

can hogs, the fact will be demonstrated; if, on the contrary, Trichinae are found, the extent of the trouble will be known and steps can be taken to protect the industry by systematic examination. We believe that the presence of Trichinae in pigs is confined to certain districts; if so, it can be localized, and the work of investigation gradually reduced within certain limits, and eventually, by proper precautions, the evil would be entirely removed.

## MOUNTAIN ELEVATION, AND CHANGES OF TEMPERATURE, IN GEOLOGY.

BY SAMUEL J. WALLACE.

It seems a very little thing for heat and cold to play over the face of a continent. But light and unnoticed as the creeping of fate it goes on forever; and the foundations of the everlasting hills are in its iron grasp. Cold and heat. What should a rock-ribbed continent care for them? What do they do?

In latitude  $40^{\circ}$  to  $50^{\circ}$  a yearly change of ten degrees of heat penetrates the upper strata to considerable depths; and the expansion of various kinds of stone for  $10^{\circ}$  varies from one to three feet in twelve thousand; making, say, one foot to the mile, which across North America is half a mile.

This is an always recurring and resistless force of outward thrust. It is probably mostly compensated for in its habitual recurrence by elasticity, slippages of strata on others, and by fissures; as well as by the fact that the expansion of solid strata is sometimes less from the deep drift or soil protecting them. But, still, as the superior force is outward, without anything to compel a full return in winter, and as the expansion is less below and greater above, the continued tendency is to push the upper strata forward over others toward the margins of extended plains, with a creeping motion, tending to force up bendings, folds and faults, and to raise mountains and plateaus slowly; and even to accumulate such strain or tension as to cause earthquakes and volcanos.

Though, as Dana and others think, there has been a singular persistence in the general features of deep oceans and of continental tables, yet, great portions of the tabular areas have had their depressions and upheavals from the sea. What must have occurred in such cases?

If a tract of sea-bed is covered by an arctic current at  $32^{\circ}$ , the cold must finally penetrate to very great depths. Then, should the polar current by any means be shut off, and a warm current flow over it, the temperature would certainly be raised several degrees, and produce an expansion which would find relief in raising mountain ridges, or in arching up its own or other regions. This might go on slowly till great areas were elevated from the ocean.

Rising from the sea, also, would increase the temperature very much, to heave up mountains and plateaus, or still other lands from the sea. This result it seems would have to occur, because of the great depths to which the expansion would reach, and because there would exist no provision for relief of the tension, such as the repeated yearly expansion would work out for itself.

It seems these results must flow from what we already know, whether there is or not, any other cause of elevation. There are some further considerations that may be noticed here.

Where a deep ocean trough bearing an arctic current lies along beside a continent it would form a fixed barrier to such expansions, and probably a chain of mountains would be forced up along it, together with volcanos and

earthquakes. The region of least yearly change and greatest cold is said to be in the northern edges of America and Siberia, and the bar connecting them across the pole. From the ends of this region the annual change increases southward and laterally. Singularly, the principal mountain systems of the northern hemisphere seem as if raised by forces or thrusts radiating from this bar and its ends. In America, as Dana shows, the original core of the continent was V-shaped, with its two ridges facing the end of that cold bar between them. And the later elevations preserve parallelism to these original lines, as if showing thrusts from that bar and from each other. In Europasia occur continuations of the same parallelism of elevations as facing thrusts radiating from the sides and the other and broader end of the same cold bar, to the areas of greatest annual changes southward, with still increased force and complexity.

In the southern hemisphere the bases of thrust seem as if, on the contrary, they were the three great ocean beds. And the great mountain systems of the world seem as if raised by thrusts of force radiating from these great northern and southern centers of land and ocean, opposing each other, together with some cross thrusts over broad areas of land. This feature of opposition between the northern land thrusts and the southern ocean beds, brings some of the principal lines of elevation in the northern hemisphere into diagonal courses, except where sweeping around the northern projections of the oceans, especially that of the Indian ocean, and its former connection west to the Atlantic south of Europe.

