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ARTICLE XXV.

An Essay on the Adaptation of Climate to the Consumptive, for a permanent residence ; embracing an Examination of the climate of certain localities of frequent resort ; and also, an Investigation of the degree of adaptedness of the Pacific Climates of the United States. Presented to the Medical Society of the State of Georgia, at its annual meeting, held at Atlanta, April 13th, 1859. By WILLIAM HENRY DOUGHTY, M. D., of Augusta, Ga. (Ordered to be printed.)

(Continued from August No. page 525.)

THE STATE OF OREGON, AND THAT PORTION OF WASHINGTON TERRITORY UPON THE COAST, AS CONTRASTED WITH THE COLUMBIA VALLEY.

IN the investigation of the climate of the Pacific slope, increased difficulties are met at every step, as we advance from California northward ; because none of those sections at which we arrive seriatim, have been occupied sufficiently long to admit of a full comprehension, much less a correct appreciation of their meteorology. Hence, any attempt at their elucidation must be liable to error, as it is based to a great extent upon those generally recognised, and controlling physical laws in their various climatological bearings, which have been already adverted to in this essay. Guided then by these laws, and such of the develop-

ments of the special climate, as have been made, we shall endeavor to present, as lucidly as possible, those of the meteorological features of this portion of it, which influence a residence of the consumptive there.

One would suppose, that really the differences between California, and the State and Territory north of it, excepting those due to latitude, would be so small and insignificant, as not to require elaborate exposure. And, there is much plausibility in the idea, when regarded in reference to the general investigations of promiscuous writers; but when viewed in connexion with a special examination of its adaptedness to invalids of peculiar habits, and requiring certain peculiar, unchanging meteorological surroundings, we cannot but feel justified in the attempt, to expose as thoroughly as possible, its meteorological conditions. Here local conditions, not general ones, are to be enquired into, and in order to their study, some degree of minuteness must be descended to.

In the advance up the Pacific slope from California, we observe a continuation of the same general mountain features, yet these undergo differences both in the mode of their arrangement, and in the degree of their influence upon climate. The Cascade mountains, the prolongation of the Sierra Nevada, preserves, by its almost perfect parallelism with the coast, its great height, its precipitous and abrupt sides, and its power to modify climate upon the right and left, all of the general features of the latter range; yet from a simple change of position somewhat nearer the coast line, it presents manifest differences in the neighboring climate, since it extends the western limits, of the arid and bleak interior, and increases the relative humidity of the coast.

Greater alterations mark the northward extension of the Coast Range of California. Having approached the boundary line between the two States, it is apparently broken down in all effort at continuity, resolving itself into an irregular plateau region. And it is not until after having been thus broken down, that we observe it rising again about the central line of latitude of Oregon, possessed of sufficient dimensions to alter the phases of climate. From this point to beyond the Columbia River, it presents the character and appearance of a range

of small mountains. Another noticeable distinction which results from the respective changes of these two mountain ranges, is the destruction of that great valley formation between them, which characterizes the topography of the State of California. From the point of reappearance of the Coast Range, to the Columbia River, and between the two systems of mountains, there is an imperfect valley formation, the Willamette Valley, the meagre representative of the great conjoined valley to the south, whose impressibility of climate is not to be compared with the latter.

Looking to the interior, we also descry some differences in local topography worthy of notice. Here, the great Columbia with its forming streams, the Lewis and Clark Rivers, embracing within the limits of their origin, the entire extent of the territory to the Rocky mountains, instead of seeking a southern outlet, as does the Great Colorado River, assumes at the centre of the area, an almost due-west direction to the Pacific, passing through the Cascade Range. Again; near the junction of the Sierra Nevada, and the Cascade Range, there is a mountainous extension almost at right angles to them, which reaches across Washington Territory, nearly to the Rocky mountains. The Blue mountains of Oregon are also connected with this latter branch, and they together constitute that elevation of the southern part of the territory, which both yields the precipitation of the Lewis or Snake River, and determines a northward flow of its waters.

Besides these, there are material differences in the altitude of different parts of this interior. Its southern part continues the great elevation of the plateau of the Great Basin, but towards its centre, the altitude greatly diminishes to about eight hundred or a thousand feet, with inclined planes extending to the surrounding mountain bases. In this central depression is the junction of the two rivers already mentioned, the point at which the Columbia River is formed.

A remaining field for an inspection of local differences of climate, is the circulation of the Pacific waters. We have seen that the distinctiveness of the climate of California is to a great extent attributable to a peculiar circulation of the ocean itself. Wherefore we are led to enquire whether similar peculiarities

do not mark that off the coast of this section of the western slope, or whether the same mode of circulation prevails here also. The importance of examining this feature, will appear as we advance. We may therefore ask, do the cold masses or currents of water, found prevailing along the coast of California, extend as high up as the State of Oregon? or in other words, what is the most northern point on the coast touched by this mass of water?

From the few authors which it has been our privilege to examine on this subject, it is impossible to fix definitely, either the exact place of origin, the northern or northeastern line of limit of this cold current, or to define its specific width. Indeed whilst they have at an early period detected the presence, and recognised the influence of some such controlling climatological agent, as evidenced in the singularly mild and uniform temperature of the southern part of the coast; in the variations of the summer distribution of heat, in the advance up the coast; and also in the differences of the entire coast, as compared with the west of Europe, (its want of analogy); yet beyond the mere differences of the absolute temperatures of the waters off the Californian coast of the winter and summer, little or nothing is known. They speak of it in general terms, as originating somewhere "northward and westward from San Francisco," and "westward of Alaska," and recognise "the existence of a general movement from the northwest," but do not attempt to fix its limits, or to particularize its dimensions.

To one in our present line of study, it is a matter of deep regret, that the northeastern boundary line of this summer current, has not been accurately pointed out, because wherever along the coast it is found to prevail, it exerts a direct influence, both upon the temperature and humidity of the land atmosphere, which it cannot exercise indirectly, or by mere proximity to it. This difference of its effects, authors generally need not enquire especially into, because the results at which they aim, are general in their nature, and tend only to the elucidation of general facts. But when, as in the present case, those local, but distinguishing features of climate, which are often observed between places and localities, even under the same parallel of latitude, or it may be upon the same meridian

of longitude, are to be exposed, a specific designation of the dimensions, and the degree of access of such currents, is absolutely necessary, since their effects upon land climates are in direct relation with them. Hence the necessity for an endeavor on our part to give this current other than a general signification and location.

All perhaps agree that the reduction of the summer temperature along the Pacific, from thirty to fifty degrees of latitude, which so greatly distinguishes it from the western coast of Europe, is due to a great extent to the operation of this mass of water. But their writings tend also to the presumption that the effect upon at least one-half of this extent, namely from forty to fifty degrees, is more or less indirect, and is not effected by direct contact of the waters with the shore.

Mr. Blodget says, (page 195, *Climatology of the U. States*,) "apparently an immense cold current approaches the coast here at thirty-five to forty-five degrees of latitude, which in summer exercises a wide and decisive influence on all the included coasts, its maximum and central point being nearly at San Francisco." Again he remarks; "but the refrigerating current appears to originate westward of Alaska, and to *pass nearly due southeast* from that point toward the continent in the latitude of Monterey; not entering the indentations of the Russian and British American coasts, probably in any degree," (See page 278.) But in another place, (page 261) this author, while treating of the distribution of heat for the spring, uses the following significant language: "for the mean of the three months, the sea-temperatures observed off this coast,* (Pacific,) are strikingly uniform, and they show but little if any advance on those of winter. *For some hundreds of miles on the fortieth parallel, there is little difference in the sea-temperatures, for the entire year*; and in spring, such observations as we possess, show them to be quite the same between the thirty-fifth and fortieth parallels, for thirty degrees of longitude westward from San Francisco." Yet again, he says; "a portion of the coast of Oregon has temperatures noticeably higher. for the brief period of the record there, which may perhaps prove the *position of the cold line to be a little distance off that coast at sea*," (See page 275.) Moreover, he as-

* The parenthetic word is our own.

serts, that "the cold current is a comparatively narrow mass at the point of its rising on the coast, and evidently is not felt north of the forty-fifth parallel." (See page 118.)

These quotations are sufficient to show, that this writer was unable to designate the precise point at which the northern edge of this current impinged against the continent, nor indeed to indicate the exact direction of its northern boundary towards the place of its origin. To say the least, they were not well appreciated by him, for some of his remarks are somewhat contradictory of each other on these points. To this, however, we would attach no censure, for the generalized facts of climate as sought to be developed in the scope of his work, required only the knowledge of the existence, and the demonstration of some such controlling refrigerating agent, without regard to any definite size, and treated of it only in its relation to the entire continent. Hence there was no need of greater specification, than has been given to it.

Now with a view of attempting to fix the northernmost point at which this current touches the coast, amidst the obscurity with which it is surrounded, we propose to ascend the coast from San Francisco, and observe the distribution of heat, with its differences of degree, at such places as have it recorded. It must not be forgotten, however, that it is not necessary, that direct contact of the cold waters with the shore should be had, in order to refrigerate, or to reduce the mean of its temperature distribution. For, simple proximity, with a general atmospheric circulation, and other contributing local peculiarities, as heated plateaus, valleys, etc., may produce such a degree of refrigeration as is scarcely transcended by an immediate contact: For example, if we suppose the northern limit of this current, to be in a direction due southeast from the peninsular point of Alaska, and to impinge against the western coast at the fortieth parallel of latitude, we would not expect its influence upon that coast, to be limited to that south of the point of contact, but on the contrary, would look for it many degrees farther northward. Moreover, the direction of the winds which blow over and about that mass, would deeply affect the result, since they must inevitably carry away with them the attendant cold surface atmosphere. An analogous instance has already been

mentioned in this paper, in the character of the atmosphere induced by the prevalence of easterly winds along the Atlantic coast for a few days, during which the air is rendered "sultry, oppressive, and heavy," by a transfer of these conditions from the Gulf Stream.

The first post that we meet in the ascent of this coast, is Fort Humboldt, lat. $40^{\circ}.46'$, near Humboldt Bay, California, and eastward two miles from the sea. "The locality is a plateau of slight elevation, fronting the Bay, and fully open to the Pacific." About a hundred miles interior to this, is Fort Jones, lat. $41^{\circ}.36'$, having a location "not closely confined, though with high mountains on the south and east."* The first has an altitude of fifty feet, and the second an altitude of two thousand five hundred and seventy feet. The next station is Fort Orford, in Oregon, lat. $42^{\circ}.44'$, situated upon a bay with a direct exposure to the ocean.

The last that we shall introduce to the notice of the reader, is Fort Astoria, lat. $46^{\circ}.11'$, situated on the "south bank of the Columbia River, near ten miles from the coast of the Pacific. The locality has a free exposure over water surface and low plains, to the sea, with rough hills at the south and east."

MEAN TEMPERATURES.

	MAY.	JUNE.	JULY.	AUG.	SEPT.	OCT.	Sum'er.
San Francisco, Cal.....	55° 29	56° 86	57° 90	57° 22	58° 26	57° 91	57° 33
Fort Humboldt, "	53° 94	58° 04	56° 71	57° 00	57° 02	53° 00	57° 70
Fort Jones, "	54° 69	61° 56	71° 56	68° 66	62° 72	51° 84	67° 26
Fort Orford Oregon,.....	54° 80	59° 03	59° 73	61° 09	58° 91	54° 88	59° 95
Fort Astoria, "	55° 00	59° 53	61° 55	63° 76	58° 13	55° 45	61° 58

Bearing in mind, that the maximum effect is produced at San Francisco, the degree of refrigeration should diminish as we proceed northward. We have placed the posts in the above table, in the order of their succession along the coast. Comparing the first two posts, we find, notwithstanding the difference of latitude, ($2^{\circ}.58'$,) that they manifest great parallelism in the mean monthly distribution of heat. Indeed there is an identity of the temperature statistics, which points unmistakably to the same active, operating agent at both places. Nevertheless

* We have taken the descriptions from the Army Met. Register.

at Fort Humboldt the sea influence does not indicate that intensity of operation, which was noticed at the former station; for Fort Jones, one hundred miles in the interior, shows no appreciable effect from this cause. In other words, it does not penetrate so far into the interior as in the more southern part of the State, but limits itself to the immediate coast. Its mean temperatures for the summer months are much higher, than those of either of the preceding posts, and its mean summer temperature is fully ten degrees (10°) greater. It presents in the course of the mean temperatures then, more of the ordinary manifestations of interior localities of similar altitude. This confinement to the coast may be accounted for by reference to the absence of the San Joaquin and Sacramento Valleys, with their furnace-like temperature.

Examining the next in the table, Fort Orford, whose situation upon the immediate coast, and direct exposure to the ocean, makes it a true exponent of the measure of refrigeration at this latitude, we remark an elevation of two and sixty two hundredths degrees, ($2^{\circ}.62$) per month for the summer season over San Francisco; and a monthly average of two and seventy hundredths degrees, ($2^{\circ}.70$) over Fort Humboldt. Adding September to the three summer months, and taking the average increase per month, it stands two and thirteen hundredths degrees, ($2^{\circ}.13$) over the first; and two and forty-nine hundredths degrees, ($2^{\circ}.49$) over the second. Looking also at the mean summer temperatures, that of Fort Orford is two and sixty-two hundredths degrees, ($2^{\circ}.62$) higher than that of San Francisco; and two and twenty-five hundredths degrees, ($2^{\circ}.25$) higher than that of Fort Humboldt.

The last post of the table, Fort Astoria, situated near the mouth of the Columbia River, latitude $46^{\circ}.11'$ N., shows a still higher elevation of temperature over these two places, the mean monthly increase over them for the summer months, being four and twenty-eight hundredths degrees, ($4^{\circ}.28$) over San Francisco; and four and thirty-six hundredths degrees, ($4^{\circ}.36$) over Fort Humboldt. Adding September to those months, we have as the monthly average of increase for the four months, three and eighteen hundredths degrees, ($3^{\circ}.18$) over San Francisco; and three and fifty-five hundredths degrees, ($3^{\circ}.55$) over Fort

Humboldt. Its mean summer temperature is four and twenty-five hundredths degrees, ($4^{\circ}.25$) higher than that of San Francisco; and three and eighty-eight hundredths degrees, ($3^{\circ}.88$) higher than that of Fort Humboldt.

Let us recapitulate; from San Francisco, lat. $37^{\circ}.48'$, northward $2^{\circ}.58'$ to Fort Humboldt, we observe a continuation of the same reduced monthly mean temperatures, with a strict parallelism throughout the dry season. To this point, the refrigerating influence appears to be about the same. Progressing $1^{\circ}.58'$ farther northward to Fort Orford, we observe a sudden and material increase of $2^{\circ}.70$ per month, over Fort Humboldt for the summer season; and from June to September, an average monthly increase of $2^{\circ}.49$; also a higher mean summer temperature by $2^{\circ}.25$. Then continuing $3^{\circ}.27'$ farther up to Astoria, we have an elevation of $1^{\circ}.66$ per month, over Fort Orford, for the summer; and a monthly average of $1^{\circ}.05$, from June to September. Hence we see within a distance of two degrees of latitude, a sudden elevation of temperature amounting to $2^{\circ}.70$ per month for the summer. And within five and a-half degrees of latitude, a mean monthly elevation of $4^{\circ}.36$ for the summer over Fort Humboldt, and a difference in the summer means of $3^{\circ}.88$. To what may this difference of temperature 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 Humboldt 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 from that point. And advancing five degrees from this point, such differences are observed as point to an almost entire absence of its influences, 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

Humboldt. Besides if we take as strictly true the remark of Mr. Blodget, that this cold current passes "nearly due south-east," from the point of the peninsular of Alaska, and estimate its breadth at seven or eight degrees of longitude, (the distance westward from San Francisco, which gives the reduced mean temperatures); 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 Forts Humboldt and Orford, latitude $40^{\circ}.46'$ to $42^{\circ}.44'$. 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° N. 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, prove "the position of this cold line to be a little distance off" at sea.

Deeming this short and imperfect notice of these surface peculiarities, and the differences of conformation, together with the difference of the external or oceanic influences, found operating upon them, sufficient to show the necessity for a separate, careful, and extended investigation of the climate of this State and Territory, we now pass to a notice of the general climatological features of each locality. The entire area included, extends from latitude 42° to latitude 49° N., and embraces about 15° longitude at its greatest width, and 11° at its narrowest. The extent of surface thus embraced, amounts to about three hundred thousand square miles. The two great ranges of mountains that are found, the one upon the right and the other upon the left, are most remote from each other at the southern boundary, and in their northern course gradually approach each other; so that having arrived at the northern limit of Washington Territory, they are nearer to each other by at least four or five degrees of longitude. The Cascade Range upon the left, runs almost entirely parallel with the shore line, which has here a somewhat north-by-east direction; whilst the Rocky Mountains on the right, have assumed a direction a little to the west-of-north. Hence, as actually

occurs higher up in British America, these ranges appear gradually to run into each other. Both are continuous in their character, and each presents a single interruption in its course, namely, the South Pass of the Rocky Mountains, and the perforation of the Cascade by the Columbia River in its course to the Pacific. They seem to be providential, as if to facilitate transit across them, and establish relationships between the two sections on either side.

This disposition of the mountain systems, reproduces the divisions adopted in the first clause of our subject, namely the coast and the interior, and it is in this way, that we propose to treat of it. The coast is a continuation of that of California, and differs from it only in point of latitude, the extent of its valley formation, and its width. It will be recollected that the average in the middle parts of the latter is, one hundred and eighty miles, and in the northern parts two hundred miles. Here, however, we suppose that the average width, which is about the same all the extent of it, is perhaps between one hundred and twenty-five and one hundred and fifty miles. The coast comprises a part of the State of Oregon, and also a part of Washington Territory. The extreme south of this coast is an imperfect plateau formation, and those few small streams that are formed by the precipitation at this point, find a quick outlet between the hills into the Pacific. About the centre of this coast line, the Columbia River discharges its contents into the ocean, having passed through the Cascade Range seventy or eighty miles to the east. Between these two points, and the two parallel ranges, on the ocean side, is the Willamette Valley, the only attempt at an analogous valley district with that of California. It has a northern declivity, and takes its name from the river of the same name, which empties into the Columbia, probably fifty or sixty miles from its mouth. North of the Columbia River, no peculiarities occur worthy of special mention.

