CHAPTER XVIII.

QUESTS FOR REMEDIES AND MEANS OF CURE.

IN the earliest days it was a part of the work of the physician to go out with his "basket" to seek the means of cure and find the natural remedies that lay at his command. The professors of the art have been divided in their search: some have sought for the means of cure solely in the vegetable kingdom; others have tested the properties of the mineral world, and a few have wandered in both. With the natural instinct of my many predecessors I have gone out to find remedies and means of cure, but have neither made Galen or Patin the exclusive guide.

It has been shown that in my first days the study of ammonia as a medicine engaged my thoughts, as well as the medicines of the haloid or chlorine series. I detected, in working with bodies of the chlorine series, like chloroform, that they possessed the property, when their vapours were inhaled, of reducing the animal temperature; and I several times undertook, with signal success, the treatment of diseases of the febrile type by putting the sick under the influence of the vapours named, inventing for the

purpose the bead-inhaler, in which a string of beads was made to receive the remedy, and from which its vapour might be regularly drawn.

Hydrogen Peroxide.

After I had settled in 12, Hinde Street, London, and had furnished my laboratory, I passed to a new study by the investigation of the medicinal qualities of a chemical fluid which the famous French chemist, Thenard, discovered in the year 1818, and to which he gave the name of Peroxide of Hydrogen. The fluid was water charged with oxygen, which gas it gave off freely on coming into contact with different substances, animal, vegetable and mineral. What led Thenard to discover this peroxide I could never detect, although all his works were in my hands in volumes that once belonged to the well-known historian Buckle. I was not the first aspirant in respect to the peroxide itself, for the authorities of the University of Haarlem had offered a tempting prize in regard to it. I was the first to make the inquiry into it as a remedy; and although the award claimed for me by the late Dr. Macdonald of St. Andrews was withdrawn, I had never any occasion to regret the researches I carried out.

It was in the early fifties I commenced, and the reason for the commencement rested on a dispute then actively in progress as to what remedy was the best in the treatment of diabetes. It seemed to me that to give oxygen freely was the most scientific

course, and, following Thenard's method of manufacture, I made the peroxide, for I could obtain none in any chemical depot, and I could hear of none except a little specimen that had been made for the late Sir Benjamin Brodie of Oxford, a chemist, and son of the well-known surgeon of that name. An immense deal of difficulty was experienced in this inquiry from the necessity there was to get a pure salt of baryta with which to start. At last the difficulty was overcome, and a fluid containing so many volumes of oxygen gas was obtained. Next, I had to ask what volume of oxygen should be used in medical art. Thenard had proceeded up to a high standard of volumes, which I, following him to the letter, could never reach. I easily got up to a strength of ten volumes, and there stopped, so as to make that the medical standard, which, except in America, where fifteen volumes have been used, remains the same. I next had to determine the dose of ten volumes solution, and did this by partaking of it myself in measured quantities mixed with water; and from one to four fluid drachms was determined on. Lastly, I had to administer the substance in disease, and in that direction arrived at the most unexpected results. I found that blood added to the solution became of a brighter red colour, and that oxygen was liberated. This fact led me to try what the effect of the addition of pus derived from an abscess would do. I found that pus caused the most copious evolution of oxygen, and that the

matter derived from the vaccine point was sufficient to start the disturbance or ferment. I tested the question by the microscope, and saw that the peroxide broke up pus cells.

I found also that it could be thrown into cavities containing pus, and would cure without danger, if care were used. In another research the curious subject of colour came under notice. I observed, from the accident of stirring the peroxide solution with feathers, that they assumed a golden colour—a fact exhibited at the Medical Society of London in 1858, when I read the first paper written on the physiological properties of the peroxide. From the fact of change of colour under its use arose the process that once made the hair of our ladies become conspicuously stained or dyed a golden hue.

The original attempt to cure diabetes by the peroxide did not turn out so advantageously as was expected, but a widespread medical use of the solution followed. I discovered it to be of true service in whooping-cough, in phthisis pulmonalis, in typhoid fever, in many other diseases, and of special service in all instances where it could be brought into contact with purulent substance. Thus, in the course of time, in the hands of my brethren it has become one of the most widely used and distributed medicines that has ever been employed.

In manipulation with peroxide of hydrogen I dropped on the manufacture of the substance to

which I gave the name of *Ozonic Ether*—a fluid that has proved of value in disease, and has gained, through the labours of the late Dr. Day of Geelong, a large amount of attention as a chemical test in regard to fluid animal excretions.

