

time to time, is weak in theory and bad in practice. This advice is just what the woman who shuns an operation wants. Where one will return, ten will never be seen again until some radical pathologic process has developed which will greatly diminish the patient's chance for recovery from an operation delayed without reason. I was myself in the habit of advising the patient with the so-called innocent fibromyoma to return from time to time. The death of two such patients from an overwhelming necrosis convinced me of the mistake.

With the mortality of ten years ago, the advocacy of the removal of all fibromyomata on diagnosis might have been questionable, but with a mortality of 6 per cent., or even less, in operations performed by competent men, a certain portion of which, it is reasonable to presume, occurred in cases where an earlier operation would have given different results, and with 12 to 14 per cent. of cases showing degenerations, threatening death if neglected, it seems that the radical view taken in this paper is the one which, if maintained, will be the means of relieving many a woman from a life of invalidism or from death.

DISCUSSION.

DR. H. J. BOLDT, New York: It is doubtful to my mind whether these sarcomas of the uterus are degenerated fibromas or have been sarcomas from the beginning. There is no proof at all to convince me that the tumors have not been of a malignant type from the beginning. In operating on cystic ovaries, conservative surgeons retain as much ovarian tissue as apparently is normal. I believe that every woman who has a myofibroma causing symptoms should be advised to have the tumor removed.

But I am absolutely opposed to removing a woman's uterus when there is nothing to indicate whether it is the seat of myoma or not. There are women who will absolutely decline to have an operation. I have watched some of these women for fifteen or twenty years and have a number under observation now. Such tumors sometimes retrograde and become smaller long years after they had first been diagnosed. I know from personal observation that they atrophy and shrink. I would not say, however, that it was a proper thing to have the patients advised against surgical intervention.

DR. HUGO O. PANTZER, Indianapolis: I do not wish to detract from the force of the argument made for the removal of uterine myomata, but some of the adverse evidence should be presented. The fatality and morbidity which follows such work by expert operators in well-appointed clinics, should be additionally stated, if not the untoward results following the work of less experienced surgeons. The incidents by infection, by anesthetics, by idiosyncrasy, by unexpected complications and sequelæ certainly justify and demand a discriminating and differential course in these cases.

DR. F. F. LAWRENCE, Columbus: It is strange that there should seem to be any necessity for discussing the advisability of removing any neoplasm from the body, except possibly in cases in which operation is absolutely impossible. The dangers of operation before complications occur are almost *nil* in hysterectomy. Noble and others have shown that the danger of malignant degeneration in fibroids is much beyond 5 per cent. The question is not simply one of malignancy. What are the dangers of suppuration of fibroids left alone? Another 5 or more than 5 per cent. Particularly is that true when these fibroids are made the subject of local treatment.

What about the resistance of the patient when she is allowed to go on with a fibroid. A patient carrying a fibroid for six or eight years is reduced in every way.

What can be gained by the expectant treatment? Some years ago we were told that we must not remove an ovarian cyst until it became as big as a wash-tub. Some

years ago we were taught that a fibroid should not be removed until after the menopause and patients had to go on to a necrotic condition of the growths with a mortality at the time of operation of 35 per cent. To-day the mortality in operation is less than 3 per cent. This difference is due to teaching such as Dr. Eastman's advocating the removal of every neoplasm, no matter where situated, as soon as it is discovered.

DR. J. H. CARSTENS, Detroit: There is something to be said about symptomless fibroids. How do you ever find the fibroids? The patient has some symptom before she consults a physician. There is no doubt that many women are going about with symptomless fibroids. When they finally come to the physician there are symptoms and the tumor should be removed.

There are some fibroids, however, that do not produce symptoms and the question is simply, what will you do? What do you call symptoms? Are there not other symptoms than pain? Does not the patient suffer in the consciousness that she has a fibroid that will cause her trouble? Is she symptomless when she goes around for a year or more dreading lest something will happen? Is it not a good thing to remove that fibroid when the danger is *nil*, not 6 per cent., when you often can do it by the vaginal method, or by simply enucleating the fibroid and leaving the uterus intact and the woman in good health?

