the ancestors of the present flora, there is hardly a trace. The same may be said of numerous other families.

(3) The evolution of the modern flower must have been closely connected with the development of the flower-visiting insects, especially the bees and their relatives. Now the earliest known bees are from the Oligocene (Baltic amber) and the oldest sphecoid wasp is from the Green River Eocene. These Hymenoptera are by no means primitive types, but are essentially similar to those now living. They certainly had Mesozoic ancestors which have not been found.

(4) To this day, bees abound especially in dry regions, such as Arizona, Turkestan, Algeria or the karroo of South Africa. There may be found a prodigious number of species, together with a rich flora to which the bees are adapted. These can, however, be a rather varied flora without a variety of bees, as shown by the Pribilof Islands, which have only one kind of bee (*Bombus kincaidi*).

Thus it appears probable that the developing angiosperms, together with their developing insect visitors, occupied mainly upland and relatively dry regions, and if so, would rarely chance to appear as fossils. The soft, non-deciduous leaves would rarely be scattered on lake shores, and still more rarely would the remains, if preserved, be recognizable. The bees, if present, would be more likely to be recognizable, but they have not been found on the Mesozoic, although they must have existed.

If we assume that the early angiosperms were not woody, but herbaceous, and were at the same time largely confined to uplands or dry regions, it is easy to understand why they do not appear in the fossil record, or rather, are represented by few and more or less doubtful fragments.

I wrote briefly to Dr. Seward on this subject and he at once referred me to the most illuminating discussion in Chapter V of Mrs. Arber's work "The Gramineae" (1934). Here the problem of herbaceous vs. arborescent origins is discussed in the broadest and most lucid way, and it is shown that the assumption that woody types came first is by no means necessarily valid. Zoological evidence is added to the botanical, and the chapter is so full of original ideas that a brief summary is impossible.

Supposing it to be true that a rich herbaceous flora of angiosperms existed during the earlier part of the Mesozoic, how can its existence be demonstrated? Just as rich early fish-faunas have been revealed by the study of scales and otoliths, so it may well be that floras will be proved to have existed by the discovery of small seeds and pollen, and very likely also of calyces or sepals. No adequate search has been made for such objects in the rocks which might contain them. R. P. Wodehouse (1933) has described and illustrated the pollen of the oil shales of the Green River Eocene, and has shown that many species, belonging to thirty-four genera, could be recognized. He states, however, that herbaceous plants, apart from a few aquatics, are not represented. This statement must be qualified by another, that only about a third of the pollen species present has been described and identified. Wodehouse says, "The absence of terrestrial herbs is entirely in keeping with the theory put forward by E. W. Sinnott, that the herbaceous type was developed in temperate regions during Eocene time in response to a progressive refrigeration. At this period terrestrial types were only beginning to be developed." Any one who has considered the slowness of plant evolution, and the small amount of change in the insect fauna since early Tertiary times, can not readily believe that the great and varied herbaceaus flora of to-day had such a recent origin. A really full and adequate discussion of the problem might well require a book, but I venture to suggest that enough has been adduced to justify a minute examination of Jurassic, Triassic and even Permian lake-bed deposits, wherever they are suitably fine-grained, in the hope of finding small seeds or other remains, and perhaps especially pollen, representing an herbaceous flora of angiosperms.

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## THE MOTION OF GLACIERS

AFTER reading Dr. Chamberlin's objection (SCIENCE, December 7, 1934) to my contribution (SCIENCE, November 2, 1934) on "The Motion of Glaciers" he and I had an oral discussion of the topic. This reply (which will have been seen by Dr. Chamberlin before it is sent to the editor) will endeavor to make clear our differences. From it the reader should without further notices be able to come to his own conclusions in regard to the merit of the several contentions. It is of course to be realized that there is not space in SCIENCE for a complete review of the problem. The monograph by Hess which prompted the first notice covers that ground fully if not comprehensively.