Amyl Nitrite.

One day, after delivering a lecture at the College of Dentists, I met a gentleman named Morrison, an Edinburgh dentist, who was reported to be a strange and accomplished man. He was said to possess a spark of the old attachment and admiration which used to be held for Mary Queen of Scots, whose gloves and some trinkets he owned and kept in reverent memory. It was to another subject, however, that he now called attention. He brought out of his pocket a two-drachm bottle—I still retain it—that was three parts filled with a brownish liquid, having a strange, pear-like smell, and which he told me was nitrite of amyl. He related that he was a friend of the eminent Professor Guthrie—a chemist I afterwards knew as occupying one of the chairs of the South Kensington Museum—and that in distilling over from his retort nitrite of amyl, the fluid in the bottle to which I have referred, the vapour was diffused, which vapour accidentally inhaled by Guthrie, made him breathless, as if he had been running. On Guthrie's explanation of this phenomenon, Morrison concluded that nitrite of amyl might be of benefit to dentists; that they

might keep it in their consulting-rooms, and that if their patients became faint they might give them a whiff of it, as they gave smelling salts or carbonate of ammonia. The history interested the Council of the College of Dentists; but Morrison's statement alarmed them, and instead of accepting the remedy at once they moved and carried that it should be placed in my hands and that I should report to them upon it. I accepted the duty, and at once began to make nitrite of amyl a particular study.

Research with the nitrite of amyl proceeded in the usual way. I made myself the first victim, and tried the effects of the substance by taking it, both in the form of vapour and fluid. I had also some friends who were willing to follow in my wake, amongst others the late Mr. Kimpton, a dentist deeply interested in the investigation, Dr., or as he was more commonly called, Sir Duncan, Gibb, and Dr. Henry. It came out that the nitrite acted on the organic or sympathetic nervous system; that it was not an anæsthetic, but that it quickened, in the most striking manner, the circulation of the blood. It acted in the same way on the inferior animals as on man, and I discovered that on both it caused the most distinct phenomena of muscular relaxation. I had seen nothing before that completely resembled it, the nearest to it being nitro-glycerine, which had been used by Mr. Field, a surgeon of eminence and of good observation. My description of the effects of

the nitrite was that it relaxed the living muscles of a limb completely; and one day a medical practitioner, Mr. Stedman, who lived at Sharnbrook in Bedfordshire, and who now and then came to see me, breathed the nitrite so freely, in my absence, that he fell on the floor and laid there for several hours in such extreme muscular collapse I was in alarm for fear he should not recover. He was like a person in catalepsy: he could not move a limb; but it turned out that he knew everything that was going on and heard every word spoken. He made a perfect recovery, and returned in about ten hours to his own home.

At the meeting of the British Association for the Advancement of Science, held in Newcastle-on-Tyne in 1863, I read the first paper on "Amyl Nitrite" and illustrated its effects. There was a large audience, and I met with an event I did not expect. Professor George Rolleston was in the chair, and was in rather a sceptical humour. I explained that the nitrite excited the circulation; that it was a substance producing local results like those following the division of a sympathetic nerve, and that the blood-vessels first dilated freely and then became what is called congested. To this statement the Chairman listened carefully; but when, in pouring some of the fluid on a piece of filtering paper, I explained that the vapour which diffused through the room would, without affecting the consciousness, make every person's heart beat more

quickly than was natural, he smiled incredulously and held out his hand for the paper. Thereupon I handed it to him, warning him not to inhale from it. He disobeyed the warning, however, and, before I could stop him, inhaled for a few moments freely, soon showing the consequences. His face became blood-red; he felt the pulsations indicated, and was obviously alarmed as well as astonished at what had occurred. I was able to assure him that there was no danger; and when he got over his shock, he not only apologised, but explained to the audience the folly of being too incredulous about what they heard from experimental observers.

At a subsequent meeting of the Association held at Bath I returned to the subject, explaining that we possessed in nitrite of amyl the most potent anti-spasmodic we had ever known; enumerated the diseases that might be successfully treated by it—such as asthma, angina, colic, and even spasmodic tetanus, in which latter disease it has been successfully used no fewer than nine times to my own knowledge, the late Mr. Foster of Huntingdon being the first to resort to it in tetanus. At this Bath meeting Bishop Colenso, David Livingstone, and Dr. Hughes Bennett were present. They were all extremely interested by the address. Livingstone talked with me about it and its history, as well as about the old school (Anderson's University) where we were both educated; and Dr. Bennett arranged to

call upon me in London to get further particulars, respecting the nitrite, to introduce into his annual opening lecture coming on at Edinburgh—a visit he duly made, and during which he wrote down the list of spasmodic affections that were likely to be relieved or cured by the inhalation of the vapour.

