the organ does not subsequently waste, seeing that its main channel of blood has been closed?

The answer to these questions lies in the fact that the vessels which accompany the vas deferens, together with a few small vessels which are derived from the sub-vaginal cellular tissue, are sufficient to nourish the testicle. From these sources the testicle derives sufficient nutrition, over and above that which it gets from the spermatic artery, to prevent its necrosis. When I first began to perform this operation I was a little apprehensive that if the attempts at obtaining asepsis failed, the testicle would be in danger of gangrene, or at all events of very rapid wasting, but this fear would appear to be groundless, for a short time since a patient in whom I performed the operation removed his dressings in the night, profuse suppuration resulting. No bad effect upon the testicle followed; the patient recovered with a perfectly sound organ in spite of the strain thrown upon its diminished vascular supply. All fear of atrophy or gangrene on these grounds may, I am sure, be dismissed, although there are still some surgeons who hesitate to use the operation as they are fearful of its possible consequences.

Wasting of the testicle after operations for varicocele certainly should not occur. Occasionally, however, wasting happens, although I have never known a case occur in my own practice. I have no doubt that wasting of the testicle is more likely to occur if the veins only are dealt with in the operation than when the principal sources of arterial supply are obliterated with them. It is also certain that fatty degeneration of the tissue of the testicle which follows sometimes after operations for varicocele is more likely to occur when all the veins are removed and the full arterial supply left intact than when the arterial supply is proportionately diminished. This is what may reasonably be expected as I pointed out long ago,³ and it has been proved that it is so beyond doubt by some experiments on animals on animals which were carried out by Dr. Joseph Griffiths at Cambridge.⁴

Let us consider for a moment the reason of this result—for at first it hardly seems rational—the spermatic artery is at all times a rather large vessel in proportion to the work required of it. In varicocele the vessel is often larger than usual, and is, indeed, sometimes more than double its normal size.⁵ Given a large spermatic artery pumping a great quantity of blood into a delicate organ like the testicle, nearly all the efferent vessels of which have been removed, it is clear that there must be an enormous engorgement of the organ for the time being, which in some cases persists long enough to lead to the fatty degeneration which undoubtedly occurs in the testicle in some of these cases. On the other hand, if the main artery is obliterated, the blood carried by the artery of the vas deferens and others is sufficient to nourish the testicle without producing engorgement, since the remaining efferents are of sufficient capacity to secure an evenly-balanced return of blood from the organ.

Before concluding, allow me to say one word further with regard to the fatty degeneration of the testicle which sometimes occurs in these cases. If, after an operation for varicocele, the testicle visibly wastes, it is naturally regarded by the patient as a serious matter, since great importance is attributed to the shrinkage of such an organ—for although it may with some truth be said that a single testicle, if it be sound, is for practical purposes functionally as useful as the double organ, it is difficult to induce the ordinary patient to regard the matter from the purely physiological point of view. In the fatty degeneration of which I am now speaking the testicle does not waste—that is, it does not diminish in size; it merely becomes abnormally soft as compared with the opposite organ. I have seen one of these soft testicles which, after removal, consisted almost wholly of fat. Such organs are sterile—a fact of some importance, perhaps, if both were concerned in the change. The surest way of avoiding the risk of this form of degeneration is, I believe, as I have already said, to proportionately diminish the arterial supply when the veins are removed. I have never known any diminution in size or any softness follow the operation I have described, although my experience of it is large.

Such are a few points which are, I think, worthy of your attention in connection with this very common complaint;

they are not altogether without interest. It is remarkable how much there is sometimes to be found of interest in these common diseases or abnormalities which are very frequently regarded as unworthy of much consideration simply because they are so common.

NOTE AND REFERENCES.

¹ *Lancet*, February, 1889. ² *Varicocele, a Practical Treatise*. ³ *Ibid.*, p. 97; also *Lancet*, March 7th and 14th, 1891. ⁴ *Lancet*, vol. i, 1895, p. 91. ⁵ This abnormal size of the main arterial channels is not confined to varicocele, but occurs in other varicose conditions; see *Clinical Lectures on Varicose Veins*, p. 44.

AD TERRAM.

II.

THE EARTH AND THE SOIL.

By SIR FRANCIS SEYMOUR HADEN, F.R.C.S.

