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The Physical Geography of the North Pacific Ocean, the Peculiarities of its Circulation, and their Relations to the Climate of the Pacific Coast of the United States. By Wm. Henry Doughty, M. D., Augusta, Ga.

The relations of medicine to the natural sciences are so apparent that no apology is necessary for the appearance of the present essay in a periodical devoted to this science. Some knowledge of physics and meteorology is indispensable to a proper medical education: the study of optics, acoustics, dynamics, of atmospheric pressure, of the laws governing the pressure of fluids and the expansion of gases, of electricity, of the distribution of heat and moisture, and of allied subjects, suggests their importance and uses to the intelligent physician. An important subdivision of climatology is topography, the study of which, in its relation to climate, should embrace the peculiar features of contiguous portions of the ocean, as well as those pertaining to the land and soil. In considering the climatic characters of any continental section, it is not sufficient merely to recognize its general con-
tiguity to the ocean; the general effects of such relation between land and water are often modified by existing local peculiarities in the circulation of the latter. Uniformity of effects can only result where the general causes are the same, and similar relations maintained; their absence produces climatic diversities. Hence, the difference between the climate of our eastern and western coasts; the adjoining water-surfaces, although equally extensive, yet differ in their topographical features, which, with the incidental assistance of prevailing winds and the continental configuration, cause the difference of climate.

This circumstance brings medicine into profitable connection with the new and beautiful science, the Physical Geography of the Sea. The rivers of the ocean, and its other geographical characters, must appear for study with those of the continents, as well in the benignity of their functions as in their commercial importance.

To the general reader, the sea is at present one of the most interesting subjects. It is not a great while ago since the philosopher and the herdsman alike viewed its waters in the light of a great waste, void of aught but dangerous breakers and striving elements, and destitute of any feature, except its immensity, calculated to exact admiration. For centuries its navigation has been successfully followed by maratime countries, some of which exist only in history, and yet, beyond its use as a medium for commercial intercourse; as a theatre for warlike operations, upon which to assert a fickle supremacy; and as a commodious fishery for our support and comfort, very little is known of it. Its harmonies with the balance of creation were unrevealed. A great mass of saline water, without order and treacherous in all of its attributes, was the substance of the rude idea entertained of it. The faithful mariner committed himself to its faithless waters, feeling no security, except in the computed strength of
his vessel to withstand the furious assaults of the winds and waves, and although often possessed of strength sufficient for this defensive purpose, yet was as often drifted and borne hundreds of miles from his course for the want of that tact and knowledge which the light of recent science furnishes. Even of those portions of the sea which are the constant thoroughfares of international trade, until recently, little was known of their precise circulation, and the developments now made, in their fullest extent, are declared by him,* whose labors have done more to harmonize its workings with the other departments of nature than all others, and to offer, perhaps, the only satisfactory explanation of certain of its recognized actions, to be “only a table or two of contents from the interesting volume which the Physical Geography of the Sea is destined, some day, to open up to us.” By the light of his labors, which have secured for him the deserved title of the “Philosopher of the Sea,” science has unfolded to our view some of its mysteries, and held up to our admiring gaze its settled harmonies with the rest of creation.

The saltness of the sea does not appear a simple chemico-physical property, designed to make it a fit medium for the countless multitudes that dwell in it, but as intimately concerned in the production and preservation of that system of currents and counter-currents which is so beautifully displayed. The little animals (corals, etc.) which cluster so abundantly upon its sides and in its bed are no longer regarded as supernumeraries in creation, but, in the excretory office that they perform in the economy of the ocean, we observe a necessary, healthful function; they, too, it is supposed, may contribute to its motive power. The various currents—surface and deep-sea currents—of differing temperatures in different parts

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of the ocean-bed, are not now considered as the results of accidental agencies, or of geographical or latitudinal acquirements, but as the grand efforts of old ocean to equilibrate its temperature and saltness, and thereby extending its munificence to the land-climates. The Gulf-stream—that "river in the ocean," as it has been called—is no longer regarded as an accidental emergence of heated and saline water from the tropical sea and gulf, its parent, but its original purposes are discovered as a boon, although at times a terror also, to navigation, a moulder of land-climates, a restorer of lost heat and salinity to the arctic waters, as well as affording an escape for corresponding surplus states from the tropics. Looking at it in its entirety, in the infinity of its operations, we recognize it as the great necessary correlative of the air and the land, without which neither could perform its destined purposes.

There is a strict conformity in all its workings to the known principles of physical and chemical science. We find there life, energy, and ceaseless activity, and above all, in its contemplation, we realize the wisdom and goodness of that Being "who weigheth the waters in the hollow of His hand and fixeth bounds for the sea that it can not pass." In its simple product of hydrogen and oxygen—water—aside from the substances held in solution, its sublime motions, its harmonizing efforts, and their beneficent results, we discover an element almost unlimited in its adaptation to the wants of nature. Without it, the world would be converted into a vast charnel-house; the air we breathe would dry up the very fountain of life; the fatness of the earth would be destroyed; and, in the language of another, "the fair face of nature, still as fresh and blooming as in her infant days, would contract in ghastly wrinkles, and the comeliest landscapes grow cadaverous with premature age."
We do not propose, however, to treat of the ocean at large, in any of its interesting characters, nor, indeed, to consider even its general meteorological influences, but simply wish to offer some considerations touching the more prominent peculiarities of one of its subdivisions, viz: the North Pacific ocean. These will relate to the peculiarities of its circulation; the reasons for and the causes of those peculiarities, as we apprehend them; and, finally, their effects upon the climate of the countries to which they are contiguous.

Variations of the form and modes of circulation of the various divisions of the ocean necessarily result from the interposition of continents and parts of continents. The configuration of these masses determines, to a greater or less extent, individual peculiarities of circulation in each of the divisions created, when sufficiently large to vary it at all. Thus, the North Atlantic ocean has the course of its currents determined and preserved, to a great extent, by the relations of the two continents to it; the course and direction of the currents of the Indian ocean result from the intrusion of the neighboring land areas; and the North Pacific ocean has similar characteristics. It follows, therefore, that the circulation of each corresponds with, or differs from, that of the others only as they acknowledge a similarity, or difference, of land relationships, and that their peculiarities are to be brought to light by comparison. Those of the North Pacific must be studied comparatively—with what division shall we compare it? Its natural ally of the northern hemisphere—the North Atlantic—is that most eligible and best calculated, by our more thorough knowledge of it, to guide us in our reasonings. And, first, let us inquire, in what respects do they differ?

The Atlantic ocean is longer and narrower than the Pacific, and in its northward extension gradually merges
itself into the Arctic sea and Polar basin. Its waters coursing northward are almost unimpeded, except by the diurnal rotation of the earth; its principal northern outlet is found between Greenland and the continent of Europe, and is large enough, and wide enough, and deep enough, to allow an easy progress of surface and deep-sea currents. Between Greenland and the American continent there is another great channel, represented by Davis' strait and Baffin's bay, which, although inferior to the other, affords a free communication and interchange of its waters with those of the Polar basin proper. Looking at it in its main body, the relations of the adjoining continents are such as to give it the appearance of a long narrow body of water occupying a valley-depression between them, and having at either end sufficient capacity to permit the free ingress and egress of other currents of water. The continuity of the mass upon its left is comparatively unbroken from the point of Florida to the Island of Newfoundland, near the fiftieth parallel of latitude, at which point the southern extremity of the Arctic waters enters the Atlantic almost at right angles to the advancing shore-line. This being crossed and the point of Greenland reached, the direction and continuity of the shore-line are maintained to the easternmost part of Greenland, whence it turns almost due north, thus increasing the width of the ocean-surface in this latitude. On the right, however, near the thirty-fifth parallel, the continuity of the coast-line is seriously interrupted by the Mediterranean sea, which both receives from and discharges into the Atlantic ocean large bodies of water. From the Straits of Gibraltar northward, the coast-line presents a succession of indentations, and, in some cases, large parts of the continent are entirely separate, standing out in the ocean-bed. In the intervening water-area, the North sea, with its extension inwardly to the Baltic sea
and Gulf of Bothnia, we have "the attempted reproduction" of the Mediterranean sea.

The Pacific ocean (N), on the other hand, appears to be a vast encroachment of water-area upon the eastward and westward extension of the continents forming its boundaries, possessing much greater width than length, and having twice the breadth of the Atlantic. Its northern limit is entirely within the embrace of the two continents, except at the single point, Behring's straits, which, in comparison with even the lesser outlet of the other ocean, is narrow and shallow. The continental arrangement is such that the two coast-lines, running respectively a northwest and northeast direction, very nearly approach each other; and throughout their whole extent do not present a single interruption that allows the descent of northern waters or the escape of southern. The Asiatic boundary, from its commencement to its end, is irregular, and numerous islands stand off from it, thereby multiplying small seas, as the China, Eastern, Japan, Okhotsk, and Kamschatka. On the American coast, no encroachment occurs worthy of mention. Its southwestern part is studded with thick clusters of islands, which, in a general view of it, materially encroach upon its superficies and depth. Moreover, across the point of escape for its waters are the Aleutian islands, extending from the Peninsula of Alaska, on the American side, in a regular continuous series, nearly to the Asiatic continent.

We perceive, then, that while the Atlantic ocean is so related to its contiguous shores as to secure freedom of ingress and egress for its waters at both ends, and has frequent inlets of its arms into them, the Pacific has only a single outlet for the northern escape of its waters, and through which to interchange with the polar waters. This, moreover, is narrow and shallow, and we find placed immediately in front of it a semi-
circle of islands, as if to submit the waters entering and escaping to a sort of percolation. Freedom of entrance to extraneous bodies of waters is only granted at its southern part (the northern being closed, except at Behring's straits), and even here other waters are brought in chiefly as under-currents.

Bearing in mind these important differences, and recalling the mode of circulation of the Atlantic ocean, ought we not to expect such alterations in the circulation of the Pacific as may be termed its peculiarities? We think so, and now invite attention to their discussion.

Along the western borders of these two oceans, the analogies of their circulation are preserved, a signal correspondence in topographical features existing; the important differences—the distinctive peculiarities—are thrown along their eastern borders in consequence of the closing up of the outlets for the escape northward of the Pacific waters. The waters of the Atlantic under the guidance of the Gulf-stream, coming full upon the western shore of Europe, escape partly above and partly below it, and, in some measure, course along its border, whilst those of the Pacific, in their great bulk, are turned down along the northwestern coast of America. At the place of distribution of the latter, or near the points where this doubling of the waters upon themselves occurs, those peculiarities are observed, and so far as our information extends, they have never been clearly elucidated.

In the origination of distinct currents, the Indian ocean sustains to the Pacific a relation precisely similar to that which the Caribbean sea and Gulf of Mexico bears to the Atlantic. It is the great central focus to which, we are taught, the moving masses of the entire Pacific ocean ultimately tend, in obedience to that demand of nature which requires the counterbalance and removal of those concentrated saline and heated waters; and from
which are dispatched those benign currents whose special object is the mitigation of the severities of climate in far-off latitudes and countries, both northward and southward. The Indian ocean differs, however, from the Caribbean sea and Gulf of Mexico, in having a higher temperature for its waters, and the force of evaporation there is much greater. Thus it is said that the “temperature of its waters is frequently as high as 90° Fahr.,” and that “fifteen or twenty feet of water” are “yearly carried off from this ocean by evaporation;” and, again, that “the evaporation in certain parts of the Indian ocean is from three fourths of an inch to an inch daily.”

The current* of most interest to us, issuing from this tropical sea, “makes its escape through the Straits of Malacca, and, being joined by other warm streams from the Java and China seas, flows out into the Pacific, like another gulf-stream, between the Phillipines and the shores of Asia.” The general character and course of this stream are closely analogous to and, in some respects, identical with the Gulf-stream; the causes which determine them; their indisposition to mix with the general sea-water, produced by their peculiar chemico-physical properties; their objects and their results are doubtless the same. This current is designed to effect in the Pacific ocean what the Gulf-stream accomplishes in the Atlantic.

From the Phillipine islands, its precise direction has been more accurately pointed out by Lieut. Bent, U. S. N., of the Japan expedition, at a meeting of the New York Geographical and Statistical society, in January, 1856.† He says: “This offshoot—the Kuro-Siwo, or

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*According to Lieut. Maury, U. S. N., there are at least two other currents that issue from this ocean: “One of them is the well-known Mozambique current, called at the Cape of Good Hope the Lagullas current.” “There is, at times at least, another current of warm water from the Indian ocean; it finds its way south, midway between Africa and Australia.”—Page 137 Physical Geography of the Sea.

† Blodget's Climatology of the United States, page 20.
Japan stream—is separated from the parent current by the Bashu islands and south end of Formosa, in lat. 22° north, long. 122° east, and is reflected along the east coast of Formosa, where its strength and character are as decidedly marked as those of the Gulf-stream on the coast of Florida. This northerly course continues to the parallel of 26° north, where it bears off to the northwest and eastward, washing the whole southeast coast of Japan as far as the Straits of Sangar, and increasing in strength as it advances, until reaching the chain of islands southward of the Gulf of Yeddo, where its maximum velocity, as shown by our observations, is eighty miles per day.” From the Japan islands, which correspond in their relation to it, to the grand banks of Newfoundland, it acquires a northeastwardly direction, and “attempts the great circle route to the Alentian islands.” As it advances farther and farther into the Pacific, it slowly spreads itself out upon its common waters, and silently dispenses its higher temperature to the air and water around; and, finally, having reached its ultimate destination—the northwest coast of America—there, after a journey of seven thousand miles or more, exhausts its fertility, and, having accomplished its destined purposes, commences its circuitous return to the attracting centre. By far the largest part of this current thus turns down the American coast, the remainder finding an outlet as a surface-current, through Behring’s straits into the Polar sea. The place of separation of the latter from this great whirl of warm water is the projecting extremity of the Peninsula of Alaska.*

* So far as the ultimate distribution of this current and of the Gulf-stream reflects their original design, it seems that the latter was intended chiefly for distribution through the Arctic waters, or, in other words, for its ameliorating influence upon the climates of the seas, although also conferring benefits upon countries contiguous to its waters in their passage, whilst the former, from its limited access to polar waters, would, on the contrary, appear to be intended more particularly to recover the vast extent of land area, comprising the northwest of America.
Considering now the starting point of this current and its destination, the extreme difference of latitude between them, and the great distance to be travelled over; and knowing that the shortest distance between any two points on a sphere is "the arc of a great circle," we perceive that the course marked out and pursued by it is perhaps the shortest possible way, and just such as any other body would have pursued under the present physical requirements. Moreover, it will be observed that, at the various points of higher or lower latitude at which it strikes the American coast, a series of super-imposed, and to some extent, concentric arcs will be described of varying length and dimensions, according to the part of the coast taken. For instance, the arc described by those of its waters which first reach the continent in the latitude of San Francisco or Monterey would be less than that described by such as at first touch the British American coast; or than the arc described by the Japanese junk that was borne to the mouth of the Columbia river in 1831: just as, in like manner, the most southern portion of the great whirl of the Gulf-stream necessarily describes a smaller arc than that which first reaches the higher latitudes of the continent of Europe. The principle of action is this, that the shorter the distance between any two points upon a sphere, the smaller the arc described by any object in passing between them.