Of the interior, which comprises all that expansive area northward from Utah to the British possessions, and enclosed between the Rocky and the Cascade Ranges, we shall treat under the head of the Columbia Valley. It constitutes the northernmost part of that interior basin, which Baron Humboldt declared to be "the broad, undulating, flattened crest of the chain


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 of the Mexican Andes, and is not the swelling of a valley between two mountain chains."\*

With slight differences in degree, the same general characters which were remarked in the Great Basin of Fremont, are here repeated, or rather continued. There is a similar aridity of soil and of atmosphere; beds of alkaline earths, and salt lakes are still to be seen; a corresponding scarcity in the amount of precipitation; scattered ranges of mountains here and there; the same vegetable productions, namely, the artemisia and the cactus, the sure exponents or indicators of a soil and atmosphere, unsuited to the cultivation of the higher forms of vegetation; and finally, there are similar changes of temperature, accumulated heat in the day and as excessive cold in the night, thus making an extreme daily range of the thermometer the rule, and not the exception. Periodicity, according to Mr. Blodget, still marks its seasons, although he asserts that more or less rain may fall all the year round. Near the junction of the Sierra Nevada, with the Cascade Range, there is a mountainous extension almost at right angles to them which reaches nearly across the valley to the mountains on the right; farther interior but connected with this lateral arm are the Blue Mountains of Oregon; and in the east from the lateral projection of the Rocky Mountains, is an offshoot, the Bitter Root Mountains, which juts out into the eastern part of this valley. It is within the area enclosed by these latter ranges, that is found the southern head of the Columbia River, the Lewis or Snake River, whose transit through this region is secured by those mountain gorges, denominated canons by the Spaniards, and which also characterize the entire plateau regions.†

### *Special Climate.*

It would be entirely superfluous to dwell here upon the general features of the spring climates, after all that has been said in connexion with the general climate of the Pacific slope. Those most interesting to us have been already noticed in the investigation of the climate of California and its contigu-

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\* (Aspects of Nature.) See Blodget, page 113.

† "The Colorado of California and its great branches, the Grand and Green Rivers, traverse these gorges through their whole course to within three hundred miles of the sea." (Blodget.)



ous Territories, and also under the general consideration and comparisons of it with the eastern slope, and the southern of Europe. The variability of the coast here is greater than that of California. Along the coast the rate of advance from month to month is much more gradual than in the interior,—the differences of alternate months, being scarcely greater than those of the succeeding months. At Fort Orford, on the southern part of this coast, the least difference in this monthly advance is noted in the passage from March to April, and the greatest from May to June. At Fort Vancouver, an intermediate post in the north of the Willamette Valley, these observations are reversed, the greatest difference being from March to April, and the least from May to June. Greater uniformity of advance, for the entire season, as far as the record at present extends, although quite extreme in degree, is found at Fort Dalles, on the Columbia, in the interior, than to the west of the Cascade Range. And compared with Fort Defiance, its analogue in the territories to the south, it is both more mild and less extreme; for the mean monthly advance there is  $8^{\circ}.56$ , whilst here it is only  $6^{\circ}.88$ . But the spring features of the coast, are placed in decidedly the most interesting view, when contrasted with corresponding ones for the coast lower down. Making due allowance for the difference of latitude, the accessibility of each to oceanic influences is the same, therefore the differing results at each of the places indicate distinctive differences in the character of those oceanic influences. On the coast of California, both at San Francisco and at Fort Humboldt, some distance farther northward, there occurs in the passage from April to May an actual retreat in the position of the mean temperatures, instead of an advance, which we have attributed to an increasing intensity of action of certain cold currents of the ocean, found prevailing at that time. At Fort Orford, however, a progressive advance is observed, which can only be brought about by a diminished effect of those controlling sea-temperatures. Hence that degree of refrigeration which is observed along the former coast is not experienced here, being only subject to the ordinary influences of sea-coast localities generally. This, of itself, is collateral proof of the position we have assumed in regard to the course and direction of the cold waters off this coast, for from Fort

Humboldt, south five degrees of latitude, their influence is such as to subdue or overwhelm the ordinary continental influences by reducing the mean of May below April, and in the middle of the dry season, that of August below July. So that twice during the dry season, this inordinate reduction of temperature happens, and indeed it is not improbable that the same effect would be produced as far south as San Diego, if it was not for the mere latitude of this place, and their own increase of temperature in their course southward. Certainly if they extended as high up as Fort Orford, with latitude in their favor, they ought to manifest at least an equal degree of influence. Their withdrawal, on the other hand, produces greater abruptness, and a greater increase of temperature for the season, than at the south.\*

Upon an inspection of the general range of the monthly mean temperatures for the entire region, the Willamette Valley shows the greatest variation in their relative position: the range for March transcends largely that of the far interior post in New Mexico, namely, Fort Defiance, and equals many of those variable posts in the southern part of the eastern slope of the United States, and around the Gulf. Even posts in the Columbia Valley, as Fort Dalles, present a much fairer record, for the mean range for the season there is only  $3^{\circ}.84$ , whilst here it is  $8^{\circ}.09$ . The coast, however, as usual, presents by far the least variation in this respect, for the greatest range is only  $4^{\circ}.52$ , which took place in May; and it has an average for the season of only  $2^{\circ}.91$ . Indeed for the limited period of observation here, the coast of Oregon, as exhibited in the range of the monthly mean temperatures at this post, is superior to that of California. For March here gives a difference between the highest and lowest means of only  $3^{\circ}.31$ , whilst at San Francisco it is  $6^{\circ}.82$ : and the mean of the entire season is only  $2^{\circ}.91$ , that at the latter place, being  $3^{\circ}.76$ . This superiority

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\* It is as well to state here as elsewhere, that the temperature of the waters off this part of the Pacific coast, are perhaps of a more or less uniform temperature all the year round, and also that the waters themselves constitute a part of the same current which has been seen to prevail during the wet season of California. If so, they are probably at or about the same temperature of the latter. So that we have at this point, and doubtless farther northward also, an unchanging temperature of the sea at all times.

may be attributed to the uniform temperature of the seawaters, which secures a regulation in the advance of temperature for the season, and a mitigation and moderation of continental influences. At the south at this time, the currents are shifting and substituting; hence greater abruptness in the monthly advance. The proximate cold masses cannot exert any influence here, until their full sway is established below, and the heat and rarefaction of the plateaus and valleys of the interior, attract their atmosphere inland.

The extent to which the thermometer may fluctuate throughout this area, as indicated by the extreme single monthly observations, can, at the present time, be only approximative of the constants. The freezing point may be attained at any part of it, and the lowest recorded point is about nineteen degrees ( $19^{\circ}$ ) at Fort Vancouver. Low down upon the coast, it is always reached in the month of March, although the few records furnished do not show its occurrence during April and May. The degree of cold experienced at Fort Orford during the months of March and April, is greater than at San Francisco, although their possible ranges of the thermometer do not indicate that extent of fluctuation in temperature, which is observed at the latter place. The possible range here for March, is two degrees ( $2^{\circ}$ ) less than at San Francisco; that of April is fourteen degrees ( $14^{\circ}$ ) less; and that of May is just equal to it. These cannot be taken as altogether reliable, for the observations of a single year are not sufficient, nevertheless they give a foreshadowing of their probable temperature statistics.

As we recede from the shore line towards the Cascade Range, the extremity of these observations rapidly increases. In the Willamette Valley, at Fort Vancouver, the possible range of the thermometer for March is increased over that of Fort Orford, fifteen degrees ( $15^{\circ}$ ); and over that of Fort Astoria, thirty-three degrees ( $33^{\circ}$ ); the lowest at Fort Orford, being nine degrees ( $9^{\circ}$ ) greater, and the highest six degrees ( $6^{\circ}$ ) less; and at Fort Astoria, the lowest is eleven degrees ( $11^{\circ}$ ) higher, and the highest is twenty-two degrees ( $22^{\circ}$ ) less. April, however, in this valley is not so extreme, the two extremes manifesting a tendency to come together; yet the highest for the month, is ten degrees ( $10^{\circ}$ ) higher than at Fort Orford on the coast, and its



lowest is nine degrees ( $9^{\circ}$ ) less, so that, the possible range for the month, remains nineteen degrees ( $19^{\circ}$ ) greater. Moreover its possible range is seven degrees ( $7^{\circ}$ ) greater than that of Fort Astoria, for the same month. The extremes here for May are excessive, extending from the frost point to blood heat. The highest at Fort Vancouver for three years; was ninety-eight degrees ( $98^{\circ}$ ;) and the lowest, thirty-nine ( $39^{\circ}$ .) There is an increase of twenty degrees ( $20^{\circ}$ ) over the highest of April, and eighteen degrees ( $18^{\circ}$ ) over that of March; whilst the lowest is eight degrees ( $8^{\circ}$ ) higher than that of April, and eighteen degrees ( $18^{\circ}$ ) higher than that of March. The possible range for the month, however, is only equal to that of March, and is twelve degrees ( $12^{\circ}$ ) greater than that of April. The highest single observation recorded throughout the entire section at this season, occurred at this post. It is eighteen degrees ( $18^{\circ}$ ) higher than the highest of the coast, and four degrees ( $4^{\circ}$ ) above that of the interior station, Fort Dalles. It also approaches those extraordinary records\* for this season in the San Joaquin Valley.

Looking now to the far interior, we find at Fort Dalles smaller differences between it and the coast, than between the latter and the Willamette Valley. When compared with Fort Defiance, in the first part of this essay, it shows a much higher attainable degree of heat, and more conservatism in its distribution.

*Winds and Weather of Oregon and the Columbia Valley, for the Spring Season.*

No more striking contrast of the two sections of coast, examined in this essay, could be exhibited, than is found in a comparison of their mode of circulation of the atmosphere. If the changes in the temperature manifestations between San Francisco and Fort Orford are great those in the circulation of the atmosphere are greater. At the former place, throughout the season, winds from the south and west were in the ascendancy, although of the single winds, those from the northwest were oftenest observed, and increased in force and prevalence with

\* In May 1852, at Fort Miller, the thermometer reached the enormous height of  $113^{\circ}$ .



its advance. At the latter,\* the southwest and west winds, during the month of March, were rarely recorded, and during the remaining two, no record whatever is made of their prevalence, except of the west wind, once in May. Winds from the north, southeast, and northwest, were prevalent throughout the entire season, and of these no particular one maintained a uniform ascendancy. Those from the southeast, were by far the most forcible; those from the northwest next, although the latter greatly increased in strength with the advance of the season.

Advancing still higher up to the mouth of the Columbia River at Fort Astoria, there are other recognizable differences. During March, there was no particular circulation; winds from all points were recorded, those however, from the east and southwest, were the predominant single ones. In April and May, the south and west winds were ascendant, and of the single ones, the southwest and northwest. Here then, a partial restoration of that along the southern coast of California occurs. Whence the difference of circulation at the intermediate coast station, Fort Orford? Why, an almost complete reversal here? Is it attributable simply to local peculiarities of configuration, or has it some connexion with those occurring differences of temperature, and of circulation of the sea-waters near it? These are interesting questions at this juncture, and will require for their answer a longer period of observation. At Fort Vancouver, great irregularity exists as to the prevalence of the winds. In March, during four years' observations, the north and east winds were most numerous, those from the east and southeast predominating. In April and May, the south and west were ascendant, but during the latter, the north-west was frequently recorded.

Repairing to the interior at Fort Dalles during two years, we find the same general features, that were stated as belonging to Fort Defiance. But between it and the coast stations, especially Fort Orford, there is great dissimilarity. The south and west winds were always in the ascendancy and the southwest and west of the single ones.

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\* This remark is based upon the observations of a single year, and are therefore of little reliance; but they are sufficient to show, that the course of the winds off the Californian coast does not extend to all parts north of it.

*Of the weather, the subjoined table affords a sufficient exposition.*

|                                    | MARCH. |        |       |       | APRIL. |        |       |       | MAY.  |        |       |       |
|------------------------------------|--------|--------|-------|-------|--------|--------|-------|-------|-------|--------|-------|-------|
|                                    | Fair.  | Clo'dy | Rainy | Sno'y | Fair.  | Clo'dy | Rainy | Sno'y | Fair. | Clo'dy | Rainy | Sno'y |
| Fort Orford, Oregon, (1 year.).... | 10.    | 21.    | 13.   | 1.    | 6.     | 24.    | 19.   | 0.    | 14.   | 17.    | 8.    | 0.    |
| " Vancouver, " 4 " ....            | 11.7   | 19.2   | 10.   | 2.5   | 10.7   | 19.2   | 10.7  | 0.    | 19.7  | 11.2   | 4.    | 0.    |
| " Dalles, C. Valley, 2 " ....      | 19.5   | 11.5   | 3.5   | 3.0   | 20.0   | 10.0   | 8.0   | 0.    | 26.5  | 4.5    | 2.5   | 0.    |
| " Astoria, Oregon, 1 " ....        | 11.0   | 20.0   | 19.0  | 0.0   | 10.0   | 20.0   | 16.0  | 0.    | 10.0  | 20.0   | 16.0  | 0.    |

The respective means for the same season are, as follows:

Fort Orford,—Fair 10.0—Clo'dy 20.6—Rainy 13.3—Sno'y 0.3  
 " Vancouver " 14.0— " 16.5— " 8.2— " 0.83  
 " Dalles, " 22.0— " 8.6— " 4.6— " 1.00  
 " Astoria, " 10.3— " 20.0— " 17.0— " 0.0

Along the coast, the number of fair days for March and April, as compared both with the interior post, and with those on the coast of California, is greatly diminished. For the three stations, there is about an average of ten fair days for March, and eight for April; and of cloudy days, an average of twenty for March, and twenty-one for April. Here also the proportion of rainy days, as compared with the cloudy, is very large, there being an average of fourteen rainy days in March, and fifteen in April. At Forts Orford and Vancouver, for May, there is a decided increase in the number of fair days, and a diminution in both cloudy and rainy days. At Fort Astoria, however, the same general features are noticed throughout the season. The greatest number of fair days are recorded at Fort Dalles, and they steadily increase with the advance of the season; whilst the others decrease. The respective means for the season, show nearly three times the number of rainy days at Fort Orford, that there are at San Diego, and nearly twice that of San Francisco. Fort Astoria gives three and a half times the number at San Diego, and two and a half times that of San Francisco. We may add here also, that although there is a large proportion of cloudy days recorded of the latter places, as the mean for the season, yet the number of rainy days is scarcely one half of them; whilst along the coast of Oregon, the latter have increased to about two-thirds the former.

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*Rain in Inches.*

At Fort Orford, the monthly means are as follows: March, 8.24 inches; April, 5.64 inches; May, 5.24 inches; the mean for the entire season is 19.12 inches. At Fort Vancouver, as follows: March, 3.79 inches; April, 2.74 inches; May, 2.75 inches, with a mean for the season of 9.28 inches. At Fort Dallas, as follows: March, 1.07 inches; April, 0.99 inches; May, 0.57 inches; and of the season, 2.63 inches.

As we advance up the Pacific coast, the duration of the wet season increases to such an extent, that the dry season cannot, strictly speaking, be said to commence until the summer months are reached; and if we should continue our advance to Russian America, and thereabouts, so great an encroachment would be found to be made upon the latter as to obliterate it, this being a region of almost constant precipitation. The amount of precipitation also increases rapidly with the latitude; the mean quantity for the spring at San Diego, is 2.74 inches; at San Francisco, 8.81 inches; and at Fort Orford, 19.12 inches; an increase of 16.38 inches in ten degrees of latitude, which is about 1.63 inches to every degree of latitude. As we recede from the coast, the degree of precipitation declines, that at Fort Vancouver for the season, being only one-half, and across the Cascade Range, the proportion is reduced to about one-seventh that at Fort Orford.

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ARTICLE XXVI.

*Fracture of the Neck of the Scapula.* By L. A. DUGAS, M. D.,  
Professor of Surgery in the Medical College of Georgia.  
(Read before the Medical Society of the State of Georgia, at  
the meeting in April, and ordered to be printed.)

At the annual meeting of this Society in 1857, I had the honor of reading a paper "Upon Fractures of the Scapula, with Cases, &c." which was ordered to be published. It accordingly appeared in the pages of the Southern Medical and Surgical Journal in June of that year, and although well received by the profession in general, I am sorry to say that it did not altogether escape the notice of one of those writers of "Biblio-

graphical Notices" who seem to think their only duty to find fault with the papers placed in their hands, even at the expense of justice. The disingenuous perversion of quotations and inferences in the "notice" to which I refer, cannot fail, however, to be detected by any one familiar with the subject, and therefore needs no refutation in this place. With your permission, I will now read the history of another instance of Fracture of the Neck of the Scapula, which came under my observation since the publication of those to which I have just referred. My apology, if any be necessary, for bringing this case before you, will be found in the fact that injuries of the shoulder-joint are often so obscure that, although they have been long studied by able and experienced surgeons, their diagnosis is still the subject of controversy. In the absence of post-mortem confirmation, we have to rely principally upon the accuracy with which symptoms are investigated and detailed.

On the 9th of May, 1858, Mr. Partlow, of Abbeville District, South Carolina, brought to me for consultation his negro boy, Lewis, between six and seven years of age, well formed and well grown. Mr. P. stated that about two months before, the boy, while at play in the woods, was stricken down by the falling of a tree, one of the limbs coming down upon his left shoulder. The direction in which the blow was received was not known, but upon approaching the boy he found him rational, and was told that he had not lost his consciousness at the time of the accident. There was no evidence of injury to the head, nor any where else than in the shoulder. The boy complained of his shoulder, and said that the pain was also very great in the arm, the ability to move which was entirely lost. Upon feeling the pulse, it was found much more feeble in the left than in the right limb, and the temperature of the injured arm was lower than that of the other. In handling the limb and shoulder Mr. P. detected nothing like crepitation, and therefore concluded that there was no fracture. Swelling supervened in the shoulder, soon after, accompanied with great pain in the arm down to the elbow, but none below this. At the end of ten or fifteen days the swelling had disappeared, the shoulder was flattened and seemed to droop, but the pain in the arm continued much longer.

Cold water dressings, and subsequently stimulating frictions,



constituted the treatment to which the patient had been subjected prior to my seeing him.

*Examination.*—By causing the boy to stand erect before me, with his chest bare, the left shoulder appears drooped, emaciated and very much flattened. The deltoid muscle feels loose and flabby, as though in a state of atrophy. This is not so perceptible in the other muscles connected with the shoulder; yet the supra-spinatus, infra-spinatus and pectoralis major are evidently smaller here than on the other side. The acromion process projects in a sharp angle, and the head of the humerus is depressed about three-fourths of an inch; but may be easily replaced by pushing up from the elbow. The left hand can be carried upon the right shoulder while the elbow rests against the thorax, thereby precluding the idea of a dislocation properly so-called.

By grasping the head of the humerus, which is distinctly felt, with one hand, and rotating the lower end of the bone with the other hand, it is found that the head moves continuously with the shaft, and without any crepitation whatever. There is therefore no fracture of the humerus at any point.

