ously active when present in the blood, are inert when introduced into the stomach. Even when circulating in the blood, toxins, in common with other organic poisons, are probably being constantly eliminated into the alimentary canal, where they would at once lose their destructive power by contact with the bile, and thereby the total effective quantity of the toxin in the body would be reduced. There are also many poisonous substances generated in the intestinal canal, which in some circumstances produce intoxication. The production of this auto-intoxication may partly be due to the failure of the liver to secrete a sufficient amount of healthy bile, and the therapeutic value of cholagogue substances, so long employed on empirical grounds, may thus also obtain a satisfactory explanation.

Bile is produced in large quantity by all animals, and it is present in the intestinal canal throughout its whole length, but beyond the fact that it is an important excretion, removing waste products from the body, its functions are not known to be of much value. So much is this the case that a recent writer has remarked, "the bile is a most elaborate secretion; it is poured into the intestine, and finds apparently little to do." To such recognised functions as the promotion of fat absorption and the laxative and indirect antiseptic actions, there may be added the additional function of rendering inert many organic poisons introduced into or generated in the alimentary canal.

REFERENCE.

- ¹ Halliburton, *A Textbook of Physiology and Pathology*, 1891, p. 687.

THE BACTERIOLOGY OF ALOPECIA AREATA.

By GEORGE THIN, M.D.

A STUDY of Sabouraud's remarkable paper on Alopecia Areata in the *Annales de l'Institut Pasteur* of February 29th, 1897, has led me to believe that a short statement of some observations of my own in connection with this subject may be of interest. As an excellent summary of Sabouraud's work is given in the *BRITISH MEDICAL JOURNAL* of April 24th by Dr. Wickham, of Paris, it is not necessary for me now to restate the views of the talented French investigator. My work as compared with Sabouraud's is fragmentary, but in the uncertainty of being able to devote sufficient time to the study in the immediate future, I think it well to put it on record.

In March, 1881, Professor Huxley communicated for me to the Royal Society a paper entitled *On Bacterium Decalvans*, an Organism associated with the Destruction of the Hair in Alopecia Areata. At that time I occupied myself a good deal with the examination of hairs from cases of alopecia areata, and after having observed minute bodies which I considered were organisms, I selected a certain number of cases for more systematic examination. The method I employed at that time was to pass the hairs successively through a solution of potash, distilled water, alcohol, and ether. They were then placed again in alcohol, and afterwards distilled water, and finally mounted for examination in diluted Goadby's solution. I stated in the paper that the method was not perfect nor always successful, but in some instances it sufficed to show bodies in the cuticle of the hair, which I believed it was justifiable to consider as bacteria. Other methods, designed with a view to free the hairs from fatty matters and render them transparent, seemed to me to afford similar results. The bodies which I considered to be bacteria were round or elongated in shape, were found frequently in pairs, the long axis of each member of the pair forming a continuous line. Sometimes three of them were found end to end with an appearance of one continuous sheath for the three. Clusters of the organisms were also found in dammar preparations, in which the hair was found split into shreds, the shreds being sometimes so thin that the objects were seen with much distinctness. I inferred from the appearances observed that the bacteria grow downwards between the internal root sheath and the shaft. Towards the root of the hair they penetrate the hair substance, and as they multiply they ascend upwards in the substance of the hair. The breaking up of the hair was attributed to the disorganisation caused by the products of the growing organisms, for it is unreasonable to suppose that a free development of bacteria can take place in the shaft of

the hair without some of its substance being decomposed and its integrity destroyed. I was careful in the summary of my paper to divide the statements into two categories: one as to facts, the other as to theory. The facts were that minute bodies of definite and fixed shape and size, having the refractive quality of bacteria, were found in and on the hairs in alopecia areata. The theory was that these bodies were bacteria, and that the disappearance of the hair was due to a breaking up of the hair shaft by the multiplication in it of the organisms. For these organisms I proposed the name of "decalvans," and if it becomes definitely proved that an organism is the actual cause of alopecia areata I venture to think that no better name could be found for it.

