THE NARRATIVE
OF
A BUSY LIFE

AN AUTOBIOGRAPHY

HASSALL

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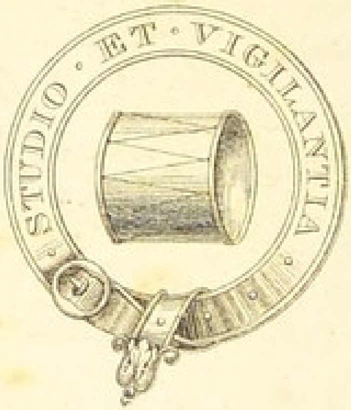

CAR. I. TABORIS

A Goddard

THE NARRATIVE.

"As a boy, I wanted to know about the clouds and the grasses, and why the leaves changed colour in the Autumn. I watched the ants, bees, birds, tadpoles, and caddis worms; I pestered people with questions about what nobody knew nor cared anything about".

JOHN HUNTER.

THE NARRATIVE OF A BUSY LIFE.

AN AUTOBIOGRAPHY.

BY

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PREFACE.

In preparing this narrative of the work of my life, I have been but little influenced by any feeling of vanity or self-seeking. Having contrary to all reasonable expectations been spared through a long series of years; having outlived many whose health and strength were greater and whose future seemed therefore more promising than my own, I have now arrived at the period at which ordinary worldly considerations carry with them but little weight.

I have been an active worker, investigator and writer for little short of sixty years; the investigations have embraced a considerable variety of subjects; their results have appeared in a great many different publications, some no longer accessible, except in our public libraries, some of them so long since indeed, that they had either been forgotten or only a dim recollection remained of them, even in the mind of their author. Added to these considerations was the desire to review the whole of my labours, to take stock of them and so to obtain a clearer insight into their extent and nature.

This review has now been made and one result arrived at which I did not anticipate. In bringing together the chief events and incidents of my life, as far as scientific work has been concerned, I have in a measure lived that life over again, and in some respects more pleasantly than at first, because the cares and anxieties which attended some of my labours no longer existed and were more or less forgotten and thus only the more agreeable feelings and results were recalled.

Should this narrative fall into the hands of any student and seeker after knowledge and should it in any way stimulate and encourage him in its pursuit, this effect will in itself be an additional gratification. The biography at all events shows how much may be accomplished by the diligent use of time and opportunities, even in the face of many adverse circumstances, such as serious and repeated illness, many cares and much strictly professional work. It should be borne in mind, that out of more than seventy years over which this narrative extends, during fifty I have been and happily still am occupied in medical practice, this indeed being of necessity the main object and purpose of my life; the periods devoted to natural history, chemistry and other kindred subjects, were for the most part interludes.

The great and foremost pleasure of my life has consisted in my work, selected for the most part because of its congenial character, in my natural history, microscopical and chemical investigations. From other sources I have derived in comparison but little enjoyment; I have cared but little and have given but little time to the ordinary pursuits and occupations to which so many people devote most of their lives and energies, to general society with its balls, parties, theatres and other entertainments. Neither have I devoted much time and attention to money getting; sufficient has come to me in the ordinary course of my professional work to satisfy most of my very moderate needs and requirements, though when I have possessed more than was wanted for immediate purposes, the surplus has been usually spent in books, or apparatus, or some object connected with my enquiries. Nor have I been in any sense ambitious beyond the desire that my scientific work should be well received.

I have here again to acknowledge, as on previous occasions, the help I have received from my wife in the revision of the M. S. and proof sheets, also for translations from Italian and German works and writings.

In conclusion it should be remarked, that this narrative is not simply a review of the work of a life now almost past, but an endeavour has been made throughout to convey in a simple and intelligible form as much information of a useful and interesting character as was permitted by the nature of the subjects treated.

San Remo, September 1893.

CONTENTS.

APPENDIX.

THE NARRATIVE

OF A

BUSY LIFE.

FAMILY HISTORY.

There are but few persons who have long passed the mid-day of their lives, who do not from time to time look back and recall the chief events and circumstances of their careers, to determine therefrom how far those lives have been well or ill spent; what lessons are to be learned from the experience gained, what opportunities lost, what faults and sins committed; in fine, to judge whether their lives have been devoted to merely selfish ends, or whether they have been of any benefit to their fellow creatures, their country or the world.

It is not only, that we look backward as years roll on and accumulate, but our thoughts go forward and we note what yet of life may remain to us and how the brief space to follow may best be utilized.

It is proposed in these pages to record, not merely some of the thoughts and reflections arising out of a somewhat wide experience, but briefly to narrate the chief occupations and events of a tolerably active and varied life, extending over a lengthened period, embracing more than half a century of professional and scientific work; my first diploma, that of the Royal College of Surgeons of England having been obtained in 1839.

I am induced to write what follows not so much from personal considerations, but partly in the hope that the record may prove of service as a guide and encouragement to some one or more, who may be about to enter on active and responsible careers.

In order the better to understand the narrative, it may be well to give some account of my family and my own early years.

Of my grandfather's family I know but little and it is now too late to learn more; he had however sons and daughters through whom the Hassalls became connected by marriage with the Sandersons, Coppins and Strakers, all families well known in the North of England. I have been informed that he was a medical man, practising in Sunderland. My father, Thomas Hassall was a member of the same profession and a Durham man. At the period of the great Irish Rebellion in 1798 he joined, through the instrumentality of Mr. Burdon Sanderson, then M.P. for the County of Durham, a volunteer regiment called the Durham Fencibles, which was raised to aid in quelling the rebellion; in this regiment he held the double commission of Captain and Surgeon, a combination which at that time was allowed.

From him in my boyhood, I heard many particulars of the rebellion, its hardships, fightings and perils, notably at Gorey, Arklow and Vinegar Hill. I remember well with what emphasis and pride he quoted a paragraph from some well-known historian of the time to the effect, that the conduct of the Durham Fencibles, in intercepting the march of the rebels on Dublin with a view to sack the city, had been "the salvation of Ireland, if not of the British Empire!"

It was in Ireland, that my father first saw the lady who was to become his wife. It was a case of "love at first sight". He was standing on the steps of a hotel in Downpatrick when a ladies' school passed; he was struck with the appearance of one of the young ladies, boldly made himself known to the mistress of the school and after due enquiry and the necessary preliminaries the lady, a Miss Anne Sherrock, became Mrs. Thomas Hassall.

At the termination of the rebellion, the officers of the Regiment of Fencibles were offered commissions in the regular Army, but they thought from the important services their regiment had rendered, that it ought not to be disbanded, and should be constituted a line regiment, hence the offer was in many cases refused. Subsequently through the action of the Lord Arthur Hill of that time, my father became Surgeon to the 1st Surrey Militia and he was attached to that regiment for a long series of years, retiring ultimately on two pensions, one for length of service and the second, granted long afterwards through the influence of Lord Palmerston, for a wound received at Arklow, these pensions amounting together to about £200 per annum. It was in recognition of the gift of the commission, that in 1817 the Christian names of Arthur Hill were in Teddington Parish Church bestowed upon me, a circumstance of which I am sure the present holder of the title of Lord Arthur Hill is quite unaware.

An incident of the rebellion may here be recorded. At Vinegar

Hill my father and mother had a narrow escape of being killed as they were one day out driving. They were about to be attacked under the idea that they were protestants, but the rebels seeing that my mother was wearing a green veil, came to the conclusion that they must belong to the Catholic party and hence their lives were spared. To form any idea of the cruelties and horrors perpetrated at that time, it is necessary to read an authentic history of the Rebellion of 1798.

My father some years afterwards resided at Teddington, practising his profession in that village, Hampton Court, Kingston and the neighbourhood. The Duke of Clarence with the Misses Fitz-Clarence were residing at Bushey Park at that time and on the suicide of His Royal Highness's resident medical attendant, my father in the absence of the Duke, was hurriedly summoned to Clarence House by Miss Fitz-Clarence. The Misses Fitz-Clarence were pleased with my father and he was for a time in attendance; they were most kind and condescending to him. A carriage was daily sent for him, as he was himself not well, and occasionally he was asked to afternoon tea. The use of tea in England was then only beginning and it was a novelty and a rarity. My father happened to remark on one occasion, that "the tea had done him more good than all the medicines of the Pharmacopœia"; shortly afterwards the Misses Fitz-Clarence left Bushey Park temporarily and forwarded him before leaving a bag of the same tea, which he said had done him so much good. Miss Fitz-Clarence also intimated to my father that she would secure for him the appointment of medical attendant to the Household and that she would at once speak to the Duke on his return; this she did, but His Royal Highness, not knowing that my father had been called in during his absence and had been in attendance, expressed his regret and stated that he had only the evening before promised the Duchess of Kent the vacancy for a friend of hers; another illustration being thus added to the many showing the truth of the adage, "there's many a slip between the cup and the lip". The recipient of the appointment was Mr. afterwards Sir Herbert Taylor. Disappointed at the failure of her application Miss Fitz-Clarence expressed her wish to be of service to my father and hoped that if she could be of use he would make it known to her. Years subsequently when Miss Fitz-Clarence had become Lady Sidney, my father once set out from Kew, accompanied by myself, who must then have been very young, to see her Ladyship who was at the time residing in London, he, after much pressure very reluctantly consenting to ask a favour;

the day turned out to be very stormy and my father was not sorry to have an excuse for abandoning the journey, though at the time we had actually started. He could never afterwards be induced to make a second attempt.

It was during my father's residence at Teddington that I was born on the 13th day of December 1817, my mother dying three days after my birth; so that I never had the happiness and advantage of a mother's love and care. My two dear, good elder sisters, Eliza and Ann, long since removed from this world, did their utmost to supply her place. My parents had five children, three sons and two daughters; the eldest son, being of an adventurous disposition, chose the sea, went out in a merchant sailing ship and never returned: the captain reported that he had deserted his ship and was drowned in the attempt to reach another vessel. The account given was so unsatisfactory that my father was fully persuaded there had been foul play, though of this he could never get clear and conclusive evidence. In those days great cruelties were often practised on board merchant vessels. My second brother Richard, did not till somewhat later, adopt any profession; in fact, owing to my father's then very narrow resources he was unable to do so, but my brother was nevertheless determined to make an independent position for himself. To that end he taught himself astronomy and chemistry and prepared a course of lectures on these subjects adapted for schools; these he delivered for some years at different establishments, gaining much approbation, and at length realizing sufficient funds to enable him to purchase the business of a Pharmaceutist at Cheshunt in Hertfordshire; this under his management prospered, so that he next determined to qualify as a medical practitioner. He used to start on horseback at 5 o'clock in the morning from Cheshunt for the Charing Cross Hospital, in the medical school of which he became a pupil, returning in the evening in the same way. These journeys were kept up for some years and were of course very fatiguing. What would many modern workmen, who are so senselessly clamouring for a legal eight hours' work day, say to such days of labour as these! In due course my brother obtained the diploma of the Royal College of Surgeons of England and the extra-license of the Royal College of Physicians of London. He then started in practice in Richmond, Surrey, where our family had resided for some years and was well known; after working, almost slaving, for a quarter of a century and having gained one of the best and most lucrative general practices in the kingdom, he retired, having been able to amass an ample fortune for his family.

These few preliminary particulars bring me to the more immediate subject of this biography.

EARLY EDUCATION.

My early education commenced at a small and very primitive day school, kept by an elderly matron whose name I forget. The instruction was limited to the three Rs, and not much of these even was taught; at least I can affirm that the knowledge of them acquired by me was very little. The school was for the more respectable younger children of the then unpretentious village of Teddington; it was situated in the main street, along which most of the inhabitants passed more than once each day; the schoolroom abutted on this street, the door being generally open, so that anybody passing could look in and see the children on their forms; now although the instruction given was not very extensive, the matron had adopted tolerably effectual methods of keeping her pupils in order. One was to place the refractory subject at the open door with a tall fool's cap on his head, so that all the passers by might see him. I fear I was but a bad boy and I well remember that I often had to be carried to the school every inch of the way in the arms of a stout domestic, kicking and yelling till I was deposited at the door. On one occasion when I had been misbehaving, I was placed in this unenviable and undignified position, adorned with the usual fool's cap, when to my great consternation I saw my father coming along the street; I need scarcely say that I quickly knocked off the cap and withdrew behind the door. Another punishment which in the case of some children was even more effective, was to detain them after school hours, during which time the mistress occupied herself in frying or otherwise preparing her dinner, the savoury and appetising smell of which was a great trial and sometimes brought tears into the eyes of the hungry child.

After a while, I went as a boarder to a school at Knapp Hill in Surrey, an out of the way, primitive and sparsely peopled district about three miles from the market town of Woking. I well remember the day I was taken there. I was driven over in a sort of market cart and as the way was long, the driver, a countryman of those parts kindly offered me some of his own refreshment in the shape of a black pudding or sausage. I did not like its look, but as I was hungry and boys in general are not over particular about their food I ate it and found the sausage much better than I had expected. Afterwards I learned that these puddings are made with blood. This was a case in which ignorance was bliss. I never again regaled myself on black pudding.

The school at Knapp Hill was kept by a worthy and kind hearted man, a Mr. Parker; the pupils consisted of the strong healthy boys and youths of the few tradesmen and farmers of the district and the education was of a restricted character. The surroundings of the house were free and open; the situation cold and bracing, the school-room large, with at one end a lofty pulpit, from which prayers were read and a sermon preached on Sundays, as the schoolroom did duty as a chapel for the few inhabitants of the neighbourhood. The chief corporal punishment in this establishment consisted in the boxing of the ears of any misbehaving boy; but such boxes, not to be forgotten, administered by an enormous hand and strong arm. On one occasion I was the offender; the box on the left ear threw my head over to the right side and into the left hand already in position to receive it and the blow was repeated. One could not stand many such blows; one or two of them was sufficient to suddenly confuse and stun the senses, so that for a while the recipient did not know whether he was standing on his head or his heels. Mr. Parker had even in those days some loose, but as I dare say, some of our sanitary and funeral reformers would teach, correct and advanced notions on the subject of burials. He held the usual mode of interment in little regard and said he would like to be buried at the foot of an apple tree, "so as to ensure a good crop of fruit." I do not think his wish was gratified.

On my father's retirement from practice at Teddington we went to reside at Richmond in Surrey, where we remained for many years and I was then sent as a day scholar to a school well known at that time on Richmond Green. The master, whose name I withhold, was a strict disciplinarian and held a high opinion of the virtues of the rod; he was in fact a tyrant and a most unmerciful beater. On one occasion two boys, unable to endure his harshness, ran away, as boys sometimes will and were not captured for some days; they were placed in a room and fastened by the waist to a heavy weight, were kept thus for at least a day and night and cruelly beaten at short intervals; there was much excitement in the school, the master's daughters interceded and at last the punishment came to an end. Were any schoolmaster in these days to attempt anything of the kind, great would be the outcry and severe would be his own punishment, but at the time I refer to cruel flogging was commonly practised, even grown up youths being subjected to the same brutal treatment; indeed corporal chastisement was considered to be absolutely necessary to the efficient control of the pupils and the maxim thoroughly acted up to, was: "Spare the rod, spoil the child".

My next school experience was of a milder and more favourable character. I became a day pupil at a large school kept by a Mr. Gibbons. This gentleman I never saw, as he was very ill at the time, but the school was, so far as the teaching was concerned, mainly in the charge of a Mr. John George, a Welshman, as is evident from the name; a man of very high attainments as will presently appear. He did his duty most effectively and yet kindly, indeed he exerted himself beyond his strength and to the lasting injury of his health; he used often to harangue the boys in very emphatic language, entreating them earnestly to study and to make the most of their educational opportunities. When any of the boys were careless or stupid and his strength in addressing them was nearly at an end, he would wind up his harangue by exclaiming, with one hand on his side, "One need have lungs of iron and bones of brass to teach you boys".

Shortly after Mr. Gibbons' death the school was given up, but Mr. George remained at Richmond and received a few pupils on his own account, I being one of them. Now, Mr. George was not only a very earnest and conscientious teacher, but he was a man of very remarkable acquirements. He was equally accomplished as a mathematician and as a linguist; his memory was marvellous; he could repeat verbatim any chapter of the Bible on reading it over once only; he could read and write eighteen languages and he continued to add to this number; he stated that from the knowledge already acquired, aided by his memory, he could master some languages sufficiently to read and write them in six weeks. Had Mr. George been attached to any of our Universities, he would have acquired a great reputation and would have been honoured and rewarded; as it was, his attainments were only known to a few and by still fewer were they appreciated. The end of it all was that after some years of tuition, "the lungs of iron and bones of brass" failed him and he died of an affection of the organs of respiration at a comparatively early age. I have retained through life the warmest affection and the most sincere regard for the teacher to whom I was indebted for any love of books and for any scholarly knowledge I ever possessed. This was never very great for I was not a bright or ready learner; hence though I was diligent and painstaking, on the whole my education left much to be desired and it terminated earlier than usual from need of funds and just as I was beginning to take an interest in acquiring knowledge for its own sake.

LIFE IN IRELAND AND MEDICAL STUDIES.

The questions soon arose, what was to be done with me? how was I to earn my living? what calling was I to follow? these questions were soon settled by my uncle, Sir James Murray M.D. of Dublin, who had married my mother's sister, offering to take me into his house and to give me the indentures of apprenticeship which in those days were necessary. This generous offer relieved my father of a great deal of anxiety and much expense on my account. Accordingly in the autumn of the year 1834, I left the Thames in the S.S. Shannon for Dublin, there being no railway to Liverpool at that time. The passage occupied five or six days, the ship on the way putting in at Falmouth and Plymouth. This voyage was subsequently repeated several times and being a good sailor, I enjoyed these trips immensely. The last I heard of the Shannon was that she had been totally wrecked on one of her journeys. On one occasion I remember I went to Liverpool with a cousin, Mr. John Murray on the outside of the Tally Ho! coach; the journey extended over 24 hours, that is a day and a night; the cold at night was intense and I thought I should have died, as it was my cousin was laid up in Liverpool with pneumonia brought on by the exposure. Such occurrences in the coaching days must have been common.

Mr. Murray was the author of some political novels and of a pamphlet bearing the striking title "The Court Doctor dissected", having reference to the sad case of Lady Flora Hastings.

Arrived in Merrion Square I was most kindly received by my uncle and aunt; soon occupation was found me and I quickly became aware that the business of my life, with for me its great events and issues, had commenced in earnest.

My uncle, Sir James Murray, was an energetic self-reliant, thoughtful and indeed, clever man; his earlier years were passed in Belfast, where as a chemist he flourished and made a fortune. Eventually he removed to Dublin and when I arrived he was living in Merrion Square practising as a physician. He was physician to the then Lord Lieutenant of Ireland, the Marquis of Anglesea, who was a martyr to neuralgia. Sir James was enabled to afford him considerable relief and received at his hands the honour of knighthood. My uncle was the author, amongst other publications, of an original work on the use of compressed and rarefied air, applied by means of certain ingenious mechanical arrangements to the surface of the body for the treatment of various morbid conditions and symptoms. There can be no doubt of the value of some of the suggestions made therein, although the book itself is now forgotten and the methods described in it but rarely put into practice.

Sir James being a good practical chemist, perceived at a very early period the objections to be urged against the administration of the but little soluble preparations of magnesia and he succeeded in making an agreeable effervescent solution. He was the first to devise this preparation, which has now for a great many years been known as "Murray's Magnesia". The solution possessing many advantages, was and still is in considerable demand and it has yielded for a long period a handsome revenue.

Amongst other appointments held by Sir James was that of Inspector of Anatomy for Dublin. In that city the bodies of all persons who had died in public hospitals, infirmaries and such like institutions, if not claimed within forty eight hours of death, were delivered over to the different medical colleges and schools for dissection. The chief duties of the office, were to receive notice of the unclaimed and to determine to which medical school or college each body should be sent, the removal and distribution being carried out by the Inspector's staff. In the discharge of these duties difficulties and unpleasantness sometimes arose, occasioned chiefly by relatives turning up and claiming the bodies after the expiration of the statutory limit. After a time some of these duties were entrusted to me and I had to see that the distribution of the subjects was impartially and discreetly carried out. It was necessary that I should occasionally visit the different dissecting rooms to ascertain that the directions given were duly executed and these inspections in those early days of my medical career were by no means pleasant and I naturally shrank from the awful sights of poor dead humanity, emaciated and disfigured by disease, and made still more hideous, necessarily and unavoidably, by the anatomist's scalpel. Indeed Madame Tussaud's Chamber of Horrors is far less trying to view than a large dissecting room with several bodies, each on a separate table, often more or less decomposed and in various stages of dissection. Here I will relate, in a few words, a curious incident which occurred in connection with the distribution of the bodies or "subjects", as they were usually called. The chief of the men employed to convey the unclaimed bodies to the schools was a man named Bowman, a remarkably civil and obliging fellow, fond though he was of a drop of the "cratur". It appeared he had a dread, that some day or other he might himself be extended full length upon one of the usual tables; this fear I afterwards learned was not unfounded, as his body actually was conveyed to one of the schools; some of the pupils recognizing the man and knowing of this dread, clubbed together and had him decently buried.

Soon my professional studies commenced and my uncle gave me some introductory works to read as a preparation for the lectures which were to follow. The first course of lectures I attended was delivered in the medical session of 1834-35 and this was followed by other courses in the four following winters, and in one or two summer sessions, but usually I returned to England in the summer.

At the period to which I refer, the Dublin School of Medicine, ranking always high, stood at its highest: there was a crowd of able men in the profession, including both teachers and practitioners. Amongst the former may be mentioned Harrison, Apjohn, Hilles, Hayden, Ellis, Ireland and Bellingham; while amongst those who were devoted more particularly to medical and surgical practice, Colles, Carmichael, Stokes, Graves, Crampton, Corrigan, Marsh, may be enumerated. Some of these were world renowned men, and even in these days they are well remembered names and high authorities on the subjects to which their talents were specially devoted. The celebrity of the Dublin School and the greater moderation of the charges for attendance on lectures at the hospitals attracted many students from England.

The schools which I chiefly attended were those of Peter's Street, Digges Street and Trinity College; the Hospitals, the Jervis Street and Mercers' Hospitals and the Netterville Dispensary; this last had only a small number of beds and was founded by Sir James Murray. I was more than diligent, made fair progress and gained some prizes, including one for Botany of which through life I have always been fond.

Sir James Murray, from his influential position was enabled to obtain for me free admissions to the whole of the requisite courses of lectures and to the Hospitals; this was of the utmost consequence, as my father's resources, he having retired from practice long before, would not allow of his furnishing the necessary funds, indeed but for this great help I should not in all probability have been enabled to enter the medical profession. I have therefore ever rested under the deepest obligations to my uncle for this, as well as on other accounts.

On first going to Ireland I resided at Merrion Square with my uncle; subsequently I removed to a well known first class boarding house in Harcourt Street, and afterwards, as all my time was now required for my studies, I took a very cheap and humble apartment, being now entirely on my own resources. These were very limited and I was anxious to save expense as far as possible. The lodging I occupied was a sparsely furnished bedroom on the third floor of a house in Digges Street, where for such slight attendance as was needed I was waited on by a red legged and bare footed peasant girl. I now diligently devoted myself to my studies; most of my reading was done in the early mornings and evenings. I used to get up at five and six o'clock in the dark wintry mornings and read by the aid of a rush light, and my breakfast consisted of a sugarless decoction of cocoa nibs and dry butterless bread; the other meals not being on a very liberal scale. This was not so much that funds were deficient, as that any small sum I could save out of my allowance was spent in books or on an occasional treat to the gallery or pit of the theatre, sometimes accompanied by my cousin, Edward Murray. At the time, the great attraction was that inimitable master of melodrama, Power, who it will be remembered was lost years ago on his way to New York, when every soul on board the vessel perished, not one being left to tell the tale of this great catastrophe.

SERIOUS ILLNESS.

What with hard study, poor food, and insanitary surroundings after a while I fell ill. For a time I struggled against the illness, but one day, when more from habit than anything else, I had made my way to the dissecting room and not being able to work, perched myself on one of the high stools usually found in similar places, I became speedily worse and was compelled to go to my room and to bed; before leaving the dissecting room I gave one of my English fellow students some money to buy leeches and I asked him to apply them to my temples; the remedy was one which in these days would not be generally approved for a severe case of typhoid fever, but before they were detached I became insensible. Sir James Murray was sent for and he, with occasionally Dr., afterwards Sir Domenic Corrigan, attended me throughout a very long and dangerous illness, during part of which I was delirious and worried about my studies. The united skill and care of these physicians brought me through the crisis and after some weeks, I had so far recovered, as to be able to be removed into the country. Through the agency of friends, Mr. and Mrs. Alder, to whom I became everlastingly indebted for kindnesses innumerable, continued over a long series of years, I was transferred to a scrupulously clean and bright apartment at Monkstown. I can only give but feeble expression to the feelings I experienced on the journey into the country from the darkness of Digges Street; it was like a resurrection. How sweet and pleasant was the sense of partially restored health, to feel the cool fresh, outer air once more on one's cheek, to bask in the bright warm sun, to see the trees and flowers once again and as the city was left behind to look upon the glorious sea and the beautiful Bay of Dublin! Such sights and feelings were powerful aids, united to youth and hope, to a speedy recovery and so in my sunny bedroom, in full view of the sea, with good food and the kindest of friends my health and strength were soon restored. All my life I was never so happy as when out in the air, in the fields or woods; no matter how mentally disturbed or irritated, I soon experienced a feeling of peace and rest. The winter session now being over and the spring far advanced, I was enabled to look forward to a period of leisure and freedom from care: soon my thoughts turned to other than strictly medical and professional studies.

EARLY NATURAL HISTORY PREDILECTIONS.

I had from my earliest years a great admiration for all that was beautiful in nature, and very early in boyhood I began to study botany and to collect plants and hence became very fond of gardening. When my father and his family were all living in Kew Footroad, Richmond, we had a large garden attached to the house; it was one of those nice old gardens, now not often seen in connection with any modern house; a long strip with a brick wall on one side on which peach, apricot, nectarine, plum and greengage trees were trained, while numerous apple and pear trees were scattered over the ground; it was indeed a very fruitful and productive garden, and in this my brother Richard and I used to work, digging, wheeling, weeding under the direction of an old soldier who had fought at Waterloo and who was a great favourite with my father and indeed with us all. The next house and garden to ours, separated only by a paling was that in which Mr. Gibbons had his large boys' school, but which at a later period became a school for young ladies under the management and authority of Mrs. Gibbons. It was a very prosperous school and I who had been a pupil under Mr. Gibbons, was regarded as a privileged person, being allowed sometimes to join in the evening amusements and always permitted to keep the pupils' little plots of garden in order. This was a real pleasure to me. I used frequently to be up, even in cold wintry mornings at 6 o'clock working away at these plots. Sometimes the frost at this early hour greatly affected me, numbing my hands and feet and as a reaction from the exercise ensued a deathlike feeling of oppression and faintness, ending in a profuse perspiration, would set in. This occupation was continued for a considerable period, till at length it was thought that I was getting too big a boy to have the run of the school and I was in a measure banished; this was a great mortification. Some time after, my father migrated to another house in Richmond, in the Vineyard; indeed during my father's lifetime we changed our residence from one place to another every three or four years, he being somewhat restless and changeful. Here also we had a large but not so fruitful a garden and I took to rearing orange, lemon and tamarind trees from the pips and stones and was very proud of my success. I formed a little hot or forcing bed with the grass mown from the lawn, and used to take our visitors to see my orange plants, now and again bestowing one on any favourite friend or visitor.

STUDY AND COLLECTION OF ZOOPHYTES.

But to resume and return to Ireland. Placed as I was under such favourable circumstances, close to the sea, with its mixed rocky and sandy shore, my old love of nature and natural objects returned with increased force. I soon began to pay almost daily visits to different parts of the neighbouring coast, chiefly between Monkstown and Bray Head. As the tide receded, especially when the sea was very rough, mixed and many coloured sea weeds, corallines and a variety of other productions were left stranded. The sea weeds I collected and dried; the zoophytes or corallines I not merely gathered up, but took home to study, ascertain their names, characteristics and organization. Now the zoophytes, especially the corallines constitute one of the most interesting and graceful of organisms in the whole range of Natural History. The class may be described in a general and popular way as divisible into Anemones, Corallines, and Corals; the first division consists of soft bodied, naked, mostly single animals; the second possesses a flexible skeleton or polypidom composed of a semi-transparent horny substance, usually in the form of thread-like hollow tubes, simple or variously branched; along the sides of these hollow filaments a multitude of small cup-like cells are ranged; these tubes are filled with living threads of animal matter which terminate in each cell in a beautiful little bell-shaped animal, furnished with tentacles disposed in circles. This disposition gives some of the species a vegetable or plant-like appearance; these little creatures are extremely sensitive and on the least alarm, movement, or noise, withdraw themselves at once into the shelter of their protecting cells. A very curious fact in regard to these corallines, is, that although each little polype is perfect in itself and capable of acting independently and has a separate life, yet they are all united the one with the other by means of the ramifying threads, so that the impressions made on any of the polypes are communicated to the whole body or colony of them and they then act together according to the nature of the stimulus, either expanding their feelers ready to catch their prey or food, or to retire in a body to their cells. The third division comprises the corals proper; these have hard and non-flexible calcareous polypidoms or skeletons, and there are but few species of this division to be found on the coasts of the British Isles.

It was with the second division, the corallines, that I was chiefly concerned. For their more complete study a microscope was employed; this was especially necessary for determining the character of the little inhabitants of the polypidoms. At the time to which my observations refer, the horny skeletons chiefly were regarded and thus the knowledge obtained was very imperfect and errors were often fallen into, which would otherwise have been avoided, while the most beautiful and distinctive feature of the entire organisation, the living animal, was frequently overlooked.

In time I made a considerable collection of Irish Zoophytes, many of these being new to the British and Irish fauna; while others had not been before observed, though described in some foreign works; the results of my findings and investigations were published in a series of papers which appeared in the Annals of Natural History in the years 1841-2-3. These papers were illustrated by numerous drawings from the microscope, for which I was indebted to the painstaking pencil of Miss Hunter, a sister of Mrs. Alder.

I afterwards presented a collection of the Zoophytes I had gathered to the Dublin Natural History Society, where they were preserved for many years. I also published in the Annals a list of the Invertebrata found in Dublin Bay and its vicinity. See List of Publications.

But I was not content to limit my searches to the sea shore, so in the hope of obtaining some rare and new species, I used to go into the sea itself: for this purpose I provided myself with a special costume, this allowed of my turning over the loose pieces of rock which were under the water, but still near the shore and which had in great abundance been placed there as a barrier against the encroachments of the sea; this proceeding yielded a rich reward; I thus secured a number of new or rare species, including several belonging to the genus Lepralia, which form living incrustations upon any suitable surfaces; one of these was named after me by Dr. Johnston, *Lepralia Hassallii*.

In this way a variety of other curious and interesting creatures were brought to light. I usually started on these expeditions early in the morning in the winter when the water was frequently very cold, causing my hands and feet to become benumbed; from this at first I felt no inconvenience, but after a time, as reaction took place and warmth was beginning to return, the distressing deathlike faintness was experienced, to which reference has already been made. I now know that the attacks were dangerous and I have never met with any one who suffered in the same way.

DEEP SEA TRAWLING.

Encouraged by the results just recorded, I next determined that I would see what was to be obtained from the deep sea itself. At the period to which I allude the chief fishing and trawling on the Irish coast were in the hands of a number of fishermen or trawlers from Devonshire: these men had good boats, they were steady and experienced and mostly teetotallers. I therefore arranged to accompany them on one of their trawling expeditions. The crew consisted of three men, all strangers to me.

We started from Kingstown Harbour one bitterly cold afternoon in November; a fresh breeze was blowing and the boat pitched about so much that I found it necessary to sit down and catch fast hold of the taffrail to prevent myself, as it seemed to me, from being thrown bodily into the sea. I had not been in this position very long before I heard the crash of broken bottles; this was occasioned by an accident to the hamper of good things in the way of food and drink I had brought with me and which had been abundantly provided by my ever kind friends the Alders, with whom I was staying at the time. We started somewhat late in the afternoon, so that is was soon night; the boat was strongly built, with a large deck and a cabin with four berths; but these were filled with different kinds of gear and tackle and it was obvious that they were never used; one of the berths was cleared out for me; it was very dirty and had only a thin hard mattress, however I soon turned in, but at first not to sleep; the surroundings and situation were so new and strange and then the incessant tramping of the men on deck in their heavy boots, the clanking of chains, the noise of the sea and wind effectually prevented sleep the first night.

Somewhat late, two of the men descended from the deck and at once lay down, fully extended on the bare boards at the bottom of the cabin in their immense jack boots and tarpaulin hats and soon they were fast asleep, as was plainly manifested by their loud snoring which was even more disturbing than the noise of the sea; they were not allowed to sleep very long at a time as the calls to duty in the night were pretty frequent. Of course at first I deplored the loss of my hamper, but afterwards not at all; although I was an exceptionally good sailor I found I had little appetite for ordinary food and cared still less for stimulants. The crew of the trawler lived principally on fish, chiefly soles and plaice, baked before the fire and on soup made with a little meat or fish, mixed vegetables and an abundance of salt. I found this kind of food so nice and appetising that I partook of the same meals as the fishermen; the contents of the hamper were not wholly lost and some meat, fowls and whiskey remained; these I offered the men, but the whiskey they would not touch. We used to travel backwards and forwards some miles from the shore along the Morne Mountains between Kingstown and Belfast, making several trips during the week.

The trawl or net is heavily weighted and sinks readily in the sea, scraping the bottom as it is drawn along, catching any fish which may come in its way and at the same time gathering up any corallines, shells, starfish and other curious inhabitants of the deep. I noticed particularly that very few sea weeds were brought up by the trawl, but chiefly corallines, the very things I was in search of; the comparative absence of sea weeds is explained by the fact, that they require more light than is to be found at the depth at which the trawling was carried on. The trawl is usually down some six or eight hours, being dragged along by the boat with its sails set; it is raised by means of the capstan and windlass, the labour of this being somewhat hard and the time required considerable; the trawl itself is heavy and the weight is increased by its being swayed about in a rough sea by the waves; and the sea was very rough during the week we were out. As the trawl nears the surface of the water, the efforts of the men increase and as soon as it is hauled into the boat, the net is opened and a sight revealed, which if the haul is as large and successful as it usually is, must be seen to be fully realized.

The contents of the net fall out with a rush and spread themselves over the deck; fish of all sorts and sizes, flat fish and long fish, turbot, plaice, soles, brill, hake, ling, cod, haddock and a host of other kinds, some common, others rare and interesting principally to the naturalist; intermingled with these fish are sometimes lobsters, crabs, shells and starfish, besides a whole crowd of other creatures and for my purpose the most important, numberless corallines. Some of the fish when the net is opened are found to be alive and leap about, others are dead. Among the fish caught there are some which the fishermen dislike and even hate, for they are very superstitious; some they dislike because they are said to prey upon the other more valuable fish, others they regard as of bad omen, and to both these kinds they are often very cruel, tearing them up and casting them back into the sea, not without sundry imprecations; indeed the susceptibilities of fishermen in this particular are not very keen and some of them think very little of cutting up live skate, as in crimping.

Now the work of sorting commences, a number of hampers are produced, the turbot and brill are packed in one hamper, the soles in another and so on, little regard being paid to whether the fish are alive or dead. When a sufficient number of baskets have been filled, they are taken ashore at some convenient place and dispatched to their destination.

PHOSPHORESCENCE OF ZOOPHYTES AND OF THE SEA.

The raising of the trawl and opening of the net even in the daytime is always interesting and instructive, but at night, the sight is brilliant and beautiful in the extreme. As the trawl nears the surface and is swayed to and fro by the waves and the motion of the boat, great flashes of phosphorescent fire and light are evoked; these become more frequent and conspicuous as the surface of the water is reached and as the trawl by the aid of the winch and all hands is brought alongside and raised over the bulwark, the spectacle is one of rare brilliancy; the meshes of the capacious net shine out and are outlined as it were and beset with myriads of richly coloured diamonds. A close examination shows that this phosphorescence is mainly due to the numberless corallines raked up from the bottom of the sea and entangled in the net in great profusion. This spectacle is not evanescent, but lasts for a long time and is always much increased by any movement or agitation of the net or sea. If the corallines are collected in a hamper and this is put aside for some three or four days, they are still capable of giving forth their phosphorescent light when briskly shaken.

The corallines and different objects of interest brought up were of course carefully examined at the time, but afterwards more thoroughly and a selection made from them on my return to land.

Another beautiful phenomenon which I had an unusually favourable opportunity of witnessing while on board the trawler, was that of the phosphorescence of the sea itself, of which I gave a description at the time in the Annals of Natural History. The vessel impelled onwards by the force of its sails cleaved a passage through the rolling waters which fell from its sides and from the rudder in a broadening line. On looking over the boat it was seen that the sea fell from it in never ceasing drops, flashes and wavelets of colour, bright and varied as that emitted by the diamond, while from the rudder a long stream of phosphorescent light followed in the wake of the vessel. Altogether the sight was one not to be forgotten; the sea seemed all aglow, as if on fire. Although this phenomenon was not produced by corallines, as in the case previously described, it was yet due to a similar cause; namely, the presence in the water of myriads of minute gelatinous organisms, which when the water was agitated freely gave forth their living fire.

Having been at sea for about a week in the coldest and roughest weather of the month of November we returned to Kingstown, I laden with a goodly assortment of the treasures of the deep. I shook hands, most cordially and not without regret, with my hardy, adventurous and kind companions. When we started they were entirely unknown to me or my friends. I neither knew their names or characters, nor even the name of their boat and it occurred to me afterwards that under other circumstances the trip might not have been free from danger. I might have been tossed overboard in the dead of the night and there would have been no one to tell the tale.

On landing and walking from Kingstown to Blackrock, the road seemed to be thrown into undulations and to heave up and down like the sea and my sight thus deceiving me I could not help kicking the ground as I walked along the road, although it was perfectly level. On presenting myself at the house of my friends I was received with a burst of laughter which at first surprised me. During the whole week I had neither washed, nor shaved, no other water than the spray of the sea had touched my skin and I was so begrimed as to be scarcely recognizable. A warm bath soon set this all right. I recounted my experiences and adventures and when the evening set in I had the lights removed from the room, produced my hamper and displayed to the wondering eyes of my friends the beautiful phenomenon of the phosphorescence of corallines when fresh and living. Of course pretty and striking as was the sight, it was not to be compared in brilliancy to that which was seen on the hauling up of the trawl.

Very early in my collection and study of marine zoophytes, I entered upon a correspondence with Dr. George Johnston of Berwick on Tweed, who was the great authority at that time on the subject and who had published a most important and charming work entitled, "A History of the British Zoophytes". From him I derived much help and encouragement and for a considerable time a frequent correspondence was kept up. I much regret that his letters have not been preserved. He was at the period to which I refer, engaged in the preparation of a second edition of his book on zoophytes. Perhaps I may here be excused for quoting a short paragraph from the preface of another work of his, an illustrated volume on "The British Sponges", a kindred subject. "Mr. Arthur H. Hassall, whom I claim as my pupil in zoophytology and likely ere long to surpass his master therein, also sent me several sponges from Dublin Bay and other localities."

This 2nd edition was published in due course many years since and consisted of two volumes, one entirely devoted to illustrations. This work, strange to say I have never even seen, but I have just learned through my ever kind and obliging friend Mr. Clayton, that it contains numerous references to my investigations and writings on the zoophytes; in fact that it embraces the chief and more interesting results at which I had arrived and which had been recorded in my publications on the subject. Space will only allow reference to one point out of many which are dealt with by Dr. Johnston in his beautiful work.

There is frequently encountered on shells cast upon the sea shore a spinous incrustation, the nature of which was for a long time undetermined and had given rise to much speculation. At length I discovered from the examination of fresh specimens, dregded up from deep water, that the incrustation was really the work of a zoophyte and that it constituted a new genus on which I bestowed the name of *Echinocorium clavigerum*. This conclusion was warmly combated by Professor Edward Forbes, a distinguished naturalist, who maintained that the polype to which I attributed the incrustation had really no connection therewith and that its presence was merely accidental. He even went so far as to name the zoophyte after myself, *Coryne Hassallii*, as this he considered was really a new species.

In reference to this point I cannot do better than quote a paragraph from Dr. Johnston's new edition. "It was long believed that the connection between this muricated crust and the soft polype, was only accidental. In this country Mr. Hassall was the first to affirm the contrary. His observations appeared to be insufficient to prove their constant and organical connection and Professor E. Forbes contended strongly for the older view. It is now unnecessary to enter into the discussion for the correctness of Mr. Hassall's conclusion is proved by the prior observations of van Beneden and by the more recent reseaches of Philippi and M. de Quatrefages".*

I have had but few opportunities of examining English zoophytes, but my cousin Mr. John Coppin, many years since brought me some he had found for my examination. Amongst these were two new species, one of which ranked as a new genus and I named it *Coppinia*, a genus which has been accepted by subsequent writers.

At the same time that I was studying and collecting zoophytes and sponges I also made a collection of marine shells, for which purpose I had to pay frequent visits to the splendid sands of Clontarf, so well known to the conchologist for the variety, rarity and beauty of some of the shells which are frequently to be obtained there, with the living animals or mollusks still in occupation of their houses.

Amongst the students with whom I was brought into contact, was Mr. George Allman, who like myself had a strong fondness for natural history generally. He also used to collect marine objects and study them afterwards in the evenings at his own lodgings. These evenings we sometimes spent together; we compared notes and described what each had found. The evenings thus spent were very pleasant to me and I have cherished a lively recollection of them through these long years. Allman afterwards became a distinguished naturalist, he published some masterly memoirs on Freshwater Zoophytes and held for a time the Chair of Botany in the University of Edinburgh.

The above brief record closes the story of my natural history proceedings in Ireland. It was somewhat fortunate, that my illness fell upon me towards the close of the winter medical session so that this was not really lost but counted in the curriculum of my medical studies; it was however a sad disappointment to me as I had been reading hard and preparing to go in for certain modest prizes, of books chiefly; there were no great pecuniary rewards in those days, such as now are given in most medical schools and colleges, to stimulate study and help the impecunious but diligent student on his uphill way; however the disappointment was in a subsequent session made up by the prize for Botany being awarded me.

My natural history pursuits and exertions occasionally led me into positions of some danger. One of these was when I passed rather late in the evening, round Bray Head, a steep and rocky headland separating Killiney and Dublin Bays. At no time of the tide does

* "The genus *Hassallia* of Berkeley is the reward of his services to science." Johnston.

the water leave the rocks, a fact of which I was not aware when I started with the intention of passing from Killiney to the Dublin side of the headland. As I was not able to swim and evening was closing in, I had at some risk to take to the water and with considerable difficulty, to clamber over some awkward boulders and rocks.

On another occasion I was staying at North Shields with relatives; the family consisted of my cousin Mrs. Coppin who was a Miss Hassall, her son and my life-long friend, Mr. John Coppin, to whom I have referred on the previous page, and my aunt, Mrs. Sanderson, a sister of my father. I was in the habit of paying occasional visits to Cullercoats near Tynemouth, in search of zoophytes and shells. One day I was busily at work on a ridge of rocks called the Bear's Back, bare at low water, but covered when the tide was at the full with some feet of water. While intent on this ridge with hammer and chisel in cutting out some boring shells, or Pholades, I heard cries and shouts, but not thinking they had anything to do with me, I paid no heed, but soon they grew louder evidently denoting alarm and my attention was now attracted. I looked up from my kneeling posture and saw to my dismay I was surrounded with water and that the "Bear's Back" was very nearly covered; the shouts were intended to warn me and proceeded from a crowd on the sea shore. Of course, I lost not a moment in making for the land, leaving no doubt, hammer and pholades on the rocks. I took to the water already some feet deep in places, floundered and fell over sundry impeding boulders and finally reached the shore half drowned and quite exhausted. I took care however not to land in the midst of the crowd, but at a little distance from it as I was rather ashamed of my stupidity and dripping wet as I was, I ran all the way to Shields.

Having mentioned the name of Mrs. Sanderson, a very old lady who was at the time quite blind I will narrate an amusing little incident. In one of my rambles I had come across a toad and had put it in my pocket, intending later on to study its ways and habits. Forgetting the toad I was sitting on the sofa beside my aunt, when suddenly there arose a cry of alarm. It appeared that the toad had escaped from my pocket on to the sofa and that my aunt's hand had come into contact with its soft body. The toad was afterwards placed in the garden where he made himself a suitable habitation in the ground, maintained himself for a long time, and aided the gardener in freeing the garden from injurious insects and such other inimical creatures.

COMPLETION OF MEDICAL STUDIES.

My medical studies were now far advanced and I had but one more winter session to complete the curriculum necessary to enable me to undergo the requisite examinations.

One such ordeal I had already passed in 1837, that for the Diploma in Midwifery. At a very early period of my studies, I was required to attend lectures on Midwifery and the Diseases of Women and afterwards to attend cases at the Anglesea Lying-in-Hospital.

Certain students were told off in turn to be on duty at this Hospital, some by day, others at night, they being also liable to be called to confinements within a certain radius around the Hospital. Of these duties I had my share, young as I was, indeed so youthful was I both in appearance and reality, that I was called "the boy doctor", whose services nevertheless were in frequent request.

I could relate some curious experiences during my attendance at this Hospital; the district surrounding it was extremely poor and wretched and some of the patients to whom I was summoned were in the most abject poverty. I have seen them in the hour of their need lying on straw on the floor, in a room without furniture, without food, not even the ordinary gruel, fire or light. Under such circumstances the first thing to be done was to provide these requisites. Often the rooms were inconveniently crowded with numbers of sympathetic friends, as well as occasionally a drunken husband, who had to be turned out; an operation not always easy to accomplish. I remember well one frosty night in mid-winter being called out to attend a poor woman who was suddenly taken ill in the street. I found her lying in the middle of the bare, frost-bound and sparkling road, with no other attendant than an old watchman with his dull and dismal lamp. The woman and her infant were conveyed to the Hospital, where the mother made quite as good a recovery and the child thrived as well as if they had been born to the purple and surrounded by every comfort and luxury. The above incident occurred shortly before the inauguration of the new police or Peelers, between whom, on their first appearance and the medical students, a fierce war was carried on for some time with snow balls flying in all directions.

My last medical session having come to an end, I had once more to think with the greatest regret of quitting Ireland and of returning to England, my permanent home. I was ever most kindly treated in Ireland; I was under great obligations to my uncle and his family and I had to part with many kind friends, especially Mr. and Mrs. Alder. My pleasant natural history pursuits in Ireland being brought to an end I paid a last, if not a final farewell to the beautiful Bay of Dublin and its picturesque coast scenery, the Hill of Howth, Killiney Bay, Bray Head, Wicklow, with its sugar loaf mountains, the Dargle, the Devil's Dyke, the Meeting of the Waters, the Vale of Avoca and other lovely spots and places within easy reach of Dublin. I may mention here, that I did once return to Ireland for a few days shortly after the Dublin Exhibition, but though the place was the same, the people with whom I was formerly associated were gone, for the most part; some to their last resting places, while others were scattered in different directions. The scenery was of course as beautiful as ever and there were surprising evidences of advancement and prosperity; houses and villas had multiplied everywhere, but especially about Killiney Bay with its charming marine promenade.

RETURN TO ENGLAND.

Returned to England for a permanency and having still some leisure before undergoing the examinations necessary, in order to become a fully qualified medical man, and being once again in Richmond my thoughts turned to my favourite natural history pursuits. I now directed my attention to the study of structural and physiological botany. In the study of the minute structure of the vegetable tissues I found that the microscope rendered the greatest possible service, indeed it was indispensable; I had no thought at the time of the important practical uses to which this knowledge was to be applied. In those days people often said to me, "Ah! the microscope is all very well as an amusement, but of what practical use is it in life?" these people little dreaming of the many and vastly important facts which in the future were to be brought to light by its instrumentality; indeed I myself at that time had no adequate conception of the capabilities of the microscope and to what striking results it would lead; it was sufficient for me that it revealed things curious and beautiful, the beholding of which afforded infinite pleasure and some instruction. This knowledge was to come afterwards.

BOTANICAL ENQUIRIES AND INVESTIGATIONS.

Being at Richmond and in the neighbourhood of the Royal Botanical Gardens at Kew, I had now the most favourable opportunities of acquiring a further knowledge of botany. Sir William Hooker, the then director of the gardens and Mr. Smith were both most kind to me and granted every needful facility: to the latter I had constantly to apply for information, which from his great knowledge and experience he was always able to furnish and although some of my questions were doubtless very elementary he was ever ready to impart the needed help.

One of the subjects to which I gave some attention was the form and structure of the Stigmata of Flowers and the disposition of the minute prolongations or hairs with which these are frequently clothed. The chief purpose of the hairs was obviously to arrest the fall of the pollen cells and to aid in keeping them in contact with the stigmata until fertilization was complete. A stigma thus laden with pollen grains, forms a very beautiful microscopical object.*

At the same time my attention was directed to the structure of the Pollen itself, a powdery substance discharged often in great abundance from the anthers of flowering plants. This pollen under the microscope is seen to consist of innumerable little cells or utricles of various sizes and shapes and some of complicated structure, the different kinds also forming very beautiful objects. The surface of these utricles is usually moist, is coated with an adhesive material and in many cases is covered with a number of short pointed spikes whereby they are the better able to retain their hold on the stigmata. The outer envelope of all the grains is furnished with a variable number of slits or apertures which when the grains are in contact with the moist stigma open and from these openings issue the tubes which penetrate the stigma and so enter the ovary. The object of all these contrivances is the same; namely, to aid in keeping the pollen in contact with the stigma. Another curious fact in connection with the pollen is, that certain families and genera are characterized by pollen of special and characteristic forms and structure, so that it is often possible to divine the family or genus of plants and even occasionally the species from which the pollen is derived.

BEES AND HONEY.

The above fact is illustrated in a very interesting manner by Honey. The bees in collecting, carry away with them, mixed up with the saccharine secretion itself, more or less of the pollen of the plants from which this is obtained, so that if we carefully examine with the microscope a sample of honey we shall find it to contain several different kinds of pollen, by the characters of which we are often able to declare whether the honey has been gathered from the heath, the mountain, or the garden. The colour of honey also affords in some cases an indication of the source from which it

* Annals of Natural History Vol 8. 1842.

has been derived; thus, my friend, Mr. Austin of Lucerne informs me, that honey from cherry blossom is very pale, almost colourless, while that gathered from pine trees has a more or less dark hue, in some cases being, when in bulk almost black; the cause of the darkening being due, I believe to the formation of a minute quantity of tannate of iron. In Switzerland of course, with its pine forests and abundance and variety of wild flowers, the bees obtain large quantities of honey; but it must not be supposed that what is placed on the breakfast tables of nearly every pension and hotel, is pure honey; it is designated "Tafel Honig", table honey, and pure honey enters into its composition in but small proportion, in some cases forming not more than one fifth of the article, the rest being a kind of non-crystalizable sugar, termed glucose, which is frequently made by the action of sulphuric acid on potatoe or some other kind of cheap farina or starchy substance. It is commonly supposed that bees gather or collect the honey ready made from flowers, but this is a mistake; they simply gather up the saccharine material which they convert and elaborate into honey. One proof of this is, that when bees are largely fed on a syrup of sugar, as is frequently done in the winter, the product is still honey, though one would then be scarcely able to do that which in the case of good genuine honey it is stated can readily be done; namely, "taste the flowers".

This brings me to refer to another very curious particular, illustrating the evidence of intelligence shown by bees in collecting honey. I observed a great many years since, certainly more than forty, and published the fact at the time, that the corollas of a great many flowers are pierced by a small hole on the upper surface and near the base of the flower, just over the position of the anthers. The idea at once occurred to me, that these apertures must be the work of bees. I found that the flowers, thus pierced were nearly always long and tubular, too long for the proboscis to reach the pollen by the ordinary channel so that the bees were thus enabled to extract the honey from outside the flower. On further examination I found that nearly every mature blossom was thus penetrated, even in the case of mountains covered with heather. I have myself frequently seen the bees go from flower to flower and make these holes. When from the visits of previous bees these apertures already existed, other bees would often pass these blossoms by and confine their attention only to the newer and untouched blossoms, though sometimes a bee would put his proboscis through an old hole, as if to make sure whether any more honey could be obtained.

Another instance of apparent intelligence on the part of a large black bee, Zolocopa violacea, in the collection of honey may here be related. This creature is of considerable size, shining, very active and vigorous and a great rifler of flowers; it is somewhat common in the gardens of the Riviera and it is often very interesting to watch its proceedings. Should there be any anterrhinums, or "Snap dragons", in flower he will visit the blossoms in succession; it will be remembered that the corollas of these plants are of a peculiar shape, they are two lipped, with a wide mouth, which although naturally closed is capable of being opened. This bee seems to know well what he is about; he settles upon the lower segment of each blossom, which has the effect of depressing the segment and of thus opening wide the mouth of the flower; he now bundles in and is more or less hidden from view, but he soon backs out again, his shining dark coat being covered with the golden pollen.*

Mr. Austin finds that bees are very susceptible to changes of weather. When this is bad, they become irritable and angry so that it is not prudent to interfere with them or their hives. On signs of approaching change, especially a thunder storm, they perceive whether the signs will culminate in bad weather, or will pass away; if the former they show their anger, if the latter they manifest no concern; Mr. Austin states that the bees are never mistaken and after observing them on doubtful days, he is always able to advise his friends accordingly.

MEDICAL EXAMINATIONS.

Although my medical studies, so far as lectures and "walking" the hospitals, were concerned, were nearly at an end, yet much remained to be done before I could claim to be a duly qualified medical man. There were two serious examinations to undergo and two diplomas to be obtained.

In England the dual system of medical education and practice still exists, that is to say the medical profession is divided into two branches, the surgical and the medical. For each of these a somewhat different course of study and two separate diplomas were formerly required; for Surgery that of the College of Surgeons of England and for Medicine, that of the Apothecaries' Company. The one qualifies for surgical and the other for medical practice; but in order to legally hold certain appointments in the Army and Navy, in the Poor law service and in many other public offices, it is necessary to possess the double qualification. Nor is this all; if one desires to qualify in the higher branches of the profession other

* See Illustrated Memoir on Pollen. 1842. Annals.

examinations and diplomas have to be procured. The two diplomas named do not give the right to practise as a physician nor to put the words M.D. after the name; for authority to do the former one has to apply to a College of Physicians and for the latter to some duly qualified University. These remarks apply for the most part to England; in Scotland and Ireland somewhat different regulations exist.

Abroad and in most foreign countries the medical profession is regarded as a whole, medicine and surgery are not divorced and generally only one diploma is needed.

Now this multiplication of examinations and diplomas in England is fraught with evils and disadvantages; the system is wasteful of time and money, it increases immensely the mental strain, labour and anxiety, augments the risk of rejection, is harassing and exhausting to the last degree. For this state of things there is no real necessity; it has grown up partly from want of sufficient consideration, but more from selfishness and the desire to secure exclusive benefits by some of the medical corporations. But I have not yet enumerated all the trials of this system or rather want of system; take the example of the College of Physicians of London; there are no less than three orders of medical practitioners in that body; licentiates, members and fellows. With all this, the College of Physicians is not able to confer the degree of M.D. which is the province of a university to give and the obtaining of this title requires in most cases two other stringent examinations, more money and two other diplomas, that for the M.B. and the M.D. Of the three grades, two only confer the status of a Physician.

Before presenting oneself for examination for any of the diplomas it is usual, to ensure fuller preparation and to lessen the risk of rejection, to have recourse for a few months to a special teacher, vulgarly known as a "grinder"; nearly all candidates avail themselves of the services of these gentlemen and it is very necessary that they should do so, no matter how diligent they may have been or how well they have prepared themselves. In my day it was well known that several of the examiners of the Royal College of Surgeons held on certain subjects, special and sometimes peculiar and crotchety views, with which the grinder was fully conversant. If the candidate were unaware of these, his chances of rejection were increased; again, the teacher would test thoroughly his pupils' knowledge, especially with a view to ascertain whether he was prepared in all subjects or whether there was any one in which he was more or less deficient. The two chief teachers in London at the time to which I refer were Mr. and Dr. Power, both clever men, admirably fitted for their duties: the work of teaching several hours daily was very exhausting and only strong men could for long periods endure the fatigue and strain. In the classes of these two gentlemen I met with several students who afterwards became famous in the profession of medicine.

EXAMINATION AT THE COLLEGE OF SURGEONS.

It was as far back at the year 1839 that I presented myself for examination as the College of Surgeons of England. I remember well most of the incidents of the day. It was late in the afternoon that the candidates were ushered into a by no means imposing waiting room, which was vulgarly, but very appropriately named the "funking room" and the anxiety of the hour or so passed there before being called before the examiners was trying indeed. It was curious to note the different ways in which the anxiety manifested itself; some were depressed and silent, their faces bearing evidence of their mental condition; others were unduly excited, but none were at their ease and on all the strain and suspense were manifest. One man was very talkative and promised if he passed, he would treat us all to the theatre and afterwards to a supper to celebrate the event. At length we were marshalled into the examining room. In this were four small tables, well separated from each other, and at each were two examiners, the candidate passing from one table to the other in succession after being questioned at each for about twenty minutes. The examination was conducted principally by one of the two gentlemen who also took notes of the replies; these were passed on afterwards to the examiners at the next table. In these notes usually some opinion was expressed as to the manner in which the questions, which were wholly *viva voce,* had been answered, a proceeding which could not fail to exert a considerable influence on the final result, good or bad as the case might be. At the first table Mr. Anthony White was the chief examiner; he was a friend of my father and I was myself acquainted with him; this served to put me more at my ease and all went well, but at the next table I was not so fortunate and a difficulty arose as to some point in anatomy, a speciality of the examiner; at the third table nothing very particular occurred. At the fourth table Sir Astley Cooper and Mr. Samuel Cooper presided. I had always entertained a feeling of admiration and almost of veneration for Sir Astley and I was somewhat taken aback when I found his manner discouraging and calculated to upset the equanimity of a nervous man. I remember one of the questions he asked me was: "What in a case of amputation of the foreleg would determine you as to the place where you would make your first incision?" I replied; "the position in life of the patient." "What! Sir" he exclaimed in angry tones, "do you mean to tell me you would operate differently in the case of a poor and a rich man?" I said "yes", not immediately following this up with any explanation, as I was somewhat disquieted. Mr. Samuel Cooper then most kindly came to the rescue. "You mean to imply perhaps that you would have regard to the man's means of getting his living afterwards? "Yes" I said, "that is exactly what I did mean. In the case of a rich man I should have more regard to his appearance and needs and should so operate as to allow of his wearing an artificial limb whereas in the case of the poor man I should think more of giving him a sound stump to which a wooden leg might be attached, so that he would be able to trudge about and earn a livelihood." "Ah!" Sir Astley then said, "that answer will be accepted." I left the table vexed and disappointed, for I had always heard that this great surgeon was remarkable for his urbanity. The answer really required had reference to whether the patient's leg was thin and wasted or stout and muscular; if the former the incision would have to be somewhat higher up than in the latter case, so as to allow of a sufficient covering for the bones. The net result of the examination was that at two of the tables I had acquitted myself well and at the other two less successfully.

