sheet is to this extent incorrect and the income and expenditure account is incomplete; if these accounts are not intended to be interpreted according to the Uniform System, then the hospital committee are confessedly not observing the system and are making the hospital ineligible for a grant. There is thus nothing arbitrary in the King's Fund action; it is the automatic result of the free choice by the hospital committee of a course which inevitably produces this dilemma.

The Fund appreciates the courteous terms in which the chairman of the hospital refers to its previous relations with the Fund, but we must point out that the Fund has not interfered with the management of the hospital. The customary stipulation that there should be an inquiry officer can hardly be so described, and if it could it is not the point at issue. Even with regard to the method of publishing the accounts, the Fund is only concerned because the receipt of a grant would imply that the accounts were in the recognised form which enables the financial position and operations of one hospital to be compared with those of other hospitals. Unfortunately the accounts of the Evelina Hospital are not in this form.

We are, Sir, your obedient servants,

SAVILE CROSSLEY, } Honorary
JOHN G. GRIFFITHS, } Secretaries.

7, Walbrook, London, E.C., Feb. 8th, 1912.

M. BERGSON'S "CREATIVE EVOLUTION."

To the Editor of THE LANCET.

SIR,—Dr. R. F. Licorish has referred to me in his letter in THE LANCET of Feb. 10th on the above work, and I should like to be allowed to add a few observations. M. Bergson is not the first to deal with what I would call the "New Teleology." The first to raise the question of a "determining" power was James Croll in his essay, "What Determines Molecular Motion?—The Fundamental Problem of Life."¹ Darwin left it unanswered, but while his theory of natural selection only *assumes* the possibility of adaptive variations arising, what *causes* them is not suggested, beyond recognising some "direct action" of a changed environment. However, he more than once attributes them to the Creator.² Since, however, the existence of adaptive variations is (as he says) a mere "accident,"³ there being no natural law of cause and effect to account for adaptations or uses, Haeckel was justified in saying, "Darwin gave us the clue to the monistic explanation of organisation,"⁴ for any *directed* adaptation is incompatible with a non-directed accident.

As far as I know, the next attempt was in an article of my own on "Directivity" in the *Hibbert Journal*.⁵ This word was coined by the eminent chemist, Sir H. H. Church, upwards of half a century ago. He told me that he felt the want of a word to express the fact that as he could "direct" various inorganic substances in his laboratory, with which, by synthetic processes, he could produce certain so-called organic compounds as madder, indigo, certain sugars, urea, &c., what is it in plants and animals which can do the same thing as himself? So he invented the word "directivity."

Directivity, however, covers a much wider ground than organic products, for it applies to the *making of any and every structure* which has some definite purpose in view. In other words, it lies at the basis of evolution. Scientists limit themselves to all phenomena which come under the possibilities of inorganic matter and force. What transcends their activities falls under philosophy and natural theology. Directivity stands on the borderland of both, as being a purely abstract term. But if we ask what or who directs? the only scientific answer is "Life."

The activities of life are observable, just as those of electricity, heat, magnetism, and other forces are; but we know no more of the *nature of force* than we do that of *life*. Dr. A. Russel Wallace in his latest work, "The World of Life, a Manifestation of Creative Power, Directive Minds, and Ultimate Purpose," would endeavour to combine "directivity" with, or as an "extension" of, natural selection; but,

they are, in fact, mutually exclusive. He is right, however, when he says: "What we must assume is not merely a force, but some agency which can and does apply, direct, guide, and coördinate a great variety of forces so as to build up that infinitely complex machine, the living organism, which is not only self-repairing but self-renewing, self-multiplying, *self-adapting to its ever-changing environment*." These words which I have italicised remind me of the title to one of my volumes of the International Scientific Series—viz., "The Origin of Plant Structures by Self-adaptation to the Environment" (1895). Protoplasm has been called "Purposive Matter" as the cause of adaptations; but matter, being inert and lifeless, is powerless to do anything. It is the life infused in protoplasm which is the active agent throughout all the activities of organic beings. Similarly, the old term "Vital Force" is a misnomer, for no force is alive; but it is *Life* which *directs forces to move matter*, so that a purposeful, concrete, organic structure is the result.

The conclusion, therefore, is that directivity resides in life alone, and not in any force or kind of matter itself. Beyond that science cannot advance; but philosophy or natural theology takes up the question and returns to Paley, but with a change of the title. "Argument of Design" becomes "Self-adaptation" by response to changed conditions of the environment; the result is evolution under the directivity of life.

I am, Sir, yours faithfully,

Bournemouth, Feb. 11th, 1912.

GEORGE HENSLOW.

To the Editor of THE LANCET.