By placing one hand upon the shoulder so as to fix the body of the scapula, the acromion process and the clavicle, at the same time that various movements upward and laterally are imparted to the humerus, very distinct crepitation can be perceived in the shoulder-joint. Crepitation is also distinctly induced by grasping the head of the humerus with the ends of the fingers resting upon the margin of the glenoid cavity in the axilla, where it is easily felt in this case, and moving simultaneously both the head and the glenoid cavity, while the body of the scapula is fixed as above mentioned. There must then be a solution of continuity somewhere between the glenoid cavity and the body of the scapula.

The coracoid process is here very distinctly felt, and it does not follow the movements of the humerus when this is alternately carried up into its proper position and allowed to fall. By placing the fingers upon the coracoid and the acromion processes they are found to move in unison with the lower angle of the scapula without crepitation, and are therefore continuous with it, showing that the fracture must implicate some point between the glenoid cavity and the root of the coracoid process.

The body of the scapula, its spine, the acromion process, and the clavicle are uninjured.

The boy is unable to move any muscle of the limb—not even those of the fingers. The whole limb is shrunk and much cooler than the other.

The pulse at this wrist is more feeble than it is on the right side; and there seems to be no abnormal distribution of the arteries. The sensibility of the skin is very obtuse, and no degree of pressure upon the ulnar nerve at the elbow gives any pain.

REMARKS.—This case was peculiarly favorable for a minute examination, in consequence of the atrophy of the muscles, which allowed the bones to be distinctly felt at all points. In muscular adults, and during the existence of swelling, the difficulty is very much increased and may render accurate diagnosis almost impossible. I believe that it will be conceded that this examination demonstrates satisfactorily that this was a case of fracture and not of dislocation; and that the seat of fracture was between the glenoid cavity and the roots of the coracoid and acromion processes. Was it what is technically called a fracture of the glenoid cavity, or was it a fracture of either the anatomical or the surgical neck of the scapula? It was certainly not a fracture of the *surgical* neck, or, in the language of Sir Astley Cooper, “through the narrow part of the bone, immediately opposite the notch of the superior costa,” for the coracoid process was not implicated. I must confess that it is difficult to determine whether it was a fracture of the glenoid cavity or one of the *anatomical* neck of the bone. But, when we take into consideration the age of the patient and the comparative facility with which in early life articular surfaces are detached, together with the positive fact that the margin of the glenoid cavity was felt in the axilla and could be elevated with the head of the humerus, I must believe that this was a fracture of the *anatomical* neck. One or more fissures in the glenoid cavity would not satisfactorily account to me for the phenomena of this case.

The sudden paralysis induced by this accident is worthy of attention, especially as it constituted also a peculiarity in the cases heretofore reported by me. The censorious critic above alluded to, takes exception to my statement, that writers make

no mention of it in such cases, and cites instances of dislocation in which it occurred. He forgets that I was not treating of *dislocations*, but of *fractures*. He says, however, that he reported somewhere a case of sudden paralysis of certain muscles, following a fall upon the shoulder, in which there was *no dislocation*. I have no recollection of seeing his report, but even this was unlike my cases, inasmuch as he says nothing about the existence of any *fracture*. I therefore reiterate the assertion, that this peculiarity has hitherto passed unnoticed, at least by systematic writers, in *Fractures of the Neck of the Scapula*. As this sudden paralysis occurred in the three cases I have seen, should it not be taken into consideration in establishing the diagnosis of injuries of the shoulder? It has been long known that it may accompany dislocation; we *now* know that it may also be induced by fracture of the neck of the scapula; and it will be interesting to determine with precision under what other circumstances it may be looked for.

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*Clinical Lecture upon Cerebral Fever.\** By TROUSSEAU. Translated from *La Clinique Européenne*, by J. C. REEVE, M. D., Dayton, Ohio.

I am about to speak, to-day, of an infant which appeared doomed to a certain and almost speedy death. The disease from which it suffered, merits, in many respects, your most careful attention. It was a case of cerebral fever, which followed a regular course in its premonitory as well as its complete stage.

The patient was a little girl aged eight months, nursed by its mother. It was taken ill about six weeks ago, its constitution being good. At that time it presented a peculiar sad or heavy appearance, which was not usual, and which could not be attributed to dentition. The first group of teeth had been cut at the age of four months, and the superior incisors, the next to be expected, had not yet made their appearance, and would not probably do so within fifteen or twenty days, supposing the child lived that length of time. Dentition, then, could not occasion the illness which caused this heaviness, a symptom the importance of which I can not too strongly impress upon you, and which, succeeding all at once to the liveliness and playfulness of the

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\* Synonyms: *Tubercular meningitis*—Wood, Meigs; *Acute hydrocephalus*—Watson, West; *La meningite granuleuse*—Bouchut; *Die hitzige Wassergehirnsucht*—Meissner.



child, surprised and troubled the mother. Its sleep became broken and unequal, but there was an absence of one symptom so frequently seen at the commencement of this disease; it had none of those startings from sleep, accompanied by a peculiar cry, which I shall have to describe in giving an account of this terrible affection. Eight days ago vomiting commenced. Everything which was given to it, drinks of various kinds, and its natural food, were thrown up again, and the mother became seriously alarmed. Her apprehensions were increased five days ago, by the appearance of another symptom, which she has well described to us, and which it is essential to mark. Whenever she took up the child it commenced to cry; it seemed as if every touch gave it acute pain; and this was the fact, for there was then general hyperæsthesia. Finally, four days ago, convulsions came on—first on the right side, then on the left; and then the little patient was brought here. Let us pass rapidly in review the symptoms which it presented, and compare them with those which may occur in other similar affections.

At first sight we observed *strabismus*—convergent strabismus of the right eye, the pupil of which was dilated, but not so widely as that of the left. Upon that side the child seemed blind, for upon moving the finger rapidly before the left eye, there was no closure of the lids, that natural and involuntary movement for the protection of the menaced organ. There was then blindness, or great feebleness of vision; and this is an accident of cerebral fever, which children who can talk complain of readily, and which is here very plainly indicated by the wide dilatation of the pupil, and the convergent strabismus of the other eye. We observed, besides, a slight bending of the head backwards, and some stiffness of the left arm, which was agitated from time to time with movements of extension and flexion. The thumb of this side was forcibly adducted into the palm of the hand and covered by the fingers convulsively flexed upon it; this flexion, however, yielded readily to our attempts at extension. Upon removing the child's clothing, we observed that the abdomen was excavated, hollowed out—a symptom almost constantly to be met with in cerebral fever, and of great value, as it serves, in a large number of cases, to distinguish the disease from cerebral affections occurring secondarily in other diseases—typhoid fever, for example. When the hand was carried to the face of the child, as I did to examine its mouth in regard to dentition, we were struck with the redness which immediately colored the skin. And if we passed the nail over the abdomen, however lightly, in such a manner as to make longitudinal and transverse lines, thirty seconds did not elapse before the whole surface was covered with a lively redness, which, diffuse at first, remained along the lines where the nails passed, of a more intense and persistent color. This is



the cerebral blush, which I first pointed out twenty years ago, and which I have called the *meningitic blush*. This singular and inexplicable phenomenon is of great value, whatever may be said of it, and of great aid in establishing the differential diagnosis of cerebral fever. It is worth while to dwell upon it a moment, for often this cerebral blush alone is of very great signification. The parts upon which it most readily appears are, first and before all others, the anterior face of the thighs, then the abdomen, and next the face. Its characteristics are those we have just pointed out in the case before us. In making light friction upon the skin of the little patient, with the nail, or with a pencil, we saw the points touched redden with remarkable vivacity. This redness persisted a longer or shorter period—eight, ten, fifteen minutes. The existence of this cerebral blush has not been denied, it is true, but the value which I give it has been strongly contested. It has been said that it is a sign of no importance, since it occurs in other diseases as well as in cerebral fever. This blush it is true, does sometimes occur in other diseases, but it is then accidental and exceptional, while in the complaint under consideration, it is a constant phenomenon, invariably occurring during the whole duration of the disease, from its initiatory symptoms until death—a capital point as to its value in diagnosis. But a still greater objection has been made; it has been said that this blush can always be found, when sought for, in any child having fever, even of the simplest kind; but this is an error. I have pointed out to those following me in the wards, children suffering with fever accompanying violent stomatitis and severe pulmonary inflammation, and we have sought to produce this blush by rubbing the skin rudely, even to rubbing off the epidermis; the friction has caused a light redness, but this redness was not comparable, either in intensity or persistence, with that seen after much lighter friction in cases of cerebral fever. The redness of the skin in those patients disappeared almost immediately; in the little girl it persisted eight, ten, fifteen minutes; and it not only occupied the points upon which friction was directly made, but extended several centimeters around, while in the other cases it remained perfectly localized to the points touched.

If I insist so much upon this sign, I repeat, it is because, in a great number of cases, it is one of considerable value, especially in avoiding the possible confusion between cerebral fever and other maladies, such as typhoid fever with cerebral complications, and the convulsions to which children are liable, and those which occur during the access of the exanthemata. This blush is never produced in these kinds of convulsions, and if ever seen in typhoid fever, it does not attend all stages of the disease, and never has the same intensity nor the same persistence.

I have dwelt upon this point to prevent repetition. Let us

return to the other symptoms which make up the history of the disease.

The *precursory stage* is of very great importance. The writer who has insisted most strongly upon this period, is M. Rillet, of Geneva, the collaborator of Barthez in the work upon diseases of children. He has reported a large number of cases in which he was able to foresee an attack of cerebral fever from certain symptoms which I am about to indicate.

The first symptoms presented by a child about to be attacked by a cerebral fever is generally, but not always, a change of manner, which manifests itself during the month, or six or eight weeks, preceding the outbreak of the disease. The patient is sad and heavy, and takes less pleasure in its sports than usual; its *character changes*, it becomes morose and irritable towards its playmates and friends; at the same time a very perceptible *emaciation* takes place; it does not sleep so soundly as formerly, or there may be complete sleeplessness. In some cases the slumbers are disturbed by painful dreams, and broken by sudden startings accompanied by a peculiar cry which is characteristic of the disease. M. Rillet attributes these general symptoms to lesions of the brain which already exist, and which, although giving a chronic or subacute form to the malady, exercise, nevertheless, a certain and injurious influence upon the functions of the encephalon. When we recollect that we almost invariably find tubercular deposits in children who have died of cerebral fever, either in the bronchial or mesenteric ganglia—or, what is more rare, in the cervical—we can easily understand how this tubercular affection can occasion the general symptoms we have mentioned, and especially the emaciation. Further, as we almost always find in these subjects, granulations occupying the periphery of the brain, the fissures of Sylvius, and other points—granulations which are, in truth, tubercular, as the microscope incontestably proves—we can conceive that the morbid action, under the influence of which these deposits are made, although taking place slowly, has nevertheless a decided influence upon the cerebral functions—an influence which is manifested by the change of character, by the agitated and broken sleep, and by the insomnia, of which we have spoken.

Although the precursory symptoms more frequently precede cerebral fever, than any other disease whatever, I do not think we can regard them, as does M. Rillet, as exclusively characteristic of this affection. They appear to me, indeed, to depend far less upon the local lesions than upon the general pathologic state, which, preceding in this case meningo-encephalitis, precedes also in other cases, latent pleurisy, or tuberculous engorgement of the mesenteric ganglia, or in others, again, pulmonary or bronchial tuberculation. In a word, the precursory symptoms indicate

rather a morbid diathesis than a declared disease. We know how much the character of a child changes under the influence of sickness, but few among ourselves have not learned, by experience, that such a change takes place in adults, even under the influence of a light ailment, and the change takes place in children as much more readily as their characters are more mobile.

We can easily understand, then, this sadness of the child, this unaccustomed repugnance to the sports of its age, this moroseness which is not habitual to it, without calling to our aid any local encephalic lesion. We have for their explanation the existence of a most grave, morbid state of the system—a tuberculous state not yet manifesting itself by local affections, but profoundly affecting the entire economy. I should say, however, that, although these precursory symptoms occur in the other diseases mentioned, they are in no other cases so striking as in cerebral fever.

Finally, more decided symptoms of the disease manifest themselves. Most generally it is vomiting, and *uncontrollable vomiting*, which announces the commencement of cerebral fever. In the greater number of cases, the friends of the patient look upon this symptom as of slight importance; and as the child has eaten but a few hours before, and perhaps even with good appetite, they consider it as only the effect of indigestion. During a day, or perhaps two, they remain of this opinion, but finding the vomiting persist, are compelled to abandon it. And, in truth, it is very rarely that vomiting is repeated in indigestion after the stomach has once emptied itself. This persistence of the symptom is then a fact of great importance. When it occurs without febrile action in a child which has been vaccinated, our attention should be immediately directed to cerebral fever; occurring in one not vaccinated, accompanied with fever more or less severe and continuous, with profuse perspiration, diarrhœa, and pains in the loins, we should fear variola; but, I repeat, in the absence of these conditions, the vomiting described announces the commencement of cerebral fever. It should be added that there is generally obstinate *constipation*.

At the same time the patient complains considerably of headache; and although this is a symptom which strongly attracts the attention of the friends and the physician, it is a symptom which cannot be considered as of any great value in this complaint, for there are many other diseases which are attended by headache, more or less violent in proportion to the intensity of the fever. However, there is this peculiarity in the headache attending cerebral fever: it is rare that the *initiatory fever*, of which it is one of the epiphenomena, is limited, as in the other diseases, to a single accession; generally the patient has two or three chills in the twenty-four hours, and after each chill, some heat of skin and perspiration; sometimes the chill returns each day at about the



same hour, in other cases, but more rarely, the fever is continued, but moderate, with frequent remission. The febrile movement, the violent headache, more or less limited to one part of the cranium, the moroseness of the patient, its sleeplessness, the persistent vomiting, are signs of capital importance. When the physician observes them, he should be carefully upon his guard. It is not rare, even at this stage of the disease, to be able to discover more serious symptoms, amblyopia, hemiopia, etc.

Such are the symptoms of the first period of cerebral fever.

In the *second period*, to the sleeplessness, the febrile movement, and the violent headache, succeed a deceitful calm and repose, which is likely to tranquilize the uninformed physician, and above all to quiet the apprehensions of the parents, who are always ready to seize upon any favorable indications. But the practitioner who has been taught by experience, informed by the symptoms of the preceding period which he has seen or been told, knows too surely that cerebral fever is established, that it has entered upon its *apyretic stage*, and that henceforward it will steadily pursue its fatal course. The *pulse* now shows a peculiarity; ordinarily regular in the first days of invasion, (I say ordinarily, for sometimes in the first period there is irregularity which gives valuable information to the physician,) it becomes now, in the second period, remarkably slow, at the same time unequal, but, above all, excessively irregular; while normally, in a child four or five years old, there are between ninety and a hundred beats per minute, and in an infant at the breast, from one hundred to one hundred and twenty, in the second period of cerebral fever the number of pulsations falls to sixty, sometimes fifty-five, or fifty; and it may fall even below that, while at the same time, it presents great inequality and irregularity.

At this time the child remains in a state of somnolence which contrasts singularly with the agitation of the first period. It enjoys, in appearance, a peaceful slumber, which rejoices and comforts the friends who surround it; but soon, seeing it prolonged, they become justly troubled and alarmed. This somnolence persists from four to five days. If the little sufferer is awakened, it utters a cry of impatience, and immediately drops asleep again; yesterday terrified at your presence, afraid of the physician whose examination harrassed it, it no longer exhibits any alarm; then it could not endure even an examination of the pulse, the slightest touch agitated and tormented it,—now it appears indifferent to anything you may do; you open its eyes, you pinch it to discover the degree of cutaneous sensibility, and if it appears for a moment irritated, it drops immediately into its quiet slumber. This is a symptom of most serious character, and which we find in scarcely any other disease. About the end of two days, the countenance presents a strange appearance. From time to time

the child opens widely the eyes ; they are brilliant, and at that moment a bright redness overspreads the face, comparable to the blush which modesty sends to the cheeks of a young woman. This redness disappears in a minute or two, the eyes close again, and the child returns to its former state. This *reddening of the countenance* is repeated from time to time during the day ; it is also of value. Soon it occurs more frequently, and in a great number of cases ; when it takes place the child utters a few plaintive cries,—a characteristic phenomenon which Coindet has particularly pointed out ; this is the *hydrocephalic cry*, the cry of cerebral fever ; it may be repeated every hour, every-half hour, or at much greater intervals. It is a cry of pain, and the patient having uttered two or three of these, falls again into the calmness and into the sleep from which its sufferings had for a moment roused it. This peculiar appearance of the countenance and the hydrocephalic cry completes the description of the second period, with the exception of one sign of which it is necessary to speak. I allude to *retraction of the abdominal parietes*. The abdomen is hollowed out, and this fact is especially important in establishing the differential diagnosis between the disease under consideration and typhoid fever, a disease in which the abdominal parietes are always protuberant.

One other symptom, however, must still be mentioned ; it has struck those who observed the little girl, who is the subject of the present lecture. I refer to *irregularity of respiration*. It was well marked in our patient, as I have said, but less so than in many others I have seen. How did she respire ? In counting the respirations, watch in hand, there was extreme difficulty in following them ; first, there was a feeble inspiration followed by a feeble expiration, then a stronger inspiration and expiration, and again a feeble one, and finally a period of cessation. These four respiratory movements were accomplished rapidly, and the chest remained motionless afterwards during three, four, five, and six seconds. That is what occurred yesterday ; that is what occurs to-day ; to-morrow, in place of an interval of five or six seconds, there will be one of ten, twelve, or fifteen.

In a child of two years, in the Hospital Necker, afflicted with this disease, I was able to count by the watch, periods of cessation of respiration of thirty, thirty-five, forty, and even of fifty-seven seconds ; and it is remarkable that this irregularity of respiration extends to the third period of the disease, when the slowness of the pulse has been succeeded by great frequency—this infrequent respiration occurs with a pulse beating, as in the case of our little patient, as high as one hundred and sixty per minute. In no other disease can this singular anomaly be found ; this unequal respiration does not occur in the idiopathic convulsions of infancy, nor in typhoid fever,—so that in cases where we hesitate

in making a diagnosis, and these cases are still too frequent, this fact, in addition to the cerebral blush upon which I have so strongly insisted, should have a great significance. It is of importance in view of diagnosis, but it is far more so in regard to prognosis—and for this reason: the termination of cerebral fever is almost always, if not always, fatal. In the course of a long medical career, I have never but twice seen recovery take place. Once it occurred under my own care, in the children's hospital, where we were able to verify the fact by an autopsy. There seems a contradiction between the words *recovery* and *autopsy*, which may require an explanation, and this can be easily given: the acute disease had subsided, but was followed by a serious chronic affection, which terminated the life of the patient five months afterwards. My *interne* presented to the Society of Anatomy, the brain, which had evidently been the seat of softening four or five months before.