Furthermore, if this reasoning be correct, the lesser arcs of the current, possessing at the commencement the same temperature and travelling a shorter distance, would indicate at their termination a higher temperature than the greater ones; therefore, upon such parts as the waters forming them would impinge, a higher absolute temperature would be expected. We cannot assert positively that this observation has been actually verified by instrumental observations, for the record of sea-temperatures in the
Pacific is at present too incomplete, but as far as known they do tend to verify it—the sea-temperatures in the latitude of San Francisco being in the winter season somewhat higher than for several degrees above it, and higher also than the water temperature for several degrees below, it in the summer. It is highly probable that the part of this coast just mentioned is that at or near which the lowest circles, or the southernmost layers of this Japan current, reach the American coast; a supposition well calculated to throw some light upon the phenomena of the ocean at this point.

Just here, we would venture another remark in reference to this current. It is well known that, in certain latitudes, the Gulf-stream does not preserve at all times a uniform width or possess uniform dimensions, but, being subjected to alternate pressure upon the right and upon the left, its two edges are made to vary their position. In the month of September, its northern limit is in close proximity to the Newfoundland shores; but in March, is removed several degrees of latitude farther south. The intervening space is successively travelled over "once each way during the year." This, then, is the free edge of the current, and has, therefore, a greater tendency to alter its position. With the China stream, however, by reason of its land-locked distribution and the want of a free northern escape for its waters, its southern limit becomes the variable one, and hence, the same causes operating in both cases, in March and September, the points of approach to the shore would also vary several degrees of latitude. In this, we believe, is to be found the true reason why the warm waters of the winter months off the coast of California are supplanted in the summer by those having a lower temperature. But we must reserve such observations as we design making upon this point
until the discussion of the alternation of sea-temperatures here indicated is directly in hand.

Westward of San Francisco, and bounded on the north by this current, is the ocean-expanse of its common waters, possessing the ordinary characters of oceanic-waters generally, a part of which forms also the centre of drift for the Pacific, analogous to the Sargasso sea, midway of the Atlantic. Between this mass and the smaller portions flowing southward along the coast of California there exists, at all seasons, a difference of temperature, to which we wish now to direct attention.

"During the winter," says Mr. Blodget,* "at sea, on the Pacific side, the absolute temperatures are at once higher than those of the land, and higher than in summer for two or three degrees of longitude next the coast. The thermal lines bend abruptly to conform to this difference, but it is probable that, after changing position four or five degrees of latitude, they follow the parallels for an indefinite distance toward the central region of the Pacific ocean."

Here, then, the isothermals, after leaving the shore, acquire a northward turn for several degrees of latitude, and then assume the direction of the parallels toward the heart of this great ocean. The general waters must then, at this time, have a lower degree of temperature than those next the coast. In the summer season, the order of things is reversed—the cold waters being nearest the coast, whilst the warmer ones are out from it. The same author remarks on this point as follows: "Taking the observations of the Pacific, in means for areas of five degrees of latitude and longitude, we find the areas westward of San Francisco to give 56° 5', 62° 3', 64° 4', and 68° successively. The areas next southward, or between 30° and 35° of latitude, increase in temperature westward from longitude 120°, by the successive numbers of

* Blodget's Climatology of the United States, page 301. The italics are our own.
60° 5', 63° 3', 65° 7', and 66° 7', to the meridian of 140°. South of the parallel of 30° there are no summer observations on the coast. In the latitude of the Sandwich islands (20° to 25°), the temperatures increase from 72° at the meridian of 120° to 77° at that of 150° in the vicinity of those islands.” (Page 278). The isothermals, upon reaching the water-surface, are at once depressed southward several degrees, and having passed the narrow belt of cold waters, rise again to the northward in pursuing their course to the interior of the ocean. It appears, therefore, that the waters which are subject to this alternation of temperature do not compose a part of the ordinary sea-water, properly so called, or as distinguished from the current under consideration, but that they are distinct and separate. The latter experience changes of temperature only as similar parts of other seas, in obedience to the nearer or more distant approach of the sun.

Again, we have already stated that a correspondence of circulation of the two oceans, Atlantic and Pacific (N), is more fully established on their western sides than elsewhere. The existence of a stream or current of cold water coursing along the Asiatic continent southward, between it and the China stream, fixes this similarity: this current is analogous to the returning cold waters between the Gulf-stream and the American continent, and has in view the accomplishment of the same objects. This, we feel authorized in saying, is that visible part of the polar waters which goes to supply the place of the great equatorial current, of which we have spoken. It is undoubtedly only a part of the masses dispatched for this purpose, and finds its exit from the Polar basin through Behring's straits as an under-current, or partly as an under, and partly as a surface-current.
Lieut. Maury says that "the surface-current flows north through Behring's straits into the Arctic sea," but Prof. Henry relates it as an interesting fact, which he acquired from Capt. Rodgers, "that an offshoot from the great whirl in the Pacific, analogous to that which impinges on the coast of Norway, enters along the eastern side of Behring's straits, while a cold current passes out on the western side, thus producing almost as marked a difference in the character of the vegetation on the two shores of the strait, as between Ireland and Labrador."* In either case, the result would be the same to us; if it enters the Pacific through this strait, even as a surface-current, the warm and lighter waters of the other would overlay it, just as occurs in the case of the Gulf-stream and its cold counter-current. Now, bearing in mind the fact that the only access of the Arctic waters to the Pacific ocean is by means of the limited capacity of this strait—limited in comparison with those performing a similar duty in the Atlantic, we may assume that wherever, in its northern area, cold currents are found, they obtain entrance through it, and may be taken as divisions of the under-current which there flows southward. An attempt to account for them in any other way, as by deep-sea currents from the south, would be contrary, in our judgment, to all the other actions of the sea, and violative of its harmonies.

Allusion has already been made to one such body of cold water in this ocean, not immediately traceable to, although undoubtedly derived from, the waters let in at this point. This is found off the coast of California during certain parts of the year, but is not of clearly definable limits. The mysterious character of this mass of cold water—lower in temperature, even in the summer months, than the waters in its immediate vicinity, and lower also than those by which it is supplanted in the

* Patent Office Reports for 1855, page 363. The italics are our own.
winter season—is at present a source of much perplexity to climatologists and physical geographers. They recognize in its low temperature its unnatural influence upon land-climates; rudely define it; and refer it very properly to its great supposed head, the polar waters, but are at a loss how to account for its remarkable intrusion in the dry season of the Pacific slope: the rationale of its appearance at this time and place is unexplained. We have some reflections to offer upon this subject, and hope to be able to throw some light upon it.

We have already stated that its dimensions have never been determined, but the results of our investigations, with the aid of the published labors of others, lead to the conviction that its most northern point of contact with the shore is near the 41° of north latitude, and the direction of its northern or northwestern line of limit, if it can be represented by a straight line, is toward the point of Alaska. It probably extends southward on the coastline about from seven to ten degrees of latitude, and in width, or extent from the coast, several degrees of longitude. In the subjoined note,* which contains an extract

*This is a recapitulation of certain temperature data which had been given in the ascent of the coast from San Francisco, lat. 37° 45', to Fort Astoria, lat. 46° 11', and contains also the conclusion as to the highest point of latitude at which the cold masses reach the continent.

"Let us recapitulate: From San Francisco, lat. 37° 45', northward 29° 58' to Fort Humbolt, we observe a continuation of the same reduced monthly mean temperature with a strict parallelism throughout the dry season. To this point the refrigerating influence appears to be about the same. Progressing 1° 58' farther northward, to Fort Orford, we observe a sudden and material increase of 2.70 per month over Fort Humbolt, for the summer season, and from June to September an average monthly increase of 2.49; also a higher summer mean temperature by 2°.25. Then continuing 3° 27' farther up to Astoria, we have an elevation of 1°.66 per month over Fort Orford for the summer, and a monthly average of 1°.05 from June to September. Hence, we see within a distance of two degrees of latitude a sudden elevation of temperature, amounting to 2.70 per month for the summer, and within five and a half degrees of latitude, a monthly elevation of 4°.35 for the summer over Fort Humbolt, and a difference in the summer means of 3°.85. To what may this difference within so short a distance be attributed?

Now, perhaps we are better able to locate the northern edge of this cold current, and to appreciate the point near which its direct effects cease and where those of simple proximity begin. Fort Humbolt certainly falls within the range of its region of contact, for the slight and mere nominal differences that exist between it and places five or six degrees of latitude farther south afford positive proof of the fact. That it extends any distance above this point is exceedingly problematical, for an advance of not quite two degrees of latitude gives a much greater difference of temperature than has been noted for at least six degrees south of that point, and advancing five degrees from this point, such differences are observed as point to an almost entire absence of its influence, direct or indirect. It is likely that its proximity to the coast off Fort Orford largely affects its temperature distribution and prevents the more material and increased differences which would otherwise occur between it and Fort Humbolt. Besides, if we take as strictly true the remarks of Mr. Blodget, that this cold current passes "nearly due southeast" from the point of the Peninsula of Alaska, and estimates its breadth at seven or eight degrees of longitude (the distance westward from San
from an article presented by the author to the Medical Association of Georgia, at its annual session in 1859, entitled "An Essay on the Adaptation of Climate to the Consumption," etc., and including an investigation of the climate of the Pacific slope of the United States, may be found the principal circumstances that have induced us to assume the limits here assigned it, particularly its northernmost point on the coast.

In regard to the origin and track of this current, we believe the following explanation sufficient:

"It will be remembered by those who are familiar with the physical geography of the Atlantic ocean, that the cold surface-current which enters it through Davis' strait, after meeting in its southward flow the Gulf-stream, becomes divided into two parts—one of which runs as a surface-current between the latter and the continent, and the other, becoming an under-current, underlies it, forming its bed, even to the Pass of Florida, it is thought. Now, in the Pacific, the cold waters enter principally as an under-current through Behring's straits, a part of which becomes the surface-flow along the Asiatic coast, while the remainder must continue an under-current, which will either lose itself gradually in its southward course, or, if proper conditions arise sufficiently far north, will rise to the surface. We can not say whether the China stream is underlaid to its ultimate source by the cold waters here obtaining entrance, as is the case with the Gulf-stream, but we do believe that at least one of the divisions of the latter is entirely overlaid by the

Francisco, which gives the reduced mean temperature, and establish its probable northern line of limit by this assumed direction and width, it will appear to approach the United States coast somewhere between Port Humbolt and Orford, latitude 40° 46' to 42° 44'. We do not presume that this northern boundary is as distinct from the waters beyond it as some may suppose, or even as distinguishable by the thermometer as the Gulf-stream is by the eye from its cold banks. Yet, if it can be represented by a straight line, that would, we are led to believe, extend from the point of Alaska to the western coast, at or about latitude 41° north. This then, would bring within the scope or direct range of this current but an extremely small portion of the coast of Oregon, and, as has been intimated, show the position of this cold line to be "a little distance off at sea."—Southern Medical and Surgical Journal, vol. xv, Nov. No., page 781.

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former during certain parts of the year, and but partially at others, having, during the latter, found those proper circumstances to which we have alluded, admitting its appearance upon the surface.

Incidentally, we would add, Lieut. Maury states that this strait is too shallow to admit of "mighty under-currents;" and as the Pacific waters in their greatest bulk do not escape through it, but return toward the equator—thus supplying a southward flow—there is no necessity for such "mighty under-currents" as might otherwise be expected. If we assume the amount entering this ocean by means of this under-current to be equal to that escaping from it, it must appear insignificant in comparison with the amount borne northward by the China stream, and therefore can not be intended as a full supply for that withdrawn from the Indian ocean.

As to the causes of a division of this under-current from the Arctic sea, we may remark that, besides the lighter properties of the warm southern current, which preserves a part of it as a deep-current, the physical obstacles—the Alentian islands—between which its waters have to pass as through the meshes of a sieve, are themselves sufficient to divide it, and to promote a wider separation of those divisions. This being effected, and the subdivisions being now, as it were, unrestrained in their southern tendency, would select such routes as are most favorable to a direct passage. Thus we may suppose that those upon the right would tend toward the Asiatic coast, whilst those upon the extreme left would be led to seek a more direct or shorter pathway to the south, along the American coast. More than this: do we not see, in the presence of those numerous groups of islands which are crowded together so thickly in the southwestern part of the ocean-area, another obstacle calculated to aid in determining the course of the latter
branches? It may be that the capacity of the ocean, in that portion of it, is so fully taken up by the remarkable current that we have considered, and so far encroached upon by these islands, that the great body of under-moving cold water can not accompany the China stream, with its channel as a guide, to the southern sea. In other words, by reason of the shallowness of this part of the ocean, as indicated by the island-areas, the great channel, so to speak, for the southward flow of arctic waters, is thrown partly along the "American continent. These several circumstances tend, in our opinion, to point out the reasons for the adoption of the course pursued by that branch which at times wells up to the surface on the coast of California. The well-known fact that the Gulf-stream and its underlying cold bed occupy the deepest part of the Atlantic ocean is, to some extent, corroborative of the supposition that this left-hand branch is seeking a deeper channel.

