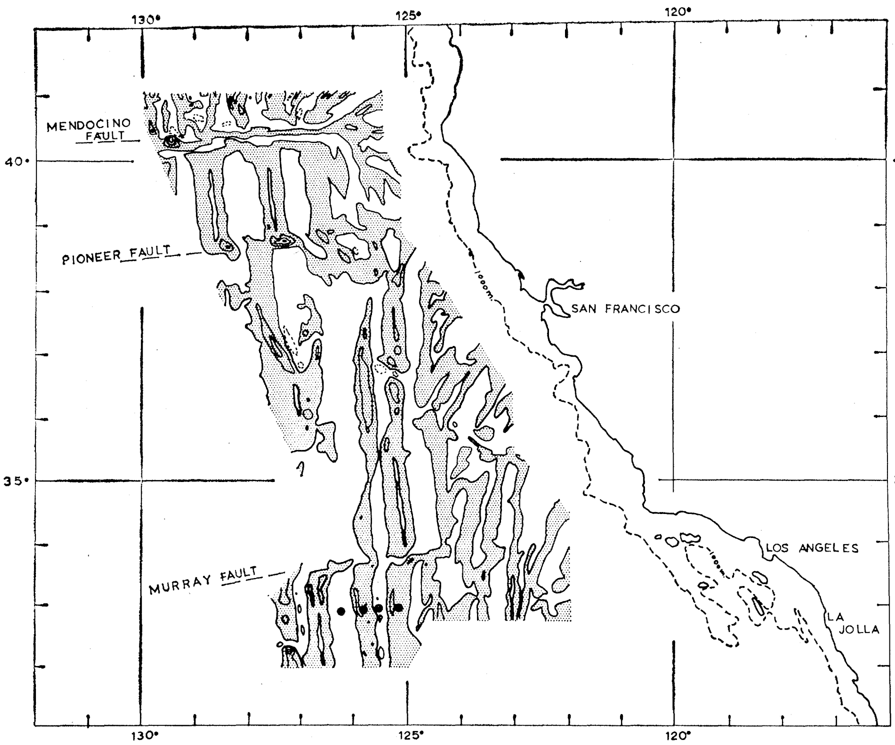

Fig. 1. Tentative sites for drilling the first four holes (solid circles). Stippling indicates areas of positive magnetic anomalies.

33°N, long. 127°W, about 650 km west of Los Angeles. These holes will be drilled to about 300 meters, the depth depending on the maximum which can be attained with one bit because hole reentry will not be feasible.

The drilling will test the hypothesis of ocean floor spreading (*1*) and the generation of long, linear, magnetic anomalies according to the Vine-Matthews hypothesis (*2*), relating them to spreading coupled with reversals of the earth's magnetic field. The main factual information to be sought will be (i) the age of the oldest sediments lying upon the harder rock of layer 2 of the oceanic crust; (ii) the age of the top of layer 2, presumably basalt, by radiometric age determination; and (iii) whether, on magnetic positive and magnetic negative residual anomalies, the remanent magnetization of the rock is normal and reversed, respectively.

The tentative geographic positions of the holes with respect to the magnetic anomalies are shown in Fig. 1. The data used in compiling it come from magnetic surveys (*3*) and from Menard's physiographic diagram of the area (*4*).

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References

1. H. H. Hess, "History of ocean basins," in *Petrologic Studies: A Volume to Honor A. F. Buddington*, A. E. J. Engel, H. L. James, B. F. Leonard, Eds. (Geological Society of America, New York, 1962), pp. 599–620; "Mid-oceanic ridges and tectonics of the sea-floor," in *Submarine Geology and Geophysics: Proceedings of the 17th Symposium of the Colston Research Society*, W. F. Whittard and R. Bradshaw, Eds. (Butterworths, London, 1965), pp. 317–332.
2. F. J. Vine and D. H. Matthews, *Nature* **199**, 947 (1963); F. J. Vine and J. T. Wilson, *Science* **150**, 485 (1965).
3. V. Vacquier, A. D. Raff, R. E. Warren, *Geol. Soc. Amer. Bull.* **72**, 1251 (1961); R. G. Mason and A. D. Raff, *ibid.*, p. 1259.
4. H. W. Menard, *Marine Geology of the Pacific* (McGraw-Hill, New York, 1964).