SIR,—Dr. R. F. Licorish, in THE LANCET of Feb. 10th, advances what professes to be an interpretation of Bergson's philosophy. His main point is that Bergson agrees with the Neo-Lamarckians in believing in the inheritance of acquired characters. Bergson condemns the Neo-Darwinian theory as being an inadequate explanation of evolution, but the one point on which he agrees with the Neo-Darwinian is that a tendency to the inheritance of acquired characters, if it ever occurs, can occur but rarely. This is one of the most definitely expressed opinions in his "Évolution Créatrice."

I disagree absolutely with Bergson's theory, but this is no reason for allowing a serious misrepresentation of it to pass unnoticed. Fundamentally Bergson's idea of the universe is no more than a modernised version of Paley's "Evidences." It is perfectly obvious that the most modern of the most modern branches of science fail to explain many important phenomena, among them the origin of life and consequently of man's intellectual capacities. Failing the explanation of such phenomena, Bergson, following in the footsteps of many others of at least equal reputation, assumes the existence of a metaphysical power which has directed and continues to direct evolution. Whether, in the absence of physical evidence with regard to the cause of a phenomenon, it is justifiable to assume the existence of a power beyond our understanding and incapable of proof, must remain a matter for each to decide for himself. Personally, I prefer to wait with an open mind in the hope that some further evidence may be forthcoming.

The fact that has struck me most forcibly in Bergson's writings is his total misunderstanding of the theory of evolution by natural selection. His misrepresentation of the belief of the Neo-Darwinian is almost as grotesque as that of the Neo-Lamarckian. And the curious thing is, that one of the most important *raisons d'être* of his theory as propounded by himself is that the Neo-Darwinian theory is inadequate.

I am, Sir, yours faithfully,

Glasgow, Feb. 11th, 1912.

CHARLES WALKER.

THE FATE OF THE APPENDIX AFTER ABSCESS FORMATION.

To the Editor of THE LANCET.

SIR,—I can fully endorse many of the points which Mr. Dodds-Parker has emphasised in his interesting and valuable contribution to the surgery of the appendix. That it is not only desirable but essential for the safety of the patient to perform a secondary operation for the removal of the appendix after it has been the seat of suppuration there can be no doubt whatever; it is a damaged organ and is therefore liable to be the cause of further trouble. I have often seen it distorted in outline, kinked, or bound down by

¹ Phil. Mag., July, 1872.

² See Origin, &c., the last sentence.

³ An. and Pts., under Dom., ii., p. 431.

⁴ Riddle, &c., 264, 5.

⁵ Vol. vi., p. 159.

adhesions, and I have also seen the distal part almost separated except for a band of cicatricial tissue. At the same time I suppose every surgeon can recall cases in which there has been no more trouble after an appendix abscess after the lapse of years, and where the patient was willing to run the risk rather than undergo a secondary operation; although this may occasionally happen it is well recognised that further trouble is by no means infrequent.

I should also like to refer to the frequency of ventral hernia in children after appendix abscess; I believe it is of more frequent occurrence than in the adult. I quite agree with Mr. Dodds-Parker that the rectus incision is the best for interval operations, but where a ventral hernia or a weak scar exists I excise the scar and utilise the incision for the removal of the appendix and restoration of the abdominal wall.

I have often been struck by the almost complete absence of adhesions after an appendix abscess, nothing but a few web-like threads are sometimes met with except just around the appendix itself, which may be bound down by adhesions to the cæcum and surrounding structures.

I am, Sir, yours faithfully,

Wimpole-street, W., Feb. 14th, 1912.

DOUGLAS DREW.

To the Editor of THE LANCET.

SIR,—I was much interested in Mr. A. P. Dodds-Parker's paper, read before the Oxford Medical Society on Dec. 8th, 1911, and published in your columns last week. For the past eight years it has been my practice to remove the appendix after every case of abscess formation as soon as the resulting sinus has healed (if I could obtain permission to do so). My experience with regard to hospital and private patients has been different from Mr. Dodds-Parker; that in the last eight years of hospital practice I have only had one patient who refused to be operated on after the abscess had healed, and, curiously enough, this patient returned one year later under my care with an acute attack of perforative appendicitis, so I was able to remove his appendix at the operation, whereas in private practice I have only been able to get permission in a few cases to remove the appendix after abscess formation, owing in most cases to the popular idea that the appendix is destroyed in such cases. My operative experience in the past eight years of removal of the appendix after abscess formation is 34 cases. In all these cases the appendix has been found and removed, and in all it could be clearly demonstrated that the appendix was not destroyed, the perforation had healed, and that the lumen was patent to a greater or lesser extent. Many contained concretions, and several were examined pathologically, and I firmly believe that every one of these appendices was capable of taking part in another attack of acute appendicitis. It was interesting to note that in two of my cases the appendices had been separated from the cæcum, and that the separated base had got attached to parietal peritoneum in the iliac fossa, the lumen was patent in both, and both contained concretions. And that such a separated appendix was capable of producing an acute attack of appendicitis I was able to prove a few weeks ago when I was asked to operate on a little boy 36 hours after the commencement of an acute attack. This patient had had a severe attack a year and a half previously but was not operated on. I found the appendix lying under the cæcum, its tip was attached to the mesentery of the small intestine, and its base was adherent to a mass of adhesions adherent to the peritoneum in the iliac fossa. Having separated this end I found it was a blind end also, there was a concretion in the middle and a perforation by the side of it. The pelvis was full of muco-pus. I could clearly demonstrate the healed stump of the appendix on the cæcum. Drainage tubes were used, there was no sign of a faecal fistula, and the wound healed in three weeks.

