extended study,⁹ but the observations are too scanty to admit of further consideration in detail.

Hypertrophy of Accessory Thyroid Glands.—It has been observed after complete thyroidectomy in man that other islands of thyroid tissue became enlarged, and that whereas temporary symptoms of cachexia (Reverdin) may appear, they improve in proportion apparently to the degree of swelling of the accessory thyroid glands. In one interesting case (Shattock), destruction of the thyroid by a neoplasm led apparently to hypertrophy of an accessory thyroid gland. Following closely on the evidence afforded by hypertrophy of the remaining thyroid tissue when part is lost is the remarkable series of experiments on transplantation of thyroid tissue commenced by Schiff, and so thoroughly elaborated by von Eisselsberg. Schiff found in the dog that transplantation (intra-peritoneally) of the healthy gland shortly before the operation of thyroidectomy was sufficient to protect the animals from the fatal cachexia, although they appear to have constantly suffered from the tetany. Carle found that transplantation a week after the thyroidectomy was inefficient to ward off the cachexia. This experiment repeated a few times by other investigators, however, did not yield the same positive result until von Eisselsberg renewed the research on a larger scale, and found that in cats, if the transplanted gland underwent atrophy and degeneration, the usual cachexia supervened. If, on the other hand, the gland became properly vascularised and resumed its functional activity, then the cachexia did not show itself. This very conclusive evidence suggested to me the advisability of introducing the practice of transplantation as a rational treatment of myxœdema and cretinism. I was not aware until some months later, by the subsequent publication of Bircher, that this had already been performed by him as treatment of cachexia strumipriva, and I afterwards learnt, through the kindness of Professor Kocher, that he had commenced it as long ago as 1883. In France it has been taken up, and operations by Lannelongue, Merklen and Walther, Bettencourt and Serrano have been performed in myxœdema and sporadic cretinism. The sum of all these operations is unquestionably a notable improvement in the patients and amelioration of the symptoms in those cases where the grafted gland survived.

Amount of Gland which in the Carnivora must be left for Functional Purposes.—In accordance with what has been said, it will be recognised that a valuable point in connection with this branch of the subject is the question as to how much of the gland is necessary to meet the needs of the organism. The statements of investigators (Colzi, von Eisselsberg, Fuhr, Sanguirico, with Canalis, Weil) are pretty close, and show that in dogs and cats there must be at least one-third, or in some cases one-fourth of the gland left to prevent the onset of the fatal cachexia. It follows from what I suggested above that the value of this remnant will be very different according to the constitutional state of the animal. Coupled directly with this point of residual portions of thyroid tissue being available for the metabolic necessities is the question whether gradual extinction of the gland (thus anticipating the effect of senility) could be accomplished without causing the fatal cachexia. Schiff first attempted this by successive extirpations at considerable intervals in dogs, but he found that as a rule the procedure was only occasionally survived. In his and all the various methods of gradual extinction of the thyroid gland, a remarkably constant feature appears, and that is the outbreak of many of the initial symptoms of the cachexia, that is, tetany, hebetude, etc. These are developed (Schiff, Colzi, Fuhr, Fano, with Zanda) in proportion to the destruction of the gland tissue, and disappear when the metabolic equilibrium is regained, often by hypertrophy of accessory tissue. Colzi and Fano (with Zanda) found, further, that transfusion of healthy blood caused these preliminary symptoms to diminish. Munk stated that if the gland were isolated, shelled out of its bed, but left *in situ* with the vessels ligatured and primary union secured, the animal would probably recover, but that if suppuration occurred, as seems to have happened in the large majority of his cases, death ensued. His experiment has been repeated, however, by several

