

surface is exposed to a severe chill, is it unreasonable to imagine that the opposite extremes of temperature to which the blood alternately becomes exposed, in the skin and the internal organs respectively, may have a somewhat similar effect to that which is produced by such means, on blood outside the body? In a paper by Dr. Bristowe and myself, published in THE LANCET of Aug. 10th and 17th, 1889, we endeavoured to show that such was really the case, basing our opinion mainly on the fact that the characteristic urine may only appear once, that voided both before and after the paroxysm being perfectly normal; the change in the urine, however, being preceded for sometimes half an hour, or even more, by an enormous destruction of red corpuscles in the circulating blood. This, it seems to me, could hardly be the case if the destruction took place in the glomeruli of the kidney, as then by catheterising the bladder in the manner we adopted a certain amount of pigmented urine should have been obtained, at least, as soon as any great alteration in the number of the corpuscles was to be detected in the blood. Dr. Hood says nothing abnormal was noticed in the blood in his case, but he apparently did not examine it with the hæmocytometer. In a recent case I have obtained even more convincing evidence on this point. A patient before experimental exposure to cold had a blister applied to the lumbar region at such a time that serum commenced to collect just before the initial rigor came on. His bladder was evacuated and the urine found to be normal, but serum taken immediately after from the blister, when examined with the spectroscope, showed well-marked bands of oxy-hæmoglobin. The urine was subsequently removed every quarter of an hour, and it was not until the third attempt that it was found to contain hæmoglobin. After a comparatively slight exposure to cold we obtained no pigment in the urine, although there was distinct evidence of destruction of corpuscles, but a proteid only, which consisted for the most part of a globulin, the liver apparently being able to deal with a certain amount of free hæmoglobin, which would account for the temporary increase of bile described by Noel Paton. Ponfick, too, observed that whilst the injection of large quantities of blood caused hæmoglobinuria, the injection of small quantities caused no such elimination. Moreover, if the pigment in the urine were due to rupture of the glomeruli, there is surely no reason why the blood should be present in the "laky" form, especially if extracted in considerable quantities at a time. With regard to Dr. Hood's difficulty in there being no intense discolouration of the skin, it must be remembered that, as he himself says, the blood pigment is got rid of within at most an hour or so by the united efforts of the liver and kidneys, whereas in jaundice bile pigment may be circulating in the blood for days or weeks.

Again, attacks experimentally produced certainly *do* show a relation between the area of chilled surface and the amount of specific urine subsequently passed—a man on whom some of my experiments were carried out having much severer attacks after a bath than after immersion of the hands merely in much colder water for a similar length of time.

I am, Sirs, yours obediently,

S. MONCKTON COPEMAN.

Physiological Laboratory, St. Thomas's Hospital, Oct. 6th, 1890.

* * It may be convenient here to correct two misprints which occurred in the 13th and 14th lines of the last paragraph of Dr. Donald Hood's paper published in our columns last week. The figures 7 and 9 respectively should have been printed with the decimal point: thus .7 and .9 — ED L.

RECURRENT APPENDICITIS.

To the Editors of THE LANCET.

SIRS,—An annotation in the last issue of THE LANCET, dealing with an article by Dr. Dennis on recurrent appendicitis, calls for some comment. Although I have not had an opportunity of reading the original article, yet from a recent conversation I have had with my friend Dr. Dennis, I am convinced that your annotation gives a very concise and accurate summary of his views. In matters involving new elements of surgical treatment there is not infrequently a disposition to pass into extremes. When mercury was first employed for syphilis, it was used with such indiscriminate vigour that patients suffered as much from poisoning by the drug as from poisoning by the disease. Then came a reaction, led by those who, with no compromise,

urged that mercury should never be administered in syphilis. The modern treatment represents the reasonable mean between the two excesses. The surgical treatment of recurrent typhlitis or appendicitis is passing through similar phases. In 1888 I ventured to propose that certain cases of recurrent typhlitis should be treated by removing the appendix at what I believed to be the safest and most convenient time—viz., during the quiescent period (Med. Chir. Trans., vol. lxxi) Since that date the appendix has been excised with quite reckless frequency, and from the writings of some surgeons it would appear as if the removal of the vermiform process constituted the sole mode of treating this common disease. Deaths have resulted from these meaningless and indiscriminate operations, and with Dr. Dennis' protest against this too-exuberant surgery I am in the fullest accord. Dr. Dennis, however, appears to me to incline somewhat to the other extreme, since he objects to the operation almost *in toto*, and is especially opposed to interference except during an inflammatory attack. Ovid's advice, *in medicotutissimus ibis*, may not be inappropriate here.

I have recently discussed the question so fully in opening the debate on the Surgical Treatment of Typhlitis at the Leeds meeting of the British Medical Association that I would now only emphasise certain points. The great majority of all cases of typhlitis get well without surgical treatment of any kind. There are cases of relapsing typhlitis which are not due to troubles in the appendix. On the other hand, in nearly every fatal case of so-called typhlitis the vermiform process has been found involved. The operation for removing the appendix is only called for in a few selected cases, and I should be within the mark in saying that out of ten patients I see with a view to operation, I do not operate in eight. No time is worse for excising the appendix than during the progress of an acute inflammation. One of the best substantiated of surgical axioms is that which opposes any but most pressing operations in an inflamed area. The operation for removing the process is often difficult and intricate, and can only be safely carried out during the quiescent period. If acute inflammation be present and an incision be called for, the less that is done in addition to the mere incision the better.

Dr. Dennis advises that the appendix when excised should be excised on the second or third day of an actual attack. Those who have experienced the difficulties of this operation, and who have been impressed with the terrible consequences which often follow the attempt to carry out a plastic operation in an acutely inflamed district, will shrink in dismay from Dr. Dennis' advice. It is true, as Dr. Dennis would urge, that the patient's next attack may be his last, and the operation, if carried out in the quiet period, may be uncalled for. This may be true in more senses than one. It may be the last attack and the man may live, or the last attack and the man may die. In most of the instances in which I have decided to operate the diseased appendix could be felt either per rectum or in the iliac fossa, and it could be established that its condition was such as to render a fresh outbreak probable. I have up to the present time had no death from this operation. In three cases I have abandoned the attempt to remove the process, having learnt that in some instances removal may be impossible.

I am, Sirs, yours faithfully,

London, Oct. 6th, 1890.

FREDERICK TREVES.

THE MEDICAL STUDENT'S "EDUCATION" IN MIDWIFERY.

To the Editors of THE LANCET.

SIRS,—The 18th resolution of the General Medical Council is a standing disgrace, and the sooner it is removed the better. It is as follows: "Every student should be required to attend for three months the in-door practice of a lying-in hospital; or to have been present at not less than twelve confinements, at least *three* of which he should have conducted personally under the direct supervision of a registered practitioner." Would the profession like the public to know that each doctor may secure a degree on such terms? Even now does the profession engage a newly qualified doctor to attend their wives' confinements? In 1887 Dr. Glover called the serious attention of the Medical Council to the fearfully loose condition of training in midwifery. However, if we remember that there are men in the profession determined to divorce midwifery and pharmacy