In a plate which accompanied the paper in the *Proceedings* of the Royal Society, the bodies I described are drawn, and the drawings give a very good idea of the way in which they were grouped and arranged. I call special attention to that paper and to the plate which illustrates it, because, so far as I know, it is the first time that an attempt was made to show by direct observation that alopecia areata might be caused by a bacterium. I do not make the statement that this is the same organism as that which has been so ably studied by Sabouraud in sections of the skin, but I see no reason to believe that it might not be the same. At the *Versammlung Deutscher Naturforscher und Aerzte* in Strassburg, September 1885, Dr. von Sehlen demonstrated to the Section of Dermatology and Syphilis microscopic preparations of a typical case of alopecia areata in which organisms which he describes as micrococci were to be seen in the sheaths of the hair root. The following year at the meeting of the *Versammlung* at Berlin he expressed himself as concurring with Max Joseph in considering that experiments by the latter observer showed that the primary cause of the disease is of a trophoneurotic nature, but added that the micro-organisms were necessary to supplement it—"eine notwendige Hilfsursache Alopecia Areata."

In the year 1889 I caused to be removed, from a patch of alopecia areata in the scalp of a young man, a small portion of skin, which I examined microscopically. In sections stained with gentian violet I was struck by finding in some of the hair follicles a sac-like dilatation in the upper part of the follicle, just above the entrance of the duct of the sebaceous gland, and in this sac-like dilatation I found numbers of well-stained micro-organisms. I found that the same organisms stained faintly with hæmatoxylin. In some of the follicles, in which there was not much distension, a smaller number of similar organisms were found lining the inner root sheath of the hair. I proposed investigating in a similar way a series of cases, but my attention was directed to other matters, and, being unable at the time to continue the study, the sections were put away for future use. When reading Sabouraud's paper in the *Annales*, and when examining the plate which illustrates it, I at once observed the similarity of the appearances in his plate to those which I remembered having seen in my sections, and on re-examining the sections I found that, although most of them had lost their colour, in some the staining remained sufficiently good to enable me to again study the organisms. In a few preparations the gentian violet coloration still persists, and although the hæmatoxylin staining, which was faint at the outset, had become still fainter, it was still sufficient to bring the bacteria into view.

I am not aware that anyone has published drawings similar to those of Sabouraud, and, therefore, although I possess sections of only one case, I hope it will be interesting, as well as instructive, if I remove them from the drawer in which they have lain for eight years, and describe them in the *BRITISH MEDICAL JOURNAL*. Those readers who have not had an opportunity of seeing Sabouraud's original drawings will be able to form, from the figures which I give, a better idea of what the appearances are to which he refers than they could by simply reading a description of them. Readers familiar with the histology of the skin, on looking at Fig. 1, will at once see that the upper part of the hair follicle is in an abnormal condition. Above the point of entrance of the sebaceous gland into the follicle, and between that point and the rete mucosum, the follicle is seen to be dilated, and at the dilated part to be filled with a deeply stained, irregular mass, and when looked at with a magnification of 400 to 500

diameters, it is seen that the colour is due to the broken up and irregular root sheath having retained the stain. In the preparation this part stained violet, while the outer root sheath of the hair had retained a carmine stain. On

Fig. 1.—a. Horny layer of epidermis. b. Rete mucosum. c. Hair shaft. d. Border of detached inner root sheath, shown more highly magnified in Fig. 2. e. Outer root sheath of the hair. f. Altered inner root sheath. Groups of bacilli are present at the parts marked as dark patches. g. A group of bacilli are to be seen at this point in the altered inner root sheath. (See Fig. 3.) h. Sebaceous gland as seen magnified by a low power.

examining the disintegrated inner root sheath with a high power, very minute bacteria could be seen in considerable numbers in its substance, surrounding the shaft of the hair. (See Figs. 2 and 3). They were not present in the outer root

Fig. 2.—a. Outer edge of the hair. b. Space between hair shaft and the inner root sheath. c. Bacilli lying on the inner root sheath. d. Inner root sheath. Very highly magnified; about 2,000 diameters.

sheath, nor in the surrounding connective tissue. They were seen in greatest abundance on that part of the inner root sheath which was close to the hair. These organisms are so small that the artist found it very difficult, in drawing them, to be sure that the size was accurately given, and in order to secure as nearly as was possible a representation of their size and form, I was obliged to have the drawings made with an unusually high power.