I HAD hoped, after the record which you were good enough to enable me to make, in the *BRITISH MEDICAL JOURNAL* of May 27th, 1899, of my views as to the competency of the earth, properly treated, to deal effectively and harmlessly with all forms of organic decay returning in the course of Nature to its surface, that I should not again have to trouble you on the subject; nor should I be doing so now if continued investigations had not satisfied me that we hold in our own hands a key to other equally great truths which, in our engrossing pursuit of the microbe, we have allowed to escape us—a key also which, if applied with a firmer faith in the ordinary operations of Nature than we seem just now disposed to accord them, I would fain hope may one day win us back to those larger methods of inductive research which we associate with the name of Bacon, and to which, indeed, we owe, directly or indirectly, nearly every great discovery that we have made our own since his time.

It is, however, not alone to regret our growing tendency to break away, on grounds which I must consider altogether speculative, from the surer methods of the Baconian philosophy, that I am again venturing to address you, but rather to invite the attention of the profession to the experiments we are making and the language we are employing to chronicle their results; experiments which, since we are making them not in the earth, but in the soil which covers the earth, do nothing to prove the latter the hotbed of infection which some would have us believe it to be, but rather suggests one of two things—either that we are failing to recognise the real nature of the medium we are employing in our investigations, or, which is even more likely, that we are taking for granted that the soil and the earth are one and the same thing, whereas, from the point of view of the present article, not only are they not the same thing, but in all but the equal and independent share which they have in the conversion of effete organic matter into its elements, two essentially different bodies; different in their origin, different in their nature, and different in the parts which they play in the world's economy.

Nor is there, I would respectfully submit, any room to doubt the prevalence of the additional oversight which we are now making, since evidence of that tendency confronts us at every turn: in our choice of the soil as a medium in which to make our experiments—a medium in which we may, if so minded, cultivate anything from a microbe to a mushroom; in the adoption of a terminology, fast degenerating into a vernacular, which is intelligible only to those of us who are content to live under the predominant dispensation of the microbe; in our lectures, our correspondence, our reports, and even in our textbooks, which one and all, so far as I can see, speak of the earth as a sort of composite body which they describe as “the soil,” or it may even be, when put to it for a pseudonym, as the “subsoil.” Thus we are in no way surprised when so accurate an observer as Professor Poore candidly tells us that when he speaks of “earth” he means “humus” (which is soil),¹ or when so practical and conscientious an experimentalist as Dr. Klein uses the term “earth” in the same sense as his equally careful fellow reporter, Dr. Houston, uses the word “soil,”² or, finally—

since it is useless to recapitulate examples of the same thing—when my courteous critic and mentor, Sir Charles Cameron, of Dublin, referring to “gravels and boulder clays” (which are earths), calls them “soils,” and even goes out of his way to emphasise the kindly lesson he is reading me by a formal record of his belief that “the cleaner the soil we live on the longer we shall live on it.”³ But the soil which I am speaking of is never clean, never has been clean since the globe became populated, and never will be clean till it reverts to the comparative sterility which at one time appears to have largely affected it.

The soil, in fact, according to experiments which have been now uninterruptedly carried on at Woodcote for fourteen years, and which, though still incomplete on one or two points, I consider fully warrant the crude results I am now giving of them, is “soil” in the most ordinary acceptation of the word—organic refuse—that is, animal as well as vegetable, which having lived its life and served its purpose on the outermost confines of the globe, has dropped out of existence, and, in obedience to a law which neither it, or any other material body, is able to resist, is already when we meet with it on its way towards its centre of gravity. Soil, in fact, according to my understanding of it, is *manure—the very medium in which with so much confidence we are making our experiments*—the one great fertilising agent without which the earth would starve, its populations die out, and the atmosphere itself, deprived as it would then be of its means of renewal, and consequently without a purpose, not impossibly also cease to exist.

The earth, on the other hand, properly so called from my point of view, is the very opposite of all this. While the soil is organic, the earth is inorganic. While the soil has its origin on the periphery of the globe, the earth has its origin in its own centre, and its earliest developments in the breaking up and gradual disintegration (by a variety of forces the nature of which being well understood I need not here dwell upon) of its own primitive rocks, till at last they are reduced to the friable matter which we know as “earth.” Again, as to the comparative age of the two bodies: while that of the soil can in no case exceed the age of the animal which produces it, or, in the case of vegetables, the duration of the seasonal intervals which mark the rotation of crops, the fall of the leaf, and so forth, the age of the earth is measurable only by the countless ages during which we know it to have been in the course of formation. And yet, again, separated as the two bodies originally are by a space as great as that which separates the poles from the equator, and approaching each other as, in a sense they may be said to do, from opposite ends of the earth, they have, and can have, nothing in common till by a provision of Nature (gravitation?) they are brought into actual contact—a contact differing in nothing, so far as I can see, from that of any two bodies which, being chemically incompatible, give rise on their admixture to a change of their elements, and it may even be to the creation of a new force—such a force, for instance, as has its analogue in the production of the electric spark by the contact of the opposite poles of the voltaic pile. For the interposition of the microbe in changes and metamorphoses of this kind I must confess I can see no necessity whatever, or, in fact, for that of any foreign body, saprophytic, bacterial, or otherwise cryptic and gratuitous.

