which has recently occurred amongst the coolies in St. Lucia, Sir Charles Wood has expressed an opinion that the Indian coolies are likely to suffer from the loss of betel, to the consumption of which they are accustomed in their own country; and Sir C. Wood thinks it probable that an adequate supply of this article would not only be likely to promote the health of the immigrants, but that it would remove the temptation to consume vegetable substances of an unwholesome quality.

Under these circumstances, I have to request that you will take such steps as may seem to you proper for placing a supply of betel-nut within the reach of coolies, at a reasonable price.

I have, &c.,

(Signed)

NEWCASTLE.

Governor Darling, &c. &c., Jamaica."

LEPROSY.

The condition of the unfortunate sufferers from leprosy, and other forms of malignant ulceration, is surely discreditable to a Christian community, if the subjoined statement from the *Jamaica Advertiser*, of June 27th, be correct:—

"It is now more than six months since the Legislature of this island, with a liberality which most persons were not disposed to give them credit for, made provision for the reception and support of persons afflicted with leprosy and yaws; and the Government, with all its vaunted alacrity in carrying out measures for the public welfare, has not yet thought proper to exercise the power vested in it under the Act; while, on the other hand, the unfortunate victims of leprosy and yaws are to be met with in every part of the island—nay, at our very doors—seeking charity. Indifference on the part of the Government in a matter of such vital importance to the inhabitants of this island deserves the severest censure; and we trust that at the next meeting of the Legislature some honourable member, unencumbered with official obligations, will cause to be made known to the public why it is that so much delay has taken place in carrying out the provisions of the Act in question."

Medical men in Jamaica and other West India islands might contribute valuable aid to Professor Virchow, of Berlin, whose "Appeal on behalf of the History of Leprosy" was brought under the attention of the recent International Statistical Congress, and well deserves the cordial co-operation of the profession.

PUBLIC HOSPITAL AND LUNATIC ASYLUM.

It would seem that at length the object for which Dr. Bowerbank has, in spite of all difficulties and obstructions, laboured so energetically during the last two years—viz., a thorough and impartial inquiry into the condition and management of these institutions—is likely to be soon effected. The Committee of the Legislative Council of the island, to which the subject had been referred by the Governor, have recently reported their opinion, "that a commission should be appointed to inquire into the allegations respecting the government and management of the Public Hospital and Lunatic Asylum, and that such commission be composed of individuals entirely unconnected with the island."

As the formation of a school of medical instruction in connexion with the Public Hospital is at present openly canvassed, and is favourably entertained by men in authority, the necessity of a preliminary searching investigation into the whole matter is the more apparent, so that all grounds of public distrust may be effectually removed, and the two institutions, against which such grave charges have been made, be placed on the best possible footing as regards their salubrity and general régime, executive as well as administrative.

DEATH OF M. LEPERDRIEL.—M. Leperdriel, whose name has for many years been before the French public, has just died at his seat in the neighbourhood of Paris. Originally a herbalist, and subsequently a diplomated pharmacien, he made a large fortune by introducing into medical practice modes of blistering and establishing issues which became extensively known and used by dint of clever advertising. It was he who first introduced the india-rubber stockings, which have since been so much improved as to throw the original invention into the shade.

Correspondence.

"Audi alteram partem."

IS THE HEART ORGANIZED FOR PERFORMING OF ITS OWN ACT THE MOTION OF DIASTOLE AS WELL AS THAT OF SYSTOLE?

(LETTER FROM MR. J. MACLISE.)

To the Editor of THE LANCET.

SIR,—Having, in the communication lately addressed to you and your professional readers, stated some of the anatomical reasons which induce me to believe that the heart's pectoral throb is due solely to its diastolic or expanding motion, the above question next suggests itself to me, and this I have no hesitation in answering negatively, according to the structure and mechanism of the organ.