But why, it may be asked, should this current make its appearance upon the surface on this coast during the summer months alone, and why its disappearance in the winter? And in answer to this, we must be allowed to refer again to the analogies of the Atlantic ocean. We have spoken of the changing limits of the Gulf-stream: what were the instrumentalities by which those changes were effected? We can not do better than to give the language of the great expositor himself: * "Therefore, though the waters of the Gulf-stream do not extend to the bottom, and though they be not impenetrable to the waters on either hand, yet, seeing that they have a waste of water on the right and a waste of water on the left, to which they offer a sort of resisting permeability, we are enabled to comprehend how the waters on either hand, as their specific gravity is increased or diminished, will impart to the trough of this stream a vibratory

* Lieut. Maury: Physical Geography of the Sea, page 44. The italics are our own.
motion, pressing it now to the right, now to the left, according to the season and the consequent changes of temperature in the sea." Here is the manifest reason for the appearance of these waters at this season at this place. As soon as the sun crosses the equator and commences his course through the northern hemisphere, in March, the vast ocean-mass in the interior, and standing off a few degrees of longitude from this coast, begins slowly to imbibe heat, which rapidly increases to the summer months. In proportion as it becomes elevated in temperature, its specific gravity is altered and its expansion produced; and by reason of the consequent pressure upon the free southern edge of the warm and resisting waters, the latter is forced several degrees of latitude above its lowest winter position. As this northward displacement of the warm waters is taking place, the cold masses, which have heretofore been pursuing their silent course as an under-current, lift themselves to the surface at such points in their pathway as have been relieved of the superincumbent warm waters, by reason of a lower specific gravity than the common sea-water. Their specific gravity, then, though greater than that of the great western current of warm water, is nevertheless less than that of the common sea-water, by virtue of which they rise to the partial displacement of the latter. It may be that these three different bodies or classes of water are identical in character, though not superimposed one above the other, with the stratified layers of the Arctic sea, as reported by Commodore Rogers: * "His observations show uniformly this arrangement or stratification in the fluid masses of the Arctic ocean—warm and light water on the top, cold water in the middle, and warm and heavy water at the bottom." Just as long, therefore, as the present relations are sustained and the

* Smithsonian Report for 1856, page 349.
pressure maintained, just so long will the cold masses continue as surface-waters. And this is found to be the case, for throughout the months embraced in the dry season of California—the period of increasing heat—they are present in some degree, and are felt in some measure in their chilling influence.

On the other hand, as the sun retreats to the southern hemisphere, carrying with him his warming influences, the sea-water slowly reduces its temperature, contracts upon itself, and recovers its lost winter specific gravity; and with these changes gradually withdraws its pressure, consequently the free edge of the warm current again returns southward, overlying the cold ones, and causing their disappearance. Hence, in the winter season, the cold ones have been entirely supplanted by those warmer and lighter. In these various modifying and forming circumstances is also seen the reason of the indistinct and indefinable limit of this body of cold water. It is simply a remarkable intrusion of cold water in the summer, irregular in outline and transient in duration. The limit of the northern edge of the Gulf-stream at certain latitudes varies, from March to September, from five to six degrees of latitude (from 40°–41° to 45°–46° at "the meridian of Cape Race"), and this, we think, is about the extent of variation of the southern edge of the warm waters in the Pacific. The variation of the former, off the grand banks of Newfoundland, "is, as the temperature of the waters of the ocean changes, first pressed down toward the south, and then again up to the north, according to the season of the year." The same is true of the other, and, except as a result of scientific inquiry, would be as little thought of, were it not for the unnatural impressions made upon us by its occurrence in the summer season; and this last, we the more readily perceive, because its refrigerating character is made more sensible to us in consequence of
the aid furnished by the general atmospheric circulation which drives the air inland. On the Atlantic side, the Gulf-stream's variations, or the nearer and more distant approach of its warm waters to the Newfoundland shores, and the subsequent substitution of cold waters for them, are of little consequence, because the westerly atmospheric movements bear its influences from us, and, as is the case, we would seek no knowledge of it, as we have said, except as a scientific fact, and as a curious feature in the history of the Gulf-stream.

Passing now to the last clause of our subject, we would remark that we propose simply to indicate the general influences of these peculiarities of the Pacific circulation upon the climate of the American coast, and not to attempt a minute examination of them. The latter, as far as the present record will admit, we have already done in the essay before referred to, to which we would respectfully refer our readers.* Moreover, in our notice of the general influences, we shall be very brief, and shall, at the same time, draw from the writings of others.

These general influences are not difficult to trace, for the warm waters of the China stream, coming full upon the extreme northwestern part of the continent, confer upon its immediate coast features similar to, and to some extent identical with, those conferred upon the corresponding latitudes of western Europe by the Gulf-stream. The difference of the results of the two currents, or the loss of a positive identity of effects, results from the greater loss of temperature sustained by the former, by reason of the greater width of the Pacific ocean: the difference, however, is one of degree only. Wherever these waters strike the coast they exert a softening influence upon its climate, by mitigating the severities of the winter season and by elevating to some extent the summer heat, in the

* Southern Medical and Surgical Journal, Vol. XV, commencing with the May number.
higher latitudes; and withal, tend to the preservation at all times of a comparatively uniform temperature condition.

"North of the 45th parallel," says Mr. Blodget, "the cool, humid summer of the west of the British islands and Norway exists, with apparently no great measure of difference in like latitudes. It is little known as yet, except on the coast of Oregon and at Sitka, but where not shut in by rugged mountains it is very favorable, at least to the 55th parallel. Vancouver's island is peculiarly favored, and its area is large enough for a flourishing state. It is said that here the summer is warm and productive, and that all branches of agriculture common to the latitude, 48° to 52°, in Europe flourish whenever undertaken. It cannot be otherwise than that a large area of valuable lands and favorable climates extend between this point and Sitka, at lat. 57°. At this last point, the saturation becomes excessive in summer as well as at other parts of the year, and there is almost constant cloudiness and rain. Richardson says that 'the climate of Sitka is much warmer than that of Europe at the same parallel, but the atmosphere is charged with vapors, whose condensation occasions almost constant rains. In the month of July, the sun is seldom visible more than three or four days, and then only for an instant. The humidity gives astonishing vigor to the vegetation, yet corn does not grow there; and, in fact, the want of level surface is an impediment to cultivation.'" Some idea may be had of the excessive vegetation, when we consider the size of the pine and spruce trees grown here, as given by the same author: they attain "a diameter of seven feet, and a height of one hundred and sixty feet." The productive capacity of the region, however, is not so extensive. It is said that "along all this immediate coast Indian corn, the characteristic American staple, fails to
come to perfection, and at the greatest exposures will not grow at all. In the valleys opening to the sea it will often grow a slender stem of nearly full height, but with no tendency toward formation of grain. The summer at Vancouver's island is more favorable to it than at Monterey, thirteen degrees of latitude southward, though it is believed that it is scarcely cultivable at the first locality—at least its cultivation there does not appear in notices of the islands."

In regard to the correspondence of the temperature condition on this coast and along the west of Europe, the same writer (Mr. Blodget, page 209) continues as follows: "the identity of the two west coasts in regard to climate may require the citation of some statistics in this connection, to establish it. In latitude there is little difference between the southwest of England and Vancouver's island, and the correspondence of climate is quite decided. Though we have no instrumental observations at the last point, we are informed by navigators that there is little frost in winter, and that vegetation advances rapidly in February and March. The whole climate, indeed, is peculiarly soft, equable, and English. It is such on the coast of Oregon and California as has been described, and particularly at Sitka and the upper intervening islands. In the statistics the same low curve of differences among the months, and the same low range of variation for the day, belong to both." "The average ascending difference being 4°.2 for each month at London, and 4°.4 at Steilacoom, the most nearly corresponding American position. At Paris this average is 5°; at Sitka 3°.8, and at Fort Ross only 1°.88"

This is, perhaps, sufficient to convey a general idea of the character of the climate, at all seasons, north of the latitudes which are the seat of the alternating warm and cold masses of water. The latter comprise the coast of
California and a small part of Oregon. During the winter, or wet season, of the Pacific slope, in which the warm waters circulate off the entire coast, the same general features are continued to the more southern part—due allowance being made for the difference of latitude. Thus at this season the mean temperatures along the coast are brought very nearly to the means of the water-temperature, and a degree of uniformity and regularity of temperature maintained, that, in some of its parts, is without a parallel in the eastern United States. So great is the consequent uniformity that, with the aid of the altitudinous interior of the Pacific slope, "the decrease in the mean is but ten degrees for fifteen degrees of latitude, from San Diego to Astoria, or two thirds of a degree of temperature to one of latitude. Continuing to Sitka, there is a diminution of six degrees of temperature for eleven of latitude, or nearly the same proportion;" and "of isothermals differing five degrees, but three can be made to cut the Pacific coast from San Diego to the 49th parallel."

But in the summer, or dry season, of the southern part of the Pacific coast, the refrigerating influence of the cold waters circulating off the coast is so great that the climate is rendered exceptional, and its various mean temperatures are reduced below those higher up on the coast of Oregon and Washington territory. The effect of this mass of cold waters upon the adjoining land-area is greatly enhanced by the configuration of the continental mass itself; the close proximity of the San Joaquin and Sacramento valleys to the sea, with their furnace-like temperatures, is the chief element of the latter. As the temperature increases in these valleys with the approach and progress of the dry season, and simultaneously with the

* On the Atlantic coast, the temperature diminishes "at the rate of two and seven tenths degrees of the thermometer to one degree of latitude—a ratio, in comparison with that of the Pacific coast, of more than four to one."—Army Meteorological Register, page 710.
rise and influence of the cold waters themselves, the impressions of the latter are more perceptibly felt. So that in the summer months the greatest intensity of action of the latter is manifested, and so great is it that, at certain points of the intervening coast structure, the mean temperatures of the summer months indicate but a slight advance over the spring means, and the first fall month affords the highest monthly mean for the year.*

The indraught of the cold atmosphere of the Pacific in this latitude is the result of the combined agencies of the heat of the interior valleys and the cold off the coast. And where these conditions obtain, the remarkable fact that we have alluded to is produced—that the means of the summer months at places from two to fifteen degrees farther north are above them. The positive reduction of temperature along the coast of California would be an interesting fact, if it were possible to determine it; but an approximate calculation made by the writer in the essay already quoted from, in which the most exposed point of the coast, San Francisco, and the temperatures at Fort Miller, in the interior valley, were taken, gave an

* Some idea may be formed of the degree of refrigeration experienced at such points by the following extract from our former article: "At San Francisco, we find the mean temperature of the month of June, so great is the influence of the sea, reduced virtually to the temperature of the latter (the sea-temperature 56°.5), there being a difference of only 0°.36 in its favor. This influence of the sea-temperature is still farther exhibited in the trifling increase of July over June, the advance being only 1°.04, and over that of the sea itself only 1°.40. August, again, while it recedes 0°.68 from the mean of July, is yet 0°.36 higher than that of June, and only 0°.72 higher than the mean water-temperature. September, instead of manifesting a decline in its mean temperature, when compared with the last two summer months, at San Francisco, as is also true of Monterey, shows the highest mean temperature of the dry season along the coast of California." "Comparing it with the mean of the sea-waters, it is 1°.76 higher in its mean temperature."—(Southern Medical and Surgical Journal, vol. xvi, July number: An Essay on the Adaptation of Climate, etc. By the author.) And again, when comparing the Pacific coast with the State of Florida: At San Francisco, "the whole amount of augmentation of temperature during the entire period from April to September, as evidenced by the difference of their means, is only 2°.80. May, instead of giving a mean from 4°.72 to 6°.94 above that of April, declines slightly from it, and June only increases over May so far as to exceed the mean of April by 1°.49; from June to July, the increase is only 1°.04, whilst August retreats so far from July as to make its mean only 0°.36 higher than that of June; and, finally, September gives a mean of 1°.04 above that of August, but only 0°.36 over that of July. The entire period of a declination of temperature occupies here a single month, and amounts to only one third of a degree. By this nominal decline, the month of October presents the same mean that July has. Hence, an unparalleluniformity of temperature-condition exists here from April to October. At other places on the coast of California, similar, though not identical results are manifested, and for some degrees of latitude, both northward and southward of San Francisco, a corresponding uniformity is observed, so that the advance of temperature, during the summer, amounts to only "one degree for one hundred and twenty miles north."—Southern Medical and Surgical Journal, vol. xvi, February number; same article.
average reduction at the first of $28^\circ.97$ per month for the summer season. Finally, it thus appears that the relation which these peculiarities of the Pacific circulation sustains to the climate of the contiguous shores is a controlling one—endowing each season of the climatic year with the features common to the particular current then present off the coast. Moreover, the climate of the immediate coast in the dry season is subdivided in such a manner as to create the lower mean temperatures in the southern latitudes, and the higher in the northern, during the summer months proper.

In conclusion, while making a free acknowledgment of the liberal use of the labors of others, we would remark that we have only been induced to record our reflections on this subject by the remarks of Lieut. Maury, that "'stay-at-home travellers,' as well as those who 'go down to the sea in ships,' are concerned in the successful prosecution of the labors" involved in a scientific study of the sea. And the writer trusts to the shield thus generously thrown by this encouraging call over the apparent presumption of one who is practically but little acquainted with the sea.

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Compound Fracture of the Os Femoris healed in four days.
By L. A. Dugas, M. D., Professor of Surgery in the Medical College of Georgia.

Capt. A. D., about twenty-two years of age, was wounded on the 21st of August, 1864, near Winchester, Va., in the battle of Summit Point. The missile, supposed to be a minnie ball, struck him on the left side of the scrotum, and, passing between the testicles, entered the right thigh, fracturing the femur near the junction of the upper and middle third, and remaining in the limb. The wound being considered mortal, he was carried to a neighboring house and left there with a cold-water dressing, but without being splinted.
On the fourth day the wound in the thigh was entirely healed—no suppuration having taken place. On the 19th of September the fracture had united sufficiently for him to undertake the journey to Augusta, Ga., without a splint. Upon his arrival here (27th Sept.) I visited him, and found that the fragments of the femur had united, with some overlapping and a shortening of two and a half inches, but no other deformity. The captain related to me the history of his case as now written out.

The wound of the scrotum proved very painful; one of the testicles became much swollen, and he suffered severe attacks of neuralgia along the spermatic cord, on his way home; but when he reached here all was well. He subsequently suffered again several attacks of the neuralgic pains.

May 20, 1866.—Capt. D. informs me that, during a recent visit to the seaboard, he had an abscess formed, which opened at the orifice of entrance of the missile in thigh, but that it soon healed without discharging any foreign body nor fragment of bone. He states that it was carefully probed by a skillful physician, who ascertained that it did not communicate with the bone nor ball.

The captain is now (Nov., 1866) in fine health, and feels no inconvenience from the presence of the ball.

This case is remarkable as an illustration of union by first intention in a gunshot wound of the thigh, and of the successful treatment of an important fracture without the use of splints nor of any other retentive appliances. Every one must have observed with what facility fractures become consolidated in the lower animals without the interference of art. A fracture of the leg in a cow came under my observation, which, although causing the limb to dangle loosely for some time, ultimately united without leaving the slightest deformity. Such facts teach us, at least, that it is not absolutely necessary to prevent all
motion between the fragments in order to secure their union.

I may be permitted to relate another case of

*Gunshot wound which healed by first intention.*

In 1862, Mr. J., a young man, twenty years of age, was handling a small pistol, when he accidentally shot himself through the palm of the hand. No bone appeared to be broken and there was but little hemorrhage; yet the blood continued to flow, and seemed to be arterial. I applied a thick compress, wet with cold water, to the palm of the hand, and bound it down firmly with a roller bandage, which closed the dorsal orifice of the wound also. The dressing was ordered to be kept wet with cold water and the hand placed in a sling. On the fourth day, I removed the dressing and found both orifices healed without the least evidence of suppuration. The dressing was, however, reapplied and worn for eight or ten days longer, merely as a measure of precaution. No deformity resulted.

