disease was of neurotic origin or otherwise, and the need for increased caution in suggesting such origin was strongly enforced. Diseases of the skin directly due to nerves should, it was contended, always display certain definite features as regards the distribution of their patches. These patches ought always to be racemose or corymbiform. The coalescence of such spots might produce a long oval patch, but such patch ought always to be more or less irregular at its edge, and would almost always be attended by outliers. No eruption produced directly by nerves could possibly assume a rounded form. The terms "areate" and "circumscribed" could never be applicable to disorders of nutrition located by the filaments of nerves. Under this rule it was held that such diseases as alopecia areata and leucoderma were certainly not of neurotic origin. A second rule was that disorders originated by nerves never spread serpigiously—that is, they did not creep at their edges by infection of adjacent parts, as lupus, psoriasis, and leucoderma, with many others, so conspicuously did. On account of the slowness of its evolution, and the difficulty in making sure in its early stages of its precise limits, mistakes had occurred in respect to scleroderma on this point, and in not a few cases it had been asserted that it spread at its edges. That in a general way such tendency to spread was conspicuously absent there could not be the slightest doubt.

Hebra's observation as to the occasional corymbiform arrangement of the pustules in variola was next adverted to, and with it were mentioned the disease described by Dr. Duhring under the name of "dermatitis herpetiformis" and certain forms of lichen ruber, and of some syphilides which might also assume the corymbiform arrangement. It was admitted to be possible that in many diseases not primarily affecting the nervous system a tendency to peripheral neuritis might exist, which would explain the peculiar arrangement of the foci of inflammation in the skin.

The question was next asked Whether any disease produced by the nerves could be arranged in lines or streaks? and in reference to this the very remarkable conditions presented in congenital ichthyosis in streaks were discussed. Although these streaks had been assumed to be neurotic, there were, it was remarked, great difficulties in accepting such a theory. The streaks often did not follow the course of any known nerves, nor was it easily comprehensible that the transit of a nerve-trunk deeply placed under the skin should evoke any phenomena along its course. It was admitted that the explanation of these congenital streaked eruptions was extremely difficult. Their arrangement on the walls of the chest and abdomen were often so extremely like that of zoster that Mr. Hutchinson said he had himself many years ago ventured the crude suggestion that they might be due to intra-uterine herpes.

At the conclusion of the address a number of drawings were exhibited illustrating various points which had been adverted to. With them was shown a very remarkable, and it was believed unique, skull, in which all the bones on one side of the head showed exostoses, with other evidences of overgrowth, of which on the opposite side there was not the slightest trace. In this specimen the right half of the jaw, as well as the bones of the skull, was affected, the state of hyperostosis ending precisely at the symphysis. Of this remarkable unilateral asymmetry it was suggested that two explanations were alone possible—either that the disturbances of nutrition were located by the distribution of the fifth nerve, or that they were due to some influence exerted probably during foetal development. If the latter, nothing was known as to its precise nature, and the former appeared the most probable. A contrast was suggested between the condition of unilateral overgrowth exhibited in this skull and the cases of unilateral undergrowth in the so-called hemiatrophy of the face.

OPHTHALMIA IN SCHOOLS.—At a recent meeting of the Managers of the Central London School District a communication was read from the Local Government Board concurring in the Board's decision to make temporary provision for children suffering from ophthalmia, and expressing their opinion that the structural changes indicated by Dr. Bridges and Mr. Smith were absolutely needed before the school could be brought into a satisfactory condition. The Board referred to a letter of April 13th, 1875, in which suggestions were made for the proper treatment of the children. In the course of the discussion that followed, Mr. Searle, the vice-chairman, said that from about the year 1875 up to 1885 the number of children suffering from the disease was reduced from 345 to 84, but since then the cases had risen very considerably. The letter of the Local Government Board was referred to the special ophthalmic committee.