At the end of the examination we were once more ushered into the "funking room", some of us relieved in a measure only of our anxiety. While thus waiting, the examiners were conferring together and deciding upon our fate. The first called out and this was thought to be rather a bad omen and conducted back to the examining room, was our generous fellow student who had promised us tickets for the theatre and a supper afterwards, in case he passed. In a few minutes he rushed back to us exclaiming "I am plucked"; he seized his hat, rapidly disappeared and that was the last we ever saw of him.

In due course we were called collectively before the President of the College, Sir Astley Cooper, and the other examiners and members of the board. Sir Astley, whose grand presence and dignified bearing were so conspicuous, then addressed a few suitable remarks to us; we subscribed our names to the usual conditions of fealty &c. and left the College as fully qualified members; once more free and for a time relieved of such a load of care and anxiety as only those can realize who have undergone a similar ordeal. This diploma was granted in 1839, just fifty four years ago.

THE EXAMINATION AT THE APOTHECARIES' COMPANY.

But as yet only one professional portal was open to me, since as member a of the College of Surgeons I was legally qualified simply to practise surgery. In pursuance therefore of the old dual system it now became necessary to prepare for a second examination, that of the Apothecaries' Company. For this purpose recourse was again had to the very useful but somewhat abused "grinder". After due preparation I found myself before the examiners of the Company. Of one incident of this ordeal I have a clear recollection. I was asked to give an illustration of the rapidity and force of the growth of vegetable cells and tissues. I cited the case of fungi, some species of which I said, had been known in the course of a single night, to raise up and disarrange even heavy flag stones. "Ah indeed!" exclaimed the examiner; he beckoned to two or three of the other gentlemen present and related to them what I had stated; they all seemed incredulous and I was getting uneasy, as it seemed to me that the gathering round of several examiners portended no good. At length he said, "you have told us a pretty tale, pray Sir, where did you get it from?" I replied "you will find it related in Gilbert Burnett's "Outlines of Botany", a voluminous and noted work not often in the hands of a medical student. The manner of the examiners changed and in the end I came off with flying colours and was complimented. The Diploma of this Society was obtained in 1841. I was now a full fledged medical man and duly qualified to hold most public medical appointments.

REMOVAL TO CHESHUNT.

During the time of preparation for the two examinations my father was residing at Upper Bedford Place, Kensington. Shortly afterwards we all removed to Cheshunt in Hertfordshire, to be with and help my brother Richard in establishing himself there; in this he was, as already explained, very successful.

THE BRITISH FRESHWATER ALGÆ.

Not having at the time any clear views as to the future, I had now a period of comparative rest and leisure, which however did not last very long. Hertfordshire as is well known, is on the whole a damp county, abounding in little streams, dykes, ditches, and ponds, some perennial. In my walks about in search of freshwater and land shells and other interesting organisms, I was struck with the green, olive coloured, or yellow scums often seen, either floating in cloud-like masses on the surface of ponds, or diffused through the water below the surface, causing it to present varying hues of green, blue, or yellow. It occurred to me, that these growths would form very interesting and instructive objects for the microscope, which had already revealed to me so much that was curious and new. In this expectation I was not disappointed and soon became deeply engaged in the study of the order of productions to which these apparent scums appertained.

They are of a vegetable nature, the freshwater types of the sea weeds or algæ. At this time comparatively little was known of the Freshwater algæ; there was no separate work treating of the subject; even the Marine algæ had only been partially collected and described in the beautifully illustrated work of Dawson Turner and in the more recent book of Harvey: so here was a field for investigation which promised a rich harvest to the diligent seeker.

The Freshwater Algæ are divisible into three distinct groups; the *Diatomaceæ*, characterized by their siliceous shells or skeletons which render them so indestructible and by their brown or yellow colour; the *Desmidiaceæ*, which are green, soft, non siliceous, and many genera and species of which are distinguished by their geometrical forms, and thirdly the true *Confervaceæ*, filamentous or thread-like plants, the freshwater algæ proper. The two groups first named had been studied at an earlier period than the filamentous confervæ to which my attention was more particularly directed.

The filaments, of which the majority of the freshwater confervæ proper consist, are composed of *unbranched rootless* threads, floating freely upon or in the waters in which they are found; but there are also many kinds which are *branched*, and which like the corallines are furnished with *roots*. As might be anticipated the rootless species are met with in still and even stagnant and impure waters, in perennial ponds or in sheets of water which dry up in the summer, in tanks and cisterns; while those provided with roots grow in and inhabit running waters, streams, rivers, waterfalls and fountains.

The growth of the rootless kinds is often very rapid and so extensive that large sheets of water sometimes become covered and even filled with them; their vitality is also very considerable; thus, if from the summer heat, or want of rain a pond becomes dried up, and the confervæ are apparently destroyed, they will yet often quickly reappear and their growth be renewed on the descent of rain and the refilling of the pond with water.

At the period at which I commenced the study of the Algæ, over fifty years since, but little was known of their modes of growth and propagation; the mode of growth is very simple, namely, by the elongation and division of the thousands of cells of which each thread is composed, a method by which a very rapid increase in length is effected.

The simplest method of propagation is by the separation of the component cells, as in the vibriones, bacilli and spirilla; the mode of multiplication of many of the simple unbranched confervæ as some Spirogyræ is most interesting. The threads of these, when undisturbed, usually lie more or less parallel the one to the other; between every two adjacent threads a curious and even a marvellous attraction exists, by which they are made to approach each other and range themselves longitudinally side by side; soon a series of nipple-like prolongations become developed on each thread, a nipple proceeding from a cell of both threads, these blind and closed prolongations at length coalesce and a free passage is established between the cells on either side; next, it is seen that the contents of one of the two cells has passed into the other cell with which it is united, the two thus becoming mingled; these mixed contents next become concentrated into round or oval masses or bodies covered ultimately by protecting envelopes, the formation of the spores, being then completed. The conferva now soon dies and breaks up, the spores thus become liberated and from these, at the proper time another generation of the same species of conferva is developed. A second fact equally curious is, that in some species, this mingling of the contents of two cells takes place in the two adjoining cells of the same single thread and without the protrusion of any nipple-like prolongation. Thus it would appear that even this low order of vegetable productions exhibits some of the phenomena of sexuality.

The enveloping membranes of the spores protect and preserve their granular contents, until such time as the season and other conditions are favourable to the growth of the confervæ; in dry weather, or in winter they remain for months quiescent, as though dead, but on the return of spring, the spores burst and from them issue the rudimentary threads which rapidly become developed into new and perfect confervæ.

Now since in damp, low lying districts these confervæ abound and often occur in vast quantities, forming variously coloured frothy scums upon the surface of considerable bodies of water, we may be sure that they perform some highly important function in creation: What this is may now be considered.

The algæ and particularly the freshwater confervæ, like in fact all vegetable productions, must respire; their respiration consists mainly in the absorption of carbonic acid and the giving out of oxygen, this being to some extent a reversal of the process which obtains in the case of animals. The evidences of this respiration are very obvious. If on a sunny day we examine a portion of any floating scum we shall see that innumerable bubbles of a gas are entangled in its threads; so abundant are these that sometimes the mass will crackle under the pressure of the fingers. The same thing may be seen in a different form; if we put a portion of one of the scums in a tumbler of water and place it in the sun, bubbles of gas will be seen to appear gradually, some frequently rising to the surface of the water and there bursting.

The confervæ it is obvious have need of food for their growth and nourishment; one of the constituents which enters very largely into the composition of vegetable organisms is carbon; this they obtain by the fixation of the carbon of the carbonic acid present both in the water and in the atmosphere; but they also absorb from the water much of the albumen, a nitrogenous substance, present in solution; as also other bodies of a more or less analogous composition. But the most salutary result of these processes of respiration and nutrition is, that the confervæ are water purifiers of the most scientific order by preventing decomposition and the formation of noxious compounds. Of the vast quantity of this class of productions in some districts and of the benefit which they confer by their purifying action, few persons have even a faint notion and it is to be further observed, that they most abound where they are most needed, that is to say in stagnant and impure waters. But for these useful scavengers, who can say what diseases might not arise?

Not only are they purifiers of stagnant and polluted waters, but they afford food and shelter to myriads of animal organisms.

These confervæ therefore, simply constituted as they are, but little attractive to the uneducated eye and conveying rather the impression that they are merely scums of impurity and slime, really fulfil very important purposes in creation and they are also among the most beautiful of minute objects when viewed under the microscope.

It has been stated that the single unbranched and rootless confervæ are found chiefly in still waters, the reason why they are not furnished with roots is therefore obvious; they do not require them, they would only be a disadvantage and a hindrance; their functions are better fulfilled by their freedom from any attachment; they can thus be wafted by gentle breezes or strong winds from one part of a pond or little lake to another and they can rise or sink in the water in obedience to temperature and the sun. If the weather is dull and sunless they sink; if warm and sunny they rise to the surface, borne upwards by the countless bubbles of gas entangled in their meshes.

With the branched confervæ the case is different; these inhabit for the most part, pure running waters; some kinds are to be found in rapid streams and even cascades; hence with them, as with the zoophytes of the ever restless sea, roots become a necessity and the way in which their attachment is effected is very interesting. There is no obvious conjugation of the filaments as in the branchless confervæ, the spores, or rather zoospores are simply enclosed in the cells; but strange to say when they escape from these, they are furnished with little lashes or tentacles, like those of many fresh-water ånimalculæ, by which they are enabled to move about freely and rapidly and so to go in search of a suitable habitation to which they can fasten themselves, grow, flourish and perpetuate the species. Thus it might be said of such confervæ, that in their early embryonic life, they are of an animal nature, since they have in a marked degree the most notable characteristic of animals, special organs of locomotion.

The colouration of large bodies and sheets of water by means of confervæ is remarkable. A notable instance of this was presented by the Serpentine many years since and attracted much attention. I was commissioned by the then President of the General Board of Health, to investigate the subject. The water of the Serpentine was observed to have turned suddenly of a bright green colour. I found that the appearance of the water and the distribution of the colour changed almost from day to day; it would be light or deep green or even blue; sometimes the whole body of the water would be affected, at others the colour would be partial only and confined to one or the other end of the Lake, according to the direction of the wind; again, occasionally it would disappear altogether. These various appearances excited a good deal of wonder and speculation and were not without effect in diminishing the number of bathers. The microscope soon enabled me to declare, that the cause of the colour and of all the strange changes was to be attributed to the presence in the water of vast numbers of a very minute confervoid organism, a species of Spirillum; this was wafted hither and thither by the wind and it sunk or rose according to weather and the temperature of the water and thus simply was the changeful character of the phenomena accounted for. My original report will no doubt to found among the records of the Board of Health of that time.

My investigations of the Freshwater Algæ, commenced in 1840, will be found detailed in a series of illustrated papers and articles in volumes X, XI and XII of the Annals of Natural History for the years 1842-3.

Later on the whole of these articles, and the results of further researches and studies were collected and incorporated in two volumes, one of text and one of plates, in a work entitled: "A History of the British Freshwater Algæ." This was published by Longmans in 1845; it was dedicated to James Scott Bowerbank F.R.S., a great and deserved authority in those days on microscopical subjects, and whose weekly gatherings, at that time I frequently attended.

Fifty years have elapsed since the investigations were made on which this work was based. I refer to this remote date because looking back now by the light of the present day, no one can be more conscious than myself of the many defects of these volumes, both of the text and the illustrations. The instrument with which I first worked was a primitive, not perfectly acromatic compound microscope, very trying to the sight. The one hundred and three plates were all executed by myself, both the drawings and the engravings. If I could have had the opportunity of investigating the subject again and bringing out a new work, what a very different affair I should be able to make of it, with modern instruments and other advantages now attainable!

I may here, in taking leave of this subject, refer to rather an amusing incident in connection with my "History of the Freshwater Algæ". I happened one day to stray into an auction room in the neighbourhood of Covent Garden where the sale of the books of the deceased naturalist Mr. Yarrell, so well known for his illustrated works on British Fishes and Birds, was in progress. I had not been in the room more than a few minutes, when a copy of my Algæ was announced for sale by the auctioneer. The bidding was brisk and was still proceeding, when somebody called out; "Why that is much more than the price at which the work was published!" This little incident was of course gratifying in a small way, but I believe that at the time the book was out of print.

My search for freshwater algæ led me to all sorts of low-lying, damp, marshy and wet places, to ditches, dykes, ponds, pools, lakes, reservoirs, rivers, and cascades; indeed wherever water or even humidity was to be found, as damp walls and buildings, within a radius of several miles around Cheshunt. On many of these occasions I became very wet; indeed I seldom returned home without wet feet, as at times it was necessary to go into the water. The result was I suffered from most painful attacks of neuralgia. One of these was so bad that a neighbouring medical man was called in; he prescribed calomel freely and a generally lowering treatment under which I became much worse. I remember on one occasion it was so agonizing, the pain being unendurable, that I rushed frantically out of the house and across the fields till I nearly dropped, but this exertion gave me temporary ease. The treatment was now changed, tonics and plenty of nourishment being substituted; under this regimen as was to be expected, I soon recovered.

DECAY OF LIVING FRUITS AND VEGETABLES CAUSED BY FUNGI.

While I was engaged on the Algæ, my attention was also directed to the subject of the Decay of Fruits and some kinds of Vegetables. Attached to our house at Cheshunt was a large garden, with many fruit trees and a long high brick wall covered with peach, nectarine, apricot and other trained fruit trees; such a garden as is still sometimes found in connection with the older country houses. I had thus abundant materials for my observations.

I soon noticed that most fruits were subject to two very different kinds of decay or destruction; one was the ordinary kind, occurring in over-ripe fruits which had so far passed their maturity that they could scarcely be said to be living, but were to be regarded rather as dead matter and therefore subject to ordinary decomposition and decay. The other kind of decay was of a totally different character. It was observed to attack fruits in different stages of growth and of diverse structure and consistency; some ripe, some green, unripe and immature; some hard, green and fresh; the latter being less prone to suffer than the softer and more mature kinds.

Next it was noticed that this second kind of decay began as a spot only; if the fruit attacked were an apple or a pear, it would be seen that this spot increased in size somewhat rapidly, that is to say in the course of a day or two, or even in a few hours, according to the consistency of the fruit. The spot was usually of a brown colour, which spread as the decay extended.

Between the affected and unaffected portions of the fruit, say an apple, there is always a well marked boundary line, usually of a circular form, the part immediately outside the circle retaining its normal green colour and consistency. This brown or discoloured part is softer than the rest of the fruit and the decay gradually extending, soon involves the whole fruit and reduces it to a state of rottenness.

These several characters and appearances are so unlike those which take place in an ordinary case of decay, that I was led to seek for another explanation. I therefore had recourse to my favourite instrument, the microscope, and proceeded to examine both the sound and the unsound portions of the fruit; the former presented all the minute structural characteristics of sound fruit, in the latter the component cells or utricles, of which succulent fruits mostly consist, were separated and dislocated by the penetration and interlacing of the branched root-like threads or thallus of a species of fungus, the portion of fruit thus attacked becoming thoroughly disorganized and in this way brought under the influence of the ordinary laws of decomposition and decay.

In order to confirm this view and to test the power of the thallus to effect the destruction of many kinds of fruit, I proceeded to innoculate sound recently gathered fruits of several sorts with the thallus and in all cases in which the innoculation had been successfully performed, this peculiar and artificial form of decay ensued.

I next experimented on apples, pears, peaches and several other kinds of fruit, while still growing on the trees; here again the same results followed though somewhat more slowly, especially in those cases in which the consistency was great and the fruit hard.

Many other descriptions of fruits, in fact nearly all I found were subject to a like decay. I may name one in particular, the orange. If we divide an orange thus affected, so that the knife passes through both the sound and the unsounds parts, the difference between the two will be plainly perceptible and if the limited decayed part be cut away, as is often done, the sound part may be eaten with impunity. With a fruit decayed in the ordinary and natural way, this could not be done, because of its disagreeable taste.

But living fruits may be innoculated in a manner other than by the introduction beneath the skin or epidermis of the root-like part or thallus of a fungus. Many fungi of simple organization develope an aerial fructification and produce a multitude of spores which form the blue, green, red and yellow moulds, so often seen on diseased or decayed fruit and other organic substances; these spores are so small and light that a puff of air is sufficient to scatter them far and wide and they are so minute, that they readily enter the fruit through any broken surface.*

The decay of fruits through the growth of fungi within them, is only one instance amongst many, of the destruction of various living organic substances by the same agency. Some time antecedent

* Transactions of Miscroscopical Society 1842.

to the observations and experiments just recorded relative to fruit, my attention was directed to the Potatoe Disease, about which there had been much discussion and many ideas and opinions broached as to its cause, some of them very wide of the mark.

It struck me from what I read and observed at the time, that this disease might be the work of a fungus. There are some points in common between the decay in fruits and in the potatoe tuber, namely; the contrast between the affected and sound parts of the tuber, the marked discolouration and the gradual extension of the mischief from a small spot to the greater part or even the whole of the tuber.

I submitted minute portions and sections of sound and diseased potatoes to the microscope and soon succeeded in demonstrating the presence of the fungus, known as *Peronospora infestans.*

In the case of potatoes the progress of the malady is much slower than in many fruits, owing to the denser nature of the tuber and in consequence the greater resistance presented to the growth of the fungus. It was as far back as 1841 that I published the fact in some of the daily papers and elsewhere, that the potatoe disease was occasioned by the growth in the tuber of the thallus of a fungus and so far as I am aware, I was the first to demonstrate the fact.

But it is not only fruits and tubers which succumb to attacks of fungi; many Vegetables are thus destroyed; to one in particular I will confine these remarks, the Lettuce. This vegetable even when at its full growth, apparently flourishing and in good health sometimes becomes infected, the leaves however still keeping green for a time; if we examine a plant thus attacked we shall find that the stem near the ground is more or less discoloured, softened and decayed; it may be that one side of the stem only is thus affected, while the other is white, firm and sound. On placing a morsel of the unsound stem in the field of the microscope, the branched filaments of the fungus will be clearly recognized. It is not contended of course that these several results are all brought about by the same fungus, since no doubt a considerable variety of analogous species are capable of producing similar effects in the case of many fruits and vegetables.*

Now the foregoing innoculation experiments were at the time and still are, alike of practical importance and scientific interest. They were made as long ago as 1841-2 and taken in conjunction with some further observations of mine to be detailed later on, they are of special significance in relation to the more recent and precise investigations and discoveries which have thrown such a flood of light on the diseases of both the vegetable and animal world, including

* Annals of Natural History, Vols X, XI, XII.

even those of man himself. In the case of fruit, they showed the reasons for the adoption of certain precautions which experience had in part proved to be necessary to its preservation; the avoidance of bruising and injury to the epidermis in gathering and packing, the disposal of the fruit in single layers, the separation of one fruit from the other and finally the speedy and regular removal of the affected from the sound fruit, even when only a small discoloured spot had become visible. Now, not only is it known that such precautions are requisite, but the precise reasons for their adoption are rendered plain and convincing.

MY START IN LIFE.

Of course during the time I was engaged on the natural history enquiries and publications above referred to, the question was raised and considered as to my start in life and how I was to turn my professional qualifications to the purpose of achieving an independence. There was no money forthcoming to buy a practice, and to become an assistant to another medical man was distasteful to me; hitherto I had pursued much my own way, I was free and my own master and I was disinclined to subject myself to the authority of another. At length Dr. Burnes, brother of Sir Alexander Burnes of Indian reputation, became interested in me and gave me an introduction to Sir William Burnett, the then Medical Director General of the Navy and the inventor of Burnett's Disinfecting Liquid. Sir William received me most kindly, directed me to send in my diplomas and certificates and said that I should be appointed naturalist to the "Samarang", under the command of Sir Edward Belcher, a great martinet, and which vessel was about to start on a scientific expedition. I was rejoiced at the prospect, as this appointment seemed the very thing for me. I was a good sailor and had some knowledge of marine productions, which for me possessed a great charm. I was kept in suspense a few days only, when a letter arrived requesting me to send in another certificate for six months Hospital attendance, as one of those already forwarded could not be received, that of the Netterville Hospital and Dispensary, an institution of which Sir James Murray, my uncle, was the founder, owing to its not having the requisite number of beds. This second certificate I was unable to supply and thus the matter fell through, to my great disappointment. This incident shows upon what comparative trifles a man's whole career may depend. On my informing my kind friend Dr. Burnes of what had occurred, he wrote congratulating me on what he termed my "lucky escape". He said the ships command-ed by Sir E. Belcher were nicknamed "hells afloat" and that in all probability I should not have been on blue water an hour before my eyes and limbs would have been, — shall I say, not blessed? This information lessened my regret, as under such provocation, I might have got into trouble.

But the difficulty as to my start in life became settled very shortly afterwards in this way. My brother Richard being about to commence practice in Richmond, there was no longer any necessity for our remaining longer at Cheshunt; it was therefore determined that my father, my sister Eliza and I should remove to some likely new suburb of London, take a small house there and that I should commence practice. This plan was carried out. A corner house was secured in Addison Road North, on the Norland Estate, Notting Hill. Here I succeeded in obtaining a fair general practice and on the death of my father at the age of 73, I removed to a larger villa in the same road.

THE MICROSCOPIC ANATOMY OF THE HUMAN BODY.

As soon as I was settled in my new home, my love of investigation returned in strong force and now recognizing fully from my past experience of the microscope, its great capabilities in the determination and elucidation of the minute structure of all organized animal and vegetable substances, invisible to the unaided sight, I determined this time to apply it to some subject of more professional interest and of wider and more general importance. I therefore commenced the examination of some of the tissues of the human body and being charmed and fascinated with what I saw, resolved to microscopically examine systematically all the fluids, tissues and organs of the body; a task of no common labour, one which would occupy much time, years perhaps and which had not hitherto been accomplished in its entirety; certainly not in this country.

To carry out this design effectually, it was necessary that I should make constant visits to the post-mortem room of a Hospital and should engage a competent artist. I was therefore present at a great many autopsies at St. George's Hospital and I engaged the services of a Mr. Miller, as microscopical draughtsman. He was a most intelligent young man and he rapidly became, aided by his natural ability and constant employment, exceedingly proficient.

In this way I examined all the structures and organs of the body and ultimately resolved to bring out a separate work on the subject.

The results of this application of the microscope culminated in the publication of certain papers in The Lancet in the years 1848-49, the names of which will be found in the List of Publications and in 1852 in the production of a work, in two volumes, entitled: "The Microscopic Anatomy of the Human Body". One volume consisted of text, the other of plates, mostly coloured by hand, or printed in colour, embracing upwards of four hundred illustrations all drawn by Mr. Miller from my preparations; many of these were engraved by him on stone and others by Mr. Lens Aldous and Mr. S. W. Leonard, both very skilful microscopical artists.

The prime purpose of this work was to give faithful illustrations of the minute structures found in the liquids and solids of the body as revealed by the microscope; it did not pretend to trace the origin and development of the several tissues and structures, but was mainly limited to the one purpose named; even with this limitation, looking back over its pages I am conscious it contains some errors and omissions, but the plates are, as a whole, I believe unrivalled even to this day, though forty years have passed since their publication. My work was the first complete book in the English language devoted to this subject and though so long out of print, so far as I am aware no work of similar extent has yet appeared in this country, but German and American editions of my "Microscopic Anatomy" were afterwards published.

SERIOUS ILLNESS AT NOTTING HILL.

I continued to practise at Notting Hill till about the end of 1849, when an event occurred which changed the whole of my future life. My practice was somewhat harassing; the patients were of two classes, well-to-do people, the occupants of good houses and villas, and workmen with their families, living chiefly in the Latymer Road and in the notorious and insanitary Potteries. I had much midwifery and a good deal of night work; besides the locality was bad and did not suit my health, the soil was of clay and the situation low and damp. Walking home from one of the London theatres I got wet through, pleurisy followed, my life was in danger and on my partial recovery, I determined to part with my practice and make a fresh start elsewhere. I ultimately sold the practice to Mr. Hemming, by whom it was continued for many years. Here I may narrate an incident of my illness. Being in great pain from the pleurisy and breathing with difficulty, I summoned my assistant, Mr. Collins, in the middle of the night and directed him to bleed me from the arm. I saw that he was very pale and his hand trembled, probably he was nervous from inexperience, so I took the lancet from him and by the light of a candle bled myself with marked and immediate benefit and relief.

This reminds me of another incident of my practice which may be recorded. I was in attendance on an elderly retired physician; it was a very severe case of erisypelas of the head and face; the attack not progressing favourably, he expressed a wish to see an old friend, a distinguished London surgeon. He ordered a copious bleeding from a full stream and this order was repeated several times, chiefly on the ground that the blood abstracted after being set aside for some time exhibited the contracted or "cupped" surface and the "buffy" coat of which in those days one heard so much. I had my own scruples as to these repeated bleedings, but being by far the junior I felt constrained to obey orders. Formerly bleeding was in many cases no doubt carried too far, now it is not as frequently resorted to as it should be. The relief afforded in suitable cases is immediate and well marked and no equally effective substitute for the practice has yet been devised.

SANITARY CONDITION OF THE POTTERIES.

It was while at Notting Hill that I published a pamphlet on the Sanitary Condition of the Norland District, Shepherd's Bush and the Potteries.*

The Potteries at that time were very notorious and were in close proximity to the best part of the Norland District; the site had formerly been a brickfield, the numerous excavations made never having been filled up and hence there were many collections of foul and stagnant water. The population was of a very low class and included pig keepers, soap boilers, gypsies, tramps, beggars &c.; the place abounded in piggeries and being without drainage, was in so insanitary a condition that it was rife with disease and was a constant danger to the neighbourhood. I drew up a full report on its condition and this contained a curious illustrative map, in which each of the many stagnant pools and ponds, ditches and other sources of danger were delineated. This was my first effort as a Sanitary Reformer; but it was not destined to be my last by many.

At that time my work on microscopic anatomy was practically finished, though it was not published I believe in a separate form till 1852.

REMOVAL TO LONDON.

I removed in 1850 from Notting Hill to Park Street, Grosvenor Square and from thence sometime afterwards to Bennet Street

* Samuel Highley. Fleet Street. 1849.

St. James's, a most convenient and central situation, where I resided for some years; I here fitted up a commodious laboratory, adapted alike for chemical and microscopical research.

I had now a little time to look about me, to reflect upon and determine my future course of action. In my journeyings through some of the streets of London, my attention was often directed to the various articles of consumption and to the explanatory placards exhibited in many shop windows. I was not long in coming to the conclusion, that there was much amiss in the appearance of some of the articles and in the statements made in respect to them, moreover I saw in the newspapers frequent complaints of the bad quality of the ground coffee sold and many doubts expressed as to its genuineness. At length I proceeded to look into the matter myself.

THE ADULTERATION OF FOOD.

It was in 1850 that I first directed my attention to Coffee. I commenced by examining microscopically sections of the whole coffee berry, unroasted and roasted, and then the roasted berry after being ground. In the same way I examined the raw and roasted chicory root. I had now some valuable data to proceed upon. I found that the roasting and partial charing and blackening by no means destroyed the beautiful minute structures and tissues entering into the composition of the coffee berry and chicory root. This being so, I knew that I had already found the key to the detection of a variety of adulterations and admixtures of vegetable substances possessing organisation visible under the microscope and I rejoiced accordingly.

I now purchased a number of samples of roasted coffee at different shops and submitted them to rigid scrutiny and embodied the results in a communication which was forwarded through the then secretary, Mr. Dennes to the Botanical Society of London. These results showed that nearly all the samples were adulterated most extensively in a variety of ways, some consisted of little else than chicory, which was the most frequent adulterant, though in many cases roasted wheat, rye, beans, peas, burnt sugar &c. were also detected in considerable amounts. These spurious admixtures were sold under the most grandiloquent names and with statements absolutely false. An abstract of this communication was sent by the Secretary of the Society to the press and was inserted in most of the newspapers, followed in some cases by leading articles.

Thus encouraged I next commenced the examination of Brown Sugar. As this is a crystalline and not an organized substance, the results were different, but still very interesting. While they showed that the popular statement as to the sanding of sugar was not correct and that the grocer was probably libelled, who is reported to have summoned his assistant from the cellar somewhat after this fashion: "Have you watered the treacle and sanded the sugar?" "yes," "Then come up to prayers"; yet they brought to light the fact that most of the brown sugar of that date which had not been submitted to purification by filtration and a second crystallization, abounded with living and dead acari, louse-like creatures, in all stages of growth and development, from the ova upwards to the perfect parasite and further, that this acarus was the cause of the malady to which grocer's assistants were specially liable, namely, "Grocer's Itch". This paper was also communicated to the Botanical Society and abstracts in like manner were inserted in the journals.

I come now to mention a very curious episode in connection with the adulteration of coffee. So numerous and persistent were the complaints of the adulteration of that article, that a Chemical Commission was appointed by the Government to examine and report upon this subject. In due course their report was furnished and in reply to enquiries by certain members of the House of Commons, the then Chancellor of the Exchequer, Sir Charles Wood said, "I hold in my hand the report of three of the most distinguished chemists of the day who state that neither by chemistry nor in any other way, can the admixture of coffee with chicory be detected." When the above announcement was made, an abstract of my communication to the Botanical Society had already appeared and of course no time was lost in contradicting this statement and in affirming that nothing was more simple and certain than the detection of the admixture in question by means of the microscope.

The next event to record was the receipt of a summons from the late Mr. Thomas Wakley, the Founder and Editor of The Lancet and then M. P. for Finsbury. He said "I have observed what you have been doing, but you will never effect any lasting good until you are able to publish the names and addresses of the parties of whom the articles were purchased, giving the results of the examination in all cases whether good or bad. Do you think it would be possible to do this without an amount of risk which might be ruinous?" I replied "yes, I believe it might be done. Of course the utmost care, caution and scrupulous exactitude would be necessary." Then Mr. Wakley asked me to put my views in writing and to draw up a definite and working scheme. This after a little time I did and it was ultimately arranged, that a series of articles on the adulteration of food should be regularly published in The Lancet under the title devised by Mr.

Wakley of "The Analytical Sanitary Commission", these articles to be illustrated, each to contain a number of analyses of samples actually purchased in London in the ordinary way of business, the names and addresses of those from whom the articles were obtained being given in full in all cases, whether the samples were genuine or adulterated.

In the ordinary sense there was no commission; every thing was in my hands, the purchase of the samples, the composition and writing of the reports, their order and selection; while for the drawings and illustrations I employed Mr. Miller the artist, to whom reference has been already made and who was educated and exclusively employed by me for many years and up to the period of his death. No one was associated with me in this work, but I was at liberty to consult and to obtain the help of any others I might select as occasion might require. My name was not to appear at first, but I expressly retained the absolute right to publish at a subsequent time at my own expense the whole of the Reports in my own name. These particulars are now matters of ancient history, but it is as well that they should be here repeated, as the details of events long passed are apt to be forgotten or only imperfectly remembered.

It is obvious that such an undertaking involved considerable risk on both sides. The risk to Mr. Wakley as the proprietor of The Lancet, was very great: there was the serious risk of being involved in grave litigation and possibly of heavy, if not ruinous costs. On my part I risked all I possessed; namely; my scientific and professional reputation. I was taking upon myself an amount of labour and responsibility under which many a man would have broken down; had I failed in accuracy and had I not been able to safeguard Mr. Wakley, he and The Lancet would have been seriously damaged, while my scientific career would have come to a sudden and disastrous ending.

The exact mode of proceeding adopted in the case of each Report was as follows: selecting some suitable locality Mr. Miller and I used to sally out from time to time, usually in the evening, often on Saturday nights, in all weathers and at all seasons of the year; we were provided with a bag to receive the samples, paper and ink. Sometimes we entered the shop together, but more often I told Mr. Miller what to buy and he made the actual purchase, while I was watching closely all that took place, so that I might be, if needed, a competent witness. On leaving the shop, the name of the vendor, the date and cost of the purchase, together with our initials were at once inscribed in ink on the wrappers of the packages and it was the fixed rule, that no second purchase should ever be made until all these formalities had been carried out.

Now these nocturnal excursions brought us into many curious parts of London and gave us a wonderful insight into the habits and ways of life of the people in the poorer districts: in summer they were pleasant and interesting enough, but in winter most trying and wearisome, waiting and hanging about we became chilled to the bone, sometimes not arriving home till near midnight.

The next morning betimes, the samples were duly arranged and classified and their examination commenced, a series of samples of the same article being taken for each report. But before any satisfactory examination of the samples could be made, it was necessary that the structure and microscopical characters of the vegetable substances themselves in their pure state, both as a whole and when ground and reduced to powder, should be studied and delineated; that they should be submitted if necessary to chemical analysis and that the probable adulterants of each article should be in succession subjected to similar scrutiny and analysis. Thus it was, that the foundation was laid, on which the determination of the question of the purity or otherwise of the articles to be reported upon, could be safely and surely based.

The composition of the series of samples having now been determined, the illustrative figures drawn with the aid of the camera lucida, and the wood engravings prepared, the next and final proceeding was to write the report.

All this labour was gone through regularly, often week after week for the first two years and afterwards for another two years less frequently. The first report was published in The Lancet in January 1851; others quickly followed and were continued to December 1854. During this period upwards of 2500 samples of food were examined and reported upon, with, in all cases the names and addresses of the vendors, retail and wholesale. The reports embraced all the principal articles of consumption, both solids and liquids, as well as some other articles as, tobacco and snuff.

Their publication was of course not unattended with some remonstrances, a few lawyer's letters were received and in one or two cases actions were commenced, but one only went as far as the delivery of the declaration. This circumstance alone sufficiently establishes the fact of the remarkable accuracy of the scientific work of these reports. One incident may here be alluded to in proof of the exactness and power of the microscope in detecting many admixtures and adulterations: a mustard manufacturer sent The Lancet a sample of what he described as genuine mustard, an article, which at that date did not exist, all mustard then manufactured consisting of a mixture of mustard, wheat flour and turmeric, the flour greatly predominating in inferior samples. I examined this sample and in the next number of The Lancet the following notice appeared. "Our correspondent is deceived, the article he has sent us is not a specimen of pure brown mustard, as it contains a small quantity of turmeric." This called forth a rejoinder from the sender, who acknowledging the correctness of the statement, affirmed that the small addition of turmeric, 1 in 448 parts, was made simply to improve the colour.

It was to the microscope, that the success of these investigations was due; but for this novel application of that instrument, the multitudinous adulterations practised on nearly every article of consumption could never have been discovered and exposed. The microscope therefore was the great and chief means of detection. Chemistry rendered doubtless important aid; it was capable of detecting most of the chemical adulterations, but it was powerless to discover the innumerable organic admixtures to which most articles of consumption were subjected and which constituted the great majority of the adulterations which were rife as the period to which I refer.

The only time in the course of the investigations when I had occasion to consult any microscopist, was in the case of a threatened action, which after some correspondence between the lawyers on both sides, went as far as the delivery of a declaration; in this case I sent portions of the disputed sample to Mr. Bowerbank and Mr. Edwin Quickett, for the purpose of corroboration and without giving any particulars; from both of them in due course I received confirmatory testimony and their evidence would have been forthcoming, had the case gone into Court.

The general result of these enquiries and investigations showed that the adulteration of articles of consumption had been reduced to a system, to an art, and almost to a science; that it was universally practised; that adulteration was the rule and purity the exception, that everything that could be cheapened by admixture was so and that the articles thus debased were sold as genuine and often under the most high-flown names and with assertions of unblushing falsehood.

It must not be supposed, that my scientific work in connection with the detection of adulteration of food and drink was limited to the application and employment of the microscope; such a conclusion would be very erroneous and unjust. I also made use of Chemistry to a large extent, as in the case of bread for alum, of powdered chicory and cocoa for reddle and other ferruginous substances; of cayenne for iron, lead and mercury; of pickles, bottled fruits and vegetables for copper, of sauces, potted meats and fish for Venetian red, Bole Armenian and lead; of sugar confectionery for the many poisonous metallic pigments employed in their colouration, including various preparations of iron, lead, mercury and arsenic; of spirits, wines and beers for salt, cayenne and sulphate of iron, as also for strychnine, the search for which in itself was no little or easy matter, and so on.

It was in the analysis of tobacco, snuff and opium that I chiefly consulted and employed Dr. Letheby, the analysis of these being more complicated and difficult; but these articles, it will be observed are drugs rather than foods. I refer to this matter, because of some misapprehension which existed and an exaggerated claim which was put forward some years since; this however was disposed of by the production of the original letters and account. See "Adulterations in Food and Medicine". Longmans 1857.

In September 1855 I read a paper at the Meeting of the British Association in Glasgow "On the chemistry of the Adulteration of Food". In this the chemical sophistications were described and the chemical aspects of the subject dealt with more particularly; the dangerous and poisonous character of many of the substances employed being specially dwelt upon. It was at this meeting that the important discussion took place between Professor, afterwards Baron Liebig and Dr. Gilbert. The Baron taught and showed, that the fertility and productivness of a soil depended very much on the mineral constituents contained in it, that these varied in different vegetable foods and feeding plants and hence that the land should be dressed in each case with the special mineral substance or salt it needed. This view, founded on actual analysis, was no doubt in the main correct; it was however contraverted and questioned in some particulars.

The discussion was most interesting: it was carried on with very great spirit and ability on both sides, but the impression created was, that the practical agriculturalist carried off the honours of the day.

In the same year, 1855, I gave evidence before the Parliamentary Committee on Public Houses of which the Rt. Hon. C. P. Villiers M. P. was the very able and earnest chairman, a veteran reformer of more than half a century. That evidence of course had reference to the adulteration of beer and other malt beverages and of the various kinds of spirits sold, including gin and brandy, which in those days were so often rendered fiery and burning, to the destruction of the stomach and liver, by the addition of cayenne and sulphuric acid.

THE ADULTERATION OF DRUGS AND MEDICINES.

Further, my investigations were not by any means confined to the adulteration of articles of food and drink, but they extended to Drugs, under which head, tobacco and snuff were included; thus not only were articles successively prepared on these but also on opium, scammony, jalep, rhubarb, colocynth, ipecacuanha, liquorice, annatto, and sweet spirits of nitre. The reports on these did not form any part of the food enquiry, though some of them appeared in The Lancet, while others were published for the first time in the two editions of "Adulterations detected in Food and Medicine" brought out me in 1857 and 1861. Again, other articles on adulteration appeared in a still later work of mine: "Food; its Adulterations and the Methods for their Detection". In many of the newer processes given in the last work I had the help of Mr. Otto Hehner F.C.S. In the preparation of the articles on drugs the same method of enquiry and investigation as in the reports on food was pursued; the vegetable substance or drug itself was first studied and its structural peculiarities determined; then drawings and engravings were made in illustration of that structure, as also of some of its principal adulterants.

It is well known that there were at the time some scientific men, analytical chemists and pharmaceutists, who took a lenient view of certain admixtures and adulterations. Professor Redwood of the Pharmaceutical Society of Great Britain used very properly to draw a distinction between impurities and adulterations, while some actual adulterations he regarded as simply conventional. But these views were carried to an excessive length. On one occasion at a meeting of the Pharmaceutical Society at which I was present, Mr. Redwood denied that annatto, a vegetable dye, much used to colour milk, cheese and other articles, was adulterated.

This denial surprised me and I there and then offered to read a paper on the subject at an early meeting of the Society, in which I would undertake to prove it was adulterated. The offer was accepted. In the communication in question I produced overwhelming evidence that annatto was most grossly adulterated; in fact I proved that there was scarcely another article in the whole range of food and medicine, which was sophisticated and debased, both in kind and quantity, to the same extent.

At the reading of the paper I found that, the challenge having become known, the rooms of the Society and even the staircase were densely crowded. Mr. Redwood made a clever and fluent defence, but it was that of a sophist. He urged pleas, rather than reasons, for several of the practices described; either that they were improvements or imparted qualities which otherwise the article would not possess; but the more flagrant adulterations he simply ignored. While the oratorical honours were his, the facts were mine. The paper itself describing the adulterations of this article will be found in the Pharmaceutical Journal of 1855-6.

When we consider that many of the articles reported upon, as tea, coffee, cocoa, wines, spirits, tobacco; &c. were exciseable; and that from the duties levied thereon a large revenue was derived, and that a special scientific Board was in existence for the detection of their adulteration, furnished with a laboratory provided with miscroscopes and every needful requirement, it is indeed extraordinary that the state of things described should so long have been permitted to exist. The sums which were lost to the Revenue in past years must have been enormous. It is right to state however, that the incompetence which then prevailed has, since the exposures were made nearly ceased to exist and the present authorities are now for the most part equal to the requirements of their position. One circumstance may here be referred to in excuse for the lamentable deficiency of the Excise Chemists of that time; namely, that there were no microscopists attached to the Board possessing the requisite knowledge and skill and it may be further urged it was no part of their duty to ascertain whether non-exciseable articles were pure or not; nor again was it their duty, even in the case of exciseable commodities, to determine whether any of the adulterations they discovered were injurious to health. In the two last particulars their sphere of action afforded no protection and still fails to afford any to the public. Their position was and still chiefly is, to protect the revenue.

THE ANALYTICAL SANITARY COMMISSION.

The Lancet reports as they appeared one after the other, were quoted and commented upon by nearly the whole of the scientific journals and the daily press; notices also frequently found their way into the foreign papers and the attention and interest which the revelations excited were deep and extended. In the case of coffee and some other articles, even the wholesale market prices were affected by the exposure. The startling effect produced by the revelations made was thus graphically and happily described in the "Quarterly Review" for March 1855 in an article on my work entitled "Food and its Adulterations". "A gun suddenly fired into a rookery could not cause a greater commotion than did this publication of the names of dishonest tradesmen, nor does the daylight, when you lift a stone, startle ugly and loathsome things more quickly than the pencil of light streaming through a quarter inch lens, surprised in their native ugliness the thousand and one illegal substances which enter more or less into every description of food which it will pay to adulterate. Nay, to such a pitch of refinement has the art of fabrication of alimentary substances reached, that the very articles used to adulterate are themselves adulterated; and while one tradesman is picking the pockets of his customers, a still more cunning rogue is, unknown to himself, deep in his own".

The labour attending the preparation of the Reports of the Analytical Commission and the anxiety and responsibility entailed thereby weighed heavily on me and my health suffered greatly. Mr. Wakley observing this, said in a jocular way on one occasion that he would give me "just six weeks to live". But the reports entailed also a very grave and serious responsibility on Mr. Wakley; they might have involved heavy pecuniary loss and lasting injury to the reputation of the Journal. Fortunately no such results followed, but as was natural they brought additional fame. The Lancet's circulation and advertisement department were greatly increased, a very just and well earned reward; and here it is well, that I should quote some sentences from my introduction to "Food and its Adulterations", which in great part consisted of a reprint of the Reports of the Commission. "It is quite impossible to speak in too high terms of the great moral courage evinced by Mr. Wakley in his determination to publish in all cases the results of the investigations and to give to the world the names and addresses of all the persons concerned. The responsibility incurred was immense and had the confidence reposed not been justified and had not the greatest thought and caution been exercised most disastrous would have been the consequences. Great therefore is the debt of the public to Mr. Wakley in this matter. The author fully believes that there is not another editor to be found who would have ventured upon so bold and so unprecedented a step. Lastly, the author desires it should be known that in conducting these enquiries he possessed the greatest possible freedom of action; that Mr. Wakley never interfered with him in the slightest manner; no suggestions were at any time made as to the neighbourhood in which the purchases should be made and that in no single instance, since the enquiries began, did Mr. Wakley suggest that any one name should be either added to or omitted from the lists that were published. In fact nothing could be more strictly impartial and disinterested than the conduct of Mr. Wakley throughout, in reference to the publication of these Reports".

PRESENTATION OF A TESTIMONIAL.

In the early part of the year 1856, it occurred to some well wishers, who were cognisant of and appreciated what I had done, to present me with a testimonial. Accordingly, on the 15th May 1856 at a public dinner at the Freemason's Tavern a very elegant statuette, a real work of art, weighing some 400 ounces was presented to me. The figure represents the Angel Ithuriel, clad in armour, touching with his spear Satan, who having assumed the shape of a toad, sits close by the ear of Eve tempting her. The subject is taken from Milton's Paradise Lost and the pedestal bears these words from the same source:

"Him thus intent Ithuriel with his spear
Touched lightly: for no falsehood can endure
Touch of celestial temper; but returns
Of force to its own likeness: up he starts
Discovered and surprised."

Placed underneath is the following inscription: "In recognition of Public Benefits conferred by his Rare Scientific Skill and Indefatigable labour in the Detection and Exposure of a Pernicious and Systematic Adulteration of Food and Drink".

Lord William Lennox, in the absence through illness of Viscount Ebrington M. P. occupied the Chair; Mr. Wakley was present and of course spoke on the occasion. He remarked, it was well known that there had been a difference between Dr. Hassall and himself, but turning to the ladies, who where present on the dais, he said "it was a mere lover's quarrel and that we are now greater friends than ever" and he proceeded to express his full satisfaction and approbation of the manner in which I had performed my scientific part of the work.

On my part I bore my testimony and this in all truthfulness, to the entire conscientiousness and uprightness which throughout these reports characterized Mr. Wakley's action. Never as I have already stated, did he suggest the insertion in the reports of a single name, or the omission of one therefrom, nor did he ever screen any offender.

That Mr. Wakley should at one time have felt some disappointment, is not surprising under the circumstances. His part in the publication of the reports and his unexampled courage in the promulgation of all the names and addresses of the vendors of the articles examined, whether genuine or adulterated, and they were mostly the latter, he rightly thought well deserved some special public recognition. The sole merit of this daring proceeding rested with Mr. Wakley; my claim was of a totally different kind; on myself rested the actual work of the Commission, the scientific methods pursued and the results attained thereby. The two claims had but little in common; there was no need of the slightest antagonism between Mr. Wakley and myself. For a long series of years, I was on the staff of The Lancet, both before and after the publication of the reports; during this period, with the single temporary estrangement to which allusion has been made, the most kindly feeling prevailed and continued up to Mr. Wakley's departure for Madeira. A similar feeling existed on the part of Dr. James Wakley, who succeeded his father in the Editorship of The Lancet. In proof of this the following paragraph may be quoted from the obituary notice which appeared on his death in that journal in 1886. "He (Dr. Wakley) was also anxious, that tribute should be borne to the work done by Dr. Arthur Hill Hassall when The Lancet exposed the adulteration of the food of the people."

Mr. Wakley in an editorial article in The Lancet in 1854 at the time when my labours were approaching their termination, thus expressed himself: "We consider the time has now come, when the name of Dr. Arthur Hassall should be mentionned, on whom these enquiries have almost exclusively devolved and to whom belongs the credit of having brought to light practices in relation to the adulteration of food of the highest importance and of the extent and nature of which no one previously entertained any adequate conception. It is impossible to over estimate the importance of these labours either in a pecuniary or sanitary point of view, both as regards the public and the medical profession. To Dr. Hassall then we would say, belongs the merit of having established in this country a new and distinct department of public hygiene", and again in his evidence given before the Parliamentary Committee on Adulteration, Mr. Wakley bore the following testimony; "I think I ought to say, that the investigations must have been made with the utmost care and the greatest ability by Dr. Hassall and the gentlemen whom he consulted, or it is quite impossible I could have escaped ruin, but I had the greatest reliance on his integrity and I must say I had no reason whatever to mistrust him in any act he did. On the contrary I believe the whole of the enquiries were carried out with the greatest possible fidelity and all the purchases and examinations made with the most strict integrity".

Then a little later, on the republication of the Reports under the title of "Food and its Adulterations", The Lancet in reviewing this work, wrote as follows: "But deeply impressed as we are with immense importance to the public health of the researches, the results of which are brought together in the book before us, we feel at the same time, that we are relieved from the task of entering at any length into a description of its object and contents which are widely known to the scientific and commercial world or praising that which has already received the meed of universal approbation."

"It is however but a tribute of justice to the extraordinary scientific merit and energy of the author, that we, leaving to others, as we may now fairly do, on the occasion of the separate publication of these admirable reports, to comment upon the part we have borne in this production, should express our opinion of the general merits of the work."

"It is the great and original merit of Dr. Hassall to have applied the microscope to important uses in inquiries of this nature and to have shewn by its use, not only many things previously considered impossible to shew, but many things not previously suspected to exist."

In an able article on the whole subject of adulteration, which appeared in the "Quarterly Review" based on the same work, the following remarks appeared. "It is in the application of the microscope, that consists Dr. Hassall's advantage over all previous investigators in the same field. The precision with which he is enabled to state the results of his labours leaves no appeal."

"We cannot avoid stating, that the community is under the greatest obligation to both Dr. Hassall and the Editor of The Lancet; to the one for the energy with which he pursued his subject and to the other for his singular boldness in rendering himself liable for the many actions which the publication of the names of evil doers was likely to bring upon his journal, a liability which Dr. Hassall has since taken upon himself by the reprint of the Reports under his own name."

The "Dublin Review" wrote as follows: "The secret of Dr. Hassall's success has been, that in addition to chemical analysis, he has used the microscope in his enquiries; and his merit not only consists in the able manner in which he has employed the instrument, but in his being the first to use it practically and to such an extent for this purpose."

These quotations are here introduced, not from any feeling of vanity, but to show the opinion entertained of the importance of the investigations undertaken and that the confidence reposed in my ability to carry them out to a successful issue, was not misplaced.

The extended investigations made then, not merely proved that a wide spread, ingenious and nefarious system of adulteration prevailed in nearly every article of consumption; that the solid articles were mixed with cheap materials to add to their bulk and weight; that the liquids were diluted with water to increase their measure; that foreign colouring matters were added to hide the impoverishment and dilution thus occasioned and that the articles thus debased were sold as genuine, but the investigations accomplished much more.

Thus while they exposed the evil, they went far to provide the remedy; they supplied new methods for the discovery of adulteration and they effected so great a reduction in the practice that soon it became difficult to obtain a sufficient number of adulterated samples of any one article to make the investigations and reports any longer of the same interest as before.

PARLIAMENTARY COMMITTEE OF INQUIRY.

In the end, the disclosures made led to the appointment of a Parliamentary Committee of Inquiry in 1854 and this was followed by legislation. Of the Committee, Mr. Scholefield M.P. for Birmingham was the Chairman, than whom there could not have been a better. The enquiry was of a searching and practical character. Of course, I was examined, indeed one of the chief objects of the Committee was to test the accuracy of the statements contained in the reports of The Lancet Commission. A great number and variety of witnesses were called, amongst whom were some who were known to take a very lenient view of adulteration and who were ready to urge pleas on behalf of some of the practices revealed, manufacturers, merchants and traders in nearly every article. After myself, one of the first witnesses examined was Mr. Thomas Blackwell of the well known firm of Crosse and Blackwell, and his evidence produced a great effect on the Committee, it was so straightforward; he acknowledged frankly, that the practices I had described as to the greening with copper of many jams, preserved fruits and vegetables, the colouring of red sauces and potted meats with bole Armenian and other ferruginous substances were common and that their firm had themselves adopted them to some extent, not knowing they were so objectionable, and they being at the time almost universal.

In this way a mass of very valuable information was obtained, the result of which not only established the fact of the great prevalence of adulteration and confirmed the statements advanced by me as to many specific practices, but the inquiry brought to light many other adulterations of food in addition to those I have enumerated. Again, the Committee did not confine its enquiries to food only, but extended them to medicines. On this subject I also gave a good deal of evidence, founded on my own investigations, while much information was furnished by other witnesses, some being wholesale druggists, scientific and pharmaceutical chemists.

Mr. Scholefield was an admirable, earnest and hardworking Chairman, whose early and unexpected death was a great loss. The report of the Committee was an able document: it stated, that "the Committee cannot avoid the conclusion that adulteration widely prevails and that not only is the public health thus exposed to danger and pecuniary fraud committed on the whole community, but the public morality is tainted and the high commercial character of the country seriously lowered both at home and in the eyes of foreign countries."

The Times commenting on the evidence given before the Committee and on the prevalence of systematic adulteration in articles of daily use and of the first necessity wrote thus: "But how, the reader may ask, has the discovery at this particular period been made or certified? Partly through material improvements effected in the means of detection, but mainly by the skill and perseverance of Dr. Hassall, who by devoting to this subject the energies of a scientific mind and pursuing it with that steady zeal which its importance justified, has thus become a public benefactor of no common order. If gratitude is due to those who discover antidotes to disease, or invent appliances for relieving pain, the same obligation must undoubtedly be admitted to the man whose researches, by detecting the hidden seeds of sickness, must directly tend to prolong life and increase its comforts."

CONSEQUENT LEGISLATION.

In due course, as the outcome of the exposures made and of the evidence given before this Committee, the draft of a Bill was prepared and a measure ultimately passed the House of Commons, having for its objects the suppression of adulteration and the punishment of the adulterator.

The measure, though valuable, proved not so effective as it should have been and after some years experience, a second Act was passed to remedy some of the short comings of the first Bill.

It is now rendered imperative, that mixed articles should be labelled as mixtures, but it is not stipulated that the names of the several ingredients entering into the composition of the mixtures should be stated, nor is it required that the proportions of the ingredients should be given. In any further legislation on the adulteration of alimentary substances these omissions and some other defects will have to be remedied. Till this is done mixtures will still be sold either as genuine articles, or mixed to such an extent, as in the case of coffee, cocoa and mustard, that even if labelled as mixtures, the proportions of the added ingredients will in many cases, be far in excess of the articles under the names of which they are sold. Excluding mixtures, the articles now most frequently adulterated are, coffee with chicory, butter with margarine, milk, beer and spirits with water. The beer of other days, the product solely of malt, hops and pure water is now seldom met with; it consists for the most part of a compound infusion, the spirit being derived, not from malt, but from various saccharine materials specially manufactured for the purpose, and the bitter flavour, from quassia, gentian and other substances euphoniously and pleasantly called "Hop substitutes". About the substitution there can be no doubt.

It will also be requisite, that for certain articles of consumption, as milk and all spirituous liquids particularly, there should be authorized and "legal standards" of purity.

The punishments for adulteration now in force are either not severe enough, or they are not properly enforced. One of the most effective penalties provided by the first Act, that of the 6th August 1860, is the publication for second convictions, of the name and address of the offender "in such newspaper or in such other manner as to such justices shall seem desirable." This penalty is however rarely enforced in this country; if it had been, there would have been a still greater reduction in the number of cases of adulteration.

The passing of Acts of Parliament against adulteration and the institution of penalties for the infringement of such acts, entailed the necessity for the appointment of Analysts, skilled in the examination and analysis of articles of food. As the Adulteration Acts were the direct result of the investigations in The Lancet, so the appointment of a body of analysts was another result. Soon, the food analysts, already a numerous body, found it adviseable to enrol themselves into a society for the more effective study of the subject, for comparison of results and for mutual protection. Of this Society I was appointed, naturally enough perhaps, the first President, but here I have to acknowledge I was a very bad and neglectful president, for I never once took the chair during the year of office. How this came about I cannot fully remember or explain. For one thing I looked upon the appointment as merely honorary and complimentary and intended as a recognition of what I had done in the past; then I scarcely regarded myself as a permanent and professional analyst. I became one rather by accident than design, my profession having always been that of medicine.

MEDICAL PRACTICE.

I began to practise as early as 1842; I have continued to do so during a long life and am thankful to be enabled to state that I am still daily occupied in the discharge of the practical duties of a physician. Of course, for a period, my connection with the subject of adulteration brought me much special analytical work, but I still continued to practise all the same.

QUALITY OF THE WATER SUPPLIED TO LONDON.

On coming to reside in London my attention was early directed to the quality of the *water* used for drinking purposes and especially of that supplied to a city numbering its inhabitants by millions; this subject, was much discussed and considered at that time, as it is at the present after the lapse of more than forty years.

The quality of a drinking water is usually determined by the amounts of mineral and organic matters which it contains. Both these are liable to vary considerably in different waters, and even in the same water under certain circumstances. The mineral matters in an Imperial gallon of 70,000 grains may range from almost nothing, as in Loch Katrine water, to some twenty grains as in Thames water, fifty grains in some hard chalk and still more in some polluted waters. The organic matter in like manner may vary in the same quantity of water, namely one gallon, from a trace only, to one, two or even more grains in very bad waters.

But it must be borne in mind, that the wholesomeness or even the safety of a water cannot be determined by the mere quantity of the organic matter present; since there are cases in which the amount is almost infinitesimal and yet the water has proved to be highly dangerous and death-dealing in its effects. It is not so much the quantity, as the quality and nature of the organic matter which constitute the danger; waters when contaminated with such a mere trace of the excreta of certain diseases, as typhoid fever, and cholera as to escape detection by chemistry and sometimes even by the microscope, may still be capable of giving rise to these diseases in persons who partake of those waters. Hence the practical lesson and conclusion is, where there is any reason for doubt, not to trust implicitly to even the most complete examination and analysis of water, but to insist that it shall be obtained from the purest possible source. Chemical analysis does indeed afford much valuable information, the microscope still more, and the results obtained by these means often serve as useful and frequently sufficient guides as to the purity or otherwise of a drinking water; at the same time all the circumstances regarding any supply must be taken into consideration.

Although waters cannot be pronounced absolutely safe to drink because of the small amount of organic matter present, yet when this is in considerable excess, such waters must be condemned, since this excess can ouly proceed from a contaminated source and hence the history of the water becomes necessary.

London and its vicinity were formerly supplied by ten water companies; five of these supplied water from the Thames, and the other five from the River Lea, the Ravensbourne, the New River, the Hampstead ponds, and deep wells in the chalk near Plumstead, respectively.

The names of the Companies drawing water from the Thames were, The Grand Junction, The West Middlesex, The Chelsea, The Southwark and Vauxhall and The Lambeth; from the Lea, The East London, and from the Ravensbourne, a small stream near Bromley, The Kent Company.

Of the five Companies which took their supplies from the river Thames the intake of one was at Kew Bridge, one at Hammersmith, two at Battersea and one at Lambeth, that is to say from the most polluted portions of the river.

The River Lea, like the Thames, was subject to great pollution; the Ravensbourne was also very impure; the impurity of the Hampstead ponds was very considerable, though somewhat different in kind. The New River can scarcely be called a river at all, but is rather an open canal, having its source in certain springs near Hertford, but consisting in part of river water and running for many miles uncovered and exposed to light and air, these being the conditions which most promote the growth and development especially of the lower orders of vegetable and animal organisms: still the water supplied by this company was superior and in marked contrast to that of the Thames Companies. The water from the deep wells near Plumstead contained much carbonate of lime and hence it was very hard, but it was very free from the more serious organic impurities, while the hardness was in great part removed by means of Clark's softening process.