I think that we ought to relieve a woman and remove the fibroid whether or not it is degenerated. There is mortality in the very cases that are called symptomless but are allowed to drag along year after year until they are almost *in articulo mortis*; and then the patients present themselves for operation and the mortality statistics are not good. Is it not better on general principles to remove the fibroid and relieve the woman, not necessarily to-day or to-morrow, but as soon as possible, in two or three months, when the woman is in the best condition?

DR. T. B. EASTMAN, Indianapolis: The *reductio ad absurdum* argument has been applied to this question. Some one asks why we do not remove the uterus for fear of carcinoma. I answer for the simple reason that the conditions are not pathologic. We advocate the removal of fibroid tumors because they are pathologic and that is sufficient reason. Now, there is one thing which those who are pleased to call themselves conservatives never have explained in any discussion on this subject, and that is why in the world they demand a special dispensation for fibromyomata. They take out a cystic ovary or the appendix on the slightest suspicion. If they can show any reason why they should have this special dispensation I have nothing to say.

METABOLISM IN TYPHOID FEVER.*

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I shall not attempt to review the many-sided subject of metabolism in typhoid fever, but shall devote the time to a brief discussion of one phase of the subject, which is of great importance, as well as of interest, both to the clinician and to the pathologic physiologist.

During the course of the disease a typhoid patient loses from ten to sixty pounds, or even more, of his body tissue. This loss is divided among water, subcutaneous fat and protein from the body fluids or cells. The loss of water we may leave out of consideration because we know neither its amount or significance. Since the observation of Leyden¹ in 1869 it has been believed that there is a retention rather than a loss of water from the body in fever. This belief is supported by the

* Read in the Joint Meeting of the Section on Practice of Medicine and the Section on Pathology and Physiology of the American Medical Association, at the Fifty-ninth Annual Session, at Chicago, June, 1908.

1. Arch. f. klin. Med., 1869, v, 366.

decreased quantity of urine and by the supposed decrease in the evaporation from the skin; but few or no accurate data on this question are available, and no positive statements concerning it can be made.

The burning of body fat may, in the absence of carbohydrates, and perhaps also other conditions, lead to varying degrees of acidosis, with an abstraction of alkalis from the tissues, and possibly in other ways may be of distinct harm to the patient. This is borne out by the clinical observation that fat individuals are comparatively poor subjects for typhoid fever, or for that matter, for any infectious fever or for surgical operations. We have, however, as yet no good reason for believing that the metabolism of fat in fever is in any way different qualitatively from that in other conditions of undernutrition; and it seems to me probable that the excretion of the acetone bodies occasionally recorded in typhoid fever is merely the result of a burning of body fat in the absence of sufficient carbohydrates—the frequently observed starvation acidosis. If this is true, acetone, diacetic and beta-oxybutyric acids will not be excreted by the typhoid patient who receives an abundance of carbohydrates. Aside from the production of organic acids in the burning of body fats, the mere loss of fat is probably of no great consequence.

In addition to the loss of body fat there is a great loss of body protein in typhoid fever, and this is what I shall speak of in some detail. Many data are to be found in the literature as examples of the amount of this loss of protein. In a case reported by Leyden and Klemperer² there was a loss of 109 gm. of nitrogen, the equivalent of 3.2 kilos or seven pounds of pure muscle tissue in twelve days. In a case of Frederick Müller³ there was a loss of 86.4 gm. of nitrogen, the equivalent of 2.5 kilos or five and one-half pounds of muscle tissue in eight days. These are not very unusual figures; a loss of the equivalent of even one and one-half pounds of muscle tissue in a single day is not very rare. There are many reasons for believing that this febrile loss of protein from the body is a serious and dangerous proceeding. The emaciation, muscular weakness and decreased resistance, and the long convalescence are certainly in part the results of the loss of body protein. The amount of protein lost appears to bear a close relation to the severity of the disease, and Ewing believes that the pathologic processes concerned in the metabolism of this body protein take a prominent part in determining the patient's condition. Ewing's⁴ idea is that many of the phenomena of typhoid fever, especially in the severe and fatal cases, are due to an autointoxication resulting from the "burning of thirty pounds of body tissue in three weeks"—and not directly to the endotoxins of typhoid bacilli. This idea is to some extent supported by the fact that the so-called nitrogen partition of the urine in severe or fatal cases is decidedly abnormal, and similar to those found by Wolf, Ewing and others in toxemia of pregnancy. Furthermore there are severe so-called toxic cases of typhoid fever which terminate with acute yellow atrophy of the liver, a condition which appears to be closely associated with a particular type of faulty protein metabolism. But aside from the possibility of its creating an autointoxication, the consumption and loss of body protein must be of

