Of the total fifty cases, twenty improved, sixteen remained stationary, and fourteen became worse.

Six of the cases treated, in no instance for a less time than three months, and observed over a period of many months, retained their weight; were troubled by no symptoms, and maintained the improvement in their physical signs. Only one of these was observed over a less period than fifteen months, three over two years, and two over eighteen months. These cases may be considered, I think, as examples of the arrest of the disease, for the patients have returned to their ordinary avocations, and are able to earn their livelihood untroubled by their former disease. The only other remedies used in those cases were croton ointment, as a counter-irritant, Dover's powder occasionally to relieve the cough, and, in a few cases, a little linctus morphiae. But, as cod-liver oil was administered as well as ether, the objection may very naturally be made that the improvement was due to the use of the oil. For my own part this objection has not much value, for I had long used oil largely without obtaining any results at all equal to those stated. In order, however, to meet this objection the ether was stopped in ten instances and the oil continued; six times there was a fall in weight during the following three weeks. In one case the patient, who had taken ether for some two months, continued to gain weight. In the three other cases the gain ceased, but was again observed in these patients, as it was in all, when ether was again administered.

These results are not only instructive, as showing the effect of ether in increasing weight, but also as showing that the digestion of fatty food is restored to its normal state, in many cases, after a long course of ether. In the cases which have done best under treatment, that is, the cases which I think we may consider instances of arrested phthisis, the power of taking fats, which was at one time much impaired, has returned, and the gain in weight has been maintained over a period of many months. I may here also add that, in all the fifty cases there had been marked wasting before my treatment was adopted, in at least half the cases, cod-liver oil notwithstanding. Many of my patients have over and over again expressed their preference for the etherised oil; and many who have been unable to retain ordinary oil when taken, have been able to use etherised oil, not only without disagreeable symptoms, but almost with pleasure: "to drink it from the bottle", as some have expressed it. In advocating a new method of treatment, it is most satisfactory to be able to appeal to confirmatory evidence. It gives me great pleasure, in the present instance, to be able to call as a witness, in behalf of my views, the Secretary of this Section, Dr. E. L. Fox, to whom I mentioned the advantage of prescribing ether in phthisis in April 1867. I had the benefit of discussing the subject with Dr. Fox at that time; and he has since been so good as to send me the results which he obtained. In the early part of this year, he wrote to me as follows: "I fear I have kept no precise records of the oil and ether question; but since you mentioned it to me, I have used it very fairly. I give it in two sets of cases: 1, those who are tired of taking cod-liver oil; and, 2, those who have been taking it without gaining weight. In the first cases, I find it useful, as the ether, half an hour after the oil, seems to settle the stomach; and, I believe, induces the digestion of the oil. The nausea felt by so many patients is, I think, merely evidence of the oil not being digested. In one hospital case, the man took oil for fourteen days without any benefit; indeed, lost flesh. I continued the same dose of oil, and added the ether, and he began to gain three pounds a-week, and went on at the same ratio as long as he was in the wards. I think I have given it to about thirty patients since our talk in April, mostly in private patients of chronic phthisis, and in one case of great debility, in which I believed the pancreatic secretion was too little. In this case, the young lady had a great horror of all fat things, but took the oil with ether after it fairly well, and gained on it. It seems a plan generally liked by patients, and I am convinced of its utility." Such is the testimony of Dr. Fox, and I need hardly say how gratified I am to have the support of so able an observer. I might quote the testimony of others who have used the ether at my suggestion, were it necessary.

I may here remark, that I have used ether in the treatment of phthisis, not hoping to find a specific, for we have in these days ceased to search after specifics, but simply expecting to discover a rational means of combating one of the most serious symptoms of the malady, and restoring to its healthy state that perverted nutrition which forms so characteristic a feature in the great majority of consumptive cases. The peculiar character of this disorder of the digestive organs has been dwelt on, and the facts are, I think, sufficiently strong to show that in ether we have the best remedy for renewing the power of assimilating fat, whenever that power is diminished or lost.