In the case of all these companies the water was turned on each day for a short time only; this is called the *intermittent system* and it necessitates the storage of the water in cisterns or other receptacles.

Against the intermittent system of supply serious objections exist. For the greater part of every twenty four hours the distributing house pipes contain no water and since a vacuum cannot be maintained in these, they become filled with any gas which is able to find an entrance; this may be simply air, but it sometimes consists of sewer gas, or coal gas, or emanations from the closets, these gases being in part discharged into the houses on the return of the water each day; but an equally serious objection is the necessity which it creates for the separate storage of water in the cistern of each tenement. By *the constant system* these disadvantages and dangers are obviated. The pipes are always kept full, so that gas cannot find an entrance, water can be obtained at all hours and there is no need of cisterns or other receptacles.

From what has been stated as to the sources of supply, it is apparent that the water of all the companies derived from the Thames, the Lea, the Ravensbourne and the Hampstead ponds at the period referred to could not be otherwise than thoroughly bad, through not equally so.

It was assumed, that the water as delivered by the companies to the consumers, had undergone an efficient process of purification, either by *precipitation* in special reservoirs, or by *filtration*, or by both processes combined; we shall see presently how far the purification extended.

So imperfectly were the processes of subsidence and filtration carried out, that the water of most of the companies, as delivered for use and consumption was more or less opalescent and discoloured and when set aside for some hours, a deposit visible to the naked eye and sometimes considerable in amount, consisting of mixed organic and mineral matter, always subsided. In the case of the East London Company it was a frequent practice of the occupants of the houses, to tie over the draw-off pipes, pieces of muslin, rag, stockings, or any material at hand capable of acting as a rough kind of filter.

Then the cisterns for the storage of the water were often placed in most inaccessible situations, so as to render cleaning almost an impossibility, some even near water closets and thus the water became liable to further contamination. Again, in the frequent absence of cisterns, vessels of all kinds were used as receptacles, as barrels, tubs, in fact anything which would hold water; the cisterns themselves were rarely cleansed, sometimes never; they were frequently unprovided with covers and exposed to light, air and the sun, indeed to the very conditions best calculated to promote the growth of the living productions which they contained in such abundance. Hence in inspecting cisterns to ascertain their condition, it was no uncommon thing to find that large quantities of confervæ as well as hosts of infusoria, many water fleas and minute worms were present in them; indeed I have met with organisms in cisterns which I have not encountered elsewhere; one in particular, a very beautiful diatom, on which I long since bestowed the name of *Asterionella formosa*. In a few cases zoophytes have been found in both cisterns and the supply pipes, even to the extent of almost blocking up the latter.

The cistern being rarely cleaned out, the impurities contained in the water, as this was laid on daily, gradually accumulated.

Then the action of the sun raised the temperature of the water and expelled its contained air, thus causing it to lose its refreshing coolness and become mawkish and insipid.

THE MICROSCOPE IN THE EXAMINATION OF WATER.

Such briefly, was the condition of things which existed when I commenced my investigations. At that time in most chemical analyses of water the organic matter was included under such words and expressions as these: "traces", "amount inconsiderable", "rather much", "very much" and even in the instances in which it was quantatively determined, little or no indication of its composition was given; indeed chemistry at that period was not able to deal effectively with this subject: I am now referring to a time dating back fully forty years. A little later on, the nitrogen and carbon in the organic matter were more frequently estimated, as also the amounts of the nitric and nitrous acids, if present, and the ammonia, but the determination of these particulars involved and still require the expenditure of much time and not a little money. No precise information was given as to the kind of organic matter, whether vegetable or animal, dead or living; if vegetable, of its exact nature; if animal, of what exactly did this consist, all points of the greatest consequence in the analysis of water and in the determination of the question of its wholesomeness and even safety. It was in this dearth of definite and necessary information that my opportunity presented itself. I felt sure that much valuable information would result from the application of the microscope to the determination of the composition of the various kinds of organic matters contained in waters used for drinking, culinary and other domestic purposes.

My examinations were directed first to the condition of the several waters furnished to the inhabitants of London as obtained from the sources of supply. Samples of the water of the Thames were taken in the course of the river from Kew in the one direction, to London Bridge in the other, also from the mains of the different companies and from the supply pipes and cisterns of various houses supplied by each of the companies.

A certain quantity of each of the samples was set aside in suitable vessels to allow any solid matter which they might contain to subside; after some hours these residues were subjected to a searching microscopical examination. The Thames waters of that period were all, whether taken from the river itself, or from the supply pipes or from the cisterns of the houses, found to contain much organic matter in a variety of forms and conditions.

The "traces" of the chemists were resolved into dead and living organic matter, both vegetable and animal; the dead vegetable was also divisible into two kinds, one being derived from the plants and other aquatic productions contained in the water more or less broken up and decayed as the vessels, cells and other vegetable tissues entering into the structure of the plants; the other and the more significant kind of vegetable matter obviously had its source in the sewage poured into the river: the living vegetable organisms were many and varied; they consisted of numerous examples, some dead but the majority living, of the diatomaceæ, which have been already described in another part of this narrative, of a great variety of freshwater algæ or confervæ and of many threads and sporules of fungi; the living animal contents also abounded and were of many kinds and forms especially animalcules or infusoria, many of which were in active movememt darting across the field of vision of the microscope with great rapidity. Many hundred figures would be required to portray all the forms observed; while to describe them a volume would scarcely suffice; but among the animal organisms found in most samples of Thames water were several species of Annelidæ or worms and their ova, many Entomostraceæ or water fleas with their ova and amongst the small organisms various bacilli, either single or in groups, but which in those days were not descriminated and studied to the extent they now are and the real importance of which was completely overlooked. However I did not fail to perceive them and to record their presence.

Now it should be clearly understood that the presence of living organisms in water is by no means necessary or constant; when found, they are to be regarded as evidences of impurity and as showing that the water contains constituents on which they can feed and grow; this material consists for the most part of organic matter in solution and this is usually present in proportion to the impurity of the water. Thus living productions are not contained in the very purest and best waters, as in the water of Artesian and other deep wells and springs; in the water of shallow wells, if this be exposed to light and air, they will be found, but if those agencies are excluded they will for the most part be absent: it is in river, reservoir, and pond waters that they most abound, but from all these waters they may be almost entirely removed by efficient filtration.

It is astonishing what a change *efficient filtration on a large scale,* such as that now practised by most water companies, effects in the condition of even a very bad and especially a river water; it removes nearly the whole of the matters in suspension in the water and even lessens the amount of that which is in solution. The mechanical and other contrivances and conditions required to bring about this result, are of a very costly description. Furthermore they do not afford absolute security at any time. The organisms not wholly abstracted by filtration are just those from which danger is to be apprehended, the vibriones, baccilli and other allied living entities and their germs. Hence filtration even when carried out in a very complete manner does not afford anything like absolute protection. That this is so, is well shown by the cultivation proceedings which are now usually resorted to in the examination of waters used for drinking with a view to determine their purity; thus some of these waters even when found by the microscope to be free from all visible organic productions will not stand the ordeal of these culture tests, but will, if not absolutely free from germs, freely develope various species of baccilli. Most of the filtered waters now supplied by the water companies to the inhabitants of London, when examined by this method fail to yield satisfactory results. Absolute security for which I have contended, for now more than forty years, is only to be secured by the use, for drinking purposes, of a water pure at the source to begin with and the purity of which is carefully maintained till it reaches the consumer.

The filtration of water on *a small scale*, that is to say by means of table and house filters, though very useful in some cases is a very uncertain means of purification; thus some of these filters are, even when new and perfectly clean, very inefficient and permit various impurities to pass through, while others though acting fairly well at first, soon lose their power. The reason of this is very obvious. The action of the generality of filters is for the most part mechanical, but some also exert chemical effects, chiefly by means of oxidation. The result of such filtration principally consists in the removal of most of the solid matters in suspension in the water, but these are still retained in the filters. When the filters are in constant use, these matters undergo a daily increase in amount until at last the filtering media become fouled; the same result occurs in the case of the organic matters in solution to some extent; some of this is oxidized, part passes through the filter and a portion is retained.

Thus the several impurities go on augmenting till at last the filter, in place of acting as a purifier gives up a portion of its accumulated impurities and so renders the water more polluted than it was at first. Of course this state of things may be remedied somewhat by periodical cleanings, but this is troublesome and is rarely practised in a regular manner. Indeed it is questionable whether on the whole more harm than good does not result from the use of most household filters.

But I have not yet alluded to those organic matters which were present in some of the Thames waters, derived from the *sewage* poured into the river daily, in such vast quantities in the course of the Thames from Battersea to London Bridge, these consisted chiefly of the fibres of *cotton* and *wool*, the *hairs of animals*, portions of the structures, membranes, and hairs of the *husk* of *wheat*, cells of *potatoe* and other *vegetables*, *starch granules* and last but not least, bile-stained and partially digested *fibres of the meat* consumed, in some of which the characteristic transverse striæ of the muscular tissue could be clearly traced. It was not in all the waters taken from the Thames, that the whole of these evidences of sewage pollution were found, but more particularly in those of the companies which drew their supply from the river between the bridges of the Metropolitan area, as the Southwark and Vauxhall and Lambeth Companies.

In order to demonstrate the presence of the organized animal and vegetable matters contained in water, the plan I pursued was as follows: About a pint of each water was placed in a conical glass and left to stand at rest for some hours to allow of the subsidence of any solid matters which might be present; the sediment was then collected and a portion of it microscopically examined; this contained of course the bulk of the solid impurities present, but if there were many infusoria in the water, these being in constant motion would be more or less distributed throughout the water, some being found in nearly every drop, if the water were much polluted.

Let us reflect for a moment on the horrible significance of these revelations, which demonstrated not only that the sources from which the Thames Water Companies derived their supplies were polluted with sewage, but that some of the waters, as delivered to the houses of the inhabitants and as taken from the cisterns and supply pipes themselves, were polluted in the same way. This was notably the case with the water of the two companies named above. Thus in some instances the consumer imbibed a portion, if only a fraction, of his own excreta. Could any thing be more disgusting or more dangerous?

But it is now known, that danger arises in other ways from the drinking of highly contaminated waters, such as those with which London was formerly supplied.

Thus it has been conclusively proved, that the spread of certain diseases, as typhoid fever and cholera and doubtless some other maladies, is due to the reception into the body through the water drank, of a portion, it matters not how small, of the excreta of any person already infected with either of the diseases in question, the infection arising from the presence of certain specific organisms or bacilli.

The special examination of water for the detection and identification of these minute organisms has therefore been rendered most necessary.

The following particulars will show the nature and importance of the bacillary test. A water may be apparently entirely free from living organic productions and yet be a dangerous water for drinking purposes, since it may contain certain germs, as those of bacilli, which under favourable circumstances may be developed and their presence thus become revealed. In order to determine whether any such germs are present in any given water it is necessary to subject the water to the conditions most favourable to the development and growth of bacilli. The two principal of these are, suitable nourishment and a warm, even temperature, maintained for a certain time. Under these circumstances if germs be present the bacilli become developed and the numbers found in 1 cubic centimeter of water = 15.43 grains, may now be counted. In the purest waters either none or very few will be present. If the number does not exceed 100 per cubic centimeter and the kinds of bacilli developed belong to harmless species, the water, it is stated may be pronounced safe for drinking; but if more than one hundred be detected, then it is to be regarded as impure in proportion to the number of bacilli, this in some cases reaching to hundreds and even thousands in a single centimeter; thus, in the highly contaminated water of the Elbe at Hamburg, Koch counted no less than 2500 bacilli in 1 centimeter of water.

Now bacilli may be divided into the harmless and these occur in most impure waters, and the hurtful, as those of typhoid and cholera. When therefore a water is condemned on account of the number of harmless bacilli, it is so not because any injurious consequences are to be apprehended from the bacilli themselves, but they are to be regarded as an evidence of the contamination of the water in which they are found. With a view to ascertain whether any dangerous and disease producing bacilli are present, plate cultivations must be made and if any hurtful bacilli are identified, even in the smallest number, the use of this water must be immediately abandoned: it would be dangerous in such cases to trust to filtration, however efficiently carried out.

It must be understood however, that the microscope or chemistry, either singly or combined, is fully equal, in most cases, to the determination of the question whether a water is bad or impure or not, without having recourse to bacteriological research.

Further, the application of the microscope to the enquiry showed that some impure waters contained the ova of several species of worms and hence a ready explanation was afforded of the occurrence of intestinal worms in the human subject.

The employment of the microscope, in the determination of the composition of the "organic matter" in water has been fruitful in important results. These were first made known by me in an article in The Lancet for March 1850 and also about the same time in a work entitled; "The Microscopical Examination of the Water supplied to the inhabitants of London and the Suburban districts" published, by Samuel Highley and illustrated by carefully executed coloured plates. Like all my earlier works, it has been long out of print, I have not a copy of it myself and know not where one is to be procured, except in the library of the British Museum.

Now the discovery by means of the microscope of the many organized bodies in water, as supplied for drinking purposes, was also of importance in connection with the subject of adulteration, since in milk, beer, and spirits the chief adulterant employed in London was Thames or some other impure water, and thus in many such cases this admixture could be demonstrated in the most conclusive manner.

Subsequently in 1851, I prepared and published other articles relative to the quality of the water used for drinking; one was of a more general and extended character. In this, first the impurities of water were described and considered, the mineral and organic, the dead and the living; the circumstances which favour the growth and development of the latter; the action of water on lead and iron; the gases present in water; the purification of water; the diseases due to impure water; the different kinds of water, soft and hard waters; the advantages of soft water; sources of London water supplies, the condition of the water of the several companies and the defects of the existing supplies; the constant and the intermittent systems; the disadvantages and dangers of the latter and the evils of cisterns. This report was published in The Lancet in February and March 1851, the various vegetable and animal organisms met with were portrayed in a series of exceedingly well executed engravings and the report was afterwards incorporated with the others in my work on "Food and its adulterations".

These microscopical examinations of water in fact laid the foundation of other more minute and searching investigations which have been instituted in recent years in the same direction; thus the testing of drinking water by the growth and cultivation of any bacilli or germs which they may contain, is now a recognised method of examination and analysis.

Of the companies drawing their water from the Thames I have stated, that the water supplied by the Southwark and Vauxhall and the Lambeth Companies was the worst, even as taken from the cisterns of houses supplied by them, and in this I have often demonstrated the presence of half digested muscular fibre and other evidences of the presence of sewage. I remember well one special occasion on which I did so was after a dinner I gave when residing in Bennet Street St. James's, at which Dr. Farr, Dr. Dundas Thomson and other well known sanitary authorities of the day were present.

The water of the East London Water Company was nearly as bad as the worst of the Thames Companies; that of the Kent Company was purer, as also that of the New River Company although this contained a great many living vegetable and animal productions, such as are found in nearly all river waters which have not been subjected to efficient filtration; the water of the Hampstead ponds as delivered to houses, also abounded in living productions. The only Company near London which supplied really pure water was the Plumstead, Charlton and Woolwich Company. This water was derived from deep wells, was very hard from the presence of much lime, but this was removed by Clark's softening process, which at the same time abstracted the greater part of any organic matter the water contained.

These revelations and researches greatly intensified the dissatisfaction entertained of the then existing water supply and prepared the way for future legislation as will shortly appear.

Mr. Edwin Chadwick was in 1850 the Chairman of the General Board of Health, he sent for me and wrote down my evidence at some length; especially that relative to the organic impurities and contaminations in the waters supplied by the Metropolitan Companies. This evidence will be found in Appendix III of the Board of Health. Mr. Chadwick was greatly pleased with the results I detailed of the microscopical examinations of water and stated that my evidence was just what he had been seeking, as the chemical details relative to the organic matter rendered so little help in determining the question of the purity and wholesomeness of water and the production of disease through the drinking of impure waters.

The Board of Health was at that time greatly occupied with the question of the Metropolitan water supply and much valuable evidence and information was collected in relation thereto, which is recorded in the Appendix to which I have already referred. While condemning the then supplies of the Metropolis, the Board recommended a new source, namely, the Bagshot Sands, as the collecting ground for the rainfall. It was shown that this water would be very soft and it was contended, that it would be nearly free from organic matter, more especially of animal origin and therefore that it would be remarkably pure and wholesome. The water moreover was to be delivered on the constant system whereby cisterns and other storage vessels with all their attendant evils, would be abolished. The large storage reservoirs were to be covered, a measure I had always strongly advocated, and in some cases with success. It was proposed to place one of these reservoirs on Wimbledon Common, a position sufficiently elevated to allow of one half of London being supplied with a high pressure service.

Of course this proposal encountered fierce opposition on the part of the Water Companies and hence the Ministry of the day were led to appoint three Commissioners, Professors Graham, Miller and Hoffman to make a separate and independent examination and to report on the subject.

PARLIAMENTARY ENQUIRY AND LEGISLATION.

The disclosures made of the condition of the London waters, as supplied to the houses of the inhabitants, and revealed by the microscope, greatly augmented the dissatisfaction of the public and forced the Government to take action in the matter and to appoint in 1851 a Parliamentary Committee of Inquiry to consider the question anew. Of this Committee Sir James Graham was Chairman.

Mr. Phinn Q. C. acted for the Government; while an imposing array of bewigged and begowned legal gentlemen were engaged to look after and protect the interests of the Water Companies.

Before this Committee I was summoned on the part of the Government: all was in readiness for the day's proceedings, the witnesses and counsel being assembled. I was sitting quietly near Mr. Phinn, when one of the opposing counsel crossed over from his side and addressing Mr. Phinn asked him whom he was going to take first, "Are you going to examine that humbug, Dr. Hassall"? This of course brought me to my feet and when I told him who I was and requested an explanation, he was not a little taken aback, apologised, begged I would not take his remark seriously and said that it was meant as mere banter between counsel, as I was a hostile witness. Mr. Phinn essayed to quiet the storm and to throw oil on the troubled waters. I did not receive the explanation very graciously, nor did it improve my frame of mind for the subsequent examination. I afterwards learned that the barrister in question was Mr. Macauley.

After the examination-in-chief was finished, Mr. Sergeant Bellasis proceeded to cross-examine me. I will remark at the outset, that his manner and the wording of some of the questions put to me were such as ought not to have been adopted towards a scientific witness, present at the inquiry in the discharge of a public duty and whose desire was to speak the truth only. It was a manner, which seemed intended to discredit and intimidate. The first question put was: "Where do you live?" luckily, my address was a good one, or the inference might have been unfavourable. Mr. Bellasis was very particular in his enquiries as to the bottles in which the samples were collected, "were they clean?" "how did I know they were clean?" "did I wash them myself?" and on my replying that

I doubtless did so if necessary, he said: "you are a bottle washer, then!" The idea that all the creatures found in samples of impure water could possibly be derived from the bottles used, whether clean or dirty, was of course in itself absurd. However, Mr. Bellasis made the most of his case and exerted himself zealously on behalf of his clients. It was evident, that he had studied my book on "The Microscopical Examination of the Water supplied to the Inhabitants of London" and had noted the passages which seemed best suited to his purpose; he picked out what he deemed its weak points, he put wrong constructions and drew inferences not fairly justified by the text and did his very best to damage my evidence. This however he certainly did not succeed in doing; on the contrary, by his cross-examination and persistence he confirmed and added force to the facts telling against the quality of the water supplied to the inhabitants of London.

FURTHER MEDICAL QUALIFICATIONS.

I will now revert to strictly professional matters. Desiring to qualify myself for the higher branches of the medical profession, although I already possessed the diploma of the Royal College of Surgeons and the licence of the Apothecaries' Society, I had still to obtain the license of the College of Physicians of London and the M. D. of the University of London. Having been engaged for nearly twelve years in general practice, some of my book knowledge had more or less suffered and I had now to read and prepare again for a very formidable ordeal in the case of the University. I presented myself in 1851 at the College of Physicians and in due course obtained the Licence of the College; I have not much recollection of the particulars of the examination, but I remember that the examiners were very polite and considerate and that this was the only instance in which I had to undergo any classical examination and this was confined to translating some sentences from the work of a certain Latin author. In 1851 there were no extra licentiates; these were of later creation, the original licentiates now being termed members; but a further change has since taken place, there is a conjoint examination by the College of Physicians and College of Surgeons; the single diploma thereby obtained confers the right to practise both medicine and surgery, and the holder becomes fully qualified to practise as a general practitioner and is thus saved both time and needless expense, so far good; this is a step in the much desired direction of one examination and one portal of entrance to the profession.

The College of Physicians, like most of the other medical colleges, is a somewhat close corporation, consisting of President, Fellows, Members and Licentiates. Many of the Fellows are good and distinguished men, though none owe their position to examination, nor in all cases, to their own acquirements, but simply to selection dependent on a variety of causes, not always strictly professional. The members and licentiates have to undergo stringent examinations and yet they have no voice in the government of the college; amongst these are some who are more than the equals intellectually of many of the fellows and who have contributed in greater proportion to the increase of professional knowledge.

I allowed myself little more than a few weeks to prepare for the examinations for the M.B. and the M.D. degrees of the University of London. As I had been in practice for so many years, I was permitted by the regulations to undergo the successive examinations at much shorter intervals than is permissible in the case of ordinary candidates, the actual examinations being the same in all cases. There were two examinations for the M.B and after an interval of three months one for the M.D., both formidable affairs, each occupying many hours for several days; the ordeal was much more searching than I had anticipated or I should have devoted more time to preparation. When I realized how severe was the trial I repented of my rashness and had some fears as to the result; these were not lessened by my seeing around me each day an increasing number of empty chairs; their former occupants had found that more was required of them than they were prepared for and hence they fled in despair, thereby avoiding the risk of a much dreaded rejection. I cannot say that I was free from this fear myself; I was at the time greatly overworked, a good deal out of health and my courage at a lower ebb than usual. I was also a little anxious as to the kind of examination I should receive from one of the examiners, between whom and myself there had been some antagonism. However all's well that end's well and in due course I received the degrees of M.B. and M.D. and found myself once more a free man, divested of a load of anxiety.

The diplomas which I now possessed qualified me to hold certain hospital and other appointments for which otherwise I should have been ineligible. Ere long, two such appointments came in my way.

MEDICAL APPOINTMENTS.

About this time I was offered the Lectureship on Botany at St. Thomas's Hospital, an offer which however I declined, partly because I was much pressed with work of one kind and another and in part, because I considered it would not have helped me in practice, which of course was the chief goal at which I was necessarily aiming. In this latter view I was possibly mistaken, as the Lectureship would probably have led to my being placed on the medical staff of the Hospital.

In 1853 or thereabouts, through the influence of Mr. Wakley and the late Dr. Marsden, whose will was paramount at that time, I was appointed one of the Physicians of the Royal Free Hospital; a position which I held for many years, becoming ultimately the Senior Physician. This most useful Hospital was founded by Dr. Marsden and it well deserved its name of "Free", for its gates were open to every destitute and disabled person without the necessity of obtaining a letter of recommendation. It was, I believe the first and for many years the only really free hospital and it was to this circumstance that its great success was mainly due. At one period efforts were made by the authorities of the hospital and medical staff to establish a Medical School in connection with the hospital. A fair pathological museum was provided and an operating theatre, the number of beds was increased and the lecturers appointed, the lectureship on the Practice of Medicine being allotted to me; pupils were enrolled and the several courses of lectures were being delivered when the authorities were informed that the College of Surgeons declined to accept the certificate for hospital attendance on the plea that the number of occupied beds was insufficient. The actual number was, if I remember right, over one hundred, but one hundred and fifty beds were required; the funds of the hospital did not allow of so great an extension at that time, and so the school came to an untimely end.

Another hospital founded by Dr. Marsden was the Cancer Hospital, which has also been a great success. The late Dr. Alexander Marsden was a remarkable man, clever, practical and energetic, with the faculty of interesting people in his work and of making friends. I ever found him a most kind and loyal colleague.

Somewhere about the same time, I was appointed through Mr. Hale Thompson, second Medical Referee of the United Kingdom Life Assurance Company and I held that appointment for a long period, until the Company was merged into the North British and Mercantile Insurance Company, to which I was also attached for many years. I can now look back with satisfaction to my connection with both those companies. The latter office on my retirement, granted me an honorarium which I received for a long period.

THE OUTBREAK OF CHOLERA IN 1854.

Shortly after the outbreak of Cholera in London in 1854, Sir Benjamin Hall, having just been made President of the General Board of Health by Lord Palmerston, I was appointed one of the Medical Inspectors under the Board, and I had two extensive districts to supervise, Lambeth Parish and Wandsworth Union; these had to be visited and written reports sent in daily, and an amount of work was entailed which was both harassing and exhausting. The experience thus gained of the various insanitary conditions and surroundings under which the people in the poorer and denser districts were living, proved afterwards of much practical value to me. The particulars of these inspections will be found recorded in the official reports of the Board of that time. That there should be sudden visitations of infectious diseases after the experience I had gained, no longer occasioned me surprise.

Sir Benjamin Hall was a most efficient and energetic president; he quickly formed a Medical Council to advise and assist him, consisting of some of the foremost authorities of the day. A Scientific Committee of Investigation was also appointed; this consisted of Mr. Glaisher for the Meteorological, Dr. R. D. Thomson for the Chemical and myself for the Microscopical portions of the work.

My investigations embraced the examination of drinking water; the rice water discharges of Cholera; the blood; renal excretion, and the clothes of cholera patients. Upon each of these subjects separate reports were prepared and afterwards published by order of the Houses of Parliament, together with all the other documents having reference to this Cholera visitation.

The Microscopical investigation relative to Water was very extensive and minute; it embraced the water of all the companies, taken from the mains and also from houses in which cholera was actually present, or in which deaths had very recently taken place; water from shallow wells, in some cases those supplying cholera stricken houses; from Artesian and other deep wells; water from the chalk both before and after it had been softened. This report was largely illustrated by a number of beautifully executed figures. The drawings were made by my own artist, Mr. Henry Miller, lithographed by Mr. Tuffen West and were coloured by hand. I have not to this day seen delineations of the organic matters, dead and living, contained in waters used for drinking purposes equal to these for fidelity and excellence.

Abundant evidence was produced of mischief arising from the intermittent system of supply and of the polluting effects of the storage of water in cisterns, butts, tubs, pans, jugs &c. In many cases the cisterns had no lids and in others these were fastened down, so that the cisterns could not be cleansed.

The results were for the most part confirmatory of those previously arrived at by me and which it is not necessary to state again in detail. Of the fact of the occasional admixture of the water of the Thames in its course between the Metropolitan bridges with the water of the sea, further proofs were given.

The Microscopical examination of the Rice Water Discharges of cholera furnished interesting evidence which was really of much more importance than was at first supposed. In order that no time should be lost before the examinations were made, my microscope was placed in readiness in the Abernethy Ward of St. Bartholomew's Hospital. The autopsies necessary were made by myself in the Wandsworth Mortuary.

From my Report on this subject I will now extract certain passages, relative to the presence in the rice water discharges of *Vibriones,* or as they would now be termed *Bacilli.*

"Myriads of Vibriones were detected in every drop of each sample of the rice water discharge hitherto subjected to examination; Of these Vibriones some formed short threads more or less twisted, while others were aggregated into masses which presented a dotted appearance. See figure 26.† From observations which have been elsewhere recorded,* it appears that two of the circumstances necessary to the development of vibriones are a feebly acid or more usually *an alkaline fluid* and organic matter, especially animal, in a state of decomposition, more or less advanced. Now in the rice water discharge of cholera both of these conditions are fulfilled. We have next to enquire what is the origin or source of these vibriones and what is their relation to cholera? It is possible that they may obtain entrance into the stomach and bowels by means of the atmosphere, but it is perfectly certain that they do frequently gain admission through some of the impure waters consumed in which

† In this figure are shown the short, straight, single bacillus with slightly enlarged ends; the twisted bacillus, on which Koch has bestowed the distinctive, but somewhat fanciful name of the "comma" bacillus, the union of two or more bacilli and the occasional extension of these into a more or less twisted thread, indicating the relation of this bacillus to a species of Spirillum very similar to that found by me many years since in the waters of the Serpentine, to which reference has already been made, and lastly, the colonies of bacilli.

* Transactions of the Medico-Chirurgical Society 1853.

I have not unfrequently detected the presence of vibriones, sometimes in considerable numbers. Once introduced into the alimentary canal they are brought into contact with conditions highly favourable to their development and propagation, both of which take place with extraordinary rapidity. I made for the sake of comparison some examinations of healthy evacuations. I found only a small number of vibriones and those not in all cases and when present, they occurred in the lower part of the intestinal canal, where in healthy digestion incipient decomposition first takes place. In cholera cases the vibriones are met with as high up as in the duodenum.

"Without however at all supposing that there is an essential or primary connection between these vibriones and cholera, their occurrence in such vast numbers in the rice water discharges of that disease is not without interest and possibly is of importance; thus their presence seems to indicate that the fluid thrown out into the intestinal canal in cholera and especially into the small intestines, is in a state more than ordinarily prone to pass into decomposition and that the fluid itself is more than usually alkaline. A condition of undue alkalinity would assuredly act as a source of irritation to the mucous membrane of the intestines as alkaline urine does to that of the bladder.

"The existence of these vibriones in the evacuations may also possibly explain in some degree the success of sulphuric acid in checking the diarrhœa. Thus, that acid when freely administered destroys the conditions essential to the development of the vibriones and so destroys the vibriones themselves. It checks the tendency to decomposition and lessens or neutralizes the alkalinity of the fluid poured out by the intestines.

"The mere presence of such a large amount of decomposing organic matter in the alimentary canal so high up in the small intestines, the principal seat of the absorption of the chyle must exert a seriously depressing influence upon the system".

These vibriones, as was to be expected, were also found on the clothes in many cases and in reference to this point the following remark occurs in my report on the *Skin* and *Clothes* of cholera patients. "If these vibriones possess any influence on the production of cholera, or if the rice water discharges contain any substance or principle capable of producing that disease, we can readily understand how the cleansing of the clothes might in some cases give rise to cholera in those engaged in washing them".

Prof. Koch has stated, that in nearly half the cases of cholera dejecta submitted to him, a rapid microscopical examination has enabled him to detect the presence of the cholera bacillus and to set at rest any doubt which might be otherwise entertained as to the nature of the case. He was thus often enabled to declare with the utmost promptitude, even by telegraph, whether the discharges forwarded to him contained the cholera bacillus and really represented cases of cholera or not. Had those examinations taken place under the specially favourable conditions in which mine were made, the presence of the bacilli would doubtless have been discovered in most cases, my observations having been made on the dejecta, usually quite free from all contamination, of patients actually at the moment in the full stage of the disease.

In my examinations the microscope was placed in a Hospital ward filled with cholera patients, I was thus able to obtain the rice water discharge in its purest state and to submit it to immediate scrutiny. Some of the bacilli seen, no doubt belonged to more than one species, but from the characters exhibited and which are fairly represented in the figure above referred to and from their general agreement with Koch's description, there is not the smallest doubt that the cholera bacillus was present in the discharges in nearly every case and was first seen by me during the Cholera epidemic of 1854, now nearly 40 years since.

The examination of the *Blood* failed to furnish any results of importance; the red and the white corpuscles were of the usual form and size and presented the ordinary appearances.

The *Renal Excretion* furnished results of considerable interest and importance. Albumen was almost constantly present and it persisted for a considerable period after the attack had passed away; fibrinous casts of the renal tubules were also almost always present in the early samples passed on recovery from an attack of cholera; these casts were abundant, well defined and often contained different forms of urinary deposits, as uric acid, urates and dumb-bell crystals of oxalate of lime, &c. See figure 27 in the original Report.* Similar deposits were also of frequent occurrence in the first few samples of water passed. Kept for some days some of the samples underwent other changes; one of the principal of these was the gradual development upon the surface of the liquid of a blue pellicle; this had been before observed in other cases by Heller, who had bestowed upon it the name of *Uroglaucin*, but which I have elsewhere shown, as will presently appear, to consist of *Indigo*. Again, small quantities of sugar were met with in nearly every case, as first revealed by the development in them of the sugar fungus, a most delicate test to

*See List of Publications.

which reference will again be made later; the specific gravity of the samples in which it was actually detected, ranged from 1007 to 1018, the average gravities being 1009 and 1011. The occurrence of sugar in small quantities in urines of low gravity is by no means rare.

GENERAL SANITARY AND ANALYTICAL WORK.

The wide publicity given to the investigations and reports on adulteration and the reputation attaching thereto, as well as the attention I had bestowed on sanitary subjects generally, caused me to be consulted in a great variety of questions relating to the public health. I was therefore frequently called upon to give evidence in trials in Law Courts, relative to different sanitary questions, particularly as to the purity of water, water supplies, articles of food and other similar questions. It was for the plaintiffs that my evidence was chiefly sought, rarely for the defendants.

These occupations absorbed a good deal of time and left me less leisure and opportunity for purely professional work than I desired, although through all my engagements, with a few comparatively short intervals, I had been occupied in practice since 1846, first at Notting Hill and then in London. As success in my profession was my chief aim and desire, I now determined to apply myself to the pursuit of some special department of medicine.

INVESTIGATIONS OF THE RENAL EXCRETION.

I therefore devoted most of the time at my command to the subject of the Renal Excretion and the Diseases of the Kidneys. I selected this subject, because at that time it had not been sufficiently investigated; it required some amount of chemical knowledge and moreover, it was certain, that the microscope here also was capable of rendering valuable aid.

I had already paid considerable attention to the composition of the renal excretion and in the years 1849, 50, 51 and 52, a series of articles appeared in The Lancet, under the following title: "On certain important points in the Chemistry and Pathology of the Urine".

A few remarks may now be bestowed upon some of the principal of the matters treated of in these articles and in my review of Dr. Bird's work on "Urinary Deposits".

One of the most important of the normal constituents of the urine is *Uric acid,* which plays a principal part in gout. This acid is but sparingly soluble in water, as also in the urine, since this contains so much water; in consequence of this comparative insolubility it is readily thrown down and becomes deposited; the circumstances under which this is apt to occur, are when the quantity of acid present is greater than usual, or when the amount of liquid is lessened and when the temperature of this is reduced from any cause, as in cold weather; under any one or more of these conditions, the precipitation is very apt to take place. Then another characteristic is the proneness of uric acid to assume a crystalline form, the crystals often assuming the shape of sharp spiculæ. Now the crystallization may occur either within or without the body; it may take place in the tubules of the kidneys leading to the formation of renal calculi, or short of this, the crystals may give rise to much irritation, pain and even hæmorrhage; or they may be deposited after they have passed through the kidneys, that is to say in the visica itself, where also they would set up irritation and if the deposit were persistent it would in time form a calculus or stone.

Now at the time when I was investigating this and similar matters, the method usually described in medical works for determining the amount of uric acid was to place a thousand grains of urine in a conical glass with a drachm of strong hydrochloric acid and to set the mixture aside for some hours to allow the uric acid subside; this was the method adopted by Golding Bird, Bence Jones and other writers of that day. I shewed however that by this plan, part only of the uric acid present was recovered and that the amount actually contained in the urine was really much greater than was supposed. I therefore described an improved but by no means a perfect process; the bulk of the urine taken was considerably reduced by slow evaporation and the acid then added, a second evaporation in some cases being advisable. In this way a much larger amount was obtained and more accurate results secured.*

Now uric acid readily unites with bases to form salts; the only combination of this acid described by the English authors on urinary affections of the time to which I refer, namely, Prout, Golding Bird and Bence Jones, was *Urate of Ammonia,* but I had soon reason to doubt the accuracy of this view. I therefore subjected some samples of urates, spontaneously deposited from different urines, to quantitative analysis with the following results:

	1	2	3	4
Biurate of Lime	61	70	18.37	20.0
" " Potash	Traces	None	57.12	42.0
" " Ammonia	13	9	10.06	19.5
Moisture	19	16	11.74	10.0
Colouring Matter	7	5	2.71	8.5

*The Lancet 1849-50.

A fifth specimen consisted in great part of *Urate of Soda*, with a small quantity of *Urate of Lime*, while in other specimens *Urate of Magnesia* was found; these analyses showed that the composition of the urates varies greatly and that they rarely consist of a single base, but of two, three, or four in different proportions; their composition in some instances being determined by the remedies, especially soda and other alkaline waters, so frequently and freely employed.

These results also conclusively proved that the view so generally entertained that the urate deposited always consisted of urate of ammonia was founded on an error, which I exposed in my Review of Bird's "Urinary Deposits" and indeed so far from urate of ammonia being the only urate formed, no instance could be produced of any urate consisting entirely of urate of ammonia; further, there is no doubt, that of the small quantity of ammonia found in deposits of urates, part is present as an impurity, and is derived from the decomposition of urea. Now this fact is not only interesting chemically, but it is of importance pathologically.

It should be stated that uric acid forms at least two classes of compounds, biurates and urates, acid and neutral salts. These salts have different degrees of solubility according to the quantity and the nature of the base, those urates in union with soda or potash being more soluble than those with earthy bases, as lime and magnesia, and hence the latter are met with as deposits more frequently than the former; of the soluble urates, that in union with potash is much more readily dissolved than that with soda, indeed the latter urate when formed in the body in any quantity in cases of gout is very prone to become deposited and to form the concretions so painful and disfiguring, known as "chalk stones". For these formations the excess of uric acid in the blood and tissues is in the first degree responsible and in the second the soda so largely and so generally taken as a remedy in such cases. This alkali ought in fact to be rigidly abstained from and the preference given to potash and lithia. When chalk stones are forming, the effects of the soda are almost as bad as the disease itself.

The urates being much more soluble than uric acid, they are far less liable to become deposited in the renal tubules or in the vesica; indeed so much more soluble are they, especially in the secretion when first passed, that it is only after this has become cold that they are rendered visible, but they then appear as a copious cloud-like, more or less coloured sediment, provided the quantity present is considerable. Now it is either of uric acid or the urates, as might be expected from their sparing solubility, that a very large proportion of renal and vesical calculi are composed.

All this shows the necessity for a strict regulation of the diet in gouty and dyspeptic cases and particularly in regard to its mineral constituents, as also the even still greater need to take with each principal meal a sufficiency of water, or some other diluent to hold the uric acid and the urates in solution. There is no doubt that the habitual use of a dry diet, or of one in which strong wine only is drunk to the exclusion of water, is the chief cause of many cases of stone.

Within the last few years more attention has been paid to the composition of the urates and other analyses have been made, these being in general accord with my original analyses, but in no case have I ever seen the slightest reference to my own long antecedent observations. The fresh analyses have been regarded and treated as altogether new.

Another class of urinary constituents which claimed my notice were the *Phosphates;* these are divisible into alkaline and earthy phosphates, that is, like the urates, into those which are soluble and those which are comparatively insoluble. Hence the latter also, like the urates, when in combination with magnesia or lime are apt to be thrown out of solution under certain circumstances and form deposits, concretions, or stones.

The more soluble phosphates, as those of soda and potash, need not be further referred to in this place; it is to the Earthy Phosphates that the following remarks apply.

The only earthy phosphate described in any detail by Bird, Jones and other writers and physicians at the period when my attention was given to the subject was *Phosphate of Magnesia and Ammonia* or triple phosphate, as it is often called, and even in regard to this, much misconception existed and its pathological importance was greatly over-rated. The formation of this combination is thus brought about. The urea, a normal and essential constituent of the renal excretion, readily becomes decomposed in an alkaline medium and especially in one in which fermenting or decomposing animal matter is present. Now in the renal excretion these two conditions are often conjoined, the urea yields by its decomposition much ammonia and carbonic acid, while the phosphate of magnesia, always present, unites with the ammonia to form the "triple phosphate", which becomes precipitated in the shape of very perfect and characteristic prismatic crystals. This phosphate is sometimes formed in the urine while retained in the vesica, but it always is so when the excretion has become alkaline after being voided. The formation of phosphate of magnesia and ammonia under such circumstances, has but little pathological significance. When formed within the bladder it points simply to an alkaline condition of the urine while within the viscus, and which condition may be due to the state of the organ itself and the cause therefore would be strictly local. When the formation takes place in the urine after excretion, and this is the source of the triple phosphate in the great majority of cases, it is of scarcely any importance, since it would simply be an indication of decomposition of the urea of the urine, a normal constituent.

Still there are some few cases in which there is reason to believe that its presence may be due to excessive alkalescence of the blood and persistent alkalinity of the urine, independent of local causes. In such cases its occurrence is truly pathological and therefore of real importance.

Now the question may be put, how comes it, since *Phosphate of Magnesia* is always present in the renal excretion, that it is so rarely met with as a deposit, except in combination with ammonia? The answer is not altogether clear, but it does sometimes occur in human urine in the crystalline form, as first shown by me in a paper read before the Medical Society of London in 1853 and again in The Lancet in April of the same year. It has also at a later period been observed by Dr. Birkett.

The third earthy phosphate found in urine is *Phosphate of Lime* and to this great interest attaches. It was uniformly stated by the earlier writers on the urine that this earthy phosphate occurs only in the granular state and never in the crystalline form. The error of this statement was first demonstrated by me in the Lancet in 1850, in the "Medico-Chirurgical Review" in 1852 and in a communication to the Royal Society in 1859, which appeared afterwards in the "Proceedings" of the Society and bore the following title: "On the frequent occurrence of Phosphate of Lime in the crystalline form in Human Urine and on its Pathological Importance". I shewed that it is not only frequently present in this form, but that it is of much greater importance than the triple phosphate, and I thus expressed myself in regard to it: "I have met with deposits of crystalized phosphate of lime in hundreds of samples of urine and in many different cases, it is therefore not a little remarkable from the frequency of its occurrence and the peculiarities presented by its crystals that it should have been so long overlooked"; and again: "I have observed that when this deposit occurs it is very apt to be persistent, and when it has disappeared to return, whenever the health is reduced from any cause" and "when persistent it is usually associated with marked impairment of the health". For figures illustrating the characters and forms of the crystals of this phosphate the reader is referred to my work entitled: "The urine in Health and Disease", which will be noticed later.

ON THE DEVELOPMENTS IN ACID URINE.

In 1852 the results of a lengthened enquiry "*On the Development of Torulæ in the Urine* and on the relation of these fungi to *Albumenous* and *Saccharine Urine*", were communicated to The Royal Medico-Chirurgical Society. This communication was illustrated with five plates of engravings and was afterwards published in the "Transactions" of the Society.

Torulæ consist of a fungoid growth which takes place in certain liquids; this at first is made up of innumerable exceedingly minute sporules or germs which collect for the most part on the surface of the liquid and are at first mostly of a rounded form, but afterwards they become elongated and grow into a network of branched threads, which spread themselves through the medium in which they are developed; these threads constitute the roots, or technically, the thallus of the fungus; from these roots, under favourable circumstances, certain upright stems emerge from the liquid into the air; these bear on their summits tufts of sporules or seeds which after a time fall off into the liquid, or are dispersed through the air; these spore-bearing stems constitute the aerial fructification of the fungus, and represent in fact the plant in its perfect state.

At the time at which my attention was first directed to this subject, it was generally thought and taught, that when these torulæ, mostly in the condition of spores, were seen collected on the surface of the urine as a white scum, it indicated the presence of sugar, and was therefore a sign of diabetes. Such in those days was the opinion of Dr. Griffith, Dr. G. O. Rees and Dr. Golding Bird. On the other hand Dr. Bence Jones affirmed that torulæ might appear in urine when there was no sugar, although they certainly formed very quickly and plentifully in diabetic urine, and he stated further, that; "If this vegetation is met with in the urine, we may immediately conclude that albumen exists in solution." This last statement is incorrect.

Without going into all the details of my experiments, the general result went to show, that in urines having a decided acid reaction, two entirely distinct species of fungus, easily to be discriminated by the microscope, were developed. For the formation of one of these, three things are necessary, acidity, the presence of nitrogenous matter, as mucus or albumen, and exposure to the air or oxygen. Under these conditions the fungus developed was *Penicillium glaucum,* a fungus which attacks so many organic substances and which in its perfect aerial form constitutes the blue mould with which we are so familiar. For the development of the second kind of fungus, the three same conditions are needed, as also a fourth, the presence of sugar or saccharine matter. In such a case all the well known changes and phenomena of the alcoholic fermentation are witnessed. From all this it follows that the fungus, penicillium glaucum is not available as a test for the detection of albumen pure and simple in the urine, but that the yeast on sugar fungus, the fungus of fermenting wort, *Mycoderma cerevisiæ,* when it occurs in urine is an excellent microscopic test for sugar, even when this is present in extremely small amount.

Now the development of these fungi in organic liquids is attended with important changes in those liquids; this is so, particularly in the case of the sugar fungus, the sugar being decomposed into carbonic acid and alcohol. The active agents in these transformations of the fungi are the sporules.

In the case of decidedly *alkaline urines,* the series of changes is entirely different. If alkaline, neither of these fungi will appear, but if the reaction on either side be but feeble, they, especially the sugar fungus, will be developed to a small extent, the growth ceasing however as soon as the alkalinity becomes decided; finally, when both fungi are present at the same time, there is usually no difficulty in discriminating the one fungus from the other by the microscope, the sporules and thallus of the sugar or diabetic fungus being so very much larger.

When I first commenced this enquiry it was not known that more than one species of fungus was to be found in the renal excretion; neither had the aerial fructification, that is to say the complete and perfect plant of the sugar fungus, ever been discovered. It was suspected indeed that the growth was not confined to the state of sporules, with occasionally a few short threads, and various attempts, some of them amusing, were made to trace this higher development; thus Turpin spent a whole night vainly in the attempt to make the discovery, his zeal being greater than his intelligence, as the conditions under which his observations were made were such as could not have led to success. Fully believing, that the fungus would under proper treatment develope into a perfect plant, I proceeded on different lines. I placed some saccharine urine in a rather wide-mouthed bottle and observed for a period of some days the changes which ensued. My idea was, that previous observers had failed because the yeast plant in the brewing of beer is in constant motion, because the whole operation lasts but a few hours and for the reason also, that the layer or scum of yeast which ascends to the surface of the fermenting liquid soon becomes dry, further growth being thus stopped. I therefore placed the fungus under circumstances which allowed of the air over the surface of the liquid retaining its moisture, and I waited patiently, not a few hours, but days, for the dénouement, and I was amply rewarded. In due course the aerial fructification appeared; it proved to be very characteristic and was faithfully depicted by the artist at the time. This was a triumph for me of which I have always been proud.

The fungus test is then valuable for the detection of very small quantities of sugar in urine and which in some cases the ordinary tests fail to discover. When the amount of sugar is very minute, great caution should be observed in drawing any conclusions founded there on. Small quantities of sugar are frequently met with in samples of even very low gravity, its presence is often temporary and does not indicate any special liability to diabetes, yet I have known such cases to be very seriously treated, the patient much alarmed and even his prospects in life interfered with and damaged. Then there is another reason for caution; sugar when only in extremely small amounts may be derived from other sources than the transformation of food, particularly the starchy constituents. Thus, Schunk has stated that a glucose is sometimes found in the urine derived from the decomposition of Indican. I pointed out the fact of the occurrence of sugar in urines of very low gravity, forty years ago in my Report on the Cholera Epidemic of 1854 and have repeatedly met with it since in many other cases and yet every now and then the fact of its presence in urines of low gravity continues to be recorded as something new and surprising.

ON THE DEVELOPMENTS IN ALKALINE URINE.

Some time after my observations were made on the Torulæ and other developments in *acid urines*, a corresponding series was* carried out on *alkaline urines*. The changes and developments in these were of an entirely different character. As a rule, fungi are

* "On the development and signification of *Vibrio lineola*, *Bodo urinarius* and certain fungoid and other organic productions generated in alkaline and albumenous urine". Lancet. 1859.

not developed in them and certainly neither penicillium glaucum nor mycoderma cerevisiæ, but in place of these, vibriones or bacilli appear, often in myriads forming a whitish, greasy-looking scum which until examined by the microscope might readily be mistaken for a fungus formation: intermixed with the scum a number of crystals of triple phosphates will usually be found. If the sample were acid when first passed, but after a time became alkaline, in such cases both fungi and vibriones will be developed, the fungi first while the acidity lasts, and the vibriones afterwards.

A great many years have elapsed since I first pointed out the great effect which the reaction of organic fluids exerts on the character of the organisms developed in them; thus, those which are found in alkaline fluids differ entirely from those which grow in acid liquids and this is true in many cases as applied to the solids as well as the liquids; not only to dead solids, but to the living, both vegetable and animal, especially the latter; it is true also as applied to the blood and tissues of man. Thus I have shown repeatedly, that while vibriones are developed in liquids having a more or less pronounced alkaline reaction, fungi make their appearance in these same liquids provided they are acid. Of this fact a very apt illustration is afforded by the developments which occur in the renal excretion, according as it is alkaline or acid as described by me in the memoir on the development of Torulæ already adverted to. I dwell on this matter because of its extreme significance. But I have given other illustrations of the development of fungi in organic substances possessing an acid reaction, as in the experiments I made in 1841 of the innoculation of different kinds of fruit and vegetables. When, the decay being far advanced, as sometimes happens in the case of the tuber of the potatoe, both the thallus of the fungus and bacilli are met with, this is an evidence of acidity followed by alkalinity. On the other hand I showed in 1850 that in impure, non-acid and alkaline drinking waters, bacilli and infusoria are nearly always met with. Then, during the epidemic of cholera in 1854 I demonstrated that bacilli abounded in the alkaline rice water discharges of that disease. Many other instances might be cited of the occurrence of bacilli under similar conditions, but I have limited my references to the cases which have come under my own observation.

Now the alkalinity and acidity of organic liquids and solids, vegetable and especially animal, play a most important part both in the production of many diseases, even in the human subject and also in the counteraction of some of those diseases thereby brought about.

The diseases in question arise through the presence of certain organisms, chiefly the bacilli and fungi adverted to, and which can only live, the one in an alkaline, the other in an acid medium. This rule may not be of universal application and there may be some exceptions to it, but it doubtless generally obtains.

In these facts and particulars, we are furnished, as I pointed out very many years since, with two rational and valuable principles of treatment, the one acid and the other alkaline applicable to a variety of non infectious diseases. The acid treatment is of course indicated in those diseases, as typhoid and especially cholera, in which bacilli are present and in which they play so essential a part. This treatment has been adopted in cholera to some extent and with encouraging results, but it has not been carried out in the complete and full manner which such cases demand. The quantities of acid employed have not been sufficiently great, nor the acid selected been in all cases the most suitable for the purpose.

The mineral acids freely administered in such cases are *true germicides* and they possess this great advantage, that being non-poisonous they may be given in considerable quantities.

Another fact in their favour is that the acids are preservative and retard or prevent the putrefactive changes which so quickly ensue in alkaline organic liquids, especially at the temperature of the blood and which are in themselves a fruitful source of disease.

Although fungi are not usually developed in decidedly alkaline urines, they are so occasionally, but they are different from those met with in acid urines; thus I have occasionally encountered a species possessing very distinctive characters, which I named *Penicillium mucosum*, from its mucous and gelatinous appearance and which will be found described and figured in my work on the Urine. It is not a penicillium however, as its structure is quite different and thus it has heen wrongly named.

Another organism sometimes found in alkaline urine is a very active infusory animalcule to which I have given the name of *Bodo urinarius*; it also is figured in the work referred to above. I have observed that it is in the more decidedly alkaline urines that it usually occurs and especially in connection with indigo; indeed when this substance is in any quantity, the bodos are developed by myriads. "When they first appear, like the vibriones, they are diffused throughout the whole bulk of the liquid, but as soon as they have multiplied to any extent, they collect on the surface, forming a greasy-looking pellicle which when much indigo is present, is often of a slaty or bluish colour".

One result of my observations on the changes and developments occurring in alkaline urine was to show that the *Kiestein crust* or pellicle, to the occurrence of which so much interest was at one time attached, has no existence as a distinct and special formation, notwithstanding the ingenious and apparently conclusive evidence brought forward by Dr. Golding Bird in favour of this view and of the diagnostic significance and value of the formation or crust. I proved in fact that this pellicle of so called Kiestein, was composed mainly of vibriones and triple phosphate, that it was formed after exposure to the air on the renal excretion of both men and women and that it was simply an indication of alkalinity.

ON INDIGO IN URINE.

Another subject which engaged my attention about the same time was that of the presence of *Indigo* in the human renal excretion, "Indigo, indeed!" I think I hear some reader exclaim; "surely that pigment is derived from the vegetable kingdom". In the early part of 1853 I forwarded to the Royal Society a paper on this subject, which appeared in June of the same year in the Proceedings of the Society; the next year I sent in a fuller and more complete communication: "On the frequent occurrence of Indigo in Human Urine "and on its Chemical, Physiological and Pathological Relations" and this was afterwards published in full in the Society's Transactions.

Now the indigo here referred to was blue indigo and this it must be understood is not present at first, but is gradually developed after the exposure of the samples to the air for some days. In the cases in which it is present, a blue scum or pellicle is slowly formed on the surface of the liquid, the colour of which also becomes affected until it exhibits various shades of green or brown according to the quantity of indigo present; this varies greatly, there may be but very little, or the amount may be so considerable, that not only is a blue pellicle formed, but the colour of the whole liquid is greatly changed.

Now the discovery of indigo by me in the urine was not altogether new, for Prout many years before had detected it on one occasion only, while in 1850 Debuyne identified it in the renal excretion of a dropsical patient, but at the time my observations were made no idea was entertained of the frequency of its occurrence and no knowledge had been obtained as to its chemical physiological and pathological relations. The late Dr. Parkes in his work on the urine thus wrote some years since: "In 1853 Hassall examined this subject and may be said to have first given it its true standing; he not only proved the blue colouring matter to be indigo, having obtained isatin and anilin from it, but affirmed the possibility of producing it in a great number of urines by exposure to the air. He thought it was derived from white indigo. Very soon afterwards Scherer examined the same point and came to the same conclusion".

In my paper I pointed out the close relation in many particulars and especially in ultimate composition, between hæmatin and urine pigment and I remarked, "It would appear therefore, that there are no essential differences between cyanourin and uroglaucin; while there are so many points of resemblance between them both to indigo, that one is led strongly to suspect that they are simply some condition or modification of indigo". The suspicion then entertained has been confirmed by subsequent observations.

The samples in which the blue indigo occurs in the largest quantities are usually of a pale straw colour, they readily become turbid, are alkaline and of rather low specific gravity, but small quantities of indigo are frequently found in samples that are highly coloured and of considerable specific gravity.

For the formation of the indigo, the samples should be exposed to the air for some days in an open vessel, to allow of the absorption of the oxygen and the development of the indigo. Whatever therefore facilitates oxygenation, as free exposure, light and warmth, hastens the development of the blue indigo; hence in summer the changes described take place much more quickly than in winter. On the other hand, these changes are retarded and even altogether prevented by a more or less complete exclusion of oxygen. By this exclusion blue indigo is deprived of its colour and it may be reformed and reduced alternately according as air or oxygen is admitted or excluded. The formation of blue indigo is also promoted and hastened by the addition of hydrochloric acid.

The principal diseases in which I have most frequently met with indigo in the largest quantity are, those of the lungs including phthisis and emphysema, cholera and some cases of Bright's disease.

Its persistent occurrence in marked quantities is doubtless indicative of grave pathological disturbance, especially of blood formation and destruction and from the diseases in which it is most apt to be encountered, it would appear that any impairment of respiratory power is intimately concerned in its formation.

One other fact may here be mentioned, namely, that small quantities of sugar are frequently present in indigoferous urines, as shown by the unmistakeable presence of the sporules of the yeast fungus. Schunk obtained a small quantity of indigo from indican also found in the renal excretion, and one of the principal products of the decomposition of this, is a variety of sugar allied to glucose; now in some cases it is the sugar from this source that the presenec of the sugar fungus reveals and which causes the red oxide of copper to be thrown down when urines containing indigo are treated with the copper test.

ON URINARY DISORDERS.

Early in 1860 I brought out a distinct work on urinary disorders, this was quickly sold out and as I have no copy of it, I am unable to fix the exact date of its publication, but in 1863 I published a much larger edition under the title of; "The Urine in Health and Disease": this embraced over 400 pages of letter press and was illustrated with 79 engravings. As these were not drawn by myself but by Mr. Miller, whose services were retained by me for many years and indeed until his death, I may remark that they were admirably drawn with the aid of the Camera and were well executed in every way; but their peculiarity and novelty was that though placed in the midst of the letter press, yet many of the figures are coloured by hand. This was a new departure and it increased greatly the resemblance of the illustrations to the objects themselves.

In this work on the renal excretion, of course the several subjects noticed but briefly in the few preceding pages, are treated more fully. This 2nd edition I am sorry to say has also long been out of print, and although every copy was sold, nothing was left for the author after the expenses were paid. The book was published on the half profit system and mine is, I believe the usual experience in such cases.

Very many years ago I detected and described some very large rounded *Compound corpuscles,* each containing several granular rounded nuclei, as large as ordinary pus corpuscles. They were first encountered by me in cases of cystitis and they were described in my article on the fourth edition of Dr. Bird's "Urinary Deposits" in the Medico-Chirurgical Review for July 1853. I find that Lehmann many years since bestowed upon these compound bodies the name of "Hassall's Corpuscles".

These Corpuscles or "Giant Cells", from their great size, as well as from their containing a number of large and well formed nuclei have attracted a good deal of attention and possess a peculiar interest, since they have been found to be very favorable objects for the investigation of the structure, development and chemistry of the animal cell, upon all of which subjects so much light has in recent years been thrown.*

At the period of the publication of the work on renal affections I had removed to Wimpole Street. Here I was fully occupied with consulting practice, attendance at the Royal Free Hospital and my duties at the United Kingdom Life Assurance Company, so that I had less leisure for scientific work, still I did what I could. I had as usual a Laboratory, though not so complete and convenient a one as at Bennet Street.

PREPARATION OF A REAL FLOUR OF MEAT.

Having directed so much attention to the question of the purity of food, it was natural that the kindred subject of its Composition and Nutritive Value should interest me. Amongst other things it occurred to me, that it would be of the greatest advantage if a real *Flour of Meat* could be prepared, an article analogous to wheat flour in fineness. Of course I do not mean simply dried meat, coarsely powdered, or pounded pemmican, but a really fine powder, containing the whole of the constituents of the meat and in a readily digestible form. I forthwith set to work to ascertain if this was practicable and I succeeded perfectly. I was so pleased with the result, that I secured the invention, fitted up a most complete and commodious factory in Marylebone Lane, the Cross Keys Factory, close to my residence and here much of my leisure time was spent and often half the night, in the company of my faithful and patient assistant, Mr. Vosper.

The factory consisted of two floors; on the lower were the tables on which the meat was cut up, the mincers, mills, sieves &c., as well as the specially constructed flues and drying chambers.

