and outside, a remarkable unanimity in accepting the Chancellor's view.

Throughout months of unusual disquiet in the political and social sphere, there has been a fine response to this call for national activity against tuberculosis. A million and a half sterling have been specially voted for the establishment throughout the kingdom of such institutions as may be considered advisable for the whole population. Under the Insurance Act about one million sterling will be available annually for their maintenance, and means are foreshadowed whereby further funds will be forthcoming.

Not content with finding the money, the Chancellor of the Exchequer has proceeded to the practical determination of the form which antituberculosis measures shall take by the appointment of a Departmental Committee. The deliberations of the Committee are in progress, and a report may be expected at an early date upon the considerations of general policy in respect of the problem of tuberculosis, in its preventive, curative, and other aspects, which should guide the Government and local bodies in making or aiding provision for the treatment of tuberculosis.

The movement against tuberculosis has been baptized a crusade. Is the name quite descriptive of the need and purpose? Is it not rather a great reformation—the determined, peaceful emancipation of mankind from misconceptions and prejudices which civilization has entailed, and of which tubercularization for the most part is the result?

Each nation has its part to play in the revolution against a régime which means death and disability on so appalling a scale for its citizens. Each nation must be guided by its own traditions and methods. But the leading principle is the same for all—namely, the restitution of certain rights to the people by the removal of noxious and devitalizing influences. Our common purpose is the detubercularization of the individual and the community, with a view to the ultimate detubercularization of the nation and the world.

In "meditating this deep disease of life, what its far source and whence its remedy," we are faced continually by evidence that tubercularization is less the effect of the malign influence of a hidden foe than the outcome of blindness and unwisdom on our own part. The detubercularization of the world will be synchronous with the dawn of "that great to-morrow when mankind shall mend."

That is the renaissance towards which, to-day, Rome points the world. *Tempus est ut praetermittantur simulacra nostra. Incipienda est vita nova!*

THE END RESULTS OF OPERATION FOR CANCER OF THE TONGUE.

BY

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THE material for this study is based upon the case histories of 38 patients operated on for cancer of the tongue at the Bristol Royal Infirmary during the years from 1902 to June, 1911. It has been possible to obtain subsequent information in 29 cases.

Of the 29 followed through, the nature of the operation performed and the result obtained may be seen in the following table:

	Cases.	Died of Operation.	Died of Weakness or Re- currence.	Re- curred.	Apparently Cured.
Partial removal of tongue	10	1	6	2	1
Total removal of tongue	2	0	2	0	0
Partial removal of tongue followed by removal of glands	17	1	5	5	6(?)
	29	2	13	7	7(?)

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The glands were removed two or three weeks after the excision of the growth in the tongue.

In 5 cases it was necessary to divide the jaw. Four of these recurred, three of them locally; another patient died soon after the operation.

The site of recurrence is known in 14 instances:

Recurrence in the mouth	...	...	3 cases
Recurrence in glands	...	...	7 "
Recurrence in mouth and glands	...	...	3 "
Recurrence in spine	...	...	1 case

There is a further case, presently to be mentioned, in which the patient died of cancer of the oesophagus two and a half years later; it is uncertain whether this is to be regarded as a recurrence or not.

In several cases the patient was apparently cured for some considerable time, and then the disease recurred, in one instance in the neck, in another in the mouth. These two men were free for nearly three years after the original operation.

Two patients died of the operation, one of septic absorption and the other of gangrene of both lungs following on a profuse secondary haemorrhage due to the foul state of the mouth.

The figures bring out the superiority of the operation where glands are removed as compared with the older procedure of leaving them if they were not noticeably enlarged. Thus, only 1 patient was cured out of 12 where the glands were left, but perhaps as many as 6 out of 17 where the glands were excised. Moreover, even of the remaining 11 cases in this group there is evidence that the patient was given more chance of relief. Two of them were well and free from recurrence for nearly three years, and another died of cancer of the spine ten months later. In several instances careful sectioning of the glands excised showed no cancer cells. In 2 such cases the growth nevertheless recurred in the neck (in the submental region and behind the sterno-mastoid respectively).

