by Bucher[2]; all major igneous masses; swells and basins in areas of unfolded rocks (possibly by structure contours); salt domes and anticlines; monoclinal folds.

Time relationships should be indicated, so far as practicable, by conventional patterns or colors. If conventions are chosen judiciously, they may be superposed in areas that have experienced repeated diastrophism. It may even be feasible to indicate important vertical movements in folded belts, such as the late uplift of the Appalachian region. Considerable ingenuity will be required to represent all the complex disturbances in some western areas, even where adequate information is available. It does not seem practicable to show the geologic dates of individual faults. However, faults that are recognized as "active" can be distinguished from those supposedly "dead"; and it may be desirable to indicate that faults in an important group are essentially contemporaneous. Thrust faults in a belt of folding involve no special problem.

Considering the scale of the map, there is danger of attempting to show too much detail in areas of complicated structure. It is better to present limited information clearly than to have parts of the map overloaded to the point of confusion. The degree of complexity beyond which the law of diminishing returns begins to operate can be determined for each part of the map only by careful experiment. Rigid rules should not be strictly applied to all areas; for example, structure contours may be used with profit in areas that have simple structure, but not in belts of complex folding.

The function of a memoir to accompany the map requires little explanation. Many facts and relationships that can not be represented adequately on the map should nevertheless be made available. Reasons for interpretations given by the map should be stated, and discussion of possible alternatives will be helpful. Numerous cross-sections to show critical structural relations will be invaluable to users of the map. Diagrams and large-scale maps can be used to elucidate the structure in limited key areas. If the memoir is prepared adequately, it will be fully as useful as the tectonic map itself; and the two will be indispensable parts of a unit treatise.

It was suggested recently by R. D. Reed that a tectonic map should be accompanied by a series of paleogeographic maps of a rather special kind, to represent the evolution of major tectonic units. Such maps would show not only the outlines of subsiding and rising areas in various geologic periods, but also thicknesses and important facies of deposits in basins of sedimentation. Symbols could be used to represent facies, and either contours or actual figures or both to indicate thicknesses. Maps of this kind for limited areas have been published. A comprehensive series for each of the major tectonic units in the United States would form a proper and valuable part of the memoir to go with a tectonic map.

The efforts of the committee will be justified only if the result is equipment that can be used to advantage by numerous students of tectonics. Exactly what kind of map do these students require? Specific suggestions offered at an early date, while plans are in the formative stage, will be most useful in furthering the project.

COMMITTEE ON TECTONICS,
CHESTER R. LONGWELL, *Chairman*
DEPARTMENT OF GEOLOGY,
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[2] "The Deformation of the Earth's Crust," by Walter H. Bucher. Princeton University Press, 1933.

"THE" SOUTH AND "THE" NORTH

IN a recent number of SCIENCE (September 28, 1934), under Science News, subtitle "Air Conditioning of Hospitals," there is an abstract of a paper from which the public learns that "in acute appendicitis the fatality rate is almost three times as high in the South as in the North"; further, that acute nephritis shows a "high death rate in the South and low death rate in the more stormy North." These assertions are attributed to Professor C. A. Mills, of the University of Cincinnati. They struck me because I am somewhat familiar with living in two places, central Missouri and the southern tip of Florida. I need not point out which is more south and more north by 800 miles.

During the summer just ended, in the "tropical" climatic region of the United States, the temperature rose on three days as high as 93°, confining itself on other summer days to the customary maximum of 90°. To tell the whole truth, however, on one day it reached the terrific (?) climax of 96°. Yet no death from "heat stroke" was reported in the papers, although many aged people live here. During the same weeks and months I received letters from my friends and relatives in Missouri, saying that they were sweltering under afternoon temperatures between 105° and 110°, and that on days when it rose only to 100°, they felt blest. Further, all the year around, it is nothing undreamed of in Missouri to have to stand an average of one day thirty degrees above or below that of the directly preceding day. Eight hundred miles south looking in the records for an analogous event amounting to only ten degrees difference meets with success but rarely.

"Patients should be protected from the heat waves of summer—and the year around from sudden changes in weather." True! But the truth is stultified by translating it into "The South." I assert

that "the" South and "the" North are no concepts of medical science.