The process from first to last was as follows. Each morning the buyers, often accompanied by myself or Mr. Vosper repaired at six o'clock to Smithfield to select and buy the meat. The lean portions of the best beef, as much as could be utilized at one time, were at once despatched to the factory from day to day.

The operation of conversion then commenced and that without the loss of an hour, as the whole process had to be completed within the twenty four hours.

The several steps of the conversion or manufacture were as follow. The joints were freed from the bones, the superfluous fat and thick tendons; the lean meat was cut into pieces of suitable size and

* The Urine in Health and Disease. See Figure 78.

passed through a large mincing machine, whereby it was reduced to the state of pulp; this was then evenly spread by means of large four-pronged forks over zinc wire trays and the trays placed in a rack until a sufficient number were ready, when they were transferred to the drying chamber, which was heated by flues specially constructed and arranged in a suitable manner. The first part of the operation of drying was conducted at a comparatively low temperature, this was raised however and maintained for some time, till the meat was thoroughly dry, at something under 150° Fahr., but it was never allowed to go beyond or even to quite reach this point, as in that case the albumen of the meat would be solidified and would no longer become soluble in water. The temperature therefore required to be carefully regulated; so necessary was this and so anxious was I about it that I very frequently used to remain in the drying chamber, which often contained one or two hundred trays, for long periods at a time arranging and examining the trays. On some of these occasions the temperature approached 150° Fahr.; the extreme heat was rendered tolerable by profuse perspiration and beyond this but little inconvenience was felt, though the ordeal was somewhat exhausting. Being so much occupied in the day time, it was at night chiefly that my visits to the drying chamber were made. I frequently did not leave the factory till the small hours of the morning and although I had not far to go, the egress into the cold air of winter was trying and not unattended with risk, especially in my then condition.

The meat was fully dried, which it was known to be when the pieces were quite friable and broke readily under the fingers; if they bent instead of breaking they were not sufficiently dry; the trays were next freed from the meat which was passed through large and very effective mills and ground into a powder; this was passed first through a rather coarse wire sieve and subsequently through finer sieves, till the product obtained was as fine as the finest wheat flour. The coarser larger particles, together with some fibrous tissue which failed to pass the sieves were ground a second time and in the end only a very small quantity of the fibre had to be rejected. It was this fineness, the non-coagulation of the albumen and the retention of all the nutritious constituents of the meat, except some of the fat, that constituted the merit of my Flour of Meat. The dried and coarsely powdered and ground meats and the pemmican sometimes met with, had nothing in common with it. My flour was in fineness equal to wheat flour and its preparation from raw and moist meat was a real triumph of manufacture. Now it was necessary that the operations above described should be completed in the twenty four hours, up to and including the drying. The dried meat was then placed in large tin cannisters, ready for grinding the next day.

I would now make apparent one or two of the principal advantages of this preparation.

Some of my readers may be surprised to learn, that raw lean beef contains about seventy five per cent of water and only twenty five per cent of solid matter, consisting of albumen, which is contained in large quantity in the blood and tissues, of fibrin, the chief constituent of the muscles of meat, of gelatine from the bones and tendons, of fat and of extractive and mineral matters. Now the dried meat is deprived of nearly all the seventy five per cent of water, so that one pound of the dried flour represents four pounds of lean raw beef, a very great difference.

The percentage composition of an average sample of flour of beef when freshly made is as follows: Water, none, Albumen 14.6 Fibrin 45.3, Gelatine 13.2, Fat 15.0, Ash 4.5, Alcoholic extractive 7.4.

Dr. Parkes gives the results of the quantitative analysis of a sample of commercial flour of meat when mixed with 8 per cent of arrowroot, $2^1/_2$ per cent of sugar and 3 per cent of salt and pepper as follow: Water 12.68, Fat 10.99, Salt 3,20, Nitrogen 8.81=55.5 of nitrogenous compounds, the rest being made up of arrowroot, sugar and 1.8 per cent of free acid, reckoned as lactic acid.

Contrast the above two analyses and especially the first with a pint of good clear beef tea weighing 8818 grains, of which 8750 consisted of water only, albumen none, fibrin none, gelatine 5.7, fat none, ash 41.0, alcoholic extract 63.8 = dried residue 110 grains, or less than 6 per cent. Now the nourishing and sustaining constituents of meat are the fibrin especially, the albumen, the fat and in a less degree the gelatine; the three first of these are absent in clear and fat-free beef tea which is therefore to be regarded as a stimulant rather than a nutritious food, in fact, a form or modification of Liebig's extract, plus a little gelatine. Milk of good quality furnishes about 12.5 per cent of solids in the following approximate proportions; casein and albumen 3.7, fatty matter 3.5, milk sugar 4.6 and ash 0.7; thus milk of fair quality is twice as nutritious as good clear beef tea. If the albumenous powdery material thrown down in making beef tea, when the temperature is raised to 150° Fahr., be retained, then the amount of nourishment will be nearly doubled, and it is in the power of retaining the albumen of the beef, that the great superiority of home-made over the concentrated solid beef tea of the shops consists.

Returning now to the analysis of the flour of beef given above, it may be said, that one part consists of albumen, three of fibrin, one of gelatine and one of fat; thus the fibrin weighs nearly as much as the three other principal constituents put together and this the more nutritive half. It is of the fibrin, that the chief substance and bulk of meat is made up and in it the principal nourishment of the meat resides; but the fibrin is insoluble in water, even in the making of soups and beef teas and it remains in the saucepan, as does also the albumen, the only difference being that a little gelatine, some extractive matters and some fat have been abstracted, but the fat is rejected later on before the soup or beef tea is brought to table. This meat or fibrin therefore still contains the chief nourishment of the meat and is a very valuable article of food, though in England its value is unaccountably overlooked and in general it finds its way into the waste tub, a result due to ignorance and prejudice. Foreigners and foreign cooks know better uses for it than this and with the aid of mixed vegetables and a sauce, soon convert it into savoury and appetising dishes. As it is entirely an animal food, it is obvious, that it should not, any more than should fresh meat, constitute the only nourishment partaken of, but that bread, vegetables and a little fat would be still required to make a complete and perfect diet, capable of maintaining the body in full health. The late Dr. Parkes thus expressed himself on this point in his "Hygiene".

"As the flour of meat contains albumen and fibrin in a condition well adapted for nutrition, it is probably more suited than Liebig's extract for a diet of long continuance. I have tried some experiments with it and found that when given with bread alone it did not nourish properly. Two gentlemen who lived on it and bread for some days, both became indisposed and there was great dyspepsia, evidently imperfect digestion, with in both cases an eruption of acne. When to the same diet, a larger amount of fatty and vegetable foods were added without any increase in the meat or bread, the effect on health was described by both observers as perfectly marvellous; all symptoms of illness disappeared and with this proper admixture of foods, "Hassall's Flour of Meat" answered admirably. I have seldom been more impressed than with these experiments; the evident difficulty in digestion of Hassall's food by itself and its perfect digestibility and evident nutritive power when there was admixture of vegetables and carbo-hydrates and fat convinced me of the immense importance of attending to these points in cases of sickness. I feel sure, that in many cases by adhering too closely to one class of diet we must do injury". Similarly, "two gentlemen at my request lived for four days on Liebig's Extract and bread. One lost weight but was otherwise healthy, the other became indisposed and gained weight as if excretion were interfered with. The symptoms were at once removed by the addition of fats and starches to the same diet, as in the analogous cases noticed as occurring in the experiments with Hassall's dried meat". Parkes' "Practical Hygiene" Fifth Edition.

It was not alone beef, that was converted into a flour, but various other kinds of meat with the same favourable results; as mutton, veal, poultry, hares and rabbits.

Amongst other preparatious devised, consisting wholly or in part of flour of meat were the following:

Flour of Meat Mixture for Soups, containing beef and veal, hare or rabbit, sometimes ham, together with the requisite vegetables and seasonings.

Flour of Meat food for Children, the Aged and Invalids with chicken or rabbit.

Flour of Meat cocoa.

Flour of Meat biscuits.

Starch-free Bran and Meat biscuits for diabetics.

Flour of Meat Lozenges.

These several combinations having been made known to the public, orders came in freely and the factory was kept working at high pressure; this continued for some time and success seemed assured; at length the demand began to fall off and some complaints were received; these were confined to two points; one was, that the flour did not keep well, the other that when it was cooked it became somewhat sandy; both these complaints were to some extent well founded. The objection as to its not keeping well, arose in some cases from the packages being placed in damp situations, in others from being exposed in shop windows for an indefinite time, to the sun and other trying atmospheric conditions. The boxes in which the flour was kept were round, chip boxes, as the wood allowed the access of a little air and in these boxes the flour kept better than in tin cannisters. I find in Parkes' "Hygiene" the following remark in reference to this point, "Hassall's flour of meat keeps very well; but if the open tins are exposed to the air after several months it slightly changes colour and then acquires a peculiar odour. Subsequently it decomposes. But if well fastened it will keep for a very long time. It will certainly be a valuable addition to the resources of the Military Surgeon".

The second complaint as to grittiness was due in part to the manner of cooking. The flour contained all the albumen of the meat, mostly in a form slowly soluble in cold, or better in warm water, but this albumen and to some extent the fibrin became hardened if the water was too quickly heated, and also if sufficient time had not been allowed for it to soak into and penetrate the flour and so cause it to swell and become soft. Any meat would become more or less hardened under such circumstances and also spoiled if exposed to the air for too long a time.

The two defects complained of, I feel confident, might have both been overcome and remedied and I have often regretted that at the time, I was precluded from giving further attention to the process of manufacture. In the first place, the flour should have been reduced to a still finer state of division, so fine indeed as to all but destroy the visible structure in the meat as seen under the microscope, and in the second, the flour, with if necessary, the aid of some binding material, such as a little gelatine or extract of meat, should by hydraulic pressure have been struck into cakes or bricks, like those of compressed tea; then the flour should always be soaked for a little time and the temperature of the water should be slowly raised only, a precaution which is known to be necessary in all cases if the meat is to preserve its tenderness. The flour of meat, when thus prepared, would I am convinced keep perfectly well for a time sufficient for all practical purposes.

For such an article there ought, and I believe there would be a large demand even now and notwithstanding the great advances which have recently been made in the preparation and manufacture of various more or less nitrogenous articles of food. Formerly, the dried preparations offered for sale had consisted of coarsely powdered meat prepared without any particular regard to temperature, grinding, or sifting.

On the occurrence of serious illness the manufacture of the Flour of Meat ceased; I have recently however learned that a similar flour is prepared on a considerable scale by the Bovril Food Company Limited, of which Lord Playfair K. C. B., F. R. S. is Chairman, that the flour enters largely into the composition of the article so extensively advertised termed Bovril, and that it is sold separately for mixing with potatoe and other farinaceous substances, as also with chocolate.

In the British Medical Journal of the 10th June last, it is stated, that Lord Playfair and the Directors of the Bovril Company received a large party of visitors at their warehouse in the City Road to witness the process of manufacture of the product, which goes by the name of Bovril and to inspect the foods prepared for the use of the Nansen and Jackson Expeditions to the North Pole. The journal remarks: "Speaking in general terms, Bovril is made by grinding dessicated meat into an impalpable powder and mixing this with a due proportion of Meat Extract. By mixing the powder, which in itself is almost tasteless with chocolate, a food is obtained, which, while less immediately stimulating, is almost more nutritious". — "We were glad to note, that in the supplies for the arctic expeditions, there was a large admixture of barley, oatmeal, potatoes and other vegetable; as well as of animal fat, the dessicated beef powder being used instead of the pounded meat of the ordinary pemmican, to make the addition of the necessary proteids".

It will be seen therefore that some of the preparations of the Bovril Food Company contain dried meat reduced to a fine powder resembling closely the Flour of Meat devised by me and manufactured at the Cross Keys factory as far back as 1865. It is stated in the advertisement of the Bovril Company, that one ounce of the nutritious constituents of bovril contains more real and direct nourishment than 50 oz. of the purest meat extract. I do not know on what data this calculation is based, but meat extract containing neither fibrin, albumen nor gelatine ranks very low as a direct nutrient; it is rather a stimulant and flavouring agent. I have shown elsewhere, that 1 oz. of the flour of meat represents 4 oz. of uncooked lean beef. A special preparation termed by the Company, "Invalid Bovril", is affirmed to be even more concentrated than ordinary bovril.

DANGEROUS ILLNESS.

An event occurred about this time, that is in 1866, which once more entirely changed the current of my life. The unceasing labours arising from my various enquiries and occupations carried on for a series of years and latterly my exertions in connection with the flour of meat, told upon my health and strength and brought me to a condition rendering me liable to illness at any moment. I had noticed for some time past, that in the mornings I had a little cough and more than once I asked myself whether it was likely after all I might become consumptive. Early one day, feeling much as usual, with no sense of impending illness and fulfilling my ordinary engagements, on leaving the office of The Lancet a loud ringing cough came on quite suddenly. I had experienced a similar fit of coughing a day or two before and it then occurred to me, that if I had many such attacks I should break a blood vessel and this time a little blood followed. This gave me a shock; I got into a cab and went home. Here

I rested quietly for an hour or so when the bleeding having ceased and as I hoped, was at an end, I repaired to the Factory, which was only a short distance from Wimpole Street, and attended to some matters there. The talking brought the hæmorrhage on again and this time more freely, so I slowly returned home. It continued at intervals for some hours, when a friend in the house, without my knowledge sent for Sir George Burrows, who most kindly came at once; he ordered me to bed, and while preparing for this, a profuse hæmorrhage suddenly ensued; on the spur of the moment I sent for my will, which was not signed, but before it arrived I had swooned away and this fainting, it was afterwards said, saved my life. A very long illness followed, I was in the greatest danger and throughout it I was assiduously attended with the utmost kindness by Sir George Burrows and Sir Richard Quain. I have often thought and still think of their goodness in devoting so much of their valuable time to my case, and for which I have ever been grateful.

After some weeks I was removed for change to my brother's house at Richmond. I well remember the journey there, rather a mounful one. The vehicle was a special kind; it bore a close resemblance to a hearse, as I did not fail to notice, and as in a hearse I was put in at the back, lying at full length. My feelings at first were not of the most agreeable kind, and thus with my brother Dr. Richard Hassall, I was conveyed to my destination. The journey although made under these circumstances interested me; to be out again after so long a confinement to bed, to be in the open air, to get glimpses as we passed along of the people, houses, trees, plants and flowers was indeed a pleasure, and I was thankful. It will be remembered that I had once before gone through a somewhat similar experience.

At Richmond under my brother's care and Dr. Tweedie's medical advice I made some small advance in strength, but the progress was slow and at times doubtful; a further change was therefore deemed advisable and I was taken to Hastings. Here on arrival I was shown my bedroom; this was at the back of a house built close under a cliff and it was so dark that it had to be lighted even in the day time; it seemed like a vault or even a sepulchre and I hastily retired from the house; other quarters were sought, this time in St. Leonards, quite near the sea, bright and cheerful. Here I was professionally attended, most kindly and ably by the late Dr. Blakiston. For a long time it was a struggle between life and death and it was very doubtful which would prevail; the exhaustion was profound and sleep seemed as if it never would come. I used to exclaim, "shall I ever sleep again, except my long last sleep?" At length I gained sufficient strength to walk a few steps on the esplanade; the winter had now commenced, the sea broke upon the shore close to the house with a loud and angry noise as though it would tear me out of my bed and there being snow on the ground, still another change of climate was advised and I was removed to Ventnor.

At Ventnor my progress was quicker and more marked, I was able to be out and to walk more and at last I even dared to bestow some thoughts on the future and to consider what course it was possible and advisable for me to take, as I was entirely dependent on my profession. It was pointed out, that I should never be fit for the climate of, or for professional life in London and it was suggested that I should remain permanently in Ventnor. After some consideration I determined to adopt this suggestion and to make Ventnor the centre of my future work and new career. I therefore parted with the lease of my house and furniture in Wimpole Street to Dr. Green, who has resided there ever since.

From long habit and the reputation acquired, I could not, even at Ventnor, free myself either from my liking for analytical enquiries, or from my connection with the subject of the purity of food. I therefore had a commodious laboratory built where original investigations were carried out and analyses made. I had two assistants, Mr. Arthur Angell and Mr. Otto Hehner; both of these afterwards became well known names in chemistry and are public analysts under the Act for the suppression of adulteration. It was in this laboratory, that their investigations were conducted which resulted in the discovery of a method, whereby the adulteration of butter with beef or other animal fats cold be accurately determined. Up to that time it was declared on high authority to be impossible to detect the admixture. The method proved of great value in the detection of the adulteration of butter with beef fat, as the admixture of margarine with butter was then attracting much attention.

For myself I found, or rather made, time to prepare and publish in 1875 another and final work on the adulteration of food, under the title of "Food; its Adulteration and the Methods for their Detection".* This was a more comprehensive and systematic work than any of its predecessors.

Here perhaps it may be well to convey some idea of the nature of my illness. I have already alluded to the fact that I was never very strong or robust and also to my serious attack of pleurisy at

* Longmans, Green and C° pp. 890. Illustrated with upwards of 200 engravings.

Notting Hill in 1849. This last damaged my right lung, but little further trouble had up to this time resulted from this cause and under more favourable conditions of life and surroundings than those in which I was placed in London, probably never would have troubled me; as it was, it laid the foundation of the illness which afterwards occurred. My case turned out to be one of fibroid phthisis of the right lung, a form so graphically described by Sir Andrew Clark and distinct in some essential particulars from ordinary phthisis; it is much more chronic and results in more or less destruction and contraction of the lung affected. So great and persistent is the contraction, that in some cases, as in my own, the heart becomes displaced, and is drawn over to the affected side. This displacement embarrasses and impedes the action of the heart often seriously. But the great distinction is the absence of the bacilli of true tubercle.

Soon after my recovery, I attended a Conversazione at the Royal College of Physicians. Sir George Burrows, who it will be remembered attended me throughout my illness, was President of the college and he and the Censors were receiving the guests as they arrived. On my presenting myself, Sir George exclaimed; "Why, Hassall, what business have you here? you know you ought to have been buried long ago; why were you not?"

My surroundings now were very pleasant, with a measure of fairly renewed health and after the trying ordeal I had gone through, I experienced an increased capacity for enjoying the beauties of nature, the open air and out door life. I always had a great fondness for the sea-side, a liking confirmed and increased no doubt by my experience of the lovely shores of Dublin and Killiney Bays. Often when working in my Laboratory in London with perhaps the sun shining overhead, have I longed to rove over some hill or mountain side or wander by the sea shore, but it then seemed to me that this longing could never be gratified and now in a manner which could not have been foreseen, the wish was realized.

The Undercliff in addition to its great beauty, certainly possesses many climatic advantages. The Undercliff, the appropriate name given to the land below the cliff, is well protected, first by St. Boniface Down and then by the six or seven miles range of cliffs, extending from Bonchurch to Blackgang. This protecting range ensures moderation of tempertaure and shelter from the northerly winds; then the position is elevated, the soil chalky, the fall to the sea considerable, so that the rainfall is carried away quickly; lastly, it receives a larger amount of sunshine than is to be had in most other

English health resorts and on the whole it may be said of this unique locality, that its climate is scarcely to be equalled in Great Britain. If it were more easily accessible from the mainland great would have been its prosperity and Bournemouth never would have surpassed it in this respect.

THE NATIONAL HOSPITAL FOR CONSUMPTION.

It soon struck me as somewhat remarkable, that nothing had been done in so highly favoured a district in the way of establishing a hospital or sanitorium, especially a Hospital for the benefit of our poorer brethren suffering from Diseases of the Lungs; it seemed to me that it was just the place for such an institution, of which indeed there was great need. The Brompton Hospital for Consumption, though a valuable institution, in some respects did not, and still does not fulfil by any means all the needful conditions of climate and surroundings; in fact the climate of London is most unsuitable for diseases of the lungs, it being damp and depressing, nearly sunless in the winter and the air carbon laden and impure; certainly it cannot be said that the inmates of an institution thus situated are placed under the most favourable conditions for their recovery! For very urgent cases, or when the ailment is slight and a temporary residence only is required, such hospitals are of much value and are a necessity in a vast city like London. The Curative Hospital for sufferers from Diseases of the Organs of Respiration should be in some suitable spot in the country, to which the patients could be drafted and where the best chances for their amendment and recovery would be insured.

After due consideration I determined to devote my best energies to the establishment of such a Hospital and I soon devised its Plan and Design.

My experience had shown me the evils associated with the ward or aggregate system; the mixture of cases, medical and surgical, infectious and non-infectious, many suffering from wholly different diseases, some convalescing, some very ill, others perhaps dying; some noisy and boisterous and each being in sight or hearing of the other's sufferings. These evils have no doubt been much mitigated since the period to which I refer, but there is still room for further improvement; the wards are now usually smaller and there is a better classification.

I therefore determined that the hospital should be on the *Separate System,* that is to say, that the patients should be separated, as far as practicable.

On this fundamental basis therefore, the hospital was to consist of eight blocks of semi-detached houses, sixteen houses in all; each house was to accommodate six patients, each patient to have a separate bedroom facing south, two sitting rooms in each house, to be used in common by the six patients. The entire number of patients to be thus provided for was ninety six. All the rooms for the patients were to face due South, those to the North, being for the service of the Hospital.

Half the houses, that is eight, were to be for men, and eight for women, each set being separated the one from the other by a Chapel.

The whole range of houses, including the chapel, was to be united by a subway, the service of the hospital being thereby greatly facilitated and attendance in the chapel rendered practicable without exposure to the outer air. A tramway was to run along the subway; there was to be a flight of steps leading from the subway to each block of houses; the kitchen was to be fitted up with gas-ovens and other appliances for cooking and was to be in the centre of the subway.

There were to be special arrangements for the warming and ventilation of the hospital; the houses were to be provided with spacious corridors and verandahs and the corridors were to be afterwards connected, so as to form a long promenade.

Another feature of the hospital was, that each house should be so far complete in itself as to allow of its receiving a separate name and of its being registered as a separate or miniature hospital it being considered that such small hospitals would form admirable and appropriate memorials when dedicated to some great benefactor, or to some departed relative or friend.

A third special feature was, that the hospital was to be in part only, to the extent of something under one third, self supporting. I considered, that this provision, while it would be a great help to the funds, would not materially restrict the benefits conferred on the poor and even the poorest applicants, since the really deserving would, in most cases, readily obtain the sum necessary for their admission from friends or other helpers who were acquainted with the circumstances.

Lastly, the attendance at chapel was to be voluntary, the service moderate and each patient was to be free to attend his own place of worship in Ventnor or St. Lawrence, subject only to the consent of the Resident Medical Officer as to the patient's fitness to go out. The funds for the building of this chapel were to be separately obtained, and not one shilling of the money contributed for the hospital proper, was to be used for this purpose.

The above and some other particulars were duly embodied in a prospectus, which has since, especially in recent years, undergone many alterations and has received considerable additions necessitated by later occurrences and events.

Armed with the prospectus I now wrote a number of letters to some of the best known supporters of hospitals and other similar charities. This was a laborious task, for it had to be long continued and the effort to establish the hospital entailed a correspondence which had to be maintained for years. The replies received were encouraging beyond all expectation, owing I presume mainly to the clearly defined and practical character of the design I had put before those to whom I wrote and my own earnestness in the cause. Scarcely an answer I received at that time which was not more or less encouraging, most of the letters containing cheques of varying amounts, these accumulated little by little till they grew into what I then deemed a considerable sum.

Very early in my efforts I received a visit from Mr. Frederick Leaf of the firm of Leaf and Co. of Old Change, City, so well known for their liberal support of many charitable and worthy objects. He told me that he had seen my prospectus, that he thought he should like to do something to forward the proposed hospital and he asked what I thought he should give. I replied it would be very good of him if he became a life governor, he said he wished to do more than this, "however for the present my wife and I will both become life governors". I thanked him warmly, and they thus became the first governors of the projected hospital. Mr. Frederick Leaf, Mr. Leaf senior and other members of the same family afterwards proved themselves most munificent friends and supporters of the hospital as will hereafter appear.

After some time, about seventeen hundred pounds having been obtained; a sum which was gradually being increased I determined to take further action.

The first thing now needed was to secure a suitable site. Most of the land on the west side of Ventnor, but in the parish of St. Lawrence, belonged to the Hon E.C.A. Pelham, then a minor, Lord Monson being his guardian; to the latter therefore I applied with some trepidation, for I felt that very much depended upon his decision. To my intense satisfaction I found that he was quite willing to entertain the idea of letting a piece of land suitable for the hospital. His Lordship pointed out two or three sites, one so excellent in every way, so well sheltered and so beautiful that had I the choice of the whole Undercliff I could not have found a better. Very fortunately, next to this site was a piece of glebe land attached to the church of St. Lawrence, of which at the time the Rev. Charles Malden was Rector, who as well as Lord Monson was very favourable to the project. The result was that in this way, between the two sites, twenty two acres of land were secured at a very moderate annual rental, on leases of nine hundred and ninety nine years, as is the custom in the Isle of Wight. Thus the site was almost as good as freehold. Here was an essential point gained and a sure foundation on which to base further efforts for the promotion of the object in view.

The next step was the formation of a governing body. There was then living at Ventnor, the Rt. Hon Sir Lawrence Peel, a retired Indian judge, well known and universally respected. I explained the details of the proposed hospital and asked him if he would consent to be the Chairman of the Committee of Management; to this request he readily assented as he would have been sure to do in the case of any useful and worthy object. I next applied to the Governor of the Isle of Wight, then the Rt. Hon. Lord Eversley, more generally known as Mr. Shaw Lefevre and who was for a long period Speaker of the House of Commons and obtained his consent to be the first President of the hospital, next the members of the Committee of Management were sought and found: these consisted of some of the principal residents of the district and included, amongst others, the Rev. C. Wills, the Rev. A.L.B. Peile of Ventnor, the Rev. Charles Malden of the adjoining parish of St. Lawrence, who afterwards became the Chaplain of the Hospital as it was situated in his parish; Mr. Frere, Mr. Oliver and myself. As soon as these preliminary proceedings were arranged a meeting of the members of the Committee was summoned and the whole project was duly set before them. It may here be mentioned that the above appointments and some others including those of trustees, treasurer, auditors, were provisional only and that they were subsequently regularly corfirmed at a meeting of the Governors of the Hospital in December 1868.

Another very necessary and important post had to be filled and this without delay, that of Architect and the gentleman I found willing and able to fill this office without fee or reward, was Mr. Thomas Hellyer of Ryde. He served the hospital for many years not only cheerfully and faithfully, but with pride and pleasure in the duties of his office.

It appearing from the statement made at one of the earlier meetings of the committee, that a sum of about £2600 had been collected, consent was given to commence the erection of the first block of two semi-detached houses. This was completed and opened for the reception of patients in 1868, that is to say in about two years after my arrival in Ventnor. In 1869 a second block was completed and of this it is necessary to give some interesting details.

Her Majesty was prayed to lay the Foundation stone of this block and H. R. H. the Princess Louise was graciously deputed to do so on behalf of the Queen. Unfortunately the early morning turned out to be very wet, there was a regular downpour in fact, we were all in despair; flags wet and drooping, drapery soaking and we feared the Princess would not come. Suddenly the sun shone forth, in a couple of hours the scene changed as if by magic and the news arrived that the Queen's Representative, accompanied by H. R. H. Princess Christian was on the way. The journey was attended with what might have been a serious accident. The postillions not being acquainted with the route, drove the carriage, an open landau down the dangerously steep road which descends from the Railway Station to the town. The horses failed to keep the carriage back and had it not been for some bystanders an accident would probably have ensued.

The laying of this stone was one of the first public acts of the Princess, who was then quite young. The ceremony was carried out with great success. Bishop Wilberforce, then Bishop of Oxford and afterwards Bishop of Winchester, the President, Lord Eversley and Sir Lawrence Peel taking part in it, while the élite of the Isle of Wight and many people from Southsea and Portsmouth were present. To myself fell the duty of explaining all details, of conducting Her Royal Highness over the hospital grounds and of aiding in planting the inevitable royal trees, of which before I quitted Ventnor there were several. And here an amusing circumstance occurred. The morning having been so wet, the gardeners came to the conclusion that there would be no planting of the trees, as had been arranged. When we arrived at the spot we found that the pits had indeed been dug, but the gardeners were absent and no trees were provided. I exclaimed, "this is really too bad, get some trees"! The people ran right and left and tore up all kinds of shrubs; two were selected and the planting was duly accomplished to the amusement of the Princesses and the numerous visitors.

The following gracious reply was accorded by Her Royal Highness in answer to the address presented by the President, the Rt. Hon. Viscount Eversley: —

"The Queen, my dear mother, on whose behalf I appear among you, feels a deep interest in this admirable charity and sympathises with the effort you are making to extend its benefits.

"It has ever been the desire of Her Majesty's heart, (and every member of her family shares it) to promote every enterprise for the relief of her suffering subjects".

"The special diseases for which the Hospital is designed are those for which art can do least and nature most. May God therefore grant that the pure and health-giving climate of this beautiful district may be blessed to the restoration of all who shall be admitted to this noble institution."

This touching reply has been duly printed, framed and hung up in the Hospital where it can be seen and read by all.

But little has yet been stated respecting the neccessary adjunct, the Chapel. With a view to obtain the funds requisite for the erection of this, I made a separate appeal, wholly unconnected with that for the hospital proper, stating that not one shilling of the money contributed for the general purposes of the hospital should be used to pay any part of the cost of the chapel. This appeal was likewise successful. Its erection was commenced in 1871 and finished in 1873, all but the spire and south cloister.

The ceremony of laying the corner stone of the chapel was performed on the 11th Dec. 1871 by Dr. Samuel Wilberforce, in the presence of a numerous assembly. The address of the Bishop was most impressive and was made specially so by touching references to H.R.H. the Prince of Wales, whose illness at that moment had reached its most critical stage and whose life was trembling in the balance. During the ceremony a telegram was handed to the Bishop giving the latest information as to the Prince's condition and this was communicated to the anxious bystanders. Then the chapel was consecrated and opened for service on the 13th December 1873, my birthday as it happened, by Dr. Harold Browne, who on the death of Bishop Wilberforce had become Bishop of Winchester.

I may here state, that Ventnor, being in the diocese of Winchester, both the above named Bishops, at the time I was churchwarden to St. Catherine's Church, often came to Ventnor and the Isle of Wight. Bishop Wilberforce was throughout a warm friend and supporter of the Hospital and he further served its cause by taking the chair at the dinner given in London on behalf of the Hospital in 1872. At this of course the usual appeals were made for funds. Bishop Wilberforce in the course of his remarks dwelt upon the fact, that the builder or donor of one of the houses would have the right to name it after himself or some relative or friend and thus would secure an appropriate and lasting memorial. Mr. Leaf the father of the Treasurer of the Hospital, Mr. Frederick Leaf, at once beckoned to me and informed me he would build a house. I reported this offer forthwith to the Bishop, who immediately announced the gift in these words: "one generous and noble hearted man has already responded to my appeal and has expressed his willingness to bear the entire cost of one of the houses."

Although more than twenty years have elapsed since the chapel was erected, it is I regret to say not yet complete; it still wants the spire and south cloister. The design was in the main that of Mr. Thomas Hellyer of Ryde, who acted as honorary architect of the Hospital for many years and to whom in consequence the friends and supporters of the institution are under great obligations. The subject of the windows was however the cause of a little friendly contention between Mr. Hellyer and myself; he wished to have small, narrow windows, placed high up; such windows admit but little light, less sunshine and warmth, shut out the view of the surrounding trees and landscape and are a great mistake. They are a mistake as regards health in any case, but especially in a chapel, for the sick, rendering it dark, cold, sunless and gloomy. I therefore contended stoutly for large, handsome, mullioned windows, placed low down, allowing of the entrance of plenty of light, sunshine and warmth and at length I carried the point. Mr. Hellyer argued that to introduce such windows would spoil his design and said, "how can you be a judge in such a matter? I might as well attempt to dictate your prescriptions."

The windows being completed as I wished and every body being much pleased with the result, great praise was accorded to the architect, his amour propre was appeased and he was well satisfied himself.

Standing in the midst of a long range of houses, which form a conspicuous land mark from the sea, the absence of the spire is very noticeable and mars the effect of the whole design. So many years having passed and nothing having been done, I obtained the permission of the present Board of Management to make an effort to procure the funds for the completion of the chapel. I found however, that a tower was preferred to a spire and as the former in my opinion would have little architectural effect, would be out of keeping with the style of the chapel and original houses and not be in harmony with the surrounding scenery, I feared I should not be able to enlist the sympathy and support necessary to obtain the required sum, which according to the eminent architect, Mr. William Emerson, would amount to twelve hundred pounds. I therefore thought it best not to take any further steps in the matter.

Each of the Memorial houses bears on the face of it the arms of the donor, or those of the person to whose memory it is dedicated and also the date of erection. One of these houses was named after the founder, "The Arthur Hill Hassall Hospital". Some years afterwards, the whole of the Houses having been built and another Memorial Hospital being in requisition, I consented to relinquish this dedication; the funds of the hospital being thereby augmented by some fifteen hundred pounds. My name as founder is now placed on the Entrance Lodge, which was erected some time after I had left Ventnor.

It must not be supposed, that the progress of the hospital thus far, was allowed to proceed without difficulties and differences. When the first steps were initiated the project was pronounced by those who were unfriendly, to be visionary, incapable of ever being realized, destined to end in failure; motives were assigned; it was said I was actuated solely by selfish, personal and professional considerations; then as further progress was made, and it became evident, that there was to be a hospital in the Undercliff of the Isle of Wight, an attempt was made to cause me to modify my plan and design; as to the site, it was argued that the hospital ought to be in Ventnor and not in the adjoining parish of St. Lawrence, and in regard to the name, that this ought not to be a more ambitious one than the "Ventnor Cottage Hospital"; then, that certain necessary appointments which loomed in the future should be assigned to certain specified individuals, who were not well disposed to the object in view and who would in all probability have used their influence to mar the scheme and to act in opposition to the projector. I had therefore to stick to my guns, to be firm in my purpose, to be in fact in the earlier stages somewhat of a dictator. Had I not been so, all my efforts would have been defeated. All this occasioned me much anxiety; I and others saw clearly danger ahead, and I pondered much as to how this was to be met and avoided; at last one night not being able to sleep, a solution of the difficulty presented itself. From the encouragement already accorded and with a view to its future prospects and permanency it had become apparent to me, that the hospital was capable of being developed into much more than a local institution and that it might become a National Hospital. I therefore determined to form a second and influential committee in London. I first consulted Sir Lawrence Peel on the subject and asked him if he would be willing to be the Chairman of this Committee and to this he readily consented. I forthwith took steps to form the Committee. Mr. Frederick Leaf, a warm and highly influential friend of the Hospital, became the Treasurer, Dr., now

Sir Andrew Clark, Dr. Lumsden Propert, my brother, Dr. Richard Hassall, Dr. Paul, Mr. Stafford H. Northcote, Mr. John Holdsworth and myself being amongst the first members, while Mr. Neale F. Horne was the Secretary. It was thus it came about that there were two committees, "The Local" and "The General", the latter of course being the ruling authority. This object having been accomplished, I felt that the future of the Institution was secured. The Local Committee was irate at the change; they were very angry with me and a deputation of them arrived in London and attended before the General Committee to complain; their case was presented with all the force which a clever legal counsel could bring to bear upon it, but Sir Lawrence Peel and the Committee were firm and the deputation retired discomfited. One contention was, that the powers of the Local Committee should be coequal with those of the General Committee; in fact that there should be practically but the one committee. This plan never would have answered, as it would not have done to have one part of the same committee sitting in Ventnor, and the other in London; there would have been divided authority and divergent views. After a time matters settled down and I acted as Chairman of the Local Committee for several years.

The hospital continued to grow apace, house after house arose, in several cases erected as Memorial Hospitals, the entire cost being defrayed by the donors, including sometimes even the furniture, but still the croakers were not altogether silenced; "wait", they said, "till the houses are occupied, then the pressure will begin and where is the maintenance money to come from?"

Of course the pecuniary question, that of ways and means was a very anxious one, and in order to provide the requisite funds, continuous efforts were necessary and were diligently, willingly and successfully made. What with correspondence, appeals, the organization of the usual entertainments, the bazaars, concerts, Christmas trees; &c. in addition to professional work, my time and my energies were pretty fully engaged. These entertainments were invariably successful, because no pains or trouble was spared to render them so, and it soon became known that any bazaars or other attractions on behalf of the hospital would be sure to be worth a visit, and in this way people came from all parts of the Isle of Wight and even from the mainland. The beautiful grounds, twenty two acres in extent were made gay with hundreds of flags most kindly contributed by the naval authorities at Portsmouth, while military bands were freely lent by the commanding officers of Regiments quartered at Newport; then, amateurs volunteered their services for the exhibitions in which they were specially skilled.

One bazaar was held in Northwood Park close to Osborne; this was visited by the Queen, the Princess Royal of Prussia with her children and the Duke of Albany, who afterwards became President of the Hospital and whose early death was a great misfortune as His Royal Highness took so warm and real an interest in the institution. Of another bazaar held in the grounds of the hospital the Empress of Austria, who was then residing at Steephill Castle graciously became the patroness. Mr. Hunt, a barrister and very clever amateur conjuror, greatly amused her Majesty with his particularly adroit tricks and representations.

Then there were no less than three grand Bazaars at different periods in behalf of the hospital, by the kind permission of the Duke of Wellington, in the Riding School at Knightsbridge, all very successful. These bazaars, with other entertainments realized over £6000.

Amongst other attractions and entertainments were concerts, private theatricals, which proved a great success, illuminations and a gigantic Christmas tree.

In this and other ways, and especially through the generosity of many donors, the needful funds were obtained and the hospital grew so rapidly that in 1877, the original plan, was in the main completed, and not only so, but the houses were for the most part filled with patients, the cost of their maintenance being regularly defrayed and thus all the fears of the timid and unfriendly were proved to be groundless.

During the years which have elapsed since 1877 when the hospital was approaching completion, several important changes, improvements and additions have been made. It will be remembered, that the design was for eight Blocks of Houses, four on each side of the chapel to accomodate an equal number of men and women patients; six to each house or twelve to a block, in all 96 persons. But since the date mentioned the hospital has been extended, principally by two additional blocks on the west side; this extension gives the whole range of buildings a somewhat one-sided appearance, as there are four blocks on the east and six on the west side of the chapel. This effect is rendered the more conspicuous by the fact that these two blocks are much larger and different in some other respects from the other blocks.

One of these the ninth, is named the Jones Memorial Block, in memory of Mr. Jones, who generously bequeathed a large portion of his fortune to the hospital. It consists of three houses for the reception of 18 instead of 12 patients, as were the original blocks, three sitting rooms for 6 persons in each, a large Dining Hall 75 ft. in length and 28 ft. in breadth, a Board room and a large Kitchen placed on the third story of the building but in communication with the hall by a hydraulic Lift. Further, this block consists of four stories instead of three, as do the older blocks.

The tenth or Victoria block, so named by command of Her Majesty, also consists of three houses of four stories each and is designed to accomodate 18 patients. In this block, owing to a dip in the land, three of the sitting rooms are placed in the basement.

Thus in each of these two blocks, there are three sitting rooms for 18 patients, whereas in the original blocks of two houses each, there are six sitting rooms for the same number of patients.

The former kitchen was placed in the centre of the subway, partly because this position allowed of the ready distribution of the food by means of the tramway running to the several blocks. The kitchen in the Jones block is placed at the end of a very long range of buildings and thus is at a considerable distance from the eastern blocks occupied by the women patients, yet it possesses the advantages of greater space and more light; but the chief reason of its being situated in this block is that by means of the Lift it is brought into close connection with the Dining Hall.

In this Hall the men and women patients who are not physically incapacitated, partake of breakfast, dinner, tea and supper. It will thus be seen, that the patients in the further houses have a long distance to walk to their meals several times a day, passing however of course by the subway, which has been prolonged under the two new blocks. The common dining hall is no doubt more convenient and more economical than the plan which prevailed from 1868 up to the completion of the Jones block in 1887, of serving the food in each separate block, but this change is a serious departure from the leading principle on which the institution was based and by which the promoters of the hospital are still bound, a principle the strict observance of which, is now, since the discovery of the tubercle bacillus and increased evidence of the infectiousness of consumption, more than ever necessary.

Formerly the number of men and women patients was equal, but now there are only 51 women to 83 men, making 134 patients when the hospital is full.

The increase in the number of patients has of course entailed a considerable addition to the staff of the hospital in all its departments and for whom suitable accomodation has to be provided;

the men members of the staff mess in the Board room on the north side of the Jones block, the general superintendent is lodged in block 6, the matron in block 3, the dispenseress in block 7; these two latter and the charge nurses mess together in block 8, where also the Jewish kitchen is situated.

While special attention was from the first bestowed on the ventilation and warming of the hospital, the means taken to effect these objects were found to fall short of the requirements, especially as the hospital increased in size; hence further measures have been adopted which entailed a very considerable outlay. The General Committee, now termed the Board of Management, deserve very great credit for the care and study they have bestowed on this question. The supply of fresh air at a temperature of 62^0 Fahr. to each of the 134 patients, has for a long time past been greatly in excess of the required standard, namely, 5000 cubic feet per hour, equal to 670,000 c. ft. in all.

The Board of Management still invite the gift of funds for the erection of additional blocks of houses. Any further enlargement of the hospital, is in my opinion strongly to be deprecated, as any such extension would seriously lessen the advantages of the Separate Principle, to the carrying out of which the authorities have been pledged from the first. The bringing together, in a comparatively small area, any larger number of sufferers from consumption, an infective disease, would add to the risk, which to some extent is incurred even under the most favourable conditions. In this matter not only are the inmates of the institution to be considered, but the people and the town of Ventnor.

Indeed, I consider it would have been more in keeping with the Separate Principle not to have built Blocks 9 and 10, but to have kept to the original design of eight blocks of sixteen houses. The erection of a single extra block in the same style as the others for administrative purposes, but not for patients, would no doubt have possessed certain advantages.

As there have been changes in the Hospital itself, so there have been not a few amongst its administrators; indeed an unusually large number of the early supporters and friends have fallen out of the ranks; but other able helpers have been found to carry on the good work. Amongst these may be mentioned, the Earl of Rosebery K.G., the present President, Herbert C. Saunders Esq Q.C., Chairman of the Board of Management, Frederick C. Colman Esq J.P., Treasurer and Neale F. Horne Esq, Deputy Chairman, these gentlemen being supported by a numerous body of directors.

Of the members of the two original Committees, four only are to be found on the present Board, Rev. A. L. B. Peile, now the master of St. Katharine's, Dr. John H. Paul, myself and Mr. Neale F. Horne; the latter was the first regularly appointed Secretary and was most earnest and indefatigable in the discharge of the multifarious duties of the office and hence he is well acquainted with the early history of the hospital. He was succeeded some twenty years since by Mr. Ernest M. Morgan, who still holds the appointment, greatly to the advantage of the institution and he is thoroughly conversant with its later history. It is on the present Board of Management, that the labour, anxiety and responsibility attending the carrying on the affairs of the Hospital have rested for a long series of years and they have well earned the sincere and grateful thanks of all the friends and supporters of the Institution.

The hospital has now been in existence for nearly a quarter of a century, it has been open, not in the winter only, as are so many of the hospitals in health resorts, but throughout the year. The applicants for admission are always in excess of the number that can be accommodated, and sometimes, as entrance is by rotation, they have to wait for their turns longer than could be wished. For this reason the period of the detention of patients in the Hospital is sometimes shorter than is desirable, or than was the case when the hospital was first opened.

The three main circumstances to which the success of the hospital was due and which still ensure its continued prosperity, are first and foremost The Separate System, on which basis contributions were originally solicited and obtained; second, the institution of Memorial Hospitals, the houses being complete in all respects as small hospitals, and third the partially Self-supporting Character of the Institution.

At one time I entertained the hope that the hospital might be made to fulfil a purpose I had in view, namely, that it should not only be an institution for the reception and treatment of persons suffering from maladies of the organs of respiration, but that it should also have afforded special facilities for the study and advancement of our knowledge of these diseases. To the promotion of this object I would, had health allowed, gladly have rendered such service as I was able. To successfully prosecute the requisite enquiries and investigations, a small laboratory for chemical and microscopical research, as well as properly fitted chambers for inhalation would be necessary. The methods that have been devised for the introduction of medicaments into the lungs by means of inhalation, have not yet been tested on a comprehensive and precise scientific basis, hence no very definite data have been obtained whereon sound conclusions can be based, either for or against this mode of treatment. The trials of the methods have hitherto been carried out for the most part in a very loose and unscientific manner. The cost of these arrangements need not be too considerable; the chief expense would be the salaries of the two gentlemen in charge of the departments in question. Although I refer to this matter now, it is not with any idea that it will be carried out at present, but rather in the hope that the suggestion may hereafter be put into practice should a favourable opportunity present itself.

I will here again draw attention to the fact that the Separate System does not consist in simply providing each patient with a separate sleeping room, but a proportionate number of sitting rooms, say one for every three or four people is also necessary and all the arrangements throughout should be on the principle of separation; consequently that there should be no common dining hall and further, that the full benefits which this system is capable of affording are not, in a disease like consumption, to be obtained by an eight or ten weeks' residence in the hospital.

The Special Principle upon which this Hospital is planned, has been the means of drawing much attention to it; visitors have come from long distances to view the hospital, including many medical men, some from foreign countries, while several requests have been received for plans, with a view to erect other hospitals on the principle of separation. Prof. von Schrötter of Vienna sent his assistant to inspect the institution with the object of founding at the Professor's instance the first consumption Hospital in the Austrian Empire at Vienna; Dr. Pistor, Health officer of Berlin, has also paid a visit to the Hospital and he expressed himself in high terms as to its efficiency; again Dr. Billet of the Military Hospital at St. Omer has recently published a highly favourable article upon it, in "Le Journal d'Hygiène", lastly it is proposed to erect at Killiney, near Dublin, a Hospital for Consumption on the same system, each patient being provided with a separate bed room.

In 1881 the hospital was visited by the British Medical Association and in reference to this visit, the *British Medical Journal* wrote: "The Ventnor Hospital was much admired; no other Hospital in Europe can compare with it for the completeness with which the Cottage system is carried out and for the combination of comfort and scientific fitness for its peculiar purpose".

The names of some of its more distinguished visitors, friends, and supporters have already been mentioned, but in addition those of Her Majesty the Queen, the Patroness of the Hospital, who from the first has taken a lively interest in its success, the Empress of Austria, the Prince and Princess Henry of Battenberg, by whom in 1887 the ninth or Jones Block was opened and the Prince and Princess Royal of Prussia, afterwards the Emperor and Empress Frederick of Germany, may be especially enumerated. None could be more gracious than were the Crown Prince and Princess who were evidently pleased with what they saw; the manner of the Crown Prince was so kind and gentle, it was not easy to realize the fact that he had taken a leading and active part in the fierce strife of war, while the Princess so won the hearts of the patients by her sympathetic questions and by the genuine interest she displayed in their welfare, that as many of them as were able, assembled at the gateway and gave the Royal visitors a hearty cheer as they drove away.

The visit of Her Majesty the Queen was a marked event in the simple annals of the Hospital and will not readily be forgotten by those who had the privilege of being present. These included of course most of the chief officials of the Hospital; Herbert C. Saunders Esq Q.C. Chairman of the Board of Management and Frederick Colman Esq J.P. Treasurer. Unfortunately the notice I received was too short and the distance from San Remo too great to allow of my being of the number. As one result of this visit, Her Majesty was pleased to command, through the Home Secretary, that the block of houses inspected by Her Majesty should be named "The Victoria Block". The Editor of the Isle of Wight Advertiser, Mr. Fletcher Moor, who is conversant with the whole history of the Hospital, and who has been its consistent advocate throughout, in describing the incidents of the Queen's visit, thus refers to it. "It was a day that will long be remembered with pleasure by patients and nurses and as the Royal party drove away these were assembled at every block and took their share in the enthusiasm that marked the whole scene." "The great regret in our own mind and also we are sure in the minds of some others present, the original workers in the foundation of this now vast charity, was the absence of Dr. Hassall but for whom in all probability it never would have been, and whose anxious care and forethought are seen in every main particular of the large building."

The Royal National and Incorporated Hospital for Consumption and Diseases of the Organs of Respiration has therefore not only been a material success, but the separate system has fully realized the expectations on which it was founded. The results as regards the patients have been most satisfactory, as shewn statistically in the annual medical reports.

In order to realize at a glance what the original design of the Hospital was like, the reader would do well to procure a copy of one of the annual Reports of a date not later than 1887. These reports have contained for a series of years, an admirable lithograph, portraying with great fidelity the salient features of the site and showing the bold and commanding position of the eight blocks of the Hospital with the Chapel in the midst, the latter showing the spire in accordance with Mr. Hellyer's design. This lithograph, which has done good service for many years, is now replaced by a view which embraces the two new blocks, but which fails to convey an adequate idea of the beauty of the site and its surroundings or to do justice to the original design, the eight pairs of houses being dwarfed by the two larger four storied blocks.

The plan therefore set forth in my first prospectus drawn up in 1868 has been literally fulfilled up to 1887, as the following quotation will show: "Thus the patients will be scattered through a series of cottages or villas, situated in a locality well sheltered from the prevailing winds; they will be designed in harmony with the surrounding scenery, constructed on the most approved sanitary principles and surrounded by gardens: in these cottages the patients will enjoy the advantages of plenty of light and sea air, of effective ventilation and good drainage, and as far as possible a regulated temperature; of large sitting and separate sleeping rooms, of a lovely landscape and sea view and they will moreover experience all the comforts and conveniences of home, in place of being congregated in one large building and subject in consequence to many depressing and injurious influences".

The discovery of the bacillus of tubercle has led to the adoption of a variety of precautions with a view to diminish the risk of infection. To this subject I drew attention some time back in a letter published in the British Medical Journal. The measures recommended and now more or less generally adopted in phthisis, have reference to the expectoration; this when it becomes dry, is very friable and readily breaks up into a powder, the particles of this containing the bacilli, from their lightness are readily diffused through the air and are hence liable to be inhaled and carried into the lungs.

Now the greatest danger from this source arises from the use and the manner of use of ordinary handkerchiefs; I therefore urged in the letter in question that the employment of handkerchiefs, which at the best are but necessary evils, should be safe-guarded as much as possible; that they should be used as seldom as practicable, changed frequently and immediately disinfected after use and I further recommended paper handkerchiefs as a substitute for the ordinary kinds; there are papers now made at a trifling cost which are absorbent, very flexible and well suited to the purpose; the paper might also be used in small pieces, as well as in the form of handkerchiefs, but in either case the paper should be destroyed as soon after use as possible. The employment of paper would do away with the necessity for disinfection and washing.

I also brought the matter under the notice of the Board of Management, who in order to have full control over the use of the handkerchiefs, now supply the patients with them; this is no slight undertaking since it involved a purchase at starting of 5000 handkerchiefs. These of course before washing, are subject to efficient disinfection.

DEPARTURE FROM VENTNOR.

From what has been already stated, it will be evident that my thoughts and time were pretty fully occupied with matters relative to the Hospital, but I had other duties to fulfil and as the years passed and the Hospital grew and developed, the labour increased rather than diminished. I had not only the medical charge of the patients as Physician, but many other engagements. I had a considerable private practice, then I had to attend the meetings of the General and Local Committees and also to perform in London my duties as Physician of the North British and Mercantile Insurance Company. These several engagements necessitated journeys to London, at least twice a week, winter and summer and in all weathers. The journeys were very trying, as they entailed much waiting about and exposure, since the sea had to be crossed and there were several changes of conveyances, cabs, tramcars and railways.

I had now resided at Ventnor for a period of nearly ten years, during which I had been incessantly occupied, more particularly in the establishment of the hospital, to which object my utmost efforts had been and still were devoted. What with the hospital, my private practice, laboratory work, my bi-weekly journeys to London, it will not occasion any surprise to learn that my health suffered a good deal and that I felt the strain of so much labour both of body and mind, some of which was not unattended with anxiety. The original plan of the hospital being well advanced and seven of the eight blocks of houses completed, as well as the Chapel, with the exception of the spire, and the future of the institution well assured, I determined to have a period of rest and change. My position as founder of the hospital entailed not only much responsibility but many and varied duties. I was at this time a Vice President, one of the three Trustees, the others being the Hon. C. Pelham and Mr. Stafford H. Northcote, honorary but acting visiting physician, a member of the General and Chairman of the Local Committees. Prior to my departure from Ventnor, I resigned the office of physician and the chairmanship of the Local Committee. My influence secured the succession of the former office to its present holder, while I became one of the Consulting Physicians.

PRESENTATION OF A SERVICE OF SILVER.

On quitting Ventnor, I was presented with a Service of Silver and a purse of three hundred giuneas. The Salver bore the following inscription: "This Silver Tray, together with a Tea and Coffee Service, also a purse of three hundred guineas were presented

By a numerous body of Subscribers

to

Arthur Hill Hassall, M.D.

In recognition of his valuable Services as the Originator and Founder of the Royal National Hospital for Diseases of the Chest"

May 1877.

THE RIVIERA.

While at Ventnor I had heard and read of sunny lands in which flowers blossom all the year round, even in the so-called winter months, where orange and lemon trees with their fragrant flowers and golden fruit flourished, and the mountain sides are covered with evergreen olive trees, where date-bearing and other graceful palms abound, and where, in fine, the climate is mild, the scenery enchanting, and where nature appears under its most beautiful aspects and I longed to visit those lands. I therefore, after due consideration, broke up my English home. The first year or two were spent chiefly in Germany and one winter season in Cannes. Here I had many influential friends and I was occupied in practice that winter without any interference. My success was so encouraging that I thought I would make that flourishing health-resort, where my ideal of climate and natural beauty was fully realized, my future home. With that view I prepared myself for obtaining another medical diploma in order to acquire the right to practice in France. I visited Marseilles where the examination for the diploma of

"Officier de Santé" was to take place, saw two of the professors, who received me very kindly and in due course a notification was sent to me fixing the day for the examination, a similar notice being also forwarded to Dr. Sparks, who was then at Mentone. The time appointed was near the end of the winter season, but before the day arrived a communication was received to the effect that the examination had been postponed, in consequence of the small number of candidates presenting themselves, but that due notice would be given of the date next fixed. I waited for some time at Cannes, but the season being over there I left for England, forwarding my address and stating I would be prepared to present myself when called upon. I never heard from the authorities again. Dr. Sparks, more patient than myself waited and obtained the requisite diploma.

SAN REMO.

Subsequently I determined to select one of the health resorts of the Italian Riviera, as there were no restrictions as to practice there, and being much pleased with San Remo I settled in that place, which has been my home for the last fifteen years; there I still remain, and I have no doubt that the climate of the Western Riviera has been the means of prolonging my life very considerably. Here I have found professional and other occupations entirely congenial to my tastes and feelings.

Settled in San Remo, I was delighted with my surroundings, everything was so new to me, the beautiful mountains, the blue Mediterranean, the lovely bays and coves and the picturesque scenery everywhere; then the semi-tropical vegetation, the Olive, Orange, Lemon and Eucalyptus trees, innumerable shrubs, fruits and flowers, many being of kinds only seen in greenhouses in England; roses and many other flowers in bloom in midwinter and in such abundance as to allow of the rooms and tables being decorated with them at all times. Then there were the curious old towns, and the inhabitants with their many peculiarities and ways, so different to those of English people.

I soon began to explore the whole sea coast, with the many adjacent villages and towns, from Cannes on the one side, to Genoa on the other, a distance of about 130 miles. I chose each day a limited portion of the district, aided much by the Railway, but yet walking many miles each excursion. Note book in hand I jotted down every particular of interest which fell under my observation and each night these rough notes were with my wife's valuable aid put into form with a view to publication when complete. In this way the manuscript grew apace, till ultimately in September 1879, an illustrated work entitled: "San Remo and the Western Riviera, Climatically and Medically considered", appeared.*

This work contains a faithful representation of San Remo from a picture painted by Dr. Goodchild of Bordighera, an original map of the whole of the coast of the Western Riviera, as well as many engravings. It bears on the cover the charming but somewhat too favourable words of *Mignon.*

"Connais-tu le pays où fleurit l'oranger,
Où la brise est plus douce et l'oiseau plus léger
Où rayon et sourit, comme un bienfait de Dieu
Un éternel printemps sous un ciel toujours bleu."

The many excursions necessary for the production of this work were a source of infinite pleasure.

Later on, in 1883 I prepared and published another work, which while dealing with the climate of the Riviera generally, is devoted specially to San Remo and its Environs and is founded upon wider and more precise observation and experience: "San Remo, Climatically and Medically considered." † This work deals, firstly with the features of the climate of the Riviera, in particular with those of San Remo itself, with its encircling mountains, its bay and protecting headlands, its Meteorology, temperature, sunshine, relative humidity, rainfall and winds; secondly with the Natural History of the district, including especially its characteristic, semi-tropical vegetation, its olive, orange and lemon trees, date and other palms, its eucalyptus trees and the more novel and interesting shrubs, plants and flowers, including a full list of the Alpine flowers; thirdly, passing to the Animal Kingdom, the chief Butterflies and Moths are enumerated, and a few are figured; descriptions are likewise given of some of the Trap Door Spiders and of the battles of the Harvesting Ants; also notices of some of the more curious insects met with, the Cicada, the Praying Mantis, the Mole Cricket, the Firefly, the Blisterfly, the Scarabeus or Sacred Beetle and the Scorpion; fourthly, the chief characteristics of the climate and the effects of this on the several Functions of the Body are described and the Maladies and Diseases for which the climate is suited, as well as those for which it is not well adapted are enumerated and dwelt upon.

For eleven consecutive winter seasons, each being reckoned from the 1st of November to the 30th of April. I regularly took a series of Meteorological Observations. The instruments were

* Longmans, Green, & Co. † Longmans, Green, and Co.

Negretti and Zambra's and the thermometers were placed in a properly constructed Stevenson's screen, placed to the North, and out of reach of the direct rays of the sun The observations were on a much more extended scale than is usual; indeed I do not know of any records relating to a health resort which embrace so many particulars. Each day's record included the north shade temperature, based upon three daily observations; night minimum in the air and on the ground, the sun heat, with both the naked and vacuum thermometers, the daily duration of sunshine, the relative humidity and rainfall, the hours of day rain, and the temperature of the Sea, which being for most of the winter several degrees warmer than the air, exercises an important effect on the climate of the Riviera. The results arrived at were communicated to the British Medical Association at its Annual Meetings and with the exception of the latest communication, they will be found recorded in the British Medical Journal and in my book on San Remo. This last and more complete paper comprising the results of the eleven season observations, is printed in extenso at the end of this narrative.