observers—Weil, von Eisselsberg, Fano, Ewald—who state, on the contrary, that the effects are the same as thyroidectomy. Munk's own description, moreover, shows that where the nutrition of the gland substance and contents was completely destroyed by the operation, the cachexia inevitably followed, and the same conclusion is arrived at by the above-mentioned investigators, who have repeated his experiment, as well as by Tarchanoff, who ligatured all the vessels supplying the gland. A further point is raised by these experiments, namely, the temporary warding off of the unfavourable symptoms by the animal absorbing the secretion, yet remaining available in the residual gland structure. This was the view taken by Schiff of his transplantation experiments. A positive observation in its favour is the result of the experiments conducted by Vassale on nine dogs. This author found that if, immediately after thyroidectomy, the expressed juice and secretion of the glands were injected into the animal, the cachexia was thereby considerably diminished, that is, the venosity of the blood improved, as also the conjunctivitis, etc.¹⁰ Von Eisselsberg, however, saw no such improvement in cats similarly treated.

Evidence of Active Growth, that is, Regeneration of Thyroid Tissue when this has been in part removed.—Active growth or regeneration on the part of the thyroid tissue, if it existed, would be in support of the opinion that its integrity is a metabolic necessity. As a matter of fact, the true thyroid glandular tissue does show a strong tendency to regeneration, especially in the peripheral regions (*cf.* also Wölfler). Canalis, Ribbert, and Neumeister have found that in dogs and rabbits the removal of portions of the gland was followed in a few (two to three) days by, first, the appearance of mitotic figures in the epithelial cells of the nearest acini; and, secondly, the growth of the epithelium as cylinder masses, which subsequently formed new acini in the supporting tissue of the scar.

Summary.—To sum up the indirect facts, we see that they afford very weighty evidence in favour of the view that the thyroid is in truth the important origin of metabolic influence that the general results of thyroidectomy would lead us to believe it to be.

(To be continued.)

REMARKS ON GALL STONES AND ON

THEIR TREATMENT BY THE ADMINISTRATION
OF LARGE DOSES OF OLIVE OIL.

By JAMES F. GOODHART, M.D., F.R.C.P.,
Physician to Guy's Hospital.

THE uncertainties of medicine are so great that my sympathies have ever gone out to that candidate for a degree of whom it is told that his examiner, after appealing in vain to several lines of, as he hoped, dormant genius only to meet with disappointment, tossed him the corner of a blotting pad, with the scathing request that he would write down all he knew. In a sense we are not so far removed from that poor young man, for while we can most of us talk to any extent upon what we think, how few things are there that we really know! But a discourse upon ignorance in the abstract will not help us much, so I have selected the liver as our *locus niger*, and as our *locus nigerrimus* I will take gall stones.

We all know that gall stones produce a great deal of distress, not to say agony, that they lead to various serious changes in the liver, and in one way or another are by no means an infrequent cause of death. But it is not with points of this kind that I wish to deal at present. The question I wish to ask is a much more searching one, namely, what do we know about the why and the wherefore of the formation of gall stones, because clearly, unless we do know why and how they form, we are in no position to treat them to any good

⁹ I very greatly regret that the details of the splendid work by Professor Halsted and Professor Welch are not available for this report. I anticipate much will be learnt from their researches on this very question of "hypertrophy."

¹⁰ Compare the gratifying results obtained by Dr. G. Murray in treating myxœdema by this method. BRITISH MEDICAL JOURNAL, October 10th, 1891 p. 796.

purpose. Unfortunately, I think I am in the position to maintain that we do know almost nothing about them. We think we know that they occur in stout people of sedentary occupation and high living; that they are due to a sluggish liver and to a thickening of the bile; and that they are to be treated by a strictly regulated diet from which all sugars, starches, and fats are to be excluded, while the liver is to be stirred up to a better performance of its duties by regular exercise. In addition to this, if the patient be monied he is packed off to some foreign spa as Carlsbad. I lieve the Carlsbad physicians think very highly of their spa for such purposes; but spas seem to legitimately enjoy a licence, which, under cover of a pill-box, would be denominated quackery. I make bold to assert from the experience of no small number of cases that there is very little evidence indeed to show that a treatment that is undoubtedly useful for renal calculus is of any direct value in the prevention or solution of gall stones. I say direct treatment because, of course, there are incidental advantages to be derived from the dieting and flushing if the disease should happen to affect a fat unwieldy *bon vivant*. But incidental advantages of this kind are, I maintain, independent of any direct influence of the particular water for the time being in fashion upon the particular stone. A case that came under my notice a few weeks back will point some of the remarks I have already made, and start me on others I wish to make.