This was done by using a Powell and Lealand's 1-20th apochromatic oil immersion, a No. 10 eyepiece, and the long (10-inch) tube. Notwithstanding the high eyepiece employed the image was quite distinct. The magnification as seen

Fig. 3. Organisms seen at the part marked g in Fig. 1. The comparatively clear space indicates the canal in which the hair shaft lay. The organisms are seen to be grouped round the site of the hair shaft. Very highly magnified; about 2,000 diameters.

should be about 2,000 diameters, but I find that in drawing from the microscope, when the camera is not used, the personal equation can never be quite eliminated, different persons seeing (and therefore drawing) the same objects different sizes. In the present case I think the form of these minute bacteria has been fairly well given, and their position and number are exactly as represented in the drawing, each organism having been carefully copied from the image, the artist being instructed to take special care to reproduce the size. For the purpose of clearness a part has been chosen for drawing in which a cluster of organisms was found lying on a flat surface of the detached inner root sheath (Fig. 2), a space intervening between the inner root sheath and the border of the hair shaft. In comparing these drawings with Sabouraud's plates, I consider that the dilated hair follicle, with its stained masses, is equivalent to his "seborrhœic cocoon," and it was when I saw the drawings in his plates that I was reminded of the appearances which I had seen eight years previously. The difference between the magnifying powers used in drawing the individual bacilli in Sabouraud's plate and those used by the artist for the drawing of Fig. 2, renders a comparison of the two drawings somewhat difficult. Sabouraud's drawing was made by a 1-12th immersion with a 5 eyepiece, mine with 1-20th and a 10 eyepiece, to which must be added the increased magnification obtained by using the English length of tube. My magnification must have been, therefore, at least double his. I can only say that in both cases a small bacillus is shown, of which the length usually considerably exceeds the breadth, the proportion of the length and breadth in both cases being not dissimilar. In another part of the preparation, the part marked g in Fig. 1, a number of organisms which were present were also drawn (Fig. 3), and in that drawing there are present not only elongated bacilli, resembling those shown in Fig. 2, but shorter forms, and some almost round, resembling examples of a similar form which are also to be seen in one part of Sabouraud's plate. As far as the appearances are concerned, I see no reason to believe that the organisms portrayed by him are different from those seen by myself and shown in Figs. 2 and 3.

The relation which these organisms probably have to the loss of the hair is so well considered in Sabouraud's paper (see Dr. Wickham's abstract) that it is not necessary for me to discuss the question here. The French investigator seems on the right road to place the causal connection on a definite basis. So much for sections of the skin in alopecia areata.

In reverting to my paper in the *Proceedings* of the Royal Society, to which I have already referred, it will be remembered that I had described what I believed to be organisms in split up and disintegrated hairs, and an interesting case having presented itself within the last few weeks, I decided to utilise it, to see whether I could, by the newer methods, get more demonstrative preparations than I obtained at a time when I failed to procure satisfactory results by staining.

The commander of a merchant ship, aged 43, who had always enjoyed good health, and who appeared a vigorous and robust man, consulted me on April 30th, on account of a loss of hair on the thighs and legs, having been alarmed by being told that the disease was a form of alopecia which might extend, and cause a loss of hair over the whole body. His thighs and legs were those of a somewhat hairy man, being, in the unaffected places, covered with black hairs a little over half an inch long. He informed me that two years and a-half previously a patch of alopecia appeared on the front of the left thigh. Six months afterwards a similar patch appeared on the right thigh in a corresponding place, and three or four months later a similar patch appeared on the right ankle. The first patch observed on the thigh was the size of a florin before it attracted his attention. A year ago a patch appeared on the back of his right hand, and another patch on the left wrist about the same time. On examining the affected parts I found that the whole of the front part of the thigh was free from hair, the bald part extending almost to the knee, and a little towards the outside. There were round bald patches on the calves. The hairless part had a regular margin, as if it had been made with the sweep of a razor. The hairless follicles were indicated by small pin-headed prominences of a slightly dull brick-red colour. On the lower half of the bald patches on the thighs I noticed some small, short stumps in the follicles. All the bald patches were fairly uniform in appearance, and there were no inflammatory symptoms and no scaling, and no alopecia on the head or face. The clinical appearances were characteristic of alopecia areata, and that diagnosis was confirmed by the absence of trichophyton in the few hairs and stumps which were examined.