CHOLERA AND CHEMISTRY; OR THE INFLUENCE OF CHEMISTRY ON MEDICAL SCIENCE, AS ILLUSTRATED BY CHOLERA.¹

By WILLIAM SEDGWICK, M.R.C.S.ENG.,
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THE influence of chemistry on medical science has seldom, if ever, failed to lead to satisfactory results when the attempted progress has been strictly limited to the high road of chemical analysis, and when the by-ways of chemical assumption have been resolutely shunned; for, if the mind has been previously unbiassed by chemical assumptions, there can be no doubt that the evidence supplied by chemical analysis may be safely accepted and relied on in the scientific investigation of disease. When, for example, the medical profession had fully recognised the great fact that prolonged suppression of urine could occur during choleraic collapse without being followed by any symptoms of uræmic poisoning, it was for many years afterwards assumed that urea continued to be formed, and that it was vicariously disposed of in the flux from the stomach and bowels. When, later on, and in accordance with the unimpeachable evidence of chemical analysis, it had been admitted that the absence of uræmic poisoning in cholera is due to the arrested formation of urea, the tendency to wander off into the by-ways of chemical assumption appears to have been too strong to be resisted; for, although it had been clearly demonstrated that this prolonged suppression of urine without symptoms of uræmic poisoning is met with in a very large number, and in a very large variety, of cases in which there is collapse closely analogous to that of cholera, and in which there could be no specific blood-poison capable, as was alleged in the case of cholera, of shutting off the blood from access to air in the lungs, yet the same tendency to substitute chemical assumption for facts which not only could be, but which had been, demonstrated by chemical analysis was still too strong to be withstood. The effect of the tendency in this case was to lead to what may be referred to as one of the most remarkable series of assumptions in the history of medical science; for, in order to explain an alleged deficiency in the supply of oxygen to the blood, it was assumed that there was a blood-poison, which was supposed to produce morbid contraction of the pulmonary arterioles, and that, in consequence of such contraction, the aëration of the blood was checked. It was not until nearly two years after the production of my paper on the "Chemical Pathology of Respiration in Cholera," in which M. Doyère's analyses were for the first time made fully known to the medical profession in this country, that the author of this theory referred to the evidence of anyone who had chemically analysed the expired air, and who had consequently been able to demonstrate that the proportion of oxygen absorbed in typical cases of choleraic collapse, instead of being relatively less, is, on the contrary, always in excess of the carbonic acid eliminated. It has since been admitted, shortly before the close of the year 1887, that, as regards the pulmonary interchange of gases during collapse, there "is a more or less considerable diminution of the amount of carbonic acid exhaled, in proportion to the volume of oxygen absorbed by the blood." This admission, which has been long delayed, is irreconcilable with the theory of defective oxygenation of the blood resulting from the specific effect of a blood-poison; and, although an attempt has been made to lessen the force of the evidence adduced by fresh chemical assumptions in which it has been suggested that "these exact observations (of M. Doyère) indicate that abnormal chemical changes take place in the blood and tissues of cholera patients," and that "a portion of the oxygen enters into combination with some abnormal products, forming compounds which, as it were, take the place of carbonic acid," yet they are as much without any analytical evidence in their favour as was this author's previous assumption, which theoretically connected the arrested formation of the chief constituents of bile and urine with a relative deficiency of oxygen in the blood.

¹ Abstract of the Presidential Address delivered at the fifty-eight Annual Meeting of the Harveian Society of London, January 17th, 1889.

It is reassuring to find that the tendency to indulge in such assumptions is less common in physiology than it is in medicine; and, in consequence, it is now a fully-recognised as well as a firmly-established fact that there is always a relative excess of oxygen absorbed in cases, for example, of hybernation, of abstinence from food, and of other physiological conditions of the system, in which, as in cholera, the production of carbonic acid for elimination has been reduced.

As regards the influence of chemistry on medical science, it is unfortunate that there should have been, in the case of cholera, such a frequently recurring tendency to substitute chemical assumption for chemical analysis. For it has been chiefly, if not exclusively, owing to this tendency that, whilst a large number of our writers on cholera have always been ready to assume, and also more or less persistent in assuming, that there is defective oxygenation of the blood during collapse, yet they have made no efforts to verify this assumption by chemical analysis; nor have they even attempted to cite the observations of those who have published valuable and conclusive evidence on this subject abroad. It should, moreover, be remarked that in the directions which were issued from the medical department of the Privy Council for the chemical investigation of cholera, during the last epidemic of the disease in this country, in 1866, no reference has been made to the chemistry of respiration, although at that date it was very generally, if not officially, assumed that cholera is more or less essentially connected with defective oxygenation of the blood.