The Table on the next page sets forth the means of the whole of the eleven seasons as compiled from the data furnished by the above communications. A few minutes examination and consideration of the tabular statement will serve to convey a pretty clear idea of the chief weather characteristics of the climate of San Remo. The meteorological data usually supplied concerning the climate of Health Resorts is in most cases far too meagre to allow of any accurate and definite conclusions being drawn, the observations being generally limited to particulars of the temperature, humidity and rainfall.

The above observations carried on for several years and in all weathers consumed not a little time; certainly not less than one hour a day; this represents for the eleven seasons the large total of nearly 2000 hours and useful as the observations are, I am sometimes tempted to think that the time would have been better bestowed on some more original and scientific enquiry.

THORENC.

The summer and autumn of 1882 were spent partly at Cannes but chiefly at Thorenc, about twenty miles from that town where I had the charge of an invalid suffering from Bright's disease. The locality was very beautiful and healthy, high up in a valley of the Alpes Maritimes and environed by rocky hills and mountains. The air was cool, dry and bracing and therefore most suitable for the

THE WINTER SEASON CLIMATE OF SAN REMO

THE SEASON EXTENDS FROM 1st NOVEMBER TO 30th APRIL

Season —	79-80	80-81	81-82	82-83	83-84	84-85	85-86	86-87	87-88	88-89	89-90	Mean of the 11 years.
Mean Shade Temperature	52.00	54.30	54.40	52.58	53.98	52.72	52.82	52.30	49.61	52.02	52.23	52.63 Fahr. = 11.5 Cent.
Mean Maximum Shade Temperature	57.69	59.70	58.60	56.93	57.99	56.62	56.86	56.10	54.87	56.28	57.05	57.15 „ = 14.0 „
Mean Minimum Shade Temperature	45.76	48.60	48.17	46.76	48.07	46.08	46.32	45.14	45.28	45.83	45.80	46.53 „ = 8.1 „
Times Shade Temperature reached Freezing	6	0	0	1	0	0	0	1	0	0	2	10
Mean Minimum Temperature on Ground		44.80		39.53	42.82	38.44	40.77	38.60		39.88		40.69 „ = 4.5 „
Times of Freezing on Ground		3	0	20	4	29	13	34	12	26	12	15
Snow fell	1	0	0	2	0	0	0	0	0	0	3	

SUN AND SUNSHINE

Season —	79-80	80-81	81-82	82-83	83-84	84-85	85-86	86-87	87-88	88-89	89-90	Mean of 9 years.
Mean Sun-heat by Naked Thermometer		73	72	67.58								
Mean Sun-heat in vacuo	119		115	105.6	108	108	106	105.5		105		109 Fahr. = 43.0 Cent.
Days on which the Sun shone	166	160	167	160	171	163	162	166		157		164 days
Mean Sunshine per day in Hours & Minutes	8.05	7.18	8.27	6.23	7.07	6.51	5.30	5.50		6.12		651 hours and minutes
Total Sunshine in Hours	1331	1177	1412	1171	1290	1242	999	1255		1114		1221 Total Sunshine

In the first 3 years, the mean Sun-heat in Air and in Vacuo is based on the days on which the Sun actually shone, but in the following years it is spread over all the days of the season, even those on which the sun did not shine, hence a lower mean is shewn in the 8 later years.

RAIN, RAINFALL AND RELATIVE HUMIDITY

Season —	79-80	80-81	81-82	82-83	83-84	84-85	85-86	86-87	87-88	88-89	89-90	Mean of 11 years.
Days of Rain (*)	27	43	18	33	25	38	42	30	52	40	31	34 days of rain
Hours of Day Rain	117	81	161	132	91	140	137	134	239	174	145	141 hours of day rain
Total Rainfall in Inches	10.26	16.64	15.27	17.27	10.14	11.21	11.29	23.54	24.45		17.90	16 total rainfall in Inches
Mean Relative Humidity (**)	67.80	68.70	72.80	71.20	71.00	69.36	71.73	68.62	71.60	71.34	68.94	70.28 relative humidity

(*) Every day on which as little as the $\frac{1}{100}$ of an inch of rain fell, is counted as a rainy day.

(**) Based on 3 daily observations, at **9** a. m. **3** p. m. and **9** p. m.

invalid, but it was far away from any town and extremely lonely, there being only a few houses and peasants' dwellings scattered here and there. We used to take long drives to various places in the neighbourhood and our time passed very pleasantly notwithstanding the isolation. There being no available medical service for the poor for miles round, not even a place from which medicine could be procured, the peasants often suffered much and were greatly to be pitied. It becoming known that there was a medical man in Thorenc, numbers of people came to consult me, from long distances, involving much fatigue, some old and feeble, others suffering, unaided and unattended, from heart, kidney and other serious organic diseases. I did what I could for them but when I gave them presciptions they had still far to go for their medicines.

On the whole my time passed very pleasantly and quickly at Thorenc; for one thing I made some additions to my collection of butterflies and moths, by no means a large or scientific one, consisting of captures not in France only, but in Italy, Switzerland and Germany. The collection has at different times afforded much pleasure, marred somewhat I confess by the disagreeable necessity to sacrifice the lives of the butterflies. My pleasure was derived not so much from the beauty of form and colouring of many of the specimens, but was chiefly due to the various excursions, often to choice and charming localities which had to be undertaken to secure the rarer and more highly prized specimens.

But the chief interest of my visit to Thorenc was derived from the novel sight of mountains literally clothed with blue lavender to the exclusion of nearly all other forms of vegetation; it seemed in fact as if the rocks and mountains had themselves burst into blossom. But this lavender was a source of interest in other ways. Each season when the lavender is in bloom, men repair from Grasse to Thorenc and the surrounding district, provided with distilling apparatus and tents. The lavender is then reaped, stacked and distilled, the stills being worked day and night, the men living and sleeping in their tents. I too procured a still and so obtained many samples of oil of lavender, not only from the flowers, but from the stalks and leaves separately and collected many data with a view to the preparation of an article on the French oil, but this purpose has never been carried out nor can it be now; some of the details then gathered have either been forgotten or are only dimly remembered, while the samples of the oil, some of which I still possess, have become changed from age.

San Remo being a Winter health resort I was able to spend the summers elsewhere; for several years they were passed in London, but still my time there was not spent in idleness, or even leisure. I attended the meetings of the General Committee, now called the Board of Management, of what for the sake of brevity I will call my Hospital, and as might naturally be supposed I was still more or less occupied with questions relating to food; its quality nutritive properties and value; its adulterations and impurities.

FOODS FOR INVALIDS.

At the time to which I am now about to refer, the number of articles of food prepared especially for infants, children and invalids was comparatively limited and the composition simple; this time was before the era of soluble, cooked, peptonized, pancreatized and predigested foods, which have now become so multiplied that one is often puzzled to know which to select. The foods for infants and young children consisted for the most part of wheat flour, baked or unbaked, or of condensed milk. It occurred to me in 1875 that something better than the usual wheat flour article might be prepared and in devising this I laid down certain conditions; one was that it should have as nearly as possible the same equivalent nutritive value and approximate composition, when prepared, as human milk; another, that it should contain a constituent which would act upon, dissolve and convert the starchy matter into a form of sugar, approximating to that of the sugar of milk. The food therefore consisted of a mixture of ordinary wheat flour baked slightly, entire wheat flour, malt flour, with its diastase and cerealine, in definite proportions and a little phosphate of potash, the whole being prepared with sufficient cream or milk to furnish the requisite amount of fatty matter of which wheat flour contains only a small proportion.

This food, suitable alike for infants, children, the aged and for invalids generally, possessed a very pleasant flavour and was so much liked by all who partook of it, that it was prepared in quantity, for some two or three years by the well known firm of Goodall, Backhouse, and Co. of Leeds. It was at first in considerable demand but to ensure a permanent sale for almost any new article it is necessary to spend a fortune in advertisements and not I fear to always circumscribe the statements advanced in its behalf within the limits of sober truth.

ADULTERATION OF STARCH.

From the great diminution in adulteration consequent on the exposures made so many years since, it became in more recent times not so easy to meet with fresh instances of the practice, but in the autumn of 1878 I came across a sophistication I had not before encountered. I accordingly made this known to the public in a communication to the *Times*, "On the Adulteration of Starch". I was informed, that it was then a common practice for manufacturers to make two qualities of starch, the one genuine and the other adulterated. To put this to the test, I purchased on the same day and in the same neighbourhood, a series of eight samples of starch, for which I was charged three different prices. On subjecting them to analysis I found the whole of the samples to be adulterated with twenty, thirty and even nearly forty per cent of earthy or mineral matter. This I found to consist of mineral white, terra alba, sulphate of lime or gypsum, a substance which in contact with water quickly cakes and hardens; indeed I learned that these substances were extensively sold to some manufacturers of starch and I had before me at the time I wrote, the circulars of two different firms who offered to supply starch manufacturers with the materials in question. Now the presence of such mineral substances, if not dangerous is at least very objectionable; they may prove irritating to some delicate skins, injurious to the clothes stiffened with them, and positively hurtful, when, as sometimes has happened, such adulterated starches are employed for medicinal purposes. A very simple test whereby these admixtures may be detected is to completely incinerate a small quantity of the suspected starch, when if a solid residue remains, it may be concluded that the sample contains a mineral adulterant.

DETECTION OF LEAD IN POSTAGE STAMPS.

Some months later I addressed another communication to the same journal and part of which I here reproduce: "Fresh instances of the occurrence of lead, copper, arsenic or some other injurious metal either in food, clothing, furniture or other articles of daily use and necessity are constantly being brought to light, but the list is by no means yet complete and my purpose in the present communication is to add to it another instance".

"Those who have occasion to make use of Postage Stamps in large quantities are aware that the colour is easily removed and that the hands and lips, if the stamps be moistened with the tongue, quickly become much stained. Now into the composition of this colouring matter a poisonous metal enters largely. I have recently subjected a number of penny stamps to analysis and in all I have found lead in large quantity, derived doubtless from the red lead employed in the colouring of the stamps. The presence of such a metal must be regarded as highly objectionable and possibly in some cases, injurious, or even dangerous and the knowledge of the fact that they contain the metal in question will, I am sure, cause the Post Office authorities to discontinue the use of such stamps with as little delay as possible."

This expectation, after due enquiry, was realized; the communication from which the above is taken was followed by prompt action which resulted in the abandonment of all injurious pigments in the colouring of postage stamps, and in the production of a series of stamps of a more artistic character, although the design and colouring of some of these were capable of further improvement and were not equal to those of many foreign stamps. I afterwards found it was not alone the penny stamps which were at fault, but some others, as the half penny and two penny stamps.

THE PURE FOOD COMPANY.

Complaints of adulteration still being more or less prevalent, though the practice was far less common, and usually different in kind and character, and the desire being expressed in many quarters to secure the absolute purity of our food, especially of that required by invalids, it was suggested in 1881, that a company which would *garantee* the purity of all articles they manufactured, would meet a want and prove a great success.

Accordingly, a gentleman whose name I am not at liberty to mention, volontarily offered to provide all the funds necessary for carrying out this object.

The company bore the name of "The Pure Food Company". A commodious three-storied building was secured at 4 Princes Street, Red Lion Square, London, and this was named: "The Food Laboratory or Factory". This was fitted up at very considerable expense, with all the machinery and appliances necessary, cylinders, boilers, steam pans; &c. The water used in the manufactory was softened and purified. The chief articles prepared were the following; solidified or Concentrated beef tea, the same with the albumen of the meat, the same with arrowroot, pure beef jelly or essence, albumenous and fibrinous meat lozenges, milk food for infants, the same for children and invalids, pulsella, cooked and in part predigested; extract of coffee prepared by a special process and preserving the full aroma of the berry, extract of coffee and chicory.

The materials used were throughout of the best quality, the meat was bought daily by an experienced person in Smithfield Market.

Several of the processes of preparation and manufacture were new and all that knowledge, skill and money could effect was done. During the summer months, I threw myself earnestly into the project as also did Mr. Otto Hehner F.C.S. We both strove hard for success and were frequently engaged in the Factory till the night was far advanced. The results as regards purity and excellence were everything that could be desired. The Company and its preparations were duly advertised and made known. The prospectus issued stated truthfully the facts without exaggeration. Orders were received in due course, but the sales were not sufficient to cover the current expenses or of course to recoup the outlay and thus this well intentioned venture came to an end and the manufactory had to be closed. This result we strove to avert and deeply regretted, because of the loss to the projector which the failure involved. For myself and Mr. Hehner it may be well to state that our services were gratuitously rendered. So much for the discernment and appreciation of the public, who are ever crying out for, and writing about the purity and quality of food. Had the prospectus contained extravagant and exaggerated statements and assertions and had a fortune been spent in advertisements, then the result might have been different.

INHALATION IN DISEASES OF THE ORGANS OF RESPIRATION.

The subject of the treatment of Diseases of the Organs of Respiration by Inhalation is one which has been much discussed and about which much has been written; by it the remedies employed, when rightly applied, reach the parts affected directly, and hence it is a mode of treatment from which important results were anticipated and which has indeed been greatly lauded by some who have practised the method; still a great deal of difference of opinion has prevailed at to its effects and value. From all that I had observed and read, it became apparent to me, that the methods and apparatus employed were in many cases very defective and that the whole subject needed further enquiry. I therefore carried on for three consecutive summers, aided by the conveniences and appliances of a London laboratory, a series of investigations which in 1885 resulted in the publication of my work on "The Inhalation Treatment of Diseases of the Organs of Respiration".* This was and still is the first and only work in the English language devoted exclusively to the subject of the treatment of affections of the lungs by inhalation, although in America Dr. O. Solis Cohen published a separate book

* Longmans, Green, and Co. Crown 8vo pp. 367 with numerous engravings.

on "Inhalation in the treatment of Disease" as far back as 1867, while Dr. M. J. Oertel brought out a very voluminous work in 1882 entitled "Handbuch der Respiratorischen Therapie".

In my work the subject is treated in seven chapters or sections. In the first the facts connected with *The Entrance of Medicaments into the Lungs* are set forth; from the observations and experiments therein recorded, the following conclusions were deduced.

That substances of a *gaseous nature* and unirritating character pass readily into the air passages and lungs: that the *fumes* derived from the burning of certain mineral and organic substances when unirritating and inhaled with certain precautions also enter with comparative facility; that much of the *vapour of hot water* becomes condensed and deposited in the mouth and fauces before any of it reaches the lungs; the same thing happens except in the case of very volatile substances when the medicaments are added to the hot water and especially is this the case when these possess but little volatility, in consequence of which while some would be carried over by the hot aqueous vapour, the greater part or in some cases even the whole, would be retained in the water of the inhaler; that the medicinal substances, whether volatile or non-volatile contained in *sprays* whether warm or cold also reach the lungs and even the larynx though in greatly diminished quantities, part being lost before the spray enters the mouth and part being swallowed and so passing into the stomach; that the employment of *oral* and *oro-nasal inhalers* for the volatilization and inhalation of such substances as carbolic acid, creasote, thymol and some others, which are in fact scarcely volatile at all at ordinary temperatures, is of little utility in general, since the greater part, or even the whole, of the remedies is retained by the cotton wool or sponge of these inhalers and from which indeed most of them may be recovered after the completion of the inhalation. When it is remembered that inhalation in Great Britain is carried on chiefly by means of oral and oro-nasal inhalers the practical importance of the foregoing conclusion will be at once apparent.

In the second Chapter, "*The principles concerned in the Volatilization and Inhalation of Medicaments*" are dealt with. Under this heading the chief circumstances to be taken into consideration are the relative volatility of the substances employed, the influence of temperature on the rate of volatility, the effect of motion of the air on the same and the size of the area of evaporation. On each of these heads only a few words can here be bestowed. The volatility of different medicaments varies greatly, some pass into a state of vapour rapidly at the ordinary temperature of the air, as alcohol, chloroform and ether, while others do so only slowly as carbolic acid, creasote, thymol and many others. The effect of temperature on volatility is of course very great in the case of nearly all substances used for inhalation; it is greatest in those which are the most volatile and is usually in proportion to the increase of temperature. Tables are given in the work in which the rates of evaporation of a variety of inhalants at different temperatures is shown; these tables are very instructive and form a necessary guide in the choice of the remedies employed. The effect of dry air and of air in motion is also very considerable in increasing the evaporation of all volatile substances as is well known. But there is still a most important principle to notice in connection with the subject of volatility and that is the effect produced by augmenting the surface of exposure. This principle in its application to the subject of inhalation was not well understood till I drew attention to it. It occurred to me that since the surface of exposure in oral and oro-nasal inhalers, which are the inhalers chiefly used, is exceedingly small, if this surface could be very greatly increased there would be a proportionate increase in the quantity of the medicament volatilized. The results of actual experiments were most surprising and proved that the conception was sound even in the case of substances, the volatility of which at ordinary temperatures is comparatively slight, as carbolic acid, creasote and thymol.

This principle was at first thus tested; 50 grammes = 771 grains of carbolic acid were dissolved in enough water to saturate two Turkish towels; these were exposed to the air in a close room, the temperature of which was 22^0 Cent. = 72^0 Fahr. and after the lapse of forty eight hours the towels having become quite dry, it was found, to my great surprise, that the whole of the carbolic acid had disappeared. This experiment was repeated in a variety of ways and with confirmatory results, only that the time of exposure was often reduced even to a few hours; the rate of loss was greatest at first and became gradually reduced as time passed. These trials were carried out in an ordinary chamber with door and window closed and in which there was but little movement of the air, but when the towels were exposed to the outer air, and especially if this was in active movement, the time occupied required for the complete dissipation of the acid was reduced to some three or four hours.

It is difficult to over estimate the practical importance of the above results; by their means the air of inhalation chambers may be quickly charged with a variety of medicaments. Guided by the facts here briefly detailed I have been led to devise a number of improved forms of apparatus for inhalation.

The third section or Chapter is devoted to the subject of the "*Apparatus*" or contrivances employed for inhalation, these are numerous, and are necessarily modified according to the purposes for which they are required, whether for gases, vapours, atomized liquids, or sprays, or for fumes or smoke. Many of the forms which have been devised are now obsolete and useless. To the various oral and oro-nasal inhalers, the fatal objection is that the surface of evaporation is too small, that in general, the volatilization of most of the inhalants employed is so slight that practically inhalation by these inhalers is non-effective and in most cases of no utility whatever.

The new forms of inhalers devised by me were based upon the greatly increased vaporization of certain chemical and remedial substances by spreading them over a very considerable surface, the evaporation being further aided in some cases by augmenting the temperature at the same time. Some of these new forms yielded remarkable and striking results. Some were for the purpose of charging the air of inhalation chambers with the required remedies, while others were for individual use; these several kinds were first described by me in the Lancet for 6th October 1883 and they were subsequently manufactured by Messrs. Maw, Son and Thompson.

In the fourth section, "*Inhalation Chambers*" are described; some of these are for the inhalation of compressed and rarefied air, of oxygen or other medicinal gases; others are for atomized water, or vapour, charged with certain medicinal substances, such as carbolic acid, creasote; &c. In the case of the chambers the difficulty was to charge the air effectually and evenly with a sufficiency of the requisite agent. This difficulty has been completely overcome by the employment of the chamber inhalers devised by me, in which the evaporation takes place from extensive surfaces, aided sometimes by an increase of temperature. The conditions and principles of the construction of such a chamber were first described in an article communicated to the British Medical Association at the Meeting in Liverpool in August 1883 and which was afterwards published in the Journal of the Association.*

With a view to put my chamber inhalers to a practical test I constructed an Inhalation Chamber in my house at San Remo. It was completed and in partial operation for a year; the following are some particulars respecting it. The Chamber had a cubic capacity

* "On the principles of the Construction of Inhalation Chambers" Journal of the British Medical Association January 1884.

of 1170 feet; it had only one door and one window, but no fire place or chimney. The walls were washable and the floor tiled; it was warmed by the admission of a little hot air through a grating in the centre of the floor from a calorifère placed in the basement of the house, the temperature of the chamber itself being raised usually to about 64^0 Fahr. The chief apparatus in the chamber consisted of a very convenient and portable arrangement of a cotton fabric disposed in layers. Each of the five cloths or layers had a superficies on one side of 2500 inches and on both sides of course of 5000 inches. The apparatus was suspended from the ceiling about four feet above the grating in the floor, through which the warmed air entered and it could be raised or lowered at will. The quantity of carbolic acid used to charge the whole of this apparatus was 2000 grains, but it is better to charge part of it only and to begin with 800 or even 500 grains for the first day or two, raising the amount to 1000 or 1200 grains in the course of a few days. The latter quantity is that which I have chiefly employed and this without any inconvenience having been experienced. The time of inhalation has varied from one to two hours daily, but it is best at first to be content with half an hour only. In an experiment in my chamber, the temperature of which was maintained at about only 64^0 Fahr., using 1200 grains of carbolic acid, there were recovered from the apparatus at the end of six hours 500 grains, showing a loss of no less than 700 grains, of this a small quantity only was inhaled, part remained diffused through the air of the chamber, while some was deposited on the walls.

The apparatus used in the chamber above described was of very simple construction. It consisted of a four sided wooden frame, with five rails on each side like those of a clothes horse; to each of these rails hooks were attached and to these hooks five cloths were fastened by means of rings; by this arrangement the cloths could be removed and replaced with great facility.

In another section of the book the question of the "*Quantities of the several Medicaments*" which should be employed, was discussed, particularly in connection with the oral and oro-nasal inhalers. The surface of exposure for evaporation in these inhalers rarely exceeds two inches, now when we consider that most of the more important remedies used in these inhalers as carbolic acid, creasote; &c.; are but little volatile, it is evident that but very small quantities of these inhalants can possibly reach the parts affected, usually the lungs. Some idea of what these quantities have amounted to in the past may be formed from what follows. Recipe, Tinct. iodi etherea 60 grains, acidi carbolici 20 gr., creasoti, or thymolis, 60 grains, sp. vini rect. to 1 oz. Of this twenty minims were directed to be dropped on the cotton wool or sponge of an ordinary oral inhaler; this quantity was deemed sufficient for two days, there being two daily inhalations, each of half an hour's duration. Now the ounce would furnish enough of the mixture to charge the inhaler twenty four times and to last forty eight days, thus the amount of carbolic acid for each day would be $2^1/_2$ grains and for each inhalation $1^1/_4$ grains and of the creasote just half these amounts. But this is not all, only a fraction of these minute quantities would actually reach the lungs, since part would not be volatilized at all, but would be retained in the sponge and another part would be arrested in the mouth and fauces.*

Now the formula above given was a well known and favourite one and has probably been more frequently employed than any other for charging oral inhalers. It is therefore surprising to learn that the most encouraging and favourable results have been published as arising from the employment, in cases of consumption, of inhalers thus charged. Certainly any curative effects accruing therefrom should be assigned to faith and not to physic.

The treatment of diseases of the respiratory organs by inhalation has been a favourite one and much was expected from it, but opinions have differed greatly as to its effects; many have lauded the practice, but others have expressed disappointment; the latter being more easily explained than the praise. Prior to the publication of my work, the laws and principles which govern the volatilization of the medicaments used for inhalation, had not been investigated and determined, the absence of exact knowledge led to the employment of forms of apparatus which were of little or no utility, as also to the use of remedies some of which were not volatile, while the volatility of others was so small that the greater part of them was recoverable from the inhaler at the end of the inhalation. It may be stated of the work in question without exaggeration, that it changed all this and that it in fact revolutionized the subject. It supplied the requisite knowledge of the facts and laws relating to evaporation and in it were described new and efficient forms of apparatus suggested by that knowledge, while lastly it furnished data whereby the quantities of the inhalants which should be used could be determined.

I had hoped that the publication of my work would have imparted a new impetus to the practice of inhalation and that more

* Inhalation Treatment p. 98.

favourable results would thereafter be recorded, but in these anticipations I have been disappointed; while some of the old methods have been disused or discarded, the new have not been put to any adequate tests, so that the general effect has been to check for a time to the practice of inhalation, of which indeed but little has been heard or reported for some years past. One reason of this is, that the busy practitioner cannot afford the time or give the attention necessary to the carrying out of this method of treatment. The system can only be successfully practised by medical men who devote their whole time to it, who make it a specialty and possess the needful arrangements and apparatus. It is very desirable, that the method should be tested on a large scale in connection with one of our hospitals for consumption. I consider, that hitherto it has not had a full trial on an exact and scientific basis. When this has been accomplished, I am hopeful of beneficial results.

Since the publication of my work on Inhalation I have devised another inhaler, the principle and form of which differ from any of the arrangements in use. Of this Inhaler a description and figure will be found in the Lancet,* but a short notice of it may be not inappropriately given in this place. It occurred to me, that if air more or less dry were passed through a medicated solution it would take up an amount of that solution sufficient for medical purposes. To test whether this supposition were correct or not, I adopted the following apparatus; it consisted of a glass cylinder nine inches high and nearly two inches in diameter, closed at the top by an accurately fitting cork. In the cork four tubes were inserted; one of these, larger than the rest, pierced the cork and entered the cylinder for about an inch; the other three tubes were much smaller and longer, reaching almost to the bottom of the cylinder. In this were placed 175 cubic centimetres, equal to 2700 grain measures of a mixture consisting of distilled water and 17.50 grammes, equal to 270 grains, of pure crystallized carbolic acid. The three smaller tubes were used in preference to one larger one, because by this arrangement the air passed from the small tubes into the solution in the form of a number of small globules, instead, if only one tube had been used, of a few large bubbles. In the latter case the air would not have been brought into sufficient contact with the water and such a commotion would have been created as to cause spurting of the liquid and the liability of its occasional entrance into the mouth. This occurrence is entirely avoided, provided the due proportions of the apparatus are observed

* "On a new Method of Inhalation and a New Form of Apparatus". Lancet 30 January 1886.

and a sufficient space be left between the surface of the medicated liquid and the end of the tube which forms the mouthpiece. When this apparatus is in operation the air is drawn through the three small tubes; from the bottom of these it freely escapes, ascending through the liquid in bubbles which burst on the surface and the air thus charged with the medicament is inhaled through the tube placed in the mouth.

The following experiment, one of several, shows the effect of this inhaler. Temperature of the air 49° Fahr. relative humidity 80; the apparatus was placed in a jug of warm water, which at the commencement had a temperature of 120° Fahr. This heat caused the watery solution of carbolic acid to rise to 62° Fahr. The air inspired contained so much carbolic acid that in a few minutes it caused the throat to feel sore and somewhat painful; the inhalation had therefore to be carried on at first slowly and intermittently. At the end of thirty minutes 8.5 cubic centimetres of the mixture had passed over, or 17 cubic centimetres in an hour, containing 27 grains of carbolic acid. The residual mixture was found to be as strong after the inhalation as before. If the temperature of a ten per cent aqueous solution of carbolic acid be very low, the mixture presents a somewhat milky character, owing to a portion of the acid not being completely dissolved, but when the temperature is raised, the solution loses its milkiness and becomes quite transparent.

It is manifest therefore that it is possible by means of this inhaler to introduce into the lungs a great variety of medicaments and this to such an extent that the quantities would have to be carefully regulated in order to avoid any undesirable consequences.

THE PLASTERING OF WINE.

I have occasionally in times of leisure, endeavoured to accomplish some purpose I deemed desirable by means of invention. It is well known that the juice or must of the grape intended for the preparation of sherry, port and some others wines is, in Spain, Portugal and the South of France commonly dusted over with a considerable quantity of burnt gypsum, which consists of sulphate of lime or plaster of Paris; to this, frequently chalk or carbonate of lime is added and sometimes quicklime and chloride of lime. The practice of plastering wine is a very ancient one.

The chemical effect of adding sulphate of lime to the must consists principally in the decomposition of the tartrate of potash of the must and the formation of tartrate of lime, which is precipitated, and sulphate of potash, which remains in solution. When chalk is used as well, then part of the free acid is thrown down and the acidity is lessened. Another effect of the gypsum is that some of the soluble phosphates are also precipitated, as phosphate of lime. The process of plastering the must therefore, brings about important chemical changes; these are not for the most part to the advantage of the wine: the gypsum, sulphide of lime, and the fumes of burning sulphur also often employed, may help to control fermentation and to clarify the wine, and the chalk to remove undue acidity, but these substances also withdraw the useful phosphates and wholesome bitartrate of potash, either wholly or in part. Then the quantity of sulphate of potash formed is very considerable. According to Thudichum, the amount ranges between 361 and 1692 grains per bottle. I have found it to vary from 180 to 546 grains.

Now sulphate of potash is a bitter and aperient salt and its presence is a very serious injury to the flavour and quality of the wine; hence it is in the inferior descriptions that the practice most prevails and that the largest quantity of this salt is met with.

It occured to myself and Mr. Otto Hehner long since, that if the plastered sherries and other wines could be deplastered, the bitter sulphate of potash destroyed and the tartrate of potash restored, the flavour and quality of the wine would be greatly improved. Accordingly a process was devised which fulfils these objects in a satisfactory manner and for which protection was obtained.* Shortly afterwards a notice was received to the effect, that our invention was an infringement of a Patent taken out by Dr. Thudichum and in the end we had to surrender all claims and to pay £ 50, the cost of a patent. Thus this cunningly conceived process turned out a somewhat losing affair, as it probably would have proved to be in any case.

A REAL SAFETY BOTTLE.

Coming to a comparatively recent period, my thoughts were directed to the subject of "Safety Bottles", in consequence of the reports which appeared from time to time in the journals of the loss of valuable lives through taking by mistake or carelessness, various poisonous substances and mixtures intended for external use or for disinfecting purposes. A variety of contrivances have been described, many of which have been patented, with the object of diminishing this great risk and evil. Of these the only ones in use have reference to the shape and colour of the bottles employed, but these very

* See "Food; Its Adulterations and the Methods for their Detection". Longmans.

simple means have not proved sufficiently effectual. Hence I was led to devise a bottle which will secure absolute safety. In the medicine and indeed in all other bottles now in use, the neck is placed at the top of the bottle, in the centre, and is perforated for the cork or stopper. In my bottle this neck is closed altogether and the stopper is replaced by a glass bulb or knob; the true neck is placed at a little distance from the now superseded or false neck, but at a somewhat lower level. Thus my Safety Bottle is two necked. Now it is evident that such an arrangement affords absolute security; no person seeing such a bottle, or even feeling it in the dark could for a moment fail to distinguish it from an ordinary bottle; thus by the employment of such a simple means of ensuring safety, the lamentable accidents which now so frequently occur would be rendered almost impossible. This two necked bottle admits of some modifications; the rounded fixed and hollow knob may be provided with an aperture at the top so as to allow the interior of the knob or bulb to be coated over with a phosphorescent or luminous paint, the bottle being thus rendered visible at night. This proceeding is however unnecessary, and if adopted the luminosity would not I believe be lasting and effective, the double neck affords in fact all the security requisite. Another modification more ingenious but equally unnecessary is the following: there being no connection between the central or false neck and the interior of the bottle and this neck being somewhat enlarged, a small bell attached to a watch spring and cork, may be inserted; of course on the removal of the cork the bell rings and an alarm is given.

Impressed with the importance of the subject I obtained in 1888 provisional protection for the invention, which I thought it best to secure under the impression that it would be easier to find some manufacturer or other interested person to take the steps necessary to bring the safety bottles into use. The medicine bottles now employed are for the most part machine made; many being moulded, but these safety bottles would require to be hand made, the proximity of the two necks forming a difficulty; they would therefore have to be made by a manufacturer of hand made glass, such as the maker of chemical glasses and apparatus; this would enhance the price somewhat, but the cost would not be by any means prohibitive. I succeeded in getting some of my safety bottles of different sizes exceedingly well made by an English firm of bottle manufacturers.

The bottle fulfilling the purpose of ensuring safety and so saving life, all preliminary difficulties of manufacture being overcome and the price moderate, it might be supposed that the bottle had only to be made known to come into general use. It was made known, described and figured in June 1890 in the *British Medical Journal** and the only result of this proceeding was the receipt of a single letter from a medical man, who wished to know where he could obtain some of the bottles. The manufacturers applied to were indifferent, thus the matter rests and lives will continue needlessly to be sacrificed as they have been in the past.

METALLIC FASTENINGS.

Amongst the minor non professional matters which have received a certain amount of consideration, is that of metal fastenings for securing buttons. There are but few people who have not experienced many times in their lives the annoyance and inconvenience arising from the loss of buttons fastened in the ordinary way with thread: I therefore was led to consider whether some more desirable method could be devised as by a metallic arrangement. In order to ascertain what had been done in this direction, the Catalogues of the Patent Office were searched with the result that I found a great number and variety of inventions had been patented for this purpose. These were in nearly every case for metal fastenings; some of the arrangements were very simple but not efficient, some useless and others very ingenious and more or less effective. On making enquiries as to which of these inventions were really in use, I found that their employment was almost exclusively confined to articles in leather, gloves, boots, shoes and gaiters. I did not meet with even one which was in request for woollen or linen goods, for coats and trowsers. Several of the patents were so much alike and the differences so trivial and immaterial that there seemed to be no adequate reason for such a repetition of patents for what practically was one invention. I presume the main reason why none of the arrangements or contrivances, hitherto made known, are in general use is, that they do not fulfil the object for which they have been devised in a practical and unobjectionable manner, though here too as in so many other cases, indifference may in some instances have been the chief cause. I planned the following arrangement. To the under surface of a stout button of metal, bone, glass or any other suitable material, a short square, hollow shank was attached, split up at each corner; the shank was now passed through the cloth or other fabric, the four split sides of the shank were passed through four small or one large aperture in a circular disc or diaphragm

* "Safety Bottles." British Medical Journal June 14 1890.

and they were then opened out and turned back over the diaphragm and if needful for greater security, turned again over the edges of this. Protection was obtained for this invention which I myself put to practical tests. It is quite conceivable that for many purposes the use of such fastenings would be preferable, as being more durable than those of thread only; indeed there would appear to be an ample field for the employment of such metallic fastenings in the clothing of soldiers, sailors, sportsmen and in all cases in which additional strength and security are needed. It is of course necessary that the shank and diaphragm should be sufficiently thick and strong so as not to yield readily to pressure. These fastenings when the garments are made up would not be seen as they would be covered by the linings.

NEW FORM OF CARRYING CHAIR.

Another contrivance was a form of chair whereby a child or invalid of light weight could be carried up and down stairs, or for short distances, by the aid of only one person or porter. It is of very simple construction, weighs only some four or five pounds and costs but a few shillings. It consists of a skeleton chair, without legs and with a very low back. The chair is furnished with two strong metal handles one on each side, large enough to admit the hands of the carrier and with a long broad leathern strap provided with a buckle and eye holes so that the strap can be lengthened or shortened at will and secured to the chair by being passed through two metal loops at the bottom and near the front of the chair. The dimensions may be varied, but the following measurements are those of a chair of convenient size, height of back 12 inches, breadth 17 and depth 15 inches. For a sketch of the chair, see the British Medical Journal 11th October, 1890. The strap is intended, of course, to be passed round the neck of the carrier, from which arrangement it will be apparent that the weight is distributed between the hands and arms, neck and shoulders of the bearer. This chair has been found to answer admirably, as unfortunately, I was at one time able to testify from personal experience; it is extremely light and portable and unlike the ordinary chair with long handles, it occupies but little room and hence is more convenient and manageable, and needing only one, in place of two porters, its use is less costly.

DR. BERGEON'S TREATMENT OF CONSUMPTION.

A few remarks may here be introduced in reference to a mode of treatment of cases of phthisis which bears the name of Dr. Bergeon.

It consists in the administration of copious lavements of the sulphuretted mineral water of Eaux Bonnes.

The testimony in favour of this treatment, derived from different sources, has been of the most favourable character. It was considered that the benefit derived was due to the sulphuretted hydrogen, which the above named water contained. I therefore thought it of importance to determine how much of the gas was actually introduced into the system and with this view, I caused some determinations of the actual amounts of the gas present in the water to be made.

The quantity of the water used on each occasion by Dr. Bergeon was half a bottle; this was found to contain rather more than one tenth of a cubic inch of the gas. More recently however, recourse has been had to other sources for the sulphuretted hydrogen; thus Dr. Burney Yeo states, that Dr. Bardet has employed a solution of sulphide of sodium in the proportion of 10 grammes of the salt in 100 c. c. of distilled water. One cubic centimetre of this solution contains 10 cubic centimetres of sulphuretted hydrogen, which is set free by the action of tartaric acid. Of this solution 10 cubic centimetres in 200 grammes of water, equal to 3086 grains are employed for each injection, so that if the whole of the gas were liberated, the quantity injected would amount to 100 c. c., equal to no less than 6.10 cubic inches. Here then is a remarkable difference in the amounts of the gas injected in the two cases, in the first one tenth of a cubic inch and in the other over 6 cubic inches or 60 times as much, yet the results reported were equally favourable. It is therefore not surprising, that a suspicion should arise that any benefit accruing from the practice of Bergeon's Method should be attributed to other causes than the sulphuretted hydrogen. According to the reports, the frequency of the pulse is reduced, the temperature lowered, the night perspirations relieved, the expectoration diminished, the nutrition greatly improved, the weight rapidly increased and a great and surprising amelioration in every respect experienced.*

PROJECTED UNDERTAKINGS.

There are not many events which remain to be recorded; the narrative has already extended over a period of more than 70 years, from boyhood, youth, manhood, middle life, to advanced age and there is now but little more to tell. There are few projects or objects

* On Dr. Bergeon's New Method of treating Consumption The Lancet 2nd July 1887.

upon which my mind has been set during this long period, which I have not accomplished and if their fulfilment has not always brought success in their train, this has arisen usually from causes, such as indifference, or want of appreciation of the public, beyond the control of the narrator. Still amongst so many undertakings it has occasionally happened, that some which have been commenced and partly executed have been abandoned; that one or two have been projected, but were never even begun. Thus, about the year 1874, I published at my own cost a monthly periodical under the title of: "Food, Water and Air"; it duly appeared for a long time, but as the expenses were very considerable, the labour great and the results not encouraging, it was at length given up. Had it been more successful, it would have done good service in the furtherance and elucidation of those sanitary questions which were then only beginning to receive the attention which their public importance demanded and which has since and still continues to be so freely bestowed, with such vast benefit to mankind.

The first edition of the British Pharmacopœia was published in 1864; the descriptions and directions were very brief and to follow and understand them fully much botanical and chemical knowledge was necessary. I therefore set to work vigorously to prepare a translation of this Pharmacopœia with copious notes and explanations and I had in a very short space of time accumulated a considerable pile of M.S., when Squire's Translation appeared; this was so concise and practical and supplied the greater part of the information required, that I thought it best not to prosecute my undertaking any further, so here was a case of "love's labour lost." My work, had it appeared, would have dealt more fully with the botany and chemistry of the processes than Squire's excellent Companion.

A partly realized project was the production of a work of a practical character, consisting of a series of monographs or chapters on such subjects as Water, Air and Food in all their many bearings; thus the article on water would have dwelt not only on the subject of its purity for drinking purposes, but it would have embraced more than this, while the chapter on food would have included not merely its purity, but its wholesomeness and freedom from the germs of disease, the nutritive value of the several kinds of food, animal and vegetable, their digestibility and the processes of the digestion of each kind or class. The M.S. of a portion of such a work as I have indicated, is still in existence and had the whole design been carried out, it would have differed considerably from the very excellent works on Hygiene which have since been produced.

Then I had a project in my mind for a great work devoted exclusively to the beautiful subject of water only, in all its different conditions, aspects, uses, &c., but nothing was actually done towards the realization of this conception.

RESUMÉ OF MEDICAL PRACTICE.

From the nature and extent of the enquiries and investigations in which I have been engaged it may have been surmised, that I have had but little time to devote to the practical duties of my profession. Any such supposition would however be very incorrect. Possessing the requisite qualifications, in 1845 I settled at the Norland Estate, Notting Hill and as already mentioned acquired a fair practice; this however at the end of some five years in consequence of illness I disposed of, moving thence to London. While at Notting Hill I was appointed a medical officer of the Kensington Dispensary, a very useful institution to which most of the medical men of the district were attached.

Arrived in London and as soon as I had sufficiently recovered from my illness, I began to practise first in Bennet Street, St. James's and afterwards in Wimpole Street. It was not long before I was made one of the Physicians of the Royal Free Hospital, as elsewhere explained. I held this post for about fifteen years, became the senior physician and only relinquished this position as well as my private practice, in consequence of the still more serious illness which befell me in 1866. At the Free Hospital I used to attend the medical out-patient department regularly twice a week, seeing on each occasion a great many of the applicants and I had more beds at my disposal for in-patients than is usual in the case of the older hospitals, owing to the staff of that Hospital being less numerous. The field of practice and the opportunities for acquiring experience were thus very considerable. It was while in London as already noticed, that I became attached as physician, first to the United Kingdom Life Assurance Company in 1852 and afterwards in 1862 to the North British and Mercantile Assurance Company. These appointments enabled me to acquire experience of a different but very valuable kind. I was thus led to study and weigh the influence of predisposition and heredity and to arrive at approximately correct opinions as to the duration and value of lives for insurance.

The sudden and alarming illness in Wimpole Street, virtually terminated my career in London. After a protracted convalescence I found myself transported to Ventnor. Here with returning strength, hope sprang up afresh within me and led to efforts to repair to some extent the damage done alike to my health and prospects. By the advice of friends I was induced to make Ventnor the next scene of my labours. There I soon commenced to practise and was busily occupied as already described, with the proceedings incidental to the establishment of the Hospital for Consumption. I acquired a considerable clientèle and gained much special experience as honorary physician to the hospital. After ten years residence at Ventnor, for reasons already adverted to, mainly connected with health, I determined to give myself a good rest and to pass the remainder of my days in a warmer climate, such as that of the Riviera. In due course this determination was carried out. After travelling about and residing in Germany for a time, I repaired to the Riviera and first to Cannes, where I passed one winter season and had influential friends. Here I acquired a certain amount of practice and should have remained there altogether had the examination for the diploma of Officier de Santé taken place at the time appointed. The postponement of this and the fact that practice in Italy was free to all duly qualified medical men, led me to take up my abode in San Remo, from which place I am now writing, where I have resided for fifteen winter seasons and which I now regard as my permanent home. For the first few years after settling at San Remo, I used to be occupied in London during the summer months, but the necessity to repair there having gradually ceased and as the Swiss medical authorities very liberally and voluntarily conferred upon me the Federal Diploma, I have for the last few summers practised in Lucerne and still continue to do so, as also at San Remo in the winter. It will thus be seen, that with the exception of a few comparatively short intervals, occasioned mostly by illness, I have been engaged in actual medical practice for a period of fully fifty years. I have given this recapitulation because in the preceeding narrative the references to natural history, chemical, microscopical and various other investigations have been so numerous and extensive that they have rather thrown into the shade my strictly professional work, indeed I am sometimes referred to by the public and even occasionally by my professional brethren as "the analyst", and this sometimes rather in disparagement of my medical claims, as though the possession of a little extra scientific knowledge could be any disadvantage or detriment; that it should ever be so considered, can only be due to ignorance or an intention to prejudice. The analytical enquiries in which, after being some years in practice I became engaged, were brought about by what may be termed an accident; they lasted for a time only and did not divert me from the chief purpose and occupation of my life.

It was the application of the microscope to the detection of adulteration and the interest and importance attached to the subject, which led to its being followed up by me until legislation was secured.

CONDITIONS OF PRACTICE ABROAD.

Having been abroad now for about 17 years, partly in Germany and France, but chiefly in Italy and Switzerland and having been engaged in practice both in winter and summer nearly the whole of the time, I have become well acquainted with the conditions and circumstances under which the profession of medicine is carried on in those countries by foreign medical men and as the subject is not without interest, I will notice some of its chief features.

In the first place foreign medical men in most European countries, no matter how highly qualified or experienced thay may be, are not allowed to practise even amongst their own compatriots, unless they obtain a medical qualification or diploma of the country in which they propose to practise, and to acquire this they have to submit to certain onerous conditions, not the least of which is to undergo fresh medical examinations in a foreign language. A foreigner or invalid residing in any of these countries although he spends his money there, is not allowed to be attended by a medical man of his own nationality, but is compelled by law to accept one who is ignorant of the patient's peculiarities and is either unacquainted with his language or speaks it so imperfectly, that it is not possible for him to arrive at that complete understanding of the case which is so essential. The requirements in question are strictly enforced and are often entirely prohibitory in Germany, Austria, France, Switzerland and most other countries and in place of more liberal views obtaining as time passes, the restrictions are being more rigidly observed and the difficulties increased. In France the grade of Officier de Santé has recently been abolished and any foreigner desiring to practise in that country amongst his own people is now required to pass through the same course of study and to obtain the same diploma as Frenchmen.

In Italy however the authorities are much more liberal; no examination is needed and permission is given to practise amongst his compatriots to every duly qualified medical man producing his diplomas for registration by the municipal authorities of the town in which he proposes to reside.

Personally I have suffered but little from these restrictions. The Swiss medical authorities as already stated, very generously and quite unexpectedly, bestowed on me some years since, the Federal Diploma, which confers the right to practise in any part of Swiss territory.

The effect of the restrictions in force in most European countries is to limit considerably the number of foreign medical men and to throw part of the practice into the hands of the native doctors; still the restrictions possess some advantages for those foreigners who have acquired the right to practise, since they lessen foreign competition, increase the chances of success and afford greater security.

In Italy the absence of any restriction has just the opposite effect, competition is keen; there are usually more foreign medical men in most Italian health resorts than can possibly gain a living, so that each year is generally marked by the departure of some and the arrival of other aspirants. Still the number of those who contrive to hold out and to remain, is generally in excess of the requirements and the amount earned, except in a very few cases, is very inadequate. If there be enough occupation for two English practitioners, the chances are there will be four to share the proceeds.

The right or a permission to practise abroad having been obtained, there are other drawbacks and difficulties to be encountered. The first is to find a suitable opening, this is not easy; the number of available resorts is not by any means great and openings are few and far between.

Another of the drawbacks in most foreign countries, besides those arising from excessive competition, is the difficulty of establishing a reputation, no matter how high your qualifications, or what your experience or ability. The bulk of the patients of one season do not return again and so you lose them; the fact of your having won their confidence is of but little avail and each season has to be begun afresh. Professional attainments go for little; the newly arrived invalids have not the knowledge necessary to guide them, besides they are usually consigned by their ordinary medical attendant to one particular practitioner, so that in most cases they really have no choice.

Then the winter seasons are short and are becoming shorter. In the Riviera they used to begin on or about the 15th October, at which date most of the medical men are at their posts; now it is not till December that practice commences in earnest and in consequence the time is too short in most cases to ensure an adequate return. The shortness of the time and the smallness of the results render it necessary, that most of the English medical men practising abroad in winter resorts should also have summer stations and this constitutes another serious drawback. The stations are found chiefly in Switzerland, though the number of eligible places there is very inconsiderable and the rewards not great.

Again, the number of invalids consigned to the Riviera and especially of those suffering from phthisis and other lung affections is much less than formerly; many "poitrinaires" now go to the mountain stations, while others repair to the warmer climates of Egypt or Madeira. Further, of those invalids who do come abroad, some arrive armed with prescriptions and they often keep up a regular correspondence with their own medical men in England, so that perhaps they may not have occasion to consult any of the local medical men even once during their stay.

Although there are fewer invalids, yet there are more people than formerly who come abroad in the season to escape the trials and risks of an English winter. Amongst these of course, illness may and does arise; thus a great change has taken place in the character of the practice; this has lost in a measure its peculiar and more permanent features and is now more of an ordinary or casual description.

Formerly, the English medical man practising in some of the health resorts of the Riviera would find a certain number of patients awaiting his arrival; now he has scarcely anything to do till November or December is far advanced.

Another change in the character of medical practice abroad is, that it has become very much an affair of society, rather than of professional qualifications and this remark applies more particularly to certain of the smaller towns. Many of those who are sent to foreign winter stations arrive as strangers and since they have much to learn in coming to a new country, they are in many cases glad of any attention which may be shown them. Very soon after their arrival, they are pretty sure to be called upon by one or more of the wives or friends of the doctors; the calls are of course returned, introductions and invitations follow. The medical man, who from disinclination, disapproval or other causes keeps aloof from these proceedings and who trusts to his professional status, will in most cases soon find himself outstripped.

So great is this social influence that there are some men who keep themselves always "en évidence"; they are always to the front; they are to be seen everywhere and they take part in everything; they entertain, give receptions, and endeavour to lead the society of the place; they become churchwardens, prominent and directing members of the English and Lawn Tennis Clubs, the promoters and patrons of most of the balls, dances, concerts, bazaars and other entertainments. They are on the committees for these and have a voice in the issue of the invitations. It is curious to notice how often in the lists of the patrons and patronesses of the several entertainments, the same names occur and how nicely and cordially in this way they are enabled to act together. As important factors, the goodwill and aid is sought of the consul, chaplain, house agent and English chemist; strange to say, this latter functionary is sometimes asked to advise the visitor in his choice of a medical man and in doing so, he of course does not consider which doctor most patronizes his pharmacy.

In one town of very moderate dimensions, there are two English churches, one Low and the other High, and medical men act as churchwardens in both, an arrangement not altogether free from objection.

But there are still other matters to be taken into consideration. For anything like marked success, it is necessary that the medical man practising in a foreign health resort should stand well with the heads of the profession in England, the consultants in London and the other cities of the kingdom. The importance of this is well understood by some of those who practise abroad and part of the summer is devoted to calling upon some of the leading men with a view to strengthen their position, to increase their connections and to act as gentle and polite reminders of the callers' existence. The consultant under these circumstances is apt, not unnaturally when he sends patients abroad, to select medical men, who are to some extent personally known to him.

Abroad the English Code of Medical Ethics is not so operative as at home and indeed to meet the special circumstances of the case, some new regulations might be framed with advantage.

Much more might be said on the subject of the drawbacks and disagreeable surroundings, which in some instances are attached to medical practice in foreign health resorts, but I will refrain from pursuing the matter further.

Now it must not be supposed that the social strivings and rivalry referred to, prevail generally; these must be regarded when carried to their extreme length, as exceptional.

Amongst Italian medical men a very liberal feeling exists; I have found them invariably pleasant and polite to their foreign confrères, free from all petty jealousies and intrigues. In Switzerland it is different and foreign medical men encounter more than the usual difficulties; the motto acted on is, "Switzerland for the Swiss".

Notwithstanding the substantial benefits conferred upon the country by the presence of so many English people, English medical men are regarded with marked disfavour. The hotel and pension proprietors will seldom call them in if they can help it, but will find excuses for not doing so and but little cordiality is extended to their foreign brethren by the generality of Swiss medical men, even when they hold the same qualifications as themselves.

ON THE COLOURATION OF THE LEAVES OF PLANTS.

Having lived in the Riviera for so many years, my attention could not fail to be directed to the brilliant colours presented by the leaves of many plants, especially in the autumn season. The phenomena attending the changes at that season affect alike the leaves of both deciduous and evergreen plants. The latter in the mild climate of the Riviera are more abundant than the former and hence the transformation in the autumnal foliage witnessed in colder countries, where the deciduous trees most abound, is seen on a grander scale than in the Riviera, but this difference is fully compensated by the greater brilliancy and depth of the tints produced in the leaves of some of the evergreen shrubs and trees: these in some cases are so striking as to cause the leaves in many gardens to rival the colours of the flowers themselves.

Among the numerous plants which present this change of colour in a striking form, the numerous varieties of geranium, especially the ivy-leafed kinds, the vine and the American creeper may be mentioned.

But brilliantly coloured and flower-like leaves are freely met with at all seasons of the year in the Riviera, the colour of many of these being natural and independent of autumnal change; such leaves are seen in many species of the genera acheiranthus and which in consequence are so much used for the borders of flower beds and for other ornamental purposes.

Observing so constantly the remarkable and often very beautiful changes in the colours of the autumnal foliage, I was naturally led to seek for the causes of these changes. On referring to the few botanical works at my command, I found that the references to this subject were very brief and the explanations given not always satisfactory. I therefore determined to investigate the matter myself in the hope that by the aid afforded by chemistry and the microscope further information might be obtained. I commenced the investigation in the autumn of 1891 and continued it with little interruption for nearly eighteen months. The results arrived at were detailed in two communications. One was entitled: "On the Colour of the Leaves of Plants and on their Autumnal Changes". This was communicated to the Royal Society and was read and deposited in the Archives of the Society in November 1892. The title of the second paper was simply: "On the Colouration of the Leaves of Plants". This embraced some further observations and was forwarded to the Society a few days before the first communication was read.

I will now describe very briefly and in as simple a manner as possible, the chief facts and conclusions embodied in the two articles above named.

Leaves consist chiefly of woody fibre, vessels, cells or utricles and stomata; the fibre constitutes the framework or skeleton of the leaf, while the cells cover and clothe this skeleton.

The ordinary green colouring matter or chlorophyll is contained in the cells in the form of minute and special granules, which do not however consist wholly of the pigmentary material, but contain other constituents.

Chlorophyll is but little soluble in water, owing partly to the presence of a waxy or resinous substance, but it is readily extracted from the comminuted or powdered leaf by the action of absolute alcohol, spirits of wine, chloroform, ether and some other solvents. If the leaf be entire or in large pieces, the solvent is unable to reach the chlorophyll owing to the protection afforded by the but little permeable epidermis and hence the necessity for the comminution or pulverization of the leaf.

The chlorophyll thus procured is by no means pure, but contains a variety of constituents; neither is its composition constant, but it varies considerably according to the nature of the solvent employed. Thus the chlorophyll obtained by absolute alcohol will not in general have the same composition as that extracted by anhydrous ether: again its composition will vary to some extent even when the same solvent has been used according to the plant and leaf from which it has been extracted.

All this shows that, as obtained by the ordinary methods resorted to, the product is not pure chlorophyll, but a very variable mixture of substances soluble in the vehicles employed; indeed it may be doubted whether it has been as yet procured in a really pure condition. Perhaps the chlorophyll obtained from a strong alcoholic solution by precipitation with water, is as near an approach to purity as has hitherto been reached.

Hence the composition of chlorophyll as usually obtained is uncertain, whether even nitrogen is always present is doubted by some, but iron is universally regarded as indispensable.

There is no doubt however that chlorophyll proper consists of a mixture or combination of two colours, a blue and a yellow, the blue pigment being readily convertible into a red by an acid. The presence of two colours may even be seen in some cases, by watching the evaporation on a white porcelain surface of two or three drops of an alcoholic solution of chlorophyll derived from some plant of simple organization, as the leaves of a young fern.

When extracted from the leaf by alcohol or some other suitable solvent and even under some circumstances while in the leaf itself, it is found that the colour of chlorophyll is very easily affected by reagents and in particular that alkalies and alkaline salts heighten its green colour, while by some acids it is more or less reddened and by others gradually bleached.

Such is a brief description of chlorophyll as ordinarily met with, but *a green pigment* is of frequent occurrence in the leaves of plants which differs essentially from the usual kind. In place of two colours, it consists of one only, as will be shown hereafter.

The Red Colour of Leaves.

Red leaves are of very frequent occurrence. They are met with at all seasons; in the spring and summer, but especially as autumn approaches. The red colour is encountered also in leaves of all ages, in the very young and only partially developed, in mature and healthy leaves and in leaves as their vitality becomes impaired preparatory to their fall.

In some cases the redness is partial, the leaf not being red all over but in places only, the usual green colour elsewhere prevailing; in others the leaves are wholly red and sometimes the green colour of the leaves is transformed to a greater or less extent into the red colour. This is one of the changes which occur in the foliage on such an extensive scale and with such striking results in the autumn. There is no doubt, that this change is effected in many cases at the expense of the green pigment, certainly of its blue colour, and possibly in part also of its yellow constituent, when the chlorophyll consists of the two colours. Further, assuredly the reverse is true and the red is sometimes converted into a green colour. It may be mentioned as one proof that the change from green to red is at the cost of the former colour, that the fragments of, say a red leaf after extraction with water only, are often found to be deprived of all colour. The change of colour from red to green occurs in the young red leaves of the Gloire de Dijon rose and some other plants and especially in those in which the red pigment is converted into a green by the slightest touch of an alkali.

Frequently the red colour is confined to the upper, occasionally to the lower surface of the leaf, but both are often involved; usually the upper is of a deeper red than the under surface, but to this there are some exceptions and many examples of these several distributions of the red pigment may be seen. Again the red colour may be confined to one layer of cells, or it may occupy several layers, or it may and indeed does often extend through the whole thickness and substance of the leaf.

The leaves of the Gloire de Dijon rose afford a very striking example of very young red leaves and those of various species of plants belonging to the genera acheiranthus and coleus of mature and healthy red leaves; but it is in leaves which have attained their full age and which have begun to show signs of impaired vitality that the redness is seen in the greatest variety and abundance.

As there is more than one kind of green, so is there *more than one description or variety of red colouring matter* in leaves: there are not less than two modifications, but both agree in the fact, that unlike chlorophyll they are soluble in water, while one only is insoluble in absolute alcohol, in which chlorophyll is itself so readily dissolved.

The leaves of plants furnish many examples of the two varieties alluded to. One is met with in the more or less red leaves of various species of geranium, in the leaves of the beech and in a great many other red or reddish leaves; this variety is soluble in water and to a less extent in mixtures of water and alcohol, but the mixed alcoholic and watery solutions are usually by no means of a deep red colour and in some cases are almost colourless. Notwithstanding this fact however, some of these pale solutions yield when evaporated a red residue, which when an acid is added is much intensified.

The second kind or variety of red pigment is of less frequent occurrence and is present in the leaves of some species of acheiranthus and coleus. It is readily soluble in water and furnishes solutions of a deeper red, but the great points of difference are its complete insolubility in absolute alcohol, anhydrous ether, chloroform and some other menstrua and its not being changed to green by alkalies.

The Yellow Colouring matter.

There are very few healthy leaves which are wholly yellow; it is in variegated leaves that this colour is chiefly met with, as in those of the holly and euonymus, but it is in flowers that the best examples of the colour are encountered, though whether obtained from leaves or flowers it possesses much the same properties.

It is in leaves which can no longer be regarded as in full health and vigour in which the vital forces are weakened or even destroyed and in which these forces, acting with diminished power or ceasing altogether, allow of the chemical affinities coming into operation, that the colour is best seen. It is therefore in leaves at the period of their decadence and fall that leaves of varying shades and depths of yellow appear in such vast quantities as to impart to the autumn foliage many of its beautiful tints and characteristics. Good examples of leaves which become yellow in the autumn are furnished by some species of geranium, especially the ivy varieties and sparmannia Africana.

It will be remembered that the ordinary green colouring matter of leaves, chlorophyll proper, as I will call it, for the sake of distinction, is composed of two colours, a yellow insoluble in water, and a blue readily soluble therein and convertible by a trace of acid into a pink, red and even a yellow pigment.

Thus then it is evident, that for the appearance of the two principal and characteristic colours of the autumnal vegetation yellow and red, nothing more is necessary than the decomposition of the chlorophyll and its separation into its two component colours, the yellow and the blue, so readily convertible into a yellow. Now this is what actually occurs and thus a simple explanation is afforded of the principal and most striking change which takes place in the foliage in the autumn.