These were extracted by himself, and sent me by post after

Fig. 4.—A stump extracted from the patch of alopecia areata as seen by a low power.

he left me, there not being time for this purpose when he consulted me at my house. He sailed the next day, and in the meantime I have not had another opportunity of seeing him. I had only a few hairs and stumps at my disposal. These were stained in gentian violet by the Gram-Weigert method. In the hairs on the margins nothing abnormal was found, and, in the few stumps which were available, the majority of them had the appearance, under the microscope, simply of a short hair which has been forcibly pulled out, and nothing abnormal was seen. In one of the stumps, however, which showed to the naked eye an abnormal appearance, I found numerous micro-organisms deeply stained with gentian violet, whose general arrangement in the split hairs recalled to my memory appearances I had seen in unstained

Figs. 5 and 6.—Two ends of the stump more highly magnified, but as seen by a low-power. The masses of bacilli (a) can now be seen arranged between the splittings of the hair.

Fig. 7.—Part of a breadth of this stump as seen by a high power, probably about 700 diameters. a. The individual bacilli can now be seen. b. The edge of the hair.

preparations in previous years. It is for this reason that I consider the fact worthy of being put on record in such a way as to define more precisely the appearances which I had previously described, the successful staining affording an opportunity for drawing and for photographing which was not pos-

sible, to anything like the same extent, in unstained preparations. This hair stump was, as will be seen by looking at the figures (4 and 5, 6 and 7) split up into longitudinal bands by clefts. In these clefts numbers of well-stained small bacilli are to be observed. The organism is a minute bacillus, mostly single, but frequently in pairs, there being rarely three, and still more rarely four in a row, showing the manner in which the organism multiplies. An examination of the drawings will save a more detailed description. Fig. 4 shows the hair as seen by a low magnifying power. Figs. 5 and 6 show the ends of the hair as seen with a somewhat higher power, but still not sufficiently magnified to show the individual bacilli well. Fig. 7 is a portion of the breadth of the hair taken from near the middle of the stump, and an attempt is made to show the individual organisms. This drawing was made by a 1-20th oil immersion, a low eyepiece, and a short tube, and probably represents a magnifying power of 700 to 800 diameters. It shows the general arrangement of the bacilli in groups between the clefts of the fibres of the hair.

In Fig. 8 I have endeavoured to show the various forms in

Fig. 8.—Various forms of bacilli seen in the hair, the bacillus being sometimes seen as almost round, generally elongated, sometimes in pairs, and sometimes as double pairs. Highly magnified.

which I observed the bacillus. They are selected from a number of drawings made by myself, and give no idea of the relative proportion in which the different forms were found. The single and the double bacilli are by far the most common. The rows of three were not common, and of four rare.

In order to obtain as accurate a representation as possible of this bacillus Mr. Andrew Pringle was good enough to undertake to photograph it in the hair with a magnifying power of 1,000 diameters. Figs. 9 and 10 represent these

Fig. 9. Organisms on the hair; from a photograph. Multiplied 1,000 diameters.

photographs, and although their reproduction in the BRITISH MEDICAL JOURNAL presents technical difficulties, the engravings may give some idea of their general form and size. The arrangement in pairs and in threes and fours shown in

Fig 8 recalls forms shown in Fig. 5, Plate 4, in Sabouraud's paper—the organisms observed in the lower part of the "seborrhoeic cocoon;" but although I make this remark, I do not wish to imply any assertion regarding the identity of the organisms.

The importance which will be attached to the presence of these organisms in a split-up stump in a case of alopecia areata will necessarily depend on the subjective attitude of the reader. I do not mean to say that it proves much or

Fig. 10.—From a photograph. Organisms lying on the hair showing some long forms. Multiplied 1,000 diameters.

that it is a proof that alopecia areata is caused by an organism, but what I do believe is this, that an organism cannot multiply in the inside of a hair or in the hair follicle outside the hair without disorganising the tissue in which it is grown. Its nutrition can only take place by the splitting up of the elements of the hair sheath or hair shaft, part of the products being appropriated to its growth and part of them being left as abnormal substances in the follicle.

