tomy of the kidney in this malady; but I may at the outset be permitted to remind the reader that, by careful observers, three different views, though perhaps really not so conflicting as at first might appear, are held as to the seat of the pathological renal change. Dr. G. Johnson believes this change to occur primarily in the gland-cells of the convoluted tubes; Drs. Dickinson and Grainger Stewart hold that the inter-tubular fibrous matrix is first the seat of disease; whilst the more modern theory—that of Sir W. Gull and Dr. Sutton—is that the outer coat of the renal vessels undergoes a hyaline fibroid change. These observations have reference to the small red granular kidney, which is commonly regarded not as a local affection, but as the expression of a constitutional state.

The precise nature of this constitutional state is one of difficulty, but the commonly received opinion is that gout or other allied blood-poison is the etiological factor. Without venturing to express any dogmatic opinion as to that portion of kidney-tissue in which the initial alterations occur, we are able to come to more harmonious conclusion as to the nature of the morbid process. It is eminently degenerative in type, and, attacking the gland in the part of greatest functional activity, soon interferes with its physiological work.

I regard many of the cases of chronic Bright's disease that come before me as illustrations of malnutrition in connection with impaired and diminished nerve-power. It is not difficult—indeed, it would only be the record of every-day experience—to cite instances of the influence of insufficient or perverted nerve-force in the impairment of the integrity of tissue and interference with cell-life and development. The physician finds such instances not merely in the wasting, but in the retrograde metamorphosis of tissue seen in paralysed limbs, or in similar changes in a persistent sciatica; the surgeon, in like changes by reason of pressure in the course of nerve-trunks, or wasting of the deltoid in injury to the circumflex nerve; the practitioner of ophthalmic medicine, in a conjunctivitis or corneitis, the consequence of a disturbance of the first division of the fifth cranial nerve and branches of the ciliary ganglion. Are not these fair and incontrovertible proofs of the influence of sensory and vaso-motor nerves in the phenomena of nutrition? In support of my view of the occasional, not to say unusual, occurrence of chronic albuminuria in association with disturbed nerve-force, I will draw a picture from real life.

A man of middle age, of temperate habit, and with a good family history (free from those diseases usually credited with heredity), complains of languor, weariness, and a sense of fatigue on exertion altogether disproportionate to the effort. He is dyspeptic, flatulent, and with a tendency to constipation. The appetite is bad; he has occasional nausea; the tongue is fairly clean, but pale, and marked with the teeth. Sleep is disturbed, not from pain or other very apparent cause, but a general restlessness. He has sometimes to get out of bed to pass urine, but the total quantity does not exceed three pints in twenty-four hours. His facial expression is haggard and somewhat anxious. His memory is not so good as formerly, and mental efforts are a great labour. These symptoms may have been in existence a year or more before advice is sought. The careful physical examination of this man fails to discover any evidence of organic lesion until his urine is examined, when albumen is found, with a shedding of imperfect renal epithelium. The crystalline deposit, if any, will probably be the oxalate of lime. This condition may go on for years, and with but little variation; and I now have a case, answering to the above, that has been under observation, without material change, so long as eleven years; and I quote it as an instance of simple malnutrition associated with defective nerve-power.

For the most part, those who, in my experience, suffer from this form of disease, are men of middle life, of sanguine temperament, the subjects of depressing emotions, or upon whom unusual anxieties and responsibilities have heavily pressed. The resulting enfeeblement of health courts, if I may so speak, disease degenerative in character. That the more violent, though brief, disturbances of nerve-centres powerfully affect the physiological function of the kidney, I am able, by the kind courtesy of my friend Dr. Moxon, to illustrate in the record of the following case. A gentleman under Dr. Moxon's care three months, whose urine had been repeatedly examined for albumen without results, was, a few days ago, suddenly seized with a fit in the street, and brought to the doctor's house. Immediately after the fit, the patient voided urine, which, on examination, was found to be loaded with albumen. In twenty-four hours, all trace of this had disappeared. There can be no doubt of the relations subsisting between the nerve-storm and kidney. If, therefore, such marked results as these can occur under brief, though powerful, nerve-influence, does it seem unreasonable or improbable that slowly destructive changes should follow the more silent influence of continuous nervous depression? Why the nerves of the kidney in any particular instance should be prominently