CASE I.—A gentleman, and a healthy-looking man, came to me tired out with repeated attacks of gall stones. He had dieted himself with much circumspection, and had paid the most scrupulous attention to all the orders of his medical adviser. He had also been to Carlsbad several times; and although he thought this did him good, he had had an attack whilst there, and he had had another since. He had seen all sorts of foreign medical men, and they had doubted if it were really a case of gall stones, but his own medical man had no doubt, and I do not see how there could be any, for his repeated attacks of jaundice surely put the matter beyond question, even with the qualification that I shall make presently. His pain had required the free use of morphine.

I may say that he came away much disgusted with Carlsbad; for having heard so much of what the waters could do in such cases, he was the more disappointed, though I do not know that that is a fair argument against the water, for even specifics are liable to play the individual false.

He was ready to go anywhere, to do anything, if only he could be rid of his trouble; but I told him that there was not a water in existence that could promise him any relief. I then went very carefully into his history, and found that he dated all his ailment to the happening of a great domestic sorrow. He had never had an attack before then; they began within a short time after. He was not by any means a plethoric man, nor was he, although in good circumstances, by any means a high liver. It is true that his medical adviser, adopting, as I think, the prevailing view, that errors of diet of some sort are the cause of the disease, considered that he was not sufficiently careful in his meats, but he had lived for many months on the most monotonous diet without any benefit.

I spoke to him thus: "We don't know how gall stones come, but I am convinced, from a full inquiry into many cases, that in some way or other mental worry and fatigue are often their immediate precursors." And as he said he must go somewhere, I sent him under protest to Luchon, partly because he had never seen the Pyrenees, and it is one of the most nerve-tranquillising spots in its quiet beauty that I could think of at the moment, and partly because I had a vague idea that the sulphur in the waters might, perhaps, in some way have a beneficial effect as an hepatic stimulant. But I did not push the waters. It was, "If you want to go, go; but I won't promise you any results."

But I did make a great point of the remainder of my advice, which was that he should take as much olive oil during the day as he could without making himself sick, from four to eight tablespoonfuls or more if possible. "Well," he said, "I can do a good deal of that sort of thing, for I am very fond of oil, but you doctors do differ so, for I have been told not to touch fats." What has become of him I do not know. Possibly getting no drugs, and a very dubious recommendation of certain waters, he left my door to turn into that of someone else, who would treat him better, or at least more in accordance with his ideas of betterment.

Now I want to say a word or two about these two points—the influence of mental worry in the production of gall stones and the effect of the olive oil treatment in the dissipation of their symptoms; and as regards both I am only speaking after some considerable period of observation.

As regards the first point—the influence of the nervous system upon the formation of gall stones—it has not been my experience that these concretions are specially liable to form in those of sedentary occupation, the obese, the high living. Quite the contrary. It is a disease of women more than of men, and often of women of quite moderate, not to say spare, nutrition, and I have seen gall stones over and over again in the very sparest. One of the worst cases of prolonged gall stone misery that I have ever seen was in the person of an old lady, the thinnest of the thin, but she was a neurotic of the most extreme type. Another case that I remember in which gall stones formed unusually early in life

was a young married woman of about 22; she was decidedly stout, but she had been by no means inactive, and she had had for some years a life of much mental worry and anxiety. I could give you several other cases to illustrate the same point if it were worth while to take up your time. What the intimate pathology of this association may be I can hardly pretend to suggest; there may be none other than a coincident one; but it seems likely enough that the lessening of the nervous flow which must result from nerve exhaustion may so retard the function of the abdominal viscera as in a true, albeit a vaguely-apprehended, manner to render the liver sluggish, and the various constituents of the bile would be then improperly formed, and some—as, for instance, the cholesterin—might become disproportionate in amount, or thrown out of solution. I think that no explanation that does not actually supply the demonstration why the cholesterin crystallises out could well be more probable.