Some of the results of previous analyses, and especially of those connected with the arrested formation of urea, were fully confirmed by the distinguished analyst who was appointed on that occasion to carry out the chemical instructions of the medical officer of the Privy Council. But it is only too evident, both from the instructions issued and from the work done, that the need of any chemical investigation of the pulmonary interchange of gases was not recognised; and, consequently, no information has been or can be gleaned from this report on "Cholera Chemically Investigated," respecting a function of such primary importance as that of respiration during collapse. It is much to be regretted that a great opportunity should have been so completely lost; for a Government report on cholera thus chemically investigated, without any reference to the amount of oxygen supplied to the blood during the interchange of gases in the lungs, is wanting in that which would have given it permanent value, for it would undoubtedly have helped to check the increasing tendency to chemical assumption, which has been a fertile source of error in this disease.

It was in the year 1856 that I thought it advisable, in my first publication on cholera, to refer to the nature and extent of those changes, which I stated might be grouped together under the title of the "chemistry of respiration in cholera." "The supposition," I proceeded to add, "that the dark colour of cholera blood is owing to a deficiency in the supply of oxygen to the lungs has always been a favourite way of accounting, not only for the dark colour of the blood, but for most of the other phenomena in cholera.....There is, however, no satisfactory evidence to show either a deficiency of oxygen or an excess of carbonic acid in the blood of cholera patients during collapse; and, as there is no mechanical obstacle to the free entrance of air by the lungs, and as the inhalation of even pure oxygen is attended with no improvement, it may be inferred that, so far from a deficiency of the one or an excess of the other occurring in cholera, their relative proportion is always maintained during collapse, except in those cases in which the condition (collapse) of the lungs after death may appear to justify the inference that the amount of carbonic acid in the blood is relatively diminished, and oxygen, therefore, present in slight excess."

The inference, thus cautiously drawn from clinical and pathological observation of the disease, as far back as thirty-three years ago, has been fully confirmed by chemical analysis, which has demonstrated that when the amount of carbonic acid produced for elimination is reduced, the oxygen is therefore increased.

During the discussion on cholera at the Royal Medical and Chirurgical Society, in March, 1885, I ventured to express my regret that so little should even then be known in this country about the chemistry of respiration in cholera; and I urgently directed attention to M. Doyère's important work on the chemical analysis of the expired air in this disease. Nothing more, however, was said or done with reference to this subject until I

had brought forward my paper on the "Chemical Pathology of Respiration in Cholera," which was read early in the following year, and which was subsequently published in the *Transactions* of that Society. It is, perhaps, almost needless for me to add that this paper failed to elicit any discussion when it was read on April 13th, 1886, for it was necessarily bound to fail where nothing was apparently known about the chemical analysis of air expired during collapse. But all that was needed for the paper itself and all that I expected on its behalf was that it should later on appear under favourable auspices in print; and, having so appeared, it has served the purpose for which it was written by showing how completely without any trustworthy evidence in its favour is the long-cherished and the influentially-supported theory which is based on defective oxygenation of the blood.

There is fortunately no need to dwell on the very grave error in diagnosis which was committed when, even after the last outbreak of cholera in this country in 1866, it was alleged that cyanosis is more or less peculiar to choleraic collapse, and that pallor of the surface is characteristic of other forms of gastrointestinal collapse; for this error has been long since and fully refuted in one of that small series of papers "On Some Physiological Errors connected with Cholera," which was published in the *Lancet* on October 28th, 1871. But, with reference to the pseudo-chemical assumption which has theoretically connected cyanosis with defective oxygenation of the blood, it must, I think, be admitted, both as regards these cases as well as other and allied conditions of the system in which temporary cyanosis is more or less strongly marked, some scientific investigation of the changes affecting the aëration of the blood is needed, unless we are content to look upon some blueness of the skin in the same way that an unreflecting observer of Nature looks upon the colour of the wild flowers in bloom around him, and of whom it has been said

A primrose by the river's brim
A yellow primrose is to him,
And it is nothing more.

