SCIENCE

FRIDAY, DECEMBER 16, 1887.

THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH evidently finds difficulty in securing haunted houses to be submitted to their searching investigations. Professor Royce, who is the chairman of the committee on apparitions and haunted houses, jocularly referred to this difficulty in his report at the recent meeting of the society in Boston. As Professor Royce said, the name, suggesting as it does that the time of the committee is mainly spent in visiting haunted houses and ghost-ridden graveyards, does not describe its actual office. The committee has often expressed its willingness to visit haunted houses, or to pass the night in any promising place, for the sake of seeing, explaining, or of converting from the error of its ways, any genuine ghost in the city or in the neighborhood of Boston. The committee has heard of several houses that once were believed to be haunted, but in no case has the present condition of these houses warranted any interference on the committee's part. The phenomena have in all cases so far reported ceased for some time, usually for many years. A more hopeful field is in the direction of tracing some coincidence between a dream or presentiment and its supposed verification by events afterwards, but even in this direction the results are so scattering as hardly to justify the belief in any special significance in the few coincidences which have been traced.

AMERICAN PUBLIC HEALTH ASSOCIATION.[1]

ONE of the most original papers presented at the recent meeting of the American Public Health Association was by Dr. E. M. Hunt, secretary of the New Jersey State Board of Health, on the origin of some diseases. It was as follows: —

[PAPER BY DR. E. M. HUNT.]

The class of diseases variously known as contagious, transmissible, or communicable has ever attracted the most earnest consideration of medical men and of all sanitarians. By the quickness and often obscurity of their invasion, by the malignant type they too often exhibit, and by the large areas over which they extend, they not only make large demands upon the skill of the profession, but arrest the attention of all mankind.

The study of epidemiology has enlarged their numbers, and now shows us that many diseases once regarded as constitutional or septic are in reality specific and pathogenic or mostly parasitic.

The prevention and limitation of this class of maladies must ever, therefore, largely occupy the attention of all of those who study the causes, the courses, and the results of disease.

The study of their etiology is always a fundamental inquiry. While to a degree it is possible to treat a disease skilfully without knowing its causes, it is always more satisfactory and generally more skilful to know something of the causes.

But what do we mean by etiology or causes? Surely not always, not generally, the beginning or efficient or final cause. Professor Semmola of Naples, at the recent meeting of the Medical Congress at Washington, may have startled some by these words: "Medicine, like all other sciences, never demands why, well knowing that the first causes of things are inaccessible, and that to every scientist it should suffice to know in which physical and chemical conditions this or that phenomenon manifests itself, so that he can modify and govern it at his will." This is true. The best that we can generally hope is to find the conditions, physical, chemical, or biological, in which phenomena manifest themselves. The word 'cause' is often in the same sentence used in two or three different senses. In our etiology we must remember that by 'cause' we mean mostly the 'conditions under which phenomena manifest themselves;' also that these conditions (thus called causes) mean the modifying influences present in the host or person, and the modifying influences of surroundings, much oftener than they mean any thing in the specific entity which we are so often calling the germ, and then calling it the cause.

While reaching back toward the beginning, even though we seldom reach the starting-point, we do come to see how it is true of every disease, as of every living thing, that it must have had a beginning. It is not a mere platitude to say that there was a day when the first case of small-pox occurred.

Nor does it necessarily belong to the sphere either of creation or spontaneous generation. A case of some new disease may put in an appearance, as did cholera on the Ganges, or a case of a known and existing disease may happen that is not derived from another precisely like it, but occurs because analogous conditions to those which gave rise to the first case of the kind have again occurred, and produced a pronounced deviation.

So it is possible that at the same time, as to some one disease, we may have cases of a communicable disease which have arisen from previous cases, and other cases that have resulted from a combination of influences or conditions such as gave rise to the first case. There are at least two reasons why the old dictum of *omne vivum ab ovo* cannot now be applied with the precision or with the finality with which it used to be quoted.