Such, briefly told, are the results of my inquiry. I submit them to this meeting, in the hope that others may assist me in testing the truth of the facts advanced. Throughout, I have endeavoured to investigate the question with scrupulous care; the drug was selected only after a

laborious search, and when the evidence of its physiological action appeared to me conclusive. Starting from physiological facts, I have used the more simple science, physiology, to assist me in adding a new fact to the more complex subject—therapeutics; for it is in this way only that I believe medicine, a compound of sciences, can be advanced. First, must we learn the laws which regulate the normal before we can successfully analyse the laws which regulate the abnormal. In the selection of cases for experiment, I have also endeavoured to eliminate, as far as possible, all sources of fallacy, by selecting patients in the same condition of life, exposed very much to the same influences; and I have refused to accept any statement, as regards improvement, which has not been confirmed by the best test of all, in my opinion, the improvement of general nutrition. The cases, also, were, as far as could be determined, suffering from the same pathological conditions; they were all cases which may be termed chronic phthisis. In short, in all respects I have endeavoured to shut out from my inquiry all the accessory circumstances which might be sources of error; and I trust I have in this way avoided mistaking coincidence for the relation of cause and effect. The difficulties of avoiding all such sources of fallacy in a therapeutical inquiry are immense, and hence the slow progress which this part of medical science makes towards scientific form. If, in the present instance, my inquiry has been conducted with sufficient care to make it appear that I have indicated a new direction of fruitful research, my object will be accomplished, and I must leave it to the experience of the profession to decide on the value of ether in the combating one of the most subtle and certain approaches of our great enemy—death.

THE CRUCIAL TEST IN REGARD TO THE ORIGIN OF PHTHISIS AND TUBERCLE.

By HENRY MAC CORMAC, M.D., Belfast.

THE old alchemists were in the habit of impressing their bocal and their pots with a cross or *crux*—hence crucible—hoping thereby to propitiate the higher powers and realise favourable issues; and hence, also, the adjective "crucial", as applied to any test calculated to throw conclusive light on a given subject of inquiry. It is sometimes expedient to enter into special experimental inquiries, in order to realise the crucial test. At other times, we find the crucial test displayed, as it were, obviously before us, and needing only to be heeded, in order to be turned to avail.

I believe, and assume to show, that we have this crucial test at hand in respect of the production of tubercle. For, although tubercle may be expressly produced in the lower animals by forcing them to respire habitually air already respired, we find by the observation of facts—facts of every-day occurrence—facts lying before us—that tubercle is so induced in man and animals by reason of the habits, voluntary or constrained, which lead them habitually to respire air in part, at least, already breathed, instead of air no portion of which has been before respired.

My position is—the position which I take up—that tubercle is alone induced by the respiration of air in part already breathed; for air, all of which has been breathed before, is irrespirable, wholly, and, in fact, productive of immediate death. My averment is, that tubercle, with the diseases which tubercle is known to induce, is alone caused by air already breathed, in such wise that, if air already breathed in part be breathed habitually, tubercle is inevitable; and, secondly, that if no part of the air breathed have been breathed before, tubercle is impossible.

The notion of hereditary or inoculable tubercle I hold to be quite illusory. Tubercle is not inherited, though tubercle may be induced in the foetus *in utero* by the same causes which induce it in the mother. And, *à fortiori*, if tubercle be not hereditary, none of the diseases which tubercle induces—to wit, phthisis pulmonalis, tabes mesenterica, white swelling, scrofula, and hydrocephalus, so far as hydrocephalus depends on tubercle—are hereditary.

That this must be so is obvious, seeing that the production of tubercle is a chemico-vital process, and not producible otherwise. For what is tubercle? It is no other than the retrograde or effete carbon of degradation, the waste particles which, having discharged the purposes of life, and not being oxidised and converted into carbonic acid, are retained in the organism, and, with whatever foreign matters, water, fat, or cretaceous particles, oftentimes load the pulmonary and other tissues, to their destruction. If the effete carbon be burnt off, there is no tubercle possible. If the effete carbon be not burnt off, tubercle, sooner or later, is unavoidable.