The other example of cure was in a child I saw in Bolougne, near Paris, in consultation with M. Blache. These two children are the only ones, I repeat, which I have had the good fortune to see recover in a long practice. It shows how serious is the disease which furnishes such results; to me, its incurability seems nearly absolute. You can comprehend, then, of how much import the question of diagnosis must be when it is impossible to form a prognosis without it; and here prognosis is of vital importance. We must distinguish from cerebral fever, typhoid fever with cerebral complications, and the idiopathic convulsions of infancy. We cure, in fact, the greater number of young subjects attacked with typhoid fever, even when it is of the most grave character—even when accompanied by serious cerebral complications; and we cure ordinarily the convulsions of children. But if we do not make the distinction—if we take for cerebral fever diseases of which we have just spoken, (and typhoid fever may easily be confounded with it,) we shall imagine that we have cured many cases of the disease, and be astonished at hearing practitioners of great experience avow that they have never been fortunate enough to save a single one.

The *third period* of cerebral fever is characterized by a *return of the febrile movement*. We have seen the fever of the first period occurring in paroxysms of short duration, several times repeated in the twenty-four hours, or continued, but moderate, and with frequent remissions. In the second period of the disease, the pulse, on the contrary, was remarkably slow; in the third period, it becomes extremely frequent, and this frequency goes on increasing until death terminates the case. It is at the same time sharp, and the skin is warm, but it is singular, and the fact is of value in the diagnosis, that the thirst which generally attends fever of every kind, does not exist in the disease



under consideration. While children, suffering from cerebral complications in typhoid fever, from scarlatina, from any other pyrexia, or any inflammation, demand drink eagerly, or if unable to talk, make known, by certain pantomimic actions, the thirst which torments them, nothing of the kind is seen in patients attacked with cerebral fever. There is no thirst, or, at least, no evidence of any is given. Not only do the little patients not ask for drink, but when it is offered they manifest repugnance to it. Does this repugnance arise from the difficulty of deglutition experienced by them? This may be; but whatever the explanation, the fact is the same.

Already in the second period, a symptom had made its appearance which, although present then, is of course much more strongly marked in the third. I allude to the *state of depression* into which the patient has fallen. Completely indifferent to what passes around it, it observes nothing, is disturbed by nothing; everything which pleased it formerly, its toys, the amusements of its age, are now unnoticed, and it lies in a state of complete immobility: sometimes it responds when spoken to, but never asks questions of its own accord; while in every other disease, it will call for its mother, its nurse, and those it is accustomed to see, manifesting in a word desires, in cerebral fever it seems to have no spontaneous ideas and no wants. In the first period of the disease, we can awaken the patients by disturbing them. They complain and cry upon being irritated; but in the third period this is no longer the case—nothing rouses them from their deep depression.

Convulsions are sometimes, but rarely, observed at the commencement of the disease; in the second or apyretic period, there are none, properly speaking, but there is something resembling them—something analogous to the epileptic vertigo; the child opens its eyes quickly, and they remain singularly motionless. This convulsive movement manifests itself more decidedly in the third period, and then also appear symptoms of *paralysis*, which in some cases occur towards the end of the former stage. Thus, when our little patient opened its eyes, one of them was much more widely opened than the other, for the levator palpebræ began to be sluggish; there was also strabismus. These symptoms indicated clearly that paralysis was commencing, and an attentive examination showed that it had already extended to other parts of the body. In an infant, we can establish this fact, by laying it upon its back and tickling alternately the soles of its feet: it draws back one foot much more readily than the other: the power or motion is impaired upon one side, sensibility is also less, and a greater stimulus is required upon this side to produce the same effects. The parents will tell you, besides, that their child allows one arm to lay along by its body more than the other,

and if you examine this member you will find motion, power and sensibility more or less affected. The paralysis of cerebral fever has this peculiarity about it: it appears to be variable. I will explain what I mean; one day you ascertain the fact that upon tickling the soles the child withdraws one of its feet more readily than the other; some days afterwards, in repeating the experiment, you find that it is no longer the right leg, for example, which it moves more easily, but the left. It seems, and let me repeat the word, it *seems* as if the paralysis had ceased upon the right side, and passed to the left; but this is not so. The member primarily paralyzed remains so, but the paralysis not increasing, it retains the movements which you have seen executed more feebly than upon the healthy side; but the other side, healthy until now, is paralyzed in its turn, and the stroke being more severe than upon the side first affected, sensibility and power of motion are almost abolished, and it seems now alone affected, while in truth the other is also paralyzed, but in a less degree than the second.

The lesions which we find afterwards at the autopsy give an explanation of this fact; for if the paralysis has remained limited to the right side, the cerebral lesion is found only upon the left side, while if the paralysis has seemed to pass from one side to the other, there are cerebral lesions upon both sides, but more marked upon one side than upon the other. I have thought necessary to dwell upon this peculiarity of the paralysis in this affection, for, in my belief, it is seen only in cerebral fever.

Let us return to the *convulsions*. Rare in the first period, occurring in the second only in a modified form and resembling epileptic vertigo, in the third period they make their appearance, first in the form called *convulsions internes* [*inward fits*, in the common language of this country,] afterwards increasing to attacks of general eclampsia. If we observe closely a child in this stage of the disease, we see at certain times its countenance is distorted, and its jaws move mechanically as if chewing, while its thumb and fingers are flexed into the palm of the hand; to this stiffness complete relaxation succeeds; there has been a true tonic convulsion of certain classes of muscles, and not one alternately tonic and clonic as in general attacks implicating the muscles of the extremities. These are the internal convulsions, having a duration of eight, ten, twelve, or fifteen minutes, during which the eyes are turned upwards and inwards, and agitated by slight oscillations. The diaphragm, and the muscles of the glottis may be seized with these convulsive movements, and then the patient is suffocated—respiration can no longer be performed.

As the disease approaches its fatal termination, *general convulsions* come on, and generally death occurs in one of these attacks;

they are repeated every hour or every half hour, and the patient dies in a state of demi-asphyxia, as in epilepsy.

The convulsions are a prominent symptom of the third period of cerebral fever; but in this stage another accident also takes place. The patient fallen into a state of deep drowsiness, closes the eye-lids but partially; the eye being insensible, the cornea remains exposed to the air, the act of winking is no longer performed, the eye dries, the conjunctiva inflames, and then you observe a deep suffusion of the membrane at the same time that the edges of the lids become bleared. This is also seen in other grave diseases, (typhoid fever, for example,) and it is an accident to which I have often called attention, and which I made the subject of a note published in the first volume of the *Archives Generales de Médecine* for the year 1856.

The termination of this disease is, as I have already said in the course of this lecture, always fatal, for the exceptions of this rule are so rare, as not to be worth mentioning.

After so sad a prognosis, I should have little to say upon the subject of treatment, since whatever we do is in vain. Nevertheless you have seen me resort to remedies in the case of the little child whose case we are considering. I instituted treatment, not with the expectation of rescuing it from its fate, but to afford some consolation to the mother. Is it not in fact cruel to say to a mother who calls you to her child, or if not to say it in words to indicate by your actions, that medicine is powerless, and that her child is doomed to certain death? Although, therefore, the physician, instructed by long experience, is unable to see a glimmer of hope, he should act, he should undertake the case, and apply all the means at his disposal to sustain the courage of a family which implore him for aid, and not leave them to the bitter regret that nothing was done to save their child.

For a long time I have employed everything in the treatment of this disease, everything extolled by others, everything which, upon my own part, I had been able to imagine. I have given calomel in large doses, and I have given it in minute ones; I have had recourse to drastic purgatives and to sedatives: I have administered the iodide of potassium as recommended by Otterburg; I have applied large blisters to the head, I have applied ice and cold affusions, and never, except in two cases, have I seen the disease retrocede, and in those two cases I am confident my remedies had nothing to do with the result. Farther, at the children's hospital, I have treated the patients comparatively, some energetically and some upon the expectant plan, and I must say that the fatal event followed more rapidly in the former than in the latter. Now, therefore, I feel obliged to pursue a course of medication far from energetic, and designed, I repeat, rather



to afford consolation to the parents than to act against a disease which I consider invariably fatal.

Thus you have seen me give our little patient musk and syrup of ether, and nothing else. In other cases, you may try antispasmodics, or the mercurials in small doses; but remember, the prognosis must nevertheless remain the same.

It suffices to see the lesions produced in cerebral fever, to comprehend why the prognosis should be as I have said. Our little patient died, and these are the pathological conditions found at the autopsy: considerable softening of the cerebral centres, of the fornix, of the corpus callosum, of the median septum and floor of the ventricles; these cavities contained a certain quantity of slightly turbid serum. About the chiasm of the optic nerves, behind the decussation, a fibro-plastic, *purulent* infiltration was seen in the thickened membranes. This infiltration did not exist in the interlobular fissures, where it is usually observed, and what is also very rare, there were neither *granulations* on the surface of the hemispheres nor *tubercles* disseminated through its substance. Neither were there any in other organs where they are almost constantly found—none in the mesenteric ganglia, none in the lungs, none in the bronchial glands; and yet of thirty infants dying of cerebral fever, twenty-nine will present tubercular lesions, of which, in this child, there is not a trace.

This proves once more, that cerebral fever runs the same course and has the same characteristics in children not tuberculous as in those who are so. Because we find granulations in the encephalon of the latter, it does not prove that those granulations were the cause of the encephalo-meningitis. They do not cause it any more than do the granulations of the pleura cause tubercular pleurisy; far from having caused the inflammatory disease, they are themselves developed under the influence of inflammation. If, therefore, I refuse to cerebral fever the name of meningitic, it is because I consider the inflammation of the meninges to be only of secondary importance. The lesions of the cerebral envelopes are of very far less importance than those astonishing lesions which are always found in the brain itself, that softening which destroys the fornix, the septum lucidum, the corpus callosum, the optic thalami and the posterior portions of the lobes of the cerebrum. Cerebral fever is therefore for me an encephalo-meningitis.—[*Cincinnati Lancet and Observer*.

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*Attempted Abortion and Death from Introduction of Air into the Veins.*

ONE of the most painfully familiar topics of our current medical experience, arises from the familiarity and indifference with which the large mass of community have come to regard the

production of abortion; so that everywhere we hear the lament of the honorable physician, of the unconcern with which he is consulted for this purpose by both the unmarried female, who may be supposed to have the more anxious solicitude to hide her shame, and alike the respectably wedded mother, who has no such motive to afford a plausible pretext.

The danger which is associated with these attempts, does not appear to be duly estimated even by the members of the profession, in many instances; and popularly a great many expedients, instrumental and medical, are used and regarded as perfectly innocent and harmless. Every now and then, however, we read of and observe sudden and almost inexplicable death to ensue in cases of this kind.

An instance in point fell under our observation within less than a year in this city. We were summoned, about half past eleven o'clock P. M., (near midnight,) to see a lady, and found her already dead. Upon inquiry, and in the subsequent details of the coroner's inquest, it transpired that she, although a happy wife and mother, had determined not to allow any further additions to be made to her nursery cares. Finding herself, therefore, pregnant, she had consulted one of those dames reputed to be skilled in such matters, and had held repeated private interviews with her for a number of days before her decease. The night of her death, her husband was engaged away from home until about eleven o'clock. The servant girl remained up until after ten o'clock. The husband arrived at home at eleven, and found his wife deceased and the girl and children asleep. Certain instruments were found about the person of the deceased, which indicated that she had made an attempt to throw up a strong stimulating injection into or about the mouth of the uterus. What she actually did is, however, left in some conjecture, but death must have been very suddenly produced, considering the brief time in which she was left alone, and in view of the fact that the girl in the next room was unawakened.

It will be a happy time when this truth shall become impressed upon the popular mind, that whenever a woman places herself in the hands of "abortion procurers," she positively runs the risk of her life in every instance.

The following case, reported by Dr. John Swinburne, of Albany, N. Y., and which we copy from the *Medical and Surgical Reporter*, in full, is in point, and affords some especial light upon the character of danger in these cases, and how death may ensue, and very suddenly:

Miss M. A. S., aged twenty three, unmarried, was admitted to the house No. 40 Franklin-street, for the purpose of having an abortion procured, on or about the thirteenth of March.

It is ascertained that attempts were made, from day to day,

to rupture the membranes with a blunt steel instrument. These efforts only produced slight inflammation, softening, and partial separation of the membranes and placenta.

On the evening of the twenty-sixth of March, Dr. J. H. Case was summoned in haste to the above mentioned house, where he found that the young woman had just died. An examination before the Coroner's Jury the next morning elicited, among other testimony, the following statements :

Dr. J. H. Case, sworn : Knows Mrs. Masten ; was called to attend a patient at her house about six months ago ; her given name is Oscillea ; her ostensible business is an astrologist. The patient whom I visited six months ago was a young woman ; she had inflammation of the womb. Was called by Miss Curry last night at nine o'clock ; said that Mrs. Masten wanted I should call as soon as possible, that a lady there had fainted and was very sick ; went to 40 Franklin-street, and found Mrs. Masten on the walk ; she said she was glad I had come, as the woman was very bad, and she was afraid she was dead ; she said it was only an India rubber that she was using, and that the deceased fell right back dead. Found a body lying on the bed very pallid, and dead to all appearances ; Mrs. M. did not go in with me ; thought it might be a case of suspended animation ; gave her some stimulants, but they did her no good—she was dead ; told Mrs. M. so, and she said, " Oh, Doctor, what shall I do ? " The girl Curry, then said, wringing her hand, " What shall *we* do ? " I told Curry that they could do nothing with her ; Mrs. Masten said to her, " No, I am to blame ; I shall have to stand it. " She again asked me what she must do ; advised her to throw herself upon the mercy of the law ; she asked me if I didn't think it best for her to try and escape ; told her that it would be impossible if she undertook it ; she also said it was a bad time for her, as she had no time to fight it out or money to escape ; but that she expected some on Monday, and if she had that she would clear right out in half an hour ; she again repeated that she didn't know how deceased came to die, as she could show me the instrument, and that it couldn't hurt her ; went into the bed-room again, and another young lady said she thought deceased was reviving ; wanted me to try and revive her ; Mrs. Masten turned down the bed-clothes and produced a gutta percha catheter. [The article was produced and identified.] She said that was the instrument she used, and that it could not hurt her ; also, that it wasn't the one she generally used ; that it was milder, and the girl's death surprised her ; that while using it, the patient, as she thought, fainted away ; that she tried to fetch her to, and failing, had sent for me ; think it was about nine o'clock when I got to her house ; don't think that over fifteen minutes had elapsed until I saw the deceased, after being called. Mrs Mas-



ten indirectly asked me to loan her money to escape with; I found three or four young girls in the house when I first got there; one of them said that she had got through with her troubles, and that she thought it best for her to get away as soon as possible; asked her if she was able, and Mrs. Masten said she was all right; suppose, from what I saw and heard, that all the young ladies were "in trouble;" think I know one of the young ladies I saw there; believe I prescribed for her some time since.

Assisted by Charles H. Potter, Professor of Chemistry, Dr. C. P. Staats, and my students, Messrs. Mosier and Covell, I made a *post mortem* examination fourteen hours after death. The following detailed description is given for the benefit of medical readers.

External appearances of body natural, but very pallid. On cutting through the integuments into the cellular tissues, air was observed to issue from the divided veins in the form of a frothy fluid. On exposing the heart, its right cavities were found to be greatly distended with a spumous mixture of blood and air, and slight compression of the heart was seen to force out bubbles of air from the divided intercostal veins. A thorough examination showed that the jugulars, and the veins emptying into them, even to the small vessels of the brain, were all distended with air.

The uterus was found to be of a dark livid or maroon color at its lateral portions, and its veins and sinuses were so fully distended with air, as to give it the appearance of a bag of angle-worms. The sensation communicated to the touch was analogous to that of varicocele, with the exception that in the latter the tissues are soft and distended with liquid, whereas in the case of this uterus the presence of air was unmistakably manifested by its characteristic crepitus when the vessels were compressed by the finger.

The membranes of the ovum were entire, and contained a normal amount of amniotic liquor, and an apparently healthy female foetus of about five months' growth, presenting no appearance of decomposition, or any change to indicate death of the foetus at any period long prior to that of the mother.

On the internal surface of the membranes was a slight exudation of lymph, as from inflammatory action. Externally they were separated from the womb on its right latero-posterior surface, as was also the placenta in part. Beneath the lower border of the latter, was an effusion of blood in the form of several small coagula. The os and cervix were opened to the extent of two lines, and filled with bloody mucus.

On examining the membranes and their contents, the internal surface of the womb exhibited the following appearances: 1.

Slight softening of the tissues; 2. Several abrasions evidently not natural; 3. A perforation communicating directly with the uterine sinuses, about two inches from the cervix, and in the right latero-posterior region. This opening communicated directly with the veins of the broad ligament, and thus with the ascending cava. The direction of the perforation was parallel with the longitudinal axis of the uterus. All the other organs of the body were in a perfectly healthy condition.

These *post mortem* appearances, conjoined with the description of the young woman's death, can not be accounted for by any other cause than that of "air in the veins." Death occurred while the instrument was in the uterus, and was *immediate*, for the woman mistook *death* for *syncope*.

The point of interest in this case, is as to the *manner* in which the air was introduced. Several deaths have been reported from ingress of air into the large veins of the neck, and even the subclavian is liable to the same thing under favorable circumstances, such as tension upon the vein from the subject's position during surgical operations, or by traction upon a tumor during excision, the vein being temporarily *canalized* or prevented from collapsing.

Under all circumstances, this canalization of a vein, or its conversion into a rigid tube, is the indispensable condition requisite for the intrusion of air. But this condition is inadmissible in the case of the uterine veins and ascending cava, from the nature of physical laws which govern the movements of the fluids in the body, no less than in inorganic matter.

In the twenty-second volume of *Braithwaite's Retrospect*, on page three hundred and nine, will be found an article by Dr. J. R. Cormack, in which is discussed the possibility of introduction of air into the venous system through the medium of the uterine veins immediately after parturition. He instances the experiments of Legallois upon animals, whereby that author became satisfied of the possibility of the intrusion of air in this way, and by analogy conjectured that many cases of death in the human subject might be accounted for in a similar manner.

He also quotes from Dr. Simpson, of Edinburgh, who reports an autopsy of the body of a female who died after delivery, where the entrance of air through the uterine veins was conjectured to be the cause of death. The examination, conducted carefully, so as to exclude all apparent sources of error, resulted in the discovery that the lower cava, hypogastric, and uterine veins were distended with frothy blood and air.