The present Alleghany system seems to have been raised by the elevation of the Mississippi Valley from the sea during and after the Carboniferous period; the Rocky Mountains by that of the plains, later; and the Alps by that of Northern Africa and Northern Europe, although previous elevations existed.

The familiar example of ice creeping up the shores of ponds and lakes, from repeated changes of temperature in winter, illustrates the principle of such elevations, the walled lakes of Iowa being special illustrations; and interesting observations have been published, showing from fixed levels that oscillations of level do occur from changes of temperature.

## REMARKS ON A PATHOGENIC SCHIZOPHYTE.\*

PROF. H. J. DETMERS.

When about two and a half years ago it became my duty to investigate the prevailing Swine-plague, the so-called Hog-cholera, I first endeavored to ascertain the nature and the cause of that disease, and to accomplish my object, made numerous post-mortem examinations, and paid special attention to the microscopic examinations of the blood and of the morbid products and morbid tissues. Although the microscope at my disposal at the beginning of my investigation is only a small No. VIII Hartnack stand with three Hartnack and Prazmowski objectives— $a 1$  inch, a  $\frac{1}{4}$  inch, and a  $\frac{1}{8}$  inch imm. and correctives—and consequently not a strictly first-class instrument, and in its performance by no means equal to the work of a Tolles or a Zeiss, I soon became convinced that the blood, the morbid products, and the morbid tissues of the diseased and dead animal invariably contained, while fresh, and not tainted by putrefaction, a certain kind of Schizophytes or bacteria. The same presented themselves in three different shapes, namely as small globular bacteria or Micrococci, as Zoöglæa-masses or clusters, imbedded in, or kept together by, a viscous mass, and as little rods or filaments. I soon found that all three forms belong to the same organism, and represent only different stages of development. The first or globular form predominated in the blood, the second in the morbid tissues—for instance, in the diseased portions of the lungs and in the lymphatic

\* Read before the State Microscopical Society, of Illinois, April 8th, 1881.

glands—and the rods occurred in greatest numbers in such morbidly changed parts and morbid products—for instance, in the ulcerous tumors of the intestines—as are accessible to atmospheric air and other external influences.

The constant occurrence of these Schizophytes soon made it appear probable that their presence is not merely accidental, but that the same, very likely, are connected with, and characteristic of, the morbid process of the disease. To get at the facts was one of my principal endeavors. How far I have succeeded I leave to others to judge.

Careful and repeated macroscopic and microscopic examinations of the tissues, but especially of the lungs, which, by the way, are always more or less affected by the morbid process of Swine-plague, soon revealed the fact that the principal morbid changes are brought about in the following way: The finer capillary blood vessels become obstructed or plugged, the more fluid portions of the blood exude into the tissues—in the lungs principally and at first into the lobules, and then into the interlobular connective tissue—some, and particularly in young animals not seldom but a great many, of the finest capillaries rupture, and innumerable small extravasations of blood, visible to the naked eye as tiny red spots, are deposited into the tissue. In the skin, subcutaneous tissue, and intestinal membranes the process is essentially the same, but to follow it further would lead too far for the present. Let me, therefore, mention another fact. While the blood taken from a vein of a diseased or dead pig invariably contains a large number of spherical bacteria or Micrococci, and very few, and usually small Zoöglæa-masses, the diseased parts of the lungs, and especially the stagnant blood, which oozes out of the capillaries, if the diseased parts of the lungs are cut into small pieces, invariably contains, besides Micrococci, numerous and large Zoöglæa-masses, which are, most of them, much larger than the blood corpuscles, and abundantly large enough to clog the finer capillaries. All this, of course, does not prove that the Schizophytes constitute the cause of the morbid process. I therefore resorted to experiments. Having found that any inoculation of a healthy pig with the fresh pulmonary exudations of a diseased or dead animal invariably produces the disease in three to fifteen days, or on an average in six days, I concluded it might be ascertained in two different ways—in a negative and in a positive way—whether or not the Schizophytes constitute the cause of the morbid process. If it were possible to free the Schizophytes from everything, and to transfer the same without any vehicle whatever from one animal to another, for instance, like a louse or an itch-mite, the question would be very soon answered. But as that cannot be done, I had to get at the facts in a more indirect way. I repeatedly charged two ounces of an innocent fluid, at first pure and fresh milk, then boiled milk, mutton broth, afterwards water, and finally albumen, with one drop of the infectious pulmonary exudation, containing an abundance of Schizophytes. In about three days the fluids thus charged, which, by the way, were kept at a suitable temperature, were found to be swarming with Schizophytes, identical in appearance to those found in the pulmonary exudation; and every inoculation made with these fluids proved to be effective, but in most cases the attack produced was of a comparatively mild type. To go further into particulars would take too much time; I therefore have to refer for particulars to my reports to the Commissioners of Agriculture. One thing, however, I must state. The fluid transferred by each inoculation was less than half a drop, but this half drop contained innumerable Schizophytes, while as far as could be ascertained by careful microscopic examinations, nothing else contained in the original exudation had multiplied. Consequently, nobody, unless he believes in the power of Hahnemannian dilutions, will contradict, and say, the effect of the inoculations is brought about, not by the Schizophytes, but by an unseen and unknown virus, or