great harm to the patient, both during the disease and during convalescence.

The causes for this loss of body protein are apparently three in number.

The first is partial starvation. An individual is obviously undernourished unless he absorbs from the digestive tract food of sufficient caloric value to equal the energy expended. Except by very difficult and accurate measurements it is impossible to know the amount of energy being expended by a particular patient, and few such measurements have been made on fever patients; but we may readily calculate average figures which are satisfactory for practical purposes.

At ordinary rest the heat and other energy expended by a normal individual receiving sufficient food is about thirty-three calories per kilo body weight. In fever there is an increased heat production, with an average, according to Krehl, of 20 to 30 per cent. Twenty-five per cent. added to the thirty-three calories gives about forty calories, or, for a patient weighing seventy kilograms, or 150 pounds, 2,800 calories. This represents the minimum amount of energy which the average typhoid patient is expending in each twenty-four hours. If he does not receive food equivalent to this amount of energy he merely draws on his body tissues to make up for the deficit. Few or no typhoid patients receive enough food to maintain an equilibrium, and they consequently burn up for fuel varying amounts of their body fat and protein.

Any physician can readily calculate the probable deficit with the diets he uses. The deficit in calories is usually about 50 per cent., and this, according to von Noorden's figures,⁵ may be responsible for loss of 2 to 3 gm. of body nitrogen per day.

The two other causes for the febrile loss of body protein are the pyrexia and the action of the bacterial toxins. From the experiments of Linser and Schmidt,⁶ Fritz Voit⁷ and Schleich,⁸ we know that artificially raising the body temperature causes an increase in protein metabolism. The pyrexia itself is therefore one of the causes for the loss of body protein; but the loss due to this cause can be prevented, just as the loss due to partial starvation, by the intake of sufficient food.