Dr. Simpson also explains the manner in which air might be forced into the veins by the contraction of the uterus after having been filled with air, which is not seldom the case. This organ being distended with air, the os tincæ being closed either

by its own sphincter or by a coagulum of blood; the uterine veins being large and patulous, and the forcible contraction of the organ—these furnish, in his opinion, the mechanism capable of accomplishing the fatal accident. (See *Braithwaite's Retrospect*, xix., page 262.) In the present case no such conditions are furnished, and throwing aside the hypothesis of spontaneous ingress, we are compelled to fall back upon the presumption that the abortionist forcibly inflated the entire venous system, by means of the catheter introduced into the uterus, perforating its parietes, and in contact with the lacerated vessels of that organ. And this presumption is strengthened by the fact that the opinion prevailed, at the time of the coroner's inquest, that abortion might be produced by inflating the space between the membranes and the womb.

The fact of forcible inflation is incapable of proof, there being no third person present at the time of death, and hence no witness. Absolute certainty can only be arrived at from the confession of the guilty woman herself.—[*Cin. Lancet and Obser.*

### *Diphtheria.*

Dr. David Wooster thus sums up an able article on Diphtheria, which has appeared in the *Pacific Medical and Surgical Journal*:

I. Diphtheria is a *specific* disease.

II. It is distinguished from scarlatina by the absence of eruption; from gangrenous sore throat, by the absence of ulceration and sloughing; from croup, by the aplastic nature of the exudation.

III. Diphtheria may properly be divided into two varieties: the mild and the severe.

IV. The mild is seldom fatal; slight, or no difficulty of deglutition, little fever, no engorgement of cervical glands, neither coryza nor lachrymation, but presenting the positive diagnostic sign of aplastic exudation on the tonsil, palate, or pharynx.

V. The severe is recognised by the diagnostic aplastic false membrane, high fever at first, coryza, lachrymation, engorged glands about the jaw, difficult deglutition, difficult utterance, or complete aphonia, great diminution of animal power, cyanosis, vomiting towards the close of the affection, and intense gangrenous fœtor from the decomposition of the exudation.

VI. Diphtheria is contagious.\*

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\* The experience of the French epidemics has made abundantly clear one very important fact in the history of Diphtheria, which has not yet been so clearly eliminated from the observed facts, of the English epidemic. It may be very clearly shown by the evidence collected, that contagion plays the principal part



VII. The simple form is easily controlled by treatment which is the same as for the severe form, modified according to the comparative urgency of the case.

VIII. The severe form is with difficulty controlled, and the prognosis is always extremely unfavorable, even at the beginning.

IX. The treatment is the local application of hydrochloric acid, diluted or not; solution of nitrate of silver, one or two scruples to the ounce of distilled water; strong solution of sulphate of copper, drachm and a half to the ounce of water; concentrated solution of the perchloride of iron; Monsel's Salt, in powder; solution of chloride of sodium, etc., according to the educated judgment of the physician.

Externally—strict cleanliness; in the first stage of the disease, while the engorgement is red and hot, cold, wet compresses applied to the neck, and often repeated, can scarcely fail to relieve, at least for a time. Farther along, when the engorgement of the throat becomes oedematous, warm fomentations should be substituted. Never liniments, mustard, nor anything of the kind; for these appliances increase the anguish of the patient, and do not mitigate the affection.

Internally—first, a thorough emetic of ipecacuanha, given in full dose and largely diluted, so as to be vomited immediately. This should be repeated daily for the first two or three days. The bowels should be moved once a day by glysters, if possible; if not, by some mild laxative. Drastic cathartics should be carefully avoided. Iced milk and water, or iced gum-water, or iced infusion of *ulmus fulva*, should be given in minute quantity, frequently as the patients desire. These iced fluids will be found very grateful to them.

Quinine in small doses, say from one-fifth to one-half of a grain, often repeated, should materially assist in the tonic treatment universally recommended.\* The mixture of "sesquichlo-

in the propagation of Diphtheria. There is in this country, a great deal of skepticism as to the contagious character of this disease; but the mass of evidence to prove it is overwhelming. Thus Bretonneau has collected some crucial cases. One is that of M. Herpin, Surgeon to the Hospital at Tours, and Professor at the school. A child seized with Diphtheria, who had transmitted the disease already to its nurse, was placed under his care; and at one of his visits, by access of cough, part of the diphtheritic matter was ejected from the mouth, while the process of sponging the pharynx was being performed, and it lodged on the aperture of the nostril of M. Herpin. Occupied with his task, he neglected for a moment to remove it. A severe Diphtheritic inflammation of the part ensued, which spread over the whole nostril and pharynx. Extreme constitutional disturbance occurred, and the prostration was so severe, that convalescence occupied more than six months. Dr. Gendren, of Chateau de Loire, received a shower of tracheal Diphtheric exudations, expelled by a young patient during an access of coughing. Laryngeal Diphtheria set in, with urgent symptoms. Prompt measures saved him.—*Lancet*, April, 1859, p. 288.

\* Of the many internal remedies which have been advised, we do not know of any on which so much reliance can be placed as on the tincture of sesquichloride

ride of iron with chlorate of potass, chloric ether, and hydrochloric acid sweetened with syrup," may be employed by those who fancy many not incompatible compounds in one conglomerate. But care must be used not to give this mixture with the milk and eggs recommended in the same paragraph of the *Lancet's* Commission; and yet it is hard to see how this could be avoided, if all are to be given "frequently." I would suggest another remedy combining tonic, nutriment, and stimulus: ferruginated cod-liver oil and brandy, in the proportion of two fluid drachms of the former to one of the latter, repeated four or five times a day, or oftener, if the stomach will bear it.

I believe chlorate of potash harmless, but of unproved efficacy in diphtheria. I have employed it, but saw no results except mitigation of fœtor of expiration, when the false membrane was in the stage of decomposition. Charcoal, chloride of soda, etc., perhaps act in a similar manner. Ablation of the tonsils at the beginning of the disease, it appears to me, cannot be too strongly insisted upon. Tracheotomy cannot be ignored, but should be resorted to where other means fail to relieve the cyanosis, and suffocation is imminent from obstruction of the fauces and larynx, or even the fauces alone, the larynx being intact. In this latter condition, tubing the larynx should first be attempted.

It is less difficult to prevent this disease than to cure it. The most scrupulous cleanliness of persons and surroundings, free and constant ventilation, should be insisted on. If there are many children, and the rooms are small, as frequently happens in families where this disease appears, the well ones should be sent away, if possible; and if not, kept out of the room in which the infected child lies. M. Duche insists on the free use of sulphur as a prophylactic: he says, those children who took sulphur were not attacked, in any case; while others, under the same circumstances, fell victims to the malady. It is a simple, harmless remedy, and should be tried as a preventive. M. Duche may be right.

Finally, all our knowledge of this disease may be thus epitomized.

1. Diphtheria is a specific, new, zymotic disease.
2. Its diagnostic sign is an aplastic membranous exudation.

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of iron, with chlorate of potass, chloric ether, and hydrochloric acid, in the form of mixture, sweetened with syrup; full doses being employed, according to the age of the patient, and frequently repeated. A free use should be made of generous wine, beef-tea, coffee, eggs in combination with brandy and wine, milk, and whatever other form of nutriment the ingenuity of the surgeon or the fancy of the patient can suggest. When food is refused, then enemata similarly composed must be administered frequently, in small quantities of two ounces and upward, that they may not be rejected; for it is of the first importance that inanition should not open the last portals of life to the advancing disease.—*Lancet Commission*.

3. The indications of treatment are to remove the exudation, and prevent its re-formation.

4. The treatment is tonic, antiseptic, stimulant, and nutritious.

5. The means of prevention are cleanliness, pure air, free living, and possibly sulphur taken internally.

*Conclusion.*—Diphtheria is the most, *certainly* fatal epidemic that ever visited our race; but it is not *de natura sua* incurable.—*American Med. Monthly.*

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### *Atropine in Epilepsy.*

Dr. Max. Maresch, availing himself of his position as physician of an establishment for the insane, at Vienna, has submitted some epileptic patients to the influence of atropine, and has published the results in the *Vienna Journal of Medicine*. Dr. Maresch's experiments were made upon eight patients in the female department, and ten more in the department of the incurable insane; four of these were men, and six women.

Of the eight first patients, three were completely cured, and the condition of the five others so notably ameliorated, that it was impossible to deny the beneficial effect of the atropine. Of the ten patients belonging to the class of incurables, eight experienced a marked diminution in the violence and frequency of their epileptic attacks, as well as in the exacerbations of their physical troubles. These results, united to those obtained by others in the treatment of epilepsy by atropine, merit serious attention.

Maresch has carefully noted the therapeutic phenomena which have arisen during the administration of this remedy. One-fiftieth of a grain of atropine, gave rise, in every case, to effects which habitually follow the administration of this agent, such as dryness of the throat, difficulty in articulating, visual aberration, dilatation of the pupils, etc.; phenomena to which by degrees the patients become habituated, which, however, do not cease during the entire treatment. In every case the pulse lost eight or twelve pulsations during the first hour after taking the remedy, but the pulse resumed its normal frequency so soon as the other therapeutic phenomena manifested themselves. There did not occur, in any case, a marked and permanent acceleration of the pulse, under the influence of the dose above indicated. As special phenomena, Maresch observed in those cases while under the influence of atropine, an exanthem analogous to roseola, which soon disappeared under the influence of warm baths and the discontinuance of the remedy. It is, besides, worthy of remark, that atropine did not give rise, in any



case, to digestive derangement, or any other unfavorable symptom.

The administration of this remedy has not proved beneficial in the other forms of mental diseases. Dr. Maresch administers atropine as follows: he dissolves a grain of it in five hundred drops of rectified alcohol, and of this solution he gives from five to ten drops, (from one-hundredth to one thirtieth of a grain.) This dose is administered once, in the morning before breakfast, from which coffee, tea, and chocolate must be excluded, as these substances interfere with its action. This is continued from sixty to ninety days, without interruption, then is to be resumed after an interval of from thirty to forty-five days. With women, there is no need to suspend its administration during menstruation, as it favors and augments this discharge. Rarely does atropine give rise to constipation, more frequently to diarrhœa, which necessitates, when it becomes severe, a suspension of its administration for some days.—*Translated from L'Union Médicale, by S. E. C.—American Med. Monthly.*

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*Reflections upon the Use of Raw Meat in the Colliquative Diarrhœa of Infants at the Breast.* By Dr. J. F. WEISSE, Director of the Hospital for Children, at St. Petersburg.

A PERIOD of seventeen years has elapsed since the attention of my confrères was called to the great value of this remedy in this disease, but it did not come into general use until five years later, and after the publication of more extensive works upon the subject. About this time, Dr. Behrend, of Berlin, addressed me a letter which contained the following passage: "You cannot imagine how much interest your communication upon the treatment of the colliquative diarrhœa of infants at the breast, by raw meat, has excited; we now use it exclusively in this disease." Soon after, Dr. Behrend inserted in the sixth volume of his journal, a letter from M. Marotte, Physician of the Central Office of the Hospitals of Paris, addressed to Professor Trousseau, in which the author gave a theory to account for the results which I had obtained. From this latter time, the treatment by raw meat has been generally admitted everywhere, and its utility has become incontestable.

Among the numerous favorable reports recently published, I will cite that of Dr. Eichelberg: "In consequence of the short time which has passed since this treatment was recommended, I have by me only a limited number of observations, (twenty,) but all prove its efficiency. The cases in which infants refuse raw meat are very rare; the majority swallow it with avidity. I have observed two cases which were very striking; in these

the children took the meat for several weeks without repugnance; a very great amelioration was felt in the general system; all at once they refused it, as if guided by a natural instinct. The want of osmazome induced the infants to devour the meat, but as soon as the equilibrium was established in the economy, this want disappeared."

Dr. Eichelberg makes use of raw meat only in the treatment of diarrhœa which attacks children fifteen days or three weeks after they have begun to take the breast; the cure is then certain; for myself, in ordering this regimen, I also have employed it in this affection only; and now after twenty years of experience, I maintain that raw beef, reduced to a pap by scraping, is, to the exclusion of all other medication, the veritable specific for this diarrhœa which causes so much ravage. I am not able to admit the assertion of Mr. Charles Hogg, in recommending beef-tea, so well known in England: "Beef-tea is an excellent aliment, very nutritious, and easy to digest; it replaces completely the juice of the meat obtained by scraping, which is so lauded by Dr. Weisse, of St. Petersburg." I have found raw meat not an aliment for infants, but a remedy for diarrhœa; further I have never spoken of the juice of meat, but have recommended the muscular substance itself, hashed or grated in such a manner as to be easily swallowed and digested. The end proposed is to cause the muscular substance itself to pass into the digestive tube, and beef-tea has no more effect upon the diarrhœa of children at the breast, than the excellent decoction of meat, praised by Liebig. These two liquid aliments, from the simple fact of their fluidity, traverse too rapidly the intestinal canal. By giving the meat in pap, the solid parts sojourn longer in the intestine, acting by contact, and being able, by exciting the intestinal mucous membrane, to stimulate absorption; it is also probable that this means contributes to neutralize the acidity of the gastric juice. I cannot partake of the hope expressed by Dr. Beer, of seeing cod-liver oil replaced in the *Materia Medica* by hashed raw meat. Each of these excellent remedies has its sphere of action marked out in the treatment of the diseases of infants; raw meat combats the diarrhœa of the newborn; cod-liver oil triumphs over rachitic affections with or without atrophy.

The treatment of infants by raw meat has come into general use at St. Petersburg, and this use has been propagated rather by proofs of its good effects, than by special writings. The most part of our confrères, employing it with the ordinary means have failed, have also verified the good results obtained by myself. So far as it concerns myself, I have employed this treatment in nearly two hundred cases, and the result has been always satisfactory when the disease was taken in time. I say

when the disease was taken in time; for, if the malady is too far advanced, and has assumed the characteristics of gastromalacia, (softening of the stomach,) a cure is rarely obtained. Sometimes, even in these circumstances, we may succeed in calming some symptoms so fatiguing to the little patient, such as the inextinguishable thirst, and the vomiting.

In a large number of patients cured by raw meat, verminous affections have been observed, and particularly tænia, otherwise very rare at St. Petersburg. Dr. Braun stated this as his opinion, and two years after, a high authority, Prof Von Siebold, of Munich, expressed the same idea, in the last paper of his interesting work, "*Weber die Band und Blasenwürmer*," Leipsic, 1854: "We should no longer be surprised at the fact, and must credit those physicians who have declared that they have found in many patients submitted to a diet of raw meat, the tænia. And Herr Von Siebold has remarked that in every case the tænia solium was found, and he believes that in all probability, this worm, which is not indigenous to St. Petersburg, has been brought there by the cattle driven from Tocherkask and Podolia. Some weeks before my departure from St. Petersburg, in the month of June of the present year, a tænia, more than ten feet in length, was sent to me by a confrère, to whom I had warmly recommended meat in the case of an infant aged 18 months. The tænia was expelled after the administration of the etherial oil of male fern. This remedy was employed because the child, having been cured of its diarrhœa by the raw meat, had several times passed parts of tænia.

I ought not to forget to say that in the hospital for children, which is in my charge, I have often tried, but unsuccessfully, the raw meat in the diarrhœa of children more aged, who were sick from no cause connected with dentition. More frequently this diarrhœa was caused by ulcerations of the intestinal canal.—[Excerpta translated from *L'Union Médicale*, by S. E. C.—(*N. Orleans Med. and Surg. Journal*).

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#### *Action of Mercury on the Secretion of Bile.*

AMONG the effects of medicine which are usually considered to be well established, the action of mercurial preparations in increasing the flow of bile has been admitted without question. Physicians speak about stimulating a sluggish liver by blue pill, or calomel, as if it were not only the easiest thing in the world, but as if we had ocular demonstration of the process; and the frequency with which the liver is thus stimulated to an increased secretion of bile, is in proportion to the ease with which it is supposed this may be effected. No matter what the disease, it



is the commonest thing to begin the treatment by an active mercurial purgative.

Some experiments by Dr. George Scott, of Southampton, England, made on dogs, with a view of ascertaining whether mercury really increases the flow of bile, lead to the conclusion that the hitherto-received opinions on this subject, are erroneous, and that calomel, at least, does not increase the biliary secretion. Having ascertained the average quantity of bile secreted in twenty-four hours, by collecting it in a vessel, after the common duct was tied, Dr. Scott administered calomel to the dogs, and then noted the amount of bile, the quantity of food and drink taken being the same. The four experiments of Dr. Scott all gave the same result,—that *there was a diminution in the amount of bile secreted after the administration of large doses of calomel*. If these experiments should be confirmed by future ones, a revolution may be expected in the treatment of diseases supposed to be connected with a deficiency in the biliary secretion; and that much-abused organ, the liver, will be allowed some rest from the incessant appeals which are made to it, as the source of so many functional diseases. We are glad that Dr. Scott has undertaken to investigate the effects of calomel on the liver by direct experiment. We hope he will continue his researches, and extend them to other subjects connected with the action of medicines. There is no department of our science in which so little is known, or in which more light seems capable of being thrown by direct experiment.—*Boston Med. and Sur. Journal*, and *N. Y. Monthly Review*.

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*Defective Assimilation in Infants—its Prevention and Treatment.*

Dr. ROUTH read a paper on this subject before the Medical Society of London. The object of the paper was to show that most of the mortality of infants was due to defective assimilation. Defective assimilation was almost always the result of want of breast milk and the use of injudicious food; the disease was most effectively prevented by supplying this milk. Dr. Routh then detailed the result of breast milk exclusively given, artificial food without breast milk and with it, or the development and mortality of children, from tables of Messrs. Merei and Whitehead; from which he showed that in proportion as breast milk predominated, in proportion was good development observed, and *vice versa*. He then showed that the most frequent diseases amongst children were abdominal diseases, occurring in the proportion of 23.4 per cent.; developmental diseases in that of 8.8 per cent. of all cases; rachitic diseases constituting 3.2 per cent.; atrophy or marasmus, 5.2 per cent. He believed, however, that all these were produced by defective assimilation, the former in most

cases being sequelæ of it; atrophy or marasmus being only the more marked and characteristic stage.

Dr. Routh then described the disease as consisting of three stages: first, or premonitory, in which peevishness, some loss of flesh, occasional attacks of indigestion, acid eructations, &c., were most prevalent; in the second stage, *emaciation* was more marked, eyes became unusually bright, much loss of digestive power, sometimes with diarrhœa and lientery; third, or exhaustive stage, generally attended with diarrhœa, aphthæ, frightful emaciation, complete loss of digestion, &c. Sometimes the disease from the second stage passed on to tuberculosis, rachitism, and most developmental disorders, and not to the third stage.

*Causes.*—The predisposing causes were—hereditary tubercular habit, and exanthemata; exciting causes—bad air, want of cleanliness, injudicious food, and especially an atmosphere contaminated by too many children being congregated together.

