the contents of the bowels were pale, the gall-bladder empty, and the biliary ducts coated with a greyish mucus, notwithstanding which the skin was of a chalky paleness, and the urine contained no bile-pigment.* Similar observations have been made by Budd in cases of waxy disease and cancer of the liver,† and several instances of a like nature have come under my own notice. If bile-pigment be formed in the circulating blood, it is difficult to account for what becomes of it in these cases.

4. If the constituents of bile be formed in the blood, intense jaundice ought at once to follow the extirpation of the liver in any of the lower animals, in like manner as urea accumulates in the blood after removal of the kidneys. But, so far from this being the case, Müller, Kunde, Lehmann, and Moleschott have repeatedly extirpated the liver of frogs, and have invariably failed to find a trace of the biliary acids, or of the colouring matter of the bile, in the blood, the urine, or the muscular tissue.‡

The supposition that bile-pigment is formed in the blood appears to me for these reasons to be untenable.

[To be continued.]

ICHTHYOSIS LINGUÆ (SO-CALLED) FOLLOWED BY EPITHELIOMA.

By W. FAIRLIE CLARKE, M.A., F.R.C.S.,
Assistant-Surgeon to Charing Cross Hospital.

I AM sorry that Mr. Henry Morris had not an opportunity of speaking upon my paper, at the Royal Medical and Chirurgical Society, upon the so-called ichthyosis linguæ, for there were not many present who are better entitled to express an opinion upon the subject than he is. I will now endeavour briefly to reply to his comments.

1. Mr. Morris says truly, that I consider "the essential nature of ichthyosis linguæ to be a chronic inflammation, accompanied by an overgrowth of the papillæ"; but subsequently, when the disease has reached the stage of epithelioma, the rete mucosum, I add, "increases enormously at the expense of the papillæ, reducing them in many places to mere threads, and dipping down between them in the form of large club-shaped processes". In these statements he finds a contradiction and a *reductio ad absurdum*. "It would thus seem," he writes, "that, as the ichthyosis advances, the hypertrophied papillæ, which are its leading feature according to Mr. Clarke, must necessarily pass back to their normal life as they gradually dwindle down into a state of atrophy." The inference, then, is that there is a stage in the progress of the disease when part of its essential nature—viz., an overgrowth of the papillæ—no longer exists; or, in other words, that as the disease is growing worse and worse, it gradually recovers." But I do not see that there is any such inconsistency in my statements. The one refers to the ichthyosis, the other to the epithelioma. In ichthyosis linguæ, the papillæ are hypertrophied, but they retain their type; and in this condition they may remain for years. By and bye, however, the point is reached when the disease becomes epitheliomatous, and the rete develops in the manner I have described. The papillæ do not "gradually dwindle down into a state of atrophy", they do not "pass back to their normal size"; nor do I think that my language implies anything of the kind. They are actively invaded on all sides by the rete; their type is entirely lost; and they are very soon destroyed by the epithelial growth. The microscopical drawings and sections that I showed at the meeting demonstrated these points.

2. Mr. Morris criticises the features which I have said are distinctive of ichthyosis linguæ, and deprecates the name papilloma of the tongue, which I formerly suggested. I now readily admit that, though the disease often begins as a circumscribed wart, yet it has such a tendency to spread, that papilloma is not an appropriate name. As I said at the meeting, I think it would be best to adopt Dr. Ullmann's term, and to call it Tylosis Linguæ. From ichthyosis of the skin it differs, first, pathologically, because, in the former, the sweat-glands and the sebaceous follicles play an important part, while in the tongue there are no such glands or follicles; and, secondly, clinically, because ichthyosis of the skin, is (generally speaking) a congenital disease—a defect of development—and, moreover, has no proclivity to cancer; whereas the disease, which formed the subject of my paper, never occurs before puberty, and shows a strong tendency to become epitheliomatous. For these reasons, it appears to me to be undesirable to call it an ichthyosis.