In interpreting the facts which I adduce as supportful of this opinion, I find myself, happily, under no necessity for disturbing the existing very rational, because demonstrable, ideas entertained respecting muscular action in general, whether of the voluntary or involuntary class of organs. We see that a muscle, when put in action, manifests a simultaneous contraction or shortening of its fibres between its points of origin and insertion; and that, on being extended, it relaxes, and is then wholly passive. To this rule, at least, there is no real exception; and hereupon we conclude that no *single* muscle or muscular viscus in the living body is or can be active in contrary directions for producing opposite conditions. Relaxation appears, in regard to contraction, a negative condition, just as shadow is in regard to light. Though we give the names flexor, extensor, levator, depressor, abductor, adductor, constrictor, dilator, &c., to different individual muscles, we know that these names have reference only to the part moved, for the several actions of these muscles are generically the same, being all contractile. When a part is capable of being moved in opposite directions, or set in opposite conditions, we find invariably that the muscular agents are placed in opposite pairs in reference to each other and to that part: the flexor contracts and acts on that bone in one direction; the extensor does the same to draw the bone in a contrary direction; and when the one is active, the other is passive or permissive. If the two muscles relax together, the bone lies quiescent; if the two contract, the bone becomes fixed; and if from between both we remove the bone, the result is similar to what occurs when we disconnect and remove the beam of a balance from its scales: the two muscles can no longer alternate in action, having lost their hold of the mediate member; and, now shrinking or contracting together, they become ever after incapable of extension. It is evident, therefore, that extension on the part of any *single* muscle is not effected by its own vital act: the motion does not obey volition, because the muscle is not organized for performing any motion but that which contraction effects; and that which is not possible for the voluntary muscle to do must be equally impossible with the involuntary. As extension and expansion are but different forms of the same kind of motion, I would apply the last observation to both, and add that neither motion can be performed by *one* muscle, unless by the contractile act of an antagonist.

Besides the antagonistic action of oppositely placed muscles being necessary for effecting contrary motions in a part, there is an anatomical condition no less required for the same, and one which invariably obtains: the opposing motor powers inserted into the part to be moved must have their origins in another fixed part. This is the relation between the members of the osseous and the muscular systems everywhere; and, on considering well that relation between those systems, I become struck with the conviction that herein alone exists a provision for effecting those motions. In the osseo-muscular thorax only do I recognise that mechanism which can effect expansion and contraction of enclosed space; and, on comparing the thorax as a motor organ for acting inspiratorily and expiratorily, with the heart as an organ supposed to be capable of acting diastolically as well as systolically, the difference between their respective mechanisms is sufficient to prove that the heart *per se* cannot dilate itself. The inspiratory diastole of the thorax can be effected by a certain class of muscles, because these having insertions externally in the costal walls take their origins from skeletal parts which do not enter into the composition of the

thorax; but the diastole of the heart cannot be effected by the muscular parts of that organ, which they form by having origin and insertion in itself.

The heart, as a muscular recipient organ, is absolutely isolated from all others in the body which by structural connexion might be supposed to effect its diastole. In whatever manner we dissect it for displaying its muscular layers, the only action which we can reasonably conceive these to be capable of performing is that of contraction, and this results in systole by reason of the united efforts of all those layers or enclosing bands. In this respect the heart is circumstanced like all the hollow muscular-coated viscera. These, we know, can contract for expulsion of their contents, and we know, too, that as recipients they must expand; but still we do not require to recognise in their walls a muscular mechanism whereby dilatation is accomplished. It is true that their expansion is evidently effected in a different mode from the diastole of the heart, occurring suddenly and dynamically, as instanced in the pectoral throb; and hence we are naturally inclined to seek for the agency of this motion in the substance of the organ, but hitherto our search has been conducted in vain. However we reason (*à priori* or *à posteriori*) upon the phenomenon of diastolic motion, the cause lies unrevealed by the heart's structure, and must ever remain so; for, in fact, the organ is not empowered either by vital or physical component parts to accomplish it.

It is certain, nevertheless, that diastole, as an effect, must be attributed to some cause; and, pressed by this necessity, it has been assumed by some physiologists that the motion is due to an elastic structure pervading the substance of the heart; but such structure is not demonstrable. By others it is imagined, that while some of the muscular bands are the agents for systole, a different set are the agents for diastole; but this idea is clearly untenable, for not only are those bands not paired antagonistically, but they are all united and concerted for the same act—viz., systole, and for no other. The belief that the heart, as a whole, is capable of acting for diastole and systole by the same muscular fibres cannot be entertained for reasons above mentioned; and to suppose that the momentum of the venous current *à tergo* is that which distends the heart's cavities is to mistake the effect for the cause, as may be proved by the law in physics: when vacuum is attempted in the interior of a pump, the fluid rises and frustrates the realization of it; and, as the heart's diastole cannot be effected without threatening vacuum in its cavities, the venous blood must of necessity rise and enter the heart. This, therefore, must be regarded as one cause of motion in the blood; but what is the cause of diastole is still a mystery.