But the change from green to red and yellow frequently occurs in a manner still more simple. It has been shown that there is a green colour which is converted first into a red and then a yellow on the application of a trace of alkali, just sufficient to produce a slight alkaline reaction.

But the antumual changes and tints are not confined to yellow, pink and red, but others appear as rusty red, brown and black tints of various shades.

The principal effects of the application of reagents to the yellow pigment are as follows: alkalies deepen it, turning it from light to deep yellow and then to brown and reddish brown; the weaker acids render the colour lighter and exert a bleaching effect, but by hydrochloric acid it is turned blue and green and by nitric acid blue, green, and then reddish. The yellow pigment of *flowers* acts in the same way and is identical in its properties. The reddish yellow peel of the *orange* exhibits the same reactions with hydrochloric acid but the peel of the *lemon* differs very remarkably as pointed out to me by Mr. Clayton F.C.S.; it is not greened by that acid, but is turned blood red by sodium carbonate, while orange peel is but little changed by that alkali. These differences are so considerable that the peels of the two fruits may be readily distinguished thereby. They arise from the presence in the lemon peel of a yellow pigment soluble in water and which is turned *red* by alkalies.

The Green Colouring Matter.

A very brief description of the ordinary green colouring matter, chlorophyll has been given, but there is a second kind present in the leaves of some plants to which I have already adverted, as for example in the hyacinth.

It has been stated that the green colour of chlorophyll is produced by the admixture or combination of *two colours,* a yellow and a blue, but there is a green pigment formed of *one colour* only, and that a blue, or a red convertible first into a blue and then a green, by a minute quantity of an alkali. The red colour, soluble in water, obtained, amongst many other plants from the red leaves of the geranium, including the ivy varieties, vine, beech, American creeper, red cabbage and the Gloire de Dijon rose, is thus changed. The transformation of this red to a green may be seen by simply lightly brushing over the dried residue of the several aqueous solutions with a camel's hair brush, moistened with a weak solution of sodium carbonate. In none of these cases was the least evidence procurable, even by the microscope, of any admixture of a yellow colour. Further, the change from red to blue and then green takes place under the same circumstances not only in leaves, but in the red colour of the petals of many *flowers,* as for example the scarlet poppy, in which case also there was certainly no admixture with a yellow pigment.

Now, as I have already shown, there is *another red pigment* of less frequent occurrence in plants which is not convertible into a green by an alkali, as the colour obtained from the red leaves of some species of acheiranthus, coleus and some other plants.

Colourless Colouring Matters.

It is well known that plants grown in the dark, as in a cellar, fail to develope their usual colours, the leaves even being colourless; advantage of this circumstance is sometimes taken by florists and others. It is from the same cause, the exclusion of light, that the heart leaves of hard green and red cabbages are also destitute of colour. Exposed to both air and light, the proper colours are quickly developed and hence it is proved that these agencies are necessary for the production of the colours of leaves and flowers. The question therefore arises as to the source of the colours.

There are some few leaves, which though grown in the light and exposed to the air, remain for the most part colourless and yet when certain reagents are applied, colours are at once developed.

Again, a copious development of a yellow, mixed occasionally with a little green pigment, is of almost constant occurrence when a solution of sodium carbonate is applied to the colourless, bruised corollas and petals of a variety of flowers, as those of a white hyacinth, flag, narcissus, lily or azalia.

Further, if the very smallest colourless leaves of a red cabbage be broken up and treated with a minute quantity of tartaric acid a red colour will after a time be developed. Then, if the colourless flower stalk, with its equally colourless buds of a pink hyacinth grown in the dark, be watched for a time, it will be seen that the buds when exposed to the air and light become at first yellowish, then green and on the application of tartaric acid, pink and red.

Now the only interpretation of these particulars and facts is, that as in the case of the indigo plant, there exists in the leaves and flowers of many plants at least two colourless pigmentary substances; the one convertible by an alkali into a yellow, and the other into a red pigment by an acid, and this red again in some cases being changed to green by an alkali as already shown.

The striking and often very beautiful changes which take place in the colour of leaves when the summer is far advanced and especially in the autumn are indicative, as already stated, of diminished vitality and approaching dissolution: the vital forces are ceasing to operate and purely chemical forces come into action, the chlorophyll becomes decomposed, giving rise to yellow and red tints, which themselves are more or less changed or destroyed, the iron which enters into their composition being at the same time liberated.

Iron and Tannic Acid in the Colouration of Leaves.

Now *iron* plays a much more important part in the colouration of the leaves of plants than has hitherto been supposed. I have shown in the first place, that it is present not only in chlorophyll but in all the other colouring matters, the yellow, blue and red, while Mr. Clayton's analyses* prove that the amount of iron present

* See the second Paper on The Colouration of Leaves.

is very considerable; in dead leaves it occurs sometimes as red oxide and carbonate of iron and at others in combination with tannic acid and other bases, giving rise as will now be explained, to other tints and colours.

Nearly all leaves contain *tannic acid* and other nearly allied acids; tannic acid is almost invariably present, even in the youngest leaves, and as the analyses given in my second paper on the colouration of leaves prove, the quantities present are often very large, especially in certain plants.

Now tannic acid like iron, is a very colour-producing body and between the two there is a strong affinity, so that they readily enter into combination.

Thus, it is to the presence of iron in the leaves of plants that some of the red tints of autumnal leaves are due and to the tannic acid mostly in union with iron, that the darker shades of reddish brown, brown and black are to be attributed.

Amongst many other leaves, those of the oak, beech, ivy, cratægus glabra and sparmannia Africana may be mentioned, as containing much tannic acid. The effect of the presence of this acid on the colour is very remarkable, it often causing the leaf to change from bright golden yellow to brown and sometimes almost black, in the course of a few hours.

Such is a brief account of the colouring matters of leaves and of the remarkable changes which take place in them at the autumn season. The two Papers at the end of the narrative may be referred to for further and fuller details.

CONCLUSION.

The survey which has now been made of the work of my life, the chief events in which have been recorded in this narrative in quick succession, gives rise to very mixed feelings and to some reflections; feelings of regret on the one hand and of satisfaction and thankfulness on the other.

One source of regret is, that my early training and education were so interrupted and limited and in some respects wanting in thoroughness. The results of this defect I have felt throughout life and I know it has had its effect on the quality and character of some of my work. It was only under the tuition of Mr. George, that I really began to understand the true object and purposes of education, to experience the pleasure which even the acquisition of scholastic knowledge was capable of giving and to realize the fact, that it was the foundation upon which success or failure in life, in many cases mainly rests.

Another very serious deficiency in my early training was the want of practical knowledge and guidance as to the advantages and disadvantages of different callings and professions. With such knowledge the choice would rest upon a sure foundation and the prospect of success would be much greater. For the want of such almost elementary information, how many youths blindly choose a profession for which they find from actual experience, they either have no taste or are otherwise unfitted; thus they enter on the real work of their lives, that by which they have to support themselves and their dependants, rather by accident than design, in place of on fixed and ascertained data; hence failure too often arises and they are neither happy nor successful, whereas had the right choice been made and the profession selected for which they were best suited by their natural tendency and abilities, both happiness and success would have been ensured. Such reflections doubtless have occurred to many, but unfortunately the needful worldly wisdom is in too many cases gained from painful practical experience after our selection has been made, and too late to be of service.

I have often thought it would be a very good thing if Chairs or Lectureships were instituted in connection with our Universities and Colleges, having for their object the teaching and guidance of young men in the choice of their lives' profession or calling. Such a course of instruction should explain the nature of the qualifications required, the subjects which should be pursued and studied and the acquirements needed in each case, the advantages and disadvantages, the trials, risks and dangers, especially climatic, incidental to each occupation whether medical, clerical, military, naval, or commercial.

The Medical profession is a case in point, since it affords an apt illustration of the necessity which exists for preliminary information and guidance before coming to a final decision. From the want of these I have known very deplorable results to ensue; inability to pass the requisite examinations and if these were passed, failure from want of the physical and moral qualities necessary to ensure success.

First, there is the long course of study and application needed; the preliminary education and preparation for this should begin in the later period of school life; then the strictly professional studies are now spread over at least five years, a period not to be spent in a leisurely and aimless way, but in daily and hourly application, entailing the expenditure of much brain power.

Next there are the physical qualities necessary; how seldom are these taken into consideration; the sound health and constitution, the freedom from disease and defects of sight and hearing; the courage, nerve and endurance upon which such large demands would be made. It is no uncommon thing for candidates who have successfully passed through the whole course of study and who have obtained the necessary diplomas, on presenting themselves before the authorities, usually the naval or military, to be rejected on account of some physical defect.

Then the kind of life led by most medical men when engaged in private practice should be well considered, a life for which some men are totally unfitted from various causes; thus there is the necessity of being always on guard, by night as well as by day; there are no days of "eight hours" for medical men engaged in general practice; there is no period of the twenty four hours which they can call their own; sleep, rest, comfort, meals have all to give way and a doctor must hold himself ready at all times to obey, in many cases, the somewhat exacting calls of the public.

Again, there are dangers whish beset the path of the medical man in the daily exercise of his vocation; the risk of infection of various kinds, to which infection so many valuable lives are annually sacrificed.

Another risk which has often to be encountered, is that of injury or ruin to health from unhealthy foreign climates, from malaria, yellow and other fevers and diseases, including cholera. The medical officers of the Navy and Army are especially exposed to this danger and the deaths of not a few are each year to be attributed to this cause.

But there are still some other trials rather than dangers which await the medical man in the ordinary discharge of his professional duties; one consists in the differences and difficulties which are so apt to arise between medical men practising in the same town and which it is often not easy to avoid; the jealousy, mistrust, the bitterness which so often prevails and which go so far to destroy the peace and comfort of life. For some of these differences excuses may at times be urged; the frequency with which in small places interests clash and the conduct of the patients themselves, but whatever the origin, or whoever is to blame, the result is the same. There are few of us who have not suffered in mind and feelings from this cause.

It is in the smaller towns and localities that these rivalries and difficulties are apt to arise, in cities the circumstances are very different and in these, cordiality and sincere friendships among medical men very frequently exist.

Then, another fruitful source of many annoyances and troubles has its origin entirely in the conduct of patients, in their want of consideration, their medical ignorance, their quackery, homœopathy and Matteism, in their unreasonableness and sometimes even in their ingratitude; this last often takes the form of either declining to pay the fees charged, or tendering less than the just renumeration.

A grave danger not yet alluded to, is the liability to unjust legal actions for alleged mal-practice and the still further and more serious risk of having charges made of conduct of which the accused is entirely innocent, charges which may inflict a life long injury, although he has succeeded in fully establishing his innocence. Our Law Courts bear evidence to the frequency with which such charges are made.

Thus the doctor's path may be said on the whole to be beset with many thorns, and his bed, when he is permitted to lie in it, not a bed of roses.

We will now suppose that all the difficulties and dangers have been safely passed and that the medical man is in practice in some town of moderate size, that he is highly qualified in every respect, fond of his profession and anxious to get on; is his success assured? By no means. Success in practice in these days is not so much a question of professional attainments and aptitude; it has become unfortunately both for the profession and the public, more a social affair. It is not enough that the medical man should be highly qualified and that he should have had great and valuable experience in the treatment of disease. His social arrangements and surroundings must be such as to allow of his entering much into society, he must entertain, he must be seen at many social gatherings, put himself "en évidence" everywhere and always, and he must take a prominent part in local affairs.

The foregoing are some of the points on which young men who contemplate entering the medical profession should be enlightened before a final choice is made.

Nevertheless the profession of medicine, using the word in its widest sense is a noble one, conferring on frail humanity very great benefits and requiring on the part of those who practise it high qualifications, physical, intellectual and moral.

For any imperfection in some of my scientific work, more particularly that of the earlier dates, other reasons than the want of due preparation and experience is to be assigned. One was restricted means, another the necessities and anxieties of medical practice, which allowed only of my giving interrupted attention to the enquiries and investigations in which I was engaged. Had I possessed independent means, or had I held, as do so many scientific men abroad, some small professorship, my energies would have been more concentrated, my time more continuously devoted to the subject in hand and the results would probably have been more complete.

I make these remarks to show that I am fully sensible of the shortcomings of some of my earlier investigations, arising partly from the causes above adverted to; in particular those embodied in the "History of The British Freshwater Algæ" and in "The Microscopical Anatomy of the Human Body" and I have often thought if I could have had the time and opportunity to reinvestigate these subjects, how much more worthily I could fulfil my purpose. This vain thought has doubtless occurred under like circumstances to many others besides myself.

Still, on looking back and taking into consideration all the circumstances of my life, I have very great cause for thankfulness. Never robust, struck down on three occasions by nearly fatal illnesses and compelled thereby when far advanced on the road to success to abandon for a time my medical career and with health subsequently greatly impaired, I have yet been permitted to accomplish, as these pages testify, a very considerable amount of work, some of it I trust of a useful and practical character.

In confirmation of this statement I will refer only very briefly to three subjects, two of which relate to sanitary questions and here I would observe that I may fairly lay claim to have been one of the earliest sanitary reformers in England. I was a contemporary of a noted and earnest band of such reformers; Sutherland, Southwood Smith, Farr, Milroy, Simon, Thomson, Glaisher, Parkes and though last not least, Edwin Chadwick. I was officially associated with most of them in sanitary enquiries.

I was the first to describe and define the true character of the "Organic Matters", living and dead, animal and vegetable contained in Drinking Waters, and to apply this knowledge to the exposure of the abominable condition of the Water supplied at the period already referred to, by nearly all the Metropolitan Water Companies to the inhabitants of London and its suburbs. That exposure necessitated the Parliamentary enquiry and contributed greatly to bring about the legislation which took place shortly afterwards.

Again, I was the first to apply the microscope in a scientific and systematic way and on an extensive scale to the detection and exposure of the Adulteration of Food and Drink and even Drugs, which was at the time all prevalent, to the injury of health and loss to the consumer. This exposure brought to light such fraudulent and injurious practices, that the attention of Parliament was called to the subject and after due enquiry legislation followed, which though deficient in some respects, has put a stop to most of the grosser and more serious forms of adulteration.

Omitting much to which I might be excused for referring in relation to my sanitary work, I will bring this narrative to a conclusion by alluding to my efforts, commenced in 1867 and continued for fully ten years in the foundation and establishment of The Royal National Hospital for Consumption and Diseases of the Chest on the Separate Principle or System; this system at that time was new, but its value has been fully acknowledged and acted upon in the case of other Hospitals both special and general, since projected; it was indeed an important factor in the success of the Hospital, as it so obviously commended itself to the intelligence and observation of most people and was so consistent with the requirements of hygiene and the views entertained of the contagiousness of phthisis and other tubercular affections. This successful undertaking has been and still is a source of very great satisfaction to me. During the last twenty five years the Hospital has been open to the sick and suffering; it has brought comfort, relief and in many cases longer life and health to, exclusive of a few out-patients, eleven thousand men and women sufferers from consumption and other diseases of the organs of respiration. Since I left Ventnor fifteen years have elapsed, during which my interest in all things pertaining to the hospital has not abated, but my opportunities for usefulness have been of course lessened. The labour and responsibility of carrying on the good work have fallen upon others, as already shown in the course of this narrative.

I hope it will be quite understood that I have referred to these and similar matters in no boastful or egotistical spirit, but rather for the reasons already assigned, namely, to recall circumstances and events, some of which from lapse of time are well nigh forgotten and in the hope that the narrative may prove interesting to some and to others possibly useful. It will at all events show how much even a very humble worker is capable of doing to promote the welfare of others and I am not without hope, that some who read it, may be led to contribute what they are able towards the well being of their fellows. It would be affectation on my part if I were to state I have no pleasure in the survey made and in my past work, but any feeling of satisfaction is tempered by the thought that what has been done might have been so much better accomplished and that taken altogether it forms but a drop in the ocean of man's needs and requirements. If in preparing this narrative, I had been actuated simply by a desire to perpetuate my name, I know how futile would be the idea and that Time the destroyer, soon effaces all but a few personalities. The trumpet gives forth a blast, it is true, but the sound is heard only over a limited space and is soon scattered and lost in the distance for ever.

Such then is the brief survey of the work of my life; it is not probable that there will be much to add in the future and I now therefore lay down my pen with a feeling of great thankfulness, that my life has been so long spared and that I have been permitted to accomplish some useful work.

PAPERS, MEMOIRS AND WORKS.

WRITTEN AND PUBLISHED

FROM 1840 TO 1893

BY

ARTHUR HILL HASSALL M.D. LOND.

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APPENDIX.

CONTENTS.

ON THE COLOUR OF THE LEAVES OF PLANTS AND THEIR AUTUMNAL CHANGES

BY

ARTHUR HILL HASSALL M.D. LOND.

Communicated to the Royal Society by the R^t Honourable Professor Huxley. Received June 21, read November 1892 and now in the Archives of the Society.

Many verbal alterations have been made in the M. S. of this communication during its preparation for the press, a few paragraphs have been re-written, several have been omitted as being unnecessary or repetitions, while some three or four new paragraphs have been added, these being indicated by brackets; lastly two or three oversights or errors have been rectified, but the paper in all essential respects is the same as that communicated to the Society.

Chlorophyll, the name given by botanists to the green colouring matter of the leaves of plants, is not, as hitherto obtained, a simple substance, but is composed, in part at least, of constituents which are non essential to its green colour; iron is however deemed to be an indispensable ingredient. But little reliance therefore can be placed on any quantitative analyses hitherto published, or on the processes described for obtaining Chlorophyll in a state of purity.

Chlorophyll is generally regarded as a mixture of two colouring matters, a yellow and a blue; on each of these different names have been bestowed, as Zanthophyll and Cyanophyll, or Yellow Chlorophyll and Blue Chlorophyll. In the following pages the word Chlorophyll is applied exclusively to the green colouring matters of the leaves.

Again, although the leaves of most plants are, at one period or other of their growth, of a green colour, there are many exceptions to this, and some leaves, when even in full health, are red, yellow, or even blue.

Before proceeding to describe the results of the observations and investigations made, it will be well to point out certain preliminary difficulties presented to the action of reagents on the colouring matters as contained in the leaves themselves, arising from their epidermic covering. The epidermis not only acts as a protection to the contents of the cells, but prevents the entrance into them from without of substances which might be injurious. Thus, while gases, as oxygen and carbon dioxide, readily pass through the epidermis, both from within and without, the passage of water even, either by endosmosis or exosmosis, experiences greater difficulty, some reagents finding even a less ready entrance, and when they have entered, some of them fail to produce any effect on the colour, because they do not exert any solvent action on the chlorophyll. I enter into these particulars to simplify matters; even alcohol penetrates the epidermis very slowly and sparingly in the case of an entire and unbroken leaf and extracts so little of the chlorophyll that after two or three days immersion, the solution becomes only of a pale green colour. In order to obtain the full solvent action of the alcohol, it is necessary to cut the leaf up into very small pieces and even then the alcohol or other reagent used, enters the leaf but slowly and chiefly through the cut edges or broken surfaces of the leaves.

Chlorophyll is soluble in alcohol, ether and benzine, but not, it is usually stated, in water; it is also affirmed that it is soluble in acids, but I find that neither of these statements is altogether correct; it is slightly soluble in water, as will be shown later on, while most acids, especially the organic acids, have little or no solvent action; the alkalies on the contrary exert a marked effect.

Hydrochloric acid added to a little of the alcoholic solution of the green colouring matter partly evaporated in a watch glass, produced an immediate effect; it increased the green colour of the central liquid and this increase was maintained for several days and it gave rise to a blackish deposit of particles of cholophyll; the green colour of the outer dried ring was greatly intensified: on the other hand the green colour of the chlorophyll was wholly destroyed by an aqueous solution of chlorine and portions of leaves immersed in the same became slowly but completely decolourized. When nitric acid was added, the central liquid became of a pale pink, while the particles which subsided were red and the green outer circle turned blue. After some hours the outer circle had almost disappeared and the central liquid became nearly as clear as water, but the bright red granular particles were still undissolved. Strong sulphuric acid deepened the green colour very markedly.

But there is still another circumstance to be referred to in explanation of the difficulty with which chlorophyll is reached by some reagents and that is the presence of a wax-like material which renders the chlorophyll impermeable to water and some other substances.

Having thus cleared the ground somewhat, I will first describe the characteristics of the chlorophyll obtained by the action of alcohol of Sp. gr. 838 on the Capucin arum, A. arisarum, cut into small pieces. It may be presumed, that the chlorophyll thus obtained is of the same character as that procured from the green leaves of most plants, the sap of which is not strongly acid. The chlorophyll solution when freshly prepared in bulk is very clear and of a brilliant grass green colour; this colour is preserved for some time, but the period varies; gradually when exposed to the air it loses its beautiful green hue; it becomes in succession olive green, brownish green and dull coloured, its transparency is diminished and ultimately in the course of a few days a reddish brown precipitate is thrown down, leaving the solution, now deprived of so much of its colouring matter, comparatively pale and with all its attractiveness destroyed. Usually, when a small quantity of the alcoholic solution is evaporated in a capsule or watch glass, a grass green deposit is left, the exact tint of which varies with the quantity evaporated; if this be at all considerable it will be seen, that the principal part of the chlorophyll is deposited in a circle on the margin of the capsule and here in consequence it presents an almost black appearance, while as the centre is approaced it is more or less deep grass green.

It occasionally happens, that the alcoholic solution, when poured in a white porcelain capsule, in place of being uniformly green, presents three separate colours; an outer circle of yellow, an inner green and the central portion of the liquid of a blue tint; in such cases, a ring of a pure yellow colour often becomes deposited on the edge of the dish. But these appearances are somewhat exceptional.

Such are the outward characters and appearances of the alcoholic solution as visible to the naked eye. I will now describe those revealed by the microscope.

Two or three drops of the solution after evaporation on a glass slide, exhibited numerous deep green and beautiful oil-like globules which were deposited at the edges of the liquid; some of these had coalesced, while here and there small red granules were seen.

A larger quantity of the solution was placed in a watch glass and partially evaporated; only a few drops of this concentrated liquid were now examined, when it was found that a great many much larger green globules had been formed than in the previous case; some of these presented a bluish tinge; while others contained a number of small colourless globules of an oily aspect.

Two or three drops of a ten per cent mixture of tartaric acid and water were next applied to these large green globules, when their colour was seen to change at once, the green was more or less discharged, some of the globules became yellow, and in others a few ruby red particles were visible.

It has been pointed out, that when the alcoholic solution is kept for some days, it undergoes certain changes; amongst these is the deposition of a reddish sediment. This precipitation is mainly due to the presence in the sap of most leaves of a small quantity of acid, which in due course is transferred to the alcohol employed in the extraction of the chlorophyll and as the alcoholic solution is evaporated, the acid present becomes concentrated and thus relatively increased. Now this deposition may be hastened and indeed brought about almost at once by the addition of a small quantity of a solution of an organic acid, such as tartaric acid.

If a little of this reddish brown precipitate be examined while moist under the microscope, it is found to consist of granular masses, mostly of a yellowish and some of a brownish red hue. If a little of the granular matter be dried on a glass slide and a minute quantity of the solution of tartaric acid be added and the granular matter again dried it will now have assumed a much deeper and in some parts a decidedly reddish tint.

I will next record the results of the application of certain reagents to small quantities of the chlorophyll solution obtained from the green leaves of the Capucin arum and other plants, limiting the reagents used chiefly to those which are known to occur in the leaves of plants both deciduous and perennial.

Ten per cent solutions were prepared of most of the substances employed and in some few cases five per cent solutions were used and of each of these a very small quantity, a drop or so only, were added at one time.

It was found that the carbonates of ammonium, sodium and potassium, at first increased the depth of the green colour, but this after some hours changed to olive green and even brown; they also exerted a marked solvent action on the chlorophyll itself.

That a solution of soap intensified the colour greatly, the brilliant green hue being retained for some days;

That phosphate of sodium and chloride of sodium at first increased the green colour to a small extent only, but after a time it became dark brown; both solutions exerted a slight solvent action.

The organic acids on the contrary, as citric and especially tartaric acid, destroyed the green colour quickly, turning it from green to a light yellowish brown. They also exhibited considerable bleaching properties, but they showed no solvent action on the chlorophyll as did the alkaline carbonates.

Bitartrate of potassium changed the bright grass green to dark olive, but it exerted no solvent action.

Olive oil quickly dissolved the chlorophyll and turned it to amber or olive brown.

Accompanying the change of colour due to the action of the acids, a precipitation occurs of a granular matter, identical with that which is thrown down when a considerable quantity of chlorophyll is exposed to the air for some days; and like it exhibiting under the microscope the same mixture of yellowish, and pinkish particles.

The application of the above reagents to small pieces of torn up green leaves, gives results similar in the main, to those furnished by the chlorophyll itself.

THE RED COLOURING MATTER.

But chlorophyll, the green, while it is the principal, is not the only colouring matter of the leaves of plants; the red colour plays a very important and conspicuous part in the colouration of leaves, expecially in the sunny climate of the Riviera, where the observations now being recorded were made. So prevalent is this colour and so brilliant in some plants, that it adds materially to the beauty of many gardens, but little inferior to the flowers themselves.

It is most conspicuous in the early spring and late autumn in certain plants; it is found in leaves of all ages, in the youngest as they emerge from the bud, in full grown and healthy leaves and in others at the period of decadence and preceding their fall.

Notable instances of the occurrence of the red colour in young and partially developed leaves, is afforded by the Gloire de Dijon rose in the month of March and by the Castor Oil plant. The new leaves of this rose are of a rich, dark maroon red, while those of the castor oil plant are of a dark mahogany red. A curious fact in connection with the leaves of both these plants is that later on and when they are fully developed, they become gradually transformed into green leaves. A great many other similar instances of the occurrence of young red leaves may be cited.

In certain varieties and species of pink and red Geraniums, we have striking examples of red tints in the older, fully developed and yet apparently healthy leaves. The change here is from green to red, the green never being restored, the red colour moreover rarely extending over the whole leaf. This disposition to the developement of red leaves is increased as the period of decadence is approached, as also by any injury or other cause which interferes with the nutrition or vitality of the plant or leaf.

A very large proportion, in fact nearly all the full grown, and healthy leaves of many Ivy-leafed geraniums are more or less coloured on the upper surface, the lower being for the most part green; the tints vary from maroon, to chocolate, vermilion or pink, the really green leaves being the youngest. Attention may here be called to the broad, dark coloured almost black band in the centre of the leaves of ivy as well as many other geraniums. The nature of these rings will be explained a little later on.

As examples of the change in leaves from green to red, after maturity and previous to their fall, the Vine and a species of Cratægus, C. glabra, may here be referred to. The leaves of the vine in the autumn often present the most brilliant aud beautiful tints, especially when the sun is shining through them; indeed they occasionally form quite a feature in the landscape.

The Blue Gum or Eucalyptus Globulus affords an instance in which the young leaves in the spring are more or less red, while the older leaves do not become so until the autumn, when they are about to fall.

Instances inumerable, in addition to those already mentioned, might be cited of the occurrence of red coloured leaves, such a list however is not needed for this enquiry, but we may name in passing, that many of these belong to the genera Rumex, Oxalis, Salvia, Coleus, Acheiranthus, and though last, not least, some varieties of Brassica, as the red and blue cabbage.

As a rule the red colour is most developed on the upper surface of the leaf and in some geraniums it begins in the thin edges; frequently both surfaces are coloured and occasionally it happens that the under surface is the most red, and this is usually the case in the scimitar shaped leaves of the Blue Gum.

The red colour is often confined to a single layer of cells, in other cases it extends deeper and it sometimes involves the whole thickness of the leaf, comprising several layers of cells; these differences may be well seen in vertical sections of the leaves of many geraniums.

In the leaves, both young and old, of some geraniums, as already noticed, a blackish, somewhat starred ring or band, is to be seen; this, owing to its dark colour stands out in marked contrast to the green of the rest of the leaf. I found to my surprise, when a thin section of the leaf including a portion of the ring, was placed under the microscope that it consisted of a layer of cells filled with a ruby red colouring matter.

The dark colour first seen was due no doubt to the density of the leaf being so great as not to permit the light to pass through. When the leaf is of a light green and still more when it is of a yellowish colour, the nature of this ring becomes apparent to the naked eye, as it then exhibits a more or less pink or reddish hue.

It has already been stated, that the red leaves of certain geraniums are rarely red all over, but in part only, the change of colour beginning mostly at the edges of the leaves where they are thinnest and most permeable to light and air; the redness advances gradually and ultimately extends over a great part of the surface, the remainder of the leaf being usually of a paler green than the rest of the unaffected green leaves.

This change of colour from green to red is doubtless effected at the expense of the green colour, which in some cases is entirely replaced by the red: in others, part of the green remains undestroyed, but is often more or less concealed by the coating of red. The gradual extention of the red colour into the cells occupied by the chlorophyll may sometimes be seen under a low object glass in thin vertical sections of the leaf.

Unlike chlorophyll, the red colouring matter of plants is soluble in water and less freely in alcohol, but owing to the difficult permeability of the epidermis, the colour is but slowly extracted, so that it is necessary as in the case of chlorophyll, to reduce the leaves into small pieces.

When as in some geraniums, the red colour extends through the whole thickness of the leaf, water or alcohol, Sp. gr. 835, extracts every trace of the red colour, the pieces of leaf being rendered colourless. This result shows that the red colour is, sometimes, at least, formed from the green colouring matter.

The alcoholic or spirituous solution of the colouring matter of some red leaves is almost colourless and yet when a little of this is evaporated in a watch glass the dried residue presents a brilliant carmine appearance. In this carmine residue under the microscope sometimes two colours are plainly seen, a bright red and an equally bright yellow, each colour keeping perfectly distinct. The yellow however is in much the smaller proportion and is doubtless derived from the decomposition of the chlorophyll present to some extent in the red leaves which furnished the extracts.

A few drops of the solution of tartaric acid were next added to a little of the unevaporated spirituous solution derived from red geranium leaves and this had the immediate effect of turning it bright red. A small quantity of a five per cent solution of potassium hydrate was added to a second quantity of the solution, and this at once caused it to assume a green colour. The next morning the solution was found to have dried up, the green colour had entirely disappeared and a yellow residue now alone remained. Carbonate of sodium changed the colour of the partially dried solution to blue and then to deep green, when quite dried, to a rich amber at the outer edge, and elsewhere to a decidedly reddish brown tint. Under the microscope both red and yellow colours were seen, but not a trace of green, some of these curious changes admit of explanation; the intensification of the red colour was due to the action of the acid and the green colour to that of the alkalies used.

The alcoholic solution obtained from the chocolate coloured leaves of an Ivy-leafed geranium presented much the same characters as the above. The solution when poured into a watch glass was almost colourless, but the next morning the residue was found to be of a lovely deep pink colour; this under the microscope was seen to consist mainly of a clear, non-granular and diffused pink colouring matter, surrounded near the outer edge of the deposit by a number of bright yellow granules of irregular size and shape, these being in marked contrast to the pink colouring matter.

It has already been stated, that the young, upper leaves of the Gloire de Dijon Rose are of an intensely dark red, with in some, glimpses of a little green colour. Under the microscope the colouring matter is seen to occupy the cells in the same way as in other red leaves. An alcoholic solution was prepared from these leaves, but this instead of being, as in the geraniums, of a reddish yellow tinge, was *a bright grass green.* Notwithstanding this however, the residue left after evaporation was for the most part bright red; but outside this red deposit was a circle of a green colour, while in the centre of the red residue was a round spot composed of yellow colouring matter. The explanation of these differences will be given further on.

Another plant of common occurrence in the Riviera and which has already been referred to, is the Castor oil plant; the young shoots and leaves are of a dark mahogany colour, the younger and smaller the leaf, the deeper the colour; the young stems, veins and mid-ribs of the leaves are bluish red; both surfaces of the leaf are coloured, but the colour is confined to the superficial layer of cells, not continuously, but with intervals which show the usual green colour of the older leaves.

The alcoholic solution of the red leaves of this plant, like that obtained from the young red leaves of the Gloire de Dijon rose, was likewise of a beautiful grass green colour and it also yielded on evaporation a brilliant carmine residue. This presented the following characters: an outer green ring, an inner red ring and a central area or spot, having but little colour. The green alcoholic solution was before evaporation neutral, but subsequently and after the developement of the red colour, it was found to possess a slightly acid reaction, and it was to this acid that the appearance of the red colour was partly due. Had the final reaction been neutral or slightly alkaline, a blue or green and not a red colour, would have ensued. The fact may here be recalled to mind, that, as in the Gloire de Dijon rose, the mature leaves of the castor oil plant are also of a deep green.

It has been stated, that contrary to what obtained in the case of the solutions prepared from the red leaves of the geraniums, the corresponding alcoholic solutions made from the rose and castor oil plants were grass green. This difference ārose it would appear from the fact that the leaves of the latter plants contained both the readily soluble red colouring matter, and some chlorophyll; the spirit used, of course extracted not only the red, but also the green colour, so that the solution was a mixture of both; the green being the least soluble was first deposited on the watch glass und hence the outer green circle; the deposition of the red occurred later and its colour was due to the presence of a minute quantity of an acid derived from the alcoholic solution or possibly from the air, while the free yellow colouring matter probably indicated the decomposition of some of the chlorophyll.

Now an increased formation of a green pigment is clearly necessary to account for the conversion of the dark red into the green leaves, a change which undoubtedly takes place on a very considerable scale; of this conversion two explanations may be given, one extremely simple, since all that would be required, as already shown, is that the acid reaction necessary to the developement of the red pigment should be changed into an alkaline reaction; the other and the correct explanation is, that the green colour which underlies the red and which is insoluble in water is increased in amount in accordance with the growth of the leaves.

I have in the next place to refer to two other plants; the one deciduous and the other an evergreen. The first sheds its leaves in the autumn and the second in the early spring, in both cases the appearance of the red colour is a sign, that the leaves have fulfilled their functions, that their vitality is impaired and that the chemical forces have now come into unrestrained action. The plants referred to are the Vine and a species of Cratægus, C. glabra. In neither of these cases does the red colour differ essentially from that obtained from the plants already noticed, but in the leaves of this cratægus the tint is different and the colour, a decided blood red in most cases, especially in the older leaves; occasionally however some leaves are met with which are simply reddish or even quite brown, and sometimes it will be noticed that one half of a leaf is green and the other red, as indeed is frequently observed in the leaves of numerous plants.

The leaves of this cratægus also contain a good deal of tannic acid and perhaps an excess of iron; the presence of these is not without effect on the colour of the leaves.

Small pieces of these leaves are much darkened by immersion in water and assume a reddish brown colour, the water itself becoming brownish; again the colour of the red leaf is made of a much deeper red by a weak solution of tannic acid, this solution itself being quickly rendered a bright red. The torn up red leaves were also blackened by a weak solution of tartrate of iron, as also were the cut edges of the green leaf.

It may here be stated, that a modification of the ordinary red colouring matter is occasionally encountered. I first met with this in the old red leaves of the cratægus above referred to; it differs chiefly from the ordinary red pigment, in that it is not changed by an alkali from red to blue and green. In this cratægus it is so intimately incorporated in the upper surface of the leaf that this remains of a deep mahogany red colour even after repeated extraction with water and alcohol.

In examining and making observations on the red colouring matter of leaves, it must be borne in mind that when extracted from the leaves of plants by alcohol it is very liable, as already shewn, to be admixed to a greater or less extent, with the yellow colour which is insoluble in water and the presence of which would modify the red colour as well as the effects of the application of reagents thereto. This admixture may often be seen in the dried residue by means of the microscope, or by washing out the red with water; but not always, especially if the proportion of the yellow pigment be inconsiderable, since this may be absorbed and concealed by the red, though usually the reverse is the case, the yellow having a great capacity for absorbing the red, especially when the solubility of the yellow is increased by the presence of an alkali.

In some cases, even where no suspicion of such admixture exists, the red colour when in small granules, or in thin strata through which light can be readily transmitted, exhibits a yellow tint, passing, according to the quantity, into a light or deep red.

Another circumstance to be remembered, is the special liability of this colour to contain tannic acid, as indeed do the yellow and green colours, though to a less extent. Now the presence of this acid no doubt does, in some cases, affect the colour of the evaporated extracts and it may be the reason why an aqueous solution of the red colour when exposed to the air and evaporated, often loses its pretty pink tint and becomes of a dirty brown.

In many plants, the leaves of which exhibit a marked tendency to turn red, there would appear to be a superabundance of red or blue colouring matter. In the cratægus and many other plants this abundance or freedom of the red pigment is shown in the circumstance, that the youngest and least developed leaves, frequently exhibit a red colour.

Thus then, this red colour brightened, intensified, and even developed by acids is changed in some cases through the action of alkalies to yellow, blue and green and is made to assume all the principal colours exhibited by leaves.

The solutions of the red colouring matter of the leaves hitherto treated of, were all obtained by the action of alcohol Sp. gr. 838 and were therefore liable to contain small quantities of the green and yellow colours when these happened to be present. Desiring now to obtain a solution entirely free from these two colours, I employed water only as the solvent; the portions of leaves used were those of a geranium and were red throughout, not a trace of green being anywhere visible. The solution obtained was of *a very pale pink* colour only, yet when a small quantity of it was evaporated in a watch glass it presented, as in the alcoholic solutions already described, a bright carmine red colour. Under the microscope it was seen, that this was deposited in a broad circle, while the centre of the glass was occupied by a nearly colourless finely granular matter; not a trace of yellow colouring matter being visible anywhere. A portion of the red deposit was just touched with a camel's hair brush moistened with a weak solution of carbonate of sodium, when it immediatety turned a *bright green* which remained unchanged for some days. The green colour thus developed was brushed over with a weak solution of tartaric acid, by which proceeding the red colour was more or less restored. The fact has already been recorded, that the red residues of the alcoholic solutions of geranium leaves and of some other plants were changed to green or blue by the action of a small quantity of alkali, but it was considered, that the change to green was due to the admixture of some yellow colouring matter with the red, but in the present instance, no trace of yellow pigment was to be detected, even by the microscope. If the soda solution be added too freely, the green colour is destroyed and the residue will now exhibit tints varying from yellow to brown and reddish brown, according to the amount of the alkali.

THE BLUE COLOURING MATTER.

Hitherto scarcely any reference has been made to blue leaves and blue colouring matter, but the omission has been intentional, as it seemed best to treat of this separately. The best examples of blue leaves I have met with, are those of blue or red cabbages and beetroot, but besides those, blue leaves not unfrequently occur in other plants, both deciduons and evergreen.

On a careful examination of the leaves a small hard red or blue Cabbage, it will be seen, that the outer leaves are of a very decided blue colour, the veins being even of a still deeper blue; the leaves which succeed are less blue and these are followed by others of a reddish colour; still more internally the leaves especially in the intervals between the blue veins, are green, till gradually as the centre is approached, they lose all colour, except that the veins remain deeply coloured blue or red, and stand out in marked contrast to the uncoloured portions of the leaves. These veins in their distribution resemble in miniature a series of branching rivers and streams; on a close scrutiny it will be seen, that the blue or red colour in some of the veins ceases abruptly although the veins themselves are still continued on; again, if one of the smaller colourless leaves be held up to the light, it will be evident that the cells on either side of the coloured veins have become stained with the colouring matter derived apparently from the veins. It would thus appear that the veins, at all events in some cases are either the carriers of the pigment, or else that they convey the air or oxygen necessary for the developement of the colour.

From the correspondence of the colour of the veins with that of the leaves themselves, from the fact that they are frequently the first parts to become coloured and from the manner in which they are mapped out or distributed, there seems the reason to believe that veins play a more important part in the distribution of the sap and in the colouration of the leaves than is usually assigned them. It is admitted that a portion of the sap passes through them and that the spiral vessels are often charged with air.

In the heart of the blue cabbage, on the examination of which the foregoing observations are based, were a few perfectly colourless leaves, and these will be again referred to.

It was found, that the blue or reddish blue of the leaves extended through several layers of cells, but not through the whole thickness of the leaves. In the case of the outer and older leaves, the thickness was made up in part of cells filled with a yellowish green chlorophyll, placed beneath the epidermic layer of blue cells, but in the smaller and younger leaves, this central part was quite colourless, the green not having been as yet developed.

The alcoholic solution obtained from a portion of one of the blue leaves was of a grass green colour arising from admixture, as already explained with some small proportion of chlorophyll. Evaporated in a watch glass the solution quickly became in succession olive green, reddish brown and finally for the most part dark claret colour, leaving a residue of a bluish tinge, surrounded by a circle of bright green.

Now, as is well known the relation between the blue and red colours is a very close one ; they are really modifications the same colour ; the principal difference being one of reaction only; the blue colour must have a slight alkaline reaction, if the alkalinity be considerable the blue is destroyed and is succeeded by yellow ; the red must have an acid reaction, but the very slightest amount of acid is sufficient to convert a vegetable blue into a red colour. Both these conditions were fulfilled in the blue or red cabbage.

It follows then from all the foregoing that the red pigment obtained from so many different plants, was readily convertible into a blue pigment in the cases cited not only in the spirituous and watery solutions obtained, but in the red leaves themselves. Thus if a red leaf of a geranium were cut into narrow strips and placed in a weak solution of sodium carbonate, this slowly and gradually made its way by endosmosis through the cut edges of the leaf, changing the colour, as the solution entered, from red to blue and green. If now the strips are taken from the soda solution and placed in one of tartaric acid, the red is restored. This change from red to blue and green was effected in the same manner in the case of many other leaves.

The leaves of Beetroot furnished results very similar to those obtained from the blue cabbage. The blue colouring matter of a beetroot leaf when dried and broken into small pieces, was extracted by means of a very weak solution of alcohol; this while it removed the blue colour was not strong enough to act on the green pigment lying beneath the blue; the pieces of the leaf now therefore presented a green colour and were no longer blue as at first. The solution when evaporated yielded a rich carmine residue; this when treated with a few drops of a solution of sodium carbonate became of a brownish hue, not green as was the case with the cabbage; this again changed to yellow, yellowish brown and reddish brown as the solution dried up.

It has been affirmed that the red colour of leaves is brought about by the acidity of the sap contained in them and this statement has been generally accepted. To test this point Table I has been prepared.

I. This Table proves, that chlorophyll owing to the protection afforded by the wax-like material already referred to may, and frequently does exist unchanged in leaves for long periods, in fact as long as they remain in health and this sometimes in the presence of much acid and also in spite of the further fact that the organic acids present evince a considerable bleaching power on chlorophyll.

II. That as a rule, the sap of green leaves contains as much, and occasionally more, acid than red leaves.

III. It proves besides, that for the production of a red colour it is only necessary the sap should have a slight acid reaction, but the amount of acid present need not exceed what is required to produce this reaction. If the quantity of acid be considerable it will simply intensify the redness and hasten its production especially in those cases in which the vitality of the leaf is impaired and the natural chemical forces are therefore allowed to come into action. For the conversion of a red into a blue as already stated slight alkalinity or at all events freedom from acidity only, is required. The effect of an excess of acid in hastening the production of the red colour is shown by adding a little tartaric or other suitable acid, to a small quantity of the alcoholic solution obtained from certain red leaves, as those of a geranium. This solution may be at first almost colourless, but it turns to a bright red immediately on the addition of the acid.

TABLE I.

Percentage Composition.			
Colour of leaves	*Acidity as tartaric acid*	*Residue*	*Ash*
	Geranium Garibaldi.		
Green	0. 85	13. 70	14. 80
Red (mature)	0. 67	2. 50	2. 80
	Geranium Peltatum Zonale.		
Green	2. 75	14. 30	1. 70
Red (mature)	1. 90	13. 10	1. 14
	Vitis Vinifera.		
Green	1. 18		7. 20
Red	1. 25		8. 31
	Ricinus Communis.		
Green	0. 10	31. 40	3. 14
Red (young)	0. 10	24. 50	2. 30
	Gloire de Dijon Rose.		
Green	0. 20	55. 10	6. 00
Red (young)	0. 17	29. 40	3. 10
	Brassica Rubra.		
Blue	neutral	20. 80	2. 00
Red	faintly acid	32. 00	4. 57
	Cratægus Glabra.		
Green	0. 30	49. 00	2. 91
Red (falling)	0. 23	42. 80	2. 54
	Cratægus.		
Green	0. 60	41. 20	4. 00
Red (young)	0. 50	31. 00	3. 20

The effect of light and sunshine in the production of the red colour, must not be forgotten. In many geraniums the change of colour from green to red nearly always begins at the edges of the leaves. For this there appear to be several reasons. The leaves are dependent, the stalks being at their uppermost part; the edges are the thinnest portions and hence they are the most easily penetrated by light and air; they also contain less moisture and are the first parts to dry up. Lastly the flow of thesap is probably to some extent arrested or retarded in this situation. The effect of exposure to sun and air, is shewn in the fact, that in most cases it is on the upper surface of the leaves that the red colour is chiefly developed.

In order to determine the total amount of acid contained in the leaves of the several plants enumerated in Tables I and II the leaves were first cut into small pieces and then reduced to a pulp; this was treated with distilled water and the whole transferred to a filter. With ordinary green leaves the filtrate is usually quite colourless, but in many instances the watery solution of both the green, red and even the yellow leaves of some plants were found to be more or less red, while the colours of the residues left on the filters in some cases also underwent marked changes.

Among the leaves, to which my attention was directed and which yielded coloured watery solutions; were the following: the leaves of the Gloire de Dijon rose, of Cratægus glabra, Sparmannia Africana and Hedera Algeriensis.

The aqueous solution from the green Rose leaves was decidedly red, the residue on the filter being dark green; the solution from the young red leaves was of a pleasing pink colour and yielded a carmine red deposit, while the residue on the filter was, contrary to what might have been expected, bright olive green.

The solution from the green leaves of Cratægus glabra was of a rather deep red and yielded on evaporation a pink or red colour, according to the quantity evaporated; the residue changed from bright to olive green. The solution from the red leaves was of course of a still deeper red, while the residue was reddish brown.

The solution from the green leaves of the Sparmannia Africana was reddish, the colour being sensibly darkened on the addition of a minute quantity of potassium hydrate and on exposure to the air for some hours it became still darker; the residue was of a blackish green colour, the darkening of the colour on the addition of the alkali and on exposure to the air, was in this case no doubt due to the presence of tannic acid. The yellow leaves of this plant also yielded a deep red solution, which like that from the green leaves, was darkened by exposure; the residue on the filter was of a deep, reddish brown colour.

The solution from the green leaves of Hedera Algeriensis was of a dark brown colour, which became deeper on neutralization, the residue being of a very deep green. The filtrate from the yellow leaves was of a deep reddish colour, much darker than that from the green leaves, while the residue on the filter was of a fawn colour.

Many instances might be cited in which the crushed green and yellow leaves furnish to distilled water, a more or less red solution. In some cases the whole of the redness is not due to the pure red pigment, as when the aqueous solution becomes darkened by exposure to the air or by the addition of an alkali; this deepening of the red colour was very noticeable in the filtrates obtained from the green and yellow leaves of the ivy and of the sparmannia and was doubtless due to the presence of tannic acid. When however the solution on evaporation yields a more or less red or carmine residue, or when it turns yellow when neutralized, these indications may be taken as a proof that the redness is really due to the vegetable colour.

Now, this appearance of the red colouring matter in aqueous solutions obtained from the green and yellow leaves of certain plants may show in the first place, that the union of the blue and yellow pigments to form chlorophyll may in some cases be but feeble and hence the chlorophyll may become readily decomposed, or secondly it may indicate the presence in the leaves of an unusual amount of free red colouring matter.

It may here be pointed out, that a close relation may frequently be observed between the colours of the flowers and the proneness of certain leaves to turn red even while they are apparently in their prime. When the disposition to turn red is slight, the flowers are usually white or pale pink, but when the red pigment in the leaves is abundant, it may be confidently predicted that the flowers will be of a deeper hue, red, crimson, or scarlet. This relationship is specially noticeable in many species and varieties of geranium. Although the yellow colouring matter of leaves is insoluble for the most part in water, still there are but few yellow leaves, which when crushed do not yield to water some small amount of the yellow pigment.

THE YELLOW COLOURING MATTER.

The yellow colouring matter plays a very important part in the colouration of leaves; there are however, but few healthy leaves which are yellow all over, those which exhibit this colour are usually variegated, as the leaves of holly, euonymus and some other plants. It is in the autumn especially that leaves assume the yellow colour, at which period, by the variety of their tints they form such a feature in the landscape and add so greatly to its beauty.

This autumnal appearance it should be understood is not due so much to increased development of the yellow colour, but mainly to the decomposition of the chlorophyll, a change brought about by the abolition of the modifying and controlling effect of the vital forces of the plant. At the same time, as we have seen, the blue and red colours are also very readily converted into a yellow pigment.

Good and striking examples of yellow leaves are met with, at the period of their decadence, in Sparmannia africana, in some Geraniums and in certain species of Ivy.

Variegated green and yellow leaves, most of which belong to evergreen plants, yield their yellow colouring matter to alcohol but slowly and sparingly, in consequence of the impermeability of the epidermis of these leaves. The colour is more freely extracted from the leaves of some ivys, still more easily and abundantly from those of several geraniums and of sparmannia africana, but the best examples of the yellow colouring matter are obtained from flowers.

The yellow colouring matter obtained by means of alcohol from the variegated leaves of Euonymus, was small in quantity and did not present anything remarkable in its character, that from the leaves of the ivy, these being of a pure golden yellow, was more abundant; the alcoholic solution was bright yellow and the deposit from this, on evaporation, was in large pieces of an equally rich hue.

The alcoholic solution obtained from yellow geranium leaves was of a bright, pleasant yellow and a small quantity of it evaporated in a watch glass presented the following characters; first a broad outer yellow circle having a somewhat glossy appearance and second a central, round patch which, having yellow for its basis, yet had a distinct pink tinge and exhibited a remarkably dry and lustreless aspect. Under the microscope this dull area was seen to be made up of yellow colouring matter, of a few pink masses and innumerable very small colourless granules, to the presence of which, the dry appearance was apparently due.

The alcoholic solution obtained from the golden leaves of the sparmannia was of a bright lemon colour and when this was evaporated, the residue under the microscope was found to consist of highly refracting globules of various sizes, arranged in the watch glass in the usual outer circle.

The small pieces of leaves of this plant left after the extraction of the colour were of a decidedly reddish tint.

So far, I have rarely obtained any yellow colouring matter in which I could not demonstrate by means of the microscope the presence of a little red pigment, this however not necessarily entering into the composition of the yellow colour and it hence occurred to me that some information might be afforded by a close examination of the epidermis of ripe oranges and lemons. I will now describe the results arrived at.

The epidermis of the unripe Orange and Lemon is of a decided green like that of the leaves and as in these a red element can be discerned, so one might expect to find the same in the ripe fruits. The fruit of the orange tree in the Riviera, begins to ripen and to turn from green to yellow sometimes in really cold and wet weather, that is about November, and this change of colour does not seem to be very dependent on either warmth or sunshine, since the green colour does not first pass into yellow on the side most exposed to the sun, but the change is more general and seems to spread slowly over the whole surface. In the lemon, since this tree bears fruit of all sizes and ages at the same period, the ripening is less restricted to a special season. From the yellowish red colour of the ripe Orange it might be predicted, that this was produced by a mixture of a red with the yellow pigment, but for some time I was not able to demonstrate the presence of the red constituent.

It occurred to me, that one difficulty in the way of revealing the presence of the red colour, was the insolubility in water of the yellow colouring matter in which it might be concealed. I hence soaked a piece of the ripe peel for some hours in a weak solution of sodium carbonate, this acted freely as a solvent; the peel now presented a reddish appearance to the naked eye, while under the microscope the red was still more plainly revealed, especially when the peel was dry; there seemed therefore little doubt that the green epidermis of the unripe orange and the yellow epidermis of the ripe fruit, were composed of a red and yellow colour in the same way as chlorophyll.

The epidermis of the ripe Lemon is destitute of any red tinge like that of the orange, but is a pure pale yellow. A strip of this peel was soaked for some hours in the soda solution and was afterwards examined under an inch object glass, when many more or less rounded masses, evidently situated beneath the epidermis, were seen; these were of variable size and of a deep pink colour. Some of the masses were large enough to be visible to the naked eye and this in portions of the peel which had not been steeped in the soda: indeed I afterwards found that a considerable number of large specks of pink were freely scattered over the epidermis of all the lemons I was then able to examine.*

* I have since ascertained that these pink spots are not present in all lemons; moreover I have a suspicion that they are of a parasitic nature.

Now the yellow colouring matter of leaves possesses the following properties and characteristics; it is freely soluble in alcohol and ether, less freely in alkalies; it is almost insoluble in water and in weak solutions of the organic acids; solutions of the alkalies darken the colouring matter and change it from light to deep yellow, to brown and reddish brown, while some acids have a contrary effect, and render it lighter and indeed exert a considerable bleaching power, not only on the yellow colouring matter, but more especially on chlorophyll and even the leaves themselves containing that colour.

Hydrochloric acid however added to a little of the alcoholic extract of the yellow colouring matter partly evaporated only in a watch glass, turned the central liquid portion pink, while the outer circle was rendered at first green and then blue. When nitric acid was added, the central liquid became blood red and suspended in it were red granular particles, but after some hours nearly every trace of colour in the liquid was destroyed.

The comparative insolubility of the yellow colouring matter in water explains why, when the spirituous solution is evaporated in a watch glass the colour is always deposited on the outer edge of the glass in a circle and why also, it so generally assumes the form of rounded globules, of various sizes and is not diffused like the more soluble red colouring matter.

Now, if the larger of these globules be attentively examined under the microscope and certain reagents be added it will be found, that they absorb a portion of some of these, sufficient indeed to materially affect the colour of the globules; thus, that sodium carbonate in solution is absorbed is proved by the bubbles of carbonic acid which are seen to issue from the globules on the addition of a drop or two of a weak solution of tartaric acid; then again, the globules became much darkened on the formation of a minute quantity of tannate of iron and if afterwards a little tartaric acid be added, the colour of the globules will change to a red.

In order to ascertain whether the amount of free acid in the yellow leaves was greater or less than in the green leaves, the following determinations were made. Table II.

It is sometimes scarcely possible to determine the acidity of leaves exactly and this for several reasons. The Gloire de Dijon rose leaves contain a thick mucus-like substance which renders filtration extremely difficult. Part of the acidity is due to tannic acid present in nearly all leaves.

THE GREEN COLOURING MATTERS.

A general description of chlorophyll, the manner in which it

TABLE II.

Percentage composition.			
Colour of leaves	*Acidity as tartaric acid*	*Residue*	*Ash*
Geranium zonale (Lucy Bosworth).			
Green	8. 50	11. 10	3. 43
Yellow (falling)	3. 70	12. 50	3. 14
Arum arisarum.			
Green	nearly neutral	17. 10	2. 00
Yellow (falling)	neutral	15. 40	2. 00
Hedera algeriensis.			
Green	6. 50	25. 10	3. 14
Yellow (falling)		30. 60	5. 71
Sparmannia africana.			
Green	4. 70	33. 40	4. 86
Yellow (falling)	4. 00	30. 00	4. 57

is extracted from leaves, its behaviour with certain reagents and other particulars have already been given. I will therefore now only describe the results of my latest observations. The alcoholic solution obtained from the leaves of Arum arisarum was of a brillant grass green colour and furnished an equally brilllant residue when evaporated, the greater part of this being deposited on a watch glass in a broad outer circle. Three separate portions of the solution were placed in watch glasses; when that in the first glass was partly evaporated, a small quantity of liquid only being left in the centre of the glass, it was submitted to microscopic examination; the outer green circle was seen to be made up of inumerable globules and irregular particles of an intensely green colour, while floating in the central liquid were a great many globules of all sizes, as also a few large masses which were of a deeper green and in some cases of a blackish hue. To the second glass, the solution being like the first, partly evaporated only, a few drops of the sodium carbonate solution were added; this quickly produced a marked change of colour in the liquid portion; it became yellowish, with at first merely a tinge of red, but the redness increased after the lapse of some time and still more after a day ar two, the green outer ring not being affected by the soda, owing probably to the way in which it adhered to the surface of the glass and to its being protected by the presence of some wax-like material. Suspended in the liquid were many round green globules, some dark masses and many particles of a decided red hue. When examined later on it was seen, that most of the green globules had become changed from green to red. The red colour was due to the decomposition of the chlorophyll and the liberation of its red constituent. The third portion was treated with tartaric acid when partly evaporated; the fluid part became quickly bleached, the ring of vivid green was seen under the microscope to turn to a rich yellow, while the green globules suspended in the liquid changed to yellow and in a short time became entirely colourless; in fact after two days, scarcely a vestige of colour was anywhere to be seen.

Now, since the mixture of a yellow with a blue, or a red convertible into a blue colour, is known to form a green, it occurred to me, that one ought to be able to imitate and produce this green colour by the admixture, under the requisite conditions, of the alcoholic solutions obtained from the yellow and red colouring matters extracted from leaves. These conditions are: that the two colours should be in a state of absolute solution, that they should be in the right proportions and particularly, that the mixed solutions should either be neutral or but slightly alkaline; if acid, the attempt will fail. In the alcoholic solutions of the yellow and red colouring matters of the leaves, we have just what is required, only we must ascertain whether either of these is acid, as sometimes happens from the spirit taking up some of the acid of the sap of the leaves, should this be of an acid nature. Should an acid be present, this must be neutralized by a few drops of a weak solution of sodium carbonate.

Equal quantities of the solutions were mixed in a watch glass; these before the soda was added were bright and nearly colourless, but afterwards the mixture quickly became yellowish and in a short time a green colour was developed, varying in tint in different cases, but generally of a beautiful grass green, similar to that of the chlorophyll solution obtained from a green leaf.

The green colour thus formed does not usually last a very long time; to be more permanent it seems to require some binding material but little soluble in water, as wax, such in fact, as exists in chlorophyll itself, but the green soon passes into a rich yellow colour, that is to say, in about three or four hours.

What, it may be asked, has become of the red element or constituent of the green colour and which seems to have disappeared? It has already been pointed out, that the yellow colouring matter when in solution, has a great capacity for absorbing and incorporating into itself the red colouring matter, the yellow becoming thereby simply deepened if the quantity of the red be not too great, but if the red be more considerable, the yellow would exhibit a brownish tint and if the proportion be still greater, it will be rendered of a reddish hue and the red colour under the microscope will then be clearly visible. Again it must be remembered that the red pigment is readily changed to yellow by both acids and alkalies.

Other changes are observed in the mixed solutions after neutralization; these either accompany or follow the development of the green. The mixture becomes thick, opaque, curdled and many flocculi are formed; these contain the principal part of the green colouring matter at first, but gradually they change to yellow and the next morning when the contents of the watch glass had dried up it was seen, that the flakes had collected at the sides of the glass and had become of a bright yellow colour, but here and there showing evidences of admixture with the red pigment, although it was evident that the greater part of this had become invisible by incorporation with the yellow colouring matter. In addition to the flakes there was an outer circle of brownish yellow globules and a central dull yellow area, containing a great many extremely minute colourless granules.

