are operated by short handles, the degree of pressure exerted being controlled either by a catch system or a screw. The latter is shown here.

Tapes are attached to the distal extremities of these handles, and they can be carried round the neck and tied together. In order to make the fixation more complete a couple of short metal processes project from the other extremities of the central blades, and are also furnished with tapes. The other form differs only in the central blades being straight, and is not unlike a pair of nut-crackers. It would probably be more generally useful. With this little instrument in position the operator will have both hands free, and should not have to dig more than once at the gland. Aspiration will be easy, and if the first trial is not successful the gland is still there ready for number two.

I have had a rough model made in the Gordon College workshops, and, though not exactly like the instrument it is intended to represent, its use indicates that the latter would be likely to serve the purpose for which it is intended. I fear there may be parts of the neck where it cannot be used, but it could be employed in most situations, and I am inclined to think will fill a want.

2. Fly Traps.

Any one who has visited a sleeping sickness area, or indeed any one conversant with the recent literature on the subject, must, I think, have been driven to the conclusion that the crux of the whole matter, so far as prophylaxis is concerned, rests with the destruction of the tsetse fly. The fact that a special entomological commission is proceeding to Rhodesia for the express purpose of increasing our knowledge regarding the tsetse and its habits, shows that this fact is fully recognized. Hitherto efforts have more or less failed to deal effectively with the insect carrier. No natural enemy has been found which might be utilized on a large scale or which promises well; trapping by bird-lime and other sticky preparations has not done much good; tsetsefuges appear to be of little value, while in most places it seems hopeless to try to destroy the pupae on a large scale.

Not long ago I paid a visit to an area under cotton cultivation on the Blue Nile, and saw for the first time the ingenious "Andres-Maire" process for trapping insect pests and especially moths. It consists in using what are called "kiosk" traps, which are like large lanterns standing on legs. The sides are formed of metallic canvas, and thick canvas bands soaked in the liquid which attracts the insects to the traps are suspended in the interior. The moths are attracted by the strong smell of this special proprietary liquid, and flapping against the shutters find a narrow entrance between them and enter the trap. They remain on the bands all night, and in the morning when they want to fly away find they cannot do so owing to the way in which the trap is constructed. A basin filled with water and a little petroleum completes their discomfiture and forms their grave. These traps are said to be very successful, and it seems to me that something on the same lines might be tried in the case of tsetse flies. It should be easy enough to construct a trap into which a hungry tsetse fly could penetrate but out of which a gorged one could not get. As bait, blood might be used—preferably, perhaps, citrated—or fresh pieces of raw meat from cattle or antelope might serve the purpose, and would, I think, act as infallible lures. It should not be difficult, either, to invent a clockwork arrangement in connexion with a small petroleum tank at the top of the trap whereby once in the twenty-four hours—that is, at night—a spray of oil might descend on the occupants of the trap and slay them wholesale. Such traps would be more or less self-acting, while possibly the petroleum tank and spray might be dispensed with altogether and the flies left to the tender mercies of the native attendant responsible for fixing the bait or adding the blood, as presumably he would have to attend nightly to the traps. It would require no great intelligence to do so properly. A little experience would soon show the best way of operating them. They could be placed in the neighbourhood of villages, but sufficiently far away to prevent them merely serving to increase the visits of "fly" to the villages themselves. They could also be placed near watering places and at such other spots—

* Messrs. Andres, Maire, and Co. are an engineering firm in Alexandria, Egypt.

that is, places where native roads cross infected *khors*, etc.—as might seem desirable. I admit this is merely a suggestion, but so far as I can find out it has not hitherto been made, and it seems to me worthy of a trial, for, until we can destroy the tsetse on a large scale, we are, I believe, more or less helpless against the spread of this dread disease, and can only achieve partial results with great difficulty and at very great expense.