affected, must be determined by surroundings, if, in the present state of our knowledge, it can be determined at all; but I have not yet learned why the branches of the fifth cranial nerve are more frequently painful than any other, unless it be urged that they are more exposed, which I do not, however, accept as sufficient reason; but if the theory of exposure be accepted, then, *à fortiori*, will a nerve-sympathy and kinship be established between skin and kidney. It is important to remember, in actual bedside work, that long-continued functional disorder is very liable to lay the foundation of actual organic lesion; so that when we have involved an organ essential to life, the early recognition of such disorder becomes a question of vital importance.

I think reason exists for a belief in the milder, though not less chronic, forms of Bright's disease, as being due to imperfect nutrition of renal cells; not merely from the fact of disease continuing to be limited to cell-life, but from the absence of those secondary changes, which most frequently, within a comparatively limited period of time, occur as the result of a blood-dyscrasia.

I hope, at some future time, to follow up this subject by the record of cases in detail; at the present moment, however, my object will be attained should I be able to engage professional attention in a new field of clinical inquiry.

THE CLIMATE RESULTS OF TORQUAY AND MADEIRA.

By C. THEODORE WILLIAMS, M.A., M.D., F.R.C.P.,
Physician to the Hospital for Consumption, Brompton.

SINCE the publication of my Lettsomian Lectures, several criticisms have appeared in the BRITISH MEDICAL JOURNAL on my conclusions relative to the climates of Torquay and of Madeira, the authors being for the most part medical men residing now or formerly in these places.

It is not unnatural that such old-established health-stations should have their staunch defenders among those who have not, like London physicians, had full opportunities of judging of the results of many and various climates, and of learning lessons from the failures and successes of each.

However, the criticisms were, on the whole, acceptable, as containing some additional matter; and this was especially the case in respect to Dr. Hounsell's letter. Before discussing each in detail, I must remark that the writers appear to me to cling too much to the traditional ideas as to the ideal climate for consumptives, in which protection from winds, mildness, and equability are held to be primary requisites, and dryness and bracing influences only secondary.

Now, it seemed to me that the time had arrived when climates could be examined, not according to their theoretical value, but by the actual results furnished by them in the treatment of disease; and, if this examination has led to the depreciation of some of the leading favourites, the climates are to blame, and not I. The real deduction to be drawn from my figures is, that the old ideas of fitting climates for consumptives must be modified to bring them into harmony with ascertained facts.

My statistics have been called too small a number on which to build and promulgate opinions; I wish they had been larger; but still, as they stand, they exceed any relative climate statistics yet offered to the profession, and, when we consider how many patients, recommended to English or foreign health-stations, either do not go where they are ordered, or, if they do, furnish no results to the physician, the 251 foreign climate patients and the 248 home cases, all of whom belonged to the upper and middle classes, and were carefully observed before and after such climatic treatment by my father and myself, most of them having been many years under treatment, are surely not such small numbers.

The opinions of the most distinguished climatologists, like Sir James Clark and others, were based on less exact data.

Dr. Spencer Thomson and Dr. Strangways Hounsell find fault with my results about Torquay; and, while both allege my number of cases to be too small, Dr. Hounsell dwells on the difficulty of selecting similar cases, and intimates that the worst ones were sent to Torquay. Now, Torquay has the least right to complain on these grounds, as no fewer than 100 patients were submitted to the influence of its climate, and, so far from their being the worst cases, a reference to my second Lettsomian Lecture will show that the best cases were sent to Torquay and Ventnor, and the worst ones to Hastings and Bournemouth; so that no loop-hole remains in this direction for evading the conclusion arrived at.