According to my view a hair cannot possibly remain healthy or remain for a long time in the cutis if its elements are split up or disintegrated by the growth of a bacillus, and similarly the inner root sheath of the hair by which the shaft is attached to the cutis, if it is disorganised by the growth of a bacillus, will cease to perform its functions, and the hair must fall.

I would not have published this solitary case if it had not confirmed the results of my previous observations, and if the pressure of other matters had not rendered it improbable that I shall be able to devote much more time to this special investigation. Whatever the future may have in store in the way of discovery of the part played in the skin by pathogenic organisms, every well-ascertained fact connected with the microbes found in the hairs or hair follicles deserves at the present time to be put on record. The observations which I have now noted may be found to have some bearing on facts collected by other observers, and may, I hope, suggest further investigations by attracting the attention of workers to what seems to be a very promising field; and if I have assisted in calling fresh attention to Sabouraud's paper, and have therefore been instrumental in promoting the further study of the subject by pathologists and bacteriologists in the various parts of the English-speaking world, my paper will have served a good purpose.

Two well-attested sets of facts strengthen the view that alopecia areata is caused by a bacterium. The one is the occasional communication of the disease by contagion, and the other the arrest of its spread by antiparasitic remedies. As regards communication by contagion, the French observers (the disease seems to be comparatively common in

France) have recorded numerous examples. Even in this country, cases of the kind are occasionally found. I have at the present time under my care a lady whose well-marked alopecia areata of the scalp followed an attack of the disease in her husband. A few years ago I had under my care a young adult man, with well-marked alopecia. He had visited a married sister, who gave up her own bedroom to him. She omitted to take away her towels and hairbrushes, although she was suffering from alopecia areata. The brother used both towels and brushes from the Saturday to the Monday, and shortly afterwards observed a patch of alopecia in his scalp.

As regards the successful treatment of alopecia areata by antiparasitic remedies I entertain no doubt. In the great majority of cases this treatment, if properly carried out, arrests the disease, and my experience of the effects, in a great number of patients, of the vigorous treatment of the margins of the patches by strong sulphur ointment, which is described in this JOURNAL of 1882, ii, pp. 783, 828, has since been amply confirmed.

In prescribing this treatment it is necessary to insist on the hairs being cut from the margin of the patch for a distance of at least half an inch, and the sulphur ointment should be firmly rubbed into the hair follicles of this margin. Turpentine liniment I have also found useful. Some years ago I succeeded in arresting the spread of the disease in a lady's eyebrows by its use. The baldness had begun at the outer ends of the eyebrows, and had steadily advanced until the outer halves of both eyebrows were lost. By the cautious but thorough use of this liniment the inner halves of the eyebrows were saved, and some hairs shortly afterwards began to reappear in the halves that had been denuded.

CHLOROFORM AND THE HEART.

BY SURGEON-LIEUTENANT-COLONEL E. LAWRIE, M.B.,

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It is stated in the BRITISH MEDICAL JOURNAL of April 24th, 1897, that Mr. Leonard Hill has "brought forward additional evidence of the incorrectness of the doctrine promulgated by the Hyderabad Chloroform Commission, that chloroform has no direct action on the heart.....Recent experiments, we are glad to see, are in complete accord with clinical experience." The fallacy of the first of these statements is readily demonstrated. The Hyderabad Commission's experiments on animals have proved that the uncomplicated action of chloroform is to cause lowering of the blood pressure with, first unconsciousness, then anæsthesia, then stoppage of the respiration, and then death from stoppage of the heart. These are the elementary facts. It ought to be clear, even to Mr. Leonard Hill, that if he, and Messrs. Gaskell and Shore, and MacWilliam, obtain results, by "ingenious" methods, which are at variance with these ascertained facts, they must be due to complications introduced by their experimental methods, and not to any variation of the physiological action of the drug—which is, of course, an impossibility.