A Native Trap.—Since writing the above I have been given some interesting information by Mr. J. W. Sagar, one of the Government inspectors in Southern Kordofan, to whom I mentioned my suggestion about these traps. Formerly the district where Kawalib, a limited "fly" belt, is situated, was under his jurisdiction. The fly is *G. morsitans*, and the belt is quite peculiar. There is no water save that in the wells and what collects during the rains. The tsetse flies are not found near the latter. They haunt the area served by the wells, and are found only in the proximity of rocks and hedges. The belt is about twenty miles long and three or four in breadth. There is no evidence as regards the existence of the flies in any of the adjoining districts. This limited and peculiar distribution is said by the natives to be due to the fact that the fly was intentionally brought here from the river (presumably the Bahr-El-Ghazal) for purposes of revenge! This may or may not be true, and Major Morant's statement, quoted in our second report (pp. 29 and 30), says nothing about such an origin, but certain it is that at the present time the natives trap the fly in gourds containing blood as a bait and then liberate them in spots where the cattle or horses of their enemies are grazing or are collected together. The trap is a spherical gourd with a hole cut in the top. It is half filled with blood and carefully watched. As soon as a number of flies have entered it in quest of food, the native rushes forward and claps his hand over the aperture. He then closes the hole in some more permanent fashion, and carries off the flies in triumph for the future discomfiture of those with whom he has a feud. This native custom would certainly seem to indicate that a blood trap is a feasible method of dealing with one of the greatest pests from which Africa, the land of pests, has ever had to suffer.

THE NECESSITY FOR THE USE OF COLOUR NAMES IN A TEST FOR COLOUR BLINDNESS.

By F. W. EDRIDGE-GREEN, M.D., F.R.C.S.,

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(From the Institute of Physiology, University College.)

NOTHING, in my opinion, has retarded the knowledge of the subject of colour blindness more than the statement (attributed to Holmgren) that in any test for colour blindness names were not to be used. It was usual to state that on no account must the examinee be asked to name a colour; if any explanation were vouchsafed at all, the writer added the following, which he appeared to regard as final: "When a man matches a colour, he is doing something definite with his hands," or it was stated that a man might know the difference between colours quite well and yet name them wrongly.

I have never come across a man who was a candidate for an employment in which he had to distinguish between red and green lights, and who on examination called green "red" and red "green" through colour ignorance, and who was really able to distinguish between these colours and had a normal colour sense. I believe that I am right in stating that though the Board of Trade has regularly employed a test for colour ignorance, it has never yet found a case in which it has existed. All the tests for colour blindness which have been proposed during the last few years have been naming tests, and those who are best qualified to judge have recognized that the use of names is a necessity. There are, however, many who still adhere to the old statement that names should not be used, and I therefore propose in this article to analyse the difference between matching and naming colours, and to show that the use of colour names is a necessity, and that no test for colour blindness can be efficient which ignores them.

If an examinee be placed before a number of coloured

wools and, having been given a light green wool, be asked to pick out something similar, being told that there are none exactly alike, his mind may work in either of two ways. He may say to himself, "This is a light green wool; I will therefore pick out similar green wools lighter and darker than the test colour." In this case it will be noticed that he will go through the test in exactly the same way as if he had been first asked to name the test colour and had replied, green, and had then been told to pick out other wools with a similar hue of green. He may, however, be chiefly influenced by luminosity, and as no colour names have been mentioned, pick out all the wools of a similar degree of brightness, thereby failing in the test and appearing to be colour blind. When I first commenced the study of colour blindness one of our best known Royal Academicians, famous for his colouring, picked out all the confusion colours to match the test green. On further examination it was quite evident that his colour sense was supernormal, and he correctly and accurately named every one of the confusion colours, but he declared most emphatically that when colour names were not mentioned the similarity between two light colours was greater than two wools of the same hue but different degrees of brightness. I find that many other artists agree with him; in fact, most persons will regard a darker shade of some colour as an entirely different colour; for instance, if we take two yellows, each having the same dominant wave length, the bright one will be called yellow and the second brown, and the latter will not be put with the yellow at all. It is indeed difficult to convince most persons that brown is only a dark shade of yellow.