From our discussion it developed that Dr. Chamberlin was of the impression that my piece was in some sense unfair. This because I used as tenets of the shear theorists formulations which originated with him or with T. C. Chamberlin and which he thought should not be so applied. I wish absolutely to disclaim any intention of putting any one in a false light. Further although, in discussion, Dr. Chamberlin said it is his own conviction that inter-grain shift is the chief process of glacier motion there remains a difference of interpretation of the phenomenon that is not cleared up by the apparent agreement in views this adherence suggests. The difficulty is that Dr. Chamberlin, as I understand him, and the shear-motion adherents, insists that glaciers be regarded as essentially rigid masses, whereas the other viewpoint is that the flow function of glaciers derives wholly from a condition of viscosity or plasticity of the glacier ice. "The results speak definitely for the solidity and elastic rigidity of moving glacier ice, and decisively against liquid or viscous flow as the main type of adjustment under stress."[1] It is difficult to see how such a statement can be regarded as any less "sweeping" than the one I made and to which Dr. Chamberlin objected.

The following paragraphs apply in rebuttal to Dr. Chamberlin's point-by-point protest.

(1) "What else besides sliding could cause striation, etc.?" Why, a stiff viscous body holding graving tools large and small. Glass is a viscous substance equally with fluid lava to which latter anti-viscous glacial flow adherents immediately turn for an analogy. Further, even if a glacier is regarded as a rigid crystalline body the fact of pressure-temperature melting equilibrium at its bottom would tend to bring about imbedding in the ice of any rock graving-tool because the projection of such a tool would induce differential pressure with relief by melting and refreezing. Theoretically, one ought to expect no damage to a polished metal surface from application to it of coarse carborundum flakes on the under side of a cake of beeswax.

(2) Intermittent slip. The fact of such slip in the upper cover, marginal and terminal zones of a glacier where the thickness of the ice is insufficient to develop true glacial, that is, viscous flow, is well known and was specifically referred to in the third paragraph of my original notice. But such motion is merely an induced result of the true glacial flow.

(3) Solid shearing of aggregates of granules. Like intermittent slip applies only to marginal and cover ice.

(4) Idiomolecular exchange. Although Dr. Chamberlin here maintains that he has all along upheld the concept that "movement between the granules . . . to be the fundamental mechanism" of glacier motion, such contention does not seem to be in keeping with the declaration made in his author's abstract quoted above. If he visualizes the idiomolecular exchange as a vast succession of dot and dash yieldings between dry crystals the process could perhaps be regarded as non-viscous yield. But it should be realized that the idiomolecular exchange serves primarily (perhaps exclusively) the growth of larger crystals at the expense of smaller ones. Such idiomolecular growth was fully demonstrated to occur under conditions of minimum static pressure and essential freedom from differential stresses by Emden.[2] Further, it is well known to chemists that large crystals immersed in solutions of their constituent material will rapidly consume any small crystals present, but that such idiomolecular transfer proceeds very slowly, if at all, under dry contact. Hence it is again the salt solution, the chief tenet of my concept, that functions ideally to promote the growth of the glacier crystals. However, to return to the main argument, such idiomolecular exchange is not a *modus operandi* of glacier motion. Whether made up of a few large crystals or a vast number of smaller crystals the volume of ice must remain the same. As "explained in various text-books" (one copying from another?) it is faintly implied that the growth of crystals and the melting and refreezing under changing conditions of pressure permits a settling down of the glacier by the closing up (with escape of air) of previously existing "pore" space. Such process may be transiently operative in the upper reaches and upper levels of a glacier but can not be invoked for the main action of glacier motion. On the other hand, without such free space there can be no motion by these means. Where could the melt water go?

Instead, as previously maintained, glaciers wallow down their courses, the crystals growing and shifting in relation to each other through the medium of the liquid salt solution which surrounds them. The total volume of such solution varies with pressure and temperature, its thickness between crystals becoming greater as the size of the crystals increases because of the diminishing surface area. Thus it is constantly being made adequate to its task as a lubricant.

Dr. Chamberlin further protests that I ought not lump the propositions (1) to (4) under the heading shear concept. But as I see it they are all used to bolster up the idea of glaciers as rigid elastic bodies and to provide the conditions which will result in shear as a basic means of glacier motion. Hence the grouping is appropriate.

It is not fitting to ask space in SCIENCE to build up the case for appreciable salt supplies in the precipitation that nourishes glaciers. Further, *ad interim,*

[1] R. T. Chamberlin, Abstract in *Geologisches Zentralblatt*, Vol. 37, 1928, No. 1337, p. 412.