The readings I had given before the Association had an important bearing on my career: they led the committee of recommendation, at the instance of Mr. Gassiott, inventor of the Gassiott tubes, to vote me a small sum of money in order to continue my therapeutical inquiries, and for some years the vote was repeated. The sum was small, never exceeding thirty pounds, and never approaching the outlay I had to make; but it helped me on, and certainly, as an encouragement, kept me bound to my labours, many of which are reported in the *Transactions* of the Association.

Mercaptan.

In pursuing the quest for remedial measures, I studied the action of bichloride of methylene and the other anæsthetics already mentioned; but many others came under my observation of which something should be said.

One chemical which occupied much of my attention was mercaptan, sometimes, but not quite correctly, denominated "sulphur alcohol," an alcohol in which sulphur stands in the place of oxygen. It is a most offensive substance to the smell, and

not agreeable to the taste, although by dilution it is just tolerable. I took it because of its singular effects; and it gave me a remarkable suggestion, which I have published. It affects the mind, making it, as it were, turbid and melancholy, under which the impulse or tendency to commit suicide is one of the leading symptoms. Strangely, too, mercaptan is a body that can be manufactured by a perverted animal chemistry in the living organism, and has been detected in the intestinal excretions. I have no doubt that by its veritable presence in the body it does contribute to deranged mental conditions, and that it is specially deserving of remembrance by those who are engaged in the care and treatment of the insane.

Substitutional and Constitutional Lessons.

Previous to the time when these effects of mercaptan had been reported, I had inferred from the researches on nitrite of amyl that a new method of inquiry in regard to therapeutical substances might be carried out.

It had been shown to me from natural phenomena that nitrite of amyl not only relaxed muscular fibre, but that for a long period after what seemed to be death, it caused the muscular irritability to be retained, so that, as stated in the article on reanimation, animals under its influence would remain alive for several days, cold-bloods until the web of the foot showed actual signs of decomposition.

I stated at the meeting in Birmingham, in 1865, that medicines ought to be studied from their constitutional side, and that we might, by a complete knowledge of the action of the elements of which the remedies are composed, see what a combination of elements would perform in a medicinal way. I also taught what might be learned from nature herself as conclusions derived from the action of remedial substances, and suggested three lessons.

I showed, first, from the effects of amylene, iodide of amyl, acetate of amyl, and others of the series, that they might be understood according to the phenomena they seemed to induce, and in a case of hopeless tetanus, as an example, I reasoned that there ought to be no hesitation in administering nitrite of amyl until decided reduction of muscular rigidity was indicated.

The second lesson deducible from the researches brought forward was that in the animal chemistry itself substances might, by a perverted nutrition, produce symptoms that actually resembled disease. Nitrite of amyl, for instance, induced a condition of system allied closely to the disease known as catalepsy. Amylene, again, produced phenomena like those of somnambulism, and caused some forms of paralysis of voluntary muscular power showing peculiar dyspeptic derangements, as if the organism itself made from its own organic material something that in character was allied to an amyl or

starch compound. I tested this point by inhaling ten grains of amylene diffused through one hundred cubic inches of air, and produced in my own body, in the most distinct manner, phenomena allied to somnambulism. Soon after inhaling I forgot myself altogether, but four minutes later was quite conscious again, waking as if with a start. I thought the experiment would not answer; but, glancing at my own wrist, I found I was wrong in the suspicion I had formed, for there were deep marks of pinches at several points, and the bottle containing the amylene vapour had been moved by myself and the stopper re-inserted, so that I had been performing acts preconceived and carefully carried out without remembering any single fact connected with the process: in short, a kind of somnambulism had been induced.

A third and last lesson had relation to the modification of action exhibited by charging the body first with the same chemical base, afterwards with divers compounds of it. I took amyl as the base, and to it added new elements, trying the different effects of these additions. The order of variation was most interesting. A simple hydro-carbon like hydruret of amyl acted as a negative body, not differing from nitrogen in this respect, but destroying motor force and partly consciousness, and nothing more. I introduced the element oxygen to the amyl, and there was added to the above-named phenomena violent and persistent tremor.