I. There is such a thing as evolution, which, while recognizing an original type, also recognizes departures from the normal which may have come to be so representative and paramount as to constitute newness in all essential particulars. Since we have come to recognize that many diseases are but developments and cultures of microphytic or microbic life, we very appropriately turn to the facts of botany, not only for illustration, but for verification of our theories. And what a change has taken place in its facts since the days of Linnæus! We no longer cling to the divisions of orders, genera, and species so closely laid down by him. We recognize two forces, — nature or heredity, and environment. A plant inherits a likeness which it tends to retain, but it is often so modified by environment as greatly to change, and so sometimes as even to lose, its identity. Environment comes to predominate over heredity. The horticulturist often takes a plant which he has found to be subject to variations, and fixes and perfects it in some one of them by cultivation.

Professor Huxley has recently contributed to the Linnæan Society a paper on the classification of gentians, in which he claims that gentians are all specialized; that is, become gentians from some other form. Permanency of type has so many exceptions, that variations of type, and the power to give fixity to some of these variations by means of cultivation or environment, must be accepted as a doctrine and a fact. Species and genera have variations, sports, modifications not dreamed of by the earlier botanists. Some of these departures are so marked and so predominant as to obscure the relationship and so far ignore it as to have individuality of their own. If, as we know, cultivation or surroundings can change a poisonous plant into a mild one, or can wholly modify it, it is not remarkable that a microphytic disease should lose its apparent identity, and at length in a new culture-medium, or under special conditions, become specialized. It is a law abundantly illustrated in the vegetable world, that environment causes variations, and that some of these variations tend to fixity of type, while others do not. All the wonderful facts of evolution show full well that we may in this way have what in respect of symptoms and treatment is a new disease. Yet it is not a *de novo* origin in an absolute sense, or, if practically *de novo*, it is not *de nihilo*. It is, that a

[1] Continued from *Science* of Dec. 9.

series of changes has been evolved by environment, by conditions in persons or in surroundings, until the result of some of these changes becomes self-assertive, and prevails over its heredity so as to secure for a longer or shorter period a fixity of its own. This fixity may be a new disease.

II. The history of hybrids, the so-called accidents of their occurrence, and the fertility of hybrids, are such that we are forced to look upon them as new forms of life — as becoming established into an autonomy or individuality of their own.

The horticulturist in some of the wonderful productions of new plants in the last score of years has come to be familiar with hybridisms that wholly obscure origin, and that come to have a fixity of their own as much as some new diseases with which we have to deal, or as much as cases of disease which we cannot trace to a contagious source.

All this has been greatly emphasized by what we have come to know as the fertility of hybrids. Not over thirty years ago the general view of botanists was that hybrids were sterile. But so numerous are the exceptions, and so abundant have been the results of new cultivations, that this view is fully disproven. Says a recent botanical authority, "Among plants there are many instances of hybrids between species being perfectly fertile, and continuing so for an indefinite period. Experiments during the last twenty-five years have increased the number of such fertile crosses manifold. . . . Fertility of hybrid plants is the rule, and sterility the exception. So far as plants are concerned, there is not the slightest ground for considering sterility as a distinctive bar, separating species. These hybrids come to have a specificity of their own so different from the parentage as to be unrecognizable, and so specialized as to be permanent." As another expresses it, "The hybrid becomes an individual not responsible to its species."

Nor is this confined to plants with spores. Some of our most skilful horticulturists are now producing varieties of ferns by hybridism, and every now and then some so-called chance growth or sport shows wide departures.

The bearing of all this on the parasitic forms of disease is not far to seek. If, as now seems so nearly proven, so much of disease has to do with elementary and minute forms of vegetative life, it is easy to see how the facts of evolution and hybridism have a bearing upon the appearance and propagation of disease. The light of the botanical world and its marvellous revelations as to the actualities and possibilities of the origination of new forms distinct from the parentage, penetrates the hidden sphere of disease-origin, and shows how some diseases cease, how others arise, how some lapse back to their heredity, while others are made permanent or specialized by their environment, and how others still are hybridized into specific forms, and acquire a fertility and fixity of their own. This range of deviation is so wide as to account for very many of the anomalies of disease, and for the organization, cessation, or modification of form.