In the following case, the two solutions were simply mixed together without neutralization and hence the mixture was slightly acid; the result was, that no development of the green pigment took place; when evaporated the residue presented a deep carmine red centre, this was surrounded by a thin pink stratum and this again by a yellowish green circle. Three distinct colours were therefore seen, namely, red, yellow and green; the yellow and the green, as shown by the microscope, being for the most part in the form of very pretty globules.

It will be observed, that the above results are in marked contrast to those obtained from the solution which had been neutralized. These differences result entirely from the non-neutralization of the extremely minute quantity of free acid present in the mixed solutions; there was no development of green and the yellow and red pigments remained separate, becoming deposited in due course as the spirit evaporated. The yellow colouring matter being insoluble in water, was the first to be thrown down as the spirit was dissipated and hence it, as well as the small quantity of chlorophyll detected, were found in the outer circle, while the red colour, preserved by the trace of acid present and being soluble in the small proportion of water contained in the spirit, was the last to dry up.

When in place of being neutralized with soda, a little tartaric acid was added to the mixed solutions, an orange red colour was produced, but this was soon decomposed and the next morning it was found that the central deposit in the watch glass was of a somewhat deeper carmine red than it would have been, had tartaric acid not been added; outside the red centre was a circle composed chiefly of the yellow colouring matter.

Hitherto it had appeared, that two colours were indispensable to the production of a vegetable green pigment, but I now find that this is not the case. The yellow, as is known, is soluble in spirit, but not in water, whereas the red is easily dissolved by that menstruum. We have thus a simple and ready means of separating the two colours. Accordingly I prepared very carefully an aqueous solution of the red colouring matter obtained from the red leaf of a geranium.

A portion of this solution was evaporated and the residue examined by the microscope; this presented the usual beautiful red colour unmixed with the faintest trace of yellow. I now lightly touched the surface of the deposit with a camel's hair brush moistened only with a very weak solution of soda, when immediately *a bright green colour* was developed; having allowed the green surface to become dry, I applied in a similar manner, with the lightest possible touch of the brush, a weak solution of tartaric acid and this had the effect of at once restoring the red colour as at first.

Subsequently aqueous solutions of the red colouring matter were prepared from the leaves of a blue Cabbage and from the young, damask red leaves of the Gloire de Dijon rose. These solutions were treated in precisely the same way and with identical results, only that the tint of the green in the two cases was different. That from the blue cabbage was of a fine arsenical green hue, while that from the rose leaves was of a deeper green. These results are particularly interesting.

If a little more sodium carbonate be added the green colour becomes changed in succession to a yellowish green, to yellow, brownish yellow and to yellowish red, here then we have a remarkable series of changes, all proceeding from one colour, the red; in fact the changes pass through a whole series of colours.

There is a third way in which a green colour is produced; thus, if a brush moistened with a very weak solution of ferric chloride be lightly passed over the surface of the dried yellow colouring matter, it immediately assumes a dark green hue just like that of chlorophyll. At one time it seemed probable that this change of colour was due to the tannic acid present in all alcoholic and aqueous extracts obtained from both leaves and flowers since that acid gives with perchloride of iron a green colour exactly like that of chlorophyll. To test this point it became necessary to procure some yellow colouring matter entirely free from tannic acid. If much free tannic acid be present in the dried yellow extract the colour which ensues on the application of the ferric chloride will be black instead of green.

Accordingly every trace of free tannic acid was removed from the yellow colouring matter by means of ether and repeated washing with water; the ferric chloride was now applied to the tannin free yellow colour and with precisely the same result, the development of a green colour undistinguishable by the eye from chlorophyll. The same result ensued even when the ferric chloride was added to the prepared ethereal solution of the yellow colouring matter and also when the yellow tannin free petals of a crysanthemum were immersed for some hours in a weak solution of ferric chloride. It is thus shown that the green colour is not due to the tannic acid.

It was suggested that it would be well to try to restore the original green colour of the unripe fruit to ripe Oranges and Lemons. With this view slices of the peel in small pieces were treated with ether and water as in other cases; by this means a small portion of the colouring matter only was extracted, but the whole of the free tannic acid was removed.

The tannic free ethereal solutions were evaporated and the ferric chloride applied to the residues. That of the orange turned green as in the previous case, shewing that the yellow colour was of the same nature as that in the chlorophyll of the leaves, but in the case of the lemon the result was different and no green appeared; next, pieces of orange peel from which the free tannic acid had been extracted were brushed over with the ferric chloride, with the result that the green colour of the green orange was exactly reproduced.

When however ferric chloride was applied to the ripe peel of the orange from which the free tannic acid had not been removed by water, the result was entirely different, in place of a green, a decided brown, more or less deep according to the strength of the ferric chloride solution ensued, together with a blackening of the torn edges of the peel and of the oil receptacles. In the case of the lemon from which the free tannic acid had also not been removed it was observed not only that the peel became brown, but that the pink and red spots already noticed, gradually became black after a time, on the addition of the ferric chloride. [In drawing the inference, that the green colour produced in the ripe orange peel was due to the iron in the ferric chloride, the fact recorded in a previous part of this article was quite overlooked, namely, that the yellow colouring matter of leaves is rendered green by hydrochloric acid alone. See the second Paper on the Colouration of Leaves.]

There is reason to believe however that while the means adopted were sufficient to remove all the free tannic acid, they failed to abstract that which was either in union or was incorporated in the yellow colour and which therefore the water was unable to reach, but this point will be again dealt with.

Having regard to the constant presence in living and healthy leaves of both iron and tannic acid, it seemed both probable and natural that they should come into contact and form a tannate of iron which resembles in colour and in some of its properties the colouring matter of ordinary chlorophyll; at a later period they do come into contact, but with results altogether different.

A curious circumstance noticed in reference to the yellow petals of the crysanthemum, from which all tannic acid had been removed was, that when kept moist and exposed to the air for some days, they gradually became of a light but decided green colour and this although no ferric chloride or hydrochloric acid had been applied.

Thus, I have now shown in addition to the usual chlorophyll, composed of two colours, that two other green colours resembling chlorophyll, are produced, one from a red colour only, by simply altering the reaction and the other from the yellow pigment by means of a minute quantity of hydrochloric acid.

ON IRON AND TANNIC ACID IN THE COLOURATION OF LEAVES.

Hitherto but little of a definite character has been stated in reference to the colouration of the leaves of plants by Iron and by Tannic Acid, both of which play very important and even essential parts in this as well as in other respects.

Iron enters into a great variety of combinations and forms two classes of compounds, the ferrous and the ferric, each of which is distinguished by certain special characters and properties. Many of these combinations, including particularly those which are in union with an organic acid, as tannic, acetic, and tartaric and some other acids which are often present in the leaves of plants, are remarkable for the variety and brilliancy of their colours, which include various shades of yellow, red, blue and green among the number.

It is admitted that iron is an essential constituent of the green colouring matter, or chlorophyll, and this being so, it follows, as a necessary consequence, that it must be likewise present in the red colouring matter, since this is often derived from and formed out of and at the expense of the chlorophyll, but the actual presence of iron has been repeatedly demonstrated by analysis, not only in the green and red but also in the yellow colouring matters.

My friend Mr. Edwy Clayton, F.C.S. to whom I forwarded the samples mentioned below, for his examination and analysis, found iron in notable amount in the flower stalk and in the red flowers of a hyacinth and in less quantity in a withered leaf of the same hyacinth which exhibited a faint rusty red hue to the eye alone. He also found iron in the dried residues of the alcoholic solutions of the yellow petals of a crysanthemum. In the flower stalks and in the flowers of the hyacinth the iron was in the ferric condition, whereas in the faded leaf it was nearly all in the ferrous state.

The rind of the ripe orange and lemon also contain iron chiefly in the latter state.

But whatever the chemical condition of the iron, it is always bound up with organic products which may not only help the developement of the colour but which are necessary to ensure its stability.

But we have not done with the part played by iron in the colouration of leaves. Hitherto we have been treating of the several colouring matters as extracted from living leaves actively engaged in the performance of their vital functions, but the time arrives, principally in the autumn, when the vitality of the leaves lessens and even ceases and another set of chemical forces and changes comes into operation; some combinations are broken up and others formed.

One of the chief of these changes is the decomposition of the chlorophyll; the iron is now so far liberated as to be free to enter into other combinations. One of the principal effects of this liberation is the change of the leaf from green to yellow; it may be said that the original and primary yellow colouring matter now puts in a fresh appearance and is enabled to reveal itself once more. Another effect of the decomposition is the appearance of red tints on the yellow leaves, these varying in extent and quantity in different leaves and plants and of other tints of which iron evidently forms a principal constituent. Lastly, a third very noticeable and striking result is brought about by the union of tannic acid with some of the iron as also with other bases.

It has been both stated and shown that Tannic Acid is always present, often in a very considerable amount in the leaves of plants, in the petals of flowers and in the several colours extracted therefrom. Now it is very necessary to determine how far this acid affects the colour and composition of the yellow, green and red colouring matters; it is therefore essential to obtain these colours as free as possible from the acid. If the ethereal solutions of the yellow and green colours be separately treated with a little distilled water and if the mixture be repeatedly shaken up and then transferred to a burette, the water at once separates, falls to the bottom and can be readily drawn off. If to this a little ferric chloride be added, more or less blackening always occurs at first; the operation must therefore be repeated so long as the water exhibits the faintest discolouration, a result which usually ensues after the third washing. This proceeding removes all the free, but not any combined tannin, which may be present.

Thus after repeated washing and constant agitation and long after the water fails to show the very slightest indication of colour on the addition of the ferric chloride, the presence in the yellow colouring matter of a very considerable amount of tannic acid has been established. On adding some ferric chloride to the ethereal solution of the yellow colouring matter from which the free tannic acid had been removed, although the solution became green, I observed it quickly assumed a dark and somewhat blackish and suspicious appearance; this change of colour attracted my attention at the time. Subsequently pouring a little of the ethereal solution into a white porcelain capsule, allowing the ether partly to evaporate, then adding a little water with or without a few drops of spirit and finally testing with ferric chloride, I obtained conclusive evidence of the presence of tannic acid in very notable amount in the yellow pigment, although the acid in the free state, and therefore removeable by water, had been wholly and completely abstracted. Some time previously I forwarded an ethereal solution of the yellow colouring matter deprived of the free tannic acid to Mr. Clayton and his attention was directed to this point quite independently and he satisfied himself that the blackening was due to tannic acid, by testing with ferricyanide of potassium and ammonium which produces a reddish brown colour, the test being regarded as a very good one for tannic or gallotannic acid.

The presence of tannic acid in the yellow colouring matter, even after the removal of the free acid is remarkable, and raises the question as to its condition, whether it is in combination or merely mechanically retained, it seems to me that the latter view is probably the correct one, the yellow colouring matter being insoluble in water, this menstrumn, however much it might be shaken up with the ether, might therefore fail to readily extract the tannic acid.

The ethereal solution of *the green colouring matter* or chlorophyll also contains tannic acid as already stated in the free state and therefore removable by water, but when this has been wholly removed by the treatment described above, the behaviour of the green pigment is very different from that of the yellow colour; thus when a little of the ethereal solution, from which the free tannic acid has been abstracted, is partly evaporated, a few drops of water added, and then ferric chloride, no such discolouration appears as that which occurs in the case of the yellow colouring matter; all that is noticed is a comparatively slight darkening and discolouration.

Mr. Clayton also did not obtain from the sample I sent him any evidence of the presence of tannic acid using the ferricyanide of potassium and ammonia test and hence he was led to express the confident opinion that the green colour was entirely free from tannic acid.

Thus then the green, differs from the yellow colouring matter, in the absence of tannic acid, though why this should be so is not apparent, since it is nearly as insoluble in water and therefore almost as capable of resisting its action and so of retaining the tannic acid as the yellow pigment.

But in addition to the absence of the tannic acid, there is another difference in the two colours. Thus Mr. Clayton finds that chlorophyll, though free from tannic acid yet acts as a reducing agent on the ferric chloride, now since the iron in this is in a ferrous state and since the ferrous combinations act strongly in this direction, may not the reducing action in this case be due to this cause?

The *red colour* frequently contains large quantities of tannic acid, which however does not usually seem to exert much effect on the colour when extracted or while in the living and healthy leaf, but if the surface of the dried red extract, especially an alcoholic extract, be touched with the ferric chloride, it will often turn quite black, owing to the formation of tannate of iron. Now since the red colour is soluble in water, as is also the tannic acid, the usual means of separating the tannic acid by ether and water fails.

I find indeed that tannic acid exerts a greater effect on the colouration of leaves than is generally supposed. It is, so far as my observations extend, universally present in the leaves of plants, even in the youngest colourless leaves; the amount varies; in young leaves it is less than in the mature and is in the greatest quantity in those leaves which are at or near the period of their fall, when indeed the effects of the presence of the acid become most marked.

Now tannic acid, like iron, and specially when in combination with that metal, is a very colour producing and modifying acid, although it is itself colourless, unless, exposed to the air when it soon acquires a brownish tint. When added to perchloride of iron it forms a compound usually of a blackish blue colour, but when weak solutions of the acid and iron are brought together, the colour developed is generally a bright grass green; this is persistent and resembles closely that of chlorophyll itself. If to this bright green compound of tannic acid and iron, a little tartaric acid be added, the colour will be gradually and slowly discharged, that is to say in the course of some hours, but if with another portion of the green solution, a little sodium carbonate be mixed, the combination of tannin and iron will be destroyed and the iron thrown down as a red precipitate of ferric hydrated oxide.

Tannic acid, it has also been stated is nearly always present in the coloured extracts of the leaves obtained by alcohol and to a less extent by ether. If to a partially evaporated alcoholic solution prepared from a yellow leaf or flower, a very minute quantity of a solution of ferric chloride be applied and the effect carefully watched under the microscope, it will be seen, that the yellow globules gradually change colour and become much darkened, in fact in some cases almost black. At the same time the partly evaporated liquid itself is rendered darker, but not to the same extent as the globules; if now, a drop or two of a weak solution of tartaric acid be added, it will be found that the blackness gradually clears up and partly disappears and is succeded by a reddish hue. This alteration of colour takes place not only in the globules, which from black become reddish, but in the intervening liquid as well, and appears to be due to the solvent and bleaching action of the tartaric acid on the dark tannate, of iron first formed, iron being a constituent of the ordinary colouring matters of leaves.

It has already been mentioned, that if a solution of ferric chloride be applied to the residue left on the evaporation of a solution of the yellow colouring matter and the amount of the tannic acid present in this be not too great, the yellow colour will be converted into a green, exactly like that of chlorophyll and like the green colour which was produced by the simple admixture of a weak solution of tannic acid and ferric chloride. Under the microscope, it was evident that the change of colour occurred almost entirely in the yellow globules themselves.

As examples of leaves containing much tannic acid those of the Cratægus, the Ivy and the Sparmannia may be cited.

The leaves of the Cratægus, when their vitality is impaired, become of a deep blood red colour; if now these leaves be cut or bruised, the injured parts will on exposure to the air gradually become darkened and if the tannic acid be extracted, it will be found to be present in abundance. The healthy green leaves also contain this acid in considerable, but less amount. Now if a few of these red leaves be watched for some days, it will be seen that they change colour first to reddish brown, then to rich and finally to dull brown, changes doubtless due to the tannic acid which is now free to exert unrestrained its usual chemical action in consequence of the lessened vitality of the leaves. The red leaves of some other evergreens undergo similar changes.

The change in the yellow leaves of the Ivy and of the Sparmannia are still more marked and almost amount to a transformation.

The yellow leaves of the ivy are of a striking golden colour; when bruised or cut into small pieces however, they quickly become on exposure darkened and even black at the bruised parts and cut edges.

The aqueous infusions of both the green and yellow ivy leaves also gradually assume a dark colour and when the leaves are treated with ether and water, while the former menstruum remains almost colourless, the water, on the addition of a little ferric chloride will become of an inky blackness, indicating the presence of much tannic acid. If now a few of the yellow leaves be watched for a day or two after being gathered they will be found to have undergone a remarkable change; the bright gold colour will have disappeared, either in part or wholly, and be replaced by a dull brown; the change is very sudden and striking and has doubtless been brought about by the tannic acid, the action of which is increased after the gathering and by the drying of the leaves.

The changes in the yellow leaves of Sparmannia africana are very similar; but they take place still more quickly and not only when the leaf is gathered, but while it is still on the plant and, when the leaves, which are if possible more brilliant than those of the ivy, are put in water over night; by the next morning they will often be found to have undergone a complete transformation and to have become deep brown, reddish brown and sometimes almost black.

When gathered, the leaves quickly become dried even when placed in water and this drying allows the air to enter them and to act upon the tannic acid, the formation of tannates being thereby promoted. If a small portion of one of the golden leaves be immersed in water it soon turns brown and if a little soda be added, black, the change first taking place in the cut and exposed edges of the leaf.

It is evident therefore, that it is to the presence of tannic acid, especially when in union with iron, that most of the light, deep brown, reddish brown and black tints so conspicuous in the autumnal foliage is to be attributed.

Although in the preceding remarks tannic acid only has been specially mentioned, yet gallic acid, a derivative of the former acid is often present in leaves, but since they both give rise to very similar effects, it was scarcely necessary to discriminate between the two allied acids.

With our present knowledge of the subject, it is very interesting to note the changes which may be observed in the colours and appearances in dead and decaying leaves; the green leaf gradually becoming yellow owing to the breaking up of the chlorophyll, the red tints partly due to the liberation of the iron, the brown and even black tints produced by the union of tannic acid with iron and perhaps some of the other colouring matters and bases. Something may be learnt from the examination of even the simple leaf of a hyacinth; colourless at first, gradually a yellow colour is developed, next green appears, then as soon as the vitality of the leaf is impaired the green fades into yellow and finally into a pale rusty red tint, indicative of the presence of iron; and what is observed in this leaf takes place under like circumstances in thousands of other leaves.

THE HYACINTH.

It occurred to me, that some useful information might be obtained by observing the changes, which take place in hyacinth bulbs placed in water and allowed to grow up to a certain point in the dark. When exposed to the light, not a trace of colour was to be seen in the first hyacinth examined, except in the outer envelopes of the bulb which were deep red. The first colour which appeared was in the leaves; it commenced in the tips and thin edges and gradually spread over their whole surface, the colour being decidedly yellow without a shade of green. Soon a green tint appeared, this went on increasing in depth till a full bright green was attained and this spread over the whole leaf, the only difference being, that the leaves near the bulb, were of a lighter hue. Meanwhile the flower stalks and buds continued to grow; the buds became in succession yellow, green and then pink; the pink first showed itself as in the leaves in the thin edges of the petals, especially on the side of the spike most exposed to the light, finally the flower stalk became of a reddish colour and the flowers turned from pink to deep red.

The second bleached hyacinth had already begun to show in the leaves the yellow tinge. The flower stalk with its buds was cut off, both at the time being quite colourless; this was placed in distilled water and watched for many days. The stalks slowly turned brownish red and the buds changed to yellow and then to a light, but decided green. Things remained stationary for a long time without any attempt being made to develope the pink colour proper to the bulb; in fact for such a length of time did things remain in *statu quo,* that I had quite given up the hope of witnessing any further change. It then occurred to me to remove one of the green unexpanded flower buds. The green petals of this were placed in a few drops of a weak solution of tartaric acid and in the course of a few hours I had the satisfaction of seeing, that they had assumed a distinct and pleasant pink colour; this after a time was discharged into the acid solution, which in its turn consequently became slightly tinted, but every vestige of green had disappeared. Up to this time, the flower stalk with its buds was placed near a window where the sun shone directly on it only for a short period each day. I now transferred the partially developed bunch of blossoms to the outside of the window and fully exposed to the air and sun, the heat being strong enough to raise the temperature of the water several degrees. This seemed to rouse up the energies of the mutilated flower stalk; the green colour quickly disappeared and was succeeded by a delicate, but perfect pink; the topmost buds and their thin edges were the first to change, but soon all the buds underwent the same changes. Thus, wholly colourless to begin with, a yellow, a green and finally a pink made their appearance in succession and at length a bunch of flowers was produced as perfect as though the stalk had never been separated from the bulb and so cut off from its store of nourishment.

It remains to be mentioned, that a portion of a corolla treated with a few drops of a weak solution of tartaric acid quickly changed to a deep red colour and on the other hand, that another portion immersed in a solution of sodium carbonate speedily assumed a bright green hue. Thus, in the first case, the green was transformed into a pink and the pink again brought back to green.

The next hyacinth was exposed in the usual way to the sun and air and hence was healthy and vigorous; it possessed a fine spike of opaque white blossoms; the stalks were green and the stem deep green. One of the flowers when tested with a solution of litmus was found to be neutral; this was torn up and a few drops of a weak solution of sodium carbonate added, when a rich and abundant yellow colour was immediately developed; tartaric acid was next added and this caused the rapid disappearance of the yellow, the flower losing its opaque colour and becoming as transparent as glass; soda being added once more, the yellow colour was restored as at first, but it was not quite so deep.

COLOURLESS COLOURING MATTERS.

The question has now to be considered whether there are not colourless colouring matters in the leaves of plants in the same way as there is a colourless Indigo in plants which yield that substance. It has just been shewn that in the case of the hyacinth a yellow pigment is developed in the white petals of that flower on the application of a trace of sodium carbonate.

I now find that the torn up petals of a number of perfectly white flowers, including amongst others the marguerite, narcissus, stock, clove, lady tulip, Eygptian lily, azalia, rose and orange, when treated in the same way with a weak solution of soda, furnish a brilliant yellow, and in some cases traces of blue, green or pink Hitherto I have never met with any petals which have failed to manifest the yellow colour when thus treated. Rapidly as the yellow is developed, it is as quickly effaced on the addition of a weak solution of tartaric acid, the fragments of the leaves being rendered as transparent and colourless as clear jelly.

The statement will be remembered that the smallest heart leaves of the red Cabbage, though absolutely colourless themselves, yet when placed in water acidulated with tartaric acid turned of a pretty and decided pink. In this case, there was no question of the existence of a yellow tint or colour to serve as a foundation for the red, moreover the addition of an organic acid to a yellow does not produce any development of a red colour, but simply exerts a bleaching effect; the conclusion therefore is, that there is a second colourless principle; the one furnishing a yellow and the other a red colour.

Again, attention has been drawn to the fact, that the alcoholic solutions of the red colouring matter of the leaves of many plants are often but little coloured or even almost colourless and yet when a few drops of such solutions are evaporated to dryness they acquire a deep carmine colour: a similar development or intensification of the red colour takes place when a small quantity of tartaric or other suitable acid is added to the almost colourless alcoholic solutions themselves. These facts afford further evidence of the presence in many leaves of a colourless principle, convertible under the requisite conditions into a red pigment.

It has been shown, that the first colour to make its appearance in most cases, is the yellow; this is so in the bleached leaves of many plants, as those of the hyacinth when grown in the dark and in the central leaves of a large, hard, green cabbage which are inaccessible to light.

The fact of the existence of two colourless colouring principles or substances furnishes a very simple explanation of some of the more important phenomena attending the colouration of leaves. The yellow principle accounts for the development under certain necessary conditions, such as exposure to light and air, in a colourless leaf, of the yellow pigment, just as we see this actually occur. In the same way the presence of the blue pigment, so readily convertible into the red colour, explains the appearance, also in a colourless leaf or flower, of the red pigment. Then thirdly, when both these principles or substances are developed together in the same leaf a green colour would probably appear in some cases. An explanation approximately correct may be afforded of the development of a yellow pigment from the colourless petals of many flowers: iron is always present in these, probably in the form of a colourless organic salt the pure ferrous oxide of which is white; on the addition of the sodium carbonate this oxide would be changed into the yellow hydrated oxide and this on tartaric acid being added would become re-dissolved and the colourless form or condition restored.

CONCLUDING REMARKS.

It is therefore apparent, taking all that has been advanced into consideration, that there are two principal colouring matters concerned in the production of the various hues and tints of leaves and most flowers, a yellow and a red, into the composition of which iron enters as a necessary constituent.

It appears further, that iron also plays a conspicuous part, especially in union with tannic acid, in the colouration of the leaves of plants, more particularly in the autumn season.

The colour of the yellow pigment is readily affected, as already pointed out by a variety of causes; it is deepened by minute quantities of alkalies and changed from light to deep and in some cases reddish brown; while by some acids, as hydrochloric acid, it is rendered blue and green; this green not being so far as is at present known convertible into a red, as is the green colour which is produced by traces of an alkali applied to the red colour of the leaves of many plants.

This latter variety or modification of the red pigment is changed to green by apparently simple causes; thus it is rendered in succession blue, green and yellow by the slightest amount of an alkali, while its redness is increased by most acids.

It is from the yellow and red pigments that all the other tints, except those due directly to iron and tannic acid arise; these tints may be arranged or classified in three series.

Yellow	*Red*		*Green*
Fawn	Reddish brown	Carmine red	Yellow green
Straw	Orange	Blood red	Olive green
Light Yellow	Pink	Lilac	Light green
Deep yellow	Light red	Blue red	Dark green
Brown	Deep red	Blue	
Brownish red			

The yellow element may be traced throughout most of the above colours. All the tints in the first series may be produced by the action of alkalies on the pale yellows. The yellow pigment also enters into the composition of some of the colours of the second series, while even the reds and blues of that series are convertible into yellow with the greatest facility and by the simplest means: then the green colours in the third column pass also with readiness from green to yellow. Again yellow is the primary colour in most cases; it is the first to appear in the leaves of hyacinths which have been grown in the dark and in those of hard cabbages from which light has been excluded by the several closely fitting outer layers of green leaves.

The reddening of green leaves by the conversion of the chlorophyll into a red colour, is greatly promoted by exposure to light, warmth and sunshine, the change being generally regarded as due to oxydation of the yellow and red constituents of the chlorophyll. [It seems to me that the view commonly entertained, that the red colour is produced simply by the oxydation of the green pigment is not, in all cases at least, tenable or sufficient to explain the change, for the following reasons; in many cases green and red cells are placed side by side exposed to precisely the same conditions and yet the green and the red pigments keep perfectly distinct. A good example of this is furnished by the coloured zone on the upper surface of the leaves of many ivy-leaved geraniums. The following explanation would appear to be more in accord with the facts. The green colour of most leaves when in health, is due partly to the presence of a waxy material which enables the chlorophyll to resist the action of any acid in the leaf, but in the case of the blue or red pigment no similar material is present and the colouring matter is consequently brought into contact with any acid which may be present.] Here I would refer once more to the leaves of the Sparmannia, the examination of which has afforded so much information. The leaves of this plant are very hairy, the hairs are on both surfaces, but mostly on the upper surface and they spring from the veins; they are long and pointed and consist of several cells, placed end to end, the cell walls being exceedingly thin and diaphanous. From their extreme delicacy it is evident that gases, such as oxygen and carbon dioxide would find a ready entrance and exit through them. Now these hairs are usually quite colourless, but the cell at the base of each hair where it joins the leaf is filled with colouring matter: this may be green, but it is usually bright red. This red colour is very persistent and is found even in the bright golden yellow, as well as in the green leaves.

In the case of the very young red leaves of the Gloire de Dijon rose and many other plants the development of the red colour precedes or, is in advance of the green, which however also appears very early in the growth of these leaves, although it is more or less concealed under the red colour. The question arises, what is the source in this case of the red? it can hardly be supposed to be derived from the green colouring matter which has as yet scarcely put in an appearance. One explanation is that the red colour is due to the conversion of the colourless into the coloured red pigment as previously described.

Later on then, as the development proceeds, all the red leaves of this rose become gradually transformed into green leaves. This curious change admits of explanation, at all events to some extent. When some of the larger red leaves are examined under the microscope, it is seen, that the red colour does not usually cover in a continuous layer the whole surface of the leaf, but that there are intervals of freedom from it, these intervals being of a green colour; again, if the red pigment be extracted from the leaf by water the green underneath will be revealed and the whole surface will now appear as green. Further, the red leaves being only partly developed, are but small in comparison with the full grown leaves and thus, by the growth and expansion of older leaves green becomes the predominant colour.

In examining a number of dying or dead yellow leaves, it will be observed that in many of them red tints, more or less deep, are visible; these are due partly to the decomposition of the chlorophyll and the liberation of its red constituent and in part also to the iron.

In bringing these remarks to a conclusion, I would observe, that while the subject treated of seems somewhat complicated and the details numerous, yet that the entire result of the investigation has led up to great simplicity; thus, notwithstanding the variety of colours and tints, it appears, that the primary colours are limited to two, and that the many modifications of these, are due simply to the reaction of the fluids with which they are brought into contact, whether these be just acid, neutral, or a little alkaline. Here the fact already pointed out may be again adverted to, that the colour of chlorophyll is maintained in those cases in which the sap is very decidedly acid, as for example in many geraniums and in the vine, partly by the controlling influence of the vital forces, and partly by the protection arising from the insolubity of chlorophyll in water, due in a measure to the presence of the wax-like material.

Many minor modifications of colour arise from the age and texture of the leaves; thus, young leaves are usually of a much lighter and brighter green than the older leaves; this difference is seen especially in the spring and is not due so much to the chlorophyll as to differences in the texture of the leaves, that of the older being thicker and denser than the younger leaves and hence permitting less readily the passage of the rays of light.

Reference may here be again made to the influence exerted by the vital forces in action in healthy leaves; how great these are, is shown by the remarkable changes which occur in the colour of leaves in the autumn when their vitality is impaired, or at an end and the decomposition of the chlorophyll and other changes take place.

Then again, it is under this action of vital and chemical forces combined, that a variety of constituents, including the organic acids, as tannic, mallic, oxalic, tartaric and citric acids are formed and these acids in their turn exert marked effects on the colour of the leaves, especially on the red tints; some acids alter and destroy colour, results brought about either by a bleaching action or by the decomposition of the chlorophyll, or by both acting together.

Living in the Riviera, as I have now done for some years and not being within reach of any large public library, I have not been able to make myself acquainted with the literature of the subject, treated of in the preceding pages; this perhaps in some respects has not been a disadvantage, as it has left me free to follow out my own course of observation and investigations untrammelled and unbiased.

ON THE COLOUR OF THE LEAVES OF PLANTS

BY

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Since the date on which my Paper on the Colouration of the leaves of Plants was presented to the Society I have continued my observations and investigations. The chief results of these are now embodied in the present communication which may be regarded as a continuation of the first Paper.

Chlorophyll, as usually obtained, consists not only of the colouring matter, but is a mixture of several substances as sugar, gum, mucus &c; the presence of which interferes materially with its examination. This admixture arises mainly from two causes, the water contained in the leaves from which the chlorophyll is removed and that often present in the media employed for its extraction, as alcohol and ether. In order therefore to obtain as pure a chlorophyll as practicable, it is necessary that the leaves should be deprived of all their superfluous moisture, which amounts in many cases to 80 and even 90 per cent and that the alcohol or ether used should be anhydrous. The following is the mode of preparation and extraction which I have pursued. The leaves have been carefully selected, cleansed, finely comminuted by means of an ivory or bone paper knife, no metal being allowed to come into contact with them and then dried at a very low temperature and coarsely powdered. When thus prepared, the chlorophyll has been extracted with absolute alcohol or anhydrous ether. The alcohol was allowed to act on the leaves usually for two or three days. The first quantity of spirit used was then poured off and a fresh quantity added, this operation being repeated at least three times, until nearly the whole of the colouring matter has been removed. The solution thus obtained should be passed through a filter of fine paper and then evaporated at a very low temperature, it being carefully protected from dust. In this way a fairly pure chlorophyll will be obtained and some substances excluded which would otherwise be retained.

It is usually stated that chlorophyll is insoluble in water, but it is really much more soluble than is generally supposed. Thus if a strong alcoholic solution be diluted with two or three times its bulk of water the mixture simply becomes turbid, but no deposit takes place; if eight or ten times the quantity of water be added the mixture becomes almost, and in some case quite transparent. The green colour is proportionately diminished and if the spirit contain any acid derived from the leaves employed, the green colour will entirely disappear and be replaced by yellow. This change arises from the action of the acid which destroys the colour of the blue constituent of the chlorophyll.

If however to a highly concentrated alcoholic solution of chlorophyll a very small quantity of water be cautiously added, a copious deposit is thrown down which may be easily separated by filtration and which consists of chlorophyll, probably in the purest state in which it can be obtained.

When an alcoholic solution of chlorophyll is evaporated a residue is left, usually of a sticky nature, on which water exerts but little effect. This insolubility is due to the presence of a wax-like material in variable amount, it being in some cases greater than in others. That this explanation is correct is shown by the fact, that the residue is quickly rendered soluble by an alkali, by alcohol, or by any other similar solvent. The presence of a certain amount of an insoluble material is necessary in order to prevent the too ready action of either water or acid upon it, the latter especially, as it would speedily effect the decomposition of the chlorophyll and change the colour of the leaf. The more substantial leaves, as those of many evergreens, usually contain more of the wax-like substance than thin herbaceous leaves, as the leaves of grasses and the fronds of ferns, mosses, &c. and hence the chlorophyll obtained from these latter sources is in some respects more suitable for observation. On this account I prepared, by means of absolute alcohol, several small quantities of chlorophyll, obtained from plants of the orders above named and subjected them to a close optical scrutiny. A drop or two of each of these solutions was placed on a white porcelain slab and carefully examined by means of a strong lens as the evaporation proceeded; the drops spread themselves out in a more or less circular form, the colouring matter being deposited for the most part on the outer margins. Occasionally the chlorophyll was seen to break up into two colours, the outer circle being yellow and the inner blue; this separation of the colours I have repeatedly witnessed and it seems to show that chlorophyll consists simply of a mixture of the two colours and that these are not in chemical union or combination.

Of the two colours of which chlorophyll is composed one, the yellow is almost insoluble, while the other the blue, is soluble in water; furthermore an alkaline or neutral medium is essential to the existence of the blue colour which would be at once destroyed by the contact of the smallest amount of acid. It has often surprised me in my examinations of chlorophyll that I have met with so little ocular evidence of the presence of the red colouring matter into which the blue colour of the chlorophyll is so readily converted by the presence of an acid. The microscope does indeed reveal the presence in decomposed chlorophyll of a red constituent; nitric and some other acids applied to the chlorophyll give rise to a temporary pink colour or blush, while occasionally some evidences of a red colour are visible to the naked eye, but I have always been disappointed in not obtaining any indication of the formation of a red from the blue colour in thin layers of chlorophyll spread out on a white surface by lightly brushing them over with a weak solution of tartaric or some other suitable acid. I have however at last detected the reason of this failure; the blue of the chlorophyll under such circumstances does not pass visibly into a red colour at all on the action of an acid, but into a *yellow*. This change may often be well seen in drops of chlorophyll which have been allowed to undergo spontaneous evaporation; a somewhat similar change takes place in the blue or rather the red colouring matter of leaves on the application of a weak solution of an alkali.

This transformation of the blue colouring matter is especially brought about when the vitality of the leaf is impaired and thus a very simple explanation is afforded of the chief and most striking change of colour which takes place in the autumnal foliage, namely; that from green to yellow.

THE RED COLOURING MATTER.

There are at least two kinds or modifications of red colouring matter in the leaves of plants, each possessing very distinctive characters. As an example of the one kind, the red leaves of Acheiranthus may be cited. Not only are the leaves of this plant red, but the whole plant; on the upper surface, the leaves are plum coloured, some of them presenting a greenish hue or tinge, while the under surface is of a bright chocolate red. When prepared and dried in the ordinary manner and subjected to the action of water, the colour is quickly extracted and the liquid very speedily becomes of a beautiful madder red, the portions of leaves from which the red pigment has been extracted being now mostly of a green colour; these portions after having been dried, were next treated with absolute alcohol which soon assumed a bright green tint, leaving the broken leaves almost colourless. The same results ensued when the process was reversed, that is when alcohol was used first and the water afterwards, the spirituous solution being green and the aqueous of a red colour. Then again absolute alcohol, anhydrous ether, benzine and chloroform all failed to extract the slightest tinge of red from the red leaves, although these after the removal of these menstrua immediately yielded their colour on the addition of water.

These observations prove clearly that the leaves of this acheiranthus contain two colouring matters, a green soluble in absolute alcohol and a red freely soluble in water, but wholly insoluble in alcohol. The red colour is deepened by nitric, hydrochloric and most other acids and is made first blue and then yellow by alkalies, but in no case is the least tinge of green ever developed. It was evident that these two colours were not mixed up together in the leaves, but that each occupied its own cells; indeed it could not be otherwise, because for the red an acid, and for the green colour an alkaline medium is necessary. It may now be stated that this kind of red colouring matter is found in the leaves of some other plants, including those of the Red cabbage and Beetroot. [In my first Paper it was declared, that the red colouring matter of the red leaves of the cabbage and beetroot were to some extent soluble in alcohol. The explanation of this statement is, that in the communication referred to the alcohol employed was not absolute, but contained a small percentage of water, and hence the partial solution of the red colour. In the investigations described in this Paper the alcohol and ether used have in all cases been anhydrous, in which the red colour of the plants above named is entirely insoluble.]

The water free alcoholic and ethereal solutions of the red cabbage leaves were of a very pale greenish hue, while the watery solution was deep blue and this when evaporated deposited a residue which became of a bright and beautiful green colour when touched with a very weak solution of sodium carbonate.

The corresponding alcoholic and ethereal solutions of the beetroot leaves were also pale green, but the watery solution was red and the red colour, in place of becoming green on the addition of the soda solution was rendered of a blackish hue, not a trace of green being anywhere visible. Thus while the red colour of the leaves of the red cabbage agrees with that of some other red leaves in its insolubility in alcohol, it differs in being changed to green by the action of an alkali, it in this respect agreeing with the red pigment of the leaves of various species of geranium and so many other plants.

Good examples of a second kind of red colour are furnished by the leaves of two species of the Copper Beech which I have subjected to examination. One species has leaves two or three times larger than those of the ordinary wide spreading beech tree. These leaves are more or less rusty red on the under and of a dull sometimes reddish green on the upper surface. They convey to the unaided eye the impression of the presence of iron and much tannic acid and the latter is certainly present in considerable amount. Distilled water when added to these leaves soon acquires a very pale pink colour, in fact so pale indeed as to be scarcely coloured at all, but on the addition of a little nitric, hydrochloric or tartaric acid, the colour is immediately intensified and converted into a beautiful red. When a few drops of the aqueous solution are spontaneously evaporated on a white porcelain surface a curious and interesting change of colour is observed. As the drops dry and redden, the margins, where most of the colouring matter is deposited, gradually turn from red to blue; this change of colour is the more remarkable since the pink liquid is distinctly acid. If now a weak solution of sodium carbonate be added to the blue margins of the drops they are at once converted into a bright and beautiful green, which however soon passes into a yellowish and then a reddish brown. There is no reason to doubt that this red colour is entirely free from any admixture of yellow colouring matter, so that here we have an instance and not a solitary one of the production of a green colour from a single colour and that a red which must however be first converted into blue. It has occurred to me that since the aqueous solution not only exhibits an acid reaction, but contains tannic acid, the presence of this may possibly explain the conversion of the red into the blue colour which takes place in the drying of drops of the aqueous solution.

If instead of water we employ absolute alcohol, we obtain a dull green solution, exhibiting in some lights a decidedly reddish tint. If a little of this solution be evaporated in a watch glass the residue left is peculiar; the green colour separates in flocculi for the most part, though some is deposited forming a pale outer ring, but the red colour, and this is the chief constituent, is more generally distributed over the surface of the glass; the solution possesses to the last an acid reaction. Whether the red pigment is ever converted in the leaf, into the green, is uncertain, but if so it is necessary that it should lose its acid character and be first transformed into a blue colour. The leaves of the ordinary copper beech furnish very similar results, although they contain more of the green and less of the red pigment.

Now this second kind of red colour is of still more frequent occurrence: it is met with in the hyacinth, beech, geranium, rose, castor oil plant, and cratægus among others. At the time when my first communication was completed, the two kinds of red pigment had not been fully discriminated, but some of the descriptions therein given clearly apply to the second kind; thus, reference is more than once made to the fact, that some red leaves furnished nearly colourless solutions which on evaporation yielded a deep red residue, in the same way as does the colouring matter derived from the two species of beech.

This red colour then is distinguished from that first described in several particulars, especially in its solubility in absolute alcohol and its conversion into a green pigment on the addition of an alkali.

But it is not only from leaves that a red colour convertible into a green by the addition of an alkali is obtained, but from flowers also; thus the petals of a scarlet poppy furnished with spirits of wine a pink solution, the dried residue of which became a magnificent carmine and this on the addition of a little sodium carbonate was converted into blue and then bright green.

THE YELLOW COLOURING MATTER.

Of the two colours contained in the ordinary green colouring matter of leaves, chlorophyll, the yellow is insoluble and the blue soluble in water. Now the blue colour is exceedingly delicate and sensitive; it is readily changed either by excess of alkali or by acids, the former turning it first green and then yellow and the latter pink or red and then yellow as already shewn. Now this blue colour is much concerned in the change of the leaf from green to yellow, a change very quickly brought about by impairment of the vitality of the leaf as previously stated or by even exposure to the air and the drying of small quantities of alcoholic solutions of chlorophyll prepared from the leaves of plants of simple organisation. It is thus that the change in leaves, from green to yellow, in autumn is to be explained.

In my first communication it was stated, that hydrochloric acid rendered the partly concentrated alcoholic solution of the yellow colouring matter at first green and then blue; it was likewise pointed out, that when ferric chloride was lightly brushed over the surface of the dried yellow colour deposited from an alcoholic solution of yellow ivy leaves, a green colour was also obtained, but it was a question whether this greening was due to the action of the iron on the yellow colour itself or on the tannic acid, which was known to be present in the yellow pigment. This point I am now enabled to clear up.

I find that the green colour is still produced in the yellow obtained from geranium leaves when this is entirely free from tannic acid and is touched with ferric chloride. Further I find that the green and blue tints are also produced by hydrochloric acid alone when added to the evaporated residue of the yellow colouring matter obtained by means of absolute alcohol; hence it is evident that the green colour is neither due to tannic acid nor iron, but arises entirely from the action of the hydrochloric acid, so that thus the ivy yellow corresponds in his respect with the other yellow colours hitherto examined, with one exception to be presently noticed. Nitric acid also gives rise to some amount of greening, but the green soon passes into reddish yellow and yellow.

Again it was stated in my previous paper that the peel of the ripe orange, when freed from tannic acid was also turned green by ferric chloride and it was assumed that in this case the change was due to the iron in the ferric chloride. Mr Clayton F.C.S., who has taken great interest in these investigations, finds that the green colour in this case also is produced by hydrochloric acid alone, so that the yellow colour of orange peel follows the same rule as the colours above mentioned, but Mr Clayton has made the further observation that the peel of the ripe lemon does not under the same circumstances give rise to the formation of a green colour and he even points out that the peels of the two fruits may always be distinguished by this test. I find that there are still other remarkable differences between orange and lemon peel; thus the peel of the ripe lemon when immersed for some hours in a solution of sodium carbonate gradually becomes in some cases of a red colour, whereas the peel of the orange treated at the same time and in like manner is comparatively but little affected.

With a view to trace if possible the reason of the differences in the behaviour of the two peels, the colouring matter of each was separately extracted by water, absolute alcohol, and anhydrous ether, the fresh peels having been previously comminuted and dried.

The watery solution of the Orange peel, which was acid, was of a pale yellow colour with a tinge of red; treated after concentration by evaporation, with hydrochloric acid, it became a bright yellow, turning after a time to reddish brown; with nitric acid it became decidedly reddish, while sodium carbonate rendered it a very bright canary colour: the alcoholic solution was of a rich golden or reddish amber, this when concentrated in a watch glass and treated with hydrochloric acid became green, but with nitric acid the green colour was at once developed, though the green was less abundant and permanent than the blue and green colours produced by hydrochloric acid, while on the other hand more red was developed by the nitric acid; the ethereal solution of orange peel was quite yellow and without the slightest tinge of red; in the evaporated residue treated with hydrochloric acid, the green and blue colours were slowly developed, but with nitric acid these colours appeared more quickly.

The evaporated watery solution of Lemon peel which was also acid was of a very pale yellow or straw colour; treated with hydrochloric acid it became bright lemon yellow changing to a brownish yellow; with nitric acid it turned of a deep blood red with slight effervescence and with sodium carbonate a bright reddish yellow; the alcoholic, though somewhat deeper than the watery solution was still pale yellow and without a tinge of red; with hydrochloric acid it became of a somewhat deep yellow with a faint reddish and then brownish tint and with nitric acid a blood red colour with effervescence; the ethereal solution after evaporation was not made green by hydrochloric or nitric acids.

The contrast in the depth of the colour of the alcoholic and ethereal solutions of the orange and lemon peels was very great; the former were deeply tinted, while the latter were little more than straw coloured; there was also a great difference in the amount of extractive obtained, this being very inconsiderable in the case of lemon peel. It is evident from their behaviour with the reagents above named that there is a great difference in the colouring matters contained in the two peels.

With a view to elicit further information, the small pieces of the peel of the orange and lemon were now removed from the media employed in the extraction of the colouring matters; they were then well washed in the same media, to free them from dissolved colour and afterwards dried. After the above preparation they were subjected in succession to the action of the same reagents with the following results: It should be premised that both the peels still retained to some extent their respective colours.

The Orange peel which had been subjected to the action of water became deep green and afterwards blue when treated with nitric and hydrochloric acids and was reddened somewhat by sodium carbonate; the peel which had been extracted with alcohol on the addition of nitric acid became of a light red with just a tinge of green, the red soon passing into a light amber and finally a bright yellow colour, but with hydrochloric acid it still assumed a bright green tint, while with sodium carbonate there was but little increase in the reddening. Lastly, the peel extracted with ether presented much the same colours as that after the action of alcohol, while with sodium carbonate the colour was but little deepened.

The Lemon peel which had been treated with water, in place of becoming blood red with nitric acid exhibited only a slight reddish tinge turning to pale amber; with hydrochloric acid it became very nearly colourless and by sodium carbonate it was not reddened at all after several hours; these results show that the water had very nearly removed from the peel the whole of the colouring matter from which the red colour was developed in the first instance.

The lemon peel which had been extracted with alcohol turned brownish red with nitric acid, bubbles of gas being given off for some time; with hydrochloric acid it was rendered somewhat pale and with sodium carbonate it became for a time a bright yellow and after some hours assumed a reddish tint. The peel which had been acted on by ether became with nitric acid of a still deeper and darker red than that from the alcoholic solution, there being also a less obvious disengagement of gas; with hydrochloric acid the peel was paled somewhat, while with sodium carbonate it was first rendered a bright yellow and then decidedly reddish. These results show that the ether had but little effect in extracting the red colouring matter.

Still further observations have elicited some additional facts of interest in relation to the peels of the orange and lemon.

The watery solution or infusion of the fresh ripe peel of the Orange was of a somewhat deeper tint than that of the lemon; both when evaporated yielded a decided and yellowish red residue scarcely distinguishable the one from the other and both being changed to bright yellow by nitric and hydrochloric acids and by sodium carbonate; the red colour was no doubt due to the action of the small quantity of citric acid derived from the peels. The portions of orange peel after extraction with water when dried were nearly of as deep an orange colour as at first, while those of the lemon peel were very pale and dull looking.

The fragments of orange and lemon which had been acted upon by water after having been thoroughly dried, were divided into two parts and treated the one with absolute alcohol and the other with ether. The alcoholic solution from the Orange peel was bright yellow but the peel itself remained nearly as red as at first, thus shewing that neither water nor alcohol had extracted the red colour. The ethereal solution from the orange peel was as much coloured as the alcoholic solution, but it possesed a reddish tinge and the peel had now lost its orange red colour and was bright yellow. The residue of the alcoholic solution was yellow and as in previous trials it was turned green by hydrochloric acid; while the residue from the ethereal solution was pink, but it was made green by sodium carbonate, like the pink and red colour of so many leaves, but it was changed to green and blue by nitric and hydrochloric acids, proving that the yellow colour from the alcoholic and the pink from the ethereal solution were closely related, the former being probably convertible into the latter. The fact of the insolubility of the pink colour of the orange peel in alcohol and its solubility in ether is curious.

The alcoholic solution of the dried Lemon peel which had already been subjected to the action of water was absolutely colourless and did not yield the slightest residue on evaporation notwithstanding which the peel was semi-transparent and possessed scarcely a tinge of yellow; the ethereal solution was light yellow and now all the pieces of the peel were quite colourless, opaque and almost white, the residue from this was trifling and was not made in the least green by hydrochloric or nitric acid; thus there are essential differences in the colouring matters of ripe orange and lemon peels. It would appear that while the yellow pigment of the orange agrees with the ordinary yellow colour of leaves and flowers, that of the lemon is almost entirely soluble in water and has nothing in common with it and hence is not made green, like that of the orange, by hydrochloric acid. It is a question whether the large quantity of citric acid in the lemon may not exert a bleaching action on the peel and so account for its pale tint and possibly its solubility in water.

Finally, the portions of the orange and lemon peels after extraction with water, alcohol or ether were when dry treated with nitric and hydrochloric acids with the following results.

The portions of orange peel after the action first of water and then alcohol, were immediately turned of a deep blue by nitric acid, this colour gradually disappearing in the course of an hour or so, leaving the peel transparent and of an amber red colour. The colour developed by hydrochloric acid was deep green and was more slowly developed, but it also passed away after a time; the orange peel which had been extracted with water and then ether, when treated with nitric acid was also turned deep green and by hydrochloric acid a light bright green, both these greens being much more durable and lasting for days than the blue produced by nitric acid. These results shew that the whole of the yellow colouring matter had not been extracted by the alcohol and ether used. The lemon peel similarly treated did not give rise in any case to more than the development of slight yellow tinges.

It will be observed that some of the results of the latest observations on the ripe peels of the orange and lemon differ somewhat from those first recorded, although in most respects the two series are in accord. The variations are due to the greater acidity of the watery solutions of the peels causing the soluble colouring matter in the evaporated residues, to be converted into a decided red, to the more thorough drying of the peels and to the more complete extraction of the colouring matter by alcohol and ether in the second series of observations.

Allowance must be made for some differences of behaviour arising from the different physical conditions of the peel of the two fruits; also for changes dependent on the degree of ripening and the length of time the fruit has been gathered. This point is referred to in this place, because during the past winter in the Riviera, the reddening effect of alkalies on orange peel was very much more marked than I find it to be as the present time, the end of July in Lucerne.

The Virginian Creeper.

The examination of the leaves of the Virginian Creeper afford interesting information. The leaves of this plant during spring and the early part of the summer are of various shades of green, according to their size and age, none exhibiting even the faintest indication of redness, but towards the end of the summer and in the autumn the green leaves become quickly changed to red, the upper surface being of a dark and the lower of a lighter chocolate red colour.

The *green* leaves were first examined; after they had been comminuted and dried, separate solutions of these were prepared with absolute alcohol, anhydrous ether and water. The alcoholic solution was of an olive green, the ethereal a somewhat deeper green and the aqueous solution was of a faint yellow tinge; the latter had a strong acid reaction and contained much tannic acid: the residue of the alcoholic solution was olive green, that of the ethereal a greenish yellow, while the amount of residue of the aqueous solution was but trifling and was of a pale pinkish yellow. With nitric acid the alcoholic residue became a bright red, with hydrochloric acid a light grass green and with sodium carbonate a bright yellow. The ethereal residue with nitric acid became of a blue green, with hydrochloric acid a still deeper blue green and with sodium carbonate a brownish yellow. The aqueous residue with nitric acid became slightly pink, with hydrochloric acid faintly greenish and with sodium carbonate yellow.

These results seem to show a greater proneness of the chlorophyll of the American creeper to turn red on the application of nitric acid, than that of the leaves of most plants, as indeed was to be anticipated from the great change from green to red, which at a later period these leaves undergo.

It will be observed that the solutions of chlorophyll obtained by means of absolute alcohol and anhydrous ether differ considerably from each other. The colour of the solutions and of the residues thereof is different and the action of nitric and hydrochloric acids is not the same. These differences prove that the composition of the chlorophyll varies in this case according to the menstrua employed in its extraction. It is not only in the case of the green leaves of this creeper that proof of this fact is furnished, but abundant and more conclusive evidence is afforded by the leaves of a variety of other plants. Should the alcohol or ether employed contain an admixture of water, then sugar, gum, and a variety of other constituents soluble in water will be also extracted, as well as the chlorophyll.

The results furnished by the *red* leaves of this creeper were as follow: The alcoholic solution was a dull yellowish red; with ether, the solution was a very pale green without a tinge of red, while the aqueous solution was of a rather deep carmine red. The residue left on the evaporation of a small quantity of the alcoholic solution in a watch glass, was a deep pink, this colour being deposited in a circle, there being also evidence of the presence of a small quantity of yellow colouring matter. The residue from the ethereal solution was of a dull yellow without the slightest tinge of green or red; while lastly the watery solution furnished a beautiful and abundant carmine red deposit. From these results it is evident, that the red constituent of the leaves is only partially soluble in absolute alcohol, not at all in anhydrous ether, but freely and quickly so in water.

Nitric acid turned the pink deposit of the alcoholic solution yellow, hydrochloric acid bright pink and sodium carbonate blue, as in so many other cases. With nitric acid the residue of the ethereal solution became yellow, with hydrochloric acid it was made green. By sodium carbonate the aqueous solution was turned blue and then decidedly green, the blue and the green being immediately reconverted into red by a slight excess of tartaric acid.

On examining the remains of the red leaves after their treatment with alcohol, ether and water, it was seen that those which had been subjected to the action of the alcohol still retained some of their redness; that those acted on by ether were as red as ever, while from the leaves treated with water it was found that the whole of the red pigment had been removed.

It has been stated that the solution obtained by the action of water on the green leaves is very decidedly acid, but that from the red leaves is still more acid, while the alcoholic and ethereal solutions of both the green and red leaves contain comparatively but little acid.

Both the green and the red leaves contain a good deal of tannic acid, the red more than the green; this acid is freely imparted to water, but very sparingly only to absolute alcohol and scarcely at all to anhydrous ether.

Now when a red leaf containing any considerable portion of tannic acid is much bruised and broken up, it frequently undergoes a marked and almost immediate change of colour, usually a darkening of the broken leaf takes place; this arises from the liberation of the tannic acid and its action on the iron or some other mineral constituent of the leaf. It is to this cause, that the somewhat dark and cloudy appearance of the aqueous and alcoholic solutions of the leaves of the Virginian creeper and some other plants, is due and it is owing to the presence of this acid that drops of the aqueous solution of the red leaves turn a bluish colour in the course of evaporation and this although they still exhibit an acid reaction. It is also to the presence of the same acid, that the darkening or even blackening is to be attributed and which ensues to a greater or less extent when a weak solution of sodium carbonate is applied to the residue of the evaporation of small quantities of the aqueous solution of these red leaves. The ready conversion of the red pigment of this and other plants into a blue and then a green colour on the addition of the slightest excess of alkali and the re-conversion of the green into a red by a minute quantity of an acid, such as tartaric, seems to afford a very simple explanation of the change in the colour of leaves so frequently observed from red to green and from green to red. The colouring matter is nearly the same in both cases; the change is mainly one dependent on reaction, but it may be asked how comes it that since the watery solution of the green leaves is so decidedly acid that the green pigment in the leaf is enabled to preserve its colour? The answer to this question has already been given and it would appear to be, that the acid of the leaf does not touch the chlorophyll which is preserved from its action by the presence of a protecting waxy or other insoluble material. When the green leaves turn to red, the change occurs mostly in the latter part of the summer or in the autumn when the vital actions of the leaves undergo changes and are on the decline, so that then any acid which may be formed is enabled to come into contact with the chlorophyll and so to change its colour. Another explanation often given is, that the change of colour is due to oxydation, but the cause first assigned is I believe the correct and chief one, though oxydation no doubt may in some cases account for or aid the transformation.

A *second examination* at a later period of the leaves of this creeper furnished results, which though agreeing in the main with those first obtained, yet differed in some particulars therefrom, probably in consequence of the leaves being more thoroughly deprived of moisture. The alcoholic solution of the *green leaves* was of a bright grass green colour, the ethereal solution was of a yellowish green and the watery solution was faintly yellowish, in fact scarcely coloured at all. The alcoholic solution of the *red leaves* was red but not a very deep red, without a tinge of yellow, as in the first case; the ethereal solution a dull yellow and the watery solution a very dark or blackish plum colour.

The alcoholic residue of the *green leaves* treated with nitric acid became blood red in the centre, passing after a time into yellow, while at the circumference the deposit which had not been actually touched by the acid gradually became deep green; treated with hydrochloric acid it turned greenish yellow in the centre and dark green elsewhere beyond the reach of the acid; by a drop of nitric acid added after the action of the hydrochloric acid, the green colour was changed to red in the same way as when nitric acid only had been applied; by sodium carbonate the residue was turned at first yellow and then *pink*.

The ethereal residue became blue and green with both nitric and hydrochloric acids and a brownish yellow with sodium carbonate.

The aqueous residue was turned pink by nitric acid, yellow by hydrochloric acid and by sodium carbonate a bright canary colour at first, soon passing into a decided *pink*.

The residue of the alcoholic solution of the *red leaves* with nitric acid was changed to yellow, with hydrochloric acid to a lighter and brighter red and with sodium carbonate to a blackish green owing to the presence of tannic acid; the residue of the ethereal solution which was but small in amount was made green by nitric and hydrochloric acids and yellow by sodium carbonate, while that of the aqueous solution with nitric acid became reddish yellow passing into light yellow; with hydrochloric acid the residue passed into a lighter and much brighter red, but by sodium carbonate it was rendered quite black, owing to the presence of the very considerable quantity of tannic acid.

It will be observed, that the alcoholic solution from the red leaves was quite red as was also the residue of the evaporated solution and that neither exhibited any trace of yellow; this absence of any admixture of yellow pigment in this case was no doubt due to the more complete conversion of the green into a red leaf: it will also be noticed that the residue of the alcoholic solution was much darkened by sodium carbonate, while the dark plum coloured residue of the aqueous solution was rendered almost black on the addition of a small quantity of a solution of the same reagent, these changes being due to the very considerable amount of tannin in the leaves and in the extracts, especially the water extract.

It may be further remarked, that the change in the colour of the leaves of the Virginian creeper from green to red is to a great extent independent of the heat of the sun, that is to say, it does not take place during the hottest part of the summer, but after this is past and autumn approached; at this period the change occurs more rapidly and the effect produced by the varying shades of pink and crimson is brilliant in the extreme.