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*Report of a Compound Fracture of the Femur uniting without suppuration.* By DeSaussure Ford, M. D., Professor of Anatomy in the Medical College of Georgia.

C. Rollins, of the army of Northern Virginia, nineteen years of age, athletic, of sanguine temperament, weighing 160 lbs., a farmer, was wounded in the first battle of Manassas, and removed from the battle-field to the General hospital at Culpepper Court-house, Virginia.

The ball entered behind and below the left trocanter major, fracturing the femur, and was found in the abdominal walls, in the middle of the inguinal region of the opposite side, from whence it was extracted. The foot was extremely everted, and after being straightened,
if released, would resume the unnatural position; crepitation was distinctly felt during these movements. The shortening was half an inch. I saw him a week after the injury, and so slight, almost imperceptible, was the swelling; the wound of entrance healed, no discharge of pus ever having been noticed, and only complaining of pain when the limb was straightened. I hesitated to interfere, as by this early union of the soft parts the fracture was converted into a simple one. On the tenth day, however, I straightened the limb, and applied extension by attaching a light brickbat to the foot, by means of a bandage, which passed over a railing at the bottom of the bunk, allowing the weight to hang near the floor, and a long splint, extending from the foot to a point six inches above the iliac crest, adjusted to the outside of the thigh by interrupted bandages. The weight of the body acted as a counter-extending force.

Six weeks after the injury he was furloughed—the fracture having united without any suppurative inflammation. There was slight eversion of the foot and shortening a quarter of an inch after the treatment was completed.

The extracted ball was rough and jagged, the symmetry having been so destroyed with difficulty it was determined to have been a round ball. The minnie ball striking a long bone usually splinters it, causing fissures which extend many inches above and below the point of contact; whereas, a round ball, projected by a musket, generally merely fractures the bone, the solution of continuity being comparatively limited. It was the experience of many Confederate surgeons that compound fractures of long bones, caused by round balls, resulted much more favorably than those produced by the minnie ball.
On the Action of Fungi in the Production of Disease. By Dr. Tilbury Fox.

There are many questions connected with the action of fungi in the production of disease which are most interesting as well as important. Thus it is questionable whether these so-called parasites are vegetable or not in nature, and whether they are distinct species, or only varieties of one species. It is now admitted by all that the fungi are really of a vegetable nature, and that their germs are derived from the exterior.

The mode of entry of the Fungus into the system.—There is no difficulty in accounting for the access of germs to living bodies, for these germs are freely distributed and disseminated in the air. The best illustration of this fact may be noted in the experiments of M. Bazin (Gazette Méd. de Paris, July 30, 1864), which consisted in passing currents of air over the head of a favus patient, and thence over the open mouth of a jar containing ice. The ice cooled the air, causing the deposition of moisture, in the drops of which the achorion sporules were detected. The same thing may be shown by holding a moistened glass slip near the head of a patient, and just rubbing his scalp freely. Of course, actual contact is much more effectual in the implantation of germs. But, without delay, let us suppose that the sporular elements find their way to the human surface; how get they deeply into the tissues? In various ways, probably. Let us take a general sketch. The greater the degree of moisture and heat, the better is the chance of entrée.

First of all, the fungus elements may enter by fissures or natural orifices; for example, in ordinary ringworm the sporules lodge themselves at the opening of the hair follicles, and presently get beneath the epithelial scales. We shall see, directly, how. A great many experiments have been made at different times, upon this point, in the case of plants. De Bary (Die gegenwartig
herrschende Kartoffelkrankheit, ihre ursache und ihre Verhutung, Leipsic, 1861) found that, in terminal filaments of potato mould, so-called zoospores were formed, which bud, protrude filaments forming a mycelium which has the power "of penetrating the cellular tissue in twelve hours, and when established there it bursts through the stomata of the leaves." This "boring" operation is quite likely to occur, especially where the structures are diseased; as, for example, the muscardine in silkworm, in diseased mucous surfaces or epithelial changes; here the entrance by continuity is easily accomplished by the growing filament. It has been supposed that mycelium may get within the shaft of the hair, in some part of its course, in this way. I do not believe it. If a fungus finds an entrance, it is either through a cut end, a distinct fracture, or, what is usually the case, the soft, growing root. A good deal of doubt has been expressed as to whether the spores could find their way into the interior of plants through the stomata. It seems pretty clear that the latter are not sufficiently large for the occurrence. There can be no question, however, that, in a large number of instances, the spores send out little processes, which get into the plant through the stomata. Here the recent experiments of De Bary help us again. They are noticed by Mr. Cooke, in his admirable popular work on microscopic fungi. The observer took a large number of common garden-cress plants, placed their roots in water containing zoospores, and though the former became covered with these latter bodies, yet not a jot of evidence of penetration occurred. De Bary, however, found that if the cotyledons, or seed-leaves, are watered with fluid containing zoospores, that slender tubes put out by the zoospores enter the stomata, the terminal ends enlarge, branch out, and become the centres from which a ramifying mycelium is produced, which presently
shows itself externally. De Bary tested 105 plants in like manner and under similar circumstances, with water free from zoospores, and without the production of any sign of rust in these. De Bary concludes that plants are not infected by spores entering through the roots or leaves, or through the medium of the seed-leaves of cotyledons. But it is probable that the fluid contents of the spore-cells may be absorbed and give rise to disease. The Rev. Mr. Berkeley, several years ago, found that the germs of bunt placed in contact with seeds infected them, without there being any evidence to show that any spore or mycelial thread had effected an entrance. It seemed as if the granular fluid contents were taken up by the plant and caused mischief. It is possible that minute threads might have penetrated the seeds nevertheless. There is, however, no difficulty in supposing the granular contents of spores (sporules) capable of reproducing the typical spore. But, in the next place, there can not be a doubt but that in the human subject the germs of the fungus find their way to the roots of the hairs, and are carried bodily upward into the shaft in the process of growth, developing as they go, till at last they degenerate and break up the fibrous structure in which they are. By analogy we should quite expect that such a thing is possible, and, indeed, of frequent occurrence in the case of the tender roots of plants; and this is more likely to happen when the contents of the original spore (which is as large as the spongiole cell) happen to be discharged by bursting. Moreover, it is quite clear that the germs of parasites enter at a much earlier period than we are apt to imagine, and lie dormant, brooding mischief till the favorable opportunity arrives. De Bary proved this in the case of the white rust (cystopus) which hybernates, as it were, in the sub-epidermal structures during the winter, till the spring arrives. In addition,
the fungi "make head," so to speak, into structures in virtue of the chemical action which they set up. This is best seen in the hard structures of animals. Carbonic acid is given off at the terminal cells. This dissolves the lime of the shell and allows the parasite to effect an entrance most easily. The experiments of Wittich, quoted by Robin, all tell in the same direction. Panceri has come to the conclusion, however, that, in the case of the egg, the minute germs effect an introduction through minute microscopic holes which exist in the shell. Lastly, traumatic lesions afford an easy channel for the conveyance of fungi to deep structures. This is what happens in the mycetoma or fungus foot of India.

We have then, as modes of entrance—(1.) That through natural orifices; (2.) That in which the growing force forces the mycelial thread beneath the layers of the superficial tissues; (3.) That in which processes shoot out from the spore and enter by such openings as stomata; (4.) That where the cell contents are absorbed; (5.) That in which the spores are carried bodily inward by growing parts; or (6.) dissolve away the opposing structures by chemical action; or (7.) enter by traumatic lesions. In each and every instance the germs of parasites are derived ab externo and not generated spontaneously.

There are some special circumstances that deserve comment. It has been asserted that microscopic entophytes have been discovered in close cavities utterly cut off from communication with the external air. But these instances are open to grave objection; fungi have been found in the fluid of the ventricles of the brain, which, however, was allowed to stand all night exposed before it was examined. Again, it is asserted that in the kidneys the like has been found. This is open to exactly the same objection. The case of the egg parasite has been explained away by Panceri; and it has yet to be shown,
supposing the urine has ever during life contained fungus elements, that air can not enter the bladder. The case of germs of vegetable nature in the blood-current presents some difficulty; but even here the most considerable caution is needed. We know that fungi spring up with enormous rapidity; and it must be proved that those spores and mycelia are present at the moment of death, nay, during life, before we can give credence to any theory which asserts that they have been present and introduced during life, and not by a communication with the external air. It is still a question whether the endosmotic action of the villi may not be able to account for the presence of cryptogams in the blood-current. As far as the facts of vegetable parasitism go, we are bound to deny any such occurrence. And, upon analogical grounds, I venture to assert that the entozoa found in muscle, which have lately caused no little sensation, are not vegetable in nature. Should they be proved so, it will entirely alter the whole subject of vegetable parasitism: for we are justified at present in asserting that there is probably no known instance of a growing plant in any situation not in direct or possible communication with the air. I am bound to say that Dr. Thudicum believes in the vegetable nature (see Report of Medical Officer of Privy Council for 1865) of the rinderpest entozoa (?)..

I pass to the consideration of the part played by fungi in diseased states. Two theories the most opposite in intention have been held by writers and others, so opposed that really the conclusion is forced upon one that both must be wrong and a middle belief correct. Whilst one batch of inquirers affirms that parasites are accidental, another contends that they are the essential cause of those diseased conditions found in "association" with their growth. Ehrenberg, in speaking of organized parasites at a time when the exact nature of many of them was
indistinctly recognized, said "that there is more cause
for wonder at the limitation of their effects by the actions
of living bodies they inhabit than at any morbid effects
they appear actually to produce." It must first of all be
noted that there are certain conditions which are pecu-
liarily favorable to the growth of vegetable parasites. The
latter are ubiquitous, capable of resisting the action of
heat, cold, and decomposition, have a tremendous and
rapid power of increase, and will remain for a very long
time in a state of inactivity; yet, notwithstanding all
this facility, there are certain states of organisms against
which they fail; which will somehow resist their inroad
and attacks; and it is now clear that though parasites
may for the moment get a temporary hold, yet they will
not flourish upon a typically healthy surface. This is a
fundamental truism that must be observed in reference to
therapeutics. For rusts and mildews prevail in direct
ratio to the wetness of the season, or after drought, as in
the pea or hop; damp itself is very favorable, and where
there is much drought the vigor and the circulation of
plants are diminished very considerably. When plants
are very ripe also, there is a less degree of vitality pres-
ent, in consequence of the cessation in great extent of
the circulation and vital connection between the fruit and
the stem. The same thing holds good in every instance
where animals, plants, or men are attacked. We may
instance the case of muscardine. The experiments of
Claude Bernard also showed that frogs kept in captivity
got out of order, and aphthous conditions arose. A
healthy frog brought near its diseased fellows "set con-
tagion at defiance," but unhealthy frogs were at once
attacked by the vegetation flourishing on the aphthous
surfaces of others; and the case of favus in man, or scab
in sheep, of which an account may be seen in the Gar-
dener's Chronicle for April 24, 1864, is illustrative of the
fact under notice.
There is always a certain resistant power about all healthy living beings; and a certain amount of fungus, however it acts, may be present without giving rise to what one can possibly call disease. In young life, of course, one would expect that fungi would obtain a hold more effectually than in old life; and it is very remarkable that the white rust before referred to, according to De Bary's experiments, should effect an entrance into the system of the garden cress, by attacking the young leaves or cotyledons. The young and tender stage becomes an easy prey; and this is exactly what we find in the human subject, the young being most liable to ringworm.

Taking all things into consideration, it is clear that parasitic disease—or, as I have named it generically, tinea—can not be explained by either of the conflicting theories I have referred to, but consists of three distinct components, which must be recognized, if the physician would cure his patient well and quickly.

1. A certain state of soil: in speaking of polymorphism of fungi, I noticed that each fungus appeared to require each its special kind of pabulum.

2. The access of air, and the presence of heat and moisture—the conditions necessary to support the life of fungus. And,

3. The introduction from without to and action upon the body of the vegetable germs.

The first and second will be passed over without comment: my remarks are specially intended to define the action of the parasite in the production of diseased states. Now, fungi are not "accidental" and unimportant, but act in several distinct ways when once they take hold and grow upon the surface. This is important; if we insist upon some one modus operandi, we shall assuredly find our position utterly untenable. They act then (often in more than one way in the same instance be it remembered):
Firstly, mechanically.—If you simply rub into the surface some of the fungus elements, in many cases you get what we know as irritation. This is seen in the ordinary herpetic ringworm of the surface, where the mycelial threads range over the skin beneath the epidermis and lead to erythema, etc. A very remarkable case is recorded by Dr. Kennedy, of Dublin, in which a quantity of flax powder was inhaled by a lad who became attacked with measles and peculiarly severe local dyspnœnal symptoms, evidently dependent upon the direct mechanical irritation exerted by the fungus elements. In the case of mildew of plants, the same thing is seen—the threads of the mycelium grow, and force asunder the tender structures near it. Now, it is this mechanical action exerted by the growing force which is at work, especially in ringworm. The fungus finds its way to the sub-epidermal space, from thence to the hair follicle, irritating and interfering mechanically with the growing parts, enters into the hair, and by its increase and development simply splits up the hair-shaft, appropriating also its juices, and rendering it all the more brittle, and therefore the more easily destructible. To declare in such a case that the parasite is accidental in any sense of the word is to turn a deaf ear to the plainest voice of facts; but this very action can be isolated. I have performed a good many experiments at different times with diseased hairs out of the body, and occasionally it is possible to get a hair containing spores, which spores will germinate and actually produce the splitting up of the hair, and the other changes that are observed in ringworm—in fact, to produce the lesion of ringworm out of the body. In those instances in which the mycelium abounds, the epithelium seems to suffer particularly. On the mucous surfaces there are no such structures as hairs which form a lodgment, so to speak, for the fungi, and hence no marked
results are visible. The cells of the tissues are invaded and destroyed, the mycelial phase abounds and ramifies in the secretion, and not in the tissues themselves; but there is the same capability of damaging when parasites attack only the internal surfaces. If we would wish for examples of the enormity of the force exerted by a growing fungus, we have only to confine some of the more ordinary varieties and see the result. Now, it so happens that no other agent can produce in disease the same kind of action as that exerted by a growing fungus—such as splits up the hairs in the way in which this is observed in tinea; and it is this state of things which I regard as the pathognomonic lesion of ringworm, viz.: the mechanical action of the parasite upon the hair and epithelium in connection with other minor changes.