In reply to my inquiry on this particular point, Mr. Mehan, the distinguished botanist of Germantown, Penn., writes me, "All hybrids, that we know by actual experiment *are* hybrids, are fertile as far as I know, and reproduce their originals just the same as if they were 'original' species. I think almost every botanist of note believes in abundant, fertile hybrids in nature. Coming down nearer to your own line of thought, I believe it is conceded that all lichens are hybrids between fungi and algæ. It is tenable that new forms of disease are continually coming into existence, which can only arise from new forms of disease-producing plants (microscopic fungi) being evolved from older species; but I do not know whether there is an opinion that these new forms are the result of hybridization or of that natural law of change which seems to be a constituent part of existence — in vegetation, at least."

In a paper read by Dr. M. W. Taylor before the Epidemiological Society of London, April 13, 1887, he notes the fact that many of the fungoid varieties were but "conversions of elementary states of pencillium and oïdium. It is also maintained by Zopf (the botanist) that there may be a pleomorphism amid pathogenic micro-organisms, and that there are stages of intermediate forms resulting from the nature of the nutrient media" (see *Lancet*, May 7, 1887, pp. 933, 934).

The view is, we believe, gaining ground, that harmless microphytes may become pathogenic, and that the different forms of micro-organisms present have relation to each other, and that the culture-medium in disease, viz., the person and his surroundings, determines the character of the micro-organisms full as much as the micro-organisms determine the character of the disease.

If the views as to the microphytic origin of most of the communicable diseases are correct, the study of the laws of this evolution and hybridism is vital. We believe it is in this direction that we are to account for the origin of new diseases, or for such variations in type as obscure or destroy identity. If we can, through this study, arrive at the evidence that in this sense many diseases begin, we have a new department of study, in that we are called upon to define with accuracy how and why this origin takes place, in order that we may thwart or circumvent the conditions.

If it is the result of evolution through a long series of changes, it behooves us to study the normal, and to watch and record all the gradations by which the unfriendly result is attained, so that, at some stage, we may intercept the progressive and threatening changes that are occurring, or ascertain what condition of the person, or what condition of surroundings, constitutes the influence which brings about the change, or provides the fertilizing medium for the disease, and causes it to break forth. If it is the result of a hybridism which occurs spontaneously or rapidly, we need to study precisely what forms of vegetative life thus incline to coalesce, under what condition the union occurs, and how their conjunction, development, and fertility are to be interrupted.

If special conditions of some parts, — as the throat, for instance, — or certain conditions of the secretions, furnish a special soil or culture-fluid for the propagation of low forms of vegetative life, this is to be studied with exactness.

In each of these lines the same method of technical study and close record and analysis of facts by competent observers which has prevailed in the study of minute plant-life by botanists, and which has obtained in many other sciences, will, in this comparatively new field of biological and botanical research, accomplish equally valuable results. In it we are attempting to find out how much and under what circumstances micro-organisms imperil human life.

The practical value of such an inquiry is apparent, for sooner than is the case with most of the studies of nature, the results will be applicable in the prevention and treatment of disease. It will be a great gain if we can come to know, that either under the laws of evolution, or as the result of admixture or hybridism, symptoms and pathological effects become specialized so as to constitute a new disease which maintains its type.

As examples of how proximity of different diseases may modify symptoms, we have many suggestive facts in the history of disease. Yellow-fever is believed by many to be a mongrel, born on the high seas by admixture of the jungle-fever of Africa with the typhus of the pent-up hold of the filthy vessel. It is not certain that typhoid-fever was not once nearer to typhus, until it came to be called abdominal typhus, and then to have modifications because surroundings and the acquired power of self-propagation gave it an autonomy of its own.