In conclusion, I should like to add that in THE LANCET of Oct. 1st, 1904, having operated on several of these cases, I published a case under the heading of "An Unusual Case of Suppurative Appendicitis." I published this case chiefly for the reason that Mr. (now Sir) Anthony Bowlby, in a clinical lecture published in THE LANCET of July 9th, 1904, p. 61, on these cases, stated that "suppuration destroyed the appendix." In my remarks on the case that I published I then stated that removal of the appendix after the occurrence of an abscess seems to me to be just as important as

removal of the appendix after an attack of non-suppurative appendicitis, and from further experience I am convinced that the appendix should always be removed after abscess formation, but if every case of appendicitis was operated on within the first 48 hours there would be no need to study the condition of the appendix after abscess formation!

I am, Sir, yours faithfully,

H. J. GODWIN,

Hon. Surgeon to the Royal Hampshire County Hospital, Winchester.
Winchester, Feb. 12th, 1912.

DIABETIC GANGRENE.

To the Editor of THE LANCET.

SIR,—Mr. Lockwood, in his clinical lecture on Amputation for Diabetic Gangrene,¹ after a very complimentary reference to my book on "Physiological Principles in Treatment," goes on to say, "And yet it seems strange to me that at the end of that chapter (on Diabetes and Acidosis) he does not say, 'Therefore you must give these patients as much oxygen as you can get them to take.'" He has apparently overlooked the fact that on p. 232 (second edition) I say, "In a condition which . . . we believe to be associated with deficient oxidation, to which the characteristic air-hunger also points, inhalations of oxygen may reasonably be tried." I go on to point out that as in acid intoxications the cell lacks oxygen because a link is missing, not much can be expected from such inhalations. Just in the same way the cell needs dextrose, but in severe diabetes is unable to utilise it. We need nitrogen in our food but cannot use the nitrogen of the air for this purpose. Mr. Lockwood has seen great benefit from oxygen inhalations in his operation cases of diabetic gangrene. I would suggest that this was because of their stimulating effect on the heart, for he notes that before administration there was marked cyanosis and the exuded blood was very dark. Such cyanosis is very unusual in uncomplicated diabetic coma, and even at the necropsy the blood is brighter than usual, presumably because of the defective interchange between the blood and the tissues. I think, therefore, that we are talking of rather different conditions.

I am, Sir, yours faithfully,

Welbeck-street, W.

W. LANGDON BROWN.

THE ADMISSION OF VENEREAL CASES TO GENERAL HOSPITALS.

To the Editor of THE LANCET.

SIR,—If your correspondent had read my letter more carefully she would have seen that I expressed no opinion whatever either for or against the admission of venereal cases to general hospitals, and certainly urged no plea, valid or otherwise, against the provision of special accommodation for them. On the contrary, I took their admission for granted, and urged that in addition to the special wards and special training for the medical men in charge of them, advocated by Dr. Douglas White and Lieutenant-Colonel Melville, there should be a special staff of trained women to nurse them. Surely "Charity's mantle" can be worn by experienced women as well as by young girls in training; and have young probationers no "claim on our human sympathies" so that we matrons may protest against their being compulsorily employed in a special class of nursing for which neither their age nor experience fits them?

I am, Sir, yours faithfully,

Feb. 13th, 1912.

A HOSPITAL MATRON.

GUARDIANS AND DISSECTION.

To the Editor of THE LANCET.

SIR,—The letter which you published last week from Mr. F. G. Parsons with reference to the refusal by the South-wark board of guardians to permit unclaimed bodies under their control to be used for dissection reveals a very serious state of affairs. All medical men must agree with Mr. Parsons's fair and logical statement of the case for the schools

¹ THE LANCET, Feb. 10th, 1912, p. 349.