18 February 1966

Particle Sorting and Stone Migration Due to Frost Heave

Inglis (*1*) has recently discussed a mechanism for particle sorting and stone migration in soils due to freezing. His explanation is based on the expansion accompanying the freezing of soil water. He does not consider the effects of the motion of soil water through the soil during the freezing process, that is, the phenomenon known as frost heave. Stones, fence posts, and other objects can be lifted several inches in one thawing and freezing cycle by frost heave, whereas the normal expansion due to freezing of water could result in motion of only a fraction of an inch. The lifting occurs when the top part of the stone becomes imbedded in the frozen soil as it freezes from the top, as described by Inglis. Subsequent frost heave in the surrounding soil may lift the stone several inches. The stone is unlikely to resettle precisely into the hole where it was, and so net motion of up to several inches is possible. This phenomenon is particularly noticeable in many loamy New England fields, which yield an annual crop of stones.

Soils that heave severely have been called "frost-susceptible" and contain, according to the Casagrande criterion, (*2*) more than 3 percent of particles less than .02 mm in diameter. When these soils freeze, water is drawn to the freezing front, so that the frozen soil contains much more water (in the form of ice) than the unfrozen soil (*3*). Volume changes of 300 percent on freezing are not unusual. The excess water in the frozen soil is in the form of layers of ice, called ice lenses, which are almost free of soil particles. The measured heave of a soil is equal to the total thickness of such ice lenses (*4*).

An essential feature of the frost-heave process is the existence of a layer of water separating the soil particles from the ice in the soil. The frozen soil rides on this thin (~ 10 Å) layer, being fed water from the ground-water table as the ice freezes (*5*). The energy for the process comes from the free energy of the undercooled water in the soil near the ice lens. In the usual case, ice cannot propagate between the soil particles because of capillary action on the ice-water interface (*6*). When some of the water in the water layer between the soil particles and the ice freezes, the thickness of the water layer being thereby decreased, tension is created in the soil water (due to repulsion between the ice and particle), which draws water up from the ground-water table.

The theory of frost heave as developed by Jackson, Uhlmann, and Chalmers (*5*), based on the ideas outlined above, provides quantitative agreement with experiments on frost heave (*4*).

In addition to the migration of stones, there should also be a sorting action on fine soil particles (*7*). Frost action should result in a preferential sifting of the finest particles downwards through the soil, because the smallest particles are more easily moved by the ice interface. This movement will of course be impeded by mechanical constraints on the small particles, which reduce the relative motions of the various particles. There are thus two distinct sorting processes at work during freezing of a soil: (i) the motion of stones and large objects as described above; and (ii) the direct sorting action of the ice-water interface on small particles. Both these processes tend to sort the largest particles to the surface and the finest particles deeper into the soil.

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References

1. D. R. Inglis, *Science* **148**, 1616 (1965).
2. A. Casagrande, *Highway Res. Board Proc.* **11**, 168 (1932).
3. S. Taber, *J. Geol.* **38**, 303 (1930).
4. G. Beskow, *Yearbook Swed. Geol. Soc., 26th, No. 3, Ser. C* (1935), J. O. Osterberg, Transl. (Northwestern Univ., Technol. Inst., Evanston, Ill., 1947), p. 375.
5. K. A. Jackson, D. R. Uhlmann, B. Chalmers, *J. Appl. Phys.*, in press.
6. K. A. Jackson and B. Chalmers, *ibid.* **29**, 1178 (1958).
7. D. R. Uhlmann, B. Chalmers, K. A. Jackson, *ibid.* **35**, 2986 (1965).