But we are so crazy on diet that whenever anything goes wrong with our abdominal organs it must be due to something we have put inside us from our food, and which needs to be banished from our *menu* for the remainder of our natural lives. Such has certainly been the case with the treatment of gall stones, and it is pitiable to think what some of these people are reduced to when we come to consider how little real evidence the advice is based upon, and that the patient is certainly not cured. I venture to suggest that errors of commission of this kind as regards diet have very little indeed to do with the production of gall stones, and that their occurrence may be anticipated under any diet in a certain proportion of neurotics, and we may hope that a forecast of this kind may do something, if indirectly, towards their prevention.

This introduces us to my second point—the treatment of gall stones. It is now many months ago since I read the suggestion—made I forget now by whom—that olive oil in large doses was a good remedy for gall stones. The *rationale* of its action was said to be that by its administration fatty acids were allowed to form, and thus the cholesterin, which forms so large a part of these concretions, was kept in solution, and possibly also was redissolved again after having been precipitated. All this seems reasonable, and in the last twelve or eighteen months I have recommended it frequently on these hypothetical grounds, and of such of the cases as have come more immediately under my own notice I will now give a short note.

CASE II.—A man of 28 years was admitted, under my care, into Guy's Hospital on January 28th 1891, for jaundice and pain in the abdomen. He had always been liable to bilious attacks, and fifteen months before he had a series of attacks of pain in the right side of the abdomen, which used also to run over to the left side, and into the back. They were not bad enough to send him to bed, and they passed off leaving him well; they were unaccompanied by any noticeable jaundice. In January, 1890, he got influenza, and noticed then that his motions became white, and thinks he got a little yellow too, though he generally noticed that when he had his bilious attacks he got a little yellow. These attacks consisted of headache and sickness, relieved by blue pill and black draught. Five weeks before admission he began to be troubled with the old pain, which gradually became more intense, and ultimately his medical man was compelled to give him an injection of morphine under the skin. At the same time as the pain came on he became jaundiced, and had remained so. He was seen by two medical men, who both thought the case gall stones, although none had been found in the evacuations. He had had no recurrence of pain since the injection of morphine.

He had well-marked jaundice on his admission, the conjunctivæ being very yellow as well as the mucous membranes. On palpation of the abdomen he kept the upper belly of the right rectus very rigid; that part was tender; and an ill-defined fulness, with a rather knotty feeling, was detected. The urine contained bile. He was put upon olive oil treatment, having at first an ounce and a-half; after two days this was increased to two ounces three times a day. This he continued till he left us. For the first two or three days the jaundice rather increased in intensity, but on the eleventh day it was noted to be disappearing; he took his food better, and the indigestion did not trouble him so much. He also, within this period, began to pass motions with more colour in them, but in this respect he had a relapse after three or four days, and they again became quite white. On the twenty-sixth day after admission it was noted that he was still jaundiced, but he then began to pass olive green faeces; two days later they became of a yellow tinge, and in two days more he left the hospital, passing plenty of bile in his evacuations; all the tenderness over the liver and evidences of enlargement had disappeared.