In a test for colour blindness a person is rejected who confuses or applies the same name to two colours which are plainly distinguished by the normal sighted as different. It should be clearly recognized that the test should show whether the examinee comprehends the nature of a colour—that is to say, that he picks out the colours by their character of greenness. The argument, therefore, that when a person is matching without naming he is doing something definite with his hands is fallacious, because when he is told to pick out green wools similar to that of the test green he is also doing something definite with his hands, only any opportunity of mistake through judging by luminosity instead of colour is avoided.

I have been lately ascertaining the percentages of colour blindness in men, and find that about 6 per cent. are definitely colour blind, whilst 25 per cent. have a diminished colour perception compared with the other 75 per cent. Now the 6 per cent. when examined with my lantern make mistakes between the red, green, and white lights, when they are clearly distinguished as different by the other 94 per cent., and any onlooker would say that they were rightly rejected, and it would not be safe for them to be in command of a ship in which it was necessary for them to distinguish between these three lights. Holmgren estimated the percentage of colour blindness as 3.25 per cent., and he considered that the least degree of colour blindness which could be detected should be a cause of rejection.

Nothing shows the value of colour names better than an examination with my lantern of an educated colour-blind man who has just passed Holmgren's test with the greatest ease. He miscalls red "green" and green "red," and applies either term to yellow, thus not only proving his colour blindness, but showing how absolutely unfit he is to act as an engine-driver or a look-out. Exactly the same applies to the improved Holmgren test, the official test of the Board of Trade, in which two new test colours similar to those used in my Classification Test have been added. Many colour-blind men will pick out the wools to match all five test colours as accurately and easily as a normal-sighted person, so that, after a long, tedious examination, I should not have suspected they were colour blind. On an examination with my lantern, however, lasting less than half a minute, red has been called green, nothing or white; white has been called red or green, and green has been called red or white.

It must be noted that this method of matching is different from that in which two colours are put in close juxtaposition and compared. When the latter method is used, and particularly when light of known wave length is employed, very accurate and important information may be obtained. That is to say, two colours which look quite

different to the normal sighted appear similar in every respect to the colour blind. When, however, a difference is observed the colour blind may make matches even more accurately than most normal sighted persons. For instance, it is stated that the ordinary dichromatic cannot make a match with Rayleigh's apparatus for matching sodium yellow with a mixture of lithium red with thallium green, $\lambda 670 + \lambda 535 = \lambda 589$. I have, however, found that many dichromics are able to make this match as accurately as a normal sighted person, and one dichromatic has, in taking a considerable number of observations, less mean deviation from the normal than any other person I have examined. There is also in matching another source of error when there is shortening of the red end of the spectrum. When the shortening is absolute, any number of rays from the shortened portion may be added to one of the factors of the match without altering its appearance, therefore numerous pinks of different luminosity and redness may be regarded as absolutely identical with a given blue, whilst another pink or purple which is clearly bluer to the normal sighted than the colours confused, is easily distinguished by the colour blind, because it contains red rays which are perceptible to him, and which occupy a higher point in the scale than his shortened portion. If the colour-blind be given a large number of coloured objects, they select colours which, whilst appearing different to the normal-sighted, appear absolutely identical in hue and shade to them. For instance, many dichromics will, when a skein of yellow-green, and one of orange be intertwined, remark that the two are absolutely identical in every respect. When there is shortening of the red end of the spectrum, a blue and a pink of a lighter shade will also be regarded as identical. Two colours which are different to the normal sighted and appear identical to the colour blind are called pseudo-isochromatic, and numerous pseudo-isochromatic tests have been invented. The examinee has to read a letter of one colour which is surrounded by the colour of confusion. These tests present very many practical difficulties, as will be comprehended from the remarks I have made on the composition of colours. For instance, a pink letter on a blue ground must only contain rays from the shortened portion, and a pink which appeared in every respect similar to the normal sighted, but contained rays of a shorter wave length, is easily distinguished. As cases of colour blindness differ so much in the degree of shortening and absorption, pseudo-isochromatic tables could not be constructed for all; though, when the tables are well constructed, they are useful for the commoner varieties. Naming the colours, however, is a much better test.

