not impossible to find with certainty, in practice, we probably have an approach to it in cultures of a given organism at different ages. In an older culture competition for nutrition is keener and one might expect therefore the greatest enzyme content per cell. Experimental results indicate that old cells are in fact more resistant than young ones. In the case of spores the situation is different. The freshly formed spore contains its maximum supply of enzyme. Being in a resting condition it can generate no more, so that the effect of time would result only in a decrease of that already present. Experiments carried out by several workers indicate that old spores are less resistant than young ones.

There are other facts, besides those which we have considered, which may be explained in terms of the theory. Thus, the germicidal action of substances which lower surface tension may be pictured as due to a washing off of enzyme from the bacterial cell. Such also may be the effect of physiological saline solution. The problem of salt solutions is a complicated one, however. Differences in susceptibility depend on the kind of organism and the concentration and kind of surrounding ion.

It is a curious fact that Duclaux, although regarding enzymes as the basis of cell life, explained disinfection on a different theory. He assumed that in the case of heat, at least, a coagulation of the proteins of the bacterial cell was responsible for death. He based his theory on the observations of Marshal Ward on the *Bacillus rhamosus*. Ward had noted an apparent coagulation in the body of this organism when it was exposed to concentrated sunlight. A similar explanation of disinfection has been developed by Chick (1910) and more recently by Bancroft and Richter. Such a theory and the enzyme theory are by no means mutually exclusive, for most enzymes are either protein themselves or so closely associated with protein that they coagulate in the presence of those agents which react with proteins. The two theories differ chiefly in that one pictures the protoplasm of the organism as resistant, the other as labile.

The coagulation of the bacterial cell proteins affords in itself an adequate explanation of many of the facts observed in connection with disinfection. There are several phenomena, however, which are more easily accounted for by the theory of enzyme susceptibility. Some of these may be briefly mentioned. (1) Certain salt solutions such as physiological saline are immediately injurious or germicidal for some organisms under conditions which probably do not involve coagulation. (2) The results obtained with the acid-fast organism described above can hardly be explained on the basis of coagulation. (3) The greater resistance of young spores is difficult of interpretation in terms of coagulability. One would expect, if anything, a greater resistance in old spores.

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THE COTTON ROOT ROT FUNGUS INDIGENOUS IN ARIZONA DESERTS

INVESTIGATORS who have been studying the cotton root-rot disease in Texas and Arizona have considered for several years that the fungus (*Phymatotrichum omnivorum*) was indigenous in the areas where the disease is prevalent. The occurrence of the fungus on the roots of certain native plants in the vicinity of cultivated areas, and the immediate appearance of the disease on susceptible crops, when planted on virgin lands, has been reported by Peltier, King and Sampson, Taubenhaus, Dana and Wolff, and by King and Loomis. At the root-rot conference at College Station, Texas, in January, 1931, Wolff reported the finding of infected wild plants and sclerotia on a railroad right of way that had not been under cultivation for forty or fifty years.

The writers have been studying the disease at Sacaton, Arizona, and have made it a practice on field trips through the desert to look for indications of the fungus on the native plants. Large areas of desert land are being brought into cultivation under several new irrigation projects in Arizona and California, so that it would be desirable to detect the presence of the disease before the expensive procedures of clearing, leveling and planting are undertaken.

On August 11, 1931, two of the writers observed extensive patches of spore mats of the root-rot fungus along the roadside in a desert area twelve miles north of Florence, Arizona, on U. S. Highway No. 80. These occurred intermittently over a distance of two miles on the vertical bank of the drainage channel made by the road grader in elevating the road bed. All the mats were located on the east side of the road, which was more shaded than the west, and which was still moist on the surface from recent rains. A profuse desert vegetation existed in this area consisting largely of *Covillea tridentata, Prosopis velutina, Franseria deltoidea, Opuntia fulgida, Condalia lycioides canescens, Sphaeralcea ambigua, Chamaesyce albomarginata, Aplopappus heterophyllus,* and various quick-maturing annuals. The nearest cultivated fields were about twelve miles distant, and the area, which was a high level plain near the mountains, was far separated from any of the general drainage water channels of the region.