[2] Robert Emden, *Denkschriften d. Schweiz. Naturf. Ges.*, 33, Zurich, 1892.

J. V. Harrison has himself found an explanation for the salt glaciers.[3]

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## A SYSTEM FOR SUBJECT REFERENCE FILES FOR SCIENTIFIC LITERATURE

IN the course of a recent investigation, which has necessitated reading a portion of the extensive literature relating to anaerobic bacteria, the authors have devised a simple system for the routine cross-indexing of topics covered in the scientific articles reviewed. It is believed that the system proposed may be applied with benefit for any field of science, either in listing current publications or as a basis upon which may be built a permanent bibliography for a particular field. The system is intended for personal use, and it will probably be found to work best if applied to a limited field, in which its user is himself working.

In setting up the system it is first necessary to outline carefully the field which is to be covered by the bibliography. A portion of the system which we have found useful in our field may be given in detail to more fully explain the key. For illustration, three general divisions of our present file will serve:

A. Source of isolation
  - a. Soil
  - b. Dairy products
  - c. Food products other than b
  - d. Intestinal tract or feces
  - e. Body other than d
  - f. Water
  - g. Miscellaneous

E. Products of metabolism (other than toxin, etc.)
  - a. Acids
    - 1. Acetic
    - 2. Butyric
    - 3. Lactic
    - 4. Propionic
    - 5. Others
  - b. Alcohols
    - 1. Butyl
    - 2. Ethyl
    - 3. Isopropyl
    - 4. Others
  - c. Acetone
  - d. Intermediates of fermentation
  - f. Gases ($CO_2$, $H_2$, $H_2S$, etc.)
  - g. Miscellaneous

G. Serological reactions
  - a. Agglutination
  - b. Precipitation
  - c. Complement fixation
  - d. Toxin antitoxin
  - e. Miscellaneous

The outline key, part of which is shown in Fig. 1, is printed in skeleton of letters and numerals on the lower half of unlined 4″ by 6″ index cards (we have found it useful to provide for expansion of the outline by extra divisions of each section). The right half of the card is lined for notes.

At the time the original article is reviewed a master card is made, giving complete citation of the author or authors, title of the article and reference. Check marks are then made on letters or numerals, which

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Welch, W. H.
Morbid conditions caused by *Bacillus aerogenes capsulatus*
Johns Hopkins Hosp. Bul. *11*: 185–204, 1900
A abcdefgh G abcdefgh ________________

E abcdefgh ________________
1234567
1234567 ________________

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FIG. 1. Showing a convenient arrangement of reference and part of the key.

indicate the topics considered. For each section so indicated on the master card a separate card is to be typed with author and title citation; these cards are marked to indicate only the section in which they are to be filed. Notes or short direct statements of results may be added to the appropriate subject card, if desired. Two files are then maintained, an author file of the master cards and a subject file of the cross index cards.

An advantage of the system is that it eliminates the need for routine briefing of articles. For the average paper, a quick reading or only checking of subject-matter is all that is necessary; it is thus possible to cover several papers or even volumes in one evening. It will be found also that the key is unconsciously memorized and that there is little trouble in checking off topics rapidly and accurately, particularly if the field is limited and is of major interest to the bibliographer himself. The system is a time-saving device, for once the master card is prepared and the correct number of subject cards indicated, ordinary stenographic help can be used to copy the reference to the cross cards. If extended to cover in a systematic fashion all the back volumes of journals containing pertinent papers, it becomes a permanent file from which at least two types of questions may be answered in a minimum of time. These are: (1) what topics are covered by a particular paper (the author of which is known), and (2) what articles deal with any one specialized topic? This latter use is possible only through the multiple filing, and that is possible through elimination of abstracting, always tedious and inadequate.

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## ELECTRODES COME IN PAIRS

FOR some years there has been evident on the part of physiologists a tendency to call a pair of electrodes an electrode. Perhaps the tide of this gross misuse of physical language has gained so much momentum that nothing can stop it, but every effort should be made to do so if possible.

[3] J. V. Harrison and N. L. Falcon, *Geological Magazine*, 71: 537, December, 1934.