of the tissues, as well as the dissemination of cancer cells which conceivably take place. The facts that appear to me to be somewhat against this being the whole explanation are that these facts are now very well known to surgeons operating for cancer, and a surgeon will not, if he can help it, cut into the cancer tumour whilst operating, giving it a good wide margin; yet even then cases are seen of stitch recurrence, and it is difficult to imagine that wandering cancer cells are so numerous in the outlying tissues as readily to infect the wound like that; if they are present in such numbers, then it is very difficult to understand why operations are as successful as they are.

There may be an additional factor in the case, and may that not be the traumatism and irritation caused by the stitches? In many cases the effect of traumatism may be quite clearly traced in the origin of cancer, particularly about the breast. The evidence is indisputable. In the analysis of a number of cases published by Cecil Leaf it is mentioned that traumatism accounted for at least 35 per cent., and that the percentage was probably higher than that. This does not prove that traumatism *per se* is sufficient to cause cancer, but in a patient disposed to develop it it may be the exciting cause; and in a patient already suffering from cancer surely trauma might be expected to develop it. It would be interesting to learn whether cancer has been known to develop in patients suffering from it who have sustained an injury, particularly a wound, in some remote part of the body which has subsequently developed cancer. The extreme rarity of autoinfection in cancer of the lip and in the mouth—I have never seen a lower lip infect the upper—is remarkable, and, even when it does occur, might be accounted for in another way. I have seen cases in which cancer has developed around a suture left in for some time after an operation. Two well-marked cases I remember where a suture was overlooked, which showed recurrence nowhere else; and another case of excision of the tongue by the method of division of the lower jaw, where the silver suture left to unite the symphysis was the centre of the most active recurrence. On the theory of traumatism and subsequent irritation of the wound by a foreign body this is just what would be expected.

Why cancer should develop about the stitch holes, and not on the cut edges and other parts of the wound, when all have been equally liable to infection, is only understandable on the explanation that a clean-cut wound does not cause so much irritation and injury as a stitch which is dragged through the tissues, and left to irritate the susceptible parts for many days. It may be that in certain persons, especially when already the subject of cancer, certain cells otherwise normal may, if bruised or irritated, be capable of taking on malignant action.

Mr. Charles Ryall does not even mention the most valuable means we have at present for preventing recurrence after an operation, namely, the judicious use of the x rays. Possibly he has not had experience of this, for if he had I think he must have mentioned it. I have found that the x rays, especially if used at the time of an operation where practical, are of the very greatest value for preventing recurrence in the site of the wound, and even when it does occur, especially in stitch scars, it clears up in a beautiful and convincing manner by this treatment. I advocate, however, the use of the rays from the very first in all cases, and then all these things that Mr. Charles Ryall describes do not occur. When it can be done it is an immense advantage to give the fullest possible dose of x rays at the time of an operation to the open wound, because it is a chance for the most effective use of the rays that never occurs again, as x rays are more effective on a surface than at a depth, and possibly may act by destroying loose cancer cells scattered about the wound if such exist. I have had a few cases dating back as far as five years in which this was done in which there has never been any signs of recurrence, the cases are possibly too few and the time too short to convince the sceptical, but should be very suggestive to those surgeons who are anxious to do all that is possible to save the great risk of recurrence that is always present in these cases. I will venture to quote one solitary case, of no great value perhaps to those who require evidence by hundreds.

About two years ago a woman, aged 32, had an operation for cancer on her breast. It was only a partial operation in an early cancer. It was doubtless an inefficient operation, and should not have been done; it recurred at once, and was operated on most thoroughly by another surgeon six weeks afterwards, who removed the entire breast and cleared out all the glands; still, it recurred almost immediately, and he operated again, taking away the greater part of the pectoral muscles, and it was passed on to me at once for x-ray treatment. This was about two years ago, and up to the present there has never been the slightest sign of recurrence.

The unbelieving surgeon will regard this only as an example of thorough and successful surgery, and will hold that the x rays had nothing to do with it; but I have had very few failures, and quite enough similar successful cases to convince any unprejudiced observer, and think it is a pity that the great value of the x rays for preventing recurrence is being so slowly recognized. I think the reason is that the successful use of the rays is not so easily attained as is imagined; it is not so simple and easy as the administration of doses of salts or quinine by any one who happens to have an x-ray outfit at his disposal; and it is surprising what inefficient individuals this work is sometimes left to. Sometimes the hospital hall porter, or the handy man about the place, or a nurse; but, after all, I think the most inefficient x-ray work is done by the surgeon himself who happens to possess an outfit. No man can serve two masters, and a first class surgeon is very seldom a practical scientist, and, unfortunately, often despises what he does not understand. I hope I shall not be misunderstood as implying that surgeons do not conscientiously do everything possible for their patients, for I know many that do advise the use of x rays after operations: but, on the other hand, there are many who never even think of suggesting it. I was speaking about it to one who has operated on a good many cases of breast cancer, and he assured me that he had no recurrence in fully 80 per cent. of his operations; to such a one as that the x rays could offer no advantage.—I am, etc.,

Melbourne, Nov. 23rd, 1908. T. G. BECKETT.