There is no doubt that in several recent instances physiologists have mistaken the effects of complications, introduced into their experiments by themselves, for the effects of chloroform. For example, the Glasgow Committee gave chloroform on a "cloth saturated with the anæsthetic, held over the mouth and nose" of the animal. This produces struggling, holding of the breath, and vagus inhibition of the heart. The stoppage of the heart was attributed by the Glasgow Committee to the action of the chloroform, while the real cause of its inhibition through the vagus, the breath-holding, was entirely overlooked. In the same way Messrs. Gaskell and Shore, by means of their "ingenious" cross-circulation experiments, caused the heart to stop. But it was afterwards discovered that they had never succeeded in establishing a true cross-circulation, and that the effect which they attributed to the direct action of chloroform was an indirect one which proved nothing.

Where the Hyderabad Commission is inclined to feel resentment is against Mr. Leonard Hill's unfairness in parading the good work of the Glasgow Committee, Snow,

and Ringer, MacWilliam, and Gaskell and Shore, in contrast to the Hyderabad Commission, although he differs from all of them, as much as they differ from one another, on all points which are essential. For instance, Mr. Leonard Hill allows that cardiac inhibition is of no importance, and never entails syncope. Nevertheless, the Glasgow Committee had for years been considered to have proved that cardiac inhibition is of importance, and does bring about that identical syncope which is supposed in England to be the cause of death from chloroform. The Hyderabad Commission was instituted primarily in order to prove or disprove this very assertion of the Glasgow Committee. They did succeed in completely disproving it, and Mr. Leonard Hill might have given them credit for having performed this signal service, though it has since become very nearly necessary to have another Commission to set at rest the errors which have cropped up in its place. Similarly with Messrs. Gaskell and Shore, Mr. Leonard Hill does not scruple to quote their cross-circulation experiments with the usual heroics, while ignoring those performed in Hyderabad which contradict and overthrow them, but he admits that Gaskell and Shore are wrong in supposing that chloroform does not affect the vasomotor system. He utterly fails to see that this is a vital point, for, if vascular dilatation plays an important part in the fall of blood pressure with chloroform, Gaskell's and Shore's experiment, taken at its best, no longer proves that the fall of pressure is the result of paralysis of the heart. The Hyderabad Commission have always held that this was a mere dogmatic assertion on the part of Gaskell and Shore, introduced because they saw its necessity if their experiments were to prove anything, and we are glad to see that on this point they are not confirmed. It is very evident that Mr. Leonard Hill has either not attempted to repeat Gaskell's experiment, or if he has that he has failed to produce the same effect. It is much the same as regards MacWilliam; Mr. Leonard Hill quotes him with approval, but, as a matter of fact, there is no correspondence whatever between MacWilliam's gradual dilatation of the heart and his own sudden dilatation in the course of two or three inspirations. Finally, that Mr. Leonard Hill should quote any work of Ringer's, at any rate any that has been published, as proving with anything like exactitude that ether is fifty times more dangerous than chloroform, only shows that he is like the drowning man—ready to clutch at any straw that offers to support him in his erroneous notions.

What Mr. Leonard Hill means by stating that the Hyderabad Commission have given themselves away in the matter of the accidental deaths it is not possible to imagine. The accidental deaths which took place during the research of the Hyderabad Commission were published to show that there is no truth in the idea that there is something in the climate of India which prevents accidents from chloroform. The Hyderabad Commission never pretended to draw any scientific value from them as to the exact cause of the accidents, the circumstances under which they occurred making that impossible. Mr. Leonard Hill, on the other hand, just like the clinical accident-producing chloroformist, wants to persuade us that there was evidence of paralysis of the heart in similar cases which happened in his hands. This is exactly what the Hyderabad Commission are unable to see. There is not one tittle of evidence of any kind, still less of physiological proof, of paralysis of the heart in the cases he describes. His whole statement on the subject is purely a conception of what he imagines occurs in these cases, and has no scientific value whatsoever.

Mr. Leonard Hill admits that ether has the same action as chloroform, only that it is less intense, and yet he assumes that chloroform has some effect on the heart which is wanting in the case of ether, and he leaves his uninstructed audience to continue the fatal practice of injecting ether subcutaneously in cases of chloroform poisoning. That Mr. Hill should be able to restore the heart's activity by the process he describes of emptying the right heart into the abdomen, is itself the most convincing proof that the heart is not poisoned by the blood, which must still remain in the coronary arteries and in its muscular tissue. It is rather very strong evidence that the condition of the heart was produced by some cause outside it, most probably obstruction to the circulation in the lungs. As a practical method of restoring