One word as to definition. I use the word tinea as the generic term, and particularize each variety by the terms favosa, tonsurans, sycosis, vesicolor, circinata, etc., the tinea signifying especially the diseased state of the hair and epithelium. Now, take the case of sycosis, which means inflammation of the follicles of the chin and lips. It may not be caused by a parasite; but undoubtedly cases are sometimes caused by a fungus, and these I called tinea sycosis. Again, tinea circinata means the parasitic herpes circinatus, and tinea decalvans the baldness produced by the fungus (microsporun Audouini), as distinguished from alopecia, non-parasitic baldness, the result of many different causes. The term tinea is very distinctive.

Secondly, Fungi act by inducing local chemical change.—They absorb oxygen and give out carbonic acid; and, as has been before observed, they hereby secure to themselves the power of penetrating calcareous structures. In addition, a large amount of gas is evolved as in cases of sarcinal disease. Moreover, they lead to fatty degenera-
tion. If any one will take the trouble to examine carefully some of the old stubs in favus, he will notice a certain amount of fatty changes going on in the cell structures. Remove a hair of this kind loaded with sporules, and get the latter to germinate, and the fatty alteration goes on at a rapid rate, till after a time a large quantity of crystalline fat is produced. Now, this will not happen unless the fungus germinate; but happening, it is worked out in accordance with the views lately put forth by Dr. Bence Jones, and was expressed in precise terms in my book on parasitic diseases. It has been remarked by many observers that fat is always present in considerable amount in connection with the development of fungi. M. Signol believed that fat very much favored the development of bacteridia. Perhaps the very best exemplification of the association of fatty change with parasitism is that afforded by the case of the madura foot, where the oily matter is so very abundant. The tissues degenerate, and the crystalline fat is so varied and peculiar as to have actually misled observers into the belief that it was a form of fungus. Now, it becomes a question whether fat assists the development of fungi, or whether the latter attract fatty matter, the fungus forming a center of attraction for crystallization, or the fatty change be the result of cryptogamic growth. I adhere to my original belief, and Dr. Carter is of the same opinion, that the fatty change is coincident with, and a consequence of the growth of fungi. Nitrogenized and other matter becomes fatty in this way very readily. Of course, under such circumstances, the fungi become a center of attraction for the fat. It is a chemical action entirely, as far as the degeneration is concerned; a process of oxidation which the fungus induces under favorable circumstances, in connection with the performance of its own vital functions.
Thirdly, Fungi act as conveyors of poison.—This is a mode of influence which has been altogether disregarded by observers. If the endogenous pus-cell can convey the noxious poison of an acute disease, why may not the elements of a fungus act in a similar capacity? Recent research has shown that all fungi exhibit great transportability. Now, what action have the cells afloat in the air of hospitals during the time of epidemics, such, for instance as cholera (see Dr. Thomson’s Observations at St. Thomas’ in 1854); may they not take the virus of a hospital gangrene from one patient to another, acting the part of a fomes in the very same way, comparatively speaking, that man himself does? Suppose we inoculate with fungus elements, it is clear that in some instances symptoms ensue (as in Dr. Kennedy’s and Salisbury’s cases) before the onset of local symptoms. Again, the fungus elements would appear to be most active in their early stage, that is to say, when the poison produced simultaneously with their development is in its freshest and most active condition. Again, respirators in epidemics have been found to be efficacious. And, lastly, direct experiments, upon plants especially, have shown that disease may be produced by the contact of fungus elements, when there is not a particle of evidence to prove that sporules, spores, or mycelial threads have entered the organism of such plants, but where there is the greatest probability that the granular and fluid contents may be the poisonous compound which, when absorbed, gives rise to the subsequent malady. It is not unlikely that in catarrh and influenza especially, such a conveying property may be at work. We have the strongest possible amount of analogical evidence in regard to animal life, comprehended in all the details of the “animalculæ theory of disease”—a doctrine that may be pooh-poohed by some, but which must ere long be fairly
discussed. One might give a great deal of very interesting matter under this head. Those who are interested in the subject should read Sir Henry Holland's article, in his Medical Notes and Reflections, 4th edition, I think, on the Animalcule Theory of Life, and to Dr. Daubeny's essay in one of the volumes of the *Edinburgh Philosophical Journal*, some few years back. The occurrence of epidemics, be it noted, moreover, is often associated with the peculiar prevalence of various moulds and mildews—a source of terror and superstitious horror in bygone time, which gave rise to the idea of a raining of blood. Plutarch refers to such an occurrence in the plague of Rome. Hecker, in his work on the Epidems of the Middle Ages, also associates it with the disasters of 789 and 959. The spots were actually observed on garments, and called lepravestium; *signacula* was another term. In 1502 and 1503, it again frightened everybody. Agricola was certainly one of the first to give a rational explanation, he attributing it to the appearance of a lichen. The fungi attacked walls, bread, cheese, meat even, and garments, in Venetia, in 1819, and also articles of food, and garments, and all fruits, during the years following to 1829. And is there not something similar observable now-a-days? Have we not had some very remarkable and severe epidemics, and have not fungi been remarkably abundant on vegetation? I will not theorise, but merely just draw attention to the coincidence. The particular action of fungi now under notice will perhaps be better appreciated in connection with that now to be described.

*Fourthly, which looks upon these organisms as developers of poison,* and comprehends Dr. Richardson's forsaken theory of zymosis—a doctrine that appears to me most satisfactory. It has been suggested at different times by one and another observer, that the fungi themselves induce change actually in the circulating current, sufficient to account for
disease, either by setting up a kind of fermentative action in the blood, giving rise to the production of a specific compound—a poison, in fact, just in like way to that which happens in ordinary fermentation, or setting up change by catalysis—a wonderful enigma. Others affirm that no poison is produced in the body itself, but that the fungus helps out its increase when once introduced into the system. For my own part, I can not believe that any very important change could be induced by the growth of fungi in the blood-current. The presence of air is so very necessary; and not only mere presence, but such as is implied by a direct communication between the growing vegetation and the external air. Outside the body, or in the cavities which communicate with the air, many very important and frequent changes are induced without a doubt.

Dr. Salisbury is a careful observer. He declares, and as far as I know holds to his opinion, that a form of disease, if not identical, at any rate very like measles, results under certain circumstances from the inoculation of the fungus of wheat straw. Dr. Kennedy has given confirmatory evidence. Does the fungus, per se, produce the result, or is it a conveyer, or is it the producer of the poison outside the body in the musty straw?

Dr. Richardson, quoted by the late Dr. Barker, of Bedford, records the onset of erysipelatous mischief from a like cause. In France the most serious inflammatory mischiefs of veins and lymphatics have followed wounds inflicted with instruments used to cut off the diseased vine-shoots. Dr. Collin, the medical inspector of the waters of the St. Honoré, Nievre, records even fatal results. MM. Demartes and Bouché of Vitrany have also investigated this subject, and conclude that the oidium can produce such mischief, but they suggest some sort of coincidence between the special development of
the oidium and the occurrence in greater frequency of inflammatory disease. It is to be hoped that the French Academy will, now it has taken note of the subject, enter into a full investigation of it. It is true that ill effect does not always follow experiments with the oidium. MM. Speneux and Letellier failed to produce anything beyond a little redness and irritation by inoculating people with the rasping of leaves diseased by the oidium (Pract. Jour. of Med. and Surgery, Nov., 1864); and MM. Leplat and Taillard on the one, and M. Wertheim on the other hand, came to opposite results by injecting fungus elements into veins of dogs and other animals. There can be no questioning that some fungi are more hurtful than others, and much depends upon the concomitant conditions. The arunda donax, the large red reed of the south of Europe, is attacked by a black rust, and those who cut the reeds suffer from very violent headaches; it is affirmed by M. Michel that the spores produce a papular rash on the face, with much swelling, and a good many serious general symptoms (Yearbook, 1861-2). It would seem that the fungus, *per se*, is not sufficient, but that there is something in addition which is intimately connected with the vitality of fungus. This would seem to be taught by the case of bacteridia. Whatever they be, no injurious results happen unless the medium itself in which they exist contain some peculiar virulence of its own, just as in the case of inflammatory attacks caused by oidial inoculation. The power of vegetable organisms to induce transformation, which must of course be accompanied by distinct chemical change, has been well exemplified by an experiment of M. Lemaire, who took some beans, placed them on a moist sponge, and found that bacteridia soon sprang up, before germination, being succeeded by monads and vibriones; and the like happened after the soil used had been heated to
a red heat. Now, if a small quantity of phenic acid (which has the property of suspending infusorial development) was added, the germination came to a standstill until the phenic acid evaporated, when it recommenced. M. Pasteur's experiments on acetic fermentation tend to the like result; and M. Trècul's observations lead to the belief, that the change induced in solutions by fungi, as in the case of alcoholic fermentation, depends upon the performance of the nutritive act of the vegetable cell. The fact is, the fungus, when growing, necessarily decomposes the medium, and induces chemical change, whilst the result depends upon the composition of the material acted upon. In like manner, it is conceivable that the fungus of wheat straw acts upon the juices of the stem, producing some subtile compound; bacteridia do the same in sang de rate, and the oidium in the vine disease.

It has been supposed that the poisons of measles, influenza, cholera, nay, asthma, and some other acute diseases, may be produced in the way indicated; but it must be remembered that two or more of the modes of action already noticed may be conjoined, that is to say, a fungus may act mechanically as a conveyer and developer of poison at the same time, and in one case. But not only acute but chronic diseases are produced. I refer to the large class of instances in which vegetable parasites induce slow changes of deleterious nature in articles of diet, giving rise to "ergotism." Bad grain, bad potatoes, bad rice, bad maize, are illustrative. The late Russian epidemic, the Irish fevers, pellagra in Lombardy, gangrene in sheep and beasts, ergotism in horses, have all been regarded as taking origin especially from the play of ergotized foods. In the group of chronic maladies the material acted upon by the fungus is a solid. The access of air is not so perfect nor so free; the moisture is considerably less; all of which tends, in great measure, to
account for the difference of the quality of the resultant morbid product. The productiveness of grain so infested is considerably lowered. Sir H. Davy proved this long ago. He found that diseased wheat yielded from 21 to 65 per cent. of nutritious matter against the 95 per cent. of the healthy grain. It has been suggested at various times that the degeneration of rice by parasitic action gives rise to the formation of products which occasion very severe symptoms of intestinal irritation, resembling dysentery, and that œdema of the leg often follows. And it is not unlikely, that the many peculiar ulcerative conditions of the lower extremities are favored by the quality of food and induced in like manner. The diminution in the productiveness of the silkworm affected by muscar-dine affords a capital instance of analogical occurrence. The statistics issued by the Chamber of Commerce of Turin show that although formerly about some 650,000 myriagrammes of cocoons were produced in the country, in 1864 there were 525,000, and last year 283,000 only. I have been paying some little attention to the case of mildewed cotton—hunting after illustrative facts—and I find that the germs of mildew are really present in the cotton in its rough state, as sold in the market before it reaches the manufacturer. It of course is possible that the processes through which it passes in the hands of the latter may destroy all the vitality of the fungi, but this is not certain; but if it really does, still the fact of the presence of mildewed germs in vigor would imply the existence of a certain degree of deterioration in the actual fibre itself, perhaps induced by the bad cultivation or growing of the original plant—a point of no mean interest to the merchant. It would make the fibre less able to resist the action of the size and other agents used in the manufacture into stuffs.

I have spoken of things going on outside the body, and
then introduced to it; but within a recent time, certain facts have come to hand showing that under special conditions, though good food be taken into the stomach, yet, in the digestive tract, changes of objectionable character may be induced by the agency of fungi. I have to quote Dr. Salisbury again as my authority. He believes that chronic diarrhöea in the army is caused in this way (see the report of the surgeon-general of Ohio, in the Amer. Jour. of Med. Sciences, 1865). Wherever there is a poor amylaceous diet, and there be retention of the food, the torula, almost always present, grows, and in so doing induces fermentative changes, with the evolution of gas—the production of intestinal irritation and diarrhöea—the torula vegetating into a myceliated "algoid" mass, which may be observed in the faeces; and it appears that its amount is in direct relation to the severity of the disease; the production of sugar being rapid and detectable in the mucous tissues. The green stools of children are so produced, and Dr. Salisbury thinks also that semi-paralytic symptoms ensue. The case of sarcinal disease is on a par entirely; deranged digestion, detention of food, the presence of penicillium, and the evolution of gas with the formation of sarcinæ; vomiting is the result of gastric, as diarrhöea that of intestinal irritation. The stomach in the former, and the intestines in the latter, getting semi-paralysed, at least losing tone and getting relaxed. In both cases there is the mechanical action of the fungus and the induction of chemical changes within the body. The case of diseased foods is one of surpassing consequence, and deserves all the attention we can afford to it.

The quality and character of the poisons or products of this fermentative act are matters of no little interest. Dr. Richardson has lately deserted the zymotic, and given his adhesion to a new theory, which regards the poisons as of an alkaloid character—basing this position upon the
supposed isolation of the pyæmic poison; however, further experiment is needed to establish the truth of the new doctrine. If the poison of so-called zymotic diseases be chemically inorganic, how comes it that nothing of the kind can be obtained by chemical analysis? The diffusion and spread of disease is opposed also to such a view. There is a power of increment about these viruses which is very marvellous and peculiar. There is also a vital principle or act which is very distinct. Another feature worth notice is this, that the effect of the poison does not seem to be, as is the case with mere chemical agents, proportionate to the dose, so to speak, but to the peculiar virulence, which varies as much as the state of the nutrition of the organism acted upon. The viruses certainly, as to their characters, vary considerably, and are not definite in the way that we would expect if they were of an alkaloid nature. Independently even of these kinds of influence already noticed, fungi, in the fifth place, would seem to possess inherent noxious qualities in some cases. Just as insects have the power of producing special poisons, so may fungi in a much less degree. The anamita muscaria affords a resinous principle, which chemists isolate. In other cases—for example, the mushroom—there is evidently an alkaloid, as MM. Sicard and Schoras have shown (Journal de Pharmacie, 1865); but the action of it is different from that of viruses altogether.

Now I have mentioned five different ways in which fungi may act; and these may be summed up as follows, being divided into those which are direct and indirect: Directly, they may act mechanically, or by inducing local chemical change; indirectly, by bringing about changes in substances out of the body, which are brought to influence the latter; by setting up a kind of fermentative action in part due to the oxidation consequent upon the nutritive changes in the plant, or by giving rise to
products having an acute or a chronic action, and whose nature is at present a matter of doubt.