It is not even now certain that there are not grades of cesspool and other adynamic fevers that will some day declare another well-marked departure from what we now call typhoid, and come to have an individuality of their own. It is not certain, that, when Sydenham treated scarlet-fever and measles as one disease, their lines of difference were as well marked as now.

Diphtheria so often seems to have a localized origin, and common forms of sore throat are so often seen to pass away from their general into a special type, that it will not be strange if we can come to the law of departure (see views expressed in the *Sanitary Record* of Aug. 15, 1887, p. 88, and the contribution of Dr. Wordin, in the *Connecticut State Health Report for* 1887, that diphtheria results from the special virulence of a micrococcus which is not specific, put present in forms of foul or septic sore throat).

While typho-malarial fever has no pathognomonic lesion to distinguish it from the ordinary typhoid, yet we do know it has symptoms to distinguish it. The advances of biological investigation have put us in regions of new possibilities, that do not involve spontaneous generation, but yet do render probable what is equiva-

lent to the *de novo* origin of cases of disease which afterward are chiefly communicated by the first and succeeding cases.

Having settled that such origins do take place, we shall then pry into the secret of the laws of combinations and the conditions which favor the evil evolution or the facts of hybridism, and seek to combat these by starting similar processes in opposite directions, or by sterilization, neutralization, disinfection, and all the details of radical sanitation.

Such a view of the occurrence of old or new diseases, and of the reasons for fixity in some and changing forms and types in others, leads to several practical results.

1. The study of parasites, or germs, as they are called, is only one of the methods of informing ourselves as to the phenomena of disease, and in itself is not likely to be the key to rational and successful treament.

2. Our attention should be directed, far more than now, to the study of conditions and circumstances under which new forms appear; to the influence of persons and surroundings, instead of to the mere finding of a specific form. The latter would, of course, be most valuable as one of the facts in the chain of evidence, but we should not, as now, seek so much to look to it as the cause of disease as to inquire what conditions have caused this or that particular microphyte to be present.

3. We should be able to account for the occasional occurrence of a disease independent of any previous case, and for changing types of disease and new diseases, and would come to treat diseases less by their names and more in view of their type and the effect of surroundings upon them.

4. The tendency of all this is to magnify the importance of close observation, and to lead us to feel that success in warding off disease, and in treating it when it appears, depends mostly upon close observation and that experience which is derived from actual practice.

If we are looking to the biological laboratory for the natural history of disease, or to the chemical laboratory for the application of remedies, we shall surely fail. It is not so much that we need to find the specific germ or the specific methods. The world is always looking after specifics. But the science and art of sanitation has far more to expect from a study of the conditions of persons and surroundings under which diseases, or types and modifications of disease, manifest themselves, as also from a study of the prevention or obliteration of such conditions, than it has to expect from the finding of microphytes as the source of disease, and seeking to cure disease by expelling micro-organisms or attenuating them.

Our only design in this paper is to awaken inquiry as to modes of accounting for the localized origin of disease, without any antecedent case, on the proposition that the laws of evolution, environment, hybridism, or modification by culture, give rise to diseases so different from their prototypes as to have individuality and permanency of their own.

Because such inquiry is relevant to prevention, there is good reason to believe that by ascertaining the laws of these transformations and modifications of type and of the origination of special varieties, we shall ere long find new means for the prevention or limitation of many diseases.

'The Malarial Germ of Laveran' was the title of a paper read by Dr. William T. Councilman of Baltimore. He considers that this organism probably belongs to the *Protozoa*, a group of unicellular organisms noted for the varied changes of form which the individual examples undergo in the course of development. Of the malarial germ there are ten more or less distinct forms, of which five are always found in intermittent-fever. During the chill of the fever a definite form is seen, in which multiplication takes place by segmentation. One form has actively moving filaments. This was found in blood taken from the spleen in ten cases of malarial cachexia, and in five cases of intermittent-fever. Dr. Councilman says that too much importance cannot be assigned to this organism as a means of making a differential diagnosis between malarial-fever and typhoid-fever. In outbreaks of fever which occur in small country-towns, where it is of the greatest importance that the character of the disease should be recognized promptly, the advantage of this mode of diagnosis is most evident. There is too much reason for believing that in localities where malarial-fevers prevail, epidemics of typhoid-fever are frequently mistaken for fevers of a malarial type.