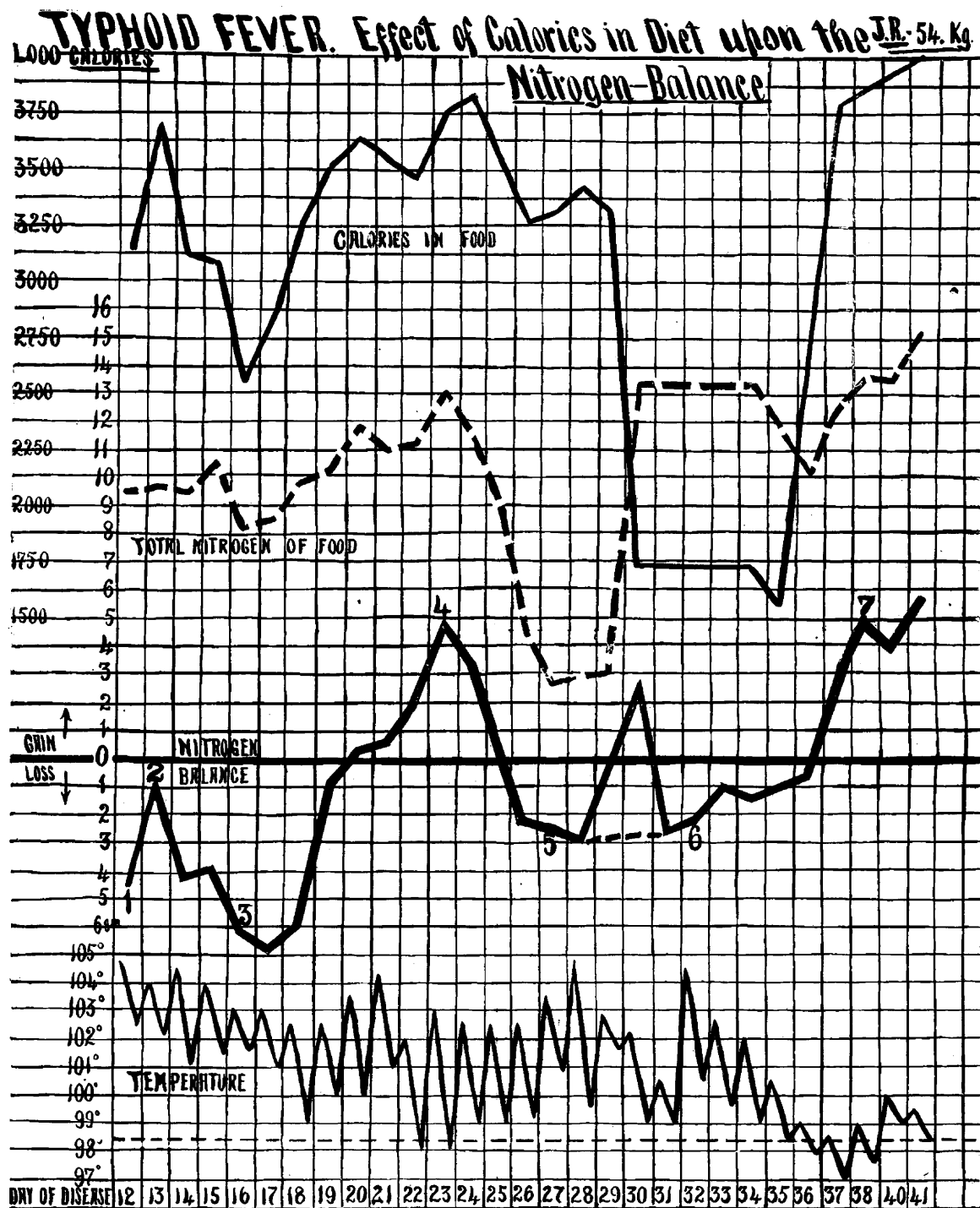
The third factor is the so-called "toxic" destruction of body protein from the poisonous action of the bacterial toxins on the body protein. This last factor is still open to discussion, but I am inclined to the belief that some such action does take part. It is, however, at present impossible to discuss the toxic destruction apart from the result of the pyrexia, because we can not distinguish between the two when both are present, as they usually are in typhoid. The question which is of importance for the practical treatment of typhoid is, can the combined effect of the pyrexia and the bacterial toxins be prevented from causing a loss of body-protein? There are already some encouraging answers to this question. May,⁹ working with rabbits infected with pig erysipelas, was able to decrease the febrile loss of body protein by carbohydrates; though May later doubted the correctness of his first interpretation. Puritz¹⁰ worked on human typhoid in St. Petersburg, and his results

2. Von Leyden: *Handbuch der Ernährungstherapie*, 1904, II.
3. *Cong. f. inn. Med.*, 1902, p. 192.
4. *Proc. Path. Soc. Philadelphia*, 1905; paper before New York Acad. Med., New York Med. Rec., 1907, p. 537.

