

the value of the F.R.C.S.I. Perhaps some of your readers might care to confirm or deny the accuracy of “Erinensis.”

I am, Sirs, yours faithfully,

Dublin, Sept. 7th, 1901.

S. WESLEY WILSON.

THE CONTAGION OF TUBERCULOSIS.

To the Editors of THE LANCET.

SIRS.—Anything which throws any light on the probable sources of infection or contagion of tuberculosis is just now of interest to the medical profession, hence my reasons for reporting the following circumstances. I was called about six weeks ago to attend a child, about 14 months old, whom I found to be suffering from tuberculous disease of the bowels: it had been quite healthy and strong up to the beginning of May, when it commenced to suffer from diarrhoea and to lose flesh. The parents thinking it due to teething did not call me in until they became alarmed at its general appearance. The parents are both strong and healthy, as well as are three other children, aged about six, four, and two and a half years respectively, and there is no history of phthisis in the family. The patient is a typical “marasmic child.” Quite by accident I discovered the following circumstances connected with the milk-supply. Last February a new tenant came into the farm where my patients have obtained their milk-supply for years and which is next door (about 200 yards away), whose wife, aged 36 years, has milked the cows and daily night and morning delivered milk in a separate vessel at my patients’ house; the separate vessel had been used as the house is not in the milk round; all the rest of the milk had been taken straight away to a large town for delivery. On visiting the house last Sunday I was told that the farmer was in trouble, as his wife, who had been ailing ever since they came, was in bed with phthisis and could not recover. Here is a member of a perfectly healthy family (as far as can be ascertained) developing tuberculosis after drinking milk which has been handled by a dairymaid suffering from tuberculosis. I state these facts simply for what they are worth and leave your readers to form their own opinion upon them, and if any of them are making any investigations on this subject and would like more details I shall be glad if they will communicate with me.

I am, Sirs, yours faithfully,

Victoria-park, Shipley, Sept. 3rd, 1901. D’ARCY B. CARTER.

“LEGISLATION AGAINST NATIONAL INTEMPERANCE.”

To the Editors of THE LANCET.

SIRS.—My note in *Nature* (August 8th) was intended to draw the attention of biologists, as such, to this question, not a purely medical one, which has been advocated somewhat too strenuously on one side by some of your correspondents. They have been driven back upon a time-worn biological line of defence, the “non-proven” doctrine of Weismann, and there “left fighting” in a somewhat languid manner. It seemed desirable that the *venue* were changed and some expression on the purely biological basis of this discussion were given. Hence any lack of controversial directness on my part. I hope I have not misunderstood the tenor of this correspondence when I ask—(1) Have there not been two contrary opinions maintained throughout as to the expediency of legislation against national intemperance, the opinion of one side being based upon the impossibility of effectual legislation? (2) Have not the two parties expressed or implied directly opposed views as to the import of alcoholism—one that it is a selective agent in the improvement of a stock of a nation by its weeding-out processes, and the other that alcoholism is only evil from the national point of view, and that continually, and largely because of its effect upon the offspring of drunkards? and (3) Has not the ultimate justification for disregarding the last point rested upon Weismann’s doctrine?

Dr. Reid asks me who stated that “alcoholism was a selective agent of value in the evolution of man which ought not to be interfered with by legislation,” and then tells us he stated that it *could* not be interfered with. May I ask him, has he not the courage of his own convictions, and will he not allow that, if it cannot be interfered with, it ought not

to be interfered with? And, further, does he not consider alcoholism a valuable selective agent? If he does, it does not follow that he holds the “atrocious doctrine” to which he refers, but certain other rather serious consequences are involved. Not having access to my copies of THE LANCET containing this correspondence I cannot do more than refer to my recollection that there have been “advocates of the view that alcoholism is a selective agent of value.” Dr. Reid may not be one of them. As to the technical point in “Hair on the digits of man” I did not challenge Dr. Reid to refute me; I referred him to it as the protagonist of the extreme Weismannian position, as I asked the biologists in *Nature*, to consider it on its merits. How comfortable it would be to be so sure, either way, as Dr. Reid is when he can say, “Doubtless man is the child of the monkey.” But we may at any rate assume that it is so for present purposes. The passage beginning “its descendants” and including the two following lines is difficult to follow and contains a mis-statement which perhaps is an oversight. The monkey has *not* worn down the hair on its ungual phalanges more than man, but a good deal less on these and all phalanges of foot and hand. I have never heard of anyone who has satisfactorily explained how it is that the man has lost most of his hairy covering and it has been looked upon as rather a *crux* for the evolutionist. But one may assume, for the sake of argument, that the monkey has retained his because it suited his requirements.

The only indication I can see of any refutation of my suggested Lamarckian interpretation of a particular fact is the allegation: “The disappearance of hair on the ungual phalanges is only part of a general phenomenon.” The inference is that, because “wearing-down” of hair on the phalanges is put forward as the only feasible interpretation for this area, one is bound to account for the disappearance of hair from the whole of man’s body except from the head, face, armpits, and pubes, in the same way. In passing, one may note that it was a little incautious in Dr. Reid to mention the only four regions of the body where “wearing down” does *not* occur and in which hair is abundant and persistent. But I will not claim this from him—he may deal with it as he likes. My modest little contention is only part of a great theory held by eminent biologists which Dr. Reid on August 10th called “wildly incredible” and now considers to be a “hypothesis so wildly improbable” and endeavours to alarm one at one’s own temerity in considering “that the wearing down of hair on the ungual phalanges so affects the germs situated in the far-distant testicle or ovum that the organisms which spring from them tend to reproduce the peculiarity the parent acquired.” It is no more deserving of scorn in this than in the greater cases seriously held as parts of a great whole which desires from selection no more than to “live and let live.” Much more has yet to be learned and said as to the bearing of hair-direction on this much-debated doctrine.

I am, Sirs, yours faithfully,

August 24th, 1901.

WALTER KIDD.

“HARVEST BUGS.”

To the Editors of THE LANCET.

SIRS.—I notice that some of your correspondents are interested in the matter of “harvest bugs,” and as I have had personal experience of their blandishments my account may be not without value. Some years ago in the island of Grenada, West Indies, and the month November, being neither “in the know” nor cautioned, I spent a forenoon writing letters, sitting at a table on the grass under a tree, and wearing trousers and thin socks, but no “pants.” The next day I was sorely troubled with itching all over the legs, particularly about the ankles; a few days later my whole body except face and hands was covered with minute pimples, or papules, which itched distressingly. This lasted for many days; I used to get a friend to rub me hard with the roughest towel procurable, and a considerable quantity of clear, probably serous, liquid came from the papules when the tops were rubbed off. I lost several nights’ sleep and was feverish, and in fact far from well. I found the insect on my body and was told that it was well known in the island and was called the *bête rouge*. It was also pointed out to me actually hanging in great clusters of red colour from the noses of mules. I took specimens and on my return home made preparations of them and then identified them as harvest bugs (*Trombidium autumnale*).