Again the transition is even independent of the age of the leaf, all the leaves, even the very smallest and youngest undergoing this change at the same time. These particulars confirm the opinion already expressed that the red colour is indicative of the decadence of the vitality of the plant, so that ordinary chemical changes now take place uninfluenced by vitality and which changes affect especially the chlorophyll of the leaves and particularly its blue element.

The red leaves of the Gloire de Dijon Rose.

I have examined these leaves again and more fully. It has been stated that the young leaves of this rose are of a bright chocolate red colour. From the extent to which this rose is cultivated, its red leaves form in the Riviera quite a striking feature in the landscape in the latter part of the month of March, especially when the sun is shining. If the smaller and younger red leaves be examined under the microscope, not a trace of a green colour will be visible, but in those of larger growth groups of cells filled with green pigment will be seen. Water applied to the dried and coarsely powdered red leaves removed all the red colour, the watery infusion being by no means of so deep a colour as might be expected, nevertheless single drops of this, although themselves almost colourless, when evaporated to dryness, furnished a pink residue or stain which became first blue and then bright green on the application of a trace of sodium carbonate, the green reverting to red when treated with an acid. The minute fragments of the leaves after the extraction of the red pigment by water were of a green colour. Every trace of red colour having thus been got red of, the green fragments were next treated with absolute alcohol which became more or less green according to the size and age of the red leaves: if these were very young, the alcohol was scarcely made green at all, if older and green the colour would be deeper. When anhydrous alcohol was applied to the leaves from which the red colour had not been previously removed by water, the solution obtained was yet green without a tinge of red, but still drops of this on evaporation to dryness in a watch glass exhibited a well marked pink or red ring. The portions of the leaves after the action of the alcohol were seen to have still retained the greater part of their chocolate red colour, which was thus shewn to be but little soluble in that menstruum. Treated with anhydrous ether the solution also became green, but the residue left on the evaporation was entirely green without a trace of red, shewing that the red colour is entirely insoluble in ether.

The foregoing particulars serve to clear up one or two points which might be open to doubt. The young red leaves, as already pointed out, gradually as they become larger and older assume a green colour, a change which is thus accounted for. However red the young leaves may be, the usual chlorophyll insoluble in water always underlies the red colour and is either wholly or partly concealed by the red pigment and this chlorophyll increases and extends with the growth of the leaves until these become entirely green.

The green colour developed from the red by a trace of an alkali is entirely different from that of ordinary chlorophyll.

Another very noticeable particular in regard to the red colour of the leaves of this rose is the comparative paleness of the watery solution and more especially the absence of the faintest tinge of red in the alcoholic solution obtained from the red leaves; the colour is to a considerable extent rendered invisible in these solutions and yet both when evaporated deposit a bright pink as carmine residue, resembling in colour the leaves themselves.

If a little of the aqueous solution of the red leaves be evaporated to dryness and the residue be examined by the microscope, not a trace of the green pigment is visible, but when the residue from the alcoholic solution is subjected to a similar examination more or less chlorophyll is always to be seen, sometimes even by the unaided sight. These particulars alone are sufficient to establish the fact that the green colour derived from the red pigment and that of chlorophyll green are wholly distinct, but there are other differences; thus if the green pigment deposited from the ethereal solution be treated with sodium carbonate it will not be changed to red.

Cratægus glabra.

In my first paper on the Colour of Leaves, I remarked on the fact that the red colour which characterizes the leaves of this evergreen in the autumn differed from the red pigment ordinarily met with. I have since submitted these red leaves to further examination. The red leaves are usually coloured on the upper surface, the lower being of a pale and dull green. Not unfrequently the young and perfectly healthy leaves are red, while the older leaves are bright green, changing in the early spring to deep blood red and even to reddish brown and brown as the period of their fall approaches.

If strips of the very young red leaves be immersed in a weak solution of sodium carbonate it will be seen that the edges of these turn blue and then green, the green colour gradually extending till the strips are green all over. If however the leaves are older and especially if the redness is indicative of their decadence, then the cut edges of the strips become black in place of green, the blackness at length involving the whole surface of the sections. This difference is due to the large quantity of tannic acid present in all but the very young leaves.

The young, mature and very old leaves, after due preparation were next separately treated with water, absolute alcohol and anhydrous ether. The watery solution quickly became of a red colour, the alcoholic of a pale yellowish green, while the ethereal solution was almost colourless. A few drops of each of the solutions were evaporated in a watch glass. The residues of the aqueous and alcoholic solutions of the younger leaves were of a carmine pink colour which when brushed over with sodium carbonate became bright green, but those of the oldest leaves when treated in the same manner were blackened in consequence of the formation of a tannate. The residue of the ethereal solution was almost colourless, but was made yellow by sodium carbonate, a change suggestive of the presence in this residue of a colourless colouring matter. The portions of leaves after extraction by ether were as red as at first, shewing that the ether had not exerted any solvent action on the red colour.

The leaves of the Horse Chestnut.

The two principal changes in the leaves of plants in the autumn are of course from green to various shades of yellow and afterwards from yellow to varying tints and modifications of pink and rusty red; these latter tints conveying to the eye alone the impression that they are in the main due to iron.

The transformation of the green into the yellow leaf has already been dealt with and it has been shown that the change is brought about by very slight causes; by whatever impairs the vital action of the leaf, by very slight acidity, by drying and even by exposure of the chlorophyll in thin layers to the air. The mere gradual dessication of the leaf is usually accompanied by marked changes in the colour.

It has been stated that chlorophyll proper consists of a mixture of two colours, one yellow, nearly insoluble in water and the other blue, soluble in that menstruum and this last is of so susceptible a nature that its colour is quickly affected and changed by extremely slight causes, as by variations in the reaction of the sap. Now the transformation of the blue constituent of chlorophyll into the yellow pigment takes place usually without any visible formation of a red colour; that this is so the observer may satisfy himself by noticing the changes which take place in very small quantities of an alcoholic solution of chlorophyll allowed to evaporate on a white porcelain surface.

Thus in the yellow leaf of autumn as in the healthy green leaf, there are still two pigments, but both are now yellow, these differing in some of their properties.

If the yellow leaves be treated with alcohol and the solution evaporated, the residue will generally, but not always exhibit a more or less pink or red hue and this would hardly be the case if the yellow colour was entirely due to the presence of the usual yellow colouring matter. Most probably this redness proceeds from a change in the colour of the blue constituent of the chlorophyll.

The leaves of the horse chestnut not merely become yellow, they undergo further important changes; the yellow leaves soon acquire a more or less pink or more frequently a rusty red colour, the causes of which may next be considered. If one of the leaves which still retains its moisture be broken up, it will be seen that even in the act of comminution it acquires a somewhat reddish tint. If now the colouring matter be extracted with absolute alcohol and the solution evaporated, the residue will usually be found to possess a more or less red tint, but it will be seen also that the comminuted leaf will even after the repeated action of alcohol still retain much of its redness; further, that this red colour is not removed by water. It is thus evident that a considerable portion of the red colouring matter is incorporated in the tissue of the leaf and is insoluble in the usual menstrua. If next, the broken leaves which have been extracted both by alcohol and water be dried and incinerated and the ash tested for iron, that metal will be found to be present in both the ferrous and the ferric states. It is thus shown that the red colour is in part due to the presence of a red organic colouring matter, a modification of the blue and in part to iron, as carbonate or peroxide.

Now what is true of the more or less red leaves of the horse chestnut applies equally to the rusty colour exhibited by most leaves in the more advanced stages of their autumnal transformation, the redness in these cases being also partly due to the presence of iron.

The series of changes undergone by most leaves in the autumn occur somewhat in the following order; impairment of the vitality of the leaf; change of colour from green to yellow; other changes due to the drying of the leaf, which exerts a great effect on the colour; the cessation of the chemical changes carried on during the life of the leaf and the substitution for these of other chemical changes, independent of the vitality and due to simple chemical affinities.

The leaves of the horse chestnut contain much tannic acid and this exerts some effect on the colour at the time of their fall; the effect of the drying of the leaf on the colour becomes apparent in many cases in the course of a very few hours.

COLOURLESS LEAVES.

A very interesting particular now to be noticed is the occasional occurrence, especially in certain plants, of white or colourless leaves. One of the best examples of such leaves is seen in the Negando, or variegated Maple. In this shrub the leaves are not only variegated, that is partly green and white, but many twigs and branches even occur which are either nearly as white as paper or which exhibit the faintest tinge of yellow. These leaves are moist, supple, and would seem to be entirely healthy. Treated with nitric and hydrochloric acids a very faint straw coloured tinge ensues and much the same effect is produced by an alkali, such as sodium carbonate. When the leaves are exposed to the air and dried a faint yellowish hue also becomes perceptible. The dried alcoholic extract of the leaves was almost colourless, as well, of course, as the leaves themselves after the action of alcohol. This absence of colour is not due to any deficiency of iron, since analysis proves that this metal exists not only in the alcoholic extract but in the leaves themselves after the removal of the extract.

Examined by the microscope no difference can be detected in the structure of the white leaf, which is furnished with an abundance of stomata.

IRON IN THE COLOURATION OF LEAVES.

This subject has already been dealt with to some extent in my previous paper, but some further observations in the same direction may now be made.

In order to put to further tests the question of the part played by iron in the colouration of leaves, a number of samples of chlorophyll and other colouring matters of leaves obtained by means of absolute alcohol, were most carefully prepared and forwarded to Mr. Clayton F. C. S. for the quantitative determination of the iron present.

The results obtained were as stated below:

Iron in the dried chlorophyll and other colouring matters

Lawn Grass	2.18
Beech	2.61
Yellow Ivy leaves	2.01
Acheiranthus, the green colour of the red leaves	1.41
Acheiranthus, the red colour of the same leaves	3.73
Chlorophyll by precipitation with water	4.02
Yellow Marigold Petals of	5.31
White Rose Petals	1.10

It will be seen that very noticeable quantities of iron are present, not only in the chlorophyll properly so called, but in the yellow and red colouring matters.

In the next series of analyses, the iron in the comminuted leaves from which the chlorophyll and other colouring matters had been extracted, first by alcohol, and then by water, was quantitatively determined as shown below.

Percentage of Iron remaining in the exhausted leaves.

Plane	0.20
Horse chestnut . . .	0.24
Beech	0.07
Lawn grass	0.02
Yellow Ivy leaves . .	0.04
Red Acheiranthus leaves	0.12
Marigold Petals of . .	0.08
White Rose Petals of .	0.08

In comparing the amounts of iron in the chlorophyll and other colouring matters with those contained in the exhausted leaves, it must be remembered that it takes a very large quantity of leaves to furnish a hundred parts of chlorophyll. Bearing this in mind it will be apparent that the exhausted leaves still contain iron in not inconsiderable amounts. Adding the two quantities together the iron could not fail to exert marked effects on the colours and particularly the red tints so conspicuous in the foliage of autumn.

TANNIC ACID IN THE COLOURATION OF LEAVES.

This subject has also been considered in my first Paper; since however this was presented to the Royal Society I have prepared a series of leaves for the quantitative determination of the Tannic Acid present. These estimations were also kindly undertaken by Mr. Clayton with the results set forth in the following table.

Estimation of Tannic acid in dried leaves.

The method employed was Löwenthal's oxydation process by means of potassium permanganate.

Beech	5.3
Oak	7.0
Virginian creeper green	4.2
Virginian creeper, red	4.3
Ivy	6.3
Horse chestnut. . .	3.5
Vine	1.3
Acheiranthus, red .	3.7

Small quantities of gallic acid are also frequently contained in leaves; and this when present is included in the tannic acid.

It is thus evident that the leaves of many plants contain very large but variable quantities of tannic acid and in none that I have examined have I found it to be entirely absent.

There is then in the several colouring matters of leaves, as well as in the leaves themselves, after the extraction of the colours, very appreciable quantities of iron, but these same colouring matters and leaves also contain still larger amounts of tannic acid: now since between the iron and this acid there is a strong affinity whereby they very readily enter into combination, forming sometimes greenish but more frequently dark or more or less inky compounds and since, when the vitality of the leaf is impaired and its disintegration is about to take place and ordinary chemical affinities come into operation, it is inevitable that the iron and the acid should play an important part in the colouration of leaves at the fall, and it is to the presence of these that the rusty red brown and dark tints exhibited by the foliage of a great variety of trees and plants at this period of the year is to be attributed. That they do so was indeed established by the observations and facts recorded in my first communication.

With one other observation I will bring this paper to a conclusion. It is stated, I am informed, in Ramsey's Chemistry, a work of recognized authority, that chlorophyll does not contain any nitrogen, a statement contrary to the view generally entertained. I therefore thought it desirable that two samples of chlorophyll should be chemically tested with a view to ascertain whether the proceeding would throw any light on this point; one of these was obtained from a concentrated alcoholic solution by precipitation with water and therefore probably was as pure a sample as could be prepared. Mr. Clayton reported that the precipitated chlorophyll contained 10.10 and the other 12.60 per cent of nitrogen.

THE CLIMATE OF SAN REMO,

BASED ON

THE METEOROLOGICAL OBSERVATIONS OF ELEVEN CONSECUTIVE WINTER SEASONS

BY

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Read at the Annual Meeting of the British Medical Association held at Birmingham August 1891.

The climatic conditions of any town or locality, are only to be accurately determined by meteorological observations extending over a series of years and embracing many particulars. In the case of health resorts such observations are imperatively necessary, yet in how few of such resorts have they hitherto been recorded in the requisite detail. Observations there are indeed, but, these are usually of a very limited and insufficient character, embracing for the most part merely the Shade Temperature and the Rainfall, but a number of other observations and details are not less important for the full elucidation and determination of any climate.

Influenced by the above considerations, I have taken at San Remo for eleven consecutive winter seasons, an extensive series of Meteorological Observations: these embrace the North Shade Temperature at three periods of the day; the Maximum Shade Temperature; the Minimum in the air, as well as on the ground; the Sun Heat, by both the naked and Vacuum Sun Thermometers; the Days and Hours of Sunshine; the Days and Hours of Rain and the Rainfall. The Temperature of the Mediterranean for some of the seasons has likewise been recorded as also the changes in the temperature and moisture of the air which occur near and at sunset, both important data in the climate of San Remo and the other health resorts of the Western Riviera.

The first three years observations, those for the seasons 1879-80, 1880-81 and 1881-82, were embodied in Papers communicated to the British Medical Association and afterwards published in the Journal. Since this time eight more seasons have been added, the particulars relating to which, are embraced in the present article, chiefly in the form of a series of condensed Tables.

The instruments used were these of Messrs Negretti & Zambra, those for the Shade temperatures being arranged in a Stevenson's screen, placed out of reach of the sun. It should be understood, that the season at San Remo commenences on the 1st November and terminates on the 1st May, that is to say, it covers a period of six months.

NORTH SHADE TEMPERATURE.

Season 1879-80.

Month	9 a. m.	3 p. m.	9 p. m.	Mean Monthly	Mean Maximum	Mean Night Minimum	Mean Day Range	Greatest Day Difference	Mean Maxima and Minima
November .	52·60	56·60	51·40	53·50	59·40	48·40	6·8	13·4	53·90
December .	43·50	49·90	42·20	45·20	51·80	39·20	8·3	13·1	45·50
January . .	45·50	52·10	44·98	47·54	53·30	40·80	7·8	14·0	47·00
February .	50·07	55·89	48·55	51·50	57·32	45·61	5·8	11·5	51·46
March . .	55·00	58·60	51·40	55·00	60·10	47·70	5·1	8·8	53·90
April . . .	59·90	61·70	56·30	59·30	64·10	52·80	4·3	10·0	58·40
Mean of Season	51·09	55·80	49·14	52·00	57·69	45·76	6·3	11·8	51·70

The temperature in the air fell slightly below the freezing point, 5 times in December and once in January; the greatest degree of frost 27.9 F. occurring on the night of December 9th. On some other occasions, the temperature approached freezing point and in very exposed situations, the Lemon trees even suffered. On the 1st December nearly 2 inches of snow fell, which did not disappear till the following day. In mid-winter the mountains surrounding the San Remo amphitheatre above the limits of the Olive-clad hills are not unfrequently more or less covered with snow for some days in succession.

Season 1880-81.

Month	9 a. m.	3 p. m.	9 p. m.	Mean Monthly	Mean Maximum	Highest Maximum	Mean night Minimum	Mean Minimum on ground	Lowest night air Minimum	Mean Day Range	Greatest Day Difference	Mean of Maximum and Minimum
November .	56·9	61·5	54·7	57·7	62·8	70.3	51·9	48·9	43·7	5·9	11·8	57·4
December .	52·6	59·2	51·6	54·5	60·5	65·6	49·9	44·6	45·0	6·9	13·7	54·7
January . .	45·3	49·8	45·1	46·7	50·9	59.1	42·7	39·0	33·0	5·5	12·8	46·8
February .	50·8	55·4	48·4	51·5	57·0	61·8	45·6	40·9	36·0	6·1	14·0	51·3
March . .	54·7	58·4	52·2	55·1	60·4	65·1	49·3	44·1	40·4	5·7	9·5	54·9
April . . .	60·8	63·2	56·7	60·2	66·5	73·1	53·0	51·5	47·4	4·4	9·3	59·8
Mean of Season	53·5	57·9	51·4	54·3	59·7	65·8	48·6	44·8	40·8	5·7	11·8	54·1

The lowest temperature in the air for this season, occurred in January and was 33° F. or 1° above freezing point; but on the ground it descended below that point on 3 occasions; viz. to 30.5; 31.5 and 31.0 F. For some days in January the snow was visible on the higher mountains.

Saeson 1881-82.

Month	9 a. m.	3 p. m.	9 p. m.	Mean Monthly	Mean Maximum	Highest Maximum	Mean Minimum	Mean Minimum on ground	Lowest Minimum	Mean Day Range	Greatest Day Difference	Mean of Maxima and Minima
November .	56·7	59·7	55·6	57·3	61·0	62·6	51·9	—	44·8	4·3	7·0	56·5
December .	49·8	54·3	50·1	51·4	55·3	58·2	46·3	—	41·1	5·5	11·1	50·8
January . .	48·1	53·8	50·0	50·6	54·8	58·7	45·7	41·0	39·9	6·7	12·1	50·3
February .	50·8	54·6	50·0	51·8	56·3	62·8	45·9	41·0	38·0	5·5	11·0	51·1
March . .	55·6	60·0	52·8	56·1	61·0	69·2	48·3	43·1	43·0	5·4	12·2	54·7
April . . .	58·9	62·2	55·9	59·0	63·3	69·5	50·9	46·8	43·9	4·4	10·1	57·1
Mean of Season	53·3	57·4	52·4	54·4	58·6	63·5	48·17		41·8	5·3	10·6	53·4

No frost occured during this season. The lowest temperature was on the 3rd February, when the thermometer on the ground marked 33.4 and that suspended in the air 38° Fahrenheit.

Season 1882-83.

Month	9 a. m.	3 p. m.	9 p. m.	Mean Monthly	Mean Maximum	Highest Maximum	Mean Minimum in air	Mean Minimum on ground	Lowest Minimum in air	Lowest Minimum on ground	Mean of Maximum and Minimum
November	56·90	59·15	49·72	54·99	60·92	66·3	51·03	42·86	43·8	34·3	55·97
December	51·04	55·44	51·65	52·71	56·40	64·7	47·12	39·10	40·8	28·2	51·76
January	48·39	52·83	47·94	49·72	54·02	64·6	43·84	36·70	31·0	22·2	48·93
February	51·15	55·00	50·20	52·12	55·62	61·3	46·60	40·84	41·4	31·4	51·11
March	48·38	51·66	47·41	49·15	53·45	60·1	42·62	33·86	33·8	26·3	48·03
April	56·90	59·44	53·94	56·76	61·14	66·5	49·32	43·80	42·5	36·2	55·23
Mean of Season	52·11	55·57	50·14	52·58	56·93	63·9	46·76	39·53	42·2	29·8	51·84

The lowest point reached in the air was 31.0 F. and this occurred once only in January, but on the ground the thermometer descended to the freezing point 4 times in December, 6 in January and 10 times in March. On January 25th snow fell for 8 hours, but it was all melted the next day; many plants were injured. February 1st snow on Monte Bignone. On the 8th March snow and sleet for 4 hours.

Season 1883-84.

Month	9 a.m.	3 p.m.	9 p.m.	Mean Monthly	Mean Maximum	Highest Maximum	Mean air Minimum	Mean ground Minimum	Lowest air Minimum	Lowest ground Minimum	Mean of Maximum and Minimum
November	55·82	59·62	54·82	56·75	60·55	66·0	51·56	44·64	46·7	35·6	56·05
December	49·18	53·25	49·18	50·53	56·43	60·5	44·86	38·78	37·9	31·5	50·64
January	48·30	54·50	48·67	50·49	55·08	63·7	44·96	40·18	40·0	35·7	50·04
February	51·39	55·40	50·48	52·42	55·76	59·5	46·19	41·10	39·4	31·8	50·97
March	54·76	59·20	52·93	55·63	57·95	65·2	48·72	44·14	42·9	36·9	53·30
April	58·03	61·40	54·94	58·12	62·18	66·5	52·12	48·02	47·4	42·2	57·15
Mean of Season	52·00	57·22	51·63	53·98	57·99	63·5	48·07	42·82	42·34	35·6	53·02

The thermometer in the air this season did not even once reach the freezing point within several degrees, while on the ground in December and February, it only just touched that point on two occasions in each month.

Season 1884-85.

Month	9 a.m.	3 p.m.	9 p.m.	Mean Monthly	Mean Maximum	Highest Maximum	Mean air Minimum	Mean ground Minimum	Lowest air Minimum	Lowest ground Minimum	Mean of Maximum and Minimum
November	52·72	57·12	45·57	51·80	57·74	66·0	46·88	37·50	38·2	28·0	52·31
December	50·93	55·00	49·83	51·25	55·36	60·7	45·61	36·35	39·4	26·8	50·48
January	45·24	51·00	45·57	47·27	51·07	56·3	41·35	31·90	35·8	25·2	46·21
February	50·37	55·28	50·33	51·99	55·35	59·4	45·59	39·10	37·8	29·0	50·47
March	54·85	58·12	52·54	55·17	59·40	66·1	47·84	41·05	42·5	32·8	53·62
April	58·28	60·69	55·69	58·22	60·81	71·0	49·20	44·71	42·0	36·7	55·00
Mean of Season	52·06	56·20	49·92	52·72	56·62	63·2	46·08	38·44	39·3	29·7	51·35

It will be seen that the thermometer in the air this season also did not once descend to freezing point, but on the ground there was frost 5 times in November, 3 in December, 17 times in January, 3 in February and once in March.

Season 1885-86.

Month	9 a.m.	3 p.m.	9 p.m	Mean Monthly	Mean Maximum	Highest Maximum	Mean air Minimum	Mean ground Minimum	Lowest air Minimum	Lowest ground Minimum	Mean of Maximum and Minimum
November	55·85	59·32	54·51	56·56	60·70	68·4	51·54	47·31	46·3	42·0	56·12
December	49·03	53·72	49·47	50·74	54·97	64·0	44·82	38·04	32·9	24·0	49·89
January	45·90	51·43	46·17	47·83	51·90	58·7	42 60	35·60	35·0	25·0	47.25
February	48 93	53·95	48·38	50·42	54·50	67·3	43·20	37·90	35·1	26·0	48 85
March	51·12	55·54	50·72	52·46	56·43	63·5	44·87	38·68	33·3	25·0	50·65
April	59·24	61·53	56·00	58·92	62·69	72·2	50·93	47·12	44·3	41·0	56·81
Mean of Season	51.68	55·91	50·87	52·82	56·86		46·32	40·77			51·58

The thermometer in the air this season also, did not reach the freezing point, but on the ground there was frost 3 times in December, on 6 occasions in January and 4 times in March.

Season 1886-87.

	November	December	January	February	March	April	Mean of Season
Mean Monthly	57·43	49·09	45·28	51·28	53·79	56·90	52·30
Mean Maximum	60·52	53·99	50·97	52·16	58·37	60·99	56·10
Highest Maximum . . .	67·50	61·40	59·50	59·60	67·20	67·60	63·80
Mean Air Maximum . .	50 80	44·80	41·58	41 64	46 69	45·31	45·14
Mean ground Minimum .	44·52	37·72	32·32	32·47	39·88	44·65	38·60
Lowest Air Minimum . .	44·00	38·80	34·40	30·80	37·30	43·80	38·16
Lowest ground Minimum .	34·40	29·70	26·40	20·80	31·40	40·40	30·51

Once only this season did the thermometer in the air indicate frost, but on the ground the thermometer reached the freezing point 5 times in December, 15 in January, 11 in February and 3 times in March. On the 10th and 11th February the frost was so severe that vegetation was much injured. A shower of hail on the 3rd December. Snow on the mountains for some days of the first week in January.

Season 1887-88.

	November	December	January	February	March	April	Mean of Season
Mean Monthly	52·62	48·79	46·91	47·27	49·08	53·00	49·61
Mean Maximum	57·20	54·88	51·70	50·10	54·47	60·99	54·87
Highest Maximum . . .			60·00	57·00	60·80	70·50	
Mean Air Minimum . .	46·83	43·95	44·93	41·03	44.93	50·00	45·28
Mean ground Minimum .			40·91	36·80	40·91	46·20	
Lowest Air Minimum . .			32·40	34·00	37·80	43·80	
Lowest ground Minimum .			24·80	28·60	32·70	40·20	

The mean of this season is by far the lowest of any of the series.

Snow fell on the 31st January for nearly 2 hours, equal when melted, to 00.2 of rainfall. On the evening of 17th February a heavy fall of hail, snow and rain. On the 18th February the mountains were covered with snow, very unusual, so late in the season.

Season 1888-89.

	November	December	January	February	March	April	Mean of season
Mean Monthly	55·48	51·97	49·57	48·05	51·08	55·83	52·00
Mean Maximum	59·00	55·71	54·16	53·20	55·72	59·98	56·28
Highest Maximum . . .	66·70	60·10	69·70	60·10	63·00	67·30	64·48
Mean Air Minimum . .	50·66	46·68	43·49	41·98	44·02	48·16	45·83
Mean ground Minimum .	45·43	40·32	36·48	34·21	38·40	44·46	39·88
Lowest Air Minimum . .	47·40	38·00	35·50	37·80	35·10	43·10	39·48
Lowest ground Minimum .	39·10	29·30	28·10	27·70	26·80	37·20	36·36

The thermometer in the air did not descend to the freezing point during the whole season, but on the ground, frost was indicated once in December; 8 times in January; 12 in February and on 5 occasions in March. On the 4th January a fall of mixed hail, snow and rain.

Season 1889-90.

	November	December	January	February	March	April	Mean of season
Mean Monthly	55·13	49·10	51·95	48·63	52·08	56·51	52·23
Mean Maximum	60·26	52·89	57·24	52·49	56·80	62·65	57·05
Highest Maximum . . .	74·10	58·00	68·20	64·00	63·60		
Mean Air Minimum . .	49·66	42·64	46·16	42·79	44·88	48·66	45·80
Mean ground Minimum .	45·06	37·94	41·23	39·28	40·01		
Lowest Air Minimum . .	39·00	35·80	42·00	30·40	32·00		
Lowest ground Minimum .	34·00	31·50	34·30	29·80	27·60		

During this season, the temperature of the air reached the freezing point on 2 occasions, but on the ground it froze slightly twice in December, 5 times in February and 5 times in March. On the 12th February snow was low down on the mountains and hills and on the 28th there was a heavy shower of hail. On the 2nd March there was a hail storm lasting 1 hour 20 minutes, followed by a fall of snow for 2 hours, the snow being still on the ground in and around the town the next morning. The snow estimated as rain equal to 0.13 inch. Again on the 20th March, there was a heavy fall of hail.

The above Tables show that the mean north shade temperature for the series of years indicated, was 52°.61 Fahrenheit; the mean maximum temperature 57°.18; the mean air minimum 46°.55 and the mean ground minimum 40°.70; they further show that the variation of the shade temperature from season to season is very limited, that the mean daily variation is very small being under 6°, while the greatest variation rarely exceeds 14° Fahr. Again, the mean difference between the day and night temperature is only aboat 6° Fahr. It will be likewise observed from an examination of the tables, that the thermometer in the air seldom reaches the freezing point, in some seasons not even once, though the thermometer in the ground nearly every season descended several times below 32° Fahr. It should be remembered, that the mean season temperature of 52°.61 is based upon three daily readings, 9 a. m., 3 p. m., and 9 p. m. The temperature of the middle of the day and even at 3 o'clock, is almost always much higher than in the morning or evening and it is chiefly to the mid-day temperature that most persons and particularly invalids are exposed and this temperature approximates to the mean maximum temperature 57°.18 Fahrenheit.

SUN HEAT AND SUNSHINE.

The temperature indicated by the Sun thermometers was recorded each day of the season, no matter whether the sun shone or not, since the sun thermometers, even on the sunless days nearly always indicated higher temperatures than those of the North Shade thermometers. Excepting in the case of the first three seasons, the mean Sun Heat has been calculated for all days, as also the mean duration of the daily sunshine. Had the calculation been based only on the sunny days, the means given in the last eight of the Tables would have been much higher. It may be mentioned that on many of the days recorded as sunless, there were gleams of sunshine.

Season 1879-80.

	Maximum Sun heat	Average Sun heat *in vacuum*	Days of Sunshine	Hours of Sunshine	Possible Sunshine	Mean Daily Sunshine
				h. m	h. m.	h. m.
November .			24	182·00	286·07	7·58
December .	120·3	113·0	28	226·00	271·30	8·07
January .	128·0	114·3	30	227·17	279·04	7·34
February .	135·6	118·5	27	208·42	293·28	7·46
March . .	135·4	123·1	29	264·14	363·38	9.10
April . .	146·1	129·7	28	222·37	398·13	7.57
Mean . .	133·08	119·72	27·6	221·4	315·20	8·05
Total . .			16.6	1330·5	1891·0	

The Sun heat and duration of the Sunshine this season were very considerable.

Season 1880-81.

	Average Sun heat *n vacuum*	Maximum heat *in vacuum*	Average Sun heat in Air	Maximum Heat in Air	Excess of Vacuum S. R. Thermometer	Days of Sunshine	Hours of Sunshine	Possible Sunshine	Mean Daily Sunshine
							h. m.	h. m.	h. m.
November .	118·6	130·8	73·0	83·7	45·6	25	190·20	286·07	7·37
December .	114·9	130·7	77·1	83·6	37·8	29	193·24	271·30	6·40
January . .	—	—	61·9	77·3	—	23	119·01	279·04	5·10
February .	—	129·0	72·2	93·0	—	24	183·03	293·28	7·41
March . . .	123·2	139·4	73·8	80·7	49·4	30	219·07	363·38	7·18
April . . .	129·7	144·0	80·0	86·4	49·7	29	272·00	398·13	9·22
Mean . . .	121·6	134·8	73·0	84·1	45·6	26·6	196·10	315·20	7·18
Total . . .						16·0	1177·0	1881·0	

A fairly sunny season.

Season 1881-82.

	Average Sun heat in Vacuum	Maximum Sun heat in Vacuum	Average Sun heat in Air	Maximum Sun heat in air	Excess of Vacuum Thermometer	Days of Sunshine	Mean Daily Sunshine	Hours of Sunshine	Possible Sunshine
							h. m.	h. m.	h. m.
November .	110·0	118·5	73·9	83·6	36·1	24	7·42	184·47	284·54
December .	101·3	107·1	71·0	70·9	30·4	28	6·56	194·12	262·09
January . .	109·1	118·0	68·3	73·0	36·1	29	6·35	184·20	284·02
February .	114·0	123·2	67·8	75·0	39·0	26	9·08	237·41	290·22
March . . .	125·7	132·2	74·2	85·4	40·3	30	10·04	302·07	368·29
April . . .	128·1	136·0	75·4	84·7	43·4	30	10·19	309·45	403·47
Mean . . .	114·7	122·5	71·8	78·8	37·5	167:27·8 per Month	8·27	1412·52 235 per Month	1893·43

A very sunny season.

Season 1882-83. *Season 1883-84.*

Month	Mean Heat Black bulb All days	Vacuum Solar Thermometer All days	Highest with Black bulb Thermometer	Highest with Vacuum Thermometer	Days of Sunshine	Mean Daily Sunshine All Days	Monthly Sunshine	Mean heat Vacuum Thermometer All Days	Highest Sun heat with Vacuum Thermometer	Days of Sunshine	Monthly Sunshine	Mean Daily Sunshine All Days
						h. m.	h. m.				h. m.	h. m.
November	71·87	107·70	81·70	129·80	29	6·17	188·47	110·6	126·8	27	202 31	6·44
December	64·73	101·21	79·00	127·20	27	4·53	151·31	100·9	117·0	29	173·39	5·36
January	63·55	97·11	76·20	118·3	25	5 00	155·00	102·7	119·2	28	216·00	6·58
February	65·18	100·80	77·20	121·8	22	5·45	163·15	105·1	121·0	27	176·00	6·17
March	64·90	111·40	74·40	126·6	29	7·18	236·20	120·7	134·5	31	257·14	8·18
April	75·28	115·16	98·00	130·6	28	9·05	276·25	114·6	136·0	29	265·00	8·50
Mean	67·58	105·56			26.6	6·23	195·13	108·1		28·5	215·04	7·07
Total					160		1171·18			171	1290·24	

A fairly sunny Season.

The number of sunny days this season was very great.

Month	*Season 1884-85.* Mean Heat Vacuum Thermometer All days	Highest Sun Heat with Vacuum Thermometer	Days of Sunshine	Monthly Sunshine	Mean Daily Sunshine All days	*Season 1885-86.* Mean Heat Vacuum Thermometer All days	Highest Heat with Vacuum Thermometer	Days of Sunshine	Monthly Sunshine	Mean Daily Sunshine All Days
				h. m.	h. m.				h. m.	h. m.
November	105·2	117·7	29	220·48	7·21	105·6	129·1	25	112·52	3·46
December	104·5	114·0	29	181·10	5·50	100·6	113·4	28	200·33	6·28
January	110·1	108·2	25	201·00	6·29	96·2	112·0	28	153·26	4·57
February	100·8	120·8	22	165·43	5·55	104·7	128·7	26	162·11	5·47
March	113·3	126·8	31	249·29	8·03	109·1	131·8	27	257·32	8·18
April	115·4	144·9	27	224·00	7·28	120·6	137·0	28	113·16	3·46
Mean	107·9		27·1	207·01	6·51	106·13		27	166·36	5·30
Total			16·3	1242·10				162	999·50	

A fairly sunny season. Much less sunshine this season.

Month	*Season 1886-87.* Mean Heat Vacuum Thermometer	Highest Heat in vacuo	Days of Sunshine	Monthly Sunshine	Mean Daily Sunshine All Days	*Season 1887-88.* Mean Heat Vacuum Thermometer	Highest Sun Heat	Days of Sunshine	Monthly Sunshine	Mean Daily Sunshine All Days
				h. m.	h. m.				h. m.	h. m.
November	104·4	126·2	26	184·56	6·09					
December	99·3	115·0	28	176·22	5·41					
January	100·4	115·3	29	204·26	6·35	94·7	114·8	28	215·22	6·56
February	101·8	121·8	25	200·41	7·10	93·6	110·0	26	164·56	5·41
March	109·4	126·0	30	232·27	7·30	89·4	125·3	30	234·54	7·34
April	115·8	131·0	28	256·43	8·33	112·8	127·4	29	274·37	9·09
Mean	105·10		27·6	209·16	5·50					
Total			16·6	1255·35						

A fairly sunny season, but few sunless days.

Season 1888-89. *Season 1889-90.*

Month	Mean Heat Vacuum Thermometer	Highest Sun Heat	Days of Sunshine	Monthly Sunshine	Mean Daily Sunshine all Days	Mean Heat *in Vacuo* all Days	Highest Sun Heat	Days of Sunshine	Monthly Sunshine	Mean Daily Sunshine all Days
				h. m.	h. m.				h. m.	h. m.
November	95·9	121·0	22	124·29	4·09	106·8	123·4	29	225·05	7·45
December	97·5	111·1	27	163·06	5·16	95·5	109·2	28	181·14	6·03
January .	89·6	117·3	28	188·22	6·04	100·1	115·0	28	184·11	5·56
February .	106·1	116·9	27	199·19	7·07	93·0	116·8	23	163·19	5·50
March . .	107·3	127·0	27	217·12	7·04	105·6	124·1	29	239·49	7·44
April . .	109·5	130·0	26	220·09	7·32					
Mean . .	105·57		26·16	185·43	6·12					
Total . .			15·70	1114·17						

From the preceding Tables, it appears, that the Mean Sun Heat for the several seasons, as shown by the naked black bulb thermometer was 70.80 and that indicated by the Vacuum Sun Thermometer 110°,4 (This very great difference is to be thus explained. The naked thermometer is exposed to the winds and the humidity of the air, from which the thermometer *in vacuo* is protected, so that the latter instrument records the *true* sun heat. It is of course to the former comparatively moderate temperature that the human body is exposed and not to the latter;) that the mean number of days of sunshine was no less than 27.2 days per month; that the mean duration of the sunshine was over 203 hours per month and for each whole season 1221 hours. Further, that the daily sunshine counting all days whether sunny or not, except in the case of the first three years, amounted to 6 hrs. 51 minutes. The smallest number of hours of sunshine was in the season 1885-86, when it amounted to only 1000 hours, but in the season of 1881-82, it reached 1413 hours. It should be stated, that a deduction was always made when the sun shone until sunset, of 15 minutes per day, in consequence of the earlier disappearance of the sun behind the western mountains of the San Remo amphitheatre.

RAIN, RAINFALL AND RELATIVE HUMIDITY.

It is necessary to define what is implied by the heading in the following Tables "Days of Rain". Every day on which the

Rainfall amounts to 00.1 of an inch, is set down as a day of rain or a rainy day, although the rain may have lasted for only a few minutes. In each of the years enumerated some such days are included and they add considerably to the general total. The rainfall includes the whole quantity of rain both by day and night.

Season 1879-80.

Month	Days of Day Rain	Hours of Day Rain	Rainfall	Mean Relative Humidity	Highest Humidity	Lowest Humidity	
		h. m.	inches				
November .	6	31·30	2·40	65·6	94·4	44·2	Strong N. W. W., and N. E. winds.
December .	3	20·00	1·65	58·7	93·1	33·3	Strong N. E. wind
January . .	1	·10	·08	63·2	84·6	44·2	Strong N. E. wind
February .	5	27·00	2·70	74·0	95·8	53·0	
March . .	3	3·00	·20	75·0	85·9	47·7	Strong N. E. wind
April . . .	9	35·30	3·30	70·6	84·6	50·6	S. E. gale.
Total . . .	27	117·10	10·26	67·8			

The rainfall this season was below the average.

Season 1880-81.

Month	Days of Day Rain	Hours of Day Rain	Total Rainfall	Mean Relative Humidity	Highest Humidity	Lowest Humidity	
		h. m.	inches				
November .	7	32·30	4·00	74·6	91·2	55·4	Strong S. W. and W. winds.
December .	2	1·05	0·28	72·6	92·3	49·0	
January . .	12	55·25	6·54	73·3	94·5	37·4	Strong N. E. wind
February .	7	30·30	1·44	68·4	88·2	48·2	
March . .	5	18·45	1·25	76·0	94·4	54·8	
April . . .	10	22·50	1·76	70·7	86·4	53·4	Strong E. wind
Total . . .	43	161·05	15·27	mean 72·8			

It will be seen from the Table, that rain fell in this season on no less than 43 days and if the nights are included, on 49 occasions; this is a very high average for San Remo, since the mean number of rainy days for the whole year, amounts to only about 48.0 days. The rainfall was not in excess and on four of the days it amounted to only 0·01 of an inch, the least quantity measurable.

Season 1881-82.

Month	Days of Day Rain	Days of Rain	Hours of Day Rain	Total Rainfall	Mean 9 a. m.	Relative 3 p. m.	Humidity 9 p. m.	Mean Monthly Humidity	Highest Humidity 3 readings	Lowest Humidity 3 readings
November .	4	5	28·00	9·47	76·8	73·1	73·5	74·5	95·8	55·8
December .	6	7	19·10	1·47	69·1	66·1	66·0	67·1	84·8	46·7
January . .	2	4	19·00	1·42	71·0	72 0	69·1	70·7	85·8	55·8
February .	1	1	1·30	0·82	66· 3	64·2	66·1	65·5	86·0	42·4
March . .	3	5	6·15	2·11	69 5	67·5	70·5	69·2	92·0	45·8
April . .	2	5	7·00	1·35	70·0	63·7	67·0	66·9	85·0	40·6
Total . . .	18	27	80·55	16·64	70·4	67·7	68·7	68·9	88·2	47·8

While rain fell this season during the day time only on 18 days, it also fell on 9 occasions in the night. Although the rainfall was somewhat considerable, the mean relative humidity was rather low.

Season 1882-83. *Season 1883-84.*

Month	1882-83 Days of Day Rain	Hours of Day Rain	Rainfall Inches	Mean Relative Humidity	Lowest Humidity	1883-84 Days of Day Rain	Hours of Day Rain	Rainfall Inches	Mean Relative Humidity	Lowest Humidity
		h. m.								
November .	2	13·20	21·4	69·0	52·9	4	15·15	2·22	71·46	51·0
December .	8	28·40	1·85	70·2	50·0	3	6·30	0·14	66·35	34·0
January . .	3	10·25	2·48	72·6	53·6	2	15·00	0·53	69·84	47·0
February .	4	3·35	3·45	75·2	46·4	4	17·10	1·05	73·40	44·8
March . .	9	44·50	3·72	69·2	43·0	4	13·40	1·21	67·50	42·2
April . . .	7	31·40	36·3	71·0	44·9	8	23·45	4·99	77·36	59·0
Total . . .	33	132·30	17·27	71·2 mean		25	91.20	10·14	70·98 mean	

A fairly average season.

Below the average season as regards rain, rainfall and relative humidity.

Season 1884-85.

Month	Days of Day Rain	Hours of Day Rain	Rainfall Inches	Mean Relative Humidity	Lowest Humidity
November .	2	11·30	0·19	63·54	50·6
December .	5	11·00	1·53	66·57	36·4
January . .	9	26·05	2·70	64·50	49·5
February .	8	43·20	2·89	79·51	59·2
March . .	4	11·30	0·94	70·03	49·4
April . . .	10	36·50	2·96	72·37	50·0
Total . . .	38	140·15	11·21	69·36 mean	

The mean humidity of the air was low, notwithstanding that the number of rainy days was in excess.

Season 1885-86.

Month	Days of Day Rain	Hours of Day Rain	Rainfall Inches	Mean Relative Humidity	Lowest Humidity
November .	17	33·57	3·42	73·09	50·0
December .	3	7·50	0 99	70·48	53·8
January . .	8	39·25	4·26	71·90	47·50
February .	6	26·05	0·58	67·40	33·00
March . .	3	16·45	1·41	74·40	45·4
April . . .	5	13·30	0·63	73·14	48·3
Total . . .	42	137·32	11·29	71·73 mean	

The rainfall and relative humidity both moderate.

Season 1886-87.

Month	Days of Day Rain	Hours of Day Rain	Rainfall Inches	Mean Relative Humidity	Lowest Humidity
November .	11	40·25	10·04	71·35	51·0
December .	6	23·05	5·52	68·42	48·0
January . .	2	12·30	1·86	70·33	52·6
February .	3	13·45	0·82	63·54	46·8
March . .	5	23·30	1·59	69·04	50·0
April . . .	3	20·60	3·71	69·04	48·3
Total . . .	30	134·15	23·54	68·62 mean	

The relative humidity was unusually low, notwithstanding the excessive rainfall of this season.

Season 1887-88.

Month	Days of Day Rain	Hours of Day Rain	Rainfall Inches	Mean Relative Humidity	Lowest Humidity
November .	17	102·00	9·20	68·26	
December .	5	16·00	1·45	70·72	
January . .	5	5·40	0·61	70·80	51·8
February .	10	48·40	5·86	76·40	57·5
March . .	8	40·05	4·81	73·90	57·8
April . . .	7	26·50	2·52	69·51	55·0
Total . . .	52	239·15	24·45	71·60 mean	

The number of rainy days, duration of rain and the rainfall were much greater in this, than in any other season of the series.

Month	Season 1888-89.					Season 1889-90.				
	Days of Day Rain	Hours of Day Rain	Rainfall Inches	Mean Relative Humidity	Lowest Humidity	Days of Day Rain	Hours of Day Rain	Rainfall Inches	Mean Relative Humidity	Lowest Humidity
November .	9	45·40	0·24†	77·12	53·6	2	11·15	1·61	70·65	47·0
December .	7	33·30	3·63	71·83	56·6	5	13·45	1·20	67·42	47·4
January . .	4	20·25	2·54	67·78	38·0	5	19·20	1·39	71·75	47·6
February .	2	15·15	0·59	64·16	42·9	3	10·00	0·18	61·30	50·0
March . .	8	24·40	4·06	71·80	44·8	6	32·30	6·29	70·87	44·2
April . . .	10	35·15	4·97	75·36	58·0	10	59·00	7·23	71·66	
Total . . .	40·0	174·45		71·34 mean		31	145·50	17·90	68·94 mean	

The rainfall in the *early* part of November was not measured. The number of days and hours, during which rain fell, was greater than any previous years. On the afternoon of the 18th January, (3 o'clock) the Relative Humidity was only 34°, the wind being S. E. (Scirocco).

A fairly average season.

The above Tables show that the mean number of Days of Rain for each season was 34.4 days; that the mean number of hours during which rain fell, was per season 141 hrs. 43 minutes; that the mean Rainfall was 15.8 inches and the mean Relative Humidity 70.28°. This mean relative humidity is based upon three daily observations, 9 a. m., 3 p. m. and 9 p. m.; of course the air both morning and evening is almost always more humid, and in the evening, this is even the case when the days have been clear, bright and sunny, since on such days there is frequently a copious fall of dew, which of course more or less affects the readings of the wet bulb thermometer. Hence if the humidity were calculated for any of the midday hours, say even at 3 p. m., the Relative Humidity would be found to be lower than the mean given above. This is shown to some extent in the Table for 1881-82 but, the difference is often greater than is there indicated. See the next Table.

Invalids and delicate people are rarely exposed to the outer

COMPARATIVE RELATIVE HUMIDITY.

Month	Season 1879-80. 9 A.M.	3 P.M.	9 P.M.	Season 1880-81. 9 A.M.	3 P.M.	9 P.M.
November	75·0	60·0	81·5	72·5	70·5	78·2
December	55·5	56·3	64·2	74·7	70·0	76·1
January	63·0	62·0	64·0	78·3	70·7	70·6
February	73·3	70·0	74·2	68·6	63·4	74·2
March	68·5	65·5	61·1	74·7	72·0	82·0
April	69·5	69·0	76·4	68·0	65·0	79·8
Mean of Season . .	67·5	63·8	70·3	72·8	68·6	76·8

air at 9 o'clock in the morning or evening, but for the most part to the drier, or mid-day air.

Another particular which should be borne in mind in reference to the humidity of the air, is the effect of wind; nothing dries the air like a strong wind, and most of the low humidities recorded in the Tables occurred in connection with such winds. One can therefore readily understand the effect produced by them on the animal economy. First, they carry off the surface heat with great rapidity and often faster than this can be reformed by weakly and delicate persons, hence chills are experienced; second, by drying the air to an abnormal extent, they affect many of the functions of the body and give rise to undue excitement.

Another feature of the climate of San Remo and the Western Riviera, is the fall of temperature and increase of humidity which take place, especially on bright and sunny days, sometime before, at and after sunset. For some time before sunset and for a still longer period afterwards the temperature begins to fall and at the same time, and as a consequence of this, dew becomes deposited; this of course increases the humidity of the air and occasions a descent of the wet bulb thermometer, this point was experimentally investigated by me in 1881, as detailed in my work on San Remo. The following figures may be given in illustration of this feature in the climate.

FALL OF TEMPERATURE AND INCREASE OF HUMIDITY AT SUNSET.

December 14th 1881.				
h. m.	Weather	Temp.	Difference of Wet and Dry Th.	r. h.
3·00	Bright	60·4	5·6	68·0
4·00	"	60·2	6·2	65·2
4·30	"	55·3	4·2	74·0
Sunset.				
5·00	"	52·2	2·9	81·0
5·30	"	50·0	2·0	86·0
6·00	"	49·7	2·3	84·8
9·00	"	49·5	2·6	82·2

The columns of figures in the above table indicate in order: the time, temperature, difference between the wet and dry bulb thermometers and the relative humidity.

TEMPERATURE OF THE MEDITERRANEAN.

A few brief remarks may be next bestowed on the temperature of the water of the Mediterranean which is considerably warmer than that of the surrounding air. It will naturally be asked, how is this remarkable circumstance to be explained? The Mediterranean is of limited extent and almost landlocked, the only water access to it being through the Straits of Gibraltar, which admit only a small quantity of the water of the Atlantic ocean, which is about 20 degrees colder than the Mediterranean; the quantity thus admitted is however too small to materially affect its temperature. During the long and sunny Italian summer, the water of the Mediterranean absorbs and stores up heat, which raises its temperature considerably and which is parted with gradually in the colder months of the year, the temperature of the air being thereby much increased. The surrounding rocks and mountains also store up heat in the summer and give it out again in the winter, but the influence of these in augmenting the temperature of the air is much less than that exercised by the sea. It is thus that the water of the Mediterranean becomes an important factor in the climate of San Remo and the other health resorts and towns situated along its shores.

Season 1879-80.

Month	Mean Temperature of the Sea	Highest Temperature	Lowest Temperature	Mean Temperature of the Air	Sea warmer than Air	Greatest Difference
	9 a. m.	9 a. m.	9 a. m.	9 a. m.	9 a. m.	9 a. m.
November .	60·2	63·2	58·8	52·6	7·6	19·6
December .	53·8	56·4	51·6	43·5	10·3	18·4
January . .	52·8	54·9	62·1	45·5	7·3	16·4
February .	53·8	55·0	52·2	50·0	3·8	6·7
March . .	55·2	56·2	52·1	55·0	0·0	6·0
April . . .	57·4	60 4	55·7	59·9	[1])	[2])
Mean . . .	55·5			51·1		

[1]) In the month of April the air was 2.5° warmer than the sea, [2]) and on one occasion it was 8.8° warmer. It appears from the above table, that the sea is warmest just when warmth is most required; viz., during the coldest months of the winter season.

Season 1880-81.

Month	Mean Temperature of the Sea	Highest Temperature	Lowest Temperature	Mean Temperature of Air	Sea warmer than Air	Greatest Difference
	9 a. m.	9 a. m.	9 a. m.	9 a. m.	9 a. m.	
November .	62 0	63·9	61·0	56·7	5·3	11·5
December .	59·8	61·0	59·0	52·2	7·6	9·9
January . .	56·3	58·7	55·0	46·9	9·4	15·8
February .	54·0	55·7	52·6	50·7	3·3	5·1
March . .	55·4	56·6	53·8	55·2	·2	7·5
April . . .	58 6	59·2	57·6	62·0	[1])	—
Mean . . .	57·7			53·9		

[1]) In April of this season the air was 3.5° warmer than the sea. This table affords a further illustration of the greater warmth of the sea during the colder months.

Season 1881-82.

Month	Mean Temperature of the Sea	Highest Temperature	Lowest Temperature	Mean Temperature of Air	Sea warmer than Air
	9 a. m.	9 a. m.	9 a. m	9 a. m.	9 a. m.
November .	61·4	62·5	60·8	56·7	4·7
December .	57·9	60·6	55·8	49·8	8·1
January . .	55·9	56·5	55·6	48·1	7·8
February .	55·1	56·0	54·3	50·8	4·3
March . .	56·4	59·0	55·8	55·6	0·8
April . . .	58·2	59·4	57·3	58·9	0·7
Mean . . .	57·5			53·3	

It will be seen that the difference of the temperature between the sea and the air was not so marked as in the previous seasons.

THE WINDS AT SAN REMO.

This paper would not be complete without the bestowal of a few remarks on the Winds at San Remo. Owing to the encircling mountains, the two headlands of the Bay and other causes, it is at times extremely difficult to tell from what quarter the winds really proceed, since they are often deflected from their original course. The northerly winds sometimes round the Capes and blow on the town as South East and South West winds; the North wind also, is sometimes drawn back from the sea and reaches San Remo as a South wind, though it still retains its coldness. The two special breezes first to be noticed are those common to most tropical and semi-tropical climates; as the Sea or Day breeze and the Land or Night breeze. The occurrence of these breezes is thus explained. The air below and near the contiguous mountains becoming rarefied by the sun's heat, ascends and passes over their tops and the cool air from the sea flows in towards the land to take its place, this in its turn becoming similarly heated and rarefied. At night, a contrary process is in operation, giving rise to the land breeze; in this case the heat of the sun being withdrawn, the cool mountain air descends to the sea. It is hence in the summer that the sea breeze is the strongest, it then commencing at about 8 a. m. and ending about 5. p. m.; but in winter it does not begin until nearer 11. a. m. and ceases about 3. p. m.

One of the best times for invalids to be out in the open air, is in the early morning before the advent of the Sea breeze, as the air is then at its calmest. English visitors and invalids are however, as a rule, somewhat tardy in their movements in a morning and many of them do not leave their Hotels until after the commencement of the sea breeze; as a consequence they sometimes become immediately exposed to wind. Foreign visitors are, as a rule, much earlier in the morning and set in this respect, a good example.

The prevailing Winds at San Remo are northerly; N.N.E. or N. West, but East and West winds are also frequent. The North winds passing over the high mountain chains of Europe, the N. E. wind or Bise, coming even from the Arctic regions are both cold and very dry, their moisture being parted with as they blow over the higher Alpine mountains, on which it becomes precipitated as snow. Two of these chains of mountains are the Alpes Maritimes and the Apennines; behind the former, the higher Alps separating Italy from Switzerland. The North West wind crossing over lower mountain chains and even the North Atlantic, is less dry than the N. and N. E. winds; but the latter, as also the East wind are more moist and less irritating than in England, although they retain in a mitigated degree the characteristics by which they are there distinguished. The N. W. wind, or dreaded Mistral, is much less felt at San Remo than at the other towns lying more to the West, as Hyères, Cannes and Nice. This wind sometimes blows with such violence that it raises clouds of dust which fill the air, as indeed do other winds when blowing very strongly and when this dust is seen people usually speak of the wind, no matter from what quarter it may blow, as the *Mistral,* which is thus sometimes wrongfully anathematized.

Of course San Remo is fully exposed on the sea side to the South winds; these winds passing over the Atlantic and Mediterranean take up much moisture. This is especially the case with the S. W. wind, which thus often brings rain. The S. E. wind or Scirocco, passing over the deserts of Africa, is at first a very hot and dry wind, but when it reaches the Mediterrenean and the South of Italy, it is hot and moist and hence very enervating; but, when it arrives at the Western Riviera, by its passage over the Apennines and the Corsican mountains, it has become dryer and cooler and deprived in a measure of some of its injurious qualities; still it is sometimes felt at San Remo as a very depressing wind.

On the whole, San Remo must be declared to be subject to much wind and this constitutes the one trying feature of the climate.

The statistical data are now before us upon which a clear and accurate statement of the climate of San Remo can be formulated and which in its main particulars is applicable to the Western Riviera generally.

The mean figures given show, that the North Shade Temperature is some 8° to 10°, above that of England; they also prove that the temperature is unusually equable, the range of variation being very limited. The winter season being spread over six months of the year and being partly in one year and partly in another, the temperature of the different months varies considerably, in fact, as far as the temperature is concerned there are really two seasons, the one comparable to an ordinary summer and the other to winter. The first comprises November, sometimes the early part of December, the latter half of March and the whole of April; the colder winter months being, December, January and February. This explains, in part, the very diverse opinions formed by visitors of the climate, some asserting, that it is hot and relaxing and others complaining of the cold. As we have seen, the temperature of the air seldom, even once in the season, marks the freezing point, although that point is frequently reached during the colder months in the night by the thermometer placed on the ground; in the day-time however, even a slight degree of frost rarely occurs, for now the influence of the sun comes into operation. Snow or hail, which are forms of frozen moisture or rain, occurs usually not more than two or three times in a season and in some seasons, none falls. I have never known the snow, when melted, to measure more than 0.14 of an inch of water and usually it is not more than from 0.02 to 0.05 of an inch.

The Tables given of the Sun and Sunshine afford a very pleasing picture of the climate. Out of the 181 days forming the season, there are on the average no less than 163 days on which the sun shines and this generally with remarkable power and brilliancy for a greater or lesser time. Of course the difference between Shade temperature and Sun Heat is sometimes very considerable, this difference being experienced particularly in passing suddenly from Sunshine into Shade.

The Tables of the Rain and Rainfall illustrate another very favourable feature of the climate. The number of days on which more or less rain falls, is subject to a good deal of variation from season to season, but on the whole year the number does not usually much exceed one fourth of those which occur in such places as Torquay, Bournemouth and Ventnor. Then the number of hours during which rain falls is but small and the total rainfall is by no means excessive. It occurs particularly in the Spring and autumn and often falls in very large quantities in a very short space of time. Again, the Relative Humidity is moderate; the air on the whole is dry, but not extremely so; the circumstances which chiefly increase the humidity are, the prevalence of rain and cloud, as well as the opposite condition of cloudless sunshine followed by a deposition of dew at and after sunset and sometimes during the night.

It now remains that the Winds and their effect on the climate, should be noticed. The influence of the day or sea breeze and the night or land breeze, when these are moderate, is usually beneficial; they give movement and freshness to the air, which is changed continually; they dissipate any bad odours which may prevail and scatter far and wide the much debated and dreaded microbes. Beneficial as, on the whole the sea and land breezes doubtless are, they sometimes blow with too great force and there is often superadded the powerful and chilling North East and North West winds which prevail in San Remo and the Western Riviera with far too great frequency. These winds being dry as well as strong, they carry off heat from the surface of the body much faster than it can be regenerated, chilly feelings as well as chills, are apt to be experienced. In fact, the frequency and character of these winds constitute the great drawback of the climate.

To sum up then; the climate of San Remo in the winter season is for the most part moderately warm, fairly dry, fresh, bracing and rather stimulating, bright, sunny and cheering. These good qualities are enhanced by the great beauty of the scenery and the semi-tropical character of the vegetation.

Formerly, it was not unusual to hear the climate of the Riviera described as hot, relaxing and enervating, qualities the very opposite of those by which it is really characterised. Complaints of cold are not unfrequent and these are often well-founded; the coldness experienced is due mainly to the winds, but partly also to the houses; the walls of these are thick and of stone, which resists the effects of the sun; then the rooms are lofty, the windows large and numerous and it is only occasionally that an efficient stove is met with in the halls of the houses and villas. It is hence usually much colder in the house than out of it and this is so well known, that many people go out of doors to get warm.

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