It might be thought that when two colours are regarded as identical, it is a matter of pure chance as to which name is given to the colour. Many of those who have now come round to my opinion that the use of colour names is a necessity, hold this opinion, and I wish, therefore, to show that this is not the case, and that the colour blind name colours according to their colour perception, and rarely guess. If they did guess they would know that they were colour blind, and those that are most difficult to test are those who are convinced that they are normal sighted. If, for instance, we take a blue and a pink, which are regarded as identical by a man with shortening of the red end of the spectrum, he will with the greatest assurance declare that both are blue. There is no reason why he should say otherwise, because he does not see the red component of the pink but only the blue. Not a single one of the very large number of people that I have examined of this kind has called the pink other than blue, purple, or violet. It is the same with the orange and yellow-green, both will be called by the dichromatic yellow. It is the same with other coloured objects, certain ideas of colour are associated with certain characteristics, and named accordingly. Naming is not haphazard with the colour blind any more than it is with the normal sighted. Another factor which makes naming more important than matching in a test for colour blindness is that simultaneous contrast is increased in the colour blind, and therefore, when two colours are compared in which a difference is perceptible, the difference becomes more marked on comparison. For instance, a trichromatic will say that a yellow light placed adjacent to a red one is green, whilst the same yellow placed adjacent to

a green will be called red; this shows how many of the colour blind are able to escape detection when methods of comparison are used. Again, why should a colour-blind person guess or attempt to guess that a red light is visible when he can see nothing, or call a light which appears to him definitely white, green. In cases where he is on the border line of uncertainty, he may guess, but does so in such a hesitating manner that his difficulty is quite obvious to the examiner. When, however, a red light is shown to him which has all the characteristics which he has associated with green, or a green light which has all the characteristics he has associated with red, he names the colour shown without doubt or hesitation. The colour names which I refer to in this paper are those which are clearly distinguished by the normal sighted—red, yellow, green, and blue—and with the use of one or more of these colour names with black and white, all colours can be sufficiently described.

Reports

ON

MEDICAL AND SURGICAL PRACTICE IN THE HOSPITALS AND ASYLUMS OF THE BRITISH EMPIRE.

TAUNTON AND SOMERSET HOSPITAL.

UTERINE FIBROID WITH CHRONIC SALPINGITIS.

(Under the care of Mr. L. H. C. BIRKBECK.)

[Reported by A. CLOUSTON RUSSELL, M.B., Ch.B., Senior House-Surgeon.]

THE following case is interesting, as it illustrates how few symptoms may be present notwithstanding extensive pathological change in the body.

E. M. A., single, a spare woman, aged 45, was admitted to the Taunton and Somerset Hospital on October 28th, 1911, complaining of slight enlargement of the abdomen.

She said she had enjoyed the best of health up to three weeks before admission, when she noticed a slight enlargement of the abdomen and began to worry about it; there were no other symptoms whatever. Menstruation was normal.

The abdomen was found to be slightly distended, due to the presence of fluid; it moved freely with respiration, and there was no tenderness anywhere. A small hard mass could be felt situated deeply on the right side of the pelvis. It was diagnosed as fibroid of the uterus. On vaginal examination extensive parametritis was found, the uterus being adherent both to the bladder and to the rectum. The heart beat was rather feeble but regular; there was slight sclerosis of the radial arteries. The pulse was 94 and the temperature 97.2°.

As there was some difficulty about consent to an operation she was simply kept in bed under observation. On November 2nd she suddenly cried out, and her face became ashen. She died in a few minutes, the attack having all the appearance of angina pectoris.