chemical something, the existence of which cannot be proved. I was, however, not satisfied with these positive results, and concluded to try also the negative way. Knowing that it is impossible to separate the Schizophytes from their vehicle, I tried to free the latter from the Schizophytes, and resorted to filtration. I filtrated the pulmonary exudations through half a dozen of the finest filtering papers obtainable, but found my effort to be in vain, for the filtrate, although freed from the Zoöglæa-masses and rod-shaped bacteria, yet contained numerous Micrococcus-forms. The filtrate was put in a vial with a tight fitting glass-stopper, and when examined three days later, it contained a great many rod-shaped bacteria, and comparatively few Micrococci. I therefore filtered it again with the same result, except that the Micrococcus-forms were not as numerous after the second filtration as after the first. So I filtered the exudation three or four times, each time through four to six filtering papers, and at intervals of about three days till I was finally not able to detect any Micrococci in the now limpid filtrate. Inoculations with this filtrate proved to be ineffective. At another time—in the following winter—I tried again to free pulmonary exudation from the Schizophytes by means of filtration, but did not succeed. The filtrate always—after each filtration—contained numerous Micrococci. Whether, in this second attempt, I did not hit the right time for my second and third filtrations, that is, a time at which most or all of the micrococci had developed to rod-shaped Schizophytes or filaments; whether the temperature was too low—the first, successful attempt was made in the summer—and therefore the development of the Schizophytes was irregular or retarded; whether my filtering papers were not fine enough; or whether all these circumstances combined made the filtration a failure, I do not know. An inoculation made with this filtrate proved to be effective, but the disease produced was of a very mild character; at any rate, the animal recovered.

If more proof is yet required that the Swine-plague-Schizophytes and nothing else constitute the infectious principle of that disease, and it seems that the above facts which have been published more fully in my reports to the Commissioner of Agriculture, are not deemed sufficient, the following facts, if not making it absolutely certain, will at any rate, especially if considered *in toto*, to a great extent, corroborate the assertion that the Schizophytes have, and must have, a causal connection with the morbid process.

1. It has been, and can be, everywhere observed, where Swine-plague is prevailing, that the infectious principle floating in the air, is attracted and taken up by sores, wounds and even scratches, but does not enter the animal organism through the whole skin and through perfectly healthy respiratory mucous membranes.