*Post-mortem Appearances.*—Three kinds: emaciation very great, loss of adipose, cellular, and muscular tissue, in all varieties; but in one, where diarrhœa has been present, red patches, or aphthæ over the alimentary mucous membrane, these aphthæ often containing the *oidium albicans*. In other cases, also with diarrhœa, the mucous membrane exuding a reddish coloured mucus, intensely acid. In others, without diarrhœa or with it, Peyer's glands projecting, and enlarged in patches, as in Asiatic cholera. In all, undigested matter in canal, with very fetid fecal matters.

The disease seems to be gradual, passing on to entire loss of *primary* assimilation: the secondary still persisting, although inactive from want of assimilable matters to take up. Albuminous, starchy and oily matters were not digested.

The *treatment* consists in supplying fatty acids and already artificially digested animal, and occasionally vegetable substances, especially human milk. If this could not be sucked, it should be collected in a cup and given by the spoon. Dr. Routh strongly animadverted here upon the absurd dogma, that it is wrong to mix human and cow's milk. He, on the contrary, believed the plan not only safe, but the very best practice in many cases, and the only means of saving an infant's life. Simple juice of meat, and this with vegeto-animal food, he had found most useful in fulfilling these indications. The remedies were of two kinds: 1st. Those calculated to increase cell growth and development. Phosphate of soda producing an emulsion with fats, thus allowing of their assimilation; chloride of potassium, to dissolve carbonate of lime; phosphate of lime, to enable the blood to take up more carbonic acid, and thus hold in solution more carbonate of lime, (these substances severally strengthen-

ing muscular and bony structure); lime-water, to provide lime to blood. 2d. These last also acted as some of the remedies calculated to allay local irritation of the alimentary canal. Carminatives were useful, such as dill, but especially cinnamon-powder, to correct flatus and to check diarrhoea. Anodynes were also (however objected to generally,) strongly recommended by the author. For the diarrhoea, when present, nitrate of silver and sulphate of copper were the best remedies. Wine was also found very serviceable, even if given in large quantities. These remedies, however, it must be confessed, proved in most cases of no avail in the third stage, which was, he might say, almost incurable; but they acted very effectively in the second and first stages.—*Lancet* and *American Journal*.

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*Perineal Section for Cure of Stricture of the Urethra.*

Mr. HENRY SMITH states, that "about a fortnight since I had in my house on the same morning, two unfortunate gentlemen, in the prime of life, who had undergone the perineal section; one, five years since at the hands of a London surgeon of large experience; the other, three years previously, had been cut by Mr. Syme himself in Edinburg. They were both in a wretched condition; the one had his perineum riddled with three fistulous openings, but as Mr. Syme himself was not the operator here, the proceeding was of course unskilfully and improperly done, and I will say no more about it. The other case, however, was treated by Mr. Syme himself, and the operation was of necessity done according to the rules laid down by that surgeon. Yet, where is the complete remedy? The unfortunate gentleman assured me, that before he left Mr. Syme's care after the operation, he felt the stricture returning, and that notwithstanding he persisted in passing the instrument every fortnight, as he was told to do, the canal contracted, so that he was unable at the expiration of ten months to pass anything at all. Since then he has been roaming about from one surgeon to another, in the hope of getting his stricture dilated. When he first consulted me some six weeks since, I could only with the greatest perseverance, succeed in introducing a small No. 4 catheter, although a distinguished surgeon in Dublin had introduced a No. 8 only a fortnight previous. There was no fistulous opening left here, as in the other; but there is the irritability and contractility of the urethra as before, although Mr. Syme performed the operation himself, and according to the information given to me by the patient, assured him it would be a certain cure. I have before enunciated the opinion, that not only does this operation not prevent the return of the stricture,



but that the recontraction becomes more severe than before. As I had not the opportunity of seeing this gentleman before he was cut, I cannot positively state whether this holds good here or not; but that the induration still remained, or had reformed at the site of the old disease, was quite evident by running the finger along the perineum. Cutting cannot possibly get rid of this; and so long as this remains, the stricture will persist.—*London Med. Times and Gaz.*

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*Cancerous Ulceration close to the inner Canthus, treated with Sulphate of Zinc Paste.*

Dr. WILLIAM MACKENZIE, of Glasgow, relates the following interesting and instructive case. A patient, aged apparently about forty-five, and who attributed the spread, if not the origin, of his complaint, to the depression of mind arising from pecuniary loss, applied to me on the first of February, 1859, on account of a cancerous ulcer on the side of the nose, close to the inner angle of his left eye. It was fully the size of a four-penny piece, of irregular shape, covered with a scab, and surrounded by hard and elevated edges. The papillæ and carunculæ lacrymales, and the palpebral conjunctiva were inflamed, and the disease seemed to be extending in the direction of these structures.

I felt no doubt, that were the ulcer left to itself, it would gradually spread, and prove rebellious to all applications of a soothing description. Having repeatedly found, also, that extirpation of such a disease by the knife, even when care was taken to cut into the sound integuments, although followed by a firm cicatrix and an apparent cure, was succeeded after a time by a renewal of the scirrhus hardness and intractable ulceration. I determined in this case to try as an escharotic, the sulphate of zinc, as recommended by Dr. Simpson, of Edinburgh. I was partly led to this course, too, by observing that I could not extirpate the diseased part without removing the lower papillæ lacrymalis.

Having driven off by heat, the water of crystalization of a few grains of the sulphate of zinc, and reduced the residuum to a fine powder, I mixed it with a little glycerine, so as to form a thick tenacious paste. Taking a little of this on the point of a bit of stick, I applied it over the scab and over the hard edges of the ulcer, and covered the part with dry lint.

Next day, I found that the application had given very little uneasiness; but that it had acted in destroying almost entirely the hard edges of the sore, and left the whole of its surface free from scab and of a florid healthy color.

Two or three times subsequently, I covered the edges again with the zinc paste; after which I left the wound to cicatrize, under the application of dry lint. I touched the conjunctiva and papillæ lacrymales occasionally with the four grains' solution of nitrate of silver, under which application they speedily freed themselves of inflammation.

On the 24th March, the patient called on me, with a firm cicatrice, of a healthy hue, in the site of his former disease.—*Ophthalmic Hospital Reports*, and *Ibid.*

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### Quinine Ether.

M. EISSEN has recommended in the *Gazette Médicale de Strasbourg*, the use of quinine, introduced into the air-passages, for the treatment of intermittent fevers. The process consists in the inhalation of quinic ether, a combination made by M. Manette, and first used by M. Pignacci, of Milan.

The substance, still incompletely defined, in a chemical point of view, is obtained by means of the distillation of alcohol, treated by sulphuric acid, (theory of ethers,) in presence of the quinate of lime. The product of this distillation is a liquid perfectly limpid, colorless, of an agreeable odor, less volatile than sulphuric ether, but still sufficiently volatile to evaporate at an ordinary temperature without leaving a deposit. It deserves, then, the name of ether; and its therapeutical action, besides, seems to justify the qualification of quinic, which has been given to it.

Quinic ether, says M. Eissen, fills all the necessary conditions of a good therapeutical agent. It acts, at the same time, *tuto, cito et jucunde*. Inhaled in the dose of a few grammes, (2 to 3,) as chloroform is generally employed upon a compress, it checks a commencing access, and prevents a return of subsequent accesses. In all the cases in which it was tried, the access gradually yielded, never to return, when the fever was simple, or under a very mild form in cases of decided cachexia.

Since the first trial upon patients in Lombardy, who could not be suspected of being slightly affected, other trials have been made by Prof. Groh, at Olmutz, and with the same success. The results have been the same: whether the inhalation was made before or during the pyrexia, the access was lessened in character in a marked degree, and the next anticipated access prevented, in the majority of cases; the tumefaction of the spleen disappearing at the same time. The inhalation, far from being disagreeable, was followed by good results, or of a sensation of decided amelioration. We may add that, in their experiments, our learned confrères, whose names we have cited, were

careful to establish negative proofs to confirm their judgment. They submitted a certain number of fever-patients to inhalations of pure sulphuric ether, or sulphuric ether holding sulphate of quinine in solution. The inhalations of pure ether produced no other effect than that of increasing, in an insupportable manner, the hot stage; while in those taking the ether containing the quinine, some anti-periodic effects were observed, after large quantities of the remedy were absorbed; but in severe cases these effects were so slight, after long trials, that the patients themselves solicited more energetic measures.—[*Amer. Med. Mon.*

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*Vegetation of the Genital Organs—Chromic Acid.* [Translated from the Gazette Hebdomadaire for the Boston Med. and Surg. Journal.] By O. D. Palmer, M. D., Zelenople, Pa.

CASE.—C., 23 years of age, of a good constitution, never having been diseased, became pregnant for the first time toward the end of October, 1856. About the same time she was taken with an abundant blennorrhagic discharge. Having arrived at the fourth month of gestation, she experienced a sense of heat and suffering in the genital organs. This painful sensation was produced by the presence of numerous prominences in the vagina, which had replaced the very copious and extremely foetid purulent discharge. Careful cleanliness, injections, and baths, procured no ease. Two months passed, during which the disease was making rapid progress.

C. presented herself at the hospital, where she was admitted on the 30th of April, in the sixth month of her pregnancy. It was ascertained that over the labia majora and minora the vaginal canal, and even the cervix uteri, was a growth of excrescences of considerable size, and in great number. The most voluminous, as large as the fist, projected outside of the vagina. Of these vegetations some were with pedicles, others were sessile; their tints were reddish, their appearance vascular. They were for the most part divided and subdivided, forming ramifications, which in their aspect offered some analogy to the corymb of the millifolium. In the vagina these excrescences had acquired such dimensions, such a development, as to fill all the cavity, and not to permit, without the greatest difficulty, the introduction of the speculum.

In searching for the cause of these vegetations, it was impossible to recognize for them a syphilitic origin. The woman had never had chancres. An attentive examination of the genital parts did not discover any ulceration. There existed no engorgement of the inguinal or sub-occipital glands, no squamous eruption.

In such a state of things we could not but foresee great difficulty at the time of accouchement. It was, then, very important



to find means to destroy this obstacle. For this end, M. Rousset applied, first, crayons of nitrate of silver, then the nitric oxide of mercury. This last means, continued during twenty-five days, with much care, caused very smart pain. Like the preceding, it was not followed by any advantageous effects. Tincture of iodine had no good result.

On the 6th of July, after all these failures, Prof. Rousset had recourse to the use of *chromic acid*. He made application of a solution prepared as follows: R. Chromic acid, 1 part; aquæ distilled., 3 parts. By the employment of a pencil, each excrescence was separately washed with this liquid, having care not to touch the mucous membrane that surrounded it. The patient experienced immediately, in the part touched by the solution, a sensation of smarting and pain, which was calmed again directly. Redness was manifested, a little swelling, in short a slight inflammation, which was terminated by the formation of pus. Some few washings with Goulard's water, and the use of dry lint, constituted the whole treatment. In proportion as suppuration was established, the excrescences were detached, disappearing, and leaving in their place a reddish surface, in some points excoriated, in others ulcerated, very superficially. The cicatrization was regularly and uniformly established.

July 20th, the cure was complete; the cervix uteri, the labia majora and minora, and the vagina, were entirely freed from these adventitious productions, and offered an aspect which was altogether normal.

On the 26th accouchement took place without any accident. The child of the masculine gender, was perfectly formed, and afforded no traces on its body that could cause the mother to be suspected of any affection of a specific nature.

The chromic acid has been extolled, these few years past, as a caustic, both by Dr. Keller, of Germany, and by Dr. Marshall, of London, for destroying vegetations developed on the genital parts. Dr. Marshall uses the acid in solution. Dr. Keller applies it in the form of paste. Whatever process is adopted, this caustic is easily managed; its action is at the same time very rapid, little painful and thorough. We have a right to conclude, then from this case, that in these excrescences the chromic acid is a powerful caustic, and preferable to others that have been hitherto employed.—[CAUSSADE of the School of Bordeaux, and *Boston Med. and Surg. Journal*.

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*Cases of Suppression of the Urine.* By JAMES ALEXANDER, Esq., Surgeon, Wooler.

THE pathology of the disease, described by our older writers under the name of *ischuria renalis*, is little known, and the dis-

case itself is a very rare one. Dr. Abercrombie treats of suppression of urine as resulting sometimes from disease of the kidney itself; sometimes from disease, generally inflammatory, of some adjacent organ; and only very occasionally as proceeding from some unknown cause affecting the nerves of the organ, and leaving few or no morbid traces after death. The same view, substantially, is taken by recent systematic writers; with a strong inclination, perhaps, to refer all, or almost all, cases of suppression to some stage or modification of granular disease of the kidney. I cannot pretend to throw any light on the intimate nature of the disease; nor have I the means of going into the literature of it. But, perhaps the two following cases, which occurred to me very recently, may possess sufficient interest to deserve being recorded in the *Edinburgh Medical Journal*:—

On the 13th February last, I was requested to visit a shepherd lad, aged 16, residing about seven miles from Wooler, who appeared to be laboring under the symptoms of ordinary continued fever, which his friends imagined had been brought on by cold and exposure to wet. He had been ill about ten days. His pulse was about 100; his tongue loaded, and his throat slightly inflamed and painful; he complained of headache, but had little delirium; there was a good deal of restlessness, and his urine was scanty and high-colored; there was no cutaneous eruption. He was ordered some mild aperient, his diet carefully regulated, and, as his pulse was weak, a small quantity of wine was directed to be taken at intervals. On the 15th, the symptoms were nearly the same, but the quantity of urine was very much diminished; on the 16th, totally suppressed; and about midnight of the 17th, he died, just a few minutes before I entered the house. Before his death there was partial stupor, but no profound coma, and slight irregular movements of the muscles of the face and eyes, but no general convulsions.

Five weeks afterward, I was sent for to see a younger brother, who was reported to be ill of the same disease of which the first brother had died. I learnt that, in the interval between the death of the elder brother and the seizure of the one I was now visiting, a sister had been ill, as the people supposed, of the same disease, but had passed through it so mildly that no medical advice had been sought for her. My present patient exhibited similar symptoms to those presented by his brother; he had been ill seven days; there was headache, slight sore throat, great general uneasiness, and already the same noticeable diminution in the quantity of water voided was beginning to manifest itself; there was also some spots on the abdomen resembling the eruption of typhoid fever; but as the youth was liable to an anomalous rash in the spring months, I would not lay much stress on that symptom. The following day the pulse had fal-

len to 70, and became much weaker, and the urine was totally suppressed; there was no delirium and no coma, no pain in the back, nor the slightest tenderness over the abdomen. Free leeching to the region of the kidneys was had recourse to, and repeated thrice in the course of the next three days; the back was rubbed with a strong turpentine liniment; and the bowels opened by compound powder of jalap. After the first application of the leeches, a small quantity of water was secreted, but no change took place in the strength or frequency of the pulse; but gradually, under the use of the remedies mentioned, the pulse began to rise, the urine became more abundant, and the symptoms of affection of the brain gradually subsided, and in a week's time all the symptoms of the urinary affection had ceased. The symptoms of general feverish action ran on for a few days longer; but, in twenty days from the date of the first shivering, the boy was convalescent, and continues to this time in good health.

It must be acknowledged, that it is at least a singular coincidence, the occurrence of two consecutive cases in one family of symptoms so unusual as those I have detailed. The progress of the case last detailed, and the occurrence of the girl's case between those of the two brothers, renders it probable that I am correct in considering these as original cases of ordinary fever; and if so, the urinary symptoms form a complication certainly not usual and not altogether without interest. I have seen, in the course of my practice, besides suppression more or less complete from evident inflammatory affection of the kidneys or adjacent organs, one or two cases of what I believe was genuine ischuria renalis, as described by our older writers; and with a very brief notice of these I shall conclude this paper. My first case occurred in a young man, 22 years old, of unusually dark complexion, and developed itself suddenly. The cessation of the urinary secretion was total in sixteen hours after the accession of the disease, and could be ascribed to no probable external cause. The pulse was slow (under 60,) and there was some degree of giddiness and somnolence almost from the beginning; otherwise the general health was not materially affected. The lad was bled to ten ounces from the arm; leeches were applied to the back; the warm bath was used; and turpentine liniments rubbed on the region of the kidneys, the bowels being sharply acted on by calomel, followed by large doses of cream of tartar. Under this treatment, at the end of sixty hours, a small quantity of urine was passed, which gradually increased, and in little more than a week's time, he had nearly regained his ordinary state of health; nor was the secretion of urine ever subsequently interrupted. Another case occurred in a boy who had passed through an exceedingly severe and prolonged attack of croup,



which had been treated in the usual way, by bleeding, calomel, and antimonials. After having coughed up considerable portions of false membrane, some fragments of which were distinctly tubular, he had seemed, in about nine days, satisfactorily convalescent, the breathing perfectly free, the pulse natural, and all the symptoms of the disease completely gone. The tenth day from his seizure, I was summoned to visit him in haste, and informed that he had made no water for nearly twenty-four hours. To guard against the possibility of mistake, I passed a catheter into the bladder, a precaution which, I forgot to say, I adopted in all the cases I have related, with the result of finding, as I did in all the rest, the organ quite empty. He was treated in a similar manner to the last-mentioned case, but without any benefit, and on the third day he died comatose, not very profoundly so, however, death being preceded, as in the first of the above cases, by slight twitchings of the facial muscles and distortion of the eyes, but not by any convulsive movements of the limbs or body. About a week after the death of this last-mentioned patient, I was requested to visit a boy aged 10, who had not made water for nearly twenty-four hours; the boy was moving about, nor was there the slightest symptom of indisposition discoverable upon examination. His pulse was natural, his tongue clean, his skin cool, his appetite good. His mother had discovered that he made no water while he was in her sight, and upon questioning him he affirmed that he had made none at all, and as his friends lived in the immediate neighborhood of the youth who had died after croup, they took the alarm and sent for me without delay. Leeches, purgatives and other remedies were employed pretty actively, but without the slightest effect in restoring the secretion; the second and third day passed and no water came, still the boy gave no signs of indisposition, and except an occasional warm bath, and attention to the state of the bowels, little or no further treatment was had recourse to. And thus the boy went on *for four weeks*, without voiding during the whole time *one ounce of water*, without any noticeable inconvenience, and without, as far as I could see, any vicarious discharge. There was no urinary smell, either in the fæces or in the sweat, which was little if at all increased. At the end of a month the urine began to be again secreted, and gradually increased in quantity till it reached its ordinary amount, the first portions that were voided producing a good deal of smarting and pain in the urethra, which, however, subsided by degrees. It was, of course, impossible for me to have this boy so constantly under my own eye, as to be able to state from my own personal observation that no urine passed; but his mother was both an intelligent and respectable person, every precaution was taken to prevent mistakes on the boy's part, and

no conceivable motive existed for deception on the part of either him or his mother. I have, therefore, myself no doubt whatever of the fact I have stated. Both this case and the two immediately preceding it were communicated at the time to the Border Medical Society; so, although the cases occurred many years ago, I am quite confident of the accuracy of the facts I have detailed. And I hope they may be deemed sufficiently interesting to deserve a place in a more permanent record.—[*Edinburgh Med. Jour.*, and *Boston Med. and Surg. Jour.*

*Epilepsy succeeding a Suppression of the Menstrual Discharge, Cured by the Supervention of Dysentery, and the Restoration of the Catamenia.* By SAMPSON EAGON, M. D., of Bot. County, Va.