I added iodine instead of oxygen, and the phenomena indicated the free elimination of fluid from the body, with vascularity of the extreme parts and increase of the action of the heart and respiration. I changed the combination once more, to bring nitrogen and oxygen into operation combined with the base, and found that the vascular action was raised beyond what is seen from any other substance, followed by a prostration so profound that the still living animal might for a time pass as dead.

It seemed to me, therefore, that in these experimental truths, so simple and yet so striking, we have presented to us a line of experimental inquiry running parallel with that so prominent amongst our learned and more exact brethren of chemistry, and called usually the "law of substitution." "Is there not," I asked, "a physiological law to be worked out similar in character, and might we not, by following it, become sure and determinate in our knowledge by a new and sound application of remedies?" What if, after having learned the exact action on the economy of an organic base, we took compounds moulded on such a base and learned their true physiological values? Surely if we did this, long though the labour might be, we could in time lay down

"this osier cage of ours
With baleful weeds and precious juiced flow'rs,"

and, without forgetting the other words of the wise friar,

"O! mickle is the powerful grace that lies
In herbs, plants, stones, and their true qualities,"

might begin to approach that accuracy of knowledge, the absence of which makes the learned so weak and the charlatan so presumptuous.

Arriving physiologically at a correct knowledge of organic chemical compounds, a plan that would aim at establishing a principle in medicine would soon be accomplished, and the difference in regard to the action of elements themselves be determined upon. In respect to amyl as the base, there can be no doubt that differences of compounds were shown to exist; but elementary distinctions were scarcely less obvious. For example, in regard to mercaptan the difference from alcohol was that, in it, sulphur replaced oxygen, and we were simply introducing the work of sulphur as compared with that of oxygen. Perhaps we were doing the same thing in every case where sulphur was the element representative of oxygen. The same rule might apply to iodine when it took the place of oxygen, for I showed at Dundee in 1867, that when iodine was combined with methyl, making iodide of methyl, an agent was produced which, by careful inhalation, could bring about anæsthesia, but was not so good as when methyl was combined with chlorine, though extraordinary in some other respects, and notable in its sedative effect, about which I have a word to add.

I commenced to learn the use of the iodide of methyl by taking it myself in diluted alcohol. I found that a grain could be taken with perfect safety. I then prescribed it in an inveterate case of specific ulceration, in which iodide of potassium had failed, and, carrying the dose up to three grains, found the most rapid curative result. Further, the great pain and irritability of ulcerated surfaces were singularly relieved by it. Repeating this observation with further success, I solicited permission of Mr. Thomas Nunn, then surgeon to the Middlesex Hospital, to treat some hopeless cases of cancerous ulcers in the cancer wards of that Institution. Four cases were assigned to me, and the suggested plan was carried out by Mr. Nunn himself. His report of the results, after four months' trial, was of the most encouraging character. One case of ulceration was reported as healed, so that the patient left the hospital; another, in which there was intense hyperæsthesia—extreme sensibility of the skin—a symptom which had resisted all previous means, was directly relieved, and the patient greatly improved; in a third example, pain of an extreme kind was relieved, and in the fourth the symptoms were kept in abeyance. Mr. Nunn stated that his observations showed that iodide of methyl could be safely administered for long periods of time; that it removed pain—particularly the form of pain called hyperæsthesia, and that cancerous ulceration might heal under its use.

Amylaceous Origins of Disease and Sleep.

In addition to the lessons given above, I added a fourth, easily demonstrated, but not publicly related anywhere. It was my intention to explain it; but such is the flight of time in the life of a busy man that I put it aside, and allowed it to become forgotten for a period of not less than thirty years. What occurred to me was, that taking amyl as a base, and getting compounds having visible varying effects, the base being itself derived from starch, we might easily account for a whole cluster of diseases, or, rather, causes of diseases, from no harder a task than that of learning what compounds could be made from starch by chemical manipulation. But it was as easy to make starch compounds in the body as out of it, and, made in the body and diffused through it, they could not fail to provoke symptoms of disease just as they did when, after being made outside the body, they were swallowed or inhaled. Amylene induces a somnambulistic state; amyl nitrite a kind of catalepsy; but either, derived from a common constituent of food, might with equal readiness be made out of the body, or by a modified vital chemistry within it, without any conception of our own. I followed up my idea by determining what the result would be of administering for a long time minute doses of amyl nitrite, and by that method synthesised so remarkable a form of phthisis pulmonalis that the symptoms, as well as the

pathology, were the most striking features, extending even to the occurrence of hæmorrhage in the pulmonary organs. It seemed also to me that the common and nightly phenomenon of sleep might be due to the formation within the body, during waking and working hours, of a sleep-producing or, more correctly speaking, anæsthetic compound diffused by the blood.