CASE III.—A woman, aged 38, was admitted to Guy's Hospital on March 1st, 1890. Five years before she had suffered first from acute pain in the right hypochondrium and had at the same time vomiting, jaundice, dark urine, and clay-coloured evacuations. This attack lasted on and off three months. No gall stones were found, although carefully searched for. Twelve months later another attack, and a severe one, came on and lasted fourteen days. Since then she had had slighter attacks at intervals, but no gall stones had ever been found. On January 18th she had

another sudden attack, with all the usual symptoms. After a fortnight she had a second, lasting fourteen hours, and since then less severe attacks, but at intervals of two or three days had had pain lasting two or three hours. She had been in bed all the time, and had wasted rapidly. She was admitted jaundiced, very weak, constantly sick, bringing up bile-stained mucus. Nothing abnormal could be felt on examination of the abdomen, but as the whole history of the case made it almost certainly one of gall stones, she was ordered a pill of half a grain of opium, and a quarter of a grain of belladonna to relieve the pain, and she was put at once upon two ounces of olive oil every six hours flavoured by two drops of oil of cinnamon and a little syrup. This made her feel at first rather sick, but she kept it down. She had no further return of pain, and eighteen days after her admission bile began to appear in her evacuations, and she left well after a month in the hospital. This patient took eight ounces of olive oil daily for twenty-three days, and although the faecal evacuations were repeatedly examined, on only one occasion was there any appearance of fat in them. On this once there were small white or yellowish masses.

CASE IV.—This case I was consulted about, but I have not seen, so I give it in the words of my friend Dr. Boothroyd: A poor woman had had several attacks of gall stones in a few months. I do not think that she had passed the stone that had troubled her; it seemed every now and again to give her pain for a little time and then retreat. Her last attack, about a fortnight before, was followed by deep jaundice which however had almost cleared up. There had never been absolutely white stools. Dr. Boothroyd was inclined to send her into the hospital to have the gall bladder explored, for although the pain had passed off, there remained as a persistent condition some tenderness about the gall bladder. This was on August 6th, and I wrote asking him to try the olive oil treatment, and to let me know the result. He wrote on September 14th that the patient had taken six ounces of oil every day for five weeks most conscientiously, with the result that her general nutrition and comfort showed great improvement. He had seen her a day or two before, after a fortnight's interval, and was much struck by her improved condition. She had been able to discover no stones in her evacuations.

CASE V.—A woman, aged 53, had had a good deal of mental trouble for the year preceding her illness. Two months before I saw her she had been taken suddenly with violent pain in the right side and vomiting; and on examining her, Dr. Dixey had found an enormous liver and gall bladder. Jaundice supervened three days afterwards. In the next two months she had repeated attacks, and she lost fourteen pounds in weight. She was decidedly jaundiced, and had a large cystic swelling on the right side of the abdomen, which I had no doubt was a largely-distended gall bladder. There was also some enlargement of the liver. Opium and belladonna were given to allay the pain, and she was directed to swallow as much olive oil in the course of the day as she could to "help to dissolve the stone." I did not see her again until six weeks had passed. She had taken the oil, and at first as much as an ounce and a half three times a day; latterly she had not taken quite so much. I could not say that she was any better, for she had never gone longer than a week without an attack, and she had lost six pounds in weight. In another six weeks she had lost another four pounds, and, considering her age, I began to think about cancer, and talk about an exploration, more particularly because she had had some very severe attacks of colic, and one even so late as the day before I saw her. There was still great enlargement of the gall bladder, and I may add that the swelling was both on this and on a former occasion so freely movable and so easily manipulable between the two hands and went so much into the right loin, that had I not had the case under observation at former times I think I should have said that it was a movable kidney, with some enlargement of the liver superadded. I told her still to persevere with her treatment. She had been taking of late some sulphur to keep the bowels open, and possibly to act upon her liver as well; she was having some chloride of ammonium, and taking as much oil as she could. Three months later she came to show herself. She had been quite well since her last visit. Her jaundice had gone, and so had all the abdominal swelling, and she had gained twelve pounds in weight. This was just a year ago, and I hear that she still continues well.