WORK IN COMPRESSED AIR.

SIR,—In his address on the Physiology and Pathology of Work in Compressed Air, in the BRITISH MEDICAL JOURNAL of January 30th, Sir Thomas Oliver describes a series of experiments carried out by Dr. Parkin and himself thus:

> We subjected frogs to extremely high pressure, and after a time suddenly decompressed some of them. The web of a frog's foot was drawn over the inside of the glass window of the compressed-air chamber, and by means of the microscope and electric light illumination we could watch the circulation of the blood in the frog's capillaries. During compression the circulation proceeded quite naturally, but after sudden decompression, while the circulation seemed to go on apparently unchanged, by degrees the rate of the blood flow diminished, and gradually ceased, preceded by a slight to-and-fro movement. All at once a bubble or two of air would appear in the capillaries, and these running together formed a large embolus inside the vessel, etc.

Sir Thomas Oliver does not give a word of acknowledgement that he owes this experiment to me. It is an experimental demonstration of which any man may be justly proud, especially when it is completed by recompression bringing about re-solution of the bubbles, and, as I observed once, recovery of the circulation. It is the most convincing demonstration of the fact that bubbles are the cause of caisson disease, a fact which Sir Thomas Oliver was very unwilling to acknowledge in a conversation with me some years ago at the London Hospital Medical College. This is the second time Sir Thomas Oliver has described this experiment as if it was his own. The first time was when he read a paper at the Society of Arts, a paper for which, I believe, he was awarded a gold medal. The second time he goes a step further and describes experiments which demonstrate that relatively high percentages of CO_2 have no effect in increasing the risks of compressed air, without any acknowledgement of the work of Dr. Greenwood and myself, work which proved the statements made by him on this very matter at the Society of Arts to be erroneous.

In the matter of the frog-web experiment, Dr. Parkin was introduced to me by a distinguished colleague, and I was asked to give him help and advice as to the performance of experiments on compressed air. Dr. Parkin, under my direction, had a chamber made like mine, and repeated my experiment, and acknowledged his indebtedness to me in the thesis he wrote on this subject.

I have yet to learn of any single new experimental fact which has been contributed to the pathology of caisson disease by Sir Thomas Oliver. How little of the experimental work he has done is shown by the fact that he describes the experiments as done on pithed frogs. He mentions with approval the method of stage decompression, which recently has been introduced as the routine practice in the Admiralty, a method founded on certain theoretical assumptions of Dr. John Haldane, and experiments on goats carried out to prove these assumptions by Dr. Haldane, Dr. Boycott, and Mr. Damant. I take this opportunity of saying that Mr. Greenwood and I have repeated on pigs some of their fundamental experiments on goats, and with contrary results. Stage decompression has proved in our experiments less safe than uniform. We do not admit the justice of the theoretical postulates on which it is based. There are many factors still unknown in the causation of decompression bubbles, and one of these we have some evidence to show is the presence of food in the alimentary canal. The escape of the men in the burst caisson mentioned by Sir Thomas Oliver may have been due to this in part, and in greater part to absence of fatigue, as the caisson burst about the breakfast hour. The increased number of cases which occur with long and repeated shifts we believe is due not to fuller saturation of the body with nitrogen, as Drs. Haldane and Boycott suppose, but to fatigue of the heart brought about by prolonged work. The circulation is then too feeble to allow the escape of nitrogen during decompression. The men sit quiet and weary in the lock. They ought during decompression to breathe oxygen and work hard.—I am, etc.,

Loughton, Essex, Jan. 30th. LEONARD HILL.

THE HOME TREATMENT OF SCARLET FEVER.

SIR,—The line of prophylaxis indicated by the pathology of scarlet fever is for clinical purposes the same as in a series of fatal diseases such as diphtheria, measles, and whooping-cough, and of less serious affections such as influenza, mumps, and others. It is admitted that in all of them the upper respiratory tract is the head quarters of infection; yet are we ever taught that the entire stress of our efforts at disinfection should be concentrated upon that region? I have consulted our best and most recent textbooks, but in most cases I have found no reference under the headings of prophylaxis and treatment to this all-important matter. Doubtless the indication is so obvious that it might be held to go without saying. But the practical result of that silence is that it commonly *goes without doing*, as is too well shown by the relapses of influenza and whooping-cough, and by the late contact cases of diphtheria and of scarlet fever.