Colloids and Dialyses.

It would take a volume of itself to record all the practical researches made and the conclusions arrived at. I believe they are mainly published in the Reports of the British Association and in similar works; but I cannot fail to recall two other researches before I conclude.

I became deeply interested in the subject of colloids owing to the teachings of the late Dr. Thomas Graham, the Master of the Mint, whom I knew, and whose able assistant—the late Dr. Frederick Versman—became an assistant of my own when Graham ceased to require his services. The division of organic substances into crystalloids and colloids, which Graham pointed out as naturally existing, was a discovery of singular comprehensiveness, and whenever I visited him in Gordon Square, whither he had retired, his conversation was of the most useful and edifying character. He was an old Professor of Anderson's University, where I had, as before mentioned, been educated, and he could recall

the famous David Livingstone; but the richest of his expositions had reference to the diffusion of gases and to the colloid and crystalloid qualities of matter. Both states of matter claimed my attention, the colloid especially, and the word "pectus," which Graham applied to the term "coagulation" of colloids, was employed by me, subjecting me to the foolish criticism of some commonplace writer who did not understand what the word meant, and whom we never condescended to notice.

It was natural enough for me to repeat the experimental researches which Professor Graham had made on the subject of the colloids and crystalloids, and on the phenomenon he called "dialysis," when bodies of an organic nature are simply separated from each other by a thin membrane. He seemed to me to have alighted on a profound organic law when he showed that osmosis—not endosmosis or exosmosis—must be the term employed in regard to the matter; and in the old *Literary Gazette*, in which I had charge of the scientific weekly article, and of which, I think, at the time the eminent John Morley was editor, I wrote two or three columns on osmosis that Graham highly commended, and from which he explained to me the vital nature of the process according to his views. These views have ever since remained in my mind, and have led me to take up the study of dialysis in various ways, especially in reference to exudation of fluids in

which a crystallisable salt forms a constituent part. I laboured at the work of dialysis in the nervous material, and strove to find if the membranous envelope surrounding the nervous pulp or fluid admitted water through the membranes or absorbed through them, and whether there was a coagulation of nervous substance, as there was of muscle and blood. I gave a lecture on these points before the Hunterian Society at the London Institution, which lecture was afterwards published in the *Medical Times and Gazette*, and another one at the Royal Infirmary at Hull, showing that water could be removed from a colloid like albumen, and that albumen could be left in the solid form and be also transparent: in fact, I artificially constructed transparent lenses so like the crystalline lens that the artificial resembled the natural, and possessed magnifying power.

But the most practical result of the experimentation consisted in the formation of remedial substances in the form of colloids. Fluid colloids, like collodion, were tested in order to see what substances they would absorb so as to produce remedial solutions, and the bodies, afterwards called "colloids," were introduced by me and brought into general use. Styptic colloid came first, and proved useful, not only in stopping hæmorrhages, but in covering and healing wounds, and in covering inflamed surfaces like those of erysipelas. Then followed iodised colloid, which acted like styptic

colloid, with the addition that it afforded at the same time an antiseptic, and provided a means by which iodine could be slowly introduced into the system, or into parts of it. I thus obtained what we found to be truly useful remedies in a practical form, always ready to hand when called for; and I have had the pleasure of living to see few remedies in greater demand. I wish I had done more in regard to the study of dialysis, but I could not have done better, I hope, than introduce the colloids as remedies.

The Ethylates.