With reference to the results first, I wish to say that it is obvious that I cannot claim for these cases anything more than a suspicion in favour of the value of the administration of oil. In no one of the cases have gall stones been proved to be passed, and in none of the cases has the improvement been so immediate that effect and cause certainly go together. There can be no doubt that the cases were cases of gall stones, and the mere fact that none have been found in the evacuations weighs very little in my opinion against the clinical symptoms. The examination of stools for concretions of this sort is not a pleasant job, and is seldom done with that thoroughness which would make it of real value as a test. And then, further, there is the nature of these concretions to be considered; an organic substance like cholesterolin is a very different thing to deal with, to the oxalates, the phosphates, the urates of renal calculi. And if we can suppose that gall stones are capable of solution, it is probable that they are so with a correspondingly greater readiness.

One could not, however, expect that their solution is an immediate affair, for it is impossible in the human body to apply a solvent with such readiness and in such quantity as to act except imperceptibly, since it has to filter through all sorts of channels and undergo all sorts of changes. These cases did well. The only question is, Did they do well as the result of patience and of Nature's perfect work, or did they owe some of the success to the administration of olive oil?

So far as they are concerned this must be an open question; but inasmuch as others have recommended the use of oil, and have thought it acted beneficially, I think I may add these also as helping to corroborate its value.

Difficulties.—One might have supposed that there would be some difficulty in getting people to take any considerable quantity of olive oil. And, as a matter of fact, it does make some feel sick, and in one case it caused diarrhoea, but in none have I found any insuperable difficulty, and this statement applies to several others besides those reported, but of which I have no notes complete enough to allow of their insertion. In hospital I have slightly emulsified the oil and it has been taken as a dose, but in the better class people I tell them to take it with mashed potato, or spinach, or salad, or even with some kinds of fish; and, indeed, there are several ways of taking a fair quantity of oil with food without in any way disgusting the patient.

Digestibility.—This is a very important point. In hospital patients I have repeatedly examined the evacuations, and have been surprised to find that there was no appreciable excess of fat in the stools. Once there were little opaque masses of fat, but in none of the others could I say that any excess existed. In other words, the fat that was administered was digested. There is another observation that may well be added to this namely—it is within my own knowledge and that of others—that an injection of olive oil is sometimes given by the bowel for eliminative purposes, and none of it returns. I have never subjected the evacuations of patients taking oil to any rigorous chemical tests, and it is probable that had I done so some excess of fat would have been found in them. But I think I may admit this, and still contend that a great deal must also have been digested and absorbed; and if so there opens up a subject for thought in the physiological management of health and disease, which I believe to be of very wide bearing indeed.

It seems to me that our knowledge of the uses of fat in the animal economy is still but rudimentary. Many of us are ready to cut off fat from the diet on very little provocation. If anyone comes to us after middle age, after young adult age even in many cases, complaining of symptoms of indigestion, of any of the teeming varieties of gout—the hydra-headed—with any threatening of the oncoming of a condition of undue obesity, off go the starches, the sugars, the fats of the diet. But I wonder if fat is ever deposited as fat. I doubt it. There is a difference between the things that go to make fat and fat itself, and the pig that became entombed in the Dover cliffs,¹ and went in a fat one and came out lean, did a great service to physiology, for it not only showed that animals could fast for far longer periods than had before that been supposed to be possible, but it showed the far more important physiological fact that by the means of fat nitrogenous products can be utilised for sustaining life which could not otherwise be. We know that fat, as fat, will not sustain life. No one can suppose that if the historic pig had gone into confinement lean that he would have come out alive. And the only reasonable conclusion, it seems to me, is that in the animal kiln the fat becomes split up and fed out to the albuminous compounds, which by this means are enabled to repair their waste, or at any rate allow of their consumption with far less rapidity than would otherwise be the case.