2. Antiseptics, or medicines, which are either directly poisonous to the lower forms of organic life, or destructive to those conditions, under which low forms of organic life thrive and develop, and among those antiseptics, especially carbolic acid, iodine, hyposulphite of soda, benzoate of soda, thymol, etc., have proved to constitute almost sure prophylactics. As one of the conditions necessary to the development of Swine-plague bacteria, it seems, has to be considered a certain degree of animal heat. At any rate, after, and while the animal heat of a pig is reduced by a continued treatment with carbolic acid, from the normal (102° to 104° F) to an abnormally low temperature (say 96° to 97° F), every inoculation with fresh infectious material has so far proved to remain ineffective. Further, the various antiseptics, which have proved to be good prophylactics, are very dissimilar in their chemical affinities and actions, and their prophylactic effect cannot very well be explained, if the infectious principle were a chemical agency, a virus, or a poison, but is explained, if the same consist in something endowed with life and power of propagation.

3. If the morbid process, the morbid changes effected, particularly the exudations and extravasations of blood on the lungs and in the skin, and the qualitatively unchanged condition of the blood—that is excepting such changes in its composition as are evidently the product, or necessary consequence, of the morbid changes—are taken into consideration, it becomes obvious that something which causes obstructions in the capillary system—embolism—must constitute the cause, and nothing whatever, able to accomplish that result, can be found, except the colonies or clusters of Schizophytes, the Zoöglæa-masses, imbedded in a viscous substance, while on the other hand, these Zoöglæa-masses are never absent in a case of Swine-plague.

If I am allowed to digress a little, it may be here mentioned that I am well aware of the fact that German and French investigators claim for certain, and it may be, for all, kinds of pathogenic Schizophytes chemical actions or fermenting properties, and undoubtedly many of them, especially among those belonging to the genus *Bacillus*—I mention *B. anthracis*—and probably some others, do possess and exercise such properties, and cause fermentation. As to the Swine-plague Schizophytes, I have not been able to observe any fermenting effect or chemical action, except such as necessarily results from depriving the animal organism of certain elements and material, appropriated by the Schizophytes, and necessary to their subsistence and propagation. All other morbid changes appear to be the consequence of the obstruction of the capillary system by the Zoöglæa-masses, and therefore, are the product of a mechanical, and not of a chemical agency.

4. The adversaries of the so-called "Germ-theory" of diseases, well knowing that a perfect separation of the Schizophytes (*Micrococci*, *Bacteria*, or *Bacilli*, as the case may be) from their vehicles, the animal tissues and fluids, is impossible, demand absolute proof. If conclusions may be drawn from analogy between diseases of animals and plants, Prof. T. J. Burrill,\* of the Illinois Industrial University, more favored by the nature of the objects of his investigation (apple-trees, pear-trees and peach-trees) has furnished evidence, amounting to almost absolute proof, that the so-called blight of apple-trees and pear-trees, and the so-called "yellows" of peaches are caused by Schizophytes similar in size, but otherwise not identical to those which I consider as constituting the cause and infectious principle of Swine-plague, as will be seen by consulting the transactions of the meeting of the American Association for the Advancement of Science in Boston, 1880.

If the infectious principle were a chemical poison or virus, its action, one should suppose, would under all circumstances be exactly the same, and the malignancy of the morbid process, and the time required for its development—the so-called period of incubation, or, more correctly, stage of colonization—would not be subject to changes dependent upon the season of the year, upon the individuality and temperature of the animal, and upon other yet unknown external influences, as is undoubtedly the case. An organic poison or virus, one should suppose, would act somewhat like the virus of a poisonous snake. In the same localities, in the same places, or the same yards and pens, and among the same breeds of hogs, in which the disease was exceedingly malignant in 1878; it was, as a rule, much milder in 1879, and still milder in 1880. As such are unmistakable facts, repeatedly and everywhere observed, it must be concluded that nothing but what is able to undergo changes is subject to growth and development, and acquires vigor and propagates rapidly under favorable, but is weakened and multiplies slowly under unfavorable circumstances—in other words, nothing but what is corporeal and endowed with life—can constitute the cause.