The other remedial research to which reference has been made relates to the substances known and used as the ethylates, or, as they are sometimes called, caustic alcohols. I reported on these at the meeting of the British Association at Liverpool in 1870, and the Report will be found in the *Transactions*. As it was a curious research, it may appear in these pages. I had found that some bodies could be decomposed by water, and, while they were decomposing, active remedial qualities could be evinced which would prove of service. This was the case with the ethylates. Sodium or potassium was dissolved in pure ethylic alcohol. When one of them, say sodium, was dissolved in pure alcohol brisk action took place, owing to the circumstance that the alcohol was, for the moment, decomposed, an equivalent of its hydrogen being set free and

becoming replaced by sodium. In this way an ethylate was formed which, with care, could be turned into a beautiful crystalline salt or could be kept in solution by the addition to it of more alcohol. In the form of ethylate the salt could be kept safely so long as water was excluded from it; but when it was brought into contact with water a reaction took place. The water became decomposed; its two elements, oxygen and hydrogen, were divided; its oxygen went over to the sodium, forming caustic soda; and its hydrogen replaced that which it had lost previously, re-forming alcohol, and making ethylate of sodium or a caustic alcohol. It struck me that the water in the tissues, or in blood or albumen, would act in the same manner as ordinary water, and so I treated blood and albumen with an ethylate to discover if this were the fact. The water again was broken up; caustic soda was formed, which acted on the organic substance of the blood or albumen, transforming it into a solid substance concreted or held together, or, we may say, coagulated, by the newly formed alcohol. This was quite a singular discovery, and I soon made it applicable in practice. I found a patient suffering from a vascular tumour who had been surgically, but unsuccessfully, attended by the late excellent surgeon, Mr. John Gay, and I treated the tumour by covering it with a solution of the ethylate. The phenomena were the same as anticipated. The water of the tumour was decomposed; caustic soda was produced which disintegrated

the mass; alcohol was formed which coagulated the organic parts, transforming them into a scale, and the whole was removed by the formation of a dead scab or scale-like surface. The tumour on which this operation was successfully carried out was classified as a vascular nævus, and since then many have been removed by the ethylate. I myself have removed a hundred, and other external uses of the remedial substance have taken place. Much more also is promised by employing the more active potassium ethylate, which destroys with astonishing rapidity, and with which I have removed much larger and more solid tumours than nævi. In many instances, indeed, potassium ethylate might be applied as a successful rival of the knife itself, and, mixed with some favourable local anæsthetic body, like cocaine, might be quite painless in operation.

Mechanical Contrivances.

Hitherto I have referred only to such remedial means as have appeared to me in the medicinal form; but others have come to light of a mechanical, rather than a chemical, nature. The oxygen-holder, or table, by means of which oxygen becomes the bearer of other remedies, is of this nature. The movable electric apparatus which wheels from bed to bed is a useful mechanical arrangement. The serrated scissors, which divide tissues without causing them to bleed, are a mechanical contrivance taken from the observation of the way in which, with their teeth,

female bovine animals divide the fœtal cord. The sphygmophone, which renders the pulsations of the pulse audible, and the system of rubber tubes which conveys a hot or cold current of water around the neck, partake of the mechanical character; while the proposal, which met, some years ago, with criticism and incredulity, that an ovarian cyst might be treated without a formidable operation by simply compressing the pedicle through which it is fed, stands in the same category, and will one day be adopted as a mechanical operation worthy of reconsideration.

Embalming the Dead.

Before I conclude this chapter it is well I should devote a few lines to the practice I have carried out in the ancient art of embalming the dead. A great many years ago a French gentleman by the name of Falcony brought over a powder to which his name was attached—Falcony's Powder—and indicated that by means of it he could preserve the dead body. The powder consisted of sulphate of zinc and sawdust, and the dead body was buried in it in an open chest. The water of the body exuded into the sulphate of zinc and escaped by the exposed surface, so that the dead body was really dried down or mummified, and remained in a state of preservation. Mr. Falcony asked me, as the then Dean of the Grosvenor Place School of Medicine, to lend him the dissecting-room for his demonstrations,

which, with the consent of my colleagues, I did. The demonstrations were exceedingly satisfactory in result, and they led me to make the whole question of preservation of the dead a careful study. On this the art of embalming came before me for investigation, and I made so much progress in it that I carried out embalming in no less than forty-eight instances, and introduced so many improvements in regard to embalming fluids that at last I could inject them by needle injection with entire success without performing any post-mortem operation at all. I did not like to continue the art further, for two or three reasons. To begin with, it meant a disagreeable undertaking. In the next place, it seemed to me an idle and absurdly sentimental practice to preserve a body that had served its purpose and could in an hour or so be destroyed or removed in the crematory by the influence of fire. Lastly, in embalming, incidents arose which were not consonant with my nature, and affected me so much I determined to give up the practice; it is many years now since I carried out a task I never wish to resume.