And as bearing on this question, let me say that I have long been hostile to the treatment of aneurysm by the Joliffe-Tufnell method strictly so-called, which, I need hardly remind you, consists of rest and starvation, and even, in selected cases, of bleeding. With the principle of rest which that plan inculcates I am, of course, in accord; with the diminution of the fluids of the body, by limiting the intake, I also agree; but that the patient should be starved and bled. I have always held to be unphysiological, and based on no reliable facts. On the contrary, I have always taught that the conditions that lead to undue coagulability of the blood, which is the condition the treatment aims at producing, and the results of which we see as pulmonary embolism and so on, are best seen in states of fever, in full-bloodedness, and

¹ Fat pig, buried 160 days; dug out at end of that time, reduced from 160 lbs. to 40 lbs. Prolongation of life attributed to retention of heat by non-conducting power of chalk and retention of moisture by saturation of air in vicinity.

conditions of rude health. And if I wanted to mention the most likely cause for the occurrence of pulmonary embolism, I should certainly say let the patient be a robust muscular man suddenly thrown upon his back with an accidental traumatic fever. It is not the emaciated subject whose blood clots, unless the emaciation be associated with pyrexia or sepsis. I was talking this subject over some years ago with my colleague, the late Dr. Wooldridge—alas! too early lost to fame—and he said, "Yes, you are right; but the best means of all of increasing the clotting power of the blood, short of injecting material into the circulation, is to feed the animal on fat." This was the result of Dr. Wooldridge's experiments on animals; and in many cases since then I have treated cases of aneurysm in this way, although I cannot say with any striking results; but I take it—and this is the purport of the introduction of the subject here—that if it can be shown that the clotting power of the blood is increased by a fat diet, a strong argument is afforded in favour of my contention that in some way or other fats are necessary to the due elaboration of the nitrogenous compounds of the body. Unless I am much mistaken, we do not think in that way, and, indeed, physiology seems to me to teach; such and such things for the formation and repair of the fabric, and such other things for storage for heat purposes; and that the two have no connection. I would maintain the opposite, and, if I may not say that the two forms of food are interchangeable, I am sure it is the outcome of clinical experience that the two are far more intimately necessary to each other than is usually thought. I mean to say that, supposing it were possible to feed a man on an exclusively nitrogenous diet, he would die, not for the want of fat as fat, but because of his inability in its absence to assimilate his stores of albumen.

Sometimes renal colic is very difficult to distinguish from hepatic colic when the right kidney is affected. I could tell of three, if not four, such cases; and one comes clearly to my mind of a gentleman who came all the way from the West Indies, for what was supposed to be gall stone, as he had had jaundice with the attacks of colic, but on weighing the history carefully I was convinced that the attacks were renal and not hepatic. He saw several physicians while he was in town, and no one was inclined to speak positively as to the nature of his calculus, and I heard no more of him for several months, when one day I received a note from him saying that he had gone abroad again; had passed a renal calculus a short time after he went out; and had been quite well since. There is perhaps no great improbability of a certain proportion of people suffering from both renal and hepatic calculi at the same time; but I also think that in connection with the disturbance, inflammatory or otherwise, that centres round a case of renal colic it sometimes happens that the liver becomes involved by its propinquity, or by its nervous supply becoming implicated in the storm, and that thus an attack of mild jaundice is the result.

Another point worth remark is the not infrequent association of fever with what is after all only simple obstruction of the ducts by gall stones or growth. I used to fear that the febrile state indicated the supervention of a suppurative form of inflammation. But I could give many cases to show that this is by no means the case, and although I do not mean to say that it does not introduce some little fresh anxiety into the case on that score, this need not be great, unless the fever and general symptoms be of a very pronounced character, for a sustained fever of moderate hectic type is quite capable of explanation by milder causes, and also by such as admit of complete resolution.