6. If the cause and infectious principle of Swine-plague were a chemical poison or virus, one should suppose a cessation of the morbid process would be impossible, and an animal would never recover, while its organism contains an abundance of the infectious principle in an effective condition, as is undoubtedly the case, because convalescents, and animals nearly recovered, frequently communicate the disease, even in a fatal form, to other, healthy pigs. Further, the fact that an animal, once recovered, possesses but little predisposition for future infection, or is seldom attacked a second time, even if ever so much exposed, and then only contracts the disease in a comparatively mild form, could never be explained; but the whole presents an entirely different aspect, and admits explanation, if low and minute forms of organic life, such as the Schizophytes of Swine-plague, which, by developing and multiplying, finally destroy or exhaust in an animal organism the conditions necessary to future development and propagation, constitute the cause and the infectious principle. (cf. an article entitled: "*The Destruction of Germs*," in "*Popular Science Monthly*," communicated in extract in *R. Hitchcock's Microscopical Journal*, Nov., 1880.)

7. If some part or organ of a pig infected with Swine-plague happens to be in a state of congestion, such a part invariably attracts the infectious principle, and becomes a prominent, if not the principal, seat of the morbid process; a fact difficult of explanation, unless the infectious principle is something solid or corporeal.

8. The adversaries of the so-called "Germ Theory," as they are pleased to call it, demand absolute proof of those who claim that certain infectious diseases owe their origin, or existence and spreading, to very minute forms of organic life. They cannot deny that these forms exist, can be found, and have been shown, but forget to show their virus, poison, fluidum, or chemical something. Does the latter exist only in their imagination? If the adversaries of the so-called "Germ Theory" demand absolute proof on our side of the question, let them set a good example and furnish it on their side, or only produce their virus, fluidum, or whatever it may be, and we will gracefully acknowledge that we are mistaken, and have labored in vain.

9. With the very best objectives ever made, and a fair ability to handle the microscope, I have never been able to find anything identical to the Swine-plague Schizophytes in the blood and tissues of other healthy animals. When I commenced my investigation, the best objective at my disposal was a very fair 1-9 four system immersion lens of Harnack & Prazmowski, but I soon found it to be insufficient, and procured a 1-16 immersion of the same makers. This, too, after a while, did not give satisfaction, and I received a 1-12 (nominally 1-10) glycerine immersion of R. B. Tolles, which that renowned maker afterwards exchanged for a duplex 1-10 homogeneous immersion. This latter objective proved to be a very superior lens, and gave me glimpses of things of which I desired to see a little more—it showed flagella on *Bacillus subtilis*, which I had never seen with any of the other objectives—and so I thought with a higher power, and a still more perfectly corrected lens, if a more perfect correction could be made, I might be able to see more plainly the distinguishing forms and characteristics of the Swine-plague Schizophytes, and also learn a little more about their mode and manner of propagation. I therefore asked Mr. Tolles to make me a higher power objective especially adapted to my work, and he has furnished me a duplex 1-15 homogeneous immersion objective (in reality a little more than a 1-16), which is, beyond comparison, the best objective I have ever seen. It is even superior, in definition and flatness of field, to a magnificent 1-18 homogeneous immersion objective (in reality a 1-20) of Carl Zeiss, made to order a month or two ago.

As to a proper generic place and name of these Swine-plague Schizophytes, I am at a loss. The best authorities—

\* "SCIENCE," Vol. I., pp. 162, 191.

Cohn, Klebs, and others—who have attempted a classification are somewhat undecided themselves, and do not agree where generic lines ought to be drawn. At any rate, the Swine-plague Schizophytes do not fit into any of the genera proposed. They are not bacteria, because the single cells are spherical and not oblong; they can hardly be considered as Micrococci, because the same are bi-spherical in their advanced stage of development; and they cannot be classed among the Bacilli on account of their forming Zoöglæa-masses. I have, therefore, preferred to use, for the present, that name, which, without any serious contradiction, is given by modern investigators to the whole family: Schizophyte or Schizophytes, or the older name, introduced by Naegeli, Schizomyces.