15 February 1966

Vegetational Continuum

Daubenmire's criticism of the "continuum viewpoint" ["Vegetation: Identification of typal communities," *Science* **151**, 291 (1966)] is a result of misunderstanding the late J. T. Curtis's concept of the vegetational continuum. In *The Vegetation of Wisconsin* (Univ. of Wisconsin Press, Madison, 1959) Curtis says, "The entire series of communities whose floristic composition gradually changes along an environmental gradient has been termed a 'vegetational continuum', to emphasize the fact that no discrete divisions, entities, or other natural discontinuities are present." He goes on to say, "It must not be assumed that this gradual blending of one community into another or one vegetation type into another is always expressed in the field. On the contrary, there are many examples of abrupt shifts from one assemblage to another, sometimes along a line so sharp that it may be crossed at a single step." These statements clearly dismiss the paradox that Daubenmire thinks exists.

As a student of Curtis, I interpret these statements to mean that plants or groups of plants vary continuously in time and in space, but *not necessarily in space at a given time.* Time is defined as actual time or theoretical successional time; the concept is thus a corollary to the theory of succession and has implications in taxonomy and evolution. The continuum viewpoint is founded in the principles of biological variability and the amplitude of tolerance. These principles are that no two species, and no two individuals of the same species (unless genetically identical), have the same amplitudes of tolerance.

The continuum and its relation to succession can be illustrated by hydrarch succession, where the vegetation varies gradually and continuously as a site progresses from hydric to mesic. One plant assemblage blends into another, and it is difficult and often arbitrary to decide the precise moment in time when a sedge-shrub association becomes a forest community. The continuum concept eliminates the need for this decision.

But not all communities are seral; many have reached a climax, a terminus of stability, or a state of quasi-equilibrium. Other communities are retrogressive as a result of catastrophe. But even in these communities, individual plants vary continuously in time, as one individual replaces another. Only abrupt, catastrophic breaks in succession result in breaks in the time continuum.

The discontinuities cited in Daubenmire's article do not refute the continuum concept. Curtis acknowledged discontinuities in space or on the land, and he and his students have used the community approach in vegetation classification. Part of the confusion and irritation concerning the continuum has resulted from publications by "continuum champions" that glorify the special methods of processing data arithmetically (indices, ordinations, 3-D diagrams), making them the focal point of their work, rather than using them in proper perspective as tools (as Curtis intended) in community classification. Daubenmire's specific complaints (concerning shortcomings in sampling methods, in the treatment of data, and so on) should be lodged not against the continuum concept but against individual investigators. These faults are by no means restricted to "continuum champions." It would be more profitable if adversaries of the continuum concept directed their attack to the theory of succession itself, to reconcile inconsistencies existing between that theory and what actually takes place in many plant communities.

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. . . If "closely similar plant assemblages" which can be combined to form types with "consistent distinguishing characters" exist, as Daubenmire asserts, why then the "spectrum of concepts, terms and methods so broad as to discourage the novice and confuse even the specialist"? The facts are that the consistent, distinguishing characters needed to recognize an association type vanish upon close examination. As Daubenmire says, a "century of development" of the type concept of vegetation has produced little trend toward standardization of "methods of analysis nor of organizing the subsequent data." The consequent state of confusion, to which Daubenmire alludes, casts doubt upon the typological approach and is a primary reason that a different viewpoint on vegetation was called for.

A major methodological difference between Daubenmire's "continuum champions" and his "hundreds of workers" who support the typological view of vegetation is the emphasis by the former upon methods of quantitative analysis; Daubenmire's comment that the results are "more satisfying to a mathematician than to a botanist" is simply rhetoric. The simple arithmetic methods of the original continuum [Curtis and McIntosh, *Ecology* **32**, 476 (1951)] and the more involved techniques of Bray and Curtis [*Ecol. Monographs* **27**, 325 (1957)] will probably prove inadequate and be replaced by more refined mathematical methods. But it is most unlikely that the "vast array of environmental gradients operating concurrently, in Daubenmire's apt phrase, will be amenable to the subjective methods which have characterized the traditional approach to vegetation studies exemplified in Daubenmire's article. His own reduction of the "vast array" to a single factor, operating independently, is a simplification unsupported even in his geographical area by the limited data he presents.

Proponents of the typological point of view believe that they can recognize