The general truth of the Linnæan adage, "Natura non facit saltus," has been well illustrated by much that is both directly and indirectly connected with the great function of respiration in cholera. When, for example, the blood, as is the case during choleraic collapse, is surcharged in the lungs with oxygen, it is found that the respiration has undergone a very important change; for in connection with such increased arterialisation of the blood, it has been observed that the inspiration is much prolonged, instead of being, as it normally should be, shorter than expiration. It is in consequence of the inspiration having been thus exceptionally prolonged that there is often the oppressive sensation of a weight on the chest, which is exaggerated by pressure on the sternum, and that the insertions of the diaphragm become in some cases even painful if the efforts to promote the access of air into the lungs have been greatly increased. When, moreover, the inspiration, which in these cases is often twice as long as the expiration, is at its height, it has been usually observed that the respiration stops, so that a distinctly apnoëic pause occurs before the short and quickened expiration is made. There is, however, nothing in such changes which can in any respect be looked upon as distinctive or characteristic either of cholera or even disease, since the same results are physiologically produced whenever the inspirations are either voluntarily or artificially prolonged. If, for example, a person takes a series of rapid and deep inspirations, the blood becomes surcharged with oxygen, and long apnoëic pauses ensue; and it has been, from a comparatively early period, more or less well known that the same effect can be artificially produced in animals by vigorously and rapidly forcing air into their lungs, when, in like manner, over-arterialisation of the blood occurs. It must, therefore, be evident to all who may have been able to bring physiological knowledge to aid them in their clinical investigation of this disease, that the remarkable prolongation of the inspiration during collapse, which is followed by an apnoëic pause before the expiration is made, shows that a relative excess of oxygen has been exchanged for a reduced amount of carbonic acid in the lungs. For it would have been physiologically impossible for any corresponding prolongation of the inspiration to have occurred if it could have been demonstrated by chemical analysis of the expired air that a relative deficiency, instead of a relative excess, of oxygen had been supplied to the pulmonary blood. In further illustration of this part of the subject, it is necessary to add that although, as I have elsewhere had occasion

to state, the lungs in cholera are comparatively free from disease, yet it has occasionally, and sometimes not infrequently, been observed that emphysema has resulted from this remarkable prolongation of the inspiration during collapse. Majendie, in directing special attention to the frequency of its occurrence during the cholera epidemic of 1832, remarked that it was rare not to meet with it in those cases in which death had occurred at the end of thirty to thirty-six hours. Jackson has also recorded the occurrence of emphysema in Paris at the same date, in no less than eight cases out of twenty-five, in persons dying without reaction. Dr. Morehead, of Bombay, observed it in seven out of twenty cases; and confirmatory evidence has been published by Dr. Parkes, Professor Virchow, and very many other observers. The emphysema which is thus liable to occur during choleraic collapse, is the physical result of an exceptional prolongation of the inspiration, which leads to over-distension and rupture of the air-vesicles. The tendency to rupture in such cases as these, in which there is prolonged and recurring over-distension with air is greatly favoured by the reduction in the amount of blood which is sent to the lungs for aëration, and which is so much reduced during collapse that Professor Griesinger has been able to demonstrate that the area of cardiac dulness is less than the normal extent, and that consequently the blood in the pulmonary artery and its branches is comparatively very small in amount. There should, therefore, be very little, if any, difficulty in recognising the fact when the air-vesicles, which expand with each normal inspiration, have been exceptionally distended in consequence of the inspiration having been exceptionally prolonged; and that when, at the same time, they have been to a great extent deprived of that soft and much needed support which only blood in the pulmonary capillaries could give, there must be a great and a long-recurring tendency in their yielding and bulged parietes to burst. It is now fairly well acknowledged that, as regards the natural history of disease, it behoves us from time to time not simply to take stock of the knowledge we possess, but also to test its value; for unless some efforts are occasionally made to separate, as it were, the corn from the chaff, many false facts in connection with false theories of disease would continue to occupy the place of much that is important and capable of being well proved in medical science.

It has been chiefly through the influence of chemistry on the scientific investigation of cholera that many false facts as well as false theories have been swept away, and, in consequence of the clearance which has been thus effected, it has become possible for us to recognise the true nature of the disease. But we must not too hastily allow ourselves to suppose that because something more is now known about cholera than was formerly the case, it has become more easy to discover its true cause, or to undertake, with any increased confidence, its treatment. There is a very noticeable agreement in the results obtained from its clinical, its chemical, and its pathological investigation, which unmistakably indicates that cholera is due to a central arrest of nutrition, and which has been fully confirmed by biological research.

Notwithstanding, however, the great advance which has been made in our knowledge of the nature of the disease, the problem of its cure, like that of its cause, remains unsolved. For although there are fortunately many cases in which this central arrest of nutrition appears to be only a temporary condition, from which there is a more or less strongly marked tendency to recover, yet there are other and very numerous cases, which in some epidemics of the disease represent a small but still a decided majority, in which there is practically very little or almost nothing that we can do but to stand by and watch whilst the great centre of organic life totters, as it were, to its fall.