The cancerous growth was preceded in 3 cases by a dental ulcer. In one of these the ulcer was excised and reported by a pathologist to be innocent, but a malignant gland appeared eighteen months later, and the patient died. In another case a papilloma had been removed two years before. One man had extensive leukoplakia and syphilitic fissures, which he used to rub with silver nitrate. Many of the patients had leukoplakia.

A few remarks may be made concerning the patients who might possibly be claimed as cures.

1. Glands Not Removed.

E. G., a woman, operated on in 1904 and again for a small local recurrence a few months later, remained free until her death in 1911, which was in no way connected with the cancer.

2. Glands Removed.

A. R. has been well now for eight years.

E. T. has been well for five and a half years. There is a tiny nodule in the neck which may possibly prove to be a recurrence. It is being watched.

R. H. has been well for five and a half years.

W. R. is a doubtful case, inasmuch as after two and a half years he was readmitted with epithelioma of the oesophagus 3 inches above the cardia, and died after gastrectomy. At the autopsy there was no other sign of recurrence—if this is to be called a recurrence. (The writer recollects finding an epithelioma of the oesophagus in a man who died after operation for an epithelioma of the lip.)

W. H. has been free from recurrence for two and a half years. He is a schoolmaster, and talks quite plainly.

G. W. has been free from recurrence for nearly two years.

In all the above cases the diagnosis was confirmed by microscopical examination, with the exception of R. H., in which the growth was regarded as so typical that it was not necessary to cut sections. In E. T. the pathologist's opinion was only formed after examining many specimens.

With reference to the size of the growth, in G. W. it was as large as a florin; in the other cases it was not more than $\frac{1}{2}$ inch across. In 2 of these 7 cases there was a gland enlarged in the neck, but it proved to be free from cancer cells.

SUMMARY.

1. The immediate mortality in 38 operations was 5.3 per cent.

2. Of 29 cases 7 were apparently cured (well after two years); this gives a percentage of 24.1

3. The results are much better when glands are removed, recurrence being greatly delayed or altogether prevented in many cases.

4. Partial removals of the tongue were followed by *local* recurrence in 6 out of 29 cases.

REMARKS ON THE ESTIMATION OF THE CALCIUM METABOLISM.

WITH A DESCRIPTION OF A CLINICAL METHOD OF ESTIMATING THE AMOUNT OF CALCIUM IN THE URINE AND OTHER PHYSIOLOGICAL FLUIDS.*

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To gauge the condition of the general metabolism of the calcium salts, which are now admitted by all to be of so much importance both in health and disease, an accurate knowledge of the amount at the disposal of the tissues is necessary, and this knowledge has been difficult to obtain.

For instance, some workers have estimated—chiefly experimentally—the amount of calcium taken in with the food, and have compared this with the amount recovered from the faeces.

I have always held that this must be absolutely unreliable in the result. For, although we know the amount taken in by the mouth, we are unable to find how much of this quantity is absorbed, and how much passes through the alimentary canal without being absorbed.

An example will make this quite clear. Suppose that 10 grains are taken in by the mouth every twenty-four hours, and that 5 grains are recovered from the faeces, those 5 grains may represent 1 grain passed through and 4 grains excreted, or it may mean that 2 grains are passed through and 3 grains excreted, and so on. That is to say, a large proportion of what is being absorbed may be excreted, or only a small proportion—there is nothing to indicate which.

Recognizing this difficulty, in 1907 I published a method of estimating the quantity of ionizable calcium in the blood.¹ By this method one is able to determine at any given time the relative amount of ionizable calcium in the blood, and so to get a good indication of the amount at the disposal of the tissues. But, as I then and have since emphasized, this method requires careful interpretation. For instance, a low calcium content in the blood might mean, either that very little was being absorbed or that too much was being excreted. I devised the method for a specific experimental purpose in regard to menstruation, and it served that purpose; but when it is used for routine clinical work, unless careful consideration be given to the findings, wrong deductions—as I have just pointed out—may be drawn from them.

If, however, the blood estimation method be used in conjunction with a method which will indicate the amount excreted a perfectly reliable knowledge of the condition of the general metabolism in regard to the calcium salts may be obtained.