Miss T. R., æt. 17, had the menstrual function established at 15, and it continued normally until the 17th year, when from exposure of the feet to moisture and cold at one of the periods, the catamenial discharge was arrested. For several periods she suffered the ordinary pain and discomfort of amenorrhœa, and at the fourth month of amenorrhœal suffering, she was attacked with epilepsy; I was at this period called to see her. After an unsuccessful effort to re-establish the catamenial flux, (it being a period,) the patient was put upon treatment for epilepsy, the treatment consisting principally in the use of tonics, antispasmodics, revellents, and emmenagogues. The epileptic convulsions, however, continued to recur with undiminished violence, notwithstanding the treatment was assiduously persevered in for more than twelve months. The argente. nitras, several of the salts of iron and copper, as well as sulphate of quinia, were given a fair trial, but without decided benefit.

The paroxysms in this case were of the most violent character, and lasted for several hours, night and morning of each day. After having fairly tested the virtues of the most popular anti-epileptic means in the case, with so little success, Dr. Woodson, (the consulting physician,) and myself abandoned all hope of relieving her. In July, 1858, dysentery became epidemic in her locality, with which disease she was attacked in a severe form. Having been called again to the patient at this period, and hoping now that the constant tenesmus and irritation of the large intestine might extend by contiguous sympathy to the torpid uterus, I was careful not to arrest the dysenteric discharge too hastily, prescribing only a half grain of opium, with two of the mild chloride of mercury, repeated every eight hours. On the second day, I learned from the patient's mother, that the menstrual discharge was regularly flowing, and I found

the young lady free from epileptic symptoms. The dysentery soon subsided, and the young lady quickly returned to almost perfect health, and is now quite well. The only means that have since been employed in the treatment, have been a pretty active purgative of aloes and rhubarb, conjoined with a warm hip-bath, used for several nights before each expected return.

There are two points of interest, as I conceive, in the case above detailed: 1st. The undoubted relation which here existed between the amenorrhœa and epilepsy as cause and effect; and 2nd. The means employed by nature to produce relief, an irritation propagated from the rectum and colon to the uterus by contiguous sympathy.—[*American Journal*.

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*After-treatment of the Extraction of Cataract.*

THE old method of bandaging the eyes is never practiced in the Central London Ophthalmic Hospital, and the modern one of plastering the eyelids is adopted in its stead. The object is to keep the eyelids motionless and closed, in order that the edges of the wound in the cornea may be kept in accurate adaptation by the gentle pressure of the eyelids. If a patient can keep his eyes shut, no appliance is needed; but few can do this. As the bandages are ordinarily applied, they are useless. To make them potent as bandages, would demand a degree of pressure that must be injurious. Very much annoyance is often produced from the cloth being stuck to the cheek, by the aqueous humor when it trickles away; and the night's rest is broken in consequence. A couple of straps of court-plaster, the eighth of an inch wide, accomplish all that is desired. Mr. Walton generally allows them to remain for a week; but if any unpleasantness is complained of, such as stiffness or hardness, they are removed after the third or fourth day. To this plan, which is wholly unobjectionable, Mr. Walton attaches much value; and he is convinced that the highest attainable results from the operation of extraction cannot be obtained without it. He always keeps the eyelids closed till the aqueous humor no longer escapes from the corneal wound; that is, till the cornea is quite healed. At the end of the week, when the patient has opened the eye, Mr. Walton does not raise the lid himself, but desires the patient to open the eye naturally, which he considers far preferable; he again closes it, if requisite, with court-plaster for some days. There can be no doubt of the prudence of this. Many an instance of prolapse of the iris, with all the irritation and ulterior consequences, has been produced by the use of the eye before there has been proper reunion. Young operators are often sadly puzzled to account for the secondary imperfection of prolapse of the iris, after they have had such promising



results, when the eye was looked at a few days after the operation. This is the solution; the eye is used far too soon, and by the action of its own muscles the prolapse is occasioned.

The great proof of cicatrization of the cornea is the fulness of the anterior chamber. So long as the iris is pressed forwards, and there is not the natural accumulation of aqueous fluid, the eye should be kept closed. A very little attention will enable the practitioner to decide this point.

When the iris is wounded in the operation of extraction, prolapse is very apt to occur under any condition; hence, Mr. Walton lays it down as a rule, that much pains should be taken to prevent the accident. He considers it far better on every occasion to finish the operation with the secondary knife rather than to complete it with the cataract knife; when, by so doing, the iris is not at all injured. The late Mr. Alexander, than whom there never was a better operator, always made the section of the cornea with two knives. As soon as he had effected the counter-puncture with Wenzel's knife, he withdrew it, and completed the section with the secondary knife.—[*British Med. Journal*, and *Nashville Med. Record*.]

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[From a valuable summary prepared by Dr. O. C. GIBBS, of Frewsburg, for the *New York Medical Monthly*, we select the following several articles.]

*Iodide of Zinc in Chronic Conjunctivitis.*—In the *Peninsular and Independent*, for August, Dr. Waggoner, of Oconee, Ill., has a short article upon the treatment of chronic conjunctivitis. After correcting the general health, if deranged, he advises the application of one-sixteenth of a grain of sulph. morphine, dissolved in water and applied with a camel's-hair brush, twice a day, until all irritability is allayed. After this follows the curative treatment, which does not materially differ from that usually put in requisition, except that advised in his concluding remarks. He says, "Latterly I have incorporated in my *materia medica* the *iodide of zinc*, the effects of which have proved satisfactory beyond all anticipation. This drug is passed over in all our standard works on therapeutics, in almost profound silence. In no case, in which I have observed its effects in the treatment of ophthalmia, has it deserved, in my humble opinion, a place second to any other remedy. In one scrofulous case, it acted like a charm. Will not the profession give attention to this very deserving agent, and more fully prove its worth?" The Dr. does not give his formula for use.

*Congenital Hernia.*—In the same number of the *Peninsular and Independent*, Prof. Moses Gunn, of Detroit, reports a case of

hernia, descending through a congenital passage for the first time after the age of thirty years. If this case is not *unique*, such cases must be of rare occurrence.

*Miasmata.*—In the *Lancet and Observer* for August, Dr. Harvey, of Springtown, Indiana, publishes a paper upon the above ever vexatious subject. It is difficult to give a synopsis of the paper that will be sufficiently condensed to answer our purpose. He says, "Those diseases originating from miasmatic causes, are produced by a change in the relative proportion of the several gaseous and mineral elements of which the living body is composed; in other words, diseases which appear both epidemically and sporadically, and some others, are produced by the addition to, or subtraction from, the essential elements of the body in health." Again, he says, "In intermittent, bilious, and remittent fevers, the crassamentum of the blood is increased, and darker in appearance than in health. The red globules are darker, and the quantity of serum less than the healthy standard; the fibrin having been either changed to albumen or coagulated to a certain extent. Hence we may infer that there has been *carbonaceous* matter from external sources added in undue proportion, or a decomposition within the body." Want of space will not permit us to give the premises upon which this is based. He adds further, "In such diseases as cholera, dysentery, &c., we must look for other causes; but these may be found in other gaseous or mineral elements or compounds, emanating from similar sources as carbon, and also from the earth. My opinion is, that cholera is produced by the action of *nitrous acid gas* upon the blood. In certain electrical conditions of the atmosphere, the oxygen and nitrogen of the air become chemically united, and form nitrous oxide or protoxide of nitrogen; and these two gasses, being disengaged from decaying animal matter, become united also, with the same result; and the resulting compound floats in the air, and is absorbed through the lungs, skin, food, &c., into the system. I believe that the great amount of moisture in the air, the great heat and electricity every where in those countries where cholera is raging, causes a greater or less chemical union of the elements of the air, thereby producing nitrous acid gas."

The miasmatic element, as productive of disease, and of varying diseases, has been an incomprehensible mystery and vexation to observing physicians for all time, and is likely to be for some time in the future. Some have denied its existence altogether, as an aerial poison, the product of animal or vegetable decomposition, and as causative of endemic or epidemic diseases. We will here instance only S. Littell, M. D., of Philadelphia, who refers such diseases to electrical agencies, and the late J.

K. Mitchell, M. D., who referred them to a cryptogamous origin. That certain diseases occur only when there is a conjunction of certain atmospheric and terrene conditions, is almost universally admitted; that the terrene conditions exercise a controlling influence, is a fact too well established to admit of doubt; and that that terrene condition or element is effluvial, is more than problematical. But why dissimilar diseases from similar causes? That the plague and yellow fever occur only under similar, if not absolutely identical conditions, is well known. High temperature, humid atmosphere, lowness of site, density of population, and animal and vegetable putrescence, with a preponderance of the former, are necessary conditions to the production of a miasm that will develop either of the above-mentioned diseases. If the causes are the same, why not the results? If these conditions develop the plague in Constantinople, why not in New Orleans; and the reverse, in regard to yellow fever? We answer by putting another question. If sugar, starch, and gum-arabic are composed of precisely the same ingredients, in two of the instances, in precisely the same proportions, why are not the physical properties of the resulting compounds identical? If dissimilar substances are isomeric, may not the causes of dissimilar diseases be also isomeric?

Intermittent, remittent, and bilious remittent fevers have also terrene causative relations and dependencies, and those causatives are apparently identical, and always present where these fevers are endemic. The miasm of intermittent differs from the miasm of yellow fever and cholera, but we are not prepared to believe the difference to be what Dr. Harvey suggests. The miasm of an intermittent has a more vegetable origin than that of yellow fever, and its conditions of development are not the same. If intermittent fever has always terrene causative relations, why is not that disease always present whenever those causative relations exist? True to our Yankee instincts, we answer by asking, Why carbon and hydrogen, in the relative proportion of five parts of the former to four of the latter, in combination, are not always oil of turpentine, and not sometimes oil of lemons, as is well known to be the case? Why is not  $C_{12}H_{16}O_{16}$  not always starch, when in combination, and not sometimes gum-arabic and gum tragacanth respectively?

This is not the time or place to enter upon controversial ground, the above thoughts are thrown out simply as suggestives.

*Laryngismus Stridulus.*—In the same number of the *Lancet and Observer*, Dr. R. R. McMeens, of Sandusky, Ohio, reports a case of the above disease, with a few remarks upon its pathology. Dr. Hord, in the *British and Foreign Medico-Chirurgical Review*, declares the exciting and sustaining cause of the disease to



depend upon an enlarged state and disordered function of the liver—a condition of that organ vulgarly termed and treated by nurses as “livergrown,” and the laryngeal difficulty, dyspnoea and spasm of the glottis, are sympathetically excited and secondarily involved; and offers, in support of the allegation, the revelations of twelve autopsies of fatal cases, all confirmatory of the fact. “To the truth of this proposition,” Dr. McMeens says, “I am disposed to differ, from the ostensible efficacy of remedies adapted to such pathological inductions. Cathartic doses of calomel, followed by free discharges of vitiated bilious secretions, were invariably succeeded by a marked amendment, while the absence or deficiency of such secretion was precursor to a decided aggravation of the disease. As an alternative, the proto-iodide of mercury had a most happy influence over the secretions. Dover’s powder usually sufficed to allay any disturbance or pain in the bowels. Frictions along the spinal column were premised for the contractions and insensibility of the extremities, and the inhalation of ether, hyoscyamus, and anise, to soothe the irritability of the larynx and allay cough. Under this treatment, at least, the disease was manifestly modified, and the patient steadfastly improved.

The pathology of this affection has been anything but definitely settled, and anything concerning it we regard with interest. Some, with Drs. Ryland and North, have supposed the disease dependent upon cerebral derangement, while others consider it dependent upon gastric irritation. Dr. Marsh thinks it may proceed from irritation of the pneumo-gastric nerve; while Dr. Ley supposes it dependent upon paralysis of the muscles which open the glottis, in consequence of pressure of enlarged glands upon the recurrent nerves. It is probable that the laryngeal affection is due to a reflex nervous action, the seat of irritation being variously located: sometimes in the gums; at others, in the medulla oblongata; and still at others, in the liver, or any part of the digestive apparatus.

*Erysipelas.*—In the *Medical and Surgical Reporter* for August 13th, J. R. McClurg, M. D., of Philadelphia, has an able article upon the above subject. In regard to treatment, after the action of an emetic and a saline cathartic, combined with a mercurial, if there be symptoms of billious derangement, he says: “I always use some tonic medicine, and my favorite prescription, and the one I uniformly use in all cases of erysipelas, of whatever form or character, as soon as the system is prepared for it, is,

|                                 |          |
|---------------------------------|----------|
| R.—Spiritus ætheris nitrici,    | f. ʒij.  |
| Tinctura ferri sesqui chloridi, | f. ʒij.  |
| Quinæ sulphatis,                | gr. xvj. |

Misce et S. Take a tea-spoonful every three hours. This constitutional treatment I have found very successful in my hands, and desire nothing better."

This corresponds very nearly to the treatment we have found very serviceable. We are, however, in the habit of prescribing the tincture of iron in a little larger doses, say twenty drops every three hours; and the quinine in combination with Dover's Powders, from one to two grains of the former to five of the latter, every four or six hours.

*A Case of Epilepsy Cured.*—In the same journal for August 20th, Dr. S. N. Pierce, of Cedar Falls, Iowa, reports a case of epilepsy, cured under treatment in compliance with the following prescription:

R.—Ext. stramonii.

“ conii,      ãã gr. xv.

Strychnia, (cry.) gr. ij.

Argent. nitr.,    ʒij.

et div. in pil. No. xxx.

“Of these,” he says, “I gave three a day. This course was pursued perseveringly, the amount of the strychnia and nitrate of silver being slowly increased, until I found the disease gradually yielding to its influence. I now have the satisfaction of feeling that the disease is subdued.” Patients have recovered in cases of epilepsy under a variety of treatment that subsequent experience has proved to be of non-effect.

One case is quite too limited to judge of the effects of any medicines. The case of Dr. Pierce loses in interest when it is remembered that the improvement at the time of the report was of but five weeks' duration.

*Mental Influence on the Products of Conception.*—In the *Medical and Surgical Reporter* for August 27th, Dr. Ziegler, of Philadelphia, reports a case of a mother, who, in the early weeks of pregnancy, was badly frightened by what she supposed to be one of her children cutting off the hand of another. She was confined at full time of a child minus the right hand—the radius and ulna terminating abruptly in a stump at the wrist. The editor adds that, four years since, he saw a woman delivered of a child with a deformed nose, which was in exact resemblance to that of an unfortunate girl's who lived next door, who had a cancer of the nose.

The first case was doubtless a case of spontaneous intra-uterine amputation, caused by the cord being wound around the wrist. The second was doubtless a case of incomplete development, which occasionally occurs as freaks of nature, of which

hare-lip is a sample. We once saw a nasal deformity similar to the above, but the lip was cleft. We hope our women are not going to reproduce all the deformities they see.

*Strychnia in Chronic Intermittents.*—In the *New Orleans Medical News and Hospital Gazette*, Dr. Harrison, of Arkansas, has an article upon the use of strychnia in chronic intermittents.

The following is his formula:

|                 |          |    |
|-----------------|----------|----|
| “ R.—Strychnia, | gr. iss. |    |
| Sulph. Quinine, | gr. xv.  |    |
| Capsicum,       | gr. vj.  |    |
| Brandy,         | ℥ iv.    | M. |

“Of this mixture, I direct one tea-spoonful (for an adult) every hour, for six or seven hours preceding the expected paroxysm; at the end of this time I require the patient to take a cup of warm sage tea, and go to bed, (if he is not already there,) and remain until the paroxysmal hours pass. This course is to be repeated on the next ‘chill day,’ after which a tea-spoonful of the medicine is to be taken two or three times per day, until the four ounces are exhausted.”

With Dr. H. F. Campbell’s views of the nature of this disease, and its relation to the nervous system, the philosophy of this treatment becomes at once apparent. This, however, is not altogether new treatment. Dr. Brainard, of Chicago, recommended strychnine, in an eighth-of-a-grain dose, three times a day, in similar cases, more than twelve years ago. (*See Indiana Medical Journal* for July, 1847.)

We are confident that the remedial powers of strychnia are not yet fully brought out. So far as we know, we were the first to use and recommend it, in *sciatica* and chronic rheumatism; and we have seen cases of dyspepsia and chronic costiveness yield to it like a charm.

### *Secale Cornutum in Disturbance of the Accommodation-power of the Eyes.*

WHEN local hyperæmia is dependent upon a laxity of the walls of the bloodvessels, advantage attends, Professor Willebrand, of Helsingfors, states, the employment of *secale cornutum*. He was induced to use it in these cases by the expectation that a means which acts so specifically upon the unstripped uterine muscular fibre must excite some power over the analogous structure of the arteries, and which its hæmostatic action proves, in fact, that it does. During his investigations he soon became struck with the fact that the heart of persons employing it soon underwent contraction in all its dimensions, and that even



within the first twenty-four hours—a circumstance which he has frequently verified since. The first case that came under his care was an example of exophthalmos, accompanied with enlargement of the thyroid gland and hypertrophy of the heart. After a few weeks' use of the secale, the hypertrophy of the heart and thyroid, as well as the projection of the eye, much diminish. The patient however left off the medicine, and the exophthalmos returned worse than ever. Since that period he has employed the secale in various cases in which increasing contractility of the muscles of the bloodvessels or other tissues seemed to be indicated. It was found of especial advantage in a disturbed state of the accommodation-power of the eye, especially induced by over-taxing the organ on small objects with an insufficient amount of light. Children from some of the schools have furnished the author with many instances, and they have always been relieved by the secale. He relates a case in which impaired vision was always brought on by sewing or reading, and wherein the signs of some amount of chronic congestion were visible. Relief rapidly followed, and when the affection recurred some months after, it was as speedily relieved. He has also found the secale of great use in several cases of acute or chronic inflammation of the eye, and especially in blepharitis and the pustular conjunctivitis of children, the case proving much more rapid, and relapse being much less rare, than when local means alone are relied upon. No benefit has been derived from it in granular conjunctivitis and trachoma.