External ablutions are indispensable, and fortunately they are practised. But Dr. Robert Milne is entitled to credit for the attention which he pays to a more dangerous surface; only, if I may say so, his method is too limited locally in its application. Whether we are dealing with scarlet fever, or with any of the other diseases I have mentioned, we should bear in mind that cleanliness is the simplest and the most efficient of our antiseptics, and that our patient's disease is unwashed until the *entire* upper mucous tract is systematically and frequently cleansed. Opinions may differ as to the merits of the simple and painless method which I have long advocated and practised in all these conditions, namely, frequent instillations of jasmin oil through the nostrils, to spread above and behind to regions inaccessible to sprays, douches, and gargles, to be continued from the onset through the entire duration of the period of observation. If this is not good enough let us find some better method. But let us not allege the fact that we cannot destroy the microbes *in situ* as an excuse for continuing to neglect adequate efforts to sweep them clean away.—I am, etc.,

London, W., Jan. 31st. WILLIAM EWART.

THE REMEDIAL USE OF ALCOHOL.

SIR,—The address delivered before the Border Counties Branch by Dr. Macdonald bristles with controversial points. I will take only one—the treatment of lobar pneumonia. As Dr. Macdonald truly observes, an ounce of practice is worth a pound of theory.

In my experience of fifteen years' practice in a purely working class district on a clay soil, I have seen from forty to fifty cases of lobar pneumonia, many of the patients being alcoholic subjects, and have yet to write my first death certificate for that disease.

I rely on digitalis, strychnine, careful feeding, and *absolute rest*, but always refuse at that critical period, when the overburdened and dilated right heart has almost reached breaking point, to help my patient over the precipice by prescribing the so-called stimulant that must often, by its paralysing effect on the cardiac nerves, take away his last chance of recovery.

In a recent severe case of double pneumonia in a lad of 18 the crisis was delayed, and my courage almost failed, especially as the relatives begged piteously to be allowed to give him brandy "before he died." I assured them that, desperate as the case appeared, if they kept to the non-alcoholic treatment the patient would recover. And he did. "Lobar pneumonia, cardiac failure"—so runs the usual certificate, and the cause of the cardiac failure in 99 cases out of 100 is—alcohol.

I send this note just to encourage young abstaining practitioners who are apt to be led astray by dogmatic statements as to the virtues of the old-fashioned alcoholic treatment of lobar pneumonia.—I am, etc.,

Stoke-on-Trent, Feb. 1st. A. A. HILL.

ALPINE OR HOME CLIMATES FOR EARLY TUBERCULOSIS.

SIR,—I have read with much interest the address delivered at the Medical Graduates' College by Dr. William Ewart on the above subject, and from a long experience of Alpine climate I am enabled to confirm all that Dr. Ewart expresses regarding the value of mountain air as a remedial agent in the treatment of early phthisical cases. The importance of the question is immense, but I can only briefly refer to the extraordinary change of opinion which has taken place in some quarters as to the virtue of climate in the open-air treatment.

For reasons which I will mention in a moment the Swiss Alps have not proved as attractive to frail people of late years as formerly. The family with a son or daughter who looked rather delicate but otherwise attracted no particular attention is not now so often seen. The young person who cannot walk uphill without a strained breathlessness, or other inoffensive individuals with a slight cough, are scarcely met with. It must be remembered that these "invalids" bring with them many relatives and friends in their train. We might therefore suppose that they would be welcomed by the Swiss hotel proprietor, and that he would do as much for the comfort and convenience of delicate clients as in days past. Instead, you see announced in hotel advertisements and prospectuses the intimation that "consumptive cases are not received," "persons with tuberculosis not admitted," and at the same time stress is laid on the "air cure" being especially potent in neurasthenia, debility, convalescence, and overwork!

I think it is almost a popular creed that mountain climate is *par excellence* more suited to lung troubles than to most other conditions, but one of the reasons for all this confusion is the "scare of infection." The belief that tuberculosis is "caught" by coming in contact with a person suffering from the complaint has taken root in places. Some persons have been known to write to an hotel to be assured that no consumptives were admitted before they settled on taking rooms. One thing and another has led the hotel proprietor to intimate publicly that his house is barred to such cases, and the "cases" have taken him at his word and remain at home, or perhaps visit the South Coast resorts. Others have the well-equipped English sanatoriums open to them. It may be asked why, if in search of health, they do not enter the sanatoriums of Switzerland and obtain with disciplinary treatment the grand advantage of Alpine air and sunshine. A few do so, but English people will never be induced to patronize largely the foreign sanatorium. This remark, of course, does not apply to the Alexandra Sanatorium at Davos, as that establishment will be English in the true sense.

It has become the fashion within the last few years for a winter holiday to be taken in Switzerland by thousands of English, and although they arrive late—say about