Since the excretions from the intestinal canal are, as I have just shown, useless for the purpose, we must fall back on the urine, in which it is safe to say that, provided the urinary apparatus be healthy, the calcium found should represent a true index of the excretory ratio. If the blood index be low and the amount excreted by the urine also be small, it is fair to assume that not enough calcium is being absorbed. If, on the other hand, the blood index be low and the urinary excretion high, then probably too much is being excreted; and conversely a high blood index and low excretory ratio might indicate that an excessive amount was being retained in the tissues.

I must here point out that, as with the blood estimation when considered alone, so also with the urinary excretion: no true interpretation can be formed of the calcium metabolism by an examination of the urine alone. Because a large amount of calcium was found in the urine it would be ridiculous to state on this evidence alone that the patient was suffering from a calcium surfeit, and to cut off all supplies of the salt. It might be that she was excreting

too much, as is the case in exophthalmic goitre, and was depleting her tissues. In such a case, far from cutting off the supply it should be supplemented. Or, again, in some cases in which very little was being excreted it might mean that the calcium was being stored in the tissues, and in such circumstances it would be the worst thing possible to assist in the process by administering more. To be scientific and accurate, when we have no definite clinical evidence or empirical knowledge to guide us, we must use all the means at our disposal to find out what is really happening with regard to the calcium metabolism; and we must not rely on one method alone without we can be quite sure of the interpretation to be put upon the result. In the future the whole subject will, of course, become more empirical, but at present it is far from being so, and I could give many instances of the value of accurate estimations and interpretations. It is, of course, not difficult with the requisite laboratory facilities to estimate chemically the amount of calcium in urine, since large quantities of the latter can regularly be obtained; but such a method of estimation is hardly possible for those engaged in clinical work, so that I think there is room for a reliable clinical method.

The method of estimating the calcium in the urine which I shall now describe owes its origin to Sir James Barr, who for some time has been in the habit of making a rough calculation of the amount excreted by adding to the urine freshly passed the solutions I originally suggested for the estimation of the blood calcium content—that is to say, oxalic acid and acetic acid—and gauging roughly the quantity of the precipitate obtained. On learning from him of this practice I undertook to have the accuracy of his findings investigated chemically. This was done by my assistants working in the biochemical laboratory at the university, with the following results:

1. The sediment sent by Sir James Barr, which was collected from many specimens of urine, was found to consist of pure calcium oxalate. It may be noted here that practically all the calcium present in urine is in an inorganic form, consequently it is precipitated. When the proper measures, to be described directly, were adopted, no phosphates were found in the many precipitates examined.

2. Of the urines submitted by Sir James Barr for chemical investigation of the accuracy of his conclusions the specimen supposed to contain the least amount of calcium was found on analysis to contain 0.008 per cent. of Ca; that supposed to contain a medium quantity was found to contain 0.007 per cent. of Ca; the specimen supposed to contain the largest amount of calcium gave on analysis 0.045 per cent. of Ca.

It is evident, therefore, that the method employed by Sir James Barr is accurate in so far as it is possible to recognize a very large deposit from a small one, and to recognize that a patient is excreting a good deal more than the normal quantity, or possibly, though this is much more doubtful, that he is excreting considerably less. The most obvious difficulty in regard to Sir James Barr's method arose in connexion with the estimation of medium and small quantities, which predominated in the specimens examined, for it was not possible to say whether these were above or below the average quantity. Nor would it be possible with the method employed by Sir James Barr to detect and recognize whether there were an improvement or the reverse during treatment, unless the changes happened to be very marked.

It was, of course, clear that one had to obtain some method of estimating accurately the amount of the precipitate, which we had proved to consist of pure calcium oxalate; and, as I have stated elsewhere, my method of blood estimation cannot be applied in its present form to urine. Consequently our experiments consisted in centrifuging and measuring the deposit in specially calibrated tubes.

Our first difficulty was to prevent the deposit sticking to the sides of the tube, and to get it to lie fairly evenly in the calibrated portion. After trying many things, we finally overcame what threatened seriously to interfere with the perfection of our method by adding alcohol or methylated spirit to the urine and reagent. Next, we found, as we anticipated, that with different rates of revolution of the centrifuge, and with different times spent in the operation, we obtained different results—that is to say, urines and solutions containing the same quantity of calcium gave deposits which stood at different levels according to the circumstances mentioned.

* Read before the Liverpool Medical Institution, March 14th, 1912.