The physical properties of the soil, again, its weight, its volume, and its extraordinary value as a fertilising agent will, from what has been said of its nature and origin, be readily understood to be measurable only by the whole extent of the earth’s surface (which it more or less entirely covers and overlies) and by the amount of organic life, animal as well as vegetable, which is its immediate source of supply. Then as to its weight and the pressure which it exercises on the earth’s surface (as to which I shall have more to say later), it is of course that of the atmosphere *plus* its own density, and the weight represented by the pressure which it exercises on the earth as the effect of gravitation. It is, however, its vast extent, its kind, and the irregularity of its distribution which for the purposes of this communication chiefly concern us.

Experiments made in the laboratory have, meanwhile, told us nothing on these points, and herein must be my apology for the noncomformity of my views with those of the eminent men who, taking the microbe for granted and making it the basis of their inquiries, have been generally received as

authorities on that part of the subject; the difference between us, however, depending I think solely on the fact that while their investigations have, for the most part, been founded on experiments made within doors, and on the strength of pre-arranged lines of inquiry, mine have been made wholly out of doors, and on lines suggested simply by what I have met with and seen there during a continuous succession of observations which have engaged much of my attention for twenty-five years, and pretty nearly the whole of it for this last fourteen.

So much for a theory in mere outline, and, as yet, with such proofs only of its stability as may reasonably be read between the lines of the present communication. Nothing will be easier at the proper time than to multiply such proofs, at all events to the extent of largely extending and strengthening the ground on which that assumed stability rests.

On the other hand, points have to be cleared up which, though by no means obscure, require time for their elucidation, and which make me unwilling to go to print with them till I have had that time at my disposal. A single example of such points will suffice to explain this indisposition on my part. Though I have long been aware that nitrification is a more rapid process than I once believed it to be, I had no idea till lately how much more rapid a process it really is; nor, though I have now no doubt of the fact, have I yet been able to arrive at anything like its exact measure.

Again, though I have been making my graves under various conditions as to depth and the contiguity of their contents with the earth enclosing them—beginning with a depth of 4 feet, as that prescribed by the Local Government Board, and ending by simply laying the body on the ground, and covering it with a single foot of earth—my examinations of the whole of these graves have only been made once a year, and the results of such examinations only recorded in print once a year—I now know that if I had examined them every half year or even oftener, I should have found, I will not say the same astonishing results, but evidence of a far more rapid progress towards a return of the body to its elements than I had then believed possible, as well as a more exact knowledge of the rate and kind of change undergone by the body in the interval.

The solution of such a moot point is, of course, of the highest practical importance, and I am led by it and by other considerations to the conclusion that, rather than present it in an imperfect form, I shall do well to let the present paper go to press as it is.

REFERENCES.

- ¹ Poore, Professor G. V., First Milroy Lecture, BRITISH MEDICAL JOURNAL, February 25th, 1899, p. 457. ² Klein, Dr., F.R.S., Report to Local Government Board, 1898-99, On the Fate of Pathogenic and other Infective Microbes in the Dead and Buried Body, pp. 344. and *passim*, 372, in Earth; Houston, A. C., M.B., D.Sc., Report to Local Government Board, 1898-99, on the Inoculation of the Soil with Particular Microbes, p. 473. ³ Cameron, Sir Charles, C.B., The Soil as a Nursery of Pathogenic Organisms, BRITISH MEDICAL JOURNAL, June 3rd, 1899, p. 1374.

(To be continued.)

THE MILROY LECTURES ON THE INFLUENCE OF THE DWELLING UPON HEALTH.

Delivered before the Royal College of Physicians of London.

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LECTURE I.

[THE particular aspect of the subject of “public health and housing” which the lecturer proposed to take as his subject was, he explained, the relation of the changing style of habitation to the health of the community, especially in regard to the “flat” and allied systems in the metropolis. He proceeded as follows:]

We are becoming more and more town bred, and the stamina, energy, and health of the population must be maintained. From time to time it has been said that the housing question is a simple one, and that the remedy merely requires that the necessary dwelling accommodation should be defined