Proceeding upon the theory of its stimulant action upon the vaso-motoric nerves, the author has extended the employment of the secale to other local disturbances of the economy; and, as already observed, he has had frequent occasions of observing its transitory influence in hypertrophy of the heart, without having any reason to believe that it is of any permanent utility in affections of this organ, the heart always returning to its former size soon after the use of the secale has ceased. In many cases of both chronic and acute hyperæmia it has proved of great service, and especially in cases of galactorrhœa, and in indurations, tumefactions, and catarrhal affections of the uterus. Also, it has been very useful in enlarged spleen from intermittent fever, and when large doses of quinine have failed. It is especially indicated in the cases of relapsing intermittent depending upon enlarged spleen. In erysipelatous affections, it has often done good service applied externally as a cataplasm. The author formerly gave ten grains *ter die*, but now gives but five, combining it with magnesia, or, when chlorosis is present, with iron.—[*Brit. and For. Med. Chir. Review*, and *Am. Jour. of Med. Sciences*.

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*The Reparative Process in Human Tendons after Subcutaneous Divisions for the Cure of Deformities.*

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W. ADAMS, Esq., read before the Royal Medical and Chirurgical Society (June 28) a paper on this subject, illustrated by a series of specimens and drawings from fifteen post-mortem examinations. Specimens of reunited tendons after division were exhibited from ten cases, and also drawings, made by Ford, of the recent appearances in thirteen cases, at periods between four days and three years after the operations. These specimens had been collected by Mr. Adams during the last eight years, and were principally from patients operated upon at the Royal Orthopædic Hospital; but for two specimens he was indebted to Mr. Erichsen and Mr. Curling. After alluding to our present scanty information on the subject, and describing the recent appearances in fifteen cases, the author gave a general summary of the reparative process, describing—1st. The immediate results of the operation. 2dly. The commencement and nature of the reparative process. 3dly. The general appearance and structure of the newly-formed connective tissue, or new tendon. And 4thly. The junction of the new with the old tendon. This was followed by an account of the circumstances which may interfere with the perfection of the reparative process, or entirely prevent it, so that non-union of the divided tendon may result. Complete failure of union had been witnessed by the author only in the posterior tibial tendon, but it appeared that there is considerable risk of such an occurrence whenever tendons are divided in or near to dense tubular sheaths. It was shown that imperfect union might result either from some constitutional defect in the reparative powers of the patient, or from injudicious after-treatment in a variety of ways, but principally from too early and too rapid mechanical extension. The conclusions which the author considered to be established by the above series of cases were arranged under nine different heads. It was stated that tendon is one of the few structures of the body capable of reproduction or regeneration, and that the newly-formed tissue acquires within a few months of its formation the structural characters of the old tendon so perfectly, as that, under the microscope, it is with difficulty distinguishable from it; but it does not acquire through its substance the uniformly opaque, pearly lustre of old tendon; in the mass it retains a greyish translucent appearance, so that the recent section affords an easy method of distinguishing the new from the old tendon. The greatest length of perfectly formed new tendon which the author had seen was two inches and a quarter, and this was in the tendo-Achillis of an adult, a year and a half after it had been divided by Mr. Curling. That the process by which new ten-

dons is formed is essentially similar in animals and in man ; that the perfection of the reparative process is in direct proportion to the absence of extravasated blood and inflammatory exudation ; and that the sheath of the tendons, when consisting of bone-textured areolar tissue, as in the tendo-Achillis and other tendons surrounded by soft tissues, is of importance—1st. In preserving a connection between the divided extremities of the tendon. 2dly. In furnishing the matrix in which the nucleated blastematous, or proper reparative material, is effused. 3dly. In giving definition and form to the newly-developed tendinous tissue. That the new tendon always remains as a permanent tissue, and as an integral portion of the tendon, the divided extremities of which it has been formed to reunite. In the specimen exhibited, in which Mr. Adams had divided the tendo-Achillis three years previous to death, an inch and a quarter of new tendon was clearly traceable. The average length of new tendon formed in children to reunite the divided extremities of the tendo-Achillis, Mr. Adams considers to be from half an inch to an inch, and in adults from one to two inches. The author considers the facts adduced in this paper were amply sufficient to disprove the linear-cicatrix theory—the theory at present in vogue, and supported by all his colleagues—which assumed that the newly-formed tendinous structure has a disposition to undergo a process of gradual contraction, such as we see taking place in the cicatrices of the skin after burns, to which it has been compared, and that ultimately it becomes absorbed, the muscular structure at the same time becoming elongated by the force of the contraction of the cicatrix, so as to allow of the reapproximation of the ends of the divided tendons, and the formation of a linear-cicatrix. From the present observations it appeared that in the cure of deformities, muscles are elongated by the increased length of their tendons, obtained by means of subcutaneous division, and the development of new tendon formed for the purpose of reuniting the divided extremities of the old tendon. The mechanical and physiological effects of this increased length of the tendons were described ; and lastly the author stated that when recontraction of the foot takes place, and the deformity returns at a distant period after tenotomy, this does not depend upon absorption of the new material, or new tendinous tissue formed previously to unite the divided extremities of the old tendon, but upon structural alterations taking place in the muscular tissue. In three cases of relapsed deformity of the foot examined by the author, the new tendinous tissue formed after the previous operations remained, and could be easily distinguished from the old tendon. These facts were regarded as additional evidence against the linear-cicatrix theory.

[*Med. Times and Gazette.*



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*Incision in Anthrax.*

MAURICE H. COLLINS, Surgeon to Meath Hospital, says (*Dublin Quarterly Journ. Med. Sciences*, August, 1859) that "the incision into anthrax, whether made early or delayed till sloughing has done part of the surgeon's work, must be deep rather than extensive. Usually it is said anthrax is a flat swelling. The fact of its flatness, or rather of its extent, hides the real amount of elevation, which is, in most cases, considerable. Hence incisions into anthrax seldom go down *through* the inflamed skin and areolar tissue. But even if they did go down to the fascia, they would fail in effect unless they also went through it. The fascia is highly inflamed in anthrax; in fact the essential difference of anthrax from furuncle consists in the inflammation being deeper and implicating the fascia. When fascia is inflamed, much plastic exudation takes place, both in its substance and under it; and the tendency of anthrax to spread indefinitely is to be thus accounted for. The pent-up plasma, quickly producing pus and slough, can get no vent until there is an adequate opening in the fascia, and this opening should be made by the surgeon as early as possible, if he would avoid the unpleasantness of useless and repeated cutting, and the extensive sloughing which will occur if he neglect to make it. Plastic exudations find great facility in travelling under the fascia, dissecting and destroying its vascular connections, and ultimately causing much of it to perish. This is well known, as a general principle of surgery, and it is strange to find it overlooked as the cause of the spread of anthrax. We readily acknowledge the mischief it does in periostitis, in diffused inflammations of erysipelatos character or connected with paronychia, and in many other analogous cases; but books of surgery are, for the most part, silent about it in the case of anthrax. And yet every one must have observed phenomena which can only be explained by it. The extent and mode of extension of the swelling, the real depth to which the surgeon must cut if he is to do good rather than harm, and the fact of large flakes of fascia ultimately coming away as dead core (in addition to areolar tissue), leaving the underlying muscles bare, must have been often observed, and must often, doubtless, have had their influence on the practical observer; but the junior surgeon and the pupil have not been shown their practical bearing. The rule I have given above, to cut deep rather than wide, is founded on the observation of these facts, and will be found satisfactory, saving the surgeon the opprobrium of cutting twice or oftener without benefit to his patient. It is very easy to know when we are deep enough; by taking hold of the flaps made by our crucial incision, we feel if they are quite loose. Our incision is not deep enough un-

less we can lift up the point of each flap with ease from the parts underneath. This cannot be done unless our knife has gone through the fascia, and made a crucial incision in it almost as extensive as in the skin. The wounds we have made should be almost as deep at their extremities as in the centre, where they intersect. If we have made our incisions early, before actual sloughing has commenced, as we sometimes, though rarely, have an opportunity of doing, the flaps will curl up if the wound is deep enough, and will leave a widely gaping wound; but if we do not see the anthrax until more or less of the skin is undermined and dead, the gaping of the wound will not be so marked, and the best test is the one I have given above, of lifting the flaps with a forceps, and proving that they are loose. If this rule is followed, we shall have few cases in which we must come and cut again.—[*Am. Jour. of Med. Sciences.*]

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## EDITORIAL AND MISCELLANEOUS.

A CONVENIENT FRACTURE APPARATUS.—About a year ago, we purchased the Fracture apparatus gotten up and improved by Dr. Welsh. We have since applied the various portions of it, to nearly every variety of fracture, and are much pleased with the comfort it affords to the patient, as well as with its great convenience of application and security in maintaining proper coaptation. In a case of extensive gun shot wound of the leg, with fracture of the Tibia, we found the fracture-box a most valuable means of keeping the injured limb quiet, while it admitted of the dressings being applied to wounded soft parts, with the utmost convenience. The splints for fracture of the fore-arm are light, and seem adapted to every variety of fracture in this region. But we are particularly pleased with that portion of the apparatus intended for fractures of the humerus—high up. This splint consists of a trough of thin material, which is fitted upon the outer side of the arm, while a cap, attached by a hinge-joint, passes over the Deltoid and rests upon the shoulder. This appears to us to be one of the most efficient and convenient arrangements for the treatment of a variety of injuries in the neighborhood of the shoulder joint, which we have ever seen. Its most striking advantage being that all the pressure of the splint is made on the *outer side* of the arm, while the nerves and bloodvessels on the inner side, which every surgeon knows become very intolerant of pressure in fractures near the axilla, are almost entirely relieved from compression.

We have particularized the above pieces, as having given us special

satisfaction, but we regard the entire apparatus, with but few exceptions, as one of the most complete and useful modifications of the various appliances for fracture, which has been presented to the Profession for many years.

We would advise those of our readers who may wish to supply themselves with the above apparatus, to insist on having from the agent, an *application* of the various pieces, especially the mere complicated ones, before he leaves, otherwise, they may be subjected to much annoyance and the patient to much pain, while they are *learning how to apply* the apparatus.

This request will doubtless be readily acceded to, by the highly intelligent and urbane gentleman, who is now presenting this apparatus to the notice of the Profession in this section of the country.

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NECROLOGY.—We take pleasure in complying with the request to present the following pleasing Tribute of Respect. We add no word of comment. Those who knew him better than we, have better recorded his many virtues.

#### TRIBUTE OF RESPECT.

At a meeting of the physicians of Montgomery, Ala., called to express a sense of their loss in the death of Dr. A. B. McWhorter, Dr. Baldwin was called to the Chair, and Dr. Seelye was appointed Secretary.

On motion of Dr. Norton, a committee of three was appointed to prepare suitable resolutions for the consideration of the meeting. Drs. S. E. Norton, R. S. Wilson and W. P. Taylor were appointed said committee, who shortly reported the following preamble and resolutions:

An eminently worthy and useful man has been called from our midst. Through a long life he has passed with a character unstained; life's labors were performed with manly integrity, and its enjoyments were received with thankfulness.

He came up to the measure of his duty as a physician. Many remember him for his skill, devotion, and humanity. Science and religion found in him a willing worker. Ready with his skill and his means, he has worked well. An all-wise Being has called him from earth to a higher, nobler life. Let the memory of such a man live with us. Let his devotion to our profession, his high-toned morals, and his active benevolence, stimulate us to the discharge of our duties; that like him we may come well to the end—loved, honored, remembered.

It is fit that we should express ourselves on this sad occasion. Therefore,

*Resolved*, That the decease of Dr. McWhorter, calls pre-eminently upon us, as a profession, for an expression of sentiment—so long among us—active, useful, beloved—we realize that a good man has indeed fallen.



*Resolved*, That not only has the profession sustained a loss, but the community which he has so long adorned, has cause for profound sorrow in this lamented death.

*Resolved*, That we unaffectedly sympathise with the family of the venerable deceased, and do hereby, as a body, tender our profound condolence.

*Resolved*, That we attend the funeral, this afternoon, and that we wear the usual badge of mourning for thirty days.

On motion it was ordered that the city papers be requested to publish these proceedings, that the family be presented with a copy, and that they be also published in the New Orleans and Augusta Medical Journals.

S. D. SEELYE, Secretary.

September 20, 1859.

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*Woman's Home Book of Health.* By John Stainback Wilson, M.D., of Columbus, Ga.

We are gratified to learn that the above work, which we noticed in these pages some months since, will soon be issued from the house of Messrs. J. B. Lippincott & Co. The readers of this Journal are too well aware of the ability of the author of the above forthcoming work, to require from us, his recommendation, as he has long been a frequent and valued contributor to our pages. On the appearance of the work, it will afford us pleasure to call attention again and more definitely to its merits.

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*Inductive Electricity, or Faradization, in Lead Colic.*—Of late, much attention has been paid in France to the employment of electricity by induction, or Faradization, as it is called, in the treatment of certain diseases. Briquet, of La Charité, has employed this with success in the treatment of the paroxysms of colica pictonum. The application is not devoid of pain, and the latter is, at times, so great as to demand the anæsthetic aid of chloroform during the operation. A wet sponge is attached to one wire of the apparatus and placed at the upper portion of the recti muscles, and a metallic brush being applied to the other, it is passed over the part of the abdomen most painful, and this is continued until redness is produced, when the pain will disappear. The operation may require repetition in some cases; but if twenty-four hours pass without the re-appearance of any pains, no further application of the process will be required. The after-treatment consists in a sulphur bath every other day; the internal administration, daily, of about four pints of lemonade, containing from thirty to forty drops sulphuric acid.

"A gum portion is also prescribed, containing from thirty to forty grains of alum; and each night, or every alternate night, as the case may be, a pill is ordered, containing one grain of the watery extract of opium. \* \* In almost all cases, Briquet has remarked that the bowels begin to act spontaneously from one to three days after the Faradization has accomplished the removal of the abdominal pains." This treatment is interesting from its simplicity, as compared with the complex routine heretofore used at this hospital, in the treatment of

colica pictorum. The latter extended over six days, and was probably one of the last relics of ancient medication to be found in modern hospitals. Our readers will find it given in detail in Nevins's translation of Trousseau and Reveil's *Prescriber's Hand-Book*.—[*Ibid.*]

*Nocturnal Incontinence of Urine.*—A writer in the *Bulletin de Therapeutique*, recommends the employment of mastic in these cases. It is given in pills made with syrup. It is necessary that thirty-two grammes should be administered in four days, if the child is over ten years of age; when under that age, the amount mentioned should be given in six or eight days. If a cure does not result from the first trial, a second trial with a like quantity should be made. But if the incontinence persists after the second trial, it is useless to continue the medication. The failures are, however, exceptional, as two-thirds of the cases have been successful, even in patients from 18 to 24 years, who had been affected with this disgusting infirmity from infancy.—[*Ibid.*]

*Value of Scientific Services.*—It is a most common complaint among scientific men, that when called upon to do any service for the public, their labors are generally miserably paid, it being thought sufficient to have the honor of doing the work. It has become necessary for them either to take the ground that they will not perform these services, which many do, or for some one to take the initiative in making the value of scientific labor appreciated. We are glad to see that the example has now been set by Prof. Doremus, of this city. For his chemical analysis in the Stephens case, in which two entire bodies were analyzed, and which was by far the most complete investigation ever attempted, he has charged and received \$3,000 for his services, and \$800 for new apparatus. This analysis will be of incalculable value to justice, and will be referred to as long as law and science exist. We are gratified to see that such a service has been appreciated; though for the amount of actual labor expended, which we have not space to detail, the pay is certainly not too much, if, indeed, it be sufficient. We hope at some future time to be able to present to our readers an account of the medical points in this interesting case.—[*N. Y. Monthly Review.*]

*Excitement from Fast Traveling.*—Lord Shaftesbury says: "I have ascertained that many persons who have been in the habit of traveling by railway have been obliged to give it up, in consequence of the effect upon the nervous system."

In our own experience of railway traveling, the dozy, listless appearance of the travelers would incline to the opinion, that it is rather productive of tranquility than nervous excitement. Of course this opinion does not include railroad collisions, etc.; when the individual, if not entirely annihilated, might probably be somewhat excited. But for that excitement, the fast traveling could not be blamed, as it would then be at an end. The passenger would then be like the Irishman, who said that he was not hurt by the fall, but "stopping so quick."—[*Med. and Surgical Reporter.*]

*Publications of the Sydenham Society.*—The following works are announced for the year 1860 :

“Clinical Memoirs on Abdominal Tumors and Intumescence,” by Dr. Bright; edited by Dr. Barlow. A Year-Book for 1859, on Anatomy and Physiology, Medicine, Surgery, Diseases of Women and Children, Forensic Medicine and Toxicology. French’s “Clinical Account of Diseases of the Liver.” Hebra’s Atlas of Illustrations of Skin Diseases; and Professor Simpson offers to edit a reprint of Smellie’s Midwifery.—[*Ibid.*]

*The Workhouse*, as pictured by Dickens, is, at even this time, no myth.

The *Medical Times and Gazette* says, that a visitor at the Cork Workhouse was shocked at the appearance of the children, of whom a large number are maintained in the building. Upon investigation he discovered that an enormous proportion were afflicted with scrofulous diseases; many of them dying prematurely; others becoming blind; and those who survived, dragging on a miserable and feeble existence to which death itself would almost appear preferable.

Dr. Callanan says, when examined on oath before the Poor Law Inspector: “On the day I arrived, it was the hour the boys were at dinner. I examined the food, and after their meal I inspected it. I asked for a bowl of the soup; I examined and tasted it, and I found it almost perfect water. I asked of what it consisted, and was told of rice, oatmeal, and vegetables; some green leaves of leek were in the soup. I went along the form while they were at the meal, and I found some of the porringers untouched. I asked them why they did not take their soup, and they said they could not drink it. I saw the bread, and thought it very inferior; it smelled sour, and was heavy and gluey.”

On this evidence, says the above journal, Mr. Arnott makes the bitter remark, that in Dickens’ “*Oliver Twist*,” the workhouse-boy astonishes his superiors by *asking for more*,—but in the case of the Cork Workhouse-boys, they prefer starvation itself to eating the food placed before them.—[*Ibid.*]

*Gallows Confession.*—We have before noticed the heresies which afflict all varieties of quacks, when they attempt to band together for the purpose of attracting public attention and mutual admiration. This has been evident in such meetings as the late Homœopathic Convention, in which it was admitted that a portion of the order were inclined to merge into “*Electicism*.” And now a noted homœopath, Dr. Hering of this city, whom we have looked on as the very personification of homœopathy, and who, we supposed, relied on it with the devotion of a fanatic, yields with an honesty not to have been expected, the following acknowledgment, which we copy from an article by him in the *American Homœopathic Review*.

He says: “We all know that the numbers in our homœopathic ranks are not lessening; but it is the general observation that the number is year after year increasing, who, instead of deriving benefit from homœopathy, are made incurable by so-called homœopathic practitioners.”—[*Ibid.*]