MAX F. MEYER

CORAL GABLES, FLA.

A CRITICISM OF THE ARTICLE "ENZYMES, VITAMINS AND THE ZONE OF MAXIMUM COLLOIDALITY"

I FEEL obliged to write in protest against the article, "Enzymes, Vitamins and the Zone of Maximum Colloidality,"[1] by Dr. Jerome Alexander, published in SCIENCE last summer. The article discloses a lack of familiarity with enzymes. Alexander speaks of the possibility of decrease of enzyme activity through too great a degree of aggregation and then says: "On the other hand, too great a degree of dispersion of the enzyme might lead to a particulate kinetic activity so intense that the number of *successful* encounters between enzyme and reactants would be reduced to the level of inefficiency." This is an improbable supposition which has no experimental evidence to support it.

Alexander says that his views regarding the necessity for a maximum zone of colloidality for an enzyme are confirmed by an experiment where pepsin coagulates a suspension of denatured egg-white and where the coagulum is dispersed by adding hydrochloric acid. Here it seems to me that there is no connection between hypothesis and experiment.

He states: "Recent work of Professors Richard Kuhn, Otto Warburg and their collaborators indicates that the newly isolated water-soluble lyochromes, the flavines, which apparently constitute vitamin B_2, exhibit enzymic activity when brought into the colloidal state, presumably by fixation on a colloidal carrier." At the time of publication of Alexander's paper an *in vitro* formation of the yellow oxidation enzyme had not been demonstrated. Shortly afterward appeared a paper by Theorell[2] describing the crystallization of the enzyme, its inactivation through splitting into its pigment and protein components and its partial reformation upon adding the pigment to the protein.

JAMES B. SUMNER

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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

MINUTES OF THE EXECUTIVE COMMITTEE

THE meeting of the executive committee was held in the office of The Science Press, Grand Central Terminal, New York City, on October 21, 1934, with the following members present: Drs. Cattell, Compton, Conklin, Curtiss, Fox, Livingston, Thorndike, Ward, Wilson and Woods. Dr. Hildebrand was excused for absence.

(1) The minutes of the last meeting, held at Berkeley in June, had been approved by mail.

(2) The financial report of the treasurer was discussed, accepted and ordered audited.

(3) The executive committee expressed its appreciation of the valuable services rendered by the treasurer.

(4) The treasurer's budget was approved as adjusted.

(5) The permanent secretary's financial report was discussed, accepted and ordered audited.

(6) The budget of the permanent secretary's office was approved.

(7) The budget for the Pittsburgh exhibition, prepared by the director of exhibits, was approved.

(8) The permanent secretary's office was authorized to sell surplus copies of the 1929–1934 Summarized Proceedings volume to non-members at $4 per copy in paper cover, and $5 in cloth binding.

(9) The executive committee voted to express to the executive assistant its appreciation for the fine work done on the volume of Summarized Proceedings just issued.

(10) The permanent secretary reported on his conference with the local committee for the forthcoming Minneapolis meeting. The dates of June 24 to 29, 1935, were approved as suggested by the local committee. A Maiben lecture was authorized to be given by a prominent worker in medicine, if feasible. Other topics were suggested for general addresses. The permanent secretary announced that the Minnesota Medical Society would meet in connection with the association at Minneapolis, from June 24 to 26.

(11) The dates of the St. Louis meeting were fixed for December 30, 1935, to January 4, 1936.

(12) The quadrennial convocation of 1936 was definitely scheduled for Washington, D. C.

(13) A brief report was presented on meetings to be held at Rochester (June, 1936), Denver (June, 1937) and Indianapolis (December, 1937).

(14) The permanent secretary was authorized to investigate the matter of extending the list of future meeting places to include 1940. The committee favored especially a summer meeting for 1938 in Canada and the following winter meeting in a southeastern city. This item was made a special order for the Pittsburgh meeting.

(15) The executive committee reaffirmed the recom-

[1] Jerome Alexander, SCIENCE, 80: 79, 1934.

[2] Hugo Theorell, *Biochem. Zeit.*, 272: 155, 1934.