And now I am prepared to meet the hypothesis that parasitic disease has nothing essential to do with the development of parasites. Mr. Hunt takes the boldest view in the ranks of the opposition, declaring that the causes of parasitic disease are four, and four only—uncleanliness, atmospheric impurities, deficient exercise, and contagion. I take my stand upon the mechanical action of fungi and the induction of fatty changes, and defy any one to shake me the least from my footing. Mr. Hunt states that the above four conditions "poison the blood, producing not only their immediate effects in the form of parasitic skin diseases, but laying the foundation probably of more serious disorders, manifested in after life by the presence of lumbrici, ascarides, tape-worm, pediculi, fungi, hydatids, tubercles, and perhaps cancerous germs, in the various organisms." What does this mean? That these varied mischiefs have not each their proper cause, but arise from one and the same influence. This is surely either subversion of the logical definition of cause—unconditional sequence—which is so tenaciously upheld and received as our only true belief. I grant that the four states lower the nutrition of the system, and make it more fit for parasitic growth; this is only one item of the total. The true state of the case I take to be this: That there is a necessary nidus, which is exalted by some into the position of the supreme disease, to the negation of any and every effect produced by the fungus itself, which finds the soil congenial—a soil associated with bad living and bad hygiene of all kinds; the fungus growing acts in the various ways already detailed; in ordinary cutaneous affections, the effect upon the hair and epithelium (mechanical and chemical) being pathognomonic. Parasitic disease, then, is a composite affair, consisting of mal-
nutrition, a growing parasite, and certain effects of such growth.

There is yet one category of facts that needs a word or two of comment, viz.: the comparative pathology or the intertransmission of parasitic (vegetable) maladies. In addition to what I have given in my work, a good deal of information has been accumulating. It is now admitted that the transmission of the common ringworm of the surface from animals to man is very common. I am informed upon good authority that this is of very frequent occurrence in Australia, the milkers of cows especially being largely affected. Professor Gerlach (abstract in Ed. Vet. Rev., vol. ii.) has noticed it in dogs, horses, and oxen, and in man, but the sheep and pig seem to offer exception. Dr. Frazer (Dub. Quart. Jour. of Med. Science, May, 1865) contributed a paper, “Remarks on a Common Herpetic Epizootic Affection, and on its alleged frequent Transmission to the Human Subject,” containing cases. This gentleman quotes Mr. Brady and Mr. Whitla, in reference to other instances. Dr. Fehr has noticed in Switzerland the transmission from cattle to man. I can confirm by my own experience the truth of these statements. I do not mention any old cases, such as mice affecting man, but my friend, Dr. Allchin, informs me that he has seen the transmission of mange from a cat to a child.

Now, I might argue just in like manner in regard to the animal parasites. The two classes of cases are mutually illustrative of each other’s modus operandi. I take the case of scabies. The acarus demands a suitable soil. It has been pretty well shown, in animals especially, that acari will not grow on all surfaces, but only on those whose hygienic condition we have reason to know, from the circumstances that have been at play, is not that of health. The limit of variation is by no means made out
in the case of the animal parasites. The relations of acari on bodies generally is being canvassed, especially by German writers. The mode of entry has an analogy also. There is the same difference of opinion as to whether the acari are accidentals or vera causa; but there is plenty of evidence to indicate the iraction as mechanical irritants, and as the possible developers of irritant products. But these points I can not now enter upon.

The matter of the action of fungi is a large and wide one; already we see enough to show that the studious inquirer will be amply repaid if he tread carefully the somewhat now uncertain path before him, and the promising indications of success are many.—*Edinburgh Med. Journal*, April, 1866, p. 875.

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**On Fevers.** By Dr. H. Bence Jones, F.R.S.

[Perhaps the term zymotic disease is as good as any to designate the action of those poisons which get into the system in such a mysterious way that we can not explain them. The term implies some action by which different vegetable or animal ferments, living or dead, can be introduced into the body and produce different diseases, such as small-pox, scarlet fever, measles, typhus fever, etc.]

Of all the modified peroxidations that can occur in the body, small-pox is the most definite, because the poison can be got apart, and the quantity necessary for producing the action can be fixed, and through the most glorious discovery of vaccination it can be set in action whenever we please. We can almost see it passing from the cellular tissue into the blood, and from the blood into every particle of every texture, rendering it incapable of undergoing the same action again.

Let us look a little closer at this action of small-pox poison. If the minutest particle of substance, a little
dried albuminoid substance, in a peculiar chemical state of action, on a lancet, or in the dust of the air, is put into the cellular tissue or is inhaled into the lungs, it passes on to the blood, and through it into every texture. In a few days the chemical actions of oxidation and nutrition throughout the body are altered, and the particle of matter has reproduced itself immeasurably. The violently increased chemical action, the peroxidation, is shown by the increased heat of the body, the violent fever. The altered nutrition is evident, not only in the eruption of pustules in the cellular tissue under the skin, but in the altered condition of the blood and in all the textures of the body; each particle of substance being rendered incapable of undergoing the same process again, and by assimilation every future particle that takes the place of every modified particle is also generally incapable of being modified again.

Throughout the course of the general peroxidation, and more especially at the end of the fermentation, local peroxidations frequently come on in any part of the body. Inflammations of the eyes, the ears, the mucous membranes, the joints, the serous membranes, the parenchymatous tissues, anywhere an unmodified peroxidation is ready to begin, and this easily gives rise to suppuration or causes obstructions which the feeble circulation can not overcome.

The most striking facts concerning this small-pox ferment are, first, the very small quantity of substance that produces so much effect; secondly, the immeasurable increase of the poison in the body, each pustule having the same property as the original ferment; thirdly, the period of incubation during which the poison must at first slowly increase in every texture, and there give rise to the modified peroxidation and altered nutrition which constitute the attack.
The poison of scarlet fever, of measles, and of typhus, though less tangible, are not less substantial than the small-pox ferment. Like it, they can most probably be dried and carried from place to place, and pass into the mouth with the dust which we each moment inhale or swallow. In chemical composition, scarlet fever, measles, and typhus ferments most probably resemble albumen in complexity, and, like albumen, they may be altered in composition and action by heat, alcohol, arsenic, tar acids, and many metallic salts. As soon as they reach any spot where they can oxidise, they set up an oxidation and reproduction in each contiguous particle of albuminous substance. From the cellular tissue, the air passages, or the stomach or bowels, the contact action spreads into the blood, and there it multiplies, whilst it is carried into all the capillaries, and through them into every texture of the body; then the increased oxidation and formation of ferment becomes most violent, and fever to a greater or less degree is present. Long after the strongest action is reached, a slower action continues, and at any time or in any part or texture of the body, whilst the specific chemistry is going on, an ordinary local peroxidation may be lit up, and a more or less acute inflammation may be added to or follow the fever which the ferment has produced.

During the height of the fermentation in typhus fever, the heat may rise to 5, 6, or even 10° Fah. above the ordinary temperature; and when the fermentation is ended, the albuminous textures of the body are so changed that they are incapable of going through the same process again. Between these two results there are innumerable other products of chemical change, varying with the kind and degree of fermentation. In typhus fever it is said that urea is increased and carbonic acid diminished. To these and a multitude of other chemical
questions regarding fermentation chemistry will give definite answers; but above all questions, one of the most difficult to answer and yet one of the most important, is the amount of oxygen that is consumed in the different kinds and degrees of peroxidation which can take place within us.

In each organ, according to the intensity of the action set up by the ferment, altered functions may arise, and these may be still more altered when an ordinary peroxidation at the same time takes place. Thus the brain, heart, lungs, kidneys, liver, or any texture composing these organs may show by more or less wrong mechanical results the effect of the ordinary or modified peroxidation; and the effects of the fever and of the inflammation may be so mixed that neither during life nor after death may any accurate separation be possible.

Closely related in chemical composition to these violent ferment are the less active ferment of ague and typhoid fever. There is so little difference in the chemical composition of animal and vegetable substances that the distinction between animal and vegetable poisons is no longer possible. Vegetable albuminous matter undergoing change may produce almost, if not quite, exactly the same poison as animal albuminous matter. Hence, probably, the resemblance between ague poison and the typhoid fever poison, and the possibility that sometimes one and sometimes the other of these poisons may be formed from the same changing matter under different circumstances.

Ague ferment is probably a highly complex nitrogenous substance, capable of being dried and carried by the wind far from the place where it was produced. It enters by the mouth with the dust, and, like animal or vegetable alkaloids, it passes from the blood into every texture of the body, and acts on each much or little, according to
its chemical properties. Probably it acts most strongly on the nerves that regulate oxidation, causing for a time contraction of the arterial vessels, and consequent sub-oxidation everywhere. The increased obstruction of the small arteries reacts on the tension of the blood, and this produces increased contraction of the heart, which continues to increase until the obstruction yields and a state of peroxidation is set up, by which the poison is partially destroyed. During the remission, probably, the poison is reproduced until sufficient, in from one day to three days, is formed to go through the same action again.

This theory of ague admits of a reasonable explanation of the action of quinine and arsenic in stopping the paroxysms of the complaint. On the ague poison itself quinine and arsenic may have no action, but they pass into every texture from the blood, and, combining with the nervous substance on which the ague poison acts, they form a compound on which the ague poison is incapable of producing an effect before it is oxidised and destroyed.

The ague poison, unlike the small-pox or typhus fever ferment, instead of protecting the body by making it incapable of undergoing the same action again, makes the nerves more ready on the slightest renewal of the poison to undergo the same action again; so that it has been said that the ague poison may lie dormant for years. It is far more probable that a much smaller quantity of the poison can produce the return of the symptoms than that the original ferment should retain its properties for months, or even for years, after its first action had passed by. In this respect, and in some others, the action of ague poison proves that it is a very peculiar ferment, and hence, though I have placed it near to the typhoid ferment because of its origin, I must shortly point out to
you the different effect which the true typhoid ferment produces.

The typhoid ferment is probably formed out of vegetable or animal albuminous substance. In sewers, in drains, in ditches, possibly even in the drains of the human body, a substance may be formed which is not volatile in itself, but by foul gases or currents of air can be carried into the mouth, and in some period between a few hours and fourteen days it sets up a modified peroxidation. More slowly absorbed and less rapidly reproduced and changed than typhus ferment, it passes into the blood, and from it into each texture; whilst some of the poison has a local action on the glands of the small intestine, and produces increased action, effusion, obstruction, and retrograde action, causing ulceration, sloughing, and even perforation, by which mechanically the contents of the bowel may escape, and an uncontrollable simple peroxidation may be set up. The poison that has passed into the tissues acts on each organ more slowly than the typhus poison; still, like it everywhere, it gives rise to altered functions, and everywhere local peroxidations are ready to occur; bronchitis, pneumonia, peritonitis, tubular nephritis, cystitis—any of these and many other inflammations may be set up at any time during the course of the fever. Probably the substances produced by the increased chemical action in typhus and typhoid fever will be found to be very similar. There may be the same amount of heat, the same excess of urea, the same excess of antecedent substances from which the urea is formed; possibly the same consumption of oxygen when the same temperature in each fever occurs; but in the properties of the ferment formed in the body a distinct difference of diffusibility must exist, the typhus poison passing with greater ease into neighboring bodies; whilst the typhoid poison rarely, if ever, is communicated by infection.
On the Mechanical Disorders that arise out of the Chemical Errors in Fevers.—In all fevers the loss of mechanical power is quite as remarkable as the increase of chemical action. The chief amount of energy liberated by the action of oxygen in the body seems expended in the production of heat, so that far less than the ordinary amount of power remains to be employed in the production of mechanical motion. The muscles may be considered as machines made for the conversion of chemical force into mechanical motion. How this is done can not be explained in the present state of our knowledge of the mechanical, chemical, or electrical actions in the muscle; but that the muscular force arises from some equivalent force, and sooner or later must come from the chemical force in oxygen, hydrogen, and carbon, opens an immense field for investigation, and is easier of belief than that force should be each moment created and destroyed. The amount of sugar and fat present in any muscle would soon be used up if in the working of the muscle itself fresh fuel was not produced. The action of oxygen on the syntonin in the muscle may be direct, and may give rise to the force required; but it is more probable that the syntonin splits into substances of two classes—one ending in urea, which is incapable almost of combustion, the other in inosit, which would immediately give water and carbonic acid.

In fever the poisonous ferment in the muscle probably determines a different chemical action from that which takes place in the muscle in health. The increased heat and the increased urea mark the increased action, but loss of motor-power in the muscles shows that the conversion of chemical into mechanical force does not take place.

This mechanical disorder becomes, by its action on the muscles of respiration or circulation, the source of com-
plications and dangers in fever, to which I must shortly allude. Gradually in the course of fevers the sounds of the heart may be found to become more and more feeble, and the respiration, without any wrong in the lung, becomes shallow and weak from the diminished power in the muscular tissue. The diminished tension in the arteries has a direct effect upon the circulation through the capillaries, and the motion in the veins is more or less stopped; hence congestion of blood in the venous system occurs—hemorrhages, effusions, and coagulations in the veins may take place anywhere. The imperfect action of the muscles of respiration produces the same mechanical effects in the circulation through the lungs; imperfect oxygenation takes place in the lungs from the stoppage of blood in the pulmonary veins; without any inflammation, edema of the lungs, hypostatic consolidation may occur. The circulation through the lung is so feeble that even the force of gravity acting on the blood in the lungs can not be counteracted, and accumulation takes place in the most dependent parts.

The muscles of the bladder are also so weakened that the urine accumulates, and frequently external muscular pressure is required, after the catheter has been passed, to cause the urine to flow.

In another large class of zymotic diseases the qualitative and quantitative errors of oxidation are scarcely detectable, whilst the qualitative and quantitative errors of nutrition chiefly mark the action of the poison.

Of these diseases true syphilis may be taken as the type.

It can scarcely now be doubted that the actions of two poisons have been included under the term syphilis. The first, like impetigo, is capable of being communicated, and often repeated, because it exists only locally, or passes up to the nearest lymphatic glands; whilst in the true
syphilis the poison from the local sore enters the blood and passes from it into each texture, where it multiplies and produces changes of nutrition, and, partly unchanged and partly changed in composition, passes out perhaps in each secretion.

This true syphilitic ferment resembles very closely the small-pox ferment in the universal diffusion of the poison, and in the consequent protection it gives from another attack by rendering a second similar change in each texture impossible. The protective power of the alteration is to a slighter degree extended to the progeny through the germ and spermatozoon; so that a race partly protected by inheritance may suffer less from these diseases than a purer race, whose textures are free to undergo the full change which constitutes the disease. Both poisons give rise to increased cell-growths, effusions, oxidations, congestions, and ulcerations; and these may take place in any part of the body, for the poison exists everywhere.

True syphilis differs, however, altogether from small-pox in its definiteness of course as to time. Syphilis produces no fever to terminate the fermentation in a definite period, and it may consequently remain active or dormant in the textures for years.