Mr. H. Lomb of Rochester offered prizes of $500 and $200 for the best essays on practical sanitary and economic cooking adapted for persons of moderate and small means. Dr. LaBerge, health-officer of Montreal, described the system employed in that city for the collection of garbage, and for its destruction by the Mann furnace. Committees were appointed on State boards of health, pollution of water-supply, disposal of garbage, animal diseases and animal food, forms of statistics, incorporation, protective inoculation, Lomb prize essays, national health legislation, and improvement of the sanitary and medical service on emigrant ships. It was decided to hold the next meeting of the association at Milwaukee. The following officers were elected for the ensuing year: president, Dr. Charles N. Hewitt, Red Wing, Minn.; vice-presidents, Drs. G. B. Thornton, Memphis, and Joseph Holt, New Orleans; executive committee, Drs. Henry B. Baker, Michigan, S. H. Durgin, Massachusetts, and J. N. McCormack, Kentucky. The secretary, Irving A. Watson, M.D., of Concord, N.H., holds over.

THE SURFACE-TEMPERATURES OF THE OCEANS.

A NUMBER of researches on the surface-temperatures of the oceans, which have recently been published, throw a new light on this complicated phenomenon. The maps accompanying the present number of *Science* have been constructed according to Dr. O. Krümmel's maps, showing the surface-temperatures of the oceans. As the Arctic Ocean must be considered part of the Atlantic, of which it forms the most northern extremity, it was desirable to include it in the map. Besides this, the Antarctic Ocean exerts a great influence upon the southern part of the Atlantic Ocean, and therefore the latter has also been included in the map, which shows two-thirds of the earth's surface in a perspective projection. The lateral parts, however, have been left off, as they do not belong to the system of the Atlantic Ocean. The Pacific Ocean has been constructed in the same way, the map extending from its northern limits to the entrance of the Atlantic Ocean. The latter map makes it very clear that the Pacific Ocean forms a comparatively well-defined basin connected by narrow straits with the basin of the Atlantic and Indian Oceans. Its southern limit is indicated by the east coast of Australia, Wilkes Land, Graham Land, and the southern portion of America.

A glance at the lines showing the surface-temperatures of the oceans reveals the remarkable fact that the warm water is accumulated in the western parts of the oceans. Krümmel designates water of more than 24° C. (75° F.) as 'tropical water.' In August the belt of such water is 21 degrees of latitude wide in the eastern part of the Atlantic, while it occupies 61 on its western side. In February it is 22 degrees wide in the eastern part, while it is 56 degrees wide in the western. In the Pacific Ocean it does not occupy more than 17 degrees in August and 25 degrees in February, while in the western parts its width is 57 degrees and 49 degrees respectively. In comparing the amount of tropical and extratropical water, Krümmel finds that twenty-nine per cent of the whole surface of the oceans has always a temperature of more than 25° C., while almost one-half of it temporarily attains this temperature.

It will be observed that in certain parts of the oceans the lines of equal temperature are much crowded, and show sharp angles. This is entirely due to currents, which carry warm water to high latitudes, and cold water to warmer regions. Thus the influence of the Agulhas current may be observed in the sharp angles of these lines near the Cape of Good Hope, while the cold Cape Horn current lowers the temperature along the eastern coast of South America. The influence of the Gulf Stream may be seen in the crowding of the lines of equal temperature near Newfoundland.

The accumulation of warm water in the western parts of the oceans is entirely due to the action of the trade-winds, which blow continually from the eastward, and drive the warm water of the ocean westward, where it is accumulated on the coasts of the continent. Buchanan has explained this phenomenon in a paper on similarities in the physical geography of the great oceans, which has been published in the *Proceedings of the Royal Geographical*