While all the hollow viscera appear organized for active contraction, their dilative increase of capacity is manifestly effected by extrinsic causes, and these are of different kinds, according to the functions which the organs respectively serve. Dilator muscles none of them exhibit. They passively receive their contents, and do not take them in the sense of action. The pharynx is distended by the aliment, and, contracting on this, passes it into the oesophagus, which tube is in turn similarly distended, and so on throughout the entire canal. The vermiform motion of the bowel is a manifestation of muscular contraction on the *passant* distending contents. The stomach, urinary bladder, and gall sac swell by, and according to, the quantity of their respective contained fluids. The uterus increases in capacity by development *pari passu* with that of the fœtus. In these instances, we feel satisfied in having referred effect to its only true causation; but not so in regard to the heart, for who will assert that the returning blood is the agent of that organ's diastolic motion, when it has still to be proved that the afflux to it is not actually the effect of that very motion, and just as the ingress of air into the lungs is the effect of thoracic diastole? The inspiratory muscles, acting on the mechanically constituted thorax, declare themselves to be the prime movers of inspiration. To what agency are we to attribute the heart's diastole, since the organ itself is vitally incapacitated for performing the motion which, as such, must be due to some force? To this inquiry the result of my own researches and reading enable me to propose unhesitatingly the following answer—viz., the heart, as prime mover of circulation, is developed in diastole; this is its physical set or state *ab initio*; and, as secondary to this state in developmental time and process, the organ becomes endowed with vital energy—with muscular, contractile, systolic power. The physical state or diastole is, throughout the term of life, intermittingly antagonized by the vital act or systole; and whereas the latter is ingestive of the blood through the arterial channels, the former must of necessity be ingestive of the blood from the veins, according to the disposition of the cardiac valvular apparatus.

I would, then, Sir, submit it to you and the profession whether or not the anatomical facts and the physiological phenomena accord with these my ideas of the heart's action and motions? I ask upon what direct evidence have inquirers, from the time of Harvey to that of Hunter and to the present day, agreed to believe that the heart's pectoral throb is systolic, and what are the facts which do not declare the contrary? Have physiologists and physicians been all this weary time but labouring to build the pyramidal theory invertedly? If this should one of these days prove to be the only result of their observations, and it will, then the followers of Laennec will have to read through the stethoscope a long recantation of what they imagine they hear by means of that instrument—"Qui enim vias naturæ noverit, is deviationes etiam facilius observabit."

I am, Sir, yours obediently,

July, 1860.

JOSEPH MACLISE, F.R.C.S.

WHAT IS THE SIGNIFICATION OF THE HEART'S THROB?

(LETTER FROM DR. W. H. BROADBENT.)

To the Editor of THE LANCET.

SIR,—I venture to request some little space in your columns, for the purpose of controverting the arguments advanced by Mr. MacLise in support of his opinion that the heart's throb is diastolic, and to show that it can be, and is, systolic.

Taking his two propositions, I find the reasons adduced to prove the first, "the heart's throb cannot be systolic," to be—

1. The impossibility that the apex should strike against the left thoracic wall.
2. The fact that the beat of the heart and the pulsation in the distal arteries are not synchronous.

The apex may not strike against the parietes of the chest, and yet the impulse there felt may be systolic, as will be seen below; and synchronism between impulse and pulse is so far from being a necessary condition of the impulse's being systolic, that it would be incompatible with this fact. The arteries being elastic, pulsation in them at any distance from the heart must succeed systole, and, if impulse is systolic, impulse; and this succession can be traced from the heart along the arteries: the carotid pulse is just after the impulse, the radial pulse just behind the carotid.

The second position, that "the heart's throb can be caused by its diastolic motion," rests, in connexion with some *à priori* considerations, upon an experiment which shows that if the right ventricle be suddenly distended, an impulse can be perceived through the walls of the chest; but it is not explained by what force this distension, sudden and forcible as it must necessarily be, is to be produced; or, supposing it to be exerted by the "rising venous tide," how it is that the veins of the neck, which so readily show regurgitation from the right ventricle, do not also give some evidence of the tension they must suffer.

Numbers of facts (physiological and pathological) might be brought forward which this theory would utterly fail to account for; but a brief description of what is seen when the heart is exposed in vivisection will give it the most complete answer. And here I am compelled to say that I have observed facts the existence or occurrence of which Mr. MacLise denies by implication. I have seen blood flow "per saltum" from a divided artery while the heart has been beating under my eyes, and I have seen distinct and strong jets from the aorta after the heart had been detached and placed in water. The movement of the heart, therefore, under these circumstances, is *not* a mere spasm and relaxation without change in the capacity of its cavities; there is a real though imperfect diastole and systole, and conclusions as to the normal heart's action may safely be drawn from what is thus witnessed.

When the sternum and costal cartilages of a living animal are removed (artificial respiration being kept up by means of bellows), before the pericardium is opened, there may be observed a sudden bulging of the body of the ventricles, accompanied with a shortening, then a more gradual subsidence and elongation, of the heart in its containing sac. This is more distinctly seen when the pericardium is opened. During contraction, the apex is approximated to the base, so that the heart becomes more globular; at the same time it becomes more firm, and there is an evident bulging of the body of the ventricles, resulting from the shortening in their axis, and the contraction