When the natural atmosphere is but once respired, it becomes loaded with about 4 per cent. of carbonic acid, not to speak of water and certain other effete matters, the result of the oxidation of the waste carbon. This, to the extent mentioned, replaces the natural 21 per cent. of oxygen; and, though it might be supposed that air, as thus, contained enough remaining oxygen to supply respiration, it is found in reality to prove quite otherwise. Air loaded with 4 per cent. of carbonic acid, the result of respiration, being breathed, produces almost immediate death. We do not know the exact quantity of carbonic acid which, being habitually present, is adequate to induce tubercle; but I assume that from 1 to 2 per cent. is, in the main, amply sufficient. The presence of carbonic acid, in any degree in excess of the minute quantity supplied by the hand of nature, is hurtful and reprehensible. But the above proportion of carbonic acid—at any rate, the air containing it—being habitually respired, will suffice to occasion tubercle.

We cannot, and we would not if we could, subject human beings to the conditions productive, and, as I allege, alone productive, of tubercle. But human beings do so of their own accord, or constrained by circumstances; and we find, I aver, the results flowing from this use and wont just as conclusive, in respect of the production of tubercle, as if it were the result of an intentional experiment. It has, I conceive, all the force and relevancy of an *instantia crucis*, or designed crucial test or experiment; with this difference, that in the one case the result is undesigned, and in the other designed. One, then, inferentially, must be just as conclusive as the other; unless, indeed, we can venture to affirm that observation, *ceteris paribus*, or other conditions being equal, cannot compare with experiment. And yet, after all, what is experiment but tentative observation? In point of fact, experience and experiment, in essentials, are one and the same.

The habit of sleeping with closed doors and windows loads the sleeping-chamber atmosphere more or less with the products of respiration, and tends to induce tubercle in the inmates. If the day-life be spent in active open-air pursuits, the tendency to tubercular development may be so slow as not appreciably to affect the duration of life. It is quite otherwise, however, when the day and night life combine to occasion the habitual respiration of air more or less prerespired at all times. It is then that the immense preponderance of tubercle takes place, to the production of the ravages which more than decimate the population. Indeed, I have come to the conclusion that tubercular deposits subsist in half the population at the period of disease, if, as Rokitsansky affirms, they do not actually subsist, more or less, in all persons at death.

The period in which tubercle ensues—the individual being constrained to breathe his own aerial excretions, or those of other persons and his own combined—varies according to the intensity of the deterioration by night, and the habits, as more or less active, more or less open-air habits, by day. In some cases, tubercles will take long years ere they lead to destructive issues; in others, months will suffice to induce serious deposits, or to render deposits already laid down fatal.

All breathing animals are subject to the law, as here laid down, of tubercle-deposit. There is no immunity, the circumstances being otherwise productive of tubercle. The most powerful and vigorous constitution *per se* will yield no exemption, if prerespired air be only adequately respired. On the other hand, the most weak and debilitated person will not incur tubercle, if only the conditions already named as alone productive of tubercle do not subsist. No animal escapes, no climate lends safety, if air untainted by the products of respiration be not habitually breathed.

The averment that there are men and animals who can breathe, by night or by day, a tainted atmosphere—to wit, an atmosphere tainted by the products of respiration—and yet escape the deposit of tubercle, is perfectly untrue. Equally untrue is it that individuals, men and animals, have escaped tubercle, and yet have breathed habitually air loaded with the products of respiration. It is untrue that the Esquimaux or the Icelanders are exempt from tubercle. That this is so, I was informed in the one case by the late Mr. Simpson, surgeon to the *Plover* exploring ship; in the other, we are instructed by the writings of Schleisner, the Dane, to which I was once referred by a writer in the *Westminster Review* for the very opposite. The inhabitants of Iceland, indeed, otherwise, and occasionally mountain-bred sheep, display pulmonary cysticerci; but cysticerci are not tubercles.