After her death her brother told me that for the past two years she had always slept best when propped up in bed; also that she had been a great walker, covering twenty miles one day within a fortnight of her admission to hospital; she had never complained of any cardiac symptoms. A *post-mortem* examination was made by Dr. Egan and myself, with the following results: The heart wall was flabby and thinned; there was slight sclerosis of the coronary arteries, but no valvular disease. About 1½ pints of ascitic fluid were found in the abdomen. There were small reddish deposits scattered all over the parietal peritoneum, mesentery, intestines, and uterus; slides of these were obtained from the Clinical Research Association, and proved to be of a chronic inflammatory nature; no evidence of tuberculosis could be found. The uterus and appendages, with the bladder and rectum, were bound together in one mass. A fibroid projected from the fundus of the uterus on the right side. The right Fallopian tube was greatly thickened, evidently from chronic inflammation, and widened out into a hydrosalpinx near its fimbriated extremity; the ovary on that side was entirely transformed into a blood cyst the size of a hen's egg. The left tube was chronically thickened and full of caseous material breaking down into pus; a somewhat larger

blood cyst represented the left ovary. Both tubes were ulcerated on their external surface, while deposits were numerous upon them and upon the ovaries.

Memoranda:

MEDICAL, SURGICAL, OBSTETRICAL.

FRACTURE IN UTERO.

ON May 13th I was sent for to attend a primipara. I found a footling presentation and the cord prolapsed. After prolonged labour a male child was delivered dead. The right leg was bent at almost right angles half way down the shin. The tibia had been fractured and united. Bony union seems to have been good as the bone was quite firm. Over the fracture was a well healed cicatrix of about 1 in. in length. The four smaller toes on the same foot were united into one. There were no other developmental abnormalities. The evidence of fracture and cicatrix was so plain that I cannot think of any other explanation, especially as there was a confirmatory history of the mother having fallen against the edge of a large tub when six and a half months pregnant.

Lisburn.

J. L. RENTOUL, M.B.

WIRING THE PATELLA TWICE IN EIGHT WEEKS.

THE case of rewiring the same patella twice in eight months, reported by Mr. Thos. North in the *BRITISH MEDICAL JOURNAL* for May 25th, and the evident rareness of the incident, make me wish to record a somewhat similar case.

J. S., aged 21 years on the day of the accident, injured his left leg while playing football on December 9th, 1911. I saw him at his home some time after the accident, and found him suffering from a transverse fracture of the left patella, with separation of about ¾ in.; the lower fragment felt very small. As I was unable to persuade his people to have the fracture wired, he was put up in a back splint, with over-extension of the leg, and the fragments approximated by a figure of 8 bandage. On December 17th permission was got to operate; he was removed to the Royal Albert Edward Infirmary, Wigan, and the operation was performed on December 20th. After clearing out some clots the pieces were wired, the lower piece being very small indeed, and the capsule sutured. He did very well, and left the infirmary a fortnight later, wearing a plaster-of-Paris case. About the last week in January the case was removed; union seemed to be good; there was a fair amount of flexion, and he was allowed to go about with a stick.

Early in February, while out walking after a sharp fall of snow, he slipped, and in attempting to recover himself felt the knee give way. He was removed at once to the infirmary, and on February 19th the knee-joint was again opened, the former scar being dissected out. The wires had torn clean through the lower fragment, and this was so injured that it was found impossible to pass further sutures through it; a thick silver wire was therefore passed through the ligamentum patellae below and the muscle above, and brought out at the outer aspect of the joint. This enabled the two fragments to be brought fairly well together, and by carefully tightening the wire the position was maintained.

He was sent home a month later with a back splint and footpiece on, and in April I put on another plaster case, which he still wears. We were much interested as to the condition of the bone on reopening the joint at an interval of eight weeks from the first operation, and were much surprised to find, so far as we could see, no attempt at union, either bony or cartilaginous—in fact, the fragments seem to have been kept together solely by the wire sutures, and yet the man was young, healthy, and vigorous. Several of the members of the staff were present at the second operation, and all agreed that there seemed no sign of attempted repair.

The fractures, as in Mr. North's case, were both the result of indirect violence, and I have been unable to find any record of a similar occurrence in any medical literature to which I have had access.

JOHN BLAIR, M.D.,

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