It is vain now to ask what circumstances at the end of the fifteenth century produced the first modified albuminoid matter which gave rise to the first true syphilitic poison. In cancer, which bears a distant resemblance to syphilis, although the spontaneous generation of the first cancer-cell is daily occurring in some predisposed texture, yet we are as yet quite unable to say what produces the first modified particle of matter which multiplies and communicates its composition to adjacent predisposed textures by contact, and is carried by lymphatics and blood-vessels to every part of the body, and affects the nutrition of each part with which it comes in contact,
provided the textures are in a condition to propagate the cancer-cells.

Another instance of spontaneous generation of a poisonous ferment is presented to us in rabies; and with this poison also, unless a peculiar condition of system exists, the ferment when inserted has no action; and here also our knowledge is at present unable to say what circumstances determine the formation of the first particle of poisonous saliva; except by its effects, the peculiar change in the albuminoid matter of the saliva in the present state of chemical knowledge could not be recognized.—*Med. Times and Gazette, March 17, 1866, p. 275.*

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**CONSERVATIVE MEDICINE.**

Among the improvements in medicine which characterize our epoch, none is more gratifying than the gradual abandonment of excessive medication, and especially of deteriorating agents, and the increasing reliance upon the resources of the economy supported by improvers of the stamina. Under the influence of the phlogistic and antiphlogistic doctrines which have so long swayed the profession, nearly all diseases were regarded as more or less indicative of exalted or excessive action and increased vitality, requiring at the hands of the physician the use of such means as might seem best adapted to the reduction of the morbid orgasm, and to the impairment of the vital forces supposed to be in excess. Hence the very general resort to the abstraction of blood and to depletion by means of emetics, cathartics, abstinence from food, and the disintegrating influence of mercurialization. The sole object of the medical adviser seemed to be to impair the energies of the system. While he was dealing her-
culean blows at the disease, he seemed to forget that he was doing so at the expense of an organism already con-
tending with an enemy which was sapping the very foun-
dations of its existence. And yet results show that all who were thus treated did not die; for while with some the thread of life is snapped by the most trivial causes, it is in others so strong as to resist the most extraordinary combinations of violence.

But the Broussaisian ultraism wrought its own cure; and Andral, Chomel, Louis, and others of the Parisian school, boldly took their stand in the breach, and success-fully inaugurated the researches and observations which are now culminating in conservative medicine. Hahneman, disgusted with destructivism, but not pos-sessing genius enough to suggest anything else, resorted to the miserable subterfuge unfolded in his "Organon," instead of manfully acknowledging that he preferred no treatment to that then in vogue. The French had already ascertained that many affections were more successfully treated by "expectation," but this did not lead them to the abandonment of professional interference where this could be manifestly advantageous. The era of observa-
tion had commenced, and we are now reaping the benefits of a more thorough acquaintance with disease as well as with physiology and therapeutics. We now look upon morbid action as an impairment instead of an exaggera-
tion of vitality, which may be arrested by antidotes when the cause is appreciable, and by sustainers or improvers of the energies of the system in other cases. We are now satisfied, in managing cases for which we know no specific, to strive to enable the patient to live through the disease, instead of striking in the dark at the expense of the vital energies.

Our malarial fevers, so called, furnish us with one of the most striking illustrations of the improvement to
which we refer. In these the antidotal has superceded the empirical and the antiphlogistic treatment. It matters little whether we regard quinine as a neutraliser of the poison which induces these fevers, or as acting in any other way. No one questions its specific effect, through the instrumentality of which we have acquired the mastery over this class of affections, and thus learnt at least that antiphlogistics are not necessary to subdue the intense excitement (I use this term for want of a better) so often apparently attending these affections, and that it may be abated simply by fortifying the system against the poison which induced it.

Again, look at the history of the treatment of typhoid fever, and see what variety of means have been invoked to subdue the fever and the local ulceration so generally accompanying it. All the deteriorating agencies have been tried in vain; expectation was somewhat more successful; but when we compare these results with those recorded by Chambers under the conservative plan of treatment so ably set forth in his Clinical Lectures, we have every reason for congratulation.

"Since the opening of the hospital in the summer of 1851, to the time of my leaving London for the vacation last August (1863), there have been registered as under my care 230 examples of continued fever. Of these, 109 have been treated on what may be termed "general principles;" that is to say, they took neutral salines three or four times a day, with small doses once or twice a day of hydrargyrum cum cretâ at first, and later in the disease bark, ammonia, ether, and wine, when these remedies seemed required by the symptoms. Leeches and cupping were employed to the exterior of inflamed viscera as occasion called, and food was administered at the ordinary four daily meal-times. The other 121 have been treated on an uniform plan of continuous nutrition; animal food, in a liquid form, has been given every two hours, day and night, while the patients were awake, and between every dose of nutriment a dose of hydrochloric acid. They have been sponged two or three times daily with tepid water, when the skin was hot and dry; and, in a few instances, leeches or cupping have been used to the exterior of inflamed localities in the abdomen or chest."
Of the first series (viz., those treated on general principles),

9 are entered as Typhus, and of these there died 4
44 are entered as Typhoid, and of these there died 16
56 are entered as of doubtful or unrecorded type 3

Total 109

Total 23

Of the second series,

25 are entered as Typhus, and of these there died 0
52 are entered as Typhoid, and of these there died 2
44 are entered as of doubtful or unrecorded type 2

Total 121

For purposes of comparison in a therapeutical inquiry, it will probably be considered right to exclude from the first table two deaths, and from the second table one death, which occurred within two days of admission: for the exhaustion caused by the journey to the hospital in severe fevers allows but little scope for judging of the action of treatment during that period. This leaves the average mortality under general treatment 21 in \( \frac{107}{109} = 19 \frac{1}{2} \) per cent., or nearly 1 in 5;* under the second method of treatment, by continuous nutriment and hydrochloric acid, 3 in \( \frac{121}{123} = 2 \frac{1}{4} \) per cent., or only 1 in 40.

The continuous liquid nutriment given every two hours consisted of strong beef-tea and milk, of which together about six pints were administered in the twenty-four hours. The hydrochloric acid was given every two hours in doses of twenty minims of the Pharmacopoeial dilute acid in water or eau sucrée. Both food and drugs were seen by the nurses to be swallowed, and not left to the discretion of patients, who from nausea and occasional delirium can not be trusted to help themselves.”—Chambers' Lectures on the Renewal of Life, pp. 118-120.

One of the first and most important steps in conservative practice was taken in the management of persons affected with phthisis pulmonalis. Who does not recollect the repeated blood-lettings for the arrest of hemoptysis, the low diet for the reduction of febrile action, the antimonials to promote expectoration, the blisters to excite revulsive action, and the confinement in a heated apartment to obviate the baneful influence of cold in winter?

* This mortality is higher than is usual at special fever hospitals, being about the same as at the other general hospitals in London.
All this was done under the impression that action or vitality was in excess, and should be lessened. But now that we look upon phthisis as characterized by defective stamina or impaired nutrition, how different the treatment. We do all in our power to improve, to strengthen, and to brace up the powers of the system in order that they may combat the tendency to death. We promote digestion by every possible means, administer nutritious oils, and order free exercise in the open air, and thus save many who would have otherwise perished.

Since then many other diseases have been successively subjected to the test of conservative medication, and always with most beneficial results. May we not hope that the profession will calmly review the grounds of their old faith in deteriorating medication, and ascertain whether they may not be more successful by the adoption of a more conservative practice? They will find much valuable aid in so doing by perusing the lectures of Dr. Chambers, already cited.

L. A. D.

OUR MOTTO.

It will be perceived that we have replaced the old motto of this Journal upon its title-page, in lieu of the one adopted by our predecessor in the first number of this volume. This is done in no captious spirit; but simply because we prefer that under which this Journal has earned its reputation for practical and impartial literature. It is concise, to the point, and a correct exponent of the principles by which we intend to be governed in filling these pages. We shall adopt what is good wherever we find it.
Pereira's *Materia Medica and Therapeutics*; being an abridgment of the late Dr. Pereira's Elements of Materia Medica, arranged in conformity with the British Pharmacopoeia, and adapted to the use of Medical Practitioners, Chemists and Druggists, Medical and Pharmaceutical Students. By F. J. Farre, M.D., Senior Physician to St. Bartholomew's Hospital, and London Editor of the British Pharmacopoeia; assisted by Robert Bentley, M.R.C.S., Professor of Materia Medica and Botany to the Pharmaceutical Society of Great Britain; and by Robert Warington, F.R.S., Chemical Operator to the Society of Apothecaries. With numerous additions and references to the U. S. Pharmacopoeia, by Horatio C. Wood, M.D., Professor of Botany in the University of Pennsylvania. Illustrated with wood engravings. First edition. Philadelphia: Henry C. Lea, 1866. 8vo., pp. 1,030.

This book supplies a deficiency long and seriously felt. The parent work of which it is an abridgment is a standard one both in this country and in Europe, and yet its elaborate, recondite character has made it more or less unsuited to strictly practical readers; in other words, it is better adapted to the purposes of men of research than the medical student and practitioner. This objection no longer exists: it has been abridged, condensed, simplified—in short, converted into a manual of easy reference for those who need it most.

The English authors state that they have retained the original classification, but have excluded all remedial agents, except those termed "pharmacological," and, of the latter, such as are not contained in the British Pharmacopoeia. To this selected material, the American editor has contributed numerous references to the U. S. Pharmacopoeia, thus adapting it to our home readers also. While, perhaps, it cannot be expected to supercede the admirable American works upon materia medica and therapeutics of recent publication, it will, nevertheless,
become a welcome addition to every physician's library and a valuable text-book for students. To such as need additional books for consultation on this branch of medicine, we would respectfully commend it. W. H. D.

*The Science and Practice of Medicine.* By Wm. Aitken, M.D., Ed. Professor of Pathology in the Army Medical School, etc., etc.; in 2 vols. From the 4th London edition, with additions by Meredith Clymer, M.D., late Professor of the Institutes and Practice of Medicine in the University of New York. Philadelphia: Lindsay & Blakiston, 1866. Vol. 1; 8vo., pp. 955.

This is one of the most extensive works upon the practice of medicine in our language, and comes to us with the prestige acquired by the rapid sale of four editions in England. We have as yet received only the first volume of the American edition, but learn that the second is nearly ready. Intended for the use of the Medical officers of the British army, the influence of climate upon disease has been kept in view, and the author has largely availed himself of the practical contributions of the medical staff of the British army and navy, and of the resident physicians of the East Indies. The Department of Medical Geography received attention for the first time in a treatise on the practice of medicine. The practical importance of the thermometry of disease is set forth, and diagrams illustrative of the typical ranges of temperature, particularly in febrile diseases, are given for the first time in a work of this scope.

The additions by Dr. Clymer will add considerably to its favor in this country; but the American editor has not done himself justice in having omitted much that he might have added in relation to our diseases and our mode of treating them.

This invaluable book has reached its second edition, and, contrary to the prevailing custom of authors, it is reduced in size—not from the want of materials, but because the author, appreciating its practical worth, sought to increase it by divesting the subject of "complexity and needless refinements."

We would not hesitate to commend it in even extravagant terms, if by so doing we could arrest the attention of those who need it most. If possible, we would place it in the hands of every student, preceptor, and professor, with the assurance that, under its careful study, more knowledge of the subjects discussed would be obtained than from any other publication extant. Ordinarily, physical exploration by the various methods now practiced is an exaggerated "bug-bear," and exactness in its results is considered so difficult as to be beyond the attainment of ordinary persons. We venture to assert that this delusion will be quickly dispelled, if any thoughtful student will follow the order of this work; it would be better for him if he had never heard of, or experienced confusion of mind from, the old, arbitrary terms applied to respiratory sounds, as "rude" respiration, a "short, whiffing" sound, ending abruptly in a "click," etc.; for, adopting the nomenclature of the author, he would have a simplified form, the types of the different sounds (except the adventitious) being found in the normal respiration. He is led first to examine the sounds of the healthy chest in its various topographical regions in percussion and auscultation. The normal vesicular resonance, and the laryngeal, tracheal, bronchial, and vesicular respiration are taken as types of sounds in their intensity, pitch,
and quality, with which are to be compared the variations or modifications produced by disease. When, therefore, in the second stage of pneumonia, the inquirer meets, perhaps for the first time, the "tubal" respiration (of old writers), he instinctively applies the word bronchial, because it is similar to the normal bronchial sounds, although heard over the vesicular structure; and so also with the "rude" respiration present in the early stage of phthisis, which is called broncho-vesicular, because it presents modifications of the normal vesicular and bronchial respiration combined. The nomenclature is descriptive, and conveys in itself the distinctive characters of the most important signs. The author devotes little attention to their mechanism, but labors to establish and define their pathological significance by means of extensive "clinical facts in connection with morbid anatomy."

The chapter upon the "Correlation of Physical Signs" is an admirable production, convenient for consultation. Taking any single sign, you can thereby easily trace its correlative associate signs in all the methods of physical exploration, percussion, auscultation, mensuration, inspection, palpation, and succession. In the new edition this is presented in a tabular form, which, we think, is not so advantageous as the chapter in the first edition.

Physical exploration ensures the exact diagnosis of pulmonary diseases—its revelations are purely objective, and, therefore, more reliable, and, when properly associated with rational symptoms, they establish the diagnosis. Every physician can attain (indeed, is inexcusable if he does not) such a degree of proficiency in the art as to approach with confidence a majority of the pulmonary affections encountered in practice. For the better qualification of all, we press upon the attention of the reader this work. We have drank freely from the fountain with intense satisfaction, and are entitled to a voice in its praise.

W. H. D.

The study of orthopedics has been too much neglected by the generality of medical practitioners, and many cases are therefore left without treatment, or are unsuccessfully managed. All endeavors to facilitate the acquisition of a knowledge of the principles and mechanical appliances by which deformities are treated should be encouraged. The work before us will be found eminently practical and useful. It is an American book, and quite creditable to the author.


As its name imports, this is a concise, comprehensive digest of medicines; their nature and value in the treatment of disease. Its conception was a happy one, however difficult its execution may prove. It is no easy task so to epitomize a diversified subject of this character as to retain only the essential materials; few writers possess the happy faculty of saying neither too much nor too little on a given subject. The author designed presenting the profession with a work which, while it omitted nothing essential, yet excluded "such details as are often embarrassing to the student and seldom necessary to the practitioner." In this he has succeeded admirably, although the therapeutical division of the subject is scarcely as complete as might be desired. We are informed, however, that this is to be followed by a "companion work," devoted exclusively to the therapeutical value of medicines. We have no doubt that its forerunner will meet with a favorable reception from the profession at large, and we take pleasure in directing attention to it.