5. *Handbuch der Pathologie des Stoffwechsels*, 1906, I, 497.
6. *Arch. f. klin. Med.*, lxxix, 514.
7. *Sitzungsber. d. Gesellsch. f. Morphol. u. Physiol. in München*, 1895, No. 2.
8. *Arch. f. exp. Pathol. u. Pharmacol.*, iv, 82.
9. *Ztschr. f. Biol.*, 1894, xxx, 1.
10. *Virchows Arch. f. Path. Anat.*, 1893, cxxxi, 327.

led him to favor a liberal diet in this disease. His diets, however, were rich in protein and of only moderate caloric value, and were therefore, I believe, not well adapted for his purpose, which was to retard the loss of protein. Weber,¹¹ working with a sheep inoculated with an extract of glanders bacilli, was able wholly to prevent any febrile loss of body protein by giving the

Last summer Dr. Warren Coleman and I undertook in Bellevue Hospital, New York, a further study of the extent to which the loss of body protein in typhoid fever might be retarded by dietetic means.¹² Our results were on the whole very encouraging, in that we were able to diminish the loss of body nitrogen to a comparatively small amount during the fastigial temperature and were



animal a liberal diet containing much carbohydrate. Leyden and Klemperer, on the other hand, conclude from their experiments on typhoid and pneumonia that, while a liberal diet is desirable, it is not possible to prevent the febrile loss of protein; this view is generally accepted.

11. Arch. f. exper. Path., xlvii, 10.

able to make patients gain body protein during the steep curve period of the disease.

I need not here go fully into the reasons underlying

12. We are indebted to Dr. Armstrong, medical superintendent of Bellevue Hospital, for placing the facilities of the hospital at our disposal. The experiments will be published elsewhere in detail.

our choice of diet, which is the opposite of that used by Puritz, and indeed the opposite, in some particulars, of the diets at present used in typhoid fever by most physicians. Our principle has been to have the diet contain a moderate amount of protein and the largest possible amount of carbohydrate. The reasons for choosing carbohydrates as the basis of the diets were briefly as follows: Carbohydrates are the strongest spacers of body protein in health, according to Voit, Lusk, Folin and others; and the experiments of May, Weber and Linser and Schmidt indicate that this sparing action takes place in fever as it does in health.

A large amount of fat is objectionable because of its tendency toward digestive disturbances and diarrhea; and the protein should be kept at the minimum effective amount because of its action in increasing the heat production (Rubner) and because of the large amount of work it throws on the intestine and the kidneys. The results from one of our cases are given in the accompanying chart. Samples of the diets used are given in the table.

TABLE OF COMPOSITION OF FOOD DURING EXPERIMENT
SHOWN IN CHART.

The numbers at the left refer to corresponding numbers on the "nitrogen balance" curve in chart.

	Gm.	Calories.	Per Cent. Total Calories.	Calories Per Kg. body Wt.
1. { Protein.....	60	245	7.8	
Fat.....	80	745	23.7	
Carbohydrate.....	525	2150	68.5	
		3140		58.
2. { Protein.....	56	230	6.2	
Fat.....	78	725	19.5	
Carbohydrate.....	670	2750	74.3	
		3705		69.
3. { Protein.....	53	217	8.7	
Fat.....	63	585	23.6	
Carbohydrate.....	410	1680	67.7	
		2482		46.
4. { Protein.....	75.5	310	8.2	
Fat.....	120.4	1120	29.8	
Carbohydrate.....	569	2330	62.0	
		3760		70.
5. { Protein.....	16.2	66	2.0	
Fat.....	108	1004	30.3	
Carbohydrate.....	547	2240	67.7	
		3310		62.
6. { Protein.....	84	345	20.0	
Fat.....	96	893	51.7	
Carbohydrate.....	120	490	28.3	
		1728		32.
7. { Protein.....	85	349	8.8	
Fat.....	122	1234	28.7	
Carbohydrate.....	583	2390	62.5	
Alcohol.....		80		
		3653		73.

Our results with other cases show essentially the same thing, although in some cases there was no gain, but a continued slight loss of one gram or so per day. On the high caloric diets, however, the patients were almost always in a condition not far from nitrogen equilibrium.

Our results show, we believe, that the febrile loss of body protein, including the result of the three factors, undernutrition, pyrexia and the action of toxins, may be retarded and even wholly prevented, or compensated for. But the results show likewise how difficult this is to accomplish. It was only when we gave sixty to seventy or even eighty calories per kilogram—between

3,000 and 4,000 calories—that the greatest sparing was observed.