The Swine-plague Schizophytes present themselves, according to their stage of development, in three different forms and shapes. Their simplest form, it seems, is that of a Micrococcus, or of a small globule of about 0.7 or 0.8 microm. ( $\frac{1}{100}$  inch) in diameter. It occurs invariably in the blood, the morbid products, and exudations, etc. of the diseased animals, and is never absent, but can always be found, though in some cases in much greater numbers than in others. The second form is bi-spherical—the spherical cell having duplicated itself by a gradual contraction in the middle, while growing endwise. These bi-spherical Schizophytes are always more or less numerous, and are motile, or move about, provided the temperature of their vehicle—lung-exudation or blood-serum, for instance—is not too low. Some of them, but probably only those, which, separated from a larger chain, as will presently be explained, are provided, at any rate at one end, with a flagellum—a post-flagellum—which, however, is so exceedingly fine that it can be seen only with the very best high-power objectives, like a Tolles 1-15, and the most favorable light obtainable, and even then only while the Schizophyte is slowly moving. I have never yet been able to see it while the Schizophyte is at rest.

These double Micrococci, or bi-spherical Schizophytes, soon undergo further development. Each single cell soon again contracts in the middle while growing endwise, and, at the same time, separates more and more, and becomes partially independent from its sister cell, with which, however, it remains connected for some time, even after it has completed its duplication. Meanwhile the sister-cell, too, has become bi-spherical, and what a short while ago was a simple bi-spherical cell, has become a double bi-spherical body, resembling a small chain of four round joints. But the duplication does not stop; each of the four single cells, within a short time, doubles again, and soon quite a little rod or filament will be formed, which, on close inspection, presents a string or chain of bi-spherical cells endways, loosely connected with each other. Under moderately high powers—say of 800 or 900 diameters—such a string presents a slender, rod-shaped moniliform bacterium. While the single cells, or each half of each bi-spherical body, soon develop into double or bi-spherical cells, the connection between the latter gradually loosens, so that finally, if the temperature is not too low, and the development a rapid one—I have frequently observed that the number of bi-spherical cells in such a chain becomes doubled in less than five minutes—the chain breaks up into smaller ones (joints), each consisting of one or two bi-spherical Schizophytes, which, in separating from their neighbors, after some swinging to and fro, spin or draw out a very slender thread, a flagellum or cilium. But before all these changes, this rapid duplication, take place, the spherical Micrococci, when about to change to bi-spherical bodies, form those clusters (Zoöglæa or Coccoglia masses), which, being imbedded in, or kept together by, an apparently viscous substance, obstruct the capillaries, and, according to my observations, constitute the principal and direct cause of the morbid process. In these Zoöglæa-masses the single Micrococci, it seems, undergo their first metamorphosis, or change to double bi-spher-

ical cells, and this change continues, till portions of the Zoöglæa-mass separate, or till finally the glia breaks and opens, when the bi-spherical bodies, and also some yet unchanged spherical Micrococci, become free. The former, very soon, commence their duplication, but as each new cell or globule soon produces another one and becomes bi-spherical, the same cannot be the source of the spherical bodies or Micrococci. The latter, it appears, have another origin, as will be presently explained.

In Swine-plague material, such as blood, blood-serum, lung-exudation, etc., if a day or two old, and sometimes while yet fresh, bacteria of a peculiar shape and form make their appearance. The same are rod-shaped, and a trifle longer than a bi-spherical Schizophyte, or two united spherical bodies, but are not moniliform, and have at one end, or in comparatively rare cases toward the middle, a bright and light-refracting globule of much more density than the rest of the bacterium. This globule is surrounded by a substance or an envelope of considerably less density and is therefore less light-refracting. If that globule is situated at one end of the bacterium as is usually the case, the whole bacterium presents the shape of a club, because the globule and its envelope have much more diameter than the rod. Billroth calls this form a Helobacterium, and the globule a lasting spore (Dauerspore). Such a lasting spore, according to Billroth and Cohn, at any rate, if developed by a Bacillus, is able to resist very high degrees of heat and cold, and is very prolific, as it disseminates a large number of germs, which, probably, constitute the source of the globular bacteria or Micrococci. As such Helobacteria are often found in perfectly fresh blood, and exudations, etc. (in the exudations most frequently) of hogs, which are affected with, or have died of Swine-plague, and are nearly always seen if the blood and exudations, etc., are a few days old, it appears probable that the same not only constitute the source of the spherical bacteria or Micrococci, but also that their great tenacity of life, or resistibility against adverse external influences, explains the ability of the infectious principle of Swine-plague to remain effective for a whole year, if protected, by clinging to, or being imbedded, in a moist and porous substance, such as an old straw stack, etc.