Dr. Hounsell meets my favourable deduction about the breezy climate of Hastings with a quotation from Mr. Haviland, "that chest-disease is in excess in proportion to the amount of exposure to high

wind, no matter what the exposure may be"; but he forgets that this refers solely to the causation of phthisis, and not to the influence of climate on persons already phthisical, two climatic subjects which differ widely.

The statistics of the Western Hospital for Consumption at Torquay have been cited by Dr. Hounsell, and I fully admit that they show fairly favourable results and a remarkable gain of weight. Dr. Hounsell has kindly forwarded these results, extending over thirteen years, and I have endeavoured to compare them with my own results; but a complete comparison is impossible, on account of the very scanty information registered about the cases. No record of the form of the disease or of its duration appears, and in only 350 out of 670 patients is the stage on admission stated. Hæmoptysis appears to have occurred in 19 per cent., and hereditary taint (the degree not being specified) in 22 per cent.

On comparing these results with mine, Dr. Hounsell has fallen into a grave error; for he says "the percentage of 'much improved' is nearly double that mentioned in Dr. Williams's tables, for, while he gives 35 per cent. 'improved' and 41 per cent. worse, the 1010 cases of the Western Hospital give 68 per cent. 'improved', and only 20 per cent. 'worse'". Here, Dr. Hounsell compares the statistics of the general state of the hospital cases (the local not being mentioned) with my statistics of the local conditions of the lungs: two entirely distinct things. But, if he refer to the statistics of the general condition of my patients, he will find the results for Torquay as follows:—Sixty per cent. improved, 10 per cent. stationary, and 30 per cent. worse: figures not greatly differing from the statistics of his hospital, though they are, I am happy to say, based on a more complete registry of cases than appears to be at present carried out at the Western Hospital. Therefore, I claim Dr. Hounsell as an ally, and not an opponent; and I cannot understand why he and others should so strongly object to my results, which, after all, do not show so very unfavourably for Torquay, though less favourably than for the three other health-stations.

Letters have also appeared from Dr. Dyster of Tenby, Commander Bell, and Dr. Lund (late of Madeira), objecting to my describing Madeira as a moist climate, and denying the correctness of the description. A reference to the third Lettsomian Lecture will show that the description is not mine, for I have never visited the island, but is from the pen of no less eminent an authority than Dr. Walshe (*Diseases of the Lungs*, 4th edition, p. 606), whose very expressions are used; and I must leave him to settle with Dr. Lund, merely remarking that the latter admits that the phenomena described are not unusual at the seaside, but not common in or around Funchal. I have read Mr. Bloxam's pamphlet; but the reason I classed Madeira among the "warm and moist Atlantic climates was in contradistinction to the group of 'dry climates', which includes Egypt, Syria, the Cape, and Natal: a distinction the truth of which, I venture to think, few climatologists would question.

Dr. Lund blames me for not quoting all Dr. Renton's tables, instead of the first one of confirmed phthisis; but of what use to the cause of statistics in consumption, I would ask, is the subjoined Table II (with tubercular lungs)?

Died here	...	...	...	...	...	30
Left the island	...	...	...	...	...	22
Still here	...	...	...	...	...	4
						56

We can only congratulate the twenty-two who "left the island" from narrowly escaping the fate of the thirty who "died here"; while we regard with gloomy compassion the future of the four unfortunates "still here", doubtful how long an interval will elapse before they join their thirty friends above mentioned. Again, can we seriously accept Table IV, which includes 108 cases "threatened with pulmonary diseases", as worthy to be enrolled in any scientific data? Dr. Renton's Table III might possibly be accepted, but the particulars are very hazy, and I must demur entirely to Dr. Lund's conclusion of the number of improved to be deduced from the four tables. Far wiser would it be for him to refer to his own statistics, which are given with more exactitude, and are far more favourable. I regret a slight error crept into my quotation of these, but they give, out of 100 patients, 47 cases of arrest. My 63 patients, who, from a comparison of the cases with those of Dr. Lund, appear to have been slightly less advanced in disease, give 53.81 per cent. improved, 14.28 per cent. stationary, and 34.29 per cent. worse. These results do not greatly differ from Dr. Lund's, but, on the whole, are more favourable to Madeira. Still, compared with my Mediterranean and Egyptian figures, they come out less favourably, and hence my conclusions. It is natural that Drs. Dyster and Lund should prefer Madeira, a place endeared to them by many associations, and where the former regained his health; but they

must not be surprised at others taking a simple common sense view of the matter, and judging by results, and results alone.