Tubercles are very prevalent in Russia, as well as throughout all Europe. They are found in the warm climates of the East; they subsist all over the Americas; they are wide-spread in the Canadas, in the United States, and among the inhabitants of the West India islands. They are not wanting in Africa. They are found everywhere, that law of Nature being violated which prescribes that the same air, no part of it, shall be respired a second time. For, if pure air be respired habitually, tubercle cannot ensue; but, if the air be impure, in the sense of

being loaded with the products of respiration, and as thus respired habitually, tubercle must and does invariably form. It was observed in the diggings in Australia—at Bendigo, I believe, where eighty thousand persons were congregated together, persons who mainly slept and lived in the open air—that there was no consumption; whereas in the Australian towns, even, the malady was not absent. Sailors, soldiers, picked men, but sleeping in places loaded with the products of respiration, become tuberculous to an alarming extent—to an extent, indeed, far in excess of that subsisting amid the ordinary civil population, where tuberculosis is already only too prevalent. The French shepherds, who live all day long in the open air, are nevertheless tuberculous, by reason of sleeping in caravans or waggons, from which every breath of air is excluded sedulously. In New Zealand, the unwitting natives built houses to resemble those of Europeans; but, being constructed without any provision for the admission of air by night, the poor people are ravaged by phthisis accordingly. Nor does there seem any one willing or able to inform them of their error.

The Bedouins, who live all day in the open air, and sleep in tents where ventilation is provided for, are entirely exempt, it is said, from phthisis. The crowning instance, however, of perfect exemption from tubercle, is furnished by the inhabitants of Argyle and the Western Hebrides: here there is no consumption at all, and yet all the alleged influences ordinarily assumed to be productive of the malady are extant. Here, then, is a population extending through a number of islands, some with thousands of inhabitants, and along a line of coast of many miles in length, where there is not any consumption whatever, unless it may be a casual instance brought thither from some other quarter. The rest of Scotland is desolated by consumption, but not the Hebrides. The circumstance is vouched for by common report; by Dr. Martin, the Inspector-General for Scotland, if that be his title; and by Dr. Morgan, an English physician who lived seven years in Raasay, one of the Hebrides, and who published an interesting paper on the subject in the *Medico-Chirurgical and Foreign Review*. These fortunate Hebrideans live, in fact, in houses so constructed as to be permeated by a ceaseless interchange of air—that is to say, of the house-air with the outer air—by reason of having a constant hearth-fire and a hole in the roof. Thus it happens that the air is never, day or night, winter or summer, for a moment stationary; and thus, too, it is that the inhabitants never re-breathe, or possibly can re-breathe, their own pulmonary excretions. The effete carbon is constantly burnt off. There is no tubercle, and no consumption: consequently, the *instantia crucis* which I set out with, I submit, is furnished in its fullest force and integrity.

THERAPEUTICAL MEMORANDA.

[Under this head, we shall publish from time to time, as materials accumulate, short records of remarkable cases in practice which are sufficiently rare, interesting, or instructive, to deserve record, but do not call for lengthened statement or comment. Brevity and point should be the valuable characteristics of cases forwarded for this column.]

THE ADVANTAGE OF ADMINISTERING COLOCYNTH IN THE FORM OF TINCTURE.

By R. W. CRIGHTON, M.D. Edin, Leamington.

THE value of colocynth in cases of habitual constipation, and likewise as a revulsant in head-affection, has been long understood; but seems to have been not quite sufficiently appreciated in this country, from the drug having generally been used in the crude state.

The tincture, as employed in Prussia and admitted into the national Pharmacopœia, is a much more agreeable and convenient method of using it; the small dose required—ten to twenty minims—being easily combined in the same mixture with tincture of hyoscyamus, bromide of potassium, or other medicinal agents. Next to croton-oil, it seems to have the greatest effect among purgatives in diminishing hyperæmia within the cranium; and, in cases of threatened or actual cerebral hæmorrhage, its administration with tincture of hyoscyamus and bromide of potassium, I have frequently seen attended with the best results. In habitual constipation, ten, or even five, minims in a little water an hour before breakfast generally suffice to ensure a full evacuation, without much inconvenience, or the dose requiring to be increased.

The introduction of this preparation into the *British Pharmacopœia* would, I am convinced, be useful in extending the agency of the drug to many cases of disease in which its action would be beneficial, but where at present it is but little employed.