Whether or not Swine-plague-Schizophytes are able to multiply in any other form and manner than stated, I have not been able to observe. One observation, made already at the beginning, has found new and repeated confirmation, viz: wherever, or as soon as *Bacterium termo* makes its appearance in large numbers, the Swine-plague Schizophytes commence to disappear and disappear in about the same ratio, in which the former are increasing in numbers. In blood kept in a vial, Swine-plague Schizophytes cannot be found when the blood commences to exhibit a purplish color, or when the blood corpuscles commence to decay, or become destroyed. Further, the Swine-plague Schizophytes, although presenting the same general characteristics when cultivated in fluids foreign to the animal organism of a hog, show differences in so far as the same present less uniformity in size, and as this development and multiplication proceed slower, and with much less regularity. It seems the cultivated Schizophytes change and develop slower, and probably on that account are less vigorous in producing mischief—at any rate, an inoculation with cultivated Swine-plague Schizophytes, although effective in producing the disease, is always followed by a comparatively milder form of Swine-plague than an inoculation with material directly from the body of a diseased hog. This, however, does not involve that every inoculation with cultivated Schizophytes produces under all circumstances a milder form of Swine-plague, than any natural infection, for such is not the case. The difference may be stated thus: A natural infection, or an inoculation with material directly from the body of a diseased hog, as a rule, produces a malignant and dangerous attack

and as an exception a mild form of the disease—the frequency of the exception depending, it seems, to a great extent upon the prevailing character of the plague, while an inoculation with the cultivated Schizophytes is, as a rule, followed by a mild attack, and as an exception, or in rare cases only, by Swine-plague in its severest form.

Wherever Swine-plague is prevailing in its most malignant or fatal form, or, what is essentially the same, wherever formation of ulcerous tumors in the cæcum and colon is a frequent occurrence, where consequently an abundance of Swine-plague Schizophytes is discharged with the excrements of the diseased animals, there the spreading from animal to animal, and from herd to herd, is a rapid one; and *vice versa*, wherever the spreading is rapid, there ulcerous tumors in the intestines are a frequent occurrence. In 1878 the same (the ulcerous tumors) could be found in about 75 per cent. of all cases that had a fatal termination, while at present (in Illinois) their occurrence is probably limited to about 5 per cent. of all cases.

### THE KANSAS CITY ELECTRIC TIME BALL.

By Prof. H. S. PRITCHETT, Astronomer at Morrison Observatory, Glasgow, Missouri.

The first time ball established in the United States was dropped from the dome of the Naval Observatory at Washington in 1855. It is still dropped at Washington mean noon, and has for a long time furnished the standard time for the city and the Departments of the Government.

The New York time ball, established in 1877, is dropped at New York noon, by an electric signal, sent from the Naval Observatory at Washington. It was erected and is maintained by the Western Union Telegraph Company, and is dropped from their building on Broadway. At 11h. 55m. the ball is hoisted half-way up the staff on the tower of the building. At 11h. 58m. it is hoisted to its highest point, when it is about 250 feet above the street and can be well seen by the shipping at the New York and Brooklyn docks, and vessels in the bay, and from suitable positions is visible to a large part of the citizens of New York, Brooklyn, Hoboken and Jersey City.

If on account of wind the ball fails to drop at 12h. 0m. os., it is held till 12h. 5m. and then dropped. In such cases a small red flag is hoisted at 12h. 1m. and kept flying till 12h. 10m. This ball was for some time dropped by hand, but for the last year the dropping has been automatically effected by the clock at the Observatory. The working of the apparatus has been in the main satisfactory, and the ball has been dropped quite regularly, the failures being caused almost entirely by temporary breaks in the wire or other causes which could not be foreseen.