The important point is, however, that it is possible to give typhoid patients such liberal diets without, so far as our experience shows, producing any harmful results, but, we believe, with decided benefit.

Having learned that it is possible to retard the febrile loss of body protein, we have still to decide whether it is desirable to do so in typhoid fever.

There are many objections, some apparent and others real, and many formidable difficulties; but I know of no reason why we should not attempt to do all in this direction that the circumstances will allow. Among the possible objections to liberal feeding in typhoid is the commonly supposed digestive limitation of these patients. The digestion in typhoid fever undoubtedly has its limitations, but they are not such as to prevent the proper absorption of amply sufficient food if given in the proper form. The experiments of von Hoesslin,¹³ Leyden and Klemperer,¹² Puritz,¹⁰ Folin¹⁴ and others show very positively that the average typhoid patient absorbs food from his intestine almost as completely as does the healthy individual. Only in the severest cases is the absorption very materially decreased. In the average case without profuse diarrhea the digestion of protein, fat and carbohydrate is within 10 or 15 per cent. of the normal. Folin,¹⁴ in his recent work on typhoid, has directly determined the degree of absorption of carbohydrates and has found it practically normal.

What seems to me an objection to liberal feeding in typhoid fever is the effect of the protein in increasing the heat production. Rubner¹⁵ has shown how a strict protein diet in a dog may increase the heat production more than 50 per cent. The effect in a human patient could never be so great, but with the decreased heat loss in fever it is quite possible that this factor may be of considerable importance. This objection does not hold against the diets Coleman and I have used, but may apply to the high protein diets used by others. Carbohydrates have only a very slight effect in increasing heat production.

The objection has been raised to the use of large quantities of carbohydrates, from the fear of fermentation and tympanites. I can only say that we have had no such experiences with the use of milk sugar or starch.

The greatest practical difficulty that we have so far encountered in this work is the choice of food products. After trying or considering various carbohydrate we have used milk sugar almost wholly. This has the advantage of fermenting only with difficulty and of being less sweet than cane sugar and much more soluble than any form of starch. Milk, diluted cream, and eggs have been used to furnish protein and fat. Cocoa, lemon juice, tea, coffee and other things have been used as flavoring agents and as vehicles for milk sugar.

The patients did not object to our diets more than to milk alone, but some persuasion was frequently necessary to get them to take the amounts of food found to be necessary for our purposes.

The full advantages as well as the possible objections to our dietetic plan have still to be demonstrated; but there are a few reasons in favor of such liberal nourish-

13. Virchows Arch. f. Path. Anat., 1882, lxxxix, 317.

14. Unpublished experiments.

15. Rubner: Die Gesetze des Energieverbrauchs, 1902.

ment for typhoid patients which are worthy of consideration at this time.

As I pointed out earlier, the average typhoid patient receives at present 50 per cent. or less of his energy requirement. This is half starvation; and we know that starvation is harmful even in health, in that it leads to weakness and to an increased susceptibility to many infectious diseases. If starvation is harmful in health why should it be beneficial in typhoid fever? During the course of typhoid fever great demands are constantly being made on the defensive power of the organism; and it certainly does not seem probable that the patient will be as well prepared to meet those demands when in a starved or half-starved conditions as when he is being supplied with sufficient energy in the form of food.

It is a common laboratory observation that strong, robust, well-fed animals develop the strongest artificial immunity; and it seems fair to believe that we are assisting the patient to acquire his immunity to typhoid fever by keeping his nutrition at the highest possible level. Is it not possible that one of the factors determining the outcome of the disease may be the state of nutrition in which the body cells are maintained? Retarding the loss of body protein should leave the patient at the end of the fever in better physical condition and so make possible a shorter convalescence.

This sort of reasoning is, however, largely speculative; and long experience alone can tell the true value of such treatment. We can merely say at present that it is possible by the means already outlined to retard and even to prevent the febrile loss of body protein.

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DISCUSSION.