W. H. D.

This little work, gotten up in Mr. Lea's best style, is a most excellent manual of ophthalmology. It contains all the modern improvements in diagnosis and treatment, and condenses in a small space what is too often made to fill a ponderous volume. The illustrations are graphic, and will aid the student very much in understanding the use of several modern instruments for examining the eye.

D.


Works upon therapeutical science multiply rapidly; it is questionable, however, whether all of them add to its perfection or material development. In this instance we confess our inability to appreciate the necessity for its appearance, possibly because we can not be classed in the author's category of "floating practitioners" (surgeons in the army, the navy, the East India Company's service, those engaged in "emigrant or merchant ships," and resident in "isolated spots" in distant colonies), for whom he thinks it most useful. He asks for the book no higher "distinction" than that of a meritorious compilation; but, while cheerfully according its proper value in this regard, we are constrained to subordinate it to the level of all works of a similar character when compared with the great representative works of the science. A signal degree of diligence and industry has been displayed in
completing it, and if any of our readers of cosmopolitan habits require such a volume, we think it a valuable one for their purposes.

W. H. D.


Already has the distinguished author's volume gone through five editions—a favorable comment upon its value and acceptance. Skin diseases have so many resemblances that the observations of careful men can not fail to place dermatology in a favorable relation to the other branches of medicine. No class of diseases is so obscure to the mass of the profession, and the editor has made it "the chief aim throughout to make this book thoroughly fit for the practical man."

The subject has been elaborated with great care, and brought up to date. The chapters treating of "Exanthemeata," the "Appendages of the Skin," and the "Therapeutics of Diseases of the Skin" will be perused with interest and profit.

F.


We recommend this volume very strongly, not only to the student of the science, but to the educated physician. It will be found particularly useful in simplifying many analyses which become necessary among critical medical observers.

F.

This manual is very full and complete for the use of those already familiar with the foundation—principles of auscultation and percussion. It will be found of less use for the medical student. We think that very few "who are entirely novices in auscultation" will adopt the recommendation of the preface, to "hold fast to this treatise." But for this recommendation, we should have concluded that this abstract from the large and valuable work of the distinguished authors was intended only as a manual for the expert.

To those using these methods of physical exploration intelligently, and ambitious of becoming experts in the arts, we commend it as a very full condensation of the large work to which they may not have access.

We are particularly gratified at the fair acknowledgement of the credit due to Drs. Camman and Clark, of New York, as the authors of auscultatory percussion; and surprised at the author's low estimate of what we regard its high value in determining, with exact precision, the limits of the solid organs of the thorax and abdomen, and of accidental tumors and their connections. F.


The recent prevalence of cholera in different portions of this country has invited a careful examination into its origin, development, and treatment. The volume before us contains many valuable suggestions of rational practice, and some practical ideas on "prophylaxis." F.
On Wakefulness, with Introductory Chapter on the Physiology of Sleep. By William A. Hammond, M. D., Fellow of the College of Physicians of Philadelphia; Honorary Corresponding member of the British Medical Association; late Professor of Anatomy and Physiology in the University of Maryland, etc., etc. Philadelphia: J. B. Lippincott & Co., 1866.

This small volume is replete with original and philosophical views. It treats of the "physiology of sleep," the "pathology of wakefulness," the exciting causes of wakefulness," and, lastly, the treatment of wakefulness. Coming from such a valuable observer, whose physiological researches have elicited so much applause from the scientific world, it can not fail to be interesting and instructive, exposing as it does the erroneous teachings of the physiology of these states still taught by prominent authors. We cheerfully recommend it. F.

Creosote in Diphtheria.

Dr. J. J. Knott, of Griffin, Ga., recommends very highly the local use of creosote in diphtheria. His formula is as follows:

Creosote.................................................. 3ij.
Aqua font.................................................. 3ij.
Pulv. acacia q. s. to make an emulsion. To be applied to the affected surface with a sponge or mop, twice a day, until the exudations disappear.

Bleeding at the Navel.

Dr. Zober, in the Monat f. Schr. Geburtsch, xxvi., as quoted by the Southern Journal of the Medical Sciences, who distinguishes bleeding of the navel proper from bleeding due to an improper tying of the chord, or to the existence of a fungous exuberant growth, bases an opinion upon the fact that, as icterus is usually present, nutrition is partly deficient, and that the bile itself plays a part in the
destruction of the coagulability of the blood. Autopsies develop the existence of coagula in the umbilical vessels, or an aneurismatic condition of them. Martin found once an unusually large umbilical artery, with an origin from the arteria sacralis media. The blood, which flows uniformly, never in a ray, and is perfectly clear, may ooze out of a swelling or without a visible opening out of the navel. The duration of the hæmorrhage is various. Besides the symptoms of anæmia we may have concurrent with them, or at a later period, eczema, and petechiæ on the skin; or bleeding from the intestines, which may produce death just as purpura after the bleeding has ceased. Treatment in these cases is very uncertain. Compression, styptics, and the actual cautery are alike unreliable. Th. Hill once succeeded by pouring a layer of plaster of Paris over the navel. Dr. J. H. Pooley, of Yonkers, N. Y. (the American Journal of Medical Sciences), failed with the ligature en masse. He passed two stout steel pins at nearly right angles to each other through the integument and under the navel, and then applied a waxed silk figure-eight ligature, with no other result than an apparent checking of the bleeding for a couple of hours. This child was also very much jaundiced with urine highly charged with bile elements. No post-mortem examination allowed.—N. Y. Med. Record.

Treatment of Fractured Patella by the Padded Ring Method.

Dr. W. A. Gibson, of St. Louis, Mo., in a contribution to the Medical and Surgical Journal of that city, relates his successful treatment of a transverse fracture of the left patella, in which there was a separation of the fragments to the distance of about an inch. He measured the sound patella and had a ring made of iron (allowing for padding), which was well guarded with cotton wadding cut in strips and wrapped round the ring, over which a bandage was applied. To each side of the ring he sewed
strips of bandage. He then placed a well padded splint twenty-four inches long to the posterior aspect of the leg and thigh, which he secured by a few turns of bandage at the lower and upper ends, the bandage being loose so as not to interfere with the circulation. He kept the fragments in apposition and the ring in place by strips of bandage over the splint. At the end of thirty days, when the ring was removed, the union was bony and complete. The appliance gave no pain, and six weeks after the injury the patient was having a very good use of the limb.—Ibid.

Action of the Bromide of Potassium upon the Nervous System.

The actions of the bromide of potassium, according to Dr. J. Crichton Browne, in the Am. Journal of the Medical Sciences, are: 1. It mitigates those convulsive movements or spasmodic twitchings which are the result of the rapid conversion of sensory impressions into motor impulses, or of morbid reflex action through the medulla oblongata, and it exercises a peculiar influence over the phenomena which are characteristic of epilepsy. Whether the increased excitability of the medulla oblongata is so great as to be productive of epilepsy, or so slight as to extend itself in minor spasmodic complaints, the bromide seems to exert an excellent effect on it. 2. It has a sedative effect upon the action of the heart in certain cases. 3. It lessens and mitigates that rapid and preternatural excitement of spasm, tremor, and other outward manifestations which, in some forms of nervous disease, follow upon any emotional or moral disturbance. 4. It acts as an anodyne, under certain circumstances relieving hyperaesthetical sensations. 5. It promotes sleep. 6. It exercises a sedative influence over the sexual functions. 7. It exercises a beneficial influence over certain mental diseases.—Ibid.
A New and Simple Bullet Probe.

Dr. Vincent Geilisch, of Los Angeles, Cal., in the Pacific Medical and Surgical Journal, calls the attention of the profession to the effectiveness of white pine wood as a substitute for the famous Nelaton probe. A pine probe-shaped splinter, when introduced into a wound, and rubbed against the suspected object and quickly withdrawn, will present traces of lead equally as well as unglazed porcelain.—Ibid.

Sulphate of Zinc vs. Iodine in Injections for Hydrocele.

Mr. Haynes Winslow, of St. Mary's Hospital, Dublin, clings to the rather old-fashioned remedy of sulphate of zinc, of the strength of three grains to the ounce, as an injection for the radical cure of hydrocele. The zinc injection excites more vascular action than the iodine and gives more pain, but the greater assurance of success is more than a set-off in favor of the zinc. He directs that “after the hydrocele fluid is withdrawn the injection should be thrown in with a syringe through a trocar, and kept in the tunica vaginalis till there is pain in the loins and groins, which usually comes on in four or five minutes. Then the fluid ought to be let out.”—Ibid.

Hyposulphite of Soda in Malarial Fevers.

Dr. W. H. Baxter, of Moscow, Iowa, writes to Prof. N. S. Davis, that he was induced by Dr. Leavitt's statement, in No. 1 of this journal, for April last, as to the efficacy of the hyposulphite of soda in malarial fever, to employ that article. In the last month, Dr. B. says he has treated “over one hundred cases of intermittent and remittent fever with this remedy alone, and in no case has there been an exacerbation after taking the remedy a reasonable length of time.” He gave it in fifteen grain doses in solution in water. He has not trusted to this remedy alone in pernicious or malignant types.—Am. Journal of Med. Sciences.
A New Caustic.

Dr. Pinckney W. Ellsworth, of Hartford, Conn. (Medical and Surgical Reporter), alludes to the discovery by a Mr. Augustus Barnes of the fact that the solar focus is a most efficient and admirable caustic.

Dr. E. states that he "saw one gentleman who had a nævus on his face, extending from the eye to below the mouth and involving the lower eyelid to the very edge, and covering four or five square inches of surface; it was of a deep cherry-red color, approaching purple, and covered with knobs of condensed tissue, an eighth of an inch high. This nævus could be seen as far off as the color of the face. After two applications the spot has nearly disappeared, the skin generally having the hue of a surface blistered some days previously, and it is now nearly well. Some portions were absolutely like normal skin, and entirely colorless. Every knob was gone, and where stood one of the largest, and where the rays were longest condensed, was a perfectly healthy looking cutis.

**This man can be considered practically cured, although there is at present the appearance stated, but which does not especially draw attention.**

* * * * The rays were condensed with excellent success, even on the very edge of the lid. * * * * Nor is the pain as severe as we might apprehend, as it is confined at each instant to a very minute point. * * * * Patients at any rate submit very readily and without the use of anaesthetics." Mr. Barnes uses a lens of two and three inches diameter, condensing the rays upon the object to be removed, and goes over the whole, if not more than three inches in surface, at one sitting. Lupus, Ichthyosis, and small tumors involving the surface of the skin, have been subjected to this experimentation with promising results, at least as far as we may infer from Dr. Ellsworth's communication to the journal above quoted.—N. Y. Med. Record.
WORKS RECEIVED.

A Practical Treatise on Fractures and Dislocations. By Frank Hastings Hamilton, M.D., Professor of the Principles of Surgery, Military Surgery and Hygiene, and of Fractures and Dislocations, in Bellevue Hospital Medical College; Surgeon to Bellevue Hospital and to the Charity Hospital, New York; Professor of Military Surgery, etc., in the Long Island College Hospital; author of a Treatise on Military Surgery. Third edition, revised and improved. Illustrated with 294 wood cuts. Philadelphia: Henry C. Lea, 1866; 8vo., pp. 775.

The Science and Practice of Medicine. By Wm. Aitken, M.D., Edin. Professor of Pathology in the Army Medical School, etc., etc.; in two vols.; from the fourth London edition, with additions by Meredith Clyner, M.D., late Professor of the Institutes and Practice of Medicine in the University of New York. Philadelphia: Lindsay & Blakiston, 1866.


Pereira's Materia Medica and Therapeutics; being an abridgment of the late Dr. Pereira's Elements of Materia Medica, arranged in conformity with the British Pharmacopœia, and adapted to the use of Medical Practitioners, Chemists and Druggists, Medical and Pharmaceutical Students. By F. J. Farre, M.D., etc., etc.; assisted by Robert Bentley, M.R.C.S., etc., etc.; and by R. Warrington, F. R.S., etc., etc.; with numerous additions and reference to the U. S. Pharmacopœia, by H. C. Wood, M.D., etc., etc.; illustrated with wood engravings. Philadelphia: H. C. Lea, 1866.


Orthopedies; A systematic Treatise upon the Prevention and Correction of Deformities. By David Prince, M.D. Philadelphia: Lindsay & Blakiston, 1866.


Why Not?  A book for every Woman. The Prize Essay to which the American Medical Association awarded the gold medal for 1865, By Horatio R. Storer, M.D., of Boston: Assistant in Obstetrics and Medical Jurisprudence in Harvard University; Surgeon to the New England Hospital for Women; and Professor of Obstetrics and Diseases of Women in Berkshire Medical College. Issued for general circulation, by order of the American Medical Association. Boston: Lee & Shepard, 1866; 18mo., pp. 91.


Report of the Committee of the Georgia Medical Society, on the Hygienic Condition of the City of Savannah and Surrounding Country. Made to the Society and adopted May 16, 1866.

Essay on the Phosphate of Iron. By E. N. Chapman, M.D., Professor, etc., etc.; with notices of its combinations and forms of preparations, etc., etc. By Caswell, Mack & Co., Family and Dispensing Chemists. New York: John A. Gray & Green, 1865.


The Hunterian Ligation of Arteries to relieve and to prevent Inflammation. By Henry F. Campbell, M.D., Professor of Anatomy in the New Orleans School of Medicine, etc., etc. Augusta, 1866; 8vo., pp. 18.

Valedictory Address to the Graduating Class of the New Orleans School of Medicine, Session 1865-6. By J. L. Crawcour, M.D.; 1866.

The Richmond Medical and Surgical Journal. Edited by Dr. E. S. Gaillard and W. S. McChesney.


The Atlanta Medical and Surgical Journal. Edited by Drs. J. G. and W. F. Westmoreland: Atlanta.


The Chicago Medical Examiner. Edited by N. S. Davis, M.D.

Buffalo Medical and Surgical Journal. Edited by J. F. Miner, M.D.

Revue de Therapeutique Medico-Chirurgicale. Par A. Martin Lauzer.

The New York Eclectic Medical Review. Edited by Drs. R. S. Newton and E. Freeman.

The Medical Record. New York.


The Medical Reporter. Edited by Dr. J. S. B. Alleyne and O. F. Potter: St. Louis.

The Druggists' Circular. New York.

The Southern Cultivator.

The Medical and Surgical Reporter. Edited by S. W. Butler, M.D.: Philadelphia.

MEDICAL COLLEGE OF GEORGIA.

The Medical College in this city is rapidly recuperating from the disasters of the war. The class in attendance this session is double that of the last year.