In the evening papers of the day and in the papers of the next morning a notice is regularly inserted, stating whether the ball dropped at correct time, and if not, its error, fast or slow. Many are at a loss to know how this correction is obtained. It is arrived at in the following manner: The time of the falling of the ball records itself automatically by electricity, near the standard clock of the Western Union Company in the building, the clock itself being regulated by the daily clock-signals from Washington. The difference between the time of falling of the ball and noon, as indicated by the clock, is thus obtained by a direct comparison. This assumes of course the accuracy of the clock, and during a long continued season of cloudy weather, or in case of accident to the clock itself, the time might be somewhat in error, although the published correction might show but a few hundredths of a second. At present however, the Western Union has the benefit also of the Alleghany and Cam-

bridge signals, for the regulation of this clock, so that even during the longest season of cloudy weather it is not probable that the clock could be much in error.

The Boston time ball, which is dropped at noon of Boston time, by means of the noon-time signal from the standard clock of the Harvard College Observatory, is placed upon the large building of the Equitable Life Assurance Company and was paid for and is now maintained by this company. The ball is of copper and weighs about 250 pounds. The machinery used in raising and controlling it is hence much more complicated and costly than in either of the cases before mentioned. The cost of ball and machinery was about \$1200. The electric signal which drops it, is given by the clock itself, the ball having a drop of fifteen feet. The nearness of the Observatory, and the fact that the wire used is wholly under its control, give additional convenience and certainty in the dropping of the ball, and reduces the probability of accidents to a minimum, so that it is effected with great regularity and precision. Prof. Pickering, Director of the Observatory, reports for the year ending Nov. 1st, 1880, the ball was dropped exactly at noon on 355 days; on four other days at five minutes past noon, in accordance with the rule adopted; on four other days it was not dropped, leaving only three cases of inaccuracy of dropping.

Quite recently a time ball has been established at Hartford, Conn., and dropped by the Winchester Observatory of Yale College.

The time ball recently erected at Kansas City, and which is dropped as a part of the time service of the Morrison Observatory, is the first attempt in this direction in the West. It was paid for chiefly by an appropriation of the City Council of that city. The site selected was the large building just erected by the Messrs. Bul-lene, Moores & Emery, on Delaware street. The ball when raised to the top of the staff is about 140 feet above the street, and is generally visible to the business portion of the city. The ball which passes over the staff, is simply a wire skeleton covered with canvas and painted black, and is about three feet in diameter. It was loaded on the inside with lead until it was found to drop instantly and without loss of time. It has a drop of about twenty-five feet and is slowed up as it reaches the bottom, and is received upon a set of tall springs surmounted by a stout cushion.

The apparatus by means of which the ball is dropped at precisely the right instant, was constructed under the direction of Mr. W. F. Gardner, the instrument maker of the Naval Observatory at Washington. It is of a very simple form, and is found to answer all requirements.

This has been found to work easily and without loss of time and can scarcely get out of order. The entire cost of mounting the ball and machinery was only about \$120, and with this small amount it was necessary to use the utmost economy in the purchase of materials and apparatus. Kansas City is about one hundred miles from the Observatory, and except in cases of breaking of the wire, when the ball cannot be dropped at all, it is dropped within one or two-tenths of a second of correct time.

The discrepancy in the local time kept by different jewelers in the city before the erection of the ball was astonishing, and led to endless confusion in business and travel.

On the first day the ball dropped, this difference, in extreme cases, amounted to fifteen or twenty minutes, some being eight or ten minutes fast, others as slow. The establishment of the time ball has brought about a uniformity never before known, and must soon make itself felt, not only as a convenience, but a promoter of punctuality in business engagements.

From the daily clock-signals sent over the wires from the Observatory it will be easy to establish a similar time signal in any city in the West, which will take the