The publication of my climate results in this JOURNAL has been, judging from the numerous letters I receive, very welcome to its readers; and, if I have succeeded in laying the matter clearly and intelligibly before the profession, I shall feel satisfied that I have done my duty.

NOTES ON PHTHISICAL LARYNGITIS.*

By JAMES SAWYER, M.D.Lond., M.R.C.P.,
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Queen's College, Birmingham.

I DESIRE to speak to you, briefly and practically, of phthisical laryngitis. For several years, I have systematically used the laryngoscope, in hospital and private practice, in the diagnosis and treatment of this affection. I exhibit some drawings I have made of the changes I have seen, and which are characteristic of the malady. Phthisical laryngitis is a peculiar form of chronic inflammation of the larynx. It is met with in most cases of chronic pulmonary consumption. It usually follows the development of disease in the chest. For a time, it sometimes exists alone; but lesions in the lungs always subsequently arise. It is the laryngitis of the phthisical. Adults are more subject to it than children, and men than women. All the local changes, as seen in the laryngeal mirror, have been frequently described. I have been able to divide these appearances into four groups, marking four distinct stages in the progress of the affection. The stages are: the anæmic, the oedematous, the ulcerative, and the necrotic. The earliest change is local pallor—anæmia of the laryngeal mucous membrane. Sometimes the anæmia extends to the whole of the upper part of the throat, to the fauces, the soft palate, and the roof of the mouth; but it never goes beyond these limits; it is always local; in its localisation is its phthisical significance. In the second stage, the characteristic tumefaction appears. The aryepiglottic folds are changed into two brawny-looking pear-shaped tumours, with their larger ends meeting behind. The epiglottis is usually similarly swollen. In the next stage, these tumefied tissues ulcerate. In the last stage, which is rarely reached, necrosis or caries of the cartilages occurs.

Phthisical laryngitis is a very painful malady, and, when it has passed into its second stage, it is always fatal. It is difficult to give much relief by treatment. But I wish to speak very confidently of the good results which arise from the frequent application to the larynx of a solution of nitrate of silver: one drachm of the salt to an ounce of water. In the first stage, this remedy stimulates the nutrition of the larynx, and so combats the local anæmia; in the second stage, it reduces the tumefaction; in the third, it checks the ulceration. In all, it deadens the morbid and painful sensibility of the affected parts. Dysphagia always arises in this disease. It is often a very serious symptom. The tumid and tender larynx makes deglutition difficult. This condition is promptly relieved by the local application I have recommended.

ANGINA LUDOVICI.

By A. DOIG, L.R.C.P.Ed., Surgeon Army Medical Department.

IN the JOURNAL of the 25th December last, appeared a note by Dr. F. Murchison on the above-named disease. It may be interesting to have some details of a series of well marked cases which occurred amongst the soldiers quartered in the South Camp, Aldershot, during the winter of 1874-5. The disease attacked young men, was in all idiopathic, and accompanied with most intense suffering and great general depression. They all presented the same symptoms as those described by Dr. Murchison, but suppuration occurred in not a few; and one case, of which the following are brief notes, unfortunately had a fatal termination.

Private R. J., 23rd Fusiliers, aged 22, four years in service, was admitted on February 4th. He had been complaining for some time prior of swelling in the neck, which had increased to such an extent that he was forced to report himself sick. On admission, the swelling was chiefly confined to the left side; the submaxillary, sublingual, and parotid glands of this side were swollen, intensely hard, and very painful. The right submaxillary gland was also very hard and tender, but not swollen to the same extent. The lower jaw was fixed and partially open; the tongue was pushed upwards to the roof of the

* Read before the Pathological and Clinical Section of the Birmingham and Midland Counties Branch.