3. Mr. Morris cannot agree with me in regarding the change in the

mucous membrane as one which is due to chronic inflammation, excited by irritation. Whether it ought to be classed among chronic inflammations is a point which is fairly open to a question. But I think I adduced enough evidence to warrant me in the opinion that the disease (whether a chronic inflammation or not) is due to local irritation. That cases occur in which we cannot trace the irritation, is not wonderful. When we consider how subtle are the nutritive processes, and how easily their balance may be disturbed, the wonder is, not that in many cases the cause escapes our observation, but that in a few we are able to assign it with a high degree of probability. Some warts in other parts of the body spring up from a recognisable irritation; others we know not how. Some cancers arise in consequence of a recognisable irritation; others we know not how. So with the disease in question. Some warty spots on the tongue are due to a recognisable irritation; others arise we know not how, but it is reasonable to suppose that there is some subtle irritation which has excited them.

Mansfield Street, Cavendish Square, March 18th, 1874.

THE COOLING OF THE BODY AFTER DEATH.

By J. WILKIE BURMAN, M.D.,
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THE subject of the cooling of the body after death, as indicated by the thermometer, though it has already been investigated by such careful observers and high authorities as Drs. Wilks, Taylor, and Goodhart, has not, it appears to me, been as yet sufficiently made the object of inductive research for its proper and full elucidation. It is a subject that has for some little time past engaged my attention, seeming to me to be of considerable interest from a practical point of view. The cases in which I have so far made observation have neither been sufficiently numerous nor varied to warrant my speaking at present authoritatively on the subject; but I think I may safely venture to corroborate those who affirm that the cooling of the body after death, as indicated by the thermometer, is a much more gradual process than the experience of our unaided tactile senses has hitherto led us to suppose it to be.

It is not an uncommon occurrence for medical officers of asylums to be called up at night to see a patient found dead by the night-watch at one of his visits; and the question as to how long the patient has been dead is of considerable medico-legal importance in such cases. I think I am right in asserting that the decision on this point of the medical officer has generally been, and continues for the most part to be, based on the feeling or degree of warmth or coldness as experienced by the mere application of the living hand to the corpse—a process which, as compared with thermometric observation, I have found to be more or less fallacious and productive of uncertain results.

I was induced to make *post mortem* thermometric observations in the hope that, by carefully studying thus accurately the cooling of the body after death, as it occurs in the various forms of disease and under the usual varying conditions in which the corpse may be found, I might with greater certainty and precision be able to fix the time at which death had taken place; and I intend continuing my observations, with this object in view. I have had a special thermometer made for the purpose, which indicates the temperature as low as 50 deg. Fahr.—the average surrounding temperature; and it will no doubt be necessary, for the thorough investigation of the subject, to have another made that will register the descending temperature down as far as zero. My method of procedure is to take the temperature of moribund persons, and then carefully note the temperature of the body and of the room at stated intervals (shorter at first, and longer later on), until it descends as low as that of the surrounding atmosphere; the thermometer being fixed from the first, and kept fixed, in the left axilla. In this way, having ascertained, in a sufficiently numerous and varied body of cases, the actual and usual rate of cooling of the *corpus* after death, it appears to me that all we should require to do in ordinary cases, on being called to inspect a dead body and determine as to the length of time that has elapsed since death took place, would be to take the temperature of the body as indicated by the careful application of a good thermometer; and, after estimating to its proper extent—as shown by previous experimental research and observation—the influence which the form of death and condition in which the body is found may be fairly considered to have had over the rate of cooling, be thus enabled to give a tolerably precise opinion in the matter, instead of making the usual vague statement. I am simply desirous, for the present, of indicating the scope of my humble researches with regard to this subject, and of drawing attention to a field for observation which, it appears to me, may be more extensively worked, with good practical results of very considerable medico-legal importance.

* *Op. cit.*, English Edition, vol. i, p. 83.

† *Op. cit.*, pp. 329, 411.

‡ Carpenter's *Human Physiology*, 7th edition, p. 434.