DR. LAWRENCE LITCHFIELD, Pittsburg: During last summer I spent a great deal of time reading all I could find in the Surgeon General's Library on metabolism in typhoid, and from the crude experiments of Leyden, published in 1869, attempts made by increased feeding to prevent or control the loss of weight during the course of the fever, through the work of von Hösslin, whose classic article was published in 1882, and the excellent work of von Leyden, Klemperer and others down to that of Lüthje, who published in 1902 the report of his successful attempts to establish a plus nitrogen balance during the period of steep curve, though with very high proteid feeding, this work of Drs. Shaffer and Coleman is unquestionably the best that has been done on the metabolism of typhoid up to the present time.

THE BACTERIOLOGIC DIAGNOSIS OF TYPHOID FEVER.*

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Since the introduction of modern laboratory aids in the recognition of typhoid fever the problem which has presented itself to practitioners of medicine is that of more accurate and earlier diagnosis. The demand for a scientifically accurate diagnosis is the outcome of the realization that clinical typhoid fever is a disease which may be caused by any one of several bacteria. The physician of the present day is no longer satisfied with the old term "enteric fever." He tries, so far as possible, to supplement his clinical diagnosis by an accurate bac-

teriologic diagnosis. The establishment of the bacteriologic diagnosis is only to be accomplished by the isolation of the specific organism infecting the individual patient.

From a practical standpoint the question of early diagnosis is of much greater importance than that of absolutely accurate diagnosis. To the individual patient early diagnosis in typhoid means better treatment, better nursing, perhaps more adequate domestic arrangements, and often a great saving of strength during the first stages of the disease. Moreover, if we are to have any specific method of treating typhoid, its value will probably depend to a great extent on the time at which its use is begun. In the sphere of preventive medicine the problem of the early diagnosis of typhoid has lately assumed enormous proportions. That the typhoid patient is a source of danger to the community during his sickness has long been recognized. That he may continue to disseminate the bacilli for months and years after his convalescence is now becoming a well-known fact. That he may, and doubtless often does, spread the contagion even before he shows any clinical symptoms of the disease, that the disease is infectious during the incubation period, has, however, only recently been proved. Conrad,¹ in a striking article, has demonstrated that the typhoid bacillus can be isolated from the blood of an apparently healthy individual, and has shown that in eighty-five cases of so-called "contact infection" the infection occurred in 58 per cent. of the cases during the first week. In the fight to limit the spread of typhoid fever the task of isolating and controlling the late dangerous cases, the chronic bacilli carriers, falls to a great extent on the public health authorities. The detection of the early bearers of contagion must, however, be the duty of the individual practitioner.

Although the bacillus of typhoid fever may be isolated from various sources, the only methods which have proved themselves to be of considerable value are the examinations of the urine, the stools and the blood. The first of these—the urine—has never come into special prominence as a routine procedure. The organisms can be isolated from the urine in about 25 per cent. of the cases, and often early in the disease; but this percentage of favorable results is hardly sufficient to warrant the method being used very extensively.

The examination of the stools as a diagnostic measure in typhoid appeared at one time to have a brilliant future. The conception of the disease as an intestinal infection went hand in hand with the idea that the dejecta swarmed with typhoid bacilli. The first difficulty encountered was the necessity of differentiating the typhoid from the colon bacillus. In answer to this need there was brought forward a long series of special culture media on which these organisms could be distinguished from one another with more or less accuracy. Some of these media were difficult to use, others comparatively simple. All of these methods, however, for their success depend on being used by skilled bacteriologists; and even so, the results obtained by them have not, on the whole, been favorable. In the hands of some observers they have done well, and there are many series of cases in which the bacilli have been isolated from a large percentage of the stools. Other equally good workers have failed to confirm these results and have only been able to find them in a comparatively small proportion of their cases. The conflicting reports as to

* Read in the Joint Meeting of the Section on Practice of Medicine and the Section on Pathology and Physiology of the American Medical Association, at the Fifty-ninth Annual Session, at Chicago, June, 1908.

1. Deutsch. med. Wehnschr., 1907, p. 1684.