Although we are thus compelled to take what it must be admitted is a very gloomy view of the nature of the disease, yet there is much that may notwithstanding be regarded as hopeful in some of the results of its scientific investigation. For we have been enabled, and especially through the influence of chemistry, to recognise more fully than would otherwise have been possible, the greatly increased efforts which are made by the curative power of Nature to maintain life, when, to the superficial observer, the tendency to death appears to be most marked. In one of the numerous cases cited in my now well-known paper on the "Chemical Pathology of Respiration in Cholera," it was analytically proved, during some hours of well-marked collapse, that whilst the average quantity of carbonic acid eliminated from the lungs was only 37 per cent., the average quantity of oxygen ab-

sorbed was 54 per cent. of the normal amount, and that when the final analysis of the expired air was made one hour and ten minutes before death, and when the cyanosis was strongly marked, the carbonic acid eliminated was only $5\frac{1}{2}$ per cent., whilst the oxygen absorbed was 29 per cent. of the normal amount, or, in other words, the oxygen absorbed was equal to nearly six times the carbonic acid eliminated. No evidence could be more conclusive than this, for it proved that the comparatively large amount of oxygen received by the pulmonary blood in choleraic collapse is due to the exercise of this curative power in Nature, and that during the struggle to avert death, oxygen necessarily becomes more and more entitled to be regarded as the breath of life, and that it is admitted as such in relatively increased quantities into the blood through the lungs. But whilst a comparatively large excess of oxygen can be thus abstracted from atmospheric air for the purpose of warding off impending death, it has been experimentally demonstrated that the addition of artificially prepared oxygen to the air for inhalation has very little, if any, influence on the amount absorbed. For it has been fully proved that the relative quantity of this gas which is exchanged for carbonic acid during health is not greater when an excess of such oxygen has been added to the air; and, consequently, all attempts to overcome the collapse of cholera by inhalations of chemically prepared oxygen have signally failed in every outbreak of the disease. This failure is undoubtedly due to the fact that, apart from any necessity there may be for distinguishing between the physiological and the chemical affinity of living structures for oxygen, there is an essential distinction made by the lungs, both in health and in disease, between natural and artificially prepared oxygen; so that whilst the latter is almost, if not altogether, rejected, the former is more or less readily absorbed. The exercise of such power of selection is the more remarkable as it is evident that there must be greatly increasing efforts made during advancing collapse to secure a relative excess of natural oxygen from the impoverished air within the air-vesicles; for such air progressively retains comparatively more carbonic acid and less oxygen than that of the surrounding atmosphere.

In conclusion, it may be remarked that although the influence of chemistry on medical science, as illustrated by cholera, has been very great, it has not as yet been fully recognised. As time, however, goes on, it may be expected that there will be increasing reluctance to accept any theories of disease which are based on chemical assumptions, unless they are supported by the evidence of chemical analysis, since it has been clearly demonstrated that one analysis of the expired air is sufficient to disprove all theoretical connection between cholera and defective oxygenation of the blood.

A BRIEF STATEMENT OF THE CAUSATION OF SEVERAL OF THE SIMPLE AND RACHITIC DEFORMITIES THAT DEVELOP DURING YOUNG LIFE.¹

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IN this communication I wish to state as briefly as possible the conclusions at which I have arrived from a study of the causation of many of the deformities which arise in early life.² I will divide them into two groups, one comprising the simple deformities, as flat-foot, knock-knee, lateral curvature, and dorsal excurvation, and the other such rickety deformities as dorsal excurvation of infancy, or rickety spine, bow-legs, rickety knock-knee, and lateral curvature.

The causation and pathology of the first group are comprised under the following statement, namely, that each is first the fixation and subsequently the exaggeration of a normal physiological attitude of rest. While a vigorous child when in the erect or sedentary posture assumes attitudes of rest but rarely, and then for very short periods of time, preferring attitudes of activity, a feeble child habitually and for considerable periods of time re-

¹ Presented to the Section of Diseases of Children at the Annual Meeting of the British Medical Association held at Glasgow, August, 1888.

² *Guy's Hospital Reports*, 1886-87.