I suppose, too, I must say a word or two about the treatment of gall stones by cholecystotomy. My own experience has not been fortunate. I saw a lady some year or two ago who was suffering from this malady, and who, after I had seen her, was operated on, and a stone crushed, and she got well. But the operation was a very severe one, and she was ill some time. For the rest, one case, a woman, was operated on, the ducts were found to be in a state of suppuration, and she died after a short time. In another case, a man of middle age was admitted for chronic obstructive jaundice and a largely-distended gall bladder. Although there was no definite history of colic, gall stone seemed the most likely diagnosis. My colleague, Mr. Symonds, opened the gall bladder, but found no stone; and when, after some months, the patient died, after

another operation, undertaken to close the fistula then made, and which had made his life not worth the living, we found that the case was not one of gall stones now, whatever it might have been at the beginning, but a case of simple stricture of the common duct. Another somewhat similar case I had in a youngish man with insuperable jaundice; I gave large doses of olive oil without any result, except that we were enabled to verify in him, as we had done in the other cases, that the oil was apparently digested. He also had a continuous mild hectic fever, and rigors also. He was a married man with a family, and reminding me, as he did, in his history and in the absence of any definite colic, of the case I have just now related, I strongly advised him not to have anything done, and he left the hospital. I saw him again some months after, in just the same condition as when he left us, but he told me that since he left us he had been persuaded to be operated upon, with the result that no stone was found.

Some two or three years ago I advocated the use of pilocarpin as a sometimes very successful means of allaying the intolerable itching of chronic jaundice. It occurred to me that the extreme action upon the skin, evidenced by the copious perspiration induced by this drug, gave a hope of in some way modifying the peripheral nervous or vascular supply, and thus putting a stop to what must largely be due to one of these factors of the sensation. Since then I have tried it again several times, and have recommended it to others; and although it is not always successful, I think it is certainly one of the most valuable means we have of controlling itchings of this sort. It may be given either by the mouth or by hypodermic injection, but it is far more successful in the latter form.

ON THE IMPORTANCE OF THE EXAMINATION OF THE EYES SEPARATELY FOR DEFECTS OF COLOUR VISION.

By SIMEON SNELL,

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THE papers read at the last annual meeting of the Association and the discussion which followed have drawn attention afresh to the very important question of the condition of the sight and colour vision of railway employes. The subject, as has been well urged in an editorial article, is, if men engaged at sea be included, as important as any medical topic of the present day. The object of this short note is to direct attention to the importance of testing each eye separately for colour vision. The need of this in ascertaining the amount of sight for form is, of course, recognised, and always acted upon, not so, I think, as to colour testing. A recent case has suggested these remarks.

CASE I. Unilateral Green Colour-Blindness.—A lady brought her little girl, aged about 11, to me, and gave the following account. During the mother's absence on the Continent the child had run against a door-latch, and struck the lower eyelid of the left side. This was followed by some slight swelling, but not enough to have attracted much notice. On the mother's return, about a month after this occurrence, her attention was drawn to the fact that the little daughter on closing the right eye was unable to give the colours of flowers with which she was quite familiar. Mrs. D., a most intelligent lady, had in her own way before bringing the little patient to me tested her with wools, and had satisfied herself that the colour perception of the left eye was defective. She was anxious to know if the injury had occasioned it. I found, however, that vision was normal, and the appearances of the fundus gave no indications of anything abnormal; nor was the field of vision affected. The slight accident had occurred about six weeks before, and there was just a faint mark on the lower eyelid showing where the latch had struck her. I tested each eye separately with Holmgren's wools. With the left she sorted the wools as a green-blind person would do, with the right she arranged them correctly; then with both eyes open she again matched the colours in a normal manner. My testing, therefore, agreed with what the mother had herself found out.

In view of the absence of any other ocular disturbance, and the trivial nature of the injury, which appeared merely to have affected the eyelid, I was led to regard the defect as congenital. It is easy to understand what dangers would have attended a man similarly conditioned if he had been employed as a fireman or driver of an engine, or in any post on sea or land in which good colour vision was requisite. Any cause excluding the perfect eye, even for a brief period, would be attended with the possibility of disastrous results. Fon-