On returning to the root-rot area three days later,

spore mats were found not only along the highway, but in several places on the shaded banks of a small wash for a distance of a fourth mile from the road. Other spore mats were found beneath the shade of mesquite and Condalia trees and Franseria plants. Several native plants were excavated, and on the roots of some of them were found typical strands of the Phymatotrichum fungus. The roots of three species, *Chamaeesyce albomarginata, Aplopappus heterophyllus* and *Sphaeralcea ambigua,* showed evidence of definite injury by the fungus, but no lesions or rotted tissues were observed on the roots of *Prosopis velutina, Condalia lycioides canescens* and *Opuntia arbuscula,* on which the mycelium also occurred.

Samples of soil about one-half cubic foot in quantity were obtained from three areas near the patches of spore mats. These were washed in a set of sieves, and the coarser materials left on the sieves were examined for the presence of root-rot sclerotia. In one of the samples obtained from the roadside two sclerotia were found. When tested for viability in the laboratory one of them germinated.

This observation seems to afford definite evidence that the fungus is indigenous in virgin lands in the Southwest, and explains the occasional occurrence of the disease in the first cultivated crops that are planted after clearing the native vegetation.

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IS EVOLUTION ONLY A DETAIL IN THE DYNAMICS OF POPULATIONS?[1]

IN reading a recent copy of SCIENCE (June 26, 1931) I was much interested in certain rather naïvely dogmatic statements anent population growth, embodied in a paper by Professor W. H. Longley, entitled, "Evolution a Detail in the Dynamics of Populations."

It seems to me, without wishing on my own part to be too dogmatic, that certain of the statements in this article can be shown to be incorrect.

For example, the paper states that "the struggle for existence proceeds with an order as definitely predictable in its outcome as that manifested in a gaseous system under the terms of kinetic theory." It is my contention that such is the case if, and only if, the individuals of all the species concerned act as independent entities in the matter of decisions involving subsequent actions, in which case the unpredictable actions of individuals can be treated in terms of statistical "mean actions." If, on the other hand, any considerable group of individuals, whether all of one species or not, are sufficiently advanced mentally to "put their heads together" and make a group decision, the nature of this decision will not be *à priori* determinable, hence, if it has any effect on the population growth, the rate and nature of that growth will not be predictable on a mechanistic basis.

Reading further we encounter the statement: "but populations may not so increase [according to the logistic S curve, the brackets are mine] unless they are in fact sensitive to fractional increases in population pressure, as the formula requires." Is this not an example of the not uncommon fallacy of thinking that a "fitted equation" is necessarily a true expression of the real mechanism of a reaction? I can assert from actual experience with "mechanistic" mathematical analyses of insect population growths that an S-shaped curve can seldom be adequately explained on such simple grounds.

As to the further statement, that "it is astonishing, but a fact, nevertheless, that the measure of difficulty the generation of 1790 had in rearing its children in America should have gauged accurately the ultimate and largely unutilized resources of so great a country," it is certainly very surprising, if true, but I believe it can be shown to be incorrect on mathematical grounds.

It can further be shown by mathematical analysis that the exhaustion of resources is by no means the only factor capable of permanent alteration in a population curve. To state that the exhaustion of resources is such a sole factor is to say that if N be the number of individuals in a population at time T, and R the amount of as yet unutilized resources, then N is a function only of R, and so we may write $\frac{dN}{dR}$ as a total derivative, an hypothesis hardly likely to lead to any very complete insight into the mechanism of growth of a population.

Again a change in the form of activity of a population may do far more than change the limiting value; it may change the whole form of the curve, so that, instead of approaching some limiting value as an asymptote, it commences to oscillate between two limits, with constant amplitude. It may even exhibit a damped fluctuation.

It seems to me also that the statement that the effect of the varying rate of immigration into the United States has been nil is erroneous. It has certainly had an effect, but one would hardly expect to detect it by the rather crude method of comparison with a purely empirical curve, especially as the variations are at best small in comparison with the total growth in population from census to census.

[1] Published with the approval of the director as Miscellaneous Paper No. 10 of the Experiment Station of the Association of